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Divide et Impera! *William James's Pragmatist Tradition in the Philosophy of Science*

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I. AN UNSIGNED REVIEW

A year after William James published his *Principles of Psychology,* an unsigned review appeared in the *Nation.*[1] The review was caustic and rude, so much so that it created a stir among James's siblings. William and Henry had been regular contributors to the magazine since the inaugural issue in 1865, as had their late father.[2] So the 1891 repudiation of William's masterpiece was a public insult. In a bitter letter, Henry complained to his brother about "the way the *Nation* treats, & has mainly always treated *us.*" Henry reported that sister Alice was consumed by the "idiotic" article (*CWJ* 2.182) even though she had received a breast cancer diagnosis only weeks before.

William tried to reassure his siblings, calling the piece "a simply excentric production, probably read by no one." He declared that he "did n't care a single straw for the matter one way or the other, not even enough to find out who wrote it." The author of the review was probably "some old fogy," William grumbled (*CWJ* 2.185–86).

Though he never discovered the author's identity, the "old fogy" was none other than Charles Sanders Peirce.[3]

Peirce's review began with an analysis—and unqualified rejection—of the scientific method employed throughout the *Principles.*[4] Peirce was especially exercised by these remarks from James's preface:

> I have kept close to the point of view of natural science throughout this book. Every natural science assumes certain data uncritically, and declines to challenge the elements between which its own 'laws' obtain, and from which its own deductions are carried on. Psychology, the science of finite individual minds, assumes as its data (1) *thoughts and feelings,* and (2) *a physical world* in time and space with which they coexist and which (3) *they know.* Of course these data themselves are discussable; but the discussion of them (as of other elements) is called metaphysics and falls outside the province of this book. (*PP,* 6–7; cited at *CP* 8.59; my underline, italics original)

In many respects, Peirce admired James's work in psychology.[5] But he strongly disagreed with James's claim that science begins by accepting a basic set of data "uncritically."

For Peirce, a science may insulate no theory, no data, no assumption from empirical scrutiny. As he put it, scientists "are not banded together to repress any species of inquiry" (*CP* 8.60). He wrote:

> The notion that the natural sciences accept their data *uncritically* we hold to be a serious mistake. . . . The principle of the uncritical acceptance of data, to which Prof. James clings, practically amounts to a claim to a new kind of liberty of thought, which would make a complete rupture with accepted methods of psychology and of science in general. (*CP* 8.61)

If the *Principles'* preface is to be taken seriously, Jamesean psychologists are to begin by uncritically assuming that thoughts exist inside a material world, a world they come to know. For Peirce, in contrast, what gives scientific inquiry its unique power is precisely that scientists refuse to believe *anything*—including metaphysical assumptions—except on the basis of empirical evidence.[6]

Peirce's review raises a question of contemporary relevance—*May science rely on substantive,* a priori *presuppositions?* James apparently answered in the affirmative. As he elsewhere wrote, every science "must make a number of convenient assumptions and decline to be responsible for questions the human mind will continue to ask about them" (PPNS, 271). This answer is *prima facie* surprising. As Peirce rightly noted, it is hard to see how science could be a rational enterprise and yet rest on a basic set of assumptions accepted "uncritically." I will suggest that James's use of "uncritical" was needlessly provocative. He meant that such assumptions could not be supported by scientific experiment; any justification for these assumptions had to be *a priori.* But even this sanitized claim is surprising given the antipathy to a priori reasoning that we typically associate with the pragmatist tradition.[7]

Nonetheless, I will argue that my reading gives a more plausible account of James's actual view, and that his view gives a more attractive picture of science, than it first appears. James held that sciences bring their objects into sharper focus when

they carefully choose questions to set aside for neighboring disciplines to tackle. In place of empirical answers to such questions, sciences rely on "convenient assumptions" about the objects under investigation. I will claim that these assumptions act as a priori frameworks (in a sense to be made clear) and are to be accepted only insofar as they help support a division of cognitive labor among intellectual groups with kindred but distinct practical aims. To put the point mischievously, these a priori assumptions are, where rational, pragmatic.[8]

II. A DEBATE ABOUT NATURALISM: FRIEDMAN AND QUINE

The issue of whether science relies on a priori presuppositions has resurfaced in a more recent debate—over philosophical naturalism. One of my tasks in this paper is to articulate and defend James's conception of pragmatically a priori assumptions in science. But my other task is to show that James's work on a priori assumptions can help illuminate this contemporary debate over naturalism.

The debate I have in mind centers on Michael Friedman's critique of naturalized epistemology, especially the sort that takes off from Quine's "Two Dogmas" and "Epistemology Naturalized" (Quine 1951; Quine 1969). Friedman's criticisms are persuasive, in my view. Insofar as Quinean holism represents a pinnacle of the pragmatist tradition,[9] one might worry that pragmatism is now seriously undermined. I will argue that this worry is not well founded, for two reasons.

First, as far as Friedman's critique is concerned, the lynchpin feature of Quinean naturalism is its complete rejection of a priori knowledge. Now, this suspicion of the a priori characterizes the Peircean tradition in American philosophy that Quine inhabits.[10] But it is not characteristic of another strain of pragmatism that traces back, through C. I. Lewis and John Dewey, to James.[11] Second, James's thinking about a priori principles developed in the context of human rather than exact sciences. It is precisely here that Friedman's positive alternative to Quinean naturalism faces some apparent difficulties of its own. I will begin by identifying the difficulties. I'll then turn to the details of James's view and argue that it can be used to extend some of Friedman's anti-Quinean insights into the realm of human sciences.

James offers contemporary philosophers a form of naturalism that is sensitive to the sort of worries Friedman raises. With naturalists, James insists that philosophy and psychology are kindred; but with Friedman, James resists attempts to *absorb* philosophy *into* psychology or any other science. Philosophy has its own aims and its own methods. To dissolve its boundary with psychology is to ruin two good things.

Friedman draws on historical scholarship both to criticize Quine's account of scientific rationality and to provide a positive alternative.[12] For Friedman, Quinean naturalism comprises four theses. First, there can be no principled distinction

between analytic and synthetic statements.[13] Second, the collapse of the analytic/synthetic distinction is supposed to show that all statements in our scientific theories are synthetic and a posteriori, in the following sense. The conjunction of our scientific statements faces empirical evidence as a whole. Recalcitrant evidence, in principle, can count against any conjunct we choose, including even logical laws.

Third, scientific beliefs are organized into a vast web. Some beliefs sit closer to the center of the web than others. The more central beliefs are more costly to revise in the sense that such revision requires corrective adjustments in large portions of the rest of our belief web. Peripheral beliefs can be adjusted with less demand for correlative belief revision (Friedman 2001, 28, 32–35).

A fourth claim is supposed to be the upshot of the first three, for Quine: all parts of our scientific theories, from abstract mathematical structures to concrete empirical statements, are to be supported or disconfirmed by evidence of the same basic kind—that is by *empirical* evidence. There are to be no a priori elements at all in our scientific theories, for Quine (Friedman 2001, 28). Moreover, all statements in a language are subject to empirical disconfirmation, according to Quine; so there is no longer any specially secure knowledge that philosophy can employ for justifying natural science. Therefore, philosophy is to be absorbed into the sciences as a branch of psychology (Friedman 1997, 7–8).

Friedman takes this fourth claim, that all parts of scientific theories are ultimately responsible to empirical evidence, as something like a *reductio* on Quinean naturalism. For this claim does not square with what we know about the most admired moments in the history of science, Friedman argues. His main examples are that of Newton's mechanics and Einstein's theory of relativity. Consider the case of Newton.

Newton's law of universal gravitation cannot be formulated without two a priori structures already in place, according to Friedman. The first is the calculus, then a new form of mathematics dealing with infinite limits and instantaneous rates of change. The calculus made it possible for Newton to formulate physical notions like *force* with mathematical rigor. For example, the second law of motion defines force as the product of mass and acceleration. Acceleration is defined as the instantaneous rate of change in velocity; and velocity is defined as the instantaneous rate of change of position. But without the mathematics of the calculus, the notion of instantaneous change cannot be formulated precisely enough for the laws of motion to have real empirical content (Friedman 2001, 35). Friedman thus calls the mathematics of the calculus a "presupposition" or "condition" of even formulating the laws of motion.

In turn, the laws of motion play a different sort of constitutive role with respect to Newton's universal law of gravitation. Since Newton, the trend in physics has been toward ever more abstract representations of empirical laws. Newton himself was faced with the problem of how to specify which concrete, observable phenomena his radically abstruse laws are supposed to describe. Friedman here draws on Hans Reichenbach's notion of *coordinating principles* to characterize the parts of physical theories that coordinate abstractly formulated laws with observable mag-

nitudes. In Newtonian physics, the laws of motion play such a coordinating role by giving rules for comparing concrete measurements (for example, of planetary motion) with predictions made by the universal law of gravitation. The universal law is highly abstract, and the coordinating principles are needed to generate predictions about what will happen not in some theoretical realm, but in the world of our actual experience (Friedman 2001, 76–77).

In short, Friedman thinks that scientific theories employ two sorts of a priori structures—*coordinating principles,* like the laws of motion, that generate empirically meaningful predictions from abstractly formulated laws; and *mathematical formalisms,* like the calculus, that give quantitative precision to other elements of the theory. So Friedman follows Carnap and Reichenbach in holding that science indeed employs constitutively a priori principles.

Now Quine, by denying a distinction between analytic and synthetic statements, is unable to capture the asymmetric way[14] in which these various parts of physical theories confront empirical evidence. This is one of Friedman's major complaints against Quinean naturalism. It is not appropriate to describe the calculus together with Newton's laws of motion and the universal law of gravitation as three conjuncts all facing empirical evidence in the same fashion, with some conjuncts more deeply "entrenched" than others, Friedman argues. This is because one can drop or revise the laws of motion without having any effect on the meanings of statements in the calculus, for example—but the reverse is not true. Without the calculus, the laws of motion cannot be precisely formulated. And without the laws of motion, the universal law of gravitation cannot be given empirical meaning (Friedman 2001, 35–36). In other words, the properly empirical parts of Newton's theory *presuppose* the a priori parts.

It is worth getting clearer on what is meant by "presupposed." For Friedman, a sentence P is presupposed by (or "constitutes") a sentence Q just in case Q is meaningless unless P is true (Friedman 2001, 74). The classic example concerns the sentence "The present king of France is bald." This sentence presupposes that there exists exactly one present king of France. Since, in fact, there exists *no* present king of France, we do not know how to assign a truth value either to this statement or to its negation[15]—in other words, the statement is meaningless, or at least misplaced. In this sense, "There exists exactly one present king of France" is a constitutive condition of the sentence "The present king of France is bald." *Contra* Quine, Friedman argues that there are parts of scientific theories that play a constitutive role with respect to other parts.

Unlike Kant, Friedman argues that these constitutive principles play a *dynamical* or *relativized* role—they change along with new developments in the exact sciences. For instance, Einstein's theory of relativity presupposes the Riemannian theory of manifolds rather than the Euclidean geometry on which Newton's physics relies (Friedman 2001, 37–38).

