One cannot approach the question of how historiography relates to psychology and social science without assuming that historiography is related to the study of human minds, society, human action and, more broadly, social events. This does not restrict unduly our topic, as most human actions of historical interest have social implications.

How can and do historians explain social actions and events if they are neither psychologists nor social scientists? We proceed on the assumption, for which there is ample evidence, that historians do, at least sometimes, engage in explanatory tasks, citing causes of significant events. They are concerned with answering questions such as: why did the English Revolution occur? What brought about the First World War? What caused the potato famine in Ireland?

Must such explanations be derived from, or depend on, the theories and laws of the disciplines in the social sciences and psychology? The notion of “deriving” or “reduction” involved here is ambiguous. It sometimes alludes to ontological reduction, where what is at stake is the commitment of some theory or discipline to entities of a distinctive kind. Ontological reductionists about the mind are typically committed to the view that the mind is a physical entity, not a distinctive kind of entity in its own right. Likewise, ontological social reductionists claim that groups imply only an ontology of individuals, groups or social wholes are redundant entities. Such ontological reduction is compatible with explanatory non-reduction, so one can, in philosophy of mind, be a non-reductionist physicalist, one can claim both that the mind is physical and that physical explanations cannot reduce psychological explanations (see, e.g., Davidson 1970).

“Reduction” can also be interpreted as explanatory reduction. This is the sense that concerns us here. This is sometimes called theoretical reduction. In positivist philosophy of science it was typically understood as taking place when the laws of one theory are “reduced” to the laws of another theory via bridge laws linking the theoretical terms of each theory, as was proposed as the relation between historically successive scientific theories. This is a strict notion of reduction that had to be relaxed to include the “amendment” of the theories in light of the reduction. We prefer the term “explanatory reduction” because we do not wish our discussion to rest upon the availability of any historical laws for which the question of “theoretical” reduction could arise. Rather, we ask whether the putative historical explanations must be dependent
on sociological, economic, or psychological explanations, and so dependent on terms borrowed from those disciplines. If the answer is “yes,” then reducibility implies the absence of *sui generis* historiographic explanations. Historiographic explanations are reducible, on this way of understanding that term, if they are simply a species of, or are essentially dependent on, some other type of theoretical explanation, in this case sociological, economic, or psychological. This way, we avoid begging questions concerning the relation between explanation and the presence of laws and so remain noncommittal as to the nomological concept of explanation (cf. Hempel 1965).

Another preliminary point that needs to be made concerns the modality used in couching our question. The issue is not whether historiographic explanations *can* borrow from the social sciences (to be understood hereafter as including psychology); clearly they can and do so while retaining a robust sense of *sui generis* historiographic explanation. There is an obvious sense in which social scientific explanations may well be useful for historiographic inquiry: they may sometimes be required as supplementary to the otherwise incomplete historiographic explanations. Even so, there could still be room for a unique, historiographic, type of explanation, even if this were necessarily only part of the full explanation of historical events. The principal concern is with whether there can be such a unique type of historiographic explanation, one that does not depend for its explanatory force on explanations from other disciplines. Our discussion will inevitably yield conclusions about the extent to which historians should consult the human sciences in their search for adequate explanations.

It is tempting to begin our inquiry with an account of what explanation is, and to move on from there to consider how putatively unique historiographic explanations fit that account. The *locus classicus* of such an approach is Carl Hempel (1942), who applied his analysis of scientific explanation to the case of historiography. That analysis is disjunctive: an explanation is legitimate *qua* explanation if and only if it provides the means to deductively derive a description of the event being explained (the explanandum) from a statement of a law and a description of some initial conditions, or if the law-statement and description of initial conditions makes the explanandum highly probable. This clearly makes the presence of a law essential to all scientific explanation, hence the designation of it as the “nomological account” of explanation.

