

Memberships and Inequality

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1. Introduction

Broadly understood, social capital refers to various types of community relationships that affect individual outcomes. As such, social capital is involved in many of the areas of current social science research where the explicitly social determinants of behavior are fundamental.

Since Loury (1977) introduced it into modern social science research and Coleman's (1988) seminal study placed it at the forefront of social research, the term social capital has spread throughout the social sciences. Despite the immense amount of research on it, however, the definition of social capital has remained elusive. From a historical perspective, one could argue that social capital is less a natural kind in social science analysis than an umbrella term that encompasses distinct research interests and questions all of which are attempting to meld individual and social explanation in ways that transcend traditional interdisciplinary boundaries; Durlauf (2002a) makes some criticisms of Putnam's work for this reason. That said, there are important commonalities in different efforts to define social capital; Durlauf and Fafchamps argue that different conceptualization of social capital generally share three main features:

(1) social capital generates positive externalities for members of a group; (2) these externalities are achieved through shared trust, norms, and values and their consequent effects on expectations and behavior; (3) shared trust, norms, and values arise from informal forms of organizations based on social networks and associations. The study of social capital is that of network-based processes that generate beneficial outcomes through norms and trust. (2006: 1644)

A fundamental problem with this definition (Durlauf, 2002a; Durlauf and Fafchamps, 2006; Portes, 1998): is that it is functional: social capital is assumed to always produce socially desirable outcomes. Such an equation means that policies that promote social capital are always good. For the purposes of this chapter the notion that social capital is always benign will be challenged from the perspective of its effects on equality. Simply put, social capital may be inequality-enhancing, and so any policymaker whose objectives include egalitarian considerations will therefore be forced to reject any necessary equivalence between an increase in social capital and better social outcomes.

To see why this can be so, I draw on the discussion Durlauf and Fafchamps (2006, section 2.4). Suppose that clubs and networks are the mechanisms by which trust is sustained across individuals. These social structures by definition exclude those who are not members, and so create differential circumstances across individuals; Fafchamps (2002) and Taylor (2000) analyze how social groupings can harm nonmembers. So to assume that social capital necessarily has a positive effect suffers from a fallacy of composition (Durlauf and Fafchamps, 2006). In those cases where a group benefits from a higher level of (bonding) social capital, enabling members of the group to have preferential access to a rationed resource, superior information in making decisions, etc., this without doubt has a beneficial effect on the group's members but not necessarily on society as a whole.

Social capital effects are part of a larger class of group memberships effects. The aim of this chapter is to discuss some ideas on the linkages between group memberships and inequality, primarily from the perspective of economics. By groups, I refer abstractly to particular configurations ranging from ethnicity to gender to residential neighborhoods to schools, each of which helps define dimensions along which interactions among individuals are organized and how individuals perceive themselves. My goal in this discussion is to describe some of the implications of group memberships for understanding socioeconomic inequalities. My conclusions on the general relationship between group memberships and inequality in turn may be applied to the social capital context. In fact, one reason for focusing on memberships rather than social capital is that there are few interesting changes when one moves from the general to the specific case.

Some types of memberships effects have long been part of conventional economic analysis; examples include the role of the composition of communities in determining levels of taxes and public good expenditures and the nature and consequences of discrimination. In contrast, the attention given to sociological and psychological effects such as peer group and role model influences is a relatively new development. The various sociological and psychological group memberships effects have been given a number of names including social effects, neighborhood effects and social interactions; I will use these terms interchangeably. The substantive idea underlying these terms is commonplace for most social scientists, namely that there exist causal influences on

individual socioeconomic outcomes that derive from the influences of group memberships such as ethnicity and community.

One important contribution of this new work is that it illustrates how one can meld economic perspectives on behaviour with those of sociology and other social sciences and thereby provide richer and more nuanced explanations of various individual and aggregate phenomena. A primary motivation for the exploration of this class of behavioural determinants is its implication for the distribution of income and other socioeconomic outcomes. In Durlauf (1999,2001) I have argued that these influences suggest a ‘memberships theory of inequality’ as they constitute a fundamentally different perspective on the sources of inequality from individual or family-based perspectives that are generally studied in economics. The differences in causal mechanisms, have, as I will describe here, implications beyond the specification of theories themselves to normative and positive aspects of policy analysis.

To illustrate the distinction between the old and new perspectives on the determination of inequality, it is useful to consider the differences between models of intergenerational mobility as developed in the late 1970s and early 1980s versus mobility models of the last ten or so years.² Becker and Tomes (1979) and Loury (1981) provide what are regarded as classic analyses of the role of family income in perpetuating inequality across generations. The key idea in this work is that individual families face borrowing constraints when providing education to children, so that poor families invest less in their children, regardless of their abilities, and thereby hurt their adult economic prospects. In contrast, mobility models such as Bénabou (1996b) and Durlauf (1996a) emphasize the role of community factors in producing persistence in family economic status.³ In models of this type, families purchase access to communities whose educational quality is determined at a community level. This dependence arises both from the public provision of education, so that communities with different tax bases provide different amounts of education (via different per pupil expenditure levels),⁴ as well as through various sociological influences such as role model effects that help determine how these expenditures translate into learning ability. So, while family income matters in understanding children’s educational outcomes as in the family dynasty models of Becker

and Tomes and Loury, the causal mechanisms are factors that are defined at a group rather than a family level.

From this it should be obvious how the discussion of memberships effects is germane to thinking about social capital and inequality. The main causal mechanisms by which social capital has been argued to affect individuals, such as information transmission, or cooperative behaviour,⁵ are forms of social interactions. My general arguments about memberships and inequality therefore apply directly to social capital contexts. For example if one argues that a businessman's organization affects levels of trust, one is arguing that common group memberships (i.e. the organizations) have produced effects on how an individual acts towards others. If membership supports repeated business relationships, or if members of the organization share information about new jobs, then those businessmen who are outside the group will have fewer opportunities. A typical theory of group memberships is based on mapping the information and occupational status of members of a group to the outcomes of an individual. So while memberships theories may not explicitly use the terminology of social capital analysis, social capital models are species of it.

