Philosophy of science emerged as a recognizable sub-discipline within philosophy only in the twentieth century. The possibility of such a sub-discipline is a result of the post-Enlightenment disciplinary and institutional separation of philosophy from the sciences. Before that separation, philosophical reflection formed part of scientific research—as, indeed, it must—and philosophy was usually guided by a sound knowledge of science, a practice that gradually lost currency after the separation.

In the nineteenth century, philosophical reflection on science resulted in a tradition of natural philosophy, particularly in Britain (with the work of Mill, Pearson, Whewell, and others), but also in continental Europe, especially in Austria (with Bolzano, Mach, and others). What is called philosophy of science today has its roots in both the British and the Austrian traditions, although with many other influences, as several entries in this Encyclopedia record (see, for instance, Duhem Thesis; Poincaré, Jules Henri).

This Encyclopedia is intended to cover contemporary philosophy of science. It is restricted to conceptual developments since the turn of the twentieth century. Its treatment of major figures in the field is restricted to philosophers (excluding scientists, no matter what the extent of their philosophical influence has been) and, with very few exceptions (notably Chomsky, Noam; Putnam, Hilary; and Searle, John), to those whose work is distant enough to allow “historical” appraisal. Conceptual issues in the general philosophy of science (including its epistemology and metaphysics) as well as in the special sciences are included; those in mathematics have been left for a different work. This Introduction will provide a guided tour of these conceptual issues; individual figures will only be mentioned in passing.

Historically, the themes treated in the Encyclopedia are those that have emerged starting with the period of the Vienna Circle (see Vienna Circle), including the figures and developments that influenced it (see Bridgman, Percy Williams; Duhem Thesis; Mach, Ernest; Poincaré, Jules Henri). The work of the members of the Vienna Circle provide a link between the older natural philosophy, especially in its Austrian version, and the later philosophy of science, which borrowed heavily from the concepts and techniques of the mathematical logic that was being created in the first three decades of the last century (see Hilbert, David; Ramsey, Frank Plumpton; Russell, Bertrand; see also Ayer [1959] and Sarkar [1996a]). The new set of doctrines—or, more accurately, methods—came to be called “logical positivism” and, later, “logical empiricism” (see Logical Empiricism; see also Sarkar [1996b]). By the 1930s these views had spread beyond the confines of Vienna and had attracted allegiance from many other similarly-minded philosophers (see Ayer, A. J.; Quine, Willard Van; Reichenbach, Hans). Two attitudes were widely shared within this group: a belief that good philosophy must be conversant with the newest developments within the sciences (see Rational Reconstruction), and a rejection of traditional metaphysics imbued with discussions with no empirical significance (see Cognitive Significance; Verifiability).

Some members of the Vienna Circle also took the so-called linguistic turn (see Carnap, Rudolf) and viewed scientific theories as systems formalized in artificial languages (Sarkar 1996c). Arguably, at least, this work lost the prized contact with the practice of science, and this development contributed to the eventual rejection of logical empiricism by most philosophers of science in the late twentieth century. However, a number of the original logical empiricists, along with many others, rejected the linguistic turn, or at least did not fully endorse it (see Neurath, Otto; Popper, Karl Raimund; Reichenbach, Hans). The tensions between the two views were never fully articulated during this period, let alone resolved, because the Vienna Circle as an
institution and logical empiricism as a movement both came under political attack in Europe with the advent of Nazism. Most of the figures involved in the movement migrated to the United Kingdom and the United States. In the United States, many of the logical empiricists also later fell afoul of McCarthyism (see Logical Empiricism).

In the United States, Nagel probably best exemplifies what philosophy of science became in the period of the dominance of logical empiricism. The discussions of Nagel's (1961) *Structure of Science* typically include careful formal accounts of conceptual issues, but these are supplemented by detailed “nonformal” discussions in the spirit of the tradition of natural philosophy—this book may be viewed as a summary of where logical empiricism stood at its peak (see Nagel, Ernest). However, starting in the late 1940s, many of the theses adopted by the logical empiricists came under increasing attack even by those committed to keeping philosophy in contact with the sciences (Sarkar 1996e). (The logical empiricists had explicitly advocated and practiced intense self-criticism, and many of these attacks came from within their ranks—see Hempel, Carl Gustav.) Some of this criticism concerned whether cherished doctrines could be successfully formulated with the degree of rigor desired by the logical empiricists (see Analyticity; Cognitive Significance).

However, the most serious criticism came from those who held that the logical empiricists had failed to give an account of scientific confirmation and scientific change (see “Confirmation,” “Scientific Discovery,” and “Scientific Change,” below). Feyerabend, for one, argued that the logical empiricists had placed science under an inadmissible rational straitjacket (see Feyerabend, Paul). As philosophy of science took a distinctly historical turn, analyzing the development of science in increasing historical detail, many felt that the logical empiricists had misinterpreted the historical processes of scientific change (see Hanson, Norwood Russell; Kuhn, Thomas). Kuhn’s (1962) *Structure of Scientific Revolutions*, originally written for an encyclopedia sponsored by the logical empiricists, was particularly influential. By the mid-1960s logical empiricism was no longer the dominant view in the philosophy of science; rather, it came to be regarded as a “received view” against which philosophers of science defined themselves (Suppe 1974). However, this interpretation of logical empiricism ignores the disputes and diversity of viewpoints within the tradition (see, especially, Logical Empiricism), arguably resulting in a caricature rather than a responsible intellectual characterization. Nevertheless, for expository ease, the term “received view” will be used in this Introduction to indicate what may, at least loosely, be taken to be the majority view among the logical empiricists.

Scientific realism and various forms of naturalism, sometimes under the rubric of “evolutionary epistemology,” have emerged as alternatives to the logical empiricist interpretations of science (see Evolutionary Epistemology; Scientific Realism). Meanwhile, science has also been subject to feminist and other social critiques (see Feminist Philosophy of Science). Kuhn’s work has also been used as an inspiration for interpretations of science that regard it as having no more epistemological authority than “knowledge” generated by other cultural practices (see Social Constructionism). However, whether such work belongs to the philosophy of science, rather than its sociology, remains controversial. While no single dominant interpretation of science has emerged since the decline of logical empiricism, the ensuing decades have seen many innovative analyses of conceptual issues that were central to logical empiricism. There has also been considerable progress in the philosophical analyses of the individual sciences. The rest of this Introduction will briefly mention these with pointers to the relevant entries in this work.