Friedman does allow that in special cases we can put empirical pressure even on the constitutive, a priori parts of our theories. But we cannot see such tests as

logically rigorous. If we do revise the a priori component of our theory, that revision will be "purely pragmatic" (Friedman 2001, 71 ff., 83–92).

Now what is the significance of all this?

Friedman attaches far-reaching consequences to his notion of constitutive principles. He thinks that as a matter of historical fact, such principles are the very hallmark of science itself:

> Quine is correct that pure formal logic is insufficient to characterize the relativized and dynamical, yet still constitutive notion of a priori principles Carnap was aiming at. . . . Although Carnap may have failed in giving a precise logical characterization or explication of such principles, it does not follow that the *phenomenon* he was attempting to characterize does not exist. On the contrary, everything we know about the history of science, I want to suggest, indicates that precisely this phenomenon is an absolutely fundamental feature of science as we know it—and a fundamental feature, in particular of the great scientific revolutions that have eventually led, in our time, to the Carnap-Quine debate. (Friedman 2001, 41; my underline)

Friedman grants that Quine's attack on the analytic/synthetic distinction did show Carnap's *formal* account of constitutive principles to be a failure. But the lesson is only that "pure formal logic" is not enough to characterize constitutive scientific principles, for Friedman. He maintains that even if Carnap's formal characterization failed, these principles are "an absolutely fundamental feature of science as we know it."[16]

Now here is the difficulty Friedman faces. On his account, physicists like Newton and Einstein need a priori, constitutive principles to give quantitative precision to their mathematical models, and then to coordinate those abstruse models with experience. But in the special sciences, many theories are cast in *natural language,* not in recondite formalisms that need to be given precise, empirical content.

For instance, Darwin's theory of natural selection is formulated in plain English, not in any formal language. So no special principles should be needed to coordinate the theory of natural selection with experience—no more than one needs special principles to coordinate statements of everyday English with experience. And further, the theory of natural selection does not make fine-grained, quantitative predictions. So there is also no need for a priori formalisms that would give Darwin's theoretical terms mathematical precision.[17]

We presumably want to maintain that Darwinian biology is a genuine science, even though it need employ neither coordinating principles nor a recondite mathematical framework. So those sympathetic with Friedman face a dilemma—either give up the claim that a priori principles are a "fundamental feature of science as we know it," or produce a broader conception of a priori principles that can accommodate the special sciences.[18]

I want to suggest that there are indeed a priori elements in special-scientific theories. But these elements are something other than coordinating principles or

mathematical frameworks. To make my case, I will turn back to the story of James and early empirical psychology.

III. BACK AT THE RANCH, A STRUGGLE OVER PSYCHOLOGY

As we have seen, Friedman's work on constitutive principles is designed to make sense of mature, exact sciences—particularly mathematical physics since Newton. Indeed, a rich literature has grown up around this notion of constitutive principles, and virtually all of it focuses on physics (e.g. DiSalle 2002; Friedman 1997; Friedman 1999; Friedman 2001; Richardson 2002b; Stump 2003).

We should grant that Friedman's examples from physics seriously undercut Quinean theories of scientific rationality. If naturalists take themselves to have an account of science in general, that account should surely fit the most admired examples from the history of physics.

But what should we say about Friedman's positive project? He claims that constitutive a priori principles are hallmarks of "science as we know it." To what extent can his insights be applied *outside* the context of mathematical physics?

On Friedman's account, physicists use a priori principles to address problems of mathematical precision and empirical testability. When one turns to the case of early psychology, one does find James introducing an a priori element in his science—though to address very different problems.[19]

In the late nineteenth century, metaphysicians such as T. H. Green rejected psychology's scientific status. They challenged the very idea that minds are appropriate objects of scientific investigation. James held that no mass of empirical results could quell this controversy.

His solution involved crafting an ingenious *definition*—a definition of psychology's proper object. To a first approximation, he proposed that mental scientists treat experience as given in a continuous "stream of thought." He defined the stream of thought as having five basic properties that the psychologist must regard as "ultimate facts," facts not subject to further psychological explanation. Any further questions about these properties were to be relegated to metaphysics.

The stream thesis (as I will call it) was constitutively a priori, though not in the sense that it conferred either precision or meaningfulness on James's theories. Instead, the thesis helped confer *scientific legitimacy* on the theories of the *Principles,* I will argue. It did this by stipulating a boundary between two groups—metaphysicians and psychologists—who had been engaged in a turf war over the study of mind. On James's view, the thesis was *rational* to the extent that the boundary actually helped establish and stabilize a cognitive division of labor.

The stream thesis is an example of what I will call an "ontological agreement" —an agreed-upon definition of an intellectual endeavor's proper object. In general, such definitions are a priori in the sense that they are freely stipulated; and they are

constitutive of the division of cognitive labor that specialized investigation requires. I will argue that ontological agreements are important a priori elements at least in special sciences like psychology.

One last remark is in order before looking at James's ontological agreements in more detail. Perhaps we should admit that the criticisms just reviewed undercut not only Quine, but also the Peircean thread of pragmatism that Quine developed.[20] But Friedman's criticism turns on a divisive issue inside the pragmatist tradition—viz., on whether there are elements of scientific theories that can be insulated from empirical disconfirmation. Thus, I want to suggest that attention to James's stream thesis not only helps us think about a priori elements in the special sciences—it also helps us pull out a thread of the pragmatist tradition that does not fall prey to these more recent criticisms.

James and Peirce were lifelong friends and philosophical interlocutors. But James's reflections on scientific assumptions emerged in the context of a fight from which his old friend was largely absent. In the 1870s, a controversy had flared over whether psychology could ever be a legitimate science. The debate was largely carried out in *British* intellectual societies and academic journals, among some of James's most important international friends and colleagues. James's use of a priori, scientific principles emerged as a strategy to help quell this controversy. So as a preliminary to analyzing the Jamesean a priori, I offer some historical context about this larger fight over psychology.

By the late nineteenth century, German research universities had been flourishing for a century, producing philosophy of a professional caliber exemplified by Kant and Hegel. British philosophy had grown something of an inferiority complex by comparison. Consider this representative passage from David Masson, a British intellectual historian and literary critic who influenced James.[21]

> The Germans, in particular, have long pitied us on this account. It is more than forty years since one of their greatest thinkers [Hegel] publicly denounced us by pointing out that England was the only country in Europe where the word Philosophy had been synonymous with natural science, where the barometer and thermometer were spoken of as 'philosophical instruments,' and where a so-called *Philosophical Journal* treated of agriculture, housekeeping, cookery, and the construction of fire-places. (Masson 1866, 2)

That British philosophy remained a crass admixture of popular science and practical wisdom was a common lament, during the late-Victorian era.

The crisis of confidence led to the establishment of *Mind,* a journal created to foster *professional* scholarship exclusively. Its first issue in 1876 contained a telling introduction by founding editor George Croom Robertson. He bemoaned what he saw as English philosophy's amateurishness. Its leading lights—Bacon, Hobbes, Locke, Berkeley, Hume, Hartley, and the Mills—"did their philosophical work at the beginning or at the end or in the pauses of lives otherwise active, and addressed for

the most part the common intelligence of their time," Robertson wrote. The resulting "informality of their thought" helped these figures become famous, Robertson continued, and perhaps encouraged a widespread, amateur interest in philosophy among the English public. But he complained that there was little sustained interest in philosophy by *professionals,* interest "like that felt in mathematics or physics or chemistry by a multitude of active workers and a multitude of trained and continuous learners" (Robertson 1876, 1–2).

For Robertson, the way to revive British philosophy was not just to make it more professional, but to make it more "scientific"—more like "mathematics or physics or chemistry." In particular, he saw the fledgling science of psychology as philosophy's ticket to professional rigor. Philosophy was to become more *like* a science by hitching its fortunes to one:

> With reference to general Philosophy or Metaphysic proper, psychology may be viewed as a kind of common ground whereon thinkers of widely different schools may meet, and, if they do not forthwith agree, may at least have their differences plainly formulated, as a first step towards any agreement that is possible. The new journal [*viz.*, *Mind*] should thus, while promoting psychological science, help also to compose that secular strife which scientific inquirers as well as popular writers are never weary of representing as the opprobrium of philosophy. (Robertson 1876, 5)

Psychology was not just philosophy's intellectual neighbor, for Robertson. He thought psychology could provide a neutral set of facts that even warring philosophical schools could agree upon. This common ground could provide a basis for transforming philosophy's characteristic "secular strife," now lampooned by scientists, into more productive disagreement, Robertson held. In other words, he saw psychology as having the potential to provide a foundation for something like *scientific progress* in philosophy.

However, not everybody agreed that the way to make British philosophy more rigorous was to make it more scientific, Robertson acknowledged. And besides, whether psychology even counted as a legitimate science was sharply contested. So Robertson's plan was to devote the journal every bit as much to nourishing psychological research as to publishing philosophical writing. He wrote:

> Now, if there were a journal that set itself to record all advances in psychology, and gave encouragement to special researches by its readiness to publish them, the uncertainty hanging over the subject could hardly fail to be dispelled. Either psychology would in time pass with general consent into the company of the sciences, or the hollowness of its pretensions would be plainly revealed. Nothing less, in fact, is aimed at in the publication of Mind than to procure a decision of this question as to the scientific standing of psychology. (Robertson 1876, 3)

Mind's emphasis on psychology is no longer evident today. But the journal was actually created by "mental scientists"—its first financier was Alexander Bain—with the avowed aim of helping to secure psychology's status as a genuine science.[22] The

journal's subtitle was no accident: "A Quarterly Review of Psychology and Philosophy." In fact, Robertson indicated that the *order* in which "Psychology and Philosophy" appear in the journal's subtitle was very much meant to suggest that mental science is of foundational significance for philosophy (Robertson 1883, 1).

When Robertson wrote about the "uncertainty" hanging over psychology, he must have had in mind a set of then-ongoing attacks.[23] Neo-Kantian idealists like Edward Caird, Francis Herbert Bradley, and especially Thomas Hill Green developed pointed criticisms of psychology during the 1870s and 1880s.[24] They argued that minds are not appropriate objects of scientific investigation.[25] They especially rejected the notion that a would-be science of mind could provide any results relevant to philosophy.[26]

The opening salvo came from Green. In 1874, he coedited a compendium of Hume's philosophical works. A centerpiece of the edition was Green's own 371-page *Introduction* to the *Treatise of Human Nature.* The *Introduction* was a lengthy analysis not just of Hume, but of Locke and Berkeley as well. Written in a Kantian spirit, the *Introduction* would become a founding document of the entire British idealist movement. Though the work is now remembered primarily as an investigation in the history of epistemology, it was designed to undercut late-nineteenth-century empirical psychology.[27]

Green agreed that British philosophy suffered from terminal amateurishness.[28] But he did not think the solution was to replace speculative metaphysics with a *scientific* study of the mind, as psychologists seemed to propose.[29] He wrote:

> The question really at issue is not between two co-ordinate sciences [viz., metaphysics and physiological psychology], as if a theory of the human body were claiming also to be a theory of the human soul, and the theory of the soul were resisting the aggression. The question is, whether the conceptions which all the departmental sciences alike presuppose shall have an account given of them or no. For dispensing with such an account altogether (life being short) there is much to be said, if only men would or could dispense with it; but the physiologist, when he claims that his science should supersede metaphysic, is not dispensing with it, but rendering it in a preposterous way. He accounts for the formal conceptions in question, in other words for thought as it is common to all the sciences, as sequent upon the antecedent facts which his science ascertains—the facts of the animal organisation. But these conceptions—the relations of cause and effect, &c.—are necessary to constitute the facts. They are not an *ex post facto* interpretation of them, but an interpretation without which there would be no ascertainable facts at all. (*GWR*, I.164–65)

Psychologists pretend to dispense with speculative metaphysics altogether, according to Green, and instead offer their work as a kind of scientific account of scientific knowledge.[30] But any would-be science of scientific knowledge must leave out an adequate account of the "formal conceptions"—he had in mind the familiar run of Kantian categories—employed in the sciences, Green argued. This is because *qua* science, psychology must *use* the very concepts it pretends to explain. Therefore,

Green argued, psychology can never provide the sort of meta-criticism of scientific knowledge that metaphysics provides.

