Case study research:
How political science underestimates it, and places obstacles in its way

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ABSTRACT

Political case studies have unjustifiably acquired a reputation for being semi-anecdotal investigations of the small details of individual circumstances, research that is incapable of generating significant empirical or theoretical advances in knowledge. It is argued that the case study is, at best, a preliminary step, in that it may generate hypotheses that can later be tested using such "more reliable" methods as standardized questionnaires or statistical data. In the study of politics, however, that sequence of research initiatives may well work better in reverse.

When political action generates new policies, or creates new states of affairs, these changes invariably come complete with a set of justifications, with or without a claim that the justifications are founded in scientific investigation or social theory. Often, an effective way of testing such claims is to do a case study of the policy, or the changed state of affairs, enquiring into its causes and the effects it has produced, in order to test the validity of the original justification. A series of such case studies may, in turn, generate insights that are capable of producing theoretical advances, provided the case studies employ sound methodology. The paper looks at how we can mobilize the mountain of data we are likely to amass in the course of a case study in order to arrive at judgements, which necessarily involve an element of subjectivity, while offering some assurance that those judgements are not, in the end, simply a mirror image of our prejudices. The paper next turns to the problems posed by institutional ethics reviews, which, though well-intentioned, inhibit the kind of critical investigation that case study research requires, both by effectively legitimizing any efforts power-holders may make to conceal facts and obfuscate analysis, and by failing adequately to protect subordinates and ordinary people from reprisals for research findings power-holders do not like.

1 Introduction

Political science case studies are often represented as a means of undertaking preliminary explorations of a subject matter, through a careful examination of a particular instance. It is argued that the case study may generate hypotheses that can later be tested using "more reliable", quantitative methods. In these pages, I argue that that sequence of research initiatives may well work better in reverse, provided case studies are well executed and used to their full potential.

Typically, case studies are seen as handmaidens to the kind of conventional political science that seeks to emulate natural science. It appears as well that, the more deeply students of political methodology delve into them, the more limited their usefulness seems. King, Keohane and Verba (1994, 43-46), focussing on comparative case studies, judge their utility in terms of their ability to provide descriptive detail that will be useful in the discovery of causal mechanisms. (Ragin, 1994)

Eckstein's conditions for the utility of single case studies in social science enquiry are so demanding that an abridged version of an article entitled "Case study and theory in political science" takes more than 40 pages to reach the conclusion that a theoretically useful case study requires such "rigour of thought and exactitude of observation … [as to be] as yet, virtually non-existent" in political science. For King, Keohane and Verba as well as Ragin and Eckstein (2000), therefore, the value of case study research is measured by its contribution to the achievement of the kind of universal and invariable knowledge that is the goal of natural science.

Flyvbjerg disagrees. He not only rejects attempts to reduce case study research to an ancillary of conventional social science, he contends that case studies are the key to reviving the lost significance of social science research. That significance, he argues, has been sacrificed in the attempt to develop a version of social science that tries, but fails, to achieve the certainties of natural science. He draws on Aristotle to distinguish three intellectual virtues, each capable of producing a distinct kind of knowledge. The virtues are:

- *Episteme*, through which it is possible to achieve knowledge that is universal, invariable and remains true regardless of context.

- *Techne,* which provides knowledge that is oriented toward production. It is context-dependent, "[b]ased on practical, instrumental rationality, governed by a conscious goal." Its purpose is to evolve methods for the achievement of pragmatically defined goals. (Flyvbjerg, 2001, 57)

- *Phronesis,* deliberation about values with reference to praxis. The knowledge it produces is "pragmatic, variable, and context-dependent." (Flyvbjerg, 2001, 57)

It is achieved by careful examination of the details of actual social and political circumstances and events, using case study methods to get answers to the following questions:

- Where are we going?

- Who gains and who loses, by which mechanisms of power?

- Is the direction of events and policies desirable?

- What should be done?

The strength of social science, according to Flyvbjerg, is the achievement of phronetic knowledge that is context-dependent, capable of specifying desirable ends, as well as techne, which provides methods for the achievement of those objectives. In this article, I accept the methodology, and the relationship between social science research and praxis advocated by Flyvbjerg, but argue, in addition, that case study research can be a means of testing the validity of policy justifications and generating new theory.

