

## A GOLDILOCKS APPROACH TO THE PHILOSOPHY-SCIENCE RELATION

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### ABSTRACT

The Goldilocks Principle recommends sometimes seeking just the right amount of something. In this paper, I apply this principle to pursue a more judicious view of the relation between philosophy (P) and science (S). Extreme views contrary to GA depict the PS relation as one of destructive incompatibility, indifferent independence, or toxic asymmetric dependence. Contrary to both extremes, the Goldilocks Approach (GA) suggests more moderate depictions of P and S as sufficiently diverse to enjoy professional sovereignty and disagree, but also sufficiently compatible to enable meaningful interdisciplinary cooperation. GA also advises a greater emphasis on case-based investigation that gives equal importance to the context of justification (analytic normative considerations about the logic of linguistic products) and the context of discovery (descriptions of the biopsychosocial aspects of the processes that lead to such products). All this makes for a more balanced, potentially constructive and fruitful relation in selected matters. I exemplify with cases at the intersections between psychology and the Ps of language, mind, and S.

*Key words:* Goldilocks principle, moderation, philosophy, science, interdisciplinary cooperation

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### A Goldilocks Approach to the Philosophy-Science Relation

In this paper, I propose a Goldilocks Approach (GA) to the relation between philosophy (P) and science (S) (ordered only alphabetically, not by importance). ‘Goldilocks’ refers to the namesake character of the 19<sup>th</sup> century British fable who seeks just the right amount of something (porridge not too hot, not too cold; a bed not too hard, not too soft). GA has had applications across various disciplines. In psychology, the Goldilocks effect is a tendency of human infants to seek moderate, and avoid too much or too little, complexity in visual stimuli (Kidd et al., 2012). GA is also present in astrobiology (the zone ideal for life around, not too far from, but not too close to, a star), medicine (drugs that have excitatory and inhibitory effects), and economics (an economy that grows moderately with low inflation).

GA is a toned-down version of the ancient Greek adage ‘Nothing in excess.’ Like the adage, GA favors moderation. Unlike the adage, GA, albeit often called ‘principle,’ is not a rigid universal rule, only a set of flexible, optional, very rough, and suggestive guidelines. Applied to the PS relation, GA suggests a more balanced depiction than other approaches do, without touting the adage’s extreme implication of everything in moderation (which presumably applies to moderation itself). GA’s general core idea is that P and S are equally valuable intellectual pursuits that enjoy sufficient autonomy to allow for interest-driven professional specialization, critique, and disagreement, but also are sufficiently compatible for potentially beneficial interdisciplinary collaborations.

This idea can develop along several lines, but I will focus on just a few, especially the extent and character of the PS relation, and on *analytic* P, mainly P of S and P of mind. GA strives for a moderate relation that is neither too strong to become toxic, intrusive, or threatening, nor too weak for P and S to become aloof from one another. Against that adage’s implication, GA does not propound seeking any of these or other vices in moderation. GA, rather, seeks to keep them at bay as much as possible. Any degree of any vice would be harmful to the relation. GA also seeks to maximize any positive vices, especially usefulness in achieving specific aims, albeit there might be too much of a good thing.

Under GA, P and S have much to offer but also not to offer one another. Their relation could sometimes be (in fact, has been) mutually supportive, but not always. As I said, and is often the case, their relation can also be critical (not everything in it is or should be rosy), although GA favors constructive over destructive critique. Much hinges on the specifics, for which GA also encourages more idiographic considerations that give equal importance to *case studies* as key counterparts to principled nomothetic considerations. GA thus also seeks to be a more judicious blend of descriptive and prescriptive (or normative) aspects: GA is as much about what the PS relation actually is, as it is about what it should ideally be, although the latter does not involve any necessary, universal principles. In what follows, I elaborate some historical-conceptual specifics, with examples from my own practice. I will shift freely back and forth between the descriptive and the prescriptive, letting the context determine which one is under discussion. I will provide some textual evidence that supports, and discuss some conditions that might favor or hinder, GA.

A familiar extreme view contrary to GA is the radical anti-P movement in S, expressed, for example, in Hawking and Mlodinow’s (2010) celebratory claim that “philosophy is dead” (p. 5). This claim is patently false: P is well and alive, at least by institutional standards, even if they define the PS distinction only partially. Many people over the world are making a living in academic P through publications, conferences, research, and education — undergraduate as well as graduate. Other scientific luminaries have expressed the destructive sentiment behind this claim. Weinberg (1992, pp. 166–190) has written “against philosophy.” Geneticist Steve Jones (1997)

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quipped crassly that, “For most wearers of white coats, philosophy is to science as pornography is to sex: it is cheaper, easier, and some people seem, bafflingly, to prefer it” (pp. 13–14). Other notable scientists like Bill “The Science Guy” Nye ([https://youtu.be/ROe28Ma\\_tYM](https://youtu.be/ROe28Ma_tYM)) and Neil deGrasse Tyson (see Pigliucci, 2014) have joined the anti-P chorus in social media.

Although far classier than Jones, Tyson and Nye nonetheless have expressed their skepticism about P quite condescendingly with cartoons in which P is ultimately, in various senses, less worthy than S. Both cherry-pick pop culture clichés to mock philosophy. For example, in his video, Nye takes the view that reality is unreal and thinking is necessary for existence as philosophical conventional wisdom, but any philosophy freshman would quickly reply that this is way off. Not only are idealism and the Cartesian cogito argument far from being representative of philosophy, but Nye’s formulations of them are embarrassingly wrong, lacking the most elementary philosophical culture. No idealist claims reality is unreal. Idealists only claim that all reality is mental, which is very different (and quite consistent with Nye’s emphasis of sensations and perceptions). Nor does any idealist claim that I will not notice if I drop a hammer on my foot (and under idealism, a tree falling in a forest *definitely makes a noise even if there is no human around to hear it*). Nor does the Cartesian cogito argument say that thinking is necessary for existence: Thinking is only *logically sufficient* for existence (if you think, you must exist). He concludes viewing philosophy as a waste of time and money that does not deserve any serious consideration beyond few casual, amusing conversations. He also warns, rather trivially, that a philosophy degree “may not lead you to a career path; it might, but it may not.” The same goes for a science degree and myriad other things: It may or may not lead you to a career path. Tomorrow may or may not rain.

It is not entirely clear what motivates these attitudes, but I suspect it is an unfortunate blend of ignorance, misunderstanding, incoherence, and, in some cases, more than a pinch of hubristic disciplinary chauvinism. Others (e.g., De Haro, 2020; Pigliucci, 2014) have pointed out some of these problems, but the most obvious hypocrisy is that all rejections of P are themselves philosophical. In disregarding P, these and other scientists are effectively doing P. The same goes for preferences of S over P, most obviously if based on empiricism, a clearly philosophical thesis. When in that video Nye says he is “skeptical” about philosophy, he is expressing a philosophical position. All this rhetoric begs the elementary questions of what P and S are, how they differ and relate, what makes one better than the other does and why, all of which have much P in them.

Before I continue, I elaborate more on the differences and similarities between P and S. I do not mean these names (and cognates) to denote unbridgeable divides between their referents. Nor do I mean to say they are synonyms. They label distinct kinds of educations, aims, activities, audiences that involve different kinds of contents (concepts, methods, theses, theories, models, problems, solutions, etc.). All these differ but are also similar in equally important and wholly compatible respects. Their differences are not more or less important than their similarities.

This supposition is integral to GA: *The differences between P and S are as defining of their relation as their similarities are.* They are similar in being intellectual pursuits practiced professionally in academia, mostly in teaching and research. As such, both have similar objectivity standards of coherence, generality, relevance, clarity, precision, and logical support (whether inductive or deductive) beyond faith or religious/secular authority. Both also engage in rational criticism and speculative, often counterfactual thinking (both often use the device known as the thought experiment), always acknowledged as such and, hence, taken as revisable.

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Most, if not all, efforts fail to meet these standards to some degree, but all are parts of the PS relation. Some are more successful than others are, but none will ever be completely successful, because *all* proposals, even the standards themselves, are revisable. In the PS relation, there is no final word, nor is there anything written on stone or with blood. What matters is an honest effort to meet the standards as closely as possible, without falling prey to overgeneralizations and oversimplifications.

Their differences come from their professional practices in academia with clear institutional delineations, but only partly. They are no reason to condemn interdisciplinary collaborations, nor do they define P and S strictly<sup>2</sup>. The two are too complex to be defined strictly in terms of necessary and sufficient conditions, let alone the kinds of one-line zingers exemplified above. Neither one is monolithic. Both consist of myriad aspects crisscrossing too intricately to allow for easy neat definitions. Furthermore, they can co-occur more or less synchronously and, hence, are not quite like sides of the same coin. The coin analogy conveys a kind of mutual exclusivity incompatible with GA's emphasis on P and S as too strongly intersecting to view them accurately as being as sharply separated as heads and tails are.