Elsewhere, Green made a similar point using language that should look familiar. Psychology cannot avoid being a theory of knowledge, and a theory of knowledge cannot avoid being a theory of the mind's cognitive objects, according to Green. So when psychology tries to purge itself of metaphysics, it "is unaware of the *assumptions* which it *uncritically* makes. . . . It is not really, nor can it be, the case that our psychology has cleared itself of metaphysics." (*GWR*, I.375, my italics). Again, Green insists that metaphysics is always prior to psychology, so the latter cannot replace the former.

For Green, psychology's problems actually run even deeper because it purports to study not just objects existing in space and time, like other sciences. It purports to study our *representations* of objects in space and time. Following Kant, Green argued that such representations are governed by synthetic, necessary principles.

What necessary principles does Green think are required before we can have representations? Spatial representations, for instance, presuppose an antecedent grasp of *Euclidean geometry,* allegedly. Since we can't represent objects as existing in anything *but* Euclidean space, Green held, we should regard Euclidean principles as placing a necessary constraint on the structure of experience.

But Green argued that necessary, synthetic truths can't be explained by appeal to any contingent facts about our actual physiology or psychology. Only transcendental argument can explain why our spatial experiences have their peculiar, necessary structure, he thought. Because psychology purports to be an empirical science, it cannot engage in transcendental argument, and thus can never give an ultimate account of its object, namely sensory experience.[31]

We need not get into the details of Green's specific arguments concerning spatial representation here.[32] What we need to pull out of this short discussion is that Green thinks empirical investigation cannot yield a genuine account of the mind. Only transcendental metaphysics can accomplish this feat.

Before moving on to James, note Green's language. We just saw Green chastising psychologists for employing *uncritical assumptions.* Elsewhere he criticized psychologists for treating sensory experiences as "ultimate data" rather than as phenomena to be given a deeper, transcendental explanation (*GWR*, I.384–85). James echoed this and other characteristically idealist language (*see PP,* 6, and PPNS, 274, for example). I will argue that James was firmly on Robertson's side in this debate, not Green's; but if this is so, then why did James use characteristically idealist rhetoric about science using "uncritical assumptions" and so forth? I will return to this point.

James's deserved reputation as an American intellectual icon has tended to obscure the fact that during the years he was composing the *Principles of Psychology* (1878–1890) he was heavily engaged with British philosophy. As a measure of the importance of British philosophy to James's early work, consider the following.

During this period, the British journal *Mind* published a staggering 47 percent of James's substantive output (305 total pages).[33] The American *Journal of Speculative Philosophy* came in a distant second place with only 17 percent of James's substantive output (117 pages).[34] In other words, while James was working on the *Principles* he actually published the bulk of his most considered work in an English journal—in Robertson's *Mind.*[35]

This is significant because in the 1880s, *Mind* was the central forum for the debates over empirical psychology we have been exploring. *Mind* became the main locus for these debates in 1882 when Green published a long essay in the journal attacking psychology from an idealist perspective ("Can There Be a Natural Science of Man?" *see* note 30, below). Following Green's lead, other idealists then began appearing regularly in the journal, and for the rest of the decade *Mind* was the central battlefield of this fight. Some of James's most important essays from the period—"On Some Omissions of Introspective Psychology," "On the Function of Cognition," and "The Perception of Space," for example (James 1884; James 1885; James 1887)—were published during the height of the controversy, and should be read against that background.

These essays were not produced in some far-off insulated corner of the American academy. James actually spent extended stretches of the 1880s in England, directly engaging with key players in the British debates over psychology. During an 1882–1883 trip, for instance, James joined Robertson's own philosophical club, a London group calling themselves the "Scratch Eight"—James became their ninth (*RBP*, I.594–96). The club consisted of prolific contributors to *Mind,* including Shadworth Hodgson, James Sully, Carveth Read, Leslie Stephen, and of course Robertson, the journal's editor (*see CWJ,* V.332).[36] Perry writes that the effects of this particular visit on James's philosophical thinking were "the most important in all James's European adventures" (*RBP*, I, 586). And he says the Scratch Eight was "the nucleus of James's 'philosophic society'"—a very strong claim indeed (*RBP*, I.596).

Further evidence that the 1880s debate over psychology was prominent in James's thinking comes from letters to and from *Mind*'s editor during that decade. Their correspondence often alludes to two sides that had been drawn in a battle between idealists and those inclined toward mental science (*e.g.*, *CWJ,* V.38, V.182, V.226, V.484; VI.62, VI.262–63, VI.288, VI.429).

James's published work from this period shows a similar theme. From his reflections on cognition to his work on introspection, one finds James either defending a scientific approach to the mind or counterpunching idealists. An example in which James levies a direct attack on idealism comes from his 1882 essay, "On Some Hegelisms." As James described it in a letter, the piece targeted "points which have been made popular by the teachings of Green, the Cairds and Palmer."[37] It opened with the claim that "Hegelism" had become "one of the most potent influences of the time." Hegelians had become so zealous that

> if perchance we essay to do some small bit of psychological detail-work for ourselves, it is lucky if someone does not trip us up at every step by

> reminding us that we forget to do homage to the Transcendental Ego which is presupposed in all the words we use. . . . The transcendental-ego-business is a good deal like interrupting a geographer at his work by telling him every five minutes that he forgets to talk about Space, which is nevertheless presupposed in all the distances and latitudes and longitudes he is discussing. . . . Hegel's philosophy mingles mountain-loads of corruption with its scanty merits, and must, now that it has become quasi official, make ready to defend itself as well as to attack others. (James 1882, 186)

Those like James who wanted to practice empirical psychology found they were accosted at every turn. Hegelians like Green objected that the mind had properties that transcend time and space, and as such could not be studied empirically. James sought to respond.

And it is not just that the British debate was prominent in James's thinking during the era. James himself came to be a prominent player in the British debates. Robertson had a great deal to do with this, as he saw James as a worthy respondent to idealists. The editor often solicited the latest from James's pen.[38]

James did not disappoint, contributing searing attacks on British idealists throughout the 1880s. For instance, James went at the Hegelians again in the winter of '83–'84, submitting "On Some Omissions of Introspective Psychology," another attack on Green et al. (James 1884). The following December, James was back in England, and attacked Green again in a talk before the newly formed Aristotelian Society. *Mind* published the piece in January under the title, "On the Function of Cognition." In the latter piece, like in "On Some Hegelisms," James continued to show interest in the proper role of a priori "assumptions" in science (James 1885, 29–30).

One gets a quick and colorful sense of how intense the debate would become from a letter James wrote to Robertson on August 13, 1885:

> Why don't you have a special "neo-hegelian department" in Mind, like the "Children's department" or the "Agricultural department" in our newspapers, which educated readers skip? (*CWJ,* VI.62)

Again, Peirce was not party to the British debate between idealists and psychologists. But it was in the context of this debate that James refined his views about the role of a priori assumptions in science. I contend that one cannot understand James's conception of scientific assumptions—the conception Peirce criticized—unless one sees how it grew out of this British debate we have been discussing. I now turn to the substance of James's view.

IV. PSYCHOLOGY AS NATURAL SCIENCE

Given the heat this debate generated, it could not have been a surprise that the *Principles* was published to some controversy in 1890. Critics often debated whether James had really succeeded in elevating psychology to the status of genuine natural

science. As in Peirce's review, a common focus of such discussion was James's choice of how to separate phenomena ripe for scientific explanation from phenomena psychologists must simply accept as "irreducible data."[39]

One such review came from the Yale psychologist George Trumbull Ladd. Ladd shared James's goal of transforming psychology into a genuine science; but unlike James, Ladd insisted that psychological science should preserve the notion of a *soul* "to which, as subject, the thoughts and feelings belong" (Ladd 1892, 39). In contrast, the *Principles* explicitly proposed that psychologists leave the idea of a soul to metaphysicians; and Ladd thus complained that James's "conception of psychology as a natural science results in a most astonishing abbreviation of the rights of the psychologist" (Ladd 1892, 28). James responded in "A Plea for Psychology as a 'Natural Science'" (James 1892), and here one finds James's most general account of scientific assumptions.

Although "A Plea for Psychology" was prompted by Ladd's review, the position James sketched is best understood in connection with the British debate we have been discussing, in my view.[40] This essay expanded on a position James had developed during the 1880s. It is not surprising, therefore, that the only person James quoted in "A Plea for Psychology" other than Ladd was the Scottish idealist Andrew Seth Pringle-Pattison (PPNS, 273–74), an important participant in the *Mind* debates.

"A Plea for Psychology" opened with a characterization of natural science that James had developed as early as 1880 (see James 1880). Science is "a mere fragment of truth broken out from the whole mass of it for the sake of practical effectiveness exclusively. *Divide et impera*" (PPNS, 271). I take my title from that last phrase as it was something of a slogan for James.[41]

So for psychology to become a natural science it, too, had to break off a "fragment of truth" for practical purposes. Echoing the *Principles* passage to which Peirce objected (*see above*, p. 130), James wrote:

> Every special science, in order to get at its own particulars at all, must make a number of convenient assumptions and decline to be responsible for questions which the human mind will continue to ask about them. Thus physics assumes a material world, but never tries to show how our experience of such a world is 'possible.' It assumes the interaction of bodies, and the completion by them of continuous changes, without pretending to know how such results can be. Between the *things thus assumed,* now, the various sciences find definite 'laws' of sequence. (PPNS, 271, my italics)

As Ladd had complained, James was indeed guilty of "abbreviat[ing] the rights of the psychologist." But James's point was that psychology can only *become* a natural science if it takes *less* responsibility for answering abstract, metaphysical questions about the mind. Special sciences must shirk some explanatory responsibility according to James—they must "decline to be responsible for" certain questions. Instead, they must start inquiry with a set of "convenient assumptions" that are not candidates for explanation.

It is important to look closely at James's examples of such assumptions in this passage. They are all *metaphysically loaded,* I want to claim, in the following sense. The assumptions are *metaphysical* in that they involve nagging questions metaphysicians actually debate. For instance, metaphysicians hotly pursue skeptical questions about the material world; but physicists simply assume that there are good answers to those questions—again, to those particular questions philosophers are actually debating.