When political action produces new policies, or creates new states of affairs, these changes invariably come complete with a set of justifications, with or without a claim that the justifications are founded in scientific investigation or well-established social theory. Often, a very effective way of testing such claims is to do a case study of the policy, or the changed state of affairs, and enquiring into its causes and the effects it has produced, in order to test the validity of the original justification, and the social science backing it. A series of such case studies may, in turn, generate insights that are capable of producing theoretical advances.

For example, a colleague and I undertook a series of studies of downtown redevelopment projects in Canadian cities, projects that were financed in part by the federal government. Our studies were designed as a means of testing the validity of the justifications for those projects. Upon reviewing the results of our research, however, it dawned on us that we had bigger fish to fry than a mere series of critical examinations of development projects.

Taken together, the projects represented a change in the way the Canadian federal government approached its urban policy, and led us to a publication in which we were able to call some previously well-established theoretical propositions into question. (Leo and Fenton, 1990) Our findings attracted little attention – undoubtedly we lacked the wit to exploit our insight to its full potential – but in retrospect it is clear that they pointed in the direction of what are now known as multilevel governance studies. Central to our findings was the observation that, over time, urban policies of the federal government had become less uniform, and more pitched to the individual circumstances of different communities. This has become a central theme in discussions of multilevel governance and subsidiarity.

Case studies, therefore, can be a great deal more than semi-anecdotal accounts of particular instances, interesting only to those who care about those particular circumstances. They can be a tool for testing received wisdoms, and they may even generate new theory, provided the case studies serve as an effective test of the truth. But what is the truth? How can we formulate a methodology for case studies? In these pages, I will look at the question of what it takes to make case studies an effective tool of critical investigation. The discussion of that question, in turn, generates a series of concerns about research ethics protocols, which, I argue, run counter to the requirements, not only of good case study research, but also of case study research ethics.

2 "The truth"

Social scientists using standardized questionnaires or gathering statistical data can often arrive at robust findings without giving explicit thought to methodology, because the methodological foundations of their research have been thought through by others and are implicit in the method they use. The matter stands differently for researchers undertaking case studies.

Good case study research does not necessarily rely on a particular set of standardized sources of data. Case studies may or may not draw on a selection of secondary sources; official documents; such informal documentary sources as personal letters, interviews or conversations; direct observation, and participant observation. Some of these sources may be eclectic, and the question of how to weight each piece of information is always a matter of judgement. (Gorden, 1987; Lofland, 1995; Stake, 1995; Yin 2003)

The problem of evaluating multiple sources is magnified when government policies, and the decisions that produced them, are under investigation. Power-holders may not be keen on providing accurate accounts of their actions and the actions that have been taken in their name, especially when their policies produce unintended outcomes – or, for that matter, intended, but controversial, outcomes. Moreover, they may well exercise personal control over at least some of the documentary evidence of their actions. All the more reason to seek out multiple sources of evidence.

On the other hand, multiple sources and their evaluation place a heavy responsibility on the researcher to try to ensure that his selection of evidence is not simply a reflection of his biases. When sources are both rich and disparate, the only defence against subjective, self-indulgent findings is an epistemology: a systematic and critical approach to the establishment of validity and significance amidst what would otherwise remain a jumble of facts, lies, half-truths, allegations and impressions. So how do we know the truth, or even what we mean by "the truth"?

Although the answers to such questions are always open to dispute, many sociologists and students of related disciplines would agree that truth is not a

shining gem we need only find in order to possess it. Rather, it is constructed. In understanding the degree to which our knowledge, so-called, is constructed rather than found, Stake (1995, 100-01) suggests we conceive of ourselves as living with three realities:

- "…an external reality capable of stimulating us in multiple ways, but of which we know nothing other than our interpretations of those stimuli."

- Experiential reality: Our understanding of the meaning of those simple stimuli, which is entirely our construction, cobbled together from a variety of sources.

- Rational reality: "A universe of integrated interpretations", a universe that is obviously rife with sectarian and ideological conflicts and contradictory interpretations, with each person, in the end, choosing her own version of rational reality.

