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IS PHYSICALISM “REALLY” TRUE?

An Empirical Argument Against the Universal Construal of Physicalism

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IS PHYSICALISM “REALLY” TRUE?

An Empirical Argument Against the Universal Construal of Physicalism

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DEDICATION

To the men and women who in the face of unrelenting criticism (and often thinly-veiled disdain) from the mainstream have nonetheless dared to pursue the science that supports my conclusions. Without their efforts, my argument could never have even gotten off the ground.

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IS PHYSICALISM “REALLY” TRUE?

An Empirical Argument Against the Universal Construal of Physicalism

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The University of Texas at Austin, 2009

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Physicalism as universally construed is the thesis that everything in the world is either physical or a consequence of physical facts. Certain consequences of physicalism for free will, religion, and so on make it unpalatable to some. Physicalism should not be dismissed merely on its unpalatability. Nonetheless, we should be very sure it is true before accepting it uncritically (as much of science and philosophy now do). Physicalism is a contingent thesis, taken as true on the basis of strong inductive evidence and an inference-to-the-best-explanation that specifies it as the best theory over any of its competitors to provide an ontological account of the universe. So long as there is no contrary evidence to the claims of physicalism, then it stands relatively uncontested.

I argue that there is a body of well-attested empirical evidence that falsifies universally-construed physicalism by violating an essential assumption of the theory –

causal closure of the physical domain. I present a detailed account of this closure-violating evidence. So that those who are unfamiliar with the body of evidence on offer may judge its validity, I include brief summations of experimental designs, findings, and analyses, plus some controversies pertaining to the data and their resolutions. I then argue why this body of empirical evidence should count against universal physicalism, argue for the evidence's scientific legitimacy, and discuss criticisms which have been lodged against it, then explain why these criticisms lack force.

I conclude that the evidence I present is sufficient to falsify the universal construal of physicalism as supported by today's and by foreseeable future understandings of the physical world. I acknowledge, though, that nothing can be guaranteed against an indefinite "wait-and-see" argument for some implausible "fully-realized" physics that *may* be able to reconcile the evidence I propose with such a fully-completed formulation of physicalism. I suggest that if this is the best physicalists can come up with, then their position is weak and the inference-to-the-best-explanation that until now supported universal physicalism should be turned around to tell against the theory.

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Chapter 1: Physicalism – True or False?

One of the thorniest problems remaining to the philosophy of mind is how to reconcile phenomenal and other features of human mentality with the rest of nature, given the truth of physicalism. By physicalism I mean the general assertion that all causality is strictly physical and all events are physical events; that all entities are either literally physical, physically realized, or a consequence of only physical facts or states of affairs.¹ For decades now there has been an ongoing argument between physicalists and anti-physicalists as to whether physics really is ontologically privileged to provide the one true explanatory framework for the universe. Though the contest seems to be going in favor of the physicalists, it is by no means yet settled. In this project I aim to discover what it would take to prove physicalism false. My first goal is to explore what it would take to show physicalism to be true, on the way to finding out what it would take to prove it false. I will argue that the former is much harder to do than the latter.

It is uncontroversial that science research programmes² must be embedded in broader theoretical contexts. Such contexts provide a framework for any new research results that are produced, within which these can be interpreted and integrated. When once

¹I will have more to say about the definition of physicalism later in this chapter.

²I use this term in the sense proposed by philosopher of science Imre Lakatos, as a guiding framework of core theory and complementing auxiliary hypotheses that guide a broad scientific research campaign. It is roughly the same level as but somewhat different from a Kuhnian “paradigm.”

confirmed, these results provide additional context for yet newer experimental results and *their* interpretation and integration. It is only slightly more controversial to suggest that a large majority of those involved in these programmes (the scientists and researchers), as well as those on the periphery (funders, interpreters, observers of scientific process), believe the theoretical context to actually be more or less true. I also take it as relatively undisputed that physicalism provides that context for the vast majority of scientists occupied in the various pursuits of science and for philosophers engaged in examining and wrestling with certain classes of questions posed by or about science.

Physicalism is not, however, just a methodological framework for scientific research. It is an ontological doctrine that grounds virtually all of science and particularly, for our interests here, those philosophical theories where ontology is relevant.³ Further, when fully realized, physicalism promises profound social, legal, and practical implications with important consequences for society as a whole.

There is little question that the doctrine holds the high ground in relevant ontological debates, particularly concerning philosophy of mind. According to Crane and Mellor (1990), “physicalism is now almost orthodox in much philosophy, notably in much recent philosophy of mind.” Andrew Melnyk notes, “a huge preponderance of current philosophers of mind happily call themselves physicalists” (2003a, 5) (even if, as

³All that follows assumes a general notion of scientific realism. The question of whether physicalism is true *or* false has little relevance outside such a realist context. As Crane & Mellor (1990) note, “physicalism is not a doctrine about universals or other abstract objects, but about the empirical world, and specifically about minds. It says that mental entities, properties, relations and facts are all really physical.” Physicalists, they

he suspects, they are often “simply taking physicalism for granted”). David Papineau agrees: “[N]early all analytic philosophers in this area . . . now accept that the mind is in some way constitutively connected with the brain” (Papineau, 2001).

Despite its widespread acceptance, the physicalist doctrine remains controversial. If physicalism were only about the mind-external physical world, there would be relatively few problems, as there is little controversy to the physicalistic-nature of explanations in physics and nearly all of the special sciences. The trouble mostly starts when we try to make sense of mind. As Jaegwon Kim puts it: “[G]iving up the Cartesian conception of minds as immaterial substances in favor of a materialist ontology does not make the [mind-body] problem go away. On the contrary, our basic physicalist commitments can be seen as the source of our current difficulties” (Kim, 2005, 9). Most philosophers of mind would likely agree.

But this raises a question: Whether “our current difficulties” are only due-course problems that merely require a bit of tidying up, or instead indicators that the move to link all aspects of mind to some physical substrate might at least be partly on the wrong track. The history of science shows that believing a current theoretical context to be true (no matter how passionately), and its actually *being* true, are two very different things. If history is a guide, the current faith in the truth of the physicalist paradigm (to use Kuhn’s now thread-bare term) may eventually turn out to be at least partially misplaced. Many of physicalism’s own disciples would agree with this at least conditionally, choosing to

further observe, “all grant physical science a unique ontological authority: the authority to tell us what there is.”

phrase their claim approximately thus: “Physicalism as we now formulate it likely isn’t fully true; but it is much closer than anything science has developed thus far. And in those areas where physicalism is wrong, we’re sure it won’t turn out to be *very* wrong.”

1. The Problem

Robinson (2001) offers this summation of the problem physicalism encounters with respect to mind:

“In order to vindicate a materialist theory of the mind it is necessary to show how something that is a purely physical object can satisfy psychological predicates. Those features of the mind which seem to be, *prima facie*, incompatible with this physicalism – such as consciousness and the intentionality of thought – must, therefore, be explained in a way that purges them of their apparently Cartesian elements, which would be incompatible with materialism” (128)

Our strongest intuitions lead us to believe that by virtue of our being conscious we are the masters of our bodies, we have free will, that our apparently autonomous minds make a difference in a world which to all appearances is otherwise physical and deterministic. Many react with a shock of disbelief when first hearing that there seem to be good reasons to believe that our choice of what to have for breakfast day after tomorrow may well have been determined many millennia ago by the circumstances that obtained milliseconds after the Big Bang (perhaps jostled around a bit here and there in the meantime by random events owing to quantum phenomena, etc.).

Kim captures the issue neatly: “When physicalism is accepted as a basic framework, the foremost metaphysical question about the mind is where in the physical world our minds and mentality fit – indeed whether minds have a place in a physical

world at all.” (Kim, 2005, 149-50). Certain apparently essential features of physicality get in the way of intuitions about our mental nature – features such as determinism and indeterminism, the requirement for causal closure, the “completeness” of physics, and the causal exclusion principle (which aims to avoid the violation of parsimony presented by overdetermination of physical causes by mental ones). If these and other principles are true with regard to mentality, then prospects of being able to justify our intuitions about the nature of human mentality look rather grim.

Certainly, if physicalism is true, there are consequences for our understanding of mind: We may have to abandon our beliefs in the uniqueness of human mentality – intentionality, consciousness, and undetermined cognition. Importantly, it becomes unclear how we warrant free will. Despite their best efforts, compatibilists have been unable to convincingly show how we can maintain a robust notion of agency in the face of deterministic and indeterministic forces that come as part of physicalism’s package-deal. Libertarian/incompatibilists are even more at a loss. “The problem of determinism threatens human agency and skepticism puts human knowledge in peril,” observes Kim. “The stakes seem even higher with mental causation, for this problem threatens to take away both agency and cognition” (1998, 32).

The implications of physicalism’s turning out true extend beyond just our conception of ourselves. Societal consequences could prove to be profound.⁴ If we discover that physicalism really is fully true, and free will cannot finally be satisfactorily

rescued (as seems the likely outcome for now), broad social consequences may affect everything from governance, to judicial systems, to education, to parental rights. Further, if physicalism proves fully true, then all notions of God and religion can be relegated to the dustbin of history (some physicalists are ready to do that now – for example Jeffrey Poland, who worries that “ghosts, gods, and the paranormal are genuine threats to physicalism” (1994, 228)). While many may applaud such a move (and I am not interested here in arguing the merits or demerits of either theism or religion in general), nonetheless conclusive evidence that led to the dissolution of religion would have profound human consequences, both for good and ill.

These are just a few examples of perhaps a multitude of effects we might expect if physicalism asserts the full influence it promises. They are not meant as arguments *against* physicalism. After all, if physicalism is fully true and our intuitions about mind, religion, and so on are false, that would be important to know. We may have to put up with interim personal and social upheaval on the way to a better awareness of who we are and what our place in the universe really amounts to.⁵ Thus, according to Jaegwon Kim:

If physicalism is to survive as a worldview for us, it must show just where we belong in the physical world, and this means that it must give an account of our status as conscious creatures with powers to affect our surroundings in virtue of our consciousness and mentality. (2005, 31)

⁴Indeed, we are already seeing some of these consequences begin to emerge, given the influence of physicalist beliefs already present in science and philosophy as they are practically implemented in the outward culture.

⁵On the other hand, we also would not want to jettison possibly true beliefs on the basis of an only partially-completed theory that, when completed, might turn out be fundamentally misconstrued or outright false.

Nonetheless, evolving philosophical and scientific debate over the past century increasingly supports the notion that mental properties and states are just a special case of the interaction of physical properties. The emerging conclusions (and consensus) is largely underwritten by principles firmly rooted in physicalist doctrine.

But the situation remains somewhat less rigid than the holders of more radical physicalist positions would prefer. As physicalist Andrew Melnyk (2003a) remarks, “It is best to admit candidly that the evidence for regarding mental phenomena as physical or physically realized is much weaker than the evidence for regarding chemical, biological, geological, and similar phenomena as physical or physically realized” (p.281).

Consequently, there remains worry about – and some resistance to – the physical-mind thesis. It seems we have much to lose if we must surrender the fondly-held belief of mind being causally relevant by virtue of *being* mind, and not just as a complex realization of underlying physical facts.

2. Figuring out Physicalism: What physicalism is...and isn't

Though physicalists tend to present a united front when resisting anti-physicalist arguments, they continue to struggle among themselves over the question of how to delineate a complete and sufficient formulation of their own physicalist doctrine. The problem rests in how to specify boundaries that capture everything that needs inclusion among the set of things that are physical or physically-dependent, while eliminating anything that is not in some fundamentally-justifiable sense ontologically physical.

Broadly construed, definitions of physicalism don't drift too far from a central core of agreement. According to Papineau, for example, "Physicalism is the doctrine that everything, including prima facie nonphysical stuff is physical" (2001, 12). Latham proposes that physicalism be "...understood as the view that the world is governed by laws of succession with purely physical antecedents" (2001, 153). Gene Witmer (2001), after thorough deliberation on a series of ever more precisely-formulated specification statements for the doctrine, arrives at the following. Physicalism requires that:

Every law of nature and every particular fact is either physical or to be explained by the physical in such a way as to imply that the nonphysical facts are nothing over and above the physical facts, where the physical facts include the actual distribution of physical properties and the laws of physics" (69)

And Kim (2005) provides us with this:

The most fundamental tenet of physicalism concerns the ontology of the world. It claims that the content of the world is wholly exhausted by matter. Material things are all the things that there are; there is nothing inside the spacetime world that isn't material, and of course there is nothing outside it either. The spacetime world is the whole world, and material things, bits of matter and complex structures made up of bits of matter, are its only inhabitants. (p.150)

The preceding formulations tacitly subsume the ontological status of the mental under the physical. Other philosophers, however, mention mind more explicitly. For one, Barbara Montero (2003, 179) describes one popular view as being that "physicalism with respect to the mental should be understood as the thesis that the mental is reducible to (or composed of, or otherwise determined in some sense by) the non-mental."

Georges Rey divides physicalism into sub-categories, the second of which he labels "substantive, explanatory physicalism," which he hopes eventually will provide "some

genuinely explanatory framework in which psychological states can actually be understood as some kind of physical state” (2001, 116)

Just what that framework might be is where one level of the debate begins. Modern philosophy of mind began with the central-state materialism (or mind-body identity) theory. When it became evident that direct mental-property-to-physical-property reductions were unlikely to succeed (thanks in varying degrees to the multiple-realization argument and intractable shortcomings in neuroscience investigative technologies) alternatives such as functionalist theories of mind and non-reductive physicalism were developed to fill the gap. Functionalism distances mental-as-physical explanations from precise reductionist mechanisms, while non-reductive physicalism stipulates that, though the mental depends ontologically on the physical, precise reductions of the mental to the physical are metaphysically impossible.

Wording and construal in formulations of physicalism vary in how their proponents characterize them, but conceptually they hew to a general line of argument. Physicalism defines all that the universe contains. And physicalism is defined by the facts contained within and the entities investigated by the discipline of Physics.

But this is where the main difficulty begins. What exactly does physics itself contain and investigate? This, it turns out, is by no means as clear as we would like. Not long after physicalism coalesced into a distinct doctrine, Carl Hempel (1969, 1980; but more explicitly stated in Hellman, 1985) defined a core difficulty. If we intend to use what is physical to delineate physicalism, we must first know what it means to be physical. The discipline of physics defines for us what is physical. But what do we mean

by physics? Do we mean physics as we understand it today? If so, then we must conclude that it is false, since we know that the physics of today is undoubtedly flawed or mistaken in at least some of its aspects, and certainly incomplete in its descriptions and explanations of the world. So if present physics is at least partially false and incomplete, then the alternative is to define physicalism in terms of a future, fully-realized physics. But that becomes question begging. We have no clear picture of what a future, fully-realized physics would even look like, making that of little use to us now as we try to define physicalism. We know what physicalism is: it is the doctrine that the world is fully and solely physical. But we don't know (for sure) what "the world is physical" fully means. This seems to leave an unsatisfactory vagueness lurking at the heart of the physicalist doctrine.⁶

A number of philosophers are less intimidated by the bite of Hempel's dilemma. Loewer (2001, p.40), for example, follows Papineau (1993) in using possible-worlds language which "require[s] that fundamental physical predicates (the atomic predicates of the language in which the complete physical description of the world is expressed) are not mental" [by 'mental' he means 'intentional' or 'phenomenal']. This seems rather

⁶Crane & Mellor (1990) use a version of Hempel's Dilemma to attack the notion of "reducibility in principle" ("RIP" – that all phenomena are 'in principle' reducible to physical facts). If present physics is incomplete or false, then it can't be reliably used as a reduction base, even an in-principle one. But to rely on a reducibility-in-principle notion to specify what a *future* physics will have to include "if is to cover everything physical, is obviously viciously circular." They conclude that we can't define the physical as "what is reducible in principle to physics," whether that physics is the current version or an ideally-realized future version.

ontologically unsatisfying, in that it appears merely to define away the problem posed by Hempel's dilemma, rather than to solve it.

Papineau (2002) provides a somewhat different version of the argument. It is not necessary to know what a complete physics would include, he believes, so long as we "know what it won't include" (p.41). Supposing for the sake of the argument that we identify those things that count as mental. We then label everything else as "non-mentally identifiable." If we then presume that our sense of physical includes the requirement that non-mentally identifiable effects are always and only the result of non-mentally identifiable causes, then we are forced to conclude that mental properties (since they clearly have effects manifest in the non-mental world) are "identical with (or realized by) something non-mentally identifiable." At first this appears circular, since it seems to merely demand that mental properties, even if we linguistically identify them as uniquely mental, must nonetheless be the product of physical properties – thus assuming the consequent of what we are trying to prove. But for Papineau's argument we must add the further condition that the physical domain be causally closed (this is a version of the argument from causal closure, which I will discuss below). Given that essential stipulation, the argument seems sound.⁷

Melnyk (2003a, 12-13), after discussing how Jeffrey Poland and Frank Jackson both attempted unsatisfactory responses to Hempel's dilemma, offers a more complex analysis, which boils down to this: If physicalism is explicated according to current

physics, and given a physicalist as one who weighs scientific hypotheses according to a scientific-realist view of the world (that is, hypotheses are “true or false in virtue of the way the mind-independent world is” [p.229]), then just because the presumed incompleteness/falseness of current physics renders today’s formulations of physicalism equally false or incomplete, this “provides no reason by itself to abandon being a physicalist; one can remain a physicalist, even though physicalism is unlikely, just so long as it is more likely than its relevant rivals” (p.234)

This strategy seems less than compelling as well. Rather than provide a successful dodge around Hempel’s dilemma, it seems instead to misdirect us from the major point. Melnyk’s argument doesn’t resolve the dilemma, but only makes it “okay” to remain a physicalist and still retain one’s dignity and integrity simply because physicalism is just slightly more likely to be true than its competitors.⁸ But this integrity is maintained at the cost of seriously vitiating the pull of physicalism which, after all, relies crucially on its presumption of truth. Melnyk appears to want us to continue to accept the implications of physicalism while leaving us much less warrant to believe the physicalist argument itself.

As it stands, there seems no clear way around Hempel’s dilemma, but that doesn’t seem to matter. Many, indeed probably most philosophers of mind are aware of the

⁷We will discover in the next chapter that this is more problematic than it presently appears – that in fact the causal closure argument merely pushes the circularity back one level.

⁸Integral to Melnyk’s argument is his reliance on abduction, “inference to the best explanation,” which I will soon consider in some detail in chapter 3.

problem (Papineau, 2002; Montero, 1999 & 2003; Melnyk, 2005; Loewer, 2001; Crane & Mellor, 1990; and Poland, 1994, among others discuss it, and some, as mentioned, have tried to tackle it). In the absence of a survey, it is impossible to know whether many scientists are cognizant of the dilemma. But whether they know of it or not, it is not unreasonable to speculate that (based on their actions and utterances) most scientists and many philosophers are inclined to ignore the obstacle presented by Hempel. Recalcitrant as the problem is, it seems not to have dissuaded anyone from embracing physicalism. Perhaps this is due to a feeling of (at least) some confidence that, if today's version of physical theory is not fully correct, it is at least largely so, and in fact is sufficient for us to recognize when some claim (say, that ghosts exist) clearly violates a reasonable construal of what it means to be physical. Otherwise, Melnyk's assertion (cited earlier) that, "a huge preponderance of current philosophers of mind happily call themselves physicalists" would be difficult to support. As Barbara Montero suggests,

Some may argue understanding the term 'physical' is no more difficult than understanding the term 'table.' They may point out that while we cannot provide necessary and sufficient conditions for *tablehood*, we nonetheless understand the concept because we can readily identify things that are clearly tables as well as things that are clearly not. And the same is true, they may argue, of our notion of the physical. (1999, 183-84)⁹

There clearly remain slippery issues in trying to form a more rigorous definition of what is meant by "physical," but there does seem to be a consensus of sorts – just not what we normally expect of a consensus upon hearing that term. It is this: That for most

⁹Montero herself does not wholeheartedly endorse this view, arguing that even if we can identify central cases of physically-constituted objects, we need a broader theory

philosophers these days physicalism (in this respect much like pornography) is something you know when you see it. As Frank Jackson (2006) put it “it has always seemed to me that we have a good enough idea of what physicalists have in mind to make sense of the debate over physicalism” (p.234).

With this final consideration, vague though it may be, of how we might define physicalism, I now embark on the balance of my project. This will unfold as follows. Chapter two explores the notion of what it would take to prove the truth of physicalism. We shall quickly see that is not so easily done, and that physicalism (for now) is not so much “proved” to be true, as that its truth claims are accepted on the basis of an admittedly very robust inference to the best explanation.

Chapter three covers arguments that have been launched against physicalism, yet eventually concludes that these arguments go aground on epistemic shoals, falling to the strength of the inference to the best explanation just mentioned. In chapter four I propose an alternative strategy that, depending on one’s point of view, either threatens or promises to defeat physicalism’s truth claims by falsifying the induction that underwrites the inference to the best explanation supporting the physicalist doctrine. I explain that physicalism’s vulnerability lies in the causal closure assumption, and that empirical evidence with certain properties is necessary to defeat closure.

I begin chapter five by making explicit the set of evidence I will offer as a counterexample to causal closure and, thus, to physicalism. The rest of chapter five, and

to tell us “how we are supposed to go on from there” in determining less obvious instances of physical entities.

chapters six through eight will give a detailed account of the evidence and its provenance. Chapter nine will begin a consideration of the philosophical status of this evidence which will be completed in the first section of chapter ten. The remainder of the chapter examines the criteria we should require of evidence for it to count as scientific, and argues that the evidence I present meets those criteria. In chapter 11 I will engage and (hopefully) put to rest some of the more prominent objections that might be lodged against the evidence, and then will argue that, if the evidence I am presenting is rejected as insufficiently meeting the criteria of acceptability, then the legitimacy of conventional scientific evidence may be called into question as well on similar grounds. In the final chapter I entertain implications and the conclusions they lead to that physicalism is false...or, perhaps, just not as true as we thought it was.

Chapter 2: The Case For Physicalism

Even if the state of current physics' incompleteness means there may be no fully sufficient definition of physicalism, there are certain important features typically attributed to or required of the doctrine that help us better define physicalism's boundaries. I would argue that, in fact, the acceptance of these features helps boost the confidence for many physicalists that physicalism, while based on an incomplete physics, is likely nonetheless fundamentally true. Foremost among these is the principle of causal closure.

Causal Closure

Physicalist doctrine relies on various underlying assumptions, but for important reasons one that is indispensable is causal closure. The causal closure stipulation requires all causal chains to be instantiated entirely within the physical domain, as a consequence of underlying physical facts.

There are a number of ways of formulating causal closure (Montero, 2003). For one, Kim (1998) puts it this way: "no causal chain will ever cross the boundary between the physical and the nonphysical." Thus, "if you pick any physical event and trace out its causal ancestry or posterity, that will never take you outside the physical domain" (p.40). Melnyk proposes that physicalism "entails the closure thesis that for every physical token there is a preceding physical condition that is sufficient for it, given the physical laws....For suppose there were a physical token that had *no* preceding sufficient condition

that was physical; since, by universal determinism, this token must have *some* preceding sufficient condition, this condition would have to be in part nonphysical and nonphysically realized, which would contradict the assumption of [physicalism]” (Melnyk, 2003a, 222).

If (as I stated in the opening paragraph in chapter 1) physicalism requires all causation, events, and entities to be “literally physical, physically realized, or a consequence of only physical facts or states of affairs,” then causal closure must necessarily be true if physicalism is to be true. For if closure is false, causes can enter from outside the physical domain, thus rendering physicalism false. Because of this, the causal closure principle is taken as foundational for physicalism, and treated axiomatically. Raymont declares that, “every good physicalist endorses the principle of closure” (2003, 225) And as Melnyk observes, “[I]f this closure thesis were found to be false, then, so long as the assumption of universal determinism was retained...physicalism would have to be rejected as false also” (2003a, 222).¹

Causal Exclusion

Other candidate principles have been offered as necessary to a full-fledged physicalism. Causal exclusion (Kim, 2005; Raymont, 2003; Yablo, 1992) seeks to avoid overdetermination of physical causes by mental ones. Exclusion requires that an event in the physical world with a sufficient physical cause not be overdetermined by the presence

¹As will shortly be discussed, there is some question whether determinism is even necessary to this formulation.

of any non-physical mental cause aimed at the same event. If closure is true, it eliminates the possibility of extra-physical sufficient causes intruding on physical causal chains. But that by itself leaves room for fundamentally-mental (hence non-physical) forces to add superfluous layers of (non-necessary, non-sufficient) causation to otherwise sufficiently physical events.

Such overdetermination is offensive in two respects: first, it violates our desire for parsimonious explanations; if one set of causes is sufficient, why populate an explanation with more when there is neither need nor evidence for them? Since physicalists maintain that mental causes are just physical causes, then there is no reason to include talk of mental causes in our causal explanations. The second reason for the exclusion principle is that to postulate such mental causes (or even simply epiphenomenal mental states) in addition to physical ones is to dilute our notions of physicalism. If non-physical mental causes or states exist, no matter how causally ephemeral they may be, it still undermines the physicalist's claim to physicalism's exclusivity, and leaves the door open to anti-physicalists who want to entertain the possibility of more efficacious non-physical causal states.

Notably, exclusion doesn't enjoy the same universal endorsement from physicalists as does closure. There are various arguments against it, amounting in effect to a concern that the worry about overdetermination is somewhat unparsimonious itself. Exclusion is not necessary to a weakened form of physicalism, in which epiphenomenal mental properties and overdetermination by mental causes are not particularly objectionable, just so long as physical causes are still decisive in all causation in the

universe. For if an event has a sufficient physical cause, plus a non-necessary, overdetermining mental cause, then that event is still physically determined, no matter what extraneous role the mental cause might play.

One philosopher puts it this way: There is no convincing reason why “...there is anything problematic in the notion of multiple explanantia bearing down on a single explanandum...Merely to allow for such explanatory overdetermination is not yet to vindicate the causal relevance of mental properties, for it must also be shown that they, at least some of the time, make a novel explanatory contribution.” But if it is ever shown that mental properties actually do have relevant causal properties, there would be “no reason to deny that they stand in causal relevance relations to both mental and physical explananda” (Raymont, 2003, 240). In other words, either non-physical mental causes (if they exist) are overdeterminate and superfluous, or they are epiphenomenal and can safely be ignored. On the other hand, should they ever be found to actually have causal relevance, they would need to be included in our ontology. If physicalism thus requires at least that much amending, that is the price of arriving at a fuller understanding of the universe. But either way, the exclusion principle may thus itself be superfluous, given the truth of closure.

Determinism

A further principle important to physicalism is determinism, the oft-demonstrated principle that events must have sufficient causes; that causes must themselves have a fully-sufficient causal history traceable in-principle back as far as the universe goes (with

some exceptions governed by truly indeterministic, yet still physical events); and that all causes are consistent with all other neighboring causes in the metaphysical context they share.

While it is evident that determinism is an important feature in our world, we must question whether it is essential to a specification of physicalism. Can we conceive of a physical world that is profoundly indeterministic, where no facts determine any other facts? In other words, a world that is completely and only indeterministic (yet still physical)? There seem to be two ways such a world might be constituted: It might be fully chaotic in the classical sense (not in the determinate sense chaos theory is understood today), but there is no reason that we know to resist the idea that such a world could still be fully *physical*. Perhaps, though, certain deterministic relations develop necessarily any time a sufficient number of fundamental physical entities populate a physical domain. It might then be reasonable to entertain the possibility of an indeterministic world so long as that world were sufficiently sparsely populated.

In either case, it seems fully possible to conceive of a physical world without determinism, so determinism appears not to be essential to physicalism. We can propose that, perhaps, determinism is a necessary consequence *of* physicalism. But it is perhaps open to question whether determinism is necessary *for* physicalism.²

²Interestingly, Papineau (2002, 236-241) is persuaded that, while *physical* determinism can preclude strictly mental causes, determinism in and of itself does not necessarily preclude the presence of non-physical mental causation within the physical domain. This is because it is possible that a causal mental, non-physical state could itself

The “Completeness” of Physics

The term “completeness of physics” has two meanings. It can refer to the hope that our *incomplete* understanding of physical laws and phenomena will eventually be remedied, and we will arrive at a full (complete) picture of physics. But it can also refer to another specified feature of physicalism, that the physical world is complete in the sense that it contains all that is needed to fill out reality, and therefore there is neither room nor need for any sort of (presumptive) non-physical causes or entities. It might seem obvious that this is largely just a matter of perspectival difference from formulations of physicalism’s other features, such as closure, exclusion, and determinism/indeterminism. But there is a difference in perspective, at the very least.

Though other philosophers sometimes make use of the “completeness” locution, Papineau is its main proponent (Papineau, 2002; 2001). He does not avail himself so much of the ‘closure’ or ‘exclusion’ terminology, choosing instead to talk of the ‘completeness of physics.’ From the perspective of causation, the physical world is necessarily complete. But the concept parses differently than causal closure, in that there is a sense in which causation in the physical world is affirmed as being fully sufficient, rather than defined by eliminating the possibility of any other kind of causation. In other words, as opposed to the sense in which causal closure uses language to seem to actively exclude extra-physical causes, completeness stipulates only that there is no *need* of or use for non-physical causes. So, “all physical effects are fully caused by purely *physical* prior histories” (Papineau, 2002, 17 – though in a footnote he offers a stricter formulation

behave deterministically.

to take indeterministic events into account, as well). In a certain sense, an overt causal closure stipulation is unneeded because the only causation that is effective, indeed the only causation that exists, is already present exclusively here in physicality.

Of course, the notion behind causal closure was never one of blocking extra-physical causes that existed but should not be allowed admittance to physicality. Rather, the presumption has always been that no such extra-physical causes exist, and closure was formulated as it is merely as a convenient way of specifying the boundaries of physical causation. In that sense, the completeness formulation is merely a different way of stating closure in that completeness, like closure, denies the presence of extra-physical causation.

In concluding this section, it seems apparent that the crucial feature of physicalism is the causal closure principle. The exclusion principle, while (perhaps) useful, is unnecessary. Determinism, which gives physicalism its bite with regard to the free will and agency issue, may be entailed by, but does not seem to be necessary to physicalism. And completeness seems to derive its efficacy from the closure principle. What this boils down to, it seems, is that physicalism is true only if causal closure is true. We will discuss this in additional detail shortly.

Proving Physicalism True

It may seem strange that we don't know to be fully true a doctrine (physicalism) that the preponderance of scientists and philosophers seem to accept almost implicitly as

true. But that is the situation physicalism confronts us with – a situation that leads to the question: How can we finally prove physicalism to be true? For the physicalist, the answer may prove discouraging.

What would prove physicalism true? Answering this question (and the one to follow: What would prove physicalism false?) requires that we understand what sort of a thesis physicalism is. I paraphrase an argument I heard in 1996 from Professor Georges Rey: “We know physicalism is true,” he said, “because we have many instances where something was believed to be non-physical, but which after rigorous scientific investigation turned out after all to be physical. And we know of none that, after a thorough examination haven’t turned out to physical. And once we have been able to thoroughly examined the mind, I’m sure we will find it to be physical, too.”

Subsequently, he made a roughly similar argument in a paper defending physicalism.

[I]t appears as though every regularity in the spatio-temporal world is, in one way or another, susceptible to an ultimately physical (and/or mathematical) explanation....[I] think there can be no doubt that paths through [successive reductive] levels, each one providing, inter alia, more fine-grained mechanisms than the last, appear to exist at least for every *nonmental* regularity. One reason for applying the strategy to the mind is the reasonable expectation that mental regularities, particularly those involving patently spatiotemporal phenomena, will not prove to be some odd exception. (Rey, 2001, 100-101)

This is a general formulation of what is often referred to as the “success of science” argument against non-physical explanations of mind. Melnyk echoes Rey’s position by arguing that the preponderance of the evidence from other fields suggest that ultimately the mental will turn out to be physical as well (Melnyk, 2003a, 258). Melnyk further elaborates on the theme – the success of physics so far in “finding sufficient

physical causes...provides inductive evidence that *all* physical events, including *both* unexamined physical events *and* examined-but-as-yet-unexplained physical events, have sufficient physical causes” (p.289). One begins from this to gain a basic understanding of the underlying argument for physicalism. Indeed, my discussion here is not an argument so much as a reminder. Physicalistic assumptions are taken to be so fundamental that we often forget that physicalism is not an *a priori* position, but rather one that is *a posteriori*.³ “But of course the world is physicalistic,” one might say. “What else *could* it be?”

Philosophy traditionally prefers *a priori* analyses since, among other things, these are less vulnerable to mistakes or changes in human perceptions of the world, mistakes in interpreting contingent and necessary truths, and gaps or confusions in our understanding of nature. When philosophers entertain empirical or *a posteriori* content at all, it is usually only to the extent that it informs the philosophical context in some useful way.

Physicalism, however, is an unusual departure from this, in that it is at root *a posteriori*, a covertly empirical proposition that despite this inherent empirical nature is pressed into service to ground subsequent logical and ostensibly *a priori* argumentative structure (Papineau, 2001; Poland, 1994; Melnyk, 2003a). I use “covert” here not because physicalism’s empirical content would be denied by its advocates if cornered, but because in arguments which appeal to the doctrine for justification its own empirical origins are often tacitly ignored and physicalism treated as if it were as settled as if

³I am momentarily side-stepping the *a priori* vs. *a posteriori* physicalism debate, but will engage it before the end of this chapter.

established on an *a priori* basis.

A simplistic formulation (though Melnyk lodges a similar argument – see Melnyk, 2003b, 160-161) of one leg of the physicalist argument might run somewhat as follows: All phenomena that have been investigated thoroughly by Physics have turned out to be physical in nature. Because of Physics’s success in describing/explaining the universe in exclusively physical terms, it is highly likely that phenomena not yet fully elucidated in physical terms will one day be found to be fully amenable to physical explanation. [A corollary: Even for phenomena not yet explained, we may have plausible conjectures as to how they *might* fit into a fully-realized physical theory.] Any phenomenon that can’t be reconciled with a physical explanation is likely not a real phenomenon.⁴

Thus formulated, this argument looks very much to be circular. If evidence for any non-physical phenomena was ever found to exist, it would be ruled not to exist no matter how robust because no physical explanation could be found.⁵ In the interest of space, I will not attempt to review arguments defending against circularity accusations

⁴ The step from not-physical to not-real might be deemed objectionable or tendentious by some. However, I believe there are more than ample grounds to characterize the position this way. For example, in his book *Physicalism*, Jeffrey Poland (1994) mentions strategies physicalists might deploy for rejecting possible counterexamples to physicalism. In one such strategy, physicalists might argue that “...some phenomenon that does not fit into the [physicalist] system is not objectively real and...claims about it are not objectively true or false” (243), echoing logical-positivists’ rejections of metaphysical claims in general as “non-sense” (*sinnlos*).

⁵ Bringing to mind the old joke about the only two rules enforced at one’s new place of employment: “Rule 1: The boss is always right. Rule 2: In the event the boss is ever wrong, see Rule 1.”

here. I believe such arguments fail in that at root, physicalist arguments *are* inherently circular, largely because physicalism is based on an induction which, when treated as if it presents an *a priori* conclusion, begs the question (I will have more to say about this in what follows). Nevertheless, most physicalists seem to merely take for granted the empirical content of their defining theory – some tacitly, often without much comment, while others are more vocal about it.

Again citing Melnyk, “. . . physicalism is a contingent thesis, true at those possible worlds at which the only causal or contingent tokens are either physical or physically realized.” He then observes: “because empirical investigation is obviously required to determine what sort of causal or contingent tokens the actual world contains, empirical investigation will be required to determine whether. . . physicalism is true” (2003a, 27). And as the closing statement to one of his seminal papers on physicalism (2001), Papineau addresses those who doubt the completeness of physics: “I see no virtue in philosophers refusing to accept a premise that, by any normal inductive standards, has been fully established by over a century of empirical research” (p.33).

Since physicalism (whether about the universe in general or the nature of mind specifically) rests almost exclusively on inductive evidence, no *a priori* argument can conclusively undermine it. Melnyk tacitly admits this when he declares that such notables as Chalmers, Kripke, and Jackson have “failed” in trying to mount philosophically-based anti-physicalist arguments (Melnyk, 2003b, 166, n. 12). Whether or not his assessment of this “failure” is correct, Melnyk is saying in essence that exclusively philosophical arguments against physicalism have failed, and since there *is*

clear empirical evidence for the thesis and (as far as he can tell) no evidence counting *against* physicalism (Melnik, 2003a, 189; 2003b, 161), then the case is closed.

Nevertheless, because the nature of inductions is such that they can at any time be shown to be false by a sufficiently robust counterexample (providing the ontology of the world allows there to be one), those whose theory rests largely or exclusively on inductive evidence are always on the lookout for arguments that bolster their positions and strengthen resistance to contravening evidence.⁶ In the case of physicalism there are a few of these.

Supporting Arguments for Physicalism (And Against Dualism)

Physicalists avail themselves of a number of arguments to help reinforce the fundamentally contingent nature of the doctrine they espouse. As we shall see, none of these arguments are immune to objections, but in aggregate they do strengthen physicalist convictions.

Argument from Causation

The argument from causation tries to show that physicalism must be true because, the ontology of causation being what it is, physical causation must be fully capable, without being overdetermined, of accounting for mental effects in the world. Papineau (2002) summarizes the argument from causation as:

⁶That is, the stronger the inductive position is, the easier it is to cast doubt on or marginalize anything but the most weighty falsifying evidence.

Many effects that we attribute to conscious causes have full physical causes. But it would be absurd to suppose that these effects are caused twice over. So the conscious causes must be identical to some part of those physical causes. (P.17)

The argument rests on three premises (Papineau, 2002, 17-18; I rearrange the premises here to strengthen the impact of the argument):

(1) *All physical effects are fully caused by purely physical prior histories.*

This is a re-statement of completeness (discussed above). Effectively, there is no need to go beyond the physical world to find sufficient causal explanations for phenomena found within the bounds of the physical universe.

(2) *Conscious mental occurrences have physical effects.*

As it stands, this statement is unclear. Do conscious states held internally but which remain externally unexpressed count as “physical”? Papineau’s examples involve only conscious states expressed through physical behavior (e.g., going to the fridge to retrieve a beer because one is thirsty). But it seems reasonable to extend the intent of premise (2) in this fashion: Even internally-held, externally-unexpressed conscious states cause changes in the neuronal configurations, synaptic firing sequences, and so on. These count as physical effects. Hence the premise applies not only to conscious states manifest by externally-executed behaviors (such as going to the fridge for a beer), but also to internally-held, externally-unexpressed conscious states (such as “merely wanting to”, but taking no action). The only conscious states unaddressed would be any that express no physical effects and, since we (presumably) have no physical evidence for

these, we have no reason to suspect there are any. The third premise is:

(3) The physical effects of conscious causes aren't always overdetermined by distinct causes.

This amounts to the causal exclusion argument and, as Papineau tells us “materialism now follows.” A point of interest about this argument comes in premises (2) and (3), neither of which are universally quantified. But that doesn't matter for the full force of the argument to come through. If only *some* conscious states – those with physical effects – are physical, there should be little reluctance to conclude that all conscious states are likely physical. And if at least some physical effects of conscious causes are not overdetermined, then why should any of them be?⁷

So here is the full argument:

- (1) All physical effects are fully caused by purely physical prior histories.*
- (2) Conscious mental occurrences have physical effects.*
- (3) The physical effects of conscious causes aren't always overdetermined by distinct causes.*

We then infer from this the conclusion that:

- (4) Conscious mental occurrences are fully caused by purely physical prior histories.*

An objection raised against this argument by Robert Bishop (2006) is aimed at what I number here as premise (1) (it is premise 2 in the original argument). Bishop

⁷The third premise adds some confusion to the argument. Since it is not universally quantified, it seems to miss the full force of the exclusion principle – allowing, it seems, the existence of *some* instances of overdeterminism in mental states. I have found no commentator who has successfully accounted for this, and I have not yet found anywhere that Papineau himself has clarified what he had in mind.

worries that there is a further, hidden premise that is necessary to make the causal argument work. He considers premise (1) to be a “typicality condition,” in that “prior physical events and the physical laws bring about subsequent physical events,” only “in the absence of nonphysical influences.” Thus, “all physical effects are fully caused by purely physical prior histories” in every case where nonphysical influences do not intrude. Bishop then argues that a tacit assumption lurks behind the scenes to keep the causal argument, meant to state the ontological exclusivity of physicalism, from being weakened by this loophole that leaves open the possibility of non-physical causes or entities. This “hidden premise” is that “the only efficacious states and causes are physical ones,” which must be added in order for the full-bodied form of physicalism desired by Papineau and others to follow, since without this premise Papineau’s argument seems to beg the question as to whether *all* states, including mental ones, are physical. Bishop’s objection then becomes that the causal argument fails through the logical fallacy of assuming the consequent.

This objection is responded to in a short paper by Francois Loth (Loth, n.d.). Loth pleads the case of completeness over closure. “Closure is an ontological thesis,” he observes. “It is the conclusion of the causal argument,” and “cannot be used as a premiss in the argument intended to prove it.” However, what I have listed as premise (1) (that all physical effects have exclusively physical histories), when “interpreted as a necessary method of physical science, does not contain the ontological conclusion of the argument” (pp.3-4). In other words, the assertion that “physical effects have only physical causes” (strong closure – at least, as identified by Loth – asserting an ontological claim) should

be contrasted with Papineau's premise "all physical effects are fully caused by purely physical prior histories," with this latter being a methodological assumption of scientific practice that does not assert an ontological claim.

Now, Loth's objection appears to hang on a rather finely-parsed distinction, perhaps best summarized as "while strong closure insists that everything is physical, Papineau's formulation only claims that, to the best of our ability to tell, only physical causes precede physical effects." Whether Papineau would actually agree with this, it does seem to capture the flavor of completeness as Papineau conceives it.

In this case, the argument does appear to go against Bishop, but not very tellingly. The hidden assumption, if there is one,⁸ doesn't seem so much to be a closure statement *in principle*. But there does seem to be a hidden assumption, or the causal argument would not follow. The assumption is this: That the assertion that "all physical effects are fully caused by purely physical prior histories" is actually *true*.

Now, someone could object by observing that premises in an argument are *supposed* to be assumed true. But the presumed truth-value is not so obvious in this case. The premise in question is not a necessary fact nor is it an obvious contingent fact. It is instead a contingent assertion whose truth value rests on sufficient empirical evidence. But that begs the question: has sufficient evidence been provided for us to be confident

⁸It is unclear in his paper whether Bishop notices the work being done by the phrase "purely *physical*" in premise (1) ("All physical effects are fully caused by purely *physical* prior histories"). This appears to be Papineau's signal that the statement is meant to be stronger than a mere specification of physical causal sufficiency that still allows superfluous mental causes as well (and of course, premise (3) presumably takes care of mental overdetermination in any case).

that the claim in premise (1) is true? In trying to answer this question we discover the real potential for circularity in the causal argument. In order for us to accept premise (1) as true, we have to first accept the causal argument itself as true. And to accept that, we have to feel confident that *all* physical effects really *do* have purely physical prior histories. But given the contingent and unfinished state of scientific inquiry, we can have no certainty that the “all” in the premise actually holds.

But shouldn't we be reasonable? Since it is unreasonable to expect us to know that “all” physical effects have purely physical causal histories, can't we fairly trust they do if we have sufficient inductive evidence, absent any warranted counterexamples, to accept the causal argument as rational and (if only provisionally) sufficient? I think the answer is yes, and this is the best defense of the causal argument that remains. We may not have sufficient *a priori* warrant to accept premise (1), but our *a posteriori* evidence gives us strong reasons to accept it. This seems to be how Papineau and others mean to justify their faith in premise (1), and therefore what justifies the conclusion of the causal argument. If we can ground premise (1) strongly enough in empirical evidence, then we have a good-enough solution that saves the causal argument from being condemned for its circularity. In large measure, this justification rests in two empirically-based arguments, the argument from the success of science, and the argument from physiology.

In discussing the inductive nature of the evidence supporting the physicalist doctrine, we have already touched on the “success of science” argument above. It runs roughly like this: “All phenomena in the world that have been thoroughly investigated by science have proved to be physical. We have no reason to doubt that remaining

phenomena, once they are thoroughly investigated, will also turn out to be physical.”

Little needs to be said further here about this argument, except to note that, in the absence of any good reason to believe otherwise, the argument seems to be plausible. More needs to be said, however, about the argument from physiology.

Argument from Physiology

The argument from physiology (AFP) appeals to the successes of modern neuroscience as being strongly evidentiary in establishing the truth of physical/mental identity and the falsity of non-physical (dualistic) explanations of mind. Some of the most prominent physicalist theorists take it as a knock-down argument in favor of physicalism. For, after all, if physicalism is true, one would reasonably expect the tools of physical science to be efficacious in establishing the thesis. Jackson (1996, cited in Montero 2003, 178) puts it this way:

[I]t is widely believed that the physical sciences, or, rather, some natural extension of them, can in principle give a complete explanation for each and every bodily movement, or at least can do so up to whatever completeness is compatible with indeterminism in physics. In this regard we do not differ from motor cars or plants. It is not plausible that in order to explain the behaviour of a motor car we need to go outside the resources of the physical sciences, and the same goes for the growth of plants since the demise of vitalism. I will take it for granted that the same goes for us...

And Montero herself (2003, 184) provides one of the simplest formulations: “The

physiological argument claims that we have good inductive evidence for the view that the mind is fundamentally nonmental, which is not something dualists can accept.”

There are really three prongs to the argument from physiology. The first might be thought of as the “absence of evidence” argument which, simply put, runs like this: Despite years of research, neuroscience has never found any evidence for non-physical causes in the brain (and science has not found non-physical mind-related evidence elsewhere). Hence, regarding mental “sensations” (qualia or other sorts of phenomenological experiences):

[T]he empirically discovered sufficiency of certain neural occurrences for certain sensations constitutes evidence for the physicalistic hypothesis that the sensations in question are either physical or functional but physically realized. And the same discovery surely also provides evidence, albeit weaker evidence, that *all* sensations are either physical or physically realized, for it would be somewhat surprising if only some, but not all, sensations were physical or physically realized. (Melnyk 2003a, 306)

Papineau (2001, 30) maintains that most, perhaps all natural phenomena are explainable “by a few fundamental physical forces.” Further, “...detailed modern research has failed to uncover any such anomalous physical processes.” Hence, “if there were such forces, they could be expected to display some manifestation of their presence. But detailed physiological investigation failed to uncover evidence of anything except familiar physical forces” (p.31). Georges Rey (referring to Foster, 1991, 200) first specifies dualism’s claim as being “that the brain is subject to certain non-physical influences which do not affect the other physical systems which science investigates.” He then counters that “there has certainly been sufficient investigation of the brain to refute this. At a certain point, the absence of evidence of any physical violations

becomes evidence of absence” (p.115).

Melnyk (2003a) agrees: “...if interactive dualism...were true, then there would be human behaviors for which no sufficient *neurophysiological* case can be found by tracing efferent neurons back into the brain; and discovering such behaviors would clearly provide spectacular support for the dualist hypothesis...However, neuroscientists have as yet failed to find any such behaviors, and my strong impression is that they do not expect to” (p.187).

A second branch of the AFP again appeals to neuroscience, but this time as an *explanator* of *prima facie* non-physical mentalistic phenomena. So, for example – schizophrenic hallucinations might once have been thought to be caused by malicious demons interfering with the subject’s mental or “spiritual” processes. Yet psychopharmacological intervention successfully ameliorates such hallucinations, bringing these patients back to a semblance of rational functioning. We can then conclude that the physiological brain mechanisms the respective psychoactive drugs alter must at root be the cause of the malady, and not some supernatural presence for which there is no actual evidence other than the now-remediated symptomology.⁹

Other psychoactive drugs also have significant effects on individual behavior, expression of personality, and the presence or absence of conscious experience. Injuries or diseases that affect brain structures and processes, as well as direct manipulation and stimulation of the brain also have related effects. Since aspects of consciousness,

⁹Of course, this is not the only evidence that schizophrenia has physical, rather than supernatural causes.

personality, and character have long been considered to be essential elements of our mental landscape, yet now seem imminently alterable through neurological intervention of various sorts, physicalists' confidence grows that all mental states are reducible to physical processes. That neuroscientists using new neuroimaging technologies can look on as various behaviors and reactions occur in correlation with neurochemical processes unfolding in the brain adds further to the levels of confidence.

A newly-developed specialized form of this second branch of the AFP is being used to deflate a dualistic strategy I shall refer to as the "argument from mysticism." The argument from mysticism (or AFM) runs something like this: There are humans that sincerely report certain subjective experiences, among them near-death experiences, out-of-body experiences, and mystical experiences. Even though such phenomenal events apparently leave no trace that can be objectively demonstrated to anyone other than the experiencer, these states are nonetheless real, have an other-than-physical quality, and therefore constitute evidence of sorts against the sufficiency of physicalist claims. Therefore, unless and until a satisfactory physicalistic explanation for such experiences can be demonstrated, physicalism cannot fairly claim to provide a full explanation of the world.

Recently, however, a counter to this attack has emerged. Loosely dubbed "neurotheology," the discipline attempts to show that these subjective phenomena, particularly out-of-body experiences and mystical states can be detected and analyzed, or even duplicated in human subjects merely by neurological intervention in specific brain functions, either by directly or indirectly stimulating certain brain structures or observing

and measuring the effects of certain brain disruptions.

For instance, in an experiment with an epilepsy patient, electrical stimulation of a brain structure called the right angular gyrus induced the sensation in the subject of her consciousness becoming detached from her body, similar to accounts of out-of-body experiences (Blanke, *et al*, 2002). Other experiments using low-strength, frequency-modulated magnetic fields applied sub-cranially, evoked in many subjects the feeling of a “sensed presence” of an entity or force to which they often ascribed divine or supernatural attributes or personalities (Persinger, 2001, 2002/2003). In studies of experienced meditators (Franciscan nuns, Buddhist monks), neuroimaging techniques were able to isolate the brain structures involved in meditative practice, suggesting a plausible neuroscientific account for peak meditative and mystical experiences (Newberg, *et al* 2002; Newberg, *et al*, 2003). An array of other neuroscientific approaches have produced related neurological correlates of mental states previously taken as evidence of non-physical mental activity (Newberg & Iverson, 2003; Newberg & Lee, 2005).

Neuroscientists make the argument in this way: If brain manipulation can reproduce these phenomena, then we may conclude that yes, such mental states are real; but they are only neurologically and experientially real – they do not serve as evidence of non-physical states, even if people are right in reporting them as real (if misinterpreted) events. Since it is only the subjective interpretation of such states’ etiology that is mistaken, then the fact of their being real poses no threat to physicalism. As one scientist puts it,

If we realize that mystical experiences originate from the same neurological mechanisms that underlie hallucinations from sensorial deprivation and drug-induced 'visions,' I bet dollar to donut that the reality experienced by meditating Buddhists and praying nuns is entirely contained in their mind and is not a glimpse of a 'higher' realm, as tantalizing as that idea might be. (Pigliucci, 2002/2003, 270)

Each of the preceding arguments counts powerfully in support of the crucial completeness premise of the argument for causation. But their contingent nature means they can never be immune to counterargument. The argument from the success of science (that all phenomena examined by science have turned out to be physical) could in principle be undermined by a phenomenon fully examined by science that turns out not to be physical or physically-explainable.

And against the "absence of evidence" prong of the argument from physiology, Barbara Montero has this to say: "It is not clear," she says, "That lack of evidence for the fundamentally mental amounts to evidence that the fundamentally mental does not exist" (2003, 185). We might flesh this out in more detail by asking, why should we assume that mere absence of evidence is persuasive? After all, there are two ways science might be missing evidence of non-physical mental causality. One is if our neuroscience is still too coarse-grained to detect subtle non-physical mental influences as they interact with physical brain processes to generate causal cascades that lead to manifesting initially-internal, non-physical mental states in outwardly-observable, physically-mediated behavior.

On the other hand, why should mental causal influences be observable by neuroscience in the first place? If those anti-interactionists who argued against Descartes

are right, a non-physical substance would be unable to influence a physical substance, given the ontologically orthogonal natures involved. Assuming (for the sake of argument) this to be true, why should we expect that mental states would ever even register on the instruments of physical science? On this argument, a living human brain might be heavily populated with non-physical causal influences, yet an EEG, fMRI, PET scanner, or MEG might be totally incapable of detecting the fact. Effects from non-physical causes might simply be attributed to some plausible physical explanation or other. One might argue that trying to find and measure non-physical interactions in the brain using physical detection mechanisms would be analogous to venturing a measurement of the distance between Austin and Lubbock, Texas with a thermometer.

This suggests an angle of attack against one variation of the “success of science” argument. Referred to by some as the Methodological Naturalism argument for physicalism, the argument asserts that “the metaphysical picture of the world that one is led to by the methods of natural science is physicalism” (Stoljar, 2005) But if one counter to physicalism is that there are non-physical causes, properties, and entities, and if the methods of natural science only elucidate these which are physical, then even if there are non-physical causes, properties, and entities, they will not be directly detectable by the methods of natural science, so the methods of natural science are unreasonably biased towards finding physicalism true.

The argument from neurotheology invites a different sort of attack. In this case the counterargument (which I dub the “argument from transduction”) might run something like this: “Of course there are structures in the brain that when stimulated

generate experiences very similar to mystical or religious ecstatic states. How else might non-physically-generated veridical mystical or religious ecstatic states be experienced? There must be mechanisms in the brain that transduce a non-physical ‘signal’ into a physical one so that this ‘signal’ can have the causal impact on the percipient that God (or Allah, or Vishnu, or the Tao, or whatever supernatural source one might choose) intends.” Such an argument suggests that certain brain features act as “receivers” and transducers for non-physical causal influences. This argument doesn’t have to be made from a strictly religious or mystical perspective, of course. It could be adapted to fit a spectrum of dualist or *dualistic* proposals.

I won’t treat rebuttals to these counterarguments here.¹⁰ But the arguments themselves are indicative of a fundamental problem in attempting a conclusive proof of physicalism. Thanks to the weaknesses inherent in induction as a truth-making methodology, all arguments for physicalism are inevitably inconclusive. So long as any lacunae in the physical description of the world exists that are either not satisfiable by scientific fact or the solution to which is not logically (deductively) implied by other known facts and laws in the physical world, there is always the possibility that there is some small corner of the universe where something non-physical or extra-physical lurks.

¹⁰In an interesting way there aren’t any fully satisfactory rebuttals, as they would prove very similar to the “God of the Gaps” argument often attributed to proponents of the Intelligent Design (ID) theory. In ID theory, the arguments in support of the theory often appeal to areas of unknowns in science. Hence, for example, since scientists thus far have no strong evidence (nor even a persuasive theory) for a fully-physical abiogenesis, theists can argue against science that whatever role evolution does or does not play in the transformation of organisms, it is plausible to think that God played the primary role in the actual creation of life.

Therefore, to prove physicalism fully true would require the following: 1) the full and complete explication of the natural world in physical terms, and 2) The complete and unambiguous reduction of all phenomena belonging to all other sciences to physics. In other words, we have to examine every crow before we can certify beyond doubt that all crows are black.¹¹

So the only way one could conclusively say that the universe is physical is by examining all facts, laws, and phenomena and discovering nothing there that is not physical, or that is not at least a consequence only of physical facts and principles.¹² The prospects for this happening in the lifetime of the human race are, of course, less than negligible, but there seems no other solution for establishing physicalism that could be as conclusive. Unless and until such an unlikely feat is achieved, physicalists must put up with their chosen theory being vulnerable to counterexamples – should any exist.

There is one fairly recently-developed strategy that tries to provide an *a priori*

¹¹In a version of the classic “one-white-crow” example (expressed most famously by William James), even conclusive evidence based on DNA samples from representative crows showing only alleles for blackness in the chromosomes is insufficient to guarantee without chance of contradiction that all crows are black. There is always a possibility of a heritable random mutation causing an individual crow and its progeny to be grey or even white. This sort of exception poses only a superficial threat to the “all crows are black” kind of induction, since a white crow doesn’t undermine the inherent ontology of all crows. However, the induction grounding physicalism is a different breed of crow. A single provable instance of non-physical causation could *in principle* falsify the physicalist claim to ontological exclusivity.

¹²This is a consequence of the “total evidence” requirement most famously expressed by Carnap (1950) and elaborated on by Hempel (1960 & 1965). I shall have more to say about this in chapter 9, but suffice it for now to mention that McGlaughlin (1970) argues persuasively that, no matter how attractive the requirement is, it is impossible to meet.

analysis of physicalism that, though it cannot completely escape its *a posteriori* foundations, at least allows a move toward relatively firmer *a priori* ones. This has come to be known as “*a priori* physicalism.” *A priori* physicalism argues that from a complete catalogue of all the physical facts, all the truths of the world can be shown to follow *a priori*. According to Gene Witmer (2006), the thesis is “that the non-physical truths in the actual world can be deduced *a priori* from a complete physical description of the actual world.” More specifically, “our world’s physical *nature* *a priori* necessitates its mental nature” (Jackson, 2005, 252). Most of the debate between *a posteriori* and *a priori* physicalism turns on relatively subtle nuances that do not concern us here (McLaughlin, 2005). But a specification of what is being argued by *a priori* physicalism helps highlight the point I just made above.

The *a priori* physicalist approach turns on a doctrine generally agreed to by physicalists, the ‘MPD’ principle, that a minimal physical duplicate of the world is a duplicate *simpliciter* of the actual world (Witmer, 2006; McLaughlin, 2005). The purpose of this claim and the arguments used to support it is to define the actual world as all and only constituted by the physical and nothing else except that which straightforwardly derives from the physical. To arrive at MPD requires what Witmer calls the *Physical Entailment* thesis, (PE), demanding first a “comprehensive physical description of the actual world” using specifically only the “vocabulary that would be found in an ideal microphysics.” This then gives us

(PE) Necessarily, if PT, then A,

where P is the comprehensive microphysical description of the actual world, T is a

constraint (borrowed from Chalmers and Jackson, 2001) standing for “that’s all there is,” thus yielding A, the “comprehensive true description of the actual world.” ‘Necessarily’ is to be understood in the strictest sense of necessity, as strong as that “in which it is necessary that everything is self-identical” (p.186). This then leads us to the Physicalist Material Conditional,

(PMC) If PT, then A.

But for PMC to hold, PE must be a true sentence. As do many others who discuss a *priori* physicalist arguments, Witmer turns to Kripke’s necessary *a posteriori* thesis, water = H₂O, where it is *a posteriori* that water is H₂O, but the identity then follows as a necessary truth, once it has been established by empirical observation. This analogy is supposed to show for *a priori* physicalism that (very roughly) once all the *a posteriori* empirical work has been done, then the complete description of the actual world follows in an *a priori* manner from all the facts – hence PE and, therefore, PMC.

The problem with using this analogy is that in the water case a definitive identity can be found and can be stated with confidence, whereas we as of now cannot infer A, whether *a priori* or *a posteriori*, because it requires that we know both P and T. In other words, we must know all the facts (including laws, properties, relations, and entities) of the physical universe (P), plus we must also know that is all there is (T) – and we don’t know either one, nor have we hope of knowing so any time soon! And that, of course, leaves us off with the conclusion that in order to fully prove physicalism true would require first knowing all the physical facts of the universe.

In discussing the confirmation of scientific hypotheses, Braithwaite (1953/1983)

affirms this: "...it is clear that [just] one piece of evidence is insufficient to prove the hypothesis...It is perfectly possible for the hypothesis to hold in this one instance, but to be false in some other instance, and consequently false as a general proposition. And, indeed, this is the case however many times the hypothesis is confirmed." And it doesn't matter how many confirmatory instances

...have been examined and found to confirm the hypothesis, there will still be unexamined cases in which the hypothesis might be false without contradicting the observed facts. Thus the empirical evidence of its instances never proves the hypothesis: in suitable cases we must say that it *establishes* the hypothesis, meaning by this that the evidence makes it reasonable to accept the hypothesis; but it never *proves* the hypothesis in the sense that the hypothesis is a logical consequence of the evidence. (46)

What I have just said about what it would take to prove physicalism conclusively true may sound gloomier and more pessimistic than might really warranted. Even as far short as we are of having an exhaustive explanatory catalogue of all facts and laws, physicalists can feel well-justified in their belief in the truth of physicalism. With each new bit of knowledge added to what is already known consistent with physicalism (always with the caveat in mind: "in the absence of contravening evidence") physicalism gains added support and justification.

There is historical warrant for such optimism. Based on ever-increasing volumes of generally-accepted evidence supportive of physicalism, the Bayesian probability of physicalism's truth has dramatically increased. Note that the argument from physiology and neurotheology are really descendants of the success of science argument. But success of science lends important support to its offspring in that, since science has done

so well in explaining the non-mental features of the world (geology, biology, chemistry, meteorology, etc.), physicalists are justified in their increasing confidence that science will have similar success with psychology. We in effect extrapolate from the past success of science and infer a high likelihood of equal success in the future.

But how does one get from noticing the growing volume of evidence for the physicalist thesis to the conclusion that physicalism is true? A brief examination of the epistemology of non-deductive inference will hopefully answer this question.

Inductive and Abductive Inference

I need now to revisit my earlier brief comments on the inductive nature of the grounding for physicalism. There is reason to think that enumerative induction itself is not the mechanism by which physicalists arrive at the presumed truth of their thesis, because induction “should not be considered a warranted form of nondeductive inference in its own right,” since technically an enumerative induction in and of itself only tells towards the *next instance* that we encounter (Harman, 1965, 88).

There may be a number of reasons for finding induction deficient as a source of warranted inferences. In a succession where an A is always observed to be followed by the appearance of a B, inductive inference may give us increased confidence with every new occurrence that the next time we observe an entity of kind A we should not at all be surprised that an entity of type B soon follows. Yet, while that (may) enhance our ability

to predict the appearing of a B,¹³ it does not seem to contribute much towards explaining *why* B's follow A's. Further, evidence from an induction may support two or more explanatory hypotheses without giving any reason why we should prefer either or any of them. Auxiliary arguments, or 'lemmas,' in Harman's usage, are often needed to either generate an explanation from inductive evidence, or allow us to choose the better of competing hypotheses supported by the same inductive evidence. But these 'lemmas' go beyond the information and warrant supplied by the inductive evidence itself. "I assert," Harman says, "that even if one permits himself the use of enumerative induction, he will still need to avail himself of at least one other form of nondeductive inference" (p.90).

