

# THEORY-CONJUNCTION AND MERCENARY RELIANCE\*

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Scientific realists contend that theory-conjunction presents a problem for empiricist conceptions of scientific knowledge and practice. Van Fraassen (1980) has offered a competing account of theory-conjunction which I argue fails to capture the mercenary character of epistemic dependence in science. Representative cases of theory-conjunction developed in the present paper show that mercenary reliance implies a "principle of epistemic symmetry" which only a realist can consistently accommodate. Finally, because the practice in question involves the conjunction of *theories*, a version of realism more robust than the "entity realism" of Cartwright (1983, 1989) and Hacking (1983) is required to explain the success of theory-conjunction.

**1. Introduction.** Modern science is the product of an enormous effort of intellectual cooperation, cooperation often made necessary by epistemic restrictions on expertise. A respected expert in one field is a humble novice in another. Yet, scientists routinely test a target theory by jointly applying it with a well-confirmed remote theory, thereby expanding the target theory's class of observational consequences; and so, the neuroscientist relies on the circuit theorist, the geneticist on the x-ray crystallographer, the psychopharmacologist on the biochemist, and so on. But theory-conjunction also demonstrates a type of social dependence that has a striking epistemological feature.<sup>1</sup> Target theorists are typically pro-

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<sup>1</sup>There are many forms of epistemic dependence in ordinary and scientific life which are too diverse, I suspect, to admit of an account which is general and at the same time illuminating. As laypersons, we rely on doctors' medical knowledge. In unfamiliar surroundings, we often rely on natives for directions. We typically accept the diagnoses of plumbers and mechanics on matters hydraulic and automotive before relying on our own judgement. In science, even similarly trained experts from the same narrow scientific field depend epistemically on one another. They seldom attempt to replicate each other's experiments, even though they would be competent to do so; instead, one simply defers to the research the other has done. Where epistemic dependence is intratheoretic, mutual deference results not from differences in competence, but from shared interest in advancing as quickly as possible to the frontiers of research. The best general discussion of epistemic dependence is Hardwig (1985). Kitcher (1990) and Lehrer (1977) also present original and illuminating treatments of this issue.

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foundly innocent of, and without sustained professional interest in, the content of the remote theory. Their reliance on the remote expert is mercenary, and at the same time enormously successful.

My purpose is to transform the fact of mercenary reliance into a powerful objection—different from the standard realist conjunction-objection—to contemporary empiricist conceptions of scientific practice. I will close with several observations about how the dependable epistemic role of mercenary reliance can only be explained on a robust realist account of scientific knowledge, rather than on the more modest “entity realism” now achieving some prominence.

**2. The Conjunction-Objection to Empiricism.** The phenomenon of theory-conjunction has long been recruited as an objection to empiricist philosophy of science. Indeed, among scientific realists, the conjunction-objection has served double duty; it has been presented both as a fundamental objection to empiricist accounts of scientific knowledge, and as the basis for a realist alternative to them. But none of the realist arguments exploit the epistemological feature of mercenary reliance to their advantage. For example, the conjunction-objection was originally posed by Putnam (1975, 210–211) as a *logical* feature of theory testing: When scientists accept  $T_1$  as true and independently accept  $T_2$  as true, normally they successfully apply them together, demonstrating that the practice of theory-conjunction is truth-preserving, and that scientists presuppose the deductive closure of theories under the union operation.<sup>2</sup>

The confidence and success with which scientists routinely infer the truth of the conjunction of two independently well-confirmed theories, it is claimed, is intelligible only on a realist conception of scientific knowledge. According to the empiricist, our knowledge extends only to observable phenomena, and this epistemological restriction does not seem to rationalize successful scientific practice. The fact that, taken independently,  $T_1$  and  $T_2$  is each empirically adequate does not imply that their conjunction ( $T_1$  and  $T_2$ ) is also empirically adequate. After all,  $T_1$  and  $T_2$  may make contradictory claims about unobservables, and these contradictory claims would then be used, in accordance with the empiricist model of theory testing, to calculate observational values in the deduction of observational predictions.

