

# That Noble Science of Politics

*Reflections on the use of qualitative approaches in  
political science*

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## 1. Introduction

*... that noble Science of Politics, ... which, of all sciences, is the most important to the welfare of nations, – which, of all sciences, most tends to expand and invigorate the mind, – which draws nutriment and ornament from every part of philosophy and literature, and dispenses, in return, nutriment and ornament to all.*

*-- Thomas Babington Macaulay<sup>1</sup>*

*Ebenso wie alle anderen Wissenschaften, so sind auch die Sozialwissenschaften erfolgreich oder erfolglos, interessant oder schal, fruchtbar oder unfruchtbar, in genauem Verhältnis zu der Bedeutung oder dem Interesse der Probleme, um die es sich handelt; und natürlich auch in genauem Verhältnis zur Ehrlichkeit, Gradlinigkeit und Einfachheit, mit der diese Probleme angegriffen werden.*

*-- Karl Popper<sup>2</sup>*

It is not uncommon to assume a connection to exist between a political scientist's positions or opinions in metaphysics and epistemology, and her commitment to either quantitative or qualitative research strategies (cf. e.g. March and Furlong 2002; Creswell 2003). Specifically, the conventional wisdom states that adherents of metaphysical realism and epistemological 'positivism' will tend to choose quantitative approaches in the study of politics. This traditional perspective also claims that qualitative research methods are correlated with metaphysical antirealism or what is sometimes known as 'social constructivism'. The basic aim of this paper is to present an alternative to this orthodoxy, and to impart what might be described as a critical perspective on the scientific study of politics and society. Under this

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<sup>1</sup> Quoted from Collini et al. (1983:v).

<sup>2</sup> (1969:105).

perspective, the choice of method is viewed as a decision which should be based in the nature of the problem to be studied, and not as a choice dependent on such metaphysical and epistemological affinities.

‘Metaphysical realism’ is perhaps an elusive concept, but with it I mean to describe the belief that there exists an external reality independently of our mental states (cf. e.g. Malnes 1997:20-34; Ferraiolo 2001). ‘Positivism’ is used in the colloquial and rather misleading sense that abounds in much of the literature, including some of the literature for this course. Among other theories, the logical empiricism of the Vienna Circle (cf. e.g. Wiener Kreis 1929; Neurath 1931) and the critical rationalism of Karl Popper (e.g. 1935; 1969) and his students (cf. e.g. Lakatos 1978; Skagestad 1980; Newton-Smith 1981; Magee 1985) are generally both subsumed under this heading. What these theories have in common, and this is the one feature which makes them ‘positivistic’ to their critics, is the belief that our theories could, and should, be judged by their agreement or correspondence with a reality deemed to exist independently of and prior to our convictions. Rather than being a fruitful tool for analysis, the concept of ‘positivism’ could be seen as having developed into a multifarious term for all epistemological theories associated with metaphysical realism, in some quarters seemingly used as a generic term of deprecation.

Likewise, I understand social constructivism, or simply constructivism, to be a particular kind of metaphysical antirealism. Like other antirealisms, it states that an external reality does not exist independently of our mental states. Constructivism is, therefore, the view that reality, or at least parts of it, is created by socially constructed beliefs and attitudes (cf. e.g. Hacking 1999). To put it in the words of Roxanne L. Doty (1993:303, author’s own emphasis): “Policy makers also function within a discursive space that imposes meanings on their world and thus *creates* reality”. This sentence seemingly states that different people live in different ‘worlds’, and that they, or at least the linguistic practices or ‘discourses’ in which they take part, modify or even bring into existence the reality in which they dwell. The natural consequence of constructivism is epistemological relativism. If reality does not exist independently

of our convictions and creative processes, then correspondence with ‘reality’ cannot function as an arbiter between competing theories or beliefs. The objective of research is therefore not to arrive at the truth, as the realist or ‘positivist’ would hold, but other goals such as ‘understanding’ – presumably of other ‘realities’ than one’s own – or just personal satisfaction.

Furthermore, I denote as *quantitative* those research techniques which produce data in the form of numbers or quantities, often associated with statistical and experimental modes of examination. Commonly, quantitative approaches attempt to study a larger group of phenomena, in order to *explain* common traits and behaviour. Conversely, *qualitative* techniques produce data in the form of texts or narratives describing the texture or qualities of a given phenomenon. Qualitative approaches include open-ended interviews and modes of observation, usually done in order to *understand* the conduct of one or a few entities (cf. e.g. von Wright 1971). While it could be argued that all successful research use data both in the form of numbers and narratives, and that it is therefore both ‘qualitative’ and ‘quantitative’, I nevertheless grant that this divide between types of research is a time-honoured analytical device. It should however be noted that these concepts have a clearly limited potential as fruitful classifiers of social research. A more fortunate manoeuvre would most likely be to separate instead between intensive studies with a descriptive end and nomological research with a generalising aspiration, or simply between ‘case studies’ and various statistical approaches (cf. Yin 2003).