The assumptions are *loaded* in the sense that the scientist "declines to be responsible" for them. The physicist does not try to give evidence that there exists an external world. She simply assumes that skepticism is false, as far as her purposes go, and gets on with the task at hand—"practical prediction and control" of nature (PPNS, 272).

Notice that for James, a science *needs* such convenient assumptions if it is to "get at its own particulars at all." According to the passage just quoted, we make assumptions about the objects of our science (matter, mental states, etc.), and the "things thus assumed" then become the subjects of natural laws.

This passage raises some important questions. First, what are to be psychology's "things thus assumed"? In other words, what are psychology's scientific objects? And second, how are "convenient assumptions" involved in their construction?

I will take up the second question in section 5, below. The answer to the first question is more straightforward: the proper object of psychology is to be what James called the "mental state." He wrote:

> Cannot both ["philosophers and biologists"] forego ulterior inquiries, and agree that, provisionally at least, the mental state shall be the ultimate datum so far as psychology cares to go? (PPNS, 274)

A page later, James acknowledged that the *Principles* sometimes actively engaged in metaphysical disputes, despite its scientific aspirations. He offered the following explanation:

> but these unfortunate episodes are for the most part incidental to the attempt to get the undivided 'mental state' once for all accepted by my colleagues as the fundamental datum for their science. To have proposed such a useful basis for united action in psychology is in my own eyes the chief originality and service of the book. (PPNS, 275)

This is a remarkable claim. In the entire, monumental expanse of the *Principles* James hoped above all to have established the mental state as the proper object of mental-scientific investigation.

Notice the way he put the point. He hoped to have formed *a consensus between two different groups* that psychology's proper object is the mental state. The two groups were the scientists and the philosophers fighting over psychology (in *Mind* and elsewhere). James characterized one group in the fight as biologists, naturalists, doctors, physiologists, and psychical researchers, who "already form a band of workers" producing practical results. The other group was more philosophically inclined.

> The actual existence of two utterly distinct types of mind, with their distinct needs, both of them having legitimate business to transact with psychology, must then be recognized; and the only question there can be is the practical one of *how to distribute the labor so as to waste it least and get the most efficient results.* For my part, I yield to no man in my expectations of what general philosophy will some day do in helping us to rational conceptions of the world. But when I look abroad and see how almost all the fresh life that has come into psychology of recent years has come from the biologists, doctors, and psychical researchers, I feel as if their impulse to constitute the science in their own way, as a branch of biology, were an unsafe one to thwart; and that wisdom lies, not in forcing the consideration of the more metaphysical aspects of human consciousness upon them, but, on the contrary, in carefully rescuing these aspects from their hands, and handing them over to those of the specialists in philosophy, where the metaphysical aspects of physics are already allowed to belong. If there could be, after sufficient ventilation of the subject, a generally expressed consent as to the kind of problems in psychology that were metaphysical and the kind that were analogous to those of the natural sciences, and if the word 'psychology' could then be restricted so as to cover as much as possible the latter and not the former problems, a psychology so understood might be safely handed over to the keeping of the men of facts, of the laboratory workers and biologists. (PPNS, 272–73, my italics)

The controversy over psychology hampered scientific progress, in James's view (James 1882, 186). As a way to overcome this obstacle to progress, he sought to establish a cognitive division of labor so that the scientists could produce results of practical benefit and the philosophers could have free reign over properly metaphysical questions.[42] The *way* a division of labor should be established, James held, is through the judicious use of convenient assumptions in science. These assumptions specify where psychological analysis ends and philosophical analysis picks up.

In my view, James held that science is characterized by a thoroughgoing commitment to *cooperation*—cooperation inside groups of specialized researchers and cooperation across their boundaries as well. Indeed, he seems to think that a refined division of cognitive labor is a hallmark of science itself.

This is relevant to our discussion of Friedman because a stable division of labor cannot be established without some *theoretical* elements already in place, according to James. In particular, James held that a stable division of labor requires that a science establish broadly acceptable, basic assumptions about the object under investigation. These convenient assumptions act as theory-level tools for demarcating one scientific discipline from neighboring specialized fields.

I use the phrase "ontological agreements" to refer to what James called "convenient assumptions." My terminology is meant to emphasize that these theoretical elements play two distinct roles in science—they help establish an *ontology,* and they act as tools for stabilizing a social *agreement* about how to divide intellectual labor.

We will get a clearer idea of the relation between these two roles by looking more closely at the actual way James used convenient assumptions in the *Principles.*

In the next section, I will claim that the ontological role of these assumptions is actually parasitic on the demarcation role in James's psychology. If I am right, then James recommends that we *divide and conquer* nature by dividing the cognitive labor of inquiry. This is a surprising view because it maintains that the ontology of a given science partly depends on the social structure of inquiry.

One final word is in order about the slogan *divide and conquer.* Although "A Plea for Psychology" was published two years after the 1890 *Principles,* James wrote about the division of labor in science as early as 1880 and used the expression "*Divide et impera*" in that context as well. I have in mind "Great Men and Their Environment,"[43] where he used the phrase in connection with Darwin. James wrote that Darwin's "triumphant originality" was to have separated the causes of what we now call "phenotypic variation" from the causes that either preserve or destroy such variation. Darwin grouped the former under the heading "spontaneous variation" and, "relegating them to a physiological cycle which he forthwith agreed to ignore altogether," Darwin instead sought to explain only the causes of phenotypic preservation and destruction—that is, in his theory of natural and sexual selection (James 1897/1979, 167). Notice James's language—Darwin "agreed to ignore" physiological questions about the causes of phenotypic variation, even though that variation is likely the "fixed outcome of natural law" (James 1897/1979, 168). Such physiological questions about variation are presumably to be left to physiologists so that the evolutionary biologist can more narrowly circumscribe his subject matter. James then applauded Darwin with my titular phrase, "*Divide et impera!*" (James 1897/1979, 167n).

V. CONVENIENT ASSUMPTIONS IN THE *PRINCIPLES*

Above (on p. 135), I used "the stream thesis" to name James's claim that experience is fundamentally continuous. I suggested that this thesis played an a priori, constitutive role in James's psychology. Attentive readers will have guessed what I am driving at. I want to claim that the stream thesis is a prime example of those convenient assumptions we have been discussing.

Remember that convenient assumptions help define the proper object of a science, for James. And psychology's proper object was to be the *mental state.* I will begin by arguing that the stream thesis is precisely where one finds the *Principles*' definition of the "mental state." Then, I will show how James's definition helps establish a division of labor between psychologists and philosophers. It does this by defining psychology's proper domain in a way that addresses idealist concerns about the very idea of mental science. Finally, I will explain the sense in which the stream thesis played a constitutively a priori role in James's science, and I will draw some connections with Friedman's account of physics.

5.1 The Stream Thesis as Definition of "Mental State"

In the 1892 essay I reviewed above, James claimed that the *Principles* aimed to establish what he called "mental states" as the fundamental objects of psychological investigation. When one turns to the *Principles* itself, one finds James explicitly introducing two synonyms for "mental state": "thought" and "feeling." He acknowledged he was using these words "in a wider sense than usual," but emphasized that he meant "thought" and "feeling" to signify "the mental state at large, irrespective of their kind" (*PP,* 186). In fact, James more commonly used the expressions "feeling" and "thought" than "mental state." So if we want to learn about the assumptions packed into psychology's proper object—the mental state—we must also look at what James wrote about "feelings" and "thoughts."

James's basic definition of "feeling" and "thought" came in the *Principles* chapter entitled, "The Stream of Thought." He began that chapter by returning to the theme of uncritical assumptions:

> The only thing which psychology has a right to postulate at the outset is the fact of thinking itself, and that must first be taken up and analyzed. . . .
>
> *The first fact for us, then, as psychologists, is that thinking of some sort goes on.* I use the word thinking, in accordance with what was said on p. 186 [where "thought" was defined as synonymous with "feeling" and "mental state"], for every form of consciousness indiscriminately. (*PP,* 219–20)

James claimed that psychology's first assumption concerned the existence of thought, where "thought" is understood as synonymous with "mental state." This squares with his remarks from the preface. But here, James provided further details about which specific assumptions *about* mental states the psychologist was to accept at the outset.

Not only should the psychologist assume that thought exists, James now claimed, but she should assume that thought has five basic properties:

> We notice immediately five important characters in the process [of thinking], of which it shall be the duty of the present chapter to treat in a general way:
>
> 1) Every thought tends to be part of a personal consciousness.
> 2) Within each personal consciousness thought is always changing.
> 3) Within each personal consciousness thought is sensibly continuous.
> 4) It always appears to deal with objects independent of itself.
> 5) It is interested in some parts of these objects to the exclusion of others, and welcomes or rejects—*chooses* from among them, in a word—all the while.

I take it that these postulates are meant to be basic assumptions in the sense first depicted in the preface to the *Principles*—the psychologist is to *decline responsibility* for explaining each postulate. The "Stream of Thought" chapter went on to describe each of these postulates in more detail, in some cases providing evidence

that thought really has the ascribed properties. But the properties are described, not explained in terms of some deeper or more basic psychological fact.

Indeed, James later referred to the stream of thought as the "ultimate fact for psychology" (*PP,* 341)—and he used similar language in connection with *mental states,* which he described as the "ultimate datum" and the "fundamental datum" for psychology (PPNS, 274, 275). Thus, it seems that the five postulates of the "Stream of Thought" chapter provide a detailed characterization of psychology's ultimate fact, the stream of thought (or mental state).

An "ultimate fact" in psychology appears to be a fact such that, when we try to explain it, we are doing metaphysics, not science. Recall that in the *Principles'* preface, James wrote that psychology's ultimate data "themselves are discussable; but the discussion of them (as of other elements) is called metaphysics and falls outside the province of this book" (*PP,* 6). "Ultimate facts," such as the facts described by the stream thesis, specify a legitimate stopping-point for scientific analysis. When we analyze a phenomenon into "ultimate facts," we have taken the analysis as far as possible without becoming metaphysical. Thus, we may postulate perceptual laws that ascribe some particular relation between swaths of the stream of thought. But we cannot (as late-Victorian psychologists) ask why there is a stream of thought, or why it always has the five basic features cited above.

So for a more detailed look at how James actually used his convenient assumptions to define psychology's basic object and thereby demarcate psychology from philosophy, one should investigate the basic postulates of the stream thesis. For ease of exposition, I will focus on just one postulate.

5.2 The Stream Thesis as a Tool for Dividing Labor

I will now show how the stream thesis's *first* postulate contributes to a definition of psychology's proper object, a definition that helps demarcate mental science from philosophy. The postulate helps with demarcation by addressing an idealist criticism of psychology. I will begin by showing that the postulate uses language that evokes one such criticism in particular: that psychology cannot explain why experiences are always bound together in one conscious life (or another). I will then analyze James's response. He defined the mental state, psychology's proper object, as having this boundedness as an ultimate feature that could not be explained from inside science. In other words, he relegated this question to metaphysics.