A number of philosophers (Pierce; Harman, 1965; Boyd, 1983; Lipton, 2000; Melnyk 2003a), have concluded that this "other form" of nondeductive inference (especially in the case of scientific explanation) is a related form of inference often called *abduction* or "inference to the best explanation" (ITBE for short). An ITBE is reached by assessing the available evidence (generated either through a standard enumerative inductive processes or collected from available sources – or both) and then, with the aid of lemmas and other auxiliary evidence, weighting the relative probability of truth of each of the competing explanations and selecting the one that best seems to fit

¹³As Hume famously argued, inductions always invite caution, as a regular procession of A's succeeded by B's does not always portend either that all A's are B's nor that a B will *always* follow an A. Goodman's famous *grue* example illustrates this, as do finite successions (so, for example, in observing thousands of Sun risings we have high inductively-inspired confidence that the Sun's setting tonight will be followed by its rising again tomorrow. However, from other evidence we know that Sun risings are a finite series in that at some point in the (hopefully) far-distant future the Sun's setting will *not* be followed by a further rising.

the data and surrounding circumstances. According to Harman, “All cases in which one appears to be using [enumerative induction] may also be seen as cases in which one is making an inference to the best explanation.”

An important challenge in developing an ITBE is determining which competing explanation counts as the best. “Presumably,” Harman suggests, “such a judgment will be based on considerations such as which hypothesis is simpler, which is more plausible, which explains more, which is less *ad hoc*, and so forth” (1965, 89). Melnyk (2003a) agrees that parsimony is one of the crucial discriminators in making the selection of the best explanation.

Lipton (2000, 184) proposes we think of ITBE as a “self-evidencing” explanation, that relies on the explanandum (the phenomenon which is to be explained) to provide important justification to ground the explananda that are marshaled to account for it. These sorts of self-evidencing explanations demonstrate a mild circularity that Lipton judges to be “benign.” In fact, ITBE makes this “a common situation in science,” since “hypotheses are supported by the very observations they are supposed to explain.” Indeed, “the observations support the hypothesis precisely because it would explain them.” As Melnyk points out, the traditional strategy for physicalists has been just this sort of inference to the best explanation. “If a certain hypothesis is the best explanation of certain facts, then those facts provide evidence for that hypothesis” (Melnyk, 2003a, 242). I would argue that, based on strong inductively-derived data, supporting evidence and auxiliary arguments, physicalists use ITBE to conclude that the physicalist thesis best explains the world.

If this is correct, then physicalism's inference to the best explanation is grounded on the strongly supportive inductive evidence provided by the success of science argument and argument from physiology (with its various offspring) that provides the justification for the problematic first premise in the Causal Argument. Therefore, though we don't have the air-tight guarantees a valid deductive argument would provide us for the truth of the statement that "all physical effects are fully caused by purely physical prior histories," we nevertheless have very good reasons for accepting it as true. This rescues the Causal Argument from circularity and/or question begging, and gives physicalists warrant for their beliefs in physicalism, in the absence of any credible counter-evidence.

Chapter 3: Failing to Prove Physicalism False

In the preceding chapter, we considered principles deemed either important to physicalism, such as exclusion, completeness, and determinism, or essential to it, such as causal closure. We discovered that to conclusively *prove* physicalism is for all practical purposes unlikely to be feasible. But we discovered that there is a “probably good enough” solution in the form of inference to the best explanation, informed by the immense overburden of inductive evidence developed by and accessible to science. But now that we have explored some of the strategies and resources available to bolster convictions that physicalism is true, perhaps we can gain further insight by looking into what would be required to prove physicalism false, if that were possible.

Though it presents the majority view, physicalism is by no means universally accepted. Implications and consequences, such as those mentioned in the introduction, which follow from its being accepted as true motivate resistance to the physicalist thesis. Intuitions that one or another feature of human mental life seems to go beyond what physicalist accounts can hope to explain also provide strong motivations for anti-physicalist views. Some philosophers remain suspicious of physicalism because of what they see as significant metaphysical and other philosophical problems the theory presents.

These anti-physicalist positions have led to a number of influential arguments which have nevertheless failed to score decisively against physicalism. A sampling of arguments against physicalism in terms of mind include the ‘knowledge’ argument;

conceivability arguments; arguments from intentionality; and the explanatory gap argument. I can't begin to address the large number of variations on these arguments here, but I will mention a representative set of them and explain why in the end none of them succeed against physicalism.

Argument from Intentionality

The first of these I shall consider is the argument from intentionality. This strategy owes its genesis to Franz Brentano, who observed that “the reference to something as an object is a distinguishing characteristic of all mental phenomena. No physical phenomenon exhibits anything similar” (1874/1973). The intuition here is that there is a certain distinct sense of “aboutness” or “awareness-of” present within any conscious human thought or perception that is directed towards an object, circumstance, etc. This “aboutness” seems *prima facie* to be wholly unavailable to physical, non-mental objects. Humans thus mentally fix the subject of their deliberations, cognitions, regardings, or other such mental act in a way that goes beyond what mere brain-state processing seems able to explain. Intentionality must thus involve some non-physical aspect, and therefore fully-fledged physicalism is false. To illustrate the force of the argument, Georges Rey (1997, 43) suggests we ask ourselves “...what physical fact makes it true that some state of a person's mind is *about* snow, or cats, or ‘Caesar’? It doesn't seem to be any sort of *local* physical relation, since we and words can refer to most anything anywhere in the world.”

John Searle (1980) illustrates the pull of intentionality in his “Chinese room” thought experiment. Consider a system for translating Chinese to English that consists in

a room with inputs and outputs (perhaps slots through which slips of paper may be passed). A human occupies the room, and receives slips with Chinese words written on them that are passed through the slots. She uses a look-up table to match the string of Chinese symbols with slips containing appropriate English words, and passes the latter through the output slots. Voila, translation occurs – only the translator has no idea of what the Chinese words mean. Indeed, she need not even know what the English words mean. What is seemingly missing from this system is *intentionality*, and this point is often used to argue that no syntactic system can capture the semantics of human mentality. Thus far, our best explanatory frameworks for mental functioning embody some kind of physical computational system. Such systems notoriously can only capture syntax, and are unable to manage intentional semantics – and no physical system that has been proposed or imagined has managed to deal with semantics. Antiphysicalists point to this as a supporting argument that intentionality represents a feature of mentality that is over and above any plausible physical or physically-realized mental system.

Variations of the intentionality argument propose that intentionality is really an admixture of phenomenal experience with more prosaic psychological representations. Proponents of these theories try to tease apart the phenomenal aspects from the (presumably) physically-explainable processes. This then renders the argument as one which more straight-forwardly involves phenomenal experience, along the lines of the knowledge argument or other qualia-based rationales.

Knowledge Argument

Perhaps the most influential antiphysicalist argument is the “knowledge argument.” Made famous in Frank Jackson’s article “What Mary Didn't Know” (1982), the argument runs something like this: Mary, a famous color scientist, has been sequestered in a colorless, black and white environment since infancy. She has been fully educated in every aspect of human color perception. She knows all the physical facts. Yet when one day she steps outside of her colorless room and encounters for the first time color in its natural habitat, something new happens for her – she encounters the actual phenomenal experience of color. The intuition here is that there is something different that can only be experienced, “something” that is over and above the physical facts of color. Thus, physicalism is false – at least so far as it pertains to color qualia (though the argument could be extended to auditory, gustatory, tactile, and olfactory qualia as well).

There is another well-known form of the knowledge argument, sometimes informally referred to as the “what-it-is-like” argument, made famous by Thomas Nagel’s classic article “What Is it Like to be a Bat?” (Nagel, 1974). The intuition at the root of this argument trades on the difference in experience one might expect in an organism whose perceptual apparatus differs profoundly from those of humans. The implication is that such differences must point up differences inexplicable in physical terms.

Conceivability Arguments

Knowledge arguments are a form of conceivability argument. Conceivability

arguments aim to establish that some feature which must be necessarily true for physicalism to be true can be conceived to be otherwise. If something can be conceived to be otherwise, it is possible to be otherwise, and if it is possible rather than necessary, then physicalism must be false (Melnyk, 2001, 2005; Chalmers 1996). Importantly, conceivability is meant as a technical term, not to be confused with ‘imaginability’ (Levine, 2007). This distinction presumably lends a modal force to conceivability which inclines some to take it seriously, though others remain suspicious of just what can actually be counted on to follow from it.

David Chalmers (1996) champions the use of conceivability strategies in the mind/brain debate, arguing that the very fact that we can conceive of conscious phenomenal experience “coming apart” from underlying physical processes is powerful evidence that qualia are extraphysical entities, and that physicalism, at least with respect to phenomenal experience, is false. He does this by proposing that one can plausibly conceive of a ‘zombie’ – a creature that functions, decides, reports mental experiences and interests, and so on just like we do – yet has no actual phenomenal experiences. Though this seemingly fully-human individual says that she sees the color blue, she has no inner blue experience but just a reliable means of recognizing the right electromagnetic frequencies associated with the color blue.

This intuition is supposed to persuade us that phenomenal experiences are something over and above their physical correlates, and hence non-physical, thus proving physicalism false, at least as far as human conscious experience is concerned. Chalmers’ zombies would be perplexed by the assertion that they have no phenomenal inner life.

As far as they know, they accurately recognize blue, and have all the inner experience there is to the color blue. However, the zombie's inner experience is severely impoverished when compared to that of phenomenally-endowed humans.

Each of these arguments turns on a similar intuition: that there are features of human mentality which physicalism cannot explain. A.D. Smith (1993) gives what is probably the most bare-bones summation of the generic argument:¹ “Physicalism excludes *qualia* from its account; *qualia* are genuine features of reality; physicalism pretends to be a comprehensive inventory of the world; so physicalism is false” (p.228). To be sure, some versions of physicalism acknowledge that humans have phenomenal experiences (*qualia*) and other mental features such as intentionality. But to preserve its legitimacy as a doctrine, physicalism must provide a plausible account of these mental features. Lacking that, it must either merely *assume* these features into the physicalist fold, or assert their non-existence. Thus physicalists have resisted antiphysicalist arguments in various ways, some more or less successful, most somewhat satisfying to the philosopher who lodges them but unpersuasive to those whose arguments are so attacked.

Certainly, based on our current epistemic state it is *possible* that theories of nonphysicality are true. Typically the question is asked, “how could insensitive matter give rise to *this* experience – say the look of magenta, the sensation of warmth, a feeling

¹Smith references *qualia* in this passage, but any of the mental features in question could be substituted without changing the argument's terms.

of relief?” thus implying that such experiences cannot possibly be physically explained. And thus far, no physicalist has been able to come up with a completely satisfactory and sufficient answer which brings phenomenal experience convincingly into the physical fold.² Well-formed arguments that trade on the apparent difference between phenomenal experiences and the sensory inertness of matter (knowledge and what-it’s-like arguments) can’t fail, barring disproof by some future incontrovertible neurophysical evidence. But no matter how well-formed they may be, they can’t succeed, either. Intuition plays an important role in many philosophical arguments, but most antiphysicalist arguments depend crucially on it: That conscious, phenomenal experiences are of a quality such that they can *only* be over and beyond the physical world.

So the catch for antiphysicalists wielding phenomenal-type intuition arguments is that they can’t actually *prove* that phenomenal states are non-physical. Nor can they even so much as provide evidence that is less-than-proof that supports the claim, since arguments for phenomenal experiences are based on introspection of subjective states. Arguments that depend one way or another on phenomenal experience can only be asserted, as the available empirical evidence is insufficient for a conclusion one way or the other. Further, these arguments must largely depend on the hearers of the argument to introspect their own inner states to find support for the arguments’ truth.

In the case of Mary the color scientist, the success of the anti-physicalist

²Perhaps, given the profoundly subjective nature of these phenomena, even a sufficient answer – should one be discovered – would always be unsatisfactory to first-

conclusion rests on whether the new experience Mary has is convincingly non-physical. Antiphysicalists seem assured by their intuitions of its truly non-physical status. And many physicalists are troubled enough to attempt sometimes rather complex rebuttals to it. But can the experience be certified to be non-physical?

Various physicalist arguments have been launched against the knowledge argument. For example, Lewis (1988) argues that Mary merely acquires a new *ability*, the ability to recognize the color red from experience. This argument, however, appears to fail merely on the notion of ability. It doesn't seem that Mary acquires any new *ability* that is relevant. After all, she already has the innate ability to *experience* red, as demonstrated by her immediate recognition that she has encountered something new. But there is also her long acquaintance with related abilities, such as recognizing black, white, and shades of grey. What she acquires is not a *new ability* to experience red, but merely the experience of what red is like, and the linguistic and much less ontologically-significant ability to be able to point to red and acknowledge "this is red!". But it is the experience itself that the Mary argument hangs on, not any new linguistic or memory-recall ability, so Lewis's attack does not go through.

Papineau (2002) rejects Lewis's strategy, but he commits the same error in a different way. He sets out to show "in each case that there is no legitimate argument from before-after difference to distinct phenomenal properties. The Mary argument does

person perceivers.

not establish any dualism of properties” (p.55). According to Papineau, our intuitions about Mary are the result of what he calls an “antipathetic fallacy,” an error in reference, brought by attributing distinctions we believe exist between what he calls ‘material’ and ‘phenomenal’ identities. The experience of sinking one’s teeth into a surprisingly sharp jalapeno pepper *seems* completely unrelated to a pulsing lump of brain matter that is (presumed to be) processing the qualitative experience of the consequences of the act even as we bite into the pepper. But, Papineau argues, despite our intuitions the experience itself and the pulsing grey matter (and all the relevant processes it contains) really *do* constitute ontologically the same event. He says we should note “that there is a sense in which the material concepts ‘leave out’ the phenomenal properties. And from this it is very easy to slide, fallaciously, into the conclusion that material concepts cannot *refer* to phenomenal properties” (p.104).

Unfortunately, this argument misses the point just as badly as does Lewis’s. What Papineau – and others who lodge similar arguments – miss, is that the Mary example hinges not on knowledge³, nor use, nor reference *per se*, but on the *experience* itself.⁴ For the sake of the argument, it doesn’t matter whether Mary even knows that the red experience is properly verbalized as ‘red,’ or whether she can use ‘red’ appropriately, or whether she can point to other instances of red, or even whether she can in the future

³Jackson’s construction of the so-called ‘knowledge argument’ is flawed in that respect, that his choice of words misleads us somewhat into thinking it has to do with knowledge, thus making it sound like an epistemic rather than a metaphysical issue.

⁴“Recall,” A.D. Smith says, “that *qualia* are properties, not mental objects: they are the various intrinsic characteristics of certain conscious experiences” [Smith, 1993, 227].)

recollect the experience of red. What matters is the experience itself – the fact that this experience that we generally label as ‘red’ is one that can be experienced for the first time, that is qualitatively different from any other experience, and that what the experience is like is something Mary can’t possibly discover from any other source than direct exposure to the stimulus itself.⁵

There have been other similarly-construed attacks on the knowledge argument (by Churchland [1985, 1989] and others), but none of them are fully persuasive, as they rely on often rather speculative connections between mental phenomena and physical causes, none of which have yet been scientifically demonstrated – nor seem likely to *be* demonstrated.

If knowledge arguments cannot *prove* that Mary’s phenomenal experience is non-physical, and if physicalists cannot prove to the contrary that the experience *is* physical, and if neither side can satisfactorily defeat the other’s argument, the issue remains inconclusive. This yields a stalemate between physicalists and antiphysicalists. But a stalemate leaves antiphysicalism at a disadvantage, since a stalemate for antiphysicalism gives physicalism the upper hand, thanks to the weight of the trove of objective evidence supporting physicalism, and the seeming rationality of the well-grounded inference to best explanation (ITBE) asserting that physicalism is the most likely account of the

⁵I am suspicious of suggestions by some philosophers of the possibility of imaginatively interpolating an otherwise un-experienced (for example) color experience if the proposed color might lie between two closely-related adjacent colors. Despite my early experience as an art student and artist, all my attempts at testing this thesis by introspection seem to utterly fail. Perhaps others might be able to pull off such a feat. But how would we ever know?

world.

Conceivability arguments and arguments from intentionality suffer similar fates. Conceivability arguments depend on a kind of intuition of the nonphysicality of qualia which is related to the knowledge arguments. David Chalmers' functionally isomorphic zombie *doppelgänger* lacks not only qualitative experiences, but also the necessary constitutive features to make them possible. Mary, on the other hand, has the required features even if she thus far, alas, has been denied the experiences. But we must not allow ourselves to be distracted by the details. At their core, the qualia-lacking zombie and color-scientist Mary are really merely different instances of the same case. Were the zombie through the addition of some corrective gene suddenly to experience a rose of a certain shade of red, his would be a similar experience to Mary's when she emerges from her black-and-white world for the first time to see a rose of that very same red hue. In this respect conceivability arguments are no more vulnerable, but also no more conclusive, than knowledge arguments.

But conceivability has its own peculiar difficulties. Many philosophers question the legitimacy of drawing metaphysical conclusions from what seems to be a merely conceptual argument, and are not fully persuaded that the move from speculative "imaginability" to a presumably more robust "conceivability" truly succeeds in adding force to the argument. Melnyk (2001), for example, argues that *most* conceivability arguments conflate the reference with the actual property itself, thus failing to identify a metaphysically-real possibility. "[T]he conceivability. . . of a proposition does not in general entail its possibility," and "it is because the reference of our concepts is an *a*

posteriori matter that conceivability (in the sense specified) does not entail possibility.” Thus, “traditional conceivability arguments fail because they rest on a false assumption in the philosophy of mind and language: that one knows, simply on the basis of one’s competence with a concept, what property the concept picks out” (p.332).

However, according to Melnyk at least one conceivability argument against physicalism does not fall prey to this weakness – that raised by Chalmers. Chalmers’ argument “employs a different and proprietary notion of conceivability according to which the inference from conceivability to possibility is unproblematic” (p.333). Though acknowledging that at least this one flavor of conceivability argument has not been successfully blocked by physicalists, Melnyk is not so quick to let Chalmers off the hook, and thinks he has cast sufficient doubt to seriously wound Chalmers’ position, noting that Chalmers’ argument, “as it currently stands, is therefore inconclusive” (p.345). So this puts conceivability in a similar boat to the knowledge argument: Even if conceivability arguments can’t be proved false by physicalism, they are at least inconclusive and, once again, the ITBE that grounds physicalism captures the high ground.

Intentionality arguments, on the other hand, at first appear to differ from knowledge, what-it-is-like, and conceivability arguments. But looking more carefully we can see they actually all bear a certain resemblance to one another. Though intentionality doesn’t (necessarily) require the same sort of obligation to recognize a particular phenomenal experience as nonphysical, it does require the recognition of the distinctive feeling of “aboutness” when regarding objects, whether introspectively or externally. The same question can be raised concerning this sense of aboutness as concerning the

look of emerald green or the feeling of satiation: Why should we necessarily believe that sufficiently complex configurations of matter are not able to give rise to these kinds of experiences? Just because we don't know *now* why such experiences seem to us as they do does not mean there isn't some fully physical explanation that we just (for now) don't have the wit to discover.

Kim (2005) thinks intentionality is reducible, as do others (though, as I will mention again below, Kim no longer believes that qualia are). Daniel Dennett (1987) has based much of his career on and written a book arguing that there is nothing ontologically special about intentionality. Chalmers (1996) agrees on the reducibility of intentionality, excepting any aspect of intentionality that may possibly involve phenomenal experience.⁶ But even if they are all mistaken and intentionality is not so reducible, we can still see that merely asserting the impossibility of reducing intentional states is insufficiently strong to successfully undermine our faith in physicalism. As Kim has argued (2005), it is not unreasonable to believe that many (perhaps most) mental states, and along with them perhaps intentional states as well *might* yet be reducible at least in part, and thus turn out to be physical through and through after all. The argument from intentional states does not introduce enough uncertainty to outweigh our physicalist convictions. So unresolved is the intentionality issue that it has brought the authors of one philosophy textbook to lament that “we do not understand intentionality well enough to say with any confidence *what* it implies for the mind/body problem. For this reason it

⁶Chalmers suggests that intentionality can be used in antiphysicalist positions only to the degree that it can be shown to incorporate phenomenal experiences.

cannot be used as an argument for anything” (Brook & Stainton, 2000, 113).

Whether or not such a defeatist position as that of Brook and Stainton is warranted, a similar complaint can be raised against arguments such as the Mary argument or conceivability which rely at least somewhat on the non-physicality of phenomenal experience. If, in the deepest recesses of the brain, neuroscientists someday discover it is really true that qualia are the result of neurophysical processes, and if it is true (as it is) that we thus far are unable even to recognize how that can be the case, then perhaps we just haven’t yet figured out the right kind of detector – one that works as well as the biological qualia-detectors nature has devised and installed in humans and other presumably “conscious” organisms. You don’t, after all, measure temperature with a stethoscope. A stethoscope couldn’t tell that anything such as temperature even exists.

To show what antiphysicalists are up against – how difficult it is for their argument to gain ground in what is for them an essentially up-hill fight – it might be useful here to consider one defense of physicalism mounted by Colin McGinn (2001). McGinn believes that finding a solution to the mind-body problem would require a concept of pain which “would connote precisely those properties of pain that render its connection to matter and the brain entirely perspicuous.” But he “also think[s] that we are unlikely to discover anything with such consequences” (p.301). Type-identity theories of mind can’t be defended against qualia-type arguments because there seems to be no way of establishing an identity between, for example, neurally-realized pain (or other qualia-type) states and the phenomenal “feel” pain has. Thus he concludes (p.305)

that a knowable empirically-true identity is unavailable, with the consequence that only a conceptually-true identity statement will do (McGinn: “There has to be such a concept as no other option is feasible” [p.300]). But that is probably not possible to establish, as it would require a “mind-of-God” sort of perspective that can omnisciently inspect both brain and phenomenal states. After all, how would you know if something were ‘conceptually-true’ if you couldn’t establish at least some of the facts that verify its truth? Hence, physicalism is true because it has to be, not because we can prove it to be.

McGinn’s approach has been styled the “mysterian” view, since it proposes that the restrictive limits of the human ability to know may place some aspects of nature and the universe permanently outside our epistemic reach. Antiphysicalists find this view understandably unsatisfactory, as it seems to leave the status of physicalism as simply assumed to be true, based on the ubiquitous ITBE.

Unfortunately, McGinn’s (as in different ways are Papineau’s, Melnyk’s, and others’) seems the sort of strategy that Kim (2005, 30) warns against. According to Kim, blaming our system of concepts, or our language, for philosophical difficulties is a familiar philosophical dodge of long standing. To motivate the discarding of a framework, we need independent reasons – we should be able to show a theory to be deficient, incomplete, or flawed in some fundamental way, independently of the fact that it generates puzzles and problems with which we are unable to deal. Declaring ontological problems to be nothing more than a confusion of terms seems a suspicious move to those doubtful about the foundations of currently proposed solutions to such problems. Nonetheless, if McGinn is right then that is the strategy to which physicalists

must resort.

In any event, if qualia, intentional states, and other mentalistic phenomena really *are* strictly physical phenomena, resulting from fully physical processes then, for example, zombies are impossible in all physical worlds sufficiently similar to our own, and Mary may indeed learn something new upon leaving her room – but whatever it is, it is not something outside the physical world. The problem is, as I have said several times now, that we can't tell for now whether qualia and intentionality are not physical, and in the absence of knowing, the best that can be said of arguments that hinge on phenomenal experience (if they can't shown to be conclusively false) is that they are inconclusive.

Thus the antiphysicalist arguments have not been resolved – they have neither been proved true *nor* false. On the one hand, Chalmers, Jackson, Nagel, and others have given provocative reasons to question aspects of physicalism. But they have not been able to show that these phenomena unquestionably fall outside the physical world. For their part, physicalist apologists have themselves failed to arrive at a convincing and armor-plated explanation that brings these phenomena firmly within the physicalist tent. As Chalmers observes: “[I]f a physicist or a cognitive scientist suggests that consciousness can be explained in physical terms, this is merely a hope ungrounded in current theory, and the question remains open” (1996, viii).⁷

No less a theorist that Jaegwon Kim has thrown in the towel on the qualia debate,

⁷Thus we have Jackson (2005, 259) making the following observation: “The hope of a new tool to counter the challenge of the zombie argument, and its many partner

willing to leave that one small corner of human mentality outside the physicalist fold, his most recent book declaring his allegiance to physicalism, “or something near enough” (2005). He decides that if everything pertaining to mind and the non-mental universe with the exception of qualia can be fit into the physicalist world view, then he’s willing to exempt conscious phenomenal experience from being fully physical – though he sees this as only a minor concession.

Antiphysicalists on the other hand, have as we have seen failed as well to provide sufficient reasons as to why we should believe there can’t be a physicalist resolution to the controversy. Physicalists always have some available response which, though perhaps no more conclusive than the antiphysicalist argument they are meant as an antidote for, nonetheless is sufficient to hold the field against their opponents. Thus, because of the vast explanatory successes physicalism can point to in its own support, inconclusiveness or stalemate is, in the face of the inductive pressure science’s past successes generate, sufficient to hold nonphysicalism at arms’ length. Since knowledge, conceivability, and intentionality arguments are all inconclusive they are, hence, unsuccessful against physicalism with its robust ITBE as a trump card.

Perhaps in recognition of this, Gillett (2001) offers a unique quasi-antiphysicalist argument against the presumption that physicalism is the ultimate arbiter of scientific truth, at least for appraising new theories. His argument is approximately like this: Physicalists mistakenly argue that only the methods that have successfully been used in other scientific disciplines should be used to explore empirically-explorable areas, and

arguments from intuitions or possibility, for dualism, has not been realised.”

that physicalistic criteria have been used historically to sort out acceptable theories from unacceptable ones. However, Gillett disputes the idea that physicalist criteria have historically been used to appraise theories. Instead, a different approach, not overtly dependent on physicalistic criteria has been used (he labels this the “minimal criterion”) in which a “local” approach applies. Theories are appraised and accepted not by how well they adhere to some physicalist dictum or other, but rather how well they cohere with theories with which they must interconnect. Gillett’s goal seems to be to show that physicalism does not exert quite the domination over theory-acceptance that is generally believed.

While this is a provocative argument, and if widely accepted could be (perhaps mildly) nettlesome to the physicalist hegemony, it is not a blow to physicalism itself but only, perhaps, to its pride. Indeed, a question seems obvious here: Even if Gillett’s argument is right – that much more important to theory appraisal is how well a theory coheres with its theory-neighbors – it seems that irredeemably physicalist criteria would still be the outer fence surrounding all of the local theory “neighborhoods.” Physicalist standards would establish the allowable terrain within which a new theory must fit or be rejected, and would specify the outermost frontiers beyond which no new theory may venture. In this respect, in its “global” construction (as Gillett himself styles it), this “physicalist criterion” would be used to measure every theory for orthodoxy, and justify the exclusion of any which fails to pass muster. Indeed, it would seem that any theory with non-physicalistic content would automatically fail to cohere even with its most proximate theoretical neighbors, who themselves *do* meet the general physicalist criteria.

We have seen that arguments within the philosophy of mind against physicalism often amount to presenting intentionality and qualia as irreducible aspects of mind that can't be reconciled to purely physical explanations. Arguments against physicalism based on qualia, intentionality, and so on make little headway not because they are false, but because they are inconclusive. Ironically, such arguments could finally be proved conclusively *false* – if they are – only if true mental-to-physical identities are discovered. And it may turn out that they could only be shown conclusively true under the same conditions in which physicalism itself could be proved conclusively true: when every fact and law about the universe is finally known. Until then, physicalists fend off such arguments by pointing to the ever growing strength of the inductive evidence supporting physicalism, just as Georges Rey did in my philosophy class in the fall of 1996.

Most intuition-based antiphysicalist arguments remain a live option because they exploit a thus-far un-bridged gap in our scientific base: That full, or even partial, reduction of mental phenomena to underlying brain processes or configurations remains unachieved. Die-hard materialist that he is, Papineau grudgingly acknowledges this, and he remains pessimistic of the prospects of being able to eliminate anti-physicalist arguments. “There are questions about the referents of phenomenal concepts that [science] is quite unable to answer” he notes (2002, 11). Further,

How should science proceed if it wants to identify the material referent of our phenomenal concept of *pain*, say? As it happens, I think that science can provide far fewer answers to such questions than many people suppose...It is a mistake to suppose that research into phenomenal consciousness can proceed just like other kinds of scientific research (176).

Papineau is not the only pessimist. McGinn (2001, 300-301) argues that “...there is no solution to the mind-body problem within the limits of our current concepts of mind and brain,” and that “...we are unlikely to discover anything with such consequences.” He goes on to express faith that “such a solution exists but is not discoverable by us,” since “discovering empirical correlations between mental and physical concepts is never enough to entitle us to assert psychophysical identities.” This fundamental pessimism is what motivates McGinn to opt for the “mysterian view” of mind-body interaction.

Even as enthusiastic a physicalist as Andrew Melnyk (2003a) acknowledges that “the evidence for physicalism about the mental is markedly weaker than that for physicalism about everything else,” and that he “cannot make an empirical case for treating mental phenomena as physical or physically realized” by using the typical arguments that establish such things as physics, geology, and biology as being physical (p.4 & 283). Thus lacking a full catalog of physical facts and relations, and hence missing conclusive proof for physicalism, many physicalists employ a strategy to rationalize their way around the obvious problems that block the path to a satisfactory reduction of mind to physics. This is the well known recourse to supervenience theories.⁸

Status of Supervenience as a Solution

Supervenience theories serve a number of ends – they fill in the gaps when no

⁸In my discussion, I only consider supervenience in relation to philosophy of

suitable reduction can be found; they allow for the retention of a physicalist account in the face of multiple-realizability; and for non-reductive physicalism (NRP) theories they preserve at least the appearance of autonomy for the mental while reassuring us of the physical dependency that is presumed to hold necessarily between the seemingly mental and the rest of the otherwise apparently physical universe (Kim, 2005, 15).

Supervenience theories also help to insulate physicalism-of-the-mental claims from attack. As Kim notes: “Just as normative/moral properties are thought to supervene on descriptive/nonmoral properties without being reducible to them, the psychological character of a creature may supervene on and yet remain distinct and autonomous from its physical nature.” But this begs an interesting question: If you can’t reduce, how do you know that supervenience is actually the proper relationship? And if you can reduce, will a supervenience relation then come to nothing much beyond a difference in terminology?⁹

The resort to supervenience appears to be either a consequence of epistemological difficulties, or metaphysical ones, or both. If metaphysical, then it seems that supervenience theories claim a causal connection between the supervenience base and the superstructure which is left unspecified. If that connection is not merely brute, then it is hard to tell how causation attributed to a supervenience relation would differ from a Humean constant conjunction or some other form of regularized correlation between

mind.

⁹John Searle (1992) observes, “once you recognize the existence of bottom-up, micro to macro forms of causation, the notion of supervenience no longer does any work in philosophy” (p.126).

events. On the other hand, if the concern is epistemic, then all supervenience seems to amount to is a tacitly-held agnosticism as to just what the causal connection is between base and superstructure.

If that is so, then we seem to encounter a set of nested assumptions. In *Mind and the Physical World* Kim notes the following: “We may know that B determines A (or A supervenes on B) without having any idea why this is so – why A should arise from B, not C, or why A, rather than D, arises from B” (Kim, 1998, 18). By this I take it he means to typify the sort of body-mind supervenience relation broadly popular in many theories of mind held today, along with the epistemic lacunae with which we must deal in trying to assess such physical-to-mental dependencies.

But this appears to beg a question: How do we even know that A supervenes on B (or, more concretely, that mental state A supervenes on brain state B)? The unfortunate reality is, that aside from some perceptual processes (in which, for example, an fMRI can interpolate an image that is being processed within a subject’s brain) we *don’t*. Finding and identifying brain states that fill the role as subvenient bases for mental states has proven more difficult than was perhaps previously envisioned by early identity theorists. The notion of supervenience has been adopted in some measure because no such coupling of the right kind has conclusively been shown, leaving philosophers obliged to develop some rationale (other than that of the failed type-physicalism) with which to reconcile the requirements of physicalism pertaining to the mind with the absence of full

evidence that it is true.¹⁰ The assumption that the mind just is the brain nests within the broader assumption that physicalism provides a full and complete framework for the universe. But then our presumed certitude about the truth of physicalism about the mental begins to look more tenuous than its advocates may prefer. Supervenience explanations can seem to come uncomfortably close to hand waving, and other ways of trying to account for the mental by reduction, type physicalism, functionalism, etc., either fail or give us as yet no clear path through to a clearly-established, fully-physical account of mind.

Some otherwise committed physicalists are coming to question the supervenience enterprise. Kim (1993, 1998, 2005) voices numerous reservations, and suggests that the physicalist/antiphysicalist debate must be solved through reduction and the finding of clear mental/physical identities. Melnyk also has concerns that, to succeed, supervenience theories may have to postulate a brute-fact relationship between the microphysical facts and the sciences (2003a, 49ff.), and he finds this unsatisfactory for grounding the truth of physicalism. Supervenience, then, seems to provide a convenient way of describing mind-brain relationships in the absence of a sufficiently-detailed reduction. And it may describe a relationship that really holds in a world (perhaps ours) in which McGinn's mysterian view of physicalism is the best we can hope for. But in terms of providing a solution for the mind-body problem which, after all, is the crux of the debate over physicalism, supervenience seems neither to promote nor undermine

¹⁰The argument from physiology (discussed) provides some evidence (but as yet no proof) that supports brain-mind linkages of some as-yet unresolved variety, leaving

physicalism's case.

Just as anti-physicalist arguments remain elusively inconclusive, pro-physicalist arguments are not conclusively grounded, either, and it is not clear they *can* be. So if we can't *argue* to a conclusion whether physicalism is true or false, is there any threat to the physicalist hegemony that remains? Given the empirical foundation to the inductions and ITBE that found physicalists' certainty as to the truth of their doctrine, it would seem that only an empirically-based attack would challenge it. To succeed, physicalism's opponents would have to defeat the inference to the best explanation that founds confidence that physicalism is true by presenting empirical evidence that casts doubt on the soundness of the evidence. But how might they pull that off? There does seem to be a way.

physicalists with hope of ultimately achieving this Holy Grail.

Chapter 4: How to Prove Physicalism False

Falsifiability was an essential criterion which Karl Popper thought requisite to establish the scientific soundness of a given theory. Indeed, he attacked Freudianism and Marxism on their propensity for self-justification based on *ad hoc* explanations that seemed to insulate both from arguments or tests for soundness. Though the falsifiability criterion remains controversial, it is still widely embraced by scientists and some philosophers. Even those less enthusiastic towards it would still regard with some suspicion any important theory that without good justification failed to admit some possibility for falsification.

Some philosophers worry that physicalism, if not carefully formulated, ends up unfalsifiable. In a discussion of the difficulty Hempel's Dilemma presents for arriving at a successful formulation of physicalism, J.L. Dowell notes that (as we discovered in chapter 1) relying on present physics as the standard against which to formulate the doctrine of physicalism essentially guarantees that physicalism will at best be incomplete and at worst, false. But formulating it in terms of a future complete and ideal physical theory threatens to make it "so vague and indeterminate [that] its truth-value becomes impossible to evaluate" (Dowell, 2006a, 3). And Smith (1993) observes

It is clearly neither necessary or sufficient for an item to be purely physical that it be describable in the vocabulary of *current* physical science; and who knows how future, or ideally completed, science will pan out?

It is not hard to see why physicalism flirts with unfalsifiability. If present physics is unable to deliver a fully comprehensive and correct delineation of just what counts as

physical, relying for a final solution on an unlikely-to-be realized completed physical theory, then *any* unfamiliar or inexplicable phenomenon encountered as we explore the world can be waved off as something that “some day” we will be able to show is fully explainable within a physicalist explanatory context. But this begins to resemble the *ad hoc* strategies which Popper found so dubious in Freudianism and Marxism. Essentially, physicalists want a strong theory. But they can’t afford to have it so strong that it is easily defeasible. Yet the weaker they make it, the more it flirts with epistemic vacuity. (Crane & Mellor, 1990)

So, under what conditions might physicalism be falsifiable? If we accept closure and exclusion, then we arrive at the following circumstance: “[P]hysics is causally and explanatorily *self-sufficient*; there is no need to go outside the physical domain to find a cause, or a causal explanation, of a physical event” (Kim, 2005, 16). But this is true only so long as we never encounter well-attested evidence of a physical event that has a cause that can only have originated *external* to the physical domain, and Kim doesn’t tell us what to do in that case. Evidence of a violation of causal closure seems to be the most likely chink in physicalism’s armor.

We need, then, to consider conditions which countervailing evidence would have to meet in order to falsify closure. First, such evidence would have to be objective, producing data accessible to those properly equipped to replicate it and evaluate it. Second, such evidence would have to be established through some process at least as reliable as sound scientific practice. Third, and substantively most important, it would have to show a clear violation of closure – a violation for which the best explanation

would be that the closure principle was false.¹ As Melnyk (2003a) notes, a sufficiently attested lacuna in a physical causal chain would produce “...a counterexample to the claim that the physical is causally closed” (p.295).

Characteristics of a Counterexample To Physicalism

My analysis so far shows that the keystone to physicalism, *any* physicalism no matter how construed, is the principle of causal closure. If closure turns out to be false, no formulation of physicalism will survive unscathed. We would suspect closure to be false only if we were to find a counterexample of some sort – an instance, a circumstance, a phenomenon – where closure evidently failed. What would be required of such an instance, or circumstance, or phenomenon? First of all, since we are concerned about *causal* closure, we would want to find some evidence of causation entering the physical domain. The idea of “entering” obviously implies that such a causal chain would need to initiate somewhere external to the physical world. This in itself is a challenging concept to work out. What do we mean by *external to* the physical world? Since when we say *external* we seem to be speaking in spatial terms, do we mean a spatial expanse or domain outside the physical one we inhabit? This notion in itself may be seen as begging a number of questions from anyone who might advocate it. If there is an “outside,” how do we know whether it is spatial in the same sense we

¹ One further condition would require that there be an abundance of such evidence. As Popper (1959) noted, because we can’t trust that our observations are error free, only one or two instances, though logically sufficient, would not be found persuasive by staunch physicalists.

understand it here “inside”? And what is the ontological status of that “outside”? Is it non-physical, or merely extra-physical – or perhaps physical in some extended sense? More fundamentally, how can we assert an “outside,” when physicalism (at least in its strongest and most widely accepted formulations) denies that anything exists outside the physical universe, since the physical universe is (presumably) all there is? Yet that begs a question of the physicalists in turn, since that is the very assertion the truth of which we are looking to test. For, as things stand right now, that the physical is all there is can only be an assertion, albeit one that rests on a broad foundation of inductive evidence and a very persistent inference to the best explanation (ITBE).

For our purposes here, we can leave the resolution of the question of what may lie outside the physical universe, how it might be configured, or even precisely what “outside” means for another occasion. All we need now find in a promising counterexample is that a candidate causal chain either *not* initiate, or not remain fully contained *within* the physical domain (that is, from start to finish). We are generally quite comfortable with the notion of being “inside” the physical universe, and, relying on our intuitive grasp of the concept, can simply specify that anything causal that isn’t *internal* must be *external*. Specifying this distinction precisely is an important task, but responsibility for it falls to the physicalist, who needs an internal/external distinction as much as a physical/nonphysical to make the physicalist thesis complete. The general structure of my argument is this: 1) It is not obvious that physicalists can explain what 'physical' means in a way that makes their thesis neither vacuous nor clearly false. 2) Even if it turns out they can, they will need both an account of 'physical' and also of what

it means for a causal chain to originate from, and remain entirely within, the physical realm. It is unclear that they can do either. 3) But, again, suppose they can -- their theory is thus open to the possibility of falsification, so long as we can find causal chains that, according to the physicalists' own criteria, originate outside the physical domain or stray outside it. I will argue that, on any construal of 'physical' and of 'originating and remaining within the physical domain' that is plausible and rules out the kinds of things physicalists typically want to rule out (e.g., gremlins, ghosts, spirits, etc.), such causal chains can be found.

To identify a candidate closure violator, we need first to specify how we recognize that something external – specifically the antecedent to a relevant causal chain – has intruded into the physical world. This may at first seem difficult to do. After all, we cannot (it seems) get outside to identify the source of such a causal chain. Further, if the *external* components of such a causal chain are in some way ontologically different from any we deal with physically, then we may not even have means to observe or analyze them – they could be effectively invisible to us.

Yet there is, it seems to me, a way to identify such chains even from our restricted vantage point here within the physical universe. A causal chain that honors the closure principle by originating, proceeding, and terminating all within physicality would be deterministic (though in certain cases it might arise as an indeterministic event such as from nuclear decay) and presumably fully traceable. In practice, of course, not all fully-physical causal chains are traceable, given the practical limits of our instrumentation and heuristic methodologies. But even many partially-untraceable causal chains can be

reliably inferred to be fully contained within the physical universe based solely on the evidence we do have of them. But a closure-violating chain would (ideally) be traceable in this way: starting at its completion point (a certain event), we could follow each preceding causal link in reverse order back to the chain's point of origin. We might outline a typical (non-closure-violating) causal chain somewhat along these lines:

Confronted with a broken lamp, we wish to know how the breaking transpired.

Investigation reveals the following links in the causal chain:

- 5) The breaking occurred as the lamp impacted the floor;
- 4) The lamp impacted the floor upon being knocked off its stand by Christopher;
- 3) Christopher fell against the lamp when bumped by the dog;
- 2) The dog bumped Christopher when startled by the exploding ceramic casserole dish;
- 1) The casserole dish exploded when the babysitter thoughtlessly heated ravioli in it on the stove-top burner.

Nothing is controversial in this causal account (other than perhaps the choice of babysitter in this unfortunately very real historical event).

What if, however, the causal chain we are attempting to trace has as its point of origin an event that seems to have emerged “out of thin air,” as it were – that is, it has no detectable physical antecedents previous to a particular link in the chain? I shall call these “truncated causal chains,” or TCCs. Let's examine the same causal chain and give it a different origin: How did the lamp break?

- 5) The breaking occurred as the lamp impacted the floor;
- 4) The lamp impacted the floor upon being knocked over by Christopher;
- 3) Christopher knocked over the lamp when bumped by the dog;
- 2) The dog retreated into Christopher when startled by the hideous, shrieking Gremlin that suddenly materialized from thin air;
- 1) The Gremlin...well, where did the Gremlin come from?

A causal chain originating outside but terminating inside the physical universe – a TCC – would from its terminating event backward appear like any other deterministic causal chain – that is, until our trace arrives at the chain’s point of irruption into the physical domain, whereupon we can trace it no further.

I used the preposterous Gremlin example only to make the account clear. Let me give a different example that rings less absurdly in our ears (in the interest of space I will combine some of the steps).

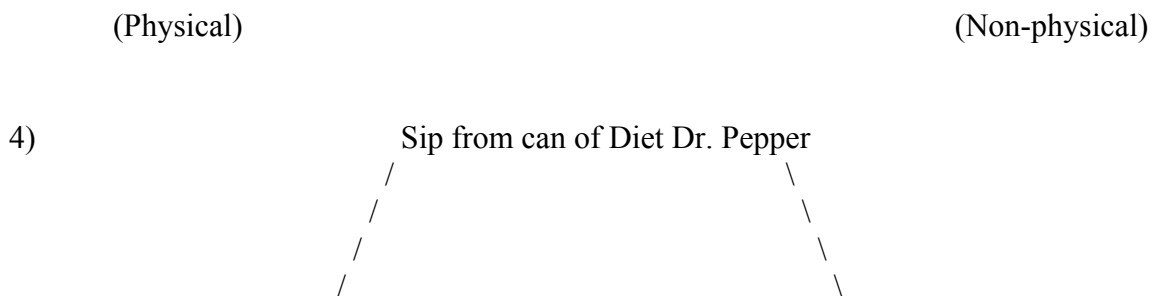
- 5) I take a sip of Diet Dr. Pepper from the can I hold in my hand;
- 4) I reach my hand forward, grasp the Diet Dr. Pepper can and raise it to my lips;
- 3) I decide to take a sip of Diet Dr. Pepper instead of regular Dr. Pepper;
- 2) I observe that someone has placed open cans of both Dr. Pepper and Diet Dr. Pepper before me;
- 1) I note that I am thirsty.

This is a more complex example, since it involves possibly two causal chains – one deterministic and likely to be fully physical, and the other that perhaps fits neither

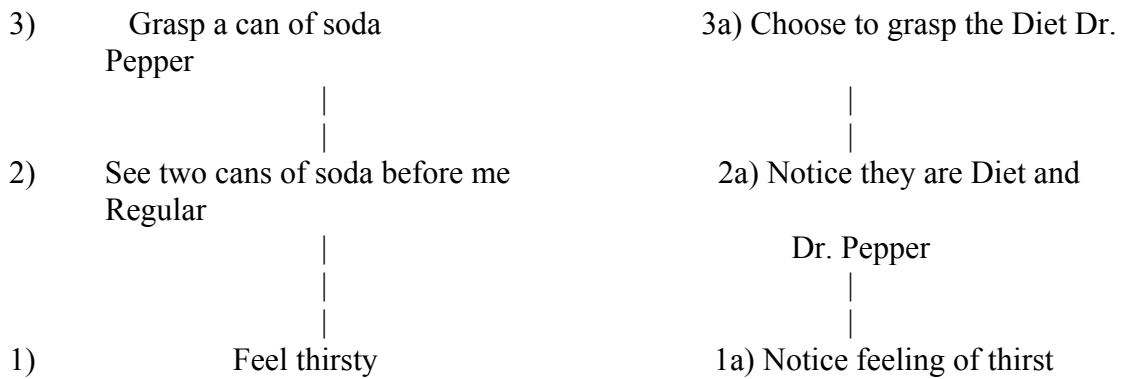
criteria.

The first, physicalistic, chain involves the feeling of a need to quench thirst, the recognition that a substance suitable for quenching thirst is present, and the reaching for the liquid refreshment to quench the thirst. (While some antiphysicalists might wish to dispute the point, I am willing for the sake of the present argument to grant that the “thirst-> recognition -> arm movement -> drink” causal chain is fully physical, originating in and controlled by body/brain processes.) But the “decision -> arm movement direction -> choose Diet Dr. Pepper” causal chain, though sharing part of its action with the “thirst” chain, *appears* (at least prima facie) to arise without an antecedent, and hence to be a possible instance of a TCC.

This, of course, is the assertion of some antiphysicalists: Though causally-relevant non-physical mental events (if they exist) may partially integrate with accompanying physical causal chains, they do not arise from a physicalistic (or, at least, not an exclusively physicalistic) causal context.² We might thus capture this more complex causal network somewhat along the lines of the following diagram:



²This raises questions of overdetermination, but they are not important here. The overdetermination issue will be considered in chapter two.



The reason I expressed reservations about the actual status of the decisional causal chain (“*appears prima facie*” to be a TCC) is because there are two ways of accounting for the seeming “out of thin air” initiation of the chain’s impetus. One is ontological, the other epistemic. The ontological account harmonizes with my current examination of what might evidence the extra-physical origination of a causal chain. From a primitively dualistic view wherein at least *some* mental events are not subject to physical constraints, “deciding” as a causal event might have antecedents that originate outside the physical domain. In such a case, the apparent “decision” point in the chain (“I think I’ll choose diet over regular Dr. Pepper”) would not just be a (physical) causal event, nor the initiating event spawning its own causal chain. Rather, it might be the point where the chain irrupts into physicality.

At least, that’s one way to look at it. But the case is ambiguous. On the Diet Dr. Pepper account, instead of being evidence against closure, the decisional “irruption” might really be only a matter of superficial appearance, owed to the fact that our neuroscience has not progressed sufficiently to show how thirst-quenching behavior and

“deciding” for Diet Dr. Pepper are merely different branches of the same entirely physical causal chain. As you can see, I am not staking anything necessarily on “decisioning” as originating extra-physically. Decision-events may turn out to really be *just* brain events. I use it here only for exemplary/explicatory purposes. However, how I use the example is but a little distance removed from the way antiphysicalists argue that the nature of intentionality (or of qualia, for that matter) challenges the sufficiency of physicalism in accounting for human mentality. And my vaguely physicalist-rejoinder is not far removed from how physicalists argue against claims that qualia are non-physical.

As discussed above, the qualia, intentionality, and other antiphysicalist arguments are inconclusive for the same reasons that prevent my “deciding for Diet Dr. Pepper” example from carrying through: the fact that we can’t tell whether the chain has an origin external to the physical domain, or whether instead we are merely ignorant of earlier steps in what could turn out to be a perfectly physical causal process after all. We are, effectively, stymied by inconclusiveness.

This, then, presents us a potentially major set-back in laying out how we might identify a possible violator of causal closure and, hence, a counterexample to physicalism. The difficulty lies in deciding whether a causal chain that *seemingly* materializes “out of thin air,” really does thus materialize, or is instead merely the consequence of hidden physical causal linkages that humans lack the wit or the instrumentation to detect.

If we could persuasively demonstrate well-attested large-scale TCCs, the argument might still go through. If, for example, shrieking gremlins frequently popped

abruptly into existence and under reliable controls were often both directly observed and caught on film doing so, we might have a strong contender for a persuasive TCC.

Unfortunately, such things are notoriously elusive. Unless we can find a something-out-of-nothing causal chain that we can guarantee has no fully-physical genealogy the facts of which we merely happen to be ignorant, all we have to fall back on as closure-violators are subjective, intra-human properties such as qualia and intentional states which, for all we know now, may eventually turn out to be fully grounded in our neurological circuitry. This is the sort of argument that Chalmers' conceivability argument for property dualism fails to gain sufficient traction against. Truncated causal chains in and of themselves may perhaps at times be interesting, and even evidential. But they seem of only limited help in finding solid evidence in our search for violators of the closure principle.

There is, however, another possibility. Earlier, I touched on the idea that there are actually two ways a causal chain might violate closure. One is when the chain simply irrupts into the physical universe with only an extraphysical causal antecedent. I've just finished considering this sort of "entering"-type truncated causal chain, and rejected it as a sufficiently unambiguous candidate for closure-violator status. Another violation of closure could occur were a causal chain to initiate within the physical domain, erupt out of it, then irrupt back into the world at some point distant in space or time. This might be called an "exiting-and-reentering" chain or – to keep my nomenclature parallel – an "intermittently truncated causal chain," or ITCC.

This sort of closure-violator would possess some virtues TCCs do not. An ITCC

would originate within the physical world, so if such an ITCC could be found, we could in principle observe its initiation, and perhaps even watch a few subsequent links of the ensuing causal chain unfold. But if it were a true closure-violator, we would then see it suddenly “disappear” as it exited physicality. Of course, if it never re-entered the world we would be epistemically no better off than with a standard TCC, since in isolation we might not always be able to distinguish the “exiting” portion of an ITCC from an event part of which is merely hidden from our view or has simply exhausted its causal properties. If, however, the chain were to reenter the physical domain elsewhere, and we were able to spot it and identify it as the very same ITCC which we previously observed exiting, then we may have discovered a plausible candidate for a violator of the causal closure principle and, hence, a counterexample to physicalism.

But (one might object), how do we know that this presumed ITCC’s missing causal links are not physical after all and just occluded from our limited view in the way that many altogether fully physical causal chains are? Or might this presumed ITCC not just present a case analogous to that of our “decisional” causal chain described above where, because of epistemic issues, its mental vs. physical status is simply not yet resolvable by current science?

The relevant question might be put this way:³

An evolutionary biologist, say, can explain a path from A to B, and later from C to D, but has not yet found a way to get from B to C. An astrophysicist can explain how a star, in going nova, goes from state A to B, and then from C to D, but as yet has no explanation for the B to C transition. There are explanatory gaps here, but I take it they don't challenge physicalism in the way [an ITCC

³Daniel Bonevac, personal communication, November 23, 2009.

might]. What is distinctive about the gaps to which an ITCC points?

There are a number of ways to respond to this question. First, the kinds of “gaps” suggested in this passage differ in important ways from a gap that might serve as a candidate ITCC. Our evolutionary biologist or astrophysicist is indeed presented with a definite “explanatory gap” which he or she must resolve to achieve a completed theory. But the entities or relations that can reasonably be expected (“might be predicted by theory”) to fill that gap are in most cases obviously and plausibly of the same ontological character as the entities and relations that are known to exist on either side of the gap. In many, if not most cases, a scientist can readily extrapolate what *kinds* and *natures* of things would most reasonably fill in the missing parts, even if there is uncertainty as to *which* entities or relations will eventually be shown to do the filling. Astrophysically speaking, for example, we may not know which things are going to bridge the gap from B to C in the causal story of a supernova, but we know that the possibilities are limited to certain categories of substances and forces, and we also know that further specific *kinds* – and only those kinds – of exploration and testing techniques are the appropriate tools to resolve the issues and fill the gaps.

However, the sorts of explanatory gaps we should look for in ITCCs are those which, on the contrary, seem to be at ontological odds with each (clearly physical) side of the gap in question – the “gap filler,” in other words, would be *unlikely* to be explainable in terms of (or compatible with) the kinds and natures of things that mark the edges of the gap we seek to fill. For our purposes, we are mostly interested in causes. So if the most obvious gap filler requires causation of a character that cannot be accommodated fully

within the physical domain, we have discovered a candidate ITCC. But how do we certify that we have found such a thing?

In the absence of the ability to actually track the links of an ITCC that are external to the physical universe, the only way to ascertain that an ITCC has components that really *do* exit the physical domain requires that we eliminate all possible physical mechanisms that might account for it. If we can with reasonable confidence eliminate every physical explanation, then we are left with an extra-physical account as the most plausible solution.⁴ This would be fairly weak “proof.” But assuming for the sake of argument that closure really *is* false, and assuming limits on our ability to see past the boundaries of the physical universe, TCCs and ITCCs demonstrate the only way such a state of affairs could *ever* appear – even if we did know all the facts and laws of the physical universe.

Let’s briefly consider the sorts of causal explanations that we would have to be able to eliminate to have good confidence we have found a suitable ITCC. First, as we shall see beginning with the next chapter, the sorts of causally-related ITCCs that are most likely to fit the bill involve unmediated causation (of some species or other) at a distance or over time. This might be referred to as “non-local” causation.⁵ It will be useful here to distinguish between “local” and “non-local” causation.

⁴As Holmes says to Watson in *Sign of the Four*: “How often have I said to you that when you have eliminated the impossible, whatever remains, *however improbable*, must be the truth?”

⁵For our purposes here, though, we should not confuse “non-local causation” with the quantum physics phenomenon known as “quantum nonlocality,” which I will shortly mention here, and which will be discussed in some detail in chapters nine and ten.

Marc Lange (2002) has given a helpful analysis of the notion of “local” causation. Importantly, local causation is a *spatiotemporal* concept, which partakes of aspects of both spatial locality and temporal locality. Roughly, spatiotemporal locality requires “that there can be no gap in space or time between a cause and its direct effects” (p.7). But spatiotemporal locality requires more than a mere additive function between the two, since a cause could conceivably violate the one and not the other (p.14). For example, Lange specifies spatial locality somewhat along the following lines: For spatially local causation to hold, event E must have a complete set of causes that occur in sufficiently close spatial proximity to the event. Thus, spatial locality requires “a complete set of causes...no member of which is separated from the effect by a finite non-zero distance” (p.292).

Temporal local causation is construed similarly, but in terms of time rather than space – event E must have a complete set of causes that occur in sufficiently close *temporal* proximity to the event. For spatiotemporal locality to exist, both conditions must apply. But it is conceivable (though not physically demonstrable) for E to have sufficiently proximate *spatial* causes, which are, nonetheless, insufficiently proximate *temporally* (and, of course, vice versa). If causation could occur in such a case, it would have to be “non-local” – which violates our notions of physical causation (p.14). But it is just such violations we are looking for in ITCCs.

As near as I can tell, there are only four possible ways of accounting for any sort of action or causation at a distance: mechanical transfer of causal influences, electromagnetism, quantum nonlocality, gravitational influence, and mere brute causal

connections. I will consider the first three (mechanical, electromagnetic, and quantum nonlocal effects) in chapters 9 and 10 in some detail, so I will not go over them here. And since it is implausible that gravitation is information-bearing, and thus cannot account for the kinds of ITCCs I will soon identify, I will dismiss it from further consideration. However, the notion of brute relationships I will briefly treat here.

Lange (2002, 95-107) wonders what it takes for causation to count as “local.” He decides that under “local causation” we understand *that* causation which transfers across space and time by means of some medium, force, or field (it appears that mechanical and electromagnetic causation fulfill the criteria for local causation). But why does causation have to be “local,” he further wonders? It turns out that there is no absolutely knock-down reason beyond that we are accustomed to associating with all causation the need for intervening steps, connections, or media to pass causal influence from its initiation on to its resultant effect (“...the argument for locality is inconclusive even granting its assumptions about the explanations that a local account would supply” [p.107]) Whether because we have only ever experienced causation in this way, or because of our psychology, we seem constitutionally indisposed to find any other sort of “non-mediated” causation congenial – at least as far as physical explanations are concerned (cf., Lange, 2002, 1-3).

The issue becomes further complicated by the question of whether the fields that are clearly so important to electromagnetic causality are themselves “real” in a relevant sense, since the only way to know a field is “there” is the propensity for an entity of the right kind with the right properties to behave in a certain way if it approaches sufficiently

close to the field's (apparent) sphere of influence. Without the effects fields have on such entities (iron filings, particles, radio antenna, EMF meters), and other detectable influences they might produce, a field might be non-detectable – and hence, perhaps non-existent. So if a field is only identifiable by its effects, how is this any evidence that the causation is anything other than “nonlocal”? Perhaps this kind of action/causation-at-a-distance really *is* unmediated – that is, just a brute relationship that obtains by virtue of the way the universe is.⁶

If this is so, then it endangers the idea of ITCCs. In the case where apparently unmediated action/causation-at-a-distance occurs, we could merely subsume it under the category of “brute causation at a distance.” The mystery is solved, with no threat to physicalism. But I suggest this solution would strike any rational person as unreasonable. Surely we want a more satisfactory response than this (and I am not suggesting anyone advocates it – but it is still something to be dealt with). In fact, this approach seems to beg more questions of physicalism than it answers. If causation-at-a-distance is merely brute, how could we possibly certify that the causation involved is physical? Indeed, brute causation would seem to be just as plausibly explained in nonphysical terms rather than physical. For, after all, how could we tell the difference? Anyone choosing to lodge a “brute causation” defense against an ITCC-based argument would seem to create more problems than solutions, especially for physicalism. If

⁶Note that Lange is not committed to this position. He later presents arguments to support the reality of fields – but even then ends with the admission that whether talk of fields describes real entities or merely employs a useful convention to describe a form of unmediated action-at-a-distance remains somewhat inconclusive. (Lange, 2002, 40-42;

causation-at-a-distance is merely brute, that would (it seems) effectively limit science's ability to come up with the full account of the universe it seeks to achieve by rendering much of the necessary causal architecture obscure (and, incidentally, prove Hume prescient!).

In the light of these problems – plus the strongly apparent local nature of at least some of the causal relations so far explored by science (mechanical transfer of forces, electrical conductivity, and so on), it seems most plausible that physical causation is local through and through. So, if we can reasonably be doubtful of brute *physical* causation as an explanatory contender, what then might an ITCC look like in terms of our Gremlin example?

Suppose we could confine a Gremlin in a laboratory so that he⁷ could not escape through any physical means, and we outfit him with a non-removable tracking collar. We record with our instruments and observe as the Gremlin dematerializes and the tracking signal suddenly disappears, after which we certify that there is no physical Gremlin present in the laboratory, nor has any been detected leaving the premisses. At likely Gremlin reappearance points elsewhere, we have stationed further observers and monitoring instrumentation has been set up.

At one of these points thousands of miles away, the Gremlin, still wearing his collar, suddenly materializes and we again detect the tracking signal. Calibrated atomic clocks show that the disappearing and reappearing occurred as close to instantaneously as

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⁷ For the sake of this thought experiment we assume that all Gremlins are male.

can be determined, down to the finest decimal place that our clocks are capable of measuring.

Clearly, something causal happened. The Gremlin was in one location, but now is somewhere else miles away. It seems evident that no physical account can give a satisfactory causal explanation, nor is there a plausible physical theory that even *promises* an explanation. The best explanation at this juncture would be that closure was violated.

This discussion of ITCCs does raise a couple of important objections. First, how do we know that even if we manage to exclude every known physical mechanism that might otherwise account for the “missing” links of an ITCC, we might not later discover other physicalist explanations that are unknown today? This is an important question, but one which I shall defer to a later chapter for consideration. Second, why should we even worry that such things as closure-violators exist? If they did, wouldn’t we have noticed one long ago? This concern we can begin to answer shortly.⁸

In principle, a standard TCC is metaphysically sufficient to falsify closure. But due to the issues just discussed, TCCs fall short of providing sufficient evidence. Though

⁸ In an insight I owe to Dan Bonevac, we are not without experience in dealing with these *kinds* of questions, at least by analogy. Through careful observation, data collection and analysis we can, for example, identify influences entering seemingly-closed physical systems. ecosystems may exemplify self-contained, mostly self-sufficient worlds. But through observing atypical or seemingly-anomalistic (for that system) changes and developments within the ecosystem, we can get our first alert that some new, heretofore unidentified influence is making itself felt from outside the system. In another example suggested by Bonevac, suppose the inhabitants of a spaceship moving at constant speed through the galaxy with no means of external observation were to experience an abrupt jolt orthogonal to their line of travel. They might conclude (likely

metaphysically ITCCs tell no more strongly against closure than TCCs, they are evidentially much stronger, and so we should be most interested in finding ITCCs.

What might such an ITCC look like in our (presumably) non-Gremlin world? I will give here a brief notional example. Suppose someone in a sealed, electrically-shielded room in Austin, Texas were to sit at a table and mentally “transmit” a sketch she was drawing, while a second person in a similar, sealed and electrically-shielded room in Freiburg, Germany were to attempt to mentally “receive” the sketch, with no intervening sensory or technical means of communication available. Suppose further, that the second person did indeed succeed in reproducing a reasonable facsimile of the sketch, not hours or even moments later, but at what calibrated clocks attested was exactly the same time as the person in Philadelphia was creating her sketch.