**Theories**

The analysis of scientific theories—both their form and content—has been a central theme within the philosophy of science. According to what has become known as the “received view,” which was developed in various versions by the logical empiricists between the 1920s and 1950s, theories are a conjunction of axioms (the laws of nature) and correspondence rules specified in a formalized ideal language. The ideal language was supposed to consist of three parts: logical terms, observational terms, and theoretical terms. Logical claims were treated as analytic truths (see Analyticity), and were thought by many to be accepted as a matter of convention (see Conventionalism). Observational claims were also thought to be unproblematic, initially understood as referring to incorrigible sense-data and later to publicly available physical objects (see Phenomenalism; Physicalism; Protocol Sentences). The correspondence rules were supposed to allow the logical empiricists to give cognitive significance (see Cognitive Significance; Verifiability) to the theoretical portion of the language, by specifying rules for connecting theoretical and observational claims. In their extreme version, these correspondence rules took the form...
of operational definitions (see Bridgeman, Percy Williams). One goal of such attempts was to distinguish science from non-science, especially what the logical empiricists derided as “metaphysics” (see Demarcation, Problem of).

Starting in the 1960s, the received view encountered a number of problems. Even earlier, difficulties had arisen for the correspondence rules, which took various forms over the years as a result of these problems. Initially understood as explicit definitions, they were later treated as partial definitions, and in the end the theoretical terms were merely required to make a difference to the observational consequences of the theory. One central focus of the criticism was on the observation-theory distinction (see Observation). It was argued that the theoretical and observational portions of language are not distinct (Putnam 1962; Achinstein 1968; see also Putnam, Hilary), that the distinction between entities that are observable and those that are not is vague (Maxwell 1962), and that observations are theory-laden (Hanson 1958; see also Hanson, Norwood Russell; Observation). In addition, there were problems ruling out unintended models of theories, which became a source of counterexamples. In hindsight, it is also clear that the problem of demarcating science from non-science was never fully solved.

More recently, a number of philosophers have questioned the important place given to laws of nature on this view, arguing that there are scientific theories in which laws do not appear to play a significant role (see Biology, Philosophy of; Laws of Nature). Others have questioned not the occurrence of laws within theories, but whether any of these entities should be conceptualized as linguistic entities (which is quite foreign to the practice of science). Still others have wondered whether the focus on theories has been an artifact of the received view being based primarily on physics, to the detriment of other sciences. As the received view fell out of favor, starting in the 1960s, a number of philosophers developed various versions of what is known as the semantic view of theories, which understands theories as classes of models, rather than as linguistic entities specifiable in an axiomatic system. While not without its problems, the semantic view seemed to bring philosophical accounts of theories more in line with the practices of scientists and has become the generally accepted view of theories (see Scientific Models; Theories). Nevertheless, there is at present no consensus within the discipline as to how theories should be philosophically characterized.

### Scientific Models

Models are central to the practice of science and come in a bewildering variety of forms, from the double helix model of DNA to mathematical models of economic change (see Scientific Models). Scientific models were regarded as being of peripheral philosophical interest by the received view. Little philosophical work was done on them until the 1970s, with Hesse’s (1963) *Models and Analogies in Science* being a notable exception. That situation has changed drastically, with models probably now being the locus of even more philosophical attention than theories.

Two developments have contributed to the burgeoning philosophical interest in models:

(i) *The Semantic Interpretation of Theories.* The development of various versions of the semantic interpretation of theories has put models at the center of theoretical work in science (see Theories). For many proponents of the semantic view, the received view provided a syntactic interpretation of theories, regarding theories as formalized structures. Scientific models are then supposed to be construed in analogy with models in formal logic, providing semantic interpretations of syntactic structures. The semantic view inverts this scheme to claim that models are epistemologically privileged and that theories should be regarded as classes of models. The various semantic views have made many contributions to the understanding of science, bringing philosophical analysis closer to the practice of science than the received view. Nevertheless, almost all versions of the semantic view are at least partly based on a dubious assumption of similarity between models in logic and what are called “models” in science.

(ii) *Historical Case Studies.* How dubious that presumed similarity has been underscored by the second development that helped generate the current focus on scientific models: the detailed studies of the role of models in science that has been part of the historical turn in the philosophy of science since the 1960s. That turn necessitated a focus on models because much of scientific research consists of the construction and manipulation of models (Wimsatt 1987). These studies have revealed that there are many different types of models and they have a variety of dissimilar functions (see Scientific Models for a taxonomy). At one end are
models of data and representational material models such as the double helix. At the other are highly idealized models (see Approximation), including many of the mathematical models in the different sciences. Some models, such as the Bohr model of the atom (see Quantum Mechanics) or the Pauling models of chemical bonds (see Chemistry, Philosophy of), are both mathematical and accompanied by a visual picture that help their understanding and use (see also Visual Representation).

At present, no unified treatment of the various types and functions of scientific models seems possible. At the very least, the rich tapestry of models in science cannot entirely be accommodated to the role assigned to them by the semantic interpretation of theories or any other account that views models as having only explanatory and predictive functions. The ways in which models also function as tools of exploration and discovery remain a topic of active philosophical interest (Wimsatt 1987).

Realism

A central concern of philosophers of science has long been whether scientists have good reason to believe that the entities (in particular the unobservable entities) referred to by their theories exist and that what their theories say about these entities is true or approximately true (see Realism). In order for theories to refer to or be true about unobservable entities, they must actually be claims about these entities. This was denied by many logical empiricists, building on concerns raised by Mach, Duhem, and Poincaré (see Mach, Ernest; Duhem, Paul; Poincaré, Henri). As noted above, the logical empiricists were interested in providing cognitive significance to theoretical terms by attempting to reduce theoretical claims to claims in the observation language. Even when this proved impossible, many nevertheless argued that theoretical terms are simply convenient instruments for making predictions about observable entities, rather than claims about unobservable entities (see Instrumentalism).

