Explanatory Pluralism and The Heuristic Identity Theory

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Abstract

Explanatory pluralism holds that the sorts of comprehensive theoretical and ontological economies, which microreductionists and New Wave reductionists envision and which antireductionists fear, offer misleading views of both scientific practice and scientific progress. Both advocates and foes of employing reductionist strategies at the interface of psychology and neuroscience have overplayed the alleged economies that interlevel connections (including identities) justify while *overlooking* their fundamental role in promoting scientific research.

A brief review of research on visual processing provides support for the explanatory pluralist=s general model of cross-scientific relations and discloses the valuable heuristic role hypothetical identities play in cross-scientific research. That model also supplies grounds for hesitation about the correlation objection to the psycho-physical identity theory and complaints about an explanatory gap in physicalist accounts of consciousness. These takes on psycho-neural connections miss both the sorts of considerations that motivate hypothetical identities in science and their fundamental contribution to progressive research. Thus, their focus on the contributions of research at multiple levels of analysis does not bar explanatory pluralists from considering *heuristic identity theory* (HIT). Arguably, it encourages it.

I. Introduction

Explanatory pluralism offers a picture of cross-scientific relations that highlights the benefits to each of separate inquiries occurring simultaneously at multiple analytical levels in the empirical sciences. Explanatory pluralism aims to establish a middle ground between the theoretical and ontological parsimoniousness of reductionists on the one hand and the metaphysical extravagances of antireductionists on the other.

Most reductionists and antireductionists subscribe to the same models of cross-scientific relations. Reductionists aim to show how these models fit the scientific cases and result in theoretical and ontological economies. Antireductionists agree that if the cases fit, then the economies would result, but they deny that the cases fit. "New Wave" reductionists concur with the antireductionists' pessimism on this front, but argue that reductive insight does not depend upon meeting the stringent logical and material expectations of the classical model. They emphasize the many cases of reductive success where intertheoretic mapping is only approximate.

Although welcoming the improved account of reductive relations the New Wave theorists offer, explanatory pluralism proposes a richer account of *cross-scientific* relations that looks at more than just the ability of one theory to map onto another before drawing any dire or dismissive ontological conclusions. Explanatory pluralism holds that a proper interpretation of the consequences of successful intertheoretic mapping depends (at least) upon the theories' respective levels of explanation in science and their temporal relations. Still, patterns of intertheoretic relations that sometimes closely approximate classical reductions can and do arise. Because they are usually only approximate, they do not inhibit new theoretical proposals at either of the relevant analytical levels so much as they inspire them. The dominant models of cross-scientific relations, which both reductionists and antireductionists accept, overlook the pivotal role that interlevel connections play in the practice and progress of science—including the accumulation of evidence. They miss the multi-level character of so much scientific research. Section II considers these matters in greater detail.

Section III briefly reviews research on visual processing. The history of research on visual processing in the brain supports the explanatory pluralist's general model of cross-scientific relations. It also illustrates why explanatory pluralism not only need not rule out proposals about type-identities between the ontologies of prevailing theories at different analytical levels in science but why it encourages them. Hypothetical identities in cross-scientific contexts serve as useful heuristics of discovery in both of the sciences involved. They generate research that reliably leads to the development and elaboration of even more detailed hypotheses about the connections between the two explanatory levels. Finally, these hypothetical identities are justified the same way any other scientific hypothesis is.

In the final section we argue that this analysis suggests that the correlation objection and the complaint that type-identity theories in the philosophy of mind manifest an explanatory gap are both ill-conceived. The correlation objection holds that any scientific evidence proponents cite in defense of psycho-neural identities supports no more than psycho-neural correlations, so we should reject the identity theory for its metaphysical presumption. The complaint about an explanatory gap maintains that physicalist accounts of consciousness provide no explanation of *how* something psychic could just be something physical.

These views of psycho-neural connections, though, miss both the sorts of considerations that motivate hypothetical identities in science and their fundamental contribution to the development of scientific explanations. An emphasis on the multi-level character of scientific research in the sciences of the mind/brain does not bar the explanatory pluralist from embracing type-identities between mental processes and brain processes. The explanatory and predictive progress that such hypothetical identities promote is the best reason anyone can have for honoring such cross-scientific connections. Section IV shows why explanatory pluralists should embrace the Heuristic Identity Theory (HIT) in the philosophy of mind.

II. Explanatory Pluralism and Reductionism

Explanatory pluralism holds that simultaneously pursuing research at multiple analytical levels in science tends to aid progress at each of those levels. (See, for example, McCauley, 1986 and 1996, Flanagan, 1992, Bechtel and Richardson, 1993, Hardcastle, 1996, and Looren de Jong, 1997.) Connections between sciences at different levels of analysis offer scientists working at each level resources (theoretical, practical, and evidential) that would be unavailable otherwise. The opportunity to exploit such resources constitutes a clear incentive for scientists to explore such cross-scientific connections. Concerning *intertheoretic* relations in particular, explanatory pluralists distinguish these sorts of connections between contemporaneous theories reigning at different explanatory levels from connections between successive theories operating at the same explanatory level. Those unfamiliar with the history of philosophical discussions of reduction over the past forty years might be puzzled why so moderate a set of claims should provoke controversy.

On the one hand, classical reductionists (e.g., Causey, 1977) are unsympathetic. To secure the metaphysical unity of science on physicalist grounds, their analyses tend to underplay the methodological diversity of the sciences and its epistemological consequences. The classical reductionist looks for interlevel identities to serve as the bridge principles by which we map theories at higher levels of explanation (i.e., theories in the social and psychological sciences) on to the more fundamental theories that reign at lower levels in the natural sciences of biology, chemistry, and physics. Ideally, at least, well-established theoretical principles of psychology and the social sciences should follow as the consequences of the theoretical principles of the natural sciences and those bridge principles. On the basis of this relationship, the classical reductionist concludes that the theories of the special sciences are replaceableCat no more cost than the practical inconveniences of monitoring and calculating many more variables at the micro-level with far greater precision than is typically necessary for applications of the theories of the special sciences. The special sciences may bear some practical benefit, but such practical considerations do not carry any epistemological import.