A crude interpretation of Stake's conception might lead to the conclusion that there are no facts, only constructed knowledge. (Denzin, 1992) That might be a reasonable formulation if the external stimuli each person experiences were idiosyncratic, but they are not – at least not in the context of the typical politics or policy case study. The bulk of the material we assemble when we do a case study consists of fragments of information that, as we know from experience, will be understood similarly by others. A particular downtown development has been completed: Its physical features can be described, some aspects of its economic impact can be measured, what company was responsible for the development, and who voted for and against it – all of this and more can be determined in a manner that is not subject to the vagaries of individual interpretation. I will refer to such data as simple facts.[1]

But simple facts are unlikely to be the focus of a researcher's interest. The findings we are usually more interested in are both more difficult to arrive at and more important to understand: What individuals and groups benefited from the development, whose interests were adversely affected, how was support for it organized, what opposition had to be overcome, what happened as a result of it?

Answering such questions involves an investigation of the causal chain leading to the development, the context within which it took place, the outcomes it produced and ultimately its political, economic or social significance. I will refer to such findings as causal, contextual and analytical. All such findings are constructed knowledge. There is no way to arrive at them without judgement calls, and therefore without an element of subjectivity.

The question we face, therefore, is this: How can we mobilize the mountain of data we are likely to amass in the course of a case study in order to arrive at

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[1] S.I. Hayakawa (1978, 33) dubs these "reports".

those judgements, while offering some assurance that our judgements are not, in the end, simply a mirror image of our prejudices?

My answer, and that of a number of other researchers who have considered these questions, is triangulation (Patton, 2002, 247-49, 555-63; Gorden, 1987, 12; Stake, 1995, 107-20; Yin, 2003, 97-101; Axinn and Pearce, 2006): seeking confirmation of all simple facts, as well as causal, contextual and analytical knowledge in more than one source. If we are told something in an interview, look for confirmation in documents; if a document discloses something interesting, interview people who were there at the time; if a secondary source makes a statement, look for confirmation on the ground, or in documents; and so forth.

The specific methods used to implement triangulation vary. Stake (1995) suggests putting a small triangle in the margins of our notes wherever a fact needs to be checked by triangulation. The trouble with this approach is that it seems to be aimed primarily at ensuring the accuracy of simple facts, and appears to court the danger of missing causal, contextual and analytical knowledge. It is possible, for example, to get a sequential series of discrete facts right, and still miss the significance of the sequence itself. Edmonton's Eaton Centre, discussed below, is a case in point.

Stake also recommends "member checking" – letting some of the subjects of the study read portions of a draft and check it for "accuracy and palatability." (Stake, 1995, 115) This procedure embodies a pitfall, especially the "palatability" check. Stake recognizes the pitfall when he rightly points to the importance of not promising that the subject's version will appear in the final draft. This is a central point, especially for political studies, where independent criticism of how power is exercised may be the very rationale for the research. Far too much so-called social science research consists of anodyne formulations designed, not to represent the researcher's best approximation of the truth, but to offend no one, especially power holders. I return to this subject in my discussion of case study research ethics below.

My approach to triangulation is not exactly the same as either of Stake's two procedures. My method involves on-going interrogation of the evidence. As I proceed with my research, I engage in an internal dialogue that involves writing hypothetical conclusions in my head and interrogating them as well as the facts and causal, contextual and analytical knowledge that might lead to them, or point in the direction of an alternative hypothesis. Both examples dealt with below, especially the case of Kenya's Million-Acre Settlement Scheme, illustrate how this works in practice.

3   Two examples of triangulation: Personal narratives

In this section, I draw on my research experience to offer two concrete examples of how triangulation works in case study research. In a tribute to my chequered

career, they involve, respectively, African agrarian politics and Canadian urban development politics.

3.1 Kenya's Million-Acre Settlement Scheme

The Million-Acre Scheme was a giant development project, in which a large proportion of land previously occupied by European settlers was subdivided and conveyed to African smallholders. As I cooled my heels in Nairobi, Kenya's capital, waiting for research clearance from the Kenya government, I read everything I could get my hands on about Kenya's recent history, its agriculture and rural society, in order to gain an understanding of the research I was embarking upon.

I gathered from my reading that, although there were two types of settlement schemes, high-density and low-density, the differences between them were relatively minor. Africans in need of land, I understood from the readings, had settled all of them. The high-density schemes had been less successful than the low-density, but – beyond the fact that low-density settlers made a down-payment on their land, while high-density settlers did not – the reasons were not altogether clear. A colonial agriculturalist, writing about African agriculture, explained them in terms of the fable of the grasshopper and the ant. Some Africans, in his opinion, were simply harder-working and more prudent than others.

