



Exemplification and the use-values of cases and case studies

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HIGHLIGHTS

- Separates out cases from case studies across a range of social science fields.
- Considers modes of comparison as ways to deepen knowledge.
- Discusses different possibilities for extending knowledge beyond the original case.
- Suggests that individual case work may catalyse or crystallize knowledge for further usage.
- Argues the best way to frame case-based knowledge transfer is as a mode of exemplification.

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ABSTRACT

This paper provides an account of the ‘use-value’ of case-based research by showing how social scientists exploit cases, and case studies, in a variety of practices of inference and extension. The critical basis for making such extensions relies on the power of a case, or the account given of a case (the case-study account), to exemplify certain features of the social world in ways which prove valuable for further analysis: either of the same case, or in many domains beyond the original case study. Framing use-values in terms of exemplification compares favourably with understanding reasoning beyond the case either as a form of analogical reasoning or in taking cases as experimentable objects.

1. Introduction

A broad reflective literature on case-based research flourishes within the social science communities, focussing primarily on the methodological issues of the genre. This literature has sometimes assumed that similar problems hold across all the social sciences, though it is clear that there is a certain particularity in both the methods and functions of cases according to the specific discipline studied. In contrast, within the history and philosophy of science commentaries, the broader questions of how ‘thinking in cases’ works, what it is good for, and how it differs from other epistemic genres (such as experimenting, modelling, statistical thinking, etc), have been relatively neglected, despite Forrester’s (1996) addition of the approach to Hacking’s list of such genres (1992).

This paper addresses this broader set of questions to enquire into the role of cases as sites for knowledge-making. The major puzzle usually noted about case-based knowledge, both for philosophers of science and practising social scientists worried about their methodology, is to focus on the inferential problems of working with cases, posing the problem as: What can you learn from a case, given it is just one case?

Philosophers of science might see this as the question of justifying external or general validity, asking: How do you extend or generalise findings from one case, either to other cases or for use with other forms of scientific knowledge? The problem in this form can be approached by charting the various means and modes in which particular knowledge from *any* form of scientific investigation - not just case studies - can be ‘re-situated’ to other sites (see Cartwright, 2012; Morgan, 2014). Another approach, specific to cases, focusses on the narrative form of explanation offered with case studies, and the extent and conditions under which such narrative explanations constituted for one case offer extensions to other cases (see Morgan, 2017).

The inference issue is re-framed here by asking: How do communities of researchers use the new knowledge established in an individual case or case study? So, the problem treated here is neither how a particular case is constructed and justified in the first place, nor to analyse some very specific methodological problems (such as how does a case study uncover a causal mechanism), nor its (conventionally understood) generalising or inferential possibilities. Rather the starting point here argues that while an individual researcher may gain considerable knowledge and insight from studying a case, the wider viability of cases

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as epistemic objects can be found by looking at how the community uses them. So this study focuses on the afterlife of cases and case studies, that is - where the case, or its materials and account, are taken up for discussion, reused and extended in various ways by a community of researchers. This activity indicates the 'use-value' of cases and the aim is to locate that value by looking at how case knowledge generates a range of more generic knowledge formation.

Researchers take certain cases, and case studies, for re-use when they see them as offering an exemplifying quality. It is this quality that offers the key to the use-value of case work. In order to consider this broad functional use of case work, the analysis differentiates cases from case studies. *Exemplary cases*, such as Athenian Democracy, are known, used and reused because the case itself stand out as unique in some way, regardless of the quality of the accounts about them. Other cases - possibly mundane cases - become known to a scientific community because the researchers are seen to offer an *exemplary account of the case* phenomenon in an exemplary narrative: the case study, which illuminates and changes the way that the community thinks about that phenomenon. That is, the particular case in the case study need not in itself be important or exemplary, it is the account given of the phenomenon in the case study that is considered exemplary and is used by the social science community wherever the value of that account is taken to be relevant. In effect, this paper distinguishes between the knowledge to be gained from 'the study' separately from the 'the case'. This may seem a little strained, after all - how do we know about the case except through study of it? But whereas the particulars of the case always continue to matter for use of exemplary cases, the same may or may not be true of exemplary case study accounts (even where exemplary case details are sometimes ignored when a case becomes well known, they are always there in the background). And even if the case carries the study or *vice versa* to start with, the two categories do come apart.¹

These two broad categories of case work can be associated with broad use-values as follows. Exemplary cases - where the cases themselves are judged extraordinary - lead to knowledge accumulation via theory-breaking, the snowballing of evidence, and the prompting of new research questions *all for the same case*. Exemplary case studies - where the cases themselves may be ordinary, but the social scientists' treatment of them are taken as exemplary by the community - act as knowledge 'catalysts' or 'crystallizers': they are taken to ask new questions, or to offer new answers, new conceptual materials or new methods, or even new policy recipes, any of which can be taken to be potentially relevant *for other cases elsewhere*. These broad functional categories of use-values cover a lot of possibilities, as we shall see in the examples below. It is important not to mistake the claim here: all chunks of scientific work, in any other mode, may have these use-values and prompt further knowledge accumulation (see Morgan, 2014). The point for cases is two-fold. One is to overturn the idea that case knowledge is of no further use because 'it is only one case'. The second is to look at *how* such knowledge accumulation takes place in the way that research communities use and value cases.

It is important to stress too that, as an epistemic genre in the social sciences, case work is not a method, but offers a generic way of studying social realms: a case or case study offers a whole, real life event or situation, studied within its context; as such it is almost oppositional to the genres of statistical thinking and hypothetical modelling.² The

¹ This account does not attempt to deal with the use-value of runs or sets of cases for example, for defining the new disease category HIV/AIDs as in Ankeny (2011); nor does it deal with the use of a run of cases in sorting out causal claims as in Ankeny (2014). These are all further use-values of working with cases that surely appear in the social sciences as much as in bio-medical sciences.