Yet, GA makes no essential qualitative distinction between P practiced by philosophers and P practiced by scientists, or S practiced by scientists and S practiced by philosophers. The differences are mostly in the amount of detail of different kinds of contents. GA acknowledges academic institutional differences, but only as *one* aspect of the relation that does not prevent interdisciplinary interaction. Nor does it imply the absence of equally defining similarities. Under GA, institutional differences define P and S only insofar as they reflect different aims and activities crucially involving different contents, but only partly so.

The differences transcend undergraduate degrees and institutional affiliations, and allow for interdisciplinary interactions. Nothing prevents someone with a P major affiliated to a P department from engaging in substantive activity involving scientific contents and, to this extent, effectively *be* a scientist, even if temporarily. Nor does anything prevent someone with a S major affiliated to a S department from engaging in substantive activity involving philosophical contents and effectively *be* a philosopher, even if temporarily.

Under GA, then, being a philosopher or scientist is not to be identified with having a certain degree or institutional affiliation (see Note 2). Being a philosopher or scientist is more fluid than

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<sup>2</sup> An insidious, pernicious practice in Latin American academia, especially in academic evaluation, is to foster extreme specialization and punish interdisciplinary collaboration, based purely on institutional differences, far more so than Anglo-Saxon academia. In Latin American academia, there are no undergrad minors, only majors, and getting a degree means being typecast academically and held in contempt for dabbling in institutionally alien fields. An undergraduate degree is academic fate and incursions into alien fields cause suspicion. Education is designed to keep different disciplines as sharply divided as possible, by tall thick academic institutional walls with as few interconnecting doors as possible. Usual companions of this practice are essentialist definitions of P as *just* having an undergraduate P degree and S as *just* having an undergraduate S degree. This chauvinistic academic culture against disciplinary diversity has spread to specializations within each field. Physics, biology, and psychology, for example, are just having undergraduate physics, biology, and psychology degrees, respectively. As an example of this from my experience, fellow psychologists have expressed to me puzzlement (to put it mildly) at my incursions into biology. They have asked me rhetorically, often mockingly, 'Why do you study biology? Are you not a psychologist?' as if I was committing professional treason. I remember vividly an episode when we were deciding on a mnemonic four-digit password for an alarm system. Some enthusiastically dropped familiar publication years: '1938!' said someone; '1957!' said another. In my naïveté, caught in the heat of the moment, I suggested '1859!' To my surprise, they asked mystified 'What's that?' To my horror, someone quickly quipped to my answer, very matter-of-factly and offended, 'I am a psychologist and do not have to know that!' These disturbing wails of the willfully ignorant often arise from zealous positions on psychology's 'proper' subject matter (whatever the hell that means), often misused judgmentally against interests in subject matters different from others.' Such positions have afflicted psychology ever since its beginnings and become divisive conventional wisdoms. Psychologists still war over whether their proper subject matter is the mind or behavior, with subdivisions within each subject matter, reaching ridiculous hair-splitting (e.g., behavior versus interbehavior). All this makes for an intellectually incestuous academia that can only result in academic malformations.

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permanent static states, although perhaps most philosophers spend most of their time in activities involving philosophical contents, and most scientists spend most of their time in activities involving scientific contents. The *same* person, however, could sometimes be a scientist, other times a philosopher (in fact, many have). The latter can be obtained equally for anyone who creates their own philosophical contents or uses content others have created.

More importantly, the differences between P and S have not prevented, nor should they prevent, substantive interdisciplinary interactions. Nor are the differences valid reasons to scold scientists for doing P or philosophers for doing S. Nor do their similarities imply they *should* interact: Under GA, no scientist is forced to engage in P, nor is any philosopher forced to engage in S. As will often come across in what follows, GA views the PS relation as one where practitioners of each are *free* to interact with one another *should they wish to do it*. Still, GA recommends that this interaction is more fertile if mutually respectful, especially if accompanied by cultivations of one another's fields.

Much of the above hinges on what philosophical and scientific contents are, and how they differ, but I cannot launch a full discussion of this here, and I doubt the two kinds of contents can be defined crisply and simply. Very roughly and intuitively, it is traditional to view philosophical contents as emphasizing conceptual (what is something) and justification (why believe something) matters. Scientific contents tend to revolve more around matters of empirical evidence, but also, perhaps to a lesser degree, explanation and theorization. But then again, these differences can be quite fuzzy, and do not make P and S as incompatible as P scoffers believe. Again, the same person could engage (in fact, many have engaged) in activities involving both kinds of contents.

One type of content that illustrates the fuzziness of this distinction comprises concepts and their definitions, building blocks of P and S alike. Some concepts are distinctly philosophical (e.g., substance, essence, universal), others distinctly scientific (e.g., gravity, neuron, learning), but many others do not admit such a crisp distinction. For example, the concepts of analysis, biological species, causation, change, computation, complexity, consciousness, definition, evidence, information, knowledge, life, meaning, method, nature, law, reduction, parsimony, space, time, truth, and validity have been equally central to philosophers and scientists alike. None of these concepts can be unequivocally pigeonholed as philosophical or scientific.

Another indicator of the fuzziness I am referring to here is that when scientists define their concepts, they also inevitably face issues that have a strong philosophical import, beginning with the nature of concepts. It therefore is very difficult, if not impossible, to pinpoint where S ends and P begins. For example, when psychologists define the concept of learning (see Burgos, 2018, 2020), an inevitable issue is what aims they seek to achieve. Do they seek to state the true essential nature of learning or just stipulate the meaning of the term 'learning'? The two are related but also different, and facing the issue is integral to the task of defining learning (or 'learning,' as the case might be). When scientists define, then, they are quite poised to do P, especially when they debate over different definitions.

This fuzziness has allowed for honorable exceptions to the extreme anti-P stances illustrated above. P scoffers forget that many luminaries in the history of S have shown much respect for P, and not merely as lip service. An example is Isaac Newton (1642–1726), whose scientific work defined physics for almost three centuries, and remains in many ways central to today's physics (by being questioned, it led to relativistic and quantum mechanics), and yet, it was intensely philosophical. In effect, he engaged in *natural P*, a label that expresses quite well the fuzzy character of the PS relation. Physicists owe it much but cannot coherently praise its

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importance while dismissing its philosophical aspects, even if they were Newton's own, which does not make it any less philosophical (or him any less of a philosopher). He was a scientist when engaged in the topic of gravity, but also a philosopher when engaged in his own natural P, and his work moved rapidly and freely between the two activities.

Another example is Dirac (1958), who wrote this: "The necessity to depart from classical ideas when one wishes to account for the ultimate structure of matter may be seen, not only from experimentally established facts, but also from general philosophical grounds" (p. 3). Einstein (e.g., 1922, 1934) engaged in Kantian P as part of his work on relativity, and his philosophical musings are no less philosophical, nor was he less of a philosopher for it. Similarly, Heisenberg (1959) dedicated a book to his P of physics. I challenge Nye, Tyson, and Weinberg to provide a cogent rationale against the relevance of all these philosophical aspects to these scientists' works.

Similarly, biologists Ghiselin (1969, 1997), Levins and Lewontin (1983<sup>3</sup>), Mayr (1988), and Monod (1971), another Nobel Prize, have engaged in P of biology. It would be hypocritical to praise their scientific work but dismiss its philosophical aspects (or worse, to dismiss their scientific contributions for too tangled with P), if, again, it is plausible to separate the two sharply. I dare Steve Jones and sympathizers to say with a straight face that those biologists are engaged in pornography.

In the spirit of GA, as I elaborate later, my point with these examples, again, is not to argue that scientists should engage in P because some influential scientists have. I do not mean GA to dictate that scientists should engage in P, nor that philosophers should engage in S. I only seek to argue that some such scientists have *in fact* engaged in P, and this invalidates some scientists' anti-P stances. These cases suggest that P is not as bad for or incompatible with S as these stances claim.

Alas, as brave as these scientists' care for P is, much of it suffers from some of the same problems as disavowals of P do, especially a lack of expertise in technical academic P, even general philosophical culture. This problem hinders the way GA envisions the PS relation, and is symptomatic of a widespread myth: That scientific expertise is sufficient to philosophize competently about S. This expertise is certainly necessary, but not sufficient. Philosophical expertise is equally necessary but tends to be lacking in scientists' philosophical reflections, as well intended as they are. Falling prey to this myth causes a major disparity in the PS relation where expertise in one field eventually becomes more important than expertise in the other. GA opposes this kind of inequity.

Believers of this myth commit the fallacy of argument from authority, invalidly inferring expertise in S as implying expertise in the P of that S. Expertise in one would suffice for expertise in the other if the two are closely related, but this decreases as the two grow apart, as is the case of most philosophers and scientists alike. P deriders tend to miss the obvious: If P is as different from S as they propound, each one requires very different expertise. Therefore, scientific expertise is necessary but not sufficient to judge its relation to P sensibly. Philosophical expertise is equally necessary.

