1. Introduction

Many consider folk psychology (FP) to be a theory that we humans wield in order to make sense of, and to anticipate, the behavior of our fellow humans. If this is an accurate characterization of FP, and if FP, in turn, is an accurate theory, then we humans spend a considerable amount of time trying to figure out how, in light of our beliefs, to fulfill our desires. Quite sensibly, philosophers have been consulting our best science of the underpinnings for human behavior, cognitive science (CS), in order to determine whether or not FP is an accurate theory. Unfortunately for FP, this endeavor has led to the assembly of a rather elaborate gauntlet of irrealism for the folk-psychological ontology.

From a distance, this gauntlet looks quite forbidding. On closer inspection, however, it appears to be peopled with some rather sickly looking foes. What I intend to show here is that the disease inflicting many of these arguments is their acceptance of some popular, though deeply misguided, assumptions regarding the predictive and explanatory practices of CS. Before proceeding, I should note that I have no intention of addressing every argument against FP, merely those that happen to be premised on the faulty assumptions to which I have just alluded. Nevertheless, those who take CS to be a legitimate arbiter of the status of FP should, in the final analysis, be convinced that CS has long since vindicated the folk-psychological ontology.
2. FP and the Gauntlet of Irrealism

The gauntlet begins for FP with the question of whether or not we are truly capable of both predicting and explaining the behavior of our fellow humans, for there is a case to be made that FP fails to do either on a consistent basis. There are, after all, many instances in which we are utterly incapable of predicting what our fellow humans—even those near and dear to us—will do. Likewise, when it comes to explaining behavior, we often have no idea why someone acted in the particular way that they did. Moreover, even in cases where we claim to be able to explain how someone’s beliefs and desires conspired to cause his behavior, it is possible that such accounts are little more than Kiplingian just-so stories. Such predictive and explanatory shortcomings seem to be what Paul Churchland has in mind when he claims that FP might provide “a positively misleading sketch of our internal kinematics and dynamics, one whose success is owed more to selective application and forced interpretation on our part than to genuine theoretical insight on FP’s part.” Nor, it seems, do the explanatory shortcomings of FP end there. After all, FP also seems to fail miserably in terms of its ability to explain such psychological phenomena as “mental illness, sleep, creativity, memory, intelligence differences, and the many forms of learning, to cite just a few.” As such, claims Churchland, it is disturbing that FP has remained stagnant for thousands of years (and thus offers no promise of ever explaining these phenomena) and has also persistently resisted integration into the rest of science (the only other thing that might have made it worth hanging on to).

Even if there are ways to address these charges of predictive and explanatory inadequacy, the gauntlet has only begun. Proceeding in logical, rather than chronological fashion, the next challenge facing FP has to do with the question of just how, cognitively speaking, predictions and explanations of everyday behavior are effected by the folk. According to one proposal, we predict and make sense of (‘explain’ would be too strong a term if the proposal is accurate) the behavior of our fellow humans by, roughly speaking, taking an imaginary walk in their shoes. For example, in order to answer the question, “Where will Maxi say the chocolate bar is hidden,” we might imagine what it would be like to be Maxi as she sees the bar hidden, leaves the room, and returns. That is, in order to answer the question concerning where Maxi will say the bar is hidden, we imagine what it would be like to be Maxi and then answer the simpler question, “Where is the bar hidden?” What is so ‘radical’ about this proposal, claims Gordon, is that the procedure can be carried out without any grasp at all of such concepts as belief and desire; it seems to obviate the need for a theory regarding the hidden underpinnings of human behavior altogether. If this radical simulation theory is correct, then,
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what we wield in order to make sense of and anticipate the behavior of our fellow humans is not a theory at all. This, claim Stich and Ravenscroft, would seem to render questions about the scientific respectability of FP moot and, thereby, undermine at least some versions of eliminativism. What these authors do not tell us, however, is that the theory of radical simulation could just as easily be construed as a threat to realism with regard to the folk-psychological ontology. There are, after all, many who feel that realism with regard to this ontology will ultimately be justified by the scientific vindication of a folk-psychological theory that counts beliefs and desires as amongst its posits. If FP is not a theory, then this avenue to realism will have been blocked.

It may be, then, that FP would fare much better if there were reasons for preferring the theory theory, according to which our proficiency at predicting and explaining the behavior of our fellow humans stems from our mastery of a body of laws which specify the relationships between, among other things, particular beliefs, particular desires, and particular behaviors. According to the theory theory, for example, a person might predict that Doug will go to the fridge and remove a pickle by invoking a law like this one:

If (x desires a pickle and has no stronger desires whose satisfaction would preclude his/her having a pickle, and x believes that there is a pickle in the fridge that belongs to x and that said pickle can be obtained by walking to the fridge and removing it), then (impedimentis absentibus) x will walk to the fridge and remove a pickle.

If experimental findings consistently favored the theory theory over the radical simulation theory, this would clearly go a long way towards alleviating the concern that there is no theoretical ontology of folk psychology to vindicate. It has, unfortunately, proven exceedingly difficult to tease apart simulation theory and theory theory on an experimental basis.

Of course, even if there were good reasons for thinking that we humans are highly effective at predicting and explaining the behavior of our compatriots in virtue of our mastery of a set of laws (like the one above) whose theoretical posits include beliefs and desires, FP still has a long row to hoe. After all, FP might still turn out to share the fate of Ptolemaic astronomy, which also happened to do a fine job of supplying predictions and explanations. Just as a more compelling theory came along and eliminated such Ptolemaic posits as epicycles, perhaps something similar will happen in the case of such folk psychological posits as beliefs and desires. CS might, for example, accomplish all of its predictive and explanatory goals by instead appealing to trajectories through n-dimensional state space or through some other, FP-unfriendly means.
Indeed, even if CS did wholeheartedly embrace FP and its ontology, this, in and of itself, would not justify realism with regard to the folk-psychological ontology. After all, as Dennett points out, we still have the option of various shades of irrealism, including instrumentalism. Beliefs and desires might, for instance, turn out to be like centers of gravity. Though (one version of the argument goes) there are no such things as centers of gravity (after all, they take up no space and engage in no causal interactions), we gain a great deal of inferential leverage by acting as if there were. Perhaps a similar set of claims can be supported with regard to the folk-psychological ontology.

It would, on the other hand, seem to constitute a major victory for FP if a particular version of the computational theory of mind—namely, Fodor's language of thought (LOT) hypothesis—were shown to be true. After all, if the LOT hypothesis is correct, then beliefs and desires are token identical with particular states of the brain and, unlike centers of gravity, play a genuine causal role with respect to behavior. Unfortunately for proponents of FP, none of the relevant token identities has ever been established. As an alternative, Fodor and others have argued that CS simply cannot do without the LOT hypothesis. This strategy has, of course, spawned great debates about just what kinds of things can and cannot be accomplished by applying syntax-sensitive inference rules to syntactically-structured representations and about whether or not connectionist systems (viz., those that do not simply implement the tenets of the LOT hypothesis) can do the same. Unfortunately for FP, even if it could be shown that the LOT hypothesis truly is the only game in town, serious concerns about FP and its attendant ontology would persist.