In this paper, I present the view that both qualitative and quantitative approaches should be a part of the prudent political scientist’s arsenal of methods. In particular, I intend to show that the political scientist informed by metaphysical realism can not and should not restrict herself to quantitative strategies. Employing Karl Popper’s critical rationalism as a theoretical starting point, I argue that all techniques which potentially could contribute to the refutation of a theory are of immense importance to the scientific community. Qualitative research strategies, useful and informative as they are, must not become the exclusive property of

constructivism and other forms of metaphysical antirealism. It should instead be reinstated as *scientific* research whose proper intention it is to move the scientific community and the world at large from ignorance to knowledge (Popper 1969; 2002).

The immediate background for writing this paper is my growing impatience with political scientists who restrict themselves to a set of methods and problems which is smaller than it could have been. This they do in order to confer their allegiance to a particular research strategy, either qualitative or quantitative, or a theory of knowledge, either informed by realism or constructivism. The paper reflects in this sense a normative stance, namely the view that political science should be the scientific study of politics, distinguished from the other sciences not by a particular method or adherence to any specific theories but by its object of study. Any technique or method which might illuminate the political process or further enhance our knowledge of politics should therefore not be excluded from the scientific study of politics, at least not from the outset.

The concept of political *science* suggests a resemblance between the activities and practices that goes on under this heading and the things that take place in the natural sciences. It is however apparent that the subject matter of political science dictates different research approaches than the ones found in natural science. Just as there are differences in methodology between the natural sciences, for instance between biology and physics, so there should be room for methodological variance between the natural and the social sciences. It would simply be an instance of lunacy if one attempted to study politics, or the migratory patterns of birds, with the help of particle accelerators. But in spite of this lack of a common methodology for all sciences, I contend that the activity undertaken in them are basically the same because the goal is or should be identical, namely to contribute to the progressive growth of human knowledge (cf. e.g. Popper 1969; 1979a; Tranøy 1986).

The choice of method is quite simply analytically inferior to this goal of attaining new knowledge about the world. Given a particular problem considered interesting, one should choose a method which seems to be able to produce satisfactory answers to it, rather than choosing less attention-grabbing problems for

the sake of utilising a particular strategy for research. The opposite perspective, claiming the primacy of one method over all others, is analogue to the craftsman who insists on using only the tools on the left side of his toolbox, no matter what job or problem he faces. It seems obvious that different research questions demand dissimilar research strategies. Especially in a field so often shrouded under a cloud of secrecy or spin as contemporary political affairs, one needs to be open to innovative research strategies (cf. e.g. Taylor 1971; Almond and Genco 1977). If an interesting hypothesis for some reason refuses to be tested by traditional research strategies it seems reckless to surrender to inconclusiveness rather than trying to devise new methods for testing it.

One needs perhaps to be reminded that even most quantitative approaches within the social sciences are ‘innovative’ in this sense, as they were developed with the specific aims and problems of social research in mind. They are, however, generally inappropriate when the research question pertains to details rather than general patterns, complex rather than simple models, or the study of unique events rather than habitually recurring incidents. It is here that qualitative methods come to the fore. The hypotheses which cannot be tested with quantitative methods can not and should not be left outside the scope of political science, provided they reflect problems that are interesting. Since there are in existence interesting and fruitful problems outside the proper scope of quantitative research approaches, it should hardly come as a surprise that qualitative and idiographic strategies have a proper place in the arsenal of methods employed by the social sciences.

In the following, I will first present the conventional view of what constitutes a scientific activity, heavily indebted to the work of Karl Popper, perhaps the most influential philosopher in the twentieth century, especially in epistemology and political theory. So influential was his proposed solutions to what he designated as the two fundamental problems of epistemology (Popper 1979), that his ideas are now taken for granted, counted among the common stock of founding ideas for science. I will then move on to describe the vices and virtues of qualitative, idiographic and

intensive research strategies in political science. I conclude this paper with a discussion on the role such approaches might play in the scientific study of social phenomena and political affairs.