On the other hand, antireductionists and advocates of disciplinary autonomy are also unsympathetic. Most (e.g., Fodor, 1975) agree with the reductionists that if theories from the special sciences map neatly on to the theories of the biological sciences and, specifically, that if psychological theories map neatly on to neuroscientific theories along the lines that the classical model specifies, then our commitments to psychological states and events are, at the very least, dispensable in principle. Not to worry, though, since these antireductionists insist that any of various considerations (such as multiple realizability or intractable complexity or the impregnable uniqueness of intentional contents or the elusiveness of subjective consciousness) suffice to block the necessary mappings the classical models of reduction require. What these sceptics about the reduction of psychology conclude depends upon the particular barrier to intertheoretic mapping that they champion. They may merely hold that psychology is autonomous, i.e., that psychological research can and should proceed completely unencumbered by any facts about the brain. More ambitious partisans have suggested that irreducible consciousness is one of the fundamental properties of the universe. (Chalmers, 1996)

On yet another hand, in order to preserve their unified model of the ontological implications of intertheoretic relations, the so-called "New Wave" reductionists are not much more sympathetic to explanatory pluralism, their recognition of the diversity of intertheoretic relations that have supported reductive accomplishments in science notwithstanding. (Bickle, 1996 and Churchland and Churchland, 1998, pp. 65-79) New Wave reductionists emphasize that cases of intertheoretic relations vary greatly as to the commensurability of the theories in question. Like the antireductionists, the Churchlands, at least, have often voiced suspicions that mappings between psychological and neuroscientific theories, in particular, are unlikely to prove sufficiently precise or systematic to sustain the sort of theoretical and ontological integration classical reductionism envisions. In contrast to antireductionists, though, New Wave reductionists underscore the wide range of cases where the inability to secure near-perfect mappings did not preclude reductive achievements. A reducing theory's ability to amplify and improve upon a reduced theory does not always depend upon our ability to trace intertheoretic identities point-by-point.

Eschewing the rigorous formal demands of the classical micoreductionists, these New Wave reductionists stress that the reducing theory need only provide an *approximation* of the reduced theory in order to generate reductive insights. The "equipotent image" that the reducing theory supplies constitutes a workable analogue of the reduced theory. (Churchland, 1979) That image is equipotent, since the reducing theory's principles will possess all of the explanatory and predictive power of the reduced theory's principles—and more. From the standpoint of traditional models, New Wave reductionists propose a form of approximate reduction, which falls well short of the classical standards, but which also makes sense of how true theories (e.g., the mechanics of relativity) can correct and even approximately reduce theories that are false (e.g., classical mechanics).

Bickle (1998) has gone to considerable lengths to provide formal means for calibrating the degrees of approximation involved. He declines, however, to provide a precise criterion for when such approximations become too bumpy to justify talk of reduction. When they do, though, it is not theory reduction but the "historical theory succession" characteristic of scientific revolutions that results. (Bickle, 1998, p. 101) The superior theory simply replaces its inferior counterpart, and if their intertheoretic mappings are negligible, as say, with Stahl's account of combustion in terms of phlogiston and Lavoisier's in terms of oxidation, we are, presumably, justified in speaking of the complete *elimination* of the inferior theory.

New Wave reductionists situate these diverse cases of intertheoretic relations on a continuum of relative intertheoretic commensurability. Cases range from uniform microreductions at one end, where intertheoretic mapping is straightforward, through approximate reductions and "bumpy reductions" to "mere historical theory succession" at the other end, where a pair of theories no longer manifests any bases for intertheoretic mapping. (Bickle, 1998, p. 101) See figure 1.

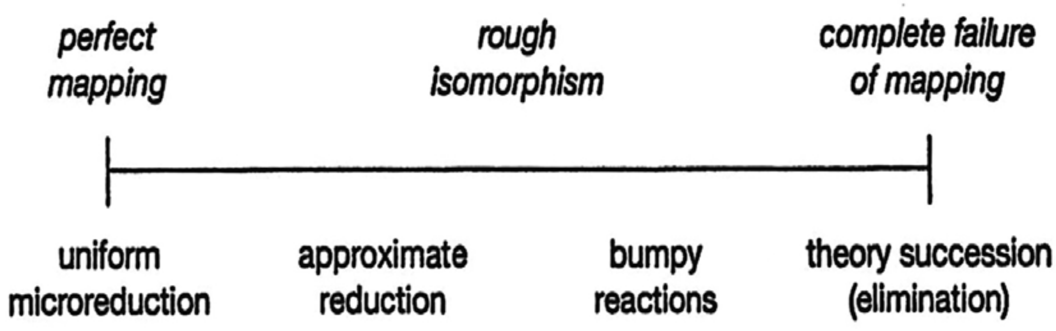

FIGURE 1. New Wave reductionists' continuum of goodness of intertheoretic mapping

The differences that separate explanatory pluralists from these New Wave reductionists do not concern grave disagreements about the model of intertheoretic reduction the latter advocates—*so far as it goes*. A continuum gauging the comparative goodness of intertheoretic mapping and the New Wavers' understanding of a range of intermediate points on that continuum provide a useful framework for sketching the constraints on the various intertheoretic connections in science that have yielded reductive achievements. Both explanatory pluralists and New Wave reductionists agree that the logical and material constraints classical models of reduction advance are too restrictive, since they would either artificially force fit all cases of reductive accomplishment into a small region near the left end of the continuum in figure 1 or, alternatively, exclude the many cases of reductive insight where theories map onto one another only partially or approximately.

For explanatory pluralists, though, the Churchlands and Bickle do not go far enough. Specifically, they have failed to free themselves from two assumptions of the logical empiricist program. First, like the logical empiricists, they hold that the only epistemologically interesting features (at times they seem to think the only interesting features, period) of cross-scientific relations are those between *theories*. Second, again like their logical empiricist predecessors, the New Wavers seek a *single model* of intertheoretic relations *with uniform implications* throughout the sciences. So blinkered a conception of science will inevitably yield conclusions that are unacceptably narrow. Each of these assumptions has one particularly unfortunate consequence.