The first postulate of the stream thesis states that all thought is part of some personal consciousness. James elaborated: "The universal conscious fact is not 'feelings and thoughts exist,' but 'I think' and 'I feel'" (*PP,* 221). Now, James often used the phrase "I think" to evoke Kant's notion of a transcendental self as well as Green's similar notion of a transcendental ego. For instance, James called the transcendental unity of apperception—an important feature of Kant's transcendental self—an attempt to explain the fact that "The awareness that *I think* is . . . implied in all experience" (*PP,* 342).

Other evidence that James used the phrase "I think" in connection with Kant can be found in unpublished notes on the *Prolegomena to Any Future Metaphysics.* Some of the notes deal with Victor Cousin's criticisms of Kant, and in this context James wrote:

> "*Transcendental* unity of apperception" wh. comprehends all other acts of union wh. the understanding performs, *including* those of matter given by the "Inner sense.["] The "conscience" Cousin says Kant makes empiric is merely the unity of the different intuitions of the Inner sense. The transcendental unity aforesaid ("I think") dominates these as well as all other intuitions.[44]

Notice that last sentence. James introduced "I think" as a shorthand for "the transcendental unity aforesaid." The word "aforesaid" refers back to the first sentence quoted, where James mentioned the "transcendental unity of apperception." Whether or not these phrases—"I think" and "the transcendental unity of apperception"—were meant to be precisely synonymous, it is clear that James used the phrase "I think" at least to evoke the transcendental unity of apperception. It is reasonable also to read the occurrence of "I think" in the description of the stream thesis's first postulate in a similar way, I submit—as calculated to evoke the transcendental unity of apperception.

The language was not James's invention. Kant famously wrote that "It must be possible for the 'I think' to accompany all my representations" (Kant 1781–1787/1965, B131). Roughly, his point was that for anything to be a mental representation, it had to belong to *someone's* conscious life. Mental representations cannot simply float free of any subject. For instance, were I to have a mental representation of a particular sculpture, I would have to be able to represent *myself* as thinking of that sculpture.

So in articulating the first postulate of the stream thesis, James used the phrase "I think" to evoke Kant's notion of the transcendental unity of apperception. Now, the postulate did more than merely *evoke* Kant—it also provided a response (of sorts) to a Kant-style attack on psychology that idealists had been developing.

To understand that attack, it is crucial to see that Kant went further than merely *pointing out* that representations must always be bound to someone's mental life. Kant also tried to give an account of *how* such a unified conscious life was possible in the first place. He called "pure apperception" the kind of consciousness that generates "the 'I think.'" He then argued that such self-consciousness is made possible by pure apperception's "transcendental unity" (B132). Put briefly, it is necessary that there be some unified subject in relation to whom our scattered, outer perceptions can be brought together in one conscious life, Kant argued (B136–37). This subject, the transcendental self, must stand outside of space and time (for reasons I leave aside here), and is what makes possible the transcendental unity of apperception.

Now, James's *point* in evoking the unity of apperception in that first postulate was twofold. First, he meant to accept Kant's claim that all representations are in

fact bound together in someone (or other's) conscious life. But second, James wanted to deny Kant's further attempt to *explain* this boundedness in terms of the synthetic unity of a deeper-lying, transcendental self—or at least to deny that any such explanation can play a role in (late-nineteenth-century) psychology. It is not that James thought he could *refute* the existence of a transcendental self standing behind all experience. Rather, he suggested that the psychologist should reject the burden of explaining the boundedness of experience altogether. This was the point of building self-awareness—the capacity to say "I think" along with any mental representation—into the first postulate of the stream of thought. James's move was to acknowledge this self-conscious aspect of representation as a brute fact of mental life, but to decline to give an explanation of it from inside science.

This was an ingenious way to separate the psychologist's work from the philosopher's, and to bolster psychology's status as a science. Neo-Kantians like Green were arguing that *precisely* because it could not explain allegedly central facts about experience, including experience's boundedness, *psychology could not be a science.* Let us look briefly at Green's version of this argument.

In one passage focusing on the temporal character of experience, Green argued that there must be a part of the mind, itself existing outside of time, which binds together fleeting perceptions into one continuous "plot," so to speak:

> Every object we perceive is a congeries of related, facts of which the simplest component, no less than the composite whole, requires in order to its presentation the action of *a principle of consciousness,* not itself subject to conditions of time, upon successive appearances, such action as may hold the appearances together, without fusion, in an apprehended fact. (Green 1882b, 185, italics mine)

Green argued that there must be a "principle of consciousness" that lies outside of time, in order that all our successive appearances *in* time can be bound together into one continuous life. He connected this "principle of consciousness" with what he called an "Ego" (e.g., at Green and Bradley 1882, 338, 346). Transcendental analysis was supposed to reveal that our perceptions of scattered, temporal events (as well as of scattered, extended objects) were impossible unless we postulated an Ego standing outside of time (and space) that binds together all perceptions into one personal experience.

This point was important to Green's case against psychology because the Ego, standing as it does outside of time and space, has no observable properties, and thus cannot be an object of empirical investigation. But the Ego is allegedly what makes coherent sensory experience possible in the first place. Thus, empirical psychology can never explain the true conditions of sensory experience, according to Green, because it cannot investigate the Ego.

So by building the notion that "thought tends to personal form" into his *fundamental definition* of the mental state, James was rejecting Green's question (Why are experiences necessarily bound into a single conscious life?) as too metaphysical for the *psychologist* to bother with. This did not mean that Green's question was

meaningless or uninteresting or even unimportant. It just meant that the (late-nineteenth-century) *psychologist* was not to take responsibility for answering it. Instead, according to James, the psychologist should proceed by simply acknowledging the fact that our experiences are bound into one conscious life, and then by insisting that this fact is "ultimate," and admits no further scientific explanation.

James made this point explicitly in the *Principles* chapter entitled "The Consciousness of Self." He considered the "transcendentalist theory" of both Kant and Green. He began with Kant:

> Kant starts, as I understand him, from a view of the Object essentially like our own description of it on p. 265 ff., that is, it is a system of things, qualities or facts in relation. . . . But whereas we simply begged the vehicle of this connected knowledge in the shape of what we call the present Thought, or section of the Stream of Consciousness (which we declared to be the ultimate fact for psychology), Kant denies this to be an ultimate fact and insists on analyzing it. (*PP,* 341)

James claimed that the psychologist simply observes that experiences always appear bound together and treats this boundedness as an ultimate feature of the stream of thought. But Kant insisted on going further by *explaining* this boundedness in terms of a deeper ego that stands behind experience.

Then James made a similar point about the *neo*-Kantian conception of a transcendental ego (*PP,* 348). James again argued that an *explanation* of the bound character of consciousness was not likely to be profitable to the psychologist. Instead, the psychologist had simply to assume at the outset that thought exists in a personal form. After quoting Green's elaboration of the passage I reproduced on page 149, above, James wrote:

> Were we to follow these remarks, we should have to abandon our notion of the 'Thought' (perennially renewed in time, but always cognitive thereof), and to espouse instead of it an entity copied from thought in all essential respects, but differing from it in being 'out of time.' What psychology can gain by this barter would be hard to divine. (*PP,* 347–48)

There is no potential profit for the Victorian psychologist in accepting responsibility for *explaining* the bound character of conscious life, James argued, particularly if that meant speculating about an ethereal entity like a transcendental ego. Indeed, in keeping with his own claims about metaphysical assumptions, James's further discussion of this first postulate (at *PP,* 220–24) did not explain *why* thought should tend to a personal form. The discussion only offered evidence *that* this is the case.

In section 4, I asked what the connection could be between the ontological and demarcation functions of James's scientific assumptions. The answer is that any science must decide which of its object's features to treat as ultimate. An ultimate feature is one the scientist declines to take responsibility for explaining. James's insight was that the success or failure of a fledgling science depends in part on *which* ontological features it declines to explain. Scientists must shirk epistemic responsibility sometimes, but they must shirk wisely. In particular, they must strive to shirk in ways that prove fruitful for the overall, social architecture of inquiry.

James saw that it was particularly important to shirk wisely when boundary disputes crop up. Such disputes suck time, money, and authority from one's scientific work, so they must be quieted. And one can help resolve a dispute about disciplinary boundaries by carefully tailoring one's intellectual project so that one clearly *shirks* only the intellectual responsibilities one's neighbor is eager to *shoulder*. That is what James has done in this first postulate of the stream thesis.

In his efforts to resolve the dispute, James even crafted his *language* with an eye toward making peace on psychology's stormy front with idealists. I have already pointed out several places where James employed characteristically idealist language in articulating his own views about the mind (*see above,* p. 139, and note 42). In one striking passage, he went so far as to concede that *reflection* about basic assumptions in the natural sciences actually requires one to adopt philosophical idealism, even though such reflection cannot be a part of science:

> In order not to be unwieldy, every such science has to stick to its own arbitrarily-selected problems, and to ignore all others. Every science thus accepts certain data unquestioningly, leaving it to the other parts of Philosophy to scrutinize their significance and truth. All the natural sciences, for example, *in spite of the fact that farther reflection leads to Idealism,* assume that a world of matter exists altogether independently of the perceiving mind. (PBC, 9, my emphasis)

Since there is scant evidence that James actually adopted idealism himself in philosophy, it is hard to see this concession as more than a rhetorical olive branch. Still, elevating psychology to the status of a legitimate science required (among other things) winning appropriate respect from neighboring intellectual communities; I submit that adopting his critics' rhetoric was a way to help accomplish this task.

In short, we can now see the sense in which the ontological function of James's assumptions depends on their demarcation function. The basic object of his psychology is the mental state—a stream of thought that always appears bound together in one person's conscious life, etc. The choice of which ontological features to regard as basic (i.e., which features to shirk responsibility for explaining) was driven by demarcation considerations[45]—considerations about how cognitive labor can most efficiently be divided. James's guiding principle, again: *divide et impera!*

5.3 The Stream Thesis as Constitutively A Priori

I have suggested that attending to James's use of convenient assumptions can help us extend a Friedmanian insight about physics to the special sciences. Jamesean psychology employs a set of assumptions (the stream thesis) that are in some sense constitutively a priori, I claim.

But the notion that James saw science as employing constitutively a priori principles may seem dubious. After all, he consistently derided Kant, rationalist philosophy, and "intellectualism," and saw himself as heir to the British empiricist tradition.[46] So I now seek to clarify the sense in which the stream thesis may be regarded as constitutively a priori—and the sense in which Friedman's claim about

constitutive principles as an essential element of modern science can be extended to early empirical psychology.

To begin with, one must acknowledge that James explicitly rejected the existence of a priori knowledge in Kant's sense:

> All philosophic interest vanishes from the question [of whether there is synthetic a priori knowledge], the moment one ceases to ascribe to *any a priori truths* (whether analytic or synthetic) that "legislative character for all possible experience" which Kant believed in. We ourselves have denied such legislative character. (*PP,* 1255n.)

Given the idealist attacks on psychology we have been discussing, it was crucial for James to deny that experience has a necessary structure—there are no a priori truths that are legislative for all possible experience, he held.

