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A National Protocol for Measuring Intergenerational Mobility?

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The question that animates this paper is whether a standardized protocol for measuring the amount and contours of intergenerational mobility in the United States might usefully be established. Throughout our discussion, we understand “intergenerational mobility” to refer to the association between (a) the social standing of an individual’s family of origin (as assessed when the individual is growing up), and (b) the social standing of that same individual when she or he is an adult. This necessarily ambiguous definition begs as many questions as it resolves, but before attending to such ambiguities it’s useful to lead off by rehearsing why social scientists should care, indeed care rather deeply, about how much mobility there is, whether it’s increasing or declining over time, and whether the United States is at all distinctive in the amount of mobility it delivers. The various motivations underlying mobility research should naturally be borne in mind when considering whether a standardized protocol for measuring mobility should be devised.

As has frequently been pointed out, there are two lines of questioning that have historically motivated sociologists who study mobility, the first pertaining to the extent to which social classes are well formed or “institutionalized” in U.S. society (i.e., the “class formation” question), and the second pertaining to the matter of life chances and whether they are much dependent on social origins (the “equal opportunity” question). For mobility scholars oriented toward issues of class formation, the presumption has long been that high levels of social mobility, manifested both within and across generations, hampers the formation of social classes. When, for example, Sombart (1906) asked why socialism didn’t reach the United States, he argued that classes were in the U.S. impermanent statuses rather than fixed identities and hence workers were oriented rather more toward moving out of the working class than acting on its behalf. The long-standing assumption has been that, insofar as individuals judge that their lives will likely be lived out in their class of origin, they will come to identify with that class and even act (e.g., vote, protest, strike) on its behalf. The correspondingly rigid boundaries between classes further allows distinctive class cultures and lifestyles to develop and harden.

The foregoing motivation for mobility analysis, once the mainstay of the sociological interest in mobility, has become gradually less important in the field, especially in the U.S. (at least as compared to Europe). In recent years, it has almost entirely given way to an interest in equality of opportunity, an interest in examining the extent to which children born into different families have different life chances and outcomes. We care about such barriers to mobility because of the long-standing and, to some extent, distinctively American commitment to free and open competition in the labor market. The conventional formula has it that Americans tolerate or even embrace extreme inequality because they believe that opportunities to get ahead are widely available and that outcomes reflect talent and effort rather than the accident of birth. The question that then emerges, and a main question to which mobility analysis is oriented, is whether the United States is indeed living up to this commitment and hence whether conventional beliefs about widespread opportunity are on the mark. Within the U.S., the commitment to equal opportunity is arguably one of our more cherished ones, and it’s regrettable in this context that we lack a nationally mandated, standardized protocol for monitoring at regular intervals whether that commitment is being realized. The monitoring task has instead been left to the academic community and is accordingly driven by the usual academic demands for creativity, innovation,

and cleverness as much as the more mundane need for reliable data-gathering and rigorously standardized measurement.

This commitment to equal opportunity may be understood as a purely normative preference for a labor market in which starting conditions (e.g., family of origin) are not allowed to color opportunities to get ahead. The labor market is, in other words, deemed unfair insofar as outcomes are affected by forces not under an individual's control (i.e., the accident of birth). Although our commitment to equal opportunity can and often is pushed on such purely normative grounds, many scholars have additionally suggested that an open mobility regime yields better matches between persons and jobs and hence increases economic output (i.e., an efficiency rationale). Under this formulation, openness is not merely a valued end unto itself, but is also a means to an end, that end being better person-job matches and the greater productivity that accrues to them. This efficiency rationale has a long provenance within economics but has also been appreciated by sociologists (e.g., Blau & Duncan 1967).

Within the discipline of economics, the study of mobility did not take off until some 25 years ago, when a wave of research was triggered by the increase in income inequality in the 1980s and 1990s and the consequent interest in testing for an offsetting increase in mobility between economic categories (see Gottschalk 2001; Corak 2005; Bowles et al. 2005). The idea here was that the rise in income inequality is rather less troubling if those at the top of the distribution remain at the top only temporarily. If there is, in other words, a constant circulation of individuals throughout the income distribution, a snapshot that reveals extreme inequality at any given point in time will mislead, indeed possibly dramatically so, about the extent of inequality in lifetime income. This motivation, which accounts in part for the characteristic focus among economists on economic rather than class mobility (cf. Kambourov & Manovskii 2004), treats the study of mobility as a necessary complement to the study of inequality. Although this motivation still informs some economic research (Henderson 2006), it has become less prominent of late, in part because the spectacular rise of inequality has clearly not been counterbalanced by any equally spectacular rise in mobility. As a result, the study of mobility among economists seems increasingly founded on a simple interest in measuring deviations from equal opportunity, a development that renders economists and sociologists increasingly alike in their motivations.

There are, then, three main rationales for studying social mobility. The standard-issue sociologist wants to know (a) whether social classes are well formed, and (b) whether our long-standing commitment to equal opportunity is being realized, while the standard-issue economist will couple an interest in (b) with a (possible) further interest in asking (c) whether there is so much flux in income (both intergenerational and intragenerational) as to call into question conventional snapshot representations of inequality. If (c) has been exclusively the economist's obsession, then equally (a) is very much the sociologist's obsession; and the main shared ground, at least when it comes to motivations for mobility research, is arguably (b).

It's important to bear these distinct motivations in mind as our discussion of a possible national measurement protocol unfolds. We operate from the presumption that all three motivations are defensible and should be serviced by such a protocol. In reviewing the main research literatures on social mobility, we will see how different motivations have yielded different approaches, differences that ought not be papered over or suppressed but instead are best incorporated into any protocol that might be proposed.