## **2. The two fundamental problems of epistemology**

When the philosophy of science as we know it today was in its infancy, in the latter part of the nineteenth and the earlier part of the twentieth century, it was concentrated around two fundamental problems. The first was the problem of demarcation, in short how one could separate scientific practices from non-scientific activities. What made for instance astronomy and chemistry into sciences, while their forerunners, astrology and alchemy, were prototypically non-scientific? Or, still more interesting, could one claim that social studies (cf. e.g. Neurath 1931), at that time mainly the quite novel disciplines of sociology and economy were, or could someday become, scientific in the same sense as physics or biology?

The second question was much older and more technical than the first, namely the problem of induction. Originally formulated by David Hume (cf. e.g. 2000[1739], book I; 2000a[1748]; Popper 1971) this problem begins with the assumption that inductive reasoning is inherently uncertain, given that we have no criterion available in which to judge any particular inductive inference to be logically sound. And yet, inductive reasoning seems to be the stuff science is made of: If one cannot infer from a given number of observations to a general statement, then science, if it is a knowledge-producing enterprise, must be contented with pure description of isolated phenomena rather than generalisation and prediction.

Both of these problems were, at least according to himself, supplied with solutions by Karl Popper (1935; 1979; 2002). His ideas set him apart from the logical empiricism of the Vienna Circle, forming a distinct theory of knowledge and science denoted by Popper and others as ‘critical rationalism’. The name might be considered somewhat misleading at least when juxtaposed with the ‘logical empiricism’ of the Vienna Circle, as it is decidedly not a reaction against logic or empiricism or in any other way an allusion to the rationalism of for instance Descartes or Spinoza (cf. e.g.



Popper 2002:3-39)<sup>3</sup>. Instead, Popper meant this name as a descriptor of his dual perspective that science is a rational undertaking, and that the key feature of a scientific frame of mind is its ability to remain critical of established theories. The growth of knowledge comes about according to Popper when the limited validity of old, cherished theories and prejudices is revealed. This stands in contrast to the views of the Vienna circle and their *wissenschaftliche Weltauffassung*, in which it is thought that unshakable, permanent facts could be ascertained and equally permanent theories produced, employing two sources of knowledge only, namely applied logic and sense-experience (hence the name ‘logical empiricism’).

The solutions Popper envisaged for the problems of demarcation and induction are closely related, to each other and to his notions of rational criticism and critical thinking. The problem of induction was seen as only an apparent problem, because inductive inferences could be, according to Popper, translated into a peculiar kind of *deductive* arguments, which was far less problematic from the standpoint of formal logic. If inductive inferences could be treated as deductive ones, it meant that one had a criterion for judging their reliability. Translation from inductive to deductive arguments is the core of the *hypothetico-deductive method*, or simply ‘the method of trial and error’, in which the tentative validity of an inductive line of reasoning is judged by its ability to produce empirical implications which withstands our best efforts at refutation. New knowledge comes about when old theories do not survive such exertions, when preceding inaccuracies and errors are exposed, forcing us to develop new theories which not only could replace the refuted scheme of thought, but which also explain why its forerunner was a success up to the point of refutation.

The proposed solution to the problem of induction led in turn Popper to his answer to the problem of demarcation. The one thing that separates the sciences from other activities, the criterion of demarcation, is their preoccupation with hypotheses

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<sup>3</sup> “I am using the latter term [i.e. rationalism] in its wider sense in which it is opposed to irrationalism, and in which it covers not only Cartesian intellectualism but empiricism also” (Popper 2002:7).

that are testable in this way. For an argument to be ‘scientific’ it needs to be able to be readily refuted. Arguments constructed so as to make them impervious to refutation are therefore prototypically non-scientific, while the hallmark of the scientific perspective of the world is the willingness to submit any belief to open-ended criticism. For the scientific mind, it is deplorable to hold beliefs that are unreceptive of criticism, and it would naturally seize the opportunity to replace such presumptions, along with previously refuted lines of reasoning, with scientific theories that provisionally withstands the tests proposed by rational criticism.

This distillation of Popper’s epistemological thought provides us with important clues about what makes qualitative and quantitative research strategies scientific, namely their ability to form a part of the critical enterprise, in which no theory or belief is considered to be above criticism. The logic of scientific discovery is itself neutral in the question of what research strategy should be employed when one sets out to test a given hypothesis. Most of the time, the choice between qualitative and quantitative is not left entirely in the hands of the researcher to decide, but more or less dictated by the problem at hand or the expected properties of the data. More than anywhere else, it is imperative in the social sciences not to be precommitted to a particular technique for research. The growth and diffusion of knowledge about political and social processes is too noble an end, too important a task, to be left to those who are willing to use only the tools on one side of their toolbox.