<sup>2</sup>The original picture of theory-conjunction drawn by Putnam is clearly intended as an *idealization* of scientific practice since he is well aware that no current scientific theory is *completely* true. Indeed, he indicates that the inference would go through even if the theories taken independently are “highly probable” or “approximately true”. So it is somewhat misleading when he states in a later work that the conjunction-objection was designed to show that any account of theory acceptance “that lacks the property of deductive closure—fails to justify norms of scientific practice” (1978, 102). Nevertheless, the phenomenon of theory-conjunction is originally framed as a *logical* feature of scientific practice.

In its logical form, the conjunction-objection meets a ready response from the empiricist. The realist, it is charged, has misunderstood the practice of theory-conjunction. Since theories are seldom simply and unhesitatingly conjoined, van Fraassen concludes, "There can be no phenomena of the scientific life of which this simple account draws a faithful picture" (1980, 85). Once properly understood, the resulting account of the joint application of theories will honor contemporary empiricist strictures on theory testing and theory choice; joint use implies only the belief that the theories in question are empirically adequate, not, as the realist claims, that they are (approximately) true.

Empiricists and realists alike recognize the methodological importance of relying on remote theories, which are often identified as "auxiliary hypotheses". Boyd (1973) invokes an actual case of the conjunction of bacteriology and an "auxiliary" theory, biochemistry, in defense of a methodological principle whose reliability putatively requires a realist explanation. The methodological principle in question states that a proposed theory must be tested in conditions under which, according to a collateral theory, the tested theory is most likely to fail. Boyd argues that it is only if the collateral field's theoretical claims are accurate, representing probable causal knowledge about underlying mechanisms, that we can adequately explain the reliable operation of this methodological principle.

In more recent work, the realist consequences of the practice of theory-conjunction have been made even more explicit. Friedman (1983, 236–263) contends that the results of this practice provide an important further source of confirmation for a literal, realist interpretation of a well-confirmed theory, a source not enjoyed by a formal (Ramsified) representation of the observable phenomena. Boyd (1985, 216–217) argues that theory-conjunction has a further striking consequence: An independently well-confirmed theory must be seen as confirming *future* conjunctions involving that theory.<sup>3</sup>

All of these realist observations about the practice of theory-conjunction

<sup>3</sup>In his attack on explanationist defenses of realism, Fine (1986) argues that the realist's inference from instrumental success to truth is mediated by the pragmatist's notion of reliability. Therefore, if the explanationist argument for realism is to be non-question-begging and depend only on the instrumental reliability of a science, then the realist's appeal to truth is frivolous, doing no more explanatory work than the instrumentalist's more parsimonious (and therefore preferable) appeal to a merely pragmatic conception of reliability. Such considerations lead Fine to advance a "proof" of a "metatheorem" which states that "to every good realist explanation there corresponds a better instrumentalist one" (*ibid.*, 154). Fine ably describes the conditions that an instrumentalist account of theories would need to satisfy in order for it to be regarded as plausible. However, he offers no independent argument that these conditions are indeed satisfied. And although Fine does discuss the tendency toward theoretical unification, he nowhere squarely addresses the conjunction-objection, traditionally thought to be the most damaging criticism of an instrumentalist account of theories.

evade van Fraassen's initial criticism of Putnam's simple account. However, they do not anticipate his empiricist reconstruction of theory-conjunction, nor do they highlight the epistemological feature of theory-conjunction—mercenary reliance—that represents the primary obstacle to van Fraassen's reconstruction.

**3. The Constructive Empiricist “Rational Reconstruction” of Theory-Conjunction.** Van Fraassen's constructive empiricist interpretation of theory-conjunction first construes the joint application of theories as a process of prior *correction*, not, as the realist initially claims, one of *conjunction*. Van Fraassen describes, or perhaps merely advocates, a certain kind of conjunctive behavior:

. . . in practice scientists must be very careful about conjoining theories, simply because even if (as the realists hold) acceptance is belief, it rarely happens that non-tentative, unqualified acceptance is warranted. There was a time when the Bohr-Sommerfeld theory of the atom looked victorious, and at the same time special relativity was generally accepted but—you see the point—it would not have made sense to conjoin them: the former would have to be subjected to a relativistic correction. Putting the two theories together would not have meant conjunction, but correction. (1980, 83–84)

Van Fraassen concedes that scientists indeed apply theories jointly, and that this practice is reliable, so his normative recommendation that scientists “must be very careful about conjoining theories” is irrelevant. The realist has an explanation for the success of the unrevised practice of joint application. The empiricist does not. What we need from the empiricist is either an explanation for the reliability of the joint application of theories, or convincing evidence that scientists routinely correct theories before jointly applying them.