But this rejection presents no special problem for the purposes of our discussion. For Friedman *also* rejects the existence of necessary truths that are legislative for all experience. He follows Reichenbach in distinguishing two different theoretical elements that may be construed as a priori—necessary principles legislative for all possible experience, on one hand, and presuppositions of particular scientific theories on the other. With Reichenbach (Reichenbach 1920/1965, 48–50), Friedman thinks modern science is characterized by its reliance on constitutive frameworks that are a priori only in this second sense (Friedman 2001, 30). I want to claim that Jamesean psychology also employs a constitutive framework that is a priori in more or less this sense as well.

In the cases Friedman discusses, the properly empirical statements in scientific theories cannot be *meaningful* or *precise* without some a priori principles (in other words, a framework) already in place. Jamesean psychology also employs a framework—the stream thesis. It is constitutive of neither the meaning nor precision of the properly empirical statements in his theory, as we have seen; but it is constitutive of his psychology's scientific status, I am claiming. Without the stream thesis already in place, the empirical statements of James's psychology would not be statements in any legitimate *science.*

So even though Jamesean psychology uses neither mathematical nor coordinating principles—the two sorts of constitutive principles Friedman finds in physics—a basic Friedmanian insight still holds true. Like the cases Friedman considers, James's psychology is *stratified* in the sense that not all parts of the theory face empirical evidence symmetrically.

We have seen that one can reject Newton's laws of motion without having any effect on the meanings of statements in the calculus, for example—but the reverse is not true. Similarly, if one rejects the stream thesis, one must reject (not the meaningfulness, but) the *scientific status* of James's other psychological theories. The reverse is not true, though. James's specific theory of emotion, for instance, can be rejected without requiring any serious alteration to the stream thesis. Thus, like the calculus in Newtonian physics, the stream thesis occupies an epistemically unique position in James's psychology.

This seemingly simple issue of whether scientific theories are stratified has a far-reaching significance. In the contemporary debate over naturalism, a disagreement over stratification issues in two rival visions of *philosophy's proper relationship to the sciences*. Both naturalists and non-naturalists see an intimate connection between philosophy and the sciences. But whereas naturalists think philosophy ought to take its place next to psychology as a *branch* of science (as I have discussed; *see above*, p. 132), Friedman thinks philosophy must preserve an independent identity.

Now the existence of constitutive frameworks that face evidence asymmetrically is crucial to Friedman's vision. For it is precisely when such frameworks break down that philosophy has historically aided science, and aided it from the outside. One example is the breakdown of the classical conception of space and time in physics. Einstein could make a rational case to scientists working in the old paradigm[47] precisely by situating his discussion in the context of early modern *philosophical* debates over absolute versus relative motion, Friedman argues. In other words, Einstein appealed to philosophy for a measure of rational continuity through a scientific revolution. So just as one must distinguish between the level of properly empirical statements and a priori frameworks *in science,* according to Friedman, one must also distinguish between scientific frameworks and "meta-frameworks"—characteristically *philosophical* discussions that have traditionally smoothed the transition of paradigm shifts (Friedman 2001, 105–6).

James of course predates such talk about paradigms. But his sense that scientific theories contain more than just straightforwardly empirical statements—that they also contain frameworks that must be accepted or rejected on the basis of non-empirical considerations—led him to a kindred view of philosophy's disciplinary independence, I want to claim.

It is helpful to compare James and Robertson here. James was clearly on Robertson's side in the battle over psychology, a field both men sought to elevate to the status of a natural science. But these allies offered contrasting visions of psychology's relationship to philosophy. We have seen that Robertson thought a genuine science of mind could provide a scientific foundation for philosophy—mental science could provide philosophy with neutral facts, a "common ground" on which warring schools could meet.

But James offered a different vision. He held that sciences rely on convenient assumptions that are not directly responsible to empirical evidence, as we have seen. This presents a problem (albeit a different problem from that in which Friedman is interested). If our various sciences rely on disparate sets of assumptions that are not constrained by empirical evidence, then how can we gain one coherent view of the world from all these separate fields? In the epilogue to the *Briefer Course,* James wrote:

> All these special sciences, marked off for convenience from the remaining body of truth . . . , must hold their assumptions and results subject to revision in the light of each other's needs. The forum where they hold discussion is called metaphysics. (PBC, 395)

Philosophy is the "forum" where we try to fashion a coherent worldview from the different assumptions each science must make. As in Friedman's case, notice that it is epistemically privileged *frameworks*—convenient assumptions, for James—that present distinctive problems for philosophical reflection.

So James may be a naturalist in that he sees philosophy and psychology as close neighbors. But unlike Quine, James resists reducing philosophy to psychology. Dissolving the boundary between psychology and philosophy "spoils two good things" (*PP*, 6).

I should note an apparent tension in James's work, here. He sometimes sees philosophy and psychology as two coordinate fields attempting to divide labor in the study of a similar subject matter. But in this last passage, he portrays philosophy as playing a meta-level role with respect to the sciences. Perhaps the first view represents James's observation of the *actual* relationship between late-Victorian philosophy and psychology, and the second represents his ideal vision of that relationship.

In any case, I must now face a potentially deeper objection. I have claimed that the stream thesis is constitutively a priori. But this thesis does seem to make empirical claims about experience. For instance, the third postulate claims that "Within each personal consciousness thought is sensibly continuous." In fact, I argue at length (in Klein 2009) that James provided experimental evidence in support of this postulate. So *qua* description of experience, we must admit that the stream thesis is definitely not a priori. At least some postulates make empirical predictions that are testable within the bounds of science.

But in this essay I have been concerned with another function of the stream thesis. Its role as criterion for dividing psychological from philosophical explanations is distinct from its role as description of experience. What is a priori is the choice of *which* features of experience mental science should treat as ultimate. Empirical data cannot establish which demands for explanation a scientist ought to accept and which she ought to parry. Insofar as the stream thesis is a tool for sorting out who ought to take what cognitive responsibilities in the study of mind, that thesis is responsible to considerations that are ultimately social and pragmatic, not empirical.[48]

A fortiori, no empirical data *inside the domain of mental science* can either support or defeat the stream thesis in its demarcating capacity. This is because the thesis must be in place before there can *be* any legitimate science of mind to produce empirical data.

Thus, I want to claim that *qua* demarcation principle, the stream thesis represents a stipulated boundary between mental science and neighboring disciplines (primarily metaphysics). What finally makes the stream thesis a *rational* yet stipulated assumption is that the thesis was adopted for good pragmatic reasons. I mean that James had a pressing task to accomplish, and the thesis was rational to adopt just to the extent that it helped him accomplish this task.[49]

The task at hand was to provide a shield from metaphysicians' arrows so that psychology could start producing real empirical successes without having to be pinned down by philosophical speculation. But James had to do at least some justice to metaphysicians' widely influential criticisms. To the extent that the stream thesis actually stood to help affect this cease-fire, we should think of the thesis as rational.

Note that one cannot simply pronounce the stream thesis either rational or irrational independently of what anyone happened to think of it. This is because it was precisely by *actually persuading* warring factions—persuading them how to divide labor—that the thesis could function to secure psychology's scientific legitimacy. Effective scientific research is hampered by constant public attacks from one's intellectual neighbors. Quieting such a nuisance is a necessary and nontrivial hurdle for any aspiring science. Thus I repeat, James had good reason to demarcate psychology and philosophy in the way specified by the stream thesis only to the extent that the thesis stood actually to be accepted as noncontroversial by disputants. In fact, I think James's choice of how to demarcate psychology and metaphysics *was* rational because his choice really did stand to satisfy these disputants.[50]

Some may object that there is something untoward about my claim that the stream thesis was at once "stipulated" *and* "negotiated." But consider a couple who shares chores at home. Suppose one partner, *A*, is in charge of being sure the kitchen stays clean. *A* may stipulate different roles for the two after dinner—*B* will bus the table, say, and *A* will wash the dishes. But *B* may still have a say in the matter. Maybe *B* hates scrubbing silverware, and will be able to convince *A* to take this task. Perhaps *A* will then have leverage for placing responsibility back on *B* for wiping the table.

Similarly, James stipulated the *Principles*' definition of the mental state. But this does not mean he was free to stipulate his definition in a way that ignored the complicated negotiations that had been raging between metaphysicians and psychologists. He had to find a way to stipulate a boundary between psychology and metaphysics that would help affect a cease-fire between these warring factions.

The compliment I am paying James is much like the compliment one might pay an engineer who figures out how to build a better bicycle. For instance, in the 1890s bicycles began being mass produced with pneumatic tires, for the first time—an invention (by John Boyd Dunlop) that made for greater traction and a smoother ride. Dunlop's new design was rational not because it pictured some fundamental truth about nature. It was rational because it provided an ingenious solution to problems inherent in older bicycle designs, problems concerning the *interface* between humans and nature. Similarly, I want to claim that James's stream thesis was rational in that it was an ingenious invention for quelling the philosophical squabbling that had hobbled earlier investigations of the mind—it provided, as it were, greater traction and a smoother ride for empirical psychology.

VI. PEIRCE, AGAIN

Some housekeeping is in order.

I began with Friedman's argument that some scientific theories contain constitutive a priori elements and that Quinean naturalists cannot make sense of these. Friedman claimed that such principles are hallmarks of science as we know it; but I argued that the two types of a priori principles Friedman identifies—mathematical and coordinating principles—do not actually appear in special sciences like Darwinian biology and early empirical psychology. At least in the case of Jamesean psychology, I claimed that there are other a priori elements, though—elements I call "ontological agreements." Such an agreement provides stipulated boundaries between intellectual fields. The boundary's placement must be agreed upon not only by psychologists, but by psychologists' intellectual neighbors as well.

In section 3, I provided historical context to explain why such social agreements were needed if psychology was to achieve the status of a natural science in the late nineteenth century: the boundary between psychology and one of its neighbors—philosophy—was hotly contested, and boundary skirmishes were hampering inquiry. In sections 4 and 5 I showed *how* James sought to establish a boundary between psychology and philosophy. He tried to craft a definition of psychology's *proper object,* a definition that would parry idealist criticisms in a way that would be acceptable to both parties. This is why I call such demarcating principles "*ontological* agreements"—the demarcation is established when neighbors agree on a mutually acceptable ontology.

I finished section 5 by arguing that these agreements are constitutively a priori in the sense that they are presuppositions of a discipline's scientific status. They are not responsible to empirical evidence, but rather act like tools for supporting a cognitive division of labor. An ontological agreement is rational to adopt just to the extent that it helps support such a division of labor.

This finally brings us back to Peirce's question of how, if at all, an "uncritical" assumption like the stream thesis could ever be a rational part of a scientific theory. We have seen that this thesis was only "uncritical" in the sense that it established a set of postulates to be regarded as ultimate and not subject to further critical scrutiny in the context of psychology. I have just argued that the thesis was rational to the extent that it helped foster social conditions conducive to scientific inquiry. But then what did Peirce's complaint against James really come to?