The two traditions of intergenerational mobility research

It's of course impossible to do justice here to the extraordinary body of literature on intergenerational mobility. Indeed, the study of social mobility is often understood as constituting the very heart of sociology, although of late sociologists seem to have abandoned the heart and all but ceded the topic to economists. If sociological research on mobility thus slowed (starting in the 1990s) and is only now resurfacing (e.g., Breen 2004; Breen & Jonsson 2005; Harding et al. 2005; Beller & Hout 2006; Beller 2009; Jonsson et al. 2009), it bears noting that economists took to the field quite spectacularly just as sociologists vacated it. The upshot is that the literatures in both fields is nothing if not abundant. We review very briefly the main traditions of research and then turn to our ruminations on how a national protocol that builds on such traditions might be devised.

As shown in Table 1, it's convenient to divide mobility research into two traditions, each of which can be further divided into various subtraditions. These traditions and subtraditions are distinguished, as Table 1 suggests, by the way in which the social position of individuals or families is measured (e.g., occupation, income, education), by the underlying asset that those measurements are presumed to capture (e.g., economic standing, human capital), by the level at which measurement is undertaken (e.g., nominal, ordinal, continuous), and by the ontological standing of the social categories that the tradition or subtradition identifies. The latter categories are in some cases understood as purely nominal or statistical (e.g., income quintiles) and are in other cases institutionalized in the labor market and thus no longer understood as arbitrary statistical constructions (e.g., detailed occupations).

Occupational tradition

It's useful to begin by asking why sociologists typically carry out analyses of intergenerational mobility in terms of occupations. The short answer is that, because occupations are so deeply institutionalized in the labor market, they serve as a powerful omnibus indicator of life conditions and chances. At a dinner party, we inevitably ask "what do you do?" (a query almost always answered in occupational terms) because the response locates our new acquaintance in social space in so many ways, telling us about her or his (a) skills and credentials, (b) earning capacity, (c) social contacts and friendships, (d) prestige and social worth, (e) career trajectory and opportunities, (f) politics and attitudes, and (g) even consumption practices and leisure activities. We care, in other words, about occupations because they are so pregnant with information about the individual's life chances and lifestyles (see Weeden & Grusky 2005). The (largely untested) bias in this regard is that occupation is far more strongly correlated with these various variables than is income. In textbook descriptions of occupational categories, a common rhetorical device is to contrast a "day in the life" of incumbents of different occupations, precisely because the implications of occupation are presumed to be manifold and reliably revealed throughout the day in various ways (e.g., Kerbo 2002).

If occupations are treated in this fashion as an omnibus indicator of life conditions and chances, there are three main ways in which they can then be deployed in mobility analysis. First,

they can be *scaled or graded* in ways that signal the general desirability of the labor market position, with the origin-destination association then revealing the extent to which those born into desirable occupations are likely to themselves assume desirable occupations. This association between origin and destination desirability arises because parents at the top of the desirability distribution control the resources that make it possible for their children to get ahead. That is, their children can secure desirable occupations by virtue of (a) their access to the economic resources needed to afford an elite education or to capitalize on entrepreneurial opportunities, (b) their access to social networks providing information about or entry into the best occupations, and (c) their access to cultural resources providing them with the intellectual and social skills to qualify for and succeed in the best occupations. Although some sociologists have sought to measure desirability directly (e.g., Jencks et al. 1988), most unidimensional scales measure desirability only indirectly by asking respondents about the general “social standing” of occupations (i.e., prestige scales) or by indexing the occupational resources (e.g., income, education) that are presumed to signal overall desirability (i.e., socioeconomic scales). There is a long and lively history of debates among proponents of prestige scales (e.g., Treiman 1977; Nakao & Treas 1994), socioeconomic scales (e.g., Duncan 1961; Hodge 1981; Ganzeboom & Treiman 2006), and related spinoff scales (e.g., Hauser & Warren 1997; Chan & Goldthorpe 2004). These scales have been used by Hout & Hauser (1992), Treiman & Yip (1989), Xie (1992), Ganzeboom, Luijkx, & Treiman (1989), and many others for the purpose of parsimoniously monitoring trends and variability in mobility.

The second main way in which sociologists deploy occupations for the purpose of studying mobility is to aggregate them into *big social classes* and then examine the exchanges between these classes. The typical big-class scheme will define three, seven, or twelve categories (e.g., the salariat, craft workers, the petty bourgeoisie, farmers). Although most big-class schemes do not rely exclusively on occupational information for the purpose of defining classes (and may additionally rely on self-employment, industry, or job characteristics), in practice occupations have typically been understood as the most fundamental arbiter of class position (see Wright 2007 for an important exception). The big classes so defined are assumed to convey a constellation of working conditions, a social context that affects behavior and decision-making, and a cultural context that is an adaptation to this social context. The relevant feature of this formulation is that all children born into the same class are taken to have similar mobility chances even though their parents may hold different detailed occupations. The logic of the class situation is assumed, then, to be determinative and to control the life chances of the children born into it. Obversely, two big classes of similar desirability will not necessarily convey to their incumbents identical mobility chances, as they may differ on dimensions that have implications for mobility. For example, even though proprietors and routine nonmanuals are roughly similar in desirability or status, the children of proprietors will tend to become proprietors and the children of routine nonmanuals will tend to become routine nonmanuals. This particular pattern arises, it is assumed, because (a) the children of proprietors develop tastes for autonomy while the children of routine nonmanuals develop tastes for stability (i.e., class-specific tastes), (b) the children of proprietors develop entrepreneurial skills while the children of routine nonmanuals develop bureaucratic skills (i.e., class-specific skill formation), (c) the children of proprietors are apprised of entrepreneurial opportunities while the children of routine nonmanuals are apprised of routine nonmanual opportunities (i.e., class-specific networks), and (d) the children of proprietors inherit physical

capital (e.g., a business) that motivates them to remain as proprietors while the children of routine nonmanuals haven't access to such capital (i.e., class-specific physical capital). The signature contributions to the big-class analysis of mobility include Featherman & Hauser (1978), Hout (1984; 1988), Erikson & Goldthorpe (1992), Breen (2004), Beller & Hout (2006), and Beller (2009).