A lack of general philosophical culture tends to accompany such disdain and only worsens the situation. The fact that Jones (*ibid.*) somehow managed to publish a review of Pinker's (1997) book about the mind exemplifies this. Except for evolutionary biology and genetics, Jones' fields of expertise (which I would not dare judge), I found no evidence in [his institutional website](#) that

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<sup>3</sup>They wrote this: "We therefore cannot accept the view that philosophy must (or can) be excluded from science" (p. 165). GA agrees that no one has the *right* to say that P *should* be excluded from S. Whether it *can* is a more complicated matter I discuss later.

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made him an expert on the other disciplines relevant to that book (computer science, psychology, neuroscience, and, especially, P).

Even worse is to commit the fallacy to rule that S is inherently superior to P, because, as in Jones' wisecrack, S is like sex, P like pornography, and sex is better than pornography. The obvious blunder here is likening S to sex and P to pornography. Why not the other way around? And why is sex better than pornography? No answers are apparent in Jones' piece. GA strongly opposes these kinds of quick, baseless whims.

To be fair, and contrary to what scientists who have philosophized might suggest, their presence does not entail that *all* scientists *should* philosophize, any more than the fact that some scientists are physicists entails that all scientists should be physicists. Even more obvious, the fact that many people are scientists does not imply that everyone is (or, worse, should be) a scientist. Nor does the fact that some people are philosophers entails that everyone is (or, worse, should be) a philosopher.

The broader fallacy to avoid here is overgeneralization, descriptive and prescriptive: It is invalid to infer general accounts of a group from particular accounts about some of its members, *no matter how numerous* (unless, of course, they are true of *all* members). Even if most members of a group are *F* and just one member *a* is something else, *G*, it is invalid to conclude that *a* is or, worse, should be *F* and not *G* (assuming they are incompatible, which need not be the case). This fallacy underlies tragic injustices committed in the name of bigotry, such as sexism, racism, nationalism, and religious prosecution. GA is against this fallacy in depictions of the PS relation. The fallacy also underlies downplaying the relevance of P to S arguing that a minority of scientists philosophize (as Steve Jones suggested). In a sense, such attacks indicate a hubristic disciplinary chauvinism that is widespread throughout academia, a situation that GA laments.

These reflections also suggest an etiquette principle that rejections of P by scientists violate too often: Be respectful of others' occupations, regardless of one's expertise in them, but especially if little. I am sure Jones would be offended by amateur criticisms of genetics, and rightly so. His amateur criticisms of P, just like those from other scientists, are equally offensive. In general, inexpert criticism of others' fields are among the most egregious forms of academic disrespect and helps no one. They only escalate an already tense situation. The disregards of P from those and other scientists are equally deplorable in this regard. Their simplistic guilty verdicts against P are intellectual low points.

In the spirit of GA, the above also goes for philosophical denials of S. There is enough blame to go around in this unfortunate war between P and S, and professional philosophers have not been the exception. A famous influential example is sociologist Latour's (1987) biased, conceited, and comically oversimplifying interpretations of his social studies of working scientists (see Amsterdamska, 1990; Gross & Levitt, 2007; Searle, 2009; Sokal & Bricmont, 1998). The obvious fallacy underlying this kind of work is that casually watching molecular biologists working in a particular lab suffices to understand S<sup>4</sup>.

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<sup>4</sup> Culprits of this fallacy hope to become scientists without all the time and effort real scientists invest in learning their trade. These culprits are fake scientists. Many years ago, I asked a colleague who did research in the psychology of physics about her expertise in the contents of physics, to which she admitted she was completely ignorant, but did not care. After I picked my jaw up off the floor, I replied that she was a fake physicist and, consequently, her research would have little if any credibility among physicists, presumably a key part of her target audience. My claim above, that philosophers of S must be experts in the contents of the S they philosophize about if their work is to be meaningful to its practitioners, also applies to psychologists of S. Their work can hardly have any impact on the sciences they study without an expertise on the contents of these sciences.

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The result is a caricature of S, illustrated by the book's initial figures (e.g., see Figure I.1, p. 4) that depict it as Janus bifrons of "ready made science" opposite to "science in the making." These figures depict S as male head side profiles, the former bearded looking west, the latter clean-shaven looking east. Not only is the sexism in these figures appalling, but I also fail to understand why depict ready-made S as bearded.

There is nothing inherently wrong with social and psychological studies of scientific activity. Quite the contrary, they could be very illuminating about the *context of discovery* in the study of S, which encompasses all the extra-logical, biopsychosocial aspects of scientific activity. The problem is when these studies become grounds for deriving philosophical theses that belittle S. Even if restricted to this context, S is much more than what Latour and colleagues observed molecular biologists do in a particular laboratory. S also includes anatomy, physiology, and integrative biology in many other laboratories, as well as physics and chemistry in myriad more laboratories. There also is *theoretical S* often practiced outside laboratories, but gets far less attention in that book. To pretend to reduce all science to molecular genetics as practiced in one lab is naive at best, delusional at worst.

The linguistic products (typically, publications) of what scientists have done are equally constitutive of S' contents. I use the term 'contents' broadly and roughly in reference to anything that can be identified reasonably clearly as having been written or spoken by scientists as an end-result or output of their research work, whether theoretical or empirical, in some relatively persisting medium. The typical media are printed publications (papers, books, chapters, monographs, etc., on paper as well as digitally), but they also include video and audio recordings, quite common in the internet age. These contents consist of expressions of various sorts (e.g., statements, hypotheses, models, theories, experimental results, problems, questions). The scientific contents need not be restricted to highly specialized products, but can also be introductory (e.g., textbooks, video-tutorials).

S, then, is not *just* the processes that lead to its contents, but also the contents *themselves*. A view of S that focuses just on its constitutive processes (whatever they might be, a yet-unresolved issue endemic to the psychology of science; e.g., see O'Doherty, et al., 2019) is half-correct, at best (quite meaningless to practicing scientists if driven by ignorance about the contents of their S; see Note 4). To state the obvious, if S were only the processes of doing S, no scientific contents would be, well, scientific. Clearly, Newton's *Principia* and Darwin's *Origin are* scientific, even if they are contents that resulted from certain psychological processes, not the processes themselves. These particular processes, like those of all nonliving scientists, are not amenable for psychological or sociological studies. Clearly, then, a psychology of these scientists' activity is inevitably and highly speculative. Not that there is anything wrong with that per se, but goes awry when unacknowledged.

That is to say, the so-called context of *justification*, consisting of all considerations about logical aspects of scientific contents, is equally constitutive of S. They include considerations about coherence, clarity, truth, precision, generality, explanatory power, evidential support, and relevance of various scientific contents. They usually are *normative* in dictating how they should be fulfilled (e.g., scientific theories and explanations *should* be coherent and have evidential support). The kinds of methods used in the psychology of S are unsuitable to articulate these considerations. Technical methods of philosophical analysis of scientific contents, such as formal logic and set theory (e.g., Pollock, 1990), are more suitable.

Philosophical analysis as rational reconstruction of scientific contents *in and of themselves* through such methods is analogous to archeological artifact analysis through methods such as



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radiocarbon dating, dimensions measurements, and determinations of various kinds of materials (clay, glass, iron, bronze, porcelain, etc.). In this sense, philosophical analysis is archeology of S, although the products it studies can be quite recent. Archeology is much more than such artifact analysis, but the latter is *integral* to the former, or so I guess from my very brief, naive incursions into this discipline: Archeology also, and *equally importantly*, is about these artifacts in their cultural and historical context, but not *only* about this. I am no archeologist, but I dare say that no expert would seriously say that archeology is *just one* of these things. It is at least *both*, perhaps more.

By ‘in and of themselves’ I do not mean that archeology and P are or, worse, should be, *only* analyses of certain products (artifacts and publications, respectively). I only mean that products can in practice (and, hence, in principle) be studied *in their own right*, as a matter of temporary focus for certain aims, without any reference to the processes that led to the products. This study is entirely legitimate, even necessary, but insufficient. Archeology and P are more than analyses of products *qua* products. P is *also* the scientific study of scientific activity (the psychology and sociology of S), and archeology is *also* the study of the cultural and historical context of artifacts. The two kinds of studies are distinct and autonomous, but also closely linked, like spouses and siblings: No one would seriously believe that my sister and I, or my wife and I, are *one* person or are unrelated. We are *two* (numerically) different, independent albeit closely related people.

Same with philosophical analyses of linguistic products of scientific activity and the psychology of this activity, or archeological analyses of artifacts and the study of their historical and cultural context. In each case, there are two distinct, autonomous but closely related areas of study. Touting one as less relevant than or reducible to the other is as silly as saying that my sister is the same as, or essentially or fundamentally more or less important than, I am.

Analogously, and closer to my expertise, musical analysis is the study of products of music composition (pieces usually on printed sheet music using a special notation) per se. In this field, it is common to say, for instance, that Beethoven’s 5<sup>th</sup> is a symphony (a musical form different from a concerto, a sonata, or a suite). It has four movements, and was written in C minor and a 2/4 time signature, opening with the very famous short-short-short-long rhythmic motif used in different ways throughout the piece. All this (and much more) is *necessary* for a comprehensive understanding of the piece, but stands independently of the composer’s life.

