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MIND MATTERS: THE ROOTS OF REDUCTIONISM

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1. Introduction

How are the mental and the physical related? The question concerning the *commercium mentis et corporis* has troubled scientists and philosophers for ages. Descartes's solution in terms of a dualism of substances, interacting at the *conarion*, is now considered a relic of a very distant past. Science and philosophy have turned materialist: all that exists, exists in space and time and must be considered fundamentally physical.

Though one may be convinced that we inhabit a universe that is materially constituted, the question remains whether such an ontological physicalism at the same time commits one to reductionism: Are minds nothing but brains? Will, when all is said and done, psychology really be nothing more than a chapter in neuroscience? Oppenheim and Putnam claimed as much in 1958 when they suggested that "[i]t is not absurd to suppose that psychological laws may eventually be explained in terms of the behavior of individual neurons in the brain" (p. 7). Still, Putnam himself was to a considerable extent responsible for the firm "antireductionistic consensus" that emerged in the philosophy of science and the philosophy of mind. According to the ruling orthodoxy, mainly due to Putnam and Fodor's "multiple realizability" argument (Putnam, 1960; Fodor, 1975), reductionism cannot possibly be true. The fact that mental functions can be instantiated in a wide variety of underlying physical substrates precludes them from being reductively mapped onto neurophysiological processes. It is in this antireductionistic climate that "reductionism" became a term often used with pejorative intent: "a general term of insult and abuse" (Churchland, 1986, p. 278), "a dirty word" (Dawkins, 1982, p. 113), a term that refers to something "philistine and heartless, if not downright evil" (Dennett, 1995, p. 80). Most philosophers of mind have opted for nonreductive forms of physicalism: mental properties are not identical to physical properties and psychology will continue to enjoy autonomy relative to the neurosciences. Anyone who claims otherwise must be considered "an imperialist in the service of physics" (Brooks, 1994, p. 803).

These days however, it surely looks like the pendulum is swinging back to reductionism again. The writ of reductionism has been spreading across the sciences, and its effects on our views of the world are pervasive. The ever-increasing momentum with which in modern neuroscience and molecular biology discoveries are made and theories are formulated reinvigorates reductionist claims with respect to the traditional territory of psychology. Cracks have begun to appear in the apparently solid phalanx of support for the autonomous status of psychology. The availability of intimate correlations between psychological phenomena and neurophysiological activity has sparked a renaissance of reductionism.

In this introductory chapter, we will take reduction to be about the relations between levels: between levels of description and explanation, or between levels of reality. Higher-level explanations seem threatened in two ways, in a Catch-22-like fashion: damned when fitting in a physical world, and damned if they don't. On the one hand, when higher-level posits cannot be related to the real furniture of the world, as captured in the laws of macrophysics, they can't be real things or processes in a causally closed world, can't really explain anything. On the other hand, if a higher-level explanation can be related to physical processes, it becomes redundant, since the explanatory work can then be done by physics. So, in exploring reduction the crucial point is whether and how higher levels (biology, psychology, and their objects) connect to more basic levels of reality and explanation.

The crucial questions are whether the entities at higher levels (such as societies, minds, and adaptive functions) have a reality unto themselves and whether the theories or domains of enquiry (such as sociology, psychology, and biology) that try to describe and explain them exhibit conceptual integrity or provide genuine explanations. Presumably, the lower level (roughly macrophysics) is usually considered unproblematic: hardly anyone ever seems to question the conceptual integrity, explanatory power, and reality of physics. Intentional explanations in psychology and functional explanations in biology are under constant threat of being replaced by lower-level explanations.

Although we may feel uncomfortable with such conclusions, this may just be the road that lies ahead. As E. O. Wilson states: "reductionism is the primary and essential activity of science" (Wilson, 1998, p. 54). Klein & Lachièze-Rey agree when they say that "[s]cience is reductionistic by essence" (1999, p. 129). Reduction is essential in science because – as is often claimed – nature (and our views of nature) must be unified. One reason that has always motivated reductionist projects is the appeal to Occam's Razor or *ontological simplicity*. The "nothing-buttery" locution is the reductionist's battle cry. Accomplished reductions leave us with fewer entities in our catalog of the universe. In case one would be able to show that mental events are neural events, one would have a more parsimonious ontology. Another important motivation for reductionism is *explanatory parsimony*: a reduction leaves one with theories that are more comprehensive and more predictively and explanatorily powerful than the ones had before. As Otto Neurath, in the introductory essay to the *International Encyclopedia of Unified Science*, stated: "All-embracing vision and thought is an old desire of humanity" (cited in Suppes, 1981, p. 3). The ideal of a *Einheitswissenschaft* was not only central to the Vienna Circle positivists; the desire to integrate disparate pieces of knowledge can be found in, to name but a few, Francis Bacon, Descartes, Kant, and Leibniz. Leibniz asserted that "The entire body of the sciences may be regarded as an ocean, continuous everywhere and without a break or division, though men conceive parts in it and give their names according to their convenience." Similar ideas can be found in Kant, who wrote that "our diverse modes of knowledge must not be permitted to be a mere rhapsody, but must form a system" and "Every science is a system in its own right; . . . we must . . . set to work architectonically with it as a self-subsisting whole, and not as a wing or section of another building – although we may subsequently make a passage to or from one part to another" (citations in McRae, 1957,

p. 1). However, this regulative idea of integrating science was codified into a rigid, formalized prescription for unification through reduction by Ernest Nagel (1961). This classical view of reduction is more like seizing a neighbor's property and rebuilding it, than like making passages between domains of knowledge.

