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Economic Instability Trends and Levels across Household Surveys

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ECONOMIC INSTABILITY TRENDS AND LEVELS
ACROSS HOUSEHOLD SURVEYS

Scott Winship

Final Report Submitted for the National Poverty Center Survey of Income and Program Participation (SIPP) Small Grants Competition

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Economic Instability Trends and Levels across Household Surveys
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Abstract

Understanding trends in economic instability is vital if economic and social policies aimed at mitigating economic risk are to be effective. Despite the popular perception that economic instability has been rising—and a research literature that often support this perception—recent studies have cast doubt on the conventional wisdom. At the same time, previous research that has used disparate measures, datasets, and methodological choices provides little guidance to account for different findings across studies. This report addresses this problem by estimating comparable trends in economic instability across three of the most-used household surveys—the SIPP, CPS, and PSID. I find that comparing estimates depends crucially on how imputed income components are addressed. The SIPP indicates that economic instability has not changed much over 40 years. The PSID results mirror those of the SIPP for male and female earnings but show a small increase in the instability of household incomes. The CPS tends to show increases in instability, which become modest after excluding incomes dominated by imputation. However, features of the SIPP—along with the results of my analyses—give reason to believe the SIPP estimates are the most valid of the three surveys. The results call into question the concept of a "Great Risk Shift" in general and the validity of the recently developed Rockefeller Economic Security Index in particular.
Economic Instability Trends and Levels across Household Surveys
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In the second half of 2010, the nation found itself in the midst of its third straight "jobless recovery", following the deepest recession in at least 25 years, with a "double dip" recession a very real possibility. The tenor of popular perceptions of economic conditions since the early 1990s is best conveyed by the reigning buzzwords of the era, including not only jobless recovery, but white-collar recession, downsizing, outsourcing, off-shoring, and structural unemployment.

This growing sense of insecurity has been accompanied by increasing policy and academic interest in economic instability, including its extent and whether or not it has risen. One team of researchers—backed by a prominent foundation—has even suggested that an "economic security index" be added to the existing set of economic indicators used to evaluate economic conditions and set policy. The rapid increase in research on economic instability, security, and volatility, however, has in some ways proceeded too hastily. Basic questions of measurement have yet to be answered, such as whether different indicators (and types of indicators) produce similar results, the extent to which estimates depend on various methodological choices made by researchers, and whether different datasets produce similar estimates of the same indicator. In addition, the complexity of the available datasets raises the question of whether past estimates of instability are even accurate.

This report addresses a number of these questions by comparing estimates of economic instability produced from the three surveys best suited to examining it. In so doing, the report also assesses the levels of and trends in economic instability, measured as the risk of an earnings or income drop. This research is the first to consider patterns of economic instability across the three surveys, and to the extent possible I apply the same methodological decisions to each dataset. After a brief discussion of the surveys, I review the existing research findings from them. I then discuss the relevant methodological considerations and present my results.

The Surveys

Instability and volatility are inherently dynamic concepts focused on change over a relatively short period. As such, operationalizing them requires panel data tracking the same individuals over time. There are a limited number of panel surveys that can produce nationally representative estimates, but three surveys fit the bill—the Current Population Survey, the Panel Study of Income Dynamics, and the Survey of Income and Program Participation. These surveys differ in ways that are useful for examining how accurately and consistently economic instability is measured.¹

¹ Other research has used the National Longitudinal Surveys (Abowd and Card, 1989; Buchinsky and Hunt, 1999) or administrative earnings data from the Social Security Administration (Mazumder, 2001; Dahl, DeLeire, and Schwabish, 2007, 2008; Gottschalk and Moffitt, 2007; Nichols and Favreault, 2009; Kopczuk, Saez, and Song, 2009; Dahl and Schwabish, 2009; Sabelhaus and Song, 2009), income data from
The Census Bureau's Current Population Survey generally is not thought of as a panel survey, though sample housing units are tracked for over a year. Because the design of the survey is complicated and because data files do not include unchanging cross-wave identifiers, few researchers take advantage of the longitudinal features of the data. CPS sample members are interviewed for four months, then one year later are interviewed in the same four months again. Because the sample is continuously refreshed every month, in any given calendar month, half of CPS respondents were interviewed twelve months earlier and half will be interviewed twelve months hence.

