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Questioning structurism as a new standard for social scientific explanations

Abstract

As the literature on Critical Realism in the social sciences is growing, it is about time to analyse whether a new, acceptable standard for social scientific explanations is being introduced. In order to do so, I will discuss the work of Christopher Lloyd, who analysed contributions of social scientists that rely on (what he called) a structurist ontology and a structurist methodology, and advocated a third option in the methodological debate between individualism and holism. I will suggest modifications to three points of Lloyd's analysis, without abandoning Lloyd's intuitions completely. Firstly, the intuitions of the structurist ontology can be made explicit in a different way, without loosing the individual-society dualism. Secondly, opting for a structurist ontology does not necessarily imply opting for a structurist methodology. Ontology and methodology are related, but not as strongly as Lloyd supposes. Thirdly, the idea of a complete explanation, present in the structurist methodology, confuses causation and explanation while denying the pragmatics of explanation. A broader spectrum of explanatory forms can be defended. Criticizing Lloyd on these three points will lead me to the defence of an explanatory pluralism, which I relate to a minimal ontology. The intention of this reconceptualisation of structurism (and related Critical Realist applications) is to broaden possible perspectives on the explanatory praxis of the social scientist, and to question the reunification of the social sciences. It will also stipulate which form of interdisciplinarity is preferable for the social sciences.

Introduction: Critical realism in the social sciences

In recent years, the literature concerning Critical Realism in the social sciences has been growing fast For example, the work of Margaret Archer (1995) is very influential in social theory and sociology; in economics, we have Tony Lawson (1997), Steve Fleetwood (ed.)(1999) and others; in international relations theory, you can find David Dessler (1989), Walter Carlsnaes (1992), Heikki Patomäki (2002), etc.; and in history and historical sociology we have Christopher Lloyd (1993). Without claiming that we are dealing with a completely homogeneous movement, these different contributions do have a lot in common (as they often state explicitly themselves) and they suggest that a new model or standard for the social sciences is emerging. In order to start evaluating this new standard, I will scrutinize the work of one of the contributors, namely Christopher Lloyd. The ontological and methodological intuitions of Lloyd's structurism will be identified, and I will point to some of structurism's weaknesses and propose possible reconceptualisations. I have chosen to analyse Lloyd, as he pays a great deal of attention to actual social scientific practice in developing his analytical framework. Based on the discussion of Lloyd's contribution, I will end this article with evaluating the possibilities and problems of (current applications of) Critical Realism in relation to the idea of unifying social scientific explanation and its relation to the reunification of the social sciences and/or interdisciplinarity. My critiques, and the reconceptualisations I will propose for Lloyd's structurism, may also be taken into account in light of other current applications of Critical Realism in the social sciences.

1. Christopher Lloyd's structurism

Christopher Lloyd has developed an important contribution to the ongoing debate over the conceptualisation of the relation between structure and agency in social science (cf. Lloyd, 1986, 1989a, 1989b, 1991 and 1993). After an impressive analysis of the presuppositions on ontology and methodology made by some major social scientists,

mainly social historians and historical sociologists, Lloyd identifies a common intuition of the relation between actor and society in the works of such successful social scientists as Edward Thompson, Eric Hobsbawm, Robert Brenner, Theda Skocpol, Reinhard Bendix, Norbert Elias, Alain Touraine, Philip Abrams, etc. In order to describe the common intuitions in the works of these social scientists, Lloyd introduces the term *structurism.* The ontological intuition of *structurism* is defined as follows:

> 'A **structurist ontology** directs attention to the structuring interactions between (on one hand) individual and collective human beliefs, intentions, choices, and actions and (on the other) the externally real enabling and constraining structural conditions of thought and action. In this model *social structures are the emergent ensemble of rules, roles, relations, and meanings* that people are born into and which organize and are reproduced and transformed by their thought and action. It is people who generate structures over time and initiate change, not the society itself, but their generative activity and initiative are socially constrained. This ontology denies the legitimacy of the action/society polarity that the others are based on and attempts to conceptualize action and society as being an interpenetrating duality in the sense advocated by Jean Piaget and Anthony Giddens. There is a duality of causal power in this model, with humans having structuring power and structures having enabling and constraining power.' (Lloyd, 1993:42-43, my bold)

Lloyd links this ontological structurism with a methodological structurism. (I will describe the methodological structurism later.) While I agree with the common ontological intuition Lloyd perceives in the work of the named social scientists, I want to point out that there are other ways to make this intuition explicit. Identifying alternative ontological conceptualisations will oblige us to question the way Lloyd connects a methodological structurism to his ontological structurism and to discuss his ideas on methodological structurism. In doing this, I will develop three modifications of Lloyd's conceptualisation of the intuition of structurism:

(1) In section two, I will analyse the debate between individualists and holists₁ in a more sophisticated way than what is generally offered in the literature, and I will show that *the*

intuition of a structurist ontology could be conceptualised differently.[2] In section three, the modified conceptualisation of the ontology will be presented.

(2) The seemingly '*natural*' *connection between ontological structurism and methodological structurism* will be discussed in section four. Here I will argue for a broadening of the possible connections between ontology and methodology, instead of the strict connection presented by Lloyd.

(3) Building on the results of section four, I will then show in section five how *the methodological structurism should be further developed or could be replaced* by a methodological or explanatory pluralism, and I will discuss the advantages it gives us. In section six, I will point to some consequences of my modifications for the role of structurism (and related Critical Realist applications) as a new standard for social scientific explanations, and the relation with the unity of the social sciences and interdisciplinarity.

2. Analysing the debate between individualists and holists$_1$

Dealing with the eternal debate between individualism and holism$_1$, Lloyd draws the following conclusion:

> 'The point here is that any sociological and historical discourse has to come to terms with the general relationship of individual action to social structure, something that radical individualists and holists fail to do. I shall be arguing that structurism is **the only viable** ontology and methodology.' (Lloyd, 1986: 18, my bold)

Lloyd's conviction that only *one* viable ontology and methodology represents an alternative to individualism and holism$_1$ is partially explained by the conflation of

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[2] I will distinguish holism$_1$ from holism$_2$. Holism$_1$ is *holism* as used by Lloyd as the opposite of individualism (I will use the term *collectivism* as a synonym of this holism$_1$). Holism$_2$ is used as the opposite of atomism, and will be defined in section two.

questions in the debate between holists$_1$ and individualists. This conflation paralysed the debate for many years, reducing it to a discussion between two camps. The introduction of *structurism* as a third alternativeis certainly a step forward in the debate. Notwithstanding, I think it would be fruitful to reconsider the debate on ontology by distinguishing at least two questions that were always conflated, and as such show that there is more than the one viable ontology and methodology that Lloyd calls structurism. The two questions that should be distinguished are:[3]

(1) Is individual agency determined or compromised by higher, aggregated structural factors? If the answer is yes, you choose collectivism, if not: individualism.

(2) Are mutual relations a condition *sine qua non* for actors in their constitution? If yes, you choose – what we will call – holism$_2$, if not: atomism.

Instead of an individualist versus holist$_1$ debate in ontology, we will now have a debate between atomistic individualists, holistic individualists and defenders of collectivism. In what follows, I will argue that the holistic individualism is a possible conceptualisation of the intuitions present in the work of the social historians and sociologists analysed by Lloyd, and that this holistic individualism can be an alternative to ontological structurism. My goal in doing this, is, however, not to 'prove' that holistic individualism is the only correct ontology, but rather to question the idea of starting with identifying an ontology, considered as the only viable and correct one, and then building up a methodology. I will develop this point in the following sections. For now, it is important to consider that I take holistic individualism to be just one possible ontology, among others. Questioning the whole idea of the metaphysical debate (an unending battle of intuitions) between individualists and collectivists and the decisive weight given to it, will be done in sections four to six (see also Van Bouwel, 2003).

Let us now deal with the two questions concerning individualism and collectivism I have distinguished above. Dealing with the first question, I will – just like Lloyd – reject radical collectivism (Lloyd's *holism*). Radical ontological collectivists ascribe a distinctive reality to institutional or aggregate features and hold that they act downwards, as it were, on individuals: they pre-empt or predetermine what individuals do. For them,

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[3] This distinction was developed by Philip Pettit (1993).

social structures relate to individual action and interaction as an emergent factor, having an autonomous status. My argument to reject this collectivism will be developed in section three. So my answer to question (1) will be negative: individual agency is not determined or compromised by higher, aggregated structural factors.

Dealing with question (2) brings us to the distinction between atomists and holists₂, the question of the relation between individuals themselves: Are people's social relationships with one another a condition *sine qua non* for their constitution as subjects and actors? Atomists, on the one hand, ascribe a minimal importance to the relation with others, stating that all the capacities characteristic of our kind could be developed in total isolation from others. Holists₂, on the other hand, consider social relationships as being constitutive for the development of distinctive capacities of the subject or actor. They take people's social relations to one another to be essential to their being rule-followers, and, therefore, to their being thinkers, speakers and agents (cf. Pettit, 1993: 111-112). In my view, this holism₂ expresses part of Lloyd's intuition of structurism as expressed in the definition quoted in section one. Holism₂ points to (part of) the '*ensemble of rules, roles, relations, and meanings* that people are born into and which organize and are reproduced and transformed by their thought and action' (Lloyd, 1993: 42-43). Contrary to Lloyd, however, I would not speak of this *ensemble* as *emergent* and being the social structure, as this suggests a merely vertical relation (structure-individual), where holism₂ focuses on horizontal relations (individual-individual). In section three, I will spell out which problems – concerning causation – appear by only focussing on the vertical relation and considering social structures as emergent.[4]

Notwithstanding the conflation of vertical and horizontal relations in the conceptualisation of ontological structurism, I take holistic individualism to be arguably close to Lloyd's intuitions. Firstly, it avoids presenting the individual as being dominated by a 'supra-individual system with powers of self-regulation' (Lloyd, 1991: 191) as collectivism (or holism₁) does. Secondly, unlike atomistic individualism, it accounts for the fact that the individual is constituted by interactions with other individuals, which is

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[4] Lloyd (1989a: 461) defends the existence of 'structural powers of both strictly physical and **emergent** kinds' (my bold).

the central condition for thought and meaningful action. Therefore, I take this holistic individualism as a possible alternative to ontological structurism, while respecting the intuitions present in the work of the historians and sociologists Lloyd is referring to.

3. Social ontology and causal fundamentalism

In this section, I will present an ontological framework, called *causal fundamentalism,* which supports our reconceptualisation of the idea of structurism, i.e. the holistic individualism. Causal fundamentalism states that the higher level causal regularities – chemical, biological, psychological, and social – supervene on the regularities and background conditions that obtain at the physical level. Therefore, all non-physical causal regularities supervene on the regularities and related conditions that actually obtain at the physical level (Pettit, 1993: ch. 3 and 5).

I will start with a short description of the terms supervenience and emergence (as I interpret them while noting that there are innumerable interpretations of these terms), and then show how some problems in the use of emergence, as used by Lloyd, can be avoided. *Supervenience* makes a claim about the relation between two levels of analysis. It states that if two states (or properties, events, or any relata of the supervenience relation) are identical with respect to their descriptions at the lower level, then they cannot differ at the higher level. Consequently, two things cannot be in the very same physical property P_x, at the lower level, without thereby being in the same state S at the higher level. Therefore, there is a one-many mapping between the supervenient state and the physical states. Supervenience puts emphasis on the dependency of the lower level, and denies the autonomy of higher-level aggregates.[5] *Emergence* must be understood as the following claim: When lower-level states interact, a certain level of complexity, or a higher-level state, could be achieved. This will allow for genuinely novel characteristics to appear and lead an autonomous (separate from the lower level) existence, having their

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[5] This does not automatically imply that supervenience entails reducibility, cf. Harold Kincaid (1997: 70-74).

own ontological autonomy, and exerting causal power upon other states. As a result, we are presented with a form of dualism: the higher level has another 'nature' than the lower level. This is a stronger claim than supervenience; it states that a higher-level entity or property can be realized by different lower-level combinations of entities or properties (this exemplifies multiple realizability, e.g. anger can be realized by different physical states or energy combinations of our brain), but the higher-level entity has no causal autonomy. As already mentioned, Lloyd relies on this idea of emergence to elaborate his structurism.[6]

Let us now compare my proposal of causal fundamentalism with Lloyd's structurism in which he refers to an emergent social structure. Endorsing the ontology of causal fundamentalism implies that there cannot be a conflict between non-physical regularities, such as the conflict between intentional and structural regularities. Ontological discussions in the social sciences, between ontological individualists and collectivists are precisely based on the assumed conflict between the intentional and the structural. However, seeing as the underlying physical regularities form a coherent set, and their fixation means that the intentional and structural regularities are both wholly in place, then those two sets of regularities cannot conflict with one another (Pettit, 1993: 152). Then we must reject ontological views that oppose individual and structural powers, views that claim that one level (be it the individual or the structural) overrides the other. If they were to go in different directions, then the physical powers would be acting against themselves.

That there cannot be a conflict between the intentional and the structural, should not be understood in a straightforward, common sense way. For example, it does not imply that a citizen of a state will never have a conflict with the state. It only claims that the state supervenes on all the citizens of that state and that there cannot be a (causal) conflict between the entire citizen body and the state at a certain point in time. A single citizen can always be constrained by a neighbour or a fellow citizen. A similar reasoning goes for structures that were created in the past. The realization of those structures in the present can be understood at the individual level (ultimately at the physical level), and as

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[6] Unfortunately, Lloyd does not define how he understands *emergence*.

such we do not have to accept downward causation (i.e. from the structural to the individual level).

The conclusion of adhering to causal fundamentalism is that, in the social sciences, ontological individualism is right after all. This is not because there are no structural regularities, and not because intentional regularities override social-structural regularities; it is because, as causal fundamentalism tells us, physical powers fix the pattern of powers and regularities that rule at all levels; therefore, there must be a harmony between levels. 'It cannot be the case that structural powers ever cause the intentional to be suspended or ever deprive individuals of the autarchy associated with the rule of the intentional.' (Pettit, 1993: 152) Hence, I reject ontological collectivism as it considers aggregate features as having an autonomous status, and holds that they act downwards, .

Comparing the ontology of causal fundamentalism with Lloyd's structurism, we must place emphasis on the vagueness of Lloyd's ontology, especially concerning his idea of *emergent* social structures. This emergence risks introducing a top-down causation – autonomous structures acting downwards, in conflict with individuals – or at least leaves us with a vague concept of causation.[7] Therefore, I prefer the ontology of causal fundamentalism to structurism in order to understand the social structure in a conceptually coherent way. Causal fundamentalism will turn out to be illuminating in at least three ways. First, it clearly distinguishes the individual level from the structural level, and introduces a concept of causation that is less vague than Lloyd's. Second, we can develop a more sophisticated framework of possible explanatory strategies, as we take many levels (from the physical to the biological, psychological and the social) into account in the ontological framework. Third, it offers us the possibility to analyse analogies between the structure-individual debate in the philosophy of history, and the mind-body debate in the philosophy of mind. Although I have a preference for one of the two ontological frameworks, I do want to repeat, however, that our main purpose is to illustrate that the intuition of structurism can be conceptualised into an alternative

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[7] On top-down versus bottom-up causation and its relation with supervenience, see: Jeroen Van Bouwel and Erik Weber (2002a).

ontological framework We do not want to claim that we can *a priori* decide what the best ontological framework is, as this is an empirical issue. The best we can do is to develop a minimal ontological framework that does not obstruct empirical research in a decisive way. The proceeding sections will deal with what follows from the proposed ontology of causal fundamentalism concerning methodological (or explanatory) strategies, and they will elaborate on the three advantages enumerated above.

4. Connecting ontology and methodology

To defend an *ontological* (holistic) individualism does not necessarily entail a preference for *methodological* individualism. Although Lloyd states that '[t]he three ontologies of individualism (or aggregationalism), holism, and structurism have given rise to three methodologies' (Lloyd, 1991: 189), I am convinced that the seemingly inevitable link between ontological and methodological individualism (and between ontological and methodological structurism, and ontological and methodological collectivism) is one of the causes of the ongoing debate between methodological individualists and their opponents. The link is not inevitable, nor 'natural': adherence to ontological individualism does not entail methodological individualism, as I will point out later. However, this does not imply that adherence to ontological (holistic) individualism and causal fundamentalism has no methodological impact whatsoever. Seeing as there can never be a conflict between intentional and structural regularities, the ontological view of causal fundamentalism clearly has a methodological impact. When it comes to formulating explanations of a structural kind, we must be able to see, 'in our intentional psychology of people, why the type of linkage involved is likely to be reliable. But the capacity to see this does not mean that for any structural (or historicist) explanation we offer, we will be in a position to tell a proximate intentional story, even an intentional story of a quantificational or statistical kind.' (Pettit, 1993: 263)

Although we cannot deny the impact of ontological conceptualisations, our views on explanatory options in the social sciences have been dictated too long by our views on

ontology and causality. Many contemporary contributions, especially those of the Critical Realists, focus mainly on how the relation between agency and structure has to be understood ontologically. Once that point has been understood, the whole methodological debate seems to be solved. But while causation and explanation are not clearly distinguished in ordinary or non-philosophical thought, it is important to be aware of the distinction in philosophy, and to explore the different explanatory options that follow from a conception of causality. Too much weight has been put on the ontological debate, which has led to the neglect of the debate on methodology and explanations. In the *philosophy of mind,* there are some people using a similar emphasis to avoid becoming paralysed in metaphysical or ontological debates, and they are choosing to pay greater attention to the explanatory praxis. For example, Tyler Burge states the following about the worries that exist in the *philosophy of mind* on mental causation:

'But what interests me more is the very existence of the worries. I think that they are symptomatic of a mistaken set of philosophical priorities. Materialist metaphysics has been given more weight than it deserves. Reflection on explanatory practice has been given too little.' (Burge, 1993: 97)

A similar idea is defended by Lynne Rudder Baker:

'Given standard metaphysical and methodological assumptions, not only has the problem of mental causation proved to be intractable but even worse: the same reasoning that leads to scepticism about mental causation also leads to scepticism about almost all supposed "upper-level" causation, and hence to skepticism about explanations that mention "upper-level" properties, including explanations offered by the special sciences and much of physics. Of course, pointing out such skeptical conclusions, even of this magnitude, is not a refutation of the metaphysical assumptions that generate them. But skeptical consequences may well be a motivation for taking a different philosophical tack. (…) My proposal is to perform a methodological about-face. Instead of beginning with a full-blown metaphysical picture, we should begin with a range of good explanations, scientific and commonsensical. (…) Although my proposal has a strong pragmatic cast, it is by no means an anti-realist

suggestion. I am not equating what is real with what is needed for explanations and predictions. The point is, rather, that we have no better access reality than what is required for cognitive success, construed broadly enough to include what is cognitively required for achieving goals in both science and everyday life.' (Rudder Baker, 1993: 94-95)

This change of focus will make us abandon the seemingly inevitable connection between ontology and methodology, and it will broaden possible connections between these two areas. The dominant methodological individualism relies on an ontological individualism and prescribes that explanations should refer to the intentional attitudes of individuals or other (non-intentional) facts about individuals as explanans. Although we do rely on ontological individualism as well, we will also allow explanations on a social or structural level. These structural explanations explain social facts by invoking other social facts, and they do not refer to individuals. In the next section, this explanatory pluralism will be developed and compared with Lloyd's methodological structurism.

5. Methodological structurism and explanatory pluralism

After having introduced holistic individualism and the ontology of causal fundamentalism, thus illustrating how the intuition of structurist ontology could be alternatively made explicit, and having shown that the connection between ontology and methodology has often been understood too narrowly, I will now develop the alternative conceptualisation of *methodological* structurism. Lloyd describes this methodological structurism as follows:

'Methodological structurism approaches explanation by developing concepts of the separate real existence yet mutual interdependence of individuals and institutional structures (...). Structures qua structures have structural properties such that those properties are not merely the aggregate of the powers and behaviour of the individual people who are supposed to constitute them. On the other hand, those structural properties are not independent of the structuring practices of people. Thus methodological structurism

is explicitly based on an ontology of the social that recognizes two nodes of causal power.'
(Lloyd, 1993: 46)

From this description we can derive that methodological structurism follows ontological structurism rather strictly.[8] Given ontological structurism, it is certainly possible to understand the idea of methodological structurism, although it is not defined very rigidly. According to Lloyd, 'structure and action, then, are not the poles of society but two moments in a dialectical duality.' (Ibid.) But how do we identify this 'dialectical duality' in our explanations? How should this 'dialectical duality' be expressed in the formulation of explanations?

It seems to me that this dialectical duality, or 'the interpenetrating duality' (Lloyd, 1991: 190), as a methodological guideline risks making an error similar to what Margaret Archer has called the Fallacy of Conflation:

'Thus when discussing "structure" or "culture" in relation to "agency" I am talking about a relationship between two aspects of social life. However intimately they are intertwined (...) these are none the less analytically distinct. Few would disagree with this characterization of social reality as Janus-faced: indeed too many have concluded too quickly that the task is therefore how to look at both faces of the same medallion at once. It is precisely this methodological notion of trying to peer at the two simultaneously which is resisted here. (...) [This] foregoes the possibility of examining the interplay between them over time. (...) Any form of conceptualization which prevents examination of this interplay should therefore be resisted.'[9]

Given this risk (and putting aside the discussion of whether Lloyd actually commits the Fallacy of Conflation or not), we have to examine the interplay by clearly disentangling

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[8] Lloyd (1991: 213): 'This means these writers have a realist and structurist ontology of society as consisting fundamentally of institutionalized social relations, a theory of persons as social agents who structure the social world through time, and, consequently (sic!), a methodological structurist approach to explanation.'

[9] Margaret Archer (1988: XII). The same defence of analytical dualism and against duality and conflation was already developed in (Archer, 1982: 455-483 and especially 458) as an argument against the structuration theory of Anthony Giddens. We follow Archer in her critique on the Fallacy of Conflation, but not in her conceptualisation of the relation between structure and agency (cf. Archer, 1995).

the threads of structure and agency. This will prevent us from conflating the 'two modes of causal power' (Lloyd, 1993:46), it will oblige us to be explicit and clear about the relation between the two, and it will clarify both the importance of agency and the analysis of structure. Depending on your interest or motivation you can either choose to focus on the individual level (the agency level) or on the social-structural level, or you can give an account of both levels; different sorts of interesting explanations are thus to be found at different, non-physical levels. These non-physical levels offer us causally relevant information that is not available from physical explanations, and these different non-physical sorts of explanation offer us different forms of information. On both the intentional and the structural level, explanations with causal relevance and explanatory interest can be found: Hence, the opposition between methodological individualism and collectivism is false, and should arguably be replaced by an explanatory pluralism.[10]

To be more concrete, imagine social scientists explaining a revolution. Their explanation can be formulated at different levels by referring to social structures, referring to the properties of individuals, or by referring to a neurophysiological basis or a genetic disposition. You have to choose the most adequate level of explanation for your purpose, for answering the questions asked (given the context). In order to compare revolutions, for instance, you can, following Theda Skocpol, choose a structural explanation, and it may seem to be the best approach if comparison is your goal, as it would not be economic (though not impossible) to provide an explanation by merely referring to the actions of individuals. Some individualist explanations are cumbersome; sometimes complexity might defeat the goal of explanation. If you want to explain the specific path of one revolution, it could be more illuminating to provide an individual explanation. If you are interested in knowing why a human being revolts in the first place, you could provide an evolutionary, genetic explanation. Different epistemic

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[10] We have developed this idea of explanatory pluralism extensively in Weber and Van Bouwel (2002), using the erotetic model of explanation and distinguishing different explanatory questions that can be asked about the same social phenomenon S, compare: Why does S have property P?; Why does S have property P, rather than property P'?; Why does S have property P, while object *b* has property P'?; Why does S have property P at time t, but property P' at time t'? You do not explain S, but answer an explanatory question about S, and those answers/explanations might be very different according to the questions asked. See also: Van Bouwel and Weber (2002b).

interests and motivations lead to different questions and answers/explanations of the same social phenomenon. The (one and only) explanation does not exist or (if it exists) is not the most economic one to provide. Hence, I avoid the reductionist claim that lower-level explanations are always better explanations.

Here we part from Lloyd's methodology, as the thrust of his argument is to try to unify explanation around a common basic approach where no difference exists between an individual and a structural approach, for all explanations should be structurist (a 'combination' of the structural and the individual understood in a structurist way) (Lloyd, 1989a: 456). Defending this kind of methodology, in which the dialectical relation between agency and structure is reflected in structurist explanations, leads to defending 'complete' explanations (Lloyd, 1989a: 482).[11] *Completeness*, in Lloyd's view, depends on an 'objective' world that could be truthfully and completely described. I claim that it is Lloyd's view on causation that imposes this (too demanding) view on explanations; causation should not be confused with explanation.

Completeness of explanations, in my view, depends on the epistemic needs of the researcher and the questions involved. Formulating explanations consists of combining the causally relevant components (ontological) with what is pragmatically relevant (the parts of the causal history that have to be mentioned), and as such it is always connected to knowledge-interests. An explanation will always be a selection of an ideal, complete causal account. Therefore, if structurists would reply to me that the explanations taken on their own (individual or structural) are incomplete or insufficient to provide a full picture of social phenomena, I can only answer that any explanation is incomplete given the standards of the structurists.

Replacing methodological structurism through explanatory pluralism does not respect Lloyd's intention to enhance the unification of the social sciences by unifying

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[11] This idea of a complete explanation existing of an agency-part and a structure-part, does appear in many applications of Critical Realism, e.g. David Dessler (1989: 453) states the following: 'In Aristotelian terms, structure is a material cause rather than an efficient cause of behavior. Structure alone explains only the possibilities (and impossibilities) of action. (…) A complete explanation must appeal not only to the material but also the efficient causes of action, which can be located only within a theory of the agents.' Another example is Alexander Wendt (1987: 362), in an article very much in the Critical Realist tradition: 'they [structural and agent-based analyses] are both necessary elements of a complete explanation of social action.'

explanation around a common basic approach. The connection of methodological structurism with successful social scientists, as done by Lloyd, suggested a norm; a standard of ultimate explanations was introduced. I have defended that we cannot restrict our methodology to that very demanding and complete explanation. However, this does not imply that unification will be replaced by an *anything goes* approach: firstly, attention should be paid to the questions asked and to the epistemic interests involved (this can form a basis for rational discussion); secondly, maintaining the minimal ontological framework of causal fundamentalism ensures non-contradiction between different levels of explanation. As such, the old debate between a one-sided methodological individualism and a one-sided methodological collectivism is replaced by a new debate. This new debate contrasts a monistic approach, structurism, which relies on Critical Realism and puts new limits on social inquiry, with a pluralistic approach which relies on a non-reductive physicalism and refers to the impossibility of an ideal complete explanation.

This shift from methodological structurism to explanatory pluralism implies the following differences for social scientific practice: (a) that several of the existing forms of explanation in the social sciences can be considered compatible (to the extent that they live up to our ontological framework), and that they should not necessarily be restyled to a new structurist form of explanation; (b) we avoid introducing a very demanding standard for explanations in the social sciences;[12] (c) more attention will be paid to the epistemic interests at stake and their implications for explanatory practice, where different explanatory requests concerning the same social phenomenon lead to different forms of explanation (as such, giving up the idea of providing a 'complete' or 'full' picture of a social phenomenon).

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[12] Avoiding creating expectations similar to the ones created by the introduction of the Hempelian (Covering Law) standard of explanation.

6. Current applications of critical realism, a new explanatory standard and the reunification of the social sciences

Previous research on the ontological and methodological points of view in current (often-quoted) applications of Critical Realism, in Archer (1995), Carlsnaes (1992), Dessler (1989) and Lawson (1997, 1999), has lead me to identify two common features of these viewpoints (cf. Van Bouwel, 2003 and Van Bouwel, 2004):

(a) The agent/structure debate, and debates between individualists and collectivists, can be solved by spelling out a priori their ontological relation. Debates between individualists and collectivists are dealt with in a very general way by many advocates of the Critical Realist approach in the social sciences; they commonly replace individualism and collectivism in their respective social scientific disciplines with a version of the Transformative Model of Social Activity. Secondly, the idea of emergent properties is assumed. It is striking that contributions made in the different social sciences are interchangeable, which shows that the actual practice and ontological presuppositions of the specific discipline are not seriously taken into account (Lloyd is an exception here). Rather than a priori defining how the social world functions, it could be more fruitful to start from empirical research. [13]

(b) *The methodological consequences of Critical Realism's ontology follow 'automatically', and hence do not have to be spelled out; the pragmatics of explanation are neglected completely.* A second common feature is the lack of attention paid to the form of explanations and to methodology in general. Margaret Archer, for instance, couples her ontological realism with a methodological realism, but does not develop the latter extensively. One of the very few times she mentions the *methodological* component is in the following quote:

'Just as Individualism and Holism represented social ontologies whose commitments to what constitutes the social world then issued in programmatic injunctions about how it

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[13] The actual state of the social sciences deserves more attention, cf. 'The proponents of CR oscillate between a descriptive and a prescriptive philosophy of science, and whilst they recently purport to accomplish the latter, I argue that their strength lies in the former.' (Baert, 1996: 514).

should be studied and explained (that is Methodological Individualism and Methodological Holism as conflationary programmes working in opposite directions), so the realist social ontology also enjoins a Methodological Realism which embodies its commitments to depth, stratification and emergence as definitional of social reality. Thus the burden of this chapter has been to demonstrate that given these fundamental tenets of realism, they can only be respected and reflected by a Methodological Realism which approaches structure and agency through "analytical dualism" – in order to be able to explore the linkages between these separate strata with their own autonomous, irreducible, emergent properties and which consequently repudiates any form of conflation (be it upwards, downwards or central) in social theorizing.' (Archer, 1995: 159)

This quote shows how Archer adopts a familiar way of reasoning on the relation between ontology and methodology: first, we decide (*a priori*) on a social ontology, and secondly, we spell out (and sometimes that does not even seem to be necessary) the methodological implications. However, is prescribing a methodology (based on an *a priori* ontology) and its ideal form of explanation the way to deal with the plurality of successful forms of explanation in the social sciences? It is obvious that Archer's methodology does not do much more than repeat the *a priori* ontological stance; this methodology states nothing about pluralism of explanations or about pragmatic factors, and nothing about the actual state of disciplines in which explanations are competing.[14] In the discussion of Lloyd's ideas, I have shown how a different ontological conceptualisation could be presented for the intuition of structurism. We must emphasize that we cannot work under the current applications of Critical Realism in the social sciences. These applications first define an ontology (to avoid the *Epistemic Fallacy*), and then presume that a methodology follows automatically. There are different ontological conceptualisations possible (engaging in eternal metaphysical debates about the exact *a priori* conceptualisation often misdirects energy to the wrong issue), and relying on just one possible conceptualisation narrows our views on methodology, and a narrow approach does not satisfactorily take into

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[14] I do have to mention, however, that Tony Lawson (1999) does recognize that the context and explanatory questions at hand do affect the explanatory practice, but he considers it as a second-order issue and does not acknowledge the consequences these pragmatic factors might have on the form of explanation (cf. Van Bouwel, 2004).

account actual social-scientific practice and its plurality. Therefore, we must maximize the number of explanatory forms permitted, and then analyse their compatibility within a minimal ontological framework.