One of these has to do with the fact that FP seems to, and the LOT hypothesis certainly does, posit states with content. It is worried, however, that contents have no legitimate role to play in any science of the underpinnings for human behavior. At present there are two major proposals (i.e., internalism and externalism) concerning how it is that the folk, if only implicitly, take the contents of mental states to be determined. The arguments against the scientific legitimacy of contents are generally directed at one or the other of these proposals.

The first proposal concerning how it is that this assignment of content is effected, or one popular version of it, is that the folk implicitly take the content of a mental state to be determined by the network of further mental states—what Stich calls its 'doxastic surrounding'—within which it is embedded. There are at least two variants of the argument that this internalist scheme of content individuation is incompatible with CS. The first, attributable to Stich and Fodor, is as follows:
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The primary goal of CS—or any science—is to formulate laws. Since doxastic surrounding varies from individual to individual, mental contents vary from individual to individual. There can thus be no laws that quantify over mental contents. CS must therefore eschew mental contents.17,18

The second, also attributable to Stich, looks something like this:

CS is committed to the computational theory of mind, according to which mental states are individuated on the basis of their local, syntactic properties. Yet the contents of mental states are determined by doxastic surrounding, a non-local property. CS therefore has no use for contents.19

This seems to raise problems for FP because if FP is a theory that posits states that are individuated on the basis of their doxastic surrounding, and if CS must eschew states of this sort for either of the above reasons, then it would seem that FP must be mistaken.

There are, of course, many who feel that the folk take contents to instead be determined by the relationship between an individual and the world he/she inhabits.20 The worry about this externalist scheme of content fixation, however, is that it too might cast contents as the sorts of properties in which CS should have no interest. The worry, more specifically, is that sciences ought to individuate properties on the basis of their causal powers, but content externalism seems to cast contents as relational properties with no relevant causal powers.21 If this is true, then wide contents (i.e., contents as externalism portrays them) have no legitimate role to play in CS. As in the previous case, this reflects poorly on FP. After all, if FP is a theory that posits states that are individuated on the basis of their wide contents, and if CS must eschew such states for reasons just described, then FP will once again find itself in hot water.

To recap, popular philosophical wisdom has it that the vindication of FP requires the defense of many controversial theses.22 In particular, it needs to be shown that the folk really do enjoy a sufficiently high level of predictive and explanatory success with regard to the behavior of their fellow humans, that neither the inability of FP to explain other cognitive phenomena nor its stagnancy in this regard is any threat to FP, that the aforementioned predictive and explanatory successes stem from reliance upon an ontology that includes beliefs and desires as amongst the causal determinants of human behavior, that our adoption of this ontology is more than a useful fiction because a version of the computational theory of mind—the LOT hypothesis—is accurate, and that, in pursuit of its explan-
atory and predictive goals, it is permissible and advisable for CS to individuate the internal, syntactically structured states posited by the LOT hypothesis on the basis of their contents. Things do look quite bad for FP, but appearances can be deceiving.

3. Archaic Presuppositions

One of the principal reasons why philosophers have failed to find their way clear of this gauntlet is that they have often assumed that the mission of CS is the discovery of psychological laws of the sort that can enable the prediction and explanation of particular everyday behaviors. Given this portrayal of the mission of CS, philosophers have naturally concluded that FP will only attain scientific respectability if CS starts coming up with gobs of laws whose antecedents invoke particular beliefs and desires and whose consequents specify activities like removing pickles from refrigerators. In other words, popular philosophical wisdom seems to have it that FP will only be vindicated if CS really is in a great deal of trouble. After all, the primary mission of CS has most assuredly not been the search for reliable generalizations. Nor, for that matter, are the explananda one finds invoked in the context of cognitive scientific research particular everyday behaviors like going to the fridge for a pickle.

While many philosophers assume that the primary goal of CS is to discover laws, only Fodor has had the decency to try defending this assumption. What he has to say on this particular matter is, unfortunately, utterly lacking in merit. He argues as follows:

I cleave to [the idea that psychological explanation typically involves law subsumption] because it’s hard to doubt that at least some psychological regularities are lawlike (for example: that the Moon looks largest when it’s on the horizon; that the Muller-Lyre figures are seen as differing in length; that all natural languages contain nouns).23

There are, to be sure, many regularities that cognitive scientists find deeply interesting.24 To the above, we might add the phonological similarity effect, the word superiority effect, the STROOP effect, various semantic priming and motor learning phenomena, cognitive and motor deficits associated with disorders such as MS and Parkinson’s disease, various forms of aphasia and agnosia, critical periods, and so on and so forth. However, the reason why cognitive scientists find such regularities deeply interesting is not their explanatory power. Rather, these regularities are, one and all, precisely what cogni-
tive scientists take to stand in need of explanation. As discussed in greater detail below, what we find playing the part of explanans to these explananda are not more reliable generalizations but models of the mechanisms giving rise to them. Fodor nearly admits as much when he notes:

> An implementing mechanism is one in virtue of whose operation the satisfaction of a law’s antecedent reliably brings about the satisfaction of its consequent. Typically, though not invariably, the mechanisms that implement the laws of a science are specified in the vocabulary of some other, lower-level, science.\(^{25}\)

Fodor presumably thinks that if the implementing mechanisms are specified in the vocabulary of lower-level sciences, then he can hang onto both psychological laws and the autonomy of psychology.\(^{26}\) This disclaimer, however, does nothing to alter the fact that the reliable generalizations that interest cognitive psychologists are the explananda of research. Moreover, if it happens to be the case that psychologists typically invoke the vocabulary of lower-level sciences in the process of formulating explanans for these explananda—if that is what explanation in cognitive psychology consists of—then so much the worse for the disunity hypothesis. The disunity hypothesis is not my present target, however, and, to be fair, psychologists need not, and frequently do not, invoke lower-level vocabulary when proposing mechanisms that explain regularities. After all, a finer-grained functional breakdown of the cognitive system is very often the immediate goal of psychological investigation. To take just one example, in order to explain the fact that visual processing causes a decline in performance on a concurrent visuospatial reasoning task while auditory processing causes no such decline, cognitive psychologists posit two independent short-term memory stores (i.e., a phonological store and a visuospatial sketchpad).\(^{27}\) In and of itself, this model entails nothing about the locus, structure, or manner of operation of the implementing neural ensembles. In other words, what explains the finding is a model of the underlying mechanisms, but this model is not specified in the vocabulary of a lower-level science.