In Section 2, I review some of the theoretical issues that link memberships and inequality. In Section 3, I discuss normative and positive implications of memberships theories. Section 4 provides summary and conclusions.

2. Theories

The introduction of group influences in the determination of individual outcomes does *not* constitute a challenge to the behavioural foundations of economic theory.⁶ The introduction of group memberships effects into decision making is perfectly compatible with neoclassical decision theory, when this is interpreted at its most abstract, where individual behaviour is conceived as the purposeful outcome of the evaluation of alternative choices. Individuals make such choices on the basis of their preferences and beliefs, and the kind of constraints they face. Group memberships may powerfully affect

an individual's preferences, constraints and beliefs, but do not impinge on the logic that these constitute the building blocks of decision making.

Intragroup interactions

In the study of group memberships, most recent attention has focused on the internal influences generated by groups. There are a range of distinct mechanisms that have been posited to explain how groups affect individuals. One division of these various effects that is important for both theoretical and empirical work is the distinction between contextual effects and endogenous effects. This dichotomy was introduced into economics by Manski (1993) and is taken from sociology, cf. Blalock (1984).

In essence contextual effects refer to those influences that are directly measurable through the characteristics of the group members. In an educational setting, for instance, a contextual influence on outcomes may be the result of role models, such as those offered by the adults in one's community. Formal models of this type have been developed by Roemer and Wets (1995) and Streufert (2000) who demonstrate how, when communities are stratified by income, the differences in this joint distribution across communities can lead to substantial differences in educational investment decisions and thereby lead to persistent inequality.

Endogenous effects refer to those influences that occur because of the contemporaneous behavioural choices of other group members. Peer group effects are of this kind. One important feature of endogenous effects is that they may be reciprocal,⁷ something which is not true for contextual effects. From the perspective of inequality, endogenous effects add two important features to group memberships theories. First, endogenous effects can amplify the effects of changes in individual characteristics to outcomes when one considers a group in its entirety. The reason is that each member is affected both directly by a particular cause (e.g. a student receiving a subsidy) as well as by the social interactions induced by the changes in the behaviours of others (i.e. the changes in the behavior of the other students who have received the subsidy). In other words, there are feedback effects between members of a group, making the average effect on the group greater than the effect produced on average on a single individual.

When these feedback effects are strong enough, then it is even possible for a group to exhibit a second property: multiple equilibria. This means that there is more than one configuration of individual choices that is consistent with the decision-making structures of the individuals in the population. Intuitively, when individual decisions are sufficiently interdependent, this introduces an additional degree of freedom in the collective behaviour of a group as these interdependences require that individuals choose similarly, but do not determine what they actually choose. The presence of multiple equilibria in group outcomes has important implications for thinking about inequality, for it means that two groups with similar individual characteristics can exhibit very different behaviours. Hence inequality can emerge as a purely social phenomenon.

When groups are endogenously formed, memberships effects can play an important role in their composition. Work by Bénabou (1996a), Durlauf (1996a), and Hoff and Sen (2005) among others, has shown how social interactions can provide a theory of segregation. In turn, various types of segregation can create persistent intergroup inequalities because of social interactions. The interaction of social interactions with segregation can produce persistent inequality; this is the heart of what I mean by the ‘memberships theory of inequality.’

Intergroup interactions

The memberships literature has focused less on intergroup interactions, although issues of this type have been of longstanding interest in economics. That being said, many memberships models have implications for understanding intergroup inequality. At one level, this follows trivially from the intragroup effects. For example, social capital models that focus on the emergence of trust norms and reciprocal altruism among members of a community presumably imply differential treatment for outsiders and so implicitly suggest a source for inequality. Bowles and Gintis (2004) refer to this as parochialism; while their primary focus is not on how the formation of strong group ties can lead to inequality, it is a natural consequence of their analysis.

Other forms of intergroup interactions are not passive in the way parochialism effects are. By this, I mean that the effects of parochialism are not directed at outsiders

but rather a consequence of the formation of intragroup bonds. The most obvious example of active intergroup interactions is discrimination. While my discussion will focus on race, it applies equally well to other contexts such as gender.

The classic type of discrimination in economic reasoning is taste discrimination, the basic ideas of which are delineated in the classic study by Becker (1971). It is also the form of discrimination that I believe is meant in most public discussions. Taste discrimination refers to situations such as one where employers simply dislike employing blacks and are in principle willing to give up profits in exchange for hiring less able white workers in their place.⁸

Stated this baldly, this form of discrimination runs into an immediate problem as an argument for contemporary significance, namely, its economic sustainability. The standard argument that this type of discrimination cannot be sustained is that firms that practice taste discrimination will, via market competition, lose out to firms that do not. However, one can resurrect a role for taste discrimination if firms who do not discriminate are subject to social sanctions which then lead to profit reduction; Akerlof (1980) develops a formal model of this type and specifically uses discrimination as a motivation for his theoretical analysis. This type of argument, which is common in the evolutionary literature on group selection, presupposes that the costs to enforcing social norms are low, i.e. that it is easy to switch to a firm that discriminates from one that does not. This sort of problem arises in general when one considers the enforcement of social norms, see Kandori (1992) for an example of formal game theoretic analysis of how norm enforcement can arise and Sober and Wilson (1998) for a general discussion of the evolutionary sustainability of behaviours that are individually harmful (as in failing to hire the most qualified) via social enforcement. The point I wish to make here is that taste discrimination may not be eliminated by market forces when social norms are strong.