The contest between gradational and big-class approaches has often been acrimonious and, until recently, has obscured a third and equally fundamental way of deploying occupations for the purpose of mobility analysis. The *micro-class* approach shares with the big-class approach the presumption that contemporary labor markets are balkanized into discrete categories, but such balkanization is assumed to take principally the form of institutionalized occupations (e.g., doctor, plumber, postal clerk) rather than big classes (e.g., craft workers). By implication, the occupations comprising big classes will have differing propensities for mobility and immobility, a heterogeneity that obtains because the distinctive occupational worlds into which children are born have consequences for the aspirations they develop, the skills they value and to which they have access, and the networks upon which they can draw. The children of carpenters, for example, may be especially likely to become carpenters because they are exposed to carpentry skills at home, are socialized in ways that render them especially appreciative of carpentry as a vocation, and are embedded in social networks that provide them with information about how to become carpenters and how to secure jobs in carpentry. The micro-class approach to studying mobility has been developed by Rytina (1992; 2000), Grusky (2005), Jonsson et al. (2009), and others (e.g., Hollister 2010).

As shown in Table 1, we've privileged the omnibus interpretation of occupational measurements of mobility, as the main comparative advantage of the occupational approach is precisely the availability of such an interpretation. But this is not a point on which there's complete consensus. There's a long tradition, for example, of interpreting occupational scales not as an omnibus indicator of general desirability but as a more narrowly construed indicator of prestige, deference, or honor (e.g., Hope 1982). Similarly, other scholars (e.g., Hauser 1998) have suggested that permanent income may be usefully proxied by occupation, the argument here being that careers are often forged within occupations and that year-to-year volatility in individual income will therefore roughly center on the occupational mean. The classification schemes used within the big-class tradition have likewise been interpreted as indexing some preferred variable. For example, Goldthorpe (2000) argues that the "form of regulation of employment" (e.g., salaried, short-term contract) is the analytically crucial variable underlying class schemes, and he further shows that the categories of his preferred scheme differ in their characteristic forms of regulation (e.g., Evans 1992; Evans & Mills 1998; Rose & Harrison 2009). We downplay this line of interpretation in Table 1. If the objective is indeed to measure a single variable, such as the "form of regulation of employment," then it ought to be measured directly rather than indirectly through the fulcrum of occupations. By contrast, the proxy approach is of course more defensible when, as in the case of permanent income, no direct measurement is available.

Economic tradition

Within the discipline of sociology, there is accordingly a broad consensus that mobility should in some fashion be ascertained via occupations, yet also some amount of dissensus as regards the type of occupational scale or classification to use and how that scale or classification is best interpreted. It's a matter, then, of the operationalization being quite settled (i.e., occupations) while the interpretation attached to that operationalization still being a matter of controversy (e.g., omnibus measure, prestige). When, however, the focus shifts to mobility research as practiced within the discipline of economics, here one finds rather less disagreement about the concept that should be measured. The shared presumption among economists, and manifestly a reasonable one, is that the intergenerational association in *economic standing* should be our primary focus. The mobility studies on offer nonetheless take a heterogeneous form because of differences in how economic standing is operationalized (see Table 1). The disputes in this case are not about the concept so much as the best way to operationalize it.

The clearly dominant approach is to use the income of parents and their adult children to calculate the intergenerational elasticity of income (IGE). Because income fluctuates from year to year, one would ideally either (a) track it over the entire lifecourse, or (b) attempt to estimate permanent income at some specified period during the lifecourse (e.g., age 40). For reasons of data availability, only a rough approximation to permanent or lifetime income has proven possible, and the field has progressed largely by developing ever-better statistical fixes to the data shortfall. The first set of studies, which came in with IGE estimates of approximately 0.2 or less, implied that only about one-fifth of the differences in origin incomes are passed on to sons (Sewell & Hauser 1975; Tsai 1983; Behrman & Taubman 1986). If, for example, a father's income were 20% higher than the mean income, then his son's income would be expected to be only 4% above the mean. These results led to the cautious conclusion that the U.S. was a highly mobile society (Becker & Tomes 1986).

By 1990, the Panel Study of Income Dynamics (PSID) and the National Longitudinal Surveys (NLS) had matured, and it became possible to measure the average earnings of fathers and their adult sons over 4-5 years (see Solon 1989). The IGE for both the PSID and NLS samples were estimated at approximately 0.4 (Solon 1992; Zimmerman 1992). Although subsequent mobility analysts experimented with alternative ways of constructing the samples and specifying the models, the estimates still came in at 0.4 (Björklund & Jantti 1997; Solon 1999). The estimate of 0.4 was the consensus estimate of the IGE for more than a decade and indeed some scholars still consider it the best estimate (e.g., Beller & Hout 2006). The most recent data suggest, however, that this consensus estimate may yet be too low. When Mazumder (2005) matched the Survey of Income and Program Participation (SIPP) to Social Security earnings records, he was able to average earnings over as many as 16 years, with the IGE then increasing to the 0.6-0.7 range.