I’ll have quite a bit more to say on the nature of explanation in cognitive science below, but before proceeding we should quickly dispense with the stronger, and doubly ludicrous, suggestion that CS will vindicate FP by supplying intentional generalizations of the sort mentioned above. Whatever ones reasons are for thinking that CS should supply explanatory intentional generalizations that quantify over states like wanting a pickle, one would be very hard pressed to find evidence that CS does supply such generalizations. In fact, I do not think that I would be going out on much of a
limb were I to claim that not one law of this kind has ever been invoked in either psychology or in any of the neurosciences. 28 If we are to make any progress in our understanding of the relationship between CS and FP—indeed, if we are to make any progress in the philosophy of CS at all—we must cast aside these distorted theoretical lenses and see CS as she truly is.

4. Schematic Models

Lying at the foundation of mainstream cognitive science—I'll have more to say about the fringe elements below—are some highly schematic, collectively consistent, and intuitively plausible models of the underpinnings for human behavior. One such model, the one on which I will be focusing much of my attention, depicts humans as engaging in a process of planning, a process whereby one determines how to get from some actual state of affairs to some desired state. In order to make this determination, one represents the two states of affairs and manipulates the former until, crudely speaking, it comes to look like the latter. Planning, in other words, involves thinking ahead, or looking before we leap.

As it stands, this model is highly schematic. In other words, it supplies only a very broad functional breakdown of the underpinnings for certain human behaviors. It entails no commitments regarding the structure of beliefs or desires (e.g., whether they are sentential or imagistic), nor, for that matter, does it provide any specifics regarding how one reasons one's way from the former to the latter. Like the model of short-term memory described above, the kind of breakdown that the model supplies is functional in roughly the weak sense described by Lewis 29 and elaborated upon by Lycan 30 and Bechtel and Richardson. 31 That is, the model provides a highly schematic understanding of the parts of the cognitive system, the activities, or functions, that they carry out, and how they collectively conspire to cause the phenomena of interest. The model is thus clearly not functional in the much stronger Turing-machine sense described by Putnam. 32 'Function', according to this latter view, connotes mathematical functions, and this version of functionalism involves a commitment to the thesis that the mind is a computational device.

The planning model obviously did not originate with the contemporary mind sciences. It has, after all, been invoked in the works of countless poets, playwrights, and novelists throughout the centuries; it has been described in detail by the likes of Aristotle, 33 Hobbes, 34 and Leibniz 35, and (if everyday discourse is any guide) it is part of our intuitive understanding of how humans operate. It is, in short, an integral part of folk psychology. Of course, were common sense the only grounds we had for endorsing this model, it would fall far short of the mark
of scientific respectability. It has, however, long since been adopted, refined, and ultimately vindicated by CS.

The proposal that humans plan in the aforementioned manner first began to take on the appearance of a scientific hypothesis when it was invoked in order to explain a general feature of human behavior. As Köhler36 and Craik37 suggested, a capacity to think ahead would explain why humans often behave in such an unhesitating (once they get started, that is) and effective manner in the face of even highly novel environmental conditions.38 By the same token, this model can also explain why human behavior is so flexible in the face of similar environmental conditions. That is to say, it explains why human behavior is not, as Chomsky so famously pointed out, stimulus bound.39

Today it is considered something of a platitude amongst cognitive scientists that creatures like ourselves enjoy an advantage over many other critters in virtue of our capacity to look before we leap. As my nemesis-cum-hero puts it: “That people ... act out of their beliefs and desires, and that, in the course of deciding how to act, they often do a lot of thinking and planning, strikes me as maybe empirical in principle but surely not negotiable in practice.”40 To drive Fodor’s point a little further home and to get clearer on the precise role this model plays in CS, it bears noting that its role in CS is actually very much like the role played by the theory of natural selection in evolutionary biology.

4.1 Natural Selection and Planning: Explanatory Successes and Shortcomings

Notice, to start with, that selectionist explanations of particular traits are sometimes belittled as Kiplingian ‘just so’ stories.41 That is to say, although a particular explanation might have an air of plausibility to it, it is nevertheless often possible to construct an equally plausible alternative account by utilizing the same general explanatory apparatus (i.e., by appealing to selection pressures, variation, and heritability). For example, there are several plausible explanations for the fact that humans, unlike other primates and unlike most other land-dwelling mammals, are not covered in fur (e.g., the easy removal of parasites, cooling of the body in a savanna-like environment, and the facilitation of swimming42). In this and other cases, there is, due largely to the vast amount of time that has elapsed, too little evidence available to suitably constrain the space of plausible hypotheses. This is hardly a problem for the theory of natural selection, however. The problem instead lies entirely with the epistemically impoverished situation of the theorist, a situation that is straightforwardly implied by the theory of natural selection itself. When it comes to explaining
specific traits, then, the leverage gained through adoption of the theory of natural selection—which includes a characteristic ontology of states and processes—may be quite limited.

Be this as it may, when construed as an explanation for the more general fact that organisms tend to be well adapted to their particular environments, the theory of natural selection succeeds like no theory before or since. It is only when this model is viewed as an explanation for a very general fact about the relationship between organisms and their environments that its tremendous explanatory power becomes apparent. In fact, even were we—for the aforementioned epistemic reasons—incapable, save in a select few cases, of reaching definite conclusions about the various factors that conspired to yield particular traits, this model should still enjoy our favor. 43

The proposal that humans are capable of planning has roughly the same explanatory strengths and weaknesses as the theory of natural selection. Notice, for instance, that in many instances it would not be unwarranted to belittle a given explanation of a particular behavior as a just-so story. That is to say, although a particular explanation might have an air of plausibility to it, it is nevertheless often possible to construct an equally plausible alternative explanation by utilizing the same general explanatory apparatus (i.e., by appealing to beliefs, desires, and inferences). For example, you might recall that there was a fair amount of debate regarding the motivation behind Clinton's decision to launch air strikes against Iraq at a time when he also happened to be the subject of some rather harsh criticism. It was variously suggested that he ordered air strikes in order to divert attention from his troubles, that he wished to make himself appear more presidential, and that the advice of military strategists was the only relevant factor (i.e., the fortuitous timing was merely coincidental). Most of us lack sufficient evidence to have real confidence in any one of these explanations. Proponents of the folk ontology should hardly despair, however. After all, our inability to satisfactorily explain President Clinton's behavior is a consequence of our impoverished epistemic predicament. That is, one's access to the factors (e.g., beliefs, desires, and inferences) that conspire to give rise to particular behaviors is often quite limited. This is a situation that is straightforwardly implied by the planning model itself. When it comes to explaining specific behaviors, then, the leverage gained through adoption of this model—which includes its own characteristic ontology of states and processes—may be quite limited.