### **3. The virtues and vices of qualitative methods**

Qualitative research approaches suffer, like all other strategies for research, from several intractable vices, limiting their efficacy and utility. They are decidedly not, just like their quantitative counterparts, a universal remedy for political science. But in spite of their somewhat narrow applicability, they do supply the political scientist with valuable insights and sources of knowledge. The challenge is therefore to make use of the various research strategies in ways in which they could complement each other and supplant the restrictions of the others. Fortunately, such complementarity is

not only a real possibility but also, I contend, the hallmark of outstanding social research and political analysis. Qualitative strategies, just like quantitative methods, reach their potential only when combined with other paths of enquiry. The prudent social scientist can not, and should not restrict herself to only one such path, but instead build on, whilst retaining a critical attitude towards ‘authorities’, the work of other scientists and their findings, regardless of methodology.

### 3.1 Qualitative generalisation and falsification

The weaknesses of qualitative – intensive and idiographic – techniques include, first and foremost, their inability to produce reliable and accurate generalisations. This limitation is widely discussed in the literature, and several answers have been put forward. The first possible perspective views the lack of generalising potential as an almost insurmountable difficulty in all qualitative research (King, Keohane and Verba 1994). A second outlook sees this shortcoming as being a distressing feature of idiographic research, but which nevertheless does not reduce its potential or value to the scientific community (Yin 2003; 2003a). A third attitude is to see no problems at all, but merely a ‘misunderstanding’ of what more intensive studies is all about (Flyvbjerg 2004).

Bent Flyvbjerg’s account sounds strangely and seductively Popperian, he even mentions the observation of ‘black swans’, Popper’s famous ornithological illustration, making the case that qualitative and intensive studies can hardly corroborate general statements, but could potentially refute erroneous ones. This is however a rather ‘naïve’ portrayal of falsificationism, in which any counter-example is considered equally devastating to a given theory. Except in rare cases, Flyvbjerg’s Galilean exemplar is one of the few instances, one simple experiment is not sufficient to unravel an entire theory. Pitted against naïve falsificationism stands a ‘conventionalist’ perspective in which theoretical stringency and simplicity are considered to be just as important as strict correspondence with reality and all its quirks and complexities (cf. e.g. Duhem 1954; Popper 1959:§20). This

conventionalism dictates in turn conservatism towards stringent and elegant theories, which Popper incorporates into his overall theory (cf. especially Popper 1979a).

It will always be a matter of discretion and good judgment whether a theory, if refuted only once or perhaps even a few times, should be replaced entirely, necessarily by a more complicated and less elegant theory, or if it should rather live on with a few footnotes attached. The prime example of this is Isaac Newton's theory of gravity and motion (cf. Newton 1999), which is still taught in schools and even universities around the world, even if it has been succeeded in theoretical physics by a multitude of theories beginning with Albert Einstein (1988) and Niels Bohr (1923; cf. Popper 1982; Dyson 2004). The reason for this is its simplicity, and in spite of it having been refuted time and again, it continues to be the best available theory for a host of practical purposes. Under falsificationism proper, as *opposed to* naïve falsificationism, one counter-example is rarely sufficient to justify the dismantling of an entire theoretical framework, especially if its practical utility has been established.

In the social sciences also, one 'case study' which goes against an established and practicable theory is seldom sufficient to render that theory powerless for all eternity. One description of a complicated chain of events which seemingly refutes a time-honoured theory is simply not enough to justify a complete dismantling of that theory. If, for instance, Graham Allison's (1969; 1971) depictions of the Cuban missile crisis of 1962 made it clear that the 'realist' account of international relations (cf. e.g. Carr 1939; Morgenthau 1948; Malnes 1993) or its corollary model of the state as a rational, unitary actor suffered from severe and easily visible limitations, it simply does not follow that it is entirely without utility, explanatory power or predictive force (cf. Underdal 1984).

A related example could be found in Flyvbjerg's own work. After studying the political processes surrounding urban planning in one Danish city, he concludes that the 'economic man'-model of human motivation has been refuted. This is so because he finds that some entrepreneurs strive to achieve an 'unreasonable' advantage over others for themselves. It is however very much in doubt whether this type of behaviour, which with only a small stretch of the imagination might be described as

egoistic and utility-maximising, could serve as a final refutation of a theory which states that human action is guided by egoistic, utility-maximising considerations (cf. Flyvbjerg 1991; 1991a; 2004:423-425).