This would seem to meet the criteria for an ITCC. The front end of the causal chain – the sketching in Austin – is apparent, and can be monitored. The back end of the causal chain, the reproducing of the sketch in Germany, is also apparent. But in the obvious absence of any known physical means of communication (indeed, even of any plausibly *speculative* physical means of communication), the presence of closure-violating causal links becomes a possibility that must be taken seriously.

The question is, do such real examples exist to be found? I think they do, and it is the task of the next few chapters to point them out and assess how well they fit the criteria we have specified to qualify as a closure violator.

correctly) that the jolt was the result of some external cause.

Chapter 5: Counterexample to Physicalism

In previous chapters I argued that universal physicalism has become the dominant paradigm in both science and philosophy, thanks to an inference-to-the-best-explanation (ITBE) grounded in the vast accumulation of inductive evidence which supports physical explanations for the phenomena contained in the world. I argued further that *a priori* philosophical arguments could not defeat physicalism because physicalism is at root empirically-based, and therefore only empirically-based arguments could make any headway against it. These empirically-based arguments would require a certain kind of evidence with specific unusual features. Unless such evidence exists, physicalism remains the best account of the universe. In this and the following three chapters, I will make an inventory and assessment of just the sort of empirical evidence that can support counter-arguments to physicalism.

The kind of evidence with the best prospects for threatening physicalism must directly challenge the validity of the assumption of the causal closure of the physical domain. Causal closure stipulates that all physically-relevant causal chains must initiate and remain fully within the physical domain throughout their life-spans. The truth of closure is, like physicalism, assumed on the strength of the inductive evidence for physicalism and an ITBE which asserts that the phenomena we encounter in the world can best be explained if we consider the physical universe closed to external causal

influences. If we show that the physical universe is not causally closed, then closure is falsified and with it physicalism as a sufficient explanation of the universe.¹

But does evidence exist that proves closure false? I argue that it does: specifically, the evidence produced by scientific parapsychology. In what follows I will soon consider how it counts against closure. What is ‘scientific parapsychology’? One definition says that parapsychology is “the study of interactions between behaving organisms and their external environment, which occur under conditions precluding participation of the sensorimotor system” (Dean, 1970). The problem with this definition is that in many parapsychology experiments a subject’s sensorimotor system *does* seem to be engaged locally, even though affected by some apparent non-local influence. A better definition is that offered by the Parapsychology Association, an affiliate of the AAAS:

parapsychology studies apparent anomalies of behavior and experience which exist apart from currently known explanatory mechanisms which account for organism-environment and organism-organism information and influence flow. (Parapsychology Association, 1989)

Another source identifies parapsychology as “the scientific study of experiences which, if they are as they seem to be, are in principle outside the realm of human capabilities as presently conceived by scientists” (Irwin & Watt, 2007, 1).²

¹Notice that this falsification doesn’t render physicalism toothless – it merely redefines the boundaries of the types of phenomena that physicalism can fully explain.

²I will put off until later consideration of whether and to what degree such locutions as existing “apart from currently known explanatory mechanisms” and “outside the realm of human capabilities as presently conceived by scientists” actually matter to my overall argument.

Parapsychological phenomena seem to violate the facts and laws of science as presently understood and, according to many scientists and philosophers, cannot be accommodated within physicalism.

The term *parapsychology* was coined in 1897 by German philosopher Max Dessoir and adopted in the 1930s by pioneer parapsychologist J.B. Rhine (Rhine, 1934, 7; Dessoir, 1889). An unsigned editorial in the March 1937 maiden issue of the *Journal of Parapsychology* argued for adoption of ‘parapsychology’ as a label for the newly-emerging controlled laboratory research on so-called ‘psychic’ phenomena, and distinguish it from field work on apparitions, trance mediumship, and other anecdotal types of phenomena which had come to be identified with ‘psychical science,’ the term then in common use. I have above specified *scientific* parapsychology to emphasize it is this distinction I have in mind, to avoid potential confusion generated by less precise popular usage of the term that has accreted to it over the decades since its adoption.

There are two general branches of parapsychological research. One has to do with so-called extrasensory perception (ESP³); the other is generally referred to as psychokinesis, or PK – often informally termed “mind over matter.”⁴ ESP is a relatively

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As an alternative term to “ESP,” I shall also use the label “psi” from time to time, since this is used by some researchers as a replacement term. “Psi” is the first letter of the word “psyche” has been adopted by many parapsychologists to stand for, roughly, “the thus-far unknown feature of the universe that facilitates ESP experiences.”

⁴ESP and PK and the terms for which they are acronyms are often considered obsolete terminology, presumably replaced by “anomalous cognition” (AC) and “anomalous perturbation” (AP). Due to serious deficiencies (which are not relevant to consider here) in these more recent terms, I will default to ‘ESP’ and ‘PK’ as both are in my view more descriptively accurate and more widely understood.

passive activity usually involving apparent information transfer from targets or “senders” sequestered from the human perceiver by distance, intervening shielding, or displacement in time, or a combination of these. PK is an active phenomenon in which non-mental matter is apparently physically affected by the conscious or, in some cases, unconscious intentionality of a human subject. (Broughton, 1991)

My focus will be primarily on ESP. Because of problems regularly encountered in producing PK and replicating it in experimental environments, I will consider PK only in passing in my discussion. Further, I will not consider at all other traditional areas for parapsychological research, such as hauntings, poltergeist phenomena, death-survival evidence, and so on. These phenomena are typical truncated causal chains (TCC), which I argued in the preceding chapter were evidentially the most problematic. Second, though all this material does count as evidence for closure violation, it is largely anecdotal, not well controlled, and subject to variations in interpretation. I make no commitment nor draw any conclusions as to the nature or reality of this assortment of phenomena.

I recognize that parapsychology as a body of evidence is highly controversial. Indeed, except for the comparatively few occasions when it is under attack, it has been largely ignored or outright dismissed for a reason I shall now briefly assert: and later treat in more detail: ESP and other parapsychological effects are once acknowledged to be real, this seems to imply that closure and, hence, physicalism (as it is construed today), is false. I will consider the various aspects of the controversy in chapters 11 and 12. Suffice it for now to say that many philosophers agree that, were its claims proven to be

legitimate, parapsychology might pose a threat to physicalism. Poland, cited in chapter 1, notes that the job of physics is to study all and only what exists in nature. And for that reason “ghosts, gods, and the paranormal are genuine threats to physicalism: they appear to be *in nature* in various ways, but they are not encompassed by physics” (1994, 228).

And as Melnyk notes,

[I]f the mind were discovered to have causal powers (e.g., telepathic or psychokinetic powers) that it would be impossible, according to current physics, for a physically realized system to possess, then the mind could not be a physically realized system. So the discovery of (e.g.) telepathic or psychokinetic powers would simultaneously falsify realization physicalism and support dualism. (2005, 189)

But now, before considering all the arguments made against taking parapsychology evidence seriously as evidence against physicalism, I will examine what the evidence actually amounts to (since there is much confusion about it). Additionally, I must consider what properties sufficiently well-attested parapsychological phenomena have that suit them to be taken as a counterexample to physicalist claims. By engaging primarily with ESP-type experimental regimens, I deal for the most part exclusively with in-and-out causal chains – what I am calling ‘intermittently truncated causal chains’ or ‘ITCCs’ – since they provide the most persuasive and accessible evidence for closure violations, as explicated in the previous chapter. The experiments I will cite are designed to force a situation where, if there is to be a result at all, it must be the consequence of an intermittently truncated causal chain. The experimental paradigms I will consider are these:

- *Presentiment Experiments.* – which show that human subjects can experience an autonomic response and have limited pre-conscious cognition of emotionally-charged stimuli as many as several seconds in advance of actually being presented the stimuli. These experiments appear to violate closure through precognition (that is, becoming aware in the present of a state of affairs in the future).
- *Distant Mental Interaction with Living Systems* or DMILS – which show that human subjects can detect autonomically and sometimes be consciously aware at a distance of the focused attention of another human, though fully shielded from sensory awareness of such attention. There are two sub-categories: Remote Mental Interaction (RMI) and Remote Staring Detection (RSD), both of which seem to demonstrate closure violations with either nonphysically-mediated action-at-a-distance, or remote awareness, or both.
- Remote viewing (“anomalous cognition”) experiments, which show that human subjects can perceive and accurately describe physical locations, objects, persons, etc., even when sequestered from any and all known forms of sensory or technical sources of information. This perception occurs without artificial means or sensory connection to targets separated from the perceiver by intervening shielding, by distance – up to thousands of miles – or by time (e.g., past, distant past, or near future). This demonstrates remote awareness and nonphysically-mediated perception-at-a-distance.
- Associative Remote Viewing (ARV) experiments, – a specialized approach to remote viewing in which human subjects are asked to describe a target that will not be presented to them until some time (ranging from minutes to days) into the future as a means of predicting the outcome of a (usually binary) future event. These experiments seem to show both remote awareness *and* precognition.

There is a much larger set of experimental paradigms in parapsychology that might prove equally illustrative. Though I will likely touch on any number of these, I will only consider in detail the four I have mentioned, for two reasons: for the sake of space and these are especially straightforward examples of ITCCs. I will consider each of these four research paradigms in turn, and in detail (one will be covered in this chapter, one in chapter 6, and one each in chapters 7 and 8).

The first two paradigms I will consider fall under the category sometimes referred to as “unconscious psi” (Radin, 2006), since they involve non-cognitive or pre-conscious human psychological systems that function outside of awareness (for example, assessing autonomic nervous system reactions) (Radin, 2004, p. 254). The first I consider below, and a further, perhaps somewhat related variation (with its two sub-categories), will come in the next chapter. In chapter 7 and 8 I will consider two paradigms involving “conscious psi.”

PRESENTIMENT EXPERIMENTS

The idea behind presentiment experiments owes its genesis to two different sources. One is the corpus of anecdotal parapsychological reports, typified by the many instances of foreboding or premonition that are reported to have preceded some dire or emotionally-charged event that was yet in the experiencer’s future. Since these are all anecdotal accounts which can only be assessed retrospectively from field reports, researchers decided to explore whether the essence of the experience could be abstracted away from subjective experience and distilled down into a controllable experimental framework suitable to a laboratory environment.

The second impetus for the presentiment protocol comes from the large body of psychology and parapsychology research using physiological monitoring techniques for detecting and measuring autonomic arousal evoked by subconsciously-perceived stimuli. These two traditions came together in the mid-1990s to inspire the first presentiment experiments (Lobach, 2008; Radin, 1997, 2006)

The word “presentiment” literally means *pre*-sentiment, or subjective, emotion-based “sentiments” that are experienced by a subject in advance of an exposure to an emotionally-charged stimulus. Presentiment experiments exploit the “orienting response,” a well-attested psychological reflex that serves to prepare individual organisms, including humans, to respond to important changes in their environment, such as a threat from a predator or a dangerous natural event or, conversely, a suddenly available mating or feeding opportunity. This is a non-cognitive response, in which the reticular activating system (which governs attention and wakefulness) and parasympathetic nervous system (the branch of the autonomic nervous system concerned, among other things, with the “fight or flight” response) are particularly involved. When confronted with a startling, sudden or unexpected event, the orienting response automatically initiates to bring an organism’s perceptual resources to bear in determining whether the event portends a threat or other major stimulus and, if so, what action to take. The response is accompanied by significant autonomic arousal in the form of increased heart rate and skin conductance levels, and lower blood volume (as measured in the lab by finger tip sensors).

Researchers speculated that if indeed there was any substance to reports of premonitions which preceded fearful or highly emotional events, then these “pre-responses” should also be accompanied by a semblance of an orienting response – albeit less pronounced given the lower intensity of the not-yet-experienced stimulus. If such were the case, then the autonomic markers of such orienting “pre-responses” should in

principle be detectable with the proper instrumentation and statistical methods, given a future stimulus with the right properties and of sufficient intensity.

Researchers who carry out these experiments are interested to see if there might be an effect that is temporally displaced, whether as a backwards-looking “retrocausative” effect or a forward-looking “precognitive” effect.

Typical presentiment experiments require a strong contrast between calm and emotional stimuli. Sets of such stimuli are constructed by gathering a body of photos with widely-varied affective (emotional or otherwise arousing) content. A target photo set is constructed from a mix of calm photos and emotionally-charged, “disturbing” photos. Calm photos may be pastoral scenes, tranquil settings, and so on. Emotionally-charged photos include three categories – erotic, violent (including gruesome subjects such as accident aftermaths and autopsy photos), and disgusting/aversive/fearful (such as snakes, spiders, etc.).⁵

The photo target set is digitized and loaded into a desktop computer system. Software in the computer is configured in most cases to display photos individually only after the participant initializes the trial (usually via a mouse click) and then only after a delay.⁶ What photo is shown and whether it has calm or disturbing content is fully randomized within the computer. The photo is usually not chosen by the randomization protocol until just before it is displayed.

⁵Such a scientifically-validated photo set is available in the International Affective Picture System (IAPS), which has been widely used for many other research purposes beyond parapsychological experiments. (Lobach. 2008)

⁶Some of the more recent experiments present the images on a fixed schedule.

A typical presentiment experiment unfolds in this way: A participant takes a seat in front of the computer in an isolation room. The participant is connected to autonomic-system monitoring equipment, typically for skin conductance, heart rate, or blood volume (fingertip photoplethysmograph), or combinations of all three. Once ready to begin, the participant presses the mouse button to start the stimulus sequence. There is a delay for a set number of seconds during which the computer randomly selects a photo and the participant's physiological markers are monitored. Monitoring continues as the photo is displayed for the participant to view for a set number of seconds. At the end of the observation period, the computer screen returns to a blank state for a rest period. At the end of this rest period, the computer screen displays a message asking the participant to initiate the next stimulus sequence when he or she is ready. This process continues for the specified number of trials – for example in one early set of experiments it was 40 trials at a sitting.

Experimenters expect to see the usual dramatic increase in the participant's autonomic indicators after a disturbing photo is displayed, and a much less pronounced increase in those indicators with the calm targets. But if there is a time-reversed or forward-time displaced presentiment effect, there should also be a slight but still noticeable anomalous autonomic arousal *in advance* of the presentation of disturbing/emotional photos which is not present prior to the display of calm ones. And this is indeed what was found over several such experiments conducted in different laboratories around the world.

As Dean Radin, the designer of the original presentiment experiment puts it:

The idea of presentiment assumes that we are constantly and unconsciously scanning our future, and preparing to respond to it. If this is true, then whenever our future involves an emotional response, we'd predict that our nervous system would become aroused before the emotional picture appears. If our future is calm, we'd expect to remain calm before the picture appears. Of course, *after* an emotional or calm picture appears the response is well understood as the "orienting reflex." This is the body's predictable reaction to a novel stimulus, in which it momentarily tenses up while evaluating whether to fight or flee. (2006, p.166)

Since the participants are biological systems, there are confounding issues that must be taken into account in order to assess whether or not an anomalous effect has occurred. Some factors work to reduce the magnitude of possible real results. For example, habituation – the tendency of organisms (including humans) to become both consciously and physiologically accustomed to a repeated stimulus. As participants become habituated to the disturbing photos in the target set, they may demonstrate increasingly less autonomic arousal. This would cause a decreasing effect across a set of trials, producing a diminished significance level in the data. Measures taken to minimize this effect include mixing a higher ratio of calm versus disturbing photos into the target set, and limiting the number of trials a participant executes at one sitting.

Fatigue and boredom can also present problems. If a session lasts too long, or if the participant begins already in a fatigued state, the orienting effect both before and after display of the target photos may be attenuated. A participant's level of alertness versus inattentiveness overall can affect the data. A cough or sneeze, wandering attention, or other distraction can adversely impact the effect being studied.

Idiosyncratic responses between participants can be a further confounding variable. Individual humans manifest varying reactions to or tolerance for stimuli. For example,

participants from the Netherlands demonstrate less reaction to erotic pictures; emergency room personnel may not react to violent or gruesome images. And, as at least one study shows, females may react more strongly to violent images than do males. Protocol and statistical measures can be employed which adjust somewhat for these considerations, but they do not always fully ameliorate these complications.

There are other issues to consider that could work as sources of error – to indicate an effect when none is actually present or, when there is an effect present, to indicate the effect is stronger than it really is. Presentiment researchers have a number of these that they consistently check to make sure that observed effects are not merely the product of an artifact of experiment design or human nature.

For example, Radin (1997b) explored six possible alternative explanations to account for the highly significant findings reported in his study. These were, along with his explanations:

1) *Results were due to chance* – Statistical analyses employing two different approaches showed this not to be a tenable explanation. According to Radin, “differences larger than those observed in the actual data would be unlikely with $p = .008$,” showing that “chance is not a viable explanation for the observed results” (p. 175).

2) *Inadvertent cuing* (as one example, perhaps through sounds emitted by the computer that were different before display of disturbing vs. calm images) – Experimental design makes this impossible. A random number is generated, but does not select a calm vs. disturbing photo until the instant the photo is displayed. This occurs in RAM processes, so there is no hardware operation to create subtle sound variations. Further, software design eliminated the possibility of inadvertent display of images outside the specified time windows. Experimenters with a knowledge of the image pool were not present during trials, and in any case were unwitting as to presentation order of the photos.

3) *A fault in analysis of the pre-stimulus period* – Analysis techniques showed a false-positive during the pre-stimulus period of interest. This is also unlikely, as the same type of analysis was performed on the well-understood post-stimulus orienting response epochs, and yielded precisely the values that would normally be expected. It would therefore be reasonable to assume that since the same analytical method was applied uniformly to both pre- and post-stimulus periods, the results would be equally trustworthy.

4) *Faulty randomization in presentation of disturbing vs. calm targets* – Allow participants to guess at a higher than chance level of success. A post-hoc examination of the order of target presentation using three different statistical measures confirmed that the sequence had indeed been fully random, making participant guessing an unlikely explanation.

5) *Disturbing images not as pronounced as intended* – An examination of experimental data showed that calm pictures did not produce an orienting response, whereas the disturbing ones did. Further, a jury of six persons (three men, three women) rated all photos employed in the experiment in a randomized sequence according to a scale of 1 to 5, with 5 being most extreme, 1 being most calm. This subjective ranking confirmed the apparent significant affective dichotomy between the two categories. A further statistical test was performed to verify whether this subjective assessment of the degree of extremity among disturbing targets correlated with the pre-display response (“preresponse”) of the subjects during the experiment. This test confirmed that it did.

6) *The effect was due to anticipatory strategies* – A worry exists that, as a sequence of calm photos is presented, a participant’s anticipation grows that soon a disturbing image “has” to be presented just by the laws of chance (this is a version of the gambler’s fallacy). This could result in “false positives,” thus skewing the data. Radin employed a statistical Monte Carlo test, simulating a participant working within an optimal anticipatory strategy, and found that the “arousal levels [were] far too small to account for the observed physiological effects” (p. 177).

It is important to consider these and other possible alternative explanations for the data since, as noted in the base argument, one must rule out all physical explanations in order to establish that an ITCC is involved.

Results

The first set of four presentiment experiments was conducted at the University of Nevada, Las Vegas in 1996 by Dean Radin, the originator of the presentiment experimental paradigm, and published in a 1997 paper (Radin, 1997b). Altogether, 31 participants were presented a total of 1,060 photo targets. The presentiment effect was demonstrated at a highly significant value of $p = .002$, or odds against chance of 1 in 500 (Radin, 2006, 324).

In Radin (2000) he repeated his own results. Two experiments – the first involving 48 participants, the second with 50 – once again tested the presentiment effect, but added two new (but related) hypotheses: 1) that participants who showed the most consistent response to calm stimuli would also demonstrate the strongest presentiment response to disturbing photos, and 2) that there would be a positive correlation between level of arousal before emotional stimuli and the emotionality ratings of the photos for this same group of consistent responders.⁷ Results across both experiments for hypothesis 1 (that consistent responders would show a greater presentiment effect) yielded $p = 0.04$ and for hypothesis 2, $p = 0.008$, which supported the reasonable conjecture that greater contrast between disturbing vs. calm content would produce a stronger presentiment effect.

This showed a further dimension of replication, in that the presentiment measure seemed sensitive both to variations in individual human nature, as well as in more

⁷The second, 50-subject experiment introduced a couple of other measures, as well – a 6-second pre-stimulus recording period, rather than the previous 5-second period (it was suspected that the presentiment effect might actually begin to manifest earlier

generalized human emotional response, just as one would expect to see if it were a real effect rather than an artifact of the design. Analyzing all aspects of the study's two experiments "provided evidence for an unconscious time-reversed effect in the human nervous system" to the level of $p = .001$ (Radin, 2000, 1).

Further replications were carried out in the Netherlands by Professor Dick Bierman at the University of Amsterdam. These were first briefly reported in Bierman & Radin (1997), but more thoroughly treated in Bierman (1997) and Bierman & Radin (2000). These involved three experiments of varying configurations. The first, using 16 subjects, was a replication of Radin (1997b), with one change – instead of drawing targets from a large pool, three sets of photos with different ratios of calm-to-disturbing photos in each set were chosen randomly to assure that the set which was chosen for each participant *and* the ratio of photos within the set was unknown to the experimenter. The second experiment involved 32 subjects who were shown 10 photos, all of which were calm with the exception of one disturbing photo mixed in randomly. The participants were unaware that they might view a disturbing photo. The third experiment involved the same 32 participants, now informed that there would be occasional disturbing photos.

The various modifications to the three experiments were all intended to test for the presence of an invalidating anticipatory strategy, in addition to testing for the presentiment effect. Results of the first experiment were statistically significant at $p = 0.016$. (In this experiment, of the disturbing photos, the violent images elicited more of a

than previously supposed); and the execution of 30 trials per participant, rather than the 40-trial sets of previous experiments (this was hoped to alleviate fatigue).

response than did erotic ones among the largely female participant set, which contained 13 females, to three males. This trend was indicated in later experiments as well.) The second experiment produced results in the positive direction, but fell short of statistical significance – at least partially because of its much smaller statistical power (however, the same results-trend between violent and erotic images was indicated). The third experiment showed a stronger, but still non-significant positive result. Here, also there was a differential for violent vs. erotic photos.

Other full or partial replications include Norfolk (1999), Wildey (2001), Spottiswoode & May (2003), Broughton (2004), McCraty *et al* (2004a, b), Radin (2004), Bem (two studies – 2003 & 2005), May *et al* (2005), Hinterberger *et al* (2007), and Bierman (2007). Some of these warrant further discussion here. (Others I will elaborate on subsequently.)

Broughton (2004) for instance, reports only a partial replication of the presentiment effect. This study attempted to link sensitivity to presentiment with certain personality measures – notably extraversion and intuition, as measured by the Myers-Briggs Type Inventory (MBTI) personality assessment tool, and the Openness to Experience factor in a similar personality instrument, the NEO – which had been reported in the literature to correlate with general success at ESP-type tasks. Overall, and despite using different statistical measures to evaluate the results, no presentiment effect was noted. However, in an analysis of how the presentiment effect correlated with personality variables, there were two significant results – for those participants who scored high on the MBTI Intuition scale ($p = 0.021$), and for those who scored high for

the Openness factor in the NEO ($p = 0.012$). What this means is that, as hypothesized, participants with those particular personality traits did manifest a statistically-significant presentiment effect. The overall results in Broughton's presentiment study were disappointing, but that the personality aspect of the experiment reached significance was intriguing.

Radin (2004) collected and reanalyzed three experiments reported in Radin (1997)⁸ and Radin (2000), plus reported a further experiment done with another six participants. Combined, the experiments included 4,569 trials contributed by 133 participants. Two of these experiments had results in the positive direction but did not reach statistical significance. The combined statistics were nonetheless highly significant at $p = .000013$. (Radin, 2004, 265)

Wildey (2001) added a unique twist. Using the standard presentiment protocol, he performed 314 trials with 15 human subjects, yielding a positive result which fell just short of statistical significance.⁹ But to test a further hypothesis about consciousness,¹⁰ he also performed 231 trials (using an unpleasant vibration as a noxious stimulus) on earthworms. This also yielded results in the positive direction, but still non-significant.

⁸Leaving out two that were too dissimilar for combining but were themselves independently statistically significant.

⁹Data was presented in chart form, so extrapolating p values was not feasible.

¹⁰That proposed by Roger Penrose and Stuart Hammeroff, that consciousness is a consequence of quantum functions inherent in certain structures within all neurons, including those present in lower orders of life.

McCraty *et al* (2004a) took a different tack. Whereas standard presentiment experiments monitored electrodermal activity (EDA), using changes in skin conductance as the indicator of autonomic arousal (with some including heart rate and blood volume measures as well), McCraty (2004a) added ECG (electrocardiogram) monitoring to complement the SCR measure. The ECG tracked acceleration and deceleration trends in heart rate.

Using 26 participants (11 males, 15 females) and a standard disturbing/calm image target pool, this experiment generated 2,340 trials. Results for the skin-conductance were non-significant, though there is a slightly observable increase in EDA prior to display of disturbing photos under the study's second condition.¹¹ The ECG data, though, showed a clear pre-stimulus presentiment effect prior to display of disturbing photos, as measured by a heart-rate variability index. Cross-subject results were highly significant for condition 1 ($p = 0.001$), and was higher for female participants (condition 1, $p = 0.004$; condition 2, $p = 0.01$) than males (condition 1, $p = 0.03$; condition 2, non-significant).

McCraty concluded that physiological heart response “occurred approximately 4.75 seconds before the stimulus was actually presented. This is where the slope of the heart rate deceleration curve for the emotional trials clearly starts to diverge from the slope for the calm trials” (McCraty, 2004b, 331). Since the heart is also governed by

¹¹This research had as a secondary purpose testing the efficacy of a meditation-like technique which was expected to modify participants' response to emotional stimuli. Condition 1 was non-meditative state, while condition 2 was a post-meditative state.

autonomic functions, this does provide a plausible alternative to EDA monitoring as a measure of presentiment.

There is a further sub-class of this type of research, called “pre-stimulus response” (PSR) experiments, which are related to and suggested by the presentiment protocol, but which introduce variations on the original presentiment design. Though the experimental design for each of these may differ from the classic presentiment case, PSR experiments could be considered conceptual replications of the presentiment paradigm – or at the very least of the same category and class of experiment. This is because in both PSR and presentiment experiments the effect under examination involves non-cognitive perceptual experiences which are temporally-displaced in the same direction by approximately the same magnitude.

Spottiswoode and May (2003), for example, followed the general presentiment experimental protocol. But they recognized the problem of statistical “noise” which can be introduced by individual participants’ idiosyncratic responses to certain photographic subjects (e.g., emergency room nurses not finding gruesome photos as startling as a pre-school teacher might), and decided to forego the usual disturbing vs. calm image pool altogether. As a replacement for disturbing photos they elected to use a 97db burst of white noise as a startling sound stimulus. This was juxtaposed with short periods of silence as a control (which took the place of calm images). Sessions contained 20 stimulus periods, divided randomly between periods of silence and bursts of white noise. Using 125 participants, results as determined by skin conductance measures were found

to be highly significant ($p = .00054$, odds against chance 1 in 1,900), meaning that a pre-stimulus effect was consistently detected before the loud sounds, but no effect was observed in advance of the periods of silence.¹²

This experiment was subsequently replicated at a college-level educational institution in Hungary by May *et al* (2005) with a set of 50 participants. Two researchers who had not participated in earlier presentiment experiments conducted two identical sub-experiments (Vassy - 21 participants; Paulinyi - 29 participants) yielding closely similar effect sizes, with overall results which were also highly significant, at $p = 0.0018$.

Cornell University psychologist Daryl J. Bem's (2003) "Precognitive Habituation" (PH) experiment turned around the classic presentiment experiment to take advantage of the usually troublesome habituation effect. In this case, he exploited a version of habituation based on the "Mere Exposure Effect," a heavily-researched psychological feature in which increased frequency of exposure to a stimulus produces an increased tolerance to or preference for the stimulus.

Bem's experimental design was carefully crafted, but somewhat complex. In effect, he describes it as a Mere Exposure study run in reverse. As he explained:

Instead of exposing a participant to repeated exposures of a stimulus and then assessing his or her liking for it, the PH procedure reverses the sequence: On each trial, the participant is first shown a pair of photographs on a computer screen and asked to indicate which picture he or she prefers. The computer then randomly selects one of the two pictures to serve as the "habituation target" and displays it subliminally several times. If the participant prefers the picture subsequently designated as the target, the trial is defined as a "hit." (Bem, 2003, 2)

¹²Spottiswoode & May devoted a substantial part of their paper to a systematic analysis of plausible non-ESP ways of accounting for the data, and were able to satisfactorily dismiss each in turn. In the interest of space I will not cover those here.

This experiment exploited two features of the Mere Exposure effect. 1) The ME effect operates unconsciously – in fact, it is enhanced through subliminal exposure and, conversely, is inhibited by conscious processing; and 2) stimuli rated as strongly affective by a subject on the first exposure are reported to be noticeably less extreme after repeated subliminal exposure.

In Bem's experiment, the measure of ESP effect was the difference between whether the image chosen by the participant as the preferred image in the set of two images presented, matched what the computer subsequently randomly chose and subliminally exposed numerous times to the participant. More simply put, the test was to see whether the ME habituation effect was "transmitted" back in time to influence the participant's initial choice between photos. As Bem noted "The Precognitive Habituation hypothesis is that the repeated exposures of the target can reach back in time to diminish the arousal it would otherwise produce, thereby rendering negatively arousing targets less negative and positively arousing targets less positive" (p.4).

Photo pairs matched for valence and arousal¹³ were selected for negative-affect (violent, horrifying) images, positive-affect (erotic) images, and neutral ("calm") images. The prediction was that no precognitive habituation effect would be noted for the neutral images, but there would be such an effect observed for positive and negative affect images.

¹³Emotion can be assessed along two dimensions, valence and arousal. Valence is a measure of where on a spectrum between positive and negative an emotional indicator might fall; arousal captures the intensity of an emotion from neutral to extreme.

Chance results (e.g. no precognitive habituation effect present) should reflect a flat 50% hit rate (that is, the target photo subsequently selected by the computer should match that chosen by the participant only half the time). And this was in fact the outcome for the neutral targets, just as predicted. In six basic experiments involving 260 subjects (159 women and 101 men), the hit-rate on neutral targets produced results at chance. On the other hand, results for the negative affect images produced hits highly significantly above chance ($p = 0.0008$), while erotic (positive-affect) images scored significantly below chance ($p = 0.031$), both in the directions anticipated.

Bem ran two more experiments, testing the “supraliminal” condition, wherein the post-selection habituation process by the computer was presented consciously to participants, rather than subliminally. Consistent with the known parameters of the Mere Exposure effect, the precognitive habituation effect was eliminated or seriously degraded.¹⁴

A fortuitous discovery (styled by Bem as a “serendipitous finding”) made during the course of the experiments was a phenomenon Bem refers to as “precognitive boredom.” It was found in an earlier pilot study, and then confirmed in later iterations of his basic PH experiments, that if repeated subliminal exposures to all three categories (negative, positive, and neutral) of images is increased sufficiently, a threshold is reached

¹⁴Though a final, ninth PH replication conducted this time by a skeptical colleague took some lessons learned from Bem’s two preceding supraliminal experiments and in a third supraliminal trial achieved a marginally significant effect ($p = 0.061$) for negative affect target images with 87 participants. (Interestingly, a subset of 32 participants scaled as “emotionally reactive” achieved a highly significant hit rate ($p = 0.006$.)

at which point the hit rate *for neutral targets* also drops significantly below chance ($p = 0.04$). Further tests confirmed this finding. Bem proposed that “the many repeated exposures (precognitively) render the target picture boring, or even aversive, and hence less attractive than its matched non-target” (p.13). The “precognitive boredom” phenomenon is consistent with other literature on the Mere Exposure effect. Because this was an unanticipated finding, it adds weight to the evidentiary value of the PH experiments.

Savva *et al* (2004) attempted a replication of Bem’s PH experiment, substituting less objectionable (but they hoped equally aversive) photos of spiders in place of what they judged as “ethically problematic” negative and erotic images used in the Bem studies. These spider photos, along with a set of neutral photos were then presented to 23 male and 27 female subjects, some of whom were scaled as “spider fearful” and some who were categorized as “no fear.” The results were weaker than those produced by Bem,¹⁵ but still suggestive: the spider-fear group scored at a marginally-significant hit rate of 54% ($p = 0.051$), and showed a differential response between spider and neutral images at a statistically significant rate ($p = 0.021$). On the other hand, the no-fear group had a non-significant hit rate of 49%. “This suggests,” report the authors, “that for the spider-fear group performance on the PH task seems to be significantly different when the stimuli are spider related than when the stimuli are low-affect” (p.226).

¹⁵Perhaps because the valence and arousal factors for the spider stimuli differ in impact from the affective images in Bem’s target pool, or because the smaller number of participants yield less statistical power.

In 2005, Bem reported a new experiment that was a variation of his precognitive habituation (PH) protocol. He called this new protocol “precognitive aversion” (PA) since it capitalized on the “precognitive boredom” effect he had discovered earlier when evaluating the PH experiment results. Two hundred participants were involved, including 140 women and 60 men, each performing 24 trials. While the experiment produced overall non-significant results, the subject-populations most likely to show a response (those who scored low in either an Arousal measure or for Boredom Tolerance) did produce a significant result ($p = 0.006$ overall). Those participants measuring low in Arousal showed significant precognitive aversion ($p = 0.036$), while those measuring low in Boredom Tolerance showed significant precognitive boredom ($p = 0.005$) for positive affect images, which was consistent with experimental expectations.

A couple of years prior to Bem’s precognitive habituation experiments, Radin & May (2001) reported a successful experiment which exploited a well-known human psychological phenomenon known as the Stroop Effect. Stroop (1935) observed that when humans are asked to read a series of words naming specific colors, some printed in the same color as the word names (e.g., the word “red” printed in red ink), and others printed in different colors than the ones they name (“red” printed in blue ink; “green” printed in red), there is a variety of cognitive dissonance created, which slows the rate at which the reader can recognize and read a word written in the wrong color.

The Radin/May experiment explored whether the Stroop Effect might displace backwards in time, creating a sort of cognitive presentiment effect. It was designed along these lines:

Individual participants are presented with a randomly-selected colored patch on a screen, and type the first letter of the word describing the color they saw. The time it takes from being shown the color patch and their typing the letter is measured and called “reaction time 1” or ‘RT1.’ Because there is no dissonance between presented color and response, reaction times (RT1) for each trial of color presentation/appropriate response would be expected to be fairly uniform for each participant.

Immediately after this response, participants are shown a color word. The word is displayed in the color they have just seen, but the word itself might actually name a different color. So, for example, a participant might be shown a red patch, then after a delay shown a word printed in red. That word might indeed be the word ‘red’ printed in red, but the participant could also be randomly shown either of the words ‘yellow,’ ‘green,’ or ‘blue’ printed in red, depending on what the computer decides. The participant then would be expected to type either a Y to indicate ‘yes,’ meaning the word and the color it was printed in matched (in the case of ‘red’ printed in red), or N for ‘no,’ that the word and the color it was in did not match (in the case of either ‘yellow,’ or ‘blue,’ or ‘green’ printed in red).

The time delay between seeing the color/word combination and the typing of either Y or N is measured and designated reaction time 2, or ‘RT2.’ A color-word mismatch condition (i.e., ‘yellow’ written in red) would present the cognitive interference noted in the Stroop effect. According to the Stroop effect, if there is a color/word mismatch, it will take longer for the participant to decide whether to type either Y or N

than if the color/word matched. This means that RT2 for mismatches will be longer than for matches.

The hypothesis in this experiment was that there would be an increase in the RT1 beyond normal expectation when the future color/word combination is a mismatch, and there will *not* be an increase in RT1 when the future color/word combination matches. The notion is that some of the normal cognitive delay caused by the Stroop Effect in color/color word mismatches will retroactively (e.g., displace in time backwards) slow down the reaction times for RT1. (Normal expectations would be that RT1 should not be affected, and thus all RT1 times should remain approximately the same value, affected only by such factors as eventual participant fatigue or distraction.) Radin and May referred to this retro-causative effect as “time reversed interference” (TRI), which would produce change in RT1 times that should correlate with the variations in RT2 times. Thus, “the TRI hypothesis predicts a positive correlation between [the change] in RT1 and [the change] in RT2, under the assumption that the interference that caused the [change in] RT2 difference will leak backwards in time and affect the [change in] RT1 in a similar way” (p.6). They conducted four studies, the results of which were as follows:

Study 1: (Pilot study) one participant, 500 trials, showing a positive statistical trend, but which was not statistically significant ($p = 0.345$).

Study 2: Ten participants, producing 1,852 usable trials. Results were nearly significant ($p = 0.079$).

Study 3: Ten participants produced 111 sessions of 20 trials each, plus 1 session of 30 trials. 1,110 were in the color mismatch condition, and 1,160 in match condition. (47 trials were eliminated for procedural reasons) = 2,223 usable trials.

This study produced a strong correlation, and was statistically significant, yielding $p = 0.005$.

Study 4: Two participants performed 720 trials (366 mismatched, 354 matched), producing a statistically significant correlation ($p = 0.037$).

Combined results of the four experiments were statistically significant ($p = 0.001$), with odds against chance of one out of a thousand.

Given the relative dependability of the presentiment effect as manifest in various external measures, some researchers found it plausible that neural correlates of the effect might be detected using brain imaging or monitoring techniques. Accordingly, a small number of experiments have been done using either fMRI (functional magnetic resonance imaging) or EEG (electroencephalogram)¹⁶ equipment to look for indicators of presentiment in participants' brain functions. As part of McCraty's 2004 experiment (McCraty *et al.*, 2004a,b), study participants were monitored by an EEG during the experimental trials (this was in addition to EDA and ECG monitoring as described above). The EEG measured two dimensions – cortical event-related potentials (ERP) which arise in response to internal or external stimuli, and heartbeat-evoked potentials (HBEP), a measure of brain-related heart activity. Since this was part of the same experiment as in McCraty (2004a), the same two conditions were tested under the general presentiment model (disturbing vs. calm images): the first in a “non-meditative”

¹⁶In an interesting irony, electroencephalography was invented by German psychologist Hans Berger (b. 1873) as a consequence of his life-long interest in parapsychology and telepathy.

condition and the second in a “meditative” condition.¹⁷ For the whole group in Condition 1, McCraty reported a significant difference between calm vs. disturbing photos in the prestimulus period at two associated EEG measurement points, FP1 and FP2, beginning four seconds prior to display of the stimulus photo ($p < 0.05$).

But once again, the male-female differences in results were intriguing. Under Condition 2 (“meditative” condition) the female group demonstrated further ERP markers at two locations (O1 and T5) different from those observed in the non-meditative Condition 1 ($p < 0.05$). But for the males under Condition 1 McCraty found differences in ERP between the calm and disturbing images at three locations: O2, T5, and FP1 ($p < 0.05$). Under Condition 2, indicators were manifest at four sites: T5 ($p < 0.05$), P3 ($p < 0.01$), O1 ($p < 0.05$), and O2 ($p < 0.05$) – all occurring four seconds prior to display of the stimulus (McCraty, 2004b, p.330). McCraty also notes statistically significant differences ($p < 0.01$) in waveforms monitored at O1 and O2. Similar differences between conditions (though generally at different locations) were noted for females in heart-beat evoked potentials, but except for one instance under Condition 2, males showed no significant effect in this measure.

A more recent experiment by Hinterberger, *et al* (2007) also used differences in ERP before display of disturbing or calm images, this time with the further addition of a non-meaningful checkerboard pattern as a more stable baseline stimulus. As a further control, a series of trials were run with the computer display covered in opaque material

¹⁷I place these terms in quotes because the technique employed in condition 2 by the participants is not a classical meditative practice. But such a general categorization

while a randomized series of calm, disturbing, and checkerboard images were presented to participants, to test whether some subliminal sensory leakage could account for experimental results during trials where the screen was not thus masked.

Twenty participants were monitored by an EEG while being shown the typical randomized series of disturbing vs. calm photos (with intermixed checkerboard patterns). Hinterberger reported that “the analysis of all cortical channels merged revealed a significant increase of the EEG activity before presentation of an affective picture ($z = 2.02$, or $p = 0.0217$) compared to the 1000 arbitrary intervals.” No such effect was seen in the screen-covered condition or checkerboard condition. (A further purpose was to see if the meaningless checkerboard pattern elicited the same or less presentiment effect than meaningful calm images.)

A study by Bierman & Scholte (2002) used an fMRI to explore whether a presentiment effect would show up under this sort of brain imaging technology. Ten participants (four female, six male) were involved in the experiment. Each was presented with 48 trials of randomized calm vs. disturbing images while his or her brain was imaged by the MRI. To analyze the results, the males and the females were pooled separately, as the experimenters suspected different types of disturbing stimuli would be processed differently by male and female brains. This turned out to not necessarily be the case. Bierman and Scholte reported that for individual participants “the presentiment effect was widely distributed over many brain regions,” though individual brain regions from which the composite analysis was developed did not themselves “show striking

simplifies the discussion and the differences are not significant for my purposes here.

differences in anticipation before emotional and neutral stimuli” (p.7). The single exception for this was that there was a generally consistent activation pattern across all participants centered in the area located around Talairach coordinates (130, -80, 10).¹⁸ Of additional interest was the fact that females showed a differential reaction to the two types of stimuli (violent/gruesome) not displayed by the males. The males displayed no pre-stimulus response differential between violent and neutral (calm) images, but did for the erotic ones ($p < 0.05$). The females, on the other hand demonstrated a 22 % difference four seconds before display of erotic stimuli ($p < 0.05$) and a difference of 19% before the violent stimuli ($p < 0.05$).

Bierman replicated this experiment (Bierman, 2007) using a set of experienced meditators. Sixteen participants were involved, eight controls and eight meditators (five males and three females). They were first acclimated to the confining and noisy fMRI environment, after which they were tested with the standard randomized disturbing/calm image presentation procedure. This experiment produced remarkably strong evidence of a precognitive effect, resulting in a Z score of 5.6 ($p = 0.000003$). In an interesting twist, it was discovered that the meditators’ reaction to the violent stimuli all but disappeared altogether, but reaction to the erotic stimuli increased, to a very highly statistically significant level ($p < 0.000001$).¹⁹

¹⁸The Talairach coordinate systems allows locations in the brain to be specified independent of individual differences in brain conformation.

¹⁹Bierman urges caution regarding such a strong statistical result, since it is hard to disentangle statistical measures to clearly distinguish non-dependent effects due to the statistically-noisy fMRI environment.

Finally, suspicion developed that, if the presentiment effect, even if subtle, was as persistent across paradigms as results seems to indicate, there might be evidence for it that had gone unnoticed in mainstream research because no one had thought to look for it. Unfortunately, since most mainstream studies are generally meant to explore conscious reactions, they begin autonomic monitoring immediately before presentation of stimuli, and therefore miss any presentiment effect that might be present. However, in a broad literature search Bierman (2000) discovered three mainstream studies that indicated the presence of a presentiment effect. One study tested speed of onset in individuals with animal phobias as compared to control subjects. The second examined differences between risky and non-risky choices in gambling behavior. The third study looked at how emotional priming affected evaluation of Japanese characters. In his report, Bierman reviews each of the three studies, including methodology, participants, analysis, and results (from the standpoint of presentiment). Although only one study reached significance at the 0.05 level, all three showed strong positive trends in the direction supporting a presentiment effect. A combined analysis thus shows evidence for the presentiment effect across the three studies that is highly statistically significant ($Z = 2.748, p = 0.003$).

Critics often allege that to improve their case, parapsychologists may suppress negative data and present only what is successful (this is the crux of the so-called “file drawer” effect, which I will discuss in more detail in chapter 11). Investigation has shown that this accusation is seldom true. But in the interest of openness, I have tried in

my discussion of the presentiment research to discuss both positive and negative findings in the context in which they belong. I will do the same in the following chapters as well. There is a danger in this, as in the eyes of the reader it may seem to undermine the strength of the results. So I will take a moment here to add a little more context. There can be a number of the reasons for apparently failed replications (i.e., attempts to repeat an experiment which yield non-significant results). As I shall discuss in chapters 10 and 11, one common reason for failure to repeat results in the parapsychology experiments is departure from the original, successful protocol. Though sometimes this is due to sloppy work, or misunderstanding by the attempting replicator of some feature of a previously successful recipe, it is often intentionally done, as experimental parameters are tweaked to see which conditions are necessary or sufficient for the effect, or if a different configuration of background conditions and variables might enhance the results. May's use of quiet vs. noxious sounds instead of photos was a successful example of this. It is inevitable that some changes will weaken or even extinguish the effect.

A further issue is the fact that we don't understand the causality – there are distinct gaps in our ability to follow it from initiation to culmination (which fact is, of course, important to my overall argument). For that reason there are times when, for no reason that can be specified the presentiment is not strongly present (as studies that are nearly significant or trend in a positive direction suggest), at least to the degree that an arbitrary $p = 0.05$ level is reached.

What is important to notice here is that if there is no ESP effect present in a given body of this kind of research, one would expect *at most* one study out of 20 to produce

statistical significance at the 0.05 level by chance alone.²⁰ I have thus far identified 23 studies that fit within the presentiment paradigm as discussed here.²¹ Of those, by chance we might expect one and a fraction studies to be significant. However, my evaluation shows that 12 out of 23 produced statistically significant results, and another three produced results which, though non-significant, were in the positive direction.²² This is obviously far more than the one significant experiment expected by chance alone, provocative evidence of the presence of an effect.

It is time to assess how the body of presentiment research stacks up against the criteria expected of a violator of causal closure. The results seem to indicate either a causally-important time-displacement of up to several seconds into the past, or an equally causal access from the present into the future. Either seems to violate not only our normal concepts of cause/effect relationships, but also the standard rules of physics. Could this be evidence of a violation of the causal closure assumption? It seems that if sufficiently well attested, it likely is. Since within the physical domain time and causation only flow forward, retro-causation seems to require a violation of causation,

²⁰Though arguably even one out of 20 may be too many.

²¹This does not count Bierman's discovery of three studies in the mainstream literature which displayed presentiment features, and three very recently-reported presentiment studies, each of which were statistically significant. Results for two studies are unavailable – Norfolk, 1999 & Parkhomtchouk *et al*, 2002.

²²Despite some effort I have not been able to obtain the reports, but have been told in personal conversation with Dean Radin that Norfolk, 1999 and Parkhomtchouk *et al*, 2002 produced statistically significant results as well, though I am unaware of to what magnitude.

strongly implying a closure violation. On the other hand, “precognition” (even if, on the presentiment account, the “cognition” part is absent) implies a jumping ahead and skipping over of intervening parts of the future, presumably *which haven’t happened yet*, which even more strongly suggests that the causal closure principle is being violated.

As will be more fully considered in chapter 8, the rules of scientific inference and evidence lay particular weight on corroboration of experimental findings across varying experimental models, independent replications, varied subject bases, and monitoring techniques. If an effect persists despite changes in approach, experimenter, subject pool, and monitoring method, then confidence grows that it is no artifact, but a real effect.

This discussion of presentiment sets the stage for a consideration of the second form of “unconscious psi” evidence, to which I will now turn.

Chapter 6: Further Evidence: Staring and DMILS

In the preceding chapter I considered one form of unconscious psi, that which is demonstrated in presentiment/pre-stimulus response experiments. Now I shall consider what is broadly termed Direct Mental Interaction With Living Systems, or DMILS.

DIRECT MENTAL INTERACTION WITH LIVING SYSTEMS (DMILS)

Claims that human intentionality can have a distant effect on humans or other living organisms in the absence of any known or reasonably conceivable physical causal linkages have persisted among various cultures for centuries. Recent attempts to rigorously and objectively investigate these supposed phenomena have resulted in a broad literature embodying a range of experimental paradigms. I shall focus on two of these, and consider them generally as one set, as they seem closely related and may, in fact, be merely different manifestations of the same effect. The first of these is variously referred to as “the sense of being stared at,” “unseen gaze,” or “staring detection.” I shall follow Baker’s usage as most descriptive: “Remote Staring Detection” (Baker, 2005). The second approach is often referred to more specifically as “distant intention” or “remote influencing.” For reasons I will explain, I will refer to this paradigm as “direct mental interaction” or DMI. Both can be considered subsets of the experimental category known as Direct Mental Interactions with Living Systems, or DMILS.

Remote Staring Detection

Remote staring detection (I will use interchangeably RSD or “staring,” for short) is a further refinement of a more prosaic experimental procedure that derives from a commonly-reported folk-notion that people can become aware of others’ stares without any (apparent) alert from the physical senses. This is referred to by one of its more prolific advocates, Rupert Sheldrake (2005) by the phrase “the sense of being stared at.” Individuals describe staring events of this sort as a feeling of discomfort either suddenly noticed or of gradual onset, as if one is being stared at, and upon turning to look discovering that someone is indeed observing one intently.

Sheldrake, in particular, has taken this concept and turned it into a sort of folk experiment, and has even published a simple protocol which has been replicated numerous times for school science projects and in other relatively informal research situations. Sheldrake calls the main form for this experiment “direct looking.”¹ A typical direct looking experiment proceeds in the following way: One person (the “subject”) sits in a chair with her back to a second person (the “looker”), who also sits in a chair, but positioned several feet behind and out of sight of the first person. An audio signal (a chime, buzzer, etc.) indicates the start of an episode, either staring or non-staring, selected by a random means. The subject has ten seconds to guess whether she is being looked at or not. Guesses are immediately recorded, followed by a brief rest period until the next episode.

Success is indicated if the ratio of total correct vs. incorrect guesses by the subject

¹Recently a new term, ‘scopesthesia,’ has been coined for the sense of being stared at. (Carpenter, 2005)

is statistically significant. Stare/non-stare periods number approximately 20 in a session, and several sessions with different starers and starees are usually conducted. The experiment is elegantly simple, and Sheldrake (2005) reports “tens of thousands of trials” which have shown a consistent 55% hit rate as opposed to the 50% accorded by chance. The effect size is large, resulting in what he calls an “astronomically significant” statistical effect yielding a p value of 1×10^{-20} (as of 2002, this represented 30,803 “guesses” derived from all 21 of Sheldrake’s own previous experiments, plus 37 experiments consisting of many trials performed independently in various schools and colleges. Of the total trials, 16,849 of the 30,803 were correct when by chance only about 15,400 should have been) (Sheldrake 2005, 14-15).

Impressive as these results are, they can only be taken as suggestive, since a number of evaluators have found the protocol too vulnerable to possible sensory cuing and other artifacts, which may weaken the data significantly (Atkinson, 2005; Burns, 2005; Radin, 2004, Lobach & Bierman, 2004, Colwell, 2000). This does not rule out a real effect (given the robustness of the effect size, even a consistent 51% hit rate rather than the reported 55% would still be highly significant). But at the very least one cannot altogether eliminate the possibility of alternative explanations to the effect being claimed.² However, there is a more tightly-constructed approach to the staring experiment which has generally passed muster for rigor and careful controls. This is the

²Sheldrake recently published a new staring study controlling for most of the artifacts in previous staring studies – 2,800 individual trials, of which 1,477 were hits and 1,323 were misses, yielded a hit rate of 52.8%, $p = 0.002$. A further ‘sign test,’ which gives equal weight to each test, yielded a result significance at $p = 0.0005$. (Sheldrake &

Remote Staring Detection (RSD) protocol, using closed-circuit television cameras.

Typically, the experiment is done under carefully shielded and controlled conditions to preclude cuing, sensory leakage, and other mundane intrusions that could contaminate the data. The person being “stared at” (often called a “subject” in this protocol) is sequestered in a windowless, double-walled, acoustically- and electronically-shielded room, and the person doing the “staring” – often called the “agent” – is also confined in an isolation room some distance away, usually in the same building. A direct-channel closed circuit television camera monitors the face of the subject, which is then displayed to the agent on a television screen in the isolation room. Staring and non-staring episodes are interspersed with rest periods of seconds to minutes duration, and the order of staring/non-staring conditions is fully randomized. In most cases non-staring conditions are facilitated by temporarily interrupting the camera feed to the observer’s display screen by an automated process during both non-staring and rest conditions.

If there is a staring effect, anecdotal reports suggest it begins unconsciously, and eventually comes to conscious awareness if it persists. If this is so, then it seems plausible that the person being stared at would experience a subconscious, autonomic arousal in advance of conscious awareness that should be detectible by standard autonomic monitoring techniques. One measure of autonomic arousal is change in electrical conductivity of the skin, most often referred to in these studies as ‘EDA,’ or electrodermal activity.³ In the classic RSD protocol, the effect is measured by

Smart, 2008).

³EDA was briefly discussed in chapter 5. It may also be referred to as

monitoring changes in EDA. If an effect is present, EDA measures should remain at or near baseline during non-staring and rest conditions, while they should depart from baseline during staring episodes. Statistical analysis then yields the degree of departure that correlates with staring episodes as compared to non-staring conditions.

The first staring experiment using EDA monitoring was performed in the late 1980s by William Braud and his colleagues and first reported at a science conference in 1990 (Braud, *et al*, 1993; 2003). This study was an improvement on a handful of previous staring experiments done by Titchener (1898) and Coover (1913),⁴ both of which produced null results (Titchener published no data), and Poortman (1959), Peterson (1978), and Williams (1983), all three of which did produce significant results.

Braud followed the DMI procedure as outlined above, using 32 participants, 24 of whom were females and 8 were males. Thirty-two remote staring sessions were conducted while EDA of the subjects (“starees”) was monitored. Each session consisted of 10 staring and 10 non-staring sessions presented randomly. This generated 20 episodes per sessions which could be statistically evaluated. However, as a more conservative measure a single session score was generated using the mean of the staring versus non-staring responses. This was then used for statistical analysis.

A further variable was introduced by dividing the experiment into two phases, as

“electrodermal response,” or the sometimes-preferred ‘SCR,’ for “skin-conductance response.” I will continue to use EDA here to preserve continuity with the standard usage in most of the early studies.

⁴Though in Coover’s case there is some controversy. Later analysis showed that at least some of his experiment did in fact produce above-chance results, but he set the threshold for statistical significance arbitrarily high (Radin, 2006; Irwin & Watt, 2007).

follows. Besides determining whether a detectable remote mental interaction effect might be observed, there was also some thought that subjects who had become acclimated to the steady regard from second parties might manifest a regularized EDA effect different from those who were not so accustomed. Consequently, the participants were divided in half. The first half were considered “untrained,” while the second half were put through a 20-hour “connectedness training” program designed to make them more comfortable being under the scrutiny of a second party. Phase 1 tested the 16 “untrained” subjects, while Phase 2 used the 16 subjects who had received connectedness training.

After conclusion of the experiment and analysis of the results, the “untrained” subjects from Phase 1 showed 59.38% mean percent electrodermal activity, rather than the base rate of 50% that would be expected if there were no effect. This is statistically significant at $p = 0.018$, or odds against chance of 1 in 56. For the phase 2 “trained” subjects, results were 45.5% ($p = 0.048$; odds against chance of 1 in 21). The untrained were thus more activated and the trained more calm. An independent-group *t*-test⁵ comparing the two sets directly shows a result yielding $p = 0.002$ (odds of 1 in 500).

Braud decided to use a meta-analysis to analyze his own results together with those of all staring studies for which there were results up to that time. From the perspective of an overall statistical analysis, though, there is a problem – the above-threshold results of the ‘trained’ subjects would tend to cancel out the below threshold

⁵A *t*-test is a statistical test used to determine whether or not the observed difference between two means is statistically significant. This test showed that the

results of the ‘untrained’ subjects when combined for analysis, despite the evidence of a substantial, if opposite effect for each set. Since, depending on the assigned condition, the results were dependably either negative or positive in line with the anticipations that drove the original parsing of the study into Phase 1 and Phase 2, it makes sense to combine the absolute values of the two, rather than the positive and negative. Under these terms, the overall results for all five staring studies from Coover’s up through this first one of Braud’s were significant at $p = 0.000011$, or odds against chance of 1 in 91,000 (Braud, *et al*, 2003, 158-161).

After this first experiment, Braud and his associates performed two more staring experiments, plus a “sham” experiment which was conducted exactly as the others but where no staring actually occurred. This sham experiment was meant to serve as a control, to ensure that the results produced by the other staring EDA experiments were not merely some undiscovered artifact of the monitoring equipment or the experimental protocol itself. Results for Braud’s “Replication 1” experiment (30 subjects, producing 30 sessions of 20 staring/non-staring episodes each) were nearly significant at $p = 0.06$, while Replication 2 (16 participants, 16 staring/non-staring episodes each) was significant at $p = 0.05$. The “sham” experiment (which was a sub-portion of Replication 2) involved 16 sessions consisting of 8 non-staring episodes intermixed with 8 pseudo-staring episodes (in which everything was maintained as in the staring portions of the experiment, with the exception that the monitor was off and the agent/starer focused mentally and visually on something other than the subject). This produced a total of 256

differences were not attributable to accident or bias.

non-staring vs. pseudo-staring episodes, which yielded non-significant results of $p = 0.76$ (1 chance in 1.3), in the range of what was anticipated. (Braud, 2003, 174)

Not long after Braud published these results, Schlitz & LaBerge (1997) conducted their own replication. The study employed six starers (three males, three females) and a total of 39 subjects (19 males, 20 females). They performed 48 EDA-monitored staring detection sessions, each containing 32 episodes divided evenly between staring and non-staring, and presented in randomized order. Overall results were statistically significant at $p < 0.005$. Of the subjects, 26 (66.7%) had greater skin-conductance levels during staring episodes than during non-staring, while 13 (33%) showed lower EDA measures during staring trials than during non-staring episodes (p.191).

As Schlitz and LeBerge report (p.192), “This research provides an independent conceptual replication of the covert-observation experiments conducted by Braud, *et al*, under conditions that ruled out conventional sensory exchange between experimental participants. The work builds upon an increasing database suggesting that people are able to interact with one another at nonsensory levels, including the mental influence of one person upon another person’s physiology.”

Wiseman & Schlitz (1997) collaborated on a rather unique RSD study. The study involved two procedurally identical experiments performed concurrently in the same lab, with the same equipment, and with participants drawn from the same pool to determine whether there is a so-called experimenter effect. In the ‘experimenter’ effect, participants managed by a researcher skeptical of ESP effects often produce only chance results, while participants managed by an experimenter who believes in the reality of the

phenomenon produce results indicative of a psi effect, even when all other experimental conditions are identical. Wiseman is a well-known skeptic of ESP who had carried out four earlier staring studies, only the first of which demonstrated a significant result (but which was later attributed to a statistical artifact). Schlitz, on the other hand, was known for her extensive contribution to successful ESP experiments in the past.

Their joint study, conducted in Wiseman's lab in England, used EDA to monitor for a possible effect, and the staring was conducted via closed-circuit television. Thirty-two subjects participated, each performing one session containing 16 staring vs. 16 non-staring episodes (each 30 seconds long) according to a randomized pattern. The experimenters themselves acted as the observer/starers for their respective experiments.

Results were strongly indicative of an experimenter effect: Wiseman's participant group produced a non-significant effect ($p = 0.64$). Schlitz's group, however, produced results significant at $p = 0.04$. Both experimenters were able to mutually discount any differences in protocol, as they interacted throughout the series, and there were multiple structural and other safeguards to prevent cheating. They concluded that the most likely explanation was that the 'skeptic' had perhaps negatively influenced his participants, causing them to doubt their ability to be successful in the task, whereas the 'believer' was able to encourage her group of participants to be successful.⁶

⁶Other studies (Roe & Davey, 2006; Watt & Baker, 2002; Watt & Brady, 2002) have subsequently been done to explore the experimenter effect phenomenon, providing further suggestive evidence such an effect exists. In the Wiseman/Schlitz experiment, a further, seemingly less likely, possibility for the split outcome was suggested, that the experimenters either unconsciously applied or didn't apply their own psi abilities to influence participants' performance. However, nothing arcane or esoteric need be

The two researchers followed up this experiment with another one (Wiseman & Schlitz, 1999) where both researchers used identical procedures, but this time the work was done in Schlitz's lab in the United States. Results were very similar to the previous experiment, Wiseman's being non-significant at $p = 0.69$ (vs. $p = 0.64$ in the previous study) and Schlitz's results significant at $p = 0.05$ (vs. $p = 0.04$).

A later analysis by Schmidt, *et al* (2004) found 15 EDA-based staring studies that had been performed by a variety of researchers in a number of laboratories in three countries. A meta-analysis showed that the results overall were significant at $p = 0.01$ (or odds against chance of 1 in 100). While more than exceeding standard requirements for statistical significance, for any sort of consistent effect this is a relatively weak showing for combined results across 15 studies.

Further problems arose for the staring paradigm due to a report by Schmidt & Wallach (2000) concerning less-than-optimal application of EDA measurement techniques in some of the staring and remote mental interaction studies (I address these problems in more detail later in this chapter). A further attempt at replication by Schmidt and others in a staring-type experiment using EDA measures failed to show an effect (though results did trend in the positive direction) (Müller, *et al*, 2006).

However, RSD experiments have been conducted in other ways, and they tend to corroborate positive findings from many of the EDA-measured RSD experiments. For

proposed to explain it. There are suggestive reports that experimenter demeanor, rhetorical characteristics, etc. may have respectively encouraged or discouraged participants, and either enhanced or diminished participants' confidence in their ability to succeed at a psi task (Sheldrake, 2005).

example, Radin (2005) reports a meta-analysis of 60 standard (non-EDA) staring studies (including Sheldrake, Poortman, Radin, Lobach & Bierman, Colwell, and Coover) of the type requiring the subject to consciously determine and report if he or she has the feeling of being stared at during staring vs. non-staring episodes in an experiment. Most of these were the “close-proximity” types of studies popularized by Sheldrake which, as mentioned above, have been called into question. Radin’s meta-analysis indeed found problems with the majority of these studies which cast additional questionable light on their results.

However, a subset containing 10 of these studies were performed differently. In these studies, the observer/starer was separated from the subject/staree usually by yards rather than mere feet (as is usually the case in the Sheldrake model) and sat in a separate room behind either a window or a one-way mirror, significantly lessening the possibility of inadvertent sensory-cuing,⁷ implicit-learning effects that the close-proximity design was believed to allow. Radin found that these 10 best-controlled studies, using the through-the-window model, yielded results significant at $p = 4.8 \times 10^{-17}$.