2. Classical Reductionism and the Problem of Connectability

2.1 Classical Reduction

"[T]he phenomenon of a relatively autonomous theory becoming absorbed by, or reduced to, some other more inclusive theory is an undeniable and recurrent feature of the history of modern science," writes Nagel in his *locus classicus*, *The Structure of Science* (Nagel, 1961, pp. 336–337). What is required to effect such a reduction? On Nagel's account, reduction involves a relation between two scientific theories, a secondary or target theory T_R and a primary or successor theory T_B. In essence, according to Nagel, the satisfaction of two conditions forms the key to a successful reduction of one theory to another. The first is the "condition of derivability" [DC]: reduction is essentially a matter of the logical derivation of T_R from T_B, for instance, the derivation of thermodynamics (specifically, the Boyle–Charles law) from statistical mechanics (plus the kinetic theory). The second condition that must be fulfilled – what Nagel called the "condition of connectability" (Nagel, 1961, p. 354; [CC]) – becomes especially clear when one considers *heterogeneous* theory connections, i.e., cases in which the proprietary vocabularies of the primary and secondary theory show no (full) overlap. For a deductive argument to be valid it is required that T_B is supplemented by statements that connect the terms that occur in its laws and postulates and those terms which are peculiar to T_R. So, for instance, "temperature" does not occur in statistical mechanics and should be correlated with "mean molecular energy," one of T_B's proprietary terms.

In case both [CC] and [DC] are satisfied, the old, secondary theory (or something similar to it) is incorporated by the new (primary) one and comes out as a special case of the new theory under limited conditions. For example, Kepler's laws are reduced by subsuming them under Newton's laws of motion and gravitation; and so the former set of laws is a special case of the latter set.

2.2 Strong Connectability

On Nagel's canonical view of reduction, it is demanded that the vocabularies of T_R and T_B be correlated via bridge laws, but what is the status of these bridging principles? For Nagel, the bridging principles were universally quantified biconditionals or even one-way conditionals (Nagel, 1961, p. 355n). However, numerous commentators have pointed out that this is surely too weak (Causey, 1972; Ença, 1976). As Hooker explains: "Nagel's conditions . . . are too weak to ensure the dispensability of either the reduced theory's conceptual apparatus or its ontology" (Hooker, 1981, p. 39). We need something stronger than mere correlations, because – although T_R would be derivable from T_B – one would be faced with the further task of explaining the correlation laws. An additional (bridge) theory is needed that explains the correlations between T_R and T_B. This implies that with the fulfillment of [CC], [DC] can indeed be satisfied quite easily, however not in the way Nagel envisaged. T_R is derivable, not from T_B alone, but from a conjunction of T_B and a set of correlatory statements and, concludes Sklar, "this reduction is not the reduction of [T_R] to [T_B] originally sought for" (1967, p. 119).

Nagelian reductions fail to make good on the promise to shrink ontologies and vocabularies. Establishing correlations between theories T_B and T_R are sufficient for derivation of T_R from T_B, but it is a mistake to claim – as Nagel did – that mere correlations will result in a reduction of T_R to T_B. Neither *explanatory* nor *ontological economy*, the principal motivating aims behind reductionism, will have increased. In the words of Kim:

> By adding the bridge laws to the reductive resources as auxiliary premises, *Nagel reduction essentially extends the reduction base*. If we take reduction to be an explanatory process which yields an explanation of the laws and phenomena being reduced on the basis of the laws of the base theory, Nagel reduction fails to generate such explanations. For, to do so, the reductive derivation must derive the laws being reduced *solely from the explanatory resources available in the base domain*." (Kim, 2005, pp. 99–100; original emphasis)

Moreover, Nagelian reduction fails to deliver *ontological parsimony* as well. As a number of commentators have insisted, mere correlations can never succeed in ontological economizing since the ontologies of T_R and T_B remain distinct: to say that mental states and neural processes are correlated is "to say that they are something 'over and above.' You cannot correlate something with itself" (Smart, 1959, p. 142).

In light of these considerations, it looks as if Nagel's model is in need of modification, and many have suggested that a good place to start is [CC]. Whereas Nagel was at pains to avoid ontological commitments, it is debatable whether his neutral stance on matters ontological can be sustained. On close inspection, it looks like additional restrictions need to be imposed upon the postulated bridge laws to get the reductionist's project off the ground. In particular, Sklar (1967) argued that the only way to get rid of the correlatory statements that connect *two* classes of entities – which are themselves in need of further explanation – is to demand that T_R and T_B are (strongly) connected through empirically established *identity* statements (Schaffner, 1967, p. 144; Sklar, 1967, p. 120). Without identities strongly connecting T_R and T_B, "the underlying ontological bias of the reductionist program" would not be satisfied as mere correlations are "compatible with a nonphysicalist ontology" (Fodor 1974, p. 129). Kims dubs this identity requirement the "condition of strong connectability" (Kim, 1993, p. 151). We will refer to this condition as [sCC].

2.3 The Failure of Connectability I: Multiple Realization

What are the prospects of psychoneural reductionism? Only on a strong reading of bridge laws ([sCC]) can the classical reductionist's program be rendered truly successful in the psychoneural case. Hence, early psychoneural reductionists defended a Psychoneural Identity Theory (Feigl, 1958; Place, 1956; Smart, 1959). They claimed that mental states and events can be empirically identified with neural states and events just as lightning can be identified with electric discharges. In short, mental kinds are nothing but neural kinds. However, here the Psychoneural Identity Theory immediately faces the *correlation objection* (cf. McCauley and Bechtel, 2001). Brandt and Kim (1967) formulated this objection against the logic of the identity theory thus: "since the identity statements have no more empirically verifiable content than their associated correlations, the theory with identities will fare as well as the theory with correlation laws, in confrontation with observational fact" (Brandt & Kim, 1967, p. 530). Briefly, finding mind-brain covariances is not enough to support mind–brain identities.