Nevertheless, I believe that most economists would agree with Loury (2002) that taste discrimination is not in the first rank of factors that explain contemporaneous socioeconomic differences between races. One example of why this is so is an important paper by Neal and Johnson (1996) who show that approximately 75% of black/white wage differences in standard wage models can be explained by differences in premarket factors as captured by test scores around age 16. This finding means that educational

inequality rather than labour market discrimination is the primary source of contemporary racial inequality in wages. Another reason is that it is very difficult to identify taste based discrimination. To see why this is so, consider the recent prominent study by Bertrand and Mullainathan (2004) who found that résumés with ‘black sounding’ names fared more poorly than résumés with ‘white sounding’ names when mailed to random firms. Inferences of discrimination in studies even this well designed may be subjected to powerful criticism using arguments in Heckman (1998) and Heckman and Siegelman (1993). Generally, such findings cannot be interpreted as discrimination, at least if discrimination is understood as behaviour that is inconsistent with profit maximization. If firms have found that black employees tend to have lower productivity than white ones, when other résumé characteristics are held constant, then one has an example of statistical discrimination, which I discuss below, rather than taste discrimination; this would reconcile the Bertrand and Mullainathan findings with Fryer and Levitt (2004) who did not find any relationship between black names and labour market outcomes, given a set of control variables including family background.

Economists, particularly when theorizing, currently tend to focus on two other types of discrimination. One is known as statistical discrimination and was originally proposed by Arrow (1973) and Phelps (1972). In statistical discrimination models, firms start with prior beliefs that one group, i.e. blacks, are less productive than whites. As a result, they are less willing to hire blacks. Blacks, in turn, find the value of educational investment to be lower than whites, and so invest less. Hence a self-consistent equilibrium emerges in which the prior beliefs of firms are proven to be ex post correct which prevents firms from learning and thereby overcoming their prior beliefs.

The empirical import of statistical discrimination models is unclear, at least for labour markets. Moro (2003) shows that a pure statistical discrimination model can capture some features of the black and white wage distributions, but does not allow for competing sources of wage differences. Further, as documented by Neal (2005), the marginal wage benefit from increases in education has been consistently higher for blacks compared to whites since 1960, which means that the key microeconomic foundation for statistical discrimination models does not hold. Further, Neal’s findings suggest that there may be differential costs to skill acquisition that are not picked up using conventional

measures. If one allows for differential costs, then it is not clear that statistical discrimination forces are even identified using conventional statistical methods such as wage regressions.

While the empirical salience of statistical discrimination models in labour markets is unclear, there are many contexts where statistical profiling matters. By statistical profiling, I follow Harcourt (2007) and refer to the use of group memberships to draw inferences about individuals. Unlike statistical discrimination, the focus in statistical profiling moves from the question of how prior beliefs can become self-confirming to the question of how group memberships affect inferences about individuals. One example of statistical profiling is racial profiling in traffic stops. The oversampling of black drivers by police who stop drivers in order to search for drugs has received a great deal of attention in the media. What has received far less attention is that, as an empirical matter, the pattern of these stop differentials is consistent with a policing strategy that attempts to maximize the total number of arrests (see Knowles, Persico, and Todd (2001) for the major work of this type and Anwar and Fang (2006) for extension and corroboration in a different context.)⁹ These types of studies raise the important point that group identities provide information about individuals. There may be fairness issues involved in the use of such information (see Durlauf (2006) for discussion), suggesting another sort of equity/efficiency tradeoff associated with group memberships effects.

Statistical discrimination ideas have led to one of most interesting recent developments in the study of social influences: stigma, which may be thought of as a third form of discrimination. Loury (2002) provides a profound discussion of this concept and is the origin of its recent use to understand racial inequality. I interpret stigma as the ascription of negative attributes to individuals on the basis of group identity. An example of this is a belief in genetic differences in ability between ethnic groups. Stigma generalizes the notion of statistical discrimination in that it moves from cases where adverse beliefs prove to be correct in an equilibrium to cases where adverse beliefs are simply not disproven. Loury's vision of stigma as a source of inequality is summarized by his statement

Discrimination is about how people are treated; stigma is about who, at the deepest cognitive level, they are understood to be...A diagnosis of discrimination yields a search for harmful or malicious actions...But seeing stigma as the disease inclines one to look for insidious habits of thought, selected patterns of social intercourse and defective public deliberations... (2002: 167-168)

Stigma may also matter in terms of its direct psychological effects on African Americans. Research by Steele (1992, 1997) and Steele and Aronson (1995) has shown that the performance of African Americans on tests is substantially worse when the tests are identified to the test takers as measuring ability: this phenomenon is known as stereotype threat. The argument made in this work is that the existence of negative stereotypes about black intelligence places psychological pressures on blacks in testing environments. While there has yet to be any translation of the findings on stereotype threat into understanding differences in socioeconomic outcomes, it is easy to see they may be very important if they represent psychological costs to educational effort that are experienced by blacks but not by others.

Synthesizing intragroup and intergroup externalities

Intragroup and intergroup influences are of course likely to be simultaneously present. While this combination has not generally been incorporated into formal analyses, I believe this is an important next research direction. A case where a synthesis seems important is understanding differences in education attainment between blacks and whites. A number of authors have argued that educational outcomes between black and white students are partially attributable to the lower value put on education by black students. In popular discourse, educational achievement is regarded as ‘acting white’ by some African Americans and thus creates incentives against academic success. In scholarly work, this type of phenomenon has been specifically invoked to explain why there are persistent educational outcome differences between ethnic groups in places such as Shaker Heights Ohio, where there are good reasons to believe the school system and community have been sensitive to background differences between races and taken a range of efforts to improve black educational performance; see Ogbu (2003) for a wideranging analysis.¹⁰ Recent work by Fryer and Torelli (2005) provides specific

evidence that academic achievement among African American students is negatively associated with personal popularity.

