Why Psychology Isn’t Unified, and Probably Never Will Be

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Over the past few decades, a large literature has emerged on the question of how one might unify all or most of psychology under a single, coherent, rigorous framework, in a manner similar to that which unified physics under Newton’s Laws, or biology under Darwin’s theory of natural selection. It is argued here that this is a highly unlikely scenario in psychology given the contingent and opportunistic character of the processes that brought its original topics together into a new discipline, and the nearly continuous institutional, social, and even political negotiating and horse-trading that has determined psychology’s “boundaries” in the 14 decades since. Psychology, as the field currently stands, does not have the intellectual coherence to be brought together by any set of principles that would enable its phenomena to be captured and explained as rigorous products of those principles. If there is a kind of unification in psychology’s future, it is more likely to be one that, paradoxically, sees it broken up into a number of large “super-subdisciplines,” each of which exhibits more internal coherence than does the current sprawling and heterogeneous whole.

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In psychology, there have been many proposals for a similar kind of unification. American Psychologist was long a prime venue for such work (Kimble, 1989, 1994, 1999; Staats, 1981, 1991; Sternberg & Grigorenko, 2001). Since the founding of The Review of General Psychology, it has become a popular vehicle for them as well (Marsh & Boag, 2014; Staats, 1999; Yanchar & Slife, 1997), highlighted by a special issue on the topic in 2013 (Anderson, 2013; Catania, 2013; Charles, 2013; Chemero, 2013; Clegg, 2013; Heft, 2013; Henriques, 2013; Hutto, 2013; Lerner, Agans, DeSouza, & Gasca, 2013; Lickliter & Honeycutt, 2013; Marken & Mansell, 2013; Mayer & Allen, 2013; Petroc & Mackay, 2013). Proposals for the unification of psychology have hardly been confined to these two journals, however. There have been others as well (e.g., Goertzen, 2008, 2010; Henriques, 2004, 2011; Kimble, 1996; Staats, 1983; Kristensen, Slife, & Yanchar, 2000; Stam, 2004, 2015). An interesting counterpoint to these diverse efforts can be found in Sigmund Koch’s call to concede that psychology cannot be unified and to rename the field “psychological studies” (Koch, 1993).

The question of whether psychology can be “unified” is one that has long occupied the discipline. In order to be “unified in the sense I intend here, the discipline would have a common set of foundational principles from which detailed descriptions of its phenomena could be rigorously derived and by which they could be explained. It seems that if only psychologists could agree on these fundamentals—the mental and/or behavioral “elements,” and the basic principles of their interactions—then the discipline would start making the kind of impressive progress that we have seen in natural science over the past few centuries.

The most commonly raised example of this kind of unification is found in physics, which is often said to have been unified under Newton’s Laws which were, in turn, reliant on his mathematical discoveries. Another prime example is in biology, large parts of which were unified under Darwin’s theory evolution by natural selection. One of the most significant aspects of these two epoch-making scientific developments is that they not only provided a common explanatory framework for disparate aspects of complex fields of study (e.g., respectively, the motions of both terrestrial cannon balls and of celestial planets; the seemingly ubiquitous hierarchical arrangement of species in both the animal and plant kingdoms), but they were later extended and adapted to provide explanations for phenomena that were either unknown or poorly understood at the time the framework was first proposed (e.g., electrical force; the development of antibiotic resistance), thereby strengthening the unity of the field even further.

REFERENCES


1 I contrast this sense of unification with another that is often offered: mere descriptions of high-level similarities among diverse research areas. These do not unify a discipline in any important sense. They serve mainly as appeals to common ideas that hold psychologists together as an institutional community, but do not show that diverse psychological phenomena spring from the same fundamental bases.

2 Of course, both of these unifications have been superseded in their respective fields by more recent developments—relativistic and quantum mechanics in the case of physics, and the “synthesis” of evolutionary theory with genetics in the case of biology, but these details need not detain us here.