Another who displays the perspective that idiographic research techniques can serve as a basis for generalisations is Robert Yin, in his widely read guides to case study research (Yin 2003; 2003a). His concern is not the division between qualitative and quantitative approaches, but between case studies with one or a few units of analysis, and experiments and surveys with a sufficiently large number of respondents to employ statistical techniques of inference. Case studies can therefore be either or both qualitative and quantitative. Equally significant is his critique of the perspective that intensive research methods is in any way related to a specific set of philosophical beliefs (cf. Yin 2003:14-15).

Yin's approach is oriented towards exposition of practical and applied research, and he shows how case studies can help in several ways, for instance to decide, provisionally at least, between mutually exclusive theories (Yin 2003:31-33) and illustrate for a wider audience what really goes on in the world (Yin 2003:144-146). This is a more subtle, and ultimately more successful, framework for qualitative generalisation. A successful case study could shed light on the strengths and weaknesses of an existing general theory, to help to revise it, or to employ it as an arbiter between contrary hypotheses, for instance when the quality of the available data is less than ideal (cf. e.g. Hagtvet 1980). In other words, intensive studies might facilitate the discovery of new insights or the revision of older presumptions. This understanding of qualitative generalisation avoids the trap of naïve falsificationism. According to this understanding, idiographic methods should have a prominent place in the social sciences as it is one among many useful sources of knowledge available to the social scientist. Qualitative research is therefore thought of as a valuable supplement, not a substitute for more formal or quantitative research strategies.

If Yin represents a more moderate position than Flyvbjerg, then Gary King, Robert Keohane and Sidney Verba (King et al. 1994) embody the opposite extreme,

in which traditional qualitative approaches are ostensibly thought of as less than ideally 'scientific'. Their motto is "to connect the traditions of what are conventionally denoted 'quantitative' and 'qualitative' research by applying a unified logic of inference to both" (King et al. 1994:3; cf. also Adcock and Collier 2001). This entails, it seems, that qualitative methods should become more like quantitative approaches, by "increasing the number of observations" (King et al. 1994, chapter 6), or by utilising qualitative strategies to create wide-ranging causal explanations and nomothetic theories (King et al. 1994, chapter 3).

Underneath these adages lies a particular understanding of what social scientists should concern themselves with, namely to generalise from observations of particulars to general statements. *The* logic of scientific inference is that of inductive statistics and quantitative causal analysis. It is thought that qualitative methods should be modified in a way which makes it possible to employ such techniques. In this, they reflect a departure from the view that scientists should concern themselves with interesting problems, and the elimination of error from our provisional answers to them, and instead restrict themselves to problems which could be answered in particular ways, more or less regardless of their significance. As long as one explains what goes on in the world with a reference to broad generalisations and causal mechanisms, one is, it seems, on scientifically solid ground. Alongside this belief stands of course their unreserved rejection of the kinds of research which has a more 'humanistic' end, more concerned with understanding motives and reasons behind human action than explaining and predicting patterns of human behaviour (King et al., chapter 1; cf. also Neurath 1931:1-3).

Of these three perspectives on the prospects for qualitative generalisation, it seems to me that Yin's outlook is the one that most closely resembles the principles embedded in Popperian falsificationism and critical rationalism, and which also seems most compatible with the demands of practical, idiographic research. Flyvbjerg's optimistic account of the prospects for qualitative generalisation fails because he does not emphasise that it usually takes more than one isolated counter-example to justify the total elimination of a larger theory. On a similar note, the

authors at the other extreme ends up in difficulties of their own, because they insist that intensive research must assume an outward appearance of statistical inference. Qualitative research must therefore increase the number of observations, and thus become less oriented towards studies of subtle nuances and unique occurrences. But it is exactly this orientation towards detail which provides qualitative research with its virtues, its descriptive opulence and its ability to put observed phenomena into their proper context (cf. sections 3.2 and 3.3 below).

It seems obvious to me that studies which store the recorded data as texts or other types of narratives might contribute to the growth of knowledge. This they do not because they can be expected to function as instruments of formal generalisation, or because they might be moulded into the semblance of a statistical survey. Instead, such studies contribute to scientific progress as they point to shortcomings and weaknesses in acquired beliefs and theories, while still being only rarely able to offer genuinely devastating refutations of wider theories. It is a genuine limitation to qualitative studies that the prospects for direct generalisation are not the best, but that does not mean they are unable to contribute to the elimination of error in our existing systems of belief. Quite the opposite, it seems apparent that qualitative research efforts actually do contribute to the progressive modification of our beliefs, either in the form of contributing to the revision of scientific theories, or as illustrations of more general models and hypotheses to a wider audience. Especially when they are seen in conjunction with other techniques for research.