One explanation of the presence of dysfunctional educational attitudes in a community is that they are simply a manifestation of intragroup social interactions. If the perceived benefit to educational effort is sufficiently sensitive to the effort levels of one's peers, then large differences in efforts will emerge in equilibrium, even after one controls for family specific factors. Why should these feedback effects be strong? An important new literature which provides a deep set of microeconomic foundations for such an abstract explanation and which seems important in contexts such as education concerns the role of identity in decision making. The study of identity in economics was initiated by Akerlof and Kranton (2000); Austen-Smith and Fryer (2005) and Fang and Loury (2004) make important theoretical advances in these ideas and apply them to race. In this work, individuals are conceptualized as choosing (not necessarily consciously) ways of acting and understanding in order to situate themselves psychologically and socially. So, following Fang and Loury, agents choose identities to facilitate how they process past experiences and in Austen-Smith and Fryer they do so to achieve group acceptance.

One lacuna in this work is the absence of a theory as to why certain identities become salient. My conjecture is that they constitute a response to stigma. If the academic abilities and achievements of blacks are denigrated by the majority population, an identity that undervalues education implicitly rejects the significance of the stigma. I would argue that it is no coincidence that a minority group whose intellectual abilities are held in contempt by many in the majority group would develop identities that undervalue such abilities. If so, this would illustrate how intergroup interactions, can produce, via intragroup interactions, strong and persistent forms of inequality.

Taken as a whole, intra- and intergroup influences can have profound effects on inequality, at least in theory. I now consider the question of policy implications. I will focus on intragroup interactions since the evidence here is far more clear and because the policy remedies for intergroup interactions either are straightforward (enforcement of antidiscrimination laws) or are associated with micro-level interventions beyond my expertise (teaching styles) or are qualitatively similar to those that apply to intragroup effects (i.e., involve associational redistribution, which I define below).

3. Public Policy

From the perspective of public policy analysis, memberships theories have normative as well as positive implications.

Normative issues

One can identify two distinct ways in which memberships theories matter for justifying redistributive government policies. First, memberships theories have implications for strictly egalitarian perspectives on policy. By strictly egalitarian perspectives, I refer to those that directly regard inequality as intrinsically bad, as opposed to perspectives which contain instrumental objections to inequality, i.e. oppose inequality because of its consequences.¹¹ Intuitively, it is natural to regard the effects of some memberships as unjust. One does not hold a child responsible for the neighborhood in which he grows up. Hence the effects of a low tax base on school quality, the absence of certain types of role models, and the presence of peers whose behaviours are not conducive to school effort represent factors which are unfair to children in disadvantaged communities and would intuitively call for redistributive policies.

This intuition can be made rigorous. Within political philosophy, one of the key developments in the study of egalitarianism concerns the role of responsibility in evaluating inequality; a wide range of scholars subscribe to what E. S. Anderson (1999) has referred to as luck egalitarianism, the idea that it is those inequalities which occur due to luck rather than to individual choice that are appropriately reversed by government action. Luck in this description is a shorthand for those factors for which a person should not be held responsible¹²; Roemer's (1993) development of a positive theory of redistribution when a policymaker seeks to move society towards greater equality of opportunity is based on this distinction. Childhood residential neighborhood characteristics are a clear example of the sorts of variables for which a person is not responsible.

A second justification may be derived from what is often called prioritarianism. Prioritarianism essentially represents a defense of the view that society should attempt to improve the status of the badly off, i.e. those who are badly off should receive priority in the allocation of resources. Group memberships can be the mechanism that produces serious personal disadvantage. Notice that one can introduce considerations of responsibility into prioritarian arguments, see Arneson (1999). The basic idea is that the claim of the disadvantaged for an improved situation may depend on the extent to which they are not responsible for their current situation. Thus, to the extent that memberships effects produce significant hardship, prioritarian considerations would argue that these consequences be reversed by policy.

Group influence thus can, for either egalitarian or prioritarian reasons, produce a justification for redistributive policies. The notion that ghettos contain dysfunctional behaviours that are socially reinforced does not represent an effort to ‘blame the poor,’ but rather represents a recognition of the social harms that disadvantaged communities impose on their members. Anderson’s (1999) well known ethnographic work on the ‘code of the street,’ which describes how violent behaviours are reinforced as a defense mechanism, is a good example of these social costs.

From the perspective of normative arguments on redistribution, is there a principled difference between the effects of an ethnic group or residential neighborhood versus the effects of parents on a child’s future socioeconomic prospects? My argument thus far is that group influences can represent the sort of inequality-inducing factor that is beyond an individual’s responsibility. This does not imply that social factors are more compelling in redistributing resources than individual-specific ones. What I mean is that while one may care in terms of choice of policy which class of factors matters, it may be irrelevant to the abstract question of whether some policy intervention is justified. If a defense of redistribution relies solely on its reversal of those effects for which one is not responsible, then it is hard to see that the two types of factors differ in their implications. Rawls suggests such an equivalence when, in a famous passage, he writes about

...the principle that undeserved inequalities call for redress; and since inequalities of birth and endowment are undeserved, these inequalities are somehow to be compensated for. Thus the principle holds that in order to treat all persons equally, to

provide genuine equality of opportunity, society must give more attention to those with fewer native assets and to those born into the less favorable social positions... (1971: 100)

So, one might conclude that memberships effects do not alter the normative basis for redistributive policies in a way different from family-based effects.