Besides this objection, there were other influential lines of argumentation directed against the view that there are strong (identity) connections between the mental and the physical, e.g., Davidson's *anomalous monism*. Davidson argued that unlike physical events, mental events are not governed by strict laws; hence, there are no nomological connections between the mental and the physical. Another important pressure source is the well-worn multiple realizability argument, first formulated by Putnam and later generalized by Fodor. Putnam claimed that mental states can be and typically are implemented by many, wildly diverse physical states. This makes the implementation level explanatorily uninteresting. In Putnam's happy phrase, "We could be made of Swiss cheese and it wouldn't matter" (Putnam, 1975, p. 291). Moreover, the fact that mental functions can be instantiated in a wide variety of material substrates precludes them from being reductively mapped onto, say, neurophysiological processes. Again, for the reductionist program in psychology to succeed, psychological kind predicates should be lawfully coextensive with neural kind predicates, but they are not. Hence, given multiple realizability, psychoneural reductionism must be ruled out: "what corresponds to the kind predicates of a reduced science may be a heterogeneous and unsystematic disjunction of predicates in the reducing science" (Fodor, 1981). Hence, Fodor's conclusion is that multiple realizability "refutes psychophysical reductionism once and for all" (Fodor, 1998, p. 9).

2.4 The Failure of Connectability II: Approximation, Correction, Radical Falsity

The very soundness of the classical model of reduction has been disputed in other quarters of philosophy as well. One objection often raised against Nagel's treatment of theory reduction is that it fares badly in terms of historical accuracy (Caplan, 1981). There are few – if any – cases of intertheoretic relations that qualify as reductions on this view. The most quoted example is the reduction of classical thermodynamics to statistical mechanics supplemented by the kinetic theory of matter. The classical model apparently not only fails as an account of the psychology-neuroscience case; nor can it explain the relation between classical genetics and molecular genetics (Hull, 1974). Can the classical model be remedied so as to provide a better fit with scientific history? A number of authors have thought so.

Let us start with what is commonly seen in the philosophy of science literature as the main failure of classical reductionism, the fact that it disregards incompatibilities or discrepancies between T_R and T_B and that it fails to account for the possibility of correction of the theory targeted for reduction (see Kemeny & Oppenheim, 1956; Popper, 1957; Sellars, 1965). Popper claimed that "from a logical point of view, Newton's theory, strictly speaking, contradicts both Galileo's and Kepler's" (Popper, 1957, pp. 29–30). For instance, for Galileo – contradicting Aristotelian physics – a stone that is thrown follows a parabolic trajectory, whereas for Newton – contradicting Galileo – the path of the stone will be elliptic. The trajectory becomes *approximately* a parabola only at relatively small distances. Similar things can be said about the alleged reduction of Kepler's laws to Newton's mechanics so that "Kepler's laws are only approximately valid" (Popper, 1957, p. 32). Sklar (1967) is clear on the implication: "even in the case of homogeneous theories reduction is very rarely derivation" (p. 2). Although in general sympathetic to Nagel's project, Hempel diagnosed the received (Nagelian) view of reductionism to be an "untenable oversimplification which has no strict application in science and which, moreover, conceals some highly important aspects of the relationship to be analyzed" (Hempel, 1969, p. 197). The conclusion seems warranted that Nagelian reduction is just an empty formalism, an idealization at best (in fact, this is consistent with Nagel's characterization of reduction as laid out in his 1961 as an "ideal demand" – see p. 347).

Prompted by the critics of the received view of reduction, Nagel, Hempel, and Kenneth Schaffner have attempted to handle these objections. What they claimed is that by introducing a notion of *approximative reduction*, the incompatibilities between T_R and T_B could be accounted for (Gaa, 1975). As Nagel recognizes in response to some of his critics (and speaking of homogeneous reductions): "the laws derivable from Newtonian theory do not coincide exactly with some of the previously entertained hypotheses about the motions of bodies" (Nagel, 1970, p. 120), however "the initial hypotheses [T_R] may be reasonably close approximations to the consequences entailed by the comprehensive theory [T_B]" (p. 121). What is deduced from T_B is not T_R but "the *approximate* truth of the reduced theory" (Putnam, 1965, p. 206).

As Schaffner (1967) points out, in case some new lower-level theory T_B shows up, T_R is often revised into a theory T_R^* which stands in a relation of "strong analogy" to T_R and in which T_R's false elements are removed. For example, statistical mechanics redefines "temperature" (Brittan, 1970); what has taken place is not a reduction of classical thermodynamics, as the received view pictured the intertheoretic relation, but "something resembling it" (p. 453). Thus, Nagel's [DC] remains in force, but now the entailment holds between T_B and an appropriate, "strongly analogous" image T_R^* of T_R (Schaffner, 1967; 1974), a "corrected secondary theory" (Schaffner, 1967). According to Schaffner, one will now be able to bypass the incompatibilities between T_R and T_B because these can be removed in T_R's approximative image T_R^*. As Gaa succinctly formulates it: "The condition of derivability, so important to Nagel, now requires, for the relation of reduction to hold between two theories, that an *appropriate* analog of the reduced theory, and *not* the reduced theory itself, be derived from the reducing theory" (Gaa, 1975, p. 355).

Many problems with this notion of approximative reduction have been pointed out. What may count as an adequate approximation to T_R? Feyerabend claimed that the relation between T_R and T_R^* is "too vague" and "essentially subjective" (1981, pp. 58–59). The real problem is that "real theories, theories which have been discussed in the scientific literature, are replaced by emasculated caricatures" (Feyerabend, 1965, p. 229). In actual science, Kuhn and Feyerabend argued, there are many cases in which T_R and T_B are incommensurable. Cases of theory change often violate what Feyerabend termed the "condition of meaning invariance" and this implies that T_R/T_R^* cannot simply be derived from T_B. What one often observes in science are revolutions rather than the cumulative and progressive change envisioned by Nagel and his followers.

Time to take stock: Nagel's elegant account of theory reduction does not work. First, the world does not cooperate: the kinds of psychology (representations, consciousness, qualia) do not have neat bridge law-like connections with the kinds of physiology or physics (the same applies to biology, for that matter). Second, science does not conform to this model: with the progress of science, meanings change and old theories are rewritten (sometimes beyond recognition) rather than smoothly incorporated in the new theory.