The first assumption leads New Wave reductionists, in their formal pronouncements at least, to undervalue the *epistemological* significance not merely of retaining but of fostering multiple explanatory levels in science. If the only available accounts of science were those of philosophers, a ready conclusion might well be that *theorizing* exhausts scientific activity. When characterizing cross-scientific relations, the Churchlands focus overwhelmingly, if not exclusively, on relations between theories and their putative ontological consequences. This is so even though these philosophers (and neuroscientists) regularly appeal to the methods and findings of the psychological sciences to support the various neuroscientific models they favor. Crucially, they appeal to those methods and findings not merely for guidance—for example, about what phenomena even qualify as mnemonic (Gazzaniga, 1988)—but for *support* for their favorite neuroscientific and neurocomputational hypotheses. The New Wave reductionists, just like the classical reductionists, acknowledge the role that the special sciences play in scientific *discovery*, but their preoccupation with the alleged ontological import of theory reduction often renders them blind to the vital role endeavors in those sciences also play in the *justification* of scientific theories, including theories at the neuroscientific level. By emphasizing the methodological, evidential, and theoretical integrity of initiatives at various explanatory levels in science, explanatory pluralism looks to a much wider range of considerations in its analysis of cross-scientific relations. These include not just theoretical assets, but sources of evidence, problem solving techniques, experimental procedures, and more. (See McCauley, 1996 and 1998.)

The New Wavers' second assumption reflecting a conservatism akin to that of logical empiricism is their insistence that a single model can properly capture not only the wide variety of intertheoretic relations that exist in science but the general character of their ontological implications too. The first half of this claim amounts to holding that all theory relations fall at some point or other on the New Wavers' continuum of intertheoretic commensurability. The problem arises in the second half, i.e., with the New Wavers' further assumption that the ontological implications of falling at any particular point on this continuum are unaffected by *how and where* the pertinent theories are situated among the sciences. The explanatory pluralist holds that these issues matter particularly when considering those cases that fall on the half of that continuum that describes increasingly meager intertheoretic mappings, i.e., the right half of the continuum in figure 1. The New Wavers seem to reject the contention that contextual, pragmatic, problem solving, and (even) evidential considerations can or should bear on the interpretation of the ontological implications of the varied cases of poor intertheoretic mapping.

The cases in question are just those that have motivated the Churchlands' eliminative talk. In the same vein, as noted above, Bickle speaks of them as cases of "mere historical theory succession." The New Wavers presume that any case of serious incommensurability will inevitably and uniformly result in one theory completely superseding another, permanently removing the latter from the scientific stage. They insist on this outcome, regardless of:

(a) the amount of empirical support that each theory enjoys,

(b) the level of explanation in science that each theory occupies,

(c) the institutional health and longevity (as measured by university departments, professional societies, journals, etc.) of those sciences in which the theories arise,

(d) the relative status and position of theories within their respective sciences (for example, is either a central, well-entrenched, repeatedly corroborated theory that continues to motivate progressive programs of research or a theory that addresses but a small set of peripheral problems in what increasingly appears to be an ad hoc fashion or a mostly untested upstart, etc.?), and

(e) the amount of fruitful interaction between each theory and other theories at explanatory levels *other* than those at which the two target theories occur,

to name but a few of the considerations concerning scientific theories' situations that will affect the (im)probability that one or the other will undergo elimination exclusively on the basis of its inability to map very well on to the favored theory of the pair.

One of the most important examples, though by no means the only one, of how the New Wave reductionists' all-purpose interpretation of their continuum botches some jobs, concerns item (b) above. As we noted earlier, the New Wavers fail to distinguish between (i) the relations of successive theories within a particular science over time and (ii) the relations between theories that reign in different sciences at different levels of explanation at the same time. They ignore the distinction between intertheoretic relations in intralevel (or successional) contexts and those in interlevel (or cross-scientific) contexts.

The former sort include every stock example the New Wavers offer of elimination in science including the theories of the crystalline spheres, the bodily humours, the alchemical essences, phlogiston, caloric fluid, and the luminiferous aether. It also includes recent cases they typically do not cite, such as a unitary, static crust (in contrast to shifting geological plates) or acute gastric dysfunction (in contrast to the new bacterial theory of ulcers). (Thagard, 1998) Thorough-going failure of intertheoretic mapping between two competing theories within some science, say, for example, the failure to map the principles and dynamics of the theory of caloric fluid onto the kinetic theory of heat, results—sooner or later—in the displacement of one of the competing theories. These are paradigm cases of Kuhnian scientific revolutions. These cases concern rapid transitions from one theory to another *within a particular science* where the two competing theories are, as Kuhn put it, utterly incommensurable. (Thagard (1992) argues that Kuhn overplayed the difficulty of these transitions and underplayed the rational grounds for the emergence of the new theory.)

Of course, not all transitions to new theories in the history of science are so abrupt and discontinuous. Many intralevel cases, such as the transition from Newtonian mechanics to the mechanics of relativity, fall further to the left on the continuum in figure 1. In these cases we often retain the older theory as a heuristic of calculation in contexts where its results approximate the predictions of the newer, more sophisticated theory within an acceptable margin of error, even though, technically, that older theory is false.

By contrast, our general inability to map the currently reigning theories from *sciences at two different levels of explanation* does not result in either theoretical or ontological elimination, at least not if the sciences in which those theories are embedded hail from different scientific families (for example, the physical as opposed to the biological sciences) and if those sciences have gained a sufficient measure of institutional health and longevity. (See items (b) and (c) in the list above.) Certainly, over the last century and a half, no science institutionalized in university departments, research institutes, professional societies, and journals has disappeared because of scientists= inability to map its dominant theories on to those of sciences at more fundamental levels of analysis.

Developments at one explanatory level exert selection pressures on developments at others. Unquestionably, sometimes these influences contribute to theoretical reconfigurations at other levels. These two qualifications, however, occasion three clarifications. First, these cross-scientific selection pressures are not unidirectional (i.e., they are not exerted only from the bottom up). Second, except, perhaps, in a science's earliest stages, such interlevel selection pressures rarely, if ever, supersede those arising from theoretical conflicts *within* that science. Third, the resulting reconfigurations have never involved the overthrow of the dominant, central theories at any explanatory level (let alone the sort of fell-swoop eliminations of entire disciplines that eliminativists have sometimes seemed to envision). Rather than a motive for elimination, these kinds of interlevel discontinuities serve as a (soft) constraint on and an impetus to subsequent theorizing and research at *both* of the levels of explanation in question.