An even better-controlled study (Radin, 2004b) was done using electroencephalographic (EEG) measures of event-related potentials (ERP) in subjects’ brains. The experiment, involving 26 participants, comprising 11 pairs of adult friends and two mother-daughter pairs tested two hypotheses: First, that the brain of a subject/staree would show event-related potentials that correlated with those episodes

⁷Unintentional conveying of information to the subject through other than ESP means, such as non-verbal actions, sounds, etc.

when the respective observer/starer would view a live closed-circuit television feed of the subject/staree. And the second was that the magnitudes of the subjects' ERPs would also be strongly correlated with the magnitude of the ERPs the observer experienced as he or she viewed the live image of the subject.

The results showed that for the first hypothesis, the correlation between the ERPs of the subjects with the display of their images to the observer/starers was highly significant at $p = 0.0005$. Results for the second hypothesis showed that the magnitude of ERPs for both observers and subjects was also significant, at $p = 0.0008$. A further analysis demonstrated that the trend was relatively consistent across the observer-subject pairs and not attributable to one or two exceptional performers.

As mentioned before, the larger DMILS remote mental interaction model includes two subsets – staring and direct mental interaction, or DMI. The two approaches seem to involve merely different modes of the same overall effect.⁸ In the direct mental interaction (DMI) paradigm, the agent⁹ does not “stare” at the receiver, but rather directs attention towards him or her without the aid of a visual image or direct observation. But one could think of ‘staring’ in similar terms. Whenever the agent is regarding the receiver visually, the agent’s attention is also focused there as well. While there does seem to be an interesting effect present with the staring experiments, they are less

⁸Stevens (2000, p.406) tends to agree this is likely as well.

⁹“Agent” and “receiver” are terms often employed in these experiments. These roles may also be signified by “sender” and “receiver,” and other labels. For this set of experiments I will generally use the agent/receiver locution where human subjects are concerned.

persuasive than if we consider them in terms of a larger body of attentional sorts of experiments. I turn to those now.

Direct Mental Interaction

The second DMILS experimental paradigm I shall discuss has been variously labeled “Distant Mental Influence,” “Distant Intentionality,” “Remote Mental Influence,” or “Direct Mental Influence.” For reasons suggested in Braud (2003), I shall opt for Direct Mental Interaction (DMI). “Remote” or “distant” suggests that “targets” (receivers) are spatially distant. Yet that is not always the case in these studies. Receivers may instead be blocked by shielding or intervening obstacles, in addition to or instead of distance. Further, “influence” seems to suggest an active, one-way intervention from ‘agent’ to ‘receiver.’ However, the actual causative factors are somewhat ambiguous. Resulting effects often seem equally explainable in terms of passive acquisition of information by the receiver, as much as by some sort of influence employed consciously or unconsciously by the agent. They are thus nearly as likely to be an ESP effect as one due to some form of influence (Delanoy, 2001, p.34; Braud, 2003, pp. xxvii). Along with Braud, then, I will adopt the notion of “interaction” as a more neutral term.

A typical DMI protocol is set up in the following way (Delanoy, 2001; Braud, 2003; Radin, 1997, 2006, Schmidt *et al*, 2004): As in RSD experiments, the receiver is typically enclosed in a shielded room, and is monitored for EDA changes (in some DMI experiments, the agent is also similarly monitored). The agent is also isolated in a

separate room. In many DMI experiments, the agent is asked to direct his or her mental activity towards an attempt to initiate either a calming tendency or an active tendency in the receiver's autonomic system, according to a randomized schedule. A 40-episode DMILS session would typically be divided between 10 activating and 10 calming periods, interspersed between 20 rest periods. The agent attempts activation episodes by using internal imagery, directed thought, or any other strategy that seems appropriate for conveying influences to the receiver's system. In many of these experiments, the agent is presented a real-time trace of the autonomic system being monitored, to encourage a more focused approach and give immediate feedback of progress. The receiver, on the other hand, is always blind to all such information. Again, the object is to look for deviations from the receiver's autonomic baseline that correlate with purposeful attempts by the agent to mentally affect active versus calm episodes.

There are variations on this model. For example, researchers have applied versions of the DMI protocol to non-human receiver populations. Some experiments have explored human influence on swimming patterns of electric knife fish (a variety of fish native to murky Amazon River waters). Others have demonstrated changes in behavior of Mongolian gerbils correlated with intentionally-directed human mentation (Braud, 2003).

One remarkable study changed the paradigm even more radically by employing a small robot whose movements were controlled by a random event generator. A group of newly-hatched chicks were imprinted on the robot, then confined in a pen next to the area within which the robot performed a random walk. Over time the robot's random

movements became coherent, focusing on the side of the enclosure closest to the chicks' location. I will consider a number of these experiments in more detail later in this chapter.

But there have also been more conventional studies using other measures of effect besides changes in EDA. Some of these have monitored other physiological measures, such as muscle tension, tremor, and unconscious muscle movement; blood pressure, heart and breathing rhythms. (Braud 2003). More recently, DMILS experiments have involved EEG and fMRI monitoring.¹⁰ And there are DMILS protocols that involve no monitoring apparatus at all, relying on conscious reports of perceived helping influences and increased concentration on a focusing task.

EDA-Monitored DMILS Studies

William Braud and his associates performed the first EDA-measured remote mental interaction experiment. During the 1980s Braud conducted 13 DMI experiments. Six of these experiments or 40%, produced statistically significant results,¹¹ where only 5% (less than one experiment) should have been significant by chance alone (Braud, 1989/2003). Overall results for these 13 studies were significant at $p = 0.000023$.

Additional series of these experiments were continued in Braud's lab until a total of 37 had been performed, including 19 involving EDA measures – with the balance

¹⁰E.g., Wackermann, *et al*, 2003; Kittenis, *et al*, 2004; Standish, *et al*, 2004; Richards, *et al*, 2005; Achterberg, *et al*, 2005).

¹¹Of these, the lowest significance was $p = 0.043$, and the highest was $p = 0.0065$.

using other means of measurement. Altogether, the work involved 449 receivers, 153 agent/senders, and 13 different experimenters, producing a total of 655 sessions, each involving multiple individual trials (Delanoy, 2001). Braud and Schlitz (1991) calculated the results to be highly significant at $p = 2.58 \times 10^{-14}$. Of these 37 experiments, 57% (or 21 experiments) were independently statistically significant, where again by chance one would expect only 5% being significant – or slightly less than two of them.

An experiment conducted by Sah and Delanoy in 1994 (Delanoy, 2001) tested whether unconscious, autonomic responses of a DMI effect were more reliable than conscious report. The study involved 32 sessions, each of which was comprised of 16 activating episodes randomly dispersed among 16 calming episodes. For half of each session (hence 16 activate/calm episodes), the receiver was also asked to guess whether the preceding episode had been an activation or a calming one. At the conclusion of the study, correlations between autonomic arousal as measured by EDA and the randomly-selected calming vs. activating episodes showed an effect significant at $p = 0.043$. The episodes involving conscious response were non-significant.

In a two-part DMI experiment in Germany also described in Delanoy (2001), the first part of the experiment involved training sessions for novice experimenters, who traded off duties as their own agents and receivers. This half of the experiment produced slightly below-chance results. However, when these experimenters worked with their own untrained subjects in a more typical DMI environment, results once again neared significance ($p = 0.08$, or odds against chance of 1 in 13).

Stevens (2000) preformed a more in-depth evaluation of the physiological

measures behind two DMILS studies, one reported by Watt, *et al* in 1999, and the other by Delanoy *et al*, also in 1999. The Watt study involved two experiments, and as an added feature attempted to determine if distant mental interaction could be intentionally blocked by the receiver. The first experiment produced non-significant results – in their view because there were fewer participants and hence less statistical power – and neither supported the possibility of blocking.¹² However, the second experiment, with a larger number of receivers, was significant at $p = 0.038$.

The Delanoy study (80 participants), explored whether related agent/receiver pairs would be more or less successful than agent/receivers pairs that were strangers to each other. Results indicated that, contrary to the experimenters' expectations, the unrelated pairings showed the stronger effect.¹³

Stevens reevaluated data from both studies, and examined all three conditions present in each – activate, calm, and rest periods. What he discovered was that the difference in effect between the activate and calm periods was relatively minor – and much less than that between calm/activate (“influence”) periods and rest periods. Most DMILS EDA studies assume a “sensory-like” effect on autonomic measures, and focus only on relative difference between calm vs. activate periods (a difference which the two studies being evaluated found to be relatively marginal), ignoring the differential between either and the rest condition. Stevens reevaluated the data not just between calm/activate

¹²In fact, receivers' attempts to block interaction seemed to slightly *enhance* the effect.

¹³The overall results in this experiment, however, were in the positive direction but fell short of the statistical significance threshold.

episodes, but also between both of those and the rest periods which were interspersed between.

He discovered that it was the difference in EDA between the rest condition and *both* the calm and activate conditions that seemed to be the most significant measure. The reason for this, he concluded, was that for both calming and activating conditions the agent was trying to influence the receiver to *do* something, whether that was to be calmed or autonomically aroused. It made sense, then, that the receiver experienced both sorts of attempts as merely differing activation effects. Results for both studies turned out to be highly statistically significant when evaluated according to Stevens' analysis.

Schneider, *et al* (2001) reported a further study, which tested whether receivers would experience significant changes in EDA when a sender/agent participated than when there was no agent, even when the receiver believed there was. Seventy participants performed one multi-episode session each. Half of each session was performed under the with-sender condition, and half under the without-sender condition. Which half constituted which condition was assigned randomly, and the receivers were kept unwitting as to whether a sender was involved. The experiment's results showed a highly significant difference between the sender/no-sender conditions as measured by EDA ($p = 0.005$).

Criticisms of the EDA-monitoring Protocol

In general, the DMI methodology and controls are of sufficient quality that they have withstood most critical objections (Delanoy, 2001). But they have not been

completely immune. One of the most serious criticisms was presented by Schmidt & Walach (2000), who analyzed the state-of-the-art psychobiology measurement standards published in 1981 (and current at the time of their analysis), then compared them to EDA measurement techniques employed in the DMI and RSD experiments performed during the previous 20 years. They found that the published DMI reports either failed to describe the EDA monitoring methods with sufficient detail to judge how closely they complied with the standards, or their descriptions revealed various inconsistencies and departures from standard procedures.¹⁴ Schmidt & Walach concluded that the errors made it difficult to rule out two possible (and opposing) consequences for the research: either a spurious positive effect might be demonstrated or real effects might not be detected. These findings were further examined and elaborated on in an extensive series of statistical analyses and a pilot study using the proposed new standards in Schmidt *et al* (2001).¹⁵

This finding raised questions about the results of most of the EDA-based DMI research – not that they were discredited, but precisely *how* robust the growing evidence was for the effect was now uncertain. However, a number of points should be kept in mind. The Schmidt & Walach findings did not necessarily mean that all results of the experiments were faulty, only that the possibility existed that they were. Irwin & Watt (2007, 17) note that, though they found the report worrisome, discovery of non-standard

¹⁴Although it should be noted that several of the studies predated introduction of the standards.

¹⁵Though the pilot study nonetheless produced highly significant results, somewhat unexpectedly.

methodology in a study does not in itself necessarily account for the effect that was demonstrated in the research. Indeed, as the authors acknowledge in Schmidt *et al* (2001), “the question of which [measurement] component would be more appropriate – or, in statistical terms, which would be more powerful for detecting this effect – remains unanswered.”

Still, the Schmidt/Walach study *seemed* to render the EDA subset of data less trustworthy. The DMI researchers could respond with a couple of important rebuttals. First, control experiments conducted at the same time as some of the DMI research using the disputed EDA measurement methods (for example, the ‘sham’ experiment reported in Braud, 2003, and the conceptually similar work done in Schneider, *et al* 2001) produced null results and no false-positive effects, just as the model predicted. Yet the “live” EDA-measured DMILS experiments done in conjunction showed statistically significant effects, some robustly so. Thus, if the DMI experiments showed positive results, while the control experiments did not, this was evidence that failure to comply with the presumed standards was not as much of a problem as Schmidt & Walach suggested.¹⁶

But if Schmidt and Walach were right about the standards for EDA measurement, what could account for the clear difference between the real condition and the control condition in these experiments? The DMI protocol itself may have worked to filter out the “noise” attributable to less-than-optimal applications of EDA measurement procedures. Recall that the effect was determined by the *difference* between the EDA

¹⁶Interesting to note, the standards being urged by Schmidt and Walach were proposed by the laboratory with which they were affiliated.

measures for the ‘activate’ episodes when compared to ‘non-activate’ EDA base rates. But both conditions were being monitored by the same methods. Even if these methods turned out to follow non-standardized procedures which *may* produce a confounding effect on measurements, it would have affected both activate and non-activate measurements equally. Since it is the *difference* between the two that registers the results, it would seem that the effect would still come out in the statistical analysis, even if the measuring methods were flawed, *so long as* the same methods were employed for both.

To test whether an effect was still detectable in the data from the earlier experiments when steps were taken to adjust for the flaws, Schmidt *et al* (2004) reanalyzed 36 DMILS and 15 staring experiments that relied on EDA measures to detect an effect. The studies being evaluated were weighted for quality according to various measures, including the EDA measurement issues reported earlier in Schmidt & Walach (2000), eliminating or discounting data that might be affected by a non-standard approach. Schmidt, *et al* reported that “higher *overall study quality* seemed to be related to lower *effect sizes*” (p.241). Yet even with data eliminated or weakened through the weighting and error correction process, the 36 DMI studies evaluated still showed “a small significant effect size,” with a p value of = 0.001. The 15 staring studies retained a more robust effect size but yielded somewhat less significant results at p = 0.01. Schmidt *et al* concluded that when evaluated according to weighting deemed sufficient to adjust for the standards specified in Schmidt & Walach (2000) and Schmidt *et al* (2004), the body of EDA-based research did not reliably demonstrate as strong an effect as originally

believed. Yet the authors also conceded that there was still evidence for an effect present, and additional, more rigorous studies were warranted.

Non-EDA-Monitored Studies

A different set of non-EDA experiments support the results of the EDA-measured DMILS experiments. One set of these involves a focused-attention approach which for its measure employs an indirect form of conscious self-report from receivers, rather than any special autonomic monitoring techniques and equipment. Other experiments involve human mental interaction aimed at modifying the behavior of living non-human biological systems. And there is even direct mental interaction between biological systems (both humans and animals) with machines. I will discuss these in somewhat more detail shortly, but first I must discuss the rationale that underlies these experiments and to a degree all the DMILS/Staring research.

The virtue of using living systems (and some mechanical or electronic systems driven by random event generators) in DMILS studies is their high degree of *lability*, or systemic changeableness. “Lability,” as Delanoy explains it, “refers to the target system’s intrinsic variability or its ability to change, and thus its ability to respond to external influences” (2001, 34). According to Braud, “[s]ince living systems possess a great deal of lability or free variability, they would seem to be excellent candidates for sensitive and effective detectors of distant mental influence” (2003, 127). The reasoning is thus: If any system in the world is susceptible to subtle mental influences remotely, it stands to reason that living systems (and certain randomly-driven non-living systems) would be the most likely prospects because their internal controlling systems are so

highly malleable. Accordingly, some of the DMILS research went beyond the format typical of EDA-measured studies. I cite several of them below (Braud, 2003, 86-98):

Ideomotor reactions. Three experiments of 10 sessions each were conducted with properly isolated agents and receivers to try to influence subconscious muscle motion (the so-called “ideomotor response”) involving movements of a freely-swinging pendulum held between a receiver’s fingers. Agents attempted to influence receivers to perform either circular or linear pendulum movements according to a randomized schedule. The scoring was done double-blind by the receivers themselves. Results from experiment 1 were significant at $p = 0.0158$. Experiment 2 was significant at $p = 0.000011$. Experiment 3 was non significant at $p = 0.891$.

Blood pressure. Two DMI experiments were performed that monitored blood pressure as an alternative to EDA as an indicator of autonomic activation. There were eight 2-minute epochs per session, measured automatically by electrospphygmomanometer (a blood pressure measuring device). Experiment 1 (which used only one receiver) was significant at $p = 0.025$. The second experiment, which involved 40 sessions, was non significant, but in the positive direction at $p = 0.23$ (odds against chance of 1 in 4).

Spatial orientation of freely swimming fish. This unique experimental design involved four experiments with ten sessions each, attempting to alter the swimming pattern of an electric knife fish. These fish emit a weak electrical body-field that is presumed to guide them in their murky Amazon environment. Electrodes were placed around the sides of the tank to detect changes in the fish’s orientation, with the goal being to influence the fish to adopt a perpendicular orientation with reference to electrodes at the ends of the tank. Experiment 1 was significant at $p = 0.00492$, experiment 2 at $p = 0.00433$; experiment 3 at $p = 0.035$; but experiment 4 was non-significant though (again) slightly in the positive direction at $p = 0.314$ (odds against chance of 1 in 3.2).

Activity rate of a small mammal. In another study involving an animal target rather than a human, in four experiments (ten sessions each) a human tried to influence a gerbil to run more vigorously on an exercise wheel during influence periods as opposed to during non-influence periods. Chart recorder pen deflections caused by rotations of the wheel were blind scored. In this case, experiment 1 was marginally non-significant at $p = 0.0839$ (odds against chance of 1 in 12); experiment 2 was significant at $p = 0.035$; experiment 3 at $p = 0.0224$; and experiment 4 at $p = 0.00894$.

***In vitro* cellular preparation (hemolysis).** In a fairly complex set of three experiments, an agent attempted to “protect” as many of a colony of red blood cells placed in an inhospitable saline environment as possible. Hemolysis, or the process of disruption of cell membranes and destruction of the cell can be detected by light transmission through the mixture of cells and saline solution – the more advanced the level of hemolysis, the more transparent the mixture. Using light transmission as measured by a spectrograph as the indicator, the three experiments produced the following results: Experiment 1 (10 sessions) was significant at $p = 0.0000056$; experiment 2 (32 sessions) was clearly non-significant at $p = 0.89$; and experiment 3 (32 sessions) was again significant at $p = 0.000019$.¹⁷

Facilitation-of-Attention Studies

An alternative approach to EDA-measured DMILS can be termed “attentional” or “focusing” studies. There are a number of variations, but in general agents serve as “helpers” to assist receivers (often termed “helpees”) concentrate or focus on a simple task of extended duration. Typically, agents and receivers are isolated in separate rooms, usually some distance apart so there is no possibility of sounds or other sensory cues being communicated between the two. A series of randomly-varied help vs. no-help episodes are generated and presented to the agent, usually by computer. During help episodes, the agent mentally directs encouraging thoughts toward the receiver, forms visual imagery of the receiver concentrating, or employs other similar mental strategies that will hopefully remotely assist the receiver in concentrating on the task.

The receiver, on the other hand, is unaware of the sequence of help vs. no-help episodes, and merely tries to focus on the task throughout the trial period (usually 20 to

¹⁷However, in Palmer (2007) a possible artifact in this experiment was reported which, if valid, would decrease the significance of the results, indicating “a genuine directional effect in Braud’s data, but only suggestively” (151).

30 minutes). He or she is directed to indicate whenever his or her attention wanders from the task, usually by pushing a button held in one hand, which then is recorded automatically in a computer. Results are indicated by whether the number of button presses during helping periods is significantly lower than during no-help sessions, indicating that the remote “help” has indeed improved concentration.

The first of such experiments was again performed by Braud and three of his colleagues (1995). In this experiment, a “helper” agent sequestered in an isolation room attempted to assist the receiver (“helpee”), also isolated in a separate room some distance away but in the same building, to concentrate for an extended period in a meditative way on a flickering candle on a table a few feet away. The receiver was to press a button whenever his or her mind wandered from the task. During each 20-minute experiment period there were 16 episodes divided randomly between 8 helping and 8 control (no help) periods. Each period was announced to the agent by a notice on a computer screen in the agent’s isolation room, and a low tone played through headsets worn only by the agent. During helping periods the agent concentrated on a candle in his or her own room and directed encouraging thoughts about concentration toward the receiver. During the control periods the agent/helper was to avoid attending to the candle and to think about other things than the person being helped.

Altogether, this first experiment involved 60 subjects, each providing a session (thus 60 episodes of 16 each), and three “remote helpers,” each of whom worked with 20 subjects. At the conclusion of each session, the receiver was asked to complete a number of questionnaires and psychological assessments. The difference between the tally of

button presses during help vs. no-help episodes would demonstrate any effect that might be present. As Braud put it, “The primary experimental hypothesis was that the participants’ distraction scores (button presses) would differ for Control (baseline) *versus* remote Helping periods.” An analysis was performed to certify that the results of all three agents were statistically consistent, then all responses were pooled.

The results showed the mean number of distractions per 16-trial session to be 13.60 for the control condition and 12.43 for the helping condition, which averages to 1.70 distractions vs. 1.55 distractions in each of the 16 control vs. helping episodes in each session. This turned out to be significant at $p = 0.049$. Of further interest was the comparison of experimental results to the patterns demonstrated in the participants’ responses to the psychological and personality assessment questionnaires each had completed at the end of his or her session. Of the 10 psychological/personality dimensions that were measured, five were independently significantly correlated with the experimental results.

One particularly relevant factor that strongly correlated was “need-relatedness” – those 19 participants who were measured as “more needy” by the psychological instruments (in other words, had a harder time concentrating or focusing on tasks in everyday life, or had stronger need for support and assistance from others) were more successful at concentrating during the ‘help’ episodes to a statistically-significant degree ($p = 0.01$; odds against chance = 1 in 100). On the other end of the scale, the 19 participants judged less needy by psychological assessment scored non-significantly just below chance. In this case, “needy” receivers were responsible for the entire effect.

These findings have some interesting (and confirmatory) implications for the DMILS research. If there is a real effect, one could plausibly expect participants with a higher level of neediness to respond with more sensitivity to the help of others, while those with much lower levels of neediness would also show a much lower effect, and this, of course, is what was demonstrated. However, “more-needy” and “less-needy” participants were not identified until after the experiment had been run and responses collected, so the fact that results correlated not only with helping sessions, but also with ‘neediness’ measures adds a further degree of confirmation to the evidence of an effect.

This experiment was replicated by Brady & Morris in 1997. Using the same research design, they produced a fairly large effect size ($r = .27$) with results significant at $p < 0.05$.

Not all attention-facilitation DMILS studies have produced significant results. Two experiments conducted by Braud and associates, and reported in Braud (2003) deviated from the original DMI model to explore whether receivers who were attempting to guide a metal stylus through a small hole in a metal plate could be assisted by distant “helpers.” If the stylus contacted the edge of the hole, an electrical circuit would be completed and a buzzer would sound, indicating a “miss,” which would also be recorded. Agent/helpers were to remotely assist with the task as directed by a randomized schedule. According to the stated hypothesis, there would be fewer “misses” under the helping condition. This study, however, produced non-significant results – the first study, with 10 sessions yielding a p value equal to 0.81 (odds against chance of 1 in 1.2) and the second with nine sessions yielding results at $p = 0.48$ (odds of 1 in 2). This outcome is

not surprising, given the physically more demanding task, and the introduction of a task the tolerances of which are considerably more strict than in other concentration-focusing approaches. There is therefore some question *a priori* whether the task tested here is optimal for demonstrating a distant “helping” effect.

Parapsychologist Caroline Watt, along with various co-researchers, performed four experiments, the primary goal of which was to explore different possible causal aspects of the so-called “experimenter effect” (the apparent tendency of some researchers to consistently get positive results in parapsychology experiments, while others consistently fail). All four experiments involved remote facilitation of attention DMILS studies as the ESP test.

Watt, together with Claire Brady (Watt & Brady, 2002) performed two experiments, each involving 60 participants, 30 of whom were assigned to a group with positive expectations of the experimenter’s success in ESP experiments, and 30 to a negative expectation condition. This was accomplished by having each of the members in one 30-participant positive-attitude group read a fictitious article about Dr. Brady’s consistent success in psi experiments, and each of the 30 members of the negative-attitude group read an article about her consistently failing to achieve above-chance results in her research. The groups would then be split up into 15 pairs each, and the attention facilitation trials undertaken as usual.

Unfortunately, a computer error rendered all the data from this first experiment unusable. The second experiment was a nearly-identical replication of the one just described. Results from the attention-facilitation portion of this study were non-

significant, and in a slightly negative direction. (There were 12.58 mean button presses in the help condition, and 12.20 under the control (no-help) condition.¹⁸)

A third such study (Watt & Baker, 2002) involved 40 pairs of friends who participated alternately as agents and receivers. The agents “helped” the receivers to concentrate on a burning candle for one session, and then they traded roles for a second session. According to a randomized schedule, the experimenter made either supportive or non-supportive comments about psi and the prospects for success of the experiment to each pair of participants as part of a pre-test orientation period, which required them to fill out various assessment questionnaires. The performance of the remote attention-facilitation task followed. The results showed that, overall, the mean number of button presses during the help-condition were 10.35, compared to 10.76 under the control condition ($p = 0.151$, or chance of 1 in 7) – This was again non-significant, but in the positive direction. (No experimenter effect one way or the other was detected in this experiment.)

The final experiment in the series (Watt & Ramakers, 2003), explored a different facet of the experimenter effect, again using a remote attention study. The experimenters (who also worked as agent/helpers) were comprised of two groups. One group were actual believers in the reality of psi, while another group were actual disbelievers. Two groups of participants were distributed between them. The experimental design was, again, the same as Braud’s (1995) and Brady’s (1997),

¹⁸No p-value was calculated, because the trend was in the opposite direction from what was predicted, making the one-tailed binomial calculation required by the design

involving an agent “helping” a receiver focus on a burning candle, with scores being measured by button-presses when the receiver’s attention strayed. Altogether, 36 subjects participated, with nine believer experimenter/helpers and five disbelievers. At the conclusion of the experiment, mean overall help presses were 12.03, while no-help presses were 13.47, a statistically-significant result ($p = 0.02$), showing “significantly fewer distractions during the epochs when they were being remotely helped compared with the control epochs” (p.109).

The believer vs. disbeliever scores were somewhat revelatory. Receivers working with believing helpers showed 12.25 presses under the helping condition, compared to 14.54 for the control condition, which was significant at $p = 0.005$. On the other hand, those who worked with the disbeliever helpers scored 11.58 to 11.33 – nearly a chance result (though even here slightly in the positive direction, at $p = 0.415$, or one chance in 2.4).

Of the attention-facilitation DMILS studies I have described, three of the six that were completed produced significant results, two of the others were not statistically significant but showed effects in the positive direction, and only one was negative, though only slightly so (the failed study showed strongly positive results, but it is impossible to know how much of that could be attributed to the artifact caused by the computer scoring error).

Two further attention-facilitation DMILS studies continued to explore the general candle-focusing/button press experimental design, but changed the context from England

not possible.

and the US where most of the previous work had been done to Bali. In the first Balinese study, 40 subjects performed a total of 35 usable sessions, each consisting of 16 one-minute segments randomly divided between eight helping and eight non-helping episodes. The experimenters hoped to shed light on three hypotheses. Hypothesis 1 was that button-press averages would be lower during helping periods and higher during non-helping periods (the usual standard with this experimental paradigm). This hypothesis was supported significantly at $p < 0.02$.

Hypothesis 2 explored two conditions – that needy helpees (assessed by questionnaire) would show a stronger psi effect, and that needy helpers would be less helpful agents. As in the Braud experiment, needy receiver/helpees did in fact show a stronger effect (supported at $p < 0.01$). However, they were not shown to be less helpful when performing as agents ($p < 0.53$).

The third hypothesis was that meditation training would have an effect on agent/receiver results (half of the participants had received preliminary meditation training to test this condition). This was not supported in the case of the receivers/helpees (meditators did not perform any better as receivers/helpees than non-meditators), but was nearly significant for agents/helpers who had meditation experience, at $p = 0.058$ (odds against chance of 1 in 17).

The second Balinese experiment was a conceptual replication of the first. Though the same candle-focus, button press model was followed, the agent/helpers were specifically selected for being meditation-trained, though a mix of regular meditators vs. infrequent meditators was intentionally selected to test this condition in remote attention-

facilitation. The receiver/helpes were also specifically selected to rate high on the “neediness” scale, as explored in earlier similar experiments. A total of 15 agents and 15 receivers participated. Results again supported the presence of an attention-facilitation effect, significant at $p < 0.02$.¹⁹

Physiological, Electroencephalographic, and fMRI Measures

Other independent support for the DMILS EDA results comes from alternative means of autonomic and nervous-system monitoring. Schmidt *et al* (2001) questioned whether the DMI effect on autonomic functioning was a global one, broadly impacting a spectrum of autonomic measures, or whether instead it was restricted to just EDA. Since EDA had shown fairly consistent indications of the DMI effect, and since change in respiration has been found to influence changes in EDA (p.67), Schmidt and his colleagues decided to monitor both EDA and respiration in an experiment.²⁰ From a total of 26 sessions of 20 epochs each (10 calming, 10 activating, randomly ordered with 15 second rest periods between), Schmidt was able to establish a robust effect (p values ranging from 0.038 to 0.017, depending on the statistical method used²¹) from both EDA

¹⁹The fact that this result is similar to that of the previous study suggests that in this experiment at least, either meditation experience or “neediness” factors, or both do not play a role.

²⁰Two of the authors of this study, Schmidt and Walach, were the co-authors of the earlier study alerting DMILS researchers to the failure to meet modern standards in EDA measurement practice.

²¹One of the goals of the project was to evaluate the advantages and shortcomings of various statistical measures. Four different methods were used, and compared to the “overall mean” approach used for previous EDA studies (in which all positive results

and respiration measures, even using the more stringent state-of-the-art standards specified in his earlier paper.

A further, interesting approach involves brain monitoring during DMILS experiments. Just as was found in the preceding chapter with the presentiment research, EEG and fMRI brain imaging devices are a further resource that can be used to detect neural activation patterns consistent with a DMI effect, and a number of experiments have exploited this approach.

EEG Studies

The use of EEG monitoring to explore DMI dates to 1963 (Tart, 1963), in a study that showed some evidence that receivers in an isolation chamber experienced EEG activation correlated with episodes when a sender was subjected to an electrical shock at randomized intervals. A further early study, published in *Science* (Duane & Behrendt, 1965), reported correlations between increases in EEG alpha rhythms in one monozygotic twin's brain with the closing of the other twin's eyes²² (subjects were isolated in separate rooms 20 feet apart). Further research reported by Targ & Puthoff (1974) in *Nature* showed significant results in extended experiments with one receiver, yielding statistically significant results of $p < 0.03$. The receiver, sequestered in a

were pooled and one mean for the entire set be taken, and the same for negative results). Interestingly, this traditional approach was shown to seriously under-represent the effect, yielding a positive but non-significant p value of 0.123, rather than the more significant result the stricter measuring techniques produced.

²²Typically, alpha rhythms increase substantially when an individual closes his or her eyes.

shielded room, was reacting to the photo stimulation (using a flashing light) of an agent/sender isolated in a room approximately 22 feet away. A sham experiment using the same parameters but no receiver showed null results. Other experiments by Orme-Johnson *et al* (1982), Grinberg-Zylerbaum & Ramos (1987), and Grinberg-Zylerbaum *et al* (1994) pushed this experimental paradigm yet further, with some reported success.²³

However, Fenwick *et al* (1998) produced only slight positive indications in a replication of the Grinberg-Zylerbaum *et al* (1994) experiment (though Fenwick used clicking sounds instead of visual stimuli, which may have had less impact on the receiver's brain activity). Sabell *et al* (2001) produced non-significant results, also using a small audio signal as stimulus. Other recent studies using more up-to-date analytical methods and EEG equipment show more robust results.

For example, Wackerman *et al* (2003) conducted an experiment to see if the EEG measures of a receiver would correlate with the EEG measures of an agent/sender when a visual stimulus (a flickering lighted grid pattern) was flashed in the sender's eyes at randomized intervals. Participants were in adjacent (but not adjoining) acoustic and electrically-shielded isolation chambers. There were 38 participants (17 pairs and four individuals) divided into two "experimental" groups and two control groups. The first experimental group was made up of pairs of people who were related to one another, while the second group contained pairs of unrelated people. Of the two control groups, the first was made up of three related pairs, while the second contained four unrelated

²³Though Grinberg-Zylerbaum & Ramos were subject to some criticism for insufficiently rigorous controls.

individuals.

Before testing, pairs in the ‘related’ group spent 20 minutes in each others’ physical presence “tuning in,” to enhance the feeling of psychological and emotional connectiveness. Members of the ‘unrelated’ group were kept apart from their pair-partners and, in fact, were unaware there was a another person in the other isolation chamber during the experimental phase. EEG data was recorded from both participants in the pairs from each experimental group over a six minute period while the visual stimulus was presented on a cathode ray screen 72 times to the agent on a randomized schedule interspersed with periods of randomly-assigned lengths. In the case of the control group with the three related couples, the experimental session proceeded just as with the previous groups (included EEG monitoring of both participants), except that the monitor was hermetically covered so there was no stimulus presented to the agent, and thus nothing ‘transmitted’ to the receiver. The second control group, conversely, only had a participant in the receiver role – there was no agent/sender and that isolation chamber remained empty. Results showed “no difference between the experimental groups...but strong deviation from the control group” ($p < 0.01$), indicating “a high coincidence of variations of the brain electrical activity in the *non-stimulated* subjects with brain electrical responses of the stimulated subjects” (pp.63-64).²⁴ (In a later paper, Wackerman (2004) describes a further conceptual replication of this experiment which

²⁴Interestingly, there was no consistent direction of the correlated brain-voltage change between participants – it could present as an increase or decrease. Further, the effect seemed somewhat more “global,” or involving several areas of the brain, rather than being localized to a specific brain region.

produced significant results, but in unexpected ways. Details were preliminary, and insufficient for a more thorough evaluation here).

In a joint NIH-funded study (Standish *et al*, 2004) between researchers at the University of Washington and Bastyr University, 60 participants divided into 30 agent/receiver pairs were tested in separate isolation rooms for correlated EEG responses when the agent of the pair was stimulated by a flickering light pattern. Each member of the pair served once as receiver and once as agent, yielding a total of 60 experimental sessions. Five of the participants demonstrated independently-significant results above $p = 0.01$ (the threshold set for significance in this experiment). EEG data analysis over all 60 participants showed results were significant at $p = 0.0005$. According to the authors, these effects could not be explained by “spontaneous EEG correlations [since] statistically significant ($p = 0.01$) correlations were only found in the receiver’s brain activity associated to the sender’s stimulus-on conditions. No correlations were found associated with the stimulus-off condition...there were no false positives” (p.313).

Kittenis *et al* (2004) conducted an experiment, the goal of which was to see if EEG-measured potentials evoked in the agent by a light flash would be correlated with event-related potentials (ERP) in the sender. Both agent and receiver were placed in a mildly altered state by an audio-recorded progressive relaxation exercise, with further audio of rhythmic drumming at specific frequencies being subsequently added to increase the uniformity of the altered state between agent and receiver. The agent/sender wore dark glasses with eight white LEDs, four over each eye, which flashed or did not flash according to a randomized computer process which simultaneously entered an event

marker on the EEG trace. These flash periods were interspersed randomly with event markers placed in the data but without a flash occurring. Of the 41 participants, there were 13 pairs that were related, five pairs that were unrelated, and five unpaired individuals who participated as controls.

When the results were tabulated, they showed a clear positive effect demonstrated by both unrelated and related receiver pairs, while that for the five control subjects was just as clearly null. The difference between the stimulus and control conditions for the related pairs was $p = 0.023$. When the unrelated pairs' results are also included, the effect was more pronounced, at $p = 0.007$. The unrelated pairs showed a stronger effect, but there were fewer of them, so the authors recommended caution in drawing conclusions about their results.

fMRI Studies

Finally, an additional measurement dimension that adds support for the EEG data involved two studies employing functional magnetic resonance imaging (fMRI), and both produced positive results. The first was another collaboration between Bastyr University and the University of Washington, and funded by the NIH. Richards *et al* (2005) used a successful agent/receiver pair from the Standish *et al* (2004) EEG study to perform an experiment with the receiver ensconced in an fMRI. While the receiver was being monitored within the MRI, a flickering vs. a static visual grid pattern was shown to the agent according to a random schedule. Participants alternated roles in two sessions of two trials each. The agent was stimulated by a flicker pattern, while the receiver was

fitted with goggles which presented a relaxing static (and non-stimulating) image. Any effect registered by the fMRI would be attributable to “decrease in fMRI brain activation, relating to blood oxygenation” (p.958). Results for the first receiver in both standard and replication trials showed a significant effect at $p = 0.017$. When the roles were reversed, the second subject showed no significant effect for the first trial, but the replication trial showed significant results – also at $p = 0.017$. This p value of 0.017 represents a conservative statistical threshold set to guard against a false-positive and corresponds to a t -score of -4.8. As anticipated, brain activations correlated with stimulus periods and, more interestingly, occurred in the same brain regions of both receivers – the occipital region, consistent with optical stimulation. This finding adds further confirmation to the presence of a real effect.

Finally, Achterberg *et al* (2005) performed an fMRI study at the Department of Radiology, North Hawaii Community Hospital in Waimea under supervision of the University of Hawaii. The study involved 11 Hawaiian healers (three men, eight women), each with a selected partner (also 3 men, 8 women) with whom they felt a connection, to act as receiver. The selected receivers were scanned in the MRI as the healers ‘projected’ their own particular form of remote influencing toward the partner over 12 two-minute episodes (six activate, six control) arranged randomly between active and control periods. Results showed highly significant differences between activate and control periods ($p = 0.000127$, or odds against chance of nearly 1 in 8,000). Both individual results and overall results were significant.

Biological/Machine Interactions

Finally, I shall consider a small number of experiments that do not fall strictly within the DMILS paradigm but seem to share some features between DMILS and certain mind/machine and micro-psychokinesis research formats which have produced highly-significant results. The reason I consider this small experimental set is because they involve apparent intentional interaction with non-biological, yet animate, highly-labile systems – the movements of which are driven by true-random event generators (REG).

The first of these involve rather whimsical but nonetheless effective experiments involving newly-hatched chicks as agent-participants. Both were conducted by René Peoc'h at the University of Nantes (Peoc'h, 1995). In the first experiment, young chicks were imprinted on a small robot programmed to restrict its movements within an approximately 12 square-foot rectangular space according to a fully-random pattern. The chicks were enclosed in a transparent cage placed just to the side of the robot's random-walk area, affording them a full view of the robot's movement area. The robot was recorded as having spent 2.5 times more time in the half of the area closest to the chicks than it did under two control conditions – when no chicks were present, and when chicks which were not imprinted on the robot were in the cage. Significance for this experiment was at $p < 0.001$.

A second experiment did not involve imprinting the chicks on the robot, but exploited their desire for light during active periods. Twelve hundred chicks were hatched in the dark and divided into 80 groups of 15, then raised for seven days in a dim

to dark environment. They were fed automatically and had no exposure to the experimenter until prior to completion of the experiment. During the trial period, individual groups of 15 chicks were placed in a transparent cage at the edge of the robot's random-walk area as in the first experiment. In this case, though, the room was darkened, the only source of light being a lit candle on the top of the robot. The test was to see whether the robot spent a statistically-significant portion of the 20-minute experimental period in the vicinity of the cage. Each set of 15 chicks repeated the experiment three times on consecutive days. Of the 80 sets of chicks, 57 of them, or 71% showed increase dwell-time of the robot in vicinity of the cage, which was statistically significant at $p < 0.01$.²⁵

The “chick studies” were replicated, albeit with less robust results, using humans who specified in advance whether they would attempt to intentionally influence a small REG-driven robot to perform in certain ways (Jahn *et al* 2007). Under the hypothesis that “an anthropomorphic resonance with the behavior of such a robot would enhance anomalous alterations of its random trajectory, with corresponding departures of the digital output of the REG unit directing it” (p.28), an appropriately-sized randomly functioning robot and a suitable platform were constructed.²⁶ Agent-participants would

²⁵Interestingly, it was observed that on occasions when the chicks went to sleep during experimental periods, there was a noted *repulsive* effect, in that the robot spent more time in the opposite half of the area away from the chicks. The researcher speculated that perhaps once in sleep mode the chicks found the light source bothersome. Of further note, degree of illumination emitted by the candle (e.g., as determined by height of candle and length of wick) seemed to affect the strength of results for individual sets of 15.

²⁶For visual interest, the robot was enclosed in a dome-shaped case resembling “a

seat themselves to one side, then prior to start of the experimental be directed by the experimental protocol whether to attempt to influence the robot to perform one specific set of maneuvers or another. One set, for example, required the agent/operator to cause the robot to exit the maneuver area either in front of the agent's seated position or directly across from that position (zero degrees vs. 180 degrees). Participants acting as agents included individuals, pairs, and groups. Overall results were non-significant, but the female sub-group (scores from all females aggregated) showed significance at $p \leq 0.05$, and group scores were somewhat stronger yet.

A second maneuver set involved "duration" experiments – to cause the robot to stay longer or exit sooner from the operating area, or to execute longer or, alternatively, shorter movement segments. These results were overall non-significant, but in the positive direction, and several individual agents approached significance with one exceeding it at $p = 0.0043$.

I here conclude my consideration of two "nonconscious" modes of ESP. In advance of moving to the next chapter, I pause to take a brief inventory of the research covered in this and the preceding chapter. The first experimental paradigm considered was presentiment and pre-stimulus response – the apparent ability for humans to detect or be influenced or autonomically affected by emotional, unsettling, or otherwise influential stimuli which will not actually be presented to them for up to several seconds in the

miniature Zamboni machine" upon which "a toy frog perched in a driving position" (p.28).

future. The effects persists across a spectrum of stimuli, experimental designs, and measuring methods. Effects have been produced in wide variety by several different researchers in different labs and universities. They also show up as correlations in brain-imagery and EEG monitoring.

The DMILS/Staring material demonstrates similar ubiquity. The basic design has been successfully replicated numerous times by different researchers in varied research settings at several locations around the world. Yet the results persist across a number of different measuring and detecting methods – some, again, involving brain-imaging and monitoring techniques that pursue evidence of the effect even into the inner reaches of the human nervous system. They also show up in experiments involving other biological and labile systems besides humans.

I have discovered 68 DMILS studies, including a handful of staring experiments using appropriate controls and measures to warrant inclusion here. Of these, 40 were statistically significant. Of the remainder, six were significant but demonstrated mixed results (for example, the Schlitz/Wiseman experimenter-effect research). Eight were non-significant but indicated a positive direction. And for six I either could find no record of a statistical outcome, or none was reported. Of the 11 “failed” experiments, several were attempts to alter the formula to see how that would change the effect. Of 68 experiments, one would expect by chance to see at most just over three turn out significant at the $p = 0.05$ level. Yet more than ten times that many produced significance, from several different laboratories and groups of researchers. As in the case of the presentiment paradigm, this is provocative evidence for a real effect.

The detail in which I have described this material, while perhaps at times tedious, also serves to point up the rich technical depth and breadth in which the research has progressed. It illustrates a serious and productive research programme demonstrating phenomena which, though at times recalcitrant, are neither ephemeral nor transient.

As I just pointed out, not all replications have succeeded. But as in any other scientific pursuit, these have led to a refinement of the process which has then produced improved design – and further significant demonstrations of the effect – showing that even as careful examination and self-criticism within the community tightens the controls and increases understanding of the protocols involved, the effects clearly continue to persist.

Before going on to examine the evidentiary legitimacy of these facts (in chapter 10), I will first look into two research paradigms which provide not only statistical, but also objectively observable *prima facie* evidence for consciously-perceived psi effects.

Chapter 7: Counterexample to Physicalism: Remote Viewing

In previous chapters I considered experimental paradigms pertaining largely to *unconscious* manifestations of ESP. In this chapter and the next I will examine two paradigms that involve consciously-detectable instances of ESP. These, too, are related to each other, but involve somewhat different modes of evaluation, rendering them sufficiently distinct that I will consider them separately. The first of these is generally called “remote viewing” (or RV – though some protocols refer to it instead as “remote perception”). The second is referred to as “associative remote viewing,” or ARV. Even more so than in my previous discussions of ESP evidence, I will frame my discussion of remote viewing in a historical-narrative style more typical of a history-and-philosophy-of-science treatment, since the import of evidence I present and its implications are best understood within the scientific dialectic from which they emerged.

REMOTE VIEWING/REMOTE PERCEPTION

Remote viewing grew out of a series of what were intended to be “out of body” (OBE) experiments conducted at the City College of New York and the American Society of Psychical Research. The subject in these experiments, a New York artist and parapsychology researcher named Ingo Swann, modified the OBE protocol in such a way that it developed into a new experimental approach to test for clairvoyance. Swann came to the attention of a theoretical physicist named Harold Puthoff who was a staff scientist at what was then a research institute belonging to Stanford University (this research

facility later became independent, and was renamed SRI-International). In 1972 the Puthoff and Swann collaborated on a successful experiment, the report of which attracted the interest of the Central Intelligence Agency. At the time, the CIA was looking for a science entity to clandestinely explore parapsychological research to determine if a discipline then being tested in by the Soviet Union might eventually pose a threat to the United States' security interests. The CIA formed a program around Puthoff and other researchers at Stanford Research Institute (now SRI-International). Little more need be mentioned of the history of this program, other than to say that its management was eventually passed on to the Department of Defense where it remained until the program was terminated in 1995, after the end of the Cold War. (Kress, 1977/1999; Puthoff, 1996; Targ., 1996; Bremseth, 2001; Smith, 2005)

Remote Viewing Procedures and “Modes”

Remote viewing is based on the notion that humans have the capability to obtain perceptual material from remote or shielded locations via some as-yet undiscovered extra-sensory perceptual means. Developers of the protocol hoped to bypass problems with the forced-choice experiments that had been the mainstay of such researchers as J.B. Rhine and others during the 1930s through the 1960s.¹ Insights gleaned from work done

¹Forced-choice experiments – involving a limited target pool from which the participant must select (examples are card-guessing trials using either standard playing decks or specially-designed Zener decks) – have the virtue of facilitating easy quantitative analysis. On the other hand, since thousands of repetitions are required, participants often witness a decline effect, typified by significant boredom or “burnout.” Further, there is little qualitative material to assess, and the utilitarian value for possible

by novelist Upton Sinclair and his wife Mary (Sinclair, 1930) and French parapsychologist René Warcollier (Warcollier, 1948), among others, were incorporated into the remote viewing process.

There are three main modes for conducting remote viewing experiments. The first of these is commonly called the “outbounder” or “beacon” protocol (Puthoff & Targ, 1978).² In a typical beacon remote viewing experiment, the percipient remains in a sealed and shielded room, usually in the company of a second person serving as an “interviewer” or “monitor.” Meanwhile an “outbound” or “beacon” team has randomly and “blindly” selected a target envelope from a pool of identical envelopes (each with different contents from all others in the pool). Inside the envelope are directions to guide the beacon team to a specific geographical location. The envelope is opened in a setting where the remote viewing subject and any experimenters associated with him or her will have no opportunity to gain knowledge of the chosen location.

The instructions direct the beacon team to arrive at this location at a specified time, and to remain there for a specified duration, at the end of which they return to the site of the experiment. The viewer begins the remote viewing process at the agreed upon time, and ceases at the end of the specified period. The process involves verbal, written, and graphical (sketching and drawing) reports of what the viewer perceives to be at the

applications is negligible. Free-response experiments, alternatively, often provide rich qualitative data, and have high potential for applications once refined. But this is at the cost of often difficult quantitative assessment (Schlitz & Gruber, 1980).

²I shall standardly use the term “beacon” and will for the purposes of this project refer to it as BRV for short.

chosen location to which the beacon team has been directed. Before the return of the beacon team, the viewer's results will have been secured against tampering. At this point the viewer is provided feedback of what the target location had been, either through photos or video footage, or by physically visiting the site.

Evaluation can be by *prima facie* assessment, but for scientific purposes is usually instead by a double-blind judging process in which an evaluator, unwitting as to the chosen target location, examines the viewer's description and compares it to the set of all possible targets in the pool, selecting the one the judge feels is a "best match." This provides a means of converting a subjective judging processes into an objective, quantitatively-assessable one. If, for example, there were five possible targets in the pool, there is a 20% probability that the judge will match the viewer's results to the correct target just by chance. If, over the course of ten experiments, the judge matches the correct target to the results five out of the ten times, this produces odds against chance of 1 in 40 or a statistically significant value of $p = 0.03$.

However, rather than the simple single-match "best choice" approach illustrated here, many RV experiments use a somewhat more fine-grained process, where the judge rank-orders the transcribed results of each individual remote viewing trial against each possible target in the experimental target pool. The ranking goes according to which transcript the judge thinks matches the actual target best, which matches second best, and so on. This "sum-of-ranks" procedure generates a matrix of rankings which, through simple addition and statistical manipulation yields a more nuanced statistical measure that can help data fall out of an otherwise noisy perceptual channel, and provides some

accommodation for relative quality of detail in the results. Later research adopted a more sophisticated procedure called direct-count-of-permutations which was not sensitive to target-order dependence³ and other artifacts that could bias a statistical analysis (Puthoff, *et al*, 1979).⁴

Many people are confused about the nature of extrasensory perception, and think it is all simply different forms of telepathy. Because the use of a ‘beacon’ in the remote viewing mode I have just explained may suggest to some that telepathy is involved, it is important to stress the difference between remote viewing (a form of clairvoyance) and telepathy. In telepathy, a distant person’s (a “sender’s”) mental contents presumably become available to the perceiver (“receiver”). If there is any transfer of information about the location where the “sender” is, it would be through the “receiver’s” access to the sender’s mental contents.

However, in remote viewing the beacon person or team is not meant to be a “sender” of data. Rather, the beacon merely demarcates the target location, on which the viewer “homes in” to find the correct site. Remote viewers seem not to detect telepathic impressions from beacon persons at the target, and they are able to obtain impressions from features of the target to which the beacons have no access nor of which they have

³In a sum-of-ranks procedure, skewed statistical measures can result from a “process of elimination” strategy when some matches are obvious and increase the chances of correctly matching the remainder (think of doing well on a multiple choice question to which one has no idea of the answer because one can eliminate some of the choices).

⁴Though direct-count-of-permutations provided a more statistically-secure evaluation, in many instances it produced significant results not importantly different from that produced by the sum-of-ranks method.

no knowledge (e.g., details of building roofs, structural interiors, features hidden by shrubbery, etc.).⁵ This distinction between remote viewing as a clairvoyant phenomenon and telepathy is important, for two reasons. For one, if an experiment is intended to be a remote viewing experiment, but is set up as if it involved telepathy, it may not succeed well. Second, as I will shortly explain, successful remote viewing can be conducted with no beacon involved at all.

The second remote viewing mode follows the same procedures as the beacon process except that the remote viewing session is performed some hours or days *prior* to the target being randomly and blindly selected, and the beacon excursion conducted. This is the *precognitive remote perception* protocol (PRP), and was explored primarily at Princeton University in the Princeton Engineering Anomalies Research (PEAR) lab.

The final primary remote viewing mode uses instead of a beacon, an abstract referent (geographic coordinates, encrypted coordinates, target reference numbers) to focus a viewer on the intended target, and the viewing process generally follows a more structured methodology. This was originally termed “coordinate remote viewing,” but is now standardly referred to as “controlled remote viewing” or CRV, though a number of offshoots (named variously by their developers as TRV, SRV, etc.) have recently been developed by hobbyists and entrepreneurs in the civilian community. In principle, these offshoots generally follow a similar tasking mode and methodology as their CRV progenitor. The purpose of this and the other tasking methods is to direct the viewer’s

⁵Indeed, there is an ongoing debate in parapsychology whether there is actually a such thing as strict telepathy. Many instances of what is taken to be telepathy are

awareness toward the desired target without revealing any details or information about it to the viewer. (The majority of remote viewing research considered here and in the next chapter was conducted in ‘beacon’ mode.)

No matter what mode of remote viewing tasking is employed, correct protocols require that the viewer be at least double-blind (that is, neither the viewer nor anyone else who associates with him or her before or during the session should have knowledge of what the designated target is, its location, or any details about its nature). The viewer should be isolated from any aspect of the tasking process that might convey such information, and the tasking means should convey no meaningful information about the target. Hence, in the case of BRV and PRP, the viewer is (usually) acquainted with the members of the beacon team in advance of their receiving their target and departure, but has no knowledge of where they will be sent, or even of what targets may be included in the target pool from which the destination has randomly been drawn. In the case of CRV, except in earlier trials where geographic coordinates were used, the referral number will also be empty of information.

How the viewer is able then to focus on the correct target and provide veridical information in the absence of cognizable targeting information has been speculated upon but is thus far unknown. Based on resemblances between remote viewing responses and certain types of subliminal and subconscious behavior, it is speculated that the viewer acquires some awareness at the subconscious level of the intentionality associated with the designated target, and this drives the localization and acquisition of target-related

actually better explained by clairvoyance.

material.⁶

Remote Viewing Idiosyncracies

There are certain idiosyncracies of the remote viewing process that must be taken into account by any practitioner or researcher, and I will consider some of these briefly. The first and perhaps most important of these is can be generally characterized by the term “mental noise.” Mental noise is that overlay of memories, conjectures, inferences, concept-associations, pattern-fitting, speculation, conclusion-jumping, and so on that tends to compete with the comparatively ephemeral ESP signal which apparently emerges from subconscious perceptual channels. This “noise” must be taken into account in consciousness-mediated ESP phenomena such as remote viewing. If not, veridical data may become embroidered with speculative or inferred connotations that render otherwise-accurate data “false.” There is good evidence to think this conflation of data with conscious mental processing occurs when the language-centered left hemisphere⁷ attempts to interpret or “explain” the output of a small-bandwidth perceptual channel based on information insufficient to make an accurate judgment. (See Puthoff & Targ, 1976; Puthoff & Targ, 1975; Smith, 2005.)

A second characteristic of the remote viewing phenomenon is that, while

⁶I recognize the vagueness of this explanation. But extensive trials and research carried out in this subject-area indicate a correlation between viewer behavior and RV results that support this speculation fairly strongly.

⁷Designated the “Left-Brain Interpreter” by psychologist Michael Gazzaniga (Phelps & Gazzaniga, 1992; Gazzaniga, 1991).

perceived data can often be quite robust and provide striking matches with the intended target, it can frequently instead tend towards vagueness and a somewhat disconcerting mix of vivid, accurate details with poorly defined generalities and some outright inaccuracies. Though novice remote viewers on occasion can produce high-quality results, accuracy and clarity tend to improve with practice and experience – though even accomplished remote viewers will provide less-than-optimum results from time to time. Since the actual causation is not understood for ESP phenomena in general, including remote viewing, it is not always certain why some results are better than others, and what boundary conditions and parameters may facilitate or hamper the process. Some headway has been made in recent years that seems to have improved RV performance in those cases where they have been applied (in the consideration of space and the relative complexity of the topic I will forgo further discussion of these).

The requirement for feedback is another important feature of remote viewing. Though experienced remote viewers have been shown not to need it in all cases, receiving veridical confirmation of what the target was that the viewer has remote viewed contributes to remote viewing success. This is particularly so with the novice subjects who typically have made up many of the participant pools for remote viewing experiments. Feedback may consist of the viewer being taken to the chosen target after the session to see what it was she was meant to have viewed, or it may in some cases instead be photographic or video depictions of the target site. It is only provided *after* the completion of the remote viewing trial and after all results have been secured from tampering. When feedback is given, it *must* be fairly soon after the trial, and in any case

should not be delayed until after the viewer has performed other remote viewing trials, and especially not delayed so long that feedback from other trials has already been presented to the viewer. To do otherwise risks conflating a viewer's descriptions of the various targets, which can detract from the overall results of the remote viewing experiment.

A phenomenon that tends to affect novice remote viewers is sometimes called the "first time" effect (Targ & Puthoff, 1977, 100; Puthoff & Targ, 1975, 49ff; 330; Smith, 2005, 175-176). Often, when someone attempts remote viewing for the first time, a fairly high-quality result is produced, and may continue through another one or two experimental trials. But after this, there is a tendency towards a decline in quality which lasts for a nonspecific number of attempts, depending on the novice's aptitude and perseverance. With varying amounts of time and experience, quality begins to improve again. There is a possible explanation for this, which is not important here. But the first time effect may be relevant to the many remote viewing experiments which use novice or inexperienced subjects over a series of trials – especially if the trials occur too closely together, which can lead to fatigue or "burnout."

Remote Viewing Research

Of all the most recent ESP research paradigms, remote viewing may well be the most extensively tested. This research has, however, followed a somewhat different and, to some degree, obscure path. Since much of the research was done on behalf of the United States military and civilian intelligence establishments, a large portion of it was

hidden behind a barrier of classification and generally outside the standard peer-review process. To make up for that, the research was continuously overseen by federally constituted research boards to which accredited and often prominent scientists were appointed. Periodically, inspection boards of scientists were also appointed to provide third-party assessments of progress and scientific viability.

Since the majority of the government investigation and application of the remote viewing phenomenon was classified, there are two research threads to follow – one pursuing the standard civilian trajectory more or less typical of other parapsychological disciplines, and the other behind the wall of classification. The latter body of research emerged in small increments over the 23-year life of the program, but the bulk of it became available in 2004 with the CIA’s declassification and public release of 90,000 pages of archives from the by-then defunct program.

The first published results from the Stanford Institute Research (SRI) program appeared in *Nature* in 1974 (Targ & Puthoff, 1974).⁸ Three sets of results were reported. The first of these was a test of Israeli psychic entertainer Uri Geller’s alleged ability to remotely-perceive and reproduce hand-drawn graphics generated by second or third parties out of his line of site (usually in another room). Puthoff and Targ established rigorous controls, conducting a series of 13 trials under double blind and acoustically, visually, and electromagnetically-shielded conditions. In 10 of the 13 trials Geller

⁸It may at first seem odd that a classified program would publish results in an open-source, peer-reviewed science journal. Since it would be difficult to keep the fact secret that a research organization of the stature of SRI was engaging in ESP research, CIA officials decided it would actually help security to publish non-sensitive basic

produced accurate reproductions of the sketches, but ‘passed’ on three others.⁹ A second research series involved an early form of the DMILS paradigm to determine if percipients could autonomically detect a flashing light, as demonstrated by an electroencephalograph (EEG) trace. One of the participants twice produced statistically-significant results.

The third experiment was of most interest from the perspective of remote viewing. This consisted of a series of beacon remote viewing experiments with an unusually successful subject named Pat Price. To ensure the blinding condition for the target pool, an SRI administrator not otherwise associated with the experiment created and maintained the target pool. Out of the presence of the remote viewer an experimenter blindly and randomly selected the target for the beacon team at the start of each trial. The standard beacon protocol was followed, with the viewer and monitor sequestered, the beacon team members opening the sealed envelope only after entering their automobile, then proceeding on to arrive at the designated target at the specified time, and so forth.

As explained in more detail in a later paper (Puthoff & Targ, 1976), at the close of the experiment series the results were independently blind-judged twice – once by an SRI research analyst who was unaffiliated with the project, and a second time by a panel of five SRI scientists also otherwise unaffiliated with the experiment (judging was

research results on a recurring basis.

⁹Contrary to contemporaneous media reports and skeptical claims, the SRI researchers specifically stated they did *not* confirm Geller’s claims about paranormal metal bending.

performed twice as an added quality check for the judging process). In all cases, the transcripts were presented to the judges unlabeled and in random order for comparison. A judge would visit each of the locations, and rank the verbal and sketched responses in each of the transcripts for correspondence from best to worst to each of the target locations. This yielded a 9 X 9 matrix where a perfect score would be nine and the worst score 81, while chance would be 55. The results for the completed judging yielded seven direct hits of the nine possibilities, and a very high sum-of-ranks score of 16 yielded statistical significance at $p = 2.9 \times 10^{-5}$ (Puthoff & Targ, 1976, 335).

This later paper was published in the *Proceedings of the IEEE*¹⁰, two years after the *Nature* paper appeared, and reported a number of other successful experiments. Among these was another nine-target series against a different target-set by novice remote viewer Hella Hammid, which proved highly significant at ($p = 1.8 \times 10^{-6}$). Replication experiments were also performed with four additional subjects, two of whom were experienced viewers and two who, like Hammid, were also novices. Each did four sessions, but for evaluation purposes (to provide a statistical base for comparison as close as possible to the Price and Hammid series) transcripts from the sessions of the two experienced viewers were combined for judging. The same was done for the novices (although in this case there were only seven rankings possible, since one target came up twice in the randomization process). The sum-of-ranks procedure gave a significance value of $p = 3.8 \times 10^{-4}$ for the two experienced viewers and came up just short of being

¹⁰The Institute of Electrical and Electronics Engineers.

statistically significant, for the novices, at $p = 0.08$.¹¹

The experimenters had adopted a further experimental practice which allowed an assessment of how successful unselected participants might be with the remote viewing process. Puthoff and Targ would often persuade visitors to the laboratory to serve as remote viewing participants. These were designated 'Visitors' (as in V1, V2, and so on), and each contributed one or more sessions to the data base.¹² At the time the IEEE paper was published two 'visitors' had contributed five sessions. Of the five, three were first-place matched and one was second-place ranked, with statistical results significant at $p = 0.017$.

Another series of trials were run to assess the resolution capabilities of the remote viewing technique.¹³ Five remote viewers (two of whom were visiting government scientists) performed 12 remote viewing trials against randomly selected targets that were mid-scale technological equipment, including a floor drill press, Xerox photocopier, IBM Selectric typewriter, video terminal and so on (since the target pool was with replacement – that is, target were put back into the pool after being used – some of the targets came up twice). Results were significant at $p = 0.036$.¹⁴

¹¹Though even in this series there were two first ranked hits and two second ranks out of the seven in the series (Puthoff & Targ, 1976, 340).

¹²The 'visitor' designation was often used as a euphemism to disguise the fact that the visitor-participants were actually CIA scientists come to observe the research.

¹³This was part of an overall strategy to see what if any role electromagnetism played in remote viewing. This will be covered in more detail in chapter 10.

¹⁴In an interesting alternative evaluation method to illustrate quality of the work, a visiting scientist randomly selected one of the 12 transcripts and submitted it to an

Altogether, the IEEE paper reported 55 trials with 12 subjects, roughly grouped into seven experiment sets. Of these seven experiments, six had yielded statistically-significant results, some highly so. Only one was non-significant, and even in that case the results were nearly significant.

Replications of the Early SRI Work

This newly-developed protocol sparked considerable interest in both the scientific parapsychology and critical/skeptical communities.¹⁵ A number of both close and conceptual or in-principle replications followed. Hastings and Hurt (1976), using a beacon RV model with 36 individuals in a group setting reported significance at $p = 6 \times 10^{-7}$ against an outbounder target selected blindly and randomly from, then judged against, a six-target pool.

