

A New Infrastructure for Monitoring Social Mobility in the United States

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The country's capacity to monitor trends in social mobility has languished since the last major survey on U.S. social mobility was fielded in 1973. It is accordingly difficult to evaluate recent concerns that social mobility may be declining or to develop mobility policy that is adequately informed by evidence. This article presents a new initiative, dubbed the American Opportunity Study (AOS), that would allow the country to monitor social mobility efficiently and with great accuracy. The AOS entails developing the country's capacity to link records across decennial censuses, the American Community Survey, and administrative sources. If an AOS of this sort were assembled, it would open up new fields of social science inquiry; increase opportunities for evidence-based policy on poverty, mobility, child development, and labor markets; and otherwise constitute a new social science resource with much reach and impact.

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As Richard Reeves notes in his introductory essay, the commitment to equal opportunity is such a fundamental feature of the U.S. experiment with democracy that it shows up in early drafts of the country's founding documents, albeit mainly via abstract references to the "independence" of individuals. This early commitment, which has since been concretized through various defining events in U.S. history (e.g., the Civil War, World War II, the Civil

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Rights movement), is now expressed in well-known tropes to the effect that (1) material success should depend on ability and hard work alone; (2) each generation should enjoy material comforts exceeding those enjoyed by the preceding generation; and (3) opportunities to get ahead should be conferred without regard to color, creed, or social origins. Although the concepts of mobility and opportunity cannot be equated, it is widely appreciated that mobility data provide suggestive evidence about the extent to which opportunities are equally distributed, which is why most late-industrial countries have well-developed systems for monitoring social mobility.

Given our country's quite special commitment to equal opportunity, one might imagine that we likewise have a well-developed infrastructure for monitoring intergenerational mobility and for assessing labor market opportunities, just as other late-industrial countries do. This is not the case. It is surprisingly difficult to characterize trends in U.S. mobility because currently available surveys are based on small samples and because contemporary administrative data are not as accessible as one might like. This is a troubling state of affairs for a country that was once a world leader in mobility studies. In the 1960s and 1970s, the United States mounted two large-scale surveys of mobility (e.g., Blau and Duncan 1967; Featherman and Hauser 1978), both of which were treated as templates for other efforts throughout the world. The last such survey in the United States was fielded in 1973 (Featherman and Hauser 1978). By contrast, most other late-industrial countries have continued to monitor mobility with repeated cross-sectional surveys (e.g., Japan), panel surveys (e.g., Germany), or register data (e.g., Sweden).

In the four decades since our last mobility survey, U.S. society has changed in profound ways, many of which may have affected the amount and patterns of social mobility. It is not simply that income inequality has increased dramatically during this period. In addition, a growing number of immigrants have arrived from Mexico, Latin America, Asia, and Africa; women have entered the labor

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force in large numbers and increasingly entered male-typed occupations; manufacturing jobs have disappeared and service and information-sector jobs have become more prominent; families have become more complicated by virtue of breakups, cohabitation, and other new living arrangements; incarceration rates, especially for black men, have soared; new forms of childcare, preschool, and primary and secondary education have emerged; new types of colleges have become more prominent (e.g., community colleges, for-profit schools); and some disadvantaged groups, especially young black men, are experiencing profound delays in labor market entry (see Grusky and Cumberworth 2010; Smeeding, Garfinkel, and Mincy 2011).

These changes may have affected opportunities for mobility, yet we lack the data needed to assess whether they have. In recent years, many commentators have boldly claimed that social mobility is declining (e.g., Obama 2014), even though the data behind this claim are at best suggestive. There is a pressing need for a new infrastructure to reliably monitor the durability of what Truslow Adams (1931) tagged the “American dream.” It is extraordinary that a country so committed to mobility has let its measurement infrastructure fall into such disrepair. If our commitment to mobility and equal opportunity is a real and meaningful one, we obviously must develop the capacity to assess whether it is being realized. At minimum, we should monitor social mobility as reliably and comprehensively as we monitor other key indicators of the country’s health, such as unemployment, poverty, or income inequality.

This new monitoring infrastructure should of course take into account ongoing advances in social science methods, research, and data. These advances, especially the increasing availability of administrative data, make it unwise to simply default to the conventional measurement approaches of the past. The contributors to this volume of *The ANNALS* explore how a new infrastructure for monitoring mobility can and should be configured in light of these opportunities. Although one could settle for a quick incremental improvement in our capacity, it is arguably an opportune moment to take stock and consider the viability of a lasting infrastructure that exploits new developments.

The commitment to doing so is already in place. In 2012, the National Science Foundation awarded an infrastructural planning grant to the National Academy of Sciences (NAS), with the title of that grant, “Designing a New National Survey on Social Mobility,” betraying the presumption that a new survey should be mounted (National Academy of Sciences 2012). The panel of mobility scholars that convened as part of this grant nonetheless took care to review all available options before developing a new initiative that has come to be labelled the American Opportunity Study (AOS).¹ Because many of the contributors to this volume will refer to this initiative, an overview of the AOS is provided in this article. We first set the stage by discussing (1) the high cost of carrying on with our existing inadequate infrastructure for measuring mobility and (2) the various survey and administrative options that are available insofar as it is decided that the current infrastructure should be upgraded.