3. The New Reductionisms

3.1 New Wave Reductionism

In science, one is often confronted with what may be termed "replacement reductions" (Sklar, 1967, p. 4). Hooker even claimed that in fact one of Nagel's cherished examples of a theory reduction may be such a replacement reduction: "thermodynamics is simply conceptually and empirically wrong and must be replaced" (Hooker, 1981, p. 49). In cases of replacement, (strong) bridge laws or reduction functions are obviously not obtainable: one cannot formulate identity statements if at least one of the terms is referentially empty. Applicability in science being an important desideratum for any model of theory change, the question becomes: how to account for T_Rs which are *radically* false?

Schaffner's (1967) General Reduction Paradigm, later remodeled into the General Reduction-Replacement Paradigm (GRR), was the first attempt at a formal rewrite of Nagel's model by weakening [CC] and [DC]. GRR aimed to reconcile the seemingly incompatible views of scientific change, i.e., Nagel's account of "smooth" theory changes and Kuhn/Feyerabend's view of "bumpy" theory changes, of scientific progress might be reconciled in one comprehensive model. Even when Nagel's [DC] cannot be met, in particular in cases that involve false theories, one may still be able to map theories on one another. New Wave Reductionism is based on Schaffner's "General Reduction-Replacement model"; however, it considers Schaffner's view way too liberal because the latter allows T_R^* to be built out of materials supplied by the uncorrected T_R and this theory may be completely mistaken. In contrast, NWR demands that a corrected image or analog of T_R is constructed *out of the conceptual resources furnished by T_B* (Hooker, 1981, p. 49). This analog, T_R^*, mimics to some extent the formal/structural properties of T_R. T_R^* is an "analog" *within T_B*. It is this appropriately revised version of T_R in the base theory T_B which is derived, not the laws of T_R themselves: "what is explained directly by the reducing theory are the *corrected* statements derivable from it" (Hooker, 1981, p. 46). Thus, deduction on the "new wave" model is always an *intra*theoretic relation, not an intertheoretic one, as in Nagel reductionism. This feature allows the model to account not only for reduction cases in which T_R's referents are retained, but also for cases of elimination (Bickle, 1998, p. 29; see also Schouten & Looren de Jong, 1999).

With NWR, demands with respect to the relation between T_R and T_R^* are less stringent than [CC] and [sCC]. Only a relation of analogy is required, not a strict identity expressed in a bridge law. This feature allows the obtained intertheoretic analogies to be ordered along a dimension of "perfectly smooth" cases (*retentive reductions*) to "extremely bumpy" cases (*eliminative reductions*). Whenever the mapping of T_R onto T_R^* is subject only to comparably minor revisions, the mapping is smooth. This means that a reduction has been achieved, which implies that the ontology of T_R is preserved. In those instances in which T_R and T_R^* are relatively or even radically dissimilar, however, because large-scale revisions were necessary to construe T_R^*, an elimination of (parts of) T_R will have obtained. In particular by loosening Nagel's [CC], NWR is able to sidestep many of the problems that troubled *ancien régime* reductionism. It is by dropping [CC] (and [sCC] for that matter) that NWR accommodates eliminative reductions. More specifically, it accounts for the possibility of (folk) psychology being eliminated by neuroscience.

Eliminative materialism was brought into stark relief by Paul Churchland. It asserts that folk psychology is a relic of the past, hopelessly disconnected from the rest of the scientific world. Churchland's diagnosis is that "Folk Psychology is a modern cousin of an old friend: Ptolemaic Astronomy" (Churchland, 2005, p. 38) and its items belong in the museum of antiquities along with such curiosities as entelechies, élan vital, crystal spheres, phlogiston, ether, witchcraft, sunrises, and so on, which have all been displaced from our best scientific ontologies. In terms of NWR this means that the (folk)psychology-neuroscience case falls at the eliminative end of the continuum of intertheoretic analogies.

3.2 Functional Reductionism

Recall Fodor's remark that the multiple realizability argument "refutes psychophysical reductionism once and for all" (1998, p. 9). Many in the philosophy of mind followed Fodor and embraced the multiple realizability argument as a "Declaration of Independence" (Shapiro, Chapter 5, this volume), as it apparently succeeded in securing a robust autonomy for the mental *vis-à-vis* the neurophysiological. In recent years, however, many authors have expressed doubts concerning the force of the argument (Bechtel & Mundale, 1999; Bickle, 1998; Churchland, 1986; Enç, 1983; Polger, 2004; Shapiro, 2004). According to Churchland, for instance, multiple realizability can not be an obstacle to intertheoretic reduction since multiple realizability even obtains in such a textbook case of intertheoretic identification (and reduction) as temperature being identical to mean molecular kinetic energy (discussed in Bickle, 1998). Whereas temperature in a gas is identical to mean molecular kinetic energy, temperature in a solid or in a plasma is not. Moreover, if multiple realizability really were a problem, it is hard to explain the current successes of neurosciences, as these are built on the premise that there is genuine continuity of function across individuals and even across species. Even if multiple realizability is real, it may not be able to block intertheoretic reduction. Even if "global" bridge laws are unavailable, "species- or structure-specific bridge laws" remain a possibility; while we may have to give up on "global" (Nagel-style) reductionism, one might still have "local reductions" and therefore psychology can no longer enjoy autonomy (Kim, 1998).

Kim's (1998, 2005) metaphysical work illustrates the physicalist Catch-22 mentioned earlier: if mind fits in a physical world, it exists only in virtue of a physical realization; if it does not fit in the physical world, it cannot be real. Saving mental causation (and by analogy, higher-level explanations) consists in showing its physical realization. This, briefly, is how the argument is supposed to undercut nonreductive physicalism.

The starting-point for Kim is nonreductive physicalism's commitment to the view that mental properties have new causal powers over and above the causal powers of neural, or other physical, properties. When I want to have a beer and I think I can have one by opening the fridge, I walk up there because I have this desire and this belief – and not, say, because my neurons in this or that part of my central nervous system are firing. But what relation between mental and neural properties justifies the nonreductionist in his claim that it was my belief and my desire that caused my behavior and not the activity of my central nervous system, especially given the nonreductionist's commitment to physicalism. If this relation is not identity, then what is it?