Because of the difficulties with theory-observation distinction discussed above (see Observation; Theories), this view fell out of favor and was replaced with a milder version of anti-realism. Van Fraassen (1980), for example, argues that while claims about unobservables might have a truth-value, scientists only have good reason to believe in their empirical adequacy, not their truth. Such a view might broadly be understood as instrumentalist in the sense that the truth of theories does not underwrite the functions they serve. There are two main arguments provided in support this version of anti-realism. First, given the problem of underdetermination raised by Duhem and Quine, there will always be more than one rival hypothesis compatible with any body of evidence (see Duhem Thesis; Underdetermination of Theories). Therefore, since these hypotheses are incompatible, the evidence cannot provide adequate reason to believe that one or the other theory is true. Second, some have argued that history provides evidence against believing in the truth of scientific theories. Given the large number of theories once thought true in the past that have since been rejected as false, history provides inductive evidence that science’s current theories are likely to be false as well (see Laudan 1981).

There have been a number of responses to these arguments, including attempts to show that the problem of underdetermination can be solved, that anti-realism depends on a distinction between observable and unobservable entities that cannot be sustained, and that the realist need only claim that theories are approximately true or are getting closer to the truth (see Verisimilitude). In addition, arguments have been provided in support of realism about theories, the most influential of which is Putnam’s miracle argument (see Putnam, Hilary). There are various versions of this argument, but the central premise is that science is successful (what this success amounts to varies). The contention is that the only way this success can be explained is if scientific theories are approximately true (see Abduction); otherwise the success of science would be a miracle.

This argument has been criticized in three central ways. First, Fine (1986) criticizes the miracle argument for being viciously circular. Second, some have argued that science is in fact not very successful, for reasons outlined above. Third, it is argued that the success of science does not depend on its truth, or perhaps does not even require an explanation. Van Fraassen (1980), for example, has argued that it is not surprising that scientific theories are predictively successful, since they are chosen for their predictive success. Therefore, the success of theories can be explained without supposing their truth. Others have responded that this would not, however, explain the predictive success of theories in novel situations (e.g., Leplin 1997).

Due to these problems, other forms of realism have been defended. Hacking (1983), for example, defends entity realism. He argues that, while
scientists do not have good reason to believe their theories are true, they do have good reason to believe that the entities referred to in the theories exist, since scientists are able to manipulate the entities. Others have attempted to defend a more radical form of anti-realism, according to which the entities scientists talk about and the theories they invent to discuss them are merely social constructs (see Social Constructionism).

**Explanation**

In an attempt to avoid metaphysically and epistemically suspect notions such as causation (see Causality), Hempel and Oppenheim (1948) developed a covering law model of explanation: the deductive-nomological (D-N) account (see Explanation; Hempel, Carl). Rather than relying on causes, they argued that scientific explanations cite the law or laws that cover the phenomena to be explained. According to the D-N model, explanations are deductive arguments, where the conclusion is a statement expressing what is to be explained (the *explanandum*), and the premises (the *explanans*) include at least one law-statement. Often statements about particular antecedent conditions from which the *explanandum* can be derived. Initially developed only to cover explanations of particular facts, the D-N model was expanded to include explanations of laws, such as the explanation of Kepler’s laws by deriving them from Newton’s laws of motion (along with particular facts about the planets). To account for explanations of particular events and laws governed by *statistical* laws, the inductive-statistical (I-S) and deductive-statistical (D-S) models were developed (Hempel 1965). According to the D-S model, statistical laws are explained by deductively deriving them from other statistical laws. However, statements describing particular facts cannot be deduced from statistical laws. Instead, according to the I-S model, the explanans containing statistical laws must confer a high inductive probability to the particular event to be explained. In this way, the covering law model of explanation was able to link explanation with predictability (see Prediction) and also make clear why the reduction of, say, Kepler’s laws to Newton’s laws of motion could be explanatory (see Reductionism).

In the ensuing years, these accounts ran into a number of problems. The covering law model seemed unable to account for cases where scientists and non-scientists appear to be giving perfectly good explanations without citing laws (see Biology, Philosophy of; Function; Mechanism; Social Sciences, Philosophy of). Several counterexamples were developed against the D-N model, including the purported explanation of events by citing irrelevant factors, such as the explanation of Joe’s failure to get pregnant by citing the fact that he took birth-control pills, and the explanation of causes by citing their effects, such as the explanation of the height of a flagpole by citing the length of its shadow. Deductive relations, unlike explanatory relations, can include irrelevant factors and need not respect temporal asymmetries. The I-S model also encountered difficulties. According to the I-S model, improbable events cannot be explained, which runs counter to many philosophers’ intuitions about such cases as the explanation of paresis by citing the fact that a person had untreated syphilis. Moreover, developing an account of inductive probability proved difficult (see Inductive Logic; Probability). Attempts to provide an adequate account of laws within an empiricist framework also encountered problems. According to Hempel and Oppenheim, laws are expressed by universal generalizations of unlimited scope, with purely qualitative predicates, and they do not refer to particular entities. The problem is that there are accidental generalizations, such as ‘All pieces of gold have a mass of less than 10,000 kg,’ that satisfy these conditions. Laws appear to involve the modal features that Hume and the logical empiricists were intent on avoiding; unlike accidental generalization, laws seem to involve some sort of natural necessity. The difficulty is to develop an account of laws that makes sense of this necessity in a way that does not make knowledge of laws problematic (see Laws of Nature).

In response to these problems, some have attempted to rescue the covering-law model by supplementing it with additional conditions, as in unificationist accounts of explanation. According to these accounts, whether an argument is explanatory depends not just on the argument itself, but on how it fits into a unified theory (see Unity and Disunity of Science). Scientists explain by reducing the number of brute facts (Friedman 1974) or argument patterns (Kitcher 1989) needed to derive the largest number of consequences. Others have developed alternatives to the covering law model. Van Fraassen (1980) has defended a pragmatic account of explanation, according to which what counts as a good explanation depends on context. Others have developed various causal accounts of explanation. Salmon (1971) and others have argued that explanatory and causal relations can be understood in terms of statistical relevance; scientists
explain by showing that the *explanans* (a causal factor) is statistically relevant for the event to be explained. Salmon (1984) eventually rejected this view in favor of a causal mechanical model, according to which explanations appeal to the mechanisms of causal propagation and causal interactions (see Mechanism). Along with the development of various causal accounts of explanation have come numerous accounts of causation, as well as attempts to develop a better epistemology for causal claims through, for example, causal modeling (see Causality).

**Prediction**

Traditionally, prediction has been regarded as being as central to science as explanation (see Prediction). At the formal level, the received view does not distinguish between explanation and prediction. For instance, in the D-N model, the conclusion derived from the laws and other assumptions can be regarded as predictions in the same way that they can be regarded as explanations. While prediction is generally taken to refer to the future—one predicts future events—philosophically, the category includes retrodiction, or prediction of past events, for instance the past positions of planets from Newton’s laws and their present positions and momenta. (On some accounts of hypothesis confirmation, retrodiction is even more important than forward prediction—see Bayesianism.)