A more ambitious group experiment was conducted by Whitson, Bogart, Palmer, and Tart (1976). One goal of the study was to determine if RV ability might be widespread even in untrained, unselected populations. Using as subjects two art classes at the University of California at Davis ($n = 27$, and $n = 14$, respectively), and following a still more carefully controlled beacon protocol than that followed by Hastings and Hurt,

engineer for blind, independent analysis (e.g., “what does this verbal description and these sketches describe?”). The engineer’s analysis concluded that the target was most likely a “man-sized vertical boring machine.” The target which corresponded to the transcript was in fact the free-standing floor-mounted drill-press. (Puthoff & Targ, 1976, 344-346)

¹⁵I will have more to say about criticisms and skeptical responses later in this chapter and in the next.

the experiment produced composite results significant at $p = 0.03$ against a target selected from a ten-target pool.¹⁶

Vallee, Hastings, and Askevold followed with a novel approach to the remote viewing protocol, using one of the earliest examples of computer conferencing over ARPANET (predecessor to the Internet) as the mechanism for remote viewers to report their impressions. This allowed viewers to be insulated by distance from sensory leakage¹⁷ and other sorts of extra-experimental artifactual cuing, plus allowed verification that submissions of results were properly made according to experiment time lines through a machine-based time/date.

Another novel feature of the experiment was that, instead of geographical locations, samples of commercially important minerals were used as targets. There were 10 samples, including halite (macro-scale crystals of NaCl, regular table salt), opal, gold ore, magnetite, cinnabar, and so on. The ‘beacon’ was a geologist who, on a pre-designated schedule on the specified day, briefly removed and handled an arbitrarily selected mineral sample, after which the viewers typed their perceptions into their computer terminals and forwarded them to the central terminal. There were two parts to the experiment – the “open” condition where the geologist interacted with the mineral

¹⁶Results from the first art class were impressive, with the correct target ranking in first place out of ten. The second art class demonstrated much less enthusiasm for the project, and produced noticeably lesser-quality transcripts – yet the correct target still came in third out of ten in the judges’ rankings.

¹⁷Inadvertent, non-ESP-derived disclosure or communication of information about the target to the viewer from sources or persons possessing such target-relevant information.

samples, and the “double-blind” condition, in which an undisclosed second sample was left in a sealed, opaque, randomly-numbered envelope and placed in a designated location for the balance of the specified day.

Altogether, six novice viewers participated, producing 33 descriptions which, when blind-judged, yielded eight first place matches out of the 33 where chance expectations would be just over three matches in 33. Odds of this happening by chance are less than one in 100. The experimenters reported that features most accurately described by the viewers included color, shape, “relative weight, presence of crystals, type of material (e.g, metallic) and geological formation process (e.g. volcanic)” (Vallee, *et al*, 1976, 1552). But they almost always got the location where the sample had been found wrong. Of further interest was that the double-blind results were of the same order as the “open” ones, suggesting that a human “transmitter” (in the sense of telepathy) was not essential to remote viewing success (this finding was further substantiated in a number of later experiments).

Bisaha and Dunne (1979b) explicitly attempted a replication of the SRI research in the Chicago area, but used a modification suggested in the Puthoff/Targ IEEE paper in which the remote viewer provided his or her impressions *precognitively*, before the blind, random selection of the target and the dispatch of the beacon. The experiment was conducted in spring 1976, and involved two percipients (one performing two sessions, the other six, solely based on their availability), with one beacon (or “agent,” in Bisaha’s and Dunne’s parlance).

The beacon departed in advance of the session, carrying 10 identical envelopes

from the 100-target pool. After driving around aimlessly for a set time, she randomly drew one envelope from the container, and followed the instructions contained therein to arrive at the indicated target 20 minutes after the percipient had completed the session. After all eight trials were finished, all data was collected and collated. Eight judges were independently presented with only one transcript each, plus the entire set of eight target photos and descriptions made by the beacon while on site (which were randomized before being passed to the judges). Each judge was to rank his or her transcript from 1 to 8 against the eight possible targets. A sum of ranks analysis of all eight judges' scores was 20, yielding $p < 0.008$. Four transcripts were ranked in first place (chance would have been only a single transcript being so ranked), and one each was ranked as 2, 3, 5, and 6.

In a subsequent attempt to repeat their success with the Chicago study, but this time with more percipients, Bisaha and Dunne (1979a) engaged seven inexperienced remote viewers in an experiment that produced a total of 14 remote viewing trials following the usual beacon protocol, modified as before to accommodate the precognitive element. In each session, the remote viewing was done 20 minutes before target selection and 35 minutes before the beacon person arrived at the target site. The resulting transcripts were judged by six independent judges following a somewhat more complex procedure that it was hoped would determine comparative quality across session transcripts.

In this procedure, two judges each ranked half (7) of the 14 transcripts. A sum of ranks (SOR) analysis of one judge's results scored 15, which is significant at $p < 0.01$.

The score for second judge's SOR was 13, which is even more significant at $p < 0.005$ (in sum of ranks, a lower score means higher correspondence, due to a greater number of first or second place matches for the correct target). Two more judges were asked to blind-rank the other half (each had SOR ratings of 15, for significance at $p < 0.01$). The remaining two judges compared one set of transcripts against the other, trying to match the ones that were descriptions of the same targets as a determination of quality. The SORs for these two judges were 12 and 14 respectively, yielding $p < .001$ and $p < 0.005$. This produced altogether 42 judging matches, of which 17 were direct hits/matches, nearly three times chance expectation of six out of 42.

A second series of experimental trials by Bisaha and Dunne were conducted long-distance, with the percipient in Wisconsin and the beacon in Eastern Europe. The percipient attempted to describe five locations on each of five days where the beacon would find himself 24 hours and 30 minutes in the future of each remote perception attempt. The percipient's sessions were tape recorded and transcribed, as were those of the beacon, who also took photos of the targets. Five sets of transcripts were produced, .

Upon return of the beacon to Wisconsin, he gave the viewer the photos and descriptions of the target locations in random order for her self-judging. At the same time, the viewer had randomized the transcript order and expurgated them of any comments or descriptions that would give clues as to proper order, and passed them on to the beacon for his own judging. Finally, a third party unconnected with the experiment blind-rank-ordered the randomized transcripts to the beacon's target descriptions and photos. The evaluations showed the viewer's rank-ordering to be significant at $p <$

0.025; the beacon's rank-ordering also significant at $p < 0.025$; and the independent judge's was significant at $p < 0.05$. The experimenters believe this second experiment "demonstrated the capability of an individual to extend this ability over extensive spatial, as well as temporal, distances" (Bisaha & Dunne, 1979a, 110)

Another long-distance remote viewing replication was performed by Schlitz and Gruber (1980/2001), this time with the viewer (Schlitz) in Detroit, Michigan, and the beacon (Gruber) in the Rome, Italy, area. A pool of 40 possible targets was selected, from which the target for each daily trial was blindly and randomly drawn, as determined by the output of a random number generator. Altogether 10 trials were performed in the series, at the end of which photocopies of all 10 transcripts were prepared for judging by a third party otherwise unconnected with the experiment, two sets of copies made, one of which was forwarded to researcher Hans Bender in Germany as a neutral broker. The transcripts were then translated into Italian, and checked to make sure no inadvertent ancillary hints or cues were included that might skew the judging process. Five blind judges were recruited in the Rome area, and they scored and ranked every transcript against every site, plus gave a subjective rating of session quality. Transcripts were randomized differently for each judge, and a direct-count-of-permutations statistical evaluation method used for evaluation of both their rankings and ratings, which yielded two combined scores, both highly significant. The score for the judges rankings was $p = 5.8 \times 10^{-6}$; and for their quality ratings, $p = 4.7 \times 10^{-6}$.¹⁸

¹⁸In evaluating this first experiment, Schlitz & Gruber recognized that some statistical anomalies could creep into the evaluation method they used, so in a later paper

In 1980, Schlitz and Haight (1984) performed a further replication of the Schlitz/Gruber long-distance experiment. This experiment, too, comprised 10 trials, with the respective targets randomly drawn without replacement from a larger pool of geographical locations, this time in the Cocoa Beach, Florida, area (the viewer remained in Durham, North Carolina). The viewer received no feedback for several weeks after the end of the experiment. Results were sent to a disinterested third party for preparation for judging, after which they were passed to two independent judges in the Cocoa Beach area. The judges personally visited the sites in random order, and compared the randomized, expurgated transcripts to each, following the practice of rank-ordering each transcript to each of the set of targets for closeness of fit, and rating for quality. Results were calculated for both rankings and ratings, which turned out statistically significant: For rankings, $p = 0.048$; and for ratings $p = 0.025$.¹⁹

Other successful replications of the SRI research were performed by Chotas (1978) and by Schlitz and Deacon (1980; $p < 0.05$). Altogether, as of 1984 a review of remote viewing experiments in the civilian community found 28 published studies, of which more than half (15) were significant at $p = 0.05$ or better (where only 1 in 20 significant experiments would be expected at most by chance). Also, 18 unpublished studies were found, 8 of which reported statistical significance. (Hansen, Schlitz, and

(Schlitz & Gruber, 1981) they published a re-judging and analysis correcting for the possible flaw, which yielded a smaller, but still significant effect, at $p = .0016$.

¹⁹Though still statistically significant, these scores were not as robust as in the previous experiment. The experimenters suggest possible explanations (see Schlitz & Haight, 1984, 45).

Tart, 1984)

‘Failed’ Replications

It is important here to consider several of the apparently failed replications. In 1979, Karnes and Susman published what they termed a “remote viewing” study involving 90 subjects (described alternately as “remote viewers” and “receivers”) divided into nine perceiver groups. The target pool consisted of 20 buildings of varying shapes and designs, observed from varied indoor and outdoor perspectives. Of these buildings, nine were included in the target pool, nine were kept to contribute “noise” to the experiment, and two were used for two control groups of 25 participants each.

The Karnes/Susman experiment protocol departed noticeably from the standard Puthoff/Targ-inspired pattern. In that sense, this experiment does not constitute an actual replication of the Puthoff/Targ protocol, nor does it actually test any sort of remote viewing model, as I will shortly explain. Despite this, it is often included among that set of experiments cited as unsuccessful replications of the remote viewing research paradigm.

The Karnes/Susman experiment differed from the Puthoff/Targ model in the following ways. First, instead of viewer attempting to describe verbally and sketch the target site, participants in this case were given booklets containing 18 color photos, somewhere among which would be the picture of the actual site to which the “sender” (beacon) had been sent. “Receivers” were to attempt to “communicate” with the sender, while the sender tried to “transmit” perceptions about the target back to the receivers.

Receivers would then indicate on the answer sheet which photo represented the target they thought the sender was attempting to communicate to them. According to the authors, “It seemed reasonable that, if the receivers are capable of verbally describing and drawing sketches of their impressions of the sender’s location, they should be equally, or perhaps more easily, capable of simply visually recognizing color photographs of the sender’s location” (Karnes & Susman, 1979, 472).

This means of eliciting responses from the participants had the virtue of allowing a straightforward statistical assessment of the results, thus dispensing with the need for subjective evaluation and, hence, the requirement for blind judging.

One further provision of the protocol allowed the subjects to make as many guesses as they wanted among the 18 choices. This Karnes and Susman felt was justified because in the Puthoff/Targ approach viewers could record as many verbal impressions and as many sketches as they felt desirable within the 15 minutes time constraint; thus, making selections from among a set of possibilities was presumed to be commensurate with sketching and recording internal impressions.

A second difference from the Puthoff/Targ protocol was that Karnes and Susman felt that there was no way of actually assessing what constitutes ‘chance’ in the Puthoff/Targ model, so two control groups were added. This was justified because according to their assessment chance did not necessarily constitute $1/x$ (as is typically assumed), where ‘x’ represents the number of possibilities in the target-choice space, or in this case, $1/18 =$ chance for each receiver, since there were 18 possible choices, with only one being correct. Instead, the authors maintained that the measure for a chance

outcome had to be empirically-derived.²⁰ This added feature seems to make no difference one way or the other for the Puthoff/Targ model (notably since, as actually executed in the Karnes/Susman experiment, the chance value arrived at by the control groups seems to have differed little from 1/18). But it does add an additional level of complexity to the execution of the experiment.

Finally, the third difference from the Puthoff/Targ model was that multiple sets of viewers were employed, as this was deemed a more efficient way to develop experimental findings. (This final provision also seems relatively innocuous as far as the Puthoff/Targ model is concerned, especially since, as we have seen, others have taken similar liberties with the model and shown success with it.)

Karnes/Susman's completed analysis at the conclusion of the experiment showed that the receivers had performed merely at chance, leading to the conclusion that the experimenters found "no support for the reliability and validity of the paranormal phenomenon termed 'remote viewing'" (p.479). They did note a slight statistical bias in favor of a remote viewing effect, which they attributed to the fact that, since receivers were allowed to guess as many times as they wished, those who made the most guesses were also likely to include the correct target among their accumulated choices. In fact correct guessers made on average twice as many guesses as those who did not. Therefore, merely on the basis of a larger number of attempts the odds were in favor of more frequent guessers including the correct target among those guesses. Karnes/Susman felt that this amounted to a "response bias" interpretation, and

²⁰Why this was presumed to be the case was not further explored.

extrapolated it to the Puthoff/Targ protocol, in which viewers can respond verbally and graphically as much as they like within the time window for the experiment (usually 15 minutes), and therefore are likely to accidentally have more ‘correct’ data in their results which the judges then proceed to correctly match. This Karnes and Susman considered “especially damaging to a ‘remote viewing’ interpretation” (Karnes & Susman, 1979, 477).

But there are serious flaws in the Karnes/Susman ‘replication’ far beyond what one should expect in a true attempt at replicating a previously successful experimental paradigm. First, the “response bias” interpretation fails in a free-response form of remote viewing such as the SRI model. As a viewer’s responses accumulate in a trial in which no ESP effect is present, details that are only accidentally correct will be “snowed under” by the larger volume of incorrect data. So, assuming for the moment the absence of ESP in a trial, increased verbal and sketching responses are more likely only to inject added noise. This will tend to *decrease* the chances of a correct match instead of increase them, as Karnes/Susman conclude. This is so because the absence of signal and presence only of noise increases the likelihood that the data in some part would match *any* or even *every* target in a pool – in every case a judge would end up with merely random matches. (Puthoff & Targ, 1975; Smith, 2005)

Second, from the outset Karnes and Susman seem to have misunderstood and, hence, misconstrued the principle involved in the remote viewing protocol. They apparently assumed that remote viewing was a telepathic phenomenon, and inexplicably set up their experiment in such a way as to test for telepathy, and not for remote viewing

– which is a form of clairvoyance.²¹ As such, the Karnes/Susman experiment appears to set up a straw-man against remote viewing. Third, there is insufficient diversity in the target set. In a pool consisting only of architectural targets, there are too many isomorphisms and structural overlaps for satisfactory

disambiguation of subtle impressions. This alone could seriously diminish the statistical strength of a presumed effect.

Finally, though using the booklets of photos allowed for easy statistical evaluation, it raises a serious problem for successful outcome of a true remote viewing experiment. Contrary to what Karnes and Susman believed, it is in fact *not* reasonable to expect their booklet approach to be as successful as that used by SRI. The Puthoff/Targ approach of having the viewer sketch and describe perceptions of the target attempts to exploit human subconscious perceptual processes which most ESP research indicates constitute the primary mechanism for such phenomena, and to minimize conscious ratiocination and analysis which, when encouraged, injects copious noise into the process (Puthoff & Targ, 1975).

The Karnes/Susman approach of having the viewer select from a series of photos maximizes conscious analysis and limits the role of subconscious-level perception, from which the data should be expected to emerge. This has the potential to significantly increase the ratio of noise to signal.

A subsequent attempt at a replication of the SRI model by Karnes, et al (1979),

²¹I say “inexplicably,” since in the earlier remote viewing literature with which they claimed to be familiar the clairvoyant nature of the remote viewing task was

one that also seemed to indicate a failure of remote viewing, was only marginally better constructed. This protocol included some trappings of the actual remote viewing experimental model, in that rather than marking an answer sheet according to photos in a booklet, receivers were asked to provide free-form verbal and sketched impressions. Two trials were conducted, plus a control group was included. Trial 1 included ten receivers, who were introduced to the sender/beacon before the experiment, and a 10-member control group, who were not. Feedback was presented to both the experimental group and control group at the end of the trial. Trial 2 included 20 receivers, but apparently differed from Trial 1 in that participants received no feedback at the end of their sessions. (Unfortunately, the published description of this experiment is vague in a number of details – including precisely how the control group functioned and what its results indicated – and fails to describe the procedure for Trial 2 altogether.) A convoluted judging procedure involving 120 independent judges operating under four different conditions produced results that again reportedly showed null statistical results for the remote viewing effect.

Unfortunately, this attempted replication, too, was flawed. It still presumed to test a telepathic model. The senders were instructed to record impressions “of the target site that they were trying to convey to the receiver” (p.965), and to “concentrate on the target surroundings and to endeavor to convey telepathically” these impressions (p.967). As before, this sets up a scenario that may well be a test for telepathy, but not remote viewing. If such an experiment fails to show positive results (and if the judging and the

explicitly stated.

statistical analysis presented can be trusted, it did apparently fail), then it is telepathy, and not remote viewing that is unconfirmed.

Karnes *et al* (1981) performed one further failed ‘replication’ attempt, which was published in a non-peer-reviewed journal. A number of the same issues were apparent with this experiment as had been present in the two previous studies. This one, even more so than the others, for example, demonstrated a bias towards the telepathic model. Participants were divided into “sender/receiver” pairs; the “sender” was supposed to transmit “his/her impressions” (p.68). Subjects were selected by self-report “based on their previous successful experiences with paranormal communications” (p.69), and so on. There was no precedence for this in the SRI protocol.

Additionally, once again a large number of judges were used (64), but the judging process is not clearly explained in the published report. It seems that the 64 judges were divided among the resulting trials, with four judges independently judging each trial. The target pool was as before a mixture of indoor and outdoor structural features and, once again, there was significant overlap between several members of the target set, especially the indoor ones. Complexity of the experiment was increased by added variables being tested – e.g., feedback vs. no-feedback condition, presence or absence of a learning effect over multiple trials, etc. Because of the latter, some sets of participant pairs performed six trials, while some performed only two (the test for both conditions, unfortunately, was undermined by a further procedural requirement that sender/receiver pairs switch roles after each trial).

Perhaps the most problematic aspect of this experiment, though, was the choice of

participants. Instead of naive or previously-tested remote viewing subjects, the experimenters elected to use self-proclaimed psychics. They based this choice on the assumption that, “the psychic awareness of self-proclaimed psychics should be stronger than that of inexperienced subjects,” and felt it was “reasonable to assume that reliable remote-viewing should be more easily demonstrated with experienced psychic subjects than with inexperienced subjects.” They emphasized that these were “*self-proclaimed psychics*,” who “*claimed*” to have a strong track record in such arcane skills as “trance regressions, psychometry, psychic communications with one another, programmed dreaming, and psychokinesis” (pp. 68-69; my emphasis).

But there are three things wrong with this participant pool. The first is that, even if these subjects are capable in these alleged psychic skills as claimed, the skills differ significantly from what is expected in remote viewing. Second, the qualifications of the experimental recruits were accepted on their own claims – no preliminary testing or checking appears to have been done to verify that there was at least some substance to the claims being made. This means the experiment then devolves from testing for a remote viewing effect, to a generalized test of the claims of competence of this particular set of subjects. If the test fails, it does not necessarily mean that remote viewing has failed – only that these subjects’ assurances that they can perform the task were empty or misguided.²² The third problem is that a set of participants was selected with pre-

²²To be sure, in a more subtle way this is a feature of *any* test of a presumed human ability – that the subjects can indeed produce the behavior. But to select subjects based solely on the basis of their own claims that they can perform as requested ups the ante in this regard.

existing notions about how the phenomenon should be experienced and how the effect should be produced.²³ It is doubtful that the few preliminary instructions from the experimenters would be able to significantly change the internal habits and practices these subjects had habitually relied upon before.

All things considered, it seems this series of presumed replications fail to measure up to the requirements necessary to adequately falsify claims as to the reality of remote viewing.

Other attempted but failed replications and quasi-replications of the Puthoff/Targ model include Rauscher, *et al* (1976), Allen, *et al* (1976); and Solving, *et al* (1978). However, each of these showed noteworthy departures from the general Puthoff/Targ format, either due to design, or error. Rauscher, *et al* (involving a single viewer, eight trials, and five judges) for example, mismanaged the feedback portion of the experiment (which is presumed crucial to the Puthoff/Targ model), such that the viewer on at least some occasions either received no direct feedback or received it out of sequence – which could lead to conflation of results or viewer confusion. Further, the experimenters themselves acknowledged that the target set was insufficiently diverse, which because of too much similarity between targets may increase the difficulty of the discrimination task for the judges, resulting in lower statistical scores. Still, despite the statistical non-

²³Indeed, Puthoff and Targ were already on record that a reason for the remote viewing research protocol was to bypass “occult” and other metaphysical theories about ESP that not only reflected a non-scientific attitude, but tended to get in the way of accurate results (Targ & Puthoff, 1977, 6; Puthoff & Targ, 1975; Puthoff & Targ, 1976,

significance of the evaluation, the experimenters were impressed that “only a high degree of coincidence could account for some of the correlations between the actual target and subject’s descriptions” (p.43).

Allen, *et al*, involved a three-person team, the members of which rotated duties between ‘receiver,’ beacon, and experimenter, for a total of 12 trials, which was a significant departure from the Puthoff/Targ model. (The fact that the remote viewers were called ‘receivers’ in this experiment suggested that the researchers were also laboring under the mistaken notion that remote viewing is premised on telepathy.) Further, the receivers were given feedback only from the beacon person upon his or her return, and then only in verbal form – the receivers did not visit the target sites post-experiment, nor were they shown photographs of the location as was the case in successful replications. Finally, the three blind judges were allowed to use differing judging methods according to choice – one judge visited sites and matched transcripts to the targets using a sum-of-ranks method, whereas the two other judges used a 12-item rating sheet when they visited targets, which allowed a degree of quantification.

Solfvin, *et al* was an attempt, among other things, to see if prior meditation training would affect remote viewing results. After preliminary meditation training, the session began with a beacon person being randomly selected from the participant pool and sent to a target. The group meditated for 20 minutes, then tried “to experience what the agent was experiencing” for five minutes (once again suggesting a bias towards telepathy). The participants were then asked to self-judge which target the beacon person

330; Smith, 2005, 94-98).

had visited by choosing among randomized photos and descriptions of four possible targets in the given set. Scoring was by majority vote. There is no indication of how many participated other than that there were seven target sets of four targets each, and no statistical analysis of the remote viewing portion of the experiment are included in the published report other than to indicate results were non-significant.

These latter attempted replications and the three involving Karnes are often cited as persuasive evidence against remote viewing, but as can be seen, their value in that regard is at best questionable. In the next chapter I will discuss what is considered by many to be the most serious challenge.

Chapter 8: Remote Viewing (continued)

In the preceding chapter, after describing the early research supporting the existence of the ESP-based remote viewing phenomenon I considered some of the failed replications and quasi-replications that have often been cited as counter-evidence to the RV effect. A more important response to the early remote viewing work is that of Marks and Kamman (1978 and 1980/2000). Their response consists of two parts: A series of failed attempted replications, and an analysis of the published claims of successful remote viewing studies – primarily that of Puthoff and Targ presented in the IEEE paper – for unreported or undiscovered flaws. I will consider the Marks/Kamman replication effort first.

Between 1976 and 1978 the two psychologists conducted 35 remote viewing trials gathered into five experiments. They claimed to have followed the Puthoff/Targ model explicitly, and in their 1980 book report their findings and draw up the general outlines of a beacon-style experiment, mentioning the random selection of the target, and giving brief summaries of what both the viewer and the outbound team were expected to do. Yet after evaluation by multiple blind judges they reported that

None-repeat, none-of the results was statistically significant. In not a single case did a judge do better than chance at ranking the transcripts – a total of twenty sets of judgments and not a single significant result. (Marks & Kamman, 1980, 19-20 – emphasis in the original).

Their failed replication and associated analysis have stood for years as the quintessential falsification of the remote viewing model, and are frequently and

ubiquitously cited in critical works on the subject of ESP. Their conclusions deserve closer scrutiny.

How explicitly did they follow the standard protocol? Based on what they have published, that is difficult to tell. Though we do have the general outlines of the experiment, there are many important details left out. For example, though they specify that each target used was randomly selected, there is no mention of blinding conditions. Additionally, except in one case there is no discussion of target pool size or composition, target selection criteria, target-pool management, nor target-pool security. In most successful remote viewing experiments, all these details are offered. Marks and Kamman specify the number of judges and how the transcripts were checked for ancillary cues or clues. There is a hint that sum-of-ranks was used to evaluate the viewer transcripts against the various targets (transcripts were “rank ordered” by the judges “for appropriate fitness to the location” – p. 19). But there is no mention further of statistical analyses, nor is there a display of results beyond the fact that “*none-repeat, none*” were statistically significant.¹

It might be argued that, given the non-significant results produced, it was not essential for such details to be presented when it would be necessary in case a significant effect was being claimed. However, this would be a mistake. In a case where attempted replications are used to refute claims for the existence of a certain phenomenon such as, in this case, remote viewing, it is essential for the researchers arguing against the

¹This is ironic, since Marks and Kamman chastised remote viewing researchers for flawed or incomplete reporting.

phenomenon to make their procedures and analyses just as transparent as that which is expected of researchers who argue for it. Otherwise, it is impossible to tell whether the researchers producing negative results might not in some way, intentionally or otherwise, be biasing their results against the phenomenon being tested.

In the one experiment for which they do provide more details, the final one, there are some hints as to procedure, but also some suggestion of possible flaws in their own approach. First, there is an indication that Marks and Kamman made the same error as Karnes and others, in assuming that remote viewing was telepathic in nature (“...everybody concerned seemed to believe that Chris and Sally had communicated at an extrasensory level...” [p.39]). Further, there is a suggestion there may be some issues with the judging procedure. For this final experiment (seven trials, one viewer), there was only one judge, when the previous experiments had all had five judges each. The experimenters expressed great satisfaction with their choice for judge, Pavel Tichy.² Tichy was an accomplished logician and they seemed to consider this a high qualification for judging remote viewing results.

However, whatever Tichy’s other accomplishments (and they were noteworthy), a logician is likely to be a poor choice for a remote viewing judge. The reason is this: logicians tend to be analytical and linear in their approach to problem-solving. But what is called for in remote viewing judging is more often an holistic approach, with a propensity for global thinking and pattern recognition. Of course, now that many years

²“If anybody was motivated and capable of making accurate matches, Pavel Tichy was” (Marks & Kamman, 1980, 23).

have gone since his death, there is no way to tell whether Dr. Tichy possessed these qualities as well. But more promising choices for judge would have been a studio-art instructor, visual artist, or an accomplished photographer.

It is also difficult to tell anymore whether Tichy's judging work was in any way inferior to what might otherwise be expected, since as far as could be determined, Marks and Kamman never made public any of the transcripts and other raw data. However, there are some indications of judging errors. Notably, on what arguably might otherwise have been the easiest and best match of the series, the judge violated a cardinal rule of remote viewing evaluation by selecting a viewer response on the basis not of how the target itself looked or was composed as compared to the transcript and sketches. He instead chose a different transcript based on the fact that the accompanying sketch closely matched a distant scene that could only be seen by looking directly *away* from the designated target. But one should judge a transcript/target match based *only on the intended target and its immediate vicinity*, not on anything that might be seen at a distance or in a different direction. (Targ, 1994; Puthoff & Targ, 1978.) One cannot say that Tichy or the other judges in case of the preceding 28 trials did not otherwise follow sound judging procedures. But the fact that the one instance that Marks and Kamman chose to illustrate a judge's performance in the series turns out to be flawed raises at least some reason for concern about the rest of the judging.

The Marks and Kamman remote viewing replication series may or may not be a good example of a failed replication of the remote viewing research model. Given the relative paucity of details about their process, though, and a few tantalizing hints that

point to possible flaws in their own work, whether the Marks and Kamman replications gain any ground against remote viewing claims is at best inconclusive.

Protocol Artifacts

A far more important objection lodged by Marks and Kamman against the early remote viewing work appeared first in *Nature* (Marks & Kamman, 1978), to be further elaborated in their later book (Marks & Kamman, 1980; Marks, 2000). This work was intended as a direct rebuttal to the Puthoff/Targ experiments published in *Nature* (1974) and the IEEE journal (1976). The essence of the Marks/Kamman argument was that the raw data transcripts of Pat Price's nine remote viewing trials contained enough extraneous comments and cues that judges could produce statistically-significant rankings even in the absence of any ESP effect whatsoever. Marks and Kamman suspected that comments in the body of the transcript reflecting banter between the interviewer and viewer such as those mentioning target locations visited in earlier trials in the series, and side-comments about feelings after a certain number of experiences, and so on, could make it possible to accurately rank-order the transcripts without needing to even consider the content of the purported remote viewing results themselves.

After a drawn-out and clearly irksome process, in which Puthoff and Targ allegedly alternately refused access to the original materials or failed to deliver them as promised, Marks and Kamman acquired the transcripts from a third party and did their own examination, finding indeed the clues they had expected to discover. Eventually, they arranged two separate rejudgings of their own. The first was performed in the actual

target locations in California by two research psychologists who compared five of the nine transcripts (with ancillary cues and comments expurgated) to the target sites themselves, producing non-significant results, as Marks and Kamman had suspected would be the case if there were no ancillary cues to help. In the second rejudging process, which Marks and Kamman called “remote judging,” the eight individuals involved rank-ordered the transcripts (with ancillary comments and clues included) without ever visiting the actual target locations, ostensibly relying exclusively on the clues contained on the unedited transcripts. All eight produced highly significant rankings, three of which were as good or better than that of the one SRI judge for whom Marks and Kamman had individual results. A similar counter-experiment was done with six of the nine transcripts from the first Hella Hammid series that had also been reported in the 1976 IEEE paper, and which allegedly also contained extraneous cues. (Marks & Kamman, 1980, 27-35)

This indeed posed a serious challenge to the credibility of at least the earliest Puthoff & Targ results, and by extension other of their studies for which less raw data was publicly available. The SRI researchers were victims of their own attempts to anticipate critics’ objections. Apparently concerned that if any alterations were made to the transcripts they would be accused of selectively reporting evidence, Puthoff and Targ had taken care to pass *unedited* transcripts to the judges (Puthoff & Targ, 1976, 335). What may thus have been intended as an attempt at scientific openness turned out to be a pitfall, and Marks’ and Kamman’s criticism seems persuasive.

However, though clearly valid, the Marks/Kamman critique turns out not to be as

powerful as public wisdom has come to believe. There are reasons to think that, while these first Price and Hammid studies are flawed enough that the very high significance value may be less trustworthy, the results nonetheless do demonstrate an unambiguous remote viewing effect. The first and most important of these reasons is the attempts made by Puthoff and Targ, and a colleague, Charles T. Tart, to examine the impact of the Marks/Kamman criticism. Tart, who was not affiliated with the original research, engaged another independent judge to carry out a rejudging. After ascertaining that the new judge had no prior exposure to any of the Price results, Tart provided her with all nine transcripts, not only expurgated of suggestive ancillary cues and comments, but also fully randomized and accompanied by a randomized list of the target sites (Tart, *et al*, 1980, 191). The judge visited each target site and rank-ordered every transcript from 1 to 9, matching 7 of the 9 correctly, yielding a sum-of-ranks statistical analysis significant at $p < 10^{-4}$. A further analysis, which Tart described as “an exact factorial method,” yielded significance at $p = 2.2 \times 10^{-5}$. Furthermore, the judge reported that, “with the exception of two transcripts that did not seem to correspond to any site, the remaining seven transcripts each showed high correlation to one (correct) site and low correlation to the others.”

By this time, parts or all of four of the best transcripts of the nine had been published in various books and science papers to illustrate some of the striking matches that had accrued in the Price series. Marks (1981, 177) objected that “it is not permissible to include material for re-judging which has already been published or which may be available in some other form.” Mark’s concern was that the judge might

subliminally recall the correct matching order from a report he or she had read or heard, or the judge could intentionally look up the previous material to guarantee a good result. To avoid these two possibilities, Marks felt that only the five un-published transcripts should have been used in any rejudging. The possibility of the judge experiencing subliminal recall of something she had seen or read is perhaps conceivable – if improbable. Cheating is more feasible, at least in principle. But Tart’s conscientious effort to assure the judge’s independence and integrity seriously vitiates Mark’s objection on this count.

It is also important to consider the nature of the transcripts that Marks wanted excluded. All four of them were *prima facie* matches for the targets they were first-place rankings for. By “*prima facie* match” I mean a viewer’s description captures the target location so well and bears so little resemblance to any other target in the pool that there is no question as to which target it describes. Importantly, the target pool selected for these experiments by otherwise uninvolved SRI officials were well chosen for heterogeneity and orthogonality, as can be verified in Puthoff and Targ (1975), (1976), and (1977). (This is in contrast to the often overly-homogenous target sets selected for some of the failed replications, as discussed above.) Brief summaries of the four transcripts in question are as follows:

Target 1 – *Hoover Tower*. Price’s response: “The area – I have a place – it seems like it would be Hoover Tower” (Tart, *et al*, 191).

Target 4 – *Redwood City boat marina*. Price’s response: “a little boat jetty or little boat dock along the bay...I see the little boats, some motor launches, some little sailing ships, sails all furled, some with the masts stepped and others are up.

Little jetty or little dock there” (Puthoff & Targ, 1977, 50-51).

Target 7 – *Allied Arts Plaza* (an arts and crafts plaza featuring extensive floral beds and shrubbery, surrounded by Mission adobe style architecture and many small shops and kiosks.) Price’s perceptions included numerous references to flowers, redwood arbors, a marketplace atmosphere, pathways, decorative ponds and a fountain, etc. The description was “accurate in almost every detail,” as can be verified by the complete transcript, in Puthoff & Targ (1977, 52 & 63-68).

Target 9 – Rinconada recreational swimming complex: Price “said that he saw a circular pool of water, about a hundred feet in diameter (it was actually hundred and ten feet in diameter). He also saw a rectangular pool about 60 by 80 feet on a side (this pool happens to be 75 by 100 feet). He went on to describe a concrete block house, which is also at the site.” Price also sketched a quite accurate diagrammatic representation of the complex, including both pools and the blockhouse (though with a left-to-right reversal, and including features not present at the time of the viewing). (Targ, 1996)

There is a certain logic to Marks’ and Kamman’s point about excluding previously-published material from a rejudging. But on the other hand, it doesn’t seem to be an essential requirement if the judge’s blindness can be reasonably assured, as Tart did. More significantly, Price’s descriptions are of such high quality for these four sessions that neither ancillary clues, unconscious memory recall, nor even cheating were necessary for judges to rank them as first round hits.³

In a subsequent letter to *Nature* in 1986, Marks lodged a further objection. He had by then acquired from Puthoff copies of not only the nine original, unexpurgated transcripts but also of Tart’s expurgated re-judging ones. Marks expressed dissatisfaction with how the ancillary clues had been redacted. For instance, he was concerned that one phrase was still included that *might* have cued a judge as to the proper

³It does raise some question as to Mark’s and Kamman’s other motivations for

match for the first transcript (“the feeling that one can’t or won’t be able to do it, and shouldn’t even try it”). Marks implied that the phrase expressed a hesitancy in the face of an initial session of the new experiment, hinting this was the first trial. However, even if a judge had tried to leverage this “clue” into a correct choice, the issue is moot, as the target for the first trial was the Hoover Tower which, as we have seen, was an unambiguous *prima facie* match, for which a clue would have been superfluous. The few other problems Marks cites seem equally unsupportable.

Finally,⁴ in a further rebuttal to the Marks/Kamman allegations, Puthoff and Targ (Targ, *et al*, 1981, 24-25) did a statistical re-analysis taking into account the cues present in the raw transcripts. In their calculations they assumed that the cues would be used “to maximum advantage.” But their analysis indicated that even by the most conservative statistic, “non-independent assignment of transcripts to target sites” decreased the significance to $p = 3.9 \times 10^{-4}$ (as compared to the original figure of $p = 2.9 \times 10^{-5}$), which Puthoff and Targ deemed “still a quite significant result.”

Though seriously flawed, the Marks/Kamman analysis was an important contribution to remote viewing research. Even before this objection became known some experimenters had recognized the problem and were taking care to check their raw data for inadvertent clues prior to judging. But once the Marks/Kamman critique was

wanting them excluded.

⁴I say “finally” here in the interest of space and time – though there are other points on which Marks and Kamman could be attacked.

published, those safeguards quickly became standard procedure for all responsibly done remote viewing research.

The Marks/Kamman criticisms miss the mark as a way of accounting for successful remote viewing results for the following reasons:

- 1) A re-analysis by a judge unaffiliated with the previous experiment using transcripts with all potential cues removed still produced highly significant results.
- 2) A statistical re-analysis by Puthoff and Targ which took all possible cues and embedded suggestions into consideration also continued to yield highly significant results.
- 3) Marks and Kamman failed to adequately perform their own rejudging of the Price material. They claim all rejudgings were done “utilizing only the clues” on the transcripts (Marks, 2000,49; Marks & Kamman, 1980, 30). For this to have occurred, *all* data would necessarily have to be expunged from the transcripts *except* for the alleged cues, and the judging would have to commence based on this minimalist data set. Otherwise, there is no way to guarantee that the judges do not rely on possible ESP-derived content in the test of each transcript to aid with the matching. Marks and Kamman do not mention removing all data and leaving only the clues, nor is there any indication such measures were taken.
- 4) Further experiments published not long after the nine-trial Pat Price and Hella Hammid sets did not have, and in some cases due to design could not have had the flaw found in the Price set. Subsequent experiments done at the SRI laboratory incorporating precautions recommended by Marks and Kamman continued to produce significant- to highly-significant results.
- 5) Successful replications done by experimenters other than those at SRI continued to produce significant and highly-significant results while taking the precautions recommended by Marks and Kamman.

When the first edition of Marks’ and Kamman’s *Psychology of the Psychic* was published in 1980, there may have been some reason to question the original remote viewing research and replications, since there was still only a relatively small number of

trials (certainly not yet even 200) available in only a few publicly accessible studies. However, by the time Marks published the second edition of the book in 2000 (some years after Kamman's death), there was much less justification – and justification has grown even less in the intervening years since that time. I will here briefly turn to the much larger body of remote viewing material published post-1980.

Precognitive Remote Perception

After Bisaha and Dunne's series of successful remote viewing experiments in the 1970s, Brenda Dunne moved to Princeton University where she joined Robert Jahn, a prominent physicist and dean of Princeton's School of Engineering and Applied Science. Jahn was founder and head of the Princeton Engineering Anomalies Research (PEAR) laboratory. Dunne's earlier precognitive remote perception (PRP) experiments lead to a long research program at PEAR based on the preliminary SRI-inspired precognitive remote viewing model. Jahn and Dunne preferred the "remote perception" terminology over the more widely-used "remote viewing" because they felt it more accurately captured the phenomenology encountered by participants, which is generally acknowledged to include more sensory modalities than just visual experience.

With some refinements, PEAR's PRP protocol remained the same in principle as that sketched in the Puthoff/Targ IEEE paper as further elaborated by Dunne and Bisaha. One of the advantages of the precognitive aspect of the paradigm was that it eliminated some of the sources for potential error and sensory cuing. Since at the time a PRP trial was conducted *no one* knew what the target was to be, it would be impossible for sensory

leakage to the perceiver ('viewer') to occur.

Further contributions of the PEAR PRP research program involved improvements in judging, evaluation, and statistical analysis of trials results (Nelson, *et al*, 1996).

There had always been issues with the subjective nature of the judging techniques used in remote viewing research. But due to the experiential content of remote viewing/perception results, there was no substitute for at least some level of evaluation by human judges who could identify fine distinctions in the raw data, could assess approximate quality of fit between the candidate target and a percipient's perceptions, and who could draw conclusions from non-quantifiable results such as drawings and sketches.

The motivation behind provisions for blind judging, randomization protocols, and such statistical tools as sum-of-ranks and direct-count-of-permutations was to convert subjective, qualitative evaluations performed by human judges into quantitative measures that could not only be more easily compared to results from other similar research, but also allowed objective evaluations that were buffered from well-known weaknesses such as confirmation bias in human-based assessments. This came at a cost of a loss of sensitivity to the quality of the data. A transcript merely had to be a better match than any of the others to be ranked first against the correct target. Whether it was only a marginally better description or a high-quality, near-photographic match (as a certain percentage of trials produce), could not be taken into account. Still, it was a relatively small price to pay for allowing subjective interpretations to be transformed into objective measures.

Recognizing some of the problems with the standard ways of assessing remote viewing/perception results, PEAR experimented with a variety of statistical scoring systems before settling on five binary systems that were largely based on an evaluation tool of 30 binary descriptive queries that provided a more fine-grained way of turning subjective judgments into quantified data (Dunne & Jahn, 2003, 213). Examples of the elements that could be assessed included outdoor vs. indoor, water present or absent, whether the setting was noisy or quite, expansive or confined, and so on (p.211). Binary parsing such as this reduced subjective interpretation to a minimum, since whether (for example) water is present or absent at a location is an objective matter requiring little subjective reflection by a human perceiver to arrive at a correct determination. The process has a further virtue in that it allows easy machine-based processing. A large number of targets can be canvassed in advance of any experimentation and coded with the 30-element questionnaire, to arrive at a fairly unique profile for each site. Then, once a trial has been evaluated using the same 30 elements, this can be machine-scored against the target set and objectively compared and ranked against the possibilities. According to Dunne and Jahn, “this descriptor-based process had the advantages that such ranking could proceed on a more standardized analytical basis and that many more alternative targets could be ranked by the computer than by a human judge” (p.211).

Interestingly, each of the five binary scoring systems explored by PEAR, when evaluated against one another and against the original sum-of-ranks approach, shows largely consistent cross-system results. In other words, though they do produce some differences in final outcome of their statistical analyses of the same data sets, these

outcomes, system to system, are close enough to being the same. This is important because it constitutes a confirmatory “cross-check” verifying the soundness of the approach and the accuracy of the evaluations over all. It also allowed the PEAR lab to use the least complex analytic procedure that produces sufficiently consistent results.

Employing a comparatively sizable number of assessment variables has the further benefit of recapturing some of the nuance present in the perceiver’s subjective experience. But it has had a somewhat unexpected downside, as well, in that scores have tended to become less robust as perceivers, aware of the specific data expected in the binary query set, direct their attention to those elements (p.215). This moves the perceiver away from the intuitive mental state most amenable to perception-at-a-distance and into a more consciously analytical mode, which experience has shown tends to attenuate veridical perceptions. (p.230)

Nonetheless, the PEAR precognitive remote perception program has over two decades produced overall robust results. During its life, the program has amassed nearly 1,000 PRP trials. Of these, 653 were considered formal trials suitable for statistical evaluation.⁵ Statistical analysis demonstrated results very highly significant at $p = 3 \times 10^{-8}$ (p. 207).

Classified Department of Defense Studies

⁵The balance of 300-some-odd ‘informal’ trials were largely exploratory in nature, or not as tightly controlled, and therefore did not reach the PEAR standards for inclusion in the overall statistical evaluation, though their cumulative success ratios would have enhanced PEAR’s results.

The military-funded SRI-International program continued uninterrupted throughout this entire era. Because of the classified nature of the program, most of the research was not openly published. But some occasionally was, notably a contribution to a symposium of the American Association for the Advancement of Science (AAAS) in 1979. (I will discuss most of this set of experiments in chapter 10). The article that was subsequently published (Puthoff, *et al*, 1981) in the symposium proceedings mentioned the completion of “more than 100 experiments in the remote viewing of targets ranging from objects in nearby light-tight canisters to geographic sites at transcontinental distances, viewed from locations which include shielded Faraday cages and a submerged submarine” (p.37).

Nine years after the AAAS symposium, the classified program (now under nuclear physicist Dr. Edwin May – Targ had departed in 1982, and Puthoff in 1985) was directed by the Defense Intelligence Agency to publish a comprehensive review and summary of psychoenergetic⁶ research produced by the SRI laboratory from its official start on 1 October 1973 through 30 September 1988, the end of the Fiscal Year. The entire research effort over the program’s 15-year history amassed 25,449 trials conducted under a number of different protocols.⁷ According to May,

⁶‘Psychoenergetics’ was a term borrowed from Soviet usage to denote the entire category of ostensible mental events typically attributed to ESP, psychokinesis, and related phenomena.

⁷This number only includes the work done specifically at SRI-International and does not include several thousand more operational and informal experimental trials performed by the active duty military arm of the program at Fort George G. Meade, MD and at the Army Materiel Systems Analysis Activity (AMSAA) at Aberdeen Proving Grounds, MD.

analysis indicates that the odds that our results are not due to simple statistical fluctuations alone are better than 2×10^{20} to 1 (i.e., 2 followed by 20 zeros). Using accepted criteria set forth in the standard behavioral sciences, we conclude that this constitutes convincing, if not conclusive evidence for the existence of psychoenergetic functioning. (May, *et al*, 1989, 2)

The nearly 26,000 trials were conducted within 154 experiments involving 227 individual participants (p. 5).

Of these overall trials, 24,440 were evaluated remote viewing trials, 19,675 of which were relatively short-duration forced-choice type trials aimed at trying to use RV to reliably obtain alpha-numeric type information. These yielded statistical significance at $p = 6.12 \times 10^{-14}$ (but only a small effect size, as the task proved to be only very marginally successful). The 3,790 “search” trials (attempts to use RV to indicate hidden locations of objects or persons) were less significant ($p = 4.53 \times 10^{-3}$) but had an also relatively small effect size. However, the standard laboratory remote viewing trials ($n = 966$) demonstrated a robust effect size, and were very highly significant at $p = 4.33 \times 10^{-11}$.⁸ This was particularly the case for the subset of six experienced viewers who had long been affiliated with the SRI program (some of them retired military remote viewers). Their subset of 196 remote viewing trials showed a strong effect size and yielded significance at $p = 3.49 \times 10^{-8}$. (May, *et al*, 1989, 13) The clandestine program continued to add to these results, and I will discuss some of these later in chapter 10.

Some of the later SRI data emerged in the peer-reviewed parapsychology

⁸Nine operational intelligence-collection RV sessions (out of 106) were also conducted under sufficiently controlled circumstances as to be scientifically-admissible and statistically-analyzable. These nine were significant at $p = 3.45 \times 10^{-5}$, with a very strong effect size.

journals. One example of this was a 1994 article by Russell Targ reporting on a series of trials used to test a new concept-analysis judging tool which allowed an even finer-grained parsing of viewer response content versus target details. Six inexperienced remote viewers each worked with an interviewer under double blind conditions and following standard beacon protocols to perform six sessions each once a week. Four of the six produced results that were highly significant at $p < 8 \times 10^{-5}$, with a large effect size.

Besides the already-discussed PRP work at the PEAR laboratory, Targ and a varying assemblage of associates continued to publish results into the present century, as did May and some of his joint researchers, by 1990 now moved from SRI to another government contractor, Science Applications International Corporation (SAIC). As mentioned, some of this material will be discussed later, but some of will be considered below, in a brief discussion of the last of bodies of evidence to be presented against the claim of Universal Physicalism.

ASSOCIATIVE REMOTE VIEWING (ARV)

Associative remote viewing attempts to retro-temporally convey the outcome of a future event to a perceiver in the present. As such, it is a targeting methodology, rather than a specific way of doing remote viewing (RV) itself (any RV method that produces reasonably accurate results may be employed in the ARV process). Once again, it remains undetermined whether a retro-causative effect or a precognitive element is at play. Early on in the government remote viewing program researchers found that merely

asking a viewer to predict a future event with two possible outcomes would produce only chance results (Puthoff, 2009; Targ & Harary, 1985, 82). One hypothesis suggested that the inference-producing role dominated by conscious, left-brain-hemisphere processes served to confuse the viewer's perceptions, resulting in non-significant results.

But an alternative was available. As noted above, the remote viewing protocol is intended to allow a viewer to produce often high-quality verbal and sketched descriptions of targets otherwise inaccessible to normal perception before and during the time of the viewing. Available data suggested that a viewer could discern a target that would be presented some time in the future. A protocol was developed to take advantage of this. If two targets were associated with the two possible outcomes of a future binary event, then perhaps the viewer would describe with sufficient accuracy the target associated with the correct outcome of the event as it was realized in the future. This, then, would lead to drawing the correct conclusion as to which outcome would be realized in the future. A simple ARV protocol proceeds as follows:

- 1) A person (often referred to as the "tasker") selects a target pool of two objects. These objects must be as orthogonal as possible from each other (for example, a pencil and an apple) so that even poor quality remote viewing results can distinguish the correct target from the decoy. The viewer *must* be *kept fully blind* to both targets until after conclusion of the event to be predicted.
- 2) A future event with at least two mutually-exclusive possible outcomes is chosen for prediction. For practical reasons the selected event will usually have a binary set of possible outcomes, though events with multiple outcomes can also be managed with ARV. Representative types of events include team sports contests; stock or currency values (up vs. down); and so on. The viewer may (but does not need to) know what event is to be predicted.
- 3) The tasker assigns one of the targets to each of the outcomes. The apple might be associated for example with the Texas Longhorns winning a football game,

while the pencil could represent the A&M Aggies winning instead.

4) The tasker and the viewer agree on a future time *after* the realized outcome of the event for the viewer to be presented with the feedback object associated with the outcome that will actually be realized. The viewer will usually only be shown the target associated with the correct outcome, and *not* be shown the target which was associated with the outcome that *failed* to be realized.

5) Using his or her preferred remote viewing method, the viewer produces a transcript of verbal and sketched responses describing as clearly as possible the impressions that are received of the appearance and configurational aspects of the object which will later be shown at the appropriate time as feedback. Once completed, the viewer transmits the transcript to the judge sufficiently prior to the event to allow a careful judging, and execution of indicated action.

6) The judge is given access to the two objects and compares the viewer's transcript to each. The judge decides which target is best matched by the viewer's transcript.

7) The selected match represents the predicted outcome of the experiment. If, for example, the viewer's description is "red, round, sweet smelling," and he or she sketches a roughly spherical shape, the judge will likely conclude that the apple is a better match than the pencil and, therefore, the Longhorns will win the football game. [Since this result is produced *before* the event has reached a conclusion, action could in principle be undertaken, such as bets placed in the case of a sporting event, or stocks bought (or sold) prior to market close. In a commercial enterprise such action might actually be taken; in an experimental context the events are merely used as an otherwise non-predictable outcome.]

8) At the appointed time after the event has taken place and an outcome decided, the tasker presents the viewer with the *correct* object to complete the feedback loop and confirm to the viewer the intended target.

The protocol is resistant to fraud. Since the outcome of the ESP-component of the experiment must be decided and declared *in advance* of the culmination of the event, it becomes hard for an unprincipled researcher to fraudulently alter results. The actual outcome of the event is in most cases a matter of public record, and thus also cannot be tinkered with.

There are several conflating factors that can degrade the ARV results (but none known that can artificially enhance them). The first of these is called “displacement,” the tendency of viewers to sometimes perceive and describe the *wrong* target, or even parts of both targets. Because of this, some ARV applications projects (e.g., those investing large sums of money) only act on results that are sufficiently unambiguous, and will take a “pass” on any that do not meet a certain threshold (Targ, *et al*, 1995).

Another conflating factor is the judging process itself. The judge is expected to employ standard (that is, non-ESP) faculties in analyzing and comparing the viewer’s transcript to the possible targets. But different judges have different levels of discriminatory, pattern-recognition, and analytical abilities. Thus a less-competent judge may produce weaker results than one who is proficient in the task, without the underlying ESP effect produced by the viewer being any stronger or weaker. Quality of judging thus acts as a constraint on reported results⁹ which at best may present a slight degradation to the strength of results and at worst a serious decrement.

A further potential conflating factor is error in experimental set-up, including poor selection and matching of target sets. If insufficient attention is paid to the qualities of individual targets and, further, to which targets are matched together, results can be seriously affected. For example, if a mounted snarling wolf’s head is used as one target, and a soup strainer used for the other, viewers may be subconsciously repelled from

⁹It is conceivable, but hard to verify, that some judges might exercise some ESP ability of their own, and thus enhance results – though this would seem to be relatively

describing the frightening visage of a wolf, even if it turns out to be the correct target.

These issues illustrate the factors that may affect the results of ARV experiments. On the other hand, they also suggest that results produced in ARV experiments are likely to be conservative measures of ARV experimental outcomes. Thus, when positive results are produced, we can have added (if hard to quantify) reassurance that an ESP effect is indeed manifest.

Evaluating ARV Experiments

There are two ways to evaluate the outcome of an ARV experiment. One can qualitatively examine the prima facie evidence for indications of a success (or failure). Or one can consider the judging results in terms of hit/miss in predicting the outcomes of the selected events and analyze them statistically.

The qualitative, subjective approach, is essential to providing the raw material for the judging process which produces quantitative results. However, it is in itself not crucially helpful from the perspective of an overall scientific evaluation, nor useful for comparative analysis across sets of experiments of the same type and order. However, prima facie examination can be valuable in improving experimental protocols, increasing understanding of human factors affecting the experiment and providing data to inform decisions as to action to be taken.

Still, the best and most assessable evidence from a scientific standpoint would seem to come from the quantifiable approach, which involves a judge analyzing the ARV

unobjectionable from a research standpoint.

transcript and making a decision as to which of the two targets in the pool it best matches, after which the judge's choice is either justified or invalidated by the subsequent event over which he or she has no control. In a sense, it is a way of converting the subjectivity inherent in the remote viewing evaluation process into an objective measure that can be statistically analyzed. This latter approach is the one I shall rely on in presenting ARV results (with some recourse to actual example ARV results for illustrative purposes).

Survey of Results

Though relatively unexplored as a psi methodology, ARV has already generated impressive results among a small group of professional and amateur researchers. I will present a full summary of those I have been able to locate that are sufficiently well-attested.

In three pilot studies in which I myself participated, the results were as follows:

Experiment A (3/5/2002 - 6/5/2002): 19 Attempts, yielding 14 correct, 3 incorrect, and 2 passes.¹⁰ Statistical analysis yields a p value of $p = 0.006$, or odds against chance of 1 in 167 (statistically significant).

Experiment B (8/3/2002 - 12/2/2002): 39 Attempts, yielding 22 correct, 15 incorrect and 2 passes. $P = 0.16$, yielding odds against chance of approximately one in six, which is not statistically significant.

Experiment C (10/12/2002 - 12/5/2002)¹¹: 14 Attempts, 8 correct, 3 incorrect, and

¹⁰A pass occurs when the results are so ambiguous that no decision can be made with any level of confidence. For statistical purposes, passes are not counted, and totals are adjusted accordingly.

¹¹Experiments B & C were run concurrently, with different (and unaffiliated) researcher/taskers, but the same viewer.

3 passes. Results yielded $P = 0.11$, or odds against chance of one in nine (also not statistically significant).

All three experiments taken together yield a highly statistically significant result of $p = .003$, or one chance in 333 that the results were produced by chance. Experiment B produced the least-significant results, though they were still noticeably positive in the same direction as were the other two experiments. The researcher in experiment B used a different approach than was used in the other two experiments (in which researchers personally selected and paired the target photos). In this case Researcher B loaded a large set of target photos into a computer, which randomly selected two-photo sets for each trial. This meant that several photo-pairings were sub-optimal, producing either pairs that were too similar or that were seriously unbalanced in terms of interest levels between targets (e.g., interesting vs. bland in the same target set). As a consequence, several of the remote viewing transcripts and, hence, the judging decisions were less than optimal as well, showing noticeable displacement between targets. There were nonetheless a number of high-quality results and unambiguous “hits” even in this set of trials.

ARV is less formally attested than any of the four research paradigms I am discussing in detail here, largely because it has not been widely discussed in the literature. As far as I have been able to determine, there are only a small number of published studies in peer-reviewed publications (though more are in the works as I write). However, there has been a comparatively large body of carefully-done informal and pilot studies whose results have been made available to the public, and taken together these

turn out to be quite persuasive.¹²

Some early associative remote viewing experiments and their results are as follows:

The first known associative remote viewing experiment occurred in 1977, as reported in Puthoff, Targ, & May (1981; also, Puthoff 2009; Targ, 1984; Smith, 2005). In this experiment two separate remote viewing attempts were made from the *Taurus*, a deep-submersible research vessel. With a viewer aboard, the *Taurus* submerged approximately 500 miles from the intended targets – which in this case were locations, one for each viewer, respectively, to which two researchers acting as ‘beacons’ had been directed via a random selection process out of a target pool of six possible target locations. Viewers were blind to the entire target pool.

The participating remote viewers (one male and one female) worked double blind while in the *Taurus*, one while at the 170 m. level (558 ft.) in 340 m. (1,115 ft.) deep water. The other viewed while the submersible rested on the bottom in 78 m. (256 ft.) of water.¹³ At the conclusion of the viewing, each viewer was given access to a sealed envelope containing photos of each of the six possible target sites. The viewer was required to match his or her results to one of the six photos. On the back of each photo

¹²Thanks to the structure of the ARV process (as mentioned above), successful results produced by individual researchers are more trustworthy than is perhaps true of other experimental paradigms, since lapses in protocol are most likely to actually degrade reported results and are highly unlikely to enhance them artificially.

¹³The primary purpose of the experiment was to determine if an environment shielded from electromagnetic influences would promote or degrade remote viewing performance. The ARV aspect of it was a secondary experiment.

(hence “associated” with the target) was a different message that might be sent to a naval submarine on patrol, such as “Surface for radio contact” or “Return to port.” Both viewers correctly matched their results to their respective correct target photo (and, hence, to the correct message).¹⁴

In 1980 two informal ARV trials were attempted in the offices of the Radio Physics Laboratory of SRI-International (Targ & Harary, 1984). Each used a group of five different possible targets, and both trials successfully identified the correct target. In 1982 Puthoff (1984, 2009) produced 127 correct out of 202 trials, yielding a highly significant result of $p < 1.6 \times 10^{-4}$. A consensus analysis honed this further for applications.¹⁵ Also in 1982, Targ and Harary (1984; and Harary & Targ, 1985) replicated this experiment, running nine trials, again with the silver futures market as the event-generator. All nine were successful.¹⁶

With some refinements, Targ *et al* (1995) replicated this experiment, again using the silver futures market. Of 18 trials, 12 were acted on (the other six rated as ‘passes’),

¹⁴This of course leaves out the predictive element typical of most recent ARV research. But it nicely demonstrates the principle behind the “associative” aspects of the ARV protocol.

¹⁵This was both a scientific experiment and an applications-oriented project. The overall task was to predict the course of the silver futures market over a sequence of 30 trades. Using consensus analysis, correct decisions were made 21 times out of the 30, resulting in gross earnings at the level of investment totaling approximately \$250,000. The researcher’s share was \$25K, which was committed to establish a Waldorf school in Palo Alto, California.

¹⁶The experiment was subsequently reported in the *Wall Street Journal* (Larson, 1984). A later experiment against the silver futures market failed after two trials and was discontinued due to significant financial losses when the experimenters tinkered with the protocol.

producing 11 correct descriptions, yielding $p = 0.003$.¹⁷

A number of recent studies have been exhaustive in their approach. From 1998 to 2005 an independent researcher named Greg Kolodziejzyk conducted 3,630 trials, achieving 1,922 hits. This produced results of $p = 0.0002$, or one chance in 5,000 that results were due to chance. Kolodziejzyk's method used a computer-automated process to pick and pair targets so he could act as viewer while the computer served all aspects of the tasking function. (Kolodziejzyk, n.d.)

Another mass study by researcher Marty Rosenblatt is still ongoing, but the results tabulated up through 16 March 2009, are as follows:

	<u>Hits</u>	<u>Misses</u>	<u>Passes</u>	<u>Success Rate</u> ¹⁸	<u>p value</u>	<u>Odds against chance</u>
2004	6	5	10	.545	--	--
2005	37	27	52	.578	.084	1 in 11
2006	71	54	93	.568	.054	1 in 18
2007	153	113	185	.575	.006	1 in 169
2008	241	195	233	.553	.012	1 in 81
2009	258	214	243	.547	.019	1 in 51

Total countable trials are 1,374 with 766 hits, yielding a p value equal to .0000112, with

¹⁷For technical reasons only seven silver-commodity trades were recorded as executed, of which seven were in the correct direction. Though officially recorded in the protocol, all trades were only notional, the lead author having apparently learned his lesson.

¹⁸Hits divided by total hits+misses.

odds against chance of one in 90,000. (Rosenblatt, 2009)

As should by now be apparent, even in the relatively young associative remote viewing protocol, results far beyond chance are being produced.¹⁹ ARV experiments continue to be pursued. Most recently, for example, as a class project a group of University of Colorado students and their advisors conducted an ARV project with the closing figure of the Dow Jones Industrial Average as its targeted event (Laham, Smith, & Moddel, 2008). A consensus judging process was used across the results of 10 viewers. A total of 67 individual trials were performed, producing 13 misses and 54 hits. The judging recommended seven investment decisions, all of which were successful, yielding $p < .01$. One of the participants invested \$10,000 of his own funds, as indicated by the ARV results. At the end of the seven trial series the account totaled \$26,000, for a \$16,000 gain (+160%).

There is to be sure, much additional evidence for ESP than just the four experimental paradigms I have covered. Extensive research has been conducted in dream telepathy, other forms of clairvoyance, and other forms of precognition. One prominent research trajectory is the *Ganzfeld* approach, which has been widely explored with a number of variations for nearly three decades.²⁰

¹⁹In private conversation, I have learned of a number of other informal experiments that have been done, reportedly with successful results. I have not been able to report these however, for various reasons, including poor record-keeping on the part of the researcher and desires by researchers to keep themselves or their projects private.

²⁰I will briefly describe the Ganzfeld procedure in chapter 10.

But now we need to determine whether the evidence introduced in the previous four chapters really does constitute a violation of the causal argument and, hence, causal closure – and further, whether the evidence should be ruled to be admissible.