² As the papers in this special issue demonstrate, case work is not a method, but rather case work may involve many different methods (thought experiments, statistical measurement, modelling etc). See Morgan (2012) for a fuller, but still broad, definition of case studies that seems to work across the social

paper also takes the position that across the range of case work from the human science end of psychiatry, via anthropology and sociology, to political science and economics, and thence to social science histories, there is a commonality in case thinking that is independent of differences in methodological approach, methods and particular questions. In support of that point, the examples used to realise the arguments here come from sociology, anthropology, politics, economics and economic history. The paper begins with cases and their use-values before moving onto the use-values of case studies, using examples to illustrate and explore the modes of knowledge gathering that each gives rise to.

2. The use-values of cases

2.1. The case: Athenian democracy

The discussion of cases starts from Josiah Ober's (2007) examination of the use-value (also his terminology, p 225) of the case of Athenian democracy by political scientists and historians in the context of a broader colloquium discussion of the comparisons and commonalities between the role of cases, exemplary narratives (in the humanities and social sciences), and model organisms in biology (see Creager, Wise, & Lunbeck, 2007). To situate the discussion here, it is notable that Ober is mainly concerned with what enables Athenian democracy to function as an exemplary case, somewhat in the way that the lab mouse, the arabidopsis plant, zebrafish, fruitflies, etc function as exemplary research objects for biologists.³ He does not separate out the two notions of cases and case studies, but it is clear he is mainly concerned with cases.⁴

Whilst Ober's discussion is framed within this more general comparison of case work with the model organism research of biology, there are salient differences which will emerge in the course of this paper. To be specific, he suggests that there are commonalities in usage qualities between model organisms and the case of historical Athens, which enable the latter to be used over and again by social science and humanities research communities. Ober argues that political theorists and philosophers use the case of Athenian democracy as a testing bed for thinking about democracy in political theory and political sociology, and about issues of justice and rights by moral and political philosophers. These users employ both thought-experimental methods on the case, and use the case as a comparator case in order to shed light on other possible, and current or past, political/institutional arrangements. That is, they use the case as exemplary for current issues in both positive and normative modes even though the events and situation occurred long since. While the case is an historical one, it speaks to current problems and interests: Athenian democracy offered a 'distinctive form' of democracy, which embodies characteristics which are specific but could be more generally typical and so the case offers the possibility of thinking about democracy as a generic type. This combination of specific and typical is the reason it is understood as an exemplary event/case in those fields that engage with it and have prompted a long tradition of its usage as an exemplary case.

In his account of why and how the case supports these uses, Ober points out (2007, p 229) that the case has a finite history, and a manageable and clear body of evidence easily available, which makes it easy to test new theories and decisively reject them against the

(footnote continued)
sciences.

³ A particular model organism, as a particular case or case study, may create something akin to Merton's "strategic research sites" (1987 p 1), but the sense and contexts in which a case may prove to be strategic is rarely a matter of pre-knowledge and therefore of choice. Merton of course recognises that there is an element of luck involved. I thank one of the referees for pointing to this connection.

⁴ Ober actually uses the term 'exemplary narrative' for his cases (p 225), but I keep this label for the case-study account, not for the case.

evidence. There is enough evidence available that can be organised differently, so allowing competing explanations for the specifics of the case; there is agreement on some basic elements, but disagreement over others prompting continued investigation. The materials are “sufficiently rich and fully articulated to genuinely flesh out our intuitions” (p 230), but not so overwhelming that another researcher has to do lots of research just to understand the case. But, above all (like model organisms) the case is a real-life case, not an hypothetical one, which enables political scientists and philosophers to “test [their] intuitions” in a sensible way. Ober dismisses the “bland and bizarre” (p 229) kind of hypothetical experiments (beloved in some circles of philosophy) in favour of serious usage of the actual historical case which forces attention onto its evidential richness but also allows examination of the salient details to provide greater depth of understanding. This creates a “recursive relationship” (p 232–3) between theory and the interpretation of the historical evidence. While the evidence is finite, the narrative gets richer as more intuitions are tried out, and as the interpretation moves on, so that what is seen as typical of Athenian democracy changes. That is, scholars can treat the case as an object both for theorizing and for re-thinking the evidence.

In effect, Ober explicitly uses two other modes of scientific work for comparison in this analysis: the case as an experimental object, and the case as a model organism. I take up these comparisons at various points in the paper to suggest that while both elements offer positive insights, neither works fully in practise. Nevertheless, starting with Ober's analysis does point to how users value a specific case and to how knowledge is accumulated around other such cases that have become exemplary. Two examples studied intensively by both economists and historians (and also the subject of public knowledge and commentary) are the massive depressions that occasionally hit the economy and the industrial revolutions that occur within economies. Their exemplary status as events often leads to initial capitalization of their labels: the Great Depression of 1929–36 and the First Industrial Revolution of 1780–1820, and - like the case of Athenian Democracy - they never go off the agenda. In contrast to that latter case, the continuing presence of the former cases in the academic realm does not betoken static evidence sets, but rather the opposite: they remain critical sites of research work associated with an accumulation of knowledge about the events themselves. The complex combination of events labelled under one of these umbrella titles provides a set of growing materials for endless re-examination and explanations by a variety of scholars with different interests.

Looking more carefully at these examples, we see that unlike Ober's Athenian democracy case, these economic cases remain ‘live’ cases, and so are even more like model organisms than the Athens case because of the army of researchers working on these unique or extremely unusual events who, over time, heap up new evidence around the event just as rolling a snowball around gathers more snow. But as we shall see, it is more problematic to frame the use-value of these cases as offering rocks on which to break or test theories (as Ober proposed for the case of Athenian democracy); there is a theorizing component, but this is more a recursive activity between evidence and theorizing (which he also proposed). These research-intensive activities typically raise new questions about the events, prompting further new research, including, perhaps, comparative case work.