The D-N model assumes that the laws in question are deterministic (see Determinism). Statistical explanations are also predictive, but the predictions are weaker: they hold probabilistically and can only be confirmed by observing an ensemble of events rather than individual events (see Confirmation Theory). Interest in statistical explanation and prediction initially arose in the social sciences in the nineteenth century (Stigler 1986; see also Social Sciences, Philosophy of the). In this case, as well as in the case of prediction in classical statistical physics, the inability to predict with certainty arises because of ignorance of the details of the system and computational limitations. A different type of limitation of prediction is seen when predictions must be made about finite samples drawn from an ensemble, for instance, biological populations (see Evolution; Population Genetics). Finally, if the laws are themselves indeterministic, as in the case of quantum mechanics, prediction can only be statistical (see Quantum Mechanics). The last case has generated the most philosophical interest because, until the advent of quantum mechanics, the failure to predict exactly was taken to reflect epistemological limitations rather than an ontological feature of the world. That the models of statistical explanation discussed earlier do not distinguish between these various cases suggests that there remains much philosophical work to be done. Meanwhile, the failure of determinism in quantum mechanics has led to much re-examination of the concept of causality in attempts to retain the causal nature of physical laws even in a probabilistic context (see Causality).

Prediction, although not determinism, has also been recently challenged by the discovery that there exist many systems that display sensitivity to initial conditions, the so-called chaotic systems. Determinism has usually been interpreted as an ontological thesis: for deterministic systems, if two systems are identical at one instant of time, they remain so at every other instant (Earman 1986; see Determinism). However, satisfying this criterion does not ensure that the available—and, in some cases, all obtainable—knowledge of the system allows prediction of the future. Some physical theories may prevent the collection of the required information for prediction (Geroch 1977; see also Space-Time). Even if the information can be collected, pragmatic limitations become relevant. The precision of any information is typically limited by measurement methods (including the instruments). If the dynamical behavior of systems is exceedingly sensitive to the initial conditions, small uncertainties in the initial data may lead to large changes in predicted behavior—chaotic systems exemplify this problem (see Prediction).

**Confirmation**

Hume’s problem—how experience generates rational confidence in a theory—has been central to philosophy of science in the twentieth century and continues to be an important motivation for contemporary research (see Induction, Problem of). Many of the logical empiricists initially doubted that there is a logical canon of confirmation. Breaking with earlier logical traditions, for many of which inductive logic was of central importance, these logical empiricists largely regarded confirmation as a pragmatic issue not subject to useful theoretical analyses. That assessment changed in the 1940s with the work of Carnap, Hempel, and Reichenbach, besides Popper (see Carnap, Rudolf; Hempel, Carl Gustav; Popper, Karl Raimund; Reichenbach, Hans). Carnap, in particular, began
an ambitious project of the construction of a logic of confirmation, which he took to be part of semantics, in the process reviving Keynes’ logical interpretation of probability. Early versions of this project were distant from the practice of science, being restricted to formal languages of excessively simplified structures incapable of expressing most scientific claims. Later versions came closer to scientific practice, but only to a limited extent (see Carnap, Rudolf). Whether or not the project has any hope remains controversial among philosophers. Although the relevant entries in this Encyclopedia record some progress, there is as yet no quantitative philosophical theory of confirmation (see Confirmation Theory; Inductive Logic; Probability).

Meanwhile, within the sciences, the problem of confirmation was studied as that of statistical inference, bringing standard statistical methods to bear on the problem of deciding how well a hypothesis is supported by the data. Most of these methods were only invented during the first half of the twentieth century. There are two approaches to statistics, so-called orthodox statistics (sometimes called “frequentist” statistics) and Bayesian statistics (which interprets some probabilities as degrees of belief). The former includes two approaches to inference, one involving confidence intervals and largely due to Neyman and E. S. Pearson and the other due to Fisher. These have received some attention from philosophers but, perhaps, not as much as they deserve (Hacking 1965; see Statistics, Philosophy of’). In sharp contrast, Bayesian inference has been at the center of philosophical attention since the middle of the twentieth century. Interesting work points to common ground between traditional confirmation theory and Bayesian methodology. Meanwhile, within the sciences, newer computational methods have made Bayesian statistics increasingly popular (see Statistics, Philosophy of’), for instance, in the computation of phylogenies in evolutionary biology (see Evolution). Bayesian inference methods also have the advantage of merging seamlessly with contemporary decision theory (see Decision Theory), even though most of the methods within decision theory were invented in an orthodox context.

Philosophically, the differences between orthodox and Bayesian methods remain sharply defined. Orthodox methods do not permit the assignment of a probability to a hypothesis, which, from the perspective of most Bayesians, makes them epistemologically impotent. (Bayesians also usually argue that orthodox inferential recipes are ad hoc—see Bayesianism.) Meanwhile Bayesian methods require an assignment of prior probabilities to hypotheses before the collection of data; for the orthodox such assignments are arbitrary. However, in the special sciences, the trend seems to be one of eclecticism, when orthodox and Bayesian methods are both used with little concern for whether consistency is lost in the process. This situation calls for much more philosophical analysis.

**Experimentation**

The logical empiricists’ focus on the formal relations between theory and evidence resulted in Anglo-American philosophers neglecting the role of experimentation in science. Experimentation did receive some philosophical treatment in the late nineteenth and early twentieth centuries, in particular by Mill, Mach, and Bernard (see Mach, Ernest). In twentieth-century Germany, two traditions developed around the work of Dingler and Habermas. It is only in the past three decades that experimentation has received more attention from Anglo-American philosophers, historians, and sociologists. Since then, there have been a number of careful analyses of the use of experiments by practicing scientists, with historians and sociologists focusing largely on the social and material context of experiments and philosophers focusing on their epistemic utility.