It is a familiar story that nonreductive physicalists have turned to supervenience: mental properties are dependent on, or determined by, physical properties, but physical properties are not dependent on, or determined by, physical properties. It is this relation of asymmetrical dependence that is supposed to safeguard the autonomy of the mental.

According to Kim, there are good reasons to doubt that supervenience will be able to make room for the mental in a physical world. Under the nonreductivist's assumption of supervenience, a mental property *M* is causally active because a neural property *P* is. For instance, I have a desire about a bottle of my favorite Trappist beer in the fridge.[1] This desire depends – by supervenience – on a specific neural state. So, what causes me to open my Westmalle Dubbel? Although there is a causal connection between mental property *M*, my desire, and me pouring myself a glass of beer, the latter action was also causally necessitated by the physical (presumably neural) property *P* on which mental property *M* supervenes. So, how can *M really* be a cause of my action when I have *M* because I have *P*? What causal work is there to do for *M* over and above the work already carried out by *P*? Hence, any causal story involving mental states like beliefs and desires will be pre-empted or excluded by a more fundamental neurophysiological story and the nonreductivist's claim to autonomous mental efficacy is unjustified. (See Shapiro, Chapter 5, this volume, for a critical discussion of the claim that neurophysiological distinctions will directly "trickle up" to psychology.)

From here, Kim says, three directions are open to the nonreductivist: one may become a Cartesian dualist (no option), one may become an epiphenomenalist (no option) or one may turn to reductionism in order to rescue the causal efficacy of the mental in a physically constituted world. What Kim proposes is a "conditional physical reductionism" (2005, p. 5): *if* mental phenomena enjoy causal efficacy, and most of us have strong intuitions that they do, they enjoy it in virtue of them being type identical to neural phenomena. Mental events can be causally efficacious only in virtue of them being reducible to neural events: "If mental phenomena are neural processes in the brain, there will be no special mystery about mental causation" (Kim, 2005, p. 153) and, therefore, "[r]eduction is the stopper that will plug the cosmic hole through which causal powers might drain away" (Kim, 2005, p. 68). Only reductionism will be able to vindicate mental causation in a way that is satisfying to the physicalist. (See Gillett, Chapter 4, this volume, for further development of Kim's ontological reductionism, which, however, leaves room for theory nonreductionism.)

But if we buy Kim's arguments for reductionism, what kind of reductionism can this be? In the absence of bridge laws, reductive explanation may still be possible (Kim, 2005, p. 97). Basically, reductive explanation, as Kim understands it, consists in a three-step procedure. The first step is functionalization: give a job description of the property that is to be reduced; specify its causal role. The second step is to find the physical realizer for the functionalized property. The third step is to provide an explanation of how the physical realizers fulfill the causal role specified in the first step. Examples are *gene* and *temperature* (see 1998, ch. 1). This is reduction without bridge laws: the relation between mental and neural is a role-filler relation. In accordance with the functionalizing strategy, Kim urges to look for local, presumably species-specific, reductions. Psychological states like representations or pain may have neurally different realizations in different organisms, and reducing them may produce a disjunctive series of local reductive identifications between mental and neural events. In this case, losing generalizations over the functional causes of the behavior of different species may even be good riddance.

4. New Chapters in Reduction: Metascience, Mechanicism, and Pluralism

4.1 Metascience and Mechanicism

In this section we will consider another alternative to the "sweeping," single-purpose accounts we considered above. Many have called attention to the fact that historical developments in science resist being captured in such uniform models. McCauley (Chapter 9, this volume), for instance, argues that reduction in science is neither simple nor unitary. The inadequacy of global accounts of reduction is particularly clear when one considers the life sciences. Here we don't typically find laws or sweeping, large-scale theories as was required by standard nomothetic accounts of explanation. In particular in biology and cognitive science, scientists' aims are at a much more local scale: they search for functionally characterized models of increasingly finer grain that explain selected phenomena at higher levels (Cummins, 2000). In such domains as the cognitive, biological, and neural sciences, researchers provide successful explanations without providing laws; they aim at uncovering and specifying mechanisms. Hence, as Richardson puts it (Chapter 6, this volume), what we see in science is not theory reduction, but a "succession of models constituting partial solutions based on inadequacies to specific and local problems," rather than the incorporation of one theory in the next one.

Theory reduction fares badly when one's goal is to describe what actually happens in science. Bickle argues that we should let go of the "philosopher's fantasies": classical reductionism, functional reductionism, and new wave reductionism (the latter developed by his own former self, among others). This is not to say that science is not reductionistic. The point is that we must get into the laboratory and look at actual science – from the bottom up, as it were – to find out what reduction is: reductionism can only be reductionism-in-practice (Bickle, 2003). We should leave behind philosophy and embrace ("new wave") metascience: clarifying reductionism is "letting a sense of reduction emerge from the detailed investigations drawn from recent scientific practice" (p. 31). Neuroscientific experimental and explanatory practices show that mind-to-molecule (or mind-to-cell) links are established all the time through what Bickle calls "intervene cellularly/molecularly and track behaviorally" approaches, i.e., lesioning, knocking out genes, or otherwise manipulating lower-level constituents of a system and then tracking the behavioral effects of such interventions. Molecular and cellular mechanisms are claimed to directly explain the behavioral data – and that's reduction if anything is! In his chapter Bickle argues that even consciousness, "the castle keep, the central redoubt, the core essence of true mentality" (Churchland, 1995, p. 212), might not be able to escape such "ruthless reductionism": science now offers clear views of the molecular mechanisms underlying certain aspects of consciousness at the macromolecular level of agonistic activities at subunits of γ-amino-butyric acid type A ($GABA_A$) receptor proteins.