The High Cost of the Status Quo

In recent years, interest in monitoring mobility has sharpened, the main precipitant being the rise in U.S. household income inequality and a concern that it may have reduced opportunities for mobility (e.g., Atkinson, Piketty, and Saez 2011). The former chairman of the Council of Economic Advisers, Alan Krueger, has openly worried that the negative cross-national correlation between income inequality and mobility (i.e., the “Great Gatsby curve”) implies just such a reduction in opportunities to get ahead in the United States (Krueger 2012).

Fashionable though the Great Gatsby curve now is, there are of course a host of other institutional changes in play, including the continuing growth in college attendance. We do not review these other forces here (see Mitnik, Cumberworth, and Grusky 2013). For our purposes, it suffices to note that some of them may offset the (putative) effects of rising income inequality, while others may reinforce those effects. The expansion of postsecondary education, although slowing of late, is perhaps the most obvious countervailing force. Because the association between origins and destinations is very weak among college graduates (Hout 1988), the ongoing growth in college graduation increases the proportion of the population that falls into this low-association condition, thereby inducing an overall increase in mobility (Pfeffer and Hertel, forthcoming; Torche 2011; Breen 2010).

These hypotheses about trends have proven difficult to rigorously evaluate because the available mobility data are simply inadequate. Within economics, there has nonetheless been a resurgence of research on trends in mobility, a development that has partly been motivated by concerns about the effects of rising income inequality. The resulting studies, while of great interest, have not been able to deliver definitive conclusions. In a now-classic review, Lee and Solon (2009) concluded that available estimates on trends in intergenerational economic mobility are “highly imprecise,” mainly because the available datasets (principally the Panel Study of Income Dynamics [PSID]) are extremely small. This is why Krueger (2012) found it necessary to resort to cross-national comparative data in projecting the possible effects of rising inequality on economic mobility in the United States. Although Chetty, Hendren, Kline, Saez, et al. (2014) have cleverly used administrative records to sidestep such concerns, these data are only available for relatively recent years and for relatively young adults.²

Within sociology, the General Social Survey (GSS) is an especially important resource for research on mobility, as it is one of the few surveys that continues to ascertain the class origins of respondents (via occupational and other items). It has accordingly been used to update the mobility estimates coming out of the two prior national mobility surveys (e.g., Beller 2009; Beller and Hout 2006). When Beller (2009), for example, sought to analyze how mothers matter in intergenerational mobility, she was able to tease out some evidence of rigidification with the GSS data. Likewise, Mitnik, Cumberworth, and Grusky (2013) found an increase in professional-managerial immobility, but only at the borderline of significance and under some specifications.

Because the direct evidence on trends tends to be ambiguous, recent attention has focused on various types of indirect evidence. The best-known example of such indirect evidence is Reardon's (2011) exposition of a growing performance gap in math and reading achievement between high-income and low-income children. Additionally, we have seen a threefold increase between 1972 and 2007 in top-decile spending on children, an increase that suggests that parents at the top may be investing in ever more high-quality daycare and babysitting, private schooling, books and tutoring, and college tuition and fees (Kornrich and Furstenberg 2013; Kaushal, Magnuson, and Waldfogel 2012). Within the lowest quintile of parents, spending on children has also grown, but at a lower rate. Although the resulting growth in the "spending gap" should, all else equal, work to increase the effects of origins on educational outcomes, the evidence on the trends in such effects is in fact quite mixed (e.g., Duncan et al. 2012; Reardon 2011; Hout and Janus 2011).

These latter lines of evidence are immensely important, but they cannot substitute for direct measurement of trends in mobility.³ With much statistical ingenuity, scholars of mobility have been able to overcome some of the methodological problems and carry out important analyses of trend, but clearly there remains a profound evidence deficit. It is troubling that such a state of affairs is tolerated in a country that has been founded on a commitment to social and economic mobility, that purports to continue to care about such mobility, and that is now considering new mobility-inducing policy initiatives. If these initiatives are to be more than symbolic, they should be informed by a strong base of evidence.

The Alternatives to the Status Quo

The purpose of this volume of *The ANNALS* is to consider the requirements that any new research initiative on mobility must satisfy. The contributors discuss such issues as:

- the need for large-sample analyses that can be used to monitor trends with precision and that can support key subgroup comparisons (e.g., racial, ethnic, and regional comparisons);
- the importance of taking into account the increasingly complicated family arrangements in which children are raised (and in which adults also often find themselves);
- the failure of many past mobility studies to represent the experiences of immigrants, institutionalized populations, and partly hidden subpopulations (e.g., the homeless);
- the need to extend the usual two-generation analyses of mobility to allow for "grandparent effects" and the influence of other familial networks;
- the failure of prior studies to develop a unified and comprehensive model of the many types of mobility (e.g., economic, occupational, political);

- the need to represent the growing volatility of labor market experiences (e.g., income volatility, occupation volatility) among both families of origin and of destination;
- the importance of recognizing that the mobility process is affected by the intergenerational transmission of political power and of social and civic engagement; and
- the importance of understanding how mobility is affected by new types of schooling (e.g., home schooling, for-profit schooling) and different types of skills (e.g., soft skills, cognitive skills).