As the focus has been on ontological issues within applications of Critical Realism, the methodological consequences are underdeveloped, and the ideal form of (complete) explanation, as present in Critical Realists' contributions, is at odds with the plurality we find in the explanatory practice of social scientists. I hope future contributions will shine more light on these issues, particularly by accepting or refuting some of the reconceptualisations of Lloyd's structurism I have introduced above.

In the introduction, I have mentioned the growing body of literature on the benefits of adopting Critical Realism in the social sciences. After our analysis of Lloyd's structurism and the general remarks on Critical Realists' contributions I have just mentioned, we might raise some questions about the idea of reunifying the social sciences under the banner of this 'new' ontology and methodology. Lloyd, for instance, states, under the heading of *Towards the Reunification of the Social Studies*:

'If their practice were to be based on the realist-relational approach it would provide a framework for simultaneously explaining particular acts, events, patterns of behaviour, consciousness, and structural change. (…) It is because of the deeper relation of partly intentional behaviour to both the given structural conditions of behaviour and the production, reproduction, and transformation of structures, that action-oriented and structure-oriented history can be united on a more fundamental level. Such a unified science would ideally then incorporate all the existing empirical and theoretical social and historical studies.' (Lloyd: 1993:195)

Therefore, arguing in line with his idea of unifying explanation around a common basic approach, Lloyd defends his structurist approach as the way to overcome the intellectual division of labour in studying the social world. By developing a *via media* as a synthesis of individualism and collectivism, Lloyd wants to introduce unidisciplinarity, erasing the

borders between the different social scientific disciplines.[15] Following my critique of Lloyd's explanatory model, I claim that this unidisciplinarity is not the right approach, as it imposes a standard (closely linked to the idea of a complete explanation) that does not take into account the plurality of epistemic interests (and the difference these interests imply for the explanatory information that is required), nor does it account for the plurality of existing forms of research and explanation existing in the social sciences.

In line with my defence of explanatory pluralism, and going against current applications of Critical Realism, I want to defend a form of interdisciplinarity that is driven by the research question (and underlying epistemic interests) at hand. This question-driven interdisciplinarity will make maximal use of existing explanatory forms and theories in different disciplines (depending on the question at hand), rather than following an imposed *a priori* ontological framework which narrows the use of existing forms of explanations and replaces it with an unrealistic standard of explanation, neglecting the impact of epistemic interests and pragmatics. Although the existence of (historically constructed) disciplines might not be ideal, unidisciplinarity itself cannot be our goal *per se*; we might evolve in that direction driven by research questions and answers, but not by imposing metaphysical schemes.

Conclusion

In order to improve understandings of the explanatory praxis in the social sciences, and to evaluate the contributions of Critical Realism to these issues, I have reconsidered the concept of structurism as introduced by Christopher Lloyd. Introducing holistic individualism and the underlying ontology of causal fundamentalism, I have reconceptualised the intuition of the structurist ontology in a way that offers us the possibility to look for analogies in the *philosophy of mind* and in other sciences. Secondly, I have reconsidered the relation between ontology and methodology, and

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[15] Unidisciplinarity in the social sciences might as well be reached by imposing a unifying concept (e.g. Wallerstein's (1991) historical system), or by elaborating a monism based on unifying theory (cf. *economics imperialism* based on neo-classical economics).

defended the view that a methodology should be non-contradictory with the ontology, which does not imply that a methodology is completely defined by spelling out an ontology. Thirdly, I have emphasized the importance of disentangling structure and agency in the explanatory praxis, and introduced an explanatory pluralism as an alternative to Lloyd's methodological structurism. I do not want to defend a new division of labour, nor do I wish to deny the interconnectedness required to explain structure and agency (which is expressed in the supervenience relation). What I do want to emphasize with the explanatory pluralism is that explanatory progress depends on the questions asked; therefore, it depends on the chosen level on which your explanations can differ; both structural and individualist explanations are acceptable and indispensable.

In order to analysethe variety of explanations provided by social scientists, and in order to improve the practice of explaining, an *explanatory framework* has to be developed, within which the interests and related questions of a researcher steer him to the best explanation. This framework is a necessary support to the idea of a pluralism that counters the *anything goes* approach; it provides social scientists with an analytical instrument that makes their focus explicit in studying history and society in all their complexitywhile searching for the best explanation. It will, by relying on the reconceptualisation of ontological structurism, broaden possible perspectives of the explanatory praxis of the social scientist. In their present form, the applications of Critical Realism in the social sciences do not live up to these requirements, and ambitions to reunify the social sciences on a Critical Realist's basis should therefore be questioned.

References

Archer, Margaret (1982) 'Morphogenesis versus structuration: On combining structure and action.' *The British Journal of Sociology*, 33 (4): pp. 455-483.
Archer, Margaret (1988) *Culture and Agency: The Place of Culture in Social Theory*. Cambridge: Cambridge University Press.

Archer, Margaret (1995) *Realist Social Theory: The Morphogenetic Approach*. Cambridge: Cambridge University Press.

Baert, Patrick (1996) 'Realist Philosophy of the Social Sciences and Economics: A Critique.' *Cambridge Journal of Economics*, 20 (5): pp. 513-522.

Burge, Tyler (1993) 'Mind-Body Causation and Explanatory Practice.' In: Heil and Mele (1993), pp. 97-120.

Carlsnaes, Walter (1992) 'The Agency-Structure Problem in Foreign Policy Analysis.' *International Studies Quarterly*, 36 (3): pp. 245-270.

Dessler, David (1989) 'What's at Stake in the Agent-Structure Debate?' *International Organization*, 43(3): pp. 441-473.

Fleetwood, Steve (ed.)(1999) *Critical Realism in Economics: Development and Debate*. London: Routledge.

Heil, John and Alfred Mele (eds.)(1993) *Mental Causation.* Oxford: Clarendon Press.

Kincaid, Harold (1997) *Individualism and the Unity of Science*. Lanham: Rowman & Littlefield Publishers.

Lawson, Tony (1997) *Economics & Reality.* London: Routledge.

Lawson, Tony (1999) 'Critical Issues in Economics as Realist Social Theory.' In: Steve Fleetwood (1999), pp. 209-257.

Lloyd, Christopher (1986) *Explanation in Social History*. Oxford: Basil Blackwell.

Lloyd, Christopher (1989a) 'Realism, structurism, and history: Foundations for a transformative science of society.' *Theory and Society*, 18: pp. 451-494.

Lloyd, Christopher (1989b) 'Realism and Structurism in Historical Theory: A Discussion of the Thought of Maurice Mandelbaum.' *History and Theory*, 28: pp. 296-325.

Lloyd, Christopher (1991) 'The Methodologies of Social History: A Critical Survey and Defence of Structurism.' *History and Theory*, 30: pp. 180-219.

Lloyd, Christopher (1993) *The structures of history*. Oxford: Blackwell Publishers.

Patomäki, Heikki (2002) *After international relations: Critical realism and the (re)construction of world politics*. London: Routledge.

Pettit, Philip (1993) *The Common Mind: An Essay on Psychology, Society, and Politics*. Oxford: Oxford University Press.

Rudder Baker, Lynne (1993) 'Metaphysics and Mental Causation.' In: Heil and Mele (ed)(1993), pp. 75-95.

Skocpol, Theda (1979) *States and Social Revolutions*. Cambridge: Cambridge University Press.

Van Bouwel, Jeroen (2003) 'When unveiling the epistemic fallacy ends with committing the ontological fallacy: On the contribution of Critical Realism to the social scientific explanatory practice.' *Philosophica*, 71: pp. 81-98.

Van Bouwel, Jeroen (2004) 'A monist explaining economic action or an all too economic explanation of pluralism? Comments on Tony Lawson's heterodoxy.' *Philosophical Explorations*, 7 (3) (in print).

Van Bouwel, Jeroen and Erik Weber (2002a) The LAT-relationship of causation and explanation: A Comment on Jean Lachapelle.' *Philosophy of the Social Sciences*, 32 (4): pp. 560-569.

Van Bouwel, Jeroen and Erik Weber (2002b) 'Remote Causes, Bad Explanations.' *Journal for the Theory of Social Behaviour*, 32 (4): pp. 437-449.

Wallerstein, Immanuel (1991) *Unthinking Social Science: The Limits of Nineteenth-Century Paradigms*. Cambridge: Polity Press.

Weber, Erik and Jeroen Van Bouwel (2002) 'Can we dispense with structural explanations of social facts?' *Economics and Philosophy*, 18: pp. 259-275.

Wendt, Alexander (1987) 'The Agent-Structure Problem in International Relations Theory.' *International Organization*, 41 (3): pp. 335-70.

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Studying IT and community

Methodological, ontological and epistemological approaches

Abstract

In this paper, I offer a summary of an information technology (IT) case study that involved three different approaches: social construction of technology (SCOT), social capital, and diffusion of innovations. Although these approaches first appeared highly different in theoretical and methodological terms, I began to see overlaps during the analysis phase of my study. These overlaps were further emphasized as I began to reflect on the epistemological and ontological similarities that the approaches share.

The first section of this paper offers the reader a summary of my case study. This summary is then followed by a detailed examination of each approach where I discuss the limitations and advantages of these approaches, along with the epistemological and ontological similarities that they share.

IT and community: The case of connected kids

My case study took place in Troy, a small city in upstate New York that rests on the side of the Hudson River.[1] Although it was once a city at the forefront of the Industrial Revolution, Troy has recently experienced an economic decline that has affected all aspects of the city's life: store fronts lie empty, over half the residents live on 'low' to

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[1] As a number of publications have been made regarding this research site and project, I have opted to maintain the actual names of the city and technology. A reader can gain more information on Connected Kids by visiting http://www.rpi.edu/~zappenj.

'moderate' incomes, and the industries that once drew fame to the city have not existed for years.

In contrast to this decline in Troy's material wealth stands the city's local university, Rensselaer Polytechnic Institute (RPI). Located about one mile away from Troy's City Hall, the RPI campus holds a wealth of resources, many of which are related to information technology.

This contrast between RPI and the city of Troy has caused friction over the years between students and the local residents. Students tend to avoid the city altogether, preferring to drive to nearby malls and cinemas. City residents refer to RPI as the 'school on the hill', and often think of the RPI community as a powerful, rich, yet closed-off and distant neighbour.

In reaction to these multiple divisions that separate RPI and the city, RPI Professors Teresa Harrison and James Zappen began speaking with members of City Hall about the possibility of building an information technology infrastructure that could serve the interests and needs of the Troy community. In the summer of 1999, these discussions culminated in the idea of an online information system that would distribute information on the city's youth and family services. This idea emerged from a problem presented by the municipal government: the lack of communication and collaboration among different youth service agencies. This lack of communication resulted in many agencies duplicating the services of their neighbours. Through creating an online information system that would distribute up-to-date information on youth services, the city hoped local agencies would be better informed, more willing to collaborate with one another, and thus reduce the duplication of services in the community. As will be seen in the following pages, however, the city's hopes were not the only ones influencing the technology's development.

Since this early conceptualization, the technology has gone through a number of phases. Teresa and James have encouraged community members to participate in focus groups and design sessions, and have allowed these sessions to shape the design and functionality of the technology. Although not yet finished, the current technology is referred to as the Connected Kids Project. Connected Kids will be a web-based database

system that will allow local youth service providers to input information about their agencies' services. This information will then be broadcast over the Internet through a specially designed web interface. The intended audience of this system includes children, youths, parents, and the service agencies that serve these populations. These different audiences have all participated in shaping Connected Kids current look and feel.

Studying connected kids: Research design and re-design

I began studying Connected Kids in 2000. I had originally seen Connected Kids as an opportunity to study the role information technology played among community members. Such an interest would have involved a research design where data were gathered at different points in time, namely, before and after Connected Kids' completion and use within the community. In the early phases of my research, however, I quickly began to see that the design and development of Connected Kids would take longer than I had originally imagined. Gathering pre and post-data on the potential users of Connected Kids thus became an uncertain option. In spite of this disappointment, I began to see the process of designing and building Connected Kids as an attractive area for inquiry: As Teresa and James were opening the design process to a wide community of potential users, I could see that the social interactions surrounding and involving Connected Kids were complex. Thus, I began rethinking and reshaping my original research proposal to include additional literature reviews and research questions that focused on the social aspects of technology design and diffusion. In the end, I found I was studying Connected Kids through three different theoretical and methodological approaches: SCOT, social capital, and diffusion.

My research involved three rounds of interviews: February 2000, May 2000, and June 2001. I attempted a panel study, although many respondents dropped out of the study and new ones replaced them in the three different rounds. Over the course of the three rounds, 37 respondents were interviewed: 29 came from not-for-profit agencies, two were professors, one was a city official, three were city employees, and two were county employees. For all three rounds, all respondents were interviewed using a loosely-structured script. During the third round of interviews, all respondents except for Teresa

and James were also given two questionnaires to fill in. Respondents would often share additional sources of data with me: email correspondences, transcripts from meetings, and organizational documents. These pieces of data were also included in my analysis.

Although multiple methods were used, my three main instruments of investigation were the interview script and two questionnaires, and each ot these three tools were originally intended to coincide with each of the theoretical approaches: interviews for SCOT, a questionnaire for social capital, and a second questionnaire for diffusion. In the analysis phase, however, the results from each of these methods began tosupport or provide insight into the one or the other approach. This blurring of findings, along with details of each approach, will be discussed in the following sections.

Approach #1: The social construction of connected kids

SCOT theory shows how social forces, composed primarily of social actors, surround a technology and shape that technology's design and development. These actors are seen as inhabiting their own social worlds, which are defined as social groupings formed from unique belief systems, organizational affiliations, and/or common activities (Clarke, 1991). These social worlds guide actors' interpretations of a technology's intended purpose and use while placing different demands on the technology's design and development.

These actors and their corresponding worlds are also seen shaping and influencing one another: As they come into contact through various boundary objects, i.e. activities, objects, or beliefs that cross worlds' boundaries and bring worlds together, these actors and worlds influence one another, and thus influence how each interprets and shapes the technology (Fujimura, 1992; Star and Grisman, 1989). In addition, actors are constantly interacting with the technology at its different stages of development and use; thus actors form interpretations of a technology through interacting with that technology and with one another. This constant interaction among actors, worlds, and technology forms what

some SCOT scholars have called a 'seamless web' of activity: the social and technological realms become symmetrically linked within a network of interaction and mutual shaping.

SCOT studies tend to use a range of qualitative methods that include loosely-structured interviews, snowball sampling, and participant-observation. This consistency in approach emerges from the theory's ontological contention that boundaries between social actors and technology are blurred, and such blurring of boundaries runs counter to the inclination for quantified measures. In addition, distinctions between social worlds are also seen as arbitrary and difficult to locate with any precision (Klein, 2001); the idea of using a quantitative approach to locate and measure these interactions among actors goes against SCOT's fundamental views.

I chose SCOT theory as a framework to help guide questions and methods pertaining to Connected Kids' ongoing development: I had realized early on that Connected Kids would take a long time to develop, as Teresa and James were committed to building Connected Kids through a participatory process. By choosing to engage members of Troy's community, Teresa and James were exposing Connected Kids to a range of social forces. SCOT theory provided me with a vocabulary and set of analytical tools to study this process.

I was particularly interested in how the different actors participating in Connected Kids were interpreting this technology, and whether these interpretations were influencing Connected Kids' design and development. Teresa and James had opened up the design process to many of these same community members through a series of participatory events: focus group meetings, design sessions, and user-testing sessions. This participatory aspect to Connected Kids' development afforded, what seemed to me, an opportunity to study a number of interesting interactions: social actors interacting with each other, with Connected Kids, and the mutual shapings that could result from such interactions.

To research this social shaping of Connected Kids, I conducted three rounds of interviews over the course of eighteen months. The respondents I chose to interview were employees of youth-service agencies, which included schools, local government agencies, not-for-profits, and the project leaders, Teresa and James.

My method for choosing this sample needs some discussion in relation to the SCOT literature. Many SCOT scholars opt to use snowball sampling, a method whereby a researcher initially interviews one participant in a technology design project, and asks that participant to nominate other actors relevant to the technology's design, development, or use (Bijker, 1997; MacKenzie and Wajcman, 1999). Through a series of nominations, a researcher uncovers a sample of individuals relevant to the technology. Other scholars either forego this method or complement it with a more analytical, structural approach: the researcher attempts to analyze power structures and/or other structures surrounding the technology's design and development. With this structural perspective in mind, s/he interviews respondents that reflect or represent these particular structures (Klein, 2002).

My method for deciding whom to include in my sample of respondents was based largely on seeing which actors were actively involved in Connected Kids, either through attending meetings or participating in an email list created and monitored by James. In doing so, I actively ignored certain social worlds and actors that were mentioned to me by respondents as individuals and/or groups relevant to the Connected Kids project. For example, many of my respondents mentioned children and parents as important actors for this technology project, yet I opted not to include these groups in my sample as I wished to wait until these groups became directly involved in Connected Kids. My reasons for waiting rested on my desire to watch how Connected Kids interacted with the community. If I spoke with actors who were not involved in Connected Kids, I felt I would be intruding upon the process by providing information about Connected Kids, and thus potentially affecting actors' interpretations prior to them forming interpretations of their own. These non-sampled actors were, nonetheless, included in my analysis of the relevant social worlds and actors surrounding Connected Kids. Thus, I allowed the structure of Connected Kids meetings to determine whom I interviewed, yet the respondents' nominations of other relevant actors guided my final analysis of whom to include in which social world.[2]

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[2] Bijker (1995) discusses the problem of missing social actors/worlds, and he recommends that researchers use their judgement on how to handle this. Further, he notes that the method of falling the actor is not perfect, and should thus be seen more as a heuristical tool.

In addition to the interviews, I was given access to email documents and transcripts of focus group meetings held during October 2000. These meetings were organized by Teresa and James for purposes of gathering input and feedback from the youth-service community regarding Connected Kids.

Analysis and results of SCOT data

Through analysing the data gathered, I developed six distinct social worlds that were influencing Connected Kids directly or indirectly. These worlds included (a) Rensselaer Polytechnic Institute (RPI), (b) Local government, (c) private funding agencies, (d) youth, (e) parents, and (f) not-for-profits. Below, I summarize each of these social worlds, the roles they played in Connected Kids' ongoing development, and how they interacted with one another.

The technical institute: RPI

The most powerful social world influencing Connected Kids' development was that of RPI. RPI held technical resources and a skilled student body that permitted much of Connected Kids' hands-on design and development to take place. Access to these students and resources was gained through Connected Kids' two project leaders, Teresa and James, who were faculty members at RPI.

Teresa and James were also highly influential in the early conceptual phase of Connected Kids. Through discussions with local government officials, Teresa and James established the main purpose for Connected Kids, i.e. an online database distributing information about youth-service agencies. In addition, Teresa and James were developing Connected Kids in a participatory fashion, which was a deliberate choice on their part. This decision to open up the design process to participatory practices had profound effects on the direction that Connected Kids took. In particular, Connected Kids' interface, content, structure, and intended audiences were all largely shaped by the sort of input and feedback that community members gave Teresa and James.[3] Finally, Teresa

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[3] Teresa and James have discussed these influences in some of their writings. For instance, Harrison, Zappen, Stephen, Garfield, and Prell (2001); Harrison, Zappen, and Prell (2002); Teresa and James (2003)

and James, through a series of grant-writing activities, acquired funds for Connected Kids that allowed for further resources, time, and talent to be dedicated to the Connected Kids project. These funds and added resources allowed for more community participation to occur, thus further influencing Connected Kids' final shape.

Local Government

Teresa and James originally teamed with local government officials in brainstorming the purpose of Connected Kids. Local government, which included both city and county offices, saw Connected Kids as a possible tool to help not-for-profits better coordinate their services for youth. Such coordination was seen as improving the quality of services in the area, as well as potentially saving the government money. In addition, the municipal government offered funding for the Connected Kids project and played a role in diffusing information about Connected Kids to the local not-for-profit community. Local not-for-profits turned towards these government agencies and actors for funding, and thus interacted with these actors on a regular basis. Through these interactions, news of Connected Kids was disseminated, and this process probably influenced the way not-for-profits grew to understand and become involved in Connected Kids.

Funding Agencies

Teresa and James wrote a number of grant proposals to different funding agencies in search of resources and money for Connected Kids. Each grant proposal held particular criteria for awarding funds, and these criteria influenced the path Connected Kids took. For example, the largest grant awarded to Connected Kids came from the National Science Foundation in the spring of 2001.[4] This award provided the Connected Kids project with funding for 3 years, thus allowing for the hiring of additional student designers and programmers, and multiple participatory design sessions with community members.

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[4] The National Science Foundation is a large, national funding agency of the US government. The Foundation is seen as the largest government funding source academics and researchers can turn towards. Further information of the National Science Foundation can be found at: http://www.nsf.gov.

Youth

In the eighteen-month time span of my study, youth were often discussed as important end-users of Connected Kids, yet they were not directly involved in the design of this project. During the October 2000 focus group meetings, many not-for-profits spoke of the need for including youth in design meetings, and comments I heard during my interviews with many of these same participants reflected these sentiments. Even though these youths were not actively voicing their needs and desires regarding Connected Kids, many of the not-for-profits working directly with youth spoke for this social world and made suggestions of what these youths' needs might be. Thus, although the youth were not visibly present during the course of my research, other actors spoke on their behalf. These comments regarding youth inspired Teresa, James and RPI student designers to make changes with regards to Connected Kids interface. In addition, Teresa and James held later design sessions, beyond the scope of my study, which involved youth from the local community. Future research will investigate the extent to which these youths' opinions affected Connected Kids.

Parents

Similar to youths, parents were a much-discussed social world, but one that was not directly involved in the Connected Kids process. As with youths, not-for-profit actors often voiced their opinions of what parents might need and want from such a technology, and these actors' discussion may have influenced the project leaders decision to hold later design sessions just for parents of the community.

Not-for-profits

Not-for-profits were the largest social world covered in my analysis, and they represent a powerful shaping influence on Connected Kids' development. Although this world's influence was important throughout Connected Kids' development, it was mainly seen during the October 2000 focus group meetings. These meetings were designed by Teresa and James for gaining the input and feedback from not-for-profits on an early prototype of Connected Kids. The feedback gained from these focus groups influenced the

designers' and developers' ideas for Connected Kids' interface, functionality, and intended audiences.

These focus group meetings were an opportunity for Teresa and James to influence not-for-profits' understandings of Connected Kids. Many of the not-for-profits attending the meetings had little to no knowledge of Connected Kids, and the meetings provided these participants with additional information about the motivations, goals, and ideas Teresa and James held. Later interviews with not-for-profit actors showed me that many of these actors had adopted, more or less, the view of Connected Kids presented to them by the project leaders.

The focus group meetings also gave not-for-profit actors a visual manifestation of Connected Kids. Seeing this prototype gave these actors something to which they could react, form firmer opinions, and gain clearer impressions. Finally, the focus groups permitted these actors to speak with one another and influence one another's opinions. Through providing a multiplicity of interactions among the social actors and the emerging technology, these focus groups provided an important opportunity for Connected Kids' development. They also provided an important space in which actors' views of Connected Kids could collide and merge with one another. Over time, through later interviews, I discovered that many not-for-profit actors had formed a consensual view of Connected Kids. That view consisted of Connected Kids helping not-for-profits to coordinate their services, help serve certain needs of each organization, and also to help parents and youths gain access to information on services and programmes in Troy. For example, I heard respondents quoting Teresa and James' view of Connected Kids as an IT that would help organizations in coordinating their services. In addition, I heard these same actors state that in using Connected Kids to coordinate, they would also be fulfilling certain needs of their own organizations; for example, they would be helping their clients receive the best services available in Troy.

Connected kids' role

Connected Kids also played a role in its own development. All social worlds and actors shaped their opinions and understandings of Connected Kids partially through reacting to

the technology itself. For instance, during the focus group meetings Connected Kids was seen for the first time in a material form. In doing so, social actors could react to the technology and form opinions about it.

The fact that Connected Kids was an information technology influenced actors' opinions. Many were positive towards IT and saw IT as helping their work within the agency and with youth. Thus, an IT that was being custom built for the youth of Troy spoke to these actors in a positive way.

Lessons Learned

This brief description of my SCOT findings reflects much of the theoretical and empirical literature pertaining to the sociology of technology. Actors and technology can be seen influencing one another, with this mutual shaping leading towards a more stabilized artefact. Yet my description also reveals some of the tensions of the SCOT approach, namely, the problematic nature of handling missing groups. Bijker (1995) discusses this problem briefly, noting that powerless groups of actors may escape an analyst's notice, as they do not have the ability to speak for themselves. If an analyst is able to identify these voiceless groups, s/he is still restricted in how well s/he can get to know them. Bijker's (1995) response to this issue is to see the SCOT approach as a heuristic, interpretive method. That is, that snowballing and following actors are techniques that help in the interpretive process of understanding technology development.

My own experience of not speaking directly with youths reflects some of these issues: youths were not invited to the table to participate during the time of my study, and I did not have access to their perspectives. Nonetheless, their presences was known, and an interpretive case could be made on their account. Such a solution is imperfect and clumsy, yet an understanding of how this group's influence was felt can still be ascertained.

Future research will continue observing these actors' and worlds' roles in shaping Connected Kids. In addition, future research will document how Connected Kids, once completed and introduced to the community of users, will influence relationships and practices among these community members.

Approach #2: Social capital

As mentioned, I was originally interested in exploring the relationship between information technology and community to see what role information technology played in community relations. I was particularly drawn to the community networking literature: This literature describes a participatory, democratic approach to building and using computer-networked technology. 'Community networking' thus refers to a community's intentional design, ownership, and use of computer-networked technology for purposes of strengthening that community's goals (Schuler, 1996).

The literature also discusses community networks as positively affecting a community's development through affecting that community's social capital. Social capital, in the context of this literature, refers to trust and reciprocity that flow through relationships among social actors: the better connected actors are in a group, or 'social network',[5] the more the group as a whole benefits (Bourdieu, 1986; Coleman, 1990; Putnam, 2001).[6] Community networks were seen as affecting the ties among actors through providing actors with a supplemental channel of communication. Actors who were already in contact with one another could have an additional channel through which to communicate and thus strengthen their ties with one another. In addition, actors unknown to one another could potentially form ties through use of the technology, thus extending the size of actors' networks (Blanchard and Horn, 1998).

When I learned about Connected Kids, I saw an opportunity to test this link between social capital and community networks. Although not exactly reflecting the notion of a community network, the Connected Kids project reflected many of the ideals and concepts of the community networking movement: Connected Kids was a participatory technology design project, its intended end-users and owners were

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[5] Social networks are described in greater detail later in the article. For now, a social network may be defined as a network of actors connected to one another through various types of relations
[6] I am aware of the great body of literature surrounding social capital, and the problems with definition and measurement of this theory (see Prell, 2003 for a fuller discussion on social capital and measurement). For the purposes and constraints of the present paper, I limit my definition to a functionalist view of social capital, and thus, ignore the more cultural takes of social capital as discussed by such scholars as Pierre Bourdieu.

community members, and the technology was being designed to meet specific community goals (as opposed to earning company profits, for example). Connected Kids was not intended, however, for all community members, nor was it initiated and led by community members. These aspects of Connected Kids placed the technology slightly outside the category of 'community network', yet overall, I saw the technology's goals and spirit as reflective of my readings pertaining to community networks. Thus, I hoped the early phases of my study could focus on gathering base-line data on social capital and then, once Connected Kids was complete, I could observe any changes within the community and note whether or not such changes could be linked to the adoption and use of Connected Kids.