Once I got my research clearance, I was in a position to do my own work on the settlement scheme. The heart of my planned research was two sets of interviews with large random samples of the farmers in one high-density and one low-density settlement scheme. As I began to talk to people, it quickly became evident that the differences between the two schemes, and the people who got the land, were dramatic. A preponderance of the low-density settlers were upwardly mobile: absentee landlords in many cases and often owners of multiple pieces of land or businesses in different parts of the country.

The high-density settlers, by contrast, were mostly poor. All settlers, high and low-density, were required to pay for their land in instalments, and although this clearly posed no problems for the low-density settlers, many of their high-density counterparts were scratching a living from manifestly inferior soil, and struggling, or failing, to meet the payments. The differences between the two sets of settlers were manifestly significant, despite claims to the contrary in the literature. The literature said one thing and the farmers, as well as evidence on the ground, were telling me something very different. What to do?

My puzzlement drove me to the Kenya National Archives in search of documentary evidence of who these two types of settlers were and how they had come to the land they occupied. The archives helped me to solve the puzzle. The solution is complicated, but in doing my two sets of interviews, I had stumbled upon Kenya's rural class system, and found the key to a fascinating and

disturbing story of colonial duplicity and manipulation. My discovery, and the follow-up work in the archives, transformed my study from a development politics case into a study of the evolution of Kenya's agrarian class system through the colonial and post-colonial eras. (Leo, 1984)

The farmers and the archives told different aspects of the same story. My archival sources confirmed the observations I made on the ground and in interviews. The history I was able to read out of them confirmed that the two types of settlement schemes did indeed serve two different social classes and showed that the Million-Acre Scheme served the dual objectives of settling landless people – thereby avoiding the danger of civil unrest – while ensuring that it would be African smallholders, and not the British treasury, that would bear the primary burden of compensating European settlers who wished to sell their farms and leave the country with the advent of majority rule. Included in the land that was being unloaded on landless Africans were significant acreages that were marginal, or altogether non-arable.

3.2 Lessons for case study research

In short, the archives and the interview findings agreed that the secondary sources from which I gained my initial impressions were wrong. Triangulation enabled me to unknot a tangled tale. More significantly from the viewpoint of political economy, my study offered insights into both colonial agrarian classes and the political dynamics of the transition from colonialism to neo-colonialism. Observers of Zimbabwean politics, for example, cannot fail to see parallels, both with Kenya's agrarian class system and in the character of the transition to independence. In the case of Zimbabwe, as in that of Kenya, the settlement of the land question at independence left the British treasury free and clear of any obligations to the Europeans who had settled there at the invitation of British colonial authorities.

In the Kenya case, secondary sources proved, on examination, to be less than reliable, while interviews, backed by on-the-ground observations and archival evidence, offered a more consistent, and ultimately more believable, account of the causal chain. More commonly, while interviews are helpful in understanding the context of and background to a case, it is documents and secondary sources that offer the most convincing account of the how and why of a case.

That is how it was in my other example, my study of the development of Edmonton's Eaton Centre. In this case, as in that of the Million-Acre Settlement Scheme, the research involved arbitrating between competing versions of "the truth". But in Edmonton, the significance emerged, less from a determination of who had the facts and analysis right, than from the pattern that surfaced when I followed a sequence of events.

3.3 Edmonton's Eaton Centre

In the 1970s, downtown Edmonton was the retail centre of the metropolitan area, and the city had a policy of sustaining that role by supporting the viability of residential neighbourhoods near the centre of the city and placing limits on the amount of permitted suburban shopping centre development. That policy was forgotten when the Triple Five Corporation offered to develop the West Edmonton Mall, then the largest shopping centre in the world. (Leo, 1994, 1995)

The development of massive amounts of new suburban retail floor space, accompanied by free parking and such attractions as a wave pool and a carnival ride, dealt a crushing blow to the downtown and in the 1980s empty buildings sprouted. As the city government desperately sought some way to restore life to the city centre, the same Triple Five Corporation that had developed the West Edmonton Mall offered a solution to the problem its mall had created, the development of a downtown mall, to be called the Eaton Centre.