Chapter 9: Evaluating the Evidence – Precognition/Reverse Causation

Now that I have covered the four research paradigms I am offering as candidate closure violators, it is time to see how well they fit the requisite criteria to be counted as intermittently-truncated causal chains (ITCCs). I shall consider each in turn, first as to its status as a closure violator, and second as to how far we can trust the data as evidence of closure violation. Here is how each experimental protocol exemplifies intermittently truncated causal chains:

1a. *Staring*:

Initiating cause (“exiting” portion of causal chain) – research participant distantly staring at subject through one-way television link with subject insulated from either physical influence or conscious awareness of the staring event.

Consequent effect (“re-entering” causal chain) – subject’s nervous system exhibiting a stimulus signature, which is then picked up by instrumentation, which produces a record. Or, alternatively, statistically significant number of correct guesses during staring episodes.

Causal gap: causal connection between the staring activity of the assistant and the nervous system stimulation of the subject.

Apparent closure violation: spatial.

1b. *DMILS*:

Initiating cause (“exiting” portion of causal chain) – intentional influence directed by ‘helper’ or researcher ‘towards’ sequestered subject.

Consequent effect (“re-entering” causal chain) – subject’s nervous system exhibiting an unaccountable stimulus (correlated with helping episodes), which is then picked up by instrumentation, producing a record. Or, alternatively, statistically significant results in various activities during ‘activation’ episodes.

Causal gap: causal connection between the intentional activity of the assistant and

the nervous system stimulation/improved performance of the subject.

Apparent closure violation: spatial.

2a. *Remote viewing:*

Initiating causal chain (“exiting” portion of causal chain) – tasker’s selection of target to be remote viewed, followed by double-blind (or greater) tasking provided to viewer.

Consequent effect (“re-entering” causal chain): Viewer perceives veridical target-related data and records it.

Causal gap: Viewers’ receiving and recognition of ostensibly blind tasking; viewer accessing target and acquisition of veridical target data.

Apparent closure violations: Spatial.

2b. *Precognitive Remote Perception:*

Initiating causal chain (“exiting” portion of causal chain) – target pool is prepared. At a designated point in the future, beacon person/team visits a randomly-selected target. viewer instructed to perform remote viewing against target to be selected in the future, then viewer performance of remote viewing task.

Consequent effect (“re-entering” causal chain): Prior to selection of target and beacon visiting it, viewer perceives veridical target-related data and records it.

Causal gap: Viewers’ receiving and recognition of ostensibly blind tasking; viewer accessing target and acquisition of veridical target data both at a distance and in the future.

Apparent closure violations: Spatial *and* temporal.

3. *Presentiment:*

Initiating cause (“exiting” portion of causal chain) – Selection of target pool and loading into properly-configured computer. Participant is connected to monitoring machinery and baseline condition established. Participant indicates readiness to proceed. Program begins.

Consequent effect (“re-entering” causal chain): Computer randomly selects either

a calm or disturbing photo and presents on the screen.

Causal gap: Generation of pre-stimulus autonomic response in participant prior to selection and presentation of photo.

Apparent closure violations: precognitive..

4. *Associative remote viewing*:

Initiating causal chain (“exiting” portion of causal chain) – Tasker’s selection of event to be predicted; tasker’s selection of binary target set. Double-blind tasking provided to viewer.

Consequent effect (“re-entering” causal chain): Viewer perceives veridical target-related data from target to be revealed in the future, and records the data. Judging to select correct target. Action taken as indicated by selected target. Event outcome is determined. Viewer is presented with feedback.

Causal gap: Viewer reception, subconscious recognition of the specific assignment; viewer accessing target yet in the future. Acquisition of veridical target data.

Apparent closure violations: precognitive, and (in some cases) spatial.

I propose that the two main types of effects demonstrated by these experimental paradigms are: 1) perception/action at a distance after all possible physical mechanisms have been excluded; and 2) precognition (often labeled in the literature as ‘reverse temporal’ causation or ‘retrocausation’) produced under the same exclusionary conditions. I further propose that these two effects constitute powerful evidence of violations of causal closure in the form of what I have styled “intermittently truncated causal chains” (ITCC), thus providing evidence that closure and, hence, physicalism are false, at least in their universal sense.

But my asserting these proposals raise an obvious question. Can we be sure that these phenomena really do demonstrate violations of closure? There are a few arguments

offered to counter the idea that psi phenomena really do present a threat to physicalism.¹ In this chapter I shall focus on precognition/retrocausation, and then follow with a discussion of perception-at-a-distance.

Precognition/Retrocausation²

Precognition has often been considered to be conceptually interchangeable with the notion of retrocausation, because, depending on one's perspective, the phenomenon can be characterized either as perception reaching forward in time, or instead a future event reaching into the past to exert causal leverage. As I will shortly discuss, this interchangeability may be only apparent and has led to confusion.

In my consideration I include under "precognition" all the time-displaced effects discussed in preceding chapters. One may object that presentiment cannot accurately be described as precognitive, since the measured effects involve reflexive autonomic functions operating below the threshold of awareness. However, whatever sub-conscious processes are at work must parse between whether a stimulus that will be presented a few seconds into the future is innocuous or, instead, emotionally disturbing. Such a parsing would seem to require some capacity to not just perceive, but to recognize and understand the content of a photo and decide whether that content was alarming or

¹These arguments presuppose there are real phenomena to be explained. Arguments against the legitimacy of the evidence are considered in the next chapter.

²There are several terms that have been used to capture this notion: "retrocausation," "reverse causation," and "backwards action" are the principle ones, and are largely synonymous. I will use any and all of these more or less interchangeably in the following discussion, depending on utility.

instead calming. Arguably, it would seem that some cognitive process, even if subconscious, must be at work.³

The philosophical debate over precognition dates back decades, but in the mainstream philosophical literature was most heavily pursued in the 1940s through 1960s (though it has continued sporadically since). There are three main sources of impetus for the backwards causation debate. The first of these originated with anecdotal reports and, later, research on precognition. The controversy sparked by precognition claims was later joined by conjectures about the possibility of time travel, and now most recently by micro-physical effects that allegedly show that causation at a sufficiently fundamental level of physics can work backwards against the otherwise normal time stream. In fact, it appears that these two cases – precognition and microphysical reverse causation – are the only retrocausal phenomena for which any evidence has been produced.⁴ This fact shall become important later in this chapter.

The crux of the reverse causation debate is whether some event B in the future can bring into existence or at least causally interact with an event A sometime in its past. Of course, the phrase “causally interact” implies the ability of B to change A, which has

³Admittedly, those experiments where evoked potentials stimulated by flashing lights at the sender are monitored in the receiver involve no presumed cognitive parsing of subconscious input. But these type of experiments are of such relatively small number that it seems reasonable just to include them within the class “precognition,” since the remote perception channels involved are likely the same as those activated in precognitive phenomena. It is just that there is no cognizable content presented.

⁴Precognition by far produces the most fodder for examples or thought experiments crafted to wage the reverse causation debate. More recently, time travel scenarios have come increasingly to play. But unlike for precognition, there is no evidence for time travel.

led to worries that if acknowledged as real, then reverse causation implies at least some future events can change at least some facts about the past. Though a question about violations of the standard *temporal* order seem to be at the root of the debate, it can be seen as really a debate about *causal* order – which in our world seems clearly bound together with temporal order. I do not propose in this discussion to try to resolve any of the long-standing metaphysical issues surrounding the nature of time or its perception by humans, only so far as they impinge on my central question: does precognition plausibly demonstrate a violation of causal closure? In that sense, questions about time are secondary to issues about causal order, since precognition seems to violate the former mainly because it appears to violate the latter.

This raises an argument that is still unresolved. On the one hand are those who consider the idea of retrocausation as implying “changing the past” and that such a claim constitutes a logical impossibility. As Broad (1937) describes this position,

No amount of empirical evidence would give the slightest probability to the hypothesis that there are squares whose diagonals are commensurate with the their sides, because this supposition is known to be logically impossible. Now a great many people feel that the hypothesis of veridical supernormal precognition is in this position.

Among those who feel the allegation of logical impossibility is an insurmountable objection to precognition is Anthony Flew. Flew (1954) quotes an earlier article in *Science News*, in which the author noted that “precognition – with its apparent implication that causation can work backwards in time – seems to violate one of the essential presuppositions of science” (Knight, 1950). Flew responds with the assertion that the ““apparent implication”” that precognition may involve backward causation “is

not one which any phenomena whatever could have – for that the cause must be prior to (or at least only simultaneous with) the effect is not a matter of fact but a truth of logic” (p.45). Any claim that the past might be changed – that is, “that what has been done might itself now be undone...is a piece of nonsense” (p.48). Agreeing with Flew in this instance, Larry Dwyer observes

I take it that changing the past would involve something like undoing what has been done or doing what had not been done. It would, therefore, seem to involve a change in the truth values of statements about the past, not merely on epistemic grounds (resulting from a change *in our evidence* regarding the events in question) but because of a change *in the events themselves*. This would indeed be nonsense... (Dwyer, 1987, 384)⁵

Max Black argues that “to speak of an effect preceding its cause is as absurd as to speak of Tuesday preceding Monday” (1956, 55), and Graham Oddie (1990) presents a compelling argument not necessary to fill out here, that the past is “fixed” and cannot be changed.⁶

Together, these positions express what Broad (1937) refers to as the “causal objection.” What the causal objection amounts to is that, at the time a person has a mental impression (Broad calls it an “image”) of what (apparently) eventually turns out to be a future event, the future event “cannot have had any causal descendants,” and its

⁵As we shall see, Dwyer does not endorse Flew’s main conclusion that this is problematic for precognition.

⁶In this sentiment Flew, Black, Dwyer, Oddie, and others have good company. Diodorus’s “Master Argument” takes as one of its conditions that every past truth is necessary, hence unchangeable – from which he argues for determinism. Hans Kamp’s (unpublished) “The Logic of Historical Necessity” seems to support the position. Some other relevant sources from a tense-logic standpoint are Thomason and Gupta (1980) and Von Frassen (1980).

causal precursors for the most part are usually inaccessible to the possessor of the (apparently) precognitive impression. “How, then,” Broad queries on behalf of critics of reverse causation, “could we possibly account for the occurrence in this person at this particular moment of an image which is pro-presentative of this particular future event,” since the thus-foreseen event “had no causal representative, either ancestor or descendant, in the experient at the time” that the precognitive impression was received? (p.190)

On the other side, there are those who reject the accusation that the notion of backward causation is incoherent, and believe that they indeed see evidence of reverse causation in certain research experiments. Advocates of precognition/reverse causation respond to criticisms in different ways. Michael Dummett provides an account which does as little violence to conventional positions on causation as possible. He proposes a Humean-style “quasi-causal” explanation, in which a habitual conjunction of future-causes B with past-events A is sufficient to acknowledge a causal process is indeed taking place. (Flew, as will shortly be mentioned, takes serious issue with this account.)

Broad, on the other hand, tries to show that the critics’ beliefs in the “logical impossibility” of precognition can be broken down into three fallacies. The first of these fallacies he calls the *epistemological objection*, which he describes in the following way: In order for one thing to have a relation with something else (even the relation of “being cognized”), both have to exist. However, in precognition the thing being cognized doesn’t yet exist, therefore “non-inferential veridical precognition” is a contradiction.

Broad responds by arguing that our notion of memory is of a similar logical pattern, given that we have memories of events and states of affairs which no longer exist. Since we don't have a problem with accepting memories as bearing true relations to states-of-affairs which no longer exist, we can't lodge an attack on precognition without throwing the status of memory into question.

The second "fallacy" Broad calls the *causal objection*. At the time the precognition occurred, the event being foreseen "had no causal representative, either ancestor or descendant, in the experient at the time when his [precognized impression] of it occurred." (Broad, 1937, 190). The objection then follows: there is no causal story to account for this alleged awareness of some future event, and therefore some other way of accounting for it must be looked for (such as imagination, fraud, error) that does fit within a conventional causal context. Broad argues against this objection by enumerating all the possible ways of accounting for apparently veridical instances of precognition and showing that the *prima facie* explanation of some kind of "supernormal" information transfer from the future to the past offers the least absurd explanatory avenue.

Broad's final fallacy is the *fatalistic objection*, a restatement of the argument that if precognition is real, it must mean the future is fixed and there is no real free will. But if free will exists, then precognition must not be plausible. Broad's response to this objection, trading on a difference in connotation between "determined" and "determinate," is rather obscure, and of little present consequence, given widespread doubts about the nature of free will in the face of physicalist theories current today that embody only deterministic and indeterministic causal patterns.

Of Broad's three proposed fallacies, the causal objection seems the most pertinent. Philosophical developments over the past several decades seem to have deprived the fatalistic objection of much of its relevance, and the epistemological objection can be shown to collapse into the causal objection with the right analysis.⁷

But it seems plausible that if the past is indeed fixed, then *once the past is already past*, backwards causation is in fact impossible (whether "logically" or not is moot), thus generating what Broad has called the 'causal objection.' Yet it is hard to avoid the fact that a significant body of evidence demonstrates ostensibly retro-causal effects. So how are these two apparent diametrically-opposed facts to be reconciled? One attempt appeals to certain interesting features of physics, and is an argument not so much *against* the possibility of precognition/retrocausation as evidence of a non-physical effect, so much as it is an attempt to elucidate a mechanism by which this seemingly "paranormal" effect can be shown to actually fit within the physicalist context.⁸

The Physics "Arrow of time" Argument

The argument for reverse causation from physics observes that many of the fundamental laws of physics seem to be time independent. That is, such laws are "time symmetric" in that they would presumably work whether time runs forward or backward

⁷Briefly, if knowledge depends on causal relations between facts and events in the world on one side and human perception and cognition on the other, then the causal problems of precognition would by their nature play into epistemological issues concerning precognition.

⁸See, for example, Puthoff *et al* (1981); Radin (1997 & 2006); Bierman & Radin, 2000.

through them. Margenau notes, "...we can choose the direction of time *as we please so far as the laws of mechanics are concerned*" (1954, 85). The Wheeler-Feynman theory of electrodynamics requires the micro scale symmetry of time and causation. And both the so-called "quantum eraser" experiment and Wheeler's delayed-choice thought experiment (Wheeler & Zurek, 1984) further seem to point to a fundamental reverse-causation principle in physics.

If so, some people argue, then perhaps precognitive results are merely the consequence of some as-yet unidentified physical process running in reverse.⁹ We might refer to this as the "arrow of time" argument, in that these sorts of arguments often hinge on the supposed bi-directional nature of time (as demonstrated by the aforementioned atemporal physical laws). The argument runs something along these lines: We have evidence for an apparently time-reversed precognitive effect. We also have reason to believe that there are time-reversed phenomena in micro-scale physics. It stands to reason that if retrocausation is possible at the micro scale, we might plausibly eventually find a way of linking the two scales in some future, more fully-developed physics. Therefore, we should (at least provisionally) accept precognition as a legitimate phenomenon.

However, if we rely on the arrow of time argument to attempt to reconcile physics and precognition, we run into two sets of concerns. The first is philosophical, and the second physical. Since they are more quickly dealt with, I shall consider the physical

⁹A similar, but much larger argument – that perhaps an eventual fully completed physical explanation of the world will find a way to bring psi phenomena into an

issues first. There are two problems that arise from the arrow-of-time argument from the perspective of physics. The first is that the retrocausative effects themselves are open to question and interpretation. A quantum physics counterexample to the standard flow of time that has lately been proposed is the so-called delayed-choice experiment proposed originally by physicist John Archibald Wheeler (Wheeler & Zurek, 1984) as a thought experiment, and supported by experiment in 2007 (Jacques, *et al*, 2007). Conceptually, the experiment is a further elaboration on Young’s double-slit experiment, which demonstrates the duality of quantum particles. Individual photons passing through an appropriately-configured device which includes two small apertures spaced closely together will manifest wave-like properties if measured one way, but will instead demonstrate particle-like qualities if measured a different way.

Wheeler’s delayed-choice thought experiment follows this design, but adds one feature: the ability to change the measuring method once the photon has passed unobserved through the aperture but before it reaches the terminal measuring device. If one sort of measurement device is in place, the photon registers as a particle. However, if that is removed and the other substituted *after* the photon exits the double-slit device but *before* it is measured, it will register as wave-like, as if it had retroactively “changed its mind” according to the change in observation means.

Those who advocate a microphysics solution to the reverse causation conundrum argue that since there is evidence at the photon level that some reverse-time effects can occur, then it stands to reason that there may be a macro-level effect within physics that

explanatory framework suitable to physics – will be engaged in the concluding chapter.

can account for precognition. However, there are some questions to be begged here.

This delayed-choice phenomenon occurs on a micro-scale both physically and temporally. It involves photons, one of the smallest known units of matter, and is literally less than a picosecond ($>1/\text{trillionth}$ of a second) in duration.

There are many effects present at the quantum level that work because they occur too rapidly to make a difference – for example, QM theory allows matched particle pairs to emerge out of the vacuum (“something out of nothing”) and then self-annihilate, just so long as they do it in a very restricted amount of time. Or consider other well-known quantum phenomena, such as the particle/wave duality demonstrated by the double-slit experiment, where the type of measurement makes apparent ontological difference to the quantum object being studied; the quantum wave function collapse which, according to certain QM interpretations requires an act of observation to occur; or Heisenbergian uncertainty, again involving measurement, where one can know with precision either the position or the velocity of a particle, but never both. Any of these phenomena would constitute violations of the laws of nature were they scaled up to the classical level. But it is clear they don’t scale up. Instead, any classically-awkward effects such as these are damped-out long before they become part of larger-scale systems. There seems no reason to suppose – and no evidence to support the speculation – that possible retrocausative quantum effects would scale up any better.

A second question relating to the physics issues of reverse causation involves the actuality of the phenomena. To a significant degree, the few instances of apparent or theorized backwards causation greatly depend on which interpretation of QM one

subscribes to, or on the mathematical model one employs to solve some of the relatively thorny difficulties in some QM explanatory domains. For example, backwards causation in the Wheeler-Feynman electrodynamics theoretical framework are a consequence of certain mathematical strategies aimed at resolving recalcitrant problems left by other QM interpretations. However, the consequences predicted by the mathematics is, in fact, unobservable. According to Margenau, a reverse-time construct is built in *for the sake of the theory*, not necessarily because it actually happens. So in the theory “...whenever you see an electron’s world line extending *downward* (from future to past), reverse its arrow and assign it to a positron” (1954, 86). Indeed, “the mathematics is indifferent to the direction of the world lines” (p.87), and unless time reversals are accepted conceptually, it “encumbers an otherwise beautiful theory.” But “allowing time reversals...makes the theory singularly significant, powerful, and true” (p.88) – even though no observation of the actual physical entities demonstrates any sort of time reversal in fact.

So exactly what is the ontological status of these sorts of reverse-causation claims? Perhaps it is just a necessary conceit of the mathematics to tidy up an otherwise insoluble problem. Or perhaps a subsequent reinterpretation of QM will resolve seeming conflicts in ways that do not require an appeal to retrocausation. While it seems premature to rule out the possibility of certain retrocausitive microphysical explanations, it seems just as premature to rule them in, as well.

It might be well to keep in mind Hugh Mellor’s comment on how the notion of the time-invariance of physical laws may apply to backwards causation. He observes that Goedel “infers that backward time travel is possible because some solutions of the

equations of general relativity allow it. But this assumes that anything which is ‘physically possible’ (i.e., compatible with physics) is really possible: i.e. that only physics can limit metaphysical possibility. And that’s nonsense” (Mellor, 1991, 197).

The next set of concerns surrounding the arrow-of-time argument are philosophical, and there are two of these as well. The first problem is that the arrow-of-time argument is logically troublesome. Though it is true that a retrocausative physical effect would help to preserve the cause-precedes-effect pattern of standard causation (merely running in the opposite direction), it requires that only one or a few *threads* in the time stream run backwards (since it is clear on observation that the main time stream continues to flow in its customary direction). Effectively, such a retrocausal thread “swims upstream.” But no causal theory allows a single cause and single effect to stand alone and isolated. All causation occurs in a larger context in which both an individual cause and its individual effect are influenced by and in turn influence other causal chains. Indeed, an isolated, single-link causal chain with one cause and one effect, should such a thing plausibly exist, would be epiphenomenal and, hence, undetectable. One would expect, then, to find not just one isolated reverse-tracking cause-and-effect pairing, but a full causal chain of causes followed by effects followed by causes, and so on, with all the considerable disruptive ramifications from such a contrarian chain emanating *upstream* in time.

In other words, if our pairing of a future cause correlating with a past event is an arrow-of-time effect, we should see (even if only in retrospect) a causal history approaching from the future, engaging the future event, continuing chain-wise through to

the past event, then proceeding on up the time stream against the current, like salmon returning to spawn. At least, that is how it should appear to us, if the arrow-of-time argument is correct.

Yet that is not at all how it appears. The time stream seems to flow smoothly from past to future with nary a contrarian ripple – precognitive reports being the *only* macroscopic exception. Even if the time stream as a whole were reversible,¹⁰ a contrarian causal chain (or chains) would be immediately obvious. Even in the precognitive case all we see that goes *against* the usual temporal flow are the future cause and the past event. And these sorts of apparently causally-reversed events are accompanied only by forward (“downstream”)-moving causal cascades typical of any standard cause → effect chain happening in the usual order of things.

An example from associative remote viewing is probably the easiest way to show this. At time *t-zero*, the viewer draws an accurate sketch of an apple, which unbeknownst to her is indeed one of two objects (the other is a pencil) selected for the target pool by the tasker and respectively assigned to one of the two possible outcomes of event *e* which will occur at time *t-plus₂*.¹¹ Seeing the viewer’s sketch of the apple, the tasker is caused to undertake a certain action (betting, buying, selling, etc.) at *t-plus₁*, in anticipation of the occurrence at *t-plus₂* of the event whose outcome is being predicted. At time *t-plus₃*

¹⁰As Sarah Waterlow observes: if time and causality could go either direction, then “backwards causation...would *be* no more mysterious than forwards; but nor would it even *seem* any more mysterious or for that matter any less commonplace. And this [supporters of backwards causation] have never claimed” (1974, 382).

¹¹Members of target pool set may be selected at any point up until immediately prior to *t-plus₂*, the time of occurrence of the event whose outcome is to be predicted.

the tasker hands the viewer an apple according to protocol, closing the temporal feedback loop. This action at $t\text{-}plus_3$ – the handing-of-the-apple event – initiates a standard downstream causal cascade, unfolding various ensuing cause-generated events, such as the viewer’s subsequent eating of the apple, her developing a sense of satisfaction for a job well done, bystanders experiencing amazement at such a seemingly miraculous turn of events, and the tasker’s subsequent acquiring of a new luxury auto with his winnings.

All these follow-on events occur in the standard causal and temporal order, with time flowing from upstream to downstream, and the causal chain keeping pace appropriately. The viewer’s sketching the picture of an apple at $t\text{-}zero$ seems only able to be correlated to (and thus we might be inclined to say “caused by”) the presentation of the apple to the viewer by the tasker at $t\text{-}plus_3$, thus initiating an apparently “retrocausal” event pointing backwards in time, up the temporal stream. But *there is no identifiable continuation of a retrocausal chain* from either the feedback event at $t\text{-}plus_3$ or the sketching-of-the-apple event at $t\text{-}zero$, nor does any reverse-causal chain precede (in the conventional cause-precedes-event order) the predicted event occurrence at $t\text{-}plus_2$ (that is, proceed upstream against the temporal current toward the $t\text{-}zero$ event), contrary to what one would expect to see if there were indeed some kind of physics-related arrow-of-time temporal reversal involved.

The second philosophical problem with the arrow of time argument is that it is ontologically troublesome. If a single causal thread (or even multiple ones) runs counter to the usual temporal flow, one would expect to find conflicts and paradoxes galore as the thread moves up against the temporal stream. Causal chains moving in opposite

directions frequently would, it stands to reason, clash in unexpected ways as they cross each others' paths and the paths of chains proceeding in the normal direction of temporal flow.

To continue our salmon-in the-stream metaphor, as our salmon swim upstream to spawn, they are bumping and colliding as they go with even larger throngs of other salmon heading downstream towards the open ocean, leading to patches of significant turmoil, perhaps damage to fins or scales, and certainly impedance of the normal flow of fish. Yet we do not see anything analogous in the temporal flow – time and causality seem to proceed in a generally orderly fashion from past, through present, towards future, with no apparent downstream-vs.-upstream collisions to roil the waters and confuse causal histories. This suggests that if the arrows of causation and time are potentially reversible, they could only go *one direction at a time*. In other words, they could go either forward or backward, but never a little of both.

It seems, then, that precognition in a normal causal framework must be an impossibility, but the microphysical solutions offered as a seemingly-feasible palliative are unable to account for it. Yet very strong evidence of precognitive, retrocausal effects persist and demands an explanation. What are we to do?

Changing the Past

I suggest that the whole notion of “backwards causation” as generally conceived (that is, in terms of a future event changing a past event) mischaracterizes what is actually being claimed, at least as far as precognition is concerned, and leads to insoluble

problems. To see why, consider how the problem of reverse causation is almost always expressed. Backwards causation is generally objected to in somewhat the following terms: “An event B in the future causes/affects/changes an event A in the past.” This formulation, however, leaves the points of reference as vague and ambiguous – an event in the future (“the future” with respect to whom or what?) exerts causal influence to a greater or lesser degree on a past event (again, “past” with reference to what observer, and when in time relative to both events?). The usual assumption here is that the past event has already transpired and become embedded in the upstream temporal record.

Unfortunately, as discussed briefly above, this brings up one of the most persistent objections to backward causation – that to suggest that it is possible (both logically and metaphysically) to change the past is incoherent. So, for example, according to Flew, “It would seem to follow that it must be wrong to speak of a present event bringing about a past effect; since this could only be a matter, either of undoing what has already happened, or of making to have happened what has not happened” (1973, 366). This becomes clear in considering what it would take to “change the past.”

First, though, in what sense does the past “exist”? There are two ways we are aware of the past. In the relatively short-term (that is, within the longest life-span of a living human), we are aware of the past through personal memories. And in long- *and* also in the short-term, we are aware of the past by the vestiges and evidence it leaves: books, photos, ruins, monuments, corpses, shards, flint points, cave murals, organic residue, residual Carbon-14 levels, geographic strata, red-shift, residual cosmic radiation, and so on. We know these traces and vestiges of past events and states of affairs exist.

We don't know if there is any sense in which the myriad interweaving chains of actual events that created these artifacts and traces actually exist in any meaningful ontological sense.

One way of thinking about the unfolding of time might present it as a single wave of activity (cause/event interactions) milliseconds (or less) deep that sweeps along through the universe working on the world's raw materials as it encounters them and leaving vestiges of events in its wake. One may think metaphorically of the temporal wave (perhaps, "the present") as being much like a line of army ants marching abreast through a tropical forest, converting the jungle in front of it into an altered landscape that it leaves behind. In this sense, the future exists only as potential, and the past consists in only these vestiges plus the memories resident in the sentient beings who are caught up in the moving temporal wave.

Alternatively, as some (perhaps implausibly) maintain, the past may continue to persist as a discrete ontological entity, events and causal chains frozen as they occurred, and as the present advances into the future, events and links in uncompleted causal chains continue to accrete, piled in sequence one on top of another like strata in an immensely deep geological formation stacked earliest to most recent.¹²

If either model is correct, then what does it mean to change the past? In the first scenario – that of a "sweeping wave" of change – the past consists in only memories (if

¹²There is also the view that the entire time-stream is actually a fixed continuum, every part of which exists at every moment, and only human perceptions "move" along it in some sense, giving the impression that time is continuously unfolding. But as far as the past is concerned, this scenario is identical to the one I have just outlined.

the events are recent enough) or the vestiges – the evidences of past events – remaining behind. Changing the past would mean in some way revising the physical remains and what contemporaneous memories are relevant. If one were, for example, to reach back and alter the fate of one victim of the battle of the Alamo, so that he survived instead of died, then not only would the fact of his (non) burial and the location of his grave be changed, but the entire cone of evidence that comes into being through his actions and their ramifications throughout the downstream reach of time must be created or accounted for. If our Alamo survivor becomes a builder, then structures come into existence, as well as public records, dispositions of materials, displacement of other structures, persons, or artifacts and – if he has subsequent offspring – new lives and their vestiges must be generated as well. Even in the second description of time, where the events of the past continue to persist ossified in time, the physical vestiges must be altered as just described, plus the entire frozen time-stream must be altered as well, spreading ramifications downstream as the present continues to advance.¹³ But effecting such draconian alterations – as nearly any change of the past would seem to require – seems highly implausible, which accounts for objections proclaiming the logical impossibility of reverse causation – and with it precognition.

There are other arguments supporting the impossibility of changing the past. One, explicated by Anglin (1981), is perhaps best described as a “causal loop paradox,”

¹³This recalls Ray Bradbury’s short story “A Sound of Thunder,” which depicts a group using a time machine to go back into the time of the dinosaurs, where one man inadvertently crushes a butterfly, and upon their return they discover the world disturbingly altered from what it had been.

where an event is the result of a cause of which it in turn is the cause. For example, person A fulfills an action the following day which was seen in a dream the night before. But the action would not have been performed without A's dream as impetus, and the dream would not have occurred without the action taking place the following day. This is thought to present a paradox which closes off a part of the time stream, and it is argued that this is impossible. Mellor (1981) particularly makes a strong case against the possibility of causal loops.¹⁴

Some reject such closed-loop arguments against reverse causation. Anglin, for example, observes that even 'normal' causation, when followed far enough, either turns out to be circular or results in an infinite regression. "If one continues to inquire in this fashion, asking about each cause in turn, one runs into either an infinite chain of causes or else a circle" (Anglin, 1981, 90). Huw Price specifically addresses Mellor, arguing against "the feeling that backward influence would lead to causal loops, and hence to paradox," and giving an analysis that "this is not so, providing the past effects are not accessible by irrelevant determinations before their cause takes place" (Price, 1988, 315). Price's (and others') response moved Mellor to recapitulate his position (Mellor, 1991), constructing a *reductio ad absurdum* that does seem to confirm the causal paradox argument against reverse causation. I shall, however, not go into any of these arguments in any detail. In a few pages I will argue that what is claimed about certain species of

¹⁴Susan Weir agrees with Mellor: "This is not a possible causal process," she affirms (1988, 203). And (in an insight I owe to Dan Bonevac) Thomason's and Gupta's (1980) tense logic treats time as having a branching structure, which models that included even small causal loops would invalidate.

precognition does not commit us to any sort of closed causal loop, thus avoiding this debate altogether.

Another influential argument attributed to Pear and Flew, but outlined explicitly by Black (1956) has come to be known as the “bilking argument.” In arguing that we should reject any notion that a future event can be the cause of an event that precedes it, Black suggests that “we can arrange for *T* [the future event] to *disagree* with *A* [the preceding event]” (p.54), and thus show that there is no future-to-past causal action, by waiting to see what the preceding event *A* predicts, and then “arranging” for the future cause *T* not to occur.

Borrowing now from Oddie (1990), the bilking argument in its basic form goes something like this: Let future event *c* be the *apparent* cause of preceding event *e*. We can, on observing the occurrence of *e*, arrange for *c* to fail to occur, thus establishing that no retrocausal link exists, because *e* happened even in the absence of *c*. If, on the other hand, we nevertheless try to produce *c* and despite our best efforts are unable to do so, this does not demonstrate either a necessary or sufficient causal dependency backwards from *c* to *e*, but could also be accounted for by some forward causation dependency that we simply haven’t yet identified.

Black illustrates his version of the bilking argument with a fanciful example of Harry Houdini, who under hypnosis can predict perfectly how a future series of coin tosses will turn out.¹⁵ For the sake of the argument Black stipulates that no conventional

¹⁵Thus in the passage quoted above from Black, ‘*A*’ stands for Houdini’s ‘answer’ and ‘*T*’ stands for a coin toss.

explanations (such as fraud or trickery on Houdini's part, or a common causal ancestor for both events, or some other hidden forward-causal feature linking A with T , or that *any* prior event causes A) can account for Houdini's coin-toss-guessing prowess. Black "bilks" Houdini by artificially arranging for the coin tosses to either not be performed at all, or to come out in a pattern we choose that differs from the one the hypnotized Houdini predicts. In the latter case, if we find that our coin tosses do in fact falsify Houdini's predictions, then we have evidence that no reverse causation is present. If, on the other hand, we find we cannot manage to produce our own pattern of tosses, even though the pattern that *is* produced differs from Houdini's predictions, this is not evidence of retrocausation so much as some as-yet undiagnosed form of standard forward causation.

Oddie's own notional example employs a psychic, "Cassandra," who predicts whether a person, "Stanley," will consume a macaroon or an éclair at the following morning's tea. Oddie, like Black, rules out alternative explanations for Cassandra's apparent success (such as fraud, inference from presently-known facts, and so on). But he sets up the problem in a somewhat different way. Reverse-connecting Cassandra's prediction P of macaroon (or éclair) consumption to the future act of consumption C are two causal chains. The first one links Stanley's éclair consumption CE to Cassandra's prediction of éclair consumption PE . (Oddie calls this "Causal connection 1.") The second links macaroon consumption CM to Cassandra's prediction of macaroon consumption PM (Oddie's "Causal connection 2"). Oddie takes these chains as being of equal causal valence and, in fact, seems to be proposing that for his argument they should

also be taken to exist simultaneously and to possess actual causal power (“there must be at least two obtaining causal connections from later to earlier,” and they must provide “an immediate causal connection” between *CE/PE* and *CM/PM*. [Oddie, 1990, 75]).

The bilking argument goes through when Cassandra predicts that, say, Stanley will eat an éclair and Stanley later instead eats a macaroon. This “breaks” one of the causal chains (Connection 1, or the *CE/PE* link), thus falsifying Cassandra’s precognitive claims. (Though Oddie’s construction of it seems somewhat odd, indeed. No claim of precognition suggests that a causal link extends simultaneously from each of the possible future causes back to its respective possible prediction, as Oddie seems to be proposing. In fact, a “precognizer” would only recognize the possibility of one causal chain – the one being predicted. If, on the contrary, the alternate future event turns out to be the case rather than the predicted one, the claimant would merely be considered wrong, rather than some causal chain being “broken.”)

“Affecting” vs. “Changing”

Before examining what is wrong with the bilking argument, let us return first to my claim that vagueness in placing the reference points in describing retrocausation is what leads to the objection against precognition. Do veridical precognitive impressions *really* require that an event in the past be created or changed? The answer is, in fact, no. In the standard way of discussing the issue, future-changing-past is described as if being observed from a vantage point either at some midpoint – perhaps the “present” with event B still future and event A now buried in the past – or as if being observed from some

notional objective point off the time stream altogether. But this way of setting up the problem entails that creating/changing event A will require the draconian (and implausible) changes in upstream-embedded evidence and memories that I described above as being required were we to literally to change the past.

Another, somewhat-related and equally erroneous way of looking at it takes the observer's point of reference to be co-located with future event B. From this perspective it doesn't matter if the observer is at B in the future or whether B is located with the observer in the present. In either case, event A remains embedded in the past, with the usual implausible consequences implied by any causative relation from B to A.

But neither of these constructs actually matches what is being claimed in precognition, and this erroneous formulation is what generates the confusion. The solution was actually hinted at by Michael Dummett in a 1964 article in *The Philosophical Review*. First accepting the *prima facie* asymmetry of both time and causality (that is, its downstream unidirectionality), he then considers a thought experiment based on an Orthodox Jewish injunction against retrospective prayer. Retrospective prayer is blasphemous because it enjoins God to attempt a logical impossibility – change an event in the past that has already happened. Dummett's rejoinder was that retrospective prayer is not as blasphemous as it seems, and the worthy Jewish theologians mistaken since, if God knows the future, then he already knows *at the time of the event* that you will utter your retrospective prayerful plea at a specific point in the future. If he so wills he can take measures *at the time* the event transpires according to his knowledge of the future to ensure the outcome you will later be pleading for

regarding your present circumstances.¹⁶

So, if it is possible for a human to also possess at least a modicum of foreknowledge (such as through precognition), “then a similar rationale could be provided for actions designed to affect the past, when they consisted in my doing something in order that someone should have known that I was going to do it, and should have been influenced by this knowledge” (Dummett, 1964, 344; his use of the phrase “affect the past” rather than “affect the present before it becomes the past” serves somewhat to obscure the important point). Regrettably, Dummett decides he “shall not pursue” this point, thus missing an opportunity to make an important breakthrough in the argument surrounding the logical impossibility of changing the past.

If he had gone on, this is what he might have proposed: That rather than event B in the future creating or changing an event A embedded in the past, with all the requisite trouble that would bring about, B in the future is really only *affecting* event A in *its present*, and this makes all the difference in the world to the case. In the retrospective prayer case, God does not change a past event from one with a bad outcome to one with an outcome in accord with your future desires. Rather, he looks forward in time to your pleadings, decides to accommodate them, and *as the event in question occurs*, he affects the event in *its actual* ‘present’ to come out a certain way. Thus, nothing in the past requires changing, and nothing in the present is changed, either, since in the present the

¹⁶Descartes, in his *Principles of Philosophy* (1:40-41) seems to imply that, though God has predetermined what is and what will come to be (hence implying that humans themselves are unable to change the past), what is and what has been are contingent on God’s predetermining, and hence, *could have been* different – and perhaps still can be,

event is still happening.¹⁷ The event merely unfolds the way it is influenced to and, as the present moves on towards the future, the event becomes embedded in the past just as God (or a human actor with some bit of foreknowledge) intended it to be. Notice that this is no different from the way the time stream unfolds naturally, but instead of God “deciding” which way events unfold from present to future, deterministic influences without benefit of knowledge from the future bring about how the present moves into the future.

A second opportunity to capture this point hinted at by Dummett was narrowly missed by Bob Brier (1973), writing in the *Southern Journal of Philosophy*. In the article, Brier responded to criticisms Flew (1954) had registered against Dummett’s (1954) notion of “quasi-causation.” Brier meant to defend Dummett by objecting that although it “would be nonsense” to consider altering the past, yet “one could readily make use of the distinction between changing the past and affecting the past.” In other words, “...by a present action cause something to have happened which would not have happened otherwise” (p.361). Flew (1973) rightly complained that Brier had seemingly arbitrarily offered up this notion “as if [Brier] were referring to something which had only to be mentioned to be remembered as an old familiar. But [is instead] a distinction

contingent on God’s will.

¹⁷A caveat is needed here, as this idea of “changing the present” isn’t quite right. As is brought out by Peimin Ni (1992), technically one cannot “change” the present. Ni argues that if one were to change the present one would have to bring about a situation where one was both doing X and not doing X, and this is a logical contradiction. It is rather the *future* being changed as the present unfolds into it. But talk of “changing the present” is not so serious a violation of this concept that we can’t go on using it here for convenience.

which he has himself just now introduced” and left unexplained.

Larry Dwyer (1977) came to Brier’s defense, sketching out a response much along the lines hinted at by Dummett (1964). Using a hypothetical time-travel illustration, Dwyer argued that it is logically possible to go from a position in the future to a point in the past *of that future*, and affect an earlier event during that event’s future, before the then-present completes the process of moving into the past. As indicated above, one is not *changing* something if one is merely influencing (“affecting”) it to turn out a certain way as it is unfolding.

In Dwyer’s hypothetical case, a man travels back from the present in a time-traveling rocket ship to instruct the Egyptians in how to build the Pyramids. Yet there is no logical impossibility or temporal paradox generated. This is because, even though

events in, say, 1978 (e.g., the pulling of levers and twisting of dials in the rocket) are causally efficacious in bringing about events in, say, 3000 B.C. (e.g., the arrival of the rocket)...the time traveler does not undo what has been done or do what had not been done, since his visit to an earlier time does not change the truth values of any propositions concerning the events of that period. Thus even before the time traveler enters his rocket in 1978 to begin his successful mission to the year 3000 B.C., an accurate catalogue of all the events occurring in Ancient Egypt that year would include an account of his arrival from the sky, as well as an account of his various actions and reactions in that new environment. (Dwyer, 1977, 383-384)

As Dwyer has it, it is a mistake to think of the rocket leaving *now* and going back into the *past* to effect changes. Rather, to be accurate one must take it from the Egyptian perspective, that a rocket has come from the *future* and landed in the *present*, to help create history *as it happens*, not after it has occurred. “It seems to me there is a clear distinction to be made here, between the case where a person is presumed to change the

past, which indeed involves a contradiction, and the latter case where a person is presumed to affect the past by dint of his very presence in that period” (Dwyer, 384).

To be sure, there is no evidence that rocket ships can travel between future and past. But there is extensive evidence that human perceptions can travel between future and present. Contrary to how it has over decades often been described, precognition does not involve an event “reaching back” into the past to influence a temporally-earlier event and all its causal descendants. Rather, it is best to think of it as a person in the present “reaching forward” to bring back information that affects an event or events now, as the present unfolds. Indeed, this is exactly how precognition seems – as if one is (figuratively, though sometime almost literally) “looking” into the future for information. It is *never* experienced as if projecting from the future back to influence the past.¹⁸ Of further relevance, it is also only the case that information is obtained from precognition process,. Information may be a vehicle of causation, but seems itself not to be a causal agent. While some may want to insist that one can still view this as the future affecting the past, I argue that this is a mistake in perspective carrying little metaphysical weight.

The best test of this is to show how it works out in our two main examples of laboratory precognition, presentiment and associative remote viewing. In a presentiment experiment, the participant’s autonomic state is being monitored instrumentally in real-time as events unfold. The upstream event (event A) happens in the present as an unexpected activation of certain autonomic functions seconds in advance of event B, the

¹⁸Though there is a notion of “retrocognition” in which one seems to access a past event and under some conditions produce veridical information about it. But this is both

revealing of a disturbing image in the near future. Since the monitoring is occurring in real-time (the ‘present’), it is obvious no past event is being changed. But, clearly, an event unfolding in real time *is* being affected, coupled in a cause-like correlation with a downstream (future) event yet to transpire.

In the case of associative remote viewing, a remote viewer in the present is asked to describe an object that will not be revealed to her for hours to days into the future.¹⁹ In this case, the present event A is the perceiving, sketching, and describing of the target to be observed in the future. The future event B is the showing of the object to the viewer hours or days hence. In either case, it is clear the present is being influenced by a conscious, intentional act to unfold in a certain way, and nothing is being changed from some way it had already become.

There is another way talk of the “future changing the past” gets it wrong. Though it is usually the notion of precognition that is being addressed, logical constructs of the argument are almost always put together in more general terms that can embrace any category of causally linked event-pairs. In an example consistent with precognition, if one talks of future event B causally affecting past event A, this could be fulfilled by John at earlier time t making a sketch of whatever presently unknown object he expects to be presented with at later time $t+1$. It could also be fulfilled by Shelia dreaming at $t+2$ of the very car wreck which she then narrowly avoids the next day at $t+3$ because she

conceptually – though not experientially – different from precognition. (Smith, 1005)

¹⁹In precognitive remote perception, of course, it is a location to be visited in the future, rather than an object or photo.

remembers her dream just in time to keep from going through the intersection when an oncoming truck runs a red light. However, the kind of past-changing language typically describing the pattern of Future Event B causing-to-occur Past Event A *could* just as easily be fulfilled by a boulder tumbling down a hillside five years from now, bursting through a time warp and causing a landslide that buries a city today. Or it could also be fulfilled by someone striking a match day after tomorrow, which ignites a fire that burns a hundred square miles of California today.²⁰

Bilking Argument Revisited

In light of the foregoing, I return here to a discussion of the bilking argument. The argument has two serious weaknesses as a proof against reverse causation. First, the argument only applies to precognition, not other hypothetical forms of backwards causation. The argument couldn't work, for instance, against the burning-match example of reverse causation just mentioned above, because no intentional agent is involved with the upstream ('present') A-event of the causal chain. For the bilking argument to work, there must be at least one intentional agent involved – the one at the earlier event point A – who makes the epistemic connection between ("gains knowledge of") the present event and the future cause.

Such an intentional agent is at work in the case of associative remote viewing, in that the viewer at A knows to access the future event B that is satisfied by the unveiling

²⁰Never mind puzzling over *how* such things could occur. It's doubtful they can, as we presently shall see.

in his or her presence of the intended target at a specified time two days in the future. It also works just in case a psychic at a time at or before event A (igniting of the fire) reports a perception that event B (striking of the match) will occur two days hence. However, without such precognitive access we could not know that a fire today is ignited by a match struck day after tomorrow, so we wouldn't be looking to prevent the future causal B-event, and thus would not stop it. This problem thwarts the bilking action and invalidates the argument.

On the other hand, the bilking argument fails against precognition as well. Remember, to succeed the argument requires that we first learn the content of present event A which consists in the predicting of the nature of future event B. Upon learning A's content, we then await the arrival of the time-point when future event B is to occur, and arrange either for B not to occur, or for B to be changed so as to come out differently than predicted.

But is this really sufficient to prove that there is no causal link between a future event and a past one? I would argue no. Rather than "bilking" a precognitive prediction, what has occurred instead is that the forward-moving causal chain *C2* has merely preempted the original causal chain *C1* which would otherwise have been realized had the preemption not occurred. This is captured by the counterfactual example: "The thrown ball would have broken the window if William had not jumped up and caught it in his baseball mitt." There is nothing special in preemption – it happens all the time. And simply blocking original causal chain *C1* by interposing an interloper, *C2*, does not at all show that the *C1* was causally impotent, any more than it would in a standard

forwardly-causal case.

For an ARV experiment, the bilking argument would be even less plausible. This is because preventing the B-event – the feedback phase of the experiment, the revealing-of-the-apple – will not necessarily “bilk” the process after all. How is this the case? For clarity I present it stepwise below:

- 1) The event E actually being predicted (stock close, athletic event) by the association of target objects is intermediate between present event A (remote viewing sessions) and future event B (feedback to viewer), and,
- 2) E can be so chosen such that the bilking of B has no power to halt the predicted event (e.g., the closing price of a stock or the resolution of a sporting event on day-zero, neither of which can be interfered with by processes normally within a bilker’s control).
- 3) The members of the associated target set (e.g., apple and pencil) are selected and associated with outcomes even before the occurrence of the predicted event.
- 4) The tasker knows upon closing of the market or completion of the sports event (thus, again, even *before* feedback-event B) which target object will be presented to the viewer. So bilking of event B alone will not halt the process, since the correct target has already been selected and indicated, even if it has not yet been presented to the viewer.
- 5) Some remote viewing results have shown that viewers can sometimes produce veridical results even if they never receive feedback (Targ *et al*, 1985; Targ, 1996; Smith, 2005; May, 1989).

From the preceding it is questionable that an ARV experiment could be ‘bilked’ by interfering with the downstream B event. But what if B were bilked earlier in the process, say along the following lines: the judge is prevented from learning the outcome of the stock closing or the game, and thus the judge does not know the final outcome of the predicted event and B cannot take place. Or even earlier, the judge is prevented from

even forming a target pool in the first place. In informal ARV experiments circumstances similar to these have transpired for various reasons. Usually, the remote viewer produces nothing correlating to either target (this is trivially the case in those situations where a viewing was done but no target pool had been selected, as there was nothing specified to be viewed). But as noted above, this is not really a case of “bilking” so much as a straightforward causal preemption that can occur when any causal chain is blocked by some other interfering one.

Time Loops

But what of closed time-loop paradoxes – do they not pose an insurmountable problem for precognition? Again, I argue no. Consider the case Anglin (1981) borrows from Carl Jung, wherein an individual arrives in Spain and upon entering a certain street, recognizes a scene from a previous dream. In the dream he had perceived a cathedral in a city square, “which exactly corresponded” to the scene he now sees before him. Now in a waking state, he decided to go towards the cathedral, but then remembers turning right and going around the corner to another street in his dream, so makes that choice instead. Rounding the corner, he is immediately confronted with “the carriage with the two cream-coloured horses” precisely as he remembered them from his dream (p.90).

Note the circular causal structure of this example. As Anglin points out (assuming for the argument’s sake that the story was veridical), “his turning right at the corner in reality was caused by his remembering that he did so in the dream.” But the dream, being precognitive, was presumably brought about by his future presence in the

city, his turning that corner, and then his seeing of the carriage. So his dream caused him to turn the corner, but his turning of the corner caused him to so dream. This yields the causal structure I specified earlier in the chapter when first introducing closed temporal loop paradoxes: “person A fulfills an action the following day which was seen in a dream the night before. But the action would not have been performed without the dream as impetus, and the dream would not have occurred without the action taking place the following day.” So an action causes an effect which causes the action, in an endless cycle. This is deemed a paradox and is thus, according to Mellor’s argument (1981, 1991), impossible. But it is not, I shall argue, impossible for certain species of precognition. Consider a case I learned of recently through personal correspondence.

I’d purchased an unusual carved opal and had it set into a ring. In my dream I was sitting in my old VW Bug with my hands on the steering wheel so that I could see tops of both hands and my ring looked horrible – the stone was gone and ugliness was there instead.

One to two days later I was moving – putting my hands into deep weeds to pull stuff out and so forth and loading into and onto my car. Finally loaded up I got into the car – started it up and put my hands on the steering wheel – just like in my dream. OMG!!!! The opal had fallen out of the setting. What was left was a broken apart cork under-padding. And it looked horrible – exactly like in the dream, only in color – the dream hand [had been] in black & white or gray.

I jumped out of the car with the intention of retracing my steps of the previous 3-4 hours. I ran to the first area that I had worked – an area of small pebbles – standing there on the pebbles I realized the futility of looking further. Then I looked down and there right between my feet was the opal lying face up – had it even been face down it would have been hard to distinguish from the pebbles. I picked it up and subsequently had it reset and still wear the ring today. (Potter,

2009)

Note that though this account involves a precognitive dream that (taking its veracity for granted) depicts actual events which subsequently transpire, there is no closed causal loop here. The dreamer receives information in her dream about a future event (that the cherished stone is missing from the ring), but no actual further action is presented in the dream. Her subsequent actions occur in a non-pre-specified, apparently fully agented manner. She then goes on about her life. In this case, it seems, any precognitive form of retrocausation that may be involved is not prohibited by Mellor's proscription of closed-loop paradoxes.

But it also seems the missing opal story could have presented just such a forbidden loop had it, say, lasted a little longer or added some additional features – such as portraying the dreamer exiting the car, going to the spot, and finding the stone. If such had occurred in the dream, then in reality, we see an explicit formal similarity to the case Anglin worries about being a paradoxical closed loop. It may strike us as odd, though, that the mere adding of a sequel to the original dream should nudge us over the border into forbidden territory. Let us reexamine the causal chain at work here, adding our notional sequel. The dreamer dreams of her ring missing its stone, her actions upon noticing the stone is missing, and culminates with her finding the missing stone. Two days later she revisits the dream in the waking state discovering, just as she remembers from her dream, that the stone is missing. Now alerted, she recalls from her dream that she exits the car, goes to a specific location, and finds the missing item. She goes through the motions she remembers from her dream and indeed locates the stone. Then,

as in the previous instance, she goes on about her life.

We discover here the same variety of apparently closed causal loop objected to by Mellor and Anglin: the dream portrays certain future actions and choices of the dreamer. Some time after waking, the subject of the dream, guided by recollections of what was dreamed, executes the actions as remembered which – of course – provides the precognized content of the earlier dream. *But then the dreamer goes about the rest of her life.* At best, it seems, there is but one loop, and it is merely informational, and not causal except in the very limited way that general information is causal in informing decision making for action. The dreamer in this case could have chosen to look somewhere else for the stone, or left without it, just as the dreamer of the Spanish city could indeed have elected to go on to the cathedral, which interested him more, rather than satisfy his curiosity by choosing instead to turn the corner. In either case, the loop is only gone round once, and then each dreamer moves on down the time stream. So it brings to question whether the sort of causal loop condemned by Mellor and Anglin is really the vicious paradox it seems.

Now consider how this works out in the cases of presentiment and ARV. In presentiment, there is a standard forward causal chain that has the following links:

- 1) At time t the participant takes her place in front of the computer and is wired for autonomic monitoring.
- 2) At time $t+1$ the monitoring equipment detects and records an autonomic arousal indicative of the imminent portrayal of a disturbing image on the computer screen.
- 3) At time $t+2$ the computer randomly selects a photo from its target data base and displays the image on the screen.

4) The experiment concludes.

Contrary to what occurs in the cases of the dreams, no action is prescribed for the participant by the experience in the presentiment case. Yet some kind of perceptual content clearly passes between future ($t+2$) and the present ($t+1$), else no unconscious reaction indicating the pending display of an objectionable photo would have occurred. There is a causal loop here, but it is neither closed nor paradoxical.

Consider now the associative remote viewing case, which has a forward-moving causal chain as follows:

- 1) At time t the viewer is asked to describe the object he will be presented at time $t+3$ in the future.
- 2) At time $t+1$ the tasker/judge selects the target pool, and associates each target object with an outcome of the event to occur at time $t+2$.
- 3) The to-be-predicted event duly occurs at $t+2$, and the judge learns at that time which target object should be later presented to the viewer at $t+3$.
- 4) At $t+3$ the judge presents the viewer the correct target object.
- 5) The experiment concludes.

Once more, there is some sort of loop uncaptured by conventionally-perceived causation that facilitates perceptual content passing between the future ($t+3$) and the present (t). Yet this loop, too, is neither closed in the way the precognitive dream sequences were, nor a paradox. Nonetheless, that kind of loop seems to qualify as a legitimate precognitive event.

Nothing up until now explains *how* event B affects A causally. As Broad puts it: “...there seems to be no possible causal explanation of why a certain image which I now have should correspond to *any* future event or to *this* rather than to any other of the infinitely numerous events which will happen from now onwards” (1937, 194). But that they in some way are related seems unmistakable. A subject in a presentiment experiment manifests autonomic arousal before disturbing photos, but remains unaroused before pastoral scenes. The viewer in an ARV experiment sketches strikingly similar representations of the target photo she is shown, sufficiently clearly that a judge makes the correct selection tagged to the outcome of a future event as much as 70-80% of the time when chance would be 50%.

Skeptical of precognition though he is, Antony Flew would have to consider seriously this juxtaposition of events, the dependency of whose earlier outcomes on future events are so strongly apparent. In arguing against Dummett’s Humean “quasi-causal” theory, Flew attacks Hume for failing to recognize that, not only do we attribute causation to the constant conjunction of one event with another, but that it goes beyond that. A constant conjunction of one event that regularly occurs when a preceding event occurs, continues especially whenever – and only when – we intentionally undertake to *bring the occurrence about* by introducing the precursor event often and under controlled circumstances. “We are not, as [Hume] seems most of the time to be suggesting, confined to *noticing* that A and B always come together, and in that order” Flew contends. “But can check our guess that A is the cause of B by *bringing about* A and seeing whether B follows; and if we are right we can regularly *produce* B by bringing

about A.”²¹

As large volumes of experimental evidence show, we can indeed with fair reliability produce one event by bringing about another which is clearly paired with it. It just so happens that this occurs in what is standardly considered to be the “wrong” order, and it happens without any discoverable causal mechanism.

So this is where we stand: I am persuaded I have successfully argued that *when properly construed*, precognition as influencing the present from the future is logically consistent. But we still have no way of accounting for it. The available proposals to give precognition a physics explanation are unconvincing and unlikely. Yet it is unavoidable that a significant body of well-supported evidence exists for precognition when, according to the principles of science as presently understood, no such evidence should possibly exist. Further, for each of the precognitive events incorporated into this body of evidence, we know the starting point of a causal chain, and we know its ending point. But there are no discernable causal links between the two, yet we have strong reason to think they are causally linked and are not just coincidental. We also can be fairly confident that they are not merely correlated due to separate links back to some common ancestor.

Besides the seemingly reversed direction of causation inherent in precognition, another of its features that opponents find unsettling is precognition’s lack of temporal

²¹Oddie (1990) agrees on this point: “In performing an experiment to test the hypothesis we force an A-type event to occur and see whether a B-type event results.” He elaborates further on the need to make sure any relation between A and B events is strictly causal and not one of correlation attributable to some earlier common cause.

contiguity between cause and effect. That this contiguity is seen as important for our concept of forward causation is argued by Sarah Waterlow (1974). “The temporal continuity of cause and effect is something we cannot afford to drop from the concept of causation as a relation between earlier and later events, if that concept is to preserve the implications that the term ‘causation’ would suggest” (p.376).

Consider a time gap between the cause and its effect that is empty of anything that is causally relevant, leaving us “no ground for expecting the effect to occur at or within any given time...after the cause.” Waterlow suggests the upshot would be that we would then “lack any rational basis for our procedure of matching up particular cause-events with particular effect-events” (pp.377-378). We would have no temporally-founded reason to associate a cause with any number of effects that might occur downstream of it.

We see a similar problem with precognition (at least of the sort I have in mind), though temporally reversed, in that downstream (future) cause B is linked to an upstream effect A in the present, and yet across instances of precognition there seems to be no fixed intervening time interval where the effect dependably follows the cause (in other words, time intervals from B \rightarrow A *may* vary with each individual case of precognition).

There are two ways one might respond to this problem. First, the human intentionality inherent in a precognitive event identifies the two events which are correlated one with another. It is not just ‘any old’ future cause and ‘any old’ present effect that are in question, but a very specific linkage of the two, which is recognized in advance by the precognizer as arranged according to the experimental protocols. Secondly, in an experimental setting the start and end points can be specified (e.g., the

sketching of the apple will occur at $t-1$, whereas the presenting of the apple will occur at $t+1$). If the expected future causing-event and present caused-effect occur as specified (especially if this happens numerous times over iterations of trials), we have a strong empirical case for causal linkage despite the presence of apparently un-causally-filled temporal gaps between. This at least partially meets Waterlow's criteria that "events of one kind can only be supposed to cause events of another on the assumption that any time gap between them would be of regular length" (p.378).²²

My argument, of course, is not that there is *no* causal connection between future cause and present effect. But if reverse causation within the physical domain is not the answer, then what might be? Taken together, the future-influencing-the-present argument and the irrelevance (and perhaps non-existence) of micro-scale time reversals strongly argues that precognitive data is evidence for intermittently truncated causal chains, or ITCCs. Described in chapter four above, ITCCs (I contend) strongly indicate violations of the causal closure assumption. Indeed, consider the discussion of spatiotemporal locality presented in that chapter. For spatiotemporal causation to hold, both spatial *and* temporal locality have to be consistent. It is not enough for a complete set of causes to occur in sufficiently close spatial proximity to their effect. Appropriate *temporal* causal contiguity is also essential. If spatial locality holds but temporal locality is violated, yet a causal effect nevertheless occurs, it becomes a case of non-local causation unaccountable by physical explanation other than through appeal to brute

²²Merely having a regular time interval between cause and effect is not yet sufficient to establish causation, though, according to Waterlow.

relationships – which I discussed in chapter four as being physically problematic.

However, such non-local causation is *exactly* what we would expect to see in an ITCC involving a temporal violation of causal closure.

But how could an ITCC work in the present case? If human perceptions were not restricted to the confines of the time-space continuum, then it would be a (conceptually) simple matter to “pop out” of the present and “pop in” to the future to access the future event, much in the case of migrating salmon, where water flows from *upstream* to *downstream* around a dam through a fish ladder, allowing the salmon on their way to spawn to get around the dam by taking the fish-ladder detour *upstream*, avoiding the otherwise insurmountable obstacle.²³ Of course *how* this might work in terms of “mechanism”²⁴ remains completely mysterious – as would be the expected case if a truly non-physical aspect of the universe were involved.²⁵

²³Worries about precognition and the free will problem are not relevant here. Though some who argue for precognition speak as if it is always successful, in reality precognitive results only reach significance if an “assured future” is involved – that is, future feedback is guaranteed to be presented. When precognition is aimed at predicting future events in the broader future time-stream, success plummets, suggesting that in general the future is indeed *not* fixed in the way some metaphysical models presume.

²⁴“Mechanism” in scare quotes here to show it is a place-holder word for whatever thus-far unspecified process is involved – which may, in fact not resemble a “mechanism” at all.

²⁵At least two conceivable “fish-ladders” that may be available: a timeless non-physical domain that perhaps (thinking wholly speculatively) consists in a realm of pure consciousness or intentionality (though see Chalmers, 1996, for a serious consideration of this). An equally speculative candidate is the oft-discussed notion of parallel worlds. How either would allow ingress and egress between human consciousness and the physical world is unknown and, perhaps, unknowable. Most importantly, either option entails the falsity of the causal closure assumption.

Chapter 10: Evaluating the Evidence – Perception-at-a-Distance

My task in analyzing the notion of reverse causation in terms of precognition was fairly involved. I first had to determine whether the idea of a future event changing the past really *was* logically inconsistent in principle, as some argue. I discovered that regardless of whether the notion is logically inconsistent, changing an event embedded in the past does seem to be impossible or, at the very least, highly implausible. In the process though, I argued that framing precognition in terms of reverse causation is generally a mistake. A different, more accurate formulation would make precognition a matter of the present being affected from the future and that, as far as precognition is concerned, changing the past is in fact not even on the table.

I further argued that micro-physical accounts of backwards causation are unlikely to be explanations of macro-level effects such as those displayed in precognition. I went on to argue that neither the bilking argument nor closed temporal loops seem to pose obstacles to at least certain varieties of precognition. Finally, provided what I take to be convincing reasons why precognitive ESP effects I have offered as evidence do serve as provocative candidates for violators of causal closure.

My next task will prove to be somewhat easier and less complicated – to show that ESP action-at-a-distance (which I prefer to describe as “perception-at-a-distance”) in its various guises also presents persuasive evidence for a closure violation.

Perception at a Distance/ESP

I make the following claim: Certain kinds of ESP allow a perceiver to obtain impressions, experiences, and information about a distant location, object, event or living being even in the absence of any physical or physically-related channels of communication or sources of information between the target and the perceiver. For my purposes here, I shall refer to this as “perception at a distance,” a terminological choice that implies a kind of action at a distance, but one using perceptual channels and mechanisms. Action at a distance requires something to act and something to be acted upon. As I discussed in chapter four, action-at-a-distance in normal usage implies “local” causation – a transfer of causal influence through a medium, through adjoining objects or constituents, or via waveforms that may propagate through space. To support my overall claim against causal closure it is necessary to show “non-local”¹ causation – a causal effect unaccountable through physical or physically-related mechanism. Recall again from chapter four that *spatially* local causation requires a complete set of causes to occur in sufficiently close spatial proximity to their effect. If it can be shown that certain phenomena violate spatial locality, then a physically-unaccountable non-local causal effect has been demonstrated.