As noted, I soon realized that Connected Kids' development was taking much longer than I had originally hoped. Nevertheless, I decided to gather data on Troy's social capital as a means of gauging community members' relations with one another. Future research could then use this baseline data as a point for comparison.

Locating social capital: Multidisciplinary exploration

Developing measures for social capital proved particularly challenging, largely because the theory crosses disciplines and tends to get redefined according to the contexts of different research environments. Economists, organizational and management scholars, political scientists, anthropologists, and sociologists have all conducted research using social capital as a theoretical framework (e.g. Burt, 2000; Foley and Edwards, 1999; Knack and Keefer, 1997; Lin, et al., 2001; Putnam, 2001; Tsai, 2000). This variety of studies has generated a variety of methodological approaches: qualitative and quantitative, large and small scales.

In reflecting on my Connected Kids study, I realized that a small network of actors had already been assembled for me: a select number of people from local youth-service agencies were participating in the design and development of Connected Kids. These actors and their corresponding agencies had been invited to participate in the Connected Kids project through attending meetings, focus groups, and design sessions.

All of these meetings were organized by Teresa and James for the purpose of gaining these participants' input and feedback on the ongoing development of Connected Kids.

As a result of these reflections and reviewing the literature, I began to look into studies that focused on measuring social capital within social networks. My search led me to social network analysis. Social network analysis (SNA) maps the relationships between individuals, groups, or organizations to see what kinds of meaningful patterns emerge (Wellman and Gulia, 1999). In mapping out these patterns, the analyst looks at such issues as the quality of the relationships (Brass, 1992), the positions of actors within the network, and how both these aspects of the network affect the way information and resources flow (Wellman and Gulia, 1999).

For my study, I asked 33 respondents, all of whom were directly involved in the Connected Kids project, to answer questions on their relationships with one another. The 33 respondents were composed of school administrators, administrators and employees of the local youth-service agencies, and local government officials and employees. These questions focused on relations reflecting social capital concepts such as the frequency of communication, amount of trust, and the amount of reciprocity among the respondents. This survey was administered during a round of interviews in May-June 2001.

To analyze the data gathered from this survey, I calculated centrality scores for each of the 33 respondents. Centrality is defined by the number of ties an actor holds in a network. The more ties an actor holds, the more centrality s/he holds. Centrality is described in the literature as a measure for social capital: the more central actors are, the more access they have to resources, the more likely they are to be trusted, and thus the more social capital is associated with them.

The central actors in my data set proved to be ones from government agencies, which were in charge of distributing funds to the not-for-profit community. Thus, those actors who distribute the funds were the ones to whom others turned on a regular basis, perhaps in hopes of accruing future funds or perhaps to assure the lenders that the funds were being well-spent. This correlation between centrality and organizational affiliation reflected discussions found in the social capital literature. Coleman (1990) for instance, discusses social capital as embodied in organizations. For example, organizations can

deliberately structure networks of relations, and in doing so, create opportunities for reciprocity and trust.

This interpretation became complicated when I reflected on comments respondents made during my interviews. After completing the social network questionnaire, respondents spoke with me in an informal way about their relationships with the other youth-service agencies in Troy. Their descriptions showed me instances of reciprocity that I failed to measure in my survey, such as referring clients to one another's agencies and writing letters of support. These acts of reciprocity, moreover, seemed based on weaker social links: the actors involved tended not to interact much with one another, yet they were aware of each others' presence in the community and turned to one another as needed. Thus, rather than organizational affiliation, these ties were based on social knowledge gained through years of working with youth in Troy.

Methodological lessons learned

The above discussion shows how my SNA findings failed to fully capture the complexity and subtlety of the relationships among these not-for-profits. This fact calls for some reflection. Using SNA to explore measures of social capital was useful to me for viewing the map of this network according to a few variables: frequency of contact, reciprocity, and perceptions of trust. In getting this visual picture, I was able to see which actors dominated the network and how these powerful/popular actors were connecting with other actors.

However, I found the largest limitation of SNA to be the fact that I could only visualize the network according to a few variables. I can (and will) expand my survey to capture some of the missing variables, but the fact that I had supplemented my SNA survey with interviews helped me to capture some information I would have otherwise missed. I had not asked all respondents to describe in detail how they related to actors on the list, yet information about these relationships surfaced in many of my interviews. This information led me to make additional interpretations of the ways in which these actors relate to one another.

Approach #3: Diffusion of innovations

My third and final approach to studying Connected Kids was the diffusion of innovations theory. Diffusion of innovations looks at the communication, adoption, and subsequent spread of a technology. The theory describes how technologies enter social systems, systems where actors and/or groups are tied together through interpersonal relations, and explains reasons for why social systems either adopt or refuse to adopt the technology (Rogers, 1995).[7] Those actors in the system who have many ties to others within the system will most likely hear about the technology faster than others. In addition, actors in this system tend to influence one another's opinions about the technology. These interactions, over time, work to determine whether or not the technology will be adopted by the social system (Littlejohn, 1996; Rogers, 1995; Valente, 1995).

In addition to this emphasis on system structure, diffusion also takes into account qualities of the individuals within the system. Diffusion offers a listing of attitudinal variables that potentially affect the diffusion process. These variables pertain to the positive and/or negative attitudes actors have regarding a technology's perceived characteristics. These characteristics include the following: (1) a technology's perceived advantage for an actor; (2) a technology's perceived compatibility with an actor's current activities and/or practices; (3) a technology's perceived complexity for an actor, (4) the importance (to the actor) of observing a technology prior to deciding whether or not to adopt, and (5) the importance (to the actor) of trying the technology prior to deciding whether or not to adopt it. Thus, diffusion takes into account both individual level factors as well as structural factors that affect the diffusion and adoption of a technology.

As with social capital, diffusion theory has crossed a number of disciplinary borders. The theory has been popular among sociologists, communication scholars, engineers, marketing and business researchers, policy analysts, and political scientists (Calantone and Benedetto, 1990; Katz and Shapiro, 1986; Mintrom, 1997; Rogers, 1995).

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[7] Social systems can, in many ways, be seen as synonymous with social networks, and this similarity will be discussed in greater depth later on. The main difference with regards to Rogers' conception of social systems, as opposed to social networks, is that Rogers' emphasizes ties based on communication, whereas social networks can be composed of a variety of different ties, not just ties based on communication.

This variety of disciplinary interest has also led to a variety of methodological approaches.

I chose diffusion theory to study how and why this community decided whether to accept and use Connected Kids. Often diffusion studies take place at the end of a technology's development. Retrospectively, these studies analyze why or why not a technology was adopted by a community. Diffusion theory explores these questions through observing (a) actors' perceptions of and attitudes toward technology, (b) the roles actors play in a technology's diffusion, and (c) the structuring of the social system surrounding the technology (Rogers, 1995).

Measuring perceptions and attitudes for diffusion

To measure actors' perceptions of and attitudes toward Connected Kids and IT, I developed a questionnaire containing 27 Likert-scale items adapted from a previous diffusion study (Shim and Kotsiopulos, 1994). These items measured perceived characteristics of Connected Kids and information technologies associated with Connected Kids, i.e. the Web and databases. My questionnaire also contained seven items focused on respondents' attitudes toward adopting Connected Kids. I administered the questionnaire during my final round of interviews in May-June 2001.[8]

Analysis and results

In SPSS, I calculated Pearson Correlation Coefficients among the perceived characteristics and the seven outcome measures for actors' willingness to adopt Connected Kids. A one-tailed test was chosen as a positive association was predicted in advance.

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[8] Social scientists have noted the potential problems of using Likert-scales for measuring respondents' levels of agreement and/or disagreement. Research has shown that such Likert-scales can result in 'respondent acquiessence', i.e. respondents simply agreeing with the statement without any reflective thinking. I piloted by questionnaire in May 2000 where I also asked respondents open-ended questions about their views on IT and Connected Kids. The results of this pilot suggested to me that sentiments and opinions suggested in the interviews were also reflected in the questionnaire. I therefore felt my questionnaire was adequately measuring these respondents' opinions and not leading towards biased results.

Results showed that actors' positive perceptions of Connected Kids hold, by and large, significant positive associations with actors' willingness to adopt Connected Kids. Actors' views of the other information technologies, i.e. the Web and databases, were not linked to their willingness to adopt Connected Kids. The fact that actors' views were positive towards Connected Kids, that these views coincided with their willingness to adopt Connected Kids, and that actors' views toward other information technologies held no relationship with their willingness to adopt Connected Kids suggested to me that these actors were perceiving Connected Kids as a unique information technology. For example, I knew from interviews that the project leaders promoted Connected Kids as a unique technology through their public meetings and focus groups with participants. In particular, Teresa and James spoke about Connected Kids as something specific for the youth and not-for-profits of Troy. Although respondents varied over what, specifically, this technology would end up being, all most likely shared the feeling that somehow Connected Kids was a more customized, 'made-for-Troy' technology. Respondents were therefore seeing Connected Kids as different from pre-made technologies such as the Web or email.

Measuring and analyzing diffusion structure

In addition to measuring perceptions and attitudes about Connected Kids and IT, I was also curious about the role social structure seemed to play in the sorts of attitudes and perceptions respondents were expressing. To do so, I used social network data on the frequency of communication among actors that I had gathered through my social capital survey (see section on social capital). As the diffusion literature discussed technologies diffusing through interpersonal flows of communication, I used my network data on communication to locate actors' positions within the network and then correlated these positions with actors' willingness to adopt Connected Kids.

Analysis and results

In UCINET, I located actors' centrality levels. As noted earlier, centrality refers to the number of ties actors have in a network. The more ties an actor has, the more central s/he is in the network.

As noted earlier, my centrality scores revealed two actors in the network as being more central than others. These two actors, Jane and Diane, worked for city and county government respectively, and their roles in these organizations put them in close contact with many of the youth-service agencies. In addition, both actors were involved in the Connected Kids project, and both had scored high on items measuring their willingness to adopt Connected Kids.

According to diffusion theory, central actors play a significant role in the diffusion process: they tend to be the ones that hear about the technology early on, and they often are among the first actors to adopt the technology. As their positions within the network tend to place them at the centre of many communication paths, these central actors are labelled 'opinion leaders'. An opinion leader is an actor holding many ties, and one moreover who communicates news about a technology to others in the network.

My findings regarding Jane and Diane coincided with diffusion's descriptions of opinion leaders and the roles they play in the diffusion process. Although Connected Kids is still incomplete, and thus I was not able to measure adoption within the community, these two actors' scores regarding their willingness to adopt Connected Kids, and their activitiy within the youth-service community, placed these two actors in important, strategic positions with regards to communication about Connected Kids. The fact that both were also positive about Connected Kids, and were willing to adopt this technology, further suggested that they might influence others in the network to feel positive towards Connected Kids and eventually adopt it.

In reviewing transcripts from my interviews, I found instances that seemed to support this view of Jane and Diane performing the role of opinion leaders. For example, Jane described to me an instance where she led a meeting where many youth-service agency representatives were present. In this meeting, Jane announced the Connected Kids project and briefly described her view of what this technology would eventually

accomplish for Troy youth and youth agencies. Her behaviour in this instance reflected Rogers' (1995) descriptions of opinion leaders: she was using her position within the social system to diffuse information about a technology. In a separate interview with Diane, I was told that Diane had visited many youth agencies to discuss their technology needs and see what the city might do to help 'bring them up to speed'. As with Jane, I felt that Diane's behaviour was again reflective of opinion leaders in that she was using her personal communication ties to discuss IT matters, and possibly Connected Kids.

Methodological lessons learned

My diffusion survey helped me see perceptions and attitudes toward Connected Kids as they relate to social structure and views regarding IT. The fact that Connected Kids was not being seen in relation to other information technologies was a surprise for me, and was not in keeping with diffusion theory. My social network data helped me locate opinion leaders through measures of centrality. When I compared these central actors with information gained from my interviews, I was able to further interpret whether an actor was or was not an opinion leader.

I had not expected, at the onset of my study, to use my network and qualitative data in this fashion. Originally, I had intended each theoretical and methodological approach to be kept separate from one another. The fact that all three approaches were brought together in this instance resulted from my familiarity with the data, and the level of comfort I eventually acquired over time in thinking from all three perspectives.

Bringing these three approaches together

I used three different approaches to study Connected Kids, each focusing on different dimensions of the same phenomenon. Social capital focused on interagency relationships and used social network analysis. Diffusion focused on perceptions of technology and roles in the diffusion process and involved the use of both a questionnaire and network data. Finally, SCOT looked at the social dynamics surrounding and involving Connected

Kids, the changing interpretations of Connected Kids, and it involved the use of qualitative methods. As I explored different research questions in the context of each approach, I sometimes stepped outside one approach and grabbed information and/or findings from one of the other two. Thus, in the social capital findings, my analyses of my social capital data were informed by data I gathered during my interviews, which were originally structured for investigating questions coming from SCOT. My diffusion results were also informed by my social capital results as well as findings from my open-ended interviews.

I found this tacking back and forth between comparing the three approaches' findings while also studying each approach's findings on their own to be a satisfying extension of the notion of triangulation. Triangulation, in the traditional sense, involves the use of three or more data sets to study a particular phenomenon. In my study, I used three theoretical and methodological lenses to look at aspects of Connected Kids and to gather different kinds of data: (a) Social capital and network analysis focused on ties and structure, (b) SCOT and interviews on interpretations and mutual shaping, and (c) Diffusion and questionnaires on perceptions of Connected Kids. These three offered separate results, but they also informed one another's findings.

If I were to step away from the theoretical and methodological divisions of this study, and instead focus my attention on qualities shared among all three approaches, shared ontological and epistemological views would emerge and cut across all three approaches. Ontologically, all three approaches view social phenomena in terms of systems. As Polkinghorn (1984) discusses, a systems view of inquiry places emphasis on how individuals are related to each other and arranged as wholes. Individuals are not seen as isolated components, but rather as inherent parts of particular systems. Theories that take on this systems view see that wholes have their own emergent properties, and that what leads to these emergent properties is not so much the accumulation of individuals' characteristics, but rather the arrangement (sometimes called 'structure') of these individuals in relation to one another. Individuals are seen, however, as having influence in how the system operates, yet this influence is balanced by an understanding that

certain qualities can only emerge through the interactions and relations of the individuals in the system (Polkinghorn, 1984).

Interpreting these three approaches through systems' lens thus shows how SCOT views technology emerging out of interactions among technical and non-technical elements, social capital sees trust and reciprocity emerging out of relationships among actors, and diffusion sees a culture's adoption of a technology as emerging out of the structuring of interpersonal ties. In addition, each theory considers how individual components influence the rest of the system. Thus, SCOT sees individuals as influencing one another's interpretations, social capital sees certain actors as potentially dominating the network through a disproportionate number of ties, and diffusion sees individual perceptions and past adoption histories as influencing adoption.

As systems theories, all three theories can also be seen as sharing similar epistemological outlooks. The received view of social science emphasizes the use of inductive and deductive reasoning, where scholars attempt to classify entities in reality in order to ascertain the relationships among these entities. In doing so, scholars strive to locate universal laws within a static world in order to make predictions. In contrast, systems theories see the world as a series of dynamic wholes. All entities are interdependent with one another, none can exist without the others, and through these interactions, certain qualities emerge that cannot be accounted for through analysing individual elements. Deductive reasoning cannot account for these emergent qualities.

Rather than using deductive logic to try to comprehend systems, the logic of dialectic gains access to these emergent properties.[9] Dialectic logic, commonly associated with the process of thesis, antithesis, and synthesis, can also be seen as an example of this comparison of parts to the whole and back again to gain a fuller understanding of the social phenomenon. This tacking back and forth between parts and the whole is evident in all three theories described in this article. Through social capital, one looks at how individual actors relate to the overall social network; individual roles appear as the structure of relations is analysed. SCOT theory sees actors in relation to their respective

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[9] Hegel (1956 often quoted as the father of dialectic thinking. His work lead to Durkheim's (1964) notion of 'holism', and Dilthey's (1976) description and promotion of hermeneutics as a method of inquiry for the social/human world.

social worlds, and interactions among individual actors lead to the emergence of a technical artefact. Finally, through diffusion, one analyses how individual perceptions and attitudes coincide with the structure of the system. In doing so, one locates particular roles for actors and these roles are seen influencing the diffusion process.

Thus, in seeing all three theories as examples of systems theories, one is able to better see the ontological and epistemological assumptions influencing how a scholar employing these theories approaches his/her study. Studies employing a systems view do not, ultimately, lead towards prediction. Likewise, with their emphasis on dialectic processes, systems theories cannot make universal claims: systems views prevent one from hoping to uncover static relations among elements. Interactions within the systems are continually occurring, thus influencing and changing the individual components. As social scientists often hope to ultimately make generalizable, inferential claims, some readers will see these aspects of systems theories as serious limitations. Hegel (1956) as aware of these arguments against the dialectical method, and answered these arguments by noting that deductive reasoning was a more elementary stage of thought, whereas dialectical thinking moved beyond fixed categories to gain a more complete understanding of entities as fluid, dynamic, and interactive processes.

Conclusion

My research certainly does not offer the last word on IT studies, systems theories, or the use of mixed methods. My hope is that through a reflective portrayal of the three approaches used in this study, I have been able to offer readers a sense of how the different approaches stretched my views of the social dynamics surrounding Connected Kids, as well as how they supported one another in fundamental ways. My ultimate goal has been to understand how community members of Troy understand and influence Connected Kids and one another. In doing so, however, I took a number of methodological paths that needed to be brought back into focus. In reflecting on the similarities and differences of these three approaches, I am unable to say which approach

has served me best in understanding the dynamics of this community and their relationship to Connected Kids. Future research will build on lessons learned and continue observing Connected Kids through the lens of social capital, SCOT, and diffusion.

References

Bijker, W.E. (1995) *Of bicycles, backelites, and bulbs: Toward a theory of sociotechnical change*. London: MIT Press.

Bijker, W.E. (1997) 'The social construction of fluorescent lighting or how an artifact was invented in its diffusion stage.' In: W.E. Bijker and J. Law (eds.) *Shaping technology/building society: Studies in socio-technical change*. Boston: MIT Press: pp. 75-104.

Blanchard, A. and T. Horan (1998) 'Virtual communities and social capital.' *Social Science Computer Review*, 16: pp. 293-307.

Bourdieu, P. (1986) 'The forms of capital.' In: J.G. Richardson (ed.) *Handbook of theory and research for the sociology of education.* New York: Greenwood Press: pp. 185-206.

Brass, D.J. (1992) 'Power in organizations: a social network perspective.' In: G. Moore and J.A. Whitt (eds.) *Research in politics and society*. Greenwich: JAI Press: pp. 295-323.

Bryman, A. (2001) *Social Research Methods*. Oxford: Oxford University Press.

Burt, R.S. (2000) 'The network structure of social capital.' In: R.I. Sutton and B.M. Staw (eds.) *Research in organizational behavior.* Greenwich: JAI Press: pp. 31-56.

Calantone, R. and C.A. di Benedetto (1990) 'Successful industrial product innovation: An integrative literature review compiled by "economics and economic history".' Conneticutt: Greenwood Press.

Clarke, A.E. (1991) 'Social worlds/arenas theory as organizational theory.' In: D.R. Maines (ed.) *Social organization and social process: Essays in honor of Anselm Strauss*. New York: Aldine de Gruyter: pp. 119-158.

Coleman, J.S. (1990) *Foundations of social theory*. Cambridge: Belknap Press of Harvard University of Press.

Dilthey, W. (1976; orig. 1900) 'The rise of hermeneutics.' In: P.Conneron (ed.) *Critical sociology*. New York: Penguin Books: pp. 104-116.

Durkheim, E. (1964; orig. 1895) *The rules of sociological method*. New York: Macmillan.

Fujimura, J. (1992) 'Crafting science: Standardized packages, boundary objects, and "translation".' In: A. Pickering (ed.) *Science as practice and culture.* Chicago: University of Chicago Press: pp. 168-211.

Harrison, T. and J. Zappen (2003) 'Methodological and theoretical frameworks for the design of community information systems.' *Journal of computer-mediated communication*, 8 (3). Http://www.ascusc.org/jcmc/vol8/issue3/harrison.html.

Harrison, T., J. Zappen, and C. Prell (2002) 'Transforming new communication technologies into community media.' In: N.W. Jankowski and O. Prehm (eds.) *Community media in the information age: Perspectives and prospects.* Cresskill: Hampton Press: pp. 249-270.

Hegel, G.W. (1956) *The philosophy of history*. New York: Dover Publications.

Katz, M.L. and C. Shapiro (1986) 'Technology adoption in the presence of network externalities.' *Journal of Political Economy*, 94 (4): pp. 822-41.

Knack, S. and P. Keefer (1997) 'Does social capital have an economic payoff? A cross-country investigation.' *The quarterly journal of economics*, 3: pp. 1251-1288.

Lin, N., K. Cook and R. Burt (2001*) Social capital: theory and research*. New York: Aldine de Gruyter.

Littlejohn, S.W. (1996) *Theories of human communication*. Belmont: Wadsworth.

MacKenzie, D. and J. Wajcman (1999) 'Introductory essay: the social shaping of technology.' In: D. MacKenzie and J. Wajcman (eds.) *The social shaping of technology*. Philadelphia: Open University Press: pp. 3-27.

Mintrom, M. (1997) 'Policy entrepreneurs and the diffusion of innovation.' *American journal of political science*, 41 (3): pp. 738-770.

Polkinghorne, D.E. (1984) *Methodology for the human sciences: systems of inquiry*. Albany: SUNY Press.

Putnam, R.D. (1993) *Making democracy work: Civic traditions in modern Italy*. Princeton: Princeton University Press.

Putnam, R.D. (2001) *Bowling Alone: the collapse and revival of American community*. London: Simon & Schuster.

Rogers, E.M. (1995; orig. 1962) *Diffusion of innovations*. New York: Free Press.

Schuler, D. (1996) *New community networks: Wired for change*. Reading: Addison-Wesley.

Scott, J. (1991) *Social network analysis: A handbook*. Newbury Park: SAGE Publications.

Shim, S. and A. Kotsiopuls (1994) 'Technology innovativeness and adopter categories of apparel/gift retailers: From the diffusion of innovations perspective.' *Clothing and textiles research journal*, 12: pp. 46-57.

Star, S.L. and J.R. Griesemer (1989) 'Institutional ecology, "translations" and boundary objects: Amateurs and professionals in Berkeley's museum of vertebrate zoology, 1907–39.' *Social studies of science*, 19: pp. 387–420.

Tsai, W. (2000) 'Social capital, strategic relatedness, and the formation of intraorganizational linkages.' *Strategic management journal*, 21: pp. 925–939.

Wasserman, S. and F. Faust (1994) *Social network analysis: methods and applications*. Cambridge: Cambridge University Press.

Wellman, B. and M. Gulia (1999) 'Virtual communities as communities: Net surfers don't ride alone.' In: M.A. Smith and P. Kollock (eds.) *Communities in cyberspace*. New York: Routledge: pp. 167-194.

Valente, T.W. (1995) *Network models of the diffusion of innovations*. Cresskill: Hampton Press.

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Hidden in the small ads

Researching working-class lesbians

Abstract

This article will consider strategies and difficulties in attempting to conduct research on a 'hard to reach' group, working-class lesbians, in the United Kingdom. It is based upon my PhD research which reports on the life biographies, identities and everyday experiences of women who identify themselves as working-class and lesbian, achieved through fifty-three interviews with women living in a range of localities: the Highlands, Edinburgh, Glasgow in Scotland and Yorkshire and Manchester, England. The particular difficulties of researching lesbian lives have been outlined by many (John and Patrick, 1999; Kitzinger, 1987) but I would argue that problems of exclusion are compounded by class, given that working-class women are often absent from support networks and scene space, as well as research agendas. I illustrate the difficulties, and pleasures, both practically and theoretically and include quotes from interviewees as a way of demonstrating these issues. I end with a discussion on reciprocity or irrelevance to those 'excluded' participants, the 'usefulness' of my research, and the in/competence of the 'authoritative', 'reflexive' researcher. While my research is situated within Sociology/Women's studies, I believe that the questions raised, in terms of conducting qualitative research, finding a sample and establishing rapport (or otherwise) with interviewees, are more broadly applicable across disciplinary boundaries. Issues of class inequality and sexuality are relevant across the social sciences and indeed across society as everyone occupies these categories, although only some are 'marked' by them.

Finding working-class lesbians

I write this essay from the position of having found my research sample, thus perhaps solving the dilemma of how to locate working-class lesbians. But the practical difficulties including financial, spatial and time constraints and the emotional costs, for both my research participants and myself, are worth highlighting in order to cast light on these issues. The socially excluded are not so easy to locate or recruit but failure to 'find them' certainly results in further exclusion from academic agendas.

Research into lesbian lives often rests upon access into scene spaces or 'key informants'. These approaches, although still utilised, worked less for my research – often I was advertising in the 'wrong' places, where inclusion rested upon 'knowledge', commercial savvy – and gender. Yet I knew that working-class lesbians were 'out there' and, in fact, everywhere. So where did I find them? Valentine (1995) asserts that lesbian spaces are there 'if you know what you're looking for', suggesting that while lesbians may not have recognisable spaces, they are not without location. However, 'recognition' and 'location' differs across class contexts. I believe that my own identifications enabled me to understand where working-class lesbian may be but most of my research methods used have involved guesswork (hard work) and 'risk' – these 'risk' have been borne by interviewees too in, for example, the lack of a safe space.

The problems of recruiting research participants were highlighted in my very attempts to utilise pre-existing lesbian and gay infrastructures and support groups. I forwarded details of my project to an organisation established for lesbians and gays with disabilities and to other groups and voluntary agencies, including several that provide support for those who experience homelessness. While some groups had an explicit welfare agenda, others were more implicit; often social functions merged with more practical and pragmatic purposes and in these groups I was looking for individuals who inhabited a different kind of 'scene' space, other than commercialised venues. But, to my disappointment, generally the confused response when contacting specific groups was that they did not think they 'had any working-class lesbians'. Contacting organisations raised both issues of access and whether or not working-class lesbians were included in

their networks; networks with clearly inclusive agendas still seemed to be somewhat classed, through the absence of working-class participants. Yet there is a limit to the assumptions I can make here given that I was not an 'insider', I was, to some extent, an intruder and in this respect was occasionally met with a degree of suspicion. Concern about my research request seemed motivated (justifiably) by issues of confidentiality and protection, as well as perhaps fear. One group stated: 'Obviously we have to respect our young people's and volunteers need for confidentiality and privacy and so we do not participate in research projects.'

The problem of participant recruitment, maximised when trying to reach certain groups and individuals, has been managed in various ways by different researchers. Adler and Brenner (1992), for example, were grateful to lesbian organisations that enabled them to use their mailing lists whilst maintaining the confidentiality of their membership. This perhaps raises questions about the varying 'authority' of different research projects. Still, my own desire to research seemed somewhat unimportant when considering the limited resources, both in terms of time and finances, such groups had.

I continued my search for respondents by regularly advertising in the lesbian and gay press. I drew up an advertisement in which I gave some brief personal details, outlined my research subject and my intentions and offered assurances of confidentiality for potential participants. As well as using national media, such as 'Diva' [1] and the Libertas newsletter,[2] local newsletters and organisations were also contacted. An advertisement was placed in 'Centrepoint Magazine', which advertises lesbian and gay events in Glasgow, the Glasgow Women's Library newsletter and in the newsletters of the Rural lesbian group, the Older lesbian group and the Manchester lesbian group. All three groups finally participated in this project.

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[1] UK lesbian magazine.
[2] This newsletter, distributed throughout the UK, operates from the women's bookshop, Libertas, in York and was a convenient way to begin my search.

Advertisements acted as a preliminary facilitator but more effort was often required.[3] I continued writing in the lesbian and gay press, believing that short articles would perhaps offer more appeal and generate more interest than research requests alone. I wrote two articles that appeared in 'Scotsdyke' (a page within the 'Scotsgay' magazine), and in 'Diva', while considering the affordability and accessibility of such magazines and consequently, their potential audience/buyers. Nevertheless, these methods did generate interest, which I followed up with phone calls and meetings.

To a great extent, I replicated the methods used by Adler and Brenner when identifying areas of 'lesbian concentration', while remaining aware of the potential to generate exclusions in the research sample. In their study, Adler and Brenner used: (1) informants from the lesbian community; (2) the location of lesbian social spaces; (3) the location of lesbian businesses, professional services and social services; and (4) two mailing lists of lesbian organisations. I wanted to elicit responses that were not drawn from such places alone, a strategy which perhaps finds very particular people in particular locations, for example, those who self-identify as lesbian and are involved in the 'community' in some way.[4] Such strategies also reinforce the tendency to concentrate on the urban at the expense of the rural due to the lack of such extensive infrastructure and scene spaces within rural locations (Binnie and Valentine, 1999). Attempts were made to generate a rural sample, in particular a focus group was carried out in the Highlands with the Rural Lesbian Group.