### 3.2 Richness of description and theoretical stringency

The most apparent forte possessed by many qualitative approaches is their ability to observe and record nuances and complex interaction between many variables at once, and witness patterns which might not be easily discovered with quantitative strategies. But this ability to provide vivid descriptions comes at a price, as it naturally becomes more demanding to compare across cases or to discover broader propensities.

There are in the main two central disadvantages connected to the extensive employment of qualitative techniques. First of all, there is the problem of theoretical stringency. As narratives of some particular event or phenomenon become more numerous and ever more complex, it becomes increasingly difficult to get an overview of general tendencies, which is especially necessary in the construction of broader theories. It could of course very well be the case that one particularly illustrative case study might be sufficient to supply the theorist with inspiration to produce a convincing theory of some larger class of phenomena. It should however not come as a surprise that one at some point needs the kind of concise resolution between competing theories which only generalising, even quantitative, studies might provide.

A second problem, and an upshot to the first, is the problem of testability. If all the data are narrative or textual, it is to a considerable degree, definitely even more so than with quantitative data, open to diverging interpretations. This is a reflection of the more general problem of what has been called the 'underdetermination of theory by the data' (cf. e.g. Newton-Smith 1978; 1981:19-43; Lukes 1978; Horwich 1982; Bergström 1984; Laudan and Leplin 1991). As the mentioned works suggest, it is not always perfectly clear to us which theory best corresponds to the available data, and therefore also best describes the facts of the matter. This problem becomes, of course, more apparent as the data becomes more and more ambiguous and susceptible to interpretation, as it often is in the social sciences (cf. Lukes 1978). Since there are more accurate rules and conventions for the use of numerical symbols compared to textual bits of information, it should not come as a surprise that quantitative data are to a lesser extent open to misinterpretation. While qualitative research provides us with better descriptions of the complex, it seems that it is precisely this endeavour to portray multifaceted phenomena in a way that does them justice, which reduces our ability to conclude with concision and cogency.

The picture is however decidedly not entirely bleak for the prospects of qualitative social science reaching a conclusion on the subject matter at hand. Rather, it depends on the nature of the problem whether it makes sense to employ either



quantitative or qualitative techniques in order to shed light on a given problem. When the ultimate aim is that of determining the validity of a fairly simple model or causal theory of frequently recurring events, and it is possible to satisfy the other formal limitations and conditions which usually come with the use of quantitative techniques, then such approaches would be in order. But when the objective is to describe a handful of social phenomena more profoundly, it would seem misplaced to try to force this study into becoming strictly numerical. Whenever it is thought that appreciation of complexity is necessary to grasp the subject matter fully, then less austere representations of reality are needed than what can be achieved with the overwhelming majority of quantitative research strategies.

It might also be the case that a problem just cannot be illuminated using statistical or experimental techniques because it is impossible to produce meaningful data in such a way. There might be several reasons for this. There could be purely 'formal' limitations such as the inability to draw a representative sample of the group under study. A more 'substantive' set of problems occur when our questions and hypotheses have not been developed with the degree of precision needed to employ most quantitative approaches (cf. e.g. Christie 1972:13-14). A third group of problems are ethical, as many quantitative techniques, for instance experiments modelled on the ones found in medical science, could be found to be particularly intrusive for subjects participating in the project when these methods are transplanted to the domain of social science (cf. e.g. Milgram 1963). All in all, there are several reasons why quantitative techniques might quite simply not be available to the social researcher, and in which case one must choose between qualitative studies or complete indeterminacy.

We are, then, in a position to see more clearly what some of the strong points of qualitative research really are. Most important, already mentioned in the title of this section, is its ability to provide us as social scientists with more opulent descriptions of our objects of study. When one really makes an effort to depict the complexities involved in some social or political phenomena, one is able to see connections and

patterns which might have gone unnoticed if one restricted oneself to quantitative approaches because of a misplaced ‘metaphysical’ or ‘epistemological’ affinity. The same kind of criticism could however also be levelled against those who invariably choose *qualitative* techniques for the same, misguided reasons. Instead, there seems to be a proper scope for idiographic techniques in political science, and even if such research strategies have their quite specific limitations, they do not seem to represent insurmountable difficulties. And, it seems equally apparent that the main alternative suffers from other inadequacies of its own, limiting its applicability.