The vital point is that prognostications about the elimination of psychological theories on the basis of their failure to map neatly on to our best theories in neuroscience concern precisely cases of this sort. The Churchlands have repeatedly proposed that research on theories about the brain within the family of the biological sciences will motivate the elimination of contemporaneous theories within psychology. McCauley (1986) has argued (following suggestions by Wimsatt (1976)) along the lines sketched in the previous paragraphs that this is ill-conceived from a descriptive standpoint. The contention, in short, was that insisting that all cases of extreme theory incommensurability reliably resulted in theoretical and ontological elimination, *regardless of the context*, did not square very well either with actual scientific practice or even with some of the cases the Churchlands themselves have cited. (See McCauley, 1996.) Recently, the Churchlands have conceded the force of such criticisms (Churchland and Churchland, 1996), and behind Patricia Churchland's (e.g., 1986) long-standing discussions of the *co-evolution* of theories have always been currents that flow in directions quite different from both Churchlands' eliminativist pronouncements about cross-scientific contexts.

In the next section of this paper we shall briefly review cross-scientific connections concerning research on visual processing in cognitive neuroscience and cognitive psychology. Our aims are to show: (1) how the initial inability to connect psychological and neuroscientific theories in one domain was not treated as grounds for contemplating the elimination of the psychology but, instead, as the occasion for improving both sciences' accounts and (2) that the principal heuristic promoting this improvement is the consideration of hypothetical identities between psychological and neural processes.

Discussions of explanatory pluralism, including ours here, have rightfully spotlighted how it differs from the views of those who wish to draw eliminativist conclusions in interlevel contexts. Although explanatory pluralism aims to supersede both the classical and New Wave accounts of intertheoretic reduction in cross-scientific contexts, it does not rule reduction out. That would be unwise, since the pursuit of reductions is, surely, the single most powerful engine of discovery in cross-scientific inquiry. Unlike earlier discussions of this position (McCauley, 1986 and Looren de

Jong, 1997), in what follows we will focus on how the dynamics of a co-evolutionary pluralism of explanatory levels can not only yield reductive insights but bases for addressing standard objections to psycho-physical identities.

III. Accounts of Visual Processing and the Heuristic Value of Hypothetical Identities

The history of research over the past one hundred fifty years on the brain area(s) responsible for visual processing provides a straightforward illustration of the dynamics of cross-scientific investigation explanatory pluralism highlights. Behavioral tools psychologists have developed guide the discovery and elaboration of hypotheses about underlying neural mechanisms and those same neuroscientific hypotheses, in turn, prompt the development of increasingly sophisticated information processing models at the psychological level. Contrary to the antireductionists, though, the principal force driving these transactions is the hypothetical identifications of information processing activities with brain processes (in characteristic brain areas). In contrast, the legacy of computational research that derived from Marr's (1982) attempt to formulate the task of vision primarily in information processing terms, which was carried on largely independently of research into brain mechanisms, failed to discover some of the central ideas about visual processing that now guide both psychology and neuroscientific research.

Reflection on the psychology of visual experience inspires the very notion of a center for visual processing. Neural hypotheses about the putative location and functioning of such a center provoke increasingly sophisticated inquiries into the information processing involved in visual experience. Such hypothetical identities serve as the fundamental heuristics of discovery in these sciences. The crucial point for the philosophy of mind is that these hypotheses are justified in the same fashion that any hypothesis in science is.

In the last half of the nineteenth century controversy arose about the location in the brain where the processing of visual information occurred. The attempts to localize visual processing in one brain region accorded with the general neo-phrenological research program that viewed the brain as consisting of different regions which were engaged differentially in different mental tasks. Lesion studies, such as those by Broca (1861) and neural stimulation studies, especially by Ferrier (1876), had resulted in proposals linking a number of brain areas with specific tasks, the best known of which was Broca's identification of articulate speech with an area in the frontal cortex that came to bear his name. With respect to vision, the dominant view, supported both by mapping of neuroanatomical pathways (Meynert, 1870) and lesion studies on both animals (Munk, 1881) and humans (Henschen, 1893), was that vision occurred in the occipital lobe at the rear of the brain. But, relying on both his own stimulation studies and lesion studies, Ferrier proposed an alternative locus, the angular gyrus.

Most of the proposals for localizing functions like vision in the brain involve a combination of behavioral and neural research. Lesion studies, for example, require behavioral research to determine that it is, for example, vision that is impaired and neural research to fix the locus of damage. One of the key factors in settling the dispute was even more fine grained analysis of deficits resulting from more specific lesions. These resulted in the discovery of a topographical representation of parts of the visual field over regions of the occipital lobe. The Russians' introduction of new bullets in the Russo-Japanese War, which resulted in blindness in only parts of wounded soldiers' visual fields, made these finer localizations possible. By correlating the site of damage Inouye advanced a map projecting the visual field onto the occipital lobe, a map that was further improved as a result of similar studies during the First World War by Holmes and Lister. (Glickstein, 1988) Later Talbot and Marshall's (1941) single cell recordings in cat and monkey offered further support for the topographical representation of the visual field in the occipital lobe and confirmed its role in visual processing. (See figure 2.)