Whereas sociologists have settled on occupation-based tabular analyses of mobility, economists have thus settled, by contrast, on analyses in which income is treated as a continuous variable and the IGE is calculated. There is nonetheless a secondary tabular tradition within economics as well. The characteristic approach is to divide the income distribution into quintiles and then examine transition rates between quintiles (e.g., Bradbury & Katz 2002; Mazumder

2008). As with occupational mobility tables (e.g., Featherman and Hauser 1978), one finds under this approach that reproduction at the top and bottom of the distribution is especially extreme, a finding that again undermines the early characterization of the U.S. labor market as highly mobile (Beller & Hout 2006; Isaacs 2008; Mazumder 2008).

The tabular design is, however, only infrequently deployed in economics, no doubt in part because the quintiles (or other divisions) are arbitrarily chosen and the resulting categories are mere statistical constructions. The occupations deployed within the sociological tradition are, by contrast, meaningful and quite deeply institutionalized groupings. As a consequence of this difference, an occupational mobility analysis must allow for non-uniform and changing marginal distributions (e.g., the growth of the professional class), whereas an income mobility analysis tends to fix the marginal distributions by design (e.g., quintiles). When the marginal effects are instead allowed to vary, it's useful to distinguish between the mobility that is directly observed in transition matrices and the "social fluidity" that obtains once one controls for such variability (typically via log-linear or log-multiplicative models). Although one could and should allow the marginal distributions in an income mobility analysis to likewise vary (by using absolute breakpoints in the income distribution), such an approach is not conventionally taken.

The remaining economic approaches in Table 1 pertain to the distribution of wealth rather than income. The study of wealth mobility is very much a cottage industry because the requisite data are only available in small PSID samples. When the PSID is analyzed, the intergenerational elasticity of wealth comes in at roughly the same level as the intergenerational elasticity of income, with Mulligan (1997) reporting a value between 0.32 and 0.50, and Charles & Hurst (2003) reporting an elasticity of approximately 0.37. As Charles & Hurst point out, some of the intergenerational transmission of wealth is attributable to the intergenerational transmission of income (and vice versa), a problem to which we will return shortly.

The foregoing review may cover the dominant approaches to studying mobility, but it hardly exhausts the literature. If a more complete review were attempted, it would at minimum make reference to (a) the famous and influential sibling studies that allow us to estimate within-family variability in economic or occupational outcomes (e.g., Hauser 1988; Levine & Mazumder 2007; Solon et al. 1991; Björklund et al. 2002), (b) the long tradition of status attainment models focusing on the social psychological and other mechanisms through which origins are converted into destinations (e.g., Blau & Duncan 1967; Hauser 1973; Harding et al. 2005), (c) the smaller set of studies of educational mobility based on tabulations of educational origins and destinations (e.g., Hertz 2007; Pfeffer 2007; Mare & Schwartz 2006), (d) the venerable tradition of studying elite mobility via elite surveys, the *Who's Who in America*, and other sources (e.g., Hanley & Treiman 2005; King & Szélenyi 2004), and (e) the ongoing research on the duration of poverty spells (e.g., Duncan, Brooks-Gunn, & Smith 1998). The protocol that we develop builds more directly on the economic and occupational traditions reviewed above, but we will also be suggesting ways in which those traditions can be usefully expanded to deliver on some of the objectives motivating these closely related literatures.

A national protocol for studying mobility

The question that then emerges is whether these substantial research commitments within sociology and economics have delivered results that are roughly in accord with the investment. It would of course be a heroic task to attempt to compare productivity across the many subfields in sociology and economics and reach conclusions about which ones have yielded the most valuable output for the resources invested. But our instinct nonetheless is that the various mobility subfields would score relatively high in such an exercise if ever it were completed. In the case of sociology, for example, the mobility research tradition is typically counted as one of the discipline's great successes, in part because a consensus over methods has allowed mobility analysts to turn to research rather than squabble endlessly over how it should be completed. The same claim may be made for the various mobility subfields within economics. These subfields have mobilized quite straightforwardly and productively around the need to overcome profound data limitations.

Although there is much to celebrate within these fields, two problems stand out as worthy of addressing in any attempt to develop a national protocol for measuring intergenerational mobility. The first problem of note is the balkanization of the various subfields focused on analyzing mobility, while the second problem is the severe data limitations that all subfields have sought, with only partial success, to overcome. We review each of these two problems in turn in the course of discussing how a standardized framework for monitoring mobility might be developed. As will become clear, our comments are oriented toward solving enduring problems in the field, and the various fixes that we propose have to be understood as possible long-term solutions rather than ones that could be immediately implemented.

The balkanization of subfields

We begin with the suggestion that the many approaches to studying mobility are unnecessarily balkanized into quite distinctive enterprises. The standard practice of analyzing income, wealth, class, and status mobility in isolation from one another may have initially served an important function in protecting scholars from unproductive debates about what types of mobility are best monitored. Indeed, the short-run virtue of balkanization is that, like trade protectionism, it shields fledgling fields from potentially destructive competition during the startup period. This period has, however, long since passed, and the benefits to developing a more comprehensive and inclusive framework would now seem to outweigh the costs.

What are those benefits? First and foremost, when scholars examine one type of mobility in isolation of all others, they tend to interpret any estimated trends or cross-national differences exclusively in terms of mechanisms pertaining to the examined type. This is surely a leap of faith. Indeed, because the various dimensions of inequality are highly correlated with one another, the appearance of trend in any one type may in fact be generated by changes in the association with other dimensions of inequality. In explaining, for example, the tendency for working class children to “underinvest” in schooling, Goldthorpe (2002; 2000) emphasizes that such decisions may reflect the precarious economic situation within which such children are operating. This argument goes

further than the standard claim that working class children cannot afford tuition, cannot forego wages while attending school, or cannot readily borrow money to finance an investment in schooling (because capital markets are imperfectly developed). It's also a matter of lacking a viable fallback strategy; that is, when working class children do poorly and drop out of college, they may not have adequate reserves to finance a replacement investment in vocational education or to otherwise salvage the situation and avoid gross downward mobility. By this logic, working class "underinvestments" in schooling are not underinvestments at all, just rational responses to a tenuous financial position. The important point for our purposes is that, under this very reasonable formulation, the real determinant of change in class investment decisions (and hence outcomes) may simply be change in the wealth or income that a class position implies. If the financial situation of working class children is becoming more tenuous, it will accordingly create the appearance of a deterioration in working class prospects that, in the absence of explicit measurements of wealth or income, might be misinterpreted as a change in pure class reproduction.