2.2. *The single case: theory-breaking rock or rolling snowball?*

The case of the Great Depression of 1929–36 is characterised by a number of basic elements: about the causes, consequences and interaction of those elements, and the timings of events within the case. Economists did try to use the Great Crash of 1929–32 and the Great Depression that followed until at least 1936 (longer in the USA) as a rock on which to test, or rather break, theories, not in general terms, but in terms of the critical causal account of these particular events. And, for a while, the most active debate over the causes pitted the

monetarist account against the fiscal account: that is, a normal cyclical depression became a ‘great’ depression because of faults either in monetary policy or in fiscal policy.⁵ However such examinations did not simply or easily confirm or refute the relevance of either theory for the particular case. While it was possible to show that some critical causes were not irrelevant to the overall causal story, the record of the Great Depression did not form a viable theory-breaking rock, and did not support clear mechanistic accounts. It is a significant feature of case work that the whole case is analysed within its context. So, on the theory-testing front: while individual and distinct causes (such as fiscal or monetary policy) could have been, to some extent, isolated for examination in a kind of thought experiment, and even in statistical testing, these causes were too highly interconnected with other factors, including the functioning of economic institutions, to be easily separable for definitive theory-testing judgements. The experimental requirement for isolating causal features and controlling for others is difficult to maintain in case work, and from this point of view, case work is more akin to field work than to laboratory work (see Morgan, 2013).

One might wonder - why were economists and economic historians so obsessed with this case? The answer is that just because the Great Depression case served as an uncompromising event that stood out in the path of history, it could not be ignored by the community, but neither could it be easily explained. For economists seeking parsimonious explanations, the continuing aim was to come down on one side or another of the debate about causes. For economic historians, trained as economists but cognisant of the rich possibilities of gathering further evidence about the case, and with broader historical requirements for explanations of events, the project proved more eclectic. As historians, they were bound to look for more complete explanations for the event, to bring in all the relevant factors and their interactions, thus making accounts of the event more complicated.

Like Athenian democracy, the Great Depression was a case that could be returned to again and again, and because it's so well-known, it could be thinly characterised in discussions. This contrasts with the actual thinness of the Athens case evidence and the limited availability of new evidence which supports Ober's claims about Athens having use-value as a (thought) experimentable object for political science. Unlike Athenian democracy, the Great Depression is a case about which more evidence could be collected, and as evidence became thicker it also became messier and this undercut its plausibility as an experimental object on which to test or break theories. But at the same time, this supported a process in which more possibilities arose to rethink the existing accounts. With new evidence accumulating, new questions were raised, new explanations could be tried out, privileging new causes or giving them different emphases - but all for the same one case. The case-event became, for the community of researchers, less a rock for breaking parsimonious theoretical accounts and more like a large rolling snowball as the exemplary case gathered more evidence and was endlessly re-interrogated in the light of that new evidence.⁶

For many decades after this case-event, the cycles of economic activity offered no comparative case-events of such magnitude. There were financial crashes, and depressions, but the Great Crash and its associated Depression stood out as a lone boulder in an ice field. Until

⁵ For this US case, either the event was caused by monetary mismanagement (over-expanding money supply in 1920s and then cutting it too far and too fast causing the crash and depression, or perhaps prompting a small reversal into a catastrophic depression), or it was a minor crash which was exacerbated by fiscal mismanagement (by the combination of national and local governmental responses) which turned the crash into a depression.

⁶ As trained economists they were also interested in the international transmission of the financial crash from the USA to Europe and ‘periphery’ countries; in the international co-ordination or lack of it to prevent the financial crises becoming a general depression; in the role of economic fundamentals and policy in perpetuating or mitigating the depression; and so forth.

2008–9 that is, when the Global Financial Crisis (now referred to by its initials: GFC) happened (notice how economists somehow make these events sound like meteors hitting the earth: they ‘happen’, then they have to be examined, explained, and made sense of, and their consequences mitigated in various ways by policy actions). At this point, the case-events of 1929–36 suddenly began to play a new role, not just in the specialist academic communities, but in the immediate world of policy action. It turned from a one-off phenomenon to the only available case for comparison. Over the past decade, this has mostly been a one-way comparison, using the earlier case of the Great Crash and Depression, with its many theoretical interpretations and empirical characterizations, to understand the current GFC events and policy possibilities. No doubt in time, these two events will provide comparative sites that enable ‘reciprocal comparison’ (see below), the later event being used to cast light on the earlier just as the earlier is already used to cast light on the latter.

2.3. Comparing cases: digging deeper and heaping up

The mode of ‘matched pair comparisons’ is practised at many sites in the social sciences, either formally or informally. The Millian injunction to find two cases with the same features except in one respect is designed to trace the importance and role of that one respective difference.⁷ The matched pair idea is often aligned with case work, but in such contexts, it rarely works to assess a *single difference*. In practice, it is very difficult to find such well-endowed, Millian-inspired, matched pairs of cases, and social scientists looking for such comparable cases are more likely to find cases for which their precise differences are not fully known, and in which there is nearly always more than one difference that is likely relevant. Nevertheless, comparative case work does have considerable use-value even when the cases do not exhibit the ideal matched-pair status.