Be this as it may, when construed as an explanation for the more general fact that organisms like ourselves are able to respond so effectively in the face of novel circumstances and so flexibly in the face of similar ones, the planning model succeeds
like no theory before or since. It is only when it is viewed as an explanation for a very general fact about the relationship between humans and the various contingencies they confront that the true explanatory power of the theory becomes apparent. In fact, even were we—for the aforementioned epistemic reasons—in incapable, save in a select few cases, of reaching definite conclusions about the various factors that conspired to yield particular behaviors, this model should still enjoy our favor.

There is one other parallel between the two theories that bears mentioning. Notice that most people readily acknowledge the profound effects that selective breeding can have on the character of descendents. The folk, in other words, have a reasonable grasp of, and—whether they like it or not—they tacitly accept some of the basic tenets of the theory, and this was the case long before these tenets were invoked as part of an explanation for the relationship between species and their environments. So too do the folk have a reasonable grasp of and tacitly accept some of the basic tenets of the planning model, and this was the case long before these tenets were invoked by CS as part of a scientific explanation for the fact that humans are quite adept at dealing with even very novel environmental contingencies.

4.2 Natural Selection and Planning: The Goal of Prediction

The analogy between the theory of natural selection and the theory of planning does break down to some extent where prediction is concerned, but the points where the analogy holds are quite illuminating. Notice, to begin with, that the predictive leverage gained through adoption of the theory of natural selection is probably quite limited, at least insofar as the natural emergence and increasing prevalence of a particular trait in a given population are concerned. A number of factors are responsible for this limitation: The time frame undercuts the utility of offering such predictions, adoption of the theory does not enable one to predict which of potentially very many useful phenotypic variations will obtain, and it is certainly not the case that species tend to evolve with respect to an unchanging environment (i.e., the environments are themselves subject to evolutionary pressures). These factors add prohibitive complexity to the problem of predicting the natural emergence and increasing prevalence of a particular trait in a given population and quite clearly rule out the possibility that the theory of natural selection will be corroborated on the basis of such predictions.

On the other hand, where selective breeding is the outcome of conscious human intervention, we can—insofar as we are
Jonathan A. Waskan

aware of the selection criteria—advance some pretty accurate predictions concerning what a given line or breed will come to look like. In such cases, selection for reproduction is not based upon natural fitness, it is comparatively insensitive to environmental changes, and there is little danger that some other set of features will be selected for. Moreover, because selection pressures are so strong, the course of evolution is greatly accelerated.45

Be this as it may, the theory of natural selection is most assuredly not valued for its ability to support predictions concerning the emergence of particular traits. Nor, for that matter, do the predictions that hold the greatest interest for evolutionary biologists have the power to either falsify or corroborate the theory of natural selection, at least not directly. The predictions that are of the greatest interest from the standpoint of evolutionary biology are those that figure into the progressive refinement of the theory of natural selection through the testing of competing models concerning, for example, the levels at which natural selection is operative (e.g., individuals vs. populations) and the underlying mechanisms for natural selection (e.g., the nature of environment–gene interactions or gene–gene interactions). The guiding assumption behind most of this research is that the theory of natural selection is basically correct. The work of the evolutionary biologist is generally geared towards filling in the details of this broad explanatory framework. To put it in Bechtel and Richardson's terms, the evolutionary biologist is engaged in an iterative process of decomposition and localization whereby one determines the relevant functional parts of a system and how those functions are effected by those parts, which, in turn, often involves an appeal to further, functionally individuated parts.

Predictions play a crucial role in this process because they figure into the testing of competing models, but they are not the sorts of predictions that are capable of falsifying or corroborating the broader theory, at least not directly. I add the disclaimer because there is a case to be made that these predictions do play an evidentiary role with regard to the matter of the truth or falsity of the broader theory. In short, the fact that the theory of natural selection has shown itself amenable to ever-greater refinement seems to be a real testament to its viability. Ongoing research has revealed mechanisms and processes capable of filling the various functional roles, and our understanding of these mechanisms and processes has itself undergone a tremendous degree of further refinement. A related, perhaps less controversial evidentiary consideration is the fact that the theory of natural selection happens to be consistent with, and well integrated with, the rest of science, including geology, chemistry, microbiology, and even physics. Our endorsement of the theory of natural selection is thus not warranted on
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Popperian grounds (i.e., by the fact that the theory has passed a series of severe tests), but it is warranted for reasons that are far more Quinean (i.e., the theory lies at the nexus of a larger network of beliefs, and abandoning it up would cause a large-scale disruption to the coherence and simplicity of this network).

Many of these same points apply in the case of the theory of planning. For instance, although the time frame is far shorter, our ability to predict what someone will do is limited by several factors. For one thing, there are usually many ways of getting from the way things are to the way one wants them to be (i.e., there is more than one way to skin a cat). So even if you know what someone desires and a great deal about what they believe, you might still have a very hard time figuring out how they will go about trying to fulfill their desires. Nor should we make light of the fact that under normal circumstances we have at best only a superficial understanding of what someone believes and desires and that any of their unknown beliefs or desires may well directly influence their behavior. That is to say, isotropy—the fact that anything you know might figure into the determination of what else you believe, including what you believe you ought to do—infests the processes of behavior-guidance as surely as it does the process of belief formation. As obvious as this point is, it has gone strangely unnoticed by those philosophers—and there are lots of them—who are committed to the idea that the goal (or a central goal) of psychology is to come up with laws relating particular stimuli, particular mental states, and particular behaviors. I'll return to this point below. For now, suffice it to say that the sheer volume of potentially relevant causal factors seems, in and of itself, to preclude a high degree of success at predicting particular everyday behaviors.

On the other hand, there are certain cases in which much of this complexity is factored out and in which we are, as a result, able to advance some pretty accurate predictions concerning what someone will do. These are the sorts of cases to which Fodor draws our attention when he suggests that FP works so well it disappears. To take one example, if a friend of mine tells me that he is coming to town on a particular day, I will feel pretty confident that he will indeed arrive on that day. In cases such as this, I need not take into account the effect that all of my friend’s beliefs and desires might have on his behavior, for in telling me of his plans he has done this for me. By the same token, when a perfect stranger approaches a red light at a busy intersection, I will feel quite confident that he/she will not drive through the intersection while the light is red. There is a simple inductive justification for this confidence: People tend not to drive through red lights. There is, in turn, a folk-psychological explanation for this pattern. It seems reasonable to suppose
that people are aware of the dire consequences of driving through red lights. The severity of the consequences in this case suffices to render irrelevant factors that would otherwise cause a great deal of uncertainty. For instance, in such cases it no longer matters whether or not an individual is late for an appointment, is hungry, or believes the nation has been corrupted by socialists. There are surely many cases in which complexity is effectively reduced, and these appear to be the only cases in which our predictions of behavior are justified.