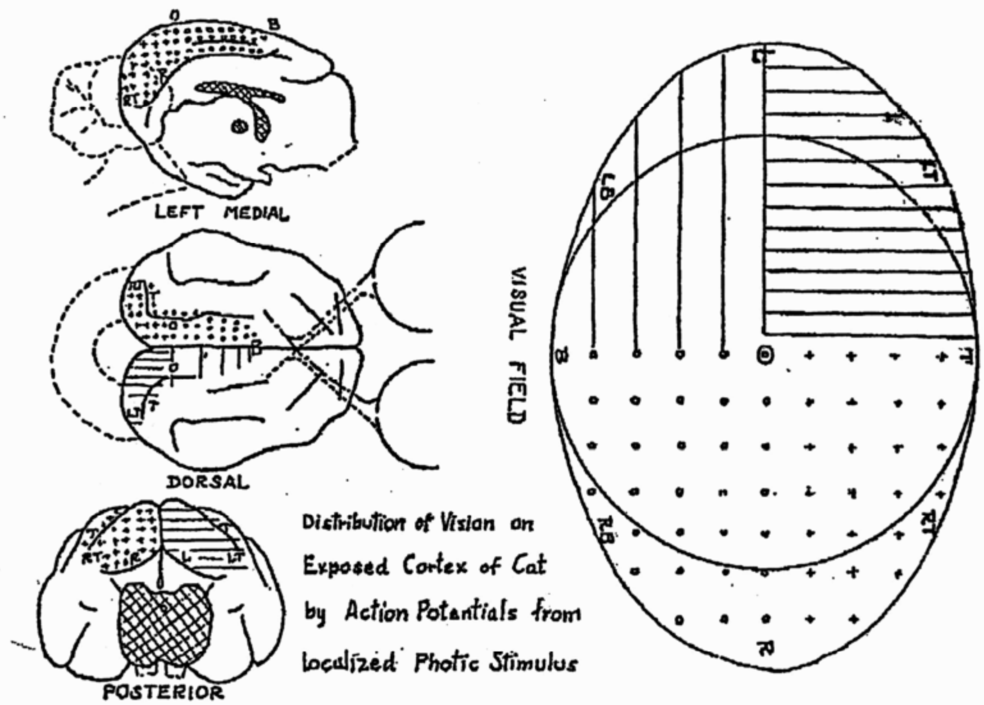

FIGURE 2. Talbot and Marshall's (1941) mapping of areas in the visual field (right) on to regions of the occipital lobe of the cat (left)

Once the locus of visual processing was identified, the challenge was to determine how this area could perform the task. Beginning with the landmark work of Hubel and Wiesel (1962) and continuing for the next three decades, the ongoing interplay of behavioral and neurophysiological research began to fill that outline in and, in the course of those endeavors, led to repeated revisions of the hypotheses about the brain areas and, critically, the conception of the information processing performed in vision. This co-evolutionary process not only preserved a plurality of explanatory perspectives but resulted in a refinement of both psychological and neural models.

Beyond just correlating stimuli presented in specific portions of the visual field with responses of individual neurons in the occipital lobe, Hubel and Wiesel investigated the specific nature of the stimulus that would drive a cell. Following the lead of their mentor, Kuffler, who found that dark or light circles would stimulate cells in the lateral geniculate nucleus (the thalamic region to which the optic nerve projects and which sends input to the visual cortical area), Hubel and Wiesel initially tried similar stimuli without success. By accident, they discovered that a bar of light would generate a response in the cell from which they were recording. They subsequently distinguished three sorts of cells all of which, in one way or another, were concerned with the orientations of bars of light and which could be construed as comprising an information processing network. (Subsequent research by Lehky and Sejnowski (1988) at a different explanatory level (viz., neurocomputational modeling) from that at which Hubel and Wiesel's work arose suggests that these response patterns are feature detectors for deriving shape from shading.)

By beginning to reveal the nature of the processing in occipital lobe cells, Hubel and Wiesel's work also revealed that the region originally identified as the locus of visual processing only performed a small part of the task. They ended their 1968 paper (p. 242) by noting that fact. Thus began the search for other visual processing areas and the designation of the area on which Hubel and Wiesel had done their original work as *primary* visual cortex or V1. Already in 1964 Cowey had identified a second area with a topographical map of the visual field (which came to be known as V2) and in 1965 Hubel and Wiesel discovered a third map (in what came to be known as V3). Tracing the degeneration of fibers resulting from lesions in these regions, Zeki (1969) discovered systematic projections to two other areas, V4 and V5 (or MT) even though the topographical mapping in these areas was less precisely defined. Following Hubel and Wiesel's lead, Zeki (1973 and 1974) employed single cell recording to ascertain what stimuli would induce cells in these areas to fire and determined that cells in V4 responded to wave length whereas the cells in MT responded to specific directions of motion.

The discovery of different brain areas that responded to different features of a stimulus began a process of decomposing the task of vision into distinct subtasks. Over the subsequent two decades, at least thirty-three different brain areas involved specifically in processing visual information have been identified (van Essen and Gallant, 1994) as have the distinctive information processing contributions of many of them. Here we can mention just two central examples (for more details, see Bechtel, in press). Gross et al. (1972) showed that cells in areas TE and TEO in inferotemporal cortex responded differentially to particular shapes. (Like Hubel and Wiesel, they made this discovery by accident, when one of the experimenters simply waved a hand in front of the monkey=s stimulus display.) Goldberg and Robinson (1980) discovered, by contrast, that cells in posterior parietal cortex were sensitive to the locations of objects in the visual field.

In 1982 Ungerleider and Mishkin (1982) advanced an hypothesis that integrated the burgeoning number of studies that identified different visual processing areas in occipital, temporal, and parietal lobes. Their proposal has provided the basis for much subsequent thinking about vision in both neuroscience and psychology. They hypothesized two separate streams in visual processing after V1 (see figure 3). In the dorsal stream that proceeds through V2 and MT into the posterior parietal cortex, cells are sensitive to objects' locations and motions in the visual field. Cells in this stream seem to be concerned with *where* objects are. By contrast, in the ventral stream that proceeds through V4 and on to TEO and TE in the inferotemporal region, cells seem to be concerned with identifying *what* objects we see. Although the precise functional characterization of Mishkin and

Ungerleider's two streams has been the subject of much disagreement (see Milner and Goodale, 1995), and the independence of the streams has also been challenged, the general framework of conceptualizing vision as involving at least two streams of information processing has been generally adopted in both neuroscience and psychology.