While the analysis of similarity and difference has a particular association with Mill’s recipe, such comparisons were given a new spin by the economic historian Marc Bloch (Bloch 1928, 1953). For him, the relevant starting point might rather have been the writings of Durkheim than Mill (see Riemersma, 1953), but at any rate, his interest was to champion the use of comparisons within social science history. For Bloch, the method of comparison - the hard questioning equally of *similarity* and *difference* - provided: first, a recipe for discovery of hidden facts; second, a challenge to easy interpretation of those facts; and third, an explanation of both similarities and differences, in which particularities and generalities might be questioned and ‘pseudo-causes’ and ‘dead ends’ avoided. This was an even-handed and open comparative approach in which nothing could be taken for granted, but much could be gained. Kocka (2003), following Weber rather than Durkheim, took up the ‘discovery’ point: historians (in effect case workers), are highly specialised in their field, so asking them to look at a new period or place can have a “liberating, eye-opening, effect” (p 41).⁸ In contrast, Sewell (1967) argued that Bloch’s approach across all three of his aims provides - for historians - a “logic” of hypothesis testing: a regime of “testing, reformulating, and retesting ... an adaptation of experimental logic to investigations in which actual experimentation is impossible” (p 209). In effect, Sewell portrayed comparative history more narrowly than Bloch (and Kocka) as an experimental method for gaining secure explanation in history, i.e. justificatory aims rather than discovery aims in a way that parallels Ober’s view of the use of the Athens case - and indeed other single historical cases. Perhaps it does work in this way for

⁷ For a recent example arguing in this Millian vein for comparability as a resource of inference in social science case work in political economy, see Ruzzene, 2012.

⁸ Model organism researchers are - in a similar way - typically loyal to one organism, and asking them to look at a different organism may be similarly surprising (as was revealed in the colloquia for the Creager et al., 2007 vol).

political science, where these comparator cases might be many, and thinly described in a coded form. For other social scientific fields, the comparative method has functions closer to those envisaged by Bloch and Kocka than to Sewell: the comparative case mode is not so much a testing regime, or an experimental regime, but one of interrogation and perhaps discovery, relying on the flexibility of comparison, and its capacity for in-depth study of the cases, that seems antithetical to the narrow label of hypothesis testing.⁹

The eye-opening effect of comparison has been critical for other fields, for example in studies of economic development. While traditional economic accounts began with Western economic development as the dominant and earlier case providing the developmental recipe, and then compared both Eastern and non-developed countries’ experience against that, recent social science history has become more enamoured of the mode of reciprocal comparison.¹⁰ Global economic historians have followed Pomeranz’s (2000) injunction to make more serious two-way comparisons in taking both non-European and European experiences as equally valid benchmarks for each other in learning from their similarities as well as their differences as Bloch advocated. Since, in addition, the comparative economic performance of one area may constrain, or enable, the experience of the other case, and so be part of the causal set of possibilities under consideration for both sides, these reflective comparisons are expected to be more illuminating, and prompt more research and questions, than the simple benchmark comparison of an old with a new case event.¹¹

In industrial revolutions, we have several exemplary events and the ways in which these successive cases have been discussed shows how valuable this reciprocal comparison of exemplary events can be. Getting a sense of the cases in this instance is a necessary starting point for appreciating this process. We don’t have one Athens, or even two great crashes and depressions, but a group of three ‘industrial revolutions’ and perhaps we are in the midst of the fourth. The historiography outlines how these events have been re-interpreted over the years.

The First Industrial Revolution is dated at 1780–1820 or thereabouts, situated in Britain, and characterised by the ‘factory system’ of production, with textiles taken as the exemplary industry, and involving a larger scale usage of new machinery powered by the new steam power which together reorganised the nature of work. Because this list of characteristics betokens a qualitative change in modes of production, rather than a step change in scale, this has been taken by economists and historians as a revolution in how things are made. Originally the causal prompt for the step change was taken to be individual inventors and engineers who developed the machineries of production and of power. Of course, this is a thin, idealized, description and analysis of the case (which would horrify historians, but would be recognised by economists).

As a case, it has been heavily researched and analysed, but has proved too amorphous and too unbounded to be treated as experimentable (as Sewell proposed). Even more than the Great Crash and Depression, industrial revolutions have tremendous scope: they change

⁹ There is a very considerable literature in the philosophy of history and by practising historians on the topic of comparative history (e.g. see Cohen and O’Connor, 2004). In short form: comparative history was originally conceived as a recipe for getting away from nation-based history and so primarily involved geographical comparison; but the focus more recently moved to ‘crossed’ or ‘entangled’ histories of different people and nations.

¹⁰ See Austin, 2007 for an account of the development of this social science stance.

¹¹ I thank Gareth Austin for the additional point that earlier industrial revolutions constrain later ones (they need to compete on different grounds) compared to the ‘parallel’ developmental paths assumed in global history. This joint constraint is similar to the wonderful conceit of David Lodge’s hero of *Small World*, Perse McCarrigle, who startles the world of literature by talking of the influence of Yeats on Shakespeare: how our interpretation of Shakespeare changes if we begin by reading Yeats (Lodge, 1984).

more than the economy. Here the comparison with Ober's Athens is instructive. Whereas economists can run counterfactual experiments on some economic events, for example, about what would have happened to an economy without the development of the railways (e.g. Fogel, 1964), they could not carry out that kind of test of effects or causes of the Great Crash and Depression and nor can they do so with an event designated as an Industrial Revolution. Why not? Because these kinds of events are just too complex and widespread in content to be thought-experimental systems, or to run statistical equivalents in theory testing. Here is where we see the importance of the reciprocal comparisons in gaining use-value from cases, for that first Industrial Revolution was effectively re-characterised when the Second and then the Third Industrial Revolution were taken into account.