### 3.3 Contextualisation, flexibility and the space for rational criticism

A somewhat related problematic feature of qualitative research could be described as involving a trade-off between the ability to placing the subject matter or problem of the research effort in its proper context, with all its complexities, and the facility with which other scientists might subject it to rational criticism. The scientific enterprise is traditionally, but one might also argue that it is so because of its proper goals of increasing and diffusing knowledge, a collective endeavour. The scientific community have therefore developed several institutional mechanisms and guidelines which makes possible reciprocal criticism between researchers. These schemes, for instance the customary ‘peer review’ prior to publication of scientific articles, have been put in place mainly because it is thought that the critical watch of other researchers might help the original author to develop a more objective account of his subject matter.

When considering the carrying out of a qualitative research project, which often involves prolonged study periods, frequently in unfamiliar surroundings, it is imperative that the researcher is diligent in providing enough background information to make critique of his analyses and interpretations possible. The potential rewards of a successfully executed qualitative research project are many, but many such efforts are unnecessarily reduced in importance because of an inability on the researcher’s side to accommodate and facilitate the critique of other scientists. A fundamental element in the internal critique of the scientific community consists of repeating, or

replicating, parts of the original research effort. This is done in order to validate the conclusions of the original author. In many qualitative studies, however, this task is often surrounded by overwhelming difficulties. Because the data which the initial research report is built on often only survive as 'field notes' or partial transcripts of interviews and the like, they are not ideally suited for replication and reinterpretation.

There are several reasons for this. The premier cause is that such qualitative or narrative data are in a sense already interpreted simultaneously with the actual production of the data. When such research is conducted, the scientist naturally makes selections of what is heard and seen and especially written down or otherwise preserved, and thus providing potential critics with only a partial depiction of the studied entities. An important, often indispensable, ingredient in a successful qualitative study is the gradual building of trust between researchers and informants, limiting the prospects for replication and independent confirmation of the data by other scientists. Quite frequently, it is the reality of qualitative research that such outside review of the data is impossible to carry out, making it difficult at best to ensure that as much of the relevant information as possible is incorporated into the final analysis. The threat of biased selections of the data, leading to predisposed conclusions, should always remain vividly present in the mind of the researcher. Concrete steps should therefore be taken to avoid such a regrettable demise of the research project, including the facilitation of critical review from other researchers.

The final virtue of qualitative research I will mention is the flexibility it gives to the process of knowledge production. Unlike quantitative techniques, which often relies on the unrelenting following of a particular set of rules in order to produce meaningful results, many qualitative approaches are less rigid and open to modification as the research project progresses and new knowledge about the subject matter is gained. This feature of qualitative research is especially valuable when the researcher considers the object for study to be shrouded in mystery, bereft of what could reasonably considered to be fruitful theories or adequate hypotheses (cf. e.g. Christie 1972; 2002; Whyte 1993; Gullestad 1984). In such 'exploratory' research

situations, the ability to learn as one proceeds could easily be considered to weigh up for some of the mentioned shortcomings of qualitative research. For instance, the lack of accuracy or the absence of real possibilities for generalisation and rational criticism could be made up for by this flexibility.

Quantitative approaches are perhaps rigorous and systematic in a way which qualitative approaches can not hope to become, but they do come out short when compared to intensive or idiographic studies on their strong points, for instance their ability to put the entities of study in their proper context, or the flexibility with which they allow us to proceed. We must as researchers at some point choose, it seems, between rival goals. One might at one time think accuracy, rigour and the ability to arrive at fairly determinate conclusions to be overridingly important. In another situation, it would seem more natural to conclude that flexibility and descriptive opulence is what is really needed to illuminate the given problem. In any instance, it seems ill-advised to suggest that only one type of method are able to produce the desired results, which in scientific contexts ultimately will be to contribute to the growth of knowledge and the reduction of ignorance.

## **4. Conclusion**

What is then the proper place of qualitative methods in political science? With the preceding discussion in mind, it seems that we are better equipped to answer this question. Successful qualitative research must be seen in conjunction with other research strategies. It is in fact doubtful whether it is possible to be a successful qualitative researcher without being informed by quantitative research and findings. The situation is similar for scientists employing quantitative methods. Without the help of qualitative research and its appreciation for complexity, one can not hope to be able to develop those 'sharper instruments', the ability to ask the right questions to the right group of people, on which profitable quantitative enquiries are dependent. Qualitative and quantitative research works better together than separately, as social

science is inherently both idiographic, like the humanities, and nomothetic, as the natural sciences.