However, this equation of social and family influences as equally morally compelling reasons for redistribution is less obvious once one moves beyond egalitarian and prioritarian considerations to more general considerations of what constitutes a good society.¹³ In other words, while each embodies important social objectives, neither egalitarianism and/or prioritarianism captures all aspects of what I regard as a good society. In particular, these abstract conceptualizations of the requirements of justice ignore the intrinsic value of human relationships. And it is here that I see important qualitative differences between some family-specific and group-specific sources of inequality. Appiah comments

...we should start with the assumption that the role parents play in the raising of their children gives them rights, in respect to the shaping of their children's identities, that as a necessary corollary of parental obligations...we believe that children should be raised primarily in families and that those families should be able to try to induct their children into the mores, identities, and traditions that the adult members take as their own. (2005: 201)

Parental and group influences differ in terms of intent. One does not think of the efforts of a parent to help their children in school in the same way that labour market connections in a community assist the young. To the extent that a parent is attempting to improve his child's prospects, then strict egalitarianism requires violating this form of self-actualization, i.e. requires that these efforts be fully negated by society.

Appiah's argument, in my view, is suggestive of limits to equalization. In other words, while one could imagine justification for tax breaks to poor families who made education related purchases, one does not naturally expect equalization of these expenditures across all families. There is an intrinsic value worth preserving when a parent chooses to spend extra money (or extra time) on his children, even though that entails future inequality. In contrast, it is difficult to see a comparable argument for

differences in per pupil school expenditures. The desire of a parent to see his child get ahead is not unworthy and sacrifices that try to achieve this are a natural part of parenting. A policy that completely reverses such investments detracts from the nature of the parent child relationship as it rules out a domain for giving by the parent and would thereby impinge on the nature of the parent child relationship.¹⁴ An argument of this type is made by Schoeman:

Even if someone could demonstrate that there were some more efficient and effective institution for governing the interests of children than the traditional family, I would still think that the family would have a strong, though, rebuttable, moral presumption in its favor...The presumption would seem to imply that the state should not, to the extent possible, make the family and parental responsibility otiose through the provision directly to children of services which parents are in a position to supply. (1980:18-19)

The divisions between purposeful family-based inequalities and impersonal group inequalities may not always be clear. But that is relevant only with respect to the concrete determination of the domain for redistribution, not for the abstract claim that they differ. I therefore conclude that there are good reasons to believe that memberships-based sources of inequality are more compelling than some family-based sources in terms of their implications for justifying government interventions to promote equality.

While memberships have powerful normative implications for the justification underlying redistributive policies, the positive implications are far less clear, with respect to the question of policy design. On the one hand, memberships theories of individual outcomes can be used to motivate what I have referred to in Durlauf (1996b) as associational redistribution. Associational redistribution refers to the idea that the government can in principle take steps to alter the composition of various public associations in order to redistribute the effects of group memberships. There is nothing new about associational redistribution as a government policy. Affirmative action may be thought of as a set of policies that alter memberships in the student bodies of schools or the workplaces of firms. School busing to achieve racial integration was an earlier policy of this type. And of course, other policies have associational effects. The allocation rules for government scholarships help determine the allocation of students across colleges;

similarly the decisions concerning the public finance of education prior to college as well as decisions about the size and locations of schools determine the compositions of student bodies, to name two additional examples.

Policy design

While the desirability of associational redistribution is in principle suggested by memberships theories, difficulties emerge when one attempts to translate this abstract claim into specific policy recommendations. This is not always the case; certainly the passage and enforcement of anti-discrimination laws was a straightforward (intellectually) response to the sorts of memberships-effects that produced black disadvantage in the Jim Crow South. My argument is that those memberships effects which are most likely salient as sources of contemporaneous disadvantage are qualitatively different from taste discrimination and much harder to counter.¹⁵

The reasons why this translation is problematic are severalfold. First, empirical work on memberships effects is mixed. This means that it is far from clear which sorts of memberships matter for various socioeconomic outcomes. To be clear, there are many studies in social science that provide evidence of memberships effects. For phenomena that are directly related to inequality, findings range from crime (Glaeser, Sacerdote, and Scheinkman (1996), Sirakaya (2006)), to labour market outcomes (Conley and Topa (2002), Corcoran, Gordon, Laren, and Solon (1992), Topa (2001), Weinberg, Reagan, and Yankow, (2004)) to fertility (Crane (1991)), to education (Aaronson (1998), Borjas (1995)) to the use of public welfare (Aizer and Currie (2004), Bertrand, Luttmer, and Mullainathan (2000)).¹⁶ However, the existence of this large body of work does not mean that the evidence of memberships effects is policy-relevant.

As guides to policy, the empirical failings of memberships studies, from the perspective of policy design occur at several levels. First, there is the problem of identification of memberships effects. The basic problems in identifying memberships effects occur at several levels. Whenever group memberships are endogenous, as occurs in cases such as residential neighborhoods, identification of the group's role must be disentangled from unobserved individual characteristics within the group; formal analysis

of this is found in Brock and Durlauf (2001b,2007). To make this concrete, suppose that one observes a correlation between the types of role models present in a community and student achievement. Does this imply a causal relationship? The answer is no. The obvious problem in moving from the correlation to a causal explanation is that the distribution of role models in a community says something about the sorts of parents that are present. If the absence of high achievement role models is associated with relatively unambitious parents, and this lack of ambition cannot be observed, then it is possible that the correlation between role models and outcomes is spurious. In principle, self-selection can be sufficiently strong to produce evidence of memberships effects when none is there. Some evidence of this is developed in Evans, Oates and Schwab (1992) who showed that instrumental variables estimates, designed to account for the endogeneity of memberships, reversed evidence of a feedback from school characteristics to educational outcomes. To be clear, there is no logical reason why instrumental variables estimates produce much weaker evidence of neighborhood effects, although intuition suggests this; Rivkin (2001) in fact showed how the opposite could be true in a context quite similar to Evans, Oates, and Schwab.