A better understanding results from speculations about Beethoven’s possible psychological states while he composed his symphony. A famous but controversial speculation is that his deafness, a tragic event in his life, was becoming apparent at the time, and led him to think of the motif as fate knocking at the door, which gave it the name of “fate motif.” Another speculation is that he was resilient to not letting fate bring him down, supposedly expressed (with the same rhythmic motif) as the euphoric C-major finale that contrasts starkly to the ominous opening C-minor beginning. This finale has made the piece earn the name of ‘Victory Symphony’ (the same rhythmic motif is the Morse code for ‘Victory,’ although this code does not capture the elated C-major feature). All this makes for an intriguing story about an inner struggle in Beethoven’s psychology.

The point of this example, also key to GA, is that the two kinds of studies are equally sovereign, but wholly compatible, even complementary. Neither one is more ‘essential’ or ‘fundamental’ than the other (whatever that means), or, worse, that practitioners of one *should* dedicate more efforts to the other. The two kinds of study (in the music example, musical analysis

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of products of musical composition in and of themselves, and the psychology of music composition) are autonomous but entirely compatible and, in fact, mutually supportive. *Both* further an understanding of the musical piece. There is no need to reduce one to the other, or view one as more ‘essential’ or ‘fundamental’ (whatever that means) than the other.

The same applies to studies in the context of justification (the logic of scientific linguistic products) and the context of discovery (the psycho-sociology of scientific activity): The two are entirely compatible and mutually nourishing. There is little if any point to try to reduce one to the other, or view one as more ‘essential’ or ‘fundamental’ than the other.

All of the above is part of the more harmonious view of the PS relation GA favors. It seeks to avoid extreme views that reduce the P of S to either context of discovery (e.g., see Kornblith, 2014) or context of justification (to which logical empiricists, logical positivists, and falsificationists restricted the P of S). Such reductions are as pointless as are those of all archeology to only analysis of artifacts in and of themselves (or their historical, cultural, psychosocial context), or all music only to either musical or psychological analysis.

Nor does GA seek a sharp divide between the two contexts either. For example, there is much legitimate and illuminating research on the psychology of logical reasoning, whether inductive or deductive (the two forms of reasoning most widely discussed in normative P of S). GA, rather, favors a *pluralistic* view where philosophers, psychologists, and sociologists of S are free to study either or both contexts, should they need and wish to do it, without seeking to reduce all P of S to psychology of S, or vice versa.

All of the above has broader implications for how GA views the PS relation: It is pointless to elevate one above the other in any way, reductive or otherwise, especially based on ignorance. The relation could be far less bigoted and, to this extent, more constructive than that. By portraying it through the kinds of one-line zingers anti-P scientists and anti-S philosophers have used (“Philosophy is pornography,” “Philosophy is dead,” “Against philosophy,” “Science is Janus bifrons”), they only make fools of themselves.

Experimental psychologists, major representatives of scientific psychology, have been also wary of P, but more scholarly, cautiously, and respectfully so than anti-P physicists and biologists. Historically, the experimental psychologists’ wariness toward P has arisen largely from psychology’s widely heralded independence from P. The official textbook story is that experimental psychology began with the foundation of the first laboratory by Wilhelm Wundt in Leipzig 1879. This landmark event, the story goes, made psychology more scientific in that matters only philosophers had thus far studied through speculative reasoning, in particular, the nature of consciousness, was also amenable to experimental and scientific study, insofar as experimentation is defining of S (again, ironically, a philosophical thesis in itself).

The assumption that experimentation made psychology more scientific and, to this extent, less dependent on P, was present in early textbooks. In what seems to be the earliest one, Lindworsky (1922) stated that “The experimental psychology is currently in the process of detaching from philosophy, which physics has had for a long time” (p. 2, translated with Google Translate). In another early textbook, Pierón (1929) claimed that

Science and philosophy have been for a long time confounded. What characterizes science is the appeal to verification, the subordination of theories to facts, in short, the experimental attitude; while philosophy is satisfied with internal logical coherence and limits itself progressively to problems which cannot be submitted to experimental control.  
(p. 1)

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This quotation exemplifies and allows elaboration of the P naysayers' hypocrisy I pointed out earlier regarding the strongly philosophical character of some scientists' antipathy toward P. The quotation raises several obvious questions in which experimental psychologists have not shown much interest. What is verification? What is so good about "the experimental attitude"? How do theories subordinate to facts? What are facts? What is logical coherence? If S is also concerned with logical coherence, how does it relate to experimentation? Does this concern not indicate a substantial overlap with P that challenges its sharp separation from S? Philosophers have had much to say about all these questions and dismissing them would be not only hypocritical but also self-defeating. GA encourages scientists to look at what philosophers have had to say about these matters, if only to realize that they are not as straightforward as they seem to be.

In similar spirit, Woodworth and Schlosberg (1954) stated at the outset that "experimental psychology came as a challenge to the older mental philosophy" (p. 1). Underwood (1966), in the second edition of his classical textbook, used the term 'philosophy' only once to claim that "In its early days, psychology was closely associated with philosophy, but as psychology became more and more empirical, the commonality of interests was lost, and two strikingly different independent disciplines now exist" (pp. 299–300). This quotation also begs the obvious philosophical questions of what it means to be empirical (again, empiricism is a philosophical thesis!) and what is so righteous about it.

Exceptionally, in the text I used as an undergraduate psychology student, McGuigan (1968) was much more open to academic P: "An understanding of the philosophy of science is important to an understanding of what science is, how the scientific method is used, and particularly of where experimentation fits into the more general framework of scientific methodology" (p. viii). This claim, however, is still too restricted. I would add ontological considerations in the P of S and the P of mind as potentially beneficial to experimental psychology. Discussions about the nature of consciousness and its relations to brains, for instance, have been integral to scientific psychology since its beginnings. With the advent of cognitive psychology in the late 1950s, the nature of cognition and its relation to brains became another central ontological issue in scientific psychology. Equally integral have been methodological discussions in the P of mind (e.g., the validity of introspection).

As intuitively appealing as it might be, the independence metaphor does not quite do the job psychologists might intend it to do, as it does not mean that P and psychology should forever remain in complete isolation from one another. The USA independence from the British Empire did not stop USA or British citizens from visiting one another's countries. These citizens are still allowed, even welcome with open arms, into one another's territories. Independence does not entail visit bans. In fact, the two nations have shared numerous historic productive alliances (e.g., in World War II) without losing their sovereignty. These alliances evidence that cooperation is sometimes better than conflict (sometimes it is not, admittedly, and when it is, it always is imperfect), and entirely compatible with independence. Analogously, many if not most humans leave their parents in pursuit of independence, but it would be fallacious to conclude that they should never again visit one another. There is a complete breakup in many cases but, fortunately, this is not by any means universal.

P and psychology need not be the exception: Experimental psychologists (and scientists in general) can still visit the philosophical country (or, more precisely, continent), and vice versa, as often as they need and wish to. This more constructive relation, which GA fosters, echoes McGuigan's (1968) favorable attitude toward P, but is also consistent with the independence

rhetoric of other experimental psychologists. Pierón (1929), for example, accompanied it with the admission that “the separation between philosophy and psychology has not been effected without allowing some connections to persist” (p. 10), but it is unclear if he approved of this situation.

The independence metaphor thus seems more likely to encourage fruitful P-psychology (and, more generally, PS) collaborations, but does not preclude other vices. One relates to the other, aforementioned metaphor of P’s death. Framing it in the parenting analogy, and indulging in a bit of metaphysics, my coming into existence depended on my conception by my parents, but after I was born, my continued existence became independent of theirs. My mother’s death, in particular, as sad as it was, did not mean the end of me, fortunately. Nor does my demise need mean that of my daughter’s, even if her coming into existence depended ontologically on my conceiving her with my wife.

This clarification leads to another common metaphor: P as the “mother” of all sciences (e.g., Ferrater Mora, 1960, p. 70; Koffka, 1935, p. 6; see also Pigliucci, 2014). A problem with this metaphor, under GA, is that it gets too close to other common metaphors: P as the ‘Queen’ of all sciences (an extension of the Kantian aphorism for metaphysics), and the Platonic Philosopher King. GA is against both metaphors because they lead too easily to arguments from authority. Rejecting them is key to P and S alike, as well as their relation, not only for being fallacious and against the kind of rationality supposedly sought-after in P and S. Such arguments can also backfire on philosophers: Scientists could turn them right back and say that the Philosopher Mother/Queen/King is dead. Arguments from authority can also engender what Skinner (1953) called “countercontrol” (e.g., pp. 323, 358), a kind of resistance reaction against what is perceived, sometimes correctly, as attempts at hostile takeovers (of which monarchies are all too reminiscent).

Paradoxically, many scientists today fall prey to the fallacy, replacing secular and religious authority with scientific authority, inspired by Francis Bacon (1561–1626), Galileo Galilei (1564–1642), and René Descartes (1596–1650). These scientists thus seem to adopt the metaphor of the Scientist King, also contrary to GA for the same reason: A rejection of arguments from authority as fallacious (see Boden, 1980, for a related criticism that dubs appeals to authority as “paternalist intellectual imperialism,” p. 133). Under GA, these authority metaphors result in the PS relation.