Many of the authors in this volume share Bickle's naturalistic view that an understanding of reduction and reductive explanation should start in science. Bechtel, Clark, Bickle, Richardson, Wright, and McCauley (this volume) all point out that, especially in the life sciences, the search for and identification of mechanisms that are responsible for a phenomenon under investigation is of central importance. On "ruthless reductionism," however, we can and should descent immediately to the lowest possible levels, i.e., the levels of cells and molecules, and this allows us to "*set aside causal-mechanistic explanations* offered at intermediate levels of theorizing" (Bickle, Chapter 12, this volume). Bechtel, Clark, Richardson, Wright, and McCauley disagree. Clark, for instance, says that "intermediate-level analyses are of great importance." We should be careful to note, however, that claiming that higher-level analyses are important is not the same as claiming that there is no room for reductive explanation.

Bechtel claims that mechanistic explanations are reductionistic in their appeal to lower levels. In this respect, Clark speaks of "homucular explanations" which he sees as "the contemporary analogue to good old-fashioned *reductionistic* explanation." However, as both Bechtel and Clark tell us, looking down to lower-level – cellular, molecular, or systemic components, for instance – does not suffice. One must move beyond accounts of the parts of a mechanism and how they operate. The *organization* of the parts and interactions of the mechanism with its environment requires (semi-) autonomous higher-level research. Thus, some kind of autonomy for psychology can be maintained without multiple realizability since, according to Bechtel, higher-level accounts provide "additional information." Clark emphasizes that cognitive science should strive for "a satisfying and mutually illuminating interlock" between three different explanatory styles: homuncular, interactive, and emergent explanation. Homuncular explanation alone can never suffice when one's goal is to understand embodied, embedded agents. In such cases, entirely different explanatory strategies must be pursued. Thus, whereas Bickle takes molecular and cellular neuroscience to bypass higher-level analyses altogether, higher-level and lower-level investigations complement one another according to Bechtel, Clark, and others. Hence, these authors dismiss Bickle's explanatory monism according to which a behavioral phenomenon is explained by a single account furnished at the lowest possible level.

4.2 Pluralism and Co-evolution

The new mechanicists offer a very moderate kind of reductionism. They argue that it is important in the cognitive, biological, and neural sciences to specify lower-level mechanisms to explain selected higher-level phenomena, such as the behavior of systems under specified conditions. This does not involve the reduction or elimination of entire upper-level theories. The importance of higher-level theories, is not denied: explanatory ascent is as important as explanatory descent. Clark describes such an outlook as “explanatory liberalism.” We will now see how these ideas concerning mechanistic analysis and emergent explanation can, according to some authors, be embedded in a more general view of explanatory pluralism.

Wimsatt (1976a) argued that not all reductions are of a uniform nature (as suggested by Nagel, Schaffner, and the new wave reductionists) and proposed to distinguish between two types of reduction, what he labeled “interlevel reduction” (roughly what Nickles (1973) had called “reduction_1”) and “intralevel” or “successional reduction” (roughly Nickles’s “reduction_2”). Intralevel reduction involves the relations between an older theory and a newer, succeeding theory (say, T_R and T_R^*), with the latter correcting the former. The intralevel or diachronous context is the context of intertheoretical relations that involves the modification and succession of theories over time. Such reductions concern transformational, possibly non-deductive and diachronous relations between theories (see also McCauley, 1986). Looking at successive scientific theories, one sees the *transformation* of theories in the light of mutual similarities and differences. Within-level reductions are about localizing, demonstrating and analyzing the analogies obtaining between theories T_R and theories T_R^*.

Interlevel (or explanatory) reductions, on the other hand, are of an altogether different kind. Wimsatt asserts that in contrast to the formal or structural models discussed above, we never find “total deductive systematization” as in classical models of reduction (like Nagel’s or Schaffner’s) and such global systematization is also “clearly unnecessary and irrelevant to the search for explanations” (Wimsatt, 1976b, p. 684). As Wimsatt pointed out many years before the current wave of mechanicism in the philosophy of science, biologists are reductionistic, not in the sense that they are interested in explaining theories through derivation, but because they aim at explaining phenomena by discovering mechanisms. Whereas for Nickles interlevel reductions are obtained by Nagel-style derivational reductions, according to Wimsatt (and this accords nicely with ideas formulated by mechanistic philosophers of science), interlevel contexts do not engage relations between theories at all, rather in such contexts one considers properties of higher-level entities and how they relate to properties of lower-level entities. What most scientists mean when they talk about reduction or reductive explanation is answering questions like: how is this or that phenomenon produced by causal interactions at lower levels? Here, Wimsatt explains, identificatory statements also play a role, but again not in the way envisaged by reductionists in the tradition of Smart, Sklar, Schaffner, and Kim: they are not ends in themselves, but rather tools that guide scientific progress. Scientists are not primarily interested in ontological claims of the sort $A = B$; rather their purposes are first and foremost of an explanatory nature. In Wimsatt's reconstruction of actual scientific practices, identity statements are hypothetical and heuristic and are used to detect and locate explanatory failures which in their turn drive intralevel theory changes (Wimsatt, 1976a, pp. 225–230). Wimsatt's suggestions thus embody a reading of the identity theory which was later developed in McCauley and Bechtel's heuristic identity theory (McCauley & Bechtel, 2001): "the optimal strategy for the identity theorist is not to waste time arguing for the in principle possibility of the identity theory, but to look for plausible explanations for the important and relevant differences between the mental and physical realms. If the explanations are forthcoming, the identities will be assumed. If not, the explanatory failures will force a careful use of Leibniz's Law to detect differences which might be used as the basis for new explanatory hypotheses" (p. 229).

Rather than theories being constantly under threat from lower-level ones, explanatory pluralists have discerned a "peaceful coexistence" between theories and models (McCauley, 1986; 1996; Schouten & Looren de Jong, 1999). They typically follow, again, Wimsatt's arguments developed in the 1970s: "Theoretical conceptions of entities at different levels co-evolve and are mutually elaborated . . . under the pressure of one another . . . [A]ll corrections in theory get packed into a 'successional' component and all unfalsified explanatory and compositional statements get packed into the 'explanatory reduction' component" (Wimsatt, 1976b, p. 682). Thus, distinct, though typically adjacent, levels mutually exert *selection pressures* and are engaged in a process of *co-evolution*. Bechtel, McCauley, and Wright show how this two-way flow of information works for the psychology-neuroscience, while Richardson speaks of "bidirectional exchange" between chemistry and physics.