These requirements and considerations, most of which are presented in the second section of this volume (i.e., “Special Topics Relevant to Building a New Infrastructure”), reflect a tension between (1) building an infrastructure that is oriented to descriptive questions about the direction and patterning of trends in mobility and (2) building an infrastructure that is oriented to causal questions about the sources of mobility and of trends in mobility.

The National Research Council steering committee was of course obliged to sort through these competing commitments and requirements. In the course of its deliberations, a variety of options were considered, a process that was crucially informed by an early draft of Warren’s contribution to this volume. As Warren notes, a straightforward approach to improving our infrastructure is simply to increase the sample size of an ongoing mobility survey, perhaps most obviously the PSID. As an alternative to the PSID option, a permanent intergenerational module could be incorporated into the Survey of Income and Program Participation (SIPP), or improved measures of parental income and other family origin measures could be incorporated into an enlarged GSS. We might alternatively design a stand-alone survey or add comprehensive intergenerational modules to existing large federal surveys (e.g., Current Population Survey [CPS], American Community Survey [ACS]). Because Warren addresses these alternatives comprehensively, we forgo a review here of their costs and benefits, turning directly instead to the case for the AOS.

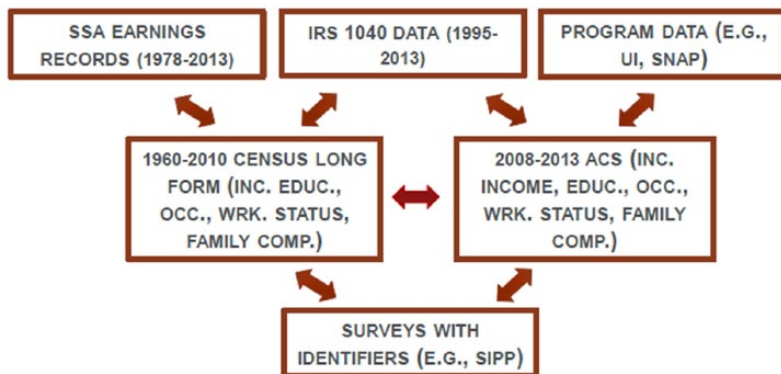
The Proposal

We lead off with a little-known fact: the United States already has in place the constituent parts of a large-sample panel with a comprehensive array of intergenerational items. This panel has, however, gone quite unappreciated because it is in unassembled form and has never been used in the extensive and ambitious way that we envisage.

We propose to develop an on-demand capacity to assemble this panel by undertaking the following steps:

- assigning identification keys to the individual records in the 1960–1990 decennial censuses;
- using these identifiers to then track the same individuals into the 2000–2010 decennial censuses, the 2008–2013 ACS, and ultimately future decennial censuses and ACSs;

FIGURE 1
The AOS Initiative



NOTE: SSA = Social Security Administration; IRS = Internal Revenue Service; UI = Unemployment Insurance; SNAP = Supplemental Nutrition Assistance Program; ACS = American Community Survey; SIPP = Survey of Income and Program Participation.

- extending the resulting panel by using the same identifiers to link to data from administrative sources (insofar as approval to do so is secured); and
- effecting intergenerational links between parents and children within the AOS by drawing on existing databases that match the Social Security numbers (SSNs) of parents to those of their children.

The resulting AOS would only be assembled for the duration of a qualified project and only draw on the datasets or resources that are directly needed for the research or policy purpose at hand. The AOS project is in this regard best viewed as an initiative to develop the country's capacity to better exploit existing data by linking them on an on-demand basis. When the proposed project passes a stringent review, the AOS would allow the necessary linkages to then be implemented, with the resulting deidentified data passed on to the researcher only for the purpose of carrying out the pre-qualified research, presumably in Census Bureau research data centers (RDCs) or other secure venues. We discuss issues of security below, but before doing so it is important to clarify the structure of the proposed AOS, a task that we take on by elaborating the heuristic steps presented above (see Figure 1).