Be this as it may, the planning model is most assuredly not valued by CS for its ability to support predictions concerning particular behaviors. Nor, for that matter, do the predictions that hold the greatest interest for cognitive scientists have the power to either falsify or corroborate this model, at least not directly. The predictions that are of the greatest interest from the standpoint of CS are those that figure into the progressive refinement of the planning model through the testing of competing models concerning the underlying mechanisms. In the end, this model turns out to be amenable to a high degree of refinement and, as such, well integrated with the rest of science.

These points are both important and controversial, so it is worth devoting a bit more time to their defense. Before proceeding, however, I would first like to draw attention to one disanalogy between the theory of natural selection and the planning model. While the theory of natural selection is arguably the fundamental starting point for mainstream evolutionary biology, the planning model is (as I noted at the start of Sec. 4) just one of a collection of highly schematic, collectively consistent, and intuitively plausible models of the underpinnings for human behavior that lie at the foundation of mainstream CS. These models come as a package, and they have been refined and vindicated as such.

4.2.1 Other Folk Models Endorsed by CS

If what poets, playwrights, and novelists have written throughout the centuries is any guide, then the folk have a lot more to say about what it is that makes humans tick than is encompassed by the bare planning model. What the folk actually seem to adhere to includes each of the following interrelated proposals concerning the underpinnings for human behavior:

- People are able to plan how to get from the way things are to the way they would like them to be by thinking about the consequences of their actions.
- What people believe is sometimes based upon what they perceive to be the case.
- What people believe is sometimes based upon what they are informed (e.g., through language) is the case.
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• What people believe is sometimes based upon what they infer, from their other beliefs, to be the case.
• People are able to express their beliefs and desires in written and spoken form.
• People are able to remember and recall their beliefs and desires.

Many of the same points about the predictive and explanatory utility/disutility of the planning model apply to the rest of these models. To be brief, there is obviously a very close match between these models and the broad explanatory models advanced and refined by cognitive scientists who are investigating the processes of planning, inference, perception, language comprehension/production, and memory. While these models do a nice job of explaining some general features of human behavior, the predictive and explanatory leverage they afford when it comes to particular everyday behaviors may be quite limited. In fact, the predictions that are of the greatest interest from the standpoint of CS are those that figure into the progressive refinement of this set of models through the testing of competing hypotheses regarding how the various processes are effected.

4.2.2 Refinement and Vindication of FP

It has been suggested elsewhere that there is a close relationship between FP and CS, but the details of this claim have yet to be fleshed out in a way that suffices to clarify the fact that FP has already been fully vindicated by CS. For instance, while Horgan and Woodward draw attention to the intimate relationship between FP and CS, they also argue that FP is an autonomous, high-level theory and, as such, the failure to find the neurological realizers of particular beliefs and desires would not undermine it. The problem with this strategy is that it fails to justify the jump from instrumentalism to full-blown realism. After all, one can get great predictive and explanatory mileage out of cognitive theories that invoke beliefs and desires, computations, or even intentional causation, but as long as the implementation details remain an utter mystery, the possibility remains open that the mileage is gotten merely by treating cognition as if it involved beliefs and desires, computations, or intentional causation.

What Burge has to say on this count is also relevant and merits quoting at length. He explains:

In taking psychology as it is, I am assuming that it seeks to refine, deepen, generalize and systematize some of the statements of informed common sense about people's mental activity. It accepts, for example, that people see objects with certain
shapes, textures and hues, and in certain spatial relations, under certain specified conditions. And it attempts to explain in more depth what people do when they see such things, and how their doing it is done. Psychology accepts that people remember events and truths, that they categorize objects, that they draw inferences, that they act on beliefs and preferences. And it attempts to find deep regularities in these activities, to specify mechanisms that underly them, and to provide systematic accounts of how these activities relate to one another.⁴⁹

Indeed, cognitive psychology has done more than seek to refine our understanding of the mechanisms underlying planning, language comprehension/production, memory, and inference. Cognitive psychology has actually provided a fine-grained functional breakdown of the cognitive system that includes a specification of the various way-points of processing and what it takes to get from one to the next.

To take just one case in point, consider the study of declarative memory, which includes memory for facts about the world of a public and often general nature (e.g., the fact that Bush defeated Gore or the fact that water is H₂O) and memory for more personal and specific facts (e.g., that you wanted a particular toy for Christmas one year). There is, in other words, not much difference between beliefs and desires, on the one hand, and declarative memories on the other. Also in keeping with FP, memory researchers countenance such means of acquiring declarative memories as natural language comprehension, perception, and inference. For the cognitive scientist, however, this is merely the jumping off point for investigation.

Starting at the top, a look at even the most basic findings of cognitive psychology shows that research in this discipline has greatly refined our understanding of the myriad mechanisms and processes involved in the encoding, storage, and retrieval of declarative memories. There are, for instance, good reasons for thinking that there are two distinct modes of storage. One of these seems directly implicated in the process of inference, it is quite transient (perhaps due to the overwriting of earlier memory traces by later ones), and it is subdivided into multiple, independent, possibly modality-specific types. The other is far more enduring, and such factors as the conditions of encoding and what information has been stored previously dictate the facility with which information can be later retrieved.

These findings, which only scratch the surface, are paralleled by further findings regarding the mechanisms and processes involved in perception, various forms of language comprehension/production, and inference. This marks a major advance over the bare folk models described in the previous section, and it puts us well on the path towards their full-scale vindication. In and of itself, however, research in cognitive psychology might
not suffice to placate the die-hard instrumentalist. What should placate these individuals, however, is a demonstration that the broad functional breakdown accepted by the folk and refined by cognitive psychology maps straightforwardly onto what is known about the brain. One of the achievements of the collaboration between cognitive psychology and the various branches of neuroscience (i.e., cognitive neuropsychology, cognitive neuroscience, neuroanatomy, and neurophysiology) has been to demonstrate precisely this.

Admittedly, many of the models advanced by cognitive neuropsychologists are pitched in fairly abstract terms. For example, many of the models of various forms of agnosia, dyslexia, and aphasia are implementation nonspecific flowcharts. These flowcharts overlap nicely with those formulated on independent grounds by cognitive psychologists, and they add to the former’s depiction of the established routes of processing a specification of various bumpy detours. However, unlike the research undertaken in cognitive psychology, neuropsychological research also does much to chip away at instrumentalism, for the diagnoses offered for the various pathologies also suggest—or at least begin to suggest—ways in which the various routes and way-points of processing map onto specific structures. An additional, largely independent source of evidence for these function-to-structure mappings comes by way of the functional neuroimaging research carried out in cognitive neuroscience. This research is giving us a clearer picture of the loci of the component mechanisms responsible for language comprehension/production, the various forms of short-term memory, encoding, storage, retrieval, perception, and so on. These findings, in turn, have been related to even the lowest-level facts of neuroanatomy and neurophysiology.