From a philosophical perspective, the neglect of experimentation was particularly problematic, since experimentation seems to affect the very evidential relations empiricists were interested in formalizing. Whether experimental results are good evidence for or against a hypothesis depends on how the results are produced—whether the data are reliably produced or a mere artifact of the experimental procedure. Moreover, this reliability often comes in degrees, thereby affecting the degree to which the data confirms or disconfirms a hypothesis. In addition, how data are produced affects what sorts of inferences can be drawn from the data and how these inferences might be drawn. As Mill argues, “Observations, in short, without experiment . . . can ascertain sequences and coexistences, but cannot prove causation” (1874, 386). How experimental results are obtained can also affect whether replication is necessary and how statistical methods are used. In some cases, statistics is used to analyze the data, while in others, it is involved in the very production of the data itself (see Experimentation; Statistics, Philosophy of’).

One of the central issues in the philosophy of experimentation is what experiments are. Experiments
are often distinguished from observations in that
the former involve active intervention in the world,
whereas the latter are thought to be passive. How-
ever, it is unclear what counts as an intervention.
For example, are the use of sampling methods or
microscopes interventions? There are also ques-
tions about whether thought experiments or com-
puter simulations are “real” experiments or if they
merely function as arguments. Moreover, it is not
always clear how to individuate experiments—
whether it is possible, especially with the increasing
use of computers as integral parts of the experimen-
tal set-up, to disambiguate the experiment from the
analysis of the data.

Another fundamental issue is whether and what
estptic roles experiments can play (Rheinberger
1997). They are purportedly used in the testing of
tories, in garnering evidence for the existence of
entities referred to by our theories (see Realism), in
the creation (and thereby discovery) of new phe-
nomena, in the articulation of theories, in the
development of new theories, in allowing scientists
to “observe” phenomena otherwise unobservable
(see Observation), and in the development and
refinement of technologies.

Whether experiments can reliably serve these
estptic functions has been called into question in
a number of ways. First, sociologists and histor-
ians have argued that social factors affect or even
determine whether an experiment “confirms” or
“disconfirms” a theory (see Social Constructionism).
It is also argued that experiments are theory-laden,
since experiments require interpretation and these
interpretations rely on theories (Duhem 1954).
Whether this is a problem depends in part on
what use is made of the experiment and what
sorts of theories are needed—the theory being test-
ed, theories of the phenomena being studied but
not being tested, or theories about the experimental
apparatus being used. As Hacking (1983) and Galli-
son (1987) both argue, experiments and experimental
traditions can have a life of their own independent of higher-level theories.

The theory-ladenness of experimentation also
raises questions about whether experiments can be
used to test hypotheses in any straightforward
way no matter which level of theory is used, since
predictions about experimental results rely on aux-
illary hypotheses that might be called into question
(see Duhem Thesis). Experiments are also purport-
ed to be “practice-laden,” relying on tacit know-
ledge that cannot be fully articulated (Collins 1985;
see also Polanyi 1958). According to Collins, this
leads to problems with replication. The reliability
of experiments is often judged by the ability of
scientists to replicate their results. However, what
counts as replication of the “same” experiment is
often at issue in scientific disputes. Since, accord-
ing to Collins, tacit knowledge (which cannot be made
explicit) is involved in the replication of experi-
ments and even in judgments about what constitu-
tes the “same” experiment, adjudicating these
disputes on rational grounds is problematic. Col-
kins, in addition, questions whether there can be
independent grounds for judging whether an exper-
iment is reliable, which he calls “the experimenters’
regress.” Whether an experimental procedure is reli-
able depends on whether it consistently yields cor-
rect results, but what counts as a correct result
depends on what experimental procedures are
deeded reliable, and so on (Collins 1985; for a
reply, see Franklin 1994). Experiments also typically
involve manipulation of the world, often creating
things that are not naturally occurring, which has
led some to question whether experiments represent
the world as it naturally is. At one extreme are those
who argue that experimentation actually constructs
entities and facts (Latour and Woolgar 1979;
Pickering 1984; Rheinberger 1997; see also Social
Constructionism). Others argue that experiments
can produce artifacts, but that these can be reliably
distinguished from valid results (Franklin 1986). A
milder version of this worry is whether laboratory
settings can accurately reproduce the complexities of
the natural world, which is exemplified in debates
between field and experimental biologists. The effect
of interventions on experimental outcomes is even
more problematic in quantum physics (see Quantum
Measurement Problem).

Scientific Change

Scientific change occurs in many forms. There are
changes in theory, technology, methodology, data,
institutional and social structures, and so on. The
focus in the philosophy of science has largely been
on theory change and whether such changes are
progressive (see Scientific Change; Scientific Prog-
ress). The primary concern has also been with how
scientific theories are justified and/or become ac-
cepted in the scientific community, rather than how
they are discovered or introduced into the commu-
nity in the first place. Over the years, there have
been various notions of progress correlated with the
different goals scientific theories are purported to
have: truth, systematization, explanation, empirical
adequacy, problem solving capacity, and so on.
(Notice that if the focus were on, say, technological
or institutional changes, the goals attended to might
be very different; for example, does the technology have greater practical utility or is the institutional change just?)

Traditionally, scientific change has been thought of as governed by rational procedures that incrementally help science achieve its goals. For the logical empiricists, the aim of scientific theories was to systematize knowledge in a way that yields true predictions in the observational language (see Theories). As such, science progresses through the collection of additional confirming data, through the elimination of error, and through unification, typically by reducing one theory to another of greater scope. To make sense of these sorts of changes, the logical empiricists developed accounts of reduction, explanation, and inductive logic or confirmation theory (see Confirmation Theory; Explanation; Inductive Logic; Reductionism; Unity and Disunity of Science). Others, such as Popper, offered a different account of theory change. Popper defended an eliminativist account much like Mill’s, whereby science attempts to eliminate or falsify theories. Only those theories that pass severe tests ought to be provisionally accepted (see Corroboration). This was also one of the earliest versions of evolutionary epistemology (see Evolutionary Epistemology; Popper, Karl Raimund).