Assigning PIKs

The first step in assembling the proposed AOS is to assign a protected identification key (PIK) to each individual in the 1960–1990 decennial censuses. This step can be carried out by using a set of variables (e.g., first name, last name, year

of birth, address, sex) that, when taken together, allows us to reliably find an individual's SSN in the Social Security Administration (SSA) Numident file. The technical challenges behind this procedure are discussed in detail by Warren (this volume) and by Johnson, Massey, and O'Hara (this volume). This step is costly because the 1960–1990 censuses are not yet PIKed (whereas the 2000 census, 2010 census, and 2008–2012 ACSs are already). Once the PIKs are assigned and the post-1990 linkages completed, we will have a panel for all individuals appearing in the 1960–1990 censuses, with post-1990 information (e.g., education, occupation, income) available for each year in which the respondents show up in later censuses or ACSs. That is, the AOS infrastructure will be seeded with individuals appearing in the 1960–1990 censuses, but it will then be “refreshed” by including new entrants to the population in subsequent censuses (and intervening ACSs).⁴

Administrative linkages

The resulting panel could in principle be supplemented by acquiring administrative records for the individuals within it. If approval to link to IRS 1040 and SSA earnings records could be secured, additional high-quality reports of income, earnings, and other variables would become available on an annual basis. This enhanced panel, if it were indeed assembled, would no doubt be subjected to far more stringent controls on access than would an AOS panel based on census products alone.⁵ Although IRS 1040 and SSA earnings reports are perhaps the most valuable linkages for the purposes of mobility research, other administrative records could also be usefully incorporated (e.g., vital statistics, social program participation records). The practical and legal obstacles to linking to administrative data are discussed by Johnson, Massey, and O'Hara (this volume).

Intergenerational matches

The AOS panel, as laid out above, would provide repeated observations on income, education, occupation, and other demographic variables for individuals appearing in the 1960–1990 censuses, any additional linked administrative data, and subsequent censuses or ACSs. The next step in creating the proposed AOS is to convert this intragenerational panel into an intergenerational one by establishing links between parents and children. These intergenerational matches can be made by referring to existing “Kidlink” files that identify, for each parent's SSN, the corresponding SSNs for his or her children. The Kidlink files, which are currently used by the IRS to determine whether tax filers are making legitimate claims to dependent children, could in principle be used for our matching purposes as well (see Johnson, Massey, and O'Hara [this volume] for details and limitations). Additionally, IRS 1040 forms could be directly used to improve the quality and scope of parent-child matches, given that parents claiming children as dependents have been required, since 1987, to list the SSNs of the claimed children. Finally, the ACS and decennial censuses also identify children of the household head, thus providing a further source of parent-child matches.

Although more research on these and other approaches is required, the initial evidence on intergenerational matching rates is promising (see Johnson, Massey, and O’Hara, this volume).

“Sliding in” surveys

The AOS would provide a high-quality infrastructure for monitoring mobility without the cost of mounting a new mobility survey and without burdening existing surveys with (possibly low-quality) intergenerational modules. This is not to suggest that mobility surveys would no longer be needed in the post-AOS world. To the contrary, the AOS would allow surveys to become far more efficient vehicles, given that they could be used exclusively for the purpose of ascertaining variables that were not already available in the AOS. Given the proposed architecture, any sufficiently large survey with individual identifiers could be linked to it, thus making it possible to supplement the AOS with any of the additional variables collected in the linked survey.⁶ Although an analysis based on the AOS alone would suffice for a wide range of descriptive analyses, a survey supplement to the AOS might be useful for studies of the causes, consequences, and social correlates of mobility.

The obstacles to assembling the AOS, even the version based on Census Bureau products alone cannot be overstated. However large such obstacles may be, they are worth taking on given that the AOS would, we suspect, come to be viewed as one of social science’s major infrastructural resources. The dividends would come in the form of

- substantial cost savings and efficiencies that arise from exploiting information that has already been collected for other purposes (rather than mounting a new and duplicative data collection effort);
- the capacity to characterize intergenerational parameters on the basis of contemporaneous reports (and hence obviate the need for retrospection);
- the capacity to exploit high-quality administrative data and high-quality census products rather than field new and likely lower-quality surveys (given cost constraints);
- the spinoffs and cost savings to various census products that accrue to advancing methods for PIKing and intergenerational matching (see Johnson, Massey, and O’Hara, this volume);
- the development of a monitoring infrastructure that, by virtue of being automatically “refreshing,” sidesteps the problems with unrepresentativeness present in other long-running panels (e.g., the PSID);
- the opportunity to gradually grow the AOS and extend its research uses by adding new administrative records (e.g., program use data); and
- the capacity to field leaner and more efficient surveys by relying on the AOS for core economic and demographic items.

This list of benefits is further explicated below. But even the foregoing cursory list serves to convey the reach and potential of the proposed AOS. In the social

sciences, opportunities for transformational investments come about rarely, and when they do the price tag is usually very high. The AOS is, by contrast, quite affordable because it exploits data that have already been collected for other purposes and adds value to those data by assembling the latent panel underlying them. In this sense, the AOS substitutes for new and replicative data collection efforts, thereby making it consistent with the Paperwork Reduction Acts of 1980 and 1995.

The Research Payoff

These cost savings and efficiencies are of course attractive. But the case for the proposed AOS must ultimately rest on the research that it makes possible and the policy that it can inform. We next provide a brief and incomplete sampling of AOS-enabled research opportunities that would advance basic social science and provide a rich body of policy-relevant evidence.