3 Some have argued that psychology can never be unified in the ways that physics and biology were because the field is inherently embedded in historically contingent sociocultural practices (e.g., Teo, 2010; Walsh-Bowers, 2010). If these critics are correct, then unification may be blocked for reasons that are even more fundamental than those discussed in this article.

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Perhaps ironically, the various proposals for unification have come from a diverse range of epistemological and ontological perspectives. All too often, however, they have amounted essentially to the author asserting that the perspective on psychology to which he or she is already committed on prior grounds—whether behaviorist, humanistic, evolutionary, computational, and so forth—is, in principle, sufficient to capture and explain all of psychology, or at least enough of it to create a kind of dominant subdiscipline within the broader psychology that implicitly challenges all other areas to either get on board or find another boat. This tactic, popular as it might be, is highly unlikely to succeed because the perspective that is put forth as the basis for unification is already well-known to the audience to whom it is proposed, and those individuals who did not accept it before in the area where it first gained credibility are virtually certain not to accept it as the basis for psychology more broadly.

What is required from a unification proposal is a theory, probably framed in a mathematical or otherwise formal language, which rigorously captures apparently disparate phenomena. The chief reasons to employ a formal language are essentially threefold. First, a formal language can bridge across areas that differ in terms of their concrete content, but that have similar underlying structural properties. Second, it is in formal languages that the work of prediction, and the sort of explanation that relies upon prediction, can be most satisfactorily achieved. I am not among those who believe that these are the only legitimate goals of science, but the accurate prediction of new phenomena is certainly an impressive achievement when it can be attained, and it is most convincingly accomplished in the context of a formal language. That is to say, formal language (the “right” language, not just any superficial formalism) enables one to achieve both breadth of scope and fidelity to detail at the same time. Finally, formal languages often have the ability to short-circuit the all-too-common impulse to become mired in endless (and often pointless) a priori disputes about which theoretical ideology is to be preferred—for example, behavioral, evolutionary, humanistic, and so forth. If the predictions work, they work.

Newton’s laws of motion did precisely this—explaining why the apple falls directly to the ground, why the cannon ball flies for some distance before returning to the ground, and why the moon revolves about the earth in apparent perpetuity. The Newtonian framework was later successfully extended to matters such as fluid dynamics and electromagnetism, among others—areas that are different in content from those that Newton began with, but that turned out to be structurally similar. Darwin’s theory was not framed in formal language at first, but neither did it immediately capture its field. Its gradual integration with Mendelian genetics in the early decades of the 20th century, and the later rise to dominance of the “Modern Synthesis” (Huxley, 1942) was, in no small part, dependent on its redescriptions in formal terms (e.g., Hardy, 1908; Weinberg, 1908). As a result, evolutionary models have been successfully applied to topics far removed from the biological realm where Darwin first developed them.

By contrast, psychological attempts at unification have rarely even aspired to this degree of formal rigor and, as a result, they have been unable to facilitate psychological prediction or explanation to any significant degree. Even when psychological theorists have attempted to develop a formal language to achieve both predictive accuracy and broad scope, they have fallen dramatically short (e.g., Hull, 1943).4

There was a time when it was popular to argue that psychology was following Thomas Kuhn’s famous formula for the progress of mature science under the auspices of successive “paradigms” (see, e.g., Coleman & Salamon, 1988 for a summary and assessment). This happened (and continues, to some degree) despite the countervailing fact that the early psychological systems—Structuralism, Functionalism, Psychoanalysis, Gestalt, Behaviorism, and so forth—were obviously overlapping, competing “schools of thought,” rather than consensual “paradigms” that successively captured the entire field. This state of affairs is precisely what Kuhn himself said characterizes the state of science in its “pre-paradigmatic” phase: “schools of thought.” There is also the glaring fact that Kuhn specifically excluded the social sciences from his scheme, using psychology as a key example (see, e.g., Kuhn, 1970, p. 160). Kuhn’s model of paradigmatic science is inapplicable to psychology, whether or not it is true elsewhere in the sciences.