Likewise, Jonsson et al. (2009) have shown that much of the association in a mobility table is generated at the detailed occupational level (e.g., doctor, secretary, carpenter), not the big-class level (e.g., professional, clerical worker, craft worker) to which most class analysts unthinkingly default. This means that children tend to take up their parent's occupations (e.g., doctor, carpenter, baker) and that net big-class association, after controlling such micro-level reproduction, is in fact quite limited. The children of doctors, for example, are very likely themselves to become doctors, but they're not all that likely to become other types of professionals (e.g., lawyers, accountants) insofar as they don't become doctors. It follows that, when big-class reproduction is monitored in isolation of micro-class reproduction, we may therefore misattribute the sources of any observed trend. It bears noting in this regard that much of our equal-opportunity policy has been devised to root out big-class reproduction when in fact micro-class effects are an important force behind reproduction. If we're serious about reducing reproduction, we'll need to develop strategies targeted to the underlying mechanisms at work, which in turn requires an accounting framework that allows us to distinguish among trends in different types of mobility.

The same problems arise in monitoring trends in income and wealth reproduction. We don't know, for example, to what extent such trends are driven by occupational reproduction (see Erikson & Goldthorpe forthcoming), nor do we know to what extent income reproduction is driven by wealth reproduction. There is of course inevitably some relationship. If estate tax law were changed in ways that enabled large estates to be more readily passed on, this would generate some corresponding trend in the intergenerational association in income (given that wealth generates income). This is all to make the simple point that we should want a national accounting system that allows us to tease out net trends in different types of mobility.

We take up subsequently the question of how the substantial data requirements for such an accounting system might be met. Before doing so, we provide next some brief comments on the type of statistical model that we're envisioning, a model that may be motivated with the increasingly popular conception of a multidimensional inequality space. Within economics, the income-based measurement paradigm that underlies conventional economic mobility research has come under increasing criticism, the main concern being that measuring inequality and

mobility exclusively in terms of income fails to “take cognizance of other aspects of the quality of life that are not well correlated with economic advantage” (Nussbaum 2006, p. X; also Bourguignon 2006; Sen 2006). This reaction against the income paradigm has also taken the form of increasing sensitivity to the “lumpiness” of labor markets. By “lumpiness,” we mean that income-based measures and arbitrary discretizations of those measures fail to capture the social organization of inequality, including the emergence of social networks, norms, and “adaptive preferences” (i.e., tastes, culture) within various social groupings (see Grusky & Kanbur 2006). This critique thus levels two challenges at the income paradigm: first, that income does not exhaustively describe the quality of life; and, second, that it fails to capture the social organization of inequality as expressed in the tendency for groups in the inequality space to develop distinctive cultures and tastes (e.g., Sen 1997). These two critiques can be addressed by tying together the economic and sociological traditions and analyzing the multidimensional space defined by wealth, income, and occupation (measured for both origins and destinations). By adding a measurement of occupation, we not only incorporate an omnibus measure of the “quality of life,” but we do so in a way that reflects the main institutionalized groupings that emerge in the labor market and that are the settings within which distinctive cultures and adaptive tastes emerge.

In a prior article (Grusky & Weeden 2006), we suggested that a latent class model might be applied to this multidimensional space, a model of the general type represented in Figure 1. This heuristic model has three components: a measurement model specifying the structure of origin classes; a measurement model specifying the structure of destination classes; and a mobility model specifying the relationship between origin and destination classes. The structural part of the model, which grafts together the two measurement models, could be assumed to take on log-linear form (see Marsden 1985 for a related model). Under this setup, origin and destination classes are latent rather than manifest, but even so the usual array of log-linear models could be applied (e.g., Hagenaars & McCutcheon 2002). The measurement model for each generation is complicated under this setup because some of the indicators will be continuous and others will be categorical (see Grusky & Weeden 2006).

We offered that model principally as a heuristic that reveals the assumptions of conventional mobility research. With a sufficiently large sample, a model of that type could be estimated, but that’s clearly a task for the future and in any event inappropriate for a national accounting system that should remain close to the data rather than rely on latent classes. The simpler model of Figure 2 achieves the objective of teasing out net reproduction and hence serves, we think, as a viable foundation for a national accounting system (see Erikson & Goldthorpe forthcoming). Although one could again fit this model with some continuous (i.e., income, wealth) and some categorical (i.e., occupation) variables (see Grusky & Weeden 2006), our discussion can be simplified by instead assuming that income and wealth are discretized, thus yielding a straightforward tabular array that cross-classifies origin occupation (O_o), income (I_o), and wealth (W_o) with destination occupation (O_d), income (I_d), and wealth (W_d). The main model of interest would (a) saturate the association among the origin variables (O_o , I_o , and W_o) and among the destination variables (O_d , I_d , and W_d), (b) allow for direct intergenerational reproduction for occupations ($O_o \times O_d$), income ($I_o \times I_d$), and wealth ($W_o \times W_d$), and (c) allow for cross-form effects (e.g., $O_o \times I_d$, $O_o \times W_d$, $I_o \times O_d$, $I_o \times W_d$, $W_o \times O_d$, $W_o \times I_d$). The trends in the direct and cross-form effects could be parsimoniously monitored through the usual log-multiplicative specifications (e.g.,