The constructive empiricist account of theory-conjunction fails first as a description of scientists' behavior. As Demopoulos (1982) points out, the realist need not deny this prior process of correction since the conjunction-objection would then apply to the *corrected* version of the theory in any case. Although this realist response is admirably charitable in conceding to the empiricist that correction *sometimes* occurs *before* joint application, it is just as important to recognize that such cases are relatively infrequent. In the typical case, the scientists lack the appropriate expertise in the collateral theory to carry out the prior corrections, and, since our current sciences are still incomplete, often investigators do not yet know prior to conjunction that the corrections are strictly necessary. So the constructive empiricist still must explain why theory-conjunction succeeds even though the putatively necessary corrections were

not made. By contrast, the realist is in a position to say that the theories which are in need of prejunction correction can be successfully conjoined in uncorrected form as long as the theories' projected values for unobservable causal mechanisms are suitable approximations of their actual values. Van Fraassen draws attention to the process of prior modification in order to suggest that theory-conjunction should be assimilated to the merely pragmatic aim of theoretical unification, in stark contrast to the special epistemic function of theory-conjunction required by the realist argument. However, because scientists' reliance on the remote theory is mercenary, their behavior hardly represents a merely pragmatic concern for unification, for "a single theory to cover disparate domains of phenomena" (1980, 86).

Second, according to van Fraassen the scientist's reliance on the joint application of theories is *tentative*, an important consideration supposedly misrepresented in the realist's understanding of theory-conjunction as the union of two theories *believed to be true*:

. . . as long as we are scientific in spirit, we cannot become dogmatic about even those theories which we whole-heartedly believe to be true. Hence a scientist must always, even if tacitly, reason *at least* as follows in such a case: if I believe  $T$  and  $T'$  to be true, then I also believe that  $(T$  and  $T')$  is true, and hence that it is empirically adequate. But in this new area of application  $T$  and  $T'$  are genuinely being used in conjunction; therefore, I will have a chance to see whether  $(T$  and  $T')$  really is empirically adequate, as I believe. That belief is not supported yet to the extent that my previous evidence supports the claims of empirical adequacy for  $T$  and  $T'$  separately, even though that support has been as good as I could wish it to be. Thus my beliefs are about to be put to a more stringent test in this joint application than they have ever been before. (1980, 85)

Missing from van Fraassen's scenario is the realist appeal to the belief that  $(T$  and  $T')$  is true. In an effort to cast the scientist's attitude as involving some sort of acceptance weaker than belief, van Fraassen now presents the scientist's doxastic commitment as an antecedent in a protracted pattern of *conditional* reasoning. The scientist's reasoning must be represented as conditional because her epistemic commitment is thought to be tentative, and her epistemic commitment must be construed as tentative if we are to avoid casting the scientist as dogmatic.

In the case studies that follow, we will see just how far van Fraassen's reconstruction departs from actual cases of the joint application of theories, and how easy these examples of mercenary reliance are to come by. In particular, these scenarios will illustrate the relation of mercenary reliance between scientists: The target theorist is typically profoundly in-

nocent of, and without sustained professional interest in, the content of the remote theory.

#### 4. Examples of Mercenary Reliance in Theory-Conjunction.

*4.1. Evolutionary Biology and Paleontology: Their Dependence on Paleomagnetic Dating.* Evolutionary biologists and paleontologists offer explanations for (among other things) the extinction of certain species, the generation of new species, and the flourishing of still others. These explanations typically appeal to climatic conditions and food sources during the species' period of existence. In order to correlate fossils of a particular species with important changes in climatic conditions and in the availability of food that might have affected the species, evolutionary biologists and paleontologists defer to theoretically remote procedures for dating rock strata.