Subgroup analyses

The most obvious opportunity, and one that we have stressed throughout, is that the proposed AOS would yield samples that are large enough to examine mobility within subgroups defined by race, ethnicity, immigration, and gender. The resulting analyses would make it possible to develop policy for key subgroups on the basis of evidence of the same caliber as the well-known unemployment time series. It bears stressing that the AOS, as envisioned here, would be self-propagating (at a very low marginal cost) and thus deliver an ongoing and highly consistent time series on mobility. As new ACSs are fielded and new administrative data become available, the latest income, occupational, and other data could be appended to the individual records already in the sample, and new entrants into the United States (either via birth or immigration) would of course be added.⁷

A comprehensive stock of mobility time series

We suspect that many, if not most, of the analyses coming out of the proposed AOS would be of the stock disciplinary type. That is, we expect and hope that the usual intergenerational income and earnings elasticities will be estimated; that the usual tabular arrays of class, occupation, income, and earnings mobility will be analyzed; and that the usual structural equation models of educational, occupational, and income attainment will be estimated. It will be contribution enough within each of these disciplinary fields to update the time series that, for lack of data, have long been languishing.⁸ These time series will provide the basis for a comprehensive monitoring of U.S. mobility and opportunity, allow for experimental and nonexperimental assessments of the effects of mobility policy, and provide better evidence of the conditions under which different types of mobility tend to move together.

Multidimensional analyses

The proposed AOS also allows us to go beyond the usual disciplinary division of labor that has economists studying economic mobility, sociologists studying occupational mobility, and education scholars studying education mobility. When scholars examine one type of mobility in isolation of others, they tend to interpret trends exclusively in terms of mechanisms pertaining to the examined type. This disciplinary approach can be misleading insofar as the various dimensions of inequality are highly correlated with one another. By virtue of these correlations, the appearance of trends in one dimension (e.g., income mobility) can be generated by trends in another omitted dimension (e.g., occupation mobility), with consequent misunderstandings of the sources of change and the policies that might bring about change. The proposed AOS will make it possible to develop a comprehensive model of mobility in which trends in education, income, and occupational mobility are examined at once.

Sibling models

We have assumed to this point that the effects of social origins are best assessed by examining the relationship between the corresponding parent and child variables (e.g., income mobility, occupation mobility). However, sibling correlations provide a broader measure of the total effects of the “accident of birth,” as they incorporate the family, community, and neighborhood forces shared by siblings when they are growing up together (e.g., Mazumder 2008; Björklund et al. 2002; Warren, Hauser, and Sheridan 2002; Hauser, Sheridan, and Warren 1999; Solon 1999). Because the ACS and decennial census identify children growing up in the same household, the AOS could be used to estimate various types of sibling models, with the distinct advantage that the samples would be larger than those previously available and thus allow for breakdowns by period, geographic area, and subgroup. Although sibling models are sometimes regarded as the gold standard, they have not been brought into the mainstream arsenal for monitoring trends because, absent an AOS architecture, such models require large sibling samples that are just too costly to generate. The AOS would change that calculus and thereby improve our monitoring of inequalities in opportunity.

Family and mobility

If a full range of administrative data were secured, the proposed AOS could be used to identify “social parenthood” (via coresidency), “biological parenthood” (via Kidlink files), and “financial parenthood” (via tax returns). Given its panel design, the AOS can (imperfectly) detect changes in family situations during both childhood and adulthood, thus making it possible to capture some of the complex family histories and living arrangements that may be mobility-reducing (see Tach, this volume). If new types of family complexity (e.g., single parenthood, blended families) are indeed working to reduce mobility, the AOS will give us the capacity to detect just that.

Multigenerational analyses

In recent articles, Mare (2011, 2014) notes that conventional analyses of parent-child mobility fail to take into account the wider effects of kin networks, especially those of grandparents. As the proposed AOS accretes over time, an ever-growing web of multigenerational relationships will gradually emerge, making it possible to evaluate some of the hypotheses advanced by Song and Mare (2013) and others (e.g., Zeng and Xie 2014; Clark 2014). If Clark (2014) is right, for example, that conventional two-generation analyses understate the amount of constraint and inequality (perhaps dramatically so), there is no alternative but to build a monitoring infrastructure with all the intergenerational reach of the AOS.

Innumerable spinoffs

The AOS would be a major social science resource with future uses that cannot be fully envisioned now. As but one example, it would make it possible to track outcomes for individuals who, early in their life, were exposed to various treatments (e.g., divorce, single parenthood, incarceration, early childhood education, job training programs), thus giving us a new and unprecedented capacity to evaluate the effects of (1) social change and social programs, (2) participation in the criminal justice system, and (3) innovations in education and other public policies. Because a very large proportion of the U.S. population would be represented within the AOS, we could retrospectively evaluate the effects of treatments already conducted as well as evaluate ongoing or future policy experiments without the high cost of tracking participants over time. The latter capacity has such profound implications for developing science-informed policy that the full cost of the AOS could easily be justified on that basis alone.