Wilhelm Wundt, as is well known, strove from the 1860s forward to render psychology scientific by employing the methods and, perhaps more important, the equipment that had succeeded in reshaping physiology as an experimental science in previous decades to address his questions about the mind. He achieved a fair degree of success, founding the first university-authorized institute dedicated exclusively to experimental psychology at Leipzig in 1879, and drawing dozens of students from all over the world to study with him and learn his methods. But even he recognized that the range of psychological phenomena that he could tame into a form suitable to laboratory investigation was highly restricted. Fractionating reaction times into sensations, perceptions, acts of will, muscular reactions, and, at the center of these, the all-important apperceptions—impressive as the feat might have been—was only a small part of the totality of the mind (see Robinson, 2001). Although he continued to manage his very productive laboratory into the 1910s, Wundt devoted his own greatest efforts to writing the 10-volume Völkerpsychologie, in which he described the artistic, religious, and broader cultural lives of various peoples—all human mental activities that he did not believe to be amenable to the constraints of the laboratory.

Many of Wundt’s American students brought his ideas about the psychological utility of kymographs and chronoscopes back across the ocean, and they established similar laboratories in their home colleges and universities. This importation of the physical manifestations of German expertise was very impressive to college trustees and presidents, but it left many of the psychologists themselves frustrated with the inherent limitations of such a view of the field. The momentary reactions of the well-prepared normal adult male human mind to a sparse array of artificial laboratory stimuli did not bring them intellectual satisfaction. Instead, many sought to found scientific psychology anew on the basis of Darwinian evolutionary theory. They sought to expand the repertoire of mentalities explored to include children and women, workers

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4 Various interesting candidates for “rigorous, formal over-arching psychological framework” have appeared over the decades: for example, psychophysics, signal detection theory, decision theory, game theory. While all have been valuable, none has gone on to attain the range of application that would be needed to unify the discipline as a whole.
and asylum patients, cats and chickens and fish, and even insects. In short, they wanted psychology to cover the full array of mental variation, and to consider what forces—both internal and external—might have led it to evolve in the way it had, and how it might evolve further in the future (see, e.g., Green, 2009).

This extremely tumultuous state of affairs was given the name “Functionalism” by its most vocal opponent, E. B. Titchener (an Englishman who was trained in Germany and then employed, sometimes to his chagrin, in America). Functionalists shared some broad aspirational themes, to be sure, but Functionalism by no means represented a unification of the discipline of psychology. Quite to the contrary, its ethos was more in line with letting a hundred flowers bloom. Indeed, G. Stanley Hall was fairly explicit about this in an 1894 article, though, in his customary promotional style, he presented it as a strength; a measure of the wide range of material covered by the new discipline:

> It [psychology] studies the instincts of animals from the highest to the lowest... It shows how the highest intuitions of reason and conscience are rooted in the lowest animal instincts... It studies the myths, customs, and beliefs of primitive man... It devotes itself to the study of insanity and nervous disease, and has already begun to introduce new methods and utilize new results. It has a special department of neurology for exploiting all the properties of brain, nerve, and sense... It has transformed and shaped the problems of logic and ethics; is slowly rewriting the whole history of philosophy, and... is showing itself not only to be the long hoped for, long delayed science of man... but is introducing a period that will be known hereafter as the psychological era in scientific thought. (Hall, 1894, pp. 160–161)\(^6\)

It would only require the exchange of a few obsolete turns of phrase for more current ones for this paragraph to have been written by a psychological enthusiast of the past decade.