As discussed in the previous sections, these accounts ran into difficulties: Quine extended Duhem’s concerns about falsification, criticized the analytic/synthetic distinction, and raised questions about the determinacy of translation (see Duhem Thesis; Quine, Willard Van; Underdetermination); Popper and Hanson argued that observations are theory-laden (see Hanson, Norwood Russell; Observation; Popper, Karl Raimund); there were problems with Carnap’s inductive logic; and so on. Partly influenced by these difficulties and partly motivated by a concern that philosopher’s theories about science actually fit the practices of science, Kuhn’s The Structure of Scientific Revolutions (1962) challenged the way philosophers, historians, sociologists, and scientists thought about scientific change (see Kuhn, Thomas). He argued that scientific change is not in general cumulative and progressive, but develops through a series of distinct stages: immature science (when there is no generally accepted paradigm), normal science (when there is an agreed upon paradigm), and revolutionary science (when there is a shift between paradigms). Kuhn’s notion of paradigms also expanded the focus of scientific change beyond theories, since paradigms consisted, not just of theories, but of any exemplary bit of science that guides research. While the development of normal science might in some sense be incremental, Kuhn argued that the choice between paradigms during a revolution involves something like a Gestalt shift. There are no independent methods and standards, since these are paradigm-laden; there is no independent data, since observations are paradigm-laden; and the paradigms may not even be commensurable (see Incommensurability). Consequently, paradigm shifts seemed to occur in an irrational manner.

The responses to Kuhn’s influential work took two very different paths. On the one hand, strongly influenced by Kuhn, members of the Strong Programme argued that scientific change ought to be explained sociologically—that the same social causes explain both “good” and “bad” science. Others (e.g., Latour and Woolgar 1979) went further, arguing that scientists in some sense construct facts (see Social Constructionism). Focus on the social aspects of scientific research also led to developments in feminist philosophy of science, both in the close analysis of the gender and racial biases of particular sciences and in the development of more abstract feminist theories about science (see Feminist Philosophy of Science).

The other, a very different sort of response, involved a defense of the rationality and progress of science. There were attempts to show that competing scientific theories and paradigms are not incommensurable in the sense of being untranslatable. Davidson (1974) argues the very idea of a radically different, incommensurable paradigm does not make sense; others (e.g., Scheffler 1967) argued that sameness of reference is sufficient to ensure translatability, which was later buttressed by referential accounts of meaning (see Incommensurability). The rationality of scientific change was also defended on other grounds. Lakatos developed Popper’s ideas in light of Kuhn into his methodology of scientific research programs (see Lakatos, Imre; Research Programmes); and Laudan (1977) argued that progress can be made sense of in terms of problem solving capacity. Another approach to showing that scientific change is progressive can be found in realism. Rather than arguing that each change involves a rational choice, defenses of realism can be seen as attempts to establish that science is approaching its goal of getting closer to the truth (see Realism). Of course, anti-realists might also argue that science is progressing, not toward truth, but toward greater empirical adequacy.

More recently, there have been attempts to develop formal methods of theory choice beyond confirmation theory and inductive logic (see Bayesianism;
Statistics, Philosophy of. There have also been attempts to model discovery computationally, which had been thought not to be rule governed or formalizable. Some of these try to model the way humans discover; others were developed in order to make discoveries (e.g., data mining), whether or not humans actually reason in this way. As a normative enterprise, such modeling can also be used as a defense of the rationality of scientific discovery and, therefore, scientific change (see Scientific Change).

Perhaps the longest-lasting influence in the philosophy of science of Kuhn’s influential work has been to encourage philosophers to look more closely at the actual practices of the various sciences. This has resulted in a proliferation of philosophies of the special sciences.

**Foundations of the Special Sciences**

The logical empiricists believed in the unity of science (see Unity of Science Movement). However, the theme was interpreted in multiple ways. At one extreme were views according to which unification was to be achieved through hierarchical reduction (see Reductionism) of sociology to individual psychology (see Methodological Individualism), psychology to biology (see Psychology, Philosophy of), biology to physics and chemistry (see Biology, Philosophy of), and chemistry to physics (see, Chemistry, Philosophy of); for an influential defense of this view, see Oppenhiem and Putnam (1958). At the other extreme were those who believed that unification required no more than to be able to talk of the subjects of science in an interpersonal (that is, non-solipsistic) language—this was Carnap’s (1963) final version of physicalism. Somewhere in between were stronger versions of physicalism, which, for most logical empiricists and almost all philosophers of science since then, provides some vision of the unity of science (see Physicalism).

Perhaps with the exception of the most extreme reductionist vision of the unity of science, all other views leave open the possibility of exploring the foundations and interpretations of the special sciences individually. During the first few decades of the twentieth century, most philosophical attention to the special sciences was limited to physics; subsequently, psychology, biology, and the social sciences have also been systematically explored by philosophers. In many of these sciences, most notably biology and cognitive science, philosophical analyses have played a demonstrable role in the further development of scientific work (see Biology, Philosophy of; Cognitive Science; Intentionality).

**Physical Sciences**

The first three decades of the twentieth century saw the replacement of classical physics by relativity theory and quantum mechanics, both of which abandoned cherished classical metaphysical principals (see Quantum Mechanics; Space-Time). It is therefore not surprising that many philosophers interested in “scientific philosophy” (see Logical Empiricism) did significant work in this field. In particular, Popper and Reichenbach made important contributions to the interpretation of quantum mechanics; Reichenbach and, to a lesser extent, Carnap also contributed to the philosophy of space-time (see Carnap, Rudolf; Popper, Karl Raimund; Reichenbach, Hans). In both quantum mechanics and relativity, philosophers have paid considerable attention to issues connected with causality and determinism, which became problematic as the classical world-view collapsed (see Causality; Determinism). Arguably, Reichenbach’s work on space-time, especially his arguments for the conventionality of the metric, set the framework for work in the philosophy of space-time until the last few decades (see Conventionalism). Reichenbach also produced important work on the direction of time.

Several philosophers contributed to the clarification of the quantum measurement problem (see Quantum Measurement Problem), the concept of locality in quantum mechanics (see Locality), and the nature and role of quantum logic (see Putnam, Hilary; Quantum Logic). Meanwhile, many physicists, including Bohr, Einstein, Heisenberg, and Schrödinger, also produced seminal philosophical work on the foundations of physics (see also Bridgman, Percy Williams; Duhem Thesis). The only consensus that has emerged from all this work is that, whereas the foundations of relativity theory (both special and general) are relatively clear, even after eighty years, quantum mechanics continues to be poorly understood, especially at the macroscopic level (see Complementarity).