Respondents participating in March are administered the Annual Social and Economic Supplement (ASEC, formerly the Annual Demographic Supplement), which includes an extensive battery of questions about income received in the previous calendar year. Therefore, half of CPS sample members interviewed in March have detailed income data for both the previous calendar year and the year before that.

Because the supplement is administered around the time that taxpayers are preparing their tax returns, the income reports may be more accurate than in surveys where interviews occur at other times of the year. In addition, the relatively short duration of the panel makes sample attrition less of a concern than in other surveys. Furthermore, the survey content and questions themselves have been fairly consistent over time, in many cases going back to the early 1960s. When the survey design has changed—for example, to adjust population controls to the latest decennial census—the Census Bureau has conducted extensive research to verify that the impact on estimates is minimal.

However, there are significant shortcomings of the CPS for longitudinal research. As noted, there are no unchanging person or household identifiers to link records separated by twelve months (or even one month). The absence of identifiers means that linking individuals' records requires matching on a set of variables that can cause errors in two different directions. If the set of variables is too restrictive, then many records will not be linkable, and particularly in the presence of recording or response errors, the matching process will fail to link many individuals' records. However, if the set of variables used to match records is not restrictive enough, records that do not represent the same person or household may be incorrectly "matched" while other records will not be linkable to a

the Internal Revenue Service (Slemrod, 1992; Carroll, Joulfaian, and Rider, 2007) or earnings data from unemployment insurance systems (Gottschalk, McEntarfer, and Moffitt, 2008; Celik, Juhn, McCue, and Thompson, 2009; Juhn and McCue, 2010). The NLS samples are based on birth cohorts followed over time, and so they are only representative of certain age groups. In addition, trends cannot be distinguished from life course changes because of this sample design (although estimates from different NLS panels can provide some trend information). Administrative data has great advantages over survey data, but are relative inaccessible, since privacy restrictions require analysts to apply to use the data (and often to analyze it only at certain sites). When linked to survey data, as in many of these studies, biased estimates may result if some survey members cannot be matched to administrative data, with refusal to provide a Social Security number a potentially large selectivity problem. Earnings data miss government workers who are not covered by Social Security, immigrants and others with no Social Security number, and workers paid under the table. Income data is available for "tax units" rather than families or households. Earnings are available in the Longitudinal Employer-Household Dynamics data, but only for a limited number of states.
unique match. Both of these matching issues could affect estimates of economic instability. If, for instance, those who are most likely to make mistakes in responding to survey items that are then used for matching experience more instability, their absence from the matched sample will understate instability. On the other hand, incorrectly matching records from different people will result in instability estimates that are likely to be too high.

These problems are exacerbated by a key feature of the CPS survey design—the survey samples housing units, not households. If a household moves away from its address, the CPS continues to focus on the address, interviewing the new occupants who replace the household. Therefore, households who are more residentially unstable will be less represented in the CPS than other households, making attrition bias a concern. In addition, if the linking of records is not conducted carefully, households who move may be "matched" to the new residents of their housing unit. While these issues clearly have the potential to affect estimated levels of economic instability, they will only affect trends in instability to the extent that their impact varies over time. Even in terms of levels, it is unclear whether CPS estimates of instability are likely to be biased upward or downward, and much will depend on the details of the matching process used.

Another potential issue with the CPS is the fact that changes in household composition make income reports from the previous year somewhat difficult to interpret. Respondents are asked about the income received the previous year by current members of the household. But some of those household members may not have been present in the previous calendar year, while other people may have been present but are no longer members of the household. This issue is likely to increase measured economic instability.

Other concerns with the CPS include recall errors, since respondents must think back as many as 14 months about the income they received, and under-representation of disadvantaged groups due to nonresponse. As I will show below, survey and item nonresponse are more general problems that have gotten worse over time. Nonresponse problems are magnified when the multiple waves of the CPS are used, since instability measures require at least two observations for each person. And nonresponse may be a problem either in the basic monthly CPS survey or in the ASEC itself; respondents who complete the basic monthly survey may decline to participate in the ASEC.

Finally, because the CPS is generally used as a cross-sectional survey, there are no weights to address attrition.