Cheryl Misak has written about Peirce's work on the role of regulative a priori assumptions in science (Misak 1991, 140 ff.). Peirce held that rational inquiry requires regulative assumptions, such as the assumption that we may someday find a good answer to whatever question we are inquiring about. But he distinguished his view from "a transcendentalist" who would attach greater weight to such presuppositions. Unlike the Kantian, Peirce held that the indispensability of presuppositions was not good grounds for believing them true. He wrote:

> I do not admit that indispensability is any ground of belief. It may be indispensable that I should have $500 in the bank—because I have given checks to that amount. But I have never found that the indispensability directly affected my balance, in the least. (*CP*, 2.113)

Nevertheless, to deny a regulative assumption is to block inquiry. What justifies us in adopting regulative assumptions is "the justification of desperation," as Peirce called it. We simply have no hope of "know[ing] anything of positive fact" unless we adopt such assumptions (*CP*, 5.603).

Perhaps Peirce was accusing James of uncritically accepting a thesis *that is not actually indispensible* to psychological inquiry. After all, there is nothing indispensible about James's particular way of dividing psychology from its intellectual neighbors. Somebody else might have come up with a different definition of the mental state—a different framework for psychology—that could perhaps have been just as successful as the stream thesis in establishing a division of cognitive labor.

If this is Peirce's objection, it seems undermotivated. For Peirce should accept that "the justification of desperation" forces us to adopt *some* demarcation hypothesis or other. Why not accept James's hypothesis? Peirce does not offer reasons for thinking that James's particular hypothesis is ill-suited for helping divide cognitive labor.

The deeper disagreement between Peirce and James, I think, lies in a difference over what *makes* a hypothesis like the stream thesis "indispensible." The stream thesis surely stands to mollify idealists by consigning the explanation of important features of the mind to *philosophy*, and by doing this in a spirit of cooperation. But for Peirce, perhaps mollifying one's neighbors is not genuinely indispensible for scientific inquiry, at least not in the sense needed to be a candidate for the justification of desperation.

But James *does* seem to think that mollifying testy neighbors can be indispensible to advancing scientific inquiry. As I have been arguing, James thinks science can only be practiced in a larger context of social cooperation. If one wants to help foster such cooperation, one must find a way to divide labor. But finding a stable division of labor among real people requires finding an arrangement that is actually satisfying to the affected parties.

Finally, I want to suggest that it is helpful to compare the Quine/Friedman and the Peirce/James debates not only because the comparison helps us see how theories outside the exact sciences might be stratified, as I have suggested. The comparison also helps us tease apart two distinctly pragmatist traditions in American philosophy of science.

Contemporary naturalists, on one hand, are not just carrying the Quinean torch. I submit that they are advancing the Peircean thought that science's unique power lies in its refusal to insulate any theory, any datum, any assumption, from empirical testing.[51] In other words, naturalists refuse to place any questions about a theory off-limits.

On the other hand, Friedman and his allies are advancing the thought that different sciences operate inside fundamentally different intellectual frameworks. Frameworks help narrowly tailor inquiry for the sake of practical effectiveness, but they must ultimately be accepted or rejected on pragmatic grounds.

Friedman may seem like strange bedfellows with James, but when we start giving flesh to the Jamesean tradition with names like C. I. Lewis, Dewey, Pap, and Kuhn,[52] the connection should feel less strained. If I have succeeded in showing a substantive affinity between James and neo-positivists like Friedman, the next question is: what are the philosophical and historical relationships between positivist forebears (like Carnap and Reichenbach) and James's descendants (like Lewis and Dewey) who were contemporaries, and who extensively interacted with one another? I must leave this question to future research.

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NOTES

1. I use the following abbreviations throughout:
   CP = Peirce, Charles S. 1931–1958. *Collected Papers*. Edited by Charles Hartshorne, Paul Weiss, and Ardath W. Burks. 8 vols. Cambridge, Mass.: Harvard University Press.
   CWJ = James, William. 1992–2004. *The Correspondence of William James*. Edited by Ignas K. Skrupskelis and Elizabeth M. Berkeley. 12 vols. Charlottesville: University Press of Virginia.
   GWR = Green, T. H. 1894. *Works of Thomas Hill Green*. Edited by R. L. Nettleship. 3rd ed. 3 vols. London: Longmans, Green and Co.
   PP = James, William. 1890/1981. *The Principles of Psychology*. Edited by Frederick H. Burkhardt, Fredson Bowers, and Ignas K. Skrupskelis, *The Works of William James*. Cambridge, Mass.: Harvard University Press.
   PPNS = James, William. 1892/1983. "A Plea for Psychology as a 'Natural Science'," in Frederick H. Burkhardt, Fredson Bowers, and Ignas K. Skrupskelis (eds.), *Essays in Psychology*, Cambridge, Mass.: Harvard University Press, 270–77.

RBP = Perry, Ralph Barton. 1935. *The Thought and Character of William James.* 2 vols. Cambridge, Mass.: Harvard University Press.

2. On the James family's fraying relationship with the *Nation, see* (Habegger 1994, 449–50, 452, 467, 504). A negative review of the father's *Literary Remains* had especially strained the relationship in the mid-1880s.
3. I became aware of the letters quoted in the text by reading (Girel 2003, 195n157). Girel's essay is an essential resource for those interested in Peirce's analysis of James's psychology. Peirce's review appeared in successive issues of the *Nation*; *see* (Peirce 1891a; Peirce 1891b). The review is anthologized in (Peirce 1975).
4. Peirce's review appeared in two installments. The first criticized James's grasp of scientific method, and the second defended a theory the *Principles* had been at pains to undermine—the theory that perception requires unconscious inference. In the letter to Henry I have been citing, William pronounced the second installment "utterly unintelligible" (*CWJ* 2.185), but said nothing about the first. I focus on the first half of Peirce's review.
5. Elsewhere, Peirce called James a "great psychologist," and the *Principles* an "athlete-thinking" work; *see* (Girel 2003, 6).
6. What about mathematics? Cheryl Misak emphasizes that Peirce saw even mathematics as empirical, experimental, and fallible (e.g., at Misak 1995, 108–12). He thought mathematicians run inductive experiments on "diagrams." Admittedly, there has been some dispute over how to reconcile Peirce's experimentalism about mathematics (and his thoroughgoing fallibilism), on the one hand, with his occasional claims that mathematical judgments are in some sense a priori and certain, on the other. An in-depth discussion can also be found in chapter 6 of (Hookway 1985), and in (Haack 1979). With Misak, I read Peirce as presenting mathematics as an empirical discipline that makes falsifiable predictions about a special kind of experience—what he called "ideal" as opposed to "real" experience; *see* (*CP,* 3.363, 4.530, 5.567). I have briefly discussed scholarship on the tension between Peirce's fallibilism and his views on necessity at (Klein 2007).
7. For instance, *see* (McEvoy 2002) in addition to literature cited in notes 6 and 20.
8. *See* (Quine 1951, 43).
9. On Quine's relationship to the pragmatist tradition, *see* (Isaac 2005; Koskinen and Pihlström 2006; Richardson 2002a; Richardson 2002b, 270–71; Richardson 2003; Richardson 2008). In their first endnote, Koskinen and Pihlström give extensive references to scholarship that portrays Quine as firmly in the mainstream of the pragmatist tradition. I thank Richardson and Rob Sinclair for valuable discussions on this topic. Sinclair pursues Quine's early relationship to Lewis in (Sinclair Forthcoming).
10. *See* note 20, below.
11. For a detailed argument that Lewis's pragmatic a priori is indebted to James's conception of the a priori, *see* (Pancheri 1971). On Dewey and the a priori, *see* note 19, below.
12. *See* especially (Friedman 1997; Friedman 2001). At (Friedman 1997, 7), he cites two examples of contemporary naturalists carrying the Quinean torch: (Devitt 1996) and (Papineau 1993).
13. Friedman thinks this is fundamental for Quine's naturalism. It is fundamental in the sense that Quine's rejection of the analytic/synthetic distinction is what supports his subsequent claim that no belief, in principle, is immune from revision.
14. The word "symmetric" is Friedman's; *see* (Friedman 2001, 36).
15. When I write that we do not know how to assign a truth value to the negation of "The present king of France is bald," I am assuming that the proper negation of this statement is "The present king of France is not bald," rather than "It is not the case that the present king of France is bald."
16. Friedman softens his rhetoric in other places. For example, at (Friedman 2001, 71) relativized a priori principles are presented as characteristic features of "advanced theories in mathematical physics." He uses similar language eight pages later.
17. On the nature of Darwin's support for his theory, *see* (Lloyd 1983).
18. Friedman acknowledges that more work is needed to see how his view might fit with examples like this from the special sciences, at (Friedman 2001, 126).
19. Alan Richardson argues that naturalists of a *Deweyan* stripe also bear affinities with Friedman (and

disaffinities with Quine), particularly when it comes to the existence of a priori knowledge. In particular, *see* (Richardson 2002a; Richardson 2002b; Richardson 2003; Richardson 2008). I must leave the relationship between Jamesean and Deweyan thinking about the a priori to future research.

20. I gave references to scholarship that places Quine in the general pragmatist tradition *above*, at note 9. An important work that portrays Quine as advancing a specifically *Peircean* form of pragmatism is (Misak 1995). Most importantly for my purposes, she argues that Peirce anticipated Quine specifically by rejecting the analytic/synthetic and a priori/a posteriori distinctions; *see* (Misak 1995, 109). One should acknowledge, however, that Quine actually claimed to reject Peirce's *theory of truth*, at (Quine 1960, 23), on grounds that the theory requires a notion of "nearer than" which is defined for numbers but not theories. But Creath neatly shows that the argument only vitiates Peirce if it also undermines Quine's own epistemology, at (Creath 1998). Finally, I do not mean to suggest that Peirceans can muster no reply to Friedman-style attacks—perhaps they can, but it will take some ingenuity.
21. In 1865, Masson gave a series of public lectures at the Royal Institute on British philosophy, and these were published in America the following year. The book found its way to the personal library of Henry James Sr. William never had a formal education in philosophy, and his father's copy of Masson served as an introductory textbook. *See* (*RBP* I.497, 574).
22. Bain, an associationist psychologist, began financing *Mind* in 1876. Bain appointed Robertson, his student, as the first editor. Robertson dubbed the journal "*Mind*," and edited the publication until 1891, when he retired for reasons of ill health. G. F. Stout then took over as editor, and Henry Sidgwick as patron. Robertson died the following year at age fifty (Quinton 1976, 6, 8). For a fascinating study of the two years leading up to *Mind's* first issue, *see* (Neary 2001).
23. I should note that the only example Robertson gave here of an alleged attack on psychology was the Royal Commission on Scientific Instruction's *Third Report*, an 1873 document (jointly authored by T. H. Huxley and others) that reviewed all aspects of science instruction at Oxford and Cambridge. The document explicitly excluded "the Mental and Moral Sciences" from its purview. Nevertheless, Robertson would respond to idealists directly and extensively only a few years later when their attacks began appearing in *Mind*. *See* (Robertson 1883).
24. Caird lived from 1835 to 1908, Bradley lived from 1846 to 1924, and Green lived from 1836 to 1882.
25. Bradley was more moderate, arguing that psychology *could* be a science, though like other sciences, it could provide only an incomplete account of its subject matter. Green, Caird, and their ally Andrew Seth Pringle-Patteson categorically rejected psychology on the grounds that the mind is just not the sort of thing that can be studied using empirical methods. Recently, Fred Wilson has contrasted Bradley's more nuanced position to that of Green and Pringle-Pattison, correctly noting that "the point of idealism for Green . . . was to establish that a natural science of human being is impossible," in (Wilson 1999, 10). *Also see* (Wilson 1998, 9).
26. In the next generation, leading British philosophers like Russell would take their cue from the anti-psychologism not just of Frege, but perhaps as importantly of idealists like Green and Bradley (see Griffin 1996; Keen 1971) who participated in the debate I discuss in the text.
27. One can find discussions of empirical psychologists at, for instance, (Green 1874/1894, §§3, 6, 9, 10, 18, 24, 98, 198–200). In my view, Green does not get around to explaining his introduction's main *aim* until §§198–200. This passage occupies a more prominent position than the section numbers suggest. Green only begins discussing Hume in any detail at §195. From there until §202 Green provides his entire rationale for undertaking an investigation of Hume and his predecessors. A concise account of Green's aims in the introduction can be found in a document he published three years later, entitled "Mr. Herbert Spencer and Mr. G. H. Lewes." That piece opens with a reflection on what Green had hoped to accomplish in his introduction to Hume. The article began appearing in the *Contemporary Review* in December 1877; *see* (*GWR*, I.vi, I.373–519).
28. Green warns of the dangers of amateur, popular philosophy in "Popular Philosophy in its Relation to Life" (Green 1868/1894). A useful discussion of this essay is (Walsh 1986).
29. Green saw psychologists as part of a tradition in British thought tracing back to Locke—this tradition sought to purge philosophy of its more speculative, metaphysical elements; *see* (*GWR*, I.165).
30. This conception of psychology is surely indebted to Green's reading of the preface to the *Treatise*, where Hume says he wants to develop a "natural science of man." Indeed, Green's sharpest attack