In general, then, under GA, authority metaphors make it easy to commit the fallacy of argument from authority. Perhaps talk of expertise makes it less easy, but these reflections raise the larger issue of why speak of authority or even expertise at all. Is it necessary? Why? GA suggests the possibility that such talk may well be a hindrance, at least for the kind of positive, constructive, perhaps even realistic depictions of the PS relation GA favors. Such talk, it seems to me, seeks mainly to save us the time and effort that requires becoming an expert on something. Although this aim is fine (especially if sought after wisely, with moderation), it does not seem suitable for the kind of PS relation GA envisions.

Expertise (better than authority) can be useful to guide us where look to become experts, but does not replace a thorough, personal investigation of the topic of interest beyond abbreviated versions of a few cherry-picked experts’ work. GA recommends scientists interested in P and philosophers interested in S to go beyond such abbreviations. Do not settle for them. Dig deeper and do it critically, trying to get as many sides and details to the topic as possible. Then, assess as objectively and thoroughly as possibly the implications for the aim pursued.

Also contrary to GA is grounding the PS relation on some kind of disability allegedly suffered by either one, like blindness, confusion, muddiness, neediness, or emptiness, and helped by the other. Einstein (1949), for example, famously wrote this: “Science without epistemology is—insofar as it is thinkable at all—primitive and muddled” (pp. 683–684). Stretching the Kantian

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blindness trope, Lakatos (1971) claimed that “[h]istory of science without philosophy of science is blind” (p. 91). Some have expressed the relation of particular sciences to P in terms of needs. Cognitive scientist and philosopher, Thagard (2009), wrote that “[c]ognitive science needs philosophy,” and physicist, Rovelli (2017), wrote that “physics needs philosophy” (p. 481), a claim Laplane et al. (2019) generalized to all sciences in a *PNAS* opinion piece. In the same spirit and letter, De Haro (2020) wrote, “the natural sciences need philosophy” (p. 297).

Cooper (1965) anticipated this need and the notion that it is mutual: “[T]he reverse is also true” (p. 59). That is to say, P also needs S. Others agree, admitting that “[e]pistemology without contact with science becomes an empty scheme” (Einstein, *ibid.*), “[p]hilosophy of science without history of science is empty” (Lakatos, *ibid.*), “[p]hilosophy needs physics,” and “philosophy needs science just as badly” (De Haro, *ibid.*, p. 301).

These assertions are in the spirit of temperance favored by GA, but when made more precise, some state the obvious. P of physics, P of biology, P of psychology, and P of neuroscience evidently need physics, biology, psychology, and neuroscience, respectively. More generally, P of S undoubtedly needs S. Without S, P of S would not just be empty, but nonexistent. This most philosophers of S will admit as self-evident and, hence, indisputable.

Whether S could exist without P is more controversial (see Note 3). Anti-P scientists will quickly cry it does, but this is simplistic. Historically, the evidence in available ancient Greek texts indicates that both arose during roughly the same time and are seemingly closely tied. There also is abundant textual evidence of close relations between P and S ever since. Scientists will likely and rightly claim this does not entail that S came into existence thanks to P, but it does not entail that S *would* have arisen without P either. Any argument to the contrary would be counterfactual and, hence, contrary to the empirical stance of anti-P scientists.

It thus is very difficult if not impossible to know beyond reasonable doubt which one came first and bred the other historically, P or S. Even if this were possible, it is unclear exactly what it would imply about the nature of the PS relation. It would surely *not* imply that either one is superior in any way than the other is. Nor would it imply that they exist today without one another. If P were indeed the ‘mother’ of S (whatever that means), as some philosophers have claimed, anti-P scientists could insist that P is dead and, hence, S exists today without P. Maybe, maybe not. It is not clear-cut and strongly depends on myriad of specifics. Again, much hinges on what P and S are and how they differ from one another, which are philosophical issues. Hence, the argument that S currently exists without P, scientists must philosophize, in which case S would not exist without P either. Perhaps, then, it is better to abandon all this rhetoric about whether P and S can exist today without one another altogether in depictions of the PS relation, or so GA advises.

This narrative also quickly becomes arrogant, intrusive, and promotes countercontrol. Contemptuous reactions to it are epitomized by the rhetorical ‘Who needs that?’ and underlie many adversarial positions from scientists towards P. My quick reaction to philosophers and scientists telling me I *should* need or heed them is to ask ‘Who are you, my mother?’ (See how easily this kind of rhetoric leads back to the mother metaphor). Philosophers saying that S needs P is a fast way to lose scientists. GA’s recommendation to philosophers in this regard is to stop viewing P as the Queen Mother of S.

If one wishes to insist in a family-resemblance metaphor, it is more constructive (and valid) from GA’s perspective to view P and S as *fraternal twins* bred by the same parents: Wonder, intellect, and imagination. Sometimes the twins enjoy their mutual company, but they also spend much time without one another. Other times they might need one another, yet other times they

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have their bad moments, as all siblings do. Some have more bad times than others do, but this does not warrant overgeneralization: The fact that some or even *most* sibling relations have been a certain way does not *entail* that *all* are (or, worse, should be).

If one is not into this metaphor, an alternative that is less intrusive and less conducive to countercontrol is to view the PS relation in terms of what philosophers and scientists can *kindly offer* one another. Much in GA is about a respectful constructive attitude as key to the aforementioned etiquette: Better say ‘This might help’ than ‘You need this’ or, worse, ‘You should need this.’ The latter sounds like a car dealer shoving products and services down prospective clients’ throats. An intellectually healthier metaphor is that philosophers and scientists are like friendly farmers in a market, displaying their goods to passersby and letting them freely choose what they want, if anything.

A common form of the necessity/dependence rhetoric is to view P as *foundational* to S, yet another practice GA does not recommend. Not only does this foster countercontrol, but it also is misplaced. A familiar example is to postulate some methodology as foundational to scientific research. A common manifestation of this is the dictum that hypotheses (or theories or models) must be ‘testable’ to qualify as scientific, stated in virtually all writings on research methods in psychology over the past half century. This dictum too, begs eminently reasonable but intensely philosophical questions. Exactly what is testability? Why does it define the scientific method? What are hypotheses and how do they differ from theories and models? If they are testable in principle, should they be testable in practice?

All these questions are intensely philosophical and have multiple answers not discussed thoroughly anywhere in psychological research. Many are mutually exclusive, which raises the further issue of how to decide on the correct answer. Why are some answers preferable to others? There is nothing inherently wrong with asking and trying to answer these questions. What is wrong is to dismiss attempts to answer them thoroughly as superfluous or too philosophical. It is intellectually duplicitous to state that scientific hypotheses are testable and then refuse to elaborate.

Another example, closer to the target audience of this journal, is to use the works of Ryle (1949) and Wittgenstein (1953) to validate philosophically some form of behaviorism (e.g., Day, 1969; Deitz & Arrington, 1984; Ribes-Iñesta, 2006; Schoneberger, 1991). The same applies to Bennett and Hacker’s (2003) proposed “philosophical foundations of neuroscience,” which rely on an uncritical acceptance of late-Wittgensteinian P, as well as a very debatable accusation of cognitive neuroscience as dualistic (see Burgos & Donahoe, 2006; see later). Some efforts have combined this P with Rylean P, without considering important idiosyncratic differences between the two (e.g., Bouwsma, 1972; cf. Ryle, 1972), and how they might affect their foundational power.

My reaction to these efforts is to ask about what their aims and how successfully they are achieved. This reaction invariably goes unanswered, but points at formidable problems with those works (e.g., Ayer, 1956, p. 11; Christofidou, 2018; Goldman, 1988, p. 3; Elgin, 1996, p. 93; Nagel, 1986, pp. 22-25; Place, 1999, pp. 387–395; Sellars, 1956; Stich, 1991, p. 20; Williams, 1991, pp. 28, 157, 158). These problems are too extensive to examine here even briefly, but I still have to see a thorough critical analysis of how they affect the success of such efforts’ aims.

A general takeaway from this, integral to GA, is the futility of efforts to depict the PS relation as foundational. There is no way to demonstrate beyond reasonable doubt, philosophically or scientifically, why certain philosophical proposals are better foundations to certain parts of an S than others are. All efforts of this type that I know of take their preferred proposals uncritically for granted, because they conveniently befit a scientific agenda (e.g., validate some form of

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behaviorism or other), without considering the implications of their shortcomings for their foundational power.

This outcome also applies to uses of scientific results as foundations for philosophical proposals. An example is Kuhn's (2012) use of Gestalt psychology on perception and cognition as foundations for his philosophical musings about scientific discovery. Presumably, his own notions of paradigms, normal science, and scientific revolution apply to psychology. Some (e.g., Boden, 1980; Farrell, 1978; Joynson, 1980; Kline, 1988; Warren, 1971; cf. Palermo, 1971; Weimer & Palermo, 1973) have argued that this application implies that psychology is preparadigmatic, immature S. But if this is the case, Kuhn's use of Gestalt psychology is a weak foundation, at best, but he seems to have taken it for granted<sup>5</sup>.