One dating technique derives from the phenomenon of paleomagnetism. As volcanic flows cool, and sedimentary layers are deposited, metallic elements contained in these beds align themselves with the earth's coincident magnetic poles. The earth's poles have spontaneously switched at various periods throughout its history, producing sharp discontinuities in the orientation of metallic components within strata. Because these geomagnetic pole reversals occur in almost entirely random sequences, once the times of the reversals have been fixed by (for example) potassium-argon dating methods, the geologist can use the randomly placed reversals to produce striped maps, providing distinctive "geomagnetic fingerprints" for specific periods in the earth's recent history.

Paleomagnetism has played its most important role in confirming hypotheses about plate tectonics and the rate of seafloor spreading (see Cox and Hart 1986, Piper 1987, and Tarling 1971). But paleomagnetic data have also been used successfully to examine the distribution of fossils (Piper 1987, 133), the coincidence of polarity changes with the extinction and generation of species (Tarling 1971, 127), and the range of oil deposits (Tarling 1971, 148–150). *Paleontologists* therefore routinely appeal to the results of paleomagnetic dating to confirm a variety of *evolutionary* hypotheses about the effects of specific catastrophic events; the flora dominant within a particular global range and period; and the rate of the generation, reproduction, and extinction, of species. Despite the deep involvement of paleomagnetic dating techniques in evolutionary biology and paleontology, it is doubtful that theorists in the target field are familiar with even the most rudimentary principles of physics that make paleomagnetic dating possible—details underlying, for example, the hysteresis loop or magnetic dipoles.

4.2. *Psycholinguistics and Implementations of Signal Processing Theory.* Psycholinguists want to know, among other things, what the significant units of speech are, how they are marked, and how they contribute to our comprehension of running discourse. Some attempts to explore these questions require the juxtaposition of speech sounds that do not normally occur together. For this purpose, splicing tape-recorded segments together will not work since listeners will respond to artifactual discontinuities caused by the splicing. Speech-perception theorists circumvent this problem by digitizing the speech, at which time selected segments can be carefully manipulated. This process involves transforming the analog signal (speech or tape-recording) into a digital signal by breaking the continuous natural signal into a series of discrete units (called “samples”). Each sample is assigned a specific value. It is now standard to digitize segments at 20,000 samples per second, but the higher the sampling rate, the higher the resolution of the resulting speech. Digitizing speech also allows the scientist to isolate and systematically vary distinctive components of the complex signal, such as formant frequencies and amplitude envelope.

A sophisticated implementation of signal processing theory forms the basis of the digitizing process, a theory whose scope includes not just speech, but signals of any kind. Despite their crucial reliance on it, psycholinguists typically know very little about the implementations of signal processing theory, about the way in which it has been used to produce radar, control satellite attitudes, and analyze harmonic vibrations. They are innocent of even those principles which closely guide the appropriate digital reconstruction of speech, such as the Transfer Function and the Sampling Theorem. (The Sampling Theorem characterizes an important restriction on the digital reconstruction of natural speech: The analog signal must be sampled at a rate at least twice that of its highest frequency, a rate alternatively called the “Nyquist”, “folding”, or “aliasing” frequency. See Stearns 1975, 37–40, and Schwartz and Shaw 1975, 33–44, for a description of this sampling restriction.)

By indulging their innocence, psycholinguists are being efficient, not sluggardly. Engineers have designed the reconstruction filters, and computer scientists and signal processing theorists have developed the speech editing programs. Therefore, psycholinguists have been free to study, with notable success, the role of such factors as voicing, coarticulation, phoneme boundaries, and stress in speech perception.