Thinking that more people would see the research request if it was on a poster, I produced posters and distributed them in lesbian and gay cafes, bars and community spaces in Edinburgh, Glasgow, York and Manchester. However, despite repeated visits, I noted the near absence of women from more commercial venues: the problems of recruitment were daily experienced as I visited such places. Some places seemed rather 'cut off' perhaps as a result of where they were situated, thus ensuring that only 'insiders'

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[3] Producing advertisements was a relatively inexpensive, though time consuming, process and was quite effective in generating initial responses. Unfortunately there was a £50 charge for advertising in one magazine – its rates for business advertising were considerably more favourable.
[4] Nevertheless, Dunne and Patrick (1999) found this a productive method in investigating the extent and impact of poverty and social exclusion on lesbians and gay men in Glasgow.

knew of their whereabouts.[5] I did worry about the effectiveness of (and possible arrogance in) placing posters on walls, claiming space for myself. But several 'women's spaces', only had lesbian events once a month: what spaces could be claimed? Nevertheless, posters remained where I placed them throughout the research period, which was important given that there was often a gap between respondents initially seeing my poster (and articles) and then making the decision to get in touch, as Sally exemplifies:

> 'I've been down to Mardi Gras, the first one that I'd been to and was going into these stalls and things and I picked up a leaflet and your ad was in it. I didn't respond to it until months later. It's quite a few months away, 'cause I just started reading it again.'
> (Sally, 37, Manchester)

Contact seems accidental, almost a matter of 'luck'. But in was in the repetition of such 'lucky' encounters that I generated a sample. While Valentine (1993) found that 'snowballing' research participants was an effective research strategy this was not very effective in my case, perhaps due to a reluctance to 'out' potential candidates. Nevertheless, meeting certain 'key informants' was both fortunate and beneficial.

Margaret, the organiser of the Rural Lesbian Group, indicates the difficulties of conducting research into a sensitive issue with a 'hard to reach' group. The group venue was chosen as a location so far free from homophobic violence but safety and comfort in this environment was not felt by all; instead classed discomforts were generated. Sexuality and class combine to reduce possibilities and are the categories through which possibilities, such as meeting, socialising and supporting, must be managed. Margaret believes that location and environment contributed to the absence of working-class lesbians from the monthly meetings of the Rural Lesbian Group. Such exclusion is particularly significant considering the difficulties faced by such a group. As Margaret outlines:

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[5] Location is constrained by finances and concerns about visibility and safety as voiced by Glasgow Women's Library, which is situated in a back alley.

'The venue for our monthly meetings has been described as "posh". It's also safe, which is the main thing, but it can be a bit of a culture shock ... I am concerned about working class lesbians in [the Highlands], I know there have been homophobic assaults on them and I have heard that some women are getting into prostitution, but some working class lesbians are scared to come out in their lives and to the group. It's very difficult.'

Margaret highlights not only the difficultly in reaching certain people but also the near impossibility of doing so when there are no apparent spaces to 'come out': the situation described by Margaret is a very difficult one. When presented with the idea of taking part in my research, via Margaret, many women in the group were reluctant, leading Margaret to be concerned about the usefulness of such a meeting:

'There were nine women at the meeting last month and they didn't say very much about the idea of having a working class lesbian focus group. Four of them are very new to the group. Someone else was asking how are you defining working class... I don't want you to have a wasted journey …'

'Wasted journeys' were a frequent occurrence as potential interviewees failed to materialise in agreed locations, dates were changed at the last minute to be changed again and forgotten. Generating a sample required numerous phone calls, meetings, chasing dead ends, hanging around appropriate environments and social settings (and hours and hours of conversation and interaction). I am not resentful of 'wasted journeys' and 'dead ends', especially as my perception of such events was that people were agreeing to participate in a partial way, given that this could be fraught with tension: coming out on the one hand but fearing what that could entail on the other.

In the end, I conducted interviews with fifty-three women from Scotland (the Highlands, Glasgow and Edinburgh) and England (Yorkshire and Manchester). Three cities and two regions are included in my research as a result of my desire to represent the urban and the rural, and motivated by ease and convenience as well as by my eagerness

to generate a Scottish sample. I knew Glasgow and Edinburgh very well, Yorkshire and Manchester fairly well, but admittedly did not now much about the Highlands. These locations represent my attempts at including and managing the familiar and the unfamiliar.

Focus groups were conducted at the usual meeting place of the group. The majority of individual and 'paired' interviews were conducted at interviewees' homes, several were conducted in cafes and four interviews were conducted in my own house. I realise this may not be a recommended strategy but I felt that I perhaps should be willing to 'risk' as much as interviewees.[6] The issue of where and when to do the interview raised concerns for myself and for interviewees. There was a need for a 'safe space' but room hire was too expensive to be considered, leaving homes, cafes and libraries as make-shift interview sites.

Perhaps surprisingly, lesbian and gay venues and cafes were often 'uncomfortable' places to conduct interviews. I would have to repeatedly ask if the music could be turned down as I was recording, which generated feelings of annoyance as I felt I had a claim on that space. My immediate response was to consider the presence and consequent tensions of lesbians using predominantly gay male space. While I believe this is still an issue, on reflection I can see why my presence as a researcher was problematic: an 'outsider' coming in to gather and extract 'interesting' information.

Issues of in/accessibility and the financial constraints upon both lesbian organisations and myself hindered attempts at generating inclusive participation. These tensions also highlight the difficulties in 'coming out' without access to 'community' infrastructures – whether that be accessing scene space, taking part in lesbian groups, or participating in research projects.

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[6] However, the number of abusive (and curious) phone calls I received will act as a deterrent to complacency when considering issues of safety and danger.

Insider status or 'those kind of people'

My call for participants required identification with 'lesbian' and 'working-class', often problematic categories, regularly used as an insult, or are seen as a stigma. To accept a working-class identity, Reay suggests, is to accept a 'spoilt identity': 'dis-identification' from being working-class is another facet of managing a stigmatised identity, indicating reluctance to become known and placed through the markers of class (see Frazer, 1989; Reay, 1996; Skeggs, 1997). I had to 'risk' using these terms, in order to present my research honestly, rather than attempting to create conflict, as one email correspondent argued: 'How do u define working class?? If we all work then we're working class rnt we??? I think u r trying 2 create problems that dont exist the term working class is old hat as r the unions they no longer have a place in our society 2 day we've now moved on thanx goodness.'

This statement perhaps reveals more about the continued relevance of class given the emotional response provoked here, although it also illustrates the sensitive and controversial environment in which to explore continued class inequalities. Significantly, it is the demise of class analysis, and a corresponding 'language' to describe class inequalities, which left potential interviewees feeling uncertain, hesitant and reluctant. Here, Mandy speaks of the difficulties in coming forward as 'one of them':

'When I seen the advert for working-class lesbians I actually didn't think, I actually thought I'd be the only person…I mean I don't know how many people you got but I actually didn't think you'd get that many because thinking about it I was actually the only definitively, like I'm so obviously working-class, but I don't know, maybe people hide it better than me (laughs). I do notice people's appearances but it's not something I judge then on 'cause I wouldn't like to be judged on myself because I can often look rough.' (Mandy, 22, Yorkshire)

Mandy's statement points to the negative connotations of being working-class (e.g., looking 'rough'). Respondents had to be 'out' about their class position as well as their sexuality. Being 'out' in multiple social positions, rather than sexuality alone is rarely

considered in research into lesbian life experiences. However, the limitations in asking people to 'out' themselves needs to be considered in relation to the gains of so doing: many women had both positive and negative experiences, associations and identifications with being both lesbian and working-class.

Of course, there will always be those who remain excluded. A painful part of my research has been in coming across a (perhaps) permanently 'excluded', 'lost sample' as one woman wrote:

'As a working-class lesbian in my teens I didn't realise that such a thing existed. I'm now in my thirties and it's taken me that long to get to grips with the idea. At school we all knew about gay men but gay women, I mean, it was never discussed unless in terms of disgust ... I married and had three children, just as a nice working-class girl does and now I'm a nice working-class woman with a husband and a lonely heart.'

But just because this woman's particular experiences and feelings are not covered in my research does not mean she is 'lost'. I am aware of the way that I have constructed a group for the purposes of my research and also of the ways that this 'group' exists outwith my own agendas.

Like my interviewees, I often felt ambivalent about 'outness' – especially when 'advertising' my lesbian and working-class 'credentials' on notice boards and in newsletters (even in 'friendly' spaces). Nevertheless, the effort made was 'worth it',[7] although I'm left to consider the often harder efforts made by interviewees, such as Lynn:

'I felt that I had to try and overcome that, to get back in there, I felt that I had to become more involved and face up to this and maybe work through it. I made a decision, I mean I heard your thing was going to be the last time, I though I'm *definitely* going to go in,

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[7] The effort was worth it in terms of being able to produce a PhD – but the costs involved are indicated through the limitations of this alone, the limitation in producing an academic account rather than effecting material changes.

definitely, I'll make an effort and speak to this woman and say "Right I'm prepared to do this 'cause I feel like I need to take a step." I know I need to.' (Lynn, 44, Glasgow)

While Lynn is motivated by personal concerns, Sharon's motivations seem more politically orientated, her lesbian status seems less 'problematic', she knows what she wants, even if steps still have to be taken to get there:

'…[I] think we need to encourage the development of gay and lesbian, of what the issues are for gay and lesbian people. There's still a lot of problems out there, access to health care, to resources, to venues that aren't just about alcohol, that are family orientated. There's so much still needs to be done.' (Sharon, 47, Glasgow)

Identification with research participants is generally seen as positive, providing an 'insider status', where the spaces for lesbians to speak about their lives are often very limited.[8] I see my own identifications as consequential, as respondents often cited my 'sameness' as a reason for responding, a way of, as Sharon suggests, getting things said and done. For Fiona, the fact that I identified as working-class does not generate suspicion and doubt but instead provides the possibility of understanding:

'Em, I was reading your article, that sort of made me have a sense that yeah, the fact that you identified as being working-class as well made me think you were likely to be coming from a more intelligent, for want of a better word, perspective on it all.' (Fiona, 29, Edinburgh)

Identifications were made by interviewees (and myself), on the basis of 'sameness', including sexuality and class, as well as age and accent. For example, Angela's desire to participate was influenced by our shared location; 'It's important to me to help you,

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[8] There are many studies that make this point, for example: Dunne, G.A. (1997) *Lesbian Lifestyles. Women's work and the politics of sexuality*. London: Macmillian. Weeks, J., B. Heaphy and C. Donovan, C. (2001) *Same Sex Intimacies: Families of Choice and Other Life Experiments*. London: Routledge. Weston, K. (1997) *Families We Choose: Lesbians, Gays, Kinship*. New York: Columbia University Press.

particularly when you said you were from Glasgow!' Identifications were motivations and these came to light in the research processes itself as well as in recruiting.

I imagine both respondents and myself were using our stories to 'check each other out', in terms of 'sameness' as well as 'difference'. In Scotland, often where I come from (Drumchapel) became the 'evidence' of my working-classness and enabled trust. However, in Manchester and Yorkshire this was often not equated in the same way and the subtleties of accent as a signifier was also perhaps lost. As Sally notes:

> 'Well, when you spoke to me on the phone, I was judging you and I didn't think that, when I met you, you looked different to what I perceived on the phone … I just got this different idea of you and I couldn't, I could hardly tell what you said.'
> (Sally, 37, Manchester)

Sally's comments are not just about class – but the possibility of expecting one thing and seeing, hearing and meeting another can be effected by the visual, spoken and embodied signifiers of class.

There were difficulties in communicating even with the commonality of shared location. Becky also came from Drumchapel and it was difficult to talk about the specific social and economic meaning of that space when so much of our conversation was punctuated with 'I know what you mean'. Interviewees also offered challenges to the meaning (worth?) of my classed location, which provoked (unstated) defences within myself, proving that I was neither detached from nor immune to 'negative' emotion. Here, Sharon's statement changes in response to knowing where I am from:

> S: 'I think we should try and stay in a working-class area to try and bring, to keep the standards up, you know. Try and keep it, I don't know what area in Glasgow you come from?'
> Y: 'Drumchapel.'
> S: 'Within Glasgow there's very much pockets of deprivation. As soon as people get the chance to move out they should move out.' (Sharon, 47, Glasgow)

The reasons for escaping and 'investing' in location can be classed and there may be classed discomforts in locating ourselves. I am not reproducing this statement in an attempt to challenge or humiliate my interviewee but rather to show how the processes of class operated within the interview itself and to suggest that interviewees should not be positioned as passive in this respect. For Sharon, getting out of Drumchapel is understandable, even recommendable and her investment in 'staying put' shifts to a pragmatic desire to escape that which is not valued. But if I argue that Drumchapel is a place of worth, a place where I come from and belong, then does this inevitably put me into a classed conflict with my interviewee with whom I must try to establish rapport? If one's 'home' is also seen as a 'pocket of deprivation' then how can classed embarrassment and defence, following from such positioning, be avoided and perhaps even reconciled?

Sayer (2002) discusses why class is an 'embarrassing' subject to discuss. However, working-class homes, 'lifestyles' and 'displays' were not inevitably 'embarrassing', nor was talking about these factors inevitably awkward. Conducting interviews in women's homes did raise questions of (classed) dis/comfort: occasionally apparently embarrassed interviewees would apologised for the 'mess' of their houses, these places then becoming devalued signifiers of class position. Yet, mostly I felt 'at home' in these settings, including when I was in fact at home. Issues of sameness and 'insider status' were relevant to interviewees' motivations in participating in my research, and these appeared in interviews through shared references points, even shared 'embarrassment'. 'Differences' also had to be negotiated in the interview context both between individual women and myself and amongst interviewees, in the case of focus group and paired interviews, highlighting the difficulties – and ease – in talking about class. Rather than seeking to reconcile and avoid class positioning I would like to make these processes more apparent, to talk about class in the interview context.

'So tell me a little bit about yourself …' Talking about class

Instead of presenting a monolithic notion of class, questions were constructed to investigate how class permeated the women's lives in the context of growing up, schooling, job opportunities, family, community and sexuality. Interviewees were asked to discuss their own experiences at the beginning of the interview but more abstract questions were asked towards the end, such as 'What does class mean to you?' Savage, Bagnall and Longhurst (2001) view the strategy of asking questions on class at the end of the interview as advantageous, highlighting that some research may be criticised for using loaded questions, which then prompt them to reply in 'classed ways'. This, however, again illustrates the particularly contentious nature of researching class. There is often an assumption that the researcher has a (unjustified) pre-set agenda, whereby beliefs are rigid, inflexible and imposed. Through prior reading of advertisements and/or articles, all women who took part in my study knew that the central focus of my project was to detail the life experiences of working-class lesbians.

Having introduced class up front was neither negative nor predetermining of the agenda and outcome. Nevertheless, aware of such criticism, I made a concerted effort to discuss their own stories rather than beginning with the more abstract questions, further away from their concrete lived experience. I do feel that working-class women's accounts of what class is, means and does are vitally important as it is not just the prerogative of academics to decide what a proper 'intellectualised' meaning of class is – or to decide upon whether it exists or not. The term 'working-class' is prevalent throughout the women's accounts and I do not believe that it would have been used so extensively to describe their own experiences had it been imposed.

Interviewees were also encouraged to expand on particular areas that were of interest to them, without feeling that they were required to provide the 'right answer' or that they had to fit in with stereotypical expectations of what a lesbian should be: 'I was hoping you wirnae going to ask how I ended up wi' a bairn and you never!' (Jill, 28, Edinburgh). In responding to my (end) question: 'What did you think I would ask?' (asked as a way of bringing up issues that we had perhaps missed out), May states:

'Eh, I don't know, I didn't have much idea … I think financial questions you know and stuff like that but it's been a nice surprise. I thought it might be more focused on class and not as personal.' (May, 23, Yorkshire)

This points to the pleasure, and surprise, as well as difficulties, encountered by respondents and myself. For example, there is a 'problem' of how to express emotions and feelings attached to class and sexual identity when as Liz remarks:

'I don't know how useful my answers have been. I think it's more a subconscious thing, a subconscious way of identifying. It's kinda so much ingrained up here it's difficult to verbalise, it's hard to make what you feel into words that make sense.' (Liz, 23, Manchester)

For Liz discussing class and sexuality is something which is felt, something known and experienced at an emotional level, yet at the same time it is hard to communicate, precisely because of its 'obviousness', something which 'just is'. The answer is clear, yet ambiguous, certain yet doubtful and this makes discussion difficult. Discussion is also difficult if you speak in fear of 'bad mouthing' yourself: not conforming to what a lesbian should be, not earning enough and not speaking enough.

There were 'interruptions' to the structuring of interviewees' answers as well as my questions. I also experienced difficulties in 'expression', but I do not feel these difficulties render the process a failure. I do not want to emphasise the coherence or flow of the interview at the expense of the more 'creative' moments as well as pauses and silences (which could be characterised as mistakes and seen as negative):

M: 'I don't remember what the last question was?'
Y: 'No, neither do I.'
M: 'Bloody hell!' (laughs). (Mavis, 52, Edinburgh)

These incidents were in fact part of having a relaxed conversation. Mavis' laughter indicates pleasure, and perhaps relief, rather than uncertainty and confusion. Similarly,

interviewees' responses, questions and interventions challenged my position as 'authoritative' researcher.

It is important to illustrate the ways that interviewees were not lacking authority or opinion within the interview situation itself:

> Y: 'Are people ok to carry on?'
> C: 'Carry on, if we're bored with you we'll tell you!' (Cathy, 37, Manchester)

Cathy and her associates make clear that it is for them to decide when they are bored with me, which will ultimately end the interview. Authority is consciously recognised and played with, so that the classed dynamics are somewhat destabilized. The irony in Fiona's comment 'I don't know how this relates to class, but it's you that's doing the PhD darling!' (laughs) can hardly be missed.

Interviewees also challenged each other, apparent in paired interviews and in focus groups. Consideration was given on whether to interview couples together or apart – ultimately the decision rested with participants (see Valentine, 1993). While Mansfield and Collard (1989) warn of the tendency of couples to produce 'consensus accounts', whereby one member takes the lead and the other then seeks confirmation from the other, there is also a counter view that these can produce conflict rather than consensus. I certainly heard numerous disagreements and discomforts, as well as agreements, within focus groups and paired interviews, as demonstrated by Kelly and Lisa. The first example illustrates the difficulties of relating personal and emotional events and feelings to an audience and the second demonstrates the competing voices within this:

> K: 'It's really weird being interviewed, I'm going to get all embarrassed (laughs), I think it's 'cause you are here as well.'
> L: 'Do you want me to disappear?'
> K: 'No you're alright. It's just that it's really *personal* to me you know.'

And later:

> K: 'This is why I thought we better not have an interview together, I'd just dominate it all.'
> L: 'She hasn't dominated at all, has she?!' (laughs) (Kelly, 23, Lisa, 23, Yorkshire)

Class, for Kelly, is a personal matter and also something which could potentially be embarrassing when voiced and heard, yet Kelly is determined to make her opinions count, even if this means dominating the interview: silence turns to communication, which in turn 'silences' Lisa.

The issue of domination and silence was also apparent within the four focus groups. All of the (pre-existing) groups had member(s) who identified as middle-class – I did not separate out members into 'middle-class' and 'working-class' as this would have been unnecessarily divisive, perhaps more so in the young person's group.[9] But there were certainly tensions as the class differences of respondents were played out and conflicts, disagreements and re-definitions all occurred:

> K: 'Well I think we're all in the same position, whether you're middle-class or working-class or upper-class or lower-class.'
> D: 'Yeah, but you're middle-class.' (Kay and Doris, Manchester Focus Group)

Kay responds to the 'accusation' of being middle-class by asserting her present lack of money, as well as outlining her own attitudes – a middle-class position is rejected when she states she is not a 'snob' but Kay does not claim a working-class identity or position, she merely tries to diffuse the importance of these 'differences'. Often members of all focus groups asked more demanding questions of each other, which were generally positive and well received, than I could/would have asked of them. But this very issue

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[9] Also, as someone entering into their meeting space, I did not have the authority or desire to do this.

created some tension around class positions and feelings, especially in the Rural Lesbian Group, leading to the disintegration of the focus group session.

Class can be a very emotive issue and can generate a lot of hostility. In the Rural Lesbian group some members felt and voiced resentment at others, including myself, based on perceived class positioning and classed 'advantages' were accusingly highlighted and rebutted. While Margaret, the group organiser through whom my request for respondents had gone, 'warned' me of the potential lack of response, given that group composition and membership varied from month to month and that, in her opinion, the venue was a 'posh' one which excluded working-class lesbians, I did perhaps enter the setting quite naively and unthinkingly. Before I had a chance to establish rapport I found that I was being reminded that not all middle-class people have an easy time they, for example, 'have mortgages to pay'; my flyer preceded me and the call for 'working-class lesbians' seemed to be read as an exclusion of experience and potential contribution. I had to re-state what my focus was and why, as it would have been dishonest to do otherwise. Having travelled for three hours to get there to interview 'willing' respondents, arranged via Margaret the group organiser, and facing a three hour return journey, this was rather difficult to do in an impartial manner. The effect of this was that the group split into two – Elaine and Fliss wanted to participate while the rest did not. Despite such problems, I do believe all four focus groups were extremely worthwhile: there are inevitable conflicts generated in such situations. The focus groups differed from individual and paired interviews because of their more interactive nature, and in all situations the discussions were enthusiastic and the process was inspiring, as well as challenging.

In an attempt to overcome tension, carried through to the point of division, I adopted a different strategy for the Young Person's Group. Discussions were facilitated by the drawing of 'maps' to chart feelings of belonging across different spaces: visual displays were made of, for example, home space, leisure and group space with labels attached which described members feelings for, and associations with these locations. Drawings were used to initiate dialogue about what they represented and I believe this generated a sense of comfort in knowing that I was genuinely interested in hearing, and

seeing, their experiences, positions and feelings, rather than launching into what may be felt as interrogation.

Frazer (1988) and Phoenix and Tizzard (1996), note the intense discomfort experienced by the young people of their research; their respondents' struggles to talk about class were characterised by ambiguity and hesitancy. However, in agreement with Qvartup et al. (1994) I would argue that young people should not be positioned as immature, unreliable or unsafe research subjects – I adapted my methods to avoid such positioning, but also to avoid 'class conflict', which is somewhat ironic given that my research aim was to uncover such 'conflicts'. Interviews were conducted with my own fears and desires in mind, as well as those of participants, a theme which was often re-visited and re-negotiated.

Why I did what I did: Standing up for class

I began my research with an awareness of the depth and diversity of debates surrounding methodological issues in feminist research. Although I offer a brief overview of the insights that I feel are pertinent to my project, I will not attempt to produce an extensive review of feminist methodologies as these have been, and continue to be, well documented and debated. Nevertheless, as Skeggs notes, 'To side-step methodology means that the mechanisms we utilise in producing knowledge are hidden, relations of privilege are masked and knowers are not seen as located' (1997: 17). Such a side-step also fails to acknowledge the cultural, economic, social and educational capitals central to the production of knowledge, a point that standpoint theories try to uncover.

Feminist standpoint theory centres on the claim that knowledge derives from experience, experience of oppression then engenders particular knowledge (Smith, 1987; Stanley and Wise, 1993). Standpoint theorists argue that women's experiences entail particular knowledges because women are placed in a position of gender inequality and in this way they attempt to displace the focus on 'transcendental' and 'objective' research

which is a 'partial and perverse perspective available from the ruling genders' with a concern for women's concrete, materially grounded experiences (Harding, 1987).

There are many and varied sources of discontent with feminist standpoint theory; not only is it seen to equate ontology with epistemology, emphasising identity and experience over political practice but it is seen to neglect the ways in which people are members of multiple dominant groups and multiple subordinate groups. By privileging gender, other socially salient categories of experience are ignored or underestimated. Consideration still has to be given to middle-class position of many knowledge producers, as standpoint theorists typically did in relation to gender, in order to situate these 'truths' as similarly 'partial' – and perhaps even 'perverse'.

Fuss (1989) claims that standpoints are generative of 'epistemic privilege'; only those with the appropriate experience of oppression are able to voice it. She argues that this grants an authority and hierarchy to certain groups whilst silencing others. I do want my research to be read, received and even validated but I do not believe this should be conceptualised as "hear my difference" power play' (Probyn, quoted in Skeggs, 1997). There is a hierarchy of experience already at play whereby some women's experiences are accorded more visibility and validity than others, bringing into question the effectiveness, or even possibility, of developing a working-class standpoint.

Jackson (1998) warns of the dangers of privileging 'pure' experience thus loosing sight of the interpretative processes that shape and construct it: interpretative processes, narratives and 'stories' utilised by both researcher and researched can also come under scrutiny. Everyone draws upon culturally available discourses to interpret and tell their experiences, although some stories do not easily 'fit' (see Plummer, 1995). In this respect working-class 'stories' are again silenced, as they do not match the hegemonic narratives of 'normal' middle-class existence. Although I accept research accounts as being inevitably socially constructed, more important questions are: Whose story can be told? What ones are heard and seen as worthy (or 'interesting') ? Returning to socially situated and materially located standpoint theories, I believe accounts do speak certain 'truths' about people's (located) lives. But whose life? The researcher's life or the lives of the researched?

What you see is what you get – but what's that you're looking at?

Jackson (1998), amongst others, advocates reflexivity as an 'innovative' research strategy, proposing that researchers turn their 'analytical gaze' inwards on themselves, thus challenging the demands of objectivity which places the researcher outwith her data, 'sanitised' from the text. But as Code (1995) argues, it is impossible to be completely self-aware and to realise our, often unconscious, motivations. There are also structural impediments to this. The ability to be 'reflexive', via others, is a privilege and a manifestation of cultural and educational 'capitals' and, accordingly, a classed resource. Proclaiming 'reflexivity' can also serve to authorise one's own account – if only you are reflexive enough. Adkins (2002) notes that the perceived division between good/bad research is increasingly based on the self-reflexivity of the researcher with more 'reflexive' accounts being more positively positioned. Yet, the 'reflexive self' is also a particular classed self: welfare requirements repeatedly force working-class people to tell their (deserving) 'self' in certain ways, further suggesting that the idea of self-reflexivity as fully realisable is an issue of privilege (Skeggs, 2002).

My research is politically and personally motivated but it is still wrong to think that the problems of power, privilege and perspective can be dissolved by 'asserting one's self into the account' (Adkins, 2002). I have charted my identifications as 'working-class' and 'lesbian' in an attempt to demonstrate how these were consequential to the research process providing an 'insider status', as well as 'outsider' status, based on difference. While Walkerdine et al. (2001) may argue that my identification as working-class is a painful defence against my movement away from the women I interviewed, I would claim that processes of class are still painful – I have not moved away from this.

But I ask how much personal detail I should present to convince – and what forms this can take? The 'telling of a story' is not an innocent event; in previously providing evidence of my working-classness (to middle-class disbelievers) in order to persuade and convince I have come away, not satisfied in proving respective differences, but dissatisfied and upset by the lack of legitimacy accorded whatever is revealed. I do not

want to objectify myself, disclosing all that I can be measured by, just as I strive not to objectify interviewees.[10]

I do care about my research, not just because of literature gaps – but 'reflexivity' alone cannot capture this. Reflexivity, on my part, also leaves out a consideration of the feelings of the 'researched'. McRobbie recollects her experience of interviewing a young women who, close to tears, recalled her difficult life, while she was 'almost enjoying the interview, pleased it was going well and relieved that Carol felt relaxed and talkative' (1982: 55). I too am guilty of this. As many women I interviews spoke, I became excited that I had some 'proof' of the existence of class, via their statements: the desire to convince remains. There is, however, a distinction between identifying emotionally with respondents and aligning yourself with them in order to elicit emotional response – enabling confidence which is then exploded (see Finch, 1984; Kelly, 1988).

The interview process was often very moving, with several respondents crying during the course of the interview. I do not want to exploit this fact to make my research more 'interesting' but I do want to draw attention to it. While some feminist methodologies seek to incorporate personal experience and emotion into the research process,I am left wondering about the place and acceptance of 'negative' emotions felt by researcher and researched, such as anger, frustration, pain and envy. Morely (1992) speaks of re-cycling hurt and anger into creative energy for change and Jagger (1989) points to the place of 'outlaw emotions', conventionally unacceptable feelings, in feminist methodology: 'Only when we reflect on our initially puzzling irritability, revulsion, anger or fear may we bring to consciousness our "gut level" awareness that we are in a situation of coercion, injustice or danger' (Jagger, 1989: 161). But with class, anger and emotion can be very individualised, often representing a working-class 'chip on the shoulder'. I did feel both relieved and unsettled by interviewees' tears: knowing that they felt able to cry in front of me, I also wanted to end or 'solve' it. While a frustration occurs through the realisation that ultimately I lack the ability to intervene in the women's lives, other than to

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[10] Luke (1994) illustrates the use of silence as a politics of resistance, a refusal to confess and expose the self. I may claim this but my thesis could not be produced without the narratives of participants.

produce an academic account, I am amused by my imagined response of interviewees to this issue – I somehow don't imagine that intervention would be welcomed.

But this brings up the question of the usefulness of academic research and issues of reciprocity or irrelevance, which is highlighted in the following response:

> '...you're not doing anything useful you're writing (or trying to write) an essay. Last I heard was that it hadn't saved anyone's arse from anything and certainly does nothing to ease anyone's class struggles.'[11]

Attempted enhancements on the 'use value' of research are apparent in approaches where researchers share interpretations with interviewees. However, caution has to be exercised when considering the assumptions underpinning this, i.e. that respondents would have the time to read such analysis. Many of my respondent's lives are too busy to allow for this – the interview itself being an interruption to their everyday lives.[12] As Sacks writes: 'to ask those with whom I was working to share in my research was easier and more democratic in theory than it was in practice … for most workers this project had very low priority and they hadn't the time for it' (quoted in Armstead, 1995: 631). Armstead (1995) claims that she overlooked an important aspect of academic life when interviewing working-class women, that it is irrelevant to those outside university. I would hope to overcome this, although I am aware of the huge efforts still needed. I do not think issues of accountability are addressed solely by re-presenting interpretations to interviewees.