Such use is also quite mentalistic, which should give pause to behavior analysts who support their proposals on the idea of a paradigm (e.g., Bailey & Burch, 2018, p. 8; Baum, 1997, 2002; Haegele & Hodge, 2015; Kelly III & Gravina, 2017; Stewart, 2018). This practice is obsolete, at best; incoherent, at worst. It is incoherent because of its very mentalistic character. It is obsolete because in the second edition of his book, published in 1970, Kuhn recommended abandoning the term 'paradigm' for being too vague and replacing it with the much more accurate expression "disciplinary matrix." I still have to see a scientist who reasons in terms of 'paradigms' heed this specific recommendation (for this criticism in psychology, see O'Donohue, 1993; Weimer & Palermo, 1973). Unfortunately, it was too late. The damage was done, and 'paradigm' stuck.

Scientists might be using the term 'paradigm' loosely, broadly, and informally, but this is the problem: Such careless uses, which Kuhn eventually repudiated, result in equally careless reasoning about 'paradigm shifts,' 'new paradigms,' 'paradigmatic/preparadigmatic science,' and 'scientific revolution.' Perhaps by 'paradigm' they mean 'approach,' 'view,' 'perspective,' or 'procedure' but if they do, these terms are preferable for being more precise. Far from providing a sound philosophical foundation, reasoning in terms of 'paradigms' is unhelpful, even harmful.

Worse, such uses typically seek to favor one "paradigm" over others. This strategy, however, is inconsistent with Kuhnian methodology, where different disciplinary matrices are qualitatively distinct and, hence, *incommensurable*. That is to say, they are not quantifiable in any way that allows for a meaningful ordering. Consequently, this methodology does not validate any rationale for preferring a certain disciplinary matrix to others.

Another attempt to ground a philosophical proposal in an obsolete scientific theory is Sober's (1984) interpretation of natural selection in terms of Newtonian mechanics, which is false, at least by the truth standards of physics, which seem to be close to a correspondence theory. I would think that this weakens, if not obliterates, the foundational power of this theory for anything, including behavioral-momentum theory (e.g., Nevin, Mandell, & Atak, 1983)<sup>6</sup>.

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<sup>5</sup> Contrary to conventional wisdom, I have found no clear indication that Kuhn viewed psychology as immature science. If he did, it would denote a flagrant inconsistency, insofar as immature science is a weak foundation. Moreover, he did not acknowledge what all psychologists know very well, that Gestalt was just one of the major psychological schools of days of yore. There were also, in no particular order of importance, cognitivism, behaviorism, functionalism, humanism, structuralism, and psychoanalytic. Although Kuhn emphasized perception in his musings about the psychology of normal science and scientific revolutions, psychologists will point out that perception is just one aspect that interacts importantly with others (e.g., cognition and learning).

<sup>6</sup> Some might reply that, contrary to those widely rejected theories, Newtonian mechanics works extremely well at lengths, masses, and speeds of normal-sized objects, and that the phenomena of interest (natural selection, behavior) take place at such scales. If Newtonian mechanics was good enough to take humans to the Moon, it should be

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This obsolescence problem with both kinds of foundational efforts (philosophical foundations of S and scientific foundations of P) arises from the fact that P and S have evolved over the centuries. Amended proposals have improved those of classical philosophers and scientists. To this extent, improved proposals should presumably be preferable foundations, but even they are quite diverse, sometimes opposing – especially in psychology, and revisable. It is often unclear which one is best, largely because exactly what ‘best’ means in this context can be very difficult to pinpoint. They also tend to be sufficiently complex to make thorough assessments of their foundational uses very difficult.

Such futility also arises from another key feature of how the PS relation has often occurred: Strong reciprocity. In such occurrences, the relation has been too reciprocal to warrant generalizing the kinds of one-way portrayals favored by foundationalism and other forms of asymmetric, precedence dependence mentioned above. Insisting in a blood-kinship metaphor, the aforementioned fraternal-twins one is more akin to GA. Foundation and motherhood/monarchy are asymmetrical relations, so it makes little sense to say that P and S are foundations or mothers of one another. A house cannot be a foundation of itself. Nor can anyone be a mother of oneself (at least biologically speaking).

GA does not oppose *all* asymmetrical relations between P and S, only those that entail any kind of superiority of one over the other, like the ones discussed above. There could be asymmetries that do not have this implication, and they are fine with GA, especially if they are constructive. For example, P contents could be more extensive than S contents, or vice versa, across different relations and within the same relation at different times.

Another form of dependence GA opposes is to the PS relation as mutual therapeutic help in curing some alleged “diseases” that afflict them. Such “diseases” are too generic and metaphoric to be of much use. What does it mean that S without P is ‘blind’? Telling scientists this is a fast way to lose them. What does it mean that P without S is ‘empty’? GA, again, views P and S as standing on their own, academically and professionally, with all their strengths and weaknesses. GA encourages, as more fruitful and less invasive, efforts to treat them as matters of *internal affairs*. Each field is better off by making up for its own weaknesses and taking credit for its own strengths, although practitioners of either one are free to kindly offer their services to the other, as suggested in the farmers’ market analogy above.

All of this suggests that the PS relation is too complex to validate any of the simplistic depictions common in the literature, like the ones illustrated above. GA thus recommends abandoning such depictions altogether. A means to heed this recommendation is that interested scientists increase their expertise in P and interested philosophers increase their expertise in S, if they wish to use one another’s fields for some aim. This strategy makes for a fairer PS relation, insofar as it leads to fairer, more valid depictions of one another. There is too much ignorance about P among scientists and S among philosophers, which thwarts valid depictions. Here, ignorance is more harmful than blissful. My experience in practicing both has been that knowing more about P has made me a better scientist, and knowing more about S (not just in psychology, but also in physics, chemistry, and biology) has made me a better philosopher of the S I practice.

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equally good to explain those phenomena, the argument might go. Maybe, maybe not. I am not so sure. I would have to think more about the analogy between going to the Moon and explaining those phenomena. It also is unclear how this improves the theory’s foundational power if it crucially involves some inferential scheme defined by validity and soundness, where truth remains central. Few alternatives remain, short of adopting pragmatism about truth or dismissing truth as irrelevant, both very problematic options (the former far more so than the latter, to be sure).



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Instead of the asymmetrical views discussed before, GA recommends, as anticipated above, viewing the PS relation in terms of *using* certain contents (concepts, methods, theories, theses, models, evidence, questions, problems, etc.) as means to attain certain aims. The relation, then, is also strongly *selective*, limited to *specific* aims and means. The more specific, the better vis-à-vis more realistic depictions of the PS relation. GA thus also recommends assessing such uses on a case-by-case basis, attending to the details where God and the Devil are, preferring *special* to general P and S. The specifics are key. Viewing any of them as ‘foundational’ or ‘authoritative’ does not help in any way. Perhaps it is better not to speak of ‘foundations’ at all. It is too strong a word for what these uses seek to do. Less hostile, more sensible words, I think, are ‘framework,’ ‘background,’ and ‘context.’

To finish, I summarize three examples of uses of specific philosophical contents related to S for specific aims. The first two examples illustrate an interdisciplinary collaboration between neuroscience and the P of mind. The third example illustrates a relation between computational modeling of conditioning and a bit of P of S. The examples provide sources for case studies of the PS relation that in some ways approximate GA, but in some ways does not.

In the first example, already mentioned, Bennett and Hacker (2003), a neuroscientist and a philosopher, respectively, joined forces to make a Wittgensteinian critique of cognitive neuroscience. They argued that cognitive neuroscientists committed the mereological fallacy by attributing features of whole humans to brains, which are only parts of humans (e.g., saying that brains think, perceive, decide). The authors further argued that this made cognitive neuroscience Cartesian, a potentially serious accusation, given the strong association with dualism.

GA supports these kinds of strong critical, but also constructive interdisciplinary collaborations. A major positive feature is that the book exudes philosophical and scientific scholarship, so the authors’ expertise gives confidence that they know what they are doing, in contrast to other efforts to link P and S. The authors are very professional and respectful, without compromising their critical stance. The contents are quite specific (later-Wittgensteinian ordinary-language P and cognitive neuroscience) and seamlessly intertwined into a coherent, detailed tapestry in which the reciprocity between P and S comes through compellingly.

All these pluses notwithstanding, and as an example of how digging deeper can make important differences, there also are minuses, beginning with the title, which expresses a foundationalist outlook not supported by GA and likely to cause countercontrol in cognitive neuroscientists. In addition, and more precisely, it is far from clear exactly how committing the mereological fallacy is deleterious to cognitive neuroscience. Conversely, exactly how does not committing the fallacy improve cognitive neuroscience? Will it lead to better experiments and theories? Does committing the fallacy invalidate all previous data from cognitive neuroscience? Why? It is clear that committing the fallacy is shoddy thinking, but much less clear is exactly how this translates into bad S. Does committing the fallacy make current cognitive-neuroscientific theories false? How? There must be deep relations between concepts, statements, theories, methods, and evidence, but I am unsure exactly how such relations obtain.