Xie 1992; Goodman 1979). This would allow us, for example, to characterize trends in terms of simple percent changes in the extent of big-class, micro-class, income, and wealth reproduction. These net trends could of course usefully be compared to gross trends estimated by analyzing the occupation, income, and wealth responses separately in the usual fashion.¹

A dearth of data

It's always easier to propose a model than to estimate it. Given the (large) number of categories in contemporary occupational classifications (see Jonsson et al. 2009), the model represented in Figure 2 will be rich enough in parameters to complicate estimation, but we're confident it's viable. The main problem with adopting the model of Figure 2 is not that of estimation per se but of finding or developing a data set with enough cases to make a multidimensional analysis feasible. If the first priority, then, in setting up a national accounting system is to address continuing balkanization by bringing together several research traditions in a single model, the second priority is to develop a data collection protocol that allows such a model to be estimated. We thus turn to such data problems now.

It's perhaps surprising that a country with such a strong commitment to equal opportunity hasn't enough data to reliably monitor whether that commitment is being upheld. The main approach to date has been to rely principally on the PSID and NLS for analyses of economic mobility and on the SIPP, OCG (Occupational Changes in a Generation), and GSS (General Social Surveys) for analyses of occupational mobility. The main problem with such data sets is that they're small. In a recent review, Björklund, Jäntti, & Solon (2007, p. XX) conclude that changes in intergenerational income mobility have been vexingly "difficult to estimate," mainly because of the small size of the PSID sample. And likewise Lee & Solon (2009) recently note that available estimates are "highly imprecise" because the data are so sparse. This sample size problem has led to all manner of creative statistical fixes (e.g., Aaronson & Mazumder 2008; Hertz 2007; Lee & Solon 2009), but one wouldn't think that a country so deeply committed to equal opportunity would have to rely on the heroic model to monitor that commitment. The situation is hardly better within the sociological tradition. Indeed, efforts to estimate trends in occupational mobility have been all but abandoned of late, as the main data source for estimating mobility in recent cohorts (i.e., GSS) is extremely small and is compromised by substantial changes over time in occupational classification (cf. Beller & Hout 2006; Jonsson et al. 2010). The U.S., purportedly a country with a special interest in mobility, is arguably in worse shape when it comes to monitoring mobility than most any other late industrial country.

There's not any obvious immediate solution to such fundamental problems, and our focus here will instead be on a long-run solution that might be implemented within, say, 5 or 10 years. The conventional suggestion might be to call for a replacement PSID with a much larger sample size or perhaps to bolster SIPP and GSS in various ways. The SIPP, for example, might regularly include an intergenerational module, while the GSS likewise needs to better measure parental income. An alternative or supplementary approach is to link existing surveys with administrative

¹ For purposes of simplicity, the model represented here does not distinguish between father's and mother's occupation, but ideally it of course would (Beller 2009). It might be necessary to resort to the "dominance approach" (Erikson 1984) to make the model tractable.

records. When the SIPP is linked to the Social Security Administration's Summary Earnings Records (SER), long-term earnings histories for both parents and children become available and measures that more nearly approximate permanent income can be assembled (see Mazumder 2005).

Although there is much to be said for improving existing surveys or better linking them to administrative records, the highest payoff approach would be to build directly and exclusively on administrative records, especially individual income tax returns supplemented with data on Social Security benefits (see Auten & Gee 2009). Because all tax filers have been required, starting in 1987, to report the Social Security numbers of dependent children, it becomes possible to use tax records to go forward in time and identify the income and occupation of dependents when they enter the labor force (and of course beyond).² This approach has many virtues:

1. It delivers an extremely large data set that can meet the data-intensive demands of the multidimensional approach outlined above.
2. Because the data set is so large, it's not only possible to carry out all manner of cross-group comparisons (e.g., gender, marital status, regional), but also to examine reproduction at the very top and bottom of the distribution (thus replacing, for example, the methodologically problematic *Who's Who* elite studies).
3. The long-term income histories for parents and children can be corrected for well-known measurement bias in the IGE by assembling income data across multiple tax years (and thus pushing closer to a measure of permanent income).
4. The detailed occupations of both the filer and spouse are ascertained, albeit at this point poorly, on Form 1040 and 1040A and may again be linked to those of their dependent children (and ultimately their own spouses as well).
5. The wealth of filers may be imputed from information on dividends, mortgages, capital gains distributions, pensions and annuities, and (for the extremely wealthy) linked estate tax returns.
6. The effects of family structure, which are well-known to affect occupational reproduction (e.g., Biblarz, Raftery, & Bucur 1997), can be gleaned from changes in filing status.

This is by no means a complete listing of the opportunities that an administrative solution opens up. If there were indeed a way to make tax data widely available (presumably in secure data centers), then they could be used for all manner of labor force analyses, including of course analyses of intragenerational mobility. The viability of using tax data for intragenerational analyses has, in fact, already been demonstrated (Auten & Gee 2009). The upside, then, is impressive: We will in effect have at our disposal what begins to resemble a true Nordic population register.