I do not deny, of course, that there are different ‘research programmes’ (cf. e.g. Lakatos 1978; 1999) in social and political science, in which the admixture of qualitative methods is different from this portrayed ideal of mutual assistance. Today, there seems to exist instead several different programmes in which the relative prestige of quantitative and qualitative approaches varies quite significantly (cf. e.g. Hay 2002; Creswell 2003). And it seems that some political and social scientists have been lead to the rather absurd conclusion that it is better to display one’s philosophical sympathies than to attempt to provide answers to the most pressing problems and questions with the best available methods. To recall the mottoes at the beginning of this paper, we should in political science seek ‘nutriment’, that is to say new ideas and insights, wherever it might be found, and choose between problems competing for our attention on the merit of their gravity and interest, instead of choosing less interesting problems in order to employ a specific method.

Political science is perhaps in its modern form still a quite novel discipline, but the systematic, empirical study of political affairs is much older, dating at least back to Aristotle (especially 1957; 1961). A notable forerunner is however the more theoretically oriented speculations of Plato (1884; 2003), which Aristotle takes as his point of departure, criticising it on empirical grounds (cf. Aristotle 1957, 1261a2-9; 1264b26-1265a10). But even if Aristotle lived well over two thousand years ago, his views on the proper nature of political studies resonates quite well to this day. In his opening words to the *Nicomachean Ethics* (Aristotle 1890, 1094a1-1095a13), he lays out the considerations which inspired his political studies. Political science (*hē politikē technē*) is among the highest and most noble kinds of study, as it builds on the other sciences, and as its goals are essentially *practical* in nature. The successful student of political affairs should therefore be able to guide the statesman and the political community at large to institute wise legislation and reforms. Because of the practical nature of the study, one should also not expect it to reach the same kind of

accuracy he ascribes to mathematics and logic, and we should instead settle for a level of determinacy sufficient to realise the proper, practical goals of the endeavour. The degree of exactitude and accuracy of a science should be appropriate for the subject matter. It is as misplaced to ask the social scientist for exact proofs and predictions as it is to ask a mathematician for loose assumptions and prospects.

This Aristotelian excursion shows that the traditional outlook is to view political studies, with its practical ends, as set apart from the empirical examination of nature and the formal investigation of concepts. Pitted against this perspective is the one found among those who proclaim the unity of science, and its corollary that anything might be studied in the same manner. There is not, according to this view, a place for a separate class of *Geisteswissenschaften*, which should have ‘understanding’ as its goal separate from the explanatory objectives of natural science (Neurath 1931, especially pp. 1-18; cf. also Weber 1972[1921]; von Wright 1971). These two positions form the core of the debate over whether social science is something different from natural science, or whether it is merely the continuation of natural science, built around the same methods and regulatory ideas (Taylor 1971; Almond and Genco 1977; cf. Popper 1979a:206-255).

Between these positions lies what might be called the critical approach to the study of politics. Under this outlook, it is left open what we might finally come to know about political affairs and processes. Instead, this more sombre perspective on the conditions for political science holds that we might gain knowledge about a lot of things, but that there is also a great deal we do not know much about. In fact, this perspective tells us, new knowledge might make us more acutely aware of our ignorance, and thus help us to see our limitations better (cf. Popper 1969:103). It seems to me that a critical perspective is characterised by this dual awareness of one’s own knowledge and ignorance. Because of our at best partial knowledge of political processes, our theories become inaccurate and provisional. Sound political judgment, the practical *art* of politics, might therefore be informed by the findings of political science, but it is doubtful whether it could be replaced by it (cf. Berlin 1996:1-53; Hanley 2004).

The critical perspective on the nature of and prospects for political science remains neutral, moreover, to which sources of knowledge or ‘methods’ that should be employed in order to gain new knowledge about politics. It might very well be the case that some aspects of political affairs are difficult to grasp with the formal techniques familiar from quantitative approaches in the social sciences, and that such strategies will never completely be able to overtake more humanistic, idiographic and qualitative methods. In fact, one might say it is very likely that a comprehensive understanding of past and contemporary political processes depends on the deployment of both types of research methodology. What qualitative approaches lack in the possibility for reliable generalisations is made up for in their propensity to complement methods which have generality as their manifest purpose with rich descriptions of translucent nuances and unusual phenomena. In this sense, a critical perspective goes together with what has been successfully described as ‘methodological opportunism’ (Przeworski 1995). The fundamental goal of social science is to gain new knowledge by supplanting old prejudices with a better and more complete and profound understanding of social and political phenomena, and not to do so in a particular way.

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