The Second Industrial Revolution is associated with two countries, the US and Germany in the late 19th century. In the US, the revolution has been characterised as a new system of manufacturing which developed machines (machine tools) to make other machines, namely consumer and producer goods (such as sewing machines and engines). The causal accounts focussed initially again on engineering skills, this time fixing on ingenuity in making mechanical systems. The German revolution involved chemical industries (dyes, pharma, industrial chemicals, etc) in which German engineers industrialised the products of nature directly. Comparing the 1st and 2nd revolutions together re-enforced an Anglo-Saxon account in which Weber's protestant work ethic featured as a centrepiece in the causal account for both, but in which the reciprocal comparison (looking backward to the First Revolution and from there forward to the Second) also suggested that the failure of Britain to experience the 2nd revolution might be attributed to their class system (compared with the USA) and their poor science (compared with Germany). The Third Industrial Revolution is associated with Japan post WWII - making manufactured goods cheaper and (eventually) with better quality. Its main causal account fixed on relative factor costs: Japan had shortages of capital and labour and raw materials and so had to save on all these inputs in a 'lean production method', and succeeded by introducing stringent quality controls. In turn, this third case reflected light back onto the productive input factors of the first and second industrial revolutions. This comparison was sufficient to rewrite both their accounts, for the new focus suggested that Britain in the first event had cheap labour and capital so used more of both productively, while in the second, the USA had expensive labour and capital but very cheap raw materials, so evolved a generation of technology that saved the two former and was profligate with the latter factor. Strong economic factor arguments were privileged over earlier cultural explanations based on religion, work ethics, Yankee ingenuity, German education, and smart inventors. Once again, this is a very thin account of the main arguments, but in a sense these details are not important, right or wrong, either as bits of history or as economic arguments. It is the mode of comparison that harnesses the use-value of the cases here.

Just articulated is an account of three exemplary cases, prompting reciprocal comparisons in which it is the similarities as well as the differences which provoked redescriptions, new evidence, reassessments of old causes and new causes, new materials, and new theoretical accounts. Notice too that these Industrial Revolution labels include numbering: they are conceived of as separate cases, of the same general genre but with very different characteristics. One industrial revolution is useful; three are more than three times as valuable because the *reciprocal* comparison prompts new factors to be taken into account in the study of all three events. Here the recursive relationships are not just between case and theory (as Ober marks), but between different Industrial Revolutions with different characteristics provoking the development of theories to cover each and all cases. The knowledge accumulation here is not via rocks (the cases as detailed by their causal accounts are too unstable), nor rolling snowballs (not just adding more evidential layers) but via digging deeper and heaping up evidence on previously overlooked characteristics while theorizing new plausible

causes, and so this sequence cumulates knowledge about all three cases, not just one. This is a discovery mode of science.

I have labelled these cases as 'exemplary' and suggested, following Ober, that exemplary cases might have similarities - in the tool box of science - to model organisms, because both offer scientific objects that repay multiple and repeated investigation. There are some important differences, and so here is where Ober's analogy with model organisms in biology not so much breaks down as enables us to see two general salient points about the way social scientists use cases.

First, the Industrial Revolutions example shows the importance of the scope of comparison, both similarity and difference, as ways for social scientists to learn about their cases. Model organisms are not usually used in a reciprocal comparative mode in quite this way. Comparisons of the lab mouse and humans do surely rely on qualities judged to be similar or different, but those comparisons are likely to be specific to particular questions, rather than open-ended enquiries into the nature of *both* organisms. And while the mouse may serve as a model organism for human health issues, it is less likely that the human will serve as a reciprocal model organism for mouse health issues: this is usually a one-way and limited comparison.

Second, the point about these exemplary cases is that they exemplify something taken to be important to the research community. And because they exemplify, they have potential to speak to new questions and new problems for recognisably similar objects in the same broad domain. The different cases of the industrial revolution can be conceived of as the same kind of thing, the same class of thing, so that the similarity and difference is within the class. In contrast, the arguments for using model organisms depend, not on exemplification, but on analogical reasoning which relies on establishing limited domains of similarity between two different kinds of things, say lab mice and humans, which then justifies the knowledge (eg about disease) to transfer from one organism (lab mice) to another (humans) (see Ankeny, 2007, and further back, Hesse, 1966). Both rely on similarity judgements but made on two different bases: exemplification and analogy. The important point here is that the qualities of social science cases as a kind of scientific object do not fully determine their methods of analysis any more than the qualities of model organisms determine their methods of analysis. But whether they exemplify, or are dependent on analogical reasoning, in relation to other similar/different objects does determine *how* they can have use-value for scientists.

3. The use-values of case studies

In cases such as Athenian democracy and the First Industrial Revolution, the actual events or cases themselves have been taken by social scientists to exemplify a social science phenomenon (a type of democracy, a type of economic upheaval) and its package of characteristics. And there are surely other phenomena in the social science range that have been defined by such exemplifying events. These cases remain important as they define the generic type, and their particular details also remain important as signals of generic characteristics. But, as argued above, thinking with cases means reusing the same case again and again, thus the useful, but also limited, analogy with model organisms.

In contrast, in case studies, the case itself does not necessarily remain important to any further use - so the analogy with model organisms is immediately lost. Of course, there are many case studies undertaken of many different social phenomena and events, and most have rather limited further use. Yet there are some, where, even though the case itself is taken to be rather ordinary (just one amongst many examples of a phenomenon), the account (or aspects of the account) given by the social scientist of the case is taken to be exemplary by others in the community. The important point here is that knowledge accumulates not around the original case, as in the above examples of Athenian democracy or the Great Depression, but around the potentially more generic phenomena that was discovered or explained in the

original case study, that is, around the account given of the case. Such accounts offer, in Elgin's (2011) apt phrase: "a telling instance that exemplifies" (p 409).¹²

While we can easily mark the importance of any individual case study within a scholarly community by following citations, it is also possible to trace the kinds of materials taken from such an individual case study, and show how these materials served as exemplary materials, valued in their communities of use. There can be many aspects in a case study account that can be taken as exemplary, and thus many different aspects of case knowledge that provide use-value for other researchers, and these can be used in different ways. Analysis of the ways in which these exemplary cases study accounts are used by their communities of researchers suggests a broad distinction between two different ways in which this process of extension works: namely, where the exemplary case account acts as *catalyst*, and at other times, as *crystallizer*.