Can we allow such an analysis to dictate whether there are unique historiographic explanations, or should our account of explanation be based on actual explanations in historiography? This is an issue concerning the acceptability of any proffered analysis of explanation, particularly one which may rule out some explanations that practitioners in some disciplines may deem acceptable. Hempel himself seemed to vacillate between ruling out some putative explanations on a priori grounds and amending his own analysis in light of current scientific practice. The rejection of “particularistic” or “singular” explanations (explanations that do not cite any general laws, universal or probabilistic) manifests the first tendency, the extension of the analysis to include probabilistic explanations manifesting the second. The dilemma posed by these two possibilities is that if, on the one hand, one stipulates in advance...
that only some explanations count as data for one’s analysis, then the disqualification of some putative explanations will appear to be entirely arbitrary. If, on the other hand, one tailor-makes one’s account to fit all putative “explanations” ever offered, then one will end up with no useful notion of explanation at all. This dilemma is present for every analysis that claims to possess both descriptive adequacy and normative “bite.”

There is no simple way to move between the horns of this dilemma, but progress can be made by inquiring into the rationale behind Hempel’s requirement that laws be present, implicitly or explicitly, in every bona fide explanation. That requirement reflects three thoughts: that in explaining why an event occurred one must render the event less sheerly contingent than it otherwise would have appeared to be; that the only way of doing this is to provide a description of the prior circumstances that necessitated (or made highly likely) the relevant event, and that such necessitation requires laws to be present to “cover” the antecedent circumstance and subsequent event (relevantly described).

The first two thoughts seem unproblematic, though the degree to which an explanation must make less contingent, or “probabilify,” the explanandum is unclear. What is crucial is the third thought, that necessity (or a decrease in contingency) requires laws, or at least generality. It is here that a historian may demur, as many historians have claimed that what they are after is an understanding and explanation of why unique particular events occurred, and so the generality required for explanation in physics or chemistry is not pertinent to historiographic explanation. On this view the uniqueness of historiographic explanation, its non-nomothetic character, becomes a consequence of the uniqueness of the events historians explain.

Setting the scene in the manner above allows us to concentrate on two issues: Whether there are special, “irreducible,” historiographic laws, and whether uniquely historiographic explanations require such laws. The notion of “law” at issue here is not particularly restrictive; it need not depend on some close resemblance to laws in the physical sciences (what we can call “nomothetic law-likeness”). What is essential is the guiding idea in Hempel’s requirement on any explanation, that the explanandum be rendered, to some degree, non-contingent, and this leaves it open whether there are diverse (non-nomothetic) ways of rendering an explanandum non-contingent.

We consider next the arguments for law-based historiographic explanation, and so the possibility of explanatory reduction, and then the possibility of non-law based explanation.

An outline of an argument purporting to show that there could be no historiographic laws can be found in the work of Karl Popper (1957). Popper connected the existence of laws of history to prediction: knowledge of laws plus initial conditions should be sufficient for prediction about the future, but there can be no such prediction in human affairs. Future states of the social world depend on the knowledge available to agents in that world. In order to predict what such future states would be like we would need to have access to the knowledge possessed by those future agents, given that agents
act on the knowledge available to them in changing their social world. We could not
have access to such knowledge in the present if that future knowledge rested on, say,
new discoveries or new technologies. Given we are not in a position to predict what
new discoveries may be made, nor what new technologies may be invented (otherwise
it would not be new), we are not in a position to predict what future states of the social
world would be like. Hence there can be no historiographic laws.

The argument generalizes to any laws that could be used in predicting a future whose
features depend on the knowledge available in that future state. Economic, psychological,
and social laws are likewise ruled out. (For further discussion of this nihilistic conclu-
sion see Macdonald 1995.)