Moreover, in a review of the book, Burgos and Donahoe (2006) agreed that cognitive neuroscientists committed the fallacy, and that this was a problem, but argued that it did not (in fact, *could not*) make cognitive neuroscience dualistic. According to dualism, souls (or minds) are immaterial in that they lack all three spatial dimensions. Therefore, souls thus conceived cannot be *spatial* parts of anything, bodies and brains included. Consequently, it is logically impossible

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to commit the fallacy in dualism and the accusation of cognitive neuroscience as dualistic or even Cartesian is false.

If anything, committing the fallacy makes cognitive neuroscience materialistic through and through (I am not suggesting this as a way to argue for materialism; there are better ways), which brings me to my second example. A defining assumption of both forms of mentalism (functionalism and reductive materialism) is internalism about the mind, the thesis that mental events, states, and processes are internal. Internalism does *not* entail the fallacy. For example, reductive materialism states that all visual experiences are brain properties. If all brains are internal, then all visual experiences are internal, but this does not imply that brains have visual experiences. To say that a person sees is entirely consistent with reductive materialism (and functionalism). However, the fallacy entails internalism. If brains think and are internal, then thinking is internal, but as Burgos (2015, 2016) argued, none of this entails any form of dualism. Quite the opposite: It entails materialism.

As a third example, this time from the P of S, in a critique to a neural network model, Calvin and McDowell (2015) said the following:

The most important question from this project's results is whether the UTR [Unified Theory of Reinforcement] is falsified by them? The answer is no; these results falsify the standard UTR neural network model, but this conclusion cannot be generalized to all potential UTR models. In fact, it seems unlikely that the UTR is falsifiable through simulation at all because the theory currently permits a near infinite number of potential models. (p. 59)

Viewing this as an important question certainly is progress, although GA does not go as far as saying that it is "the most important question." This detail aside, Burgos and Donahoe (2016) replied at length by making clarifications that illustrate much of what is wrong with uses of P by scientists according to GA. For one, Calvin and McDowell (2015) confused non-falsifiable with infinitely falsifiable, an amateur mistake that denotes a lack of expertise in falsificationism (there might be something in their distinction between "the standard UTR model" and "potential UTR models," but we honestly did not understand it). Quite the opposite: The model is *infinitely* falsifiable and, in fact, has been falsified many times.

Another clarification in the reply was that this fact did not necessarily mean to abandon the model. This clarification exposes another common misuse of P in S that GA avoids: To reject a scientific model or theory for having been falsified. Popperian falsificationism does not work like that. If it did, nothing in S would stand. This methodology provides a very clear rationality to continue working with falsified theories: To better define their truth and falsity contents, needed to assess their verisimilitude and compare them.

This particular use of certain philosophical contents (falsificationism) to criticize certain scientific proposals (a neural-network model of conditioning) thus failed on two related counts due to misconceptions of how falsifiability works, according to GA. One, the use failed to depict the model as unscientific for unfalsifiable. The moral here is that anyone who seeks to criticize anything, whether in/from P or S, better be well prepared to do it. There are no easy quick ways to criticize P or S. GA is all for criticism, as long as it is scholarly, competent, and constructive. Two, the attempt also opens the door to another common misconception, that all falsified models and theories, the criticized neural-network model included, should be rejected.

In sum, GA proposes that the PS relation is strong enough to invalidate all academic regionalisms that draw hard frontiers between P and S. In this regard, GA strongly defends

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multi-, inter-, and even trans-disciplinary collaborations between the two, while acknowledging their disciplinary autonomy. GA views the relation as sufficiently reciprocal to invalidate all principled asymmetrical depictions that denote any kind of superiority of one over the other (reduction, foundation, motherhood, monarchy, therapeutic dependence, etc.). The relation is also too complex, selective, and specific to allow for overgeneralized simplistic depictions, equally frequent in the literature on the relation. Doing all this certainly takes much time and effort, but who said it should be easy? The wishful thinking behind GA is for destructive negative depictions of the PS relation to give way to more positive constructive depictions where P and S and allies, not enemies.

### References

- Amsterdamska, O. (1990). Surely you are joking, Monsieur Latour! *Science, Technology, & Human Values*, *15*, 495–504.
- Ayer, A. J. (1956). *The Problem of Knowledge*. Harmondsworth: Penguin.
- Bailey, J. S., & Burch, M. R. (2018). *Research methods in applied behavior analysis, Second Edition*. Oxford: Routledge.
- Baum, W. M. (1997). New paradigm for behavior analysis: A review of *Behavior and Mind* by Howard Rachlin. *The Behavior Analyst*, *20*, 11–15.  
<https://www.ncbi.nlm.nih.gov/pmc/articles/PMC2733538/>
- Baum, W. M. (2002). From molecular to molar: A paradigm shift in behavior analysis. *Journal of the Experimental Analysis of Behavior*, *78*, 95–116.  
<https://doi.org/10.1901/jeab.2002.78-95>
- Bennett, M. R., & Hacker, P. M. S. (2003). *Philosophical foundations of neuroscience*. Malden, MA: Blackwell.
- Boden, M. A. (1980). Artificial intelligence and intellectual imperialism. In A. J. Chapman & D. M. Jones (Eds.), *Models of man* (pp. 129–141). Leicester: The British Psychological Society. <https://archive.org/details/modelsofman0000unse/mode/2up>
- Bouwsma, O. K. (1972). A difference between Ryle and Wittgenstein. *Rice University Studies*, *58*, 77–87.
- Burgos, J. E., & Donahoe, J. W. (2006). Of what value is philosophy to science? A review of Max R. Bennett and P. M. S. Hacker's *Philosophical Foundations of Neuroscience* (Malden, MA; Blackwell, 2003). *Behavior and Philosophy*, *34*, 71–87.  
<https://www.jstor.org/stable/pdf/27759521.pdf?seq=1>
- Burgos, J. E., & Donahoe, J. W. (2016). Unified principle of reinforcement in a neural-network model: A reply to N. T. Calvin and J. J. McDowell. *Behavioural Processes*, *126*, 46–54.  
<http://dx.doi.org/10.1016/j.beproc.2016.03.003>
- Burgos, J. E. (2018). Is a nervous system necessary for learning? *Perspectives on Behavior Science*, *41*, 343–368. <https://doi.org/10.1007/s40614-018-00179-7>
- Burgos, J. E. (2020). Learning. In J. Vonk & T. J. Shackelford (Eds.), *Encyclopedia of Animal Cognition and Behavior*. Cham: Springer Nature AG. [https://doi.org/10.1007/978-3-319-47829-6\\_793-1](https://doi.org/10.1007/978-3-319-47829-6_793-1)
- Calvin, N. T., & McDowell, J. J. (2015). Unified-theory-of-reinforcement neural networks do not simulate the blocking effect. *Behavioural Processes*, *120*, 54–63.  
<http://dx.doi.org/10.1016/j.beproc.2015.08.008>