The first kind of use-value occurs when individual pieces of research act as a *catalyst* for further research on a phenomenon, a process which may be found with other research genres. But it is important to include this general category here because of the anxiety within philosophy and social science communities that case studies can only teach you something about the original case at hand. The information gathered together within a case study provides evidence on a phenomenon that can prompt further research on that phenomenon in other cases. But it is not just empirical findings that prompt further research in the community. In fact, as we shall see, there are many aspects of case studies that can be, and are, taken to exemplify and so be potentially generic for other similar cases. And because case studies may offer a great variety in kinds of evidence and methods, they may offer greater possibilities for catalysing research in related contexts than some other genres of scientific work where the methods and evidence sets are more constrained.

The second kind of use-value comes when case studies act as a *crystallizer*. Again, the point is not that this is a function only true of the exemplary narratives of case studies, but that it is an unexpected, largely unexplored and important function of case studies. And when a case study acts in this way it has the potential to inform, and be used for, a much wider set of other cases of a phenomenon or for a much wider domain of argument.

3.1. Case studies as catalysts

A case study account can be considered exemplary when the community uses it to gather knowledge around the generic account of the phenomenon suggested in the case account, deepening and extending understanding for other potentially similar cases, but the community rarely engages in further research on that particular same case. There are a number of ways in which this occurs, because almost any aspect of a case study might be taken to exemplify something of use-value to the community and which then has the effect of catalysing further work in the domain or on the topic. For example, Robert Fogel's (1964) counterfactual case study of the effects of the railroads on American economic growth in the nineteenth century provided a calculating recipe that other scholars followed in assessing the contribution of other national systems of rail transport to the growth and development of those nations. In another example, Crasnow (2012, 2017) analyses how Kenneth Schultz (2001) used causal 'process tracing' to analyse the Fashoda Incident, an exemplar case of the 'democratic peace hypothesis' that democracies don't go to war with each other. While the case

gained the status of an exemplary case for this thesis, it is the causal account or narrative explanation that Schultz gave which became the subject of further usage, analysis and discussion in the political science community. In a third example in sociology, William Foote Whyte's (1943) study of a slum community group contained studies of the relationship between an individual's performance in a group activity and their status within the group. He produced some anecdotal accounts of this relation, and prompted some contrived experiments within the community he studied. These observations and proposed explanations were taken up by sociologists and psychologists working on small group behaviour to create research designs for formal experimental situations studying that relationship.

An exemplary case study may well prompt several different kinds of uses. A good example is found in the use-value made of a case study analysis of firm exit from declining industries discussed by Charles Baden-Fuller (1989). That particular case account provided a 'narrative explanation' of the phenomenon that was extended to many further cases in other times and places: it proved flexible to extension (see Morgan, 2017). I revisit the case here to reflect on how it offered multiple use-values for the community.

Baden-Fuller studied the order of exit of firms within a declining industry in the context of policy issues about such declining industries. In this case (the steel castings industry in the UK in the 1980s), the order of exit was not as expected: the least profitable firms did not quit the industry first, and the order tended to be that the more profitable firm exited first. The explanatory account in this case became exemplary for the community because there was a strong commitment in economic theory, almost a base assumption or ideology, that such order of exit would always be 'efficient' (i.e. in order of profitability), and so, finding a case where this was not so, was a matter of considerable interest. So, first of all, the account was noticed because of the surprise factor - this was a case study whose findings went against the fundamental assumptions of the field. Second, it was one of the few pieces of empirical work on the topic. Exit happens regularly, but there are few empirical analyses of it, for it is a hard space to work in as the evidence easily disappears as firms exit and industries decline. Third, the explanation based on causal factors made sense within the economics community, it was acceptable within the context of industrial economics: the individual factors were not strange, it was their particular combination which proved salient. Fourth, the methods were normal within the community: statistical work, modelling, game theoretic considerations, and even interview (ethnographic) evidence. The findings were discussed first in a 1984 workshop and subsequently in a 1986 book and a paper (in a mainstream journal) which flagged this as a 'case' account: 'Exit from Declining Industries and the Case of Steel Castings' (Baden-Fuller, 1989). Tracing the paper citations into the places of use, it is clear that both analytical work and findings were explicitly used (not just cited) more than 50 times over the following 20 years in a variety of places. They were used in high-tech academic journals in economics, in practitioner management journals, in textbooks in game theory and in reports for policy units and policy discussion in the EU, UK, and Japan. It is important, however, to illuminate more carefully the process by which that explanatory account was used in those pieces of further research on the topic and so to examine how its use-value arose in those other contexts.

At no point did the community simply make a bold inference that because these findings were relevant to that particular case, they could therefore be used to explain the order of firm exit everywhere, or anywhere, else. But at the same time, the community did not take much interest in the specific time, place or industrial sector involved - these elements of the case were just not seen to be relevant. Rather, studying this paper trail shows exactly how the main elements of the case study were used. Whereas the case events might have been seen as abnormal in economic experience, and their explanation ingenious, the methods used to treat the problem were not. The surprise factor of the account opened up a set of questions for these other researchers to ask. On the

¹² Elgin refers here to the value of experiments in a comparison of exemplification in the arts and sciences in the context of a discussion of Nelson Goodman's views on art. It is not so clear that her further analysis of exemplification in the context of experiments is applicable here to case-based science.

one hand, these were empirical questions: does this strange pattern happen regularly, and does it occur with the same number of small causal factors in other situations? On the other hand, there were theoretical questions: how did this pattern work, how should the theory of exit be developed and how could one model this? And there were also policy issues - if this set of causal factors were involved, what would be a relevant policy for intervening in declining industries? In other words, the case account, or exemplary narrative, proved a catalyst prompting a host of new research questions and activities all investigating the exit phenomenon in different ways. Following the paper trail of citations, we can see that the case study account was used:

- * in support of general and more specific theoretical findings on the problem of efficiency of markets
- * as indicative for such theory development in a specific problem-solving mode
- * as a source of testable hypotheses for other possibly similar cases
- * as a source of variables for such statistical analyses
- * as evidence relevant for comparable situations in other times and places
- * as a rare set of empirical findings where few others existed
- * as evidence for policy interventions

That is, taken in all, the case account figured in a series of attempts to figure out the characteristics of the exit phenomenon and to give both descriptive and causal accounts of the process in empirical, theoretical, and policy domains.