Even when memberships are exogenous, there may be identification problems due to unobserved group characteristics. Returning to the correlation of role models and student outcomes, the determination of whether this relationship is causal presupposes that the analysis has accounted for other factors that systematically affect student performance outside of role models. When these factors are not fully accounted for, then the correlation may be due to their omission. To see how this can happen, suppose that school quality affects student performance. School quality is not observable, at best crude proxies such as per pupil expenditures are available. So long as the part of school quality that is not explained by the proxies systematically varies across communities, i.e. is correlated with the characteristics of role models, then spurious correlations can result. To pursue this example, if better teachers are attracted to schools and able to sort themselves into schools in affluent communities, then the spurious correlation I have described could occur.

Do self-selection and unobserved group characteristics mean that many empirical claims of memberships effects are incorrect? The answer is no. The salience of these

confounding factors is itself not known, so the problem is one of identification. Further, there are a number of studies that attempt to control for these problems. I survey these in Durlauf (2004) and conclude that there is some evidence of neighborhood effects, although the evidence is not strong enough to significantly move one's prior beliefs on the question of their importance.

Other identification problems exist once one considers the question of what sorts of social interactions matter. Manski (1993) was the first to recognize the difficulties that exist in disentangling contextual effects from endogenous effects. Suppose that one wants to measure the relative contributions of role models and peer effects in school performance. Suppose that role models matter. This implies that, even if peers do not matter, there will be a correlation between the behaviour of peers and each individual, because the students in a community are simultaneously exposed to the same peers who are all affected by the same role models. While Brock and Durlauf (2001a,b,2007) show that as a formal question of identification, one can generally disentangle contextual and endogenous effects so long as their relationship to individual outcomes is nonlinear, this does not mean that in practice, with limited data sets, that distinguishing these effects is easy. In fact, very few empirical papers have attempted to do so. This distinction between the two effects can be quite important in policy evaluations. Specifically, if one wishes to predict the effect of a change in group compositions on the outcomes of individuals, for a wide range of models, this prediction depends on the distinct magnitudes of the effects.¹⁷

When one steps back from the statistical problems that are present in studies of memberships effects, one can identify other problems that militate against using these studies as guides to policies. One major problem, discussed in Manski (2000) is the absence of any work identifying which groups matter for memberships effects. The empirical literature almost exclusively takes the composition of groups as known. This is a first order problem for policy analysis. If the groups that have been assumed to generate social interactions are, in fact, not the ones that actually do generate the effects, then one cannot predict the effects of changes in group composition. The reason the initial groups appeared to matter was the correlation of their compositions with the 'true' groups, but this correlation is not invariant to the choice of group.

Another major class of problems concerns the details of statistical models that permit the measurement of memberships effects. The memberships theories I have described, while mathematical, are sufficiently abstract that they leave many aspects of statistical evaluation unexplained. The theories do not specify the appropriate empirical proxies for contextual and endogenous effects. Durlauf (2004) catalogues a vast range of variables which have appeared in empirical microeconomic studies which examine memberships effects in inequality-related contexts. Similarly, different studies employ different dependent variables. Thus, one study such as Crane (1991) examines the percentage of professional and managerial workers on high school drop out rates in a community whereas another such as Corcoran, Gordon, Laren, and Solon (1992) studies the predictive effects of the percentage of families on public assistance on future wages. In fact, when one begins to think about individual decisions as made jointly over many behaviours at different points in time, it is not even clear if the various findings on the presence or absence of memberships effects are even logically consistent.

Similar problems exist when one considers the interaction of memberships theories with other candidate determinants of inequality. The various inequality theories are, using a term from Brock and Durlauf (2001c), *openended*, which means that a given theory, say the effects of role models on behaviour, has no implications for the validity of other theories, such as the role of parental education, sibling behaviour, etc. It is easy to see why the exclusion of relevant alternatives will render statistical analysis problematic. Ginther, Haveman, and Wolfe (2000) find that richer specifications of individual behavioural determinants can eliminate initially strong evidence of group effects.

Taken as a whole, the empirical literature on memberships effects, while providing reasonable support for the scholarly proposition that these perspectives are empirically valid, does not provide sufficiently precise guidance of the nature of the effects or the behaviours they influence to allow one to address questions of either optimal policy design or of policy comparison. Thus, the standard approach of policy analysis in economics, which relies on the specification of a behavioural environment and an objective function for a policymaker, cannot yet be done for policies that promote associational redistribution.

An alternative way to think about memberships and policy is to identify ‘simple’ forms of associational redistribution and ask about their consequences for inequality. For example, a school district can adopt an assignment rule that randomly assigns all children in a school district to schools and thereby eliminates racial and economic segregation. Or, the relevant housing authority could follow the rule that the location of public housing be distributed across a metropolitan area in a way that is proportional to the distribution of the population. Neither policy is optimal in the sense of solving a well-posed planning problem, but each attempts to promote equality via associational redistribution.