## BURGOS

- Christofidou, A. (2018). Descartes' dualism versus behaviourism. *Behavior and Philosophy*, 46, 63–99. <https://behavior.org/wp-content/uploads/2019/04/BPv46-5-CHRISTOFIDOU.pdf>
- Cooper, H. L. (1965). The uses of philosophy for the scientist. *Bios*, 36, 155–160. <https://www.jstor.org/stable/4606621>
- Day, W. F. (1969). On certain similarities between the *Philosophical investigations* of Ludwig Wittgenstein and the operationism of B. F. Skinner. *Journal of the Experimental Analysis of Behavior*, 12, 489–506.
- Deitz, S. M., & Arrington, R. L. (1984). Wittgenstein's language-games and the call to cognition. *Behaviorism*, 12, 1–14. <http://www.jstor.com/stable/27759040>
- De Haro, S. (2020). Science and philosophy: A love–hate relationship. *Foundations of Science*, 25, 297–314. <https://doi.org/10.1007/s10699-019-09619-2>
- Dirac, P. A. M. (1958). *The principles of quantum mechanics, Fourth Edition*. Oxford: Oxford University Press.
- Einstein, A. (1922). *Sidelights on relativity*. London: Methuen. <https://archive.org/details/in.ernet.dli.2015.218800/mode/2up?q=einstein+%22sidelights+on+relativity%22>
- Einstein, A. (1934). On the method of theoretical physics. *Philosophy of Science*, 1, 163–169. <http://www.jstor.org/stable/184387>
- Einstein, A. (1949). Remarks concerning the essays brought together in this co-operative volume. In P. A. Schilpp (Ed.), *Albert Einstein: Philosopher-scientist* (pp. 665–688). Evanston, IL: The Library of Living Philosophers.
- Elgin, C. (1996). *Considered judgment*. Princeton: Princeton University Press.
- Farrell, B. A. (1978). The progress of psychology. *British Journal of Psychology*, 69, 1–8.
- Ferrater Mora, J. (1960). *Philosophy today: Conflicting tendencies in contemporary thought*. New York: Columbia University Press.
- Ghiselin, M. T. (1969). *The triumph of the Darwinian method*. Berkeley: University of California Press.
- Ghiselin, M. T. (1997). *Metaphysics and the origin of species*. Albany: State University of New York Press.
- Goldman, A. (1988). *Empirical knowledge*. Berkeley, CA: Berkeley University Press.
- Gross, P. R., & Levitt, N. (2007). *Higher superstition: The academic left and its quarrels with science*. Baltimore, MD: Johns Hopkins University Press.
- Haegele, J. A., & Hodge, S.R. (2015). The applied behavior analysis research paradigm and single-subject designs in adapted physical activity research. *Adapted Physical Activity Quarterly*, 32, 285–301. <https://doi.org/10.1123/apaq.2014-0211>
- Hawking, S., & Mlodinow, L. (2010). *The grand design*. New York: Bantam.
- Heisenberg, W. (1959). *Physics and philosophy: The revolution in modern science*. London: Allen & Unwin.
- Jones, S. (1997). The set within the skull. *The New York Review of Books* (November 6): 3–16. <https://www.nybooks.com/articles/1997/11/06/the-set-within-the-skull/>
- Joynton, R. B. (1980). Models of man: 1879–1979. In A. J. Chapman & D. M. Jones (Eds.), *Models of man* (pp. 1–12). Leicester: The British Psychological Society. <https://archive.org/details/modelsofman0000unse/mode/2up>
- Kelley III, D. P., & Gravina, N. (2017). A paradigm shift in healthcare: An open door for organizational behavior management. *Journal of Organizational Behavior Management*, 38, 73–89, DOI: [10.1080/01608061.2017.1325824](https://doi.org/10.1080/01608061.2017.1325824)

## A GOLDBLOCKS APPROACH

- Kidd, C., Piantadosi, S. T., & Aslin, R. N. (2012). The Goldilocks Effect: Human infants allocate attention to visual sequences that are neither too simple nor too complex. *PLOS ONE*, 7, e36399. <https://doi.org/10.1371/journal.pone.0036399>
- Koffka, K. (1935). *Principles of Gestalt psychology*. London: Kegan.
- Kornblith, H. (2014). *A naturalistic epistemology: Selected papers*. Oxford: Oxford University Press.
- Kuhn, T. S. (2012). *The structure of scientific revolutions Fourth Edition*. Chicago: The University of Chicago Press.
- Lakatos, I. (1971). History of science and its rational reconstructions. In R. C. Buck & R. S. Cohen (Eds.), *PSA 1970 In memory of Rudolf Carnap Proceedings of the 1970 Biennial Meeting Philosophy of Science Association* (pp. 91–136). Dordrecht: Springer.
- Laplane, L., Mantovani, P., Adolphs, R., Chang, H., Mantovani, A., McFall-Ngai, M., Rovelli, C., Sober, E., & Pradeu, T. (2019). Why science needs philosophy. *PNAS*, 116, 3948–395. [www.pnas.org/cgi/doi/10.1073/pnas.1900357116](http://www.pnas.org/cgi/doi/10.1073/pnas.1900357116)
- Latour, B. (1987). *Science in action: How to follow scientists and engineers through societies*. Cambridge, MA: Harvard University Press.
- Leslie, J. C., & O'Reilly, M. F. (1999). *Behavior analysis: Foundations and applications to psychology*. New York: Taylor & Francis.
- Levins, R., & Lewontin, R. (1983). *The dialectical biologist*. Cambridge, MA: Harvard University Press.
- Lindworsky, J. (1922). *Experimentelle psychologie*. Munchen: Verlag. <https://archive.org/details/experimentelleps00lind/page/2/mode/2up?q=philosophie>
- Mayr, E. (1988). *Toward a new philosophy of biology: Observations of an evolutionist*
- McGuigan, F. J. (1968). *Experimental psychology: A methodological approach, Second Edition*.
- Monod, J. (1971). *Chance and necessity: An essay on the natural philosophy of modern biology*. New York: Vintage. <https://archive.org/details/chancenecessity00mono/mode/2up>
- Nagel, T. (1986). *The view from nowhere*. New York: Oxford University Press.
- Nevin, J. A., Mandell, C., & Atak, J. R. (1998). The analysis of behavioral momentum. *Journal of the Experimental Analysis of Behavior*, 39, 49–59.
- O'Doherty, K. C., Osbeck, L. M., Schraube, E., & Yen, J. (Eds., 2019). *Psychological studies of science and technology*. Switzerland: Springer Nature.
- O'Donohue, W. (1993). The spell of Kuhn on psychology: An exegetical elixir. *Philosophical Psychology*, 6, 267–297.
- Palermo, D. S. (1971). Is a scientific revolution taking place in psychology? *Science Studies*, 1, 135–155.
- Pierón, H. (1929). *Principles of experimental psychology* (J. B. Miner, Trans.). London: Kegan Paul & Co. <https://archive.org/details/in.ernet.dli.2015.218465/page/n11/mode/2up?q=philosophy>
- Pigliucci, M. (2014). Neil deGrasse Tyson and the value of philosophy. *Huffpost* (July 16). [https://www.huffpost.com/entry/neil-degrasse-tyson-and-the-value-of-philosophy\\_b\\_5330216?guccounter=1](https://www.huffpost.com/entry/neil-degrasse-tyson-and-the-value-of-philosophy_b_5330216?guccounter=1)
- Pinker, S. (1997). *How the mind works*. New York: Penguin.
- Place, U. T. (1999). Ryle's behaviorism. In W. O'Donohue & R. Kitchener (Eds.), *Handbook of behaviorism* (pp. 361–398). San Diego: Academic.
- Pollock, J. L. (1990). *Technical methods in philosophy*. Boulder, CO: Westview.

## BURGOS

- Ribes-Iñesta, E. (2006). Human behavior and language: Some thoughts on Wittgenstein. *Behavior and Philosophy*, 34, 109–121. <https://www.jstor.org/stable/27759523?seq=1>
- Rovelli, C. (2018). Physics needs philosophy. Philosophy needs physics. *Foundations of Physics*, 48, 481–491. <https://doi.org/10.1007/s10701-018-0167-y>
- Ryle, G. (1949). *The concept of mind*. London: Hutchinson.
- Ryle, G. (1972). Postscript. *Rice University Studies*, 58, 107–110.
- Schneider, W. (1997). Connectionism: Is it a paradigm shift for psychology? *Behavior Research Methods, Instruments, & Computers*, 19, 73–83. <https://doi.org/10.3758/BF03203762>
- Schoneberger, T. (1991). Verbal understanding: Integrating the conceptual analyses of Skinner, Ryle, and Wittgenstein. *The Analysis of Verbal Behavior*, 9, 145–151.
- Searle, J. R. (2009). Why should you believe it? *The New York Review of Books*, 24 September.
- Sellars, W. (1956). Empiricism and the philosophy of mind. In H. Feigl & M. Scriven (Eds.), *Minnesota Studies in the Philosophy of Science, Volume I: The foundations of science and the concepts of psychology and psychoanalysis* (pp. 253–329). University of Minnesota Press.
- Skinner, B. F. (1953). *Science and human behavior*. New York: The Free Press.
- Sober, E. (1984). *The Nature of selection*. Cambridge, MA: Bradford/MIT Press.
- Sokal, A., & Bricmont, J. (1998). *Fashionable nonsense: Postmodern intellectuals' abuse of science*. New York: Picador.
- Stewart, I. (2018). Derived relational responding and relational frame theory: A fruitful behavior analytic paradigm for the investigation of human language. *Behavior Analysis: Research and Practice*, 18(4), 398-415. <http://dx.doi.org/10.1037/bar0000129>
- Stich, S. (1991). *The fragmentation of reason*. Cambridge, MA: MIT Press.
- Underwood, B. J. (1966). *Experimental psychology, Second Edition*. New York: Appleton-Century-Crofts.  
<https://archive.org/details/experimentalpsyc0000unde/page/300/mode/2up?q=philosophy>
- Warren, N. (1971). Is a scientific revolution taking place in psychology?—Doubts and reservations. *Science Studies*, 1, 407–413.
- Weimer, W. B., & Palermo, D. S. (1973). Paradigms and normal science in psychology. *Science Studies*, 3, 211–244.
- Weinberg, S. (1992). *Dreams of a final theory: The scientist's search for the ultimate laws of nature*. New York: Pantheon.
- Williams, M. (1991). *Unnatural doubts*. Oxford: Blackwell.
- Wittgenstein, L. (1953). *Philosophical investigations*. Oxford: Blackwell.
- Woodworth, R. S., & Schlosberg, H. (1954). *Experimental psychology, Revised Edition*. New York: Holt.  
<https://archive.org/details/experimentalpsyc00wood/page/n15/mode/2up?q=philosophy>