There are only two known physical modalities through which causation can be passed along from one spatial location to another. The first is mechanical – adjacent physical bodies interact to conduct a causal impetus from its initiation through to its terminus. I include many notions of “physical bodies” under this description, ranging

¹Again, as distinguished from quantum nonlocality, which I will discuss more thoroughly in a few pages.

from particles to large macro-scale objects. I also include a broad spectrum of forces and interactions under the general category ‘mechanism’ – momentum, expansion, rotation, induced vacuum, chemical reaction, leverage, hydrodynamics, air pressure, hydraulics, and so on are a few of the mechanical forces or causal interactions that may transfer causality through to effects that occur distantly from their original progenitor causes. Under my definition certain perceptual modes – olfaction, gustation, tactile experience, and audition – can be categorized as being fundamentally mechanistic.² Two of these, audition and olfaction, approximately count as modes of perception at a distance. Despite this, though, it is clear that mechanical processes cannot explain the information transfer displayed in remote viewing, precognitive remote perception, and associative remote viewing, and we can quickly dismiss from our consideration the possibility that mechanical means is explanatory in this context.

Electromagnetism as a Causal Explanation for ESP

The second modality that might plausibly be a candidate explanation for perception at a distance is electromagnetism (EM). Electromagnetism already plays a role in normal perception at a distance, the main example being vision, which relies almost exclusively on interaction of biological structures with electromagnetic impulses to provide visual perceptual experience. And unlike mechanistic action at a distance, electromagnetism operates virtually instantaneously (over terrestrial distances) and can

²Ignoring for now the electro-chemical aspects of brain processing of sensory inputs.

propagate through space and (to some degree) intervening barriers. This description begins to show more resemblance to the phenomena reported in the types of ESP under consideration. Indeed, historically it was long thought that one form of electromagnetism or another would be found to explain ESP.³ Nevertheless, there are strong reasons now to believe that electromagnetism cannot be an explanation for ESP. The first of these is philosophical.

If electromagnetism were to offer an explanation for the kind of ESP under consideration here, it would by its nature require some form of information transmission. C.D. Broad (1953, 37ff) offered a thought experiment based on a typical card-guessing clairvoyance experiment typical of the sort conducted at the time.⁴ Consider a setup where the percipient would be asked to describe the face of the sixth card from the top in a stack of specially-designed cards placed face down on a table in another room. The cards are opaque and have identical backs, but their faces vary as to number and color of either circles or squares printed on them.

Say the front of the sixth card down contains eight squares printed in red ink, then the task of the percipient will be to determine four things without using any normal

³Even today, many individuals unfamiliar with current research in the field espouse a naive view that “brain waves” emanating from the subject’s head explain ESP.

⁴Though I don’t include “card-guessing” clairvoyance experiments as part of my evidence, there is a very large experimental data base containing scores of thousands of such trials, demonstrating very high statistical significance (even excluding some controversial cases, such as the Soal/Shackleton research). I refer to card-guessing because it is the case Broad uses, and the same argument goes for other clairvoyant claims as well, such as remote viewing. It is also an argument, by the way, why Superman could have no such thing as X-ray vision.

sense-perception modality: which is the sixth card down; that there are eight shapes on its face; that they are square; and that they are red. Visible light would not allow the percipient to “see” the card. If some other kind of transmissive electromagnetism were involved, it would have to possess the following features: It would have to either be received and transduced within the brain of the perceiver, or it would have to be generated and projected by that same brain, and emanations rebounding from the face of the card be received and transduced. It would have to penetrate no deeper than the sixth card down and no further – that is, the five cards above it would in some sense have to be penetrable by or “transparent” to the perceiver’s inspection so that the intervening images would not conflate the percipient’s impressions. The cards below the sixth would have to remain opaque for the same reason.

Whatever field or force involved would have to be able to detect and resolve for shape, number, and color scheme – all otherwise detectible only in the visible spectrum. As Broad describes it, “we shall have to suppose that the percipient’s body is being stimulated by some kind of emanation from the front of the sixth card in the pack, although the back of the card is towards him. We shall have to suppose that the five cards which are on top of the selected one are transparent to this emanation, though they are not transparent to light” (38).

Needless to say, no electromagnetic phenomenon is able to accomplish such a feat. To insist that any form of transmissive phenomenon can make this kind of clairvoyance work means that we must, as Broad concludes, “suppos[e] that the physical difference between the [markings on the front of the cards] and the background, which

makes the former selectively reflect red-stimulating light-waves and the latter indifferently reflect a whole mixture of light-waves, is correlated with another physical difference which is concerned with another and unknown kind of emanation.” Broad observes with some irony that “this is certainly not very plausible.”

The situation with remote viewing is not much different. In RV, similar cases of color, number, shape, plus other features of a target both concrete and abstract must be accessible to a viewer at varying distances, orientations, and despite a variety of intervening shielding. If a transmissive model seems implausible in a simple card-guessing case, it seems much more so across the range of remote viewing contexts.

This argument allows us to rule out *a priori* wide sections of the electromagnetic spectrum (Smith, 2005). Thus, frequencies above the visible light spectrum are easily excluded. Gamma radiation, the top end of the spectrum, and X-rays have insufficient penetrating power to account for long-distance remote viewing, are strictly line-of-sight, and are incapable of carrying the kinds of information available in remote viewing. Lower frequencies down through ultra-violet are easily blocked by intervening materials, and are also not information bearing. The visible spectrum and infrared range are also excluded because they are easily shielded. That leaves the sub-infrared radio spectrum, from microwaves down to extremely-low-frequency end of the EM continuum. From their wide use in communications, we know these wavelengths can be means of communications, but it is unlikely that human brain structures could avail themselves of these frequencies without customary hardware-intermediaries such as receivers or transmitters. This is beside the fact that even radio frequencies cannot capture things

such as color and number on the face of a card.

The second argument against electromagnetism as an explanation of ESP in general and remote viewing in particular is more specifically empirical. It consists of a variety of experiments aimed at eliminating various segments of the EM spectrum as conduits for ESP and particularly remote viewing data. There are two branches of the argument. First, some portion of the electromagnetic spectrum might be a candidate as a carrier of remote viewing-related information. Remote viewing trials done in an unshielded environment accessible to that portion of the EM spectrum demonstrate a certain general level of success and quality. An experimenter could create an environment that fully or partially shielded out that part of the EM spectrum, and do a series of remote viewing trials maintaining all other constraints as before. If success levels and/or quality dropped off or if null results were obtained, that would be evidence that the shielded portion of the spectrum may play an enabling role in remote viewing functioning. More trials would be called for to verify the results (perhaps tweaking other variables to make sure no other condition played a role), but there would be reason to believe EM is at least a part of the explanation of RV (and hence ESP). If, on the other hand, the trials showed generally equal success and quality as those performed under non-shielded conditions, then it seems reasonable to dismiss the shielded part of the EM spectrum as causally relevant to RV/ESP (though additional confirmatory trials might be advisable as a check). I shall call this the Blocking Argument.

The second branch of the argument works this way: Different parts of the electromagnetic spectrum have different properties. If certain features of the remote

viewing process are shown to be incompatible with specific properties inherent to relevant segments of the EM spectrum, then we can rule out those segments of the EM spectrum that demonstrate this incompatibility as contributing causally to remote viewing. I shall call this the Incompatibility Argument. Experiments have been conducted exploring both the blocking and incompatibility branches of the argument.

The Blocking Argument

The entire sub-infrared range of the EM spectrum is susceptible to empirical tests supporting the blocking argument. This range includes millimeter waves and microwaves, as well as short-, medium-, and long-wave radio, plus extremely-low-frequency (ELF) radio waves. For a significant portion of this range, blocking is simply accomplished by performing experiments inside a Faraday-shielded enclosure that attenuates significant portions of the EM spectrum. In the case of the SRI experiments, remote viewing trials were conducted with the viewer sequestered inside such an enclosure, and in some cases with the target enclosed inside and the viewer outside. The Faraday cage used in the Puthoff/Targ research produced 120-dB attenuation in the 15 kHz - 1 GHz frequency range.⁵ Many of the SRI remote viewing experiments described in chapter seven made full or partial use of electromagnetic shielding, as did a number of the DMILS (“direct mental interaction with living systems”) studies discussed in chapter

⁵This covers the radio frequency bands from the mid-ELF range at the low end to the lower portions of the microwave range. The upper microwave and millimeter range do not require specialized shielding because they are easily scattered or blocked by typical building materials and fall off quickly with distance.

six. Yet in all cases the presence or absence of EM attenuation seemed not to affect the quality of the experimental results.

A second shielding experiment had the lower end of the ELF range specifically in mind. Due to their long range and penetrating power, shielding out the lowest ELF signals requires a large thickness of intervening material. A sufficiently deep underground cavern or mine, for example, would significantly shield for ELF. But by far the best attenuator of any radio signal is sea water. Because of its electrically conductive nature, sea water is even more effective than fresh water and even earth, such that only the lowest ELF frequencies penetrate very far below the surface. To test the shielding hypothesis against ELF as a causal contributor to remote viewing, parapsychologist Stephan Schwartz arranged through his contacts to give the SRI program access to a research submersible (as briefly described in chapter eight).

Originally, four trials were planned, but the submersible's dive schedule was altered, making only two trials possible. For one trial, the vessel was positioned at a depth of 170 meters in water 340 meters deep. In the second trial it lay on the bottom in 78 meters of water. Both trials took place with the beacon team 500 miles away. Targets were selected for the beacons using a blind, randomized process from two different six-target pools (one pool for each experiment). Both trials produced one-out-of-six first-round hits, significant at $p = 0.028$. Because only two trials were done, effect size was small. And though significant ELF-wave attenuation was achieved (almost 70-dB in the lower ELF range), ELF was not eliminated altogether. (This would have required a depth of perhaps 1,000 meters, which was logistically beyond the resources available).

(Puthoff *et al*, 1981; Puthoff & Targ, 1977)

The “Deep Quest” submarine experiment, as it is often referred to, did not altogether remove ELF as a causative factor in ESP. But despite significant (if not complete) ELF attenuation, the two remote viewing trials produced quality as high as any previously seen, which seems to point away from ELF as a possible explanation. When taken in light of incompatibility research, as described below, the argument against EM becomes greatly persuasive.

As May says with respect to blocking ELF in remote viewing experiments, “too few data were collected under known shielding conditions to make definitive statements with regard to shielding...The trend, however, is clear: electromagnetic shielding does not inhibit psychoenergetic acquisition of target material” (May *et al*, 1989, 20).

The Incompatibility Argument

The incompatibility argument adds confirmation to some of the reasons already considered for excluding the EM spectrum as a way of explaining remote viewing and ESP. For example, millimeter and microwaves are excluded by appropriate shielding, but they are also easily eliminated by the incompatibility argument, since just by their nature they are relatively short range (attenuated by atmosphere) and line-of-site (unable to follow the curvature of the earth). Though not as susceptible to these problems as millimeter and microwaves, much of the rest of the radio spectrum further down – even as far as long wave (LF, or ‘low-frequency’) are affected by both distance and line-of-site constraints (though to decreasing degrees as wavelength increases).

To show remote viewing incompatibility with these parts of the spectrum, SRI conducted several long-distance remote viewing trials. One experiment included five trials, with viewers in Menlo Park, California, remote viewing beacons who had been sent by blind, randomized protocol to two different targets in New York City (Grant's Tomb and Washington Square Fountain); and one trial from Menlo Park to New Orleans, Louisiana (the Louisiana Superdome); plus one from New Orleans to Menlo Park (a bank plaza); and finally one from New York City to Ohio Caverns in Ohio (this had the added feature of shielding by virtue of being up to 150 feet underground). (Puthoff, *et al*, 59-68)

There were other (some previously mentioned) replications of long-distance targets, among them Bisaha and Dunne (1979a – Wisconsin to Eastern Europe), Schlitz and Gruber (1980/2001 – Detroit to Rome), Schlitz and Haight (1984 – North Carolina to Florida); and Targ *et al* (1984 – Moscow to San Francisco), plus numerous trials against varying targets world wide by the classified military program (Smith, 2005). All these together show that the upper-, mid-, and (to some extent) low-range radio frequencies can be excluded as causally-relevant for ESP.

Though definitive for the higher ranges of the radio spectrum, long-range remote viewing experiments were less persuasive for ruling out the lowest-frequency bands, since some of these radio waves (especially in the ELF band) can travel long distances and are relatively unaffected by intervening obstacles. Though all radio waves propagate linearly, under the right atmospheric conditions lower-frequency waves can be reflected and reach around the globe, and ELF particularly has a curve-following characteristic

that allows ELF waves to guide around the planet. ELF waves are a special case in other ways, as well. They travel the farthest and penetrate the most deeply into obstacles and intervening substances, to the extent that they are the only form of radio communications means with submarines (though bandwidth is very limited, and only the most rudimentary messages can be sent).

So a different sort of experiment had to be designed to test for the causal relevance of these characteristics for RV and ESP. Lower-frequency radio waves have at least two features that can be checked using remote viewing experiments. The lower the frequency, the less the amount of information that can be carried. ELF, for example, has a very low information transmission rate, amounting to only a few bits per minute in the lowest ranges. Further, longer-frequency waves are less able to resolve smaller-scale objects. The longer the wave length, the larger the object must be to be detectable and resolvable at that wavelength.

From the perspective of information transmission, the amount and scale of information received in a high-quality remote viewing session suggests that lower-frequency radio waves cannot account for the phenomenon – especially considering the remote-viewing production of sketched images, which require relatively high bandwidth. To test a size-resolution hypothesis, the SRI researchers conducted a series of experiments against targets of decreasing scale. The technology series, mentioned in a previous chapter, used a target pool of mid-sized objects, such as computer terminals, shop equipment, typewriters and teletype terminals, and so on. (Puthoff & Targ, 1976; 1977) The next series of targets consisted of ten small items secured in light-proof film

canisters secured against sensory leakage to the viewer and other participants, and kept altogether out of the viewer's presence prior to and throughout the entire duration of each trial.⁶ These were remote-viewed one at a time over a several-day period. Subsequent to this experiment, a further iteration was performed, this time using 1 millimeter-by-1 millimeter microdot photographs.

These and other experiments returned statistically-significant results, with transcripts demonstrating comparable quality to those produced by larger-scale targets. The researchers commented that “we thus obtained evidence that small objects can be discriminated by psi processes, and that the channel functions down to at least the order of a few millimeters spatial resolution.” This level of resolution “would seem difficult if not impossible to accommodate under an ELF hypothesis” (Puthoff *et al*, 1981).

The following attempts and observations have been made to determine what role, if any, electromagnetism may play as an explanation for ESP:

* DISTANCE: Long-range remote viewing and remote perception experiments showed that low-frequency portions of the EM spectrum could not explain remote viewing results.

* SHIELDING: Remote viewing and DMILS experiments have been conducted under a variety of shielded conditions designed to exclude various portions of the EM spectrum, with no discernible decrease in general significance or quality of results.

⁶In fact, by the design of this experiment no one knew the nature or identity of the target – not even the beacon person – until after completion of the trial. This was intended to verify the long-standing presumption in the SRI research that a “sending” of information was not involved.

* SCALE: Remote viewing experiments have been performed with a variety of targets of decreasing scale with no discernible decrease in general significance or quality of results.

* DATA RATES: Information transfer rates in remote viewing/remote perception trials are too high to be accounted for by lower-frequency EM radiation.

With the successful precognitive experiments such as the PEAR lab's precognition-based protocol and the ARV experiments discussed in chapter eight (which can't possibly be explained by either a linearly-propagating signal or field emanations, both of which are confined to real-time) factored in, it becomes highly implausible to consider electromagnetism as a candidate to account for remote viewing and ESP.

Non-locality/Quantum Entanglement

So having ruled out mechanism and electromagnetism as ways of accounting for the phenomena under consideration for causal-closure violators, what is left? One remaining feature of physics has often been offered up as a way of explaining ESP without having to wander outside the physical universe to do it. Quantum nonlocality (also known as quantum entanglement) has been established theoretically and empirically. It demonstrates an interesting and real phenomenon at the quantum level, in which matched particle-pairs can be "entangled," after which they show correlated behavior that is synchronous and independent of spatial separation. If for example a pair of photons have been appropriately entangled, then by intervening to change an attribute of one, the equivalent attribute of the entangled "sister" photon will instantly register the opposite value of the property so altered. This occurs despite the distance between the

entangled pair up to galactic scale or beyond, and instantaneously – with no elapsed time.

Because this demonstrates a *cause-like* behavior independent of time and distance, parapsychologists and consciousness theorists have proposed nonlocal correlation as a candidate physical explanation for otherwise physically-recalcitrant consciousness-based data and phenomena such as ESP. Those who have at least entertained a quantum non-local explanation for puzzles of human consciousness range from parapsychologists (Puthoff and Targ 1977; Schwartz, 2007; Jahn & Dunne, 1986, 1987/2009) to scientists (Penrose, 1989; Hameroff, 1994), to philosophers (Chalmers, 1996).

It is understandable why researchers and theorists might find quantum nonlocality attractive as an explanation for ESP. For one, ESP and related phenomena demonstrate attributes that could be described as “nonlocal” in the way they behave – e.g., seeming independence of distance, apparently instantaneous occurrence, apparent causal independence (that is, initiating event and subsequent results show no discernible causal link), and so on. The apparent analogical parallels between how parapsychological events occur and how quantum nonlocal ones occur is explanatorily attractive to many researchers. Second, if *quantum* nonlocality can successfully accommodate *parapsychological* nonlocality, then ESP becomes acceptable within physics, and hence physicalism. The argument between the two paradigms evaporates, and controversies go away.

Unfortunately, convenient as it might otherwise be, I do not see that quantum nonlocality is capable of explaining ESP. First, the kinds of correlations one sees in

parapsychological phenomena – at least of the sort I am considering here – seem to be clearly causal, whether or not we are able to identify the entire set of causal linkages from start to finish of the relevant causal chains. On the other hand, quantum nonlocal events are distinctly *acausal*. They are mathematically-correlated consequences of certain features of quantum field theory between the instances of which there is provably no causal linkage.⁷

Second, contrary to the case with ESP-related phenomena, quantum nonlocality is non-information-bearing (Lange, 2002, 280).⁸ Quantum theory dictates that no information is passed by virtue of the entanglement relation. Finally, the nonlocal quantum phenomenon is of insufficient scale to account for the macro-level effects that are involved with such ESP phenomena as remote viewing. A physics-based non-local explanation for remote viewing and similar behaviors would require reliable entanglement of human-scale biological systems with each other and with equally-large scale or greater non-biological objects. This seems *prima facie* highly implausible, and is

⁷How this can be I freely admit to finding completely inscrutable – we humans are, after all, incurably fond of framing the world in terms of cause and effect. However, I have seen quantum nonlocality worked out mathematically in a fairly comprehensible way on a couple of occasions, and several physicists have carefully attempted to explain it to me. Despite my otherwise lack of comprehension, I am persuaded they are correct as to the *acausal* nature of the phenomenon.

⁸I say this despite currently-ongoing efforts to engineer some kind of communication ability using entangled photons. The artificial constraints necessary to isolate the photons so as to preserve the entangled relation from being ‘disentangled’ through contact with the environment would seem to rule out the conditions necessary outside of an appropriately-designed containment procedure. An appropriate analogy might be electricity – it exists in a natural state, but is not a means of information transfer unless artificially controlled through telegraph or telephone, etc.

certainly not supported by any known aspect of physical theory – nor does it seem likely to be.

But if mechanism, electromagnetism, and quantum entanglement are unable to explain relevant ESP phenomena – what then? We are left with the point that I argue for – that despite the persuasive and persistent evidence for ESP-type effects, there is no satisfactory explanation to be found strictly in the physical world. I conclude that perception-at-a-distance such as that present in remote viewing and related effects is evidence of intermittently-truncated causal chains that include some extra-physical links to complete their causality.

In this and the previous chapter I have tried to argue persuasively that the ESP evidence I have described in relative detail is evidence for my thesis, that causal closure and, therefore, the universal formulation of physicalism is false. Now I turn to a consideration of why this body of research should be deemed admissible as scientific evidence.

EVIDENCE

There are, to be sure, those who are dubious about the sort of evidence I have presented. A common complaint heard among those who are skeptical but less careful in their use of language is that “there is no evidence” for parapsychological effects. As I have shown, this objection is groundless, for there is a large body of such evidence for ESP and related effects. Sometimes skeptical voices complain that there is no evidence when they really mean this as shorthand for “there is no evidence that is

uncontroversial,” or “there is no evidence that I will accept,” or “there is no evidence that is scientifically respectable.” It is to this last phrasing of their argument to which I next turn.

Evidence fills a number of important roles in science: Some argue that, in its form of observed instances of base phenomena, it sometimes provides the raw material which leads first to hypothesis formation and then on to theory development (Hanson, 1958/1983; Achinstein, 1996, 181). Necessary here is clarification of a point that is often tacitly assumed, but seldom overtly stated. Phenomena may lead to the formulation of a hypothesis, but nothing can count as evidence until a hypothesis exists to which it can refer (Adler, 1989, 231). Once a relevant hypothesis is established, then some or all of the source phenomena (plus other, newer evidence) can count as evidence for the hypothesis, but not before. This is more than a mere bookkeeping rule. Until the hypothesis exists there is no context and no explanatory framework for phenomena/evidence to be counted for or against.

Some would argue that parapsychology, lacking as it does an explanatory theoretical context (to be discussed shortly) does not therefore produce “evidence.” However, in the context in which I am considering the body of research, this is too facile a dismissal. Parapsychological findings *do* constitute evidence in the theoretical context of universal physicalism, as they present evidence *against* the fundamental thesis. Since they are thus members of the set of all evidence with a bearing on physicalism they do count in this domain as evidence so long as they meet other criteria for what is to be considered scientifically-admissible.

Evidence functions to add confirmation to true hypotheses. But it has a dual-edged role. It can also count against a false hypothesis. Evidence of the right kind is necessary to distinguish the superiority of one hypothesis over another. And in a predictive role, evidence serves to confirm not only a given hypotheses, but the larger theoretical context of which it is a part. My next step is to examine the evidence presently under consideration to see if it meets minimum acceptability requirements for the roles just mentioned.⁹

Evidence functions as an indication that a certain condition or state of affairs is the case. Scientific evidence can be concrete or, more rarely, abstract, and may be considered to consist in events, relations, traces, residue, objects, and so on. Controversially, evidence may be subjective, that is (for example) noticeable and reportable by only one person on introspection and not easily accessible to public observation. Or, alternatively, it may be objective – in principle observable and reportable by any appropriately situated and equipped observer. Items of evidence may be of different values or weights, and these weights may change according to the circumstances and background context in which the evidence is embedded. Relative weighting of evidence is conferred by intentional systems, e.g., conscious entities such as human beings (and perhaps higher animals). Bayesian probability calculus is often called upon to provide a framework in which to think about how evidence counts (is weighted) in telling for or against hypotheses, etc. However, for our purposes here it is

⁹I don't mean here to discuss the vast range of concerns, problems, and considerations that revolve around the theory of evidence. I will limit my discussion to

important to identify what features we should expect or demand in order to know whether individual bits or packages of evidence can be considered as appropriately vetted in terms of their scientific acceptability. To accomplish this, I propose the following set of evidentiary criteria.

EVIDENTIARY CRITERIA

I have found little thus far in the philosophical literature arguing for a specified set of criteria that must be met for evidence to count as scientific. It may be that evidentiary criteria of this level are more specialized than is required for philosophical analysis. For my purposes here, though, such a list would be useful. I have managed to winnow out a number of qualifications that may plausibly count as qualifiers for scientific evidence. These are relevance, independence, diversity, repeatability, accordance, publicity/objectivity, and congruence.

Relevance

That evidence which more strongly and directly supports a hypothesis, the more relevant it is for confirmation of that hypothesis. This is an intuitive concept for which there is some Bayesian support (Glymour, 1983/1975). In a Bayesian sense, we might say that relevant evidence increases the *posterior* probabilities of a conjecture, hypothesis, or theory. It is clearly important to determine whether a given piece or body of evidence is orthogonal to what is required to support a given hypothesis, or whether it

what we might agree makes evidence count as scientific.

weakly supports one hypothesis while more strongly supporting another, or whether instead it fully supports the hypothesis to which it is pointed. In many cases the relevance of evidence is immediately obvious. In some cases it may not be so obvious, and in still other cases its relevance may not be determinable until other evidence is collected and assessed against the hypothesis in question. An important role played by relevance has to do with judging the degree of *ir*relevance certain evidence might hold for a given hypothesis it might originally have been intended or thought to support. This criterion has important bearing on the important process of *dis*confirmation – the flip-side of confirmation – which can advance the process of scientific discovery by helping us *eliminate* options and competing explanations that turn out to have flaws or are otherwise unsatisfactory. (Hempel, 1945/1983, 12)

Relevance and the ESP Data:

Broadly, all four categories¹⁰ of ESP evidence that have been considered are relevant in a number of dimensions. First, the accumulation of evidence from all four categories seems clearly to increase the posterior probabilities in terms of establishing that an anomalous effect is present. Conversely, specific elements of the various experimental designs seem to increase the likelihood that known physical explanations are ruled out. Examples include provisions for shielding of various types in DMILS, staring, and remote viewing protocols, blinding and randomization requirements in all four paradigms, temporal offsets in presentiment, PRP, ARV, and some DMILS

¹⁰As a reminder, the four are: Presentiment, DMILS (“direct mental interaction with living systems”) and Staring, Remote Viewing, Associative Remote Viewing.

protocols, and so on. Finally, evidence generated from experiments in each of the individual categories increase the posterior probabilities that each of those paradigms are valid, as well. The fact that the evidence appears to raise the posterior probabilities of a causal closure violation but do not improve the posterior probabilities of the universal formulation of physicalism attests to the relevance of the evidence to the thesis for which I argue.

Independence

Evidence presents stronger confirmation if it comes as a result of a separate evidentiary chain, as opposed to some other piece of confirmatory evidence that itself stems from the same source as other evidence. Elliott Sober (1989) observes: “[I]f I examine...two dependent effects, there is a higher probability that I will obtain relevant information – a match. However, if I examine the independent effects but manage nonetheless to obtain a match, that information will be weightier than a match obtained on dependent effects” (p.285). Sober shows in his analysis that individual pieces of evidence from the same source do add confirmatory value in assessing the truth-value of a hypothesis. But, as is not just plainly intuitive but supportable as well through Bayesian probability calculations, independence confers a noticeably higher level of confirmation than does its opposite (p.280).

The danger inherent in lack of independence (especially if a common dependent source of evidence goes unrecognized) is illustrated by a real-world example from Vietnam, told me years ago by a senior Army master sergeant whom I supervised. An

independent informant had regularly been paid for information he provided about Viet Cong and North Vietnamese Army movements in South Vietnam. On gaining some new intelligence, the informant went the rounds of various US installations and units selling the same information to each. None of his customers knew about any of the others, and reported the newly acquired information through their channels as fresh intelligence. The higher-echelon agency assigned to analyze data acquired from the field treated the information as if coming from several different sources, assigning it much higher probability, and hence confirmatory value, than it would have merited had it been realized that the evidence originated with the same source.

As Sober notes, seeking independent evidence can be risky. “I could be conservative; by examining the dependent effects, I maximize my chance of obtaining relevant information, however, paltry. Or I could gamble; by examining the independent effects, I run a greater risk of obtaining useless observations, but a match between independent effects, should I observe one, would be worth more than a matching of dependent effects” (Sober, 1989, 286).

Independence and the ESP Data:

There is broad fulfillment of the independence criterion both within each of the research paradigms I have detailed, and across categories as well. Within each of the four categories, results are not just produced by one researcher or one lab. In most cases, each research entity (laboratory, institute, activity) within each of the four categories features a variety of experimenters and subjects. Results are usually not dependent on

one person in a given lab, nor are results dependent on one lab within the category.

Though those ESP experimental paradigms with the shortest histories (e.g., presentiment, ARV) tend to have the smallest number of sources of research data (labs and experimenters), even in these there is a variety of sources of successful results and new findings that increase the level of independence for the accumulating evidence.

Moving up to the category level, there are four independent sources of evidence, each addressing a somewhat different (and generally independent) aspect or species of ESP phenomena. There is, to be sure, some cross-category participation of researchers (for example, Schlitz in DMILS but also remote viewing; Radin in presentiment and DMILS; May in remote viewing and presentiment) which acts to generate some level of interdependence. To a degree, this is unavoidable given the limited resources available to the field. But in any case, it seems a relatively minor constraint on the independence factor, given the still fairly wide variety of researchers, phenomena, and unique experimental protocols that belong mostly or exclusively within a single category.

Diversity

Diverse evidence is more confirmatory than less-diverse evidence. On a Bayesian account, “diverse evidence better confirms a hypothesis than does the same amount of similar evidence (Wayne, 1995). Collins (1992) suggests that “the more different an experiment to its predecessor, the more confirming power it has” (p.34). And according to Horwich (1982, 8), “We think that theories are better confirmed by a broad spectrum of different kinds of evidence than by a narrow repetitive set of data.”

But what does it mean for evidence to be “diverse”? Diversity requires a plurality of items or instances – a singular item cannot by definition be “diverse,” so we will be interested in diversity *between* or *among* instances or items of evidence. For the purposes of this discussion, perhaps it will be less confusing if I use a plural form to capture this, hence, rather than “evidence” I will use “*evidences*.” In what ways can evidences be diverse? It seems there might be at least two ways: perhaps they spring from diverse *sources*. Certainly, variety of sources conveys diversity. But as we saw above, that is also what constitutes evidential independence. And in any case, evidence can come from the same source and still be diverse. So while there may be a connection or overlap between independence and diversity, there must still be something more to the diversity criterion.

Better, perhaps, would be that the evidences *themselves* are of diverse qualities, regardless of whether they have common or independent origins. So, for example, in an experiment to determine whether the refractory properties of a translucent material meet a certain prediction of a hypothesis being tested, one might vary the angle at which a beam of light strikes the surface. This would provide a range of values within a certain domain. But one could increase evidentiary value by varying the wavelength, dispersion pattern, or intensity of the light source, thus introducing additional dimensions of evidence that might be telling for the hypothesis being examined.

In an example from a different discipline, if one were seeking evidence to confirm or disconfirm a certain hypothesis about the religious practices of an extinct culture, one could focus on the painted ceremonial patterns on numerous pottery shards from various

digs in a specific region. This would presumably confer a degree of source independence, but would amount to a generally similar type of evidence. But to include a more diverse set of evidentiary support, one might in addition examine funerary practices, dispersion and frequency patterns of the shards, or markings and patterns on other surfaces, such as rock walls, clothing items and weapons, and so on.

This added diversity of evidence adds confirmatory power in a somewhat different way than does independence of sources. Since hypotheses, and by extension theories more often than not make not just one prediction, but a complex of inter-related predictions, diverse evidence may serve to confirm a variety of predictions or even add unexpected confirmation to features that were either not currently being tested or were previously not recognized as being linked or associated.

Along with Wayne (1995) we must keep in mind, though, that “Clearly, judgments of diversity are always made relative to a given theoretical context; what look like disparate phenomena in one context may appear closely akin in another” (p.115). Acknowledging a point he attributes to Glymour, Wayne observes that “the methodological value of diverse evidence is that it reduces the chances of a spurious agreement between an hypothesis and a body of evidence, thereby reducing the chance that an hypothesis will be confirmed yet be wrong” (p.10).

Diversity and the ESP Data:

By its nature, independence conveys diversity as the number of independent sources of results increase. We have seen that there is a least a medium level of

independence in the ESP data. Diversity is also increased by varied tests of the same phenomenon or prediction of a given hypothesis, by varied tests of *different* phenomena, features, or predictions relevant to the same hypothesis, and by fortuitous evidence that may be encountered not as a direct result of the experimental process (for example, Bierman's discovery of evidence for presentiment in mainstream studies that were not originally conducted to test presentiment). If we take as our hypothesis something like "There are extra-physical features to the universe" (the corollary to: "Causal closure – and hence the universal formulation of physicalism – is false"), there is both category-level diversity and internal diversity that increases the confirmatory value of the ESP evidence. At the category level, four different research paradigms produce evidence supporting the thesis where, if the thesis were false, none should.

Internally, an array of different approaches within each of the paradigms enhance diversity, as illustrated below:

Presentiment – Experimenting with a diverse set of stimuli (e.g., affective images; noxious sounds; electric shock; color/color name mismatch; reverse habituation). Monitoring a variety of indicators (e.g., skin conductance; blood volume; heart rate; gastro-encephalography; EEG; fMRI).

DMILS -- Staring vs. DMILS; direct attention monitored by skin conductance vs. remote facilitation of tasks measured by button pushes; monitoring a variety of physiological measures (skin conductance, ideomotor response, blood pressure, etc.); tests of a variety of different biological systems (e.g., fish, gerbils); fMRI vs. EEG; biological systems interacting with machine-based systems.

Remote Viewing – Varied target sets (geographic locations; mineral samples; photos; large objects; small objects; microdots; etc.). Varied tasking means (beacon teams; coordinates; sealed envelopes and other containers). Varied environments (within and without Faraday cages; submarines; long and near distances).

**Associative Remote Viewing and Precognitive Remote Perception –
Experimenting with temporal displacement vs. real time in remote viewing.**

In addition, experimenting with different judging and analysis methodologies adds a certain level of diversity as well. If evidence of a significant effect persists (and even if its level of significance may vary) across different judging and analysis procedures, it provides added support for the existence of the phenomenon.

The fact that evidence for an extra-physical effect is demonstrated across this relatively wide variety of approaches and methods supports the evidence set as being diverse.

Repeatability

I take ‘repeatability’ not to be the same as ‘replicability,’ in that replicability seems to require more than that it just be a repeat of an experiment that produces the same outcome, and encompasses not just the notion of repeatability, but also of independence and diversity. I shall consider replicability in the next chapter.¹¹ Evidence that can be reproduced by the creation of the same or closely similar circumstances in which it was first observed in a repetitive way more strongly confirms a hypothesis than does a singular occurrence of such evidence. Repeatability does not conflict with the principle of independence, as it says nothing about whether the same source or different

¹¹Since ‘repeatability’ and ‘replicability’ are generally used interchangeably in the literature, my differentiating between the two is, perhaps, unorthodox. But I believe there is a distinction that is being conflated when the two are taken to be synonymous.

sources contribute to the evidential repetition. Obviously, independent repetition in most cases provides stronger evidence than does repetition by the same source, and constitutes one ingredient for replication. In the sense I have in mind, repeatability might generally be considered reproducing experimental results by the same researcher or laboratory.

But even same-source repetition lends additional strength to evidence (Sober, 1989). The value of repeatability from a confirmatory perspective is that it gives reason to believe that the reported effect or result was not merely the consequence of an aberration, error, or singular anomaly never to be encountered again. Further, it may be seen to demonstrate at least the beginnings of law-like behavior, and increase our opportunities to study, observe, and evaluate the evidence thus produced. Repeatability may even contribute to the diversity criterion.

Repeatability and the ESP Data:

Repeatability varies within the four categories under consideration. Not every attempt succeeds, and not every success is of the same magnitude. In many cases, the reasons for this are well understood (habituation in presentiment experiments, for example). Further, as with many research protocols in more mainstream sciences, repeatability is sensitive to deviations from the recipe in the successful experimental protocol being repeated (as was seen in my discussion of remote viewing). This alone accounts for a number of the experimental repeats that failed or yielded diminished results.

To be fair, as I mentioned in chapter five and elsewhere, researchers often tweak

protocols in the hope of discovering sensitivity parameters, boundary conditions, or new dimensions to the effects being studied. In cases such as this, an apparently unsuccessful repetition may not be the failure it appears, if it thereby demonstrates a limiting factor to the effect under consideration. A clear example of this was seen in the Ganzfeld research.¹² A series of experiments were conducted using the Ganzfeld protocol but substituted audio stimuli in place of the traditional (and successful) visual stimuli. The researchers freely admitted they were testing the boundaries of the Ganzfeld paradigm, and did not necessarily expect significant results. Nonetheless, the skeptical literature often cites these as “failed” replications of the Ganzfeld protocol.

Still, success has been demonstrated in the majority of attempted repetitions across all four categories so repeatability seems to be a characteristic of the evidence under consideration.

Accordance

Acceptable evidence must be produced in accordance with well-formed and scientifically-trustworthy methods and techniques. “Accordance” does *not* require that acceptable methods cannot be newly-introduced or innovative. Otherwise no progress

¹²In its simplest form, the Ganzfeld protocol involves a “receiver” in a secure room relaxing in a recliner with ping-pong ball halves taped over the eyes, and pink noise played through headphones to establish a slightly altered ‘receptive’ mental state. Meanwhile, a “sender” in a distant room tries to transmit an image from a pool of four targets, while the receiver verbalizes any impressions she perceives. After the session, the receiver is shown the randomized set of four possible target and self-judges which photo was “sent.” Chance is 25% correct, but overall Ganzfeld results range around 35% and are highly statistically significant (Radin, 2006).

could be made in science methodology, nor could accommodations be made for new discoveries that might point towards or even mandate new methodology. Accordance *does* require that new methods be consistent with proven scientific practice, such that they be no less rigorous than previously-accepted methodologies, and that they otherwise be in accord with the standards and procedures of science.

Accordance and the ESP Data:

The evidence-producing practices in the parapsychology research I am considering generally accord with the accepted procedures of science. Indeed, parapsychology in general has in the past and in some respects continues in the present to represent the cutting edge in science. For example, the electroencephalograph (EEG) was invented to track ESP-related experiences. Double-blind protocols, randomization procedures, and meta-analytical and other advanced statistical techniques now ubiquitous in science were originally pioneered in parapsychology (Hacking, 1998; Bosch, 2004; Pratt *et al*, 1966; Radin, 2005).

The studies considered here generally employ standard scientific measures, though in some cases they employ innovative adaptations or create new methods, procedures, and designs (for example, Bem's "mere exposure" protocol, or Radin's presentiment model). Pains are taken to maintain a sound scientific foundation in process, even if the phenomena being examined are themselves controversial.

Despite this, the quality of *some* of the research is mixed. Among the qualified researchers that have contributed to the studies I have considered there are no clearly

badly conducted studies, in the sense of obvious negligence in ignoring standards and procedures that might be expected of competent scientists. Still, there have been mistakes and gaps in the protocols of some studies in each category. For the most part, lesser-quality studies were generally present in the early stages of each of the paradigms – which is not unusual even in mainstream sciences. Peer-review and critical appraisals then do their job, refining protocols and ferreting out weaknesses, errors, and oversights to produce sounder procedures and more trustworthy results. Examples here might be the cues inadvertently left in the early SRI remote viewing experimental transcripts, and the implementation of changing standards in skin-conductance response measuring techniques in the DMILS research. In each of these examples, later work incorporated suggested changes into experimental designs, improving the quality of research practices.

It is expected in science that as a research paradigm matures, procedures get better, yet if an effect is present positive results will still continue to be found. One skeptical complaint lodged against ESP research is that as the experimental protocols get tighter and more refined, effects fall off and eventually disappear altogether. This has, for example, sometimes been the case with forced-choice card-guessing clairvoyance tests and dice-rolling experiments that were the mainstay of early parapsychology experiments. Disappearing results have often been attributed to the “decline effect,” – strong effects demonstrated at first, followed by a decline in significance to the point of extinction of the effect. Skeptics blame this on errors generated by sloppy protocols, and allege that as controls get better, false-positives disappear, resulting in null results. Though parapsychologists disagree and have their own explanations for the decline, that

is less germane to this discussion. In the four paradigms I have considered, increasingly careful protocols have not led to extinguished results, and though effects sometimes vary among experiments, it turns out that significant effects are still manifest.

This persistence of effects is crucial. ESP experiments *must* be conducted in accordance with the best scientific practice. In fact, for parapsychology accordance may be the most important of the evidentiary criteria. As I discuss in the next chapter, parapsychological effects must survive elimination of any source of artifact, false-positive, sensory leakage or cuing, and so on to even hope to be accepted as real.

Publicity/Objectivity

Since by virtue of its privacy, subjective evidence fails to carry the weight that more public kinds of evidence bears, the best evidence for science is always objective when available – that is, a piece or body of evidence must not be accessible to just one person, but must be observable and reportable by anyone who can arrange to be in the right place at the right time under the right conditions and, when required, with the correct equipment. Thus, to be counted as scientific, evidence must be accessible *to* science, so it can be checked, tested, verified, and applied in a clearly objectively public way.

Even if the causes or events themselves are in principle unobservable (quantum-level particles, for example, or mental states) their traces or vestiges must be demonstrable and capable of public examination. Subjective experience may be veridical, but so long as it remains subjective it can't be *shown* to be veridical. Thus, if

the domain of a given body of research involves subjective experience, such as is the case with much of psychology and parapsychology, means must be developed to render the subject evidence objective. Accordingly, special techniques and tools have been created for parapsychology to accomplish the objectification of subjective phenomena. The methodology mentioned above – EEG, double-blind procedures, randomization, meta-analysis, and other statistical methods – were developed or adapted to render subjective or ephemeral data more objective.

Publicity/Objectivity and the ESP Data:

Much of ESP consists of internally-experienced mental states, subconscious processes, or externally-demonstrated phenomena whose public manifestations are but the tip of the iceberg where the phenomena are concerned. But parapsychology has a wide range of tools and methods to uncover with some reliability the presence of effects. More obviously, the self-report of subjects in specifically consciousness-based phenomena such as remote viewing (both *prima facie* and statistical) allow open inspection and comparison. For those phenomena that seem exclusively or nearly exclusively non-conscious (presentiment and staring/DMILS), experimental techniques have been developed to bring internal processes out into the open through behaviors (button pressing in attention facilitation, for example). Medical monitoring equipment such as skin-conductive measuring devices, EEG, heart-rate monitoring equipment, and even electrogastrograms have been adapted and pressed into service. Results have been subjected to generally rigorous statistical analysis to ensure as clearly as possible that

demonstrated results are best explained as being due to ESP.

Congruence

The more closely a piece of evidence can be accommodated without contradiction within an existing body of evidence that already supports a hypothesis or theory, the more strongly it confirms the same hypothesis or theory. This criterion might have been termed coherence, but I have elected to use congruence. Coherence denotes compatibility – a harmonizing or fitting in, or being consistent with other evidence. Congruence, on the other hand, not only includes coherence, but also conveys the sense of active agreement, converging towards a particular position, or mutual support. New evidence is *congruent* with a theoretical context if it is not just compatible (coherent) – that is, fits in with other, already integrated evidence – but indeed brings something contributory to the explanatory value of the theory. Some evidence could be coherent with any number of theories or hypotheses. But it would not necessarily be congruent with them in the sense I mean it.

Additionally, evidence that conflicts with a hypothesis or theory may not be *incoherent*, except in terms of the theory with which it conflicts; it may in fact be perfectly intelligible in all other contexts. But evidence can be *incongruent* without becoming incoherent or unintelligible, even when it conflicts with a deeply entrenched or dearly held theory. Lack of congruence, therefore, is not necessarily a sign of the *un-*scientific status of a piece of evidence (indeed, may even be an indicator of superior scientific value over evidence that supports an erroneous but widely accepted theory).

However, incongruent evidence may not be accepted as readily as is evidence that is congruent, and merits closer scrutiny because of its incongruence.¹³

Congruence is the only criterion necessarily violated by evidence that is contradictory. Contradictory evidence, if it is revolutionary in a Kuhnian sense (that is, incommensurate with an established theory-base but nonetheless true) may well meet all other evidentiary criteria but, because it fails to agree with the theory it challenges, it is also *incongruent* with much (though not necessarily all) of the supporting evidence for the theory it puts under pressure.

Congruence and the ESP Data:

ESP data is not necessarily congruent with the bulk of scientific evidence, but it does not necessarily conflict with it, either. This is only true, though, *if one entertains the possibility of extraphysical causal interaction* with the physical world (though such interaction would not have to be extensive). ESP data *is* systemically *incongruent* with the universal physicalism thesis. Since the ESP data is evidence against the causal closure assumption, it is at odds with any formulation of universal physicalism, which is the root of the incompatibility, and hence incongruence, of the two. (In chapter 12, I shall entertain the suggestion that a fully-completed physical account of the universe might find a way of reconciling itself with ESP, but for now will consider them incongruent.)

¹³This is more popularly construed as “extraordinary claims require extraordinary evidence.”

Not all of the seven evidentiary criteria I propose need be present to render some evidence scientific. One would expect accordance, publicity/objectivity, and relevance to be high on the list. But some well-accepted evidence may lack diversity – perhaps it is only producible in a certain way, or is so strongly congruent that other confirming instances are not essential. In many science disciplines (cosmology, paleontology, evolutionary biology, geology, to name a few), repeatability may not even be possible. Still, even if it is not necessary that evidence meet all, nor even most of these seven categories, the more that are present increases our confidence in questionable cases that the data is worthy of consideration from a scientific perspective. We have seen that, at least based on this brief survey, the parapsychology data seems to be well-credentialed in terms of characteristics we might expect admissible scientific evidence to display.

Although, as Braithwaite states “no amount of empirical evidence suffices to prove any of the hypotheses in the system, yet any piece of empirical evidence for any part of the system helps towards establishing the whole of the system” (Braithwaite, 1953/1983, 49). In other words, evidence from different but related domains has a confirmatory effect on the whole. The same holds for ESP. Evidence from a variety of researchers, labs, and varied in-principle replications all strengthen the hypothesis that ESP exists.

A final, important implication applies here: If one wishes to dismiss the ESP evidence on grounds that it is not scientific, he must take into account whether the data he is dismissing meets the criteria expected of scientific evidence. Given my argument

here that it does, he must show that these criteria are not central or important to whether evidence counts as scientific or not or, on the other hand, he must show that the evidence I have assessed does not satisfy the criteria. Otherwise, by rejecting the parapsychology evidence as unscientific, he also throws into question all mainstream scientific evidence as well, since it is vetted on the same principles. This seems a rather steep price to pay just to be rid of some inconvenient facts.

Total Evidence

There is one final concern to address before leaving our discussion of evidence. This is the often asserted “total evidence” requirement. As Carnap has observed, “in the application of inductive logic to a given knowledge situation, the total evidence available must be taken as basis for determining the degree of confirmation” (Carnap, 1950, 211). This can be interpreted to mean that for confirmation to be truly reliable – truly *scientific* – *all* and *every* bit of evidence that might have bearing on a given hypothesis or theory must be collected, examined, and analyzed. It certainly makes sense; in an inductive process it is always possible to encounter evidence that requires a reinterpretation of previous data, mandates a change to the direction of a program of scientific exploration, or even shows such a program to be invalid. It is only possible to rule out unpleasant surprises (or, if necessary, accommodate them) by corralling *all* the evidential wildlife and taming it.

Those who are aware of this requirement realize almost immediately, though, that the requirement is unrealizable in practice. For most conceivable research programs in

science the possible evidence domain is certainly indefinite, and likely in many cases infinite. Further, practical constraints may render the securing of some – and perhaps much – evidence impossible. Hempel agrees that the *value* of the total evidence requirement is obvious. It is possible, he suggests, that two true inductive observation sentences can support each of two contradictory theses respectively, which is clearly problematic. In principle, the total evidence requirement can alleviate this by revealing how the two are to be reconciled. Hempel credits Ayer with pointing out that if one starts with only *part* of the evidence, we can assign probabilities to a given hypothesis that continue to change as we consider ever bigger slices of the total evidence. It is only with the total evidence that the probabilities become fixed. (Hempel, 1960, 451-452)

However, “formal logic...does not tell us what statements to believe or to act on; indeed, the notion of accepting certain statements, like that of total evidence, is pragmatic in character and cannot be defined in terms of the concepts of formal (deductive or inductive) logical theory” (p.453). For Hempel, then, the problem with the total evidence requirement is that it is not pragmatic. In most important cases there is far too much evidence, and it is too complex for us to have a practical hope of capturing it all. It is, though, possible to exclude evidence that is irrelevant to the hypothesis being examined.

Ways have been explored to reduce the overwhelming burden levied by the total evidence requirement, while reducing as much as possible exposure to the consequences of restricting the evidentiary net too severely, thus excluding important or necessary evidence. Adler, for example, examines Goldman’s position that all ‘total evidence’

requires is that a subject examine just that evidence that is available at a given time and place. This Adler judges too weak. It runs the danger of letting the subject too easily off the hook. There should at least be a presumption that a rational actor be able to recognize that some epistemic positions are simply too weak to justify drawing sound conclusions from available evidence. Further, we are justified in holding a certain expectation of the competence of an actor who proposes to draw evidence-based conclusions – that she will exercise due diligence to gather additional relevant evidence beyond what may immediately and easily be available. Still, Adler recognized that meeting the demands of a fully-fledged total evidence requirement is an unreasonable (perhaps even irrational) proposition, and that the total evidence requirement *must* be constrained, even if there is as yet no obvious consensus on how that might be done. (Adler, 1989)

The obvious impracticality of the total-evidence requirement has not stopped some from insisting it be met by parapsychologists. In “A Field Guide to Critical Thinking,” Professor James Lett says this: “The evidence offered in support of any claim must be exhaustive – that is all of the available evidence must be considered,” and he goes on to discuss how various paranormal claims violate this stipulation (Lett, 1990). This is clearly just a restatement of Carnap’s total evidence requirement.

There are two responses possible. Even if we do levy the requirement for total evidence on ESP – surely that does not mean that every bit of evidence for the *physical* universe be gathered before we can render a verdict on ESP? After all, as I mentioned in my treatment of the congruence criterion, ESP evidence and most, if not all of the facts about physical objects, laws, properties, and systems do not conflict. For ESP to be true

only requires that there be some further extra-physical feature to the universe that under certain conditions be causative. So the total evidence for psi might include facts generated in ESP research, psychological facts (both known and presently unknown) about humans (and perhaps other autonomous complex systems, such as chimps, dolphins – and chicks), facts concerning logic and reasoning, plus any properly-developed counter evidence (those opposed to the notion of ESP are, after all, bound by the same rules as those in favor of it). At least with this point in mind, the requirement for total evidence carries no more weight against ESP than it does any other science-based argument.

The requirement to consider the total evidence actually wields a two-edged blade that cuts physicalism even more than it does ESP. This is because physicalism is a universal claim, while ESP is not. For universal claims it is even more important that not just evidence for the universal proposition being tendered must be considered, but also evidence that counts against it. “If you are willing to be selective in the evidence you consider,” Lett continues, “you could reasonably conclude that the earth is flat.”

This is true enough (as the existence of the Flat Earth Society proves). But as can readily be documented, physicalists notoriously ignore or dismiss ESP evidence from any consideration. One could consider this in itself a violation of the total evidence requirement, and the cynical might suspect that physicalists wish to maintain their own version of the flat earth theory.

A physicalist might defend herself against this implication by raising the question of just how much energy science should be required to spend in investigating claims for

anomalous findings and minority views. In a Kuhnian context one might argue that science may have *some* responsibility in this regard. But, as Fales and Markovsky (1997) tell us, “no discipline can afford to devote serious attention to every unorthodox notion that comes over the horizon” (p.511).

In terms of ESP, physicist Stephen Weinberg agrees: “...should we not test astrology and telekinesis and the rest to make sure that there is nothing to them?” He does not think so. “At any one moment one is presented with a wide variety of innovative ideas that might be followed up,” but there is “simply no time.” As far as parapsychological and other phenomena are concerned “the conventional answer would be that this evidence must be tested with an open mind and without theoretical preconceptions.” But he does “not think that this is a useful answer.”

For all of us, there is simply no alternative to making a judgment as well as we can that some of these ideas (perhaps most of them) are not worth pursuing. And our greatest aid in making this judgment is our understanding of the pattern of scientific explanation. [O]ur discovery of the connected and convergent pattern of scientific explanations has done the very great service of teaching us that there is no room in nature for astrology or telekinesis or creationism or other superstitions.” (Weinberg, 1992, 48-50)

There is, to be sure, good sense behind this. Obviously, resource-rich though science is by most standards (many billions of dollars a year, after all, are spent on all levels of research), there is always far more valuable research that *could* be done were there not finite limits on the available resources. It stands to reason that science should

not be expected to try to validate every frivolous unorthodox claim that one crackpot or another cares to raise.

But there is a certain disingenuousness to the way this argument is used against ESP. It is not unreasonable to ignore a speculative pursuit for which there is little or no well-controlled research that produces evidence on its behalf – palmistry, for example, or aura-reading, or even astrology.¹⁴ Contrast this, though, with the literally thousands of successful experiments that have produced evidence of parapsychological effects, and were done under acceptable scientific standards. It seems implausible to consider parapsychology to be no more relevant – and no more worthy of investigation – than, say, numerology. In terms of supporting evidence and scientific credentials, parapsychology is in a class by itself, beyond anything else that gets lumped uncritically under the heading “pseudoscience.”

“If it could be shown that there is any truth to any of these notions,” Weinberg himself says, “it would be the discovery of the century, much more exciting and important than anything going on today in the normal work of physics.” Leaving aside for the moment whether he really believes what he is saying, one wonders why, given the magnitude of legitimately-conducted research generated by scientific parapsychology,

¹⁴Perhaps, though, we should not be so ready to dismiss astrology altogether. Though nowhere near as well attested as ESP, there are at least two controlled studies that show an unaccounted-for “Mars effect” – one by proponents, and one by skeptics. First is that by Michel Gauquelin and various associates, which plausibly showed that athletic prowess correlated with a certain position of Mars in the Zodiac. The second, ironically, is a study done under the auspices of the highly-skeptical Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP), which unexpectedly confirmed Gauquelin’s findings.

someone as astute as Weinberg would not recognize both the qualitative and quantitative gulf that separates parapsychology from all the other “paranormal” pursuits within which he lumps it.

Perhaps it is only that he is unaware of just what evidence is available in parapsychology – and that is, to be sure, a likely explanation.¹⁵ But other possible reasons why this may be have to do with arguments that continue to be raised against the possibility of ESP. I will consider these in the next chapter.

¹⁵Though as quoted above, Weinberg does think we know enough about “scientific explanation” to rule out *a priori* any possibility of ESP.

Chapter 11: Arguments Against the Evidence

Despite the fact that the extrasensory perception evidence I present largely meets the criteria one might reasonably expect of scientifically-admissible evidence, it is generally dismissed by the mainstream. Though there are sociology- and psychology-of-science issues that contribute to this rejection, there is a general set of complaints and objections that are used to undermine the acceptability of the evidence and the field as a whole. I will not try to address these exhaustively, but will consider some of the most frequent or prominent objections to show that they are of questionable value in undermining the parapsychological evidence.

OBJECTIONS TO THE EVIDENCE

Allegation 1: *Parapsychology is the only discipline that relies exclusively on statistical evidence to prove its claims.*

This argument does not express a concrete objection or criticism. It is usually lodged in a context or with an emphasis that conveys a negative connotation. For example, Antony Flew says, “the very concept of ...and not just the best available evidence for the reality of psi, is essentially statistical.” Ray Hyman, one of the most respected of parapsychology’s critics maintains that “parapsychology is unique among the sciences in relying solely on significant departures from a chance baseline to establish the presence of its alleged phenomenon” (Hyman, 1995, p.3-51). Let us examine the tacit argument that is implied by these statements.

- 1) Parapsychological results consist only in statistical values.
- 2) Any science worth its salt should produce at least some results that are concrete and substantive.
- 3) The status of a science producing results that are only statistical in nature is suspect.
- 4) Therefore, the scientific status of parapsychology is questionable.

But is this a sound argument? Consider the second premise first. Is it true that the scientific status of a body of research should be questioned if statistical results are its only evidence? Though I know of no science that produces exclusively statistical results (save, perhaps, for statistical science itself), it seems dubious to question scientific status based solely on the *kind* of evidence it produces, so long as the evidence is legitimately and rigorously obtained, and is correctly interpreted. Population genetics, for example, is *largely* statistical yet is still considered fully scientific.

The first premise is also dubious. Do parapsychology results consist *only* in statistical evidence? The answer is, in fact, no – the assertion is mistaken. The idea of ESP producing exclusively statistical evidence seems to be a holdover from card guessing experiments where most of the effect was in fact only statistically detectable (see Flew, 1987). Even at that time, though, this statement would not have been true. Other aspects of parapsychological research at the time involved investigation of reports and *prima facie* effects which, by their nature, couldn't even *be* analyzed statistically. Indeed, Louisa Rhine, J.B. Rhine's wife and a researcher in her own right, was prominent in just these kinds of investigations.

Further, parapsychology has along the way added research paradigms that also go beyond the mere statistical – for example remote viewing or the Ganzfeld protocol, which provide concrete evidence in the way of sketches, three-dimensional modeling, or verbal reports. Though ESP evidence is nearly always statistically analyzed, to claim that the evidence is “only” statistical is to miss the point of statistics. Often, statistics are used to compile the values of dozens or hundreds, or even thousands of individual, concrete events in the world. The events are not mere numbers, but are real things that the numbers manipulated by statistics merely represent. For example, no one would argue that a star baseball player’s batting average was “only statistical evidence” for the existence of an event that involved an actual person swinging an actual bat at an actual ball. Nor does the fact that a particular remote viewer’s sessions, when ranked blindly against the set of possible targets, yields a statistically significant score of $p = 0.005$ constitute “only statistical evidence” for an actual set of remote viewing trials. To say that all evidence produced by parapsychology is “only” statistical is to deny the ontological concreteness of what the statistics represent, and is a category error.

To be sure, parapsychology relies heavily on statistics to disambiguate results, and certify that conclusions are not based on confirmation bias (drawing conclusions from ambiguous data based on what one *wants* the data to prove). Statistics are also used to render *prima facie* and qualitative data into quantitative measures that can be more objectively compared across bodies of research. In that, parapsychology is in good company with much of the rest of science. As R.A. Fisher stated

In order to assert that a natural phenomenon is experimentally demonstrable we

need, not an isolated record, but a reliable method of procedure. In relation to the test of significance we may say that a phenomenon is experimentally demonstrable when we know how to conduct an experiment which will rarely fail to give us a statistically significant result. (*The Design of Experiments* (1960) Oliver and Boyd, Edinburgh. 14)

Ray Hyman, one of the critics cited above on the use of statistics in parapsychology research, undermines his own case. After complaining that the only evidence parapsychology presents is statistical in nature, he also rejects the use of *prima facie* evidence that is offered in ESP research because it isn't possible to evaluate it statistically (Hyman, 1995, p.3-71).

Allegation 2: *Parapsychology is the only science where an effect is only ever negatively defined.*

Typical expressions of this concern are, for example, from James Alcock (2003): "...parapsychology is the only realm of objective inquiry in which the phenomena are all negatively defined, defined in terms of ruling out normal explanations" (p.33).

Psychologist and parapsychologist John Beloff put it this way: "The field...must be unique in one respect, at least: no other discipline, so far as I know, has its subject matter demarcated by exclusively negative criteria. A phenomenon is, by definition, paranormal if and only if it contravenes some fundamental and well-founded assumption of science" (Quoted in Flew, 1987, 93). And Hyman, once again, weighs in: "Operationally, the presence of anomalous cognition is detected by the elimination of all other possibilities. This reliance on a negative definition of its central phenomenon is another liability that parapsychology brings with its attempt to become a recognized science" (Hyman, 1995).

The point being argued by this objection hinges on the requirement that all possible *physical* explanations be ruled out before a result can be accepted as deriving from extrasensory perception. Accordingly, parapsychology is alleged to be the only science where the existence of an effect is only ever negatively defined. This is the corollary of my observation in chapter four that in order to isolate a psi effect, one must first exclude all other possibilities.¹ As Oddie (1990) explains it, to rule out anything but ESP an experiment must certify that A is a cause of B, while ruling out correlation from a common causal ancestor, an alternative causal chain, or some form of coincidence.

It is also to rule out the possibility that the correlation is the causal by-product of some quite distinct common cause of A and B...But how does experimentation ensure this? How do we know that the process of experimentation is not itself the causal by-product of some common cause of that process of experimentation and the occurrence of B? ...We can rule this possibility out in principle if the whole experiment is not itself totally caused by what has gone on before it. That is, if the experimenter is a free agent, or can at least arrange to have the experimental set-up triggered by an uncaused event, then there is no other (total) cause of the experimental set-up. If the experimental set-up is (at least partially) an indeterministic affair then we cannot appeal to a common cause to explain the correlation. Thus barring sheer coincidence the only explanation is that A-tokens cause B-tokens. (Oddie, 1990, 73-74)

Oddie captures here the value of double-blind protocols and randomization that have become the norm in parapsychology research. These are parts of the process essential to

¹There have been some extreme reactions to this feature of psi. For instance, as I will explore in more detail below, one critic of ESP research said he would prefer to believe that space aliens kidnap people and plant thoughts in their brains, rather than accept any sort of psi explanation. At least the space-alien hypothesis has the virtue of being accommodated under physicalism. One peer-reviewer of the seminal Puthoff/Targ IEEE paper on remote viewing retorted, “This is the sort of thing I wouldn’t believe even if it were true!” (Smith, 2005)

rule out causal influences other than those being examined. (This also indirectly provides another response to the complaint about the use of statistics in ESP research – the requirement for randomization all but necessitates a statistical presence in any research protocol in which it is employed.)

There are a couple of responses possible to the “negative definition” concern. First, one might ask – “so what?” How does this actually tell against the legitimacy of parapsychological discoveries – is it a logical argument against it? It doesn’t seem to be. Is it an empirical argument? Again, apparently not. It seems to be largely a psychological objection. Those who lodge it seem to feel that it is somehow unseemly that parapsychology relies on negative conditions, or that this somehow makes parapsychology arguments weaker. Maybe it just so happens that there is a small class of sciences, parapsychology among them, which require negation of all other possibilities to validate results.

This response leads into a second one – isn’t this in some degree true of *any* science? Each piece of evidence in any science must be shown not to be the consequence of some other force, cause, or event other than the one under consideration, otherwise it cannot unambiguously count as evidence for the phenomenon being examined. Many conventional science experiments require that possible external influences or sources of contamination be excluded before the results can be trusted. The greater the degree that other explanations can be excluded, the greater the confirmatory value of the evidence for the phenomenon or hypothesis being supported.