The key decisions concerning the Eaton Centre development were taken during two rounds of negotiations, the first taking place in 1980 and the second in 1985-86. The simple facts of the matter were that the negotiations were difficult, because financing proved to be a challenge, and because many of the details of a succession of changing development proposals proved to be politically controversial. But by following the sequence of the negotiations through government documents, interviews with politicians and public servants, and newspaper archives, I was able to discern a pattern in the Triple Five Corporation's approach to dealings with the city. I named this pattern the bait-and-switch after a well-known sales trickery technique.

In the Eaton Centre case, the bait-and-switch involved making an irresistible offer to obtain a massive government commitment and then using local politicians' commitment to keep them on-side, even as the more attractive features of the original offer were withdrawn, and its price increased. In the 1980 negotiations, Triple Five Corporation, in partnership with T. Eaton Co Ltd of Toronto, a department store chain, announced plans for a massive, $500 m residential and commercial development consisting of an Eaton's department store, a 31,500-square-metre shopping mall, three office towers of 39 to 40 storeys and two residential towers of 51 and 52 storeys, with 1,236 one- and two-bedroom rental or condominium units.

The development, taking in most of two square blocks of prime downtown land, would boast a roof-top restaurant and gardens and the residential part of the development would include a recreation centre with a gymnasium, swimming pool, exercise room, handball and squash courts and a social room. The Eaton's store was to be the second largest in western Canada, after the downtown Vancouver store.

For Edmonton City Council, the attractions were virtually irresistible: a massive boost to the economy of the inner city, including both commercial and retail elements, together with a formidable increase in housing to help rally the eroding inner city housing sector. A development agreement was signed in October 1980.

The bait was in place. Next came the switch. In December, Nader Ghermezian, managing director of Triple Five, appeared at a council meeting to demand a re-opening of the agreement and the addition of a series of concessions. He warned that if the concessions were not forthcoming that day, the entire project would be cancelled. He had a letter from a solicitor for the Triple Five Partner, T. Eaton, which was said to confirm the urgency of the need for concessions, but which only Mayor Cecil J. Purves and two councillors were allowed to see.

Among the demands was cancellation of a redevelopment levy that the developer was to pay, and of the plans for a roof top restaurant as well as agreement by the city to fund sidewalks and setbacks for the project and to relieve the developer of the costs of leaseholds covering encroachments upon city property. Estimates of the cost of these concessions ranged from $5 m to $15 m. City council, galvanized by the impending collapse of such a large project, agreed to the concessions. Enquiries by journalists later established that the letter from an Eaton's lawyer had been a formality, designed to protect Eaton's position in case of a break-down in negotiations, and had not been intended as a sign of Eaton's dissatisfaction with the terms they had received, terms with which they in fact declared themselves satisfied.

But the unkindest cut was yet to come. Nine months later, Eaton backed out of the deal despite the concessions, still denying it had sought them. In other words, the city had granted concessions, which it remained obligated to deliver, even as the rationale for them became moot.

With Eaton out of the picture, the development ground to a halt, but in time the bait and switch resumed. In 1983, a promised revival of Eaton Centre failed to materialize once an expansion of the West Edmonton Mall had been secured. In 1985, once again the project reappeared. Eaton declared it could proceed if the city offered further concessions and the negotiations resumed. In the course of those negotiations, the project changed substantially, first becoming grander in the "bait" phase of the negotiations and then contracting again in the "switch" phase, as final agreement neared.

In August, for example, the project's rhetorical status was elevated from the mediocrity of second place in western Canada to the pre-eminence of world renown. According to the *Edmonton Journal,* it was touted as including "a major recreation centre with tennis, racquetball and squash courts, an Olympic-size pool, diving tank, indoor jogging track and gymnasium for aerobics... There would be 20 theatres, a 3000-stall parkade, and more than 45,000 square metres of department store and retail space [in place of the 31,500 mooted earlier]. 'This

will be the strongest magnet in the Province of Alberta,' Triple Five's Ghermezian said... 'It will attract tourists from all over the world...'" The two apartment buildings previously promised had been transformed into a 40-storey hotel-apartment. There would be 2,000 apartments [in place of the earlier 1,236 units], and 300 hotel rooms.