The most suggestive evidence on policy effects is due to the Moving to Opportunity (MTO) demonstration that has been conducted by the Department of Housing and Urban Development.¹⁸ The MTO program is an ongoing experiment that is being conducted in Baltimore, Chicago, Boston, Los Angeles, and New York City. In each case, families in poor neighborhoods were invited to participate in the program. Those families that chose to participate were randomly assigned to one of three groups: a control group, which received no additional benefits, a second group which received housing subsidies whose use was not restricted to certain neighborhoods, and a third ‘treatment’ group that received housing subsidies that had to be exercised by moving to low-poverty neighborhoods. Families in the treatment were obliged to remain in the low-poverty neighborhoods for one year, after which they could treat the voucher the same way as the unrestricted vouchers. Assistance in locating low income community housing was provided as well.¹⁹

Katz, Kling and Liebman integrate data across the five sites and conclude:

We find no significant overall effects in this intervention on adult economic self-sufficiency or physical health. Mental health benefits of the voucher offers for adults and for female youth were substantial. Beneficial effects for female youth on education, risky behaviour, and physical health were offset by adverse effects for male youth. (2007, abstract)

This type of evidence has the critical advantage that it allows one to map a policy rule (the housing vouchers) directly into effects on a population. However there are still two limitations in using this evidence to design large scale anti-poverty policies. First, and well recognized (eg. Goering, Feins, and Richardson (2003, pg. 31)), there is a self-

selection problem involved in the initial decision to participate in the program. This means that one cannot extrapolate the benefits of program participation to the poor population as a whole. Second there is the problem of generalizability. The small scale of the demonstration means that there are no general equilibrium effects.²⁰ In other words, the movements of families induced by the program did not affect the compositions of the communities in which they moved or those that they left. In contrast, any large scale project of this type would necessarily produce these effects. In other words, as discussed in Brock and Durlauf (2001b) large scale movements of the poor into neighborhoods producing certain types of social influences would necessarily change the composition of these neighborhoods to a sufficient extent that the effects could be very different from what happens when a small number of families are moved. This is the flip side to the social multipliers described above.

These various problems in mapping evidence of memberships effects into concrete policy proposals should not lead to nihilism about the use of policy to influence group memberships. Rather, I believe they suggest the importance of incremental steps with a recognition that much is still to be learned. One way to think about the problems I have described is in terms of model uncertainty. In many policy contexts, one makes an argument about how the phenomenon of interest is determined, i.e. employs a model, and constructs a policy based on that model. In the case of group memberships, the problems of identification, group membership choice and general equilibrium versus partial equilibrium effects all constitute forms of model uncertainty. What current empirical research provides is a set of claims that represent statements about memberships effects given a data set and a set of assumptions as to what defines an appropriate model (or narrow space of models). What is needed is the development of empirical evidence that avoids conditioning on ad hoc model assumptions. This need, recognized as early as Leamer's (1978) seminal work is an active topic of current research in macroeconomics (see Brock, Durlauf, and West (2003) for both one approach to dealing with model uncertainty as well as a survey of recent work) and should be applied to memberships contexts, if the ideas of the theory are to become policy-relevant.

4. Conclusions

This chapter has attempted to develop three classes of arguments about the relationship between group memberships and inequality. First, from the perspective of current developments in economic theory, group memberships can have profound effects on inequality. These effects extend beyond the cross-section distributions of socioeconomic advantage to issues of intergenerational mobility and the emergence of socioeconomic segregation. Second, there are important normative implications of this new perspective. In particular, various conceptions of justice suggest that these influences should be countered, if possible and without unacceptable costs, by government policies. The justification for allowing group based inequalities to be perpetuated further seems weaker than for at least some family-based inequalities. Third, the translation of the abstract justification for government interventions to reverse the effects of group memberships is still problematic. Too little is known about the nature of memberships effects to allow for firm claims about the effects of various interventions to affect group memberships or other policies.

These general considerations find specific resonance in social capital contexts. Putnam (2000), building upon an idea in Gittel and Vidal (1998), draws a distinction between bonding social capital, which strengthens relationships among homogeneous and bridging social capital, which strengthens relationships among heterogeneous individuals. Social organizations such as professional clubs or bowling leagues, that have been extolled as the repositories of social capital while creating trust and friendship among their members, can just as easily become mechanisms by which labor market information is differentially made available. The same solidarity that emerges from a common church that leads members of a community to look after one another's children can lead to hostility to nonbelievers and religious minorities. It is easy to see parallels in the bridging and bonding distinction and types of social capital that are equality enhancing versus inequality enhancing. And for both types of social capital, one can readily understand how, whenever some group of a population is socially interconnected, this induces a distinction between "us" and "them" that raises issues of potential stigma and the like.

These types of egalitarian considerations illustrate how the common assumption that social capital is a social “good” may be deeply misleading. Further, the various econometric identification issues that I have discussed for memberships theories apply *a fortiori* to social capital studies. This is so because social capital is one of a range of possible social influences, and so not only must a statistical analysis identify whether any social (i.e. membership) influences matter, but whether the particular mechanism is one that captures what is meant by social capital; Durlauf (2002b) provides formal discussion. The lack of consensus on the definition of social capital adds another layer of model uncertainty when one attempts to evaluate empirical evidence. All these factors suggest that we are far from being able to make sensible policy recommendations based on social capital considerations.

Yet, the complexities in understanding memberships and inequality should neither demoralize the researcher nor lead to nihilism for public policy evaluation. Memberships theories still constitute a nascent research program and so progress is sure to continue. With respect to policy analysis, work such as Durlauf (2006) and Gaus (2006) suggests constructive ways to think about policy choice in the presence of deep uncertainties on the part of the policymaker; these may have use in the social capital context. What should be taken from this chapter is that social capital and other memberships-related phenomena, because of their implications for inequality, should lead to caution on the part of policymakers who wish to manipulate them.

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Notes

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² Economic theories of intergenerational mobility are surveyed in Piketty (2000).

³ The substantive ideas on the sociological side of this work are anticipated in Loury (1977).

⁴ The magnitude of differences in public per pupil expenditures across the United States is documented in Hussar and Sonnenberg (2001).

⁵ See Durlauf and Fafchamps (2006) for a survey of the causal mechanisms that have been proposed in the social capital literature. For my purposes, it is not important which of these mechanisms is regarded as the best conceptualization of social capital as they are, as I have argued, simply examples of memberships, and specifically, social interaction effects.