The difficulties attached to the notion of there being genuinely historiographic laws
can be illustrated by two examples. Jack Goldstone (1991) examined periods of social
and political turbulence in the histories of England, France, the Ottoman Empire, and
Quing China. He found significant similarities between those regions immediately
prior to the turbulence, such as demographic change (rapid population increase, high
number of young people), state financial crises, elite competition, and a mobile
population. On the basis of these similarities he advances a theory of revolutions, which
he claimed is supported by the evidence from the period. The underlying trigger is
demographic change, a population “explosion” producing a high ratio of young to
old, thereby causing inflation, elite competition, and “mass mobilization potential.”
The latter two are also exacerbated by inflationary price rises, leading to state fiscal
distress as the state’s revenues fail to match its expenditures. What Goldstone calls
the “political stress indicator,” psi, rises rapidly, foreshadowing the turbulent times
ahead. The result is a loose “law” relating the various factors to the psi, which is then
used as an indicator, and explainer, of political turbulence.

The historiographic examination of whether there is a common explanation for the
studied the dynamics of territorial expansion and retraction, taking as case studies
the oscillations in size of the French and Russian polities (primarily between 500 and
1900 CE). On the basis of these studies he suggested that there is a common dynamic
pattern to such expansions and retractions in size. Both Goldstone and Turchin are clearly
interested in the histories of their target areas and periods because they are interested
in whether there is something significant in common to these historical periods,
whether there is a pattern to be discerned. Their interest in particular cases would appear
to be exclusively as evidence for the theories they are advancing. Are they providing
law-based historiographic explanation? (Any such “laws” will be ceteris paribus laws, but
this is to be expected, and is not uncommon.) Perhaps Turchin’s description of what
he is doing as “historical sociology” best fits the enterprise, but the question we face is
whether such explanations are reducible to other theories in the human sciences. Must
there be lower-level laws to which these historiographic “laws” are reducible? It would
appear that there need not be. At the very least an argument would have to be given
that there must be such reduction-facilitating laws, and it is difficult to see what form
that argument could take.

There is, however, a slightly different route the reductionist might take, and that is
to claim that the formulae generated by historians such as Goldstone and Turchin remain
mere historiographic generalizations, and must remain so until the mechanisms
underlying the disclosed patterns are discovered. Once these mechanisms are known, they will provide the details required for the reduction. Thus, for example, Elster (1985) argues against the view that one could have Marxist-style explanations of economic change without providing any mechanism responsible for such change (Cohen 1978). Elster later expanded this argument to encompass all explanations in the social sciences, as Wesley Salmon argued about explanations in general. Rosenberg (2001) develops the same position vis-à-vis reduction in evolutionary biology. Cohen argues that the likely required “mechanism” will describe people, their mental states (cognitive and conative), their particular circumstances, and their interactions. The mechanism should give rise to the patterns historians discern and debate.

The suggestion, then, is that historical generalization will become law-like only when underwritten by psychological (“individualistic”) explanations. For instance, in the Marxist methodologically individualist no-nonsense explanation of a certain historical pattern, a description of the relevant mechanism is generated by considering the question: why is it that the technological forces (the “base” in Marxist terminology), tend to change in the direction of increased productive power? One plausible answer is that in conditions of scarcity rational people will exercise their rationality in pursuit of more efficient ways of producing the goods necessary for their survival, and this will engage them in inventing new technologies, thereby yielding the increased productivity of the productive forces of the society in which they live. The direction of historical change will be derived from a fairly simple psychological explanation, this providing the reduction for the macro-individual “law.”

This strategy generates two further questions. One, which we must set aside for present purposes (but see Macdonald 1986, 1992, for a negative answer; Rosenberg 2001, for a positive one), is whether knowledge of the underlying mechanisms is required for any “historiographic generalizations” to play an explanatory role, and if so, whether this provides sufficient support for reduction. Another is whether there can be irreducible historiographic explanation that is not law-based, and this raises important issues that connect with our second main topic. The possibility of such explanation is thought to depend on the historian’s interest in explaining the particular rather than the general. What is at issue, then, is the kind of knowledge of individual behavior that must be available to the historian when explaining particular historical events and actions.

The particularist who believes in the uniqueness of historical events and historiographic explanations stresses the importance of particular token events for the historian. Theoretical sciences may be interested in past events, but they would be interested in those events as tokens of types, not as “bare” tokens, and their explanations will be couched in terms appropriate to the explanation of types, and so be inherently general. The explanation given by a historian will always be an explanation of (some of) the properties exemplified by the token event. Despite this, the particularist will insist that it is the specificity of the historical event(s) that is the principal concern of the historian, firstly in discovering the detail of what occurred, and secondly in placing that detail
within the context of the period in such a way as to illuminate how what happened came about.