To make it perfectly clear that this is so, let us continue our elementary survey of declarative memory research, and let us focus, in particular, on the long-term component. According to a model that is very popular amongst cognitive neuropsychologists, the long-term storage of declarative memories itself involves two distinct stages. This model is motivated by the observation that damage to the medial temporal lobes (viz., the hippocampus) tends to result in permanent anterograde amnesia (i.e., no new long-term declarative memories can be formed) and retrograde amnesia (i.e., loss of memories formed prior to hippocampal damage) spanning up to three years. The best explanation for this regularity seems to be that the hippocampus stores long-term declarative memories for up to three years and, during that time, engages in a process of consolidation whereby the long-term memories it holds are progressively given more permanent storage at the cortical surface. For all its merit, the consolidation hypothesis would be pretty clearly falsified if the hippocampus turned out to be
wired up all wrong (e.g., if it had only afferent connections from the olfactory bulb and efferent connections to calf muscles). Neuroanatomists have shown, however, that the hippocampus is connected up in just the ways we would expect an intermediate-term storage site for declarative memories to be. Specifically, it has incoming connections from the sensory areas and outgoing connections to each of the sensory association areas. Neuroanatomists, neurophysiologists, and computational neuroscientists have, accordingly, undertaken the task of determining such things as how the hippocampus is internally wired up (i.e., the various areas and roles they play), the synaptic basis for memories (e.g., long-term potentiation), and how the consolidation process might operate.

While the science discussed here is old news, the way it fits together and, consequently, its relevance to FP has generally been overlooked. Getting back to the analogy with natural selection, notice that just as the predictions that are of interest to the evolutionary biologist lack the power to directly corroborate or falsify the theory of natural selection, the predictions that are of interest to cognitive scientists lack the power to directly corroborate or falsify the set of models constitutive of FP. Instead such predictions figure into the testing of competing hypotheses concerning how this set of models is best refined. For this reason, such predictions do ultimately play an indirect evidentiary role with regard to the broader set of models. For starters, the fact that the attempt to refine these folk psychological models is meeting with such success seems a real testament to their viability. What ongoing research is revealing is that there are mechanisms and processes capable of filling the relevant functional roles, and our understanding of these mechanisms and processes is, in turn, undergoing ever-greater refinement. By the same token, it has been shown that the folk models are not *sui generis*, but they are instead well integrated with various sciences. It is, in fact, no small matter that the folk models are constitutive of a much more elaborate set of interrelated proposals spanning the highest and lowest levels of cognitive scientific investigation. The fact that multiple independent forms of investigation have converged in supporting this set of models shows that they—and FP along with them—have passed what may be science’s most stringent test. In a word, they have shown themselves to be robust. Thus, like evolutionary biology, FP may not have much warrant on Popperian grounds, but its warrant on Quinean grounds more than makes up for this deficiency.

5. The Gauntlet Revisited

Popular philosophical wisdom regarding what sciences in general ought to look like and what CS in particular ought to look like does not comport well with the actual predictive and
explanatory practices of CS. The former has, I believe, been the source of much confusion in the philosophy of CS, and the current debate is just one manifestation of this confusion. Knowing what we now know about the latter takes much of the sting out of the arguments comprising the gauntlet.

5.1 Churchland

The gauntlet began for FP with the question of whether or not we humans are truly capable of predicting the behavior of our fellow humans. As explained above, there are good reasons for thinking that our ability to predict and explain particular behaviors is actually quite limited. Be this as it may, FP has been simultaneously refined and vindicated through ongoing cognitive scientific research in such a way as to render any such predictive and explanatory short-comings irrelevant. In fact, the manner in which CS has vindicated FP provides a straightforward way of replying to Churchland’s complaint that FP does not explain such phenomena as “sleep, creativity, memory, intelligence differences, and the many forms of learning.”

This was never a fair criticism to begin with. FP, after all, need not explain every facet of cognition in order to win our approval. Churchland is all-too-familiar with this position and none-too-sympathetic. He explains:

This is an unfortunate defense, as can be seen from other uses of this same strategy. One can defend Ptolemy’s ragtag astronomy (as Ptolemy did) by insisting that it was never supposed to address the real physics, or the actual causes, or the complete story of astronomical behavior, and by insisting that it properly serves only the narrow interest of predicting the angular positions of the planets as seen from Earth. One can defend any hangdog theory by this strategy, so long as it has some paltry success for some benighted purpose within some sheltered domain.54

While he is right to point that this strategy is easily abused, he is wrong if he thinks that it is always wrong to adopt this strategy. After all, Kepler’s account of planetary motions was restricted to the same sheltered domain as Ptolemy’s: It was meant to explain the peculiarities of the apparent motions of the planets relative to a fixed backdrop of stars. It thus fails to explain all sorts of phenomena that are properly viewed as the falling under the purview of astronomy. It tells us nothing about why the Sun generates light, why Mars is red, or why one side of the moon always faces the earth. It does, however, dispatch its limited duties quite well. Moreover, while both the Ptolemaic and Keplerian models do a fine job of predicting planetary motions, Kepler’s has the advantage of being well-
integrated with the rest of astronomy and the rest of science. Indeed, our best model of the formation of our solar system not only explains why the planets move the way they do, but also why the Sun generates light, why Mars is red, and why one side of the Moon always faces the Earth.

A similar story can be told with regard to FP. While it provides a fine explanation for some general facts about human behavior, it was never meant to explain sleep, creativity, memory, intelligence differences, or the many forms of learning. To its credit, however, it is (pace Churchland) well-integrated with the various sciences of cognition, and these sciences have a lot to say about the phenomena for which Churchland demands explanations. For instance, the study of the encoding, storage, and retrieval of declarative memories is clearly the study of an important form of learning; according to one popular view, sleep is part and parcel of the hippocampal consolidation process described earlier; and creativity may best be explained by analogical mappings between representations of familiar and unfamiliar domains. The charge that FP has remained stagnant is refuted by these same considerations. While the theory of natural selection and the Keplerian model of planetary motions have both remained relatively unchanged since their introduction, developments in a variety of fields constitute progress for these theories. In precisely the same manner, developments in CS constitute progress for FP.

5.2 Radical Simulation

Stage 2 of the gauntlet has to do with the matter of whether or not the predictive and explanatory successes enjoyed by the folk with regard to the behavior of their fellow humans stems from their reliance upon an ontology that posits beliefs and desires as amongst the causal determinants of behavior. The worry, once again, is that if the folk predict and explain the behavior of their compatriots by simulating rather than theorizing, then FP is atheoretical, and there is no folk psychological ontology to vindicate. To say the least, I have done little here to bolster the claim that folk do enjoy a good deal of predictive and explanatory success with regard to the behavior of their fellow humans. I have, in fact, taken great pains to clarify precisely why it is that they are not likely to succeed and why FP is none the worse for wear. Moreover, even if it happens to be the case that our attempts to predict and explain each other's behavior involve simulation, this is clearly not the whole truth of the matter regarding how it is that we view one another. Given what has been written throughout the centuries, one would be hard pressed to deny that we humans tend to view each other as creatures that believe, desire, plan, remember, infer, perceive, comprehend, and so on. Terms like
‘belief’ and ‘desire’ are clearly used by the folk on a regular basis to describe their fellow humans, and all indications are these terms are supposed to refer to unobservable intentional states; no one, at any rate, has come up with a viable alternative to this analysis. The simulation argument, then, was always a bit of a red herring. Indeed, even Gordon claims that the process of simulation has the ultimate effect of bootstrapping our grasp of the meanings of intentional terms. In short, whether we simulate or not, the question remains: Do these intentional terms refer? CS tells us they do.