Now we should note that it is certainly true that a number of philosophy's most uncompromising reductionists have recognized the importance of co-evolution (Bickle, 1998; Churchland, 1986). Psychology does have a role to play in developing explanations of behavior, even for reductionists. Hence, the Churchlands claim that "we count ourselves among the most fervent of the Friends of Psychology" (Churchland & Churchland, 1996, p. 219). However, one may doubt that this involves *genuine* co-evolution, with lasting contributions from both upper-level and lower-level theories (see Van Eck, Looren de Jong, & Schouten, 2006). For instance, Bickle's ideas on co-evolution amount to the view that psychological theories only provide fairly short-lived heuristics. After an *initial* co-evolutionary phase between theories at distinct levels, in which psychological theories provide crude descriptions of the phenomena to be explained, interlevel corrective, "structuring" influences between psychology and neuroscience travel from neuroscience to psychology, and not the other way around. The question for these reductionists is this: "Can we reconstruct all known *mental* phenomena in *neurodynamical* terms?" (Churchland, 1995, p. 211), a question which reductionists typically answer in the affirmative; neuroscientific results are simply fed into current psychology, which is then "simply *becoming* the Neuroscience of very Large and Intricate Brains" (Churchland & Churchland, 1996, p. 224). The inevitable outcome of this so-called co-evolutionary process will be that the neurosciences will be able to provide *exhaustive* fine-grained explanations, thereby rendering psychology explanatorily inert along the way. Bickle puts it thus: "There is no need to evoke psychological causal explanations, and in fact scientists stop evoking and developing them, *once real neurobiological* explanations are on offer" (Bickle, 2003, p. 110, original emphasis). Psychology, as Wright (this volume) puts it, simply becomes extinct.

In contrast, those with pluralist inclinations insist on *enduring* co-evolution, with higher-level sciences like psychology generating lasting influences on lower-level investigations. Wright, for instance, examines the mechanisms of motivation and brain reward function and shows that here preclusion of higher-level explanations would obstruct explanatory progress. He concludes that the idea of psychological explanations becoming extinct is a myth, not supported by scientific practice. Similar points are made by Endicott (Chapter 7, this volume) who argues that lower-level explanations require reference to higher-level properties. Reductionism fails because it does not do justice to the role of higher-level theories. Actual science shows that resources drawn from higher levels continue to play a role. In all, explanatory pluralism offers a view of scientific progress that highlights the fact that science works in a local, piecemeal fashion.

4.3 Pluralism and the Metaphysics of Science

The brief review of recent developments above indicates that reduction is a far more complex and dynamical affair than the classical picture suggested. The arrow of reduction is complemented with higher-level constraints downward; reduction can go hand in glove with higher-level explanations. Thus, reduction and autonomy are not necessarily contradictory (As the title of Bechtel's Chapter 8 in this volume shows). Looking back, we can now see that [CC] and [DC] were a concern for philosophers, generated by the Logical Positivist view of theories, not a real problem in science. The failure of finding bridge laws between two sets of theories and the unruly relations between them are part of the ongoing dynamics of scientific progress. Interestingly, the same rejection of the reduction/autonomy dichotomy can be seen in the metaphysical, if you will, metatheoretical contributions in this volume. In different ways they show the compatibility of reduction with a legitimate role for higher-level explanations. Reduction vs. autonomy is a "false dichotomy," according to Gillett, a view shared by Melnyk, Shapiro, and Polger: reductionism and antireductionism offer a "false choice" (Polger); functionalism and reductionism are "friends not foes" (Melnyk), and current empirical developments suggest "reduction *of a sort*" and "autonomy *of a sort*" (Shapiro).

Gillett presents a metaphysics of science, in particular analyses of the nature of the compositional relations, to underpin the mechanistic explanations (as provided by authors like Bechtel and Richardson) involved in such explanations. These compositional relations, uncovered in scientific investigations, drive a form of metaphysical reductionism: since the composing entities non-causally determine higher-level (composed) entities, the latter must be illusory. Nevertheless, it is argued, "New" reductionism is compatible with the nonreductivist's claim that the predicates, concepts and theories of the special higher-level sciences are (in principle) indispensable.

Melnyk too sees reductionism, properly understood, as compatible with functionalism, once the mainstay of antireductionism and autonomy. Psychological phenomena are multiple realizable, psychological explanations pick out really existing patterns, and psychological explanations may be used to revise explanations in terms of physical phenomena – as pluralists would agree. In fact, the possibility of mutual co-evolutionary feedback between psychology and neuroscience even presupposes some form of metaphysical reductionism, argues Melnyk.

Shapiro argues against Kim's claim that multiple realization leads to local reductions, and thus to the disintegration of psychology. Shapiro points out that not all causal differences in the lower-level physical properties are relevant for the higher mental level. And since these differences do not necessarily "trickle up," it is not obvious that psychology will fractionate along the lines of physical kinds. However, he also argues that psychology now faces disintegration from another direction, viz., theories of embodied cognition. These may, in Shapiro's view, have the consequence that there are as many subdisciplines of psychology as there are types of body.

Polger distinguishes various approaches to reductionism and argues that they all result from the problematic assumption that there is only one ontology and one true story of the world. He argues that there is more than one ontology and more than one explanation for a phenomenon. Hence, he defends an approach which, he says, is "genuinely nonreductive" in the sense that it is neither reductive nor antireductive, but pluralistic (or naturalistic).

To sum up, the dichotomy between reductionism and autonomy that we started with is a simplification. Careful conceptual work in the metaphysics of science (Gillett, Polger, Melnyk, Shapiro), in empirically informed work in the philosophy of science (Clark, Richardson, Endicott, Bechtel, McCauley), and empirical case studies and laboratory work in neuroscience (Wright, Bickle, Looren de Jong & Schouten) yield the picture of many connections and, in Kant's terminology, of passages between the many levels and domains of study of the mind/brain. The most reductionist position is defended by John Bickle, whose strategy of confronting philosophical problems (and philosophers) with the latest data from the laboratory bench is exceptional, but yields stimulating results. Most of the other authors, however, will acknowledge that to a more or lesser degree higher-level explanations are indispensable, but not autonomous; and that psychology and neuroscience are and should be connected and perhaps integrated, but not unified along physicalist lines.