The preceding list of analyses is hardly exhaustive. For the most part, it privileges narrowly construed studies of mobility, but it also hints at the wider range of uses that the proposed AOS makes possible if a wide range of administrative sources and add-on surveys were ultimately slotted into the AOS architecture. Although the AOS would open up a wealth of opportunities not covered here, even our very partial listing makes it clear that it would be a major new infrastructural resource in the social sciences.

Critical Commentary

This is obviously not to suggest that the proposed AOS, even when supplemented with add-on surveys, satisfies all the requirements that our *ANNALS* contributors have laid out. There are clearly many ways in which the AOS falls short of that ideal. In soliciting the *ANNALS* contributions, we asked some of our authors to attend to these shortcomings, as obviously any decision to move forward with the AOS has to be made in full light of them. The purpose of this section is to summarize and react to some of the critical commentary that has so far surfaced either in the *ANNALS* contributions or in other preliminary discussions of the

AOS. We review here five of the most important concerns, often relying on Warren's (this volume) phrasings of these concerns, as he has conveniently delivered most of the key criticisms in especially pointed terms.

Population coverage

It is useful to begin by addressing concerns about the proposed AOS's population coverage. Because the AOS is based on the decennial census and the ACS, in principle it would represent the full U.S. population in 1960 through 1990 and in every year since 2000, when the ACS was first administered. As new data become available (in future censuses, ACSs, or administrative records), they could be appended to the individual records already in the sample, and new entrants into the United States could also be added as new records. This "self-refreshing" feature of the proposed AOS overcomes the problems with unrepresentativeness and survey attrition that plague other long-running panels (e.g., the PSID).

Because the AOS is self-refreshing in this sense, it does not come with a built-in population referent, as do most conventional surveys. The latter feature of the AOS places a special burden on the analyst to define the population of interest. As Warren (this volume) puts it, "because of the complex design of the AOS, researchers will have to think carefully about the population to which their results can be generalized." The key question here is whether this flexibility of the AOS, which we regard as an asset, will introduce complexities that are too taxing for the typical data analyst. In our own view, the "too-taxing" hypothesis is quite unpersuasive, given that many workhorse panels are yet more complicated (by virtue of sample attrition, refreshment samples, and other features) and nonetheless have long been successfully analyzed. It bears noting that the AOS would likely be no more complicated than a population register of the sort that social scientists and mobility scholars routinely analyze.

This is not to gainsay the equally important point that, insofar as more specialized administrative data are linked to the AOS, such add-on data will typically not be available for the full population. These data are often the product of highly selective processes: the IRS tax data are only available for tax filers, the SSA earnings records are only available for those with reported earnings, and program participation data are only available for those who participated in the program (e.g., Supplemental Nutrition Assistance Program). In many cases, individuals with missing records in any given year (e.g., IRS nonfilers) have nonmissing records in other years, meaning that the resulting panel will have a patchwork structure to it. When analyzing such data, it may be important to take into account the selective processes that determine whether data are available (e.g., the determinants of nonfiling), a rather tractable task given that social science is nothing if not rich with models and methods for addressing selection. The main conclusion here: because the social world is rife with selection, one is best off with data that authentically capture and represent the selective processes at work, not with data that define them away with some artificially restrictive definition of the population. The AOS may be understood as just such an authentic

representation of the complicated patterns of participation that are the hallmark of modern societies.

Intergenerational populations

In a related concern, Warren (this volume) further suggests that questions of representativeness are especially fraught in studies of intergenerational mobility, as “neither the parents nor the children [in such studies] would be representative of any particular population.” If the AOS were used, for example, to build a conventional mobility table, Warren notes that “the older generation would exclude people who had no children who survived to adulthood (and would over-represent those who had multiple children).” This conclusion, although entirely on the mark, of course pertains to all conventional mobility tables, not just those based on the proposed AOS (see Hout, this volume). At the same time, Warren is right to worry that “the younger generation [in a conventional mobility table] would exclude those with parents who lived abroad,” at least insofar as their parents do not have SSNs and are not coresiding with their children at the time of the ACS or decennial census. If the child’s parents do not appear in any census, ACS, or other administrative source, then the AOS could only provide information on the parents if an add-on survey with an intergenerational module, like the GSS, were incorporated into the AOS. This shortcoming is arguably the AOS’s most fundamental one and underscores the importance of the add-on survey when analyzing the mobility of such unaccompanied immigrants.

Sample size

We have suggested that the AOS will solve the sample size problem that, to date, has made it so difficult to reliably monitor trends in mobility. The extent to which the AOS will indeed yield large samples depends in part on the linkage rate. Although much preliminary research on linkages has been completed, we do not yet have full information on the linkage rate under different linking protocols and approaches (see Johnson, Massey, and O’Hara, this volume). The early results coming out of this research are encouraging, but clearly there is a pressing need to continue with this line of research.