Studying this case study account illustrates the range of use-values from the original account that were taken up in the community. Though few case studies may catalyse so many and such different forms of work, these observed uses would be very poorly described as those scientists making inferences from the case study unless we have a very broad sense of inference. This is why I suggest we think of the case study here acting as a *catalyst* for other researchers in extending their knowledge of the phenomenon of firm exit both narrowly and broadly conceived. Again, it is worth pointing out that any genre of scientific work could have this catalysing effect, so the claim is not specific to the use of case studies, but rather the point to note is that one case study can prompt so many further uses.

3.2. Case studies as crystallizers

It is surely evident that different kinds of scientific work can, from one particular piece of work, act to crystallize some important piece of knowledge in the field. Indeed, the literature on ‘crucial experiments’ suggests just such a function for experiments. Case studies that might be deemed ‘crucial’ are not likely to be determinative in the same way as experiments, but rather to crystallize some relatively abstract level of contribution which is taken by the community of researchers to offer an idea, concept, or notion that can be used in many other cases or contexts. This crystallizing insight can be at different levels, and be useful in different domains, and may well be associated (but not necessarily) with overturning some other taken-for-granted concepts or assumptions.

One of the most impressive examples of this crystallizing role is found by following the reception of William Foote Whyte's *Street Corner Society* (hereafter SCS) which was first published in 1943, with a brief preface explaining how he came to do the work. It was re-issued in 1955 with an extensive appendix (79 pages) on his experience of research; the study was issued again in 1981, with the final edition in 1993 to mark the 50 years from its first appearance. By the time of its 50th anniversary, over 200,000 copies of the book had been sold (which implies a considerable non-academic readership), and it has been translated into eight languages (including Japanese and Chinese). Both the study, and his appendix on his methods, received separate critical and laudatory attention.

The point that most of the reviews of the 1st edition of SCS in 1943

picked out, and several times quoted directly, was that slums were not disorganised places, but societies. As Whyte wrote in his original 1943 book preface: “It is customary for the sociologist to study the slum district in terms of ‘social disorganization’ and to neglect to see that an area such as Cornerville has a complex and well-established organization of its own” (SCS, p viii, original 1943 edition). Whyte saw this as perhaps the major contribution of his book for he flagged it carefully in his concluding chapter. Sociologists of that period apparently regarded slum communities in terms of their social problems, rather than as organised social systems. This is perhaps a hang-over of the middle-class activist and settlement-house root of American sociology - the settlement's role was to bring people into their own ‘better’ society rather than to live with communities as members of their society. But it was also a feature of academic and theoretical sociology of the time to claim that slum communities were not ‘societies’. It is perhaps difficult to imagine at this distance that one of the main imports of Whyte's observation of this community of street corner-boys in SCS was not the revelations about the life of those individuals who hung out on the street corners, nor about the relationship between these groups, the police and the illegal activities of the area and the local political figures, but that these individuals formed social groups that intersected with other social groups in ways which it was reasonable to label a ‘society’ - a social organization rather than the ‘disorganization’ that other researchers typically assumed.

This was a case study whose main concept was taken up in many places - it provided an exemplary narrative account of the nature of a certain kind of community to the extent that his label became the name for a kind of society. SCS's label refers not to the urban, teen gangs who are thought to thrive on aimless violence; it is not about the juvenile delinquents known to the settlement houses and the public authorities of the late 19th century as to their equivalents of today. The label refers to a slightly different and older group who make society in street life, those whose life takes place on the street because there is no-where else for them to be because of lack of money, jobs, or domestic space.¹³ Creating the terminology ‘street corner society’ for this form of community is rightly considered one of the major contributions to sociology of Whyte's study and proved to be a label highly flexible to extension. It is not quite a concept, not a theory, not an hypothesis, but it is something more than a simple description for it carries with it a set of characteristics of that society: a ‘conceptual label’ seems appropriate. It names a phenomenon, and so *crystallizes recognition* of a particular way of life which may be known casually, but not fully appreciated, and not known previously as a sociological kind. The term's aptness is recognised by it being - once pointed out - seen as known all along: the term has a fitness that leads to multiple and easy usage. For example, and in contrast, Ware's (1935) discussion of the “hang-around” groups in Greenwich, while organising the same characteristics of behaviour, lacks the implicit paradox of naming them as a society (and it was by no means the centrepiece of her findings as it was for Whyte).

‘Street corner society’ gained an immediate life as a conceptual term of art, and of reference. Extensive searches reveal no uses of the term in the social sciences and humanities journal literature before the book was published, and considerable usage afterwards (see Table 1). SCS is usually referenced with its author attached, and in those cases where it is not they are mainly addressed to audiences who would be presumed to know the reference. It remains pretty stable in use over the decades, including the extent to which it has been used outside the main field of sociology.

¹³ Once Whyte licenses the term ‘street corner society’, it did get used to refer to other kinds of groups, particularly younger gangs, practising less law-like behaviour, so that its meaning spreads out. SCS also escapes from both academic life and policy circles: it turns up as the name for a New York *a cappella* group in the 1960s, the name for a website devoted to ‘edgy’ Quakerism, and a description for those peopling Fassbinder's film *Katzelmacher* (see Thomas, 1976).

Table 1
Citations to *Street Corner Society* by decade.