There are numerous instances in the sciences where a phenomenon or entity can

only be identified in the absence of all other explanatory factors. One example comes from physics itself. In attempts to isolate quarks which are widely accepted as theoretical entities, but for which empirical evidence is highly desirable for added confirmation of the underpinnings of quantum physics, physicists have created “quark detectors.” Quark detectors are carefully positioned and employ sophisticated shielding so as to exclude all other possible sources of intrusion into the detection device that might mimic behaviors postulated of quarks. According to theory, quarks are able to penetrate the kinds of shielding incorporated into these detectors, but few other particles, fields, or forces can. Thus, if all other influences can be excluded, whatever then trips the detector must be a quark.

Cosmology provides an even better example. Researchers noticed that the universe’s rate of expansion could not be accounted for by the amount of energy and matter observable in the physical universe. They were forced to postulate “dark” matter, which cannot be detected through normal observational means, but which can be indirectly discerned by their gravitational and other effects on observable matter. Loosely expressed, then, whatever can’t be shown to be regular matter must by the process of elimination, be “dark” matter. Admittedly, the notion of dark matter is not accepted uncontroversially. But it is not rejected as being unscientific merely on the basis of its being “defined negatively.”

What seems to be the sticking point for critics of parapsychology is that there are gaps in the causal story that apparently cannot be fit into any explanatory scientific narrative. So even once all alternative explanations are excluded – thus defining the

phenomena “negatively” – we are left with an explanatory vacuum. But what is worrisome to those who find the “negative definition” troubling is otherwise confirming for my own thesis – that the ESP results I have cited are real evidence for intermittently truncated causal chains (ITCCs), and demonstrate that psi has an extra-physical component. If my claim is true, this is *exactly how the phenomena would behave*. There would be gaps in our ability to physically account for the effects and, since physical explanations are all we can appeal to, “negatively defining” and “ruling out” are the options on the table. If the “ruling out” can be done conclusively – and hence, convincingly – then from a scientific standpoint it doesn’t matter that some oppose it.

It *does* however, demand scrupulously careful protocols so the ruling-out is as persuasive as possible (though there will be those whom no amount of evidence, whether negatively or positively arrived at, will convince).

Allegation 3: *The “File Drawer” Effect – Compiled parapsychological effects are only significant because there are large numbers of unsuccessful studies that were never published.*

The so-called “file drawer” argument was originally introduced by parapsychologist J.B. Rhine (Palmer *et al*, 1960). It has been lodged against the apparent success of parapsychology research for decades and, despite good reasons to dismiss it, is currently still resorted to by many critics. It is the nature of statistics to be sensitive to the ratio of positive outcomes to negative ones. An incomplete accounting of both positive and negative outcomes skews the overall statistics. Bodies of research in which

a large proportion of unacknowledged results lean in one direction only, will have misleading statistical results. Therefore, it is *essential* that all available data be considered when performing statistical analyses. Note that this is a form of the “total evidence” requirement, but one in which there is no alternative to considering all the evidence. (Fortunately, in most of these cases the set of available data is finite if, sometimes, large in quantity.) In practice, it is rare that *every* statistically-relevant datum is included in evaluations. But if the vast preponderance of data is considered, a few missing bits are seldom noteworthy. However, the claim as applied to parapsychology is that much of the negative data is missing, while mostly positive data gets published, thus seriously biasing the results towards high significance.

There are three ways of arguing against this claim. First, it is possible that there is a “neutral” or even “positive” file drawer problem as a negative one. A 1984 review of remote viewing by Hansen, *et al* (1984) discovered a file drawer of 18 studies, eight of which were significant. While ten negative versus eight positive studies added to the overall corpus of remote viewing research would *slightly* move the overall significance of the remote viewing findings back towards the negative, the shift would be trivial – nowhere near enough to offset the overall significance of the total results.

An earlier review by skeptical parapsychologist Susan Blackmore (1980) examined the field of Ganzfeld research for selective publication of results, and found 32 unpublished studies. Twelve of these were never completed and one could not be analyzed due to methodological reasons. Fourteen of the remaining 19 studies followed the prescribed Ganzfeld methodology sufficiently well to be included in the analyzed set.

Of these 14, five (just over a third) were statistically significant. Blackmore concluded that “the bias introduced by selective reporting of ESP ganzfeld studies is not a major contributor to the overall proportion of significant results” (p. 217).² Remote viewing researcher Ed May told me that he had a number of statistically-significant studies that he had just not bothered to publish.³ Parapsychologist Stephan Schwartz also recently communicated to me that in terms of a presumed negative file-drawer effect, “Research has shown that it is rather the reverse. People like me had a number of successful studies they had done that they had just not gotten around to writing up” (Schwartz, 2009).⁴ And in a telephone conversation with Dean Radin in early 2009, he told me he, too, had some positive studies that he hadn’t taken the time to publish.

A second argument against the file-drawer accusation is that, for the several decades the parapsychological community has made a virtue of full-disclosure by insisting that *all* data be published, whether positive or negative. The Journal of Parapsychology regularly publishes reports of failed studies in a variety of parapsychology fields, and Parapsychological and other conferences feature reports of negative results alongside those presenting positive findings. (Radin, 1997, 79) This contrasts with the mainstream scientific community, in which negative reports are seldom published (Giles, 2006).

Finally, there are statistical tools that allow the assessment of whether and to what

²Chance in this set of Ganzfeld experiments would be 25%

³Edwin May. Email correspondence. 29 October 2009.

⁴Stephan A. Schwartz, Email correspondence, 4 September 2009.

degree there is a file-drawer problem. The first of these amounts to a *reductio ad absurdum*: start with the total statistical significance of all combined published studies of a certain type, and calculate how many unpublished non-significant studies of the same paradigm would be required to render the overall results non-significant. If the number of hypothetical unpublished studies is a number of such magnitude that it would be highly implausible for that many studies to exist given the known number of researchers in the field and the logistical constraints required by that sort of research protocol, then one can conclude that even if there is a file-drawer effect, that cannot account for the significance of the published studies. Michael Scriven (1956a) was one of the first to recognize the value of this argument. He noted that the amount of time required to do an experiment plays a role in the possibility of file drawer (though Scriven did not use this term) – if it takes a long time, there is less likelihood that many unsuccessful ones have been done and not published. (p.237, n.4).

As Radin (1997) explains, “because it is impossible to know how many other studies might have been in file drawers, it is common in meta-analyses to calculate how many unreported studies would be required to nullify the observed effects among the known studies” (p.80). A meta-analysis of the Ganzfeld studies conducted jointly by parapsychologist Charles Honorton and skeptic Ray Hyman showed that a ratio of 15 unpublished non-significant studies would be required to offset each published significant one – for a total required of 423 studies in the file-drawer (and this assumes no unpublished *positive* studies). Hyman agreed with Honorton that a file-drawer artifact could not account for the positive Ganzfeld data. (Radin, 1997, 80; Hyman & Honorton,

1986)

A further file-drawer analysis of dream ESP studies showed that 700 unpublished, non-significant dream studies would be needed to offset the positive results reported in the published literature. Since there were approximately 20 dream researchers active in the field, this boils down to require 35 unpublished non-significant studies for *every* experiment that each of the 20 researchers published. As Radin points out, the average dream experiment involved 27 sessions. For the file-drawer explanation to work would therefore require 18,900 dream ESP sessions, lasting one night each. That amounts to 18,900 nights, or 50 years' of data that would have to have gone unreported. (Radin, 2006, 111-112)

To show that these sorts of meta-analysis really do work, I will cite one more meta-analysis designed to detect whether a file-drawer artifact was present. The early staring studies were meta-analyzed and a file-drawer effect *was* discovered there. Two findings of the analysis showed that the file-drawer error derived from the poorly-controlled, close-proximity staring studies (Radin, 2005, 98).⁵ It further showed that “the mean effect size for close-proximity studies might be inflated due to implicit learning of subliminal cues” – in other words, there was sensory leakage that contributed to the overall significant effects, just as many reviewers had suggested.

Though none of the meta analyses (except those involving staring) covered the data I have been considering, the findings from both the Ganzfeld and dream-ESP studies

⁵The meta-analysis also showed, however, that the ten best-controlled staring studies (which I included in my earlier discussion) were not vulnerable to these problems.

can perhaps be extrapolated – at least provisionally – to support the credibility of other bodies of research in the field. At the time the Ganzfeld and dream-ESP studies were conducted, there was no expectation by the researchers conducting them that they would be examined for file-drawer artifacts. Nonetheless, post hoc evaluation showed no file-drawer effect. This indicates one of two things – either that the majority of all research produced positive results – or that the researchers voluntarily followed good research practice that requires all results to be published (or both).

If professional parapsychologists pursuing the Ganzfeld and dream-ESP work were committed to good research practice, one could with more confidence expect similar good practices to be followed among other professional parapsychologists who were researching presentiment, DMILS, and remote viewing. Beyond this, prima facie evaluation allows us to recognize from the quantity of successful studies that were published that numbers of unpublished, non-significant studies orders-of-magnitude beyond what was published would be required to account for the data. (For instance, the 40 or so successful DMILS studies would require somewhere around 800 non-significant studies to bring them to chance levels; and given the greater volume of remote viewing work, a notably higher number would be required there).

Allegation 4: *Successful parapsychology experiments usually show serious methodological flaws that render them inadmissible.*

This criticism is more substantive than either the statistical objection or the “negative definition” complaint. It is, though, closely related to the “negative definition”

issue, in that it is basically the requirement that if extrasensory perception is to be taken as the explanation for an observed effect, all possible alternative explanations must be ruled out. So the argument can be stated in this way: If a methodological flaw exists that *might* account for prima facie or statistically-significant results, one cannot conclusively accept ESP as the explanation for the results. Such flaws have been found. Therefore, parapsychology results are invalid.

There are three ways to answer this. First, it is important to note that a flaw that might *possibly* account for all or part of an apparent effect does not *necessarily* actually account for that effect. Often, this concept is abused by those seeking to undermine the ESP research. A critic may offer an alternative explanation that describes how the effect *might* have been achieved through some flaw in the experiment. However, to be valid as an argument, the flaw must be shown to actually be plausibly *present* in the experiment, and this is often not done. Such an *allegation* of a flaw, without *demonstrating* that the flaw was plausibly present, does not count against the experiment. While the burden of proof rests squarely on the claimant when a legitimate flaw is shown to be present, the burden shifts to the critic if a flaw is claimed that is either not obviously present in an experiment, or that is too fanciful to be plausible.⁶ It is also necessary to show that a flaw that *has* been identified is one that really does undermine the data. Not all flaws are

⁶As an unscientific aside (though it illustrates the case), I once heard an interview on National Public Radio with skeptic James “the Amazing” Randi, where he debunked a claim that someone had exploded a television screen using mental powers. Randi said – apparently in all seriousness – that he could duplicate the effect merely by throwing a brick through a television screen. Therefore the mental explanation was false. The claim might have been (and no doubt was) false on other grounds. But surely Randi’s objection

relevant to the findings. Finally, one must be sure that features identified as flaws really are flaws. An example of this occurred in a review published in *Nature* of Dean Radin's book *The Conscious Universe*. The review claimed that Radin had incorrectly performed his meta-analyses, and that the extremely high significance values he had claimed for compiled parapsychology results were mistaken. Radin was in the end proved right, but the ensuing controversy required the intervention of Nobel Prize winner Brian Josephson and physicist Nick Herbert before *Nature* would acknowledge it. Despite that, it is still popularly believed that Radin's statistical conclusions were in error.⁷

Second, as possible flaws have been discovered and pointed out, measures have been taken to eliminate them in subsequent experiments. It was already growing harder even two decades ago to find real flaws in many of the parapsychology research paradigms. As early as 1988, for example, in a study for the Army Research Agency prominent skeptic Ray Hyman was having difficulty finding flaws in parapsychology research sufficient to account for strong effects (Smith, 2005, 372). And in a 1995 review of the government-sponsored research, Hyman candidly admitted he could find no methodological flaws that could account for the obvious effects demonstrated: "I cannot provide suitable candidates for what flaws, if any, may be present" (p.3-54), which he admits three times in his analysis of the data.⁸

cannot be taken seriously.

⁷The entire controversy can be reviewed on Josephson's website at <http://www.tcm.phy.cam.ac.uk/~bdj10/psi/doubtsregood.html>

⁸Because of his credentials as a scientist (professor of psychology at the University of Oregon) and his long experience in examining parapsychology research,

In the same study, statistician Jessica Utts made a further argument against the flaw hypothesis: “The various experiments in which [significant effects have] been observed have been different enough that if some subtle methodological problems can explain the results, then there would have to be a different explanation for each type of experiment, yet the impact would have to be similar across experiments and laboratories.” Basically, the argument is that despite improved methodologies; and despite different types of experiments – each with unique flaws should any be present; and despite research being conducted in different laboratories with different researchers – if flaws were the cause of the effect one would expect ESP experimental results to be far less robust and much more uneven than they are shown to be. But the effects are still present and the results far too consistent to be produced by presumed methodological flaws.

Allegation 5: *To be taken as trustworthy, scientific findings must be replicated. But parapsychological results are not replicable, therefore they do not count as legitimate scientific results.*

In the previous chapter, I drew a distinction between repeatability and replication. Repeatability I considered to be a simple feature of a valid experiment in which, when the experiment is redone as close in procedure as possible to the original, it produces similar outcomes. Most theorists would likely consider this just replication by another name. However, I argued that replication has three components, drawn from among the

Hyman has often been appointed to oversight boards and blue-ribbon panels by government agencies specifically to evaluate psi experiments.

criteria I listed as desiderata for scientific evidence. These components include repeatability (in other words, at least *conceptually* the same experiment is being repeated). But also included are independence and diversity.

I find some support for this distinction in the literature. Though Collins (1992) takes the two as being synonymous (“Repeatability, or replicability [I will use the term interchangeably],” p.18) – he subsequently suggests that there is more to the notion than mere repeatability. If a follow-on experiment “is too like the first then it will not add any confirmatory information...confirmatory power, then, seems to increase as the difference between the confirming experiment and the initial experiment increases” (p.34), suggesting that an element of diversity is important to the concept of replication. He also argues for independence as a criterion for replication. “A run conducted by another experimenter on the same apparatus is still more impressive [than merely a run by the same researcher], and a confirming run observed with a similar apparatus built and run by another experimenter is even better” (p.34).

But care must be exercised: “For an experiment to be a test of a previous result it must be neither exactly the same nor too different.” Having too much diversity in a replication may lead to two problems. First (in an example from Collins), suppose a new development in physics is discovered by experiment. Suppose further that it is confirmed by a very diverse replication, using an experimental design and equipment far different from the original, by someone originally skeptical of the first set of findings. Despite the fact that “the differences between first experiment and second experiment were maximized,” it turns out the replication was done by a “skeptical fairground gypsy who

had generated the confirmatory result by reading the entrails of a goat!” (pp.34-35). In this case, it would seem the confirmation was tainted, pointing up the fact that even successful replications are not always helpful if they stray too far from the paradigm being examined.

The second (and, it seems to me, more likely) problem that can occur if a replication includes too much diversity is that possibility of failure goes up as the replication strays further from the concept and execution of the original experimental design. This is not a problem if the intent is to test boundary conditions and limits. But it is a problem if those looking for confirmation of the original lay too much worth on the replications *as* a replication if marginal replications fail, as I mentioned previously with regard to Ganzfeld experiments.

Now for the allegation: The claim that “ESP results aren’t replicable” argument breaks down into three sub-assertions:

1) *No ESP experiments replicate in any way.*

This complaint is heard most often in informal settings, and seldom in exchanges involving critics and skeptics who are well-versed in parapsychology (though, see Rao, 1984, 22-23 for a case where the claim was lodged as a serious academic objection). Still, it is widely believed by mainstream scientists and a broad cross-section of the skeptical community. From the four domains of evidence I have presented it is obvious that this general statement is mistaken, and requires no further discussion.

2) *Psi experiments don’t replicate consistently (e.g., sometimes they do, sometimes they don’t).*

There is more substance to this objection. As we have seen, there are times when repetitions and replications fail, and the reasons for the failure are obvious. However, there are instances of failures when it is unclear why they have failed. Inconsistent replication is not necessarily an indication a phenomenon does not exist. What it does indicate, however, is that the phenomena involved are not well understood. There are clearly conditions, environments, functional boundaries, and so on that remain obscure, and which can only be defined by further trial-and-error experimentation.

The history of magnetism as a phenomenon may serve to illuminate this point. Before a fully developed electromagnetic theory became available, magnetism was somewhat of a mystery. We can imagine that when it was first discovered, it was unclear as to the conditions under which magnetism was present. Under certain conditions it was attractive, under others it was repulsive. And under still others, it was inert. Some materials manifest magnetic properties, while others never did. And some possessed no magnetic properties at first, but became magnetic with sufficient time and exposure.

By analogy, this somewhat describes the situation we encounter with extrasensory perception and other relevant parapsychological phenomena. One major difference is that magnetism by comparison is a fairly simple property whose basic operating parameters could be determined through elementary trial-and-error folk experiments, even when no explanatory theoretical context was yet at hand to account for it. Parapsychological phenomena, on the other hand are much less tractable if only because they deal with complex adaptive biological systems (humans) and involve frequently

subjective experience that must be objectified through indirect means (much as in the case of psychology in general). This means that on occasion replications fail for unknown reasons. This does not mean that there is nothing there to replicate.

3) Even when psi experiments show relatively frequent replication, it is not precise – in other words, each series of attempts produces results that while similar, still vary from one experiment to the next.

This is the crux of the objection concerning ESP and replication. “As has often been noted in criticism of parapsychology,” writes Patrick Grim, “the experimental results do not appear to be consistently repeatable” (Grim, 1987, 141). No one disagrees that the consistency of many replications differ from one successful replication to another with respect to significance levels, quality of results, and so on. This complaint goes beyond the problem of incomplete understanding of the phenomena as I discussed above, to hinge on a faulty notion of replication. Karl Popper observed that “all repetitions which we experience are *approximate repetitions*”; that they are not identical to the original experiment; but that they are “only *more or less similar*” (Popper, 1959, in Collins, 1992, 29). In a provocative article in *Nature*, Jim Giles explores the replication controversy in mainstream science. He finds that “It is simply the case that the replication of results, a process absolutely central to science, is not always possible,” and asks rhetorically “what happens when attempts to replicate an interesting experiment fail. At what point does ‘unreplicated’ give way to ‘unreplicable’?” (Giles, 2006, 344) Indeed – in some disciplines (e.g., paleontology and cosmology) “results are corroborated rather than reproduced” (p.345).

Surprisingly, replication is relatively rare in mainstream science, as Giles discovered in trying to track down replications of a number of scientific reports in *Nature* (pp.344-345). Rao notes that “in normal science, repetition of an experiment or the possibility of it is not a matter of primary experience.” (Rao, 1984, 22). Collins affirms this:

One important and ironic support for the common sense view is that replication of others’ findings and results is an activity that is rarely practiced...Science reserves its highest honors for those who do things first, and a confirmation of another’s work merely confirms that the other is prize worthy. A confirmation, if it is to be worth anything in its own right, must be done in an elegant new way or in a manner that will noticeably advance the state of the art. (1992, 19)

This changes with parapsychology (and other borderlands of science). “It is only when the existence of some phenomenon is cast into doubt,” Collins says, “that attempts are made to use replicability as a test” (p.20). Rao explains why: “in controversial sciences and when anomalous claims are made, replication takes on greater importance because questions that are of secondary concern in normal science are now raised to a level of primary importance.”

This tells us that there is no escaping the focus on replicability that has been aimed at the parapsychological enterprise. But it is not as onerous as many believe, as in an interesting way it makes parapsychology more like the rest of science. This is so because it turns out that, even in mainstream science, replication is not the cut and dried process that a naive view takes it to be. Inexact, imprecise replications are more common in mainstream science than most are aware, not just in parapsychology. Giles reports on examples of replications (both observational and experimental) gone somewhat awry in

astronomy and biology – one, possibly because the original results were wrongly interpreted; the other because would-be replicators couldn't get the procedure down properly. This was the issue with a case documented by Collins, the attempted replication of the TEA CO₂ laser. Even though detailed schematics were published and scientists interesting in replicating the device were able to consult directly with the originators, attempted replications failed eight times (including one major government lab). (Collins, 1992; Rao, 1984)

Giles observes that “it is often hard to tell whether an inability to replicate a result is due to a group's failings or a flaw in the original” design. The problem may even be inherent in the phenomena one investigates. An example from biology presented by Hempel (1945, 19), challenges the expectation that monozygotic twins are identical in appearance. They never are – in some cases there are only subtle differences, but in some cases differences are significant. And attempting to predict behavior traits from genetics and environment is notoriously imprecise. Psychology, the discipline with most in common with parapsychology, has problems with replication – often in the same ways as does parapsychology. What we see, then, is that it is not just parapsychology that has a problem with replication, but it exists in mainstream science as well. That by no means lets parapsychologists off the hook. But it does indicate that struggles with replication are not reasons to reject it as a science, nor to dismiss the evidence it provides.

Allegation 6: *Fraud is prevalent in parapsychology, and much of the success of parapsychology research was dubiously obtained .*

The argument of fraud is often the final resort of critics when confronted with successful parapsychological results for which they cannot otherwise account. (Pinch, 1979; Hardin, 1981; Hansel, 1981; Brewer & Chinn, 1994; Lin, 2007) This argument is traceable at least as far back as Hume’s “Of Miracles,” in which Hume declares that he would sooner believe a miracle was perpetrated by fraud, even if implausible, than first accept that a miracle really had occurred (Hume, *Enquiries*, x:91; Pinch, 1979, 332; Hardin, 1981). The perception of parapsychology as a field with a high rate of fraud likely dates to the 19th Century, when a number of spiritualists and trance-mediums were caught counterfeiting various ghostly effects. Critics also often conflate scientific parapsychology with the realm of “popular parapsychology,” which adds to a false impression of lack of methodological care, and openness to cheating.

However, fraud as an explanation for scientific parapsychological effects is much less tenable today. Researchers have continually tweaked protocols to make them increasingly resistant to the possibility of fraud – not because researchers are otherwise likely to cheat, but because of the need to eliminate any possible loop-hole that could be exploited as a way of accounting for positive results without having to consider that extrasensory perception is real.

It is *possible* that occasional instances of fraud might still be present in parapsychology. But it certainly cannot be prevalent, and in any case could not account for the extensive positive results. Skeptic Ray Hyman admits the incidence of fraud is relatively low in scientific parapsychology, but also alludes to a further relevant point that is a consequence of hints of fraud, whether true or false: “The few, but well-

publicized, cheating scandals that were uncovered ...worked against parapsychology's acceptance into the general scientific community” (Hyman, 1995, p.3-47). Though fraud is not unknown in mainstream science, finding it there does not have nearly the impact on legitimacy as it does in parapsychology. Cheating by one or two scientists in, say, physics, is not generally taken to reflect on the integrity of all physicists. Catching one or two parapsychologists falsifying data would tend to damage the reputations of all parapsychologists, deserving or not, though with no more justification. Ironically, parapsychologists as a class are likely to be more trustworthy for two reasons – they know they are under scrutiny, and the benefits of fraud that may accrue in mainstream science (lucrative grants, better employment prospects, increased support) are all but unavailable to parapsychologists. Perhaps even more ironically, parapsychologists tend to be even more vigilant in watching for fraud among their peers. Contrary to popular perception, few skeptics have detected instances of fraud in scientific parapsychology – but parapsychologists themselves have been active in ferreting out possible fraudulent research. (Rhine, 1974; Hansen, 1990)

As it stands, the argument from fraud in parapsychology is dubious. Even if fraud is present, it is likely to be at too small a scale to bias overall results. In a argument similar to that used against the likelihood of methodological flaws, Utts observes, “If fraud were responsible [for claimed parapsychological effects].it would require an equivalent amount of fraud on the part of a large number of experimenters or an even larger number of subjects” (1995, p.3-30). Were fraud of any significant degree present in parapsychology, keeping it hidden among a group of individuals as relatively large and

diverse as that would be implausible.

Replacement Paradigm

An argument very often launched against parapsychological evidence is that it lacks theoretical justification. The argument runs something like this: grant for the sake of the argument that ESP evidence was produced by accepted rules of scientific research; that it was not produced by fraud; and that it is not the result of erroneous interpretation – that, in short, it was legitimately produced and can be taken to be on par with other sufficiently respectable scientific evidence. Yet this is still not enough for us to take ESP seriously, because there is no acceptable theoretical context that can explain such evidence. And without such a theory, science is justified in rejecting it, despite its provenance and bona fides.

An excellent example of this argument was lodged by Steven D. Hales against a thesis argued for by Robert Almeder in favor of death survival, specifically reincarnation. Almeder had offered what Hales adjudged to be “the best set of arguments for the thesis that some people survive their deaths” that Hales had ever seen. Almeder presented a number of evidential accounts such as apparitions, possession, reincarnation, mediumship, and so on, and then concluded that so strong was the evidence that “one would be irrational not to believe in life after death” (Hales, 2001, 336). I have no interest here in defending the thesis of death survival, the empirical evidence for which I believe is notably weaker than that for ESP. However, the form of the argument Hales

uses in rebuttal to the survival hypothesis can, as he insists, “be employed against all paranormal claims that are inconsistent with what are antecedently considered the best theories of the world” (Hales, 2001, 343).

The argument Hales aims to refute is the following: “If there is such-and-such evidence, then we should believe in reincarnation; there is such-and-such evidence; therefore we should believe in reincarnation.”⁹ But he argues that even the best evidence is insufficient to prove reincarnation is true. The reason is that the evidence for reincarnation is crucially different than other kinds of scientific evidence in that it is inconsistent with our “best theories.” In fact, “reincarnation is not consistent with either our best empirical theories or with our best philosophical theories about the mind” all of which involve some form of materialism (pp.338-339). So no matter how good the empirical evidence for reincarnation is, since there is no theory accompanying it, we should reject it.¹⁰ “Suppose one were to argue in the following way,” Hales suggests

if we are justified in believing theory T (e.g., materialism about the mind), and we know that T implies not-p (e.g., that there are no reincarnating Cartesian egos), then we are justified in believing not-p. Add to this, á la Almeder, that we are not justified in believing not-p, on the grounds that there is evidence in favor of reincarnation. Therefore, by modus tollens we are not justified in believing that

⁹Framed in this perhaps somewhat oversimplified way, my own argument might go something like this: “If there is such-and-such evidence, physicalism (at least in part) is false. There is such evidence. Therefore physicalism is false.” The ‘such-and-such’ locution can be replaced by “ESP,” so: “If ESP is real and non-physical, then physicalism is false. Evidence is strong that ESP is real and non-physical. Therefore, physicalism is likely false.”

¹⁰Hales here approvingly cites Sir Arthur Eddington: “one should never believe any experiment until it is confirmed by theory,” apparently missing the point that Eddington was facetiously objecting to a position closely resembling that which Hales advocates.

theory T is true. (339)

But this, Hales believes, leads to “extreme skepticism.” All theories, even the best attested ones, have some anomalous data in their backgrounds that could serve as counter evidence. But there is “no *a priori* way” of sorting out what of this anomalous data may be truly anomalous, as opposed to that which may eventually turn out to be accommodated under existing theory, which of it is merely a result of flawed procedures, which is attributable to fraud, and so on. So just because we run into data that is inconsistent with our best theories, that is no justification to reject the data without a suitable explanation as to why it is false. To do otherwise results in an epistemic impasse where “no theory is justified” if there is *any* contrarian evidence against it. Hence, it is better to hold to a well-attested theory and assume that it is the contravening evidence that is misconstrued or ill-formed.

At first this objection seems reasonable. But at this point Hales goes seriously astray. Epistemic conservatism requires, he believes, that “*any* hypothesis that explains the data in a way consistent with our best theories is, *ceteris paribus*, superior to any explanatory hypothesis inconsistent with our best theories” (p.341, emphasis added). And he means this to be taken to its logical extremes. “[E]ven a far-fetched explanation like mind-controlling [extra-terrestrials] is superior to reincarnation” (p.343) because of the way it preserves physicalism in the face of a Cartesian onslaught. There are “indefinitely many such hypotheses” which can sustain a physicalist explanation in the

face of a non-physical explanation.¹¹ Based on Bayesian prior probabilities, he believes that even the most obscure and poorly-defended physicalist hypothesis trumps even the best attested antiphysicalist evidence.

This may not reflect the position of other physicalists; perhaps there is a threshold beyond which they are more likely to take anti-physicalist evidence seriously. But what Hales argues in summing up his position, most physicalists should find more congenial. Since most scientists and philosophers have the same starting position of physicalism with regard to the mind, they are strongly inclined to conservative explanations that reconcile anomalous data consistently with physicalist theories. Hypotheses that do this are “epistemically superior” to any that are contrary to physicalism. This, according to Hales, is a “desideratum” that parapsychologists mistakenly “fail to heed” in believing that the evidence they have is persuasive.

Hales sums up his argument with an appeal to the history of science. One should not abandon an important explanatory theory until one has an equally good or (presumably) better one to put in its place – even if the older one is obviously deficient. “[I]t would have been epistemically wrong-headed,” he observes, “to abandon geocentrism and its epicycles without its Copernican (and perhaps Keplerian) replacement.” This is something with which Jaegwon Kim agrees. “To motivate the discarding of a framework, we need independent reasons – we should be able to show it to be deficient, incomplete, or flawed in some fundamental way, independently of the

¹¹Note that Hales does not endorse a space-alien-practicing-mind-control-on-humans thesis. He considers this highly implausible. It just is less implausible than is

fact that it generates puzzles and problems that we are unable to deal with” (2005, 30). To illustrate the point, Hales calls upon an amusing analogy of a “dark-sucker” hypothesis for light bulbs, versus the standard light-emitting hypothesis. There are many pieces of evidence to support a dark-sucking hypothesis, such as the fact that the room fills with dark as soon as the dark-sucking bulb is turned off, and so on. But the dark-sucking theory fails because it fails to cohere with our best theories, which offer better alternative explanations against the dark-sucking thesis.

There are four ways in which Hales’ argument begs the question. First, his argument that one should ignore anomalous data in the interest of theory preservation depends greatly on what quantities of data are under consideration. In the face of a few data elements that seem unreconcilable with the preferred theory, his points seems reasonable. But it hardly follows that just because we can safely wink at a trivial amount of contrary evidence we should without twinge of conscience, be able to turn the same blind eye to much larger quantities of it. Any theory is likely to rapidly erode as the quantity and quality of the anomalous data increases. In terms of ESP, Hales’ argument seriously underestimates the amount and quality of the data available. As Kuhn has argued, sufficient quantities of well-attested anomalous data can seriously undermine established theories.

Second, as Hales illustrates in the light-emitter vs. dark-sucker case, our best theories have far better explanations for how a light bulb lightens a room than does the competing dark-sucker challenger. However, in the case of parapsychology, once one

reincarnation – or, by extension, ESP.

removes all the mundane explanations: cuing, sensory leakage, methodological flaws, fraud, and so on – our “best theories” have *nothing* to offer as an explanatory alternative.

Third, Hales misses the point of the parapsychologist argument: that the reason why what are considered the best theories are considered the best is because in the case of extrasensory perception the proponents of the “best” theories have all along ignored or marginalized the evidence for claims that threaten their position. If that data were fully and properly taken into account, perhaps the “best” theories would no longer count as the best.

Finally, on the matter of theory change, it is true that one should be careful of abandoning a theory that works in the absence of anything suitable to replace them. “For a false theory (with high verisimilitude) may yet be the only hope to guide research,” Jonathan Adler notes. “It is better, that is, than no theory at all. Refutation at *t* requires rejection in principle, but not rejection at *t*” (1989, 237). Hales and Adler join a long list of distinguished opponents of parapsychology in this sentiment. But to see where they all miss the point, consider the construction of Hales’ argument. Both this “dark-sucker” versus light-emitter theory and the geocentrism vs. heliocentrism pairing represent the superiority of certain epistemically-complete theories in which consequences can be deduced and demonstrated from objective observations. Both sets of theories are or conceivably could be (in the dark-sucker case) constituent theories of physicalism. But the universal formulation of physicalism – that *everything* is physical – is of a different character and quality than are its constituent theories. It is open-ended, in that short of total evidence no amount of data can come close to fully confirming it. Contrastingly, in

the case of heliocentrism, the theory is falsifiable *in principle* in a Popperian sense, but we would be hard-put to actually falsify it in *reality*. This is not because falsifying heliocentrism would be hard to do *if it really were false*. It would be hard to do because we already have *almost all* the evidence that could ever exist on whether the Sun really is the center of the Solar System, such as to make the theory virtually incontrovertible. As much as we can know anything in science, we *know* that the planets revolve around the Sun in elliptical orbits that follow precise rules for which we have a multiplicity of diverse, independent confirmatory evidence. The universal formulation of physicalism has no such support.

Hales and many others make the following argument: For one theory to replace another it is necessary that the replacement provide equal or better explanatory power across all the phenomena that the theory being replaced covers. If there is no suitable replacement theory that can explain all the phenomena, then it is better to stick with an imperfect or even false theory rather than have no theory at all. There is no such explanatory theory for ESP. Therefore ESP evidence gains no traction against physicalism.

But this argument is fallacious. Those such as myself who take seriously evidence for extrasensory perception, do not offer it as a replacement for the well-established framework of theories that comprises the explanatory context for the physical world. ESP is no threat to the vast bulk of constituent theories under physicalism, only to the universal meta-claim to exclusivity.

Universal physicalism is a metaphysical claim about the universe and is not itself

an explanatory theoretical framework in the same sense as, say, cosmology or virology. Indeed, universal physicalism is a thesis that is over and above – and to some extent independent of – its subordinate constituent theories. On the one hand, the discoveries, structure, subject matter, and methodologies of chemistry, biology, geology or even physics are for the most part unaffected whether universal physicalism is true or not. On the other hand universal physicalism draws upon all the physical and special sciences for support of its only claim – that everything is physical. And the support thus lent is rather weak – for example, chemistry is about physical things. There is no plausibly non-physical evidence against chemistry as there is against universal physicalism. Since chemistry falls under the umbrella-theory of physicalism, that part of physicalism is demonstrably physical. But for the most part the physical and special sciences don't directly say anything about whether the universal claim itself holds or not.

Everything doesn't *have* to be physical for seismology, optics, or particle physics to retain their legitimacy. The reality of ESP does not undermine chemistry; it is no threat to biology, geology, thermodynamics, astronomy, genetics, paleontology, zoology, physics, and so on. But it *is* a threat to universal physicalism. Ironically, it is also a perceived threat to psychology, possibly the one science that seems to feel most threatened by ESP¹² (though other social sciences may feel the resultant pinch).

Psychology is only vulnerable, though, insofar as certain features of mind turn out not to be physical – causing a certain discomfiture to the prevailing metaphysical commitment

¹²It is perhaps thus no accident that psychologists represent a sizable contingent among the skeptical ranks.

in psychology that everything mental *must* be physical – while leaving the bulk of psychology unscathed.

What all this means is that extrasensory perception requires explanation, but is not obligated to include a full-blown replacement theory for physicalism as a fully-featured theory. Perhaps an explanation for parapsychological phenomena would have a starting point somewhat along these lines: “There are features to mind and the universe that are non-physical.” On the face of it, this formulation seems minimalistic. It would clearly require some filling out. But some details are already available, and with further study many more could be added.

The following specification should suffice for a start: “Some things in the world are non-physical.” Notice, this is construed at the same hierarchal level that the universal formulation of physicalism occupies: “*Everything is physical*” is of the same level of specification as “*Some things are non-physical.*”¹³ Note as well that the ESP claims carry the same metaphysical weight as a minimal physicalist claim: “There are things that are physical” vs. “There are things that are non-physical.” These two claims are compatible.

Universal physicalism, however, makes a more radical claim: Not just *some* things – nor even *most* things – but *everything* is physical. Extrasensory perception is very much incompatible with this, even if ESP’s claim is merely existential, rather than universal. But the incompatibility goes both ways. “Everything in the world is physical”

¹³Note that “everything” and “something” are meant to be taken strictly as general placeholder terms, and not as a commitment to what *kinds* or *categories* the “things” thus

is itself incompatible with the claim that “There are *some* things in the world that are non-physical.”

The “*Everything* in the world is physical” claim is much harder to defend than is the claim “everything in physics is physical,” or the claim that “everything in chemistry is physical.”¹⁴ As I have, I think shown, the claim that “There are *some* things in the world that are non-physical” is easier to support. It is apparently not *much* harder than that to show that everything in physics is physical – whether those who disbelieve it are willing to be persuaded or not.

referred to belong.

¹⁴Ignoring, of course abstracta and possibly some relations.

Chapter 12: Conclusion -- Reversing the ITBE

Eleven chapters ago I set out to discover whether physicalism could be falsified. I have established that extrasensory perception is a real phenomena based on sound scientific evidence. I have shown that, as a consequence of ESP being real, universal physicalism as construed now and for the foreseeable future, is false. I have nearly, but not completely succeeded in showing that any future universal description of physicalism is also plausibly false. I will now expand on these conclusions.

My argument began with an examination of the universal formulation of physicalism, to see in what way it might prove vulnerable. I noticed that physicalism was based on inductive evidence and its truth grounded in a robust inference-to-the-best-explanation (ITBE). Several things soon became clear: First, that the usual antiphysicalist arguments failed – not because they were false, but largely because they were inconclusive enough that they were easily overcome by physicalism’s presumption of truth; second, that physicalism’s weak point seemed to be the causal closure assumption – which specifies that no causation can intrude into the physical world from beyond it. If closure could be shown to be false, then universal physicalism would automatically follow.

I came to believe that the best falsifier would be a causal chain that started inside the physical universe, exited it, then reentered at some further point. This would be an intermittently truncated causal chain – an ITCC – and if I found one, it would be evidence that closure, and hence physicalism, was false. I first found and presented four

research paradigms that seemed to fit the bill as ITCCs to work as closure-violators. But then I had to show that they really counted as closure violators by exploring various non-closure-violating alternative explanations for both precognition and perception-at-a-distance. Finding no alternatives, I next had to assess the evidence for the candidate violators against seven criteria expected of legitimate scientific evidence. My candidate closure violators passed muster. Finally, I argued that several long-standing criticisms against the credibility of my closure violators were themselves not credible.

Now that I have found not just one, but four categories of closure-violating ITCCs, with numerous actual instances of causal closure violations within each paradigm, I am confident I have achieved my goal of offering a credible, substantive challenge to the universal physicalism claim. I am equally confident that my challenge will not be met with equanimity from those whose careers and personal beliefs may be invested in physicalism.

But despite the data I have presented and my supporting arguments, I am not as confident that I have also attained my further objective: to show that physicalism – in its universal construal – is false. I will get to that shortly.

I wish to be clear at the outset what is being argued. I expressly do not assert that physicalism *as a thesis about the physical world* is false. In fact, this cannot be done, as the world described by the theoretical components of physicalism is clearly physical. Up to this point, the form of my argument might be considered simplistic: Physicalism claims that nothing exists that is not either physical or strictly a consequence of physical facts; a certain category of evidence strongly attests that there are at least some non-

physical features involved with the universe in some way; thus, physicalism is false. As

Melnyk says,

physicalism is false if there is even one causal or contingent token – past, present or future – that falls within the scope of [the physical world] and that is neither a token of a physical type nor a physically realized token of a functional type...Whether there exists any direct evidence against [physicalism] is an entirely a posteriori matter. (Melnyk, 2003a, 175)

The evidence I have provided demonstrates many causal tokens that can't plausibly be described as either a physical type or a physically realized functional token.

Up to this point, my thesis is not especially novel. Many over the years have argued informally, and a few formally, that parapsychology phenomena are convincing testimony against the universal formulation of physicalism. Where my argument is unique is in the way I have tried systematically to place the evidence in a framework of argumentation that shows its evidentiary value to its best advantage.

Implications for Physical Science

There are those who will be disturbed by the implications of my argument. Physical scientists, for example, might be among the worriers at first. But soon I think they will realize that there is no reason why they should be particularly bothered by my conclusion. Whether physicalism is universally true or not affects their research domains very little. As Barbara Montero says, denying causal closure

would not amount to the denial of a complete physics...that is, one can deny that physical effects have only physical causes while accepting that science can nonetheless provide a complete (causal) explanation of all the physical effects. *A fortiori*, our dreams of a complete science of the physical world need not be shattered if we were to deny [closure] (2003, 177).

If scientists are dubious or distrustful, let them learn the appropriate protocols and do their own replications. Despite complaints from the popular media of “millions wasted” on ESP research, as it stands, parapsychology has been a shoe-string effort since its inception.¹ More attention from objective, mainstream researchers would be welcome.

Limiting Physical Explanation

There will be some who might become anxious about the admittance of a non-physical element to the universe, for fear it may take something away from science. A frequently-voiced complaint about Cartesian dualism is that, if the world really is divided up into physical substance and mental (e.g., non-physical) substance, such that the mental substance was wholly other than physical, there would be no way to know anything conclusive about one entire aspect of the universe.

If extrasensory perception does bespeak non-physical causality, many who take it merely at first blush worry that it poses the same threat as Cartesianism. According to Montero, denying closure “is thought to amount to the view that there are phenomena that will permanently elude scientific explanation. In Kim’s words, if the physical world were not causally closed, then ‘to explain some physical events you must go outside the physical realm and appeal to nonphysical causal agents and laws governing their

¹If one were to tally up all the monies spent on parapsychology over the past 130 years, it would pay for perhaps ten MRI machines, or just over half of one F-18 Super Hornet, the Navy’s best jet fighter (compare that to the almost \$4 billion price tag of CERN’s Large Hadron Collider – which would buy 73 Super Hornets, or 1,300 MRI machines).

behavior!''' (Montero, 2003, 178)²

The worry here goes mostly to the nature of mind. But it is not necessarily the case that acknowledging *some* non-physical aspects to the universe (especially to mind) leads to a dead end. Even if human minds have features that tie into some non-physical aspect of the world (as ESP seems to demonstrate) that does not mean that the mind is completely – or likely even mostly – outside the range of physical exploration. Physical aspects of the mind and brain must still be explored, and just because some element of mind may be non-physical does not put it beyond our ability to learn *something* about it (the same could be said for Cartesian minds, for that matter). Mental components of various parapsychological phenomena clearly have effects in the physical universe – otherwise, we would not have significant correlations between helping episodes and EEG traces or fMRI results in DMILS experiments, or accurate sketches and 3-D models produced in remote viewing sessions. These components can still be explored, if only to explicitly define where the junction between physical and non-physical may be. I return to my earlier discussion of dark matter for an analogy.

By definition (and experiment) dark matter is undetectable by standard observational methods and instruments. Yet scientists think its presence is demonstrated by the gravitational effects it seems to exert on the rest of the universe. It may not be *directly* observable, but *indirect* measures point to its existence. The same is somewhat

²For some, the worry here is how the existence of a non-physical aspect to the world might undermine dreams of a neatly-packaged explanation that ties all of our theories together in one integrated bundle. But this kind of closure is a psychological, not a scientific need, and in any case, if there *is* a non-physical aspect to the world, it

true as well for our observations of the subatomic particles that populate our theories of standard matter and energy. We have no direct observational data of electrons, protons, or neutrons – we instead rely on indirect indications, such as tracks in cloud chambers or in sensitive emulsions. Yet we learn new things about unobservables all the time. No doubt there are things about a non-physical domain we might never learn. But to conclude therefore that there is *nothing* objective we could learn is short-sighted. In ways similar to how we find out about quarks, we may through inference, instrumentation, and experiment with the best ESP-detectors we have access to – humans – discover much we would never have otherwise suspected.

One can see the importance of acknowledging non-physicality (assuming that, as my argument implies, it is real) on a possible-worlds account. Possible worlds analysis is often used to show the modal force of various propositions. It is often called on to illustrate physicalism, to wit: “minimally physical duplicate” (MPD): Physicalism implies that any minimally physical duplicate world will be a duplicate *simpliciter*, meaning that there is nothing over and above the physical. However, if psi is real – and influential – then a world without it could never be a MPD of a world with it (that could only work if non-physical phenomena are epiphenomenal). Therefore, a world with causal non-physical inputs will physically *look and be different* from a physical world without it, even if that world contains every physical particle and every physical cause the psi-influenced world does. This is what ESP experiments mean to show on the mid-

does no good to wish it away.

to large-scale³ – that situations otherwise in all respects similar will have different outcomes depending on whether there is input from TCCs or ITCCs.

Implication of Rejecting the Data

There is a corollary to the preceding principle. If we reject the data and retain our faith in universal physicalism, and nevertheless the universe does have non-physical features which we ignore, that could in its own way undermine the progress of science. Suppose there are some features of the physical universe that to be understood require a full understanding of how non-physical interactions influence them. Without a realization of the interaction, scientists might research indefinitely and either come up with no solution, or with a theory that never quite matches the phenomenon being examined. It is also possible that scientists might try to understand a phenomenon that ultimately cannot be understood at all by physical science because of some unacknowledged non-physical component. In that case years and resources might be used up and still reach no conclusion. That doesn't mean such research would be useless – there would still be much about the *physical* universe that might be discovered. But even *complete* physical theories would be unachievable, and science would not know why. As it appears for now and for any physicalistic explanatory frameworks we can reasonably anticipate, the latter point describes the status of scientific understanding of

³That is, from the level of anomalous autonomic nervous-system (ANS) activation (mid-scale as compared to micro-scale nuclear and sub-nuclear phenomena) to the level of accurate sketching and 3-D modeling of target locations through anomalous processes (large-scale as compared to ANS activation).

human mentality.

The Problem of Religion

Another concern that will occur to some revolves around the question of religion. Might I not be re-opening the door to the “Demon-Haunted World” of religion and superstition made famous by astronomer and noted skeptic Carl Sagan (1995)? Probably not. Michael Scriven (1956) draws an analogy between religious beliefs, that is “beliefs in the existence of a specifically religious experience that involves communication with a supernatural being, or a specific act of creation involving homo sapiens, or miraculous healing,” and this view of parapsychological phenomena. Where religious claims differ from parapsychology claims, Scriven notes, is that claims of psi experience generally focus on the actuality of the phenomena in question, whereas religion makes metaphysical claims that have “specific theoretical import,” which are in addition “disturbingly evanescent.” Of course, my thesis goes beyond just arguing for the reality of psi phenomenon, to the implying of a metaphysical argument – that the physical world is in fact *not* all there is; that the evidence argues for some non-physical aspect of the universe as well. Metaphysical claims are notoriously difficult (if one were a logical positivist one might even say impossible) to ground. But my implied argument leaves a gap between the existential claims for extrasensory perception and the often dogmatic metaphysical claims of religion.

Readers tempted towards criticism of this position should be cautious, however: Even if my argument implies the existence of some non-physical aspect to the universe, I

propose no explanation, description, doctrine, or assumption about what that aspect might turn out to be. The possible solutions range from mere property dualism as argued for by David Chalmers and others, to a realm of influence on the same level of ontology as matter, with only ephemeral impact on the physical world, to a companion non-physical world keeping pace with our own, to a full-blown spiritual realm peopled by angels, supreme being(s), and so on. My project does not commit itself to any one of these views, and I am in fact decidedly *not* trying to imply the latter – though if my argument goes through, even such a religiously-relevant domain will be shown to be *possible* (though by no means to be taken as granted).

When I was giving an overview of this project to someone very early in the process, my interlocutor exclaimed, “why, you’re trying to prove the existence of the human soul!” I am, however, doing no such thing. Nothing that has gone before in these pages, and nothing that follows will prove, or even indicate the existence of the human soul or any other ontologically non-physical being. On the other hand, die-hard physicalists are likely to find my thesis disconcerting, since what I’m arguing for does seem to allow the *possibility* that human souls or other such entities exist. But merely allowing for an uncomfortable possibility should not stand in the way of nor be held against a sound argument well-grounded in evidence.

There will be those who want to push back against my argument. They may respond that my conclusion is premature. As both Duhem and Quine have in their own ways argued, hypotheses and theories are tested not in a vacuum, but in a larger,

interconnected theoretical framework, and an apparent falsification may be waylaid by calling upon auxiliary hypotheses or adjusting the theory somewhere else in the explanatory schema. On these grounds, there are two ways a physicalist might respond to my argument: 1) Perhaps there is some aspect of physical theory we have overlooked or which has not yet been discovered which can account for these seemingly non-physical phenomena; and 2) even if there are some non-physical features of the universe physics doesn't account for, that doesn't invalidate those implications of the doctrine which apply to the physical aspects of the universe (for example: some physicalists have been willing to tolerate the possibility of epiphenomenal non-physical properties).

A Weaker Version of Physicalism?

I shall entertain the first of these objections later in this chapter. But as to the second, I already acknowledged that I have no objection to the idea that recognizing there may be things that are non-physical does not negate the status of all the things that are physical. Rather, I defend a much narrower assertion than the falsity of all of physical science. My argument is that the universal construal of physicalist doctrine, that *everything* is physical (or a consequence of physical facts), is false. As I have said before, this leaves the bulk of physicalism as it pertains to the physical and special sciences unscathed. It further in no way undermines science's continuing project of exploring the physical properties, entities, qualities, principles and relationships wherever that might lead. For the most part, science can go on essentially the same as before. If anything, it gives science more to do: explore the boundaries of this newly acknowledged

frontier of reality, and find out more about how it works.

More specifically, I dispute the sort of contention illustrated by Melnyk in the following passage:

But despite their nonidentity with properties that are mentioned as such in fundamental physics, mental properties are nevertheless physical in the following broader sense: the mental properties of all *actual* possessors of mental properties are only ever realized by the appropriate role playing of their possessors' fundamental physical properties. Nothing in the nature of mental properties rules out the possibility that they should have been realized by properties of some utterly different kind, even by ectoplasmic properties (if such there could be); but in fact, they never are. (Melnyk, 2003a, 8).

By 'current physics' Melnyk means: "...those theories that are the object of *consensus* among current physicalists," determined by looking through physics textbooks. (Melnyk, 2003a, 15) But this has the effect of preserving a status quo, even if it is wrong.

Both Melnyk (2003a) and Kim (2005) talk about connecting up macro objects, events, properties, and so on to "functional types" as a form of physicalistic reduction. These functional types are not necessarily physical – they just depend on physical realizers to exist, physicalism being true. But (psi being real) why can't some functional types be realized by non-physical causes? Some may worry that I appear about to advocate dualism as a solution, but that is not necessarily the case.

My account may in fact imply a "dualistic" solution without arguing for a full-blown substance dualism in the Cartesian sense. A dualist solution violates only closure, not necessarily determinism. Extra-physical causal chains can legitimately integrate into our physical and natural laws through the principle of preemption. Deterministic causal

chains are frequently derailed or sidetracked by other preempting deterministic causal chains. The only difference here would be the origin of the causal chain in question. Once it irrupts into the physical domain, it behaves like any other physical causal chain.

But that is all I need say about this here, and it is only in anticipation of those who may be uncomfortable with where my argument might lead now that I have undermined physicalism as a universal doctrine. That is my only goal with this project, though I might at some later date gladly undertake a follow-on objective of exploring where all this might lead.

Can ESP and Physicalism be Reconciled?

There remains one final challenge to my thesis which I must do my best to dispose of. A frequent response to the claim that extrasensory perception is an important counterexample to physicalism runs along these lines: We all know that our understanding of the physical world is incomplete. Is it not just as likely that a fully-developed physics will have grown to include these for-now apparently extra-sensory, extra-physical phenomena within its explanatory framework? Jeffrey Poland has anticipated this argument: Perhaps “. . . any intractable phenomenon could be downwardly incorporated into the physical bases, thereby sparing physicalism from embarrassment. The *physical*, being open-ended and evolving, could incorporate whatever is required to keep physicalism afloat” (Poland, 242; emphasis in original).

As physicist Jean Burns has observed

It is, of course, possible that an addition or extension to known physical laws

could be made which takes into account the action of consciousness. Presently known physical laws were never intended to describe consciousness, but only matter. And the history of physics shows that radical additions to physics are sometimes made as new phenomena are taken into account. Nevertheless, if psi is real, a radical extension of physical laws, which takes into account the action of consciousness, would have to be made...so evidence for psi need not be considered conclusive if some alternative explanation which falls within the purview of presently known laws can reasonably be offered (Burns, 2005, 72).

And One ESP-friendly philosopher, Michael Scriven, put it this way: “It is...proper to exhibit considerable skepticism about claims that conventional science and parapsychology are incompatible, and also about the diametrically opposed conclusions that are drawn from this incompatibility” (Scriven, 1956)

Here is the challenge: How do we know a completed physics will not eventually be able to fully accommodate extrasensory perception and other kinds of parapsychological phenomena among physical forces and laws? To start with, it seems obvious that the parapsychology evidence I have cited, plus much more that there was not space to explore in this project, clearly falsifies *current* physics. If the psi data is legitimate – and if (as I briefly argued in chapter 10) one rejects its legitimacy one must question the legitimacy of any scientific evidence produced in conventional research domains as well – then causal closure has been violated many times and physicalism, as a universal claim, is convincingly falsified.

As I have said before, this most definitely does *not* mean that every physical claim is false. Indeed, every entity and every relation that has been shown to be physical or dependent on physical facts remains just as true and scientifically real as it was before I launched this argument. And everything else that scientific investigation may find to be

physical will also be legitimately physical. I do not falsify physics or physical investigation. I just show there is a boundary marking the edge of the physical domain.

One might ask, doesn't what we know of the history of science tell us that the current scientific paradigm must be false? Yes, of course, but it tells us about a different kind of falseness than what I have shown. Modern science is considered false in terms of being incomplete with regard to some future, fully-realized science. It is not false in this way, though, quite so dramatically as how the geocentric theory of the universe was false in comparison to the heliocentric theory that eventually supplanted it. Rather, today's physics may be false with regard to a future physics more in the way Newtonian physics was "false" (in terms of being incomplete, with associated false conclusions) in comparison with General Relativity and Quantum physics.

My argument is not against a scientific theory *per se*, but with a metaphysical claim *about* scientific theories, a claim that says the current scientific theories show that the physical world is all there is. It is *that* metaphysical claim that has been falsified.

But isn't it possible that psi will eventually be fully reconcilable within a physical explanatory context once we have a *fully* realized physics, with completed reductions of all the special sciences to the fundamental forces of the universe? I am not sure whether those who say this are aware of just how far-out a claim it is – one that reaches into some indefinite future in which a certain state of affairs *might* come to pass, the likelihood of which we have no way of reckoning. But it is a popular position, so I shall entertain it. There are two ways one might respond to this "futurist" claim.

One is to suggest that perhaps physics already very nearly *is* complete. Despite

obvious risks there are those who argue for this. Nobel Prize-winning physicist Steven Weinberg (1999, 1992), for example, has expressed some confidence that we are nearing completion of an overall physical account of the world. Certainly, there will be details to clean up, but we are almost there (he speculated in 1999 that we will have a rough outline of the final product by 2050).⁴ If this is so, then maybe we are a lot closer to a resolution than we thought. And if *that* is so, then it looks somewhat unlikely that there will be any reconciliation between parapsychology and physicalism. This is because, as it stands now and for the reasonably foreseeable future, physicalism and parapsychological effects look to be *prima facie* incompatible – meaning, that if the research I have presented is reliable, then universal physicalism is very likely false. Because of the way parapsychological effects conflict with the laws of physics, and considering the likelihood (by extrapolation and based on Weinberg’s testimony) that natural law as we understand it now will nonetheless still provide the general outline of any *foreseeable* physics, it seems reasonable to conclude that ESP and universal physicalism will never see eye to eye.

Having a fully-completed physical account of the world doesn’t equate to knowing every physical thing in the universe. We could (arguing against the “*qualists*”) have a complete physical story about how humans perceive and process color, right down to a full physical explanation of the *experience* of color. But we may not know what colors are on the far side of a moon in a distant galaxy (in fact, we probably won’t). We

⁴Certainly, the “end of science” has been predicted before. But this time there seems more reason to think so – if only because we now much more than we did the last

could, though, have a full story of how the brain works (barring possible non-physical affects) and the odds are very good that if future physics looks anything like present physics, there will not be a physical explanation for ESP.

But what if there is no “end of physics” in 2050? Can’t we argue that Weinberg is incurably optimistic and the possibility of such a full story of the physical world is going to be delayed indefinitely far into the future and, if it *is* finally realized, look dramatically different than physics looks today? Shouldn’t we then wait to declare universal physicalism dead until we have the coffin *and* all the nails? Then maybe it won’t be physicalism that gets buried, but extrasensory perception as a non-physical phenomenon.

What I am calling the “futurist” position requires such an open-ended claim, that it is hard to respond to. We certainly would be justified in leveling a charge of Popperian “*ad hoc*-ism” at anyone making it. As I discussed in chapter four, Karl Popper famously accused both Marxism and Freudianism of being unfalsifiable and, therefore, unscientific, because every time an objection was raised to some part of the theory, an appropriate “just-so” story (to borrow a term from Kipling and Gould) could be found that would either divert the force of the objection or incorporate it relatively unexplained into the theory.

So then the question to pose to physicalists is this: “How long until you would finally acknowledge that the universal construal of physicalism is false? I have provided plausible evidence that falsifies physicalism. You suggest that, since physics is not yet

time the end of science was predicted.

complete, if we wait long enough for science to catch up, we *may* eventually find that a physical explanation can account for the phenomena I propose as a falsifier. Is there ever a point where the likelihood of such a possibility becomes so small that you will eventually concede?”

There is, of course, no answer to this, since there is no way to know how long before – or even whether – we will arrive at a sufficiently definitive account of the universe, such that we can be certain we know whether extrasensory perception and physicalism are, after all, compatible. Of one thing we can be sure – under the conditions specified in the objection, it is likely to be a *very* long time. (But it is hard to escape the suspicion that what this amounts to is an excuse to hold on to a certain set of physicalist beliefs as long as possible.) On those grounds alone, one would think *ad hoc*-ism would be a legitimate indictment of physicalism. However, Popper himself eventually realized that too fierce an insistence on falsification and too stubborn a resistance to *ad hoc* explanations was not in the best interests of science. There really are times when an *ad hoc* explanation is true, and it is needed to preserve a legitimate theory. There is, therefore, no answer possible to the physicalists’ plea for patience. But I submit that the burden of doubt has now shifted towards physicalism, and that if it doesn’t feel so yet, physicalism really ought to be on the defensive.

A NEW INFERENCE TO THE BEST EXPLANATION?

My project never proposed to come up with solutions to any problems, nor answers to any questions – save, perhaps, the question that the majority were not asking,

Is physicalism true? For the most part, people look askance at arguments that tear down a theory with nothing new to offer in its place – which often strikes us as being both unproductive and vaguely mean-spirited. In this case, however, just showing the error of physicalism is valuable, and contributes to progress in a round-about but nevertheless indispensable way. If science is in fact working under a false theoretical construct – one that admittedly allowed great progress in certain directions, but which pursued a course that ignored certain fundamental questions – then being awakened from that dogmatic slumber would indeed be a positive thing (even if those so startled out of their sound sleep are grumpy about it). Waking up would also allow research to be done in additional directions that hold more promise of answering those same mysteries and questions. If the parapsychology project has substance, and is important, then science under the thrall of physicalism has been too much, too long like the drunk looking for his keys under the street light because that's where the light is.

In chapter two, I quoted Braithwaite on how evidence serves to confirm hypotheses that make up the structure of a scientific system. Though one piece of empirical evidence may support a hypothesis, only one bit is not enough to prove it, because it could happen that the hypothesis is right about that one thing, while being wrong about everything else. But this is true about *every* piece of evidence that comes to support the hypothesis. It could be right about all those instances, but still wrong about everything else. This is why counterexamples work – because they show what might be believed to be a proposition that is true about the whole world because it has so many confirming instances, is actually false in its universal claim. (Braithwaite, 1983, 46)

Physicalism is a general thesis about the *entire* universe (and the nature of mind in particular). Since physicalist doctrine rests almost exclusively on inductive (hence empirical) evidence, it can be confirmed by any number of true empirical facts, and (as we discovered back in chapter three) not be *proved* true without an examination of every fact in the universe. Yet, it can nominally be proved false by the right kind of disconfirming evidence.

Consider a case, though, of what we might call “dualistic” physicalism. Dualistic physicalism encompasses the entire physical world, but shares the universe (and perhaps interacts) with certain non-physical features of existence. Dualistic physicalism is not vulnerable to a counterexample, because it is not a general thesis about the universe; all it says is that *some* (or even most) aspects of the universe are physical. Since dualistic physicalism acknowledges the presence of non-physical properties and entities as co-inhabitants of the universe, merely demonstrating that “there is something non-physical” poses no threat whatsoever.⁵

We are, however, not dealing with dualistic physicalism, but universal physicalism. Just as it is essentially impossible, given the epistemic limitations of humans, to prove universal physicalism true (see chapters two and three), it is also impossible to prove it false. As the “futurist” argument for a fully realized physics demonstrates, physicalism is, in this sense, inherently unfalsifiable. And, surprisingly, in this sense so are the non-physical components of dualistic physicalism. Even if the unobserved causal properties of ESP and related phenomena are non-physical – for which

I hope I have made a persuasive case – it is just as impossible to prove conclusively that they are since, as in the case of physicalism, one would have to examine the entire universe to make absolutely certain that there was not some reticent physical feature tucked away in some far galaxy that proves a key to a physical explanation of ESP.

However, unlike the inconclusiveness of the knowledge, conceivability, and intentionality arguments for mind and consciousness I discussed in chapter three, the inconclusiveness of what could be termed the “ESP argument” against physicalism makes it far less vulnerable, since unlike the antiphysicalist arguments covered in chapter three, the ESP argument rests on robust empirical evidence. More importantly, the evidence can be demonstrated in a physical context without having to *be* physical in origin. The ESP evidence is for that reason much more of a challenge to physicalism than any argument thus far presented. (Skeptics are aware of this, which may be why they are so resistant to the evidence on which my argument is based.)

Even though I have only been able to prove my thesis to somewhere in the 90th percentile (missing only a suitable response to what some speculative, distantly future physicalist account might possibly be able to reply), I believe my argument seriously confronts and (hopefully) vigorously rattles the complacency of physicalist dogma. At the very least in some measure my argument undermines the inference to the best explanation which has long been the foundation of the physicalist enterprise. No longer should it be said with any confidence that a physical explanation is the certain, or even the most likely account for *every* phenomenon encountered in our world. Indeed, I feel

⁵Cf. Hempel, 1983, 11; Braithwaite, 1983, 46.

justified in arguing that the physicalist ITBE should be replaced by another, more complete inference-to-the-best explanation:

That the universal claim of physicalism is false, and that some phenomena experienced in the universe by humans are not physical, but non-physical instead.

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VITA

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