Behaviorism, which began just a couple of decades later, of course, is a special case. Indeed, it is probably the main source of psychology’s Kuhnian misapprehension and, as such, it requires special attention. Many believe that behaviorism constituted the first broad unification of psychology, that we somehow “lost” that unity, and that we must now recapture it either under a more adequate formulation of behaviorism or under some other overarching framework. It must be recalled, however, that there were many species of behaviorism—Watson, Tolman, Hull, Skinner, among others. Some attempted to “redescribe” conventional psychological language in behavioral terms (e.g., Tolman, 1932) while others attempted to eschew the entire traditional vocabulary in favor of a new behavioral language (e.g., Skinner, 1945). This situation hardly recapitulates the compelling unity of _F = ma_ or of evolution = reproductive variation + natural selection by fitness. Second, although behaviorism (collecting its various conflicting forms together) may have been the ascendant American metatheory during the middle of the 20th century, even in its heyday there remained large areas of psychology that it was unable to effectively address. Consider that many of the classic studies from the 1930s, 1940s, and 1950s—studies so important that we continue to teach them to our students even today—stand as monuments against the claim that psychology was unified under behaviorism: Stevens’ psychophysical power law, Gesell’s studies of childhood development, Terman’s longitudinal studies of intelligence, Dollard and Miller’s frustration-aggression hypothesis, Sherif’s “Robber’s Cave” studies of group behavior, Adorno’s “authoritarian personality,” Ash’s studies of compliance, Festinger’s theory of cognitive dissonance, to name just a few. These were in no significant way dependent on behaviorist theory (though sometimes attempts were made to shoehorn them into some version of behaviorism, often in order to shield them from the judgmental eye of one disciplinary authority or another).\(^7\)

Beyond the observation that much important mid-20th-century American psychology was not behavioristic, there is also a certain level of unreflective nationalism lurking beneath the claim that behaviorism was “dominant,” considering that the movement was never ascendant in Europe, only ever gaining a modest following there (not to mention the rest of the world).\(^8\) Thus, although behaviorism may have been an important metatheoretical move, it never commanded ascent in the discipline as a whole; it was certainly never the stuff of Kuhnian paradigms.

Of course, Kuhn’s model of scientific unification is only one of myriad models of how science develops, and it is now mainly of historical interest to most philosophers and historians of science. Contra Kuhn, the course of events may well proceed along different lines in different sciences, according to the unique features of different disciplines (see, e.g., Dupré, 1993; Hacking, 1996; Lakatoš, 1970; Laudan, 1978).

Looking specifically at the history of psychology we can begin to see that there are certain aspects to its historical “assembly” as a discipline that would seem to make it highly unlikely that the particular constellation of topics that currently fall under the heading of “psychology” would ever be unified into a strongly coherent scientific discipline (in any way tantamount to post-Newtonian physics or post-Darwinian biology).

If, for instance, one looks at the sheer variety of topics that G. Stanley Hall invited into the new discipline back in 1887 when he launched the very first U.S.-based periodical specializing on the new field of inquiry—the _American Journal of Psychology_—it is easy to see that they did not have much in common with each other either ontologically or epistemologically: experimental work on perception, qualitative writing on (what we would now call) cognition, historical studies of mind, case studies in psychiatry, and physiological work on the nervous system. What is more, some of these early invitees soon moved on to other disciplinary venues, only to be replaced by new invitees, or by Hall’s own creations:

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\(^5\) Titchener’s fervent hope was to establish the first psychology laboratory at his alma mater of Oxford. Such a move not seeming imminent, he tried at least to “return to British soil” by repeatedly inquiring about positions in Canada (Ferguson, 1982; Myers, 1982).

\(^6\) Robert S. Woodworth (1943, pp. 17–18), who quoted this passage even more extensively than I have, claimed that the article “doubtless closely paralleled” Hall’s long-lost inaugural presidential address to the APA in 1892. Because two (far from identical) abstracts of the 1892 speech are all that are left to us, we will probably never know, but it remains interesting that this was the vision of psychology that Hall chose to present to nonpsychologists in the middle of the 1890s: not of a tightly unified discipline, but of one with an enormous range of interests, from the activity of the individual neuron to complexities of human social life.

\(^7\) It is interesting to note that the second-best-known work of Dollard and Miller was their attempt to rewrite Freudian psychoanalysis into a form consistent with Hull’s drive theory.