The Panel Study of Income Dynamics, administered by the University of Michigan's Institute for Social Research, began in 1968 with a sample representative of the lower 48 states and an additional sample of disadvantaged households. It has followed these sample members ever since, interviewing them every year through 1997 and then every other year since then. What is more, the PSID has tracked sample members as they moved out of their original households, including young adults who leave to form independent households. The result is a rich multi-generational dataset. As with the
CPS, respondents are asked about income received in the previous calendar year, and as with the CPS, questions are asked about a large number of income categories individually.

As a panel dataset following individuals, the PSID facilitates longitudinal analyses by providing person identifiers, avoiding the problems of matching presented by the CPS and its focus on housing units. Furthermore, because PSID surveyors have information on family members in other households for many potential attriters, the survey can rely on them to more easily track movers or other would-be attriters, an advantage not enjoyed by panel surveys that do not follow the branches of family trees across generations. The PSID includes weights that adjust for attrition (although these weights will not adjust instability samples to account for the necessary presence of an individual in two years' of data). In addition, perhaps because of the smaller size of the PSID relative to the CPS and SIPP or because of the ongoing participation of respondents, surveyors are able to elicit more complete responses from them, meaning that imputation of incomes is less of an issue. Remarkably, like the CPS, the survey covers a forty-year period.

The PSID shares some of the weaknesses of the CPS, including the lack of correspondence between current household composition and that from the previous year. In addition, recall error is likely a bigger problem in the PSID, since interviewing generally occurs later in the year.

Perhaps most important are issues that follow from the longitudinal, multi-generational design of the PSID. Sample attrition has been significant over the years, raising concerns that more recent estimates of economic instability will be more biased than in earlier years (potentially downward, if less stable households are more likely to fall out of the survey). Nevertheless, there is little evidence that attrition bias has been significant, although few studies have been conducted. Another issue derives from the problem of following only original sample members and their offspring, which leaves immigrants and their families underrepresented (and increasingly so over time). To address this problem, the PSID added a refresher sample of post-1968 immigrants in 1997 and 1999. But this created a new inconsistency in that sampling weights were altered to make respondents representative of the national population whereas they were not meant to be prior to 1997.

The disadvantaged oversample in the PSID has been criticized for ambiguity in the methods used to actually sample respondents, with the sampling almost certainly nonrandom. But if these respondents are excluded, there are no weights that adjust the main sample for attrition.

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2 Fitzgerald, Gottschalk, and Moffitt (1998a); Fitzgerald, Gottschalk, and Moffitt (1998b); Lillard and Panis (1998); Zabel (1998); Beckett et al. (1988). See Nichols and Zimmerman (2008), however, for evidence that from year to year, attriters are different from those included in volatility samples in terms of the joint distribution of a number of demographic variables.

Finally, the administration of the PSID has changed more frequently and more dramatically than for other surveys. Questions used to collect income data have changed (e.g., adding of questions, combining or splitting of income categories, shifting to asking about month-by-month receipt of selected income sources), as have top- and bottom-coding procedures, the income processing software, efforts to bring attritors back into the sample, rules about who to follow when households become disrupted, and the length of the survey. Importantly, many changes occurred between 1992 and 1996, raising concerns that there may be a seam during this period making later estimates incomparable to earlier ones.4

The final survey I use is the Survey of Income and Program Participation, also administered by the Census Bureau. The SIPP is actually a series of panel surveys, generally covering 32 to 48 months each. Like the PSID, it follows individuals as the unit of analysis, so when households or individual members move, they are pursued rather than just continuing to sample the occupants of the housing unit. As with the PSID, new household members are incorporated into the survey, though the focus is on original sample members. Participants are interviewed every four months and asked about the previous four months. SIPP panels have been followed since 1983, but with gaps between some panels.

The SIPP has a number of attractive features for measuring economic instability. Since respondents are asked about their incomes every four months, the survey likely has less recall bias than the PSID (although in theory the CPS may obtain more accurate calendar-year information due to the interview's proximity to the tax filing deadline). The short reference period also reduces the problem of household composition change, and the frequency of interviews lessens the potential impact of nonresponse and imputation, since imputed data in a wave will affect only four months rather than a whole year. The SIPP's income questions solicit responses regarding more-detailed income categories than the CPS or PSID. Because the panels last only a few years, attrition is less of a concern than in the PSID. Finally, the focus on monthly incomes means that sub-annual incomes may be examined.