We are naturally trying to seduce by laying out the many opportunities. Are there also potential problems with the administrative approach? Absolutely. Most obviously, it will require a herculean effort to negotiate access and safely overcome confidentiality concerns, and one might reasonably judge that effort too costly in light of the low likelihood of success. It's nonetheless an

² We have assumed here that one should begin with parents and track their dependent children by going forward in time. It's equally possible (and may well be advantageous) to instead begin with the "children" and then locate their parents in earlier records (as Robert Hauser, in a personal communication, pointed out).

important struggle: It bears recalling that the Nordic registers didn't just happen and that pressure from social science interests was instrumental in making registry data more widely available. It's also worth noting that we needn't insist on Nordic-style access of the most dispersed sort. We might instead imagine developing a system in which all mobility analyses are undertaken by government employees (with academics brought in as government-employed consultants as necessary). If the objective is, as we think it should be, to develop a national commitment to measuring mobility, there is in fact virtue to a division of labor in which academia sheds the formal and regularized monitoring mission and concentrates instead on the task of improving and upgrading measurements and examining the sources and causes of mobility. We could in other words embrace a distinction similar to that which now prevails between (a) the task of developing and testing "academic" models of unemployment, and (b) the wholly bureaucratic reporting of unemployment statistics themselves. It follows that much academic work might continue to be carried out with survey data while the descriptive task of monitoring mobility trends would occur with administrative data (with the associated advantage that such a dual system allows for checking and validation).

The viability of the administrative approach also rests of course on the quality of administrative data. We might worry, for example, about the underreporting of income, about the quality of the occupational reports, about the representativeness of tax data (in light of disproportionate non-filing at the bottom of the income or class distribution), and about the understating of income for households receiving tax-exempt income from workers' compensation, Supplemental Security Income, family assistance, or certain disability programs for veterans. For at least some of these problems, it's relevant that the matched tax data will in principle provide dozens of reports per individual (i.e., one per year), meaning that one or more years of missing or error-ridden data can be overcome (at least more so than in a cross-sectional survey). As always, the question is not whether the data are error-free, as of course they aren't, but whether the errors are any greater "in total" than what prevails under the alternatives. And even this standard may be too high. If it's easier to correct administrative data collection efforts than to correct the alternative problems endemic to surveys (e.g., low response rate), then one might be willing to accept administrative data of comparatively poor quality at inception in exchange for the promise of substantial improvement over time.

Conclusions

We have raised only a small subset of the questions that have to be taken on before settling on any standardized protocol for measuring mobility. There are, for example, any number of unresolved methodological debates about how best to measure classes and how best to define and measure income. In the European Union (EU), a consensus around the Erikson-Goldthorpe class scheme is emerging, and it now appears poised to become the official EU standard for many labor market measurements, including measurements of intergenerational mobility (see Rose & Harrison 2009). The question that then emerges is whether the U.S. should adopt the (likely) EU standard, adopt some other international standard, or continue to rely on its own indigenous scheme. Likewise, we could have discussed various competing statistical approaches to estimating the IGE (e.g., Solon 2008), also various competing log-linear and other models (e.g., Logan 1996)

for characterizing the structure of class mobility. We might have additionally weighed in on debates about whether period or cohort assessments of trend are preferred (see Breen 2004), whether the distinction between relative and absolute mobility should be ported over to the income mobility tradition (Erikson and Goldthorpe forthcoming), and whether any intervening variables (e.g., schooling) should be incorporated into a standardized protocol (e.g., Breen & Jonsson 2007).

We haven't addressed these questions because they're all quite secondary at such an early point in the discussion. We sought instead to begin with first principles. If one wants to set up a national accounting system, the logically prior steps are (a) deciding on what types of mobility to measure and, in particular, whether a rationale can be forged for joining the now-independent economic and sociological traditions, and (b) deciding on what types of data (i.e., administrative or survey) are best exploited in light of these measurement decisions. These two core questions have to be resolved before any small-gauge methodological questions can be sensibly posed and addressed.

We have come out foresquare in favor of a multidimensional approach that models economic and occupational reproduction at once. The conventional wisdom on this point, a wisdom to which we subscribe, is that occupations constitute the "backbone of the stratification system" (Parkin 1978) because they signal life chances, consumption behavior, and a host of other social practices. Whereas we measure income because we care about income, we measure occupation because it's pregnant with noneconomic information about skills and credentials, social contacts and friendships, prestige and social worth, politics and attitudes, and consumption practices and leisure activities (Weeden & Grusky 2006). It's not merely that occupations may be understood as a proxy for permanent income. More importantly, income is only a partial indicator of life chances and social resources, and insofar as one wants an omnibus measure of extra-economic dimensions one has to look to occupation first and foremost.

The further virtue of a multidimensional model is that it allows one to tease out the net change in different types of mobility. The usual approach of examining each type in isolation can lead to misunderstandings because the various dimensions of inequality are so strongly correlated with one another. As but one example, many commentators have noted that the rise in income inequality (e.g., Saez 2008) may increase the resources available to parents at the top of the occupational structure (e.g., doctors, lawyers), the effect of which is to allow them to more reliably transmit their occupation to their children. Although this process would show up as increased immobility in a conventional mobility model, such a trend would also promptly disappear in the context of the multidimensional model of Figure 2 (as all change would be absorbed in the rising association between I_0 and O_0). We don't of course mean to suggest that trends in occupational mobility will always be spurious. Rather, our point is simply that a multidimensional model can identify the simplest and most basic forms of spuriousness, thus providing a powerful tool for understanding how mobility regimes are evolving and changing.

In setting up a national protocol, one also has to settle on a data base, the main choices being to (a) bolster conventional surveys (i.e., PSID, GSS, SIPP) or develop enhanced analogues to them, (b) link conventional (or bolstered) surveys to administrative tax records, or (c) rely on tax records (and associated files) as a stand-alone administrative solution. We have suggested that (c) dominates the alternatives because it delivers a large data set, allows for extensive cross-group

comparisons, can replace ad hoc analyses of elite mobility with more rigorous ones, corrects for measurement bias in the IGE, provides information on occupation and imputed information on wealth, and allows for estimates of the effects of family structure on mobility. If the administrative approach also buys all manner of methodological problems, it may nonetheless be wise to pay that price in exchange for entry into an administrative system that could then over time be improved.