Faced squarely with detailed analyses of actual cases, van Fraassen’s reconstruction of theory-conjunction appears strained. Whatever philosophical allure there may be in the sanitized reconstruction of theory-conjunction as a process of *prior correction* and *tentative* application, evidence of this fastidiousness in real science is difficult to find. Scientists

typically do not have the expertise in the relevant fields to make the (allegedly required) preconjunction corrections, nor do they routinely recognize any significant benefits of being tentative in joint applications of theories. In part this is simply because, as novices with respect to the other theory, scientists would not know what they were supposed to be tentative *about*.

At the same time, no one doubts that theory-conjunction is reliable. Therefore, if it would be dogmatic for a scientist to unhesitatingly conjoin independently well-confirmed theories, as van Fraassen claims, it would be dogmatic in some sense of “dogma” that is not pejorative. Unlike religious dogmatists who remain committed to their positions no matter how inconsistent or otherwise implausible the consequences, scientists are typically willing to revise their positions in light of postconjunction results. Null results issuing from an experimental test involving theory-conjunction may motivate the scientist to modify the target theory, or (with the guidance of a remote expert) to replace the collateral theory with one potentially more sensitive to the variable being tested. This does not necessarily indicate that it was unwise to have unhesitatingly conjoined the original theories; for, at the time, there may have been no special reason to suppose that the two theories could not have been effectively conjoined. In light of the scientist’s willingness to revise her original stance, her behavior appears to be far from dogmatic.

Once the specter of dogma has been dismissed, van Fraassen’s reconstruction of theory-conjunctive reasoning as *conditional* appears to be not only unnecessary, but inaccurate as well. It does not follow from the scientist’s respect for feedback (which sometimes counsels revision) that her original reasoning must have been conditional, or her initial epistemic stance, tentative. There are certainly cases in which scientists are tentative in conjoining theories which are on the pioneering fringes of research, but this is only because they are tentative in their acceptance of each theory taken independently. Routine and representative cases of theory-conjunction, such as those described above, reveal the scientists’ robust commitment to the approximate truth of the resulting conjunction. Scientists then leave it to the world to correct them when they are wrong, revising their beliefs accordingly.<sup>4</sup>

<sup>4</sup>Whatever plausibility van Fraassen’s strategy enjoys may derive not from an examination of the psychology of scientific reasoning, but from an independent and general dictum that a person’s posterior level of epistemic commitment to a certain belief is equivalent to the prior conditional probability of that belief. But, as we discovered above, it would be psychologically anachronistic to claim that the scientist’s *actual* reasoning to the prior belief (that two theories can be conjoined) was conditional or otherwise tentative on the mere grounds that this belief is open to revision in light of posterior evidence. Thus, as a psychological account of (scientific) reasoning, van Fraassen’s empiricist reconstruction of theory-conjunction is subject to the same criticism leveled against Bayesian ac-

**5. Motivating Epistemic Symmetry.** Having abandoned the view that theory-conjunction proceeds tentatively and by prior correction, the empiricist may concede the existence of mercenary reliance, but argue that its existence is compatible with empiricist strictures. The empiricist might argue as follows. The success of mercenary reliance can be explained in terms of the target scientist's selective familiarity with, or exclusive dependence on, the *observational consequences* of the remote theory. Of course, when conjoined with the remote theory, the target theory has observational consequences it would not have if taken in isolation. But if these additional observational consequences derive entirely from the target theorist's selective dependence on the observational consequences of the remote theory, then we need to presuppose only the empirical adequacy of the conjoined theories, not their approximate truth.

What we will find, however, is that the target scientist also relies on claims about unobservables made by the remote theory, and these claims are crucial to the observational testing of the target theory. Moreover, laboratory settings reveal the target scientist as equally familiar with and dependent upon the observational *and* theoretical claims of the remote theory, and thus (given the distance of the target from the remote theory) as equally innocent of both types of claim. If the "epistemic symmetry" implied by mercenary reliance is vindicated by actual scientific practice, a descriptively adequate empiricism could not disregard the theoretical import of the joint application of theories.

The empiricist contention that mercenary reliance involves nothing more than *instrumental* knowledge of the remote theory is perhaps understandable; the paleontologist and psycholinguist rely heavily on the (observable) output of laboratory instruments. But while mercenary reliance is no doubt observationally or instrumentally reliable, it is just as obviously motivated by theoretical considerations.