on psychology is entitled "Can There Be a Natural Science of Man?" *See* (Green 1882a; Green 1882b; Green and Bradley 1882).

31. For Green's argument about the necessary and synthetic character of Euclidean principles, *see* (*GWR*, II.246–48); and for Kant's treatment of this issue, to which Green is indebted, *see* (Kant 1781–1787/1965, B3, B16, B40–41).
32. I present a detailed account of Green's criticism of psychologists' accounts of spatial perception, as well as of James's response, in (Klein 2009).
33. By "substantive output" I mean all James's essays on any topic, from psychology to philosophy to psychical research. I exclude letters to the editor, notes, and book reviews. James published a large number of very short such contributions, which I exclude because I want to give a sense of where he was sending his most carefully written work during this period. I compiled these data from Ralph Barton Perry's annotated bibliography. The bibliography was edited and republished by John McDermott in (James 1967, 811–58).
34. James's publications in *Mind* were not confined to a burst of activity during one or two years, but were evenly distributed, more or less, across the period. To be sure, he was especially prolific in 1887, when his four-part essay on space perception appeared in *Mind*. But he published more in Robertson's journal than anywhere else during each of the years 1879, 1882, 1884, 1885, and 1889.
35. I want to acknowledge two important trends in James's publishing record during these years that my figures do not reflect. First, the inclusion of notes, reviews, and letters has the effect of highlighting popular intellectual journals like the *Atlantic Monthly* and *Nation* to which James was a regular contributor during this period. Also, I have excluded French translations of James's essays during this period. A thorough overview of James's publications during this period would surely have to take account of James's presence in François Pillon and Charles Renouvier's *Critique Philosophique*, and related French journals. The large majority of these articles were translations of pieces that first appeared in English. But the articles sparked lively discussion and rocketed James to intellectual fame in France. In many cases, Renouvier published responses, to which James offered rejoinders. This story is beyond my scope.
36. James also became enmeshed with Shadworth Hodgson's recently formed Aristotelian Society during this trip. Two years later, James would deliver "On the Function of Cognition" to the Society, and *Mind* would publish the piece a few weeks later. I learned a great deal about Hodgson's role in this context from Thomas Staley's "Sources of Contemporary Pragmatism in the Late Victorian Psychologistic Philosophy of Shadworth H. Hodgson," a conference paper delivered at the 2006 meeting of the International Society for the History of Philosophy of Science (HOPOS) in Paris.
37. This is from James's letter to G. H. Howison, dated September 30, 1881 (*CWJ*, V.180). George Herbert Palmer was an American friend of James who spent several summers in Scotland studying philosophy with the idealist Edward Caird, a practice James ridiculed throughout the correspondence of this period. "The Cairds" refers to Edward and his cousin John. Edward Caird and T. H. Green were read and hotly debated among one of James's important American philosophical clubs in the late 1870s; for more on which, *see* (Klein 2009).
38. For evidence of Robertson's confidence in James, and for the editor's eager solicitation of new material, *see* for example (*CWJ*, V.226, 484).
39. In addition to Ladd's review, which I discuss in the text, another prominent example is (Fullerton 1894), which critically examines James's choice of "irreducible data" at pages 123–33. Fullerton's criticism merits more attention than I can devote here, not least because in response, James *apparently* revised his views about psychology's relationship to philosophy. James responded in (James 1895), reproduced at (James 1895/1978); his apparent change-of-heart comes at (James 1895/1978, 88). I am not certain whether James's reversal amounts to anything more than a rhetorical shift; but in any case, the present essay should be taken as an analysis only of James's *early* views, up to 1895. I thank David Leary for provocative conversation on this topic. Leary discusses James's apparent reversal at (Leary 2007), focusing on James's cryptic remarks about Ladd at (James 1892/1984, 9).
40. I do not minimize the importance of James's participation in *American* turf wars between psychologists and philosophers; on this topic, *see* (Wilson 1990). But the significance of the British struggle to James's thinking during this period has been seriously neglected by scholars.
41. For more on this slogan, *see* the end of this section.

42. James's claim that scientists should not be pestered with metaphysical questions was not an unusual position. For instance, Robertson had taken this line in his influential *Mind* essay on the topic (Robertson 1883, see esp. 8). Even some idealists advanced versions of this position. In his *Principles of Logic,* Bradley argued that all sciences, including psychology, operate with "working hypotheses" (see especially Bradley 1883, 315–17)—a phrase James used at (PBC, 396). For Bradley, ultimate truth can be gleaned only by a metaphysical investigation of the Absolute, though, whereas James thinks every discipline, including philosophy, has its own working hypotheses. We know James read these works closely, as relevant passages of his copy of Robertson's essay and of Bradley's book are both marked up. Both can be found at the Houghton Library at Harvard—*see* Phil 22.4.6* for James's run of *Mind,* and *see* WJ 510.2.2 for his copy of Bradley. I cite these passages by permission of the Houghton Library, Harvard University. I thank David Crossley for calling my attention to the passage in Bradley.
43. The piece was first published in the *Atlantic* and appeared again in *The Will to Believe* in 1897.
44. These notes can be found at the Houghton Library, call number bMS 1092.9 (4448). I quote this passage by permission of the Houghton Library, Harvard University.
45. More recently, philosophers have apparently concluded that a priori attempts to demarcate science from nonscience are ultimately futile, following (Laudan 1983), and *contra* (Popper 1962). James saw the demarcation of psychology from philosophy as an a priori matter, but it is not clear he would have disagreed substantially with Laudan. Like Laudan, James did not think there was one big puzzle here that philosophy can solve with a formula. Instead, there are many demarcation problems, for James, and it is up to groups of inquirers themselves to negotiate viable divisions of cognitive labor. Unlike Laudan though, James obviously saw demarcation as more than a matter of emotive name-calling. For contemporary sociologists of science who also think the demarcation problem is still worth pursuing, *see* (Fuller 1991, 175–89; Gieryn 1983). Gieryn emphasizes the flexibility of criteria scientists have historically used to distinguish their practice from nonscience, and to thereby secure authority. Note that for James though, the challenge is not simply how to secure authority, but also how to organize cognitive labor in a way that will prove most fruitful overall.
46. *See* (Skrupskelis 1988, xlviii); James's commitment to empiricism is also a theme of perhaps the most important study of his life and philosophy (*RBP*).
47. I use Kuhn's terminology advisedly; Friedman argues that there is a deep kinship between his notion of relativized a priori principles and Kuhnian paradigms; for instance, *see* (Friedman 1997; Friedman 2001, 18–22, 41–45).
48. The distinction between a priori and a posteriori justification is of course notoriously slippery, so it would be nice if James offered philosophical resources to help bolster our discussion. Unfortunately, his extended consideration of a priori judgment in the *Principles* is only superficially related to our present discussion. The final chapter does defend the idea that natural sciences rely on a priori judgments. But it takes up psychological questions about how individuals *acquire* a priori, necessary truths. The more recent debate over naturalism, in contrast, concerns the *justification* of a priori judgments. Drawing a connection between these two different discussions would require a whole other essay, I fear. James did expand on his views about the a priori at the end of his life, especially in an uncompleted manuscript published in 1911 as "Some Problems of Philosophy" (James 1911/1979). But I am confining my discussion to James's early views; *see* note 39, *above.*
49. *See* (Jackman 1999) for an illuminating discussion of James's use of prudential reasoning, and for the argument that James's epistemology does bear *some* affinities with contemporary naturalized epistemology.
50. I confine myself to the claim that this thesis was well *suited* to affect a cease-fire between philosophy and psychology. A full assessment of the *actual* impact of James's stream thesis in psychology would be a very large task indeed, one that I cannot take up here. An assessment of the legacy of the stream thesis in contemporary psychology, especially in experimental psychology and psychoanalysis, as well as in literature, music, and philosophy, is (Pollio 1990). Two helpful, article-length assessments of James's impact on psychology more generally are (Leary 2003; Taylor 1995). Both conclude that James had a profound effect on psychology. Leary argues that James helped clarify key problems for the discipline to work out. Gestalt psychologists (for more on which, see

Henle 1990; Woody 1999), philosophical and psychological phenomenologists, and even behaviorists (via Dewey's "reflex arc") were all impacted by James's notion of a stream of thought. Leary also has a close analysis of the legacy in psychology of James's interpretation of the self (Leary 1990). Taylor identifies four characteristics of American psychology that trace back to James. Taylor also discusses a significant faction of American anti-Jameseans, many of whom trace intellectual ancestry to Wundt's laboratory (Boring and Tichener are the most famous examples). The centenary of James's *Principles* sparked several collections of articles that assess various aspects of James's influence on psychology, including (Donnelly 1992; Johnson and Henley 1990). Other resources that assess the historical legacy of the *Principles* include a special issue of *History of the Human Sciences* devoted to James. One useful article contained there is (Skrupskelis 1995). One helpful collection that assessed the significance of the *Principles* for philosophy is (DeArmey and Skousgaard 1986).

51. *See* note 20, above.
52. Several historians of analytic philosophy have begun studying these figures (especially Dewey, Lewis, and Pap) from a perspective relevant to my discussion. I might mention Richardson (for references see above, note 19), Don Howard, Sanford Shieh and David Stump.

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