Motives and means – methods and methodologies

I began this article by demonstrating the difficulties in finding a 'hard to reach' group, illustrating how research into lesbian lives often rests upon access into scene spaces, 'key

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[11] I am somewhat in agreement with this respondent (received via email) but my agreement perhaps undermines my own academic claims and authority.
[12] 'I'll be honest with you I haven't even thought about it there's just so much going on in the last few weeks in my life. I didn't give it much thought.' (Mavis, 52, Edinburgh)

informants' and the lesbian and gay press, and suggested that this strategy has a propensity to exclude, including those who are already, to varying degrees, included. However, it strikes me that my criticism of this still necessarily invokes it, I had to work with what was already out there and was out there shaped what I could and could not do and say. The same is true of my critique of reflexivity, reciprocity and usefulness, the structures and concepts which I still invoke and which I still make use of – even if this use is supposed to challenging. In the reading of reflexivity as situating the author through their engagement with others, this article may in fact be described as a 'reflexive' piece of work but I have resisted claiming the tile of 'reflexive researcher' for myself.

Some of the issues and constraints outlined may seem obvious, and indeed inevitable, to many experienced researchers. But I feel it is worth outlining my own struggles and uncertainties, especially as I now consider what to do with the women's experiences, voices and challenges and how to covey the complex processes of researchers' involvements and identifications – outwith and within higher education. I hope this is an issue that can be debated across disciplines to yield effective responses and enhance graduate research. 'Working-class lesbians' may be 'just' one 'hard to reach' group but I think the matters raised in this article are not exclusive to researching this group. Positioning and being positioned, accessing and being accessible, representing and being represented are all interdisciplinary concerns.

References

Adkins, L. (2002) 'Reflexivity and the Politics of Qualitative Research.' In: T. May (ed.) *Qualitative Research in Action.* London: Sage.

Adler, S. and J. Brenner (1992) 'Gender and space: lesbians and gay men in the city.' *International Journal of Urban and Regional Research*, 16 (1): pp. 24-34.

Armstead, C. (1995) 'Writing Contradictions. Feminist Research and Feminist Writing.' *Women's Studies International Forum*, 18 (5/6): pp. 627-636.

Binnie, J. and G. Valentine (1999) 'Geographies of Sexuality: Review of progress.' *Progress in Human Geography*, 23 (2): pp. 175-87.

Code, L. (1995) *Rhetorical Spaces: Essays on Gendered Locations.* London: Routledge

Finch, J. (1984) '"It's great to have someone to talk to": The ethics and politics of interviewing women.' In: C. Bell and H. Roberts (eds.) *Social Researching: Politics, Problems, Practice.* London: Routledge.

Frazer, E. (1988) 'Teenage girls talking about class.' *Sociology*, 22 (3): pp. 343-358.

Frazer, E. (1989) 'Feminist Talk and Talking about Feminism: teenage girls' discourses of gender.' *Oxford Review of Education*, 15 (3): pp. 281-290.

Fuss, D. (1989) *Essentially Speaking: Feminism, Nature and Difference.* London: Routledge.

Harding, S. (ed.)(1987) *Feminism and methodology.* Milton Keynes: Open University Press.

Jackson, S. (1998) 'Telling Stories: Memory, Narrative and Experience in Feminist Research and Theory.' In: K. Henwood, C. Griffin and A. Phoenix (eds.) *Standpoints and Differences: Essays on the Practice of Feminist Psychology.* London: Sage.

Jagger, A.M. (1989) 'Love and knowledge: emotion in feminist epistemology.' In: A.M. Jagger and S.R. Bordo (eds.) *Gender/Body/Knowledge: Feminist Reconstructions of Being and Knowing.* New Brunswick: Rutgers University Press.

John, S. and A. Patrick (1999) *Poverty and Social Exclusion of Lesbians and Gay Men in Glasgow.* Glasgow: Women's Library.

Kelly, L. (1988) *Surviving Sexual Violence.* University of Minnesota Press: Minneapolis.

Kitzinger, C. (1987*) The Social Construction of Lesbianism.* London: Sage.

Mansfield, P. and J. Collard (1989) *The beginning of the Rest of Your Life*? London: Macmillan.

McRobbie, A. (1982) 'The Politics of Feminist Research: Between Talk, Text and Action.' *Feminist Review*, 12: pp. 46-57.

Morley, L. (1992) 'Women's Studies: Difference and Internalised Oppression'. *Women's Studies International Forum*, 29 (1): pp. 23-41.

Phoenix, A. and B. Tizzard (1996) 'Thinking through class: the place of social class in the lives of young Londoners.' *Feminism and Psychology*, 6 (3): pp. 427-442.

Plummer, K. (1995) *Telling Sexual Stories: Power, change and social worlds*. London: Routledge.

Probyn, E. (1990) quoted in: Skeggs (1997).

Qvortrup, J. (1994) 'Childhood Matters: An Introduction.' In: J. Qvortrup, M. Bardy, G. Sgritta and H. Wintersberger (eds.) *Childhood Matters: Social Theory, Practice and Politics*. Aldrshot: Avebury .

Reay, D. (1996) 'Insider perspectives or stealing the words out of women's mouths.' *Feminism and Psychology*, 6 (3): pp. 57-73.

Sacks, K. quoted in Armstead (1995).

Savage, M., G. Bagnall and B. Longhurst (2001) 'Ordinary, Ambivalent and Defensive: Class Identities in the Northwest of England.' *Sociology*, 35 (4): pp. 875-892.

Sayer, A. (2002) 'What Are You Worth?: Why Class is an Embarrassing Subject.' *Sociological Research Online*, 7 (3). Http://www.socresonline.org.uk/7/3/sayer.html.

Skeggs, B. (1997) *Formations of Class and Gender*. London: Sage.

Skeggs, B. (2002) 'Techniques for Telling the Reflexive Self.' In: T. May (ed.) *Qualitative Research in Action*. London: Sage: p. 306.

Smith, D. (1987) *The Everyday World as Problematic: A Feminist Sociology*. Milton Keynes: Open University Press.

Stanley, L. and S. Wise (1993) *Breaking out again: Feminist ontology and epistemology*. London: Routledge.

Valentine, G. (1993) 'Hetero-Sexing Space: Lesbian Perceptions and Experiences of Everyday Spaces.' *Environment and Planning D: Society and Space*, 9 (3): pp. 395-413.

Valentine, G. (1995) 'Out and About: Geographies of lesbian landscapes.' *International Journal of Urban and Regional Research*, 19 (1): pp. 96-111.

Walkerdine, V., H. Lucey and J. Melody (2001) *Growing up Girl: Psychosocial explorations of gender and class*. Basingstoke: Palgrave.

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Using secondary statistical analysis to assess the level of 'high risk' behaviours for sexually transmitted infections in Ireland

Introduction

Throughout the 1990s, reported rates of sexually transmitted infections (STIs), other than HIV/AIDS, increased enormously across Western societies, especially in the under 30s age group. In the Republic of Ireland, there was a threefold augmentation in national reported rates within one decade, with 20 to 30 year olds constituting 65% of the overall reported rates (Department of Health and Children, 1999: 53-54; Department of Health and Children, 1974-1998). The prevalence and incidences of STIs, in particular gonorrhoea, are customarily used as indicators of the proportion of 'high risk' behaviours in a population.[1] A subsequent increase in reported infections is seen to represent more people engaging in unsafe sex. Given the huge changes that occurred in Irish sexual politics at the start of the 1990s and the continual secularisation of the society, the augmenting rates of STIs have recently entered the public forum through the medium of the Irish media. Incorporating voices of concern amongst medical professionals working in genitourinary medicine, articles on STIs focus on the supposed lapsed sexual morals of the Irish people, especially young adults. Such reports have been reflected in recent sexual health policy making. While health authorities should be commended for attempting to reduce levels of STIs in a population, drawing a causal link between rising number of 'high risk' behaviours and the rates is too simplistic. Like other contagious diseases and infections, the spread of STIs depends upon a number of factors, including

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[1] In this context, 'high risk' behaviours refer to unsafe sex, which can be defined as sexual intercourse (anal, vaginal or oral) without the use of a condom and where one or both persons have more than one current or recent sexual partner or one partner is known to have an STI.

improvement in the provision of Irish genitourinary medical services and changes to the surveillance system. Furthermore, conclusions about sexual behaviours can only be reached through the analysis of sexual behaviours and not thorough proxy measures.

Unfortunately, there is a lack of suitable data on STIs and sexual risk-taking in Ireland. Apart from a number of small-scale qualitative studies, there is no national survey on sexual attitudes, behaviours and sexual risk. Data protection laws and the sensitivity of the subject impedes funding, making it difficult to conduct such a survey. As a result, it is difficult to produce a definitive answer on whether the prevalence and incidences of STIs have changed in Ireland nor can information be provided on the levels of 'high risk' behaviour in the population. However, existing sources of data can have some value, even if they have been gathered for another purpose. In this case, a component of the International Social Survey Programme (ISSP) 1994 dataset on family and changing gender roles can be used to examine the proportion of the Irish population who were exposing themselves to the potential risk of STIs at certain point during the 1990s. This information is of use to the policy maker as it provides a description of target audiences for preventative measures.

The aim of this article is to demonstrate that where there is a lack of specific information, a re-analysis of an existing dataset can yield relevant details. While the ISSP dataset is usually utilised in large-scale comparative studies, this study focuses solely on the Irish component of the ISSP dataset. Through this close examination of the data, quantitative methods can be employed as an exploratory tool, generating questions about sexual risk-taking behaviours as well as describing the forms of 'high risk' behaviours in the population. This may be a deviation from the usual testing of hypotheses, but in a situation where information is lacking and where the sample is small, the utilization of secondary statistical analysis as an exploratory tool can provide a good basis for the production of knowledge. In this instance, the social research method can be used to examine levels of sexual risk taking in order to help aid the development of a preventive strategy for STIs.

Determining the proportion and characteristics of the Irish population engaging in 'high risk' behaviours largely involves an assessment of the number of sexual partners

individuals have had within specific units of time. Along with sexual orientation, age, social status and other demographic variables influencing partner choice, number of partners per unit time is a long established measure of risk behaviour in sexual behaviour surveys (Kretzschmar, 2000). Irrespective of sexuality, epidemiological studies and surveys have shown that the probability of acquiring STIs increases with the number of sexual partners with whom one has unprotected sexual intercourse (Liljeros et al., 2001; Jonsson et al., 1997; Wellings et al., 1994; Giesecke et al., 1992). STIs are spread through sexual interactions between individuals and the greater the number of people an individual has sexual relationships with, the greater the likelihood of coming in to contact with STIs (Kost and Darroch Forrest, 1992: 244). If 'lifetime risk of STI acquisition' is defined as having more than two lifetime partners, then in the USA, it has been argued that people with two to four lifetime partners have three to five-percent chance of being infected with a bacterial STI, while those with over 20 partners have between 28% and 35% likelihood (Michael et al., 1994: 193).

In the first section of the paper, the dataset is utilised to demonstrate the importance of multiple partnerships to the spread of STIs by showing how the accumulation of sexual partners increases the risk of exposure to someone with multiple partnerships and thereby potential infection. The second section is concerned with the existence of concurrency and other 'high risk' behaviours, such as homosexual and bisexual behaviours and sex with a sex worker. This is a simple description of the 'high risk' groups as the numbers are too small to carry out any significant differential analysis. Therefore, this section has to be descriptive and devoid of attempts to determine sexual linkages as possible routes of STIs. Following on from that discussion, a logistic regression analysis is employed to yield the combination of predictor variables that affect the likelihood of multiple partners within the time-frames of one year and five years.

Data and methods

The International Social Survey Programme is a voluntary grouping of study teams in 26 countries, each of which undertakes to run a short annual self-completion survey in a probability-based sample of adults over the age of 18 years. Each national survey contains an agreed set of questions, so comparisons can be made between countries and the topics vary from year to year with a view to replicate the cross-national survey every 4 or 5 years. The topic of family was examined in 1988 and again in 1994, this time with an appendix on sexual behaviours in some of the participating countries. The data on Irish sexual behaviours is taken from the 1994 ISSP module on Family and Changing Sex Roles, although the focus here is on the appendix of questions on sexual behaviours. This appendix was collected through postal questionnaires. Response by written questionnaire, anonymously returned by post, is a reliable method for collecting valid sexual behavioural data (Aral and Peterman, 1996: 32).

When using secondary data analysis, it is important to note that the datasets have usually been gathered with other research questions in mind. While the appendix of the ISSP 1994 dataset on sexual behaviours was gathered in light of the HIV/AIDS pandemic, the information collected is very general and not specific to risky sexual behaviours. Nevertheless, an individual's exposure to multiple partners can still be assessed. This measure does not however include the intervening effects of contraceptive usage or partnership characteristics and as a result, it is an over-representation of potential risk. It determines the proportion of a population exposing themselves to multiple partnerships, which increases their chances of risk exposure to a potentially infected individual. If a substantial proportion of the population have multiple partnerships, it can be surmised that the increasing reported rates could be linked to a growing number of people engaging in 'high risk' behaviour. However, if only a small minority of the population are engaging in multiple partnerships, then explanations for the increase in reported Irish STI rates lie elsewhere.

According to Liljeros et al. (2001), there is no well-defined boundary separating the 'high risk' group of multiple partnerships from other individuals. This means that all

individuals are likely to meet a sexual partner who has had multiple partnerships. Thus, all sexually active respondents in the data set were used for calculating the risk of encountering a potentially infected partner. Since we cannot control for the impact of condom usage or presence of infection, the calculations show the risk of being exposed to a partner who has had multiple partnerships. Furthermore, since the majority of respondents are heterosexual (and this is also true of the general population), the risk of encountering a sexual partner with multiple partners will be calculated on the assumption that all sexual encounters will be heterosexual. Separate estimates are made for males and females. The period used for the calculation was the last year, as number of sexual partners in this time-frame play a greater role in exposure to STIs, as relationships are of a shorter duration than those over the past five years (Morris,1994; Knox et al., 1993).

For the logistic regression, the sexual partner measures were recoded from ordinal variables into binary response variables, with a reference category of those who had one sexual partner within the specified time-period and those who had two or more sexual partners. Those who had no sexual partner were excluded from the analysis, as they were not deemed to be at risk from multiple partnerships. This accounted for 13.5% (127) of the respondents. It is intended that the time span of five years (roughly 1989 to 1994) would give some indication of the numbers of individuals exposing themselves to the risk of multiple partners and inadvertently to the risk of STIs within the very period of escalating rates. Age has an effect on the prevalence of STIs, with those aged between 20 and 30 reporting the largest proportion of STI rates in Ireland.[2] By dividing the sample into those between 18 and 30 and those over the age of 30, we can focus on this group of people and compare their behaviours with the rest of the population. Using the ISSP dataset, we can attempt to predict if membership of the younger age group does increase likelihood of having multiple partners and identify what other conditions influence the accumulation of two or more partners within a stated time-frame.

Unstandardised logistic regression coefficients (B) and their standard errors are reported rather than odds ratios. This is because standardised logistic regression

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[2] Material on numbers of new cases and demographic characteristics of the patients visiting the GUM clinics in Ireland was obtained personally from the individual GUM clinics or through written requests for the information.

coefficients are better for evaluating the strength of the influences of the independent variables on the dependant, relative to one another (Menard, 1994). Significance levels of 0.005, 0.05 and 0.1 are used, with reference given to those that were just over the 0.1 level. Since the regression models are being used here for exploratory analysis, the emphasis is not on determining causality, rather the interest is to find a set of predictors that can accurately predict the likelihood of two or more sexual partners. Therefore, 0.05 can be deemed too low and rigid for exploratory research as it can often exclude important variables from the model (Menard, 1994, 55). Because of this, a significance level of 0.1 is employed. No interactions are included as none of them brought any explanatory or specification power to the models.

Before discussing the findings, it is necessary to briefly note that a problem with the Irish component of the ISSP dataset is the large proportion of people who did not respond to the questions on sexual behaviour. Out of 938 respondents, almost half (46%) did not return the appendix to the survey. Since the non-response rate is so large, it is possible that the analysis of risk behaviour (i.e. the number of sexual partners during a specific time period) could be biased. Individuals who discuss their sexual behaviours may be different from those who do not. If there was a bias in the type of person who responded to the questionnaire, it was important to establish how likely members of the 'non-response' group were to have multiple partners, using the characteristics associated with the probability of that behaviour. A logistic regression analysis was carried out with the log-likelihood of non-response to the sexual behavioural questions being calculated using a number of demographics. If the 'non-response' group had consisted of mostly young, single urban dwellers, then the chances are that the risk assessment would be inaccurate, as the proportion of 'high risk' takers is too small. As it transpired, it was largely the older age groups who were married and lived in rural areas that declined inclusion (see table 1). This means that those who do have multiple partners may actually be a much smaller percentage of the sample, resulting in a possible over-estimation of multiple partners. However, given that the numbers of people with multiple partners are so small, it is not necessarily a bad thing to have a small over-representation of those with

multiple partners. Not only does it help the statistical analysis in terms of sample size, but it can also help predict maximum risk levels for the population.

Table 1.Results from logistic regression analysis for predicting the variables associated with the log-likelihood of non-response to sexual behavioural questions

	Model I B.	S.E.	Model II B.	S.E.	Model III B.	S.E.	Model IV B.	S.E.	Model V B.	S.E.	Model VI B.	S.E.
Reached or completed third level education	-0.557***	0.211	-0.383*	0.217	-0.461**	0.222	-0.372	0.23	-0.366	0.232	-0.39*	0.233
Reached or completed secondary school level	-0.075	0.164	-0.008	0.167	-0.065	0.171	-0.009	0.175	0.024	0.178	0.001	0.179
Reached or completed primary school level (Base)	0		0		0		0		0		0	
Reside in an urban area			-0.678***	0.145	-0.712***	0.147	-0.71***	0.147	-0.71***	0.148	-0.771***	0.151
Reside in a rural area (Base)			0		0		0		0		0	
Premarital sex is not wrong					0.254*	0.152	0.324*	0.159	0.324*	0.159	0.191	0.17
Premarital sex is wrong (Base)					0		0		0		0	
Aged between 18 and 30 years							-0.295	0.19	-0.39*	0.216	-0.441**	0.218
Aged 31 and older (Base)							0		0		0	
Single and never married									0.194	0.19	0.114	0.193
Separated, divorced or widowed									0.19	0.254	0.174	0.256
Married or cohabiting (Base)									0		0	
Never attend religious services											0.327	0.371
Attend religious services once a month to once a year											0.467**	0.194
Attend religious services once a week or more (Base)											0	
- 2ll	1118.907		1096.839		1094.042		1091.62		1090.246		1084.284	
Model Chi^2	8.194**		30.263***		33.06***		35.482***		36.856***		42.818***	

N = 435

*** = significant at 0.005 level; ** = significant at 0.05 level; * = significant at 0.1 level

Risk-taking and multiple partners

Irrespective of whether condoms are used or not, having multiple partners increases the risk of exposure to a STI for an individual. The more partners a person has in a specific time unit, the more likely it is a non-monogamous relationship (Michael et al, 1994: 195). Relationships also tend to be casual, short-term or 'one night' stands. There is a strong correlation between those with several partners and the likelihood of paid sex and casual encounters fuelled by alcohol (Michael et al., 1994: 196). This leads to a decreased usage of condoms. There is also evidence that people with many sexual relationships engage with others who are also in non-monogamous relationships (Michael et al., 1994: 195). This greatly increases the chances of acquiring a sexually transmitted infection.

The importance of multiple partnerships to the risk of encountering a potentially infected partner can be illustrated using information obtained from the Irish sample.

Table 2. Reports of sexual partners and sexual encounters for both genders

Number of sexual partners reported by women	Number of women	Number of sexual encounters
1	174	174
2	9	18
3	5	15
4	1	4
5 and 10 [7.5]	1	7.5
21 and 100 [60]	1	60

Number of sexual partners reported by men	Number of men	Number of sexual encounters
1	144	144
2	11	22
3	6	18
4	3	12
5 and 10 [7.5]	2	15
21 and 100 [60]	0	0

For the male population, the calculations are based on the number of male sexual partners reported by women for the last year and for the female population, the calculations involve the number of reported sexual partners by the male respondents (see table 2). For

the categories of five to 10 partners and 21 to 100, the midpoints are taken for the calculations. Since we are estimating the probability of meeting a sexual partner who has had multiple partnerships, we need to think in terms of sexual encounters. Therefore, we multiply the number of reported sexual partners by the number of respondents. This calculates the number of new sexual encounters in the last year. This gives us a total of 278.5 new sexual encounters involving women who had one or more sexual partners and 211 new sexual encounters involving males with one or more sexual partners for the past year. The next step in our calculations involves finding the fraction of all sexual encounters that involved a female and a male with just one sexual partner and those who had two or more sexual partners. By dividing the 174 sexual encounters that involved females with only one sexual partner by the total number of new sexual encounters (278.5), the result is 0.625. When subtracting this figure from one to get the fraction of total sexual encounters that involved females with two or more sexual partners in the last year, the resulting figure is 0.375. This means that 62.5% of male new sexual encounters were with females who had one sexual partner, while 37% of female new sexual encounters were with females who had two or more sexual partners. This was repeated for the female population, resulting in 68.2% of new sexual encounters in the last year involving males who had just one sexual partner, while 31.8% of new sexual encounters involved males who had two or more sexual partners.

To calculate the probability that a male had contact with a woman who has had two or more sexual partners in the past year, we use the following equation:

$$P = 1-(0.625)^n$$

where n equals the number of sexual partners a male has had in the last year.

For women, the probability is calculated by:

$$P = 1-(0.682)^n$$

where n equals the number of sexual partners a female has had in the last year.

For a male who has had just 1 sexual partner, he has 37.5% chance of coming into contact with a woman who has multiple partnerships, i.e. $1 - (0.625)^1$. While women who have had only one sexual partner have a 31.8% chance of having a partner who has had multiple partners. As illustrated in figure 1, for both sexes, the probability of

encountering a sexual partner who had multiple partnerships in the last year increase as the individual increases his or her number of sexual partners. For example, a woman, who has had five sexual partners has an 85.2% chance of having a sexual encounter with a male who has had two or more sexual partners.

Figure 1. Graph displaying the probability of encountering a sexual partner who has had multiple sexual partnerships by increasing number of sexual partners for the last year by gender
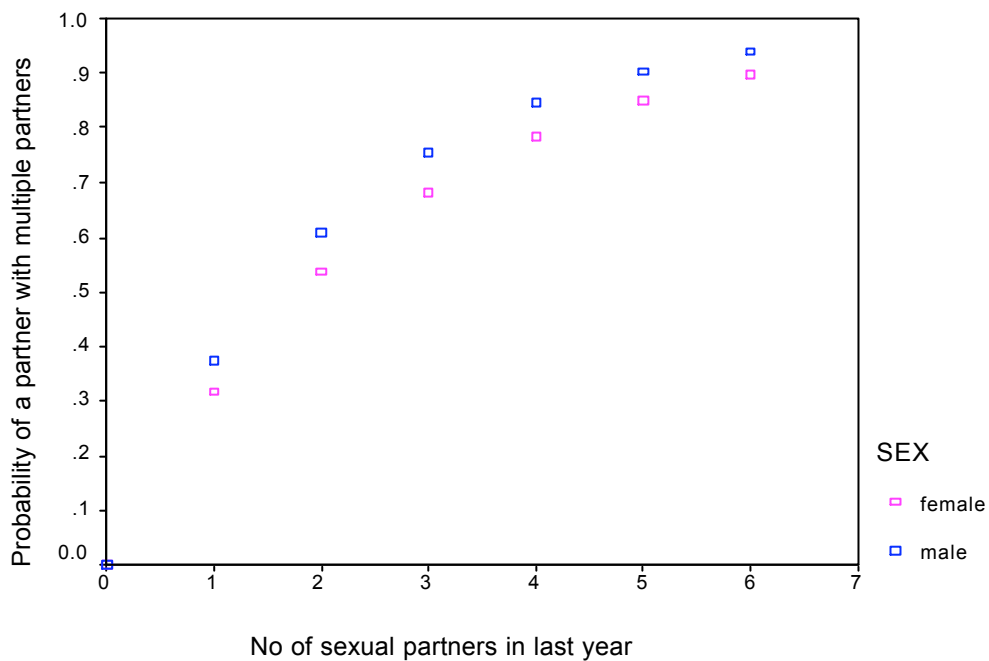
Taking into account the other influential factors, such as condom usage and whether or not the sexual partner is infected, the risk of acquiring a STI would be much lower. However, by increasing the numbers of sexual partners, an individual increases the likelihood of having a partner who has had multiple partners, thus increasing his or her risk of being exposed to an infected individual.

Concurrency

Another effect of multiple partnerships on the risk of STIs, is that people with multiple partners over a twelve month period are more likely to have concurrent or overlapping relationships, which is conducive to the spread of STIs (Overseas Development Administration, 1996; Aral and Peterman, 1996; Laumann et al., 1994). Unfortunately, there is no information to support the presence of concurrent relationships in the data. Even where respondents were married and were stated as having two or more partners in the last year, it is difficult to determine if these relationships were concurrent as the time frame of marriage is not stated. However, respondents were asked about participation in extramarital affairs. Despite not possessing any knowledge of the nature and duration of the extra-martial affair, we can assume concurrency at a particular point in time, although it is possible that a person may not be in a sexual relationship with the marital partner. It can be argued that a married individual who is having sex with someone other than their spouse, will recruit his or her partner from outside their social circle of acquaintances in order to minimize the chances of being discovered (Mai et al., 2000; Laumann et al., 1994) Laumann et al. (1994) in their study on the sexual behaviours of the USA population argued that this action increases the individual's risk of STIs more than if the individual was single and had indulged in a series of short-term monogamous relationships (172).

Eighteen men (7.7% of all males) and eight women (3.7%) admitted to having an extramarital affair. All males were aged 31 and over, while two women were less than 30 years of age (the youngest of whom was 28). With the exception of one person who had never been married, all respondents who had been involved in an extramarital affair had been married, with two men and one woman now widowed. None of the respondents classified themselves as separated or divorced, thus indicating that the marriage had continued in spite of the affair.

Sixteen of those respondents who admitted having extramarital affairs cited only one sexual partner in the last year with eight of these (50%) reporting sex with someone they had termed as a close friend, perhaps referring to a mistress. It is possible that the

other eight had their affair some time ago. Another ten respondents reported between two and 100 partners yet all cited having a 'regular sexual partner'. None of respondents had paid for sex or had casual sex. While eight respondents indicated that the sexual relationship had ceased with the marital partner due to their only sexual partner in the last year being 'a close friend', at least ten respondents were still at risk of concurrency. Furthermore, participation in extramarital affairs helps to link high-risk partners with low risk sexual partners by providing a channel for the transmission of STIs from core groups to low risk groups.

'High risk' groups

Apart from multiple partnerships, other 'high risk' behaviours associated with spread of STIs include sex with a sex worker and homosexual and bisexual behaviours. Only a tiny number of respondents reported having used paid sex. None of the females reported being involved in prostitution since their 18th birthdays, though it is possible that there is a social desirability bias with female responses (Wellings et al., 1994). Only one unmarried male under the age of 30, reported having used a prostitute. Another seven were aged between 31 and 40 and four were between 41 and 50 years of age. The majority of these older men were married (nine respondents). No one reported use of paid sex in the last year, indicating that the use of prostitution may not have placed them in contact with a high risk group within that year, but their reported past experience reveals that they have taken risks in being potentially exposed to STIs. Furthermore, given that the majority are married and of older age groups, it is conceivable that the respondents have placed both themselves and their partners at risk.

A limitation with this dataset is that the size of the subpopulation of homosexuals and bisexuals is too small to carry out any significant differential analysis. One problem with using a randomised sample for sex research is that the homosexual population tends to be underrepresented. As O'Connell Davidson and Layder (1994) have argued, the homosexual population is not geographically dispersed across a nation, as homosexuals

tend to live in cities where there are cohesive gay communities (93). Only 21 respondents reported homosexual behaviours – 4.1% of those who had exclusively male partners since 18th birthday were men and 6.1% of those who had only female partners since 18th birthday were women. The percentage of bisexual behaviour was even smaller. This low percentage reflects what has been found in other European sexual behavioural surveys. Out of five surveys done on USA, Denmark, France, UK and Sweden on aspects of sexual orientation and behaviour which are related to risk of HIV/STIs, lifetime homosexual behaviour was reported to be less than seven-percent with rates of past year same gender sexual contact of between one-percent to three-percent (Dunne et al., 1994: 41). Similar to Wellings et al. (1994) in their national study on sexual behaviours in Britain, the majority of respondents in this sample reported to have had exclusive heterosexual behaviours. Moreover, this heterosexual sub-population were more likely to have multiple partners, thus indicating that perhaps transmission of STIs in the Irish population may be more affected by heterosexuals. Indeed, the largest proportion of reported STI rates consists of ano-genital warts, an STI which is associated with heterosexual coupling (Department of Health and Children, 1999: 53-54).

It is worth noting however that irrespective of sexual identity, people can be engaging in both homosexual and heterosexual transmission of STIs (Wilton, 2000: 85). Evans et al. (1988) found in their studies on risk factors and STIs that over 50% of homosexual men had reported previous heterosexual intercourse and that the risk of heterosexual spread of HIV is increased by the combination of heterosexual and homosexual intercourse by bisexual and homosexual men (129).