By late January, 1986, with negotiations well along but not complete, the project had lost some of that sheen, with more profitable parts of the project expanding while less profitable elements contracted. It was slightly bigger overall than before (3.9 m sq ft compared with 3.85 m sq ft), but the residential component had been almost halved, from 2.276 m sq ft to 1.269 m sq ft, while the office tower component increased from 850,000 sq ft to 1.536 m sq ft and additional retail space was added.

Then the pressure was cranked up. Eaton's had said it would commit to the project provided excavation started by May 1st. In late February, with the development agreement not yet ready, city council was being asked to approve an excavation agreement in order "to maintain the timetables established by the partners in the project..." Council was becoming more and more deeply committed to the project without yet having had a chance to read the fine print.

Meanwhile, under a new Mayor, Laurence Decore, the city had committed itself to its own plan for the revival of the city's commercial heart. One of the key elements of its plan was the 102nd St Arcade, a glassed-in mall that would have cut through the centre of the Eaton development. Triple Five was not prepared to make provision for the arcade. Mayor Decore and others argued that Council was too willing to take Triple Five's claims at face value, that competing bids should be solicited for the development of the Eaton Centre project, that the project should be required to accommodate the 102nd St Arcade, and that the developer should be obligated to include actual housing, as opposed to promises of future housing, in the development.

As negotiations drew to a close, the main issues were the inclusion of residential units, provision for the 102nd St Mall, and the financial concessions demanded by Triple Five. Planners estimated the total cost of concessions at $30.4 m. In May, in a vote that overrode Mayor Decore and his supporters, Council agreed to the concessions, without guarantees of a residential component and without provision for the 102nd St Mall.

A review of the deal by Edmonton's Auditor-General concluded that the city was the loser. Projecting the financial consequences 40 years into the future. He stated that "...the Eaton Centre package... does not result in a positive cash flow to the city until approximately the year 2004. The net present value of this concessions package for the 40-year period is negative."

3.4 More lessons for case study research

Both of these cases, summarized briefly here, illustrate that case studies can be exhausting, demanding persistence in ferreting out a large body of fact, constructing causal connections and analysing their significance. At the same time, they offer a satisfaction similar to that of solving a complex puzzle. The interest that sustains such a venture is that of constructing hypotheses, seeing if the evidence supports them and, if it does not, considering what alternative conclusions *are* supported by it.

In some cases such an investigation, in the end, may turn up nothing more significant than a detailed understanding of a particular instance. But, more often than not, in my experience, a pattern, once recognized, turns out to be more than a one-time occurrence. Most long-time observers of development politics in North American cities would probably be able to confirm that they have seen more than a few instances in which developers have used tactics similar to those employed by Edmonton's Triple Five Corporation in seeking public money for development ventures. In practical politics, early recognition of such a pattern could support better decision-making.

However, the kind of critical investigation of political and administrative action that is integral to good case study research is under threat from an unlikely source: Ethics protocols. In the next section, I take a look at that problem, and consider possible solutions.

4 Research ethics

In more than 35 years of academic research I have sought information from thousands of people, and done hundreds of interviews. During that time, ethical concerns, regarding both the substance of my research and my dealings with informants and respondents, have always been top-of-mind. The concerns I raise here are not with research ethics as such, but with bureaucracies that have gone awry in well-intentioned but misguided efforts to supervise research in politics and public policy.

In my experience - which antedates ethics bureaucracies by many years - two ethical concerns have stood out. One is my obligation to examine the way power is wielded, and, where possible, look for ways of addressing shortcomings. Generally, I have found the causes of problems I identify in the course of my research to be systemic rather than personal, and I have never found it particularly useful to look for individuals to blame, but my findings do not always reflect well on individual power-holders. However, the primary ethical concern is not their sensitivities. It is the public interest in good policy. It is a bedrock democratic principle that people in positions of power must be accountable for their actions.

Power-holders sometimes deal harshly with their critics, but that is a risk academic researchers must run, and our risk is limited by the institution of tenure, which is intended to embody a guarantee that we may report our findings truthfully to the best of our ability without fear of losing our academic appointments. A major social purpose of tenure, therefore, is to ensure that issues can be debated freely despite the possibility of hostile reactions. Few things are more important to a democratic society than access to honestly critical accounts of how power is wielded. It follows that it is the ethical obligation of academics is to report fearlessly and as accurately as they can. If we do not do that, we invalidate a major rationale for tenure.