The particularist is surely right that an essential feature of historiography has been its focus on discovering what happened, when it happened, and why it rather than some other event occurred (this being phrased contrastively simply because it is often in the light of the expectation that a different course of events might have occurred that one seeks an explanation of this sequence of events). To what extent must such explanations depend on the theoretical deliverances of the human sciences (psychology or, more generally, social science)?

This question is not about the extent to which these explanations mention psychological and social characteristics of the people and societies with which the historian is concerned. It would be easy to show that historians must rely extensively on the human sciences if all that required was that their explanations cite psychological and social properties of the characters and events mentioned in the explanans, since it is difficult to find any historiographic explanation devoid of any such mention. The historian who explains why Charles I of Britain acted in the way he did must cite the social milieu, the political situation, the economic background, the aims of his adversaries (the Earls of Bedford and Warwick, for example), the place of the monarchy in the constitution, Charles' beliefs about both that role and what his adversaries were trying to do to change it, and so on. All of this makes ample use of psychological and social properties, but there is no obvious dependence here on any psychological or social theory. What is employed in this type of explanation is what has been called “folk psychology,” and, as the name suggests, it is distinctive of folk psychology that it is pre-scientific, available to all, and used in common-place explanation of human behavior (Horgan and Woodward 1985). Folk psychology embodies a (typically implicit) common understanding of ourselves, so no special knowledge of or expertise in any science is required in using it. Insofar as historians rely only on such common understandings, their dependence on the deliverances of research in the human sciences will be minimal. But can we safely rely on the veridicality of the “hypotheses” concerning human action that are embedded within our common understanding of ourselves? More needs to be said about the “folk” in question, and about the psychology presumed.

Folk psychology consists of a set of assumptions about mentality employed by us in our everyday activities and interactions. We attribute to each other beliefs, desires, emotions, hopes, and the like, as well as enduring character traits (generosity, stubbornness, kindness, etc.), on the basis of which we are able to understand and explain the actions of both ourselves and others (D’Andrade 1987; Sousa 2006). An action qua action is explained as arising from those mental states which show why that action was the rational thing to do, given the circumstances and the psychology of the individual. Such a dependence on folk-psychological assumptions underpins Hempel’s rationalizing gloss on historiographic explanation. But this dependence makes his account vulnerable to at least two distinct charges: first, that the presupposed psychology is too parochial to be reliable: it presumes the truth of the EA (European-American) folk model. Just as the anthropologist has to struggle to avoid cultural imperialism in interpreting other cultures, so ought the historian be suspicious of treating other times as though the people inhabiting them were “just like us.” Hume’s famous mistake in proposing to observe the English and the French to understand the Greeks and
Romans. The folk psychology of other folk may be different. If it is different we need to
be aware of how the difference impacts on our view of the way others behave, given
that our self-understandings permeate our social interactions. Second, the account of
rationality that is embedded within folk psychology is too simple, and credits us with
more explicit knowledge of why we act in the ways we do than is warranted by the
evidence (Dray 1957, 2000). Not only is it too simple, but it is actually false. Using
folk psychology in explanations of historical actions will result in an inevitably flawed
explanation.

These charges suggest that we cannot bypass serious empirical investigation into
the causes of our actions, since only this will provide reliable information on what
really makes us act in the ways we do. If so, then the historian cannot rest content
with folk-psychological platitudes about the sources of human action. Let us first
examine the charge of “too parochial,” before moving on to the charge of “too much
rationality.”

Historians have been aware of the danger of what T. S. Eliot called “temporal provin-
cialism,” imagining the past too much like the present. One quick way to deal with the
problem is to mount a transcendental argument to the effect that folk psychological
conceptions of the self are necessarily universal, conditions of any understanding of
others. (See Davidson 1974, for an argument of this sort.) We think that the best this
can deliver is the conclusion that any understanding of others is predicated upon
taking as universal some minimal set of basic psychological properties, principally those
of belief and desire.