What factors influence the likelihood of having multiple partners?

A series of logistic models was constructed in order to examine the effects of age group membership, area of residence, level of education religious attendance and marital status. Only these demographics were included as they were found in other studies to be strongly correlated with sexual behaviours (Sandfort et al., 1998; Wellings et al., 1994; Hendry et

al., 1993). When run in single models, all variables were shown to have an effect on the log-odds of two or more partners for both time periods and were significant at 0.005 and 0.05 levels. Gender was not used in the analysis, as it was not found to be a confounding variable. This is an interesting finding as previous sex surveys have suggested that while roughly the same proportion of men and women claimed one partner, in relation to two or more partners, men were more likely to overestimate their number of sexual partners while women underestimate the number of theirs (Wellings et al., 1994; Hendry et al., 1993; Fitzpatrick et al., 1992). It is possible that the effects of gender differences are being hampered by missing data or there may really be little difference in sexual behaviours between the sexes in Irish sexual culture.

By examining the effects of the demographic factors simultaneously, it is possible to characterise those who reported 2 or more sexual partners in the last year and the last 5 years. Tables 3 and 4 show the results of the combined models for predicting the likelihood of having two or more partners in the last five years and the last year.

Table 3. Combined models for predicting likelihood of having two or more partners in the last 5 years

	Model I		Model II		Model III		Model IV		Model V	
	B.	S.E.	B.	S.E.	B.	S.E.	B.	S.E.	B.	S.E.
Under 30 years old	2.112****	0.299	1.894****	0.312	1.849****	0.313	1.697****	0.322	0.319	0.449
31 years and older (Base)	0		0		0		0		0	
Reached or completed third level education			1.153*	0.603	1.11*	0.605	1.003♦	0.616	0.659	0.695
Reached or completed secondary school level			0.815♦	0.567	0.811♦	0.569	0.804♦	0.574	1.001♦	0.633
Reached or completed primary school level (Base)			0				0		0	
Reside in an urban area					0.473♦	0.345	0.315	0.357	0.215	0.418
Reside in a rural area (Base)					0		0		0	
Never attend religious services							0.595	0.6	0.125	0.727
Attend religious services once a month to once a year							0.731**	0.338	0.106	0.417
Attend religious services once a week or more (Base)							0		0	
Single and never married									3.358****	0.476
Separated, divorced or widowed									1.639**	0.655
Married or cohabiting (Base)									0	
- 2ll	288.83		284.620		282.674		277.942		213.292	
Model Chi^2	52.555****		56.765****		58.712****		63.443****		128.093****	

N = 356

**** = significant at 0.0005 level
*** = significant at 0.005 level
** = significant at 0.05 level
* = significant at 0.1 level
♦ = just over 0.1 level – Model II (0.l36); Model III (0.17 and 0.154); Model IV (0.103 and 0.161); Model V (0.114)

Table 4. .Combined models for predicting likelihood of having two or more partners in the last year

	Model I		Model II		Model III		Model IV		Model V	
	B.	S.E.	B.	S.E.	B.	S.E.	B.	S.E.	B.	S.E.
Under 30 years old	1.554****	0.363	1.4****	0.378	1.333****	0.38	1.088**	0.394	-0.444	0.533
31 years and older (Base)	0		0		0		0		0	
Reached or completed third level education			0.99	0.817	0.912	0.818	0.626	0.834	-0.05	0.899
Reached or completed secondary school level			0.908	0.77	0.891	0.771	0.798	0.778	0.714	0.826
Reached or completed primary school level (Base)			0		0		0		0	
Reside in an urban area					0.745*	0.45	0.477	0.468	0.292	0.513
Reside in a rural area (Base)					0		0		0	
Never attend religious services							1.409**	0.652	1.412*	0.737
Attend religious services once a month to once a year							0.992**	0.419	0.695♦	0.475
Attend religious services once a week or more (Base)							0		0	
Single and never married									3.064****	0.589
Separated, divorced or widowed									2.444***	0.805
Married or cohabiting (Base)									0	
- 2ll	213.218		211.327		208.291		200.796		166.305	
Chi^2	18.049****		19.94****		22.976****		30.471****		64.962****	

N = 347

**** = significant at 0.0005 level
*** = significant at 0.005 level
** = significant at 0.05 level
* = significant at 0.1 level
♦ = just over 0.1 level – Model V (0.144)

Age group membership

Both tables reveal that age group membership does have an affect on the log-likelihood of having 2 or more partners within a certain time frame until adjusting for marital status, when the effect disappears.[3] Throughout Models I to IV on both tables, those who are between 18 and 30 years have higher log-odds of multiple partners in the last year or 5 years than those who are 31 and older. This is expected as those of the younger age group are at the start of their sexual career and are more likely to sexually experiment with a number of sexual partners.[4] Without controlling for the other 5 independent variables, membership of the younger age group increases the log odds of having two or more partners by 1.6 times for the last year and doubles the log-odds when compared to the other age group for the period of five years. These log-odds decrease when adjusting for the effects of the other variables indicating intervening variables. When the log-odds fall to near zero in the final models, this is not unexpected given that one would expect marriage to diminish the likelihood of having multiple partners. Similar to the findings of Wellings et al (1994) and Kost & Darroch Forrest (1992), irrespective of age, those who are single are more likely to have multiple partners than those who are married. People of all ages respect the monogamous union of marriage (Wellings et al., 1994: 104). When taking age, area of residence, educational level and religious attendance into account, those who have never married have higher log-odds of 3.358 times to have had two or more partners within last five years compared to those married or living as married. They also have a higher log-odds of 3.064 times for multiple partnerships within the last year. People who are separated, divorced or widowed also had significantly higher log-odds of having multiple partners than those who were married (despite presence of extramarital affairs). Gott et al. (2000) have pointed out that

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[3] Model V is also the 'best fit' model for both time period, with a difference in −2ll from the first model of 46.913 for last year and 75.538 for the 5 year period. Thus indicating that the addition of variables provide an improvement in the predictive power of multiple partnerships for both time frames. Changes in the Model Chi-square for each new block added in the models were significant at the 0.0005 level, showing improvement in chi-square for each additional degree of freedom.

[4] The life course of sexual activity and lifestyle of an individual can be termed a sexual career. Generational effects and secular trends aside, most individuals' sexual careers undergo a similar process (Welling et al., 1994). There is an initial experimental phase with a number of partner changes. Then there is the establishment of a long term and committed relationship (e.g.: marriage). Later in life, there can be separation, divorce and widowhood, which result in either sexual abstinence or new relationships.

relationship patterns are becoming more flexible in later life in western societies with many people actively seeking sexual partners post-marriage (717). This is reflected in the positive log-odds of 1.69 for having multiple partners over the last 5 years and 2.444 for the last year.

Education

In spite of the level of education being significant when used in a single model for predicting log-likelihood of two or more partners within the last year or five years, it does not add to the explanatory power of Model II in table 4. Its addition only reduces −2 log-likelihood by 4.21. When predicting log-likelihood of having two or more partners in the last five years, education did display an increase of 1.153 in log-odds when controlling for age (Model II in table 3). It is possible that the effect of educational level reached may have higher significance with a larger sample. In Model V, those who reached or completed secondary school level had higher odds of 1.001. This is in keeping with previous research which found that despite age, the higher the level of education, the more liberal the attitudes to sexual behaviour and the greater the possibility of multiple partnerships. Lear (1997) found that unlike those who did not attend university, college populations frequently partook in risk-taking sexual behaviours, such as alcohol fuelled sexual encounters and multiple partners. Thus over the longer period, those educated at secondary school and higher have higher log-odds to have accumulated more partners than those who attended just primary school. The effect of educational level reached does lose significance for the time period of five years when attendance at religious services and marital status are controlled. Thus showing that in Ireland, the liberalising effects of increasing educational level is compromised by religious devotedness and adherence to marital vows or long term monogamous commitment.

Area of residence

Area of residence was added to the third model in both tables to see if living in an urban area provides greater opportunity to engage in multiple partners. In other words, the greater availability of sexual partners and more secularised outlooks of urban areas would increase the log-odds of multiple partners when controlling for the other variables. For the last year, the log-

odds of multiple partners was increased by 0.76 times while for the last 5 years, the log odds were increased by 0.5 times. Both log-odds are small and hover around the 0.1 level of significance. When controlling for age group membership, residence in an urban area increases log-likelihood of multiple partnerships. This effect may be found to be more significant in a larger sample. Since the effect disappears once the impact of religion was controlled for, it is possible that those living in urban areas had higher log-odds of multiple partners because they had larger numbers of those who did not attend or infrequently attended religious services than the rural areas.

Religious attendance

Religious attitudes (which can be derived from attendance at religious services) have an effect on the possibility of sexual risk-taking within the shorter period of 1 year. Although, 'attendance at religious services' is only one measure of religiosity in relation to sexual behaviours, it can be assumed that attendance at religious ceremonies would bring individuals into contact with the religious institution's teachings on sexual matters. Religious attendance has a significant effect at the 0.05 level on the number of sexual partners in the last year when controlling for age group membership, area of residence and education level reached (Model IV). Those who do not attend church services have higher log-odds of 1.409 of having two or more sexual partners in the last year than those who attended regularly. Furthermore, those who attended infrequently (i.e. between a few times a month and once a year) also increased the log-likelihood of multiple partners by 0.992 times. Since the majority of the Irish population are practising Catholics, one would expect to see either one sexual partner or no sexual partners if the Church teachings were being followed. Indeed, those who regularly attended religious services were more likely to have more conservative sexual behaviour irrespective of age, area of residence and educational level reached - i.e. they did not have two or more sexual partners in the last year.

Religious attendance is not as strong a predictor in the models for log-odds of having two or more partners in the last five years. Those who never attend did not have significantly higher log-odds than those who attended regularly. When controlling for age, area of residence and education level, those who attended services infrequently did have slightly higher log-odds of

multiple partners than those who did attend regularly (0.731). Erratic attendances could be symptomatic of family obligations rather than personal beliefs and so would display higher odds of multiple partners than those who attended church regularly. Sporadic attendance at church could mean an inconsistency in beliefs and values that spill over into sexual behaviours and lifestyles. In relation to the absence of significant differences between those who attend religious services regularly and those who never attend, the effect of religious attendance could be compromised by favourable premarital attitudes. Irish people would be influenced by the overall trend of premarital sex in Western societies (Coleman, 2000). Here the emphasis is still on long-term emotional partnerships with a sexual component but that it no longer needs to be restricted to the marital union. Irrespective of religious beliefs, Irish individuals could be culturally influenced to have monogamous sexual unions prior to or instead of marriage. This would mean that individuals who have had two or more sexual partners in the last five years are more likely to have serial monogamous relationships rather than short term 'flings'.

Discussion

While the regression models explored what best predicts multiple partnerships, they can also help determine whether or the under 30s age group (which accounts for the biggest proportion of STIs) are more likely to engage in risk-taking sexual behaviours than the older age group. When taking into account area of residence, education level and religious practice, the younger age group do display a higher likelihood of multiple partnerships than the older age group. It is not surprising that in the final and best-fit model for both time-frames that marital status would have such a big effect. One would expect that those who have never married and those who are separated, divorced or widowed would have higher log-odds of multiple partners than those who are married. Given that age ceases to be significant in the final models of tables 3 and 4, it can be surmised that within both age groups, an individual ceases other sexual relationships when they meet a long-term partner. Sexual intercourse is not age specific and the larger numbers of reported rates of STIs from under 30s could simply reflect the larger proportion of them being unmarried than the older age group.

Within the two age groups, the majority had just one sexual partner for both time-periods. Similar to other sexual behavioural surveys (Wellings et al., 1994; Michael et al., 1994), an extreme skewness of distribution occurs in relation to the reported number of partners. The vast majority of respondents only report one sexual partner with a few extreme points or outliers reporting several. These form the 'high risk' group. The degree to which those with a high rate of partner change mix with those with a low rate within a specific period is of importance to the spread of STIs in a population (Kretzschmar, 2000). Giesecke et al. (1992) have suggested that high-risk individuals choose similar high-risk partners, more often than is predicted by chance (259). If this is the case for the Irish population, then the increase in reported rates is reflective of this minority rather than changes in high-risk behaviour for the whole population. Indeed, given that the majority of the young age group have had only one sexual partner in the last year, it is very possible that at that period when the rates were increasing, it was not changes in sexual practices amongst the young that were contributing to the augmentation.

Conclusion

This purpose of this article was to illustrate how the social science tool of secondary data analysis is a useful means of extracting information from a general social survey dataset to answer a specific health related question. In this case, it was to describe levels of 'high risk' sexual behaviours, which are conducive to the transmission of STIs. Although the sample of the Irish population was small, we are able to establish individual risk factors for the potential risk of exposure to STIs. When controlling for the demographic factors of age group membership, level of education reached, area of residence and religious attendance, those who are unmarried are the most likely to have multiple sexual partners in the last year or 5 years. Because the majority of those under the age of 30 are unmarried (unlike the older age group), it is possible that this constitutes them as the age group more likely to be exposed to the potential risk of STIs. Sexual risk patterning does occur throughout the generations in the population. Certainly, the younger age groups are more likely to be exposed to STIs, simply because they are at the experimental

stage of their sexual careers. However, those over the age of 30 demonstrated use of prostitutes and extramarital affairs.

Despite the limitations of the information on sexual behaviours in the dataset, the findings do comply with the observations of more specific sex surveys (Wellings et al., 1994; Michael et al., 1994; Knox et al., 1993). A minority of respondents displayed tendencies to risk-take in sexual behaviours. This is also in keeping with studies on STI transmission, where only a small proportion of the population contribute disproportionately to the spread of STIs via high rates of STIs and partner change (Jolly et al., 2001). Since only a minority of the Irish population are indulging in multiple partnerships, the explanations for increasing reported rates of STIs may rely on other social mechanisms, such as the Irish surveillance system and the provision of genitourinary medical services. Hence, as an exploratory tool, second secondary analysis of a small sample can provide a direction for future research through the description of a population's characteristics and the revelation of gaps in the explanations of a health phenomenon.

References

Aral, S.O. and T.A. Peterman (1996) 'Measuring outcomes of behaviour interventions for STD/HIV prevention.' *International Journal of STD & AIDS*, (7; suppl. 2): pp. 30-38.

Coleman, D. (2000) 'Population and Family.' In: A. Halsey and J. Webb (eds.) *Twentieth Century Social Trends*. London: Macmillian Press.

Department of Health and Children (1999) *Health Statistics*. Dublin: Government Stationary Office.

Department of Health and Children (1974-1998) *Health Statistics*. Dublin: Government Stationary Office.

Dunne, M.P. et al. (1994) 'Age-related increase in sexual behaviours and decrease in regular condom use among adolescents in Australia.' *International Journal of STD & AIDS*, (5): pp. 41-47.

Evans, B.A. et al. (1988) 'Heterosexual behaviour, risk factors and sexually transmitted infections among self-classified homosexual and bisexual men.' *International Journal of STD & AIDS*, (9): pp. 129-133.

Fitzpatrick, C. et al. (1992) 'Teenage Girls Attending a Dublin Sexually Transmitted Disease Clinic: A Socio-Sexual and Diagnostic Profile.' *Irish Journal of Medical Science*, 161 (7): pp. 460-2.

Giesecke, J. et al. (1992) 'Sexual behaviour related to the spread of sexually transmitted diseases: A population-based survey.' *International Journal of STD & AIDS*, (3): pp. 255-260.

Gott, M. et al. (2000) 'Exploring the sexual histories of older GUM clinic attenders.' *International Journal of STD & AIDS*, (11): pp. 714-718.

Hendry, L.B. et al. (1993) *Young People's Leisure and Lifestyles.* London: Routledge.

Jolly, A.M. et al. (2001) 'Sexual Networks and Sexually Transmitted Infections: A Tale of Two Cities.' *Journal of Urban Health*, 78 (3): pp. 433-445.

Jonsson, M. et al. (1997) 'The associations between risk behaviour and reported history of sexually transmitted diseases among young women: A population based study.' *International Journal of STD & AIDS*, (8): pp. 501-505.

Knox, E. et al. (1993) *Sexual behaviour and AIDS in Britain*. University of Birmingham: HMSO.

Kost, K. and J. Darroch Forrest (1992) 'American Women's Sexual Behaviour and Exposure to Risk of Sexually Transmitted Diseases.' *Family Planning Perspectives*, 24 (6): pp. 244-254.

Kretzschmar, M. (2000) 'Modelling network structure: Implications for the spread and prevention of HIV infection.' In: J.P. Moatti et al. (eds.) *AIDS in Europe: New Challenges for the Social Sciences*. London: Routledge.

Laumann, E.O. et al. (1994) *The Social Organisation of Sexuality: Sexual Practices in the United States*. Chicago: University of Chicago.

Lear, D. (1997) *Sex and Sexuality: Risk and Relationships in the Age of Aids*. Thousand Oaks: Sage.

Liljeros, F. et al. (2001) 'The web of human sexual contacts.' *Nature*, (411): pp. 907-914.

Mai, C. et al. (viewed November 2000) 'Network data collection and its relevance for the analysis of STDS: The NHSLS & CHSLS.' Http://www.src.uchicago.edu/prc/pdfs/

lauman.pdf.

Menard, S. (1995) *Applied Logistic Regression Analysis*. Thousand Oaks: Sage University Paper.

Michael, R.T. et al. (1994) *Sex in America: A Definitive Survey*. London: Litlle, Brown & Company.

Morris, M. (1994) 'Epidemiology and social networks: Modelling structured diffusion.' In: S. Wasserman and J. Galaskiewicz (eds.) *Advances in Social Network Analysis: Research in the Social and Behavorial Sciences*. London: Sage Publications: pp. 26-51.

O'Connell Davidson, J. and D. Layder (1994) *Methods, Sex and Madness*. New York: Routledge.

Overseas Development Administration (1996) *Sexual Health and Health Care - HIV, AIDS and Sexually Transmitted Infections: Global Epidemiology, Impact and Prevention*. London: ODA.

Wellings, K. et al. (1994) *Sexual Behaviour in Britain: The National Survey of Sexual Attitudes and Lifestyles*. London: Penguin books.

Wilton, T. (2000) *Sexualities in Health and Social Care: A Textbook*. Buckingham: Open University Press.

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Generalizability and qualitative research in a postmodern world

Abstract

In this paper, I will explore the idea of 'generalizability' as a methodological concept in the social sciences. First, I will look at how generalizability is depicted as a folk notion of science. In particular, I am interested in how generalizability has been constructed as a problem for qualitative research. Second, I will review the attempts of Robert Prus and Howard Becker to construct a uniquely qualitative model of generalizability. This is a model of 'generic social processes', which attempts to generalize about social processes, rather than populations. Third, I will discuss generalizability as an ideal that has been undermined by postmodern theory. Through this discussion, I will argue that the notion of 'generalizability' remains useful for qualitative research in a postmodern era.

Introduction

In an article titled, 'Qualitatively different: Teaching fieldwork to graduate students', Sherryl Kleinman, Martha Copp and Karla Henderson (1996) discuss the 'folk notions of science' that social scientists hold. These 'folk notions' are 'ideas about how scientific work should be done'. Folk notions of science are deeply ingrained and are strongly entwined with the positivist tradition of social science. They include the official definitions of 'reliability', 'validity' and generalizability' within the social sciences. In recent years, the critiques of postmodernism, post-structuralism and other 'post-al'

theories have made these folk notions increasingly untenable (Scatamburlo-D'Annibale and Langman, 2002). From this position, these folk notions are re-defined as discourses that are mobilized during the social construction of a system of power/knowledge within the social sciences (Foucault, 1980; Gordon, 1980; Hughes, 1995). For social scientists wishing to transcend the limitations of positivism, these folk notions are barriers to be negotiated or overcome.

Using Kleinman et al.'s concept of the folk notion of science as a jumping-off point, I would like to explore the idea of 'generalizability' as a methodological concept. First, I will look at how generalizability is depicted as a folk notion of science. In particular, I am interested in how it has been constructed as a problem for qualitative research. Second, I will review the attempts of Robert Prus and Howard Becker to construct a uniquely qualitative model of generalizability. This is a model of 'generic social processes', which attempts to generalize about social processes, rather than populations. Third, I will discuss generalizability as an ideal that has been undermined by postmodern theory. In this section, I will also illustrate how Prus and Becker's model of 'generic social processes' may be qualified and retained as a useful analytic concept in light of the postmodern critique of social science. Through this discussion, I will argue that the notion of 'generalizability' remains useful for qualitative research, even in a postmodern era. While I choose to focus on postmodernism within this article, the main thrust of the argument applies to the critique of positivist social science that is found throughout various 'constructionist' forms of social science, including post-structuralism and post-Marxism.

Generalizability as a folk notion of science

Traditionally, generalizability refers to the ability to apply the results of research conducted on a sample of a population to a broader population (Babbie, 1995). This familiar notion of generalizability has been termed 'statistical generalization' by Yin

(2003).[2] As a folk notion of social science, this model of generalizability is desirable because it allows us to move beyond the boundaries of our research data. If we can generalize from a studied sample group to a population, then we feel that our research is more useful, or more important. As Babbie notes, 'Social scientists study particular situations and events to learn about social life in general. Usually, nobody would be interested in knowing about the specific subjects observed by the researcher' (Babbie, 1995: 302). Thus, generalizability is often invoked as a legitimizing discourse for social research. Research that is more generalizable may be read as more important to the collective process of knowledge-formation. Thus, the folk notion of generalizability may be used to assert the greater importance of quantitative ways of knowing, while marginalizing knowledge produced through qualitative inquiry.[3]

In quantitative research, generalizability is premised on the ability to gather a random sample of the population that the researcher is interested in. If the sample is representative of the larger population, it follows that research results can be extrapolated to the larger population. To achieve generalizability with a degree of assurance, the researcher incorporates accepted sampling procedures into the research design. From this perspective, generalizability is best achieved through the use of quantifiable measurement and random sampling. In other words, quantitative procedures are seen as more conducive to producing generalizable results, while qualitative research is seen as less generalizable.

In his discussion of field research, Babbie identifies three ways in which generalizability is problematic for qualitative research. At the risk of conflating

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[2] Yin distinguishes between 'statistical generalization' and 'analytical generalization'. The former form of generalization refers to the ability to make statistical inferences about a 'population' based on research on a small sample of that population. This is the 'folk notion' form of generalizability that I deal with in this paper. As an alternative, Yin offers 'analytical generalization'. Smaling (2003) writes that analytical generalization, or 'generalization to a theory' occurs when 'research results are generalized [from one case study] by means of a suitable theory' to other cases.

[3] This raises the question of whether the knowledge produced through qualitative research is really that different from the knowledge produced through quantitative research. I believe that there is an important distinction. Quantitative research seems to excel at providing a more abstract, simplified picture of what the social world looks like. While this can be valuable, it tends to overlook the smaller-scale social processes that construct this larger-scale picture. Furthermore, qualitative research is often more adept at exploring the 'deviant cases' that are marginalized in quantitative research.

objectivity and replicability, Babbie asserts that qualitative research is fundamentally more 'subjective' than quantitative research, as data gathering involves personal 'observations and measurements . . . that would not necessarily be replicated by another, independent researcher.' Due to the higher level of 'objectivity' obtainable through highly standardized quantitative research instruments, the subjectivity of quantitative researchers is less of a problem (Babbie, 1995: 302). Second, the focus of qualitative research on a small number of cases, which are explored deeply, is less conducive to generalizability than quantitative 'results based on rigorous sampling and standardized measurements' (302). Finally, due to the typically small number of research participants in qualitative social research, the researcher is never sure whether the sample is actually representative of the larger population. For Babbie, smaller numbers lead to an 'endless' potential 'for biased sampling' (302). Taking these comments together, we are led to a construction of qualitative research as 'more valuable as a source of insight than as proof or truth' (302). In Babbie's account, the ability to produce 'truth' is limited to quantitative research, due to its norms of random sampling, large populations and objectivity. The relationship between the folk notion of generalizability and qualitative research is easily summed up by Cresswell. In evaluating the strengths and weaknesses of qualitative research, he notes, 'Overall . . . generalizability [plays] a minor role in qualitative inquiry' (Cresswell, 2003: 195).

From this perspective, the folk notion of generalizability is not conducive to qualitative research, with its use of low numbers of cases and more open system of data collection. For qualitative researchers that adhere to the folk notions of social science, generalizability may seem like a far-off goal, something to strive for but never reach. For a qualitative researcher to be able to claim that his results are generalizable, he must either adopt research design principles from quantitative methodology, or use a mixed methods approach, wherein qualitative data-gathering is 'reinforced' with the use of a secondary, quantitative research tool. If qualitative researchers are unwilling to adapt to the positivist-defined rules of the game, they are unable to claim that their work is 'generalizable.' In order to achieve status for this work within a system of academic power/knowledge, the folk model of generalizability must be engaged.

Miles and Huberman argue in favour of adding quantitative measures to qualitative research projects as a means of increasing confidence in generalizing about results. They note that the 'careful measurement, generalizable samples, experimental control, and statistical tools of good quantitative studies are precious assets' that should not be ignored by qualitative researchers (Miles and Huberman, 1994: 42). David Silverman also approaches the issue of generalizability in this way. In order to overcome the shortcomings of sampling in qualitative research, Silverman suggests that researches obtain quantitative data within a broadly qualitative study (Silverman, 2001: 249-250). Alternately, qualitative researchers can use more rigorous sampling procedures to attempt to meet the goal of generalizability. According to Silverman, purposive or theoretical sampling can be used to 'overcome the dangers of purely "anecdotal" qualitative research' and to strengthen claims for generalizability (254). Miles and Huberman also discuss several sampling strategies that can help overcome the problems associated with small research samples. By using one of Miles and Huberman's theoretically-driven 'sampling strategies', claims for generalizability can begin to approximate the quantitative ideal (Miles and Huberman, 1994: 27-30).

Towards a distinctly qualitative model of generalizability

The strategy endorsed by Silverman, as well as by Miles and Huberman, argues for making qualitative research more like quantitative research. By doing qualitative research in this vein (which then becomes mixed-methods research), we are able to mobilize the discourse of generalizability in its 'folk notion of science' form. However, other qualitative researchers have critiqued the folk notion of generalizability that is tied to the positivist, quantitative tradition. Howard Becker critiques the folk notion of generalizability for perpetuating 'one of the great scams of our society: the notion that things called by the same name are the same in other respects' (Becker, 1990: 238). Thus, quantitative generalizability can gloss over meaningful differences in the social processes

that occur in social settings which have been sorted into the same analytical category. This process of abstraction via generalizability can sometimes obscure findings more than illuminate them. For example, as Becker writes, Some things called schools may actually resemble places that go by other names more than they do other places called schools' (239).

Instead of lamenting qualitative social science's inability to achieve the ideal of statistical generalizability, sociologists like Howard Becker and Robert Prus have attempted to construct a model of generalizability that is unique to qualitative research. This model focuses on the ways in which qualitative research can be used to generalize about social processes, while abandoning any claims to generalizability about populations. In this distinctly qualitative model of generalizability, the goal is no longer to study a sample of police officers, punk rockers, or kung fu students so that we can make generalizations about all police officers, punk rockers, or kung fu students. Instead, we examine the social processes that go on in police work, the punk rock subculture, or in the martial arts dojo. In analyzing these processes, we might see how they play out in potentially diverse social settings.

Prus describes this form of generalizability as a focus on 'generic social processes' (Prus, 1994: 394). This model focuses on 'social activity' rather than on the quantifiable attributes of social actors or institutions. Here, the term 'generic' refers to the ways in which forms of social interaction transcend the specific historical-spatial location in which they occur. Generic social processes are 'abstracted formulations of social behaviour' (395). Those who are interested in generic social processes are less concerned with how all members of a particular socially-constructed category are alike; they are more concerned with documenting processes that operate across social sites. It is important to emphasize that focusing on 'generic processes' as a form of generalizability is not an effort to render invisible the social-historical specificity that often gives qualitative research its depth. As Schwalbe et al. note: 'To call these processes "generic" does not imply that they are unaffected by context. It means, rather, that they occur in multiple contexts wherein social actors face similar or analogous problems. The precise

form a process takes in any given setting is a matter for empirical determination' (Schwalbe et al., 2000: 421). Abandoning aspirations to be more like quantitative social science, this model still allows the researcher to 'transcend the particular settings in which the data was gathered' (Prus, 1994: 394). Goffman's work on total institutions is cited by Becker as an example of this type of generalizability (Becker, 1990: 238). Through this work, Goffman illuminates how similar social processes operate in army units, convents and mental asylums. For Becker, this sort of processual generalization is ultimately more interesting than the statistical, population-focused generalizability embodied in the folk notion of science.

Schwalbe et al.'s article on social inequality provides a model for how this uniquely qualitative form of generalizability can work. Through a qualitative meta-analysis of a large and diverse body of research on social inequality, the authors distill a typology of social processes that appear to operate in many different settings. The authors describe variations on four main processes: 'othering','subordinate adaptation', 'boundary maintenance', and 'emotion management'. Through these generic social processes, the everyday interactions of individual social actors work to perpetuate social inequality, oppression and privilege across space and time until they are perceived as social 'structure' (Schwalbe et al., 2000: 439). Thus, in addition to describing how similar social processes may work across a variety of social locations, the processual focus of this notion of generalizability also emphasizes the constructed nature of 'social reality'. As such, it moves us away from a reified construction of 'social structure' as something concrete and unchanging.