My research, however, has not limited me to dealings with people in power. Over the years, I have acquired a great deal of valuable information from middle- or lower-level public servants, as well as hundreds of ordinary people. Here my primary ethical concern has been the protection of my informants from any possibility of reprisals. I have tried my best to ensure that nothing in my publications identifies ordinary individuals who might be subject to reprisals.

Enter the ethics bureaucracies. In Canada, their job is to implement the Tri-Council Policy, so named because the Canadian system of research ethics is based on a document jointly endorsed by the three funding agencies responsible for social science and humanities, natural science, and medical science research. There is a national supervisory apparatus, but the actual enforcement of the guidelines upon individual researchers is in the hands of departmental and university-wide ethics committees in each university. Individual researchers are required to file an explanation of their research project and tick off a set of assurances regarding their treatment of "research subjects". I will restrict my comments to the Canadian system, though most, if not all, would apply to similar systems in other countries.

I am in favour of the formulation of a set of ethics guidelines and a system of sanctions backing them. My concern is with the content of the Tri-Council guidelines, which actually militate against a social science researcher's obligation to look critically at the exercise of power, and, at the same time, deal inadequately with the potential plight of informants who might be subject to reprisals from their superiors. I am also concerned about the way inappropriate scientific practices are introduced by ethics committees in individual universities, as they exercise their power to interpret the guidelines. I will deal with each of these concerns separately.

4.1 Obstacles to critical scrutiny

When the Tri-Council guidelines were first introduced, researchers were not required to fill out an ethics application if their topic was limited to an examination of policy, how it works, and how it is implemented. Clearance became necessary only if the researcher initiated a line of enquiry that touched personal matters. Today, however, the Tri-Council guidelines appear to require a researcher to file

an application for investigation of policy, as well as enquiry into people's personal lives.

Many researchers concerned with politics and policy stay in regular touch with politicians and public servants and, in the process, ask them questions the answers to which may well be used in future publications. That is an essential part of the research process because regular contact with well-informed people makes it possible for researchers to stay abreast of events and identify important issues as they arise.

So when does a query become a research question and a conversation an interview that requires ethics review? The guidelines are little help in answering that question, but, if we take them literally, they would appear to have taken from university researchers a right that every ordinary citizen enjoys, namely that of picking up the phone and talking to a politician or public servant without applying for bureaucratic permission to do so.

In practice, of course, no one knows whether I am talking to a politician, and no one cares. The problem is not that I will in fact be prevented from doing so. Rather it is that the existence of such severe strictures on ordinary enquiries sends a message to both researchers and public servants that the process of what was once free inquiry is now subject to bureaucratic control. Researchers are more likely to stick to "safe" topics, and public servants will feel emboldened to try to shut down criticism.

I have personally experienced a case in which a Manitoba public servant thought that the process of ethics review gave her the right to censor a manuscript I was in the process of preparing. I am a senior professor with tenure and my response was a flat refusal, but were I less experienced, more junior, possibly still vying for tenure, who knows what my reaction might have been?

Ethics guidelines should not be written in such a way as to inhibit inquiry. Rather, they should underline a university researcher's right and obligation to engage in vigorous inquiry and factual, careful, but fearless critique. Rather than forcing researchers to apply for permission to do their work, it would make a great deal more sense to spell out clearly what they must and must not do – as I am doing in these pages – and put in place an enforcement mechanism with teeth.

4.2 Protection of informants from reprisals

Top public servants and politicians are well accustomed to daily cut-and-thrust with newspaper reporters and political and bureaucratic rivals. It is obvious that they understand their rights and know how to protect themselves, yet ethics guidelines require researchers to treat both power-holders and subordinates as if they were vulnerable children. Ethics applications require the researcher to promise to begin all interviews with a reminder to the interviewee that she need not answer questions and may terminate the interview at any time.

In the best of circumstances, such procedures insult the intelligence of power-holders and make the researchers look like fools. But the most serious shortcoming of these regulations is that they make no distinction between superiors and subordinates. Just as researchers should be entirely free to speak to top public servants and politicians without restrictions, as any citizen is, so should they proceed under a strong mandate to protect subordinate informants from reprisals. It is not enough, and not even helpful, to go through the ritual of telling public servants what even the lowest-ranking of them knows perfectly well, namely that they cannot be compelled to answer questions.