5. Gaps and Gulfs: Unity and Pluralism

Recall that in this overview of the territory we departed on the assumption that unity is a crucial desideratum in science. As Klein and Lachièze-Rey say: "without unity as a beacon, the world, indeed human thought itself, would scatter into a dust of things and ideas impossible to integrate" (1999, p. vii). Such a unity of science apparently involves what Descartes once called a *catena scientiarum* (*Cogitationes privatae*, AT 10: 215). Scientific disciplines and theories must be strung together. And science is, some argue, well on its way toward such a concatenated unity: the physicist Steven Weinberg once remarked that in science we can see "a convergence of the arrows of explanation, like the convergence of meridians towards the North Pole. Our deepest principles, although not yet final, have become steadily more simple and economical" (Weinberg, 1993, pp. 231–232). Here we see a succinct formulation of reductionism's fundamental aims: nothing less than a "final" and "simple and economical," unified view of reality. Thus, reductionism is often taken to be committed to an explanatory monism which is supposed to deliver something high on science's wish list: a unity of knowledge (see, however, the chapters by Melnyk, Polger, and Gillett in this volume). We have also seen however that it turned out to be difficult, if not impossible, to formulate what it means to reductively concatenate theories in a way that does justice to living science. Thus, our overview of the arguments pro and contra reductionism led to explanatory pluralism instead of explanatory monism.

So, one might ask, haven't explanatory pluralists sacrificed our cherished ideals of unity and integration? Not necessarily. One option that might be explored is to say "so much the worse for the unity of science" (e.g., Cartwright, 1999; Dupré, 1983; Fodor, 1981; Van der Steen, 1993), or one might argue that it was mistaken to tie unification to reductionism in the first place. For reasons of space we shall not go into the first option. It is the last option (of integration-without-reduction) that we will explore a little further in the remainder of this introductory chapter.

It is well known that the very idea of an *Einheitswissenschaft* was made famous by logical empiricism. However, we must be careful to note that opinions within the broader logical-empiricist movement strongly diverged on this issue. Not everyone in the movement agreed with Nagel that reductionism offered the royal road to a unified science.

Nagel's ideas concerning a reductionist construal of unification were foreshadowed in a paper ("The logic of reduction in the sciences") read at the movement's Prague conference of 1934 and later published, along with the other papers presented at this conference, in the movement's house organ *Erkenntnis.* Read at the same conference (and published in the same volume of *Erkenntnis*) was a paper by Otto Neurath, entitled "Unity of science as a task" (Neurath, 1935). Here Neurath emphasizes that the unity of science does not involve a unity of laws – which, for instance, Carnap (see, e.g., 1938) wished to defend, but a mere unity of language. Although clearly a unificationist, Neurath was not a reductionist. Whereas Carnap thought it possible to ultimately derive the sociological laws from the laws of physics, Neurath dismissed this possibility: "*The development of physicalist ontology does not mean the transfer of laws of physics to living things and their groups*, as some have thought possible." There is no need to "go back to the microstructure, and thereby to build up these sociological laws from physical ones" (Neurath, 1931, p. 75). Physicalism in Neurath's sense only requires that the sociologist (or psychologist) speaks of entities observable in space and time and describable in what he called the "Universal Jargon."

Neurath markets the idea of a unified science (of which he was the spiritual father) as "encyclopedic integration" and this did not involve anything like looking for a "super-science" (Neurath, 1937, p. 265). As Neurath remarks, "'*The system*' *is the great scientific lie*" (Neurath, 1935, p. 116), because, and here his statements are a distant echo of the Heraclitean *panta rhei*, "basically everything is fluid, . . . multiplicity and uncertainty exist in all science. . . . *The whole of science is basically* always under discussion" (p. 118). "*Alles fließt*" in science (cited in Reisch, 1998), and this observation indicates that Neurath was not a reductionist (see also Uebel, 2000) and a pluralist. Unified science has everything to do with a "pluralist attitude" as there is no comprehensive worldview and the encyclopedia remains full of "gaps and gulfs" (Neurath, 1946, p. 497). Encyclopedism is all about antitotalitarianism, tolerance, and *laissez faire.* In a spirit very close to what is upheld by the explanatory pluralists, Neurath adds that

> our scientific practice is based on local systematizations only, not on overstraining the bow of deduction. Very often scientists know perfectly well that certain principles applied to a certain area are very fruitful, while contradictory principles applied to a different area also appear to be fruitful. It would, of course, be nice to harmonize the demonstrations in both areas, but in the meantime, scientific research progresses successfully. (p. 498)

The pluralism involved in unified science can, according to Neurath, be best understood through Horace M. Kallen's (1946) metaphor of the *orchestration* which involves "diversities of instruments and parts, of movements and pauses, of dissonances and discords as well as harmonies" (Kallen, 1946, pp. 495–496).

So, going back to the roots of empiricism suggests a more pluralist view than the ruling consensus established by later generations. As mentioned, pluralism emerges in this volume both from conceptual and metaphysical analyses on the one hand, and from case studies on the other hand. To sum up, connecting domains of knowledge is not necessarily bringing higher levels under the rule of physics (if there is such a rule). As Kant put it (*Kritik der Urteilskraft*, 1799, p. 305), each science is a separate, whole structure, although a connection (passage, *Übergang*) can be made afterwards between them. Kant's metaphor may be too static (in Kant's own words, architectonic) for our taste, but we would like to adopt the image of passages. Connecting scientific domains may not at all be like annexating and rebuilding psychology by neuroscience, as reductionists suggest, but more like building passages between one part of the many semi-detached buildings of science and another.

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Note

1 We owe this example to Cory Wright.

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