Finally, I would like to use an article by Robert Emmet Jones and Riley Dunlap on environmental attitudes to illustrate how the notion of generic social processes can illuminate a lacuna in the folk notion of generalizability, as it appears in quantitative research (Jones and Dunlap, 1992). In a 1992 article from *Rural Sociology*, Jones and Dunlap use quantified American national survey data to look at the relationship between 'environmental concern' and various socio-economic factors. While their work might tell us, among other things, that there is a generalizable correlation between levels of formal

education and environmental attitudes, it tells us nothing about *how* the process of education works to form environmental beliefs. If we re-focused on social processes, we might begin to ask how people use educational resources to make sense, not only of environmental problems, but also of other social issues, like American militarism, First Nations land claims, or police brutality. By shifting our focus towards processes rather than populations, we may gain more insight about how attitude formation works across a variety of social sites. Working with the model of generic social processes as the guide for generalizability, we might gain a more complex understanding of attitude formation than can be captured in the statistical correlation between education and environmental belief as mathematical variables.

Prus and Becker offer one solution to the 'problem' posed by the folk notion of generalizability to qualitative social science. Rather than trying to make qualitative research more closely approximate a quantitative ideal, Prus and Becker describe a uniquely qualitative form of generalizability, one that is 'attentive to the interlinkages of theory, methods, and research' that is also 'genuinely attentive to the ways in which human group life is accomplished and experienced on a day-to-day, moment-to-moment basis' (Prus, 1994: 409). This re-construction of 'generalizability' disrupts the folk notion of science that has traditionally favoured quantitative research for being more generalizable.

Generalizability in a postmodern world

This model of processual generalizability, or 'generic social process', seems better suited to qualitative research than the folk notion of generalizability, with its emphasis on statistical extrapolation from research samples to populations. Whereas the folk notion of generalizability is a barrier to be overcome in qualitative research, the model of generic social processes works with the strengths of qualitative research, which seeks to explain how a sense of 'social reality' is accomplished by its participants. In this final section, I

would like to look at how well this model of generalizability holds up to the postmodern critique of the positivist paradigm.

The postmodern turn in the social sciences has radically disturbed the positivist moorings of the social sciences. According to Denzin, the postmodern critique has left social science in a 'triple crisis of representation, legitimation, and praxis' (Denzin, 1997: 3). To oversimplify, postmodernism encourages a deep skepticism about 'the possibility of *any* totalizing or exhaustive theories or explanations' about the social world (Gubrium and Holstein, 1997: 75). Postmodernism sees all knowledge as a social construction which is intimately connected with those who create it. Thus, knowledge is not an object that is found 'out there'. It is formed by intellectual workers who have particular experiential standpoints and ontological perspectives. As knowledge is essentially social, it becomes impossible to accurately represent any 'social reality' that exists separately from the observer. As Gubrium and Holstein write: 'Because "truth" is necessarily relativized, if not impossible, then social scientific reports should enjoy no special privilege over any other set of accounts' (92). Taking postmodernism seriously means that the positivist notions of validity, reliability and generalizability become increasingly untenable. For Denzin, social scientists should abandon the pretensions of positivism in favour of an approach that is 'post-structural to the core, . . . emotional, biographically specific, and minimalist in its use of theoretical terms' (Denzin, 1997: 26). Instead of seeing the research article as an omnipotent, neutral account, we should realize that our texts are primarily concerned with the ways in which 'our subjectivity becomes entangled in the lives of others' (27).

As Davies notes, the radical reflexivity of postmodernism is a valuable contribution to the social sciences (Davies, 1999). The postmodern critique of traditional, positivist and naturalistic social science is too compelling to ignore. Lincoln and Denzin's assessment of the situation seems accurate: 'It is not that we might elect to engage in work that is postmodern. Rather, it is that we have inherited a postmodern world, and there is no going back' (Lincoln and Denzin, 2000: 1059). If we take postmodernism seriously, then our folk notions of science, including the notion of generalizability, are seriously undermined. Instead of working as tools for establishing the 'truth' of a social

science text, generalizability, validity, and reliability become 'the researcher's mask of authority that allows a particular regime of truth within a particular text .. . to work its way on the reader' (Denzin, 1997: 7). Generalizability can no longer be invoked to prove the validity of an objective account of the 'world out there'. Rather, it is revealed as a rhetorical device for convincing the reader of the researcher's authority. Through a Foucauldian lens, generalizability may be viewed as a discourse that is invoked to privilege certain forms of research in the construction of academic knowledge.

At first glance, it might appear that the model of generic social processes is also undermined by the postmodern critique. After all, the notion of processual generalizability seems to rely upon the naturalistic assumptions of a realist 'world out there', a research model which strives to minimize its subjectivity. This notion also relies on the idea that the 'real world' can be 'faithfully' represented in an academic 'realist tale' (Gubrium and Holstein, 1997: 36). In light of the postmodern critique, where social science texts are stripped of their claims to objectivity and authority, we are left asking whether the notion of generalizability continues to make any sense. If each text is the result of a subjective interaction between an individual researcher and a particular social group, can we learn anything about social process that is applicable across social settings? In the remainder of this section, I will use Foucault's notion of power/knowledge to illustrate how a more tentative notion of 'generic social processes' can be quite useful in a postmodern world.

In the 'two lectures in *Power/Knowledge*, Foucault distills a general theory of power and knowledge from his work on sexuality, imprisonment and madness. Put briefly, Foucault writes:

> 'In a society such as ours . . . there are manifold relations of power which permeate, characterize and constitute the social body, and these relations of power cannot themselves be established, consolidated nor implemented without the production, accumulation, circulation and functioning of a discourse. There can be no possible exercise of power without a certain economy of discourses of truth which operates through and on the basis of this association' (Foucault, 1980: 93).

I believe that Foucault's model of power/knowledge may be viewed, in one sense, as a type of generic social process. In Foucault's own work, the reader sees how discourses embody power and knowledge to govern social life across several sites. In two different articles, David Ralph Matthews and Mary Curran illustrate how the generic social process of power/knowledge operates in the field of environmental regulation and management, an area far removed from Foucault's work on sexuality, mental illness, and imprisonment. Through these examples, we see how a particular generic social process can be used to illuminate similar aspects of diverse social situations.

Matthews uses Foucault's work to examine the ways in which the Canadian state used environmental discourse to legitimize their actions in the 1995 'turbot war' with Spain (Mathews, 1996). In this instance, ecological discourse was mobilized by the state to justify its claims of power over fishery resources. In essence, the state invoked environmental discourse in order to bring 'Canadian turbot' under the government of the Canadian state. Foucault writes, 'The bourgeoisie is interested in power, not in madness, in the system of control of infantile sexuality, not in that phenomenon itself' (Foucault, 1980: 102). In light of Mathews' analysis, we could add that the Canadian state is interested in power over 'Canadian' ecological resources, not necessarily in the health of turbot populations for their own sake. Environmental discourse becomes a means of creating this power.

Similarly, Mary Curran uses Foucault's notion of power/knowledge in her analysis of public hearings on the regulation of industrial pig farming in Kentucky. For Curran, the notion of power/knowledge is useful for explaining 'the role of power relations in determining what is included and excluded' in the public hearings that shape the regulatory framework for industrial pig farming (Curran, 2001: 15). By analyzing the public hearings discourse through a Foucauldian lens, she is able to illuminate 'the power relations embedded within existing social arrangements within which regimes of truth are developed and deployed' (31).

Through these two brief examples, we see how the concept of power/knowledge, as a generic social process, can be removed from its 'home' in the study of prisons and mental illness, and transplanted to the dramatically different social worlds of Kentucky

pig farming and Canadian turbot fishing. From a post-structural perspective, we should not let the fact that power/knowledge may act in various ways across diverse sites deter us from recognizing the utility of making generalizations about this process. Of course, the regulation of fisheries, or industrial pig farming, is not literally *like* the discursive construction of sexuality or madness. However, there is something familiar enough about the social processes going on in both settings that allows us to describe them using the same analytical language.

Elsewhere, Aull Davies has suggested that we should treat 'Weberian ideal types' more like literary metaphors than like objective social facts (Davies, 1999: 218). I would like to suggest that we should think about generic social processes in a similar way, as metaphors rather than reifications. If we take postmodernism, post-structuralism and other 'post-al' approaches seriously, if we let go of our ability to make authoritative claims about 'social reality', then a more tentative, qualified form of processual generalizability may continue to be useful for social scientists in a postmodern world. Through an amended notion of generic social processes, we can continue to draw on the primary benefit of 'generalizability', the ability to connect our work to the world beyond our immediate research data.

Conclusion

'Generalizability' describes the ability to make inferences about our research that go beyond the specific units of analysis that we have collected information about. As a 'folk notion of science', generalizability has been treated as a strength of quantitative research, which uses statistical methods to make inferences about 'populations' from data on smaller sample groups. This notion of generalizability is often mobilized to assert the legitimacy and relevance of social research. Insofar as generalizability has been constructed as an inherent weakness in qualitative social research, this folk notion of science has benefited quantitative forms of knowledge.

It is possible to retain an analytically useful notion of generalizability. Such a notion of generalizability is different from the 'folk notion of science' form of generalizability, wherein random sampling is used to extrapolate research results to an entire population. A distinctly qualitative model of generalizability is concerned with social processes rather than populations. It points out how similar 'generic social processes' can operate in a diversity of social settings. Whereas the statistical generalizability of quantitative research appears as a barrier to qualitative social science, this model of processual generalizability plays to the strengths of qualitative research.

However, insofar as this model is rooted in naturalistic assumptions about the nature of 'social reality' and the stance of the researcher, it should be further refined for our postmodern world. A post-structural model of processual generalizability would not claim to represent processes as social facts; it would also eschew claims to represent *the truth* about any particular social process. Without reifying the social processes it describes, a model of processual generalizability may still be useful for qualitative research. If we think of generic social processes as resembling literary metaphors more than social facts, they can be useful for increasing our understanding of how similar processes operate across such diverse settings as mental institutions, prisons, fisheries and pig farms. Such a post-structural construction of generalizability allows us abandon one of our old folk notions of science in favour of a notion of generalizability that is more tenable and useful for qualitative research in a postmodern world.

References

Babbie (1995) *The Practice of Social Research*. Belmont: Wadsworth.

Becker, H.S. (1990) 'Generalizing from Case Studies.' In: E.W. Eisner and A. Pecking (eds.) *Qualitative Inquiry in Education: The Continuing Debate*. New York/London: Teacher's College Press: pp. 233-42.

Cresswell, J.W. (2003) *Research Design: Qualitative, Quantitative, and Mixed Methods Approaches*. Thousand Oaks: SAGE.

Curran, M.E. (2001) 'Foucault on the Farm: Producing Swine and Subjects.' *Southern Rural Sociology*, (17): pp. 12-36.

Davies, C.A. (1999) *Reflexive Ethnography: A Guide to Researching Selves and Others*. London/New York: Routledge.

Denzin, N.K. (1997) *Interpretive Ethnography: Ethnographic Practices for the 21st Century*. Thousand Oaks: SAGE.

Foucault, M. (1980) 'Two Lectures.' In: C. Gordon (trans./ed.) *Power/Knowledge: Selected Interviews and Other Writings 1972-1977*. New York: Pantheon Books: pp. 78-108.

Gordon, C. (1980) 'Afterword.' In: C. Gordon (trans./ed.) *Power/Knowledge: Selected Interviews and Other Writings 1972-1977*. New York: Pantheon Books: pp. 229-60.

Gubrium, J.F. and J.A. Holstein (1997) *The New Language of Qualitative Method*. Oxford: Oxford University Press.

Hughes, D.M. (1995) 'Significant Differences: The Construction of Knowledge, Objectivity, and Dominance.' *Women's Studies International Forum*, 18 (4): pp. 295-406.

Jones, R.E. and R.E. Dunlap (1992) 'The Social Bases of Environmental Concern: Have They Changed over Time?' *Rural Sociology*, 57 (1): pp. 28-47.

Kleinman, S., M.A. Copp and K.A. Henderson (1996) *Qualitatively Different: Teaching Fieldwork to Graduate Students*. Unpublished.

Lincoln, Y.S. and N.K. Denzin (2000) 'The Seventh Moment: Out of the Past.' In: N.K. Denzin and Y.S. Lincoln (eds.) *Handbook of Qualitative Research*. Thousand Oaks: Sage: pp. 1047-65.

Matthews, D.R. (1996) 'Mere Anarchy? Canada's "Turbot War" as the Moral Regulation of Nature.' *Canadian Journal of Sociology*, 21 (4): pp. 505-22.

Miles, M.B. and A.M. Huberman (1994) *Qualitative Data Analysis: An Expanded Sourcebook*. Thousand Oaks: SAGE.

Prus, R. (1994) 'Generic Social Processes: Intersubjectivity and Transcontextuality in the Social Sciences.' In: *Doing Everyday Life: Ethnography as Human Lived Experience*. Mississauga: Copp Clark Longman.

Scatamburlo-D'Annibale, V.L. and L. Langman (2002) 'Fanon Speaks to the Subaltern.' *Bringing Capitalism Back for Critique by Social Theory*, (21): pp. 253-84.

Schwalbe, M. et al. (2000) 'Generic Processes in the Reproduction of Inequality: An Interactionist Analysis.' *Social Forces*, 79(2): pp. 419-52.

Silverman, D. (2001) *Interpreting Qualitative Data: Methods for Analysing Talk, Text and Interaction*. London: SAGE.

Smaling, A. (2003) 'Inductive, Analogical, and Communicative Generalization.' *International Journal of Qualitative Methods*, 2 (1).

Yin, R.K. (2003) *Case Study Research : Design and Methods*. Thousand Oaks: Sage.

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This precious human life

A sociological study of everyday buddhist (and other) belief and practice

Introduction

My PhD researches the integration of Buddhist teachings into everyday life in Australia. It is a small comparative study of an interconnected group of urban Western Buddhists and non-Buddhists, part of my own social network. The study focuses on stories informants told me about their social interactions, at home, at work and in the world at large, and what these stories reveal about everyday belief and behavior. It was inspired not only by an enduring interest in sociology and social theory, but also by my very catholic passion for religion; by my contact with Vedic scriptures as a young adult in the late sixties and seventies, and more currently, by my own exploration of Buddhist belief and practice.

The study of Buddhism in everyday life offers enormous interdisciplinary scope. In this paper I trace my research process. I begin by discussing the part interdisciplinary research plays in the study of Buddhism, then move to methodological and epistemological issues. I include an overview of my research methodology and where relevant, extracts of reading notes and field notes. I close with some tentative findings, and discuss the role interdisciplinary study has played in drawing my conclusions

Interdisciplinary research and buddhism

The first task for any PhD is to design a do-able study. Like many, I struggled to formulate a simple question. From my earlier research on Buddhist conversion I had discovered Buddhists Westerners often reflected on the Buddhist *suunyaata* doctrine, which explains Buddhist understandings of self. So I wanted to investigate further whether and how Buddhists Westerners integrate Buddhist teachings into their everyday lives. Two key studies also pointed me in this general direction. The first was David Ho's (1995) *Selfhood and Identity in Confucianism, Taoism, Buddhism and Hinduism: Contrasts with the West* encouraging further research into Eastern conceptions of selfhood. The second was Michele Spuler's (2000) *Characteristics of Buddhism in Australia*, which identified a need for studies exploring the integration of Buddhism in Australia.

My overarching interest is spirituality in the current era. I specifically wanted to explore some questions I had about individuals, spirituality and discourse, about Western metaphysics and about East-West dialectics. I needed, then, to develop an understanding of current theoretical positions on discourse and the various sites where Western discourses are contested, and acquire a working understanding of the history and development of Western epistemologies as a point of comparison with Buddhist philosophical views. And I needed to clarify and situate my research question.

So I began to read. I started with cultural studies and an investigation of Western notions of self and subjectivity. This led me back to beginnings and very soon I was deeply immersed in European philosophy. Within both disciplines I discovered material relating to my general research topic, and as I roamed through and across other disciplines - social theory, theology and post-structuralism in particular, I took quantities of notes, which I habitually date, that are now proving useful as a quasi-annotated bibliography. These helped me trace my reading and its developing interconnections as I moved from one paradigm to the next.

For example, an extract of my notes from Habermas (2001: 130) on modernity, which I was reading to locate a broad context in social theory, dated 14 July, 2003,

highlights his links between philosophy, sociology and social theory (130); these links assisted my recognition of the truly interdisciplinary nature of my research, and will resurface in my analyses. The extract reads:

Why the 'modern' became a topic for philosophy in the first place -
3 separate questions

1. When and how did philosophers become interested in an *interpretation of the specific condition of modernity?* [*See Giddens* (1993: 284)]

2. Why do these philosophical interpretations take the form of a *critique of reason?* [*See Derrida* (1976)]

3. Why did *philosophy* eventually cede the ongoing task of an interpretation of *modernity to social theory?* [*Explore this question as rationale for sociological study*]

As I took notes, I bracketed my own comments thus [ ], as a guide to my own interpretive links for future reference. I read a great deal. Only when I found myself struggling through the early pages of *Of Grammatology* (Derrida, 1976), one copy in English, the other in French, to acquaint myself with classic sources of post-structuralist critique, did I realize time was precious, and that I needed to make some serious decisions. What did I want to ask, exactly, of whom, precisely, and how did I expect to interpret the answers? Taking my problem to my supervisor helped. He simply asked did I want to theorize or do an empirical study. I wanted to do an empirical study. Why?

One of my principal theoretical interests is reflexivity and its role in everyday life. Given my earlier research observations of the way Buddhists reflect on and integrate the *suunyaata* teachings into their everyday lives, I wanted to explore reflexive spirituality further. Giddens (1993) and Habermas (2001: 130), among others, have written theoretical works on reflexivity and modernity, but as yet there is only a small, but

growing body of empirical studies. Empirical study of reflexive spirituality particularly merits closer attention (Besecke , 2001; Roof, 1999).

My main problem at this point was to decide within which discipline I might skillfully design such a study, define a sample, gather data and pursue the tricky task of data analysis most confidently and comfortably. I had read a lot of theory. I began to read a lot of methodology. Earlier I noted Habermas (2001: 130) asks why *philosophy eventually ceded the ongoing task of an interpretation of modernity to social theory*. This is an important question for interdisciplinary research of any kind, since understanding the answer underpins, to some extent at least, the philosophical logic of contemporary social scientific studies.

Habermas (2001: 132) writes that Enlightenment thought, in Kant and most particularly in Hegel, developed the notion of modernity as a 'temporal index' oppositional to tradition to validate philosophy's insights:

> 'If true philosophical insights are … to claim a context-transcendent validity, then philosophy must grasp and penetrate this disquieting present, and articulate it in its concepts. Philosophy can only seek to overcome the boundaries of the historical situation which philosophical thought itself occupies by conceiving "the modern" as such ... Philosophy must meet the challenge of time with an analysis of the "new age".'

The social sphere, suggests Habermas (2001: 137), because of its ambivalence - both its disintegrations and its developments - draws on reason for comprehension, but also for critical *interpretation*; '[t]hought itself is reflectively pulled into the motion of the dialectic of Enlightenment', (Habermas, 2001: 136). 'And it is only reason, in turn, that can give modernity - unprecedented, open to the future, anxious for novelty - its orientation' (Habermas, 2001: 136). Thus philosophy with its two sides of reason, explanation and critique, entwines with social theory for further explication; and although social theory found its research program - the critique of social consciousness - in philosophy, it must, according to Habermas (137), be carried out by its own methods.

Here was my rationale for turning to social science for my methodology. I turned to the sociology of religion.

There, among many studies I found two that captured my attention and perfectly articulated my research aims; the first, by Robert Wuthnow (2003), related to method and the second, by Peter Berger (2001), related to my epistemological concerns.

First I address method. Wuthnow (2003: 4)) suggests that in the study of religion and society:

> '[t]here are reasons to make the most of what science has to offer – science teaches us the value of empirical rigor and the need for systematic investigation. The scientific method involves thinking of ways in which our cherished assumptions about the world may be proved wrong. It involves the *strategic* (italics added) use of rationality, not in the interest of doing away with all that is rational (any more than the legal system is meant to replace literature and music), but to have reasons for conducting our research in one way or another. Science also involves the criterion of replicability, and that means candidly disclosing what we have done so others can track our mistakes. Those aspects of science can be followed without claiming to be finding universal laws for behavior, and they can be employed in the study of religion without "explaining away" the topic of inquiry. The more scholars have applied scientific methods to the study of human behavior, the more they have learned that human behavior is indeed contextual and contingent, and that its meanings must be examined from multiple perspectives.'

Building on the insights I gained reading Habermas, Wuthnow's perspective neatly summarized the very reasons I wanted to construct a candid, qualitative study exploring the tales people tell about everyday life - their experiences, interactions and communications and the meanings they attach to them.

Then came the second major issue, epistemology. Reading Berger alerted me to the question of what biases I, as a researcher, might bring to my study, my analyses and my interpretations. Berger (2001: 448) writes 'as I increasingly turned from theory to empirical problems, I found the sociology-of-knowledge paradigm of my early work very useful and have not been motivated to exchange it for another'. From this perspective,

and this can be said of Australia, religion is perceived as an interactive phenomenon produced by an ongoing and recursive dialectic between individuals and societies (Bouma, 1992: 33-63). For my study it means I bracket off theological truths, and concentrate on my informants' disclosures about their interactions, their beliefs and practices; I might then relate my findings back to my questions about East-West dialectics and the integration of Buddhist teachings into everyday Australian life. Taking this stand, I felt secure in the task of designing a study that simply asked informants for their stories, which I could analyze and interpret without theological comment or truth claims. My analytic perspective would focus more on discourse and context, mindful of my own part as a researcher in the hermeneutic process.

Sociology is the discipline in which I feel most comfortable, confident and competent, and a discipline I believe holds many research advantages; for me, it offers both a rigorous methodological framework and a wide analytical and interpretive horizon. Also, according to Fenn (2001: 8), it is important that sociologists of religion 'be open to insights and methods from other fields of inquiry'. I have followed Fenn in this and his next stricture, that sociologists of religion 'look to other disciplines for theories that will account for the persistence and vitality of the experience of the sacred even in societies that are relatively secular' (Fenn, 2001: 20) as is Australia. So my analyses and interpretive explications will draw on a range of disciplines - theology, philosophy, and social theory, but I will situate them within a sociological reading of religion and spirituality in modernity.

Next I address methodological issues - how I went about locating informants, eliciting interviews and processing data for the study.

Method

At this point my first methodological concern was ethical. Even before I went about sample selection, or ethics approval from my home university, it was important to me to have a senior member of the Australian Buddhist community approve my research

proposal. Fortunately I had been tutored by one such individual as an undergraduate, so I approached him for his opinion. He assured me the proposal was appropriate and even valuable, and offered a few pointers about sample selection and interview schedules for good measure. With this question resolved I felt ready to go out 'into the field'. The study is situated in Melbourne, one of the southernmost capital cities on the Australian mainland. It is a beautiful bayside, multicultural city and home to a richly diverse society.

<u>The sample</u>

At first I intended to recruit Buddhist informants from three Buddhist organizations frequented by Westerners, and to recruit the non-Buddhist group as a snowball sample. Due to time constraints, I amended this plan, and was granted ethics approval to recruit both groups as a 'snowball' sample. The only criterion stipulated for both groups was that each individual be aged between seventeen and seventy-five, and from a non-Buddhist society or culture.

The final sample consisted of ten Mahayana Buddhists, which has the largest representation amongst Australia's rapidly growing Buddhist community, six male and four female, and ten non-Buddhists, seven female and three male, who declared no particular affiliation with any religious group at all, but whose stories, it emerged, contained many unsolicited spiritual references. This is not unusual in Australia, where, according to the 2001 Household Census, the number of people who identify with the religious category 'inadequately described' is increasing (Bouma, 2003: 64). The two Jewish informants in the non-Buddhist group declared themselves to be cultural rather than religious Jews, and two other informants had earlier affiliations with Vedic spirituality. None considered themselves Christian.

Very quickly, the study took on the appearance of a small ethnography - two groups of individuals interconnected through family members, friends, or friends of friends, representing a miniature community whose lives intertwined. Often connections occurred not 'within groups' but across an imaginary dividing line 'between groups'. For example, I interviewed two women, one in each group, whose friendship spans many years. Two worked together and were both friends of a third, and almost all the Buddhists

knew each other. All twenty informants were either recruited by somebody already participating in the study, or were known to me personally and volunteered. All were part of part of a larger, unarticulated network of friends and relatives in Melbourne, subject to very similar socio-cultural influences. Although the overall sample is small, I was and still am primarily concerned with similarities and differences emerging from interview data, and how these serve as a window into broader Australian socio-cultural contexts. Below is one of my journal entries commenting on the sample:

General method notes/field notes

6/8/03

It seems to me that this research is taking the shape of an ethnography, or a study of a particular, small Melbourne coastal, family-friendship demographic (see Bordieu/habitus - supervisor). Data has been gathered as a 'snowball' sample, so all informants are linked to one another in some small way - six degrees of separation, as one informant commented. I'm beginning to think I could actually incorporate this aspect of the research into the thesis itself - to portray a 'slice of life' or a picture of a culturally and socially diverse group of Melbournians whose lives are socially interconnected, sometimes a lot, sometimes a little, but who offer a 'realist' picture of Melbourne, and specifically, local, St Kilda and its environs, its socio-cultural connection and diversity – 'Indra's net' (Loy, 1993). Not all informants are connected in this way - some (in the Buddhist sample) are simply friends of friends I have met socially (e.g. Liz K) who then referred me to other Buddhists of their acquaintance (i.e. Paul M, Ricky and Tony, and James and Tina M, who have taken vows).

Since this entry I have discovered more interconnections and plan to explore this aspect of the study in depth in the method chapter of the thesis.

In-depth interviews

Operationally I designed the study to elicit in-depth interview material about social interactions - at home, at work and in the community, from both groups of informants. I selected topics such as *gratitude, forgiveness, regret, partnership and attraction*, *pleasure* and asked informants from both groups the same questions - did they have a story about

an interaction that had left them feeling enormous gratitude - at home or amongst friends; again, at work, and finally, in the wider community? Having posed each question, I encouraged my informants to narrate at length, interjecting only occasionally to clarify a point or offer further encouragement. In operational terms I set out to investigate whether the stories Buddhist Westerners tell about their social interactions differ from those told by Non-Buddhists in ways that demonstrate significantly different belief systems and interpretive paradigms. The interviews lasted between forty-five minutes and two hours.

Data analysis

According to Bouma (1992: 28), there are three levels of analysis appropriate to sociology of religion – the socio-cultural level, the organizational and the individual level. This study engages the individual level, exploring the impact of Buddhist and other spiritual belief and practice on individual behavior (Bouma, 1992: 28). To what extent might the stories people tell about their everyday interactions contain spiritual references that seem to inform their lives, and what are some of the possible sources and consequences? What would comparisons of both sets of stories reveal about religion and spirituality and Australian society and culture?

Analyzing interviews, I was, and still am less concerned with substantive elements of why informants feel grateful and so on; my main focus is on common themes within and between groups, whether any differences emerge, and what similarities and differences might mean. Designing the study, it was my hunch, from my own experience in the Buddhist community, that some of the Buddhist sample might refer to Buddhist teachings as they reflected on their interactions. I had no hunches at all about the non-Buddhist group, and those interviews yielded unexpected data - a kind of intense spirituality was articulated again and again by both groups of informants. Even as I transcribed interviews I began to identify emerging themes that both sets of stories seemed to have in common – critical reflections on aspects of Western epistemologies, reflexive spirituality, virtuoso spiritual practice, alienation from aspects of Western society and culture and interest in the East, to name a few.

Tentative findings: Beyond reason

Jean-Luc Nancy (2000: x) recalls, on the opening page of *Being Singular Plural*, that Nietzsche wrote 'we are now "on the horizon of the infinite"; that is, we are at the point where "there is no more land", where "there is nothing more terrible than the infinite"' (Nietzsche, 1974: 180) and asks are we finally going to learn this lesson or is it now impossible for us to learn anything else (Nancy, 2000: x) ? The Buddhist group in this study expressed strongly held convictions that there is much to be learned; perceived gaps in their understandings about life and interaction, many said, offered them opportunities to change and learn, principally through recourse to Buddhist teachings and practice, their Buddhist teachers and their community. Motivated by both a desire to learn and to change, members of the Buddhist group articulated a belief that life's contradictions and paradoxes could be understood and synthesized not only through the persistent use of reason and communication but also by developing inner resources - intuitive patience and meditation practice, and more *mystical* aspects of Buddhism. For these people, a Buddhist lifestyle supported the notion that life has its own mysteries and lessons that could, with perseverance, be learned.

Across both groups, my principal finding is that both a 'reflexive spirituality' identified by Besecke (2001: 365), and Giddens' (1993: 90) more general 'reflexivity' were major factors in the process of meaning-making. Comparing the two groups, the most obviously discernible difference at this point is how reflexivity plays itself out. From a Buddhist perspective, Buddhist doctrines are *intended* not only as teachings to be reflected upon, but as guiding principles in everyday life (Loy, 1992: 151-180; Thich Nhat Hanh, 1998: 50; Lafitte and Ribush, 2002: 127-133).