Their problem is that they cannot be certain which of their statements might prove damaging to them without knowing the details of how those statements will be contextualized in a future publication. The onus of protecting subordinates from reprisals belongs, not to the subordinate, but to the researcher. If an employee in a vulnerable position helps me to get at the truth, it ought to be my obligation, not hers, to ensure that my publication does not expose her to reprisals. The Tri-Council guidelines appear to have been written in blissful ignorance of the distinction between superiors and subordinates in government offices.

4.3 Bad science

As a sometime member of the Politics Ethics Committee at my university, and a former member of a university-wide ethics committee, I have seen quite a few ethics applications, and those applications have made it clear to me that the strictures placed upon researchers are growing far beyond those in the Tri-Council guidelines, through self-censorship, as applicants impose limits upon themselves in an effort to forestall any possible future repercussions. In the process, concern with proper procedures of scientific investigation are taking a back seat. In particular, I have two concerns about items that I have seen repeatedly in ethics applications.

The first item is a promise to allow respondents to look at drafts of the research and to take their comments on board before doing a final draft. This is quite simply bad science. Scientific findings should be the researcher's best approximation of the truth, not a compromise document that offends no one. To be sure, it is reasonable to let people who have helped us with our research take a look at our findings, and correct any factual errors that may have slipped in. But it remains the researcher's responsibility to distinguish between verifiable correction of facts and "cleaning up the record" to make someone look better.

If we are genuinely concerned with scholarly ethics, ethics applications should contain no reference to anyone besides the researcher having a say in the final draft. It is university researchers who enjoy the protection of tenure, and it is they, and they alone, who should bear the responsibility that goes with it. Like Harry Truman, we ought to have a sign on our desk that says, "The buck stops here."

My second concern is an item that appears routinely in applications, though I have seen no reference to it in the Tri-Council guidelines: A promise to destroy data after five years. This is another instance of bad science. A fundamental of the scientific process is verifiability and falsifiability of data, and replicability of research. Data that have been destroyed can neither be verified nor falsified.

Records that contain personal information which can be linked to individuals who are not in the public arena should either not be kept in the first place, even for a month, let alone five years, or be kept under lock and key. If records are properly kept, there can be no valid reason for destroying them. On the contrary, they must be kept, so that future researchers will be able to verify them independently of the original data-gatherers.

Nothing is more important to social science research than ethics, but the guidelines that govern Canadian politics and public policy research inadvertently do more to undermine research ethics than to support them.

5 Summary and conclusions

Case study research has unjustifiably acquired a reputation for being semi-anecdotal investigation of the small details of individual circumstances, research that is incapable of generating significant empirical or theoretical advances in knowledge. I have tried to show, on the contrary, that case studies can serve as an indispensable tool for testing the validity of claims made for policy initiatives and political decisions, as well as the science on which they are based, and occasionally for the generation of theoretically significant insights.

In order to achieve such results, however, case study research must adopt methods to ensure that the many personal judgements that are a necessary feature of case study research do not reduce the research findings to a series of confirmations of the researcher's pre-conceived ideas. I argue the case for a method that begins by recognizing that knowledge is constructed; proceeds by drawing a distinction between simple facts on one hand and causal, contextual and analytical findings on the other, and then generates conclusions through on-going interrogation of the evidence, formulation of hypotheses, and testing of the hypotheses.

These methods can only work if researchers are free to use them. Unfortunately Tri-Council ethics protocols throw up significant obstacles. The discussion then turns to research ethics protocols which, I argue, place barriers in the way of the kind of critical investigation that case study research requires, both by effectively legitimizing any efforts power-holders may make to conceal facts and obfuscate analysis, and by failing adequately to protect subordinates and ordinary people from reprisals for research findings power-holders do not like.

Case study research can play an important role, both in advancing scholarship, and in supporting intelligent political action and policy-making. But its efficacy is

contingent upon the adoption of appropriate methods. As well, it is badly in need of protection from the intrusions of ethics bureaucracies, which, however effectively they may serve to protect vulnerable subjects from researchers, fail to protect the public, researchers, subordinates and ordinary people from mistakes and abuses in the exercise of power.

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