The paleontologist uses a magnetometer to determine the orientation of

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counts of scientific reasoning. Glymour states that

. . . particular *inferences* can almost always be brought into accord with the Bayesian scheme by assigning degrees of belief more or less ad hoc, but we learn nothing from this agreement. What we want is an explanation of scientific argument; what the Bayesians give us is a theory of learning, indeed a theory of personal learning. But arguments are more or less impersonal. . . . (1980, 74)

Giere clearly recognizes the anachronistic character of Bayesian analyses of scientific reasoning:

Scientists, as a matter of empirical fact, are not Bayesian agents. Reconstructions of actual scientific episodes along Bayesian lines can at most show that a Bayesian agent would have reached similar conclusions to those in fact reached by actual scientists. Any such reconstruction provides no explanation of what actually happened. (1988, 157)

magnetic components in rock samples. The design of the magnetometer depends in part on principles of magnetism familiar only to the remote theorist, but its *use* by the target scientist, the empiricist argues, requires only that the target scientist attend to the (observable) report of magnetic orientation yielded by the instrument. Such technical reliance might be thought to rest only on these *observational* consequences of the theory of magnetism, but, as a matter of fact, paleontologists are concerned to determine the rock sample's *age*, and they do not treat the (observable) magnetometer readings as a report of the rock's age. After all, paleontologists recognize that a variety of ways exist in which the reported orientation can fail to accurately reflect the age of the rock sample. Igneous intrusions and lightning strikes can remagnetize or "overprint" the strata, causing the magnetometer to incorrectly report the original (called "primary") remanent magnetization. Paleontologists are not taken in by such observable values, precisely because they take the instrument to be a detector not of *age*, but of such *unobservable* properties of the sample as *magnetic moment*. Paleontologists' reliance on claims concerning unobservable magnetic factors is further illustrated by their sensitivity to improvements in measurement and detection procedures; they now insist that samples identified as theoretically problematic receive magnetic scrubbing, a process which strips away layers of magnetic overprint by exposing the rock sample to an alternating magnetic field or to controlled heating. The need for magnetic scrubbing, however, could be appreciated only if the paleontologist presupposed that the sample possesses the unobservable magnetic properties reported by the magnetometer. So the paleontologist's behavior reflects a sensitivity to both the theoretical claims about magnetic properties and the observational consequences of the conjunction of the theory of (paleo)magnetism and paleontology. (The same points about mercenary reliance, epistemic symmetry, and sensitivity to error also apply to the paleontologist's use of *radiometric* dating methods and the isotope-detecting *spectrometer*.)

The psycholinguist's position with respect to the instruments of speech analysis is no different than the paleontologist's stance toward paleomagnetic dating instruments. Once conjoined with an implemented signal processing theory, psycholinguistics has observational consequences concerning the perceptual effects of digitized noise on a range of linguistic abilities. However, when analyzing suitably short segments of speech, psycholinguists do not assume that the signal processing system detects and transforms perceptually significant features of speech *per se*, but rather *unobservable* acoustic features of the signal. For example, often psycholinguists strain the perceptual system by adding digitized white noise to the speech stream in order to identify those acoustic factors that

benefit speech perception under less than ideal circumstances. One such technique adds noise to the digitized speech, and then examines the subject's ability to distinguish the noise-only stimuli from the speech + noise stimuli. Perceptual discrimination will be easy if the noise has starkly different features (e.g., is louder) than the speech sound to which it is added. In order to avoid perceptual effects that are artifacts of detectable discontinuities in the digitized speech, psycholinguists have exploited signal processing techniques which direct the noise to follow certain important features of the speech, such as amplitude envelope.