⁶ As discussed in Manski (2000), one reason why memberships-based perspectives have only recently emerged is because of their reliance on newer methodological advances that allowed their instantiation into formal economic models. My own sense is that the popularity of these models for inequality analyses also stems from the perception that individual-based models of inequality missed important aspects of the persistence of inequality.

⁷ There is no requirement that reciprocal influences be equivalent.

⁸ To be clear, this does not mean that taste discrimination is not regarded as a first-order source of historic black-white inequality. Heckman and Donohue (1991) provide a very careful discussion of the evolution of black-white wage inequality over the past 60 years and conclude that while one cannot prove that antidiscrimination laws explained the

narrowing of the wage gap after 1964, the failure of alternative explanations to do so strongly implies an important role.

⁹ The objective of maximizing arrests may not be a sensible one; as argued in Harcourt (2004) and Durlauf (2006), a stop strategy should attempt to minimize the crime rate, which depends not on differentials in crime rates across groups but on the sensitivity of these crime rates to changes in the probability of being caught if a crime is committed. But this is a criticism of current profiling practice and does not address the question of whether race-based stop strategies lead to lower crime rates than race-blind ones. General objections to racial profiling are discussed in Durlauf (2006); see Risse and Zeckhauser (2004) for somewhat different views.

¹⁰ Claims of this type have been subject to strong criticism, much of it, in my opinion, politically motivated by the belief that this is a form of blaming blacks for current inequalities. My reading of this debate suggests that the evidence developed by Ogbu (2003) is persuasive. I will argue below that the ‘blaming the poor’ concern is unwarranted.

¹¹ I will not address criticisms of egalitarianism that argue that to treat inequality as a direct bad leads to violations of the Pareto principle, i.e. that one must be willing, for some circumstances, to accept a reduction of the welfare of each member of society in exchange for a decrease in inequality; see Kaplow and Shavell (2001) for a clean version of this argument. Some reasons why I reject this and other criticisms of non-consequentialist views of justice may be found in Durlauf (2007) which draws on a powerful critique of utilitarianism by Hahn (1982).

¹² Thus a gambler who risks all of his wealth on a bet and loses is responsible for his loss even though it was directly caused by luck. In contrast, an individual who loses all of his wealth due to a natural disaster for which all reasonable precautions have been taken, e.g. a lightning strike, is the victim of what is sometimes called ‘brute’ luck and is not responsible in the same way.

¹³ While concerns over the role of emotional relationships in describing ethical conduct have existed throughout the history of philosophy, (see Aristotle, *Nicomachean Ethics*, Book VIII, section ix for a classic example and Velleman (1999) for a recent one),

relatively little attention seems to have been paid in the context of analyses of ethics and equality.

¹⁴ Issues of financial investment are more straightforward to discuss than other types of influences. Consider parental investments of time, e.g. reading to children rather than watching television or the teaching of certain values concerning effort, integrity in dealings, and the like. The case that such activities are an intrinsic good is especially strong. But efforts to reverse the effects of these activities, even if feasible, seem less likely to detract from the activities themselves than would occur for the expenditure case. Put differently, the disincentives for private investments of time may differ from the disincentives for private investments of money in response to a given redistributive policy. Further, if the issue is that some communities inculcate attitudes and behaviours that are less conducive to labor market success than others, there arises an issue of respect for other cultures that may require consideration. I believe the family/group distinction still applies here, but acknowledge it may require more elaborate argumentation.

¹⁵ This in no way disparages the great difficulties faced by the civil rights movement to end legal discrimination nor does it mean the process by which the removal of public and private forms of segregation led to increases in socioeconomic equality was simple or straightforward. My point is simply that at least part of the abstract question of what policies to implement was easy to identify.

¹⁶ Other studies provide stronger evidence of social interactions, but examine phenomena that are sufficiently far away from those where policymakers are concerned that it is unclear that they do more than help reinforce the general view that memberships matter. For example, the famous Robbers Cave experiment (Sherif, Harvey, White, Hood, and Sherif (1961)) in which teenage boys were taken to a summer camp in Robbers Cave National Park, organized into arbitrary groups from which various forms of stereotyping and group hostility emerged, make evident how group identities can affect behaviours, and so is of enormous importance in the assessment of the empirical significance of memberships theories. It is far from clear that these types of findings can say anything more if the goal is to evaluate specific policies, e.g. classroom tracking in schools, in which the interactions are delimited in time and individual actions are directed by a

teacher. Aronson (1999) provides an enjoyable survey of social psychology findings that at a qualitative level empirically buttress the ideas underlying memberships theories.

¹⁷ There are cases where the predicted effects of compositional changes do not require separate identification of the different types of memberships effects, but for our purposes what matters is that these cases are not general as they typically require the various effects to operate additively. See Durlauf (2004) for discussion.

¹⁸ An earlier program of this type occurred in Chicago and is known as the Gautreaux program. In response to a lawsuit against the Chicago Housing Authority, a number of families in public housing projects in Chicago were moved to other locations in the city while other families were moved to various suburbs outside city lines. Members of those families who moved to suburbs exhibited better educational and labor market outcomes than those who remained. See Rosenbaum (1995) and Rosenbaum and Popkin (1991) for a description of findings concerning families in the Gautreaux Program and Durlauf (2004) for a summary of the reasons why the evidence from the Program is not ideal in terms of evaluating memberships effects. These limitations do not detract from the scholarly importance of the research on Gautreaux; part of the motivation of the MTO program was to explore the implications of the Gautreaux findings, exploiting the opportunity to design the program to address some of the limitations that existed in the original data.

¹⁹ This description is taken from Goering (1999). See Goering and Feins (2003) for a survey of findings for the various cities in the demonstration.

²⁰ See Brock and Durlauf (2001a) for additional discussion of this issue. Sobel (2006) provides a careful formal discussion of generalizability of MTO from the perspective of the assumptions implicit in the analysis of treatment effects.