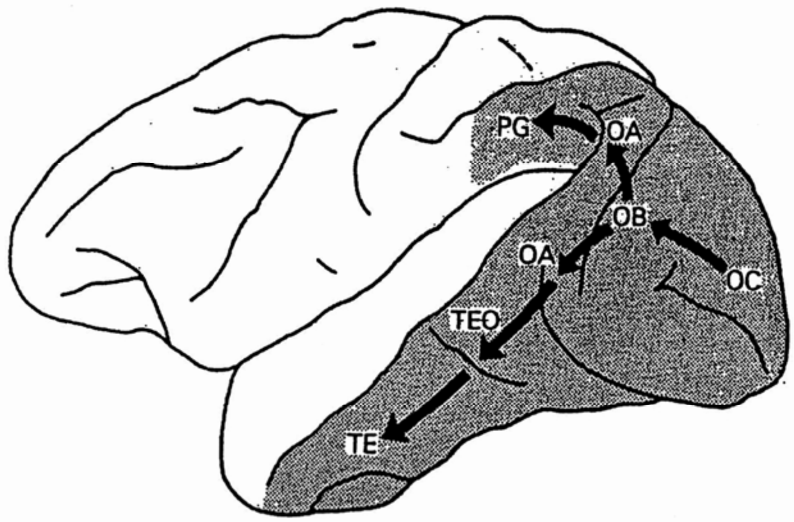

FIGURE 3. Ungerlieder and Mishkin's proposal for two cortical visual pathways (from Mishkin, Ungerlieder, and Macko, 1983)

Even this brief sketch of this research shows two things. First, in a relatively brief period, research integrating behavioral and physiological techniques had entirely defeated the hypothesized identification of the visual center with V1 exclusively. However, rather than undermining the strategy of hypothesizing identities, Hubel and Wiesel's determining the functions of cells in V1 led to more hypothetical identities that were even more precise about the visual and the neural processes identified and about the additional brain areas involved. Neural accounts of "the visual center" in the second half of the twentieth century, in contrast to those in the second half of the nineteenth century, included a complex array of brain areas and interconnections—with considerable functional localization of a much finer grain.

For over a century now, exploring the empirical merits of the hypothesized identity of the visual center and the occipital lobe has generated both more and more detailed hypotheses about the activities in the brain with which various aspects of visual processing and experience should be identified. That comment leads naturally enough to the second point, viz., that these findings about the various brain areas involved in visual processing played a key role in inspiring new, ambitious, higher-level hypotheses at the neuropsychological and psychological levels about visual processing and the organization of the human cognitive system overall.

Ungerleider and Mishkin's proposal and the neuropsychological evidence they offer in its defense are just two of the considerations that Neisser (1989 and 1994) cites in presenting his theory of human perception and cognition. He maintains that the mind is best understood as a partially integrated collection of multiple information processing systems, which often seem task specific. In addition to the neuropsychological findings discussed above, Neisser also cites an array of developmental, cognitive, and clinical evidence in support of his theory. For example, many of these systems exhibit different developmental schedules. Tests of human performance provide ways of dissociating various systems from one another that might otherwise have appeared indistinguishable (see, e.g., Chen et al., 2000) and deficits in human performance—as the result of injury or disease—are typically quite selective among these systems. So, for example, prosopagnosics are unable to remember human faces, though one farmer could recognize the faces of his sheep (McNeil and Warrington, 1993), and prosopagnosics actually do better at recognizing human faces upside down than normals do, presumably because those stimuli do not satisfy the input conditions of the face recognition system (Farah et al., 1995).

More specifically, Neisser identifies the activities in Ungerleider and Mishkin's dorsal "where" system with Gibson's proposal that we see the layout of the environment and the motions of objects in it directly. On Neisser's account their ventral "what" system is, by contrast, concerned with aspects of visual perception that are far more clearly inferential, cognitively penetrable, and concerned with recognizing particular things in the world.

Neuroscientists' proposed identifications of features of visual processing with neural processes in specific brain areas fostered Neisser's formulation of both a new approach to cognitive theory and a new account of visual perception that incorporates programs of research in perceptual psychology that had previously seemed to conflict. It has also inspired research directed at why a decomposition into what and where processing is computationally efficient (Jacobs, Jordan, and Barto, 1991). This case nicely illustrates how the characteristically reductive strategy of hypothesizing psycho-neural identities encouraged productive developments in psychology. It inspired theorizing in cognitive psychology (i.e., at a higher level) that was both more ambitious and more integrative than heretofore. It provided Neisser's theory with a variety of evidential resources (including, among other things, much of the neuroscientific research we summarized above). These hypothetical identities also offer useful guidance for the elaboration and development of Neisser's general theory and for future psychological research on the basis of available knowledge about the neural regions in question and their connections with other parts of the brain.

This is not a story of the neuroscience thoroughly swamping the psychology, though. Not only does Neisser integrate and reconceive a wide array of evidence from areas of research in psychology (e.g., Gibson's (1979) ecological approach to perception) that had proceeded completely independently of potentially related research in the neurosciences, the resulting theory Neisser advances does not square with the more neurally oriented accounts on every front. For example, Neisser (1994) ultimately proposes a third stream of information concerned with "interpersonal perception/reactivity," and he has proposed that the direct perception-where system is a newer system, from a phylogenetic standpoint, than the recognition-what system. By contrast, Livingstone and Hubel (1988), proposing connections between, on the one hand, the parvocellular and magnocellular pathways from the retina to V1 and, on the other, Ungerleider and Mishkin's two processing streams, argue for just the opposite chronology—at least with respect to *cortical* systems.

If the research about the visual system we have briefly summarized is representative, then there are five likely consequences from ironing such differences out. First, such conflicting claims generate further empirical research to ascertain either which proposal should prevail or in what directions each needs to evolve. Second, that research regularly yields even more precise hypotheses about the systems and patterns engaged. Third, this process typically provokes new speculations at each explanatory level. Fourth, within their respective levels, such speculations suggest new ways of organizing familiar facts and theories and point to new avenues of empirical investigation. Finally, some of the new arrangements that result, almost inevitably, will produce some new cross-scientific conflicts, which likely begin this cycle anew.

Hypothesizing cross-scientific identities is a pivotal engine of scientific development. Hypothetical identities in interlevel contexts serve as valuable heuristics of discovery for inquiry at both of the explanatory levels involved. Crucially, scientists accept or reject these hypotheses for the same reasons that they accept or reject any other hypothesis in science. These are the same reasons involved in establishing the truth of any abductive inference, viz., the resulting hypotheses' abilities to stand up to empirical evidence, to stimulate new research, and to foster the integration of existing knowledge.