The weaknesses of the SIPP include the fact that estimates are only available beginning in 1984-85 and that there are years for which estimates are unavailable. Attrition may be cause for concern in that estimates for calendar years that occur later in a panel may be systematically biased in comparison with estimates for calendar years earlier in a panel. Imputation procedures changed in 1996, and imputation for at least minor income categories is fairly pervasive, growing more so over time. Variable names changed across panels more than once, and the coding of not-in-universe values changed. Finally, SIPP documentation and data is somewhat difficult to track down and navigate, and matching individual records across waves within a panel requires working with many files.

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4 See Winship (2009) for a review and discussion.
Previous Research

Research using the CPS generally shows wage instability among men rising over the 1970s through the double-dip recession of the early 1980s, then falling to late-1970s levels by the mid-2000s (Gittleman and Joyce, 1995, 1996; Cameron and Tracy, 1998; Celik et al., 2009). When self-employment earnings are also considered, male earnings instability shows a similar trend, except that instability does not fall after the early 1980s (Ziliak et al., 2010). Female earnings instability apparently declined dramatically (Ziliak et al., 2010). On the other hand, family and household income appear to have increased steadily through at least the late 1990s (Hertz, 2006, 2007; Ziliak et al., 2010).

Only two studies examine earnings instability using SIPP data. Peter Gosselin and Seth Zimmerman (2008) measure earnings instability as the average within-person variance over a three-year window. They find that instability was basically flat from 1984-85 to 2001-02. Sule Celik and colleagues (2009) examine the dispersion of the change in wage and salary income, confining the analyses to men. Consistent with the CPS research, they find that instability declined from 1984-85 to 2006-07, fluctuating much more than in Gosselin and Zimmerman.5

These results are inconsistent with most of the SIPP studies that look at family or household income instability.6 Family income instability apparently increased from the early 1990s to the early 2000s (Gosselin and Zimmerman, 2008; Acs, Loprest, and Nichols, 2009; Bania and Leete, 2007). Research that carefully treats imputation of income data in the SIPP, however, raises doubt about these findings. When Dahl, DeLeire, and Mok (2010) considered the risk of a 25 percent drop in household income, they found a modest increase from the early 1990s to the mid-2000s, but when they omitted individuals with imputed income data, the association between income instability and food insecurity increased and instability declined modestly over time.

Similarly, Dahl, DeLeire, and Schwabish (2008) found that the likelihood of a 25 percent change in household income (up or down) grew slightly from 1984-85 to 2004-05 but was flat once incomes with imputed labor income were dropped. They also showed that the trend and levels of household income instability when SIPP records are matched to Social Security Administration labor income data resemble the trend and levels when non-imputed SIPP data alone is used. Orzag (2008) presented matched SIPP-SSA results showing that the risk of a 50 percent drop in income was flat from 1984-85 to 2001-02 and that dispersion in income changes had declined.

The latest set of SIPP estimates measuring economic instability, from Jacob Hacker and his colleagues (2010a, 2010b), also struggles with how to deal with imputed income

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5 Several studies use SIPP panels matched to Social Security Administration earnings records in order to take advantage of the demographic and contextual data in the SIPP while having the benefit of presumably more accurate administrative earnings data (Mazumder, 2001; Nichols and Favreauilt, 2009; Sabelhaus and Song, 2009). Since these studies do not use the earnings data in the SIPP, I omit them from the above discussion.

6 Again, Nichols and Favreauilt (2009) match SIPP panels to SSA data to look at combined husband-wife earnings instability, but they do not use any of the SIPP income data.
components. They exclude observations in any year that have "hot-deck" imputed data in any month of the year for head or spouse labor income. However, they retain observations with imputations based on cross-wave allocations, as well as those with imputation for other income components and other household members. They find that the risk of a 25 percent drop in household income rose unevenly from the mid-1980s to the early 2000s and then fell back to mid-1980s levels. They project on the basis of a regression model that instability jumped dramatically in 2008 and 2009.\(^7\)

Dozens of studies on economic instability have been conducted using the PSID. In Winship (2009), I review about 20 PSID-based studies examining earnings instability and about 15 looking at income instability.\(^8\) Based on these studies, earnings instability appears to have increased among men during the 1970s but to have declined slightly among women. There is little evidence for a secular increase in earnings instability since the early 1980s by most measures, and women appear to have experienced continual declines in instability. The exception is that instability as measured by within-person dispersion of earnings or by dispersion of shocks to earnings appears to have increased between 1980 and the early 2000s.