But there might be a more substantial set of psychosocial properties which can be
presumed to be universal. An example of a more substantial assumption concerns the
type of rationality evinced by those whose actions were in part responsible for the demise
of feudalism and the arrival of capitalism on the world scene. One explanation of this
transition claims that the psychological mechanism in play was fairly simple: rational
people could see that new socio-economic ways of organising production were being
made available by advances in technology, that these new ways would be beneficial,
so they struggled successfully against defenders of the status quo to bring about the
necessary changes (Cohen 1978). Some complain that this is anachronistic in
making the rationality of the successful agents too much like ours. In particular, it ignores
the class-specific nature of rationality, and so attributes to some of the principal agents
(peasants) an already “capitalist” mentality, concerned with an interest in optimising
output by raising levels of production through using new technologies (“developing
the forces of production”). An alternative explanation insists that prior to losing their
property, feudal peasants had no such goal in mind, being more concerned with using
the land at their disposal to eke out the living they needed (see Aston and Philpin 1985,
for an extensive review of the debate).

This alternative explanation still takes place within what we can understand as a
(broadened) folk psychological model, one that still presupposes the psychological
properties and propensities of our familiar self-conception. Other examples, however,
involve differences that are slightly more dramatic. The people of Ifaluk have terms in their conceptual repertoire similar to our folk psychological terms, but there are differences in the way they connect with each other. It is, for example, a matter of some debate whether our model of moral behavior and understanding allows for the possibility that we can understand that it is the “good” thing to do while not acting in a manner appropriate to that understanding, i.e., knowingly doing the bad thing. This is not possible on the Ifaluk’s understanding of the connection between moral understanding and action (on one construal of it): if one understands what the correct thing to do is, one of necessity does it (see D’Andrade 1987: 144, reporting work of Lutz 1985).

It is also well-documented that the western folk model tends to understand actions as being caused by people, or, more specifically, personal traits, whereas some other cultures (Saudi Arabian, Indian, Chinese) stress the action as being produced by the situation the person is in (Lillard 1998). This has repercussions in how it is that people are likely to treat one another when something “bad” happens to them. Western folk theorists tend to blame the actor, situation theorists blame the situation. (For an empirically based anthropological view that there are some universals see Wierzbicka 2006.)

Empirical knowledge of the variants available to different folk can alert the historian to possible sources of misunderstanding in the study of historical periods culturally remote from our own. This knowledge will play a supportive role in historiographic explanation, and so will not normally require wholesale revision in the nature of historiographic explanations; the model applied will still use relatively “unscientific” concepts, relating as it does to commonsense, pre-theoretical ”knowledge.” Perhaps empirical inquiry from anthropology can make the historian aware of various pitfalls in the unreflective use of a western folk psychological model. The matter will be different if the historian has to do away with such pre-theoretical understandings, either in part or altogether, replacing them with explanations derived from theories in the human sciences. It is to this possibility we now turn.

Irrational action seems to be clearly part of the subject matter of historiographic research, so a paradigm of explanation in which only rational actions are explicable will often fail to be of much use. Historians need to locate irrational behavior within the domain of the rational, as behavior that can be understood and explained by appeal to folk-psychological states that are governed by the norms of rationality, but where the behavior departs in some way from those norms. Depending on the distance from the ideal of rationality, the historian may well require guidance from experts in the field. How much guidance depends on the nature of the case. Consider the famous example of George III, who descended into madness in his later years. It may be that historians are interested in the nature of the illness that overtook him, as to whether it was a form of porphyria or inadvertent arsenic poisoning, but one does not need to know a great deal about porphyria (if that is what he suffered from) to explain his actions toward America, or France, or his dealings with William Pitt. The illness itself eventually incapacitated him, but again the historian only needs to know that fact to explain why it was that in the last decade of his reign the role of the monarch was assumed by the Prince Regent. The illness itself did not seem to change the meaning or purpose of his (relevant) actions, so the dependence on any science here is minimal, limited in
the main to the provision of information (the particular form of the illness) and not to substantive explanation.