Now, although psycholinguists embrace the ability of the analog-to-digital hardware to reconstruct with considerable fidelity the perceptible features of natural speech, psycholinguists also assume that the individual units or "samples" of the signal are unobservable, and that it is these units that the digital system processes. This assumption is evident in the methodological choices that psycholinguists make about when to employ particular instruments and techniques. The signal-dependent noise-producing technique developed by Schroeder (1968) should not be used when we want to preserve spectral features of speech. However, as is evident in the psycholinguistic use of the signal-dependent technique (see, for example, Repp and Frost 1988 and Samuel 1987), this methodological *prohibition* could be reliable (as it indeed is) only if the psycholinguist takes the technique (along with the related hardware) to really be randomly flipping the (+/-) sign of each point; and each point, taken by itself, is unobservable, having no influence on performance. In addition, the technique could yield this specific (amplitude envelope-preserving) *improvement* over the method it replaced—which instead based the noise on the digital root mean square of the amplitude of the original segment, not on its amplitude envelope—only if there are these 1/20,000-of-a-second points on the speech wave whose amplitude values (or signs) are getting randomly flipped.

These scenarios are important for two reasons. First, they reveal that the remote theory's claims about unobservable causal factors have immediate and direct observational consequences for the target theory, consequences that could not be tested if the two theories had not been conjoined. In conjunction with the suitable target theory, manipulations of digitized speech can produce predictable perceptual effects on performance. The particular magnetic declination of a rock can indicate with some reliability that natural gas deposits neighbor the rock sample's stratum. Second, the target scientist's tendency to exploit improvements provided by the remote theory can be explained only if we cast the target scientist as recognizing that the relevant instrumentation detects *unob-*

*servable* causal factors.<sup>5</sup> In the cases discussed here, the unobservable causal factors are magnetism and a 1/20,000-of-a-second sample of a sound wave.

Therefore, the target theorist's knowledge of the remote domain is *not*, as our envisioned critic might contend, purely instrumental. Nor is it, in any obvious sense, even *primarily* instrumental; most of the observational consequences of the remote theory, taken independently, are no doubt lost on the target theorist. And at the same time, the fact that the target theorist is sensitive to some of the remote theory's claims about unobservables hardly constitutes evidence of expertise in the remote domain. This familiarity could be acquired without sustained study. So the target theorist is largely innocent of the remote theory, and at those points of contact between the target and remote theories, the target scientist appears to rely in roughly equal proportion on observational and theoretical claims of the remote theory.<sup>6</sup>

**6. Scientific Realism and the Principle of Epistemic Symmetry.** The two preceding sections together suggest that mercenary reliance presents general difficulties for empiricist conceptions of scientific knowledge. Section 4 illustrates the symmetry of epistemic innocence; target scientists are so unfamiliar with the content of the remote theory that they are (roughly speaking) *equally* innocent of the remote theory's observational *and* theoretical claims. Section 5 is designed to show that, to the modest extent that the target scientist *is* acquainted with the remote theory, the epistemic reliability of her methodological judgements depends not just on her familiarity with the observational claims of the remote theory, but on her acquaintance with its theoretical claims as well. Together, these two ar-

<sup>5</sup>In each of these cases of mercenary reliance, the target theory has also been *improved* by the remote theory's development of more sophisticated instrumentation. Subsequent improvements in the target theory, however, might be regarded as incompatible with the doctrine of mercenary reliance; if the target scientist is truly innocent of the remote theory, how could the former make the type of informed judgement necessary to yield improvement in the target theory? This is an important question, and I think an acceptable answer must address the distinctly *social* features of epistemic dependence. In particular, the target theorist need not have a detailed understanding of the remote theory, but must be sensitive to feedback from conjunction and able to recognize diagnostic features of expertise in the remote domain (see section 6).

<sup>6</sup>The empiricist critic might take a related tack, this time attempting to undermine epistemic symmetry by assimilating the target theorist's reliance on the remote theory to the lab technician's reliance on the remote theory. One might think that the existence of technicians—whose knowledge is primarily observational—is a good counterexample to the principle of epistemic symmetry. To the contrary, the lab technician is not engaged in a theoretical enterprise in the same way that the target theorist is, and the difference in their respective enterprises is a relevant one. In particular, the technician does not design and execute experiments, or evaluate the plausibility of one theory in light of its integration with a more secure, affiliated theory. In short, the technician does not normally conjoin theories.

guments form a single moral, which might be called the *principle of epistemic symmetry*.