Family and household income instability, however, has been consistently found to increase over the 1970s, 1980s, and 1990s in PSID-based studies. At the same time, in my recently completed doctoral thesis (Winship, 2009), I showed that when various methodological issues are addressed appropriately, the PSID indicates little secular increase in earnings or income volatility since the early 1980s recessions.\(^9\)

Overall, most studies using the CPS, SIPP, and PSID paint a consistent picture in the aggregate. Earnings instability rose in the 1970s among men but probably did not rise much thereafter. It declined among women. Family and household income instability rose steadily from 1970 to 2000. However, as the issue of SIPP imputations suggests—and as my own work considering methodological decisions using the PSID affirms—this apparent consensus may be more fragile than it appears.

**Methods**

I use the PSID data available on the University of Michigan Institute for Social Research website and CPS data from Unicon Research Corporation. I also use the recently released 2010 CPS data from the Census Bureau's website. My SIPP data comes from the National Bureau of Economic Research website (1984-88 and 1990-93 panels) and

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\(^7\) The authors focus on an "economic security index" that incorporates additional information on medical costs, debt, and wealth, but these additions mainly increase the level of instability without affecting the trend.

\(^8\) Among the most important of these are Gottschalk and Moffitt (1994), Moffitt and Gottschalk (1995, 2008), Haider (2001), Dynan et al. (2008), Dynarski and Gruber (1997), Gosselin (2008), Hacker (2008), Rose and Winship (2009), Shin and Solon (2009), Stevens (2001), and Duncan et al. (1993). Since my review, new PSID-based studies have emerged, including Gottschalk and Moffitt (2009), Bayaz, Chen, and Couch (2009), Leigh (2009), and three papers by Jonathan Heathcote, Gianluca Violante, and their colleagues (2009). See also Dynan (2010).

\(^9\) See also Rose and Winship (2009).
the Census Bureau's SIPP website (1996, 2001, 2004, and 2008 panels). I use the SIPP core wave files in all panels rather than the longitudinal full panel files available from 1984-93, a total of 109 files in all. I also estimated results using the 1984 full panel file, which includes additional edits and imputations beyond those in the core wave files, to check whether the secular trends were affected. The 1984 estimates were very close regardless of whether the core wave files or full panel file was used, so I omit the latter.

For the PSID estimates, I use both the nationally-representative "SRC" sample and the disadvantaged "SEO" oversample, as well as the immigrant refresher samples added in 1997 and 1999. Doing so has both benefits and costs. On the benefit side, it maximizes sample size, and weights are available that adjust for attrition. The weights also adjust for other changes in the PSID sample from year to year. For instance, in 1990, 1992, 1993, and 1994, surveyors made concerted recontact efforts that resulted in the successful re-incorporation of several thousand attriters back into the survey. In 1990, 1993, 1994, 1996, 1997, and 2005, the PSID's rules for following household members that move were adjusted, and in 1994 the sample was redefined to include additional children. The SEO sample was also reduced for budgetary reasons in 1997. These changes produce "seams" in the data that make year-to-year comparisons potentially problematic.10

Using the entire core sample also has costs. The initial selection of the SEO sample involved significant departures from random probability sampling.11 Even absent this problem, using weights introduces inconsistencies into the data. With the incorporation of the immigrant sample, the weights use post-stratification to make the PSID sample nationally representative. Prior to 1997, the sample in any year is supposed to be representative of noninstitutionalized Americans alive in 1968, plus their descendants. From 1997 onward, the weighted PSID sample is supposed to be representative of the noninstitutionalized U.S. population for a given year.12

Fortunately, the decision as to whether the full weighted sample or the unweighted SRC sample should be used turns out not to be important—the estimates are very close regardless of which approach is chosen.