Perhaps because of the tradition of interest in quantum mechanics, philosophers of physics, starting mainly in the 1980s, also began to explore the conceptual structure of quantum field theory and particle physics (see Particle Physics; Quantum Field Theory). However, one unfortunate effect of the early focus on quantum mechanics and
relativity is that other areas of physics that also
deserve philosophical scrutiny did not receive ade-
quate attention, as Shimony (1987) and others have
emphasized. (See the list of questions in the entry,
Physical Sciences, Philosophy of.) Only in recent
years have philosophers begun to pay attention to
questions such as reductionism and irreversi-
Biology emerged as a recognizable entity
within the philosophy of science. The first question
that occupied philosophers was whether mole-
cular biology was reducing classical biology (see
Molecular Biology; Reductionism). Initial enthusi-
asm for reductionism gave place to a skeptical
consensus as philosophers began to question both
the standard theory-based account of reductionism
(due to Nagel 1961; see Nagel, Ernest) and whether
molecular biology had laws or theories at all
(Sarkar 1998). In the 1970s and 1980s, attention
shifted almost entirely to evolutionary theory (see
Evolution), to the definitions of “fitness” (see
Fitness) and “function” (see Function), the nature of
individuals and species (see Individual; Species),
the significance of adaptation and selection (see Ad-
paption and Adaptationism; Population Genetics),
and, especially, the units and levels of selection. Phi-
osophical work has contributed significantly to sci-
entific discussions of problems connected to units of
selection, although no consensus has been reached
(see Altruism; Natural Selection). Besides evolution,
there was some philosophical work in genetics (see
Genetics; Heredity and Heritability).
As in the case of the philosophy of physics,
the last two decades have seen a broadening of
interest within the philosophy of biology. Some
of the new work has been driven by the realization
that molecular biology, which has become most of
contemporary biology, is not simply the study of
properties of matter at lower levels of organization,
but has a conceptual framework of its own. This
framework has largely been based on a concept
of information that philosophers have found high-
ly problematic (see Biological Information). Form-
ulating an adequate concept of biological
information—if there is one—remains a task to
which philosophers may have much to contribute
(see Molecular Biology).
There has also been some attention paid to bio-
diversity (see Conservation Biology), ecology (see
Ecology), immunology (see Immunology), and
developmental biology, especially in the molecular
era (see Molecular Biology). Neurobiology has
sometimes been approached from the perspective
of the philosophy of biology, although philosophi-
cal work in that area typically has more continuity
with psychology (see “Psychology” below and
Neurobiology). Philosophers have also argued on
both sides of attempts to use biology to establish
naturalism in other philosophical areas, especially
epistemology and ethics—this remains one of the
most contested areas within the philosophy of bio-
logy (see Evolutionary Epistemology; Evolutionary
Psychology). Some philosophers of science have

Finally, beyond physics, some philosophical at-
tention is now being directed at chemistry (see
Chemistry, Philosophy of) and, so far to a lesser
extent, astronomy (see Astronomy, Philosophy of).
As in the case of macroscopic physics, the
question of the reduction of chemistry to physics
has turned out to be unexpectedly complicated with
approximations and heuristics playing roles that
make orthodox philosophers uncomfortable (see
Approximation). It is likely that the future will
see even more work on these neglected fields and
further broadening of philosophical interest in the
physical sciences.

Biology

Professional philosophers paid very little attention
to biology during the first few decades of the twen-
tieth century, even though the advent of genetics
(both population genetics and what came to be
called classical genetics [see Genetics]) was trans-
forming biology in ways as profound as what was
happening in physics. Professional biologists—
including Driesch, J. B. S. Haldane, J. S. Haldane,
and Hogben—wrote philosophical works of some
importance. However, the only philosopher who
tried to interpret developments in biology during
this period was Woodger (1929, 1937), better
known among philosophers as the translator of
Tarski’s papers into English. Philosophers paid so
little attention to biology that not only the evolu-
tionary “synthesis” (see Evolution), but even the
formulation of the double helix model for DNA
(see Reduction), went unnoticed by philosophers of
those generations (Sarkar 2005).

All that changed in the 1960s, when the philo-
sophy of biology emerged as a recognizable entity
also interpreted the philosophy of medicine as belonging within the conceptual terrain of the philosophy of biology (Schaffner 1993). Finally, work in the philosophy of biology has also led to challenges to many of the traditional epistemological and metaphysical assumptions about science, about the nature of explanations, laws, theories, and so on (see Biology, Philosophy of; Mechanism).

Psychology

Philosophy and psychology have an intimate historical connection, becoming distinct disciplines only in the late nineteenth and early twentieth centuries. Even since then, many of the topics covered by psychology have remained of interest to philosophers of mind and language, although the route taken to address these questions might be very different. However, while philosophers of science did address concerns about the human sciences more generally (see “Social Sciences” below), it is only in the last twenty years or so that philosophy of psychology has developed as a distinct area of philosophy of science.

The intimate connection between philosophy and psychology can be seen throughout the history of psychology and the cognitive sciences more broadly. In an attempt to make psychology scientific, Watson (1913), a philosopher, founded behaviorism, which dominated the field of psychology for the first half of the twentieth century (see Behaviorism). This view fit well with empiricist attempts to reduce theoretical claims to those in the observational language by providing operational definitions (see Hempel 1949; see also Bridgeman, Percy Williams; Theories; Verificationism). However, the combined weight of objections from philosophers, linguists, and psychologists led to the demise of behaviorism. These criticisms, along with developments in mathematical computation (see Artificial Intelligence; Cognitive Science; connectionists argue that it is more similar to parallel distributed processing (see Connectionism); and more recently other accounts have been proposed, such as dynamical and embodied approaches to cognition. Also at issue is whether the cognitive structures in the mind/brain are modular (see Evolutionary Psychology), whether cognition is rule-governed, and whether some of the rules are innate (see Chomsky, Noam; Innate/Acquired Distinction).

(i) The Content of Representations. One central question is what fixes the content of representations—is content determined by internal features of the agent (e.g., conceptual role semantics), features of the external physical environment (e.g., causal and teleological theories), or features of the external social environment? There are also debates about whether the representations are propositional in form, whether they require language (see Linguistics, Philosophy of), whether some are innate (see Empiricism; Innate/acquired Distinction), and whether representations are local or distributed (see Connectionism).

(ii) The Structure of Thought. The nature of cognition has also been a topic of dispute. Some argue that human cognition takes the form of classical computation (see Artificial Intelligence; Cognitive Science); connectionists argue that it is more similar to parallel distributed processing (see Connectionism); and more recently other accounts have been proposed, such as dynamical and embodied approaches to cognition. Also at issue is whether the cognitive structures in the mind/brain are modular (see Evolutionary Psychology), whether cognition is rule-governed, and whether some of the rules are innate (see Chomsky, Noam; Innate/Acquired Distinction).