If correct, the principle of epistemic symmetry diagnoses the difficulty empiricists have suffered trying to provide an account of scientific knowledge which both explains the success of scientific practice and accurately characterizes the psychological perspective of the scientist. If theory acceptance involves only the belief that the theory is observationally accurate, and if the principle of epistemic symmetry is correct, then the empiricist cannot explain how the scientist is supposed to exercise a selective and informed reliance on the observational consequences of the remote theory. Indeed, the target scientist is not in an epistemic position to preferentially believe only the observational claims of the remote theory; roughly speaking, the target scientist either believes all of the relevant information the remote theorist relates, or believes none of it; epistemic innocence prevents the target theorist from being discriminating.<sup>7</sup> And, in light of the argument presented in section 5, the target scientist's adoption of methodological improvements reveals a reliance on the remote theory's observational *and* theoretical claims. The consistent empiricist, then, faces the following dilemma: Admit that, contrary to empiricist epistemic strictures, the target scientist indeed has knowledge of unobservables; or insist that, contrary to the evidence of the cumulative development of scientific knowledge, theory-conjunction does not yield knowledge even about observables.

The realist, on the other hand, has no difficulty accommodating the principle of epistemic symmetry. In fact, the symmetry of epistemic innocence is exactly what we would expect if, as realists have urged, there is no epistemological motivation for the empiricist's proposed distinction between observables and unobservables. The positive task for the realist is to explain in adequate detail the features of epistemic dependence that make this particular form of intellectual cooperation both computationally efficient and enormously reliable. I think that these features can be explained in terms of target scientists' appropriate sensitivity to feedback from conjunction and their capacity to recognize diagnostic features of expertise in remote fields. But that proposal is beyond the scope of the present paper. Here, I have posed a difficulty for the empiricist which is generated by the familiar, crucial and pervasive practice of mercenary reliance in science. Since mercenary reliance in scientific practice is so successful, it would be little more than philosophical arrogance to insist

<sup>7</sup>In any field, of course, a handful of people of unusual background, talent, and stamina are arguably experts in a target *and* a remote domain; these scientists do not fully exhibit mercenary reliance. However, their rarity is precisely that which makes mercenary reliance so necessary, and its success in permitting us to make progress without such special talent that makes it so desirable.

on a “rational reconstruction” which treats *observational* claims as the sole source of the reliability of scientific methodology.

**7. Concluding Remarks: Entity Realism Is Not Enough.** Once we recognize the antiempiricist consequences of theory-conjunction, it is reasonable to ask which version of realism best explains the reliability of theory-conjunction. A robust version of realism holds both that the theoretical terms of mature sciences typically refer, *and* that the laws and theories of those mature sciences are typically approximately true (Boyd 1983). In an effort to finesse the phenomenon of theory-change and the role of idealization alleged to dog robust realism, the more modest “entity realism” of Hacking (1983) and Cartwright (1983, 1989) only endorses the existence of theoretical entities. Our apparent ability to perennially manipulate unobservable causes is the best evidence of their existence, while the falsehood of (theoretical) laws and theories is ultimately attested to by the obviously idealized conditions which they state, and by the historical probability that they will be rejected in the future: “Hence, engineering, not theorizing, is the best proof of scientific realism about entities” (Hacking 1983, 274).

The success of theory-conjunction, however, depends not just on the existence of postulated entities, but also on something like the deductive closure of laws and theories under the union operation. It is, after all, theories, and not entities, that are being conjoined.<sup>8</sup> The fact that approximate truth of theories is preserved under the taking of unions is therefore still inexplicable on a version of realism, such as entity realism, that eschews the approximate truth of laws and theories. Only a stronger version of realism can vindicate the success of theory-conjunction, and the adoption of this stronger version of realism will have dramatic philosophical consequences.

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<sup>8</sup>It is true that descriptive adequacy demands an account of theory-conjunction which permits something like approximate deductive closure, but of course this is not a special problem for the realist; the empiricist, who agrees that theories are routinely and reliably conjoined, must at least produce an account of the approximate closure of the theories’ joint observational consequences.

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