To match records in adjacent years of the CPS Annual Social and Economic Supplement, I first dropped the small number of cases in each year that were not uniquely identified by the matching variables. I then matched records for each pair of adjacent years that allowed for it, dropping non-unique matches and non-matches. The appendix provides details on how I conducted the matching, but essentially I required matched observations to have the same address and household identifiers across years, the same line number and sex, and age and education levels that were consistent.

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12 Heeringa and Connor (1999).
**Consistent Samples**

Because policy debates over economic instability have focused on changes in families and in labor markets, I focus on working-age adults. Specifically, I restrict the samples to adults between the ages of 20 and 59 during periods over which income changes are examined (generally either one- or two-year intervals). In the CPS and PSID, I subtract one from individuals’ ages at the time they are interviewed (since income questions refer to the previous year). In the SIPP I measure age as of each month referenced by income questions. The age range I use automatically excludes most students and retirees, but in most analyses I conducted sensitivity checks in which I explicitly excluded them.\(^\text{13}\) The results I present are not sensitive to their inclusion.

Two other sample restrictions I make are to exclude occupants of group quarters and those with non-positive incomes. The latter exclusion is a key source of differences across existing research, but CPS data demonstrates that those without income are generally special cases whose circumstances are not centrally about labor markets. In 2008, the primary reason that men went the whole year without working was a sickness or disability, and when students, retirees, and homemakers are added, 81 percent of male non-workers are accounted for. Just 13 percent said that they could not find a job. The figures in 1967 were 88 percent and 5 percent. Among women, half of those who did not work in 2008 were homemakers, and only 4 percent could not find work, versus 91 percent and 1 percent in 1967. Families and households with no or negative income generally include someone with a business who reports a loss for the year, but such circumstances are uncommon.

After dropping non-positive incomes, I trim the top and bottom 2 percent of remaining incomes within a small number of age categories.\(^\text{14}\) This approach mitigates the problem of top-coding and changes in top-coding over time. It does not address top-coding of the various components of aggregate income figures. To do so, I checked the sensitivity of my results using CPS data generously provided by Richard Burkhauser. Burkhauser and his colleagues created annually consistent topcodes for the income component variables using internal restricted-use data at Census Bureau facilities (Larrimore et al., 2008). They then built back up the aggregate income variables. My results did not change appreciably by using these variables.\(^\text{15}\)

Finally, in some years in the SIPP and CPS I checked the robustness of my results by excluding not only those retired or in school, but those who at any time were out of the labor force because they were sick or disabled or taking care of someone at home; those who were in the armed forces; those who experienced a birth in the previous year; and

\(^\text{13}\) As discussed below, excluding adults with no income also screens out retirees and students.

\(^\text{14}\) The categories are ages 20-29, 30-39, 40-49, and 50-59. For family and household income, for the CPS and PSID, the trimming is done only among heads, and other family or household members are then assigned this income (or trimmed out if the head is). In the SIPP, the head may change from month to month, so I conduct the trimming including all adults in the sample.

\(^\text{15}\) I also estimated results when using a 1\% or 4\% trim in the CPS, which affected levels but not trends. Using a 1\% trim in the PSID, however, does sometimes change trends, which is why I use a 2\% trim. Results available upon request.
those who were self-employed. In general, these restrictions had very little effect on
trends and surprisingly little effect on levels.\(^{16}\)

**Imputations**

A particularly important set of sensitivity checks is to see how the results change
depending on how imputed data is treated. The CPS has two types of imputations—item
imputes and whole imputes. The latter involves imputing values for all items for
households that sit through the basic monthly CPS interview but do not participate in the
ASEC supplement.\(^{17}\) The incidence of whole imputation has been fairly constant over
time, but the number of item imputes increased steadily between the 1992 and 2003
surveys, with the overall percent of income imputed rising from just over 20 percent to
nearly 35 percent.\(^{18}\) Imputation rates fell off somewhat thereafter but remained above 30
percent.