It is not surprising then that in the Buddhist group many stories included a Buddhist component - some reference to a Buddhist teaching or practice upon which an informant drew to understand and give meaning to their experience. A similar process occurred with most non-Buddhist informants, except references were more eclectic, referring to management theory, literature texts, popular psychology – indeed a vast range of contemporary ways of interpreting and giving meaning to everyday experience

available to Australians, and possibly globally in the twenty-first century. Other forms of spirituality appeared in the stories told by the non-Buddhist group, suggesting, as Bouma writes, that spirituality in Australia is diverse and currently 'precisely in keeping with what would be expected in a postmodern, secular and multicultural society' (Bouma, 2003: 67).

In many ways the two groups were not so very different; while the substance of their stories varied, there appear to be common focal points and ways of ascribing meaning to experience shared by members of both groups – many informants across the entire sample spoke about family as the source of both intense pleasure and conflict, about spiritual practice of varying kinds as the thing that helps to get them through the day, about interests in Eastern knowledge systems and about interconnection with friends and loved ones as extremely important.

Conclusion: Into the mystic

My conclusions, drawing on the links between philosophy and social theory discussed earlier, focus on four major topics - *the marginalization of the mystical in Enlightenment thought, the epistemological consequences of that marginalization, and the re-entry of the mystical into everyday Western socio-cultural life, specifically through the religions of the East, and through Buddhism in particular.* Although Christian (and other) mystical traditions have survived the hegemonic forces of Protestantism in Australia, I want discuss one of the less well researched ways Westerners achieve a sense of the mystical in everyday life - through the integration of Buddhist belief and practice.

Habermas suggests in *classical* sociology and critical theory, Weberian and Frankfurt school conceptions of modernity, based on abstract oppositions between a disciplinary society and the 'fragile subjectivity of individuals' need to be superseded by a communicative reason and a 'hermeneutic insight into the symmetrical structure of every attempt to reach understanding' (Habermas, 2001: 151). Such insight would facilitate an 'intersubjectivist' (Habermas, 2001: 153) neoclassical reconceptualisation of modernity,

which in turn would rely on the support of a *critical* social theory. The balance, for Habermas (2001: 156) is ideally achieved through philosophical recognition of both the *values and dangers of reason*. 'The analysis must keep in mind both the emancipating, unburdening effects of communicative rationalization of the life world, and the effects of functionalist reason run wild' (Habermas, 2001: 156).

In this discussion of reason and modernity, the explication Habermas proposes is complex to be sure; one of his most salient arguments in this context is that modern discourses - science, morality or law - are 'directed by principles, and submit themselves to self-reflective standards' (148); that they are frequently misused as a medium for social, political, epistemic and cultural violence, argues Habermas is 'by itself no basis for renouncing the promise' (148) implicit in this recursive self-monitoring, which by definition must include, most importantly, a *critique* as well as a *defense* of reason. Beyond both, I suggest, is a synthesis.

My final conclusions draw on key thinkers in contemporary philosophy and refer principally to deconstruction of the rationalist imperative through the writings of Foucault, Derrida and Jean-Luc Nancy. I draw also on the sociological insights of Merton, Beckford and Fenn. I argue for a theoretical balance between rationality and the mystical, suggesting a synthesis of the two is possible beyond reason and the critique it attracts. I conclude further that it is through the integration of Buddhist belief and practice into everyday life that many Westerners in Australia achieve this middle way.

References

Berger, Peter (2001) 'Reflections on the Sociology of Religion Today: The 2000 Paul Hanly Furfey Lecture.' *Sociology of Religion*, 62 (4): pp. 443-455.

Besecke, Kelly (2001) 'Speaking of Meaning: Reflexive Spirituality as a Cultural Resource.' *Sociology of Religion*, 62 (3): pp. 365-376.

Bouma, Gary D (1992) *Religion: Meaning, Transcendence and Community in Australia*. Melbourne: Longman Cheshire.

Bouma, Gary D. (2003) 'Globalization, Social Capital and the Challenge to Harmony of Recent Changes in Australia's Religious and Spiritual Demography: 1947-2001.' *Australian Religion Studies Review*, (Spring): pp. 55-68.

Derrida, Jacques (1976) *Of Grammatology*. Maryland: The John Hopkins Press.

Fenn, Richard (2001) 'Editorial Commentary: Religion and the Secular; the Sacred and the Profane: the Scope of the Argument' In: Richard Fenn (ed.) *Blackwell Companion to the Sociology of Religion*. Oxford, Blackwell Publishers: pp. 3-22.

Giddens, Anthony (1993; Philip Cassell ed.) *The Giddens Reader.* London: Macmillan.

Habermas, Jürgen (2001; Max Pensky trans./ed.) *The Postnational Constellation: Political Essays.* Cambridge: Polity Press.

Habermas, Jürgen (2001) 'Conceptions of Modernity.' In: Max Pensky (trans./ed.) *The Postnational Constellation: Political Essays*. Cambridge, Polity Press: pp. 130-156.

Ho, David (1995) 'Selfhood and Identity in Confucianism, Taoism, Buddhism and Hinduism: Contrasts with the West.' *Journal for the Theory of Social Behavior*, 25 (2): pp. 115-139.

Lafitte, Gabriel and Alison Ribush (2002) *Happiness in a Material World: the Dalai Lama in Australia and New Zealand*. Victoria: Lothian.

Loy, David (1992) 'Avoiding the Void: The Lack of Self in Psychotherapy and Buddhism.' *The Journal of Transpersonal Psychology*, 24 (2): pp. 151-180.

Nancy, Jean-Luc (2000; Robert D. Richardson and Anne E. O'Byrne trans.) *Being Singular Plural.* Stanford: Stanford University Press.

Roof, Wade (1999) *Spiritual Marketplace: Baby Boomers and the Remaking of American Religion.* New Jersey: Princeton University Press.

Spuler, Michele (2000) 'Characteristics of Buddhism in Australia.' *Journal of Contemporary Religion*, 15 (1): pp. 29-44.

Thich Nhat Hanh (1998) *The Heart of the Buddha's Teaching*. Sydney: Random House.

Wuthnow, Robert (2003) 'Is There a Place for the Scientific Study of Religion?.' *The Chronicle*, January 24. Http://chronicle.com/weekly/v49/i20/20b01001.htm.

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B. Hassler's

'Science and politics of foreign aid'

Hassler, Björn (2003) *Science and Politics of Foreign Aid: Swedish Environmental Support to the Baltic States*. Dordrecht: Kluwer Academic Publishers: 221 pp. ISBN 1 4020 1167 9.

Foreign aid has been subject to investigation since the beginning of worldwide aid distribution after the Second World War. Numerous studies have focused on the different factors that are important in the policy process. Several studies found evidence of the influence of donor interests in aid allocation (e.g. McKinlay and Little, 1979; Maizels and Nissanke, 1984). A new book, *Science and Politics of Foreign Aids,* by Björn Hassler,[1] continuous this old tradition, by focusing on Swedish interests in environmental support to the three Baltic States, right after their release from the Soviet yoke. Although it continuous an old tradition, it does so in a new way that is very accessible to people other than scholars that are interested in statistical evidence. It gives a broad overview of Swedish aid and environmental policy and draws some important, although debatable, conclusions with respect to the real nature of Swedish aid.

The objective of this book is to show that national interests played an important role in foreign aid delivery in one specific case, namely Swedish environmental support towards the three Baltic States, Estonia, Latvia and Lithuania, for the period 1991-1996.

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[1] Björn Hassler received his MSc in Political Science at Linköping University, Sweden in 1994 and his PhD at the same university in 2000 for the research on which this book has been based. The book has been written within the research project *Social Change and Democracy in the Baltic Sea Region*. During the work on this book he has been employed by Södertörns Högskola, Sweden.

The author uses a rational-choice approach in his search for egoistic motives. He assumes people to behave rationally and to be primarily concerned with their own interests. Furthermore, he assumes a consensus in the national policy arena, in order to use governments as central actors in his analysis. He does not deny that the starting point of foreign aid may be altruistic concern; he only claims that self-interest motives play a dominating role in the choices that are made in the foreign aid delivery process.

Hassler uses a model based on game theory to show under what conditions Sweden and a particular Baltic country would engage in a joint project in the Baltic country, assuming that both players are only interested in their own well-being. Obviously, it follows that both players would only co-operate if expected benefits exceed expected costs. For the Baltic country this would only be the case if collective and private net benefits from the joint project plus net Swedish grants exceed the collective and private net benefits from an alternative domestic project plus the collective benefits from an alternative Swedish projects. For Sweden, this would be the case if collective benefits from the joint project minus the gross grant exceed the collective and private net benefits from an alternative domestic project plus collective benefits from an alternative Baltic project. Although several factors are left out of Hassler's model it may be sufficient to give a good approximation of actual considerations.

The book is based on three hypotheses, which are derived from this model and are to be tested in a quantitative way supported by qualitative data. The hypotheses are not tested until the final chapter, after a thorough elaboration on the origins of aid, the characteristics of environmental policy, the Swedish consensus and the Baltic States' environmental preferences.

The first hypothesis states that the collective good content is higher in the set of assisted projects than in the set of projects financed exclusively by domestic resources. The hypothesis is tested by comparing the Baltic States' environmental preferences with the characteristics of joint environmental projects. Baltic States' preferences are said to consist of eleven issue areas with the same value attached to each of them by the Baltic States' governments. The collective good content of the eleven issue areas is estimated and the projects are ranked accordingly. From a diagram it is concluded that issue areas

with a high collective good content are more likely to be involved in joint projects than those with a low collective good content. There does, however, not seem to be a statistical relation between the collective good content and the number of projects or the amount of Swedish resources invested. Nonetheless, the author claims that Swedish interests play a dominant role. There are at least two important aspects to this analysis that the author fails to mention. First, it may very well be the case that the amount of collective good content is not that important, but that it is the sheer presence of some collective good content that is essential for Swedish decision making. This could be the case because it is so hard to identify the exact amount of collective good content. Second, the author does not recognise that there might be very large differences in effectiveness and efficiency of additional environmental investments in the different issue areas, which could explain differences that are not explained by a simple regression analysis on the amount of invested resources or the sheer number of joint projects.

The second hypothesis claims that the number of initiated projects will be high in the early phases of the interaction between Sweden and the Baltic countries, and thereafter stabilises and only varies with, for example, relative changes in national income, relative public spending, or external disturbances. This would be the case because there is much to win in the early years of co-operation. Marginal benefits of environmental investments tend to be decreasing. Furthermore, opportunity costs of environmental investments by low-income countries are higher than by high-income countries. This means that an increase in Baltic national income would lead to a diminishing need for assistance, because the countries are willing to invest more themselves. The exact formulation of the hypothesis does, however, not seem to be in line with an expected continuous decrease in environmental assistance. Furthermore, this hypothesis is not really tested in Hassler's work. It is just touched upon in the final chapter. It is shown that it took two to three years before the volume of assistance reached a steady rate, but a decline had not been observed by 1999. This could have at least two causes, of which only the first has been mentioned by Hassler. First, the economic downturn in all three Baltic countries did not give much room for Swedish assistance to withdraw. And second, it could be the case that there were certain

restrictions on the amount of Swedish assistance. Maximum levels, provided by reluctance of the Swedish public to spend more money abroad, could have resulted in less than optimal investments in the early years. Minimum levels, on the other hand, may have been provided by long-term contracts, the influence of vested interests, or the existence of specific policy targets, which could have been the cause of the absence of an expected decrease in Swedish environmental assistance.

The third and final hypothesis states that externally financed environmental projects tend to withdraw domestic resources from nationally prioritised areas where the collective good content is low. This seems quite obvious as well, since no country has unlimited resources and only a certain amount will be available for environmental investments. Consequently, the hypothesised increase in projects with a high collective good content will lead to a decrease in the number of projects with a low collective good content. Although no statistical tests or data have been shown to prove this claim, it is stated by Hassler that resources flew from projects with a low collective good to projects with a high collective good. Furthermore, it is argued that utility for the Baltic country would have been higher if projects with a low collective good content had as much chance of being selected as projects with a high collective good content.

In Hassler's book, it is claimed that Swedish environmental support to the Baltic States in the period 1991-1996 has primarily been driven by self-interested considerations. The evidence provided by the testing of the three hypotheses is, however, not very convincing. The book is quite accessible though, because it does not depend very heavily on statistical evidence and formal modelling. On the other hand, scholars that are very much into formal rational-choice modelling and hard-core statistical testing will be quite disappointed. Although the book has the objective to explain, it is in some respects not much more than a descriptive work on Swedish environmental aid policy towards the Baltic States. This is exacerbated by the quite lengthy elaboration on the history of Swedish aid and environmental policy. Hassler's new book is, however, very clearly written and provides the reader with a lot of information on Swedish aid and environmental policy. It is, therefore, a colourful addition to any scholar's bookshelves dealing with either one of these policy fields.

References

Maizels, A. and M.K. Nissanke (1984) 'Motivations for Aid to Developing Countries.' World Development, 12 (9): pp. 879-900.

McKinlay, R.D. and R. Little (1979) 'The U.S. Aid Relationship: A Test of the Recipient Need and Donor Interest Models.' Political Studies, 27 (2): pp. 236-250.

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Before a return to metatheory
T. Eagleton's 'After theory'

Having read Terry Eagleton's *After Theory* – a book that has been predicted to send ripples throughout cultural theory – I am left with a number of unanswered questions. *After Theory* appears to have the primary objective of encouraging cultural theorists to overcome their inhibitions and to consider the metaphysical questions that often go ignored. On the surface this appears to be a worthy direction. However, the argument is not totally convincing.

The central argument of Eagleton's text is that cultural theorists should move toward a focus upon the bigger transcendental questions required by the transforming cultural environment. Or, rather, Eagleton's text is a challenge to cultural theorists to transfer their attention to questions of truth, morality, fundamentalism and death, in order to ensure the relevance and survival of cultural theory. For Eagleton, this can only be achieved by an overcoming of the fear caused by the sustained avoidance of these universal issues in favour of the minutia of everyday life. As a result of this focal shift cultural theory will re-separate itself from everyday life and will then be in a position better suited to its critique.

It is important to acknowledge that *After Theory* is not an attempt to undermine the achievements of cultural theory. This is something that Eagleton is very careful to clarify within the text. Rather, in the tradition of critical theory, *After Theory* represents

an attempt to encourage the reader to reconsider cultural theory through critique. It is an intimated attempt to overcome uncritical consensus within cultural studies.

Eagleton's text also contains a secondary argument. This argument also adopts the form of a critique of existing contemporary cultural theory. 'Culture only seems free-floating because we once thought we were riveted in something solid, like God or Nature or Reason. But that was an illusion.' (Eagleton, 2003: 57) Eagleton dismisses the understanding of culture as a free floating, liquid, fluid substance as a construct of discourse, a language game. This particular language game exists as an opposition to the previous discourse of the heavy, hard, solid culture of history. We understand culture in a particular way because we describe it using those particular metaphors, and, furthermore, these metaphors exist in opposition to the previous discourse (in the form of metaphors) used for describing culture. This is a useful and illuminating criticism of the dominant ideologies of cultural theory. This criticism can be used to transform not only the understanding of culture, but also to transform the understanding of the construct (or language game) that is cultural theory.

Does Eagleton really want a return to narrative forms of Knowledge? The answer must be yes. Does he accept the floored nature of scientific forms of knowledge? The answer again must be yes. A reading of the text actually reveals that although this is Eagleton's position he would like the reader to disregard these two guiding assumptions. Eagleton wants a form of scientific knowledge to emerge that evokes the spirit of narrative forms of knowledge. The focus then must switch to paralogy. Paralogy is a form of (or approach to) knowledge that incorporates, accepts, and celebrates paradox, inconsistency, undecidables, and incomplete information (Lyotard, 1999: 60). This form of (scientific) knowledge is brought about by the increase in the questioning of, and incredulity toward, established scientific knowledge that is based on rigid measurement, universal acceptance of the result, absolute truths, and the totality of consensus based opinion. Paralogy appears to be the direction that Eagleton is tacitly intimating. Paralogy must be considered to be a postmodern methodology. If this is the case then Eagleton's argument that postmodernism is coming to an end must be regarded is inaccurate, or at least premature. *After Theory* suggests that our concerns and parameters must stretch to

include metaphysical questions within a contemporary context. The answers to these questions must be free from narrative formulation while at the same time embracing its essence. This is a complex perspective. The complexity of which is partially masked by Eagleton's accessible writing style.

Eagleton writes in what can be described as streams of consciousness. The book is divided into eight chapters with no individual subsections to guide the reader. Instead Eagleton grabs onto threads of thoughts and draws them out into micro conclusions. These are the streams of consciousness. He allows these ideas the space to develop on the page. The writing feels like an imprint of the thought processes involved in reaching a conclusion. It appears that Eagleton is attempting to develop a style that supports the major arguments within the text. This approach makes the text eminently readable. Once you attach yourself to the Eagleton's unpicked threads it becomes difficult to let go until a conclusion is achieved. It is a book that should not be used for reference purposes but for cover-to-cover reading.

As I have mentioned, the text leaves a number of unanswered questions – this is not to devalue the importance or quality of the text – these questions bare testament to the value of *After Theory*. The book asks us to return to metaphysics, this is fraught with danger; it can mean that the theorist loses focus upon the microscopic (intertextual) details that are interwoven within everyday life. If we look at these big questions then it is possible that the smaller ones will remain unanswered or ignored. It is often these smaller questions that combine to create mosaics of the bigger answers. Moreover, if society itself becomes preoccupied with metaphysics then the risk is even greater. In this instance the possibility of mass narcissisms, detachment and dehumanisation may be a short journey away. This is a journey that has been previously mapped out by Theodor Adorno (2001).

The text suggests that we are coming to an end in terms of the cultural applications and analytical relevance of the postmodern condition. Crucially Eagleton acknowledges that this does not mean that we will return to a world without potmodern theory (or without cultural theory), rather we will enter a new epoch. If this is the case then why is this particular text so clearly influenced by Jean-François Lyotard's *The*

Postmodern Condition (1999). It appears that Eagleton's arguments are actually undermined by the presence of his own writing, as a result *After Theory* must be considered to be paradoxical. It argues an end to something of which it is a part. Conversely *After Theory* actually supports the rather unfashionable argument that the postmodern condition is analytically profound and culturally apparent.

If, as Eagleton suggests, we now shift our focus to larger questions and dispense with our analysis of pubic hair, pornography, or sit-coms then the fate of cultural theory looks to be a rocky path. These specific examples represent the types of micro-cultural artefacts that Eagleton identifies as forming the focal point of contemporary cultural studies, and they are therefore extended examples of the narratives of gender, class, and race that, according to Eagleton, cultural theory cannot continue to recount. If these bedrocks of cultural theory are removed will it be confronted with an identity crisis of sorts? Can cultural theory still be cultural theory if it deals in metaphysics? This is a question that will only be answerable once the implications of *After Theory* have been realised.

References

Adorno, TW. (2001) 'Culture Industry Reconsidered.' In: J.M. Bernstein (ed.) *The Culture Industry*. London: Routledge: pp. 98-106.

Lyotard, J.F. (1999) *The Postmodern Condition: A Report on Knowledge*. Manchester: Manchester University Press.

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J.T.S. Madeley's

'Religion and politics'

It must have been tremendously difficult for John Madeley to make a final decision regarding what essays to include in the anthology *Religion and Politics*. Even with a length of over seven hundred pages, the subject is obviously too ripe with complex nuance, interdisciplinary overlap, subject interconnection and methodological strategy to be covered in a single volume. Nevertheless, despite the inherent difficulty of providing a comprehensive volume in a subject this complex, Madeley manages to deliver a robust examination into the interconnected relationship between religion and politics.

There is no specific normative conclusion to the volume. The goal of *Religion and Politics* is to examine the interdisciplinary, methodological and comparative relevance of religion in the study of politics. The volume's strength comes from the fact that various approaches are presented without an overwhelming representation of any particular view. In this regard, Madeley has selected essays for the anthology that represent a balanced line between various world religions, political structures, methodological approaches, theoretical conclusions, and statements for the future direction of religious/political scholarship. By creating a balance between various subject areas, the anthology maintains a solid comparative nature and guarantees relevance for anyone interested in religion and politics.

The volume is divided into four sections. Each section contains seven essays from various political and religious scholars. Although the essays are loosely associated within their respective sub-headings, each essay explores different depths and methodological approaches to the subject. Considering the vast richness of the essays, a summary review of each section can only provide limited generalities.

The first section, 'Religion and Regime', presents a series of essays that analyse how various religions react and interact with different political structures and regimes. Many methodological subjects are explored throughout this section and they cannot be summarised without an expansion beyond this review. Nevertheless, the most interesting comparative feature here is the use of historical methodology. One common theme that seems to pervade the essays within this section, is an attempt to understand the historical context of certain religious movements and the theoretical relevance of historical analysis to modern day research.

Section two, 'The Politics of Church-State Relations', attempts the impossible task of providing a comprehensive discussion in comparative church-state political analysis. Needless to say, this section only scratches the surface of what would otherwise be a topic worthy of several volumes. That said, the essays manage to span considerable distance and provide interesting case studies in almost every global region. One particularly interesting essay is provided by Mark Juergensmeyer, where he explores the distinction between religious fundamentalism and 'religious nationalism' suggesting that political motivations are often eclipsed by a general overuse (and misuse) of the term fundamentalist. The essay suggests that religion-based political movements often seek a national order and therefore are as equally politically motivated as they are religiously based. This essay in particular has considerable theoretical implication in current liberal debates concerning how to approach a conception of fundamentalism in political discourse.

The third section, 'Religion and Electoral Politics', is more straightforward than other sections. The section provides exactly what the title suggests, an examination of the role of religion in electoral politics. Several great case studies are included within this collection of essays. For example, anyone interested in the electoral impact of the

Orthodox Church in Russia or the electoral significance of the Religious Right in the United States of America, will find the essays by Stephen Write and Steven Bruce considerably enlightening.

'Religion, Public Policy and the Politics of Identity' and the essays making up section four of this volume are perhaps the most relevant to current social science research. The section deals directly with the role of religion in current liberal debates regarding public policy. The section also provides essays dealing with the role of religion in cultural identity and in the formation of identity based politics. Given the current situation in former Yugoslavia and the religious nature of new public policy in post-war Iraq, not to mention the ongoing debates of religion in liberal public policy, this section provides considerable methodological and theoretical considerations for future research in these areas.

As a whole, the interdisciplinary implications of *Religion and Politics* are numerous, but largely not linked together. Since the anthology contains 28 different essays, comprised of a vast number of different case studies, subject particularities, methodological approaches and conclusions for future research focus; the interdisciplinary relevance will have to be garnered by the reader. For what *Religion and Politics* provides, is a collection of essays, which present the wide application and implications of religion in political studies. Therefore, individual authors within their respective essays provide the connections made between various disciplines and research methods. Any cross application of various themes and methods within the volume will have to be made by the reader.

If a critical concern exists with this anthology, the concern is not with what is in the volume, but what was left out (although with a book of 724 pages you have to make cuts somewhere). In my opinion, the volume could have been improved by providing a more detailed discussion of religious fundamentalism in relation to international politics. Of course, some of the essays do discuss the political implications of religious fundamentalism, while various case studies within the volume also consider the political effects of fundamentalist movements. However, considering the global fallout and

international focus on religious fundamentalism since September 11th, I was hoping for greater attention in this area.

Nevertheless, there is little, if anything, to complain about in *Religion and Politics*. Madeley has successfully composed and edited an anthology of impressive and politically relevant essays. The essays not only show the interconnectedness of religion and politics, but also illustrate why this area of study is so important to current political issues. The book should not only appeal to academics, researchers and students, but should also benefit anyone interested in the study of religion and or politics.

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A. Sen's
'Development as freedom'

Sen, Amartya (1999) *Development as Freedom.* New York: Alfred A. Knopf: 366 pp. ISBN 0 375 40169 0.

Amartya Kumar Sen was born in Bengal (then British India) in 1933 and grew up in Dhaka (now the capital of Bangladesh). Following his Indian college-level education, Sen undertook postgraduate studies at Cambridge University, and followed an international academic teaching and research career in the UK, the US and India. He is the only Asian recipient of the Nobel Peace Prize in Economics, which he received in 1998 for his collective contributions to the field of welfare economics. He is particularly recognized for empirical research on poverty, inequality, and the causes of famine and also for defining the field of development studies to include technical analysis. Most of his research focuses on South Asia and Africa.

Development as Freedom is Amartya Sen's first book after receiving the Nobel and the most widely read of all of his works. Based on the author's World Bank Fellow Lectures in 1996, this descriptive, non-technical overview of welfare economics argues that 'development' should be viewed not in terms of economic measures (e.g. GDP growth, average annual income) but in terms of the real 'freedoms' that people can enjoy such as economic facilities and social opportunities. Sen describes human freedom as both the primary end objective and the principle means of development; economic measures are merely the means to this end.

In March 2004, Sen published *Rationality and Freedom*, a 750 page highly technical treatise of complex mathematics and philosophy. In this first of two volumes, Sen explains the interdependent relationship between rationality and freedom.

Rationality is defined as the 'discipline of subjecting one's choices - of actions as well as of objectives, values and priorities - to reasoned scrutiny' (4) rather than self-interest maximizing decisions. Freedom entails that an individual can make rational choices based on personal preferences and valuations.

Development as Freedom is an informal text that brings together multidisciplinary insights from politics, economics, ethics, economics, demography, and sociology. Sen frames development as the realization of freedom and the abolishment of 'unfreedoms' such as poverty, famine, and lack of political rights. Arguments are strongly supported with vivid accounts of real living and working conditions for men and women in developing communities in Asia and Africa.

The book is best described as a model for examining issues of development in both economic and political terms, and not as a formula for implementing change. For example, Sen writes of the need to enhance human capabilities by eliminating such unfreedoms as child labour and famine, but does not provide a structured roadmap of preventive measures and the long-term changes required for implementation. As such, Sen's treatise should be judged for how it creates awareness of development issues. Readers will be challenged to think of development in political terms, but those probing the text for rigorous research or linkages to implementation will be disappointed. This review will briefly examine Sen's ideas around development, freedom and unfreedom, capability deprivation, women's development, population growth, and shared humanity.

Sen calls for a broadening of the term 'development' beyond the current narrow focus on economic measures such as per capita GDP and income levels. He argues that there is no direct link between a measure such as a country's GNP growth rate and the real freedoms that its citizens enjoy. For example, countries like South Africa and Brazil have a higher per capita GNP but lower life expectancy when compared to Sri Lanka and China.

Though recognizing the importance of economic benchmarks, Sen argues for the need for an expanded definition of development to include real human 'freedoms' such as political freedoms, economic facilities, social opportunities, transparency guarantees and protective security. This human freedom is both the primary end objective and the

principle means of development. In tandem, Sen stresses the need to abolish 'unfreedoms' such as poverty, famine, starvation, undernourishment, tyranny, poor economic opportunities, systematic social deprivation, neglect of public facilities, intolerance, and over-activity of repressive states.

Poverty is described as 'capability deprivation' that limits an individual's realm of achievable functionings and combinations. Economic poverty and capability poverty are separated but linked, as seen in South Asia and Sub-Saharan Africa where people suffer from both dire economic poverty in the form of below subsistence earnings and capability poverty in the form of high infant mortality rates.

Throughout the book, Sen highlights the key role of women in development. Sen cites research indicating that women have 'hardier; bodies and will survive better than men given symmetrical care. For example, the female/male ratio in the developed world is 1.05 as women tend to outlive men. Meanwhile, the developing world ratio is .94 in China and .93 in India. Sen believes that the world is missing as many as 100 million women due to female infanticide, neglect of female children, maternal mortality, and poor female healthcare. As there is a strong link between women's inequality and high mortality rates, development hinges on women's earning power, economic role outside the family, literacy, education and property rights. Women's economic participation leads to enhanced status of women as well as long-term regional political and social change. For example, the successful Grameen banking loan programme for rural Bangladeshi women has a 98% payback rate. This loan programme has greatly enabled regional economic empowerment, increased child survival and reduced fertility rates.

In addressing the issue of population, Sen disagrees with Malthus' 1798 essay describing how the number of men would soon exceed the amount of food they require. Sen believes that exponential population growth will be slowed by women's empowerment for reasons described earlier. He opposes China's 'one child family' rule, arguing that such societal coercion is an unfreedom in itself and can result in violence, female infanticide, and unsteady birth rates. Instead, Sen espouses voluntary programmes such as those in place in some parts of India.

Sen concludes his treatise with a discussion of the interconnectedness of humans and our responsibility to recognize this shared humanity. Human freedoms depend on personal, social and environmental situations. Poignant stories compel the reader to recognize the very human element of any discussion on development and the concept of a shared humanity.

Overall, *Development as Freedom* is an engaging interdisciplinary text for undergraduates and graduate students, business and public policy makers, and others concerned with economic and social development, politics, sociology, human rights, demography, and ethics. The book's descriptive nature and use of memorable vignettes and simple charts makes it a perfect introductory text for *GJSS* readers, regardless of their previous knowledge in the field. As this is Sen's first book after receiving the Nobel Prize, it provides a wonderfully non-technical (yet Nobel winner's) guide to development in both political and economic terms. Many readers may find that they begin to look at development differently, though they may be challenged to identify implementable recommendations. Those craving more robust research and theory development are best directed to Sen's original research or a more technical text.

Nevertheless, some readers may grow frustrated by the repetition of topics and cases from African and South East Asian locations. Sen's treatise also fails to acknowledge the connection between freedom and other key processes, such as the market mechanisms and the balance with the need for stability and security. His failure to link these issues may leave the reader with a perhaps too-optimistic picture of development.