\(^8\) To be entirely fair, behaviorism did have an important presence in both Latin America and Japan, but it would go too far to describe it as having been the dominant form of psychology in either place.
intellectual and moral development in children, animal behavior, and so forth (see Green & Feinerer, 2015).

Of course, Hall’s journal was soon followed by other similar ventures, most notably James McKeen Cattell and James Mark Baldwin’s Psychological Review in 1894. The Review was established specifically to compete with Hall’s AJP (see Sokal, 1997) and, consequently it had a somewhat different profile of research communities: Large sections were devoted to philosophical psychology and psychological metatheory, much of the early Functionalism experimentation and interpretation, and vision research into both color and depth/distance (Green, Feinerer, & Burman, in press, 2015). But, again, there was hardly an overarching set of principles guiding this jumble of different interests. Most researchers of the era would have agreed that they were working on one or another aspect of “consciousness,” but (a) this was a term of art often deployed to fend off those who argued, at the time, that experimental psychology was not an autonomous discipline but only a branch of physiology; and (b) appeals to “consciousness” only provided a broad tent under which many different kinds of researchers could do their work, not an underlying set of principles to guide the explanatory vocabulary of the new discipline. It is important to recall that as soon as William James (1904a, 1904b) called consciousness into question, the entire rickey structure began to fail and Behaviorism burst on to the scene less than a decade later (one can find the reactions of many of James’ contemporaries collected together in Taylor & Wozniak, 1996).

The internal conflicts of the early American Psychological Association reflect some of the differences of opinion over who was truly to count as a psychologist. Some wanted to throw the philosophers out, even though nearly all of them had originally trained in philosophy departments. (The philosophers formed their own associations just after the turn of the 20th century.) Others wanted to restrict membership to laboratory researchers only, excluding those who worked on psychological “applications.” (E. B. Titchener even tried to enforce this ban by forming his own separate by-invitation-only society—The Experimentalists, later reorganized as The Society for Experimental Psychology, which continues on to this day.) Various independent associations of applied psychologists eventually sprung up as well.

Psychology became extremely popular in its early decades, and its embrace expanded rapidly. Many new scholarly journals appeared in the early 20th century to satisfy the needs of new specialties: Journal of Abnormal Psychology (founded 1906), Psychological Clinic (1907), Journal of Educational Psychology (1910), Journal of Animal Behavior (1111), Journal of Experimental Psychology (1916), Psychobiology (1917), Journal of Applied Psychology (1917), and so forth (see Osier & Wozniak, 1984). After being buffeted, but also nourished, by the trials of World War I, the discipline emerged in the 1920s into what was at the time described as an “outbreak of psychology” (Leacock, 1924, cited in Benjamin & Baker, 2004, p. 54). Not only was its professional footprint growing rapidly—new university departments, new journals, new associations—but the wider public began to clamor for advice from popular psychology magazines to enhance the quality of their lives, whether romantic, intellectual, religious, commercial, athletic, or what have you.9

All of this public attention, however, did nothing to “unify” the discipline. Indeed, the term “psychology” started being used so variously that academic psychologists further intensified their already-existing campaigns to police the boundaries of its “legitimate” use. Behaviorists, of course, began to reject any (behaviorally unreduced) mention of mind, psyche, or consciousness as being “unscientific.” The rapidly expanding psychological testing movement—intelligence, vocation, and later personality—only crossed behaviorists’ strictures when they sought to reify the various dispositions they were testing, but it was a simple enough matter to use the new testing technologies profitably without taking a strong ontological stance on the matter (see, e.g., Boring, 1923). The explosive expansion of clinical, educational, vocational, industrial, and various other species of applied psychology—much of it outside of the academy—led to the ranks of such practitioners soon outnumbering their university-bound cousins. Although they sometimes drew upon the theoretical constructions of academic psychologists, many of them worked within disciplinary languages that would be better understood and trusted by their employers and their clients, whether medical, mentalist, psychoanalytic, or something else again.