Figure 1. Heuristic latent class model

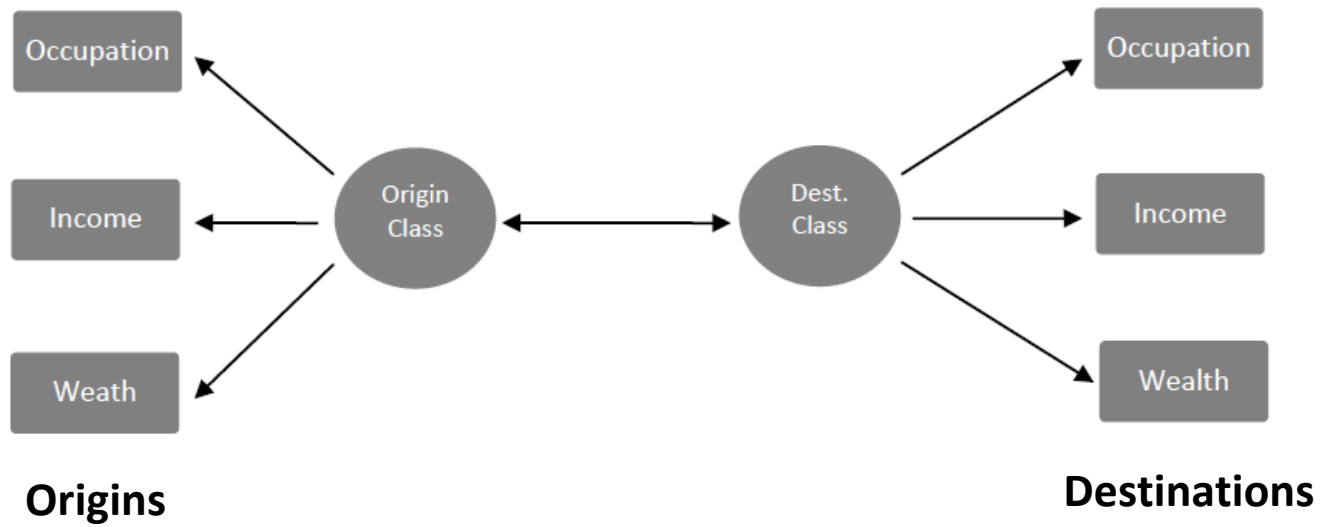


Figure 2. Heuristic multidimensional mobility model

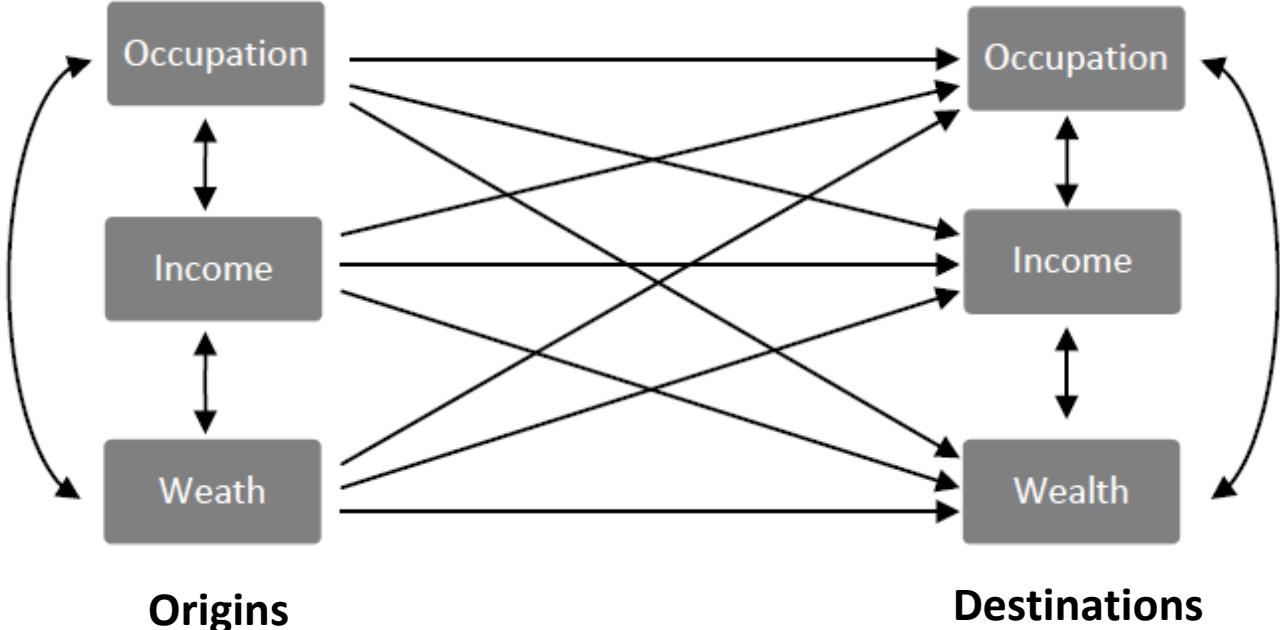


Table 1. Intergenerational mobility research traditions

<i>Mobility type</i>	<i>Underlying asset</i>	<i>Measurement level</i>	<i>Degree of institutionalization</i>
<i>Occupation tradition</i>			
Status	Omnibus	Continuous	-
Big class	Omnibus	Nominal or ordinal	Medium
Micro class	Omnibus	Nominal or ordinal	High
<i>Economic tradition</i>			
Income or earnings I	Economic (flow)	Continuous (e.g. , dollars)	-
Income or earnings II	Economic (flow)	Ordinal (e.g., quintiles)	Low
Wealth I	Economic (stock)	Continuous (e.g., dollars)	-
Wealth II	Economic (stock)	Ordinal (e.g., quintiles)	Low

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