Years	1943–50	1951–60	1961–70	1971–80	1981–90	1991–2000	2001–10
SCS	50	132	167	160	181	167	94
SCS only in Sociology	31	82	105	99	89	93	41
Cumulative Total SCS	50	182	349	509	690	857	953

Notes: JSTOR Database search shows by decade, the number of citations for the book title in row 1, and of those, the number in sociology journals (as defined by JSTOR) in row 2. The final row is the cumulative total of row 1.¹⁴

Much as the users of the exit case discussed above, and as suggested for SCS in the example of small group behaviour mentioned earlier, readers of SCS were sufficiently convinced by Whyte's explanations of some detailed events and phenomena in his case study to use those directly in their own work. And, as with the earlier exit case, these explanations and details were treated as freely detachable from the original case materials. What is different here is the acceptance and usage of the generic phenomenon of 'society' at the street corner, which had very considerable and wide scope, found and used not just in sociology and its sub-fields but in anthropology, law, criminology, education, ethnic studies, ethno-musicology, and so forth. While the degree of appropriation might imply a general inference move, the details of usage, both within and beyond the fields of sociology and social anthropology attest rather to the power of the conceptual label and description to organise or recognise similar kinds of street gangs where they are found. Thus, the label crystallizes the social science community's knowledge of a huge number of other cases and of their characteristics. Here we have the generic power of the label crystallizing knowledge in other cases, and acting as an umbrella to cover those cases, but does not imply that such groups will be found everywhere. This is in contrast to the label of the Industrial Revolution, in some ways a rather similar combination - a conceptual label with a set of characteristics - but one with relatively few attested cases, in which the label itself has a less clear and less stable set of characteristics, and yet where the original details are still taken to matter.

The crystallizing function of case studies may not necessarily produce a new label, it may, rather, suggest a new account of phenomena in situations where there is a degree of unsatisfactoriness in existing accounts and some emergent ideas about the phenomena. Thus, in the sociology domain, Kantor (1977) was instrumental in re-focussing the way sociologists conceived of the behaviour of men and women in work organisations. This seminal case account evidenced her claim that the discriminatory practices that kept women from progressing in corporate American organisations were due neither to prejudices held by men, nor to cultural attitudes which determined the respective behaviours of both women and men, but due to the structures within organisations which created behaviour patterns in how members of both groups behaved. Where the earlier existing accounts were in different ways unsatisfactory, this case study crystallized the notion that behaviour patterns were set by organisational structures, an idea soon recognised and taken up by other social scientists (see Morgan, 2017a). It located the problem away from the socio-psychology realm into the sociology of organisations to offer a different kind of explanatory framing in a way that gradually gained considerable power. Another case study account, which also cast a slower shadow compared to the immediate impact of SCS, is Davis et al.'s (1941) anthropological study of a Southern US community which showed that a society could involve both 'castes' and 'classes' - overturning social scientific beliefs of the day which assumed that a society would present one or other but not both kinds of stratification. This case study carefully delineated both the classes within white and black communities and the caste differences between white and black, and went further in delineating 'cliques' as another hierarchical set within societies. Both these two case study accounts provided the depth of evidence and analysis to convince social scientists that these new ideas and concepts held real salience elsewhere - that is, the exemplifying power of the case study changed the way scholars

thought about some phenomenon by offering an account which crystallized ideas that might be nascent, but not fully worked out, and which made sense of the particular terrain in ways that offered ideas of broader scope and worth relevant for many other sites.

Such a crystallizing function may be evident not in the academic domain but in the public domain. For an example going back to industrial economics, Baden-Fuller and Stopford's (1991) case study of washing machine manufacture in the 1980s investigated why there were still lots of national-based firms in Europe when it was widely assumed that efficiency considerations would mean there would be one large global manufacturer, supplying more cheaply because producing at larger-scale. It turned out that the reason for the continuation of smaller national firms was because different consumer groups had preferences for different designs of washing machines: Germans wanted high spin speed, French wanted top-loaders for small spaces, Italians wanted slow spin speeds to dry their washing in the sun, and so forth. This case study account was reported in the public domain, by *The Economist*, where it was taken as exemplary evidence for a larger claim that there could never be single European-wide dominant firms in these kinds of industries, that is, for the claim that the forces of globalization were limited by demand side factors. The case study account was then picked up and quoted in the UK Parliament as evidence that there never could be a single market in Europe, just at the time that the single market legislation was being debated. So, insight from the single case study account was first crystallized into a broad finding about the nature of globalization, and then its importance further expanded into a finding with political and policy implications about the nature of the European economy. The washing machine findings crystallized arguments about globalization and the European market, and here the case story remained in the discussion to exemplify the general argument (but not to prompt further research on washing machines!). So rather than an extension of the argument to other similar cases at the same level that was evident in the industry exit example, or the wide adoption of the nature of slum societies as in SCS, exemplification worked here by expanding the scope of the argument/explanation to a much broader canvas.

4. Conclusion

This analysis and these examples show how cases and case studies have independent use-values in social science communities.¹⁵ The analysis also points to the manifest ways social science communities work with cases and case studies to realise their potential use-values beyond the individual case or the original case study. While the means and modes of working with cases and case studies vary, and the knowledge accumulation possibilities also differ, it is characteristic that there are many ways of learning from cases and from case studies which go beyond making inferences, either specifically or generally, from the original findings. Indeed, the discussion here points to the many different exemplifying materials that case studies offer, and thus the very

¹⁴ I thank Judy Laffan for undertaking this search, and extensive further research work on which this case study of the impact of SCS is based.

¹⁵ There may also be hybrid objects in which the case and the study are both found usable independently.

diverse practices of inference associated with case-based modes of research. This in turn has filled in the ways in which case study findings can have extensions across other cases, increase their scope, and – especially in their crystallizing vein – initiate or contribute to the formation of new conceptual or abstract materials.

If ‘thinking in cases’ is conceived as an epistemic genre, the arguments here offer reasons for differentiating case-based thinking from model organism research and from experimental research. While the latter difference offers no real surprise for cases have all the qualities that make experimentation deeply problematic, the former difference depends on the bridge to further knowledge sites being framed as a similarity argument based on exemplification as opposed to on analogical reasoning.

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