I could continue to summarize the history of 20th century psychology—the integration of the American Association for Applied Psychology with the APA in the 1940s; the simultaneous emergence of the Bolder Model; the rise and fall of “third wave” humanism; the partial retreat of behaviorism as a theoretical basis for psychology and its reformulation as a leading basis for various therapies; the splintering off from the APA, first, of the psychonomic Society and, later, of the American Psychological Society; the appearance of the Psy. D.; the rises of cognitivism, computationalism, evolutionism, neuroscience, and so forth. Many readers will already be familiar with these more recent historical turns, however. The point I am trying to establish is that “psychology” did not originate in a well-circumscribed set of scientific problems that were pursued by a small group of like-minded intellectuals. Psychology has been a hodge-podge from its very creation, and none of the various efforts to create a theoretically unitary discipline out of that miscellany has ever been remotely successful. The unsteady and always-changing alliances of a number of distinct researcher and practitioner groups under the umbrella term “psychology” has long been more a matter of economic, political, and institutional contingency than it has been of a clear and consensual vision of the intellectual problems to be tackled and the methods to be used to resolving them.

Of course, many will object, correctly, that “origin is not essence;” that history is not destiny. The fact that psychology started out as a congeries of idiosyncratic interests bundled together more through administrative and diplomatic efforts than by underlying intellectual coherence does not in any way prevent some clever person down the line from devising or discovering a means by which they can be recognized as diverse manifestations of a small set of powerful underlying principles. That is, it is not impossible that these disparate parts might be unified. The entire aim of

9 The Cummings Center for the History of Psychology at the University of Akron, in Ohio, holds a large collection of these rare and fascinating publications: Herald of Psychology, Everyday Psychology and Inspiration, Current Psychology and Successful Living, Brain Power, and Golden Rule Magazine: The New Psychology are just a few of the titles. The collection was first assembled by Lady T. Benjamin, Jr. who has published about the history of the public view of psychology in various venues (e.g., Benjamin, 2012).
scientific unification, after all, is to show that what seems to be anarchy is, in fact, merely the superficial appearance of an underlying harmony. Who would have guessed, for instance, that the phenomena of the magnet and of lightning are really just different aspects of a single basic reality until it was shown to be so? Things are darkest before the dawn, and all that.

On the other hand, having conceded the sheer possibility that the wildly multifarious psychological phenomena of today might eventually be synthesized by some as-yet-undiscovered set of unifying principles, I see little reason to expect that this particular assemblage is ripe for unification, especially given the fairly arbitrary social and institutional processes that brought them together in the first place. What reason might there be for the particular set of facts that we call “psychological” today to be any more susceptible to unification than, say, some other more or less arbitrarily selected group of happenings in physiology, medicine, education, sociology, anthropology, and the like?

It seems more likely that any sizable unification in the near future will not capture the bulk of what is today called “psychology” (and only that). Instead, it seems more reasonable to expect that the discovery of powerful coherent principles would pull psychology apart into a set of large “super-subdisciplines” that are no more closely related to each other than, say, biology, psychology, economics, sociology, and political science are today. Indeed, one can see an illustrative historical example of this “pulling apart” in the way that Newton’s Laws “unified” mechanics: One of the concepts that was discussed and explained in the earlier Aristotelian understanding of motion was “growth.” This was not simply the growth of a mathematical curve, but growth in the fuller “biological” sense that a seed grows into a mature plant. Instead, growth became an “orphan” problem that eventually migrated to other branches of science where it could be better accommodated.