Basic monthly CPS non-interviews do not have imputed values in the ASEC supplement;
the ASEC weights incorporate adjustments to account for demographic differences in
who is successfully interviewed. The rate of "type A" non-interviews (non-interviews
that are eligible for the CPS) was stable from 1964 to 1993 but rose from around 4.7
percent to 6.9 percent between 1993 and 1995 and remained around 7 percent thereafter
(U.S. Census Bureau, 2006 and author's analyses). The change coincided with the shift
to computer-assisted interviewing. Combining the non-response with imputation, the
share of households eligible for interviews that have non-imputed income data fell from
around 75 percent in 1991 to around 60 percent in 2003. The higher non-response and
imputation rates in recent CPS surveys translate into an even lower share of my sample
with non-imputed data in both of the two matched years. As I will show below, the
fraction of cases in my sample with household income that includes imputations in one of
the two years soared to nearly 80 percent by 2003 and then fell precipitously.

Imputation is even more common in the SIPP, where roughly 80 percent of cases had at
least one component of household income in at least one month estimated through
imputation. The comparison to the CPS, however, is inapt, since respondents in the SIPP
have 24 opportunities for an imputation (one for each month in both years) compared
with just 4 in the CPS (one for both years). Additionally, there are more individual
income items in the SIPP than in the CPS. Taking into account these differences clarifies
that imputation is a much bigger issue in the CPS than in the SIPP.

For both the CPS and the SIPP estimates, I determined what percentage of each person's
income was imputed. I then produced estimates excluding adults with any imputations
and excluding adults with 75 percent of their income or more imputed.\(^{19}\) I also ran

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\(^{16}\) I exclude non-civilian adults from all CPS analyses as well.

\(^{17}\) I thank Charles Nelson for helping me identify whole imputes (one uses the variable FL-665).

\(^{18}\) Turek et al., 2009; U.S. Census Bureau, 2006

\(^{19}\) In the CPS, whole imputes are not readily distinguishable from item imputes prior to 1988, when
imputation procedures were modified. My calculations for the SIPP take into account not only allocation
flags for individual income components but "type Z", "pseudo-type z", and "little type z" non-respondents
as well as "EPPFLAG imputations" (U.S. Census Bureau, 2001). Little type z and EPPFLAG imputations,
several additional sets of CPS results, excluding cases with whole imputes in either year or with relevant item imputes in either year. For the PSID, where imputation rates are much lower, I simply produced results with and without cases that have imputations.

**Weights**

I use sample weights in all analyses. I select the weight for the last observation in the span of time being considered. That means calendar year weights for the CPS and PSID. For the SIPP estimates, I weight by calendar year weights for the 1996, 2001, and 2008 panels, but only cross-sectional weights are available for other panels. In those cases, I weight by the last month in the span of time being considered. I also confirmed that the 1996-2008 estimates were similar if I used the last month's cross-sectional weight instead of the calendar year weight. The results are relatively insensitive to the choice of weight used. For the August-December and January-March income analyses described below, I use the last-month cross-sectional weights for all panels.

**Measuring Economic Instability**

Broadly speaking, there are two ways of thinking about economic instability. On the one hand, the risk of a large drop in income may engender a "fear of falling" among many families. This form of instability is primarily concerned with sudden changes in living standards that make it difficult or impossible to maintain one's preferred lifestyle.

On the other hand, volatility in income—unpredictability due to the magnitude or frequency of swings up and down—may increase insecurity by the threat of an income drop but also simply by making it difficult to plan and make important decisions related to work, savings, family formation, childrearing, education, and retirement. For a given long-term income level, it matters whether the path of income is free of sharp fluctuations or whether income bounces around wildly from year to year, ultimately netting out.

I focus on the first conceptualization of instability here, examining the probability of experiencing a 25 percent drop in income over a one- or two-year period. This conceptualization, I would argue, is what is mainly of social and political concern. Indeed volatility, conditional on the risk of an income drop, is a good thing, as it means that recovery from drops is common. Furthermore, the question of whether to add an economic security index to the national economic indicators currently centers around a measure similar to mine.