There has been some debate about the extent to which missing data pose special problems in linked data of the AOS variety. In his skeptical view on this matter, Warren (this volume) has worried that “the more data elements that are added, the lower the overall linkage rates, and the greater the number of sample exclusions.” This argument rests on the assumption that cases showing up with any missing data would simply be excluded (i.e., listwise deletion). In many analyses of economic mobility, attenuation bias is addressed by summing across reports and then dividing by the number of (nonmissing) reports, an approach that does not entail resorting to listwise deletion (see Mazumder, this volume). There are of course all manner of other more sophisticated missing data techniques that likewise do not rely on listwise deletion (e.g., Rubin and Little 2002).⁹ The conventional view here, and one that we wholeheartedly endorse, is that the

benefits of multiple reports vastly outweigh any minor complications that arise when some of those reports are missing.

Comparability

If a main purpose of the proposed AOS is to allow for rigorous trend analysis of intergenerational and intragenerational processes, a secondary purpose is to allow for rigorous cross-national comparisons of such processes. The question that then emerges is whether the proposed AOS would provide a good foundation for cross-national analysis. In a bold claim, Warren (this volume) suggests that “the AOS would not likely permit cross-national comparison,” an argument that appears to rest on the view that administrative and survey approaches are so irreconcilably different that comparisons between them cannot safely be made. In most cross-national mobility studies, it is already standard practice to draw data from both survey and administrative sources (e.g., Breen 2004; Jonsson et al. 2009; Smeeding, Erikson, and Jäntti 2011; Ermisch, Jäntti, and Smeeding 2012); hence Warren is suggesting here a new standard that has not been insisted upon to date. Although we are unconvinced that such a mechanical standard should be adopted, the proposed AOS would nonetheless have the flexibility to realize it. That is, insofar as one wanted to break with past practice and insist upon survey-to-survey comparisons, one could simply excise all nonconforming sources from the AOS and carry out the analysis with the balance of the data. Because the AOS comprises diverse sources (e.g., survey data, administrative data), it could likewise be used to attempt to replicate other idiosyncrasies or deficiencies that arise in another country’s study.

The more important point is that, rather than mechanically replicating the deficiencies of past studies in other countries, the United States has an opportunity to lead by example and develop a state-of-the-art design. This willingness to innovate has been a hallmark of U.S. mobility research. When the Occupational Change in a Generation I (OCGI) and OCGII surveys were released many decades ago (Blau and Duncan 1967; Featherman and Hauser 1978), they set a new standard that precipitated a new round of mobility studies across the world. As was the case then, we should again insist on the highest possible standard, even if many other countries do not yet meet that standard. We can always hope that our practice will nudge them in the right direction. Even if it does not, the AOS would often be flexible enough to downgrade it to what prevails elsewhere, thereby allowing for cross-national comparisons that are narrowly comparable in the manner that Warren advocates.

Security and privacy

The most commonly voiced concerns, ones that come up more frequently than any of the foregoing, pertain of course to issues of privacy. If the AOS does not come to pass, it will likely be due to just such concerns about its implications for individual privacy. It is important in this regard to distinguish between first-order and second-order concerns, where “first-order concerns” pertain to actual compromises to

privacy, while “second-order concerns” pertain to the fallout from unwarranted public worries about such compromises. We take on each type of concern in turn.

The first-order concerns are arguably the less formidable ones. This is because, as legitimate as first-order concerns are in other contexts, no special or troubling privacy issues arise in this particular context. For the purpose of assembling the AOS, the U.S. Census Bureau would of course rely on various personal identifiers, but these are only interim “production tools” that will ultimately be stripped from the released product. This type of procedure is standard practice for a variety of census products and raises no new or special concerns in this context. Likewise, insofar as any spatial or geographic variables are released as part of the AOS, they would necessarily be provided at a level of aggregation that precludes any identification of individuals. This is again standard Census Bureau practice and raises no new or special concerns. Finally, the AOS would be made available only to carefully vetted researchers and research projects through Census Bureau RDCs, thus providing yet another layer of protection. This is again standard Census Bureau practice and raises no new or special concerns. Although a more complicated protocol would have to be devised for analyses of data subject to especially stringent restrictions, here again there are many existing templates that could be drawn upon in establishing this protocol.

We obviously cannot rule out the possibility that there are indeed legitimate first-order concerns. Rather, our point is simply that we are currently unaware of any troubling privacy issues that the preceding practices, all of which are standard and ongoing, might raise in the foreseeable future. Because we may be overlooking legitimate concerns, a crucial part of the debate about the AOS should be an open and wide-ranging discussion of the types of security breaches that might occur and how they might be prevented. If any legitimate concerns are uncovered in the course of this discussion, we suspect that they will affect not just the implementation of the AOS but also ongoing Census Bureau practices.

Although a full discussion of first-order concerns should be an important part of any deliberations about the AOS, our strong suspicion is that the most pressing worries will prove to be of the second-order variety. That is, most of us very reasonably worry about the public’s perception of the AOS, not about any actual compromises to privacy that the AOS might imply. The standard prescription for such misinformation problems, indeed the one to which most scientists routinely default, is simply a call for a full and frank discussion of the facts of the matter. In the preceding paragraph, we have very predictably issued just such a call, although we are not so naïve as to suggest that the facts will necessarily win out. Whatever the outcome in this case may be, Prewitt (this volume) is right to note that the scientist’s only responsibility is to encourage a full and open discussion, relentlessly leavening it with such facts as are available.