Similarly, rather than unifying today’s “psychology” as a whole, it seems more likely that some portion of contemporary psychology may be unified according to some set of principles. These will, perhaps, capture a large chunk of the current discipline, but not all of it, by any means. It may well be that the portion unified will, in addition, be integrated with other phenomena that are not currently considered to fall within the boundaries of “psychology.” (Consider how porous the boundaries already are between various parts of psychology and the neighboring disciplines of neuroscience, sociology, anthropology, philosophy, education, economics, etc.) The result of this partial unification of (a subset of) psychology would be the formation of a new discipline with new boundaries. Such an event, it might be said metaphorically, would show that the current form of psychology fails to “cut nature at its joints” (though there are ontological implications of speaking in this way that I, personally, would be reluctant to endorse). Later, some large part of the remaining portion of “psychology” might be unified under another set of principles entirely, separate and distinct from the first unification. Whether either of these distinctly unified pieces of today’s psychology would continue call itself “psychology” is anyone’s guess, but it would be only a matter of nominal importance, in any case. What is more, it is likely that numerous problems now considered to be “psychological” would fail to be integrated into either of the major unifications that had taken place and, thus, become “orphan” problems, just as growth did during the Newtonian unification of motion. The ultimate fate of these “orphans” is uncertain. Some might turn out to be special cases of phenomena that are already understood in other disciplines. Some might be the bases of yet other new disciplines. Some may, instead, turn out to be pseudoproblems: mere words appearing as problems.

I have ventured into this bit of “science fiction” not because I have any particular knowledge of how psychology’s future will play out but, rather, to show imaginatively that there are indeterminately many possibilities beyond psychology simply “being unified” or “not being unified,” and many of those possibilities seem, given the somewhat jumbled history of the field, at least as likely as either a Newtonian-style unification or simply continuing on indefinitely as we have for the past several decades.

Shifting to a related issue, there is a great deal of debate in philosophy of science these days among “realists” and “anti-realists.” My guess is that the antirealists are winning at present. That, I suspect, would come as something of a surprise to most scientists, who, by and large, see themselves as working to get a better understanding of the world “as it is.” Most antirealists’ view of science, however, is not that “anything goes” or that “all stories are equally valid,” as they are sometimes caricatured by their critics. Rather, the key assertion is that the history of science, examined over a period of centuries, shows not steady progress by a single scientific method toward some single Truth (as the high school textbooks would have it). Instead, it shows that the foundations, methods, aims, and configurations of the sciences are all constantly subject to negotiation among its “players” and, as a result, they all experience more or less continuous change—sometimes slow and gradual, other times violent and sudden, but never wholly stable for very long.

That observation doesn’t undermine science by showing it to be “nothing but” political subterfuge, social fashion, or arbitrariness.
It is, indeed, exactly as we should expect it to be because it is our underlying ontological conceptions, our epistemological values, and our apprehension of methods that are often in error and need to be revised. As they change, the content of the science that depends on them shifts as well. To give a simple example, up into the 19th century, probability was thought by many to be something primarily associated with gambling and, thus, could have little to offer the serious scientist who was interested in attaining Certain Truth. However, as it gradually came to be accepted that science rarely deals in certainties (even if that is its ultimate aim), but almost always in various likelihoods, and that the apparatus of probability theory could be used to measure the degree of uncertainty associated with particular claims, probability came to be accepted among scientific methods. It is difficult to even imagine modern science without the extensive use of probability and its associated statistical machinery.

This more tumultuous, but also more authentic history of science gives us scientists of the present day no more reason to assume that we, now, have finally “got it right” than pre-Einsteinian physicists had space and time “right” or than pre-Darwinian botanists and zoologists had species “right.” The one thing we can be fairly certain of is that there will be change, even in some of the things we believe to be foundational to the knowledge we have managed to so acquire through our most arduous efforts. This doesn’t mean that we have accomplished nothing of import. It means that we are a vital bridge from the less adequate understandings of the past to the (we hope) more adequate understandings of the future. That said, much of what we stand on is probably not bedrock.

Although I just presented an argument that favored antirealism (because I think that it is more widely misunderstood than realism), personally I have never been entirely comfortable on either side of the debate. I am fairly realist about some scientific objects (e.g., trees, mountains, stars) and I am fairly instrumentalist (antirealist) about others (e.g., the implicit memory system, the openness-to-experience personality trait, dissociative identity disorder). What does any of this have to do with unification? Psychologists, it seems to me, perhaps more than most scientists, cannot rely on the terms that they use to refer to “real” objects and processes. By “real” here I mean something like “permanent parts of the science.” If even so ancient and apparently straightforward a term as “planet” can be subject to debate, negotiation, and revision, as we saw recently in the dispute over the status of Pluto (and, by extension, of Eris, Ceres, Makemake, and Haumea), then what hope can there be, over the long haul, for things like intelligence, extraversion, racism, attachment, and autism (all of which have seen important shifts within our lifetimes)? Consider, by contrast, how quickly once seemingly indispensable terms like “hysteria” and “neurosis” slipped out of the disciplinary vocabulary entirely.