The case is somewhat different when we consult a less physical or “medical” cause of psychological disorder. The more the disorder conditions the actions perpetrated, the more reliance there will be on theories from the human sciences. A single example will suffice: in exploring the identity of “Jack the Ripper,” the notorious serial killer of London in 1888, the historian will inevitably be drawn to the question of why whomever he was acted in the manner he did, and the historian will be hampered in this task if the guiding assumption was that such an agent must have been rational. A recent account given by the historian Charles Van Onselen (2007) suggests that the “Ripper” was one Joseph Lis (alias Silver), who spent some time in Whitehall during the late 1880s. It is essential to the argument that Lis’ psychological condition makes him a suitable candidate for being Jack the Ripper; Lis was, on this account, psychopathic, syphilitic, and possessed by an enduring hatred of women. Van Onselen mounts a credible case that such a psychological type was quite capable of serial murder, and that the targets would very likely be prostitutes. In doing this he makes use of the relevant psychological literature on psychopaths and syphilitics, and his account would not have been plausible without that support.

Such guidance is, we suggest, necessary in cases where the agent suffers from a psychological condition that renders their actions clearly irrational. Dementia, phobia, obsessional disorders, and so on, are all part of (some of) the historical material facing the historian, and when countenancing these characters the historian will, of necessity, turn to experts for guidance. In this, rather limited, sense the historian will inevitably be dependent on research done in the human sciences, but, again, overall the dependence is supportive rather than constitutive.

There is an irony in the trajectory just sketched. We began the present section by suggesting that insofar as the historian’s explanations of particular events are committed only to a folk-psychological account of human behavior, the historian need not rely to any significant extent on theories derived from psychology or sociology. But we now see that the more agents’ psychological state give meaning and direction to their actions, the heavier the reliance on experts becomes. This indicates that rationality itself is not immune to scientific probing, and that insofar as it is the subject of scientific research the historian will be advised to take note. This is not to suggest that historians must slavishly follow the latest fashion in cognitive science or cognitive anthropology; the lesson intended is that it would be prudent for historians to be aware of the burgeoning interdisciplinary work being conducted in the fields of cognitive science, cognitive anthropology, evolutionary psychology, and cultural evolution. This work is often highly speculative and sometimes tendentious, but it also often has the virtue of subjecting our pre-theoretical understanding of ourselves to empirical scrutiny. A historian who, for example, assumed that the persistence of religious behavior was primarily dependent on cultural causes, and modeled their account on that understanding, may well be surprised to find that other investigators view such behavior “as a phenotypic analogue to the linguistic capacity that is our evolutionary birthright . . . this means that religious activity is no more ‘cultural’ than stereopsis or bipedalism” (Day 2005: 86).
We have looked at the possibility of law-based reduction of historiographic explanations to other human sciences, and argued that there was no sound argument requiring such that such a reduction be available. The plausibility of any such argument would depend on the requirement that individualistic psychological features must provide the mechanisms underlying any putative historiographic generalization. We recommended that such a requirement be rejected, but then looked in more detail at these presupposed psychological features. Insofar as historians rely only on a “common understanding” of ourselves, our folk-psychology, we suggested that theoretical reduction was not available. We then went on to examine this folk-psychology in more detail, arguing that historians need to be aware of relevant research that extends or subverts folk psychology. Reliance on a pre-theoretic understanding of individual action may undermine historiographic explanations offered in ignorance of this research. The picture that emerges is one where historiographic research is seen to be enriched by an engagement with findings in, among other sciences, cognitive science and cognitive anthropology. This enrichment is not reductive; it is enhancing.

Acknowledgment

Work on this chapter was supported by a grant from the Marsden Fund of the Royal Society of New Zealand.

Bibliography

CYNTHIA MACDONALD AND GRAHAM MACDONALD