5.3 Churchland (again) and Dennett

It is, of course, possible that CS is mistaken. For instance, perhaps dynamical systems theory or Gibsonian anti-representationalism (each of which has been claimed to do away with the need to posit mental representations by stressing the importance of environmental factors) will win the day; perhaps descriptions of trajectories through state space exhaust what we need to know about the underpinnings for human behavior; or perhaps a new type of reinforcement learning will be discovered that resurrects behaviorism. Bear in mind, however, that if CS really is so mistaken as to require the abandonment of FP, this would strike at the very foundations of CS and necessitate a paradigm shift spanning several disciplines. It would, for instance, require that we give up on the encoding specificity hypothesis, on the idea that short-term memory is the locus of inference, on the proposal that the hippocampus consolidates declarative memories, and on the Wernicke-Geschwind model of language comprehension and production. What the eliminativist advocates, in other words, is the abandonment of decades of fruitful research based upon the bare promise that something better is around the bend. The instrumentalist, for his part, simply overlooks this research.

Philosophers have, of course, made it a common practice to exaggerate the importance of the fringe elements in CS, and while predictions concerning impending paradigm shifts are about as common as doomsday prophesies, they are also about as warranted. Should one these prophesies chance upon the truth, so be it; I will stand corrected. In the meantime, those of us with a more sober disposition should be content to let the state of the art in CS, which is far from a fledgling enterprise, continue to act as our guide.

5.4 The LOT hypothesis

The strategy of showing that CS cannot do without the LOT hypothesis (i.e., that there are good a priori reasons for favoring it) seems to have been offered up as a way to defend this particular brand of intentional realism without having to
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establish that particular expressions of mentalese are token identical with particular brain states. Yet, as suggested earlier, even if these indispensability arguments were sound,\textsuperscript{59} they would only suffice to undermine Churchland’s brand of eliminativism, not Dennett’s instrumentalism.

Some might claim that, if not the LOT hypothesis, then at least some version of the computational theory of mind lies at the very foundation of mainstream CS in the same way that FP has here been said to. Fodor and Pylyshyn claim, for instance, that “It would not be unreasonable to describe Classical Cognitive Science as an extended attempt to apply the methods of proof theory to the modeling of thought ....”\textsuperscript{60} A look at the models advanced in such fields as cognitive psychology, cognitive neuropsychology, and cognitive neuroscience supplies little in the way of support for this contention. Indeed, much of what goes on in CS (see Sec. 4) is perfectly compatible with the outright rejection of the computational theory of mind. So too, for that matter, is much of the research undertaken in artificial intelligence. Thus the claim, accepted uncritically by so many philosophers, that CS is committed to the computational theory of mind is just another one of those archaic presuppositions which acts as a key premise in numerous debates and which—when the actual goings-on of CS are understood—is found to have little basis in fact. It thus is fortunate for FP that her fate is not tied to that of the computational theory of mind, nor, \textit{a fortiori}, to that of the LOT hypothesis.\textsuperscript{61}

\textbf{5.5 Content}

The final challenge confronting FP has to do with the matter of whether or not there is a place for contents in CS. It is sometimes claimed that FP will be in big trouble if CS is forced to eschew contents. It needs to be borne in mind, however, that significant portions of FP have already been vindicated by CS. This is highly relevant given that there is a growing awareness in the FP literature of the fact that a theory can be wrong in certain respects without its ontology undergoing elimination. The contrary view was, for instance, implicit in the arguments against FP offered by Stich,\textsuperscript{62} but he has since\textsuperscript{63} followed Lycan\textsuperscript{64} in adopting a more sensible position which recognizes that commonsense may direct us toward, and give us an imperfect appreciation for, natural kinds which are only later (e.g., through scientific inquiry) fully grasped.\textsuperscript{65} As such, claims Stich, we may be jumping the gun if we conclude that terms like ‘phlogiston’ and ‘witch’ fail to refer. Some of what commonsense said about these putative entities was wrong, but commonsense was not entirely wrong. The same, adds Crane, can be said of planets, but it is far from obvious that—prior to Galileo for instance—the term ‘planet’
failed to refer. By the same token, if the various folk models happened to be accurate in every respect save for their invocation of contents, then it would not be unreasonable to conclude that there really are beliefs and desires. This would surely be far less contentious than claiming that witches and phlogiston exist. After all, theories which quantified over witches and phlogiston—not to mention planets—occupied the extreme lower end of the theoretical-accuracy continuum, while FP, sans contents, sits somewhere along the upper-half of this continuum.

While these hedges should not be lost sight of, I believe that it is ultimately possible to show that FP as a whole actually lies very near the high end of the theoretical-accuracy continuum. Unfortunately the present analysis of CS does not, by itself, supply us with the positive conception of content required to show this. For present purposes, then, let me simply note that a positive conception of content for use in CS might differ from the implicit folk conception in terms of how it fixes contents. That is to say, as long as there is some theory of content that CS may legitimately invoke—even if it happens not to be the one the folk themselves implicitly rely upon—this will push FP a significant distance towards the high end of the continuum. Accordingly, proponents of FP can afford to largely ignore the question of precisely how it is that the folk take contents to be fixed and focus instead on the question of whether or not there is a theory of content that CS herself may legitimately invoke.

Finally, let me note that one other boon for FP is the fact that at least one set of arguments (i.e., the narrow content arguments) against the scientific legitimacy of contents is undermined by the foregoing considerations.

5.5.1 Narrow Content

Briefly, the first concern about narrow contents (see Sec. 2) is clearly premised upon the mistaken assumption that CS is primarily interested in formulating laws of the sort that specify the relationships between particular stimuli, particular internal states, and particular behaviors. Indeed, the problem that this argument brings to light ultimately has little to do with contents and much to do with the fact that human behavior is determined by highly complex arrangements of internal states, arrangements which can diverge in fairly radical ways from individual to individual. This simple fact renders it a fool’s errand to search for laws that quantify over particular mental state types and apply across individuals.