None of this bodes well for the possibility of a stable unification of the discipline. Principles of unification, whatever they might be, would seem to require, at minimum, a relatively stable set of concepts (and objects) in order to do their work. But so much in psychology remains highly malleable that it is hard to see how principles could be formed to rigorously capture them when their meanings might change by the time of the next textbook edition. Is it possible that principles of unification would play a significant role in stabilizing the network of psychological concepts and objects? Yes, it is possible, but now we are asking that a unification we do not yet understand do something we aren’t able to do for ourselves. It is, in effect, asking not just for unification, but for a unificatory miracle. That seems unreasonable.

Conclusions

Where have we been? First, I noted that scientific unification is an issue which has generated quite a bit of literature of the past few decades. I also noted that many of these efforts have been rather partisan, attempting to show that one “school of thought” or another can expand its boundaries to cover all of psychology, and that this is unlikely to succeed. I offered the suggestion that a successful unification would probably have to be couched in rather more neutral, and in much more rigorous (e.g., formal) terms.

Then I looked to psychology’s history to show that the discipline has been a mashup of different interests from the time its very first American journals appeared. It was not the result of a group of like-minded scientists coming together to solve problems they had in common. It was more the result of talented diplomats and administrators inviting relatively disparate groups of researchers to come together for the purposes of publication and institutional entrenchment. There may have been all kinds of good practical reasons for the discipline to proceed this way in order to “get off the ground,” but they probably do not constitute a promising intellectual basis for the future unification of the field. Then I suggested, in light of this history, that if unification comes to psychology, it may well be piecemeal—various portions of the discipline unifying and breaking away from the “mother” discipline until there are a number relatively independent “offspring” disciplines (and some remnant “orphan” problems) rather than a single unified psychology.

Finally, I took a short excursion into philosophy of science, trying to explain how antirealism is not, in the main, antiscience but, rather, an effort to come to terms with the history of science as it has actually proceeded over the past several centuries. One need not conclude that there are no “real objects out there” in order to see the power of the antirealist narrative. The point is, rather, that at any point in time, we have little reason to believe that we, of all the people in history, happened to have arrived at the uniquely correct concepts by use of the precisely attuned methods. It is much more likely that we are like the people who came immediately before us: We have descriptions of the world that are more or less adequate to our current finite set of observations, but we are always only a day way from observations that will render those descriptions inadequate. Even some of our most cherished current concepts and principles will be abandoned in favor of other new ones in our collective effort to get a more adequate grip on matters: better resolution of detail and wider scope. Psychology, at its current stage, seems particularly prone to fairly dramatic shifts in its concepts and methodological principles. We are a fairly heterogeneous group, ontologically and epistemologically, to begin with. What is more, the ontological bases of many of the “objects” we deal with are still fairly poorly understood. The odds that we are conceiving as a single kind of object a set of things that would be better handled as two or more kinds of objects, or that something we conceive of as a set of two or more kinds of things would be better handled as one, seems fairly high. There
is a bare possibility that a successful unification would simultaneously help us to stabilize our concepts; that we might see better and more consequentially what objects they refer to. Although possible, this seems to be more an exercise in wishing rather than in making any concrete progress toward a unification of the field.

Although unification in psychology may well seem to be a laudable goal, psychology may not have been configured historically in a way that will allow it to happen. We must, in a sense, let unification come to us, perhaps in a series of fragments, and then we will see what becomes of the larger psychology as a result, rather than explicitly attempt to pursue unification for the discipline as whole.

References