Conclusion

We began this article by expressing surprise that a country so committed to openness, mobility, and equal opportunity could have allowed its infrastructure for

monitoring mobility to fall into disrepair. Although there are growing concerns that social mobility may be declining, the available data are simply not up to the task of establishing whether such concerns are warranted. We are accordingly left to attempt to devise new mobility policy without the benefit of satisfactory evidence on trends in mobility. It is rather like formulating U.S. monetary and labor market policy without knowing whether unemployment is increasing or decreasing. We find ourselves in just such an untenable situation with respect to mobility policy because the last major survey on social mobility in the United States was fielded some four decades ago.

Given this state of affairs, it might be imagined that our recommendation would be to move forthwith to fielding a new stand-alone survey, ramping up an existing survey (e.g., PSID, SIPP, GSS), or including a new mobility supplement to the CPS. We cannot of course rule out any of these approaches. If, however, there is any silver lining to our country's four-decade hiatus in fielding a dedicated mobility survey, it is that we are not harnessed to an existing outmoded approach and can instead build a state-of-the-art infrastructure for the twenty-first century. We might not have envisaged such an infrastructure had the United States maintained an adequate program of data collection of the sort that most other late-industrial countries have.

The skeptic will surely emphasize the formidable legal and organizational hurdles that the proposed AOS has to overcome (e.g., Warren, this volume). Although such skepticism is understandable, we do not share it. The nation's statistical system has shown itself to be "endlessly innovative" (Prewitt, this volume) by embracing time and again new statistical and methodological opportunities. It is no less important that the Paperwork Reduction Acts of 1980 and 1995 call into question any approach that does not exploit existing data. By virtue of these acts, we have an obligation to avoid unnecessary and replicative data collection efforts, an obligation that leads quite directly to an AOS-styled solution. It is not surprising in this context that there is already political support for an AOS-styled infrastructure. The proposed Clearinghouse for Program and Survey Data, as laid out by Congressman Paul Ryan (2014, p. 67), entails exploiting administrative data and linkages in ways that are suggestive of an AOS.

The simple conclusion: The United States has an unassembled panel that is standing unused and that, for a relatively small outlay, could be transformed into a major new infrastructural resource in the social sciences. The AOS comes with substantial cost savings and efficiencies, allows the United States to formulate child development and labor market policy using high-quality evidence, and would lead to a renaissance of labor market and mobility research that would almost surely reestablish the United States as a leader in the field.

Notes

1. The members of this panel are Henry Brady, David B. Grusky (co-chair), Robert M. Hauser, Michael Hout, David Johnson, Robert Mare, Sara McLanahan, Sean Reardon, Gary Solon, Timothy Smeeding (co-chair), C. Matthew Snipp (co-chair), and Robert Warren.

2. This limitation arises because intergenerational matches only became possible when parents were required (in 1987) to provide Social Security numbers for children claimed as dependents.

3. Worse yet, the same sample-size problems also undermine analyses of mobility for many small groups, such as certain immigrant groups, some racial and ethnic groups, and those living in relatively small geographic areas. It follows that many important intergroup differences in mobility are not well understood (cf. Sharkey 2010; Chetty, Hendren, Kline, and Saez 2014).

4. It would also be possible to link to decennial censuses in 1980 and earlier (thereby securing parental information from earlier time periods).

5. For some administrative data, only a very narrow range of uses are permissible (e.g., tax records can only be used for the purposes of informing tax policy), restrictions that will of course have to be enforced when researchers propose to access these data. In assessing proposals to use the AOS, a very stringent review process would therefore be needed, a review process that would address not just the scholarly merits of the proposal but also the uses to which the scholarship would be put.

6. For voluntary surveys, respondent consent is required before any links can be made to administrative records, to the ACS, or to decennial censuses.

7. This self-refreshing feature of the AOS places the onus on the analyst to define the population of interest (rather than requiring the population to be established at the point of fielding the survey).

8. The study of occupational mobility, long a mainstay of trend analysis, has been especially hampered because the available surveys are too small to estimate recent trends with much precision (see Mitnik, Cumberworth, and Grusky 2013). Although tax-return data have recently been used to estimate trends in economic mobility (Chetty, Hendren, Kline, Saez, et al. 2014), even here the available data could usefully be shored up with an AOS infrastructure. By folding tax-return data into the AOS, it becomes possible to fill in missing income data for IRS nonfilers, thereby eliminating one of the most important biases in these data. The AOS could also be used to examine economic mobility for subgroups (e.g., racial groups) that are not identifiable with IRS 1040 records.

9. There is a growing statistical literature on the problem of incomplete matrices that will be very relevant when AOS analyses are undertaken (see Candès and Plan 2009).

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