It is worth noting straightaway that the decisive considerations do not concern the direct confirmation of the proposed identities. What, after all, could that possibly be? (McCauley, 1981) In empirical matters the evidence for an identity claim arises *indirectly*—primarily on the basis of the emerging empirical support for the explanatory hypotheses it informs. So, for example, if normal activities in V4 are identical with the processing of information about wave length, then serious abnormalities of particular types in the structure and functioning of V4 should yield abnormalities of particular types in subjects' color experiences. The point is that this is an empirical hypothesis that we can use both psychological and neural evidence not only to assess but to refine. Obtaining indirect corroborating evidence along such lines for identifying some neural process with some psychological function no more settles that hypothetical identity than it would any other hypothesis. Nor does it establish that the function under scrutiny is either the sole or even the primary function these neural processes carry out. (So, in fact, whether V4 is even primarily concerned with the processing of color is a point of some controversy among researchers.) Moreover, all research of this sort is limited by scientists' abilities both to conceive of what stimuli may happen to provoke responses in a neural area and to test those conceptions. Still, the more hypotheses of this sort the identity informs and the more successful those hypotheses prove, the more likely the identity will come to serve as an assumption the sciences lean upon rather than a bare conjecture in search of support.

Such identity claims are, of course, no less conjectures still. They are simply no longer bare ones. Such caution may displease philosophers and scientists who prefer their metaphysics definitive and settled once and for all, but, in fact, it is the best that we can ever hope to achieve with matters so thoroughly empirical.

A final note before we press these philosophical points a bit further in the final section: contrary to reductionists and antireductionists alike, these hypothetical identities do not lock upper level psychological theorizing into rigid conformity with the underlying neuroscience. Achieving reductive insight does have influences that propagate horizontally, i.e., within the pertinent explanatory levels in science, but the fact that we identify aspects of visual processing with various neural activity in the occipital cortex and some other immediately contiguous areas sets remarkably few constraints on psychological inquiry into related matters—say, for example, the relations between visual and haptic information in the activity of typing at a computer keyboard. These hypothetical identities mostly serves as a force for the elaboration and improvement of theories at the pertinent levels.

IV. Heuristic Identity Theory

Explanatory pluralism in the philosophy of science does not discourage the pursuit of reductive insights in cross-scientific research. It encourages it. What sets explanatory pluralists apart from both the standard fans and foes of intertheoretic reduction is their contention that even establishing interlevel *identities* carries neither the dire ontological consequences nor the draconian theoretical constraints that the foes fear and the fans cheer.

Much of the antireductionists' trepidation and much of the reductionists' enthusiasm on these fronts have turned on their relative inattention to what might be called issues of theoretical "grain." (Bechtel and Mundale, 1999) Typically, both sorts of philosophers have stressed our inability to map comparatively coarse-grained psychological statesCconsider, for example, Putnam's (1967) discussion of hunger in octopi and humans—on to what are far more fine-grained accounts of brain areas and processes. Finding units of analysis at the psychological and neural levels that are of comparable grain—whether coarse or fine—will provide a fairer assessment of achievable reductive insights. Which grain is more appropriate depends upon the problems under consideration. Bechtel and Mundale (1999) argue, for example, that scientists adopt fine-grained analyses when assessing individual differences or differences in the same organism over time but coarse-grained analyses when pondering evolutionary questions.

What neuroscientists are learning about the details of brain mechanisms and their functioning provides good reasons for psychologists to pursue new lines of experimentation in search of evidence corroborating (or contradicting) the neuroscientists' distinctions. Successes have and will occasion the reconsideration of our psychological taxonomies concerning both content and consciousness, as Neisser's proposal about multiple systems of visual perception illustrates.

Explanatory pluralists' campaign for the value of hypothetical identities in cross-scientific settings should look less surprising in the light of

(a) their interests in commensurate theoretical grains and units of analysis that square with the categories of our best theories at each explanatory level and

(b) their interests not only in those same theories= co-evolution but in an even broader range of cross-scientific relations.

Explanatory pluralism holds that cross-scientific hypothetical identities are perfectly common means for abetting the study of some phenomenon at multiple levels of explanation. They enable scientists working at one analytical level to exploit the conceptual, theoretical, methodological, and evidential resources available at another. The principal motives that drive the initial formulation of such hypotheses, whatever their eventual ontological consequences, concern their capacities to advance empirical research. To repeat, hypothetical type-identities (of comparable grain) are heuristics of discovery that inspire multi-level programs of research. Applying these morals of explanatory pluralism to the interface of psychology and neuroscience results in what we have called the Heuristic Identity Theory (HIT). (Bechtel and McCauley, 1999)

On HIT psycho-neural identities are not the conclusions of scientific research but the premises. The logic behind their use looks to the converse of Leibniz's law. Instead of appealing to the identity of indiscernables, this strategy exploits the indiscernability of identicals. What we learn about an entity or process under one description should apply to it under its other descriptions. Scientists do not advocate hypothetical identities because the two characterizations *currently* mirror one another perfectly. On the contrary, they advance them precisely because they do not! The theories at each level ascribe distinct properties to the entities and processes the interlevel, hypothetical identities connect. Since they both address features of the same physical systems, though, scientists have grounds from the outset to expect that these accounts will gradually evolve so as to mirror one another more and more. By virtue of the proposed identities, scientists can use related research at each explanatory level to stimulate discovery at the other. HIT shows why hypothetical identities of psychological and neural processes not only generate new hypotheses but new avenues of research that serve to direct those hypotheses' development and elaboration. The differences between theories at these two levels encourage scientists to consider adjustments to their conceptions of the pertinent processes and structures in a reciprocal process of mutual fine-tuning.

If, as we suspect, philosophy of science is (often) philosophy of mind enough, then HIT also suggests new replies to a pair of closely related objections to the psycho-physical identity theory. The first of these, the correlation objection, holds that " . . . the factual content of the identity statement is exhausted by the corresponding correlation statement. . . . There is no conceivable observation that would confirm or refute the identity but not the associated correlation." (Kim, 1966, p. 227) This amounts to claiming that from the standpoint of the logic of confirmation claims about the identity of two things are indistinguishable from claims about their correlation.

If the whole philosophical story about proposing psycho-physical identities was one about their confirmation, then this perfectly uncontroversial logical point would carry the day. HIT, however, maintains that cross-scientific hypothetical identities between psychological and neural processes are part of a multi-level scientific investigation of human beings that involves much more than the connections between theories' ontologies.