(iii) Theories and Laws. Questions have been raised about the nature of theories in the cognitive sciences (see Neurobiology), about whether there are psychological or psychophysical laws (see Laws of Nature), and about how the theories and laws in different areas of the cognitive sciences relate, such as whether psychology is reducible to neurobiology (see Neurobiology; Physicalism; Reductionism; Supervenience). In addition, there is disagreement about how to interpret theories in the cognitive sciences—whether to interpret them realistically, as an attempt to represent how the mind/brain actually works, or merely instrumentally, as a means of saving the phenomena or making predictions (see Instrumentalism; Realism). Moreover, the problems of reflexivity and the intentional circle discussed below, along with difficulties
peculiar to the various areas of the cognitive sciences, raise questions about the testability of psychological theories (see Neurobiology; Psychology, Philosophy of).

(iv) **Consciousness.** There has been a resurgence of interest in consciousness (see Consciousness; Searle, John). There have been attempts to clarify what “consciousness” involves in its various senses, as well as debates about how to explain consciousness. To this end, a number of theories of consciousness have been proposed, including higher-order theories, neurological theories, representational theories, and various non-physical theories.

**Social Sciences**

Philosophical interest in the foundations of the social sciences has a long history, dating back at least to Mill’s influential work on the social sciences. Some foundational issues have also been systematically discussed by social scientists themselves, such as Durkheim (1895/1966) and Weber (1903/1949). Around the middle of the twentieth century, the social sciences again received serious philosophical attention. The focus was largely on their being human sciences and the philosophical issues this raised. More recently, philosophers have directed their attention to the different social sciences in their own right, especially economics (see Economics, Philosophy of).

A central focus of discussion is whether the social sciences are fundamentally different from the natural sciences. Logical empiricists attempted to incorporate the social sciences into their models for the natural sciences (see Unity of Science Movement). Others have argued that the social sciences are unique. This has framed many of the debates within the philosophy of the social sciences, a number of which are briefly discussed in what follows (see Social Sciences, The Philosophy of):

(i) **Are There Social Science Laws?** Laws played important roles in empiricist accounts of explanation, theories, confirmation, and prediction, but it is unclear whether there are laws of the social sciences (see Laws of Nature). Social phenomena are complex, involve reference to social kinds, and require idealizations. As a result, many argue that generalizations of the social sciences, if they are laws at all, require ineliminable ceteris paribus clauses. Others argue that the social sciences ought not even attempt to create generalizations or grand theories, as social phenomena are essentially historical and local.

(ii) **Do Social Scientific Theories Yield Testable Predictions?** Because of the complexity of social systems, social scientific theories require idealizations. Given the nature of these idealizations, deriving empirical predictions from social scientific theories is difficult at best (see Prediction). As a result, many argue that social scientific theories are not testable. This is exacerbated by the reflexive nature of social science theories: the very act of theorizing can change the behavior one is theorizing about. Moreover, if human action is explained by agents’ desires and beliefs, social scientists seem to be caught in an intentional circle, making it difficult to derive any testable claims (see Rosenberg 1988).

(iii) **Is the Methodology of the Social Sciences Distinct?** Given that social sciences involve humans and human behavior on a large scale, experimentation has not played a significant role in the social sciences (see Experimentation). There are also many who question whether the social sciences can be naturalized. Some argue that understanding social action is essentially a hermeneutic enterprise, distinctly different from the natural sciences.

(iv) **What Are the Ontological Commitments of Scientific Theories?** Beginning with Mill and, subsequently, Durkheim and Weber, there have been debates as to whether social scientific theories are reducible to theories about individual behavior (see Methodological Individualism). Moreover, after Nagel’s influential account of intertheoretic reduction, it has been argued that social phenomena are multiply realizable, and therefore, social science theories are not reducible to lower-level theories (see Emergence; Reductionism; Supervenience). Additionally, given that social scientific theories involve idealizations, there are questions about whether these theories ought to be interpreted realistically or instrumentally (see Instrumentalism; Realism).

(v) **What Is the Nature of Social Scientific Explanations?** Some, such as Hempel (1962), have argued that social scientific explanations are no different than in the physical sciences. Others, however, have questioned this. If there are no social scientific laws, then social scientific explanation cannot be captured by the covering law
model (see Explanation). Social sciences also often rely on functional explanations, which, while similar to biology, seem to be different from explanations in physics (see Function). Others, following Winch (1958), have argued that social sciences explain action, not behavior, which requires understanding the meaning of the action (not its causes), and therefore must include the actors’ intentions and social norms. Moreover, some have argued that actions are governed by reasons, and are therefore not susceptible to causal explanation, a view that was later convincingly refuted by Davidson (1963). An alternative account of how beliefs and desires can explain actions has been formalized in rational choice theory (see Decision Theory), although there are questions about whether such explanations capture how people actually behave, rather than how they ought to behave.

(vi) What Is the Relationship Between Social Science and Social Values? There has also been concern with the connection between social values and the social sciences. Taylor (1971), for example, argues that social theory is inherently value-laden, and Habermas (1971) argues that social theory ought to engage in social criticism.

Concluding Remarks

Philosophy of science remains a vibrant sub-discipline within philosophy today. As this introduction has documented, many of the traditional questions in epistemology and metaphysics have been brought into sharper profile by a focus on scientific knowledge. Moreover, philosophical engagement with the special sciences has occasionally contributed to the development of those sciences and, as philosophers become more immersed in the practice of science, the number and level of such contributions can be expected to increase. The trend that philosophers of science engage all of the special sciences—not just physics—will also help produce a more complete picture of the growth of science, if not all knowledge, in the future.

With few exceptions (e.g., Demarcation, Problem of and Feminist Philosophy of Science) the entries in the Encyclopedia are not concerned with the social role of science. But, as science and technology continue to play dominant roles in shaping human and other life in the near future, philosophers may also contribute to understanding the role of science in society. Moreover, in some areas, such as the environmental sciences and evolutionary biology, science is increasingly under ill-motivated attacks in some societies, such as the United States. This situation puts philosophers of science, because of their professional expertise, under an obligation to explain science to society, and, where ethically and politically appropriate, to defend the scientific enterprise. How such defenses should be organized without invoking a suspect criterion of demarcation between science and non-science remains a task of critical social relevance. The Encyclopedia should encourage and help such efforts.

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