The question that naturally arises is this: How does CS cope with these vast differences between people? How does she cope, in other words, with the isotropic (see Sec. 4.2) nature of the underpinnings for human behavior? The answer is that CS
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copes with this complexity in the same way that radio engineers cope with the fact that only some radios play 'The Macharena', while fewer play the score to Carmen, and the vast majority never play 'The Time of No Reply'. As already explained, CS has adopted, refined, and vindicated a set of models concerning the mechanisms underwriting human behavior and is coming to understand what and where the key components are and how they work (e.g., what and where their components are). As such, CS offers theories that cut across vastly different belief networks. The fact that individuals have different belief networks does not belie the claim that they share mechanisms of belief-formation, memory, inference, language production/comprehension, and so on. If contents are determined by doxastic surroundings, this will not impede efforts to understand how these mechanisms are connected up and how their internal workings enable them to do what they do.

For its part, the second argument is clearly premised on the mistaken assumption that CS is committed to the computational theory of mind. As already explained, this is a myth that seems to have been propagated by philosophers who think they know what CS ought to look like without much appreciation for what it does look like.

5.5.2 Wide Content

The worry about the causal efficacy of wide contents requires a much more in-depth treatment than I can supply here. This is because, unlike the other members of the gauntlet, a more accurate portrayal of the going-on in CS does not send this one packing. It is worth noting, however, that the main argument against the scientific credibility of wide contents does mistakenly assume that sciences are obligated to eschew causally impotent properties. There are, in fact, many legitimate cases of scientific explanation that depend upon the invocation of causally impotent relational properties. For instance, as Jackson and Pettit point out, it is perfectly legitimate for physicists to explain the fact that two particles accelerate at the same rate by pointing out that the same force was applied to each. The sameness of force is clearly a causally impotent relational property (all of the causation occurs locally), but it is nevertheless an important part of the explanation for the fact at hand. Thus one relational property can explain another, even though the relational property doing the explaining is inert. Perhaps what is needed, then, is a better understanding of whether or not contents are causally impotent relational properties of the same scientifically legitimate variety.
6. Conclusion

While it would be impractical for me to address every concern voiced regarding FP, I believe that I have addressed many of the most troublesome. Much of the suspicion cast upon FP seems to dissipate once we pay closer attention to the actual predictive and explanatory practices of CS. Indeed, while philosophers have been busy placing bets on how CS will ultimately pan out, CS has been quietly effecting a rather persuasive vindication of FP and its ontology. It is, in addition, no small matter that many of the mistaken assumptions about CS described here also infect on-going debates in the rest of the philosophy of CS. In short, while CS herself does not stand in dire need of a paradigm shift, the philosophy of CS might.

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Notes

3 P. M. Churchland, *A Neurocomputational Perspective*.
5 For those unfamiliar with the adventures of Maxi, while she is out of the room the candy bar is moved from its original hiding spot to a new spot (H. Wimmer and J. Perner, "Beliefs about beliefs: Representation and constraining function of wrong beliefs in young children's understanding of deception," *Cognition* 13/1 [1983]: 103–28).
7 On the other hand, argue Stich and Ravenscroft, perhaps FP can be construed 'externally' as a body of propositions that quantify over certain theoretical posits and that entail platitudes that the folk find intuitive. *This* theory might turn out to be false, though at least one author has claimed that the appeal to external versions isn’t the boon for eliminativists that Stich and Ravenscroft suggest it might be (J. Pust, "External Accounts of Folk Psychology, Eliminativism and the Simulation Theory," *Mind and Language* 14/1 [1999]: 113–30).
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9 The phrase ‘impedimentis absentibus’ is far more appropriate than, and is recommended as a replacement for, the more common ‘ceteris paribus’. The former emanates from the collaboration of a philosopher of science (Bob Barrett) and the legwork of an historian (Eric Brown) who found precedent for the relevantly broad construal of the verb ‘impedire’ in his own translation of Seneca, De Otio 3.2, in L. Annaei Senecae Dialogorum Libri Duodeci, ed. L. D. Reynolds (Oxford: Clarendon Press, 1977).

10 Churchland, A Neurocomputational Perspective.


15 While the folk surely have no explicit, consciously accessible procedure for determining the contents of mental states (in fact, most probably have little awareness at all that there is a difference between contentful and noncontentful states), insofar as they posit intentional states such as beliefs and desires, they must at least have some implicit means of determining what the contents of such states are.


17 Ibid.


20 Save, perhaps, in cases where an individual is thinking about his/her self.


22 A notable absentee from the gauntlet is Kim’s argument against mental causation (J. Kim, Mind in a Physical World: An Essay on the Mind-body Problem and Mental Causation [Cambridge, Mass.: The MIT Press, 1998]). Bear in mind that if Kim is right, then CS is herself illegitimate and is thus incapable of vindicating FP. Here I simply assume the legitimacy of the predictive and explanatory practices of the special sciences. If I am wrong to do so, I will gladly take my lumps.

23 Fodor, The Elm and the Expert, 3.

24 Fodor, of course, does not share my enthusiasm for CS as a whole, preferring instead to place all of his eggs in psychology’s basket (J. A. Fodor, “Special Sciences [or: The Disunity of Science as a
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26 See J. A. Fodor, “Special Sciences.”
28 Insofar as one takes productions, operators, and such to be statements of laws, AI could be the exception, but we all know how well this project has fared (See J. A. Fodor, The Mind Doesn’t Work That Way [Cambridge, Mass.: The MIT Press, 2000]).
38 For evidence that our nearest relatives are far more prone to ineffective fumbling about, see D. J. Povinelli, Folk Physics for Apes: The Chimpanzee’s Theory of How the World Works (New York: Oxford University Press, 2000).
43 Even critics of the general applicability of natural selection accounts (see Gould and Lewontin) would be unwilling to abandon the basic ontology of states and processes constituting the theory of natural selection, though they do downplay their importance.
45 We should, however, not lose sight of the fact that no new species has ever been created in this way.
48 Horgan and Woodward claim that their theory overcomes the deficiencies of instrumentalism insofar as it invokes genuine causal
explanations, but surely predictive mileage can be had merely from treating a system as if it were governed by certain causal interactions. For example: If the center of gravity is too far aft, this can cause a plane to stall.


51 By the by, it seems awfully surprising that Fodor should pooh-pooh this implementation-specific research given that it is the only thing that stands between him and Dennett.


53 Churchland, On the Contrary, 8.

54 Ibid., 22–3.


57 Fodor similarly suggests that progress in psychology constitutes progress for FP, though he neglects to spell out precisely how (Fodor, Psychosemantics).

58 Gordon, “Radical Simulationism.”


62 Stich, From Folk Psychology to Cognitive Science.

63 Stich, Deconstructing the Mind.


67 Fodor, Psychosemantics, The Elm and the Expert; Stich,
Folk Psychology and the Gauntlet of Irrealism

*Deconstructing the Mind.*


69 For more on this, see J. A. Waskan, “The Causal Impotence and Explanatory Potency of Wide Contents,” under consideration.