Claims about correlations and claims about interlevel identities are different conceptual animals that thrive in different theoretical habitats. Not only does the correlation objection overlook the fundamental contribution hypothetical identities make to scientific *discovery,* it does not even get the role of these identities right in the *justification* of scientific theories. Unlike merely noting correlations, advancing hypothetical identities occasions explanatory connections that demand empirical exploration. Cross-scientific identities make evidence available from other explanatory levels, and, as we noted above, they disclose avenues of research for generating new evidence as well. Their critical contribution resides in their abilities to provoke and refine theories at both of the levels engaged. *Their success at this task is their vindication*Cnot the accumulation of some sort of direct evidence that would rule the corresponding correlation claim out of court. The whole point of the correlation objection is precisely that such evidence cannot exist!

These same considerations vanquish the recent complaints (e.g., Levine, 1983) about a putative explanatory gap that materialist theories are incapable of filling in. The complaint, briefly, is that physicalist theories of mind and the identity theory, in particular, provide no account of *how* psychological phenomena can just be neural phenomena. Chalmers (1996, p. 115) combines this with a version of the correlation objection in his attack on neurobiological accounts of consciousness:

> Neurobiological approaches to consciousness . . . can . . . tell us something about the brain processes that are *correlated* with consciousness. But none of these accounts explains the correlation: we are not told why brain processes should give rise to experience at all. From the point of view of neuroscience, the correlation is simply a brute fact.
>
> . . . Because these theories gain their purchase by *assuming* a link . . . it is clear that they do nothing to explain that link.

Whether the neuroscientists he has in mind here construe their proposals as Chalmers portrays is not the issue. Our aim is to show that

(1) by presuming the success of the correlation objection, i.e., by presuming that neuroscientific theories' claims about consciousness must be construed in terms of correlations rather than hypothetical identities, this analysis sets up a less tenable account of such positions than the one HIT provides and, as a consequence,

(2) this passage's anti-physicalist arguments are not telling.

We shall discuss each point in turn.

The passage saddles neuroscientific accounts of consciousness with commitments to correlations between neurobiological and psychological processes and then faults them for failing to explain those correlations. Since claims about correlations are logically weaker claims than claims about identities, this interpretation of neuroscientific accounts of consciousness *seems* charitable. Because correlations cry out for explanations, whereas identities, by contrast, provide them, the charity is apparent only.

Interpreting neuroscientific accounts of consciousness in terms of correlations has two particularly unfortunate consequences. First, it commits a sin of commission. Instead of exploring how such accounts have enriched our understanding of conscious processes, it accuses them of spawning an additional explanatory problem. (That problem of explaining why brain processes correlate with conscious processes, i.e., of explaining why they "give rise to experience," is the putative explanatory gap.) But, of course, no such gap arises, if these accounts involve hypothetically *identifying* neural and psychological processes. Not to put too fine an edge on it, they give rise to the experience, because they are the experience.

Chalmers' interpretation also commits at least three sins of omission. By construing the psycho-neural connections in these accounts in terms of correlations, his interpretation neglects hypothetical identities' abilities

(i) to provide the explanations he requests of the acknowledged correlations,

(ii) to suggest new explanatory hypotheses and connections (as outlined above), and

(iii) to supply theories at both of the levels involved conduits to sources of evidence that are otherwise unavailable.

Recognizing these omissions is critical for understanding why the passage's anti-physicalist arguments miss HIT.

The conclusion of the second argument that neuroscientific accounts of consciousness "do nothing to explain the link" between neural and psychological processes is either irrelevant or worseCdepending upon how strictly or how loosely we take the notion of explanation. If it presumes some strict notion of *scientific* explanation in terms of causal regularities, then although the conclusion follows, it is irrelevant to neuroscientific theories of consciousness construed in terms of HIT. It is irrelevant, because, as we argued above, cross-scientific hypothetical identities (in contrast to correlations) do not require scientific explanations of this sort, rather they occasion them. If, on the other hand, the conclusion presumes a (loose) conception of explanation that suggests that these neuroscientific accounts of consciousness supply no grounds whatsoever for adopting the hypothetical identities they propose, then not only does the conclusion not follow, it is false.

That point shows the problem with the first argument when we substitute "cross-scientific, hypothetical identity" and its cognates for each appearance of "correlation" and its cognates. Again, consider the two ways of construing the notion of explanation. On the one hand, if we adopt some strict account of scientific explanation, the second premise of the argument ("none of these accounts explains the correlation") is true; the conclusion follows, and it is true too. However, the conclusion is innocuous. If the link is a hypothetical identity (rather than a correlation), then concluding that it is a brute fact is unproblematic. Recall, as the original version of the correlation objection stressed, that no additional or special evidence will ever demonstrate that some relation is an interlevel identity as opposed to a correlation. Note that although it may be true that “from the point of view of neuroscience” *alone* the link is “simply a brute fact,” the whole point of the explanatory pluralism informing HIT is that even when we achieve rich reductive insights, those insights never arise from the point of view of a single explanatory level. (Chalmers, we should note, also emphasizes the role psychology plays even in the statement of such cross-scientific proposals.)

On the other hand, if we adopt the considerably looser conception of ‘explanation’ described above, then the second premise is false and so is the conclusion. This is just another way of saying that once we take the relations as hypothetical identities, this allegedly brute fact is not really so brutish after all. What matters about hypothetical cross-scientific identities is not how they should be explained (they can’t be) but what they explain, how they suggest (and contribute to) other, empirically successful, explanatory hypotheses, and how they create opportunities for scientists at one explanatory level to enlist methods and evidence from alternative levels of explanation. *That* is why “we are not told why brain processes should give rise to experience . . .” Scientists show why some mechanism *constitutes* some phenomenon by exploring the empirical success of the wide range of predictions and explanatory connections that assumption generates. It is that empirical success that corroborates the constitutive hypothesis and tentatively justifies its assumption. (Churchland and Churchland, 1998, pp. 120-122) But, of course, the tentativeness here is nothing special. It is the same tentativeness about justification that accompanies every empirical claim in science.

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