I save for future work a look at the second conceptualization economic instability. Most of the measures that have been used to date require more than two periods of income data for each person, meaning that the CPS cannot be used to produce such measures, and only limited measures are possible using the SIPP (and only starting in the mid-1990s).

which apply to adults for whom there is no information available about whether they worked during the reference period, are not necessarily reflected in item allocation flags. I also tried excluding SIPP adults whose data was provided by a proxy member of their household, but this had little effect on trends or levels.
Interpreting two-period measures of instability—such as those examining the dispersion of income changes, or autocorrelations—is tricky. For instance, greater dispersion of income changes may constitutes greater instability, since it implies that relatively large income changes are becoming more common and relatively small income changes less common. However, it is also true that the dispersion of income changes can increase simply because living standards are steadily rising across the board or because inequality is increasing.  

Other multi-period measures of instability suffer from similar ambiguities. Model-based measures of "transitory variance", on the other hand, are not only dependent on specific model specifications, they measure a quantity that is a statistical construct and not observed or necessarily experienced by actual households from year to year. The key weakness of all these measures is that they cannot distinguish between anticipated or voluntary instability on the one hand and unanticipated or involuntary instability on the other. Measures of transitory variance can only claim to do so under strong model assumptions about "permanent" income and effects of economic shocks experienced by people. This issue remains a topic for future research.

The CPS ASEC Supplement and the PSID include only annual income measures. Only one-year intervals between two interviews are available in the CPS ASEC. The PSID has one-year intervals through 1996 but only two-year intervals thereafter. SIPP panels can be used to look at one-year intervals or, in some panels, two- or three-year intervals. They also include monthly income measures. I aggregate monthly incomes up to calendar year totals. However, newly released data from the 2008 panel of the SIPP allows five-month (August to December) income totals to be compared in 2008 and 2009 (August 2008 being the first month for which income data is available in the 2008 panel). In some analyses, I show trend estimates based on this income measure. These estimates provide a benchmark against which to evaluate Hacker et al.'s projections for 2008 and 2009.

**Income Measures**

I focus on four different income measures below: the wage and salary income of male heads, the earnings of men, women's earnings, and pre-tax post-transfer household income. Wage and salary income is available in the PSID only for "family unit" heads, who by default are men whenever a couple heads the family. In the CPS and PSID, I focus on household heads but confirmed that trends and levels for family heads were similar.

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20 Winship (2009) proposes a measure of "pivot volatility" that examines the frequency and magnitude of individuals' income reversals within a nine-year window. The measure shows a much smaller increase in instability over time than measures of the dispersion of income changes.

21 In the PSID a family unit is defined as "a group of people living together as a family. They are generally related by blood, marriage, or adoption, but unrelated persons can be part of a FU if they are permanently living together and share both income and expenses." [http://psidonline.isr.umich.edu/Guide/FAQ.aspx#90](http://psidonline.isr.umich.edu/Guide/FAQ.aspx#90) There are a small number of households with more than one family unit.
Earnings includes not only wages and salaries, but self-employment income. In the CPS, I focus on the earnings of household heads and spouses, while in the PSID I include long-term cohabiting partners of family unit heads as well. In the SIPP I include earnings of only male household heads or, among women, heads, wives, and cohabiting partners.\footnote{In the CPS, differences between male heads on the one hand and male heads and spouses on the other were trivial. SIPP treatment of business income changed in the 2004 panel, but when I re-ran results different ways to address the change, there was little effect on the results (available from the author on request).}

As alternatives to household income, I conducted sensitivity analyses examining family income and household income adjusted for the number of people in the unit. I also used the Cross-National Equivalent File (a version of the PSID that includes tax estimates generated from NBER's TAXSIM application) to examine post-tax-and-transfer income. In general, pre-tax income, size-adjusted pre-tax income, and post-tax income show similar levels and trends, both for household income and for family income (with family income estimates slightly higher, results available on request).

Finally, using the CPS, I constructed more comprehensive measures of income, adding the value of employer-provided health insurance to wage and salary and labor income and also adding to household income net taxes, net capital gains, the value of Medicaid and Medicare, the value of school meals and food stamps, the value of subsidized housing, and the value of the flow of services from home ownership. In general, the results were largely unaffected.

All incomes are adjusted to constant 2009 dollars. In the PSID and CPS, I use annual PCE deflators, while in the SIPP I use monthly CPI-U-RS adjustors (because monthly PCE deflators are not available in recent years).

**Results**

*Male Head Wage and Salary Income; Male Earnings*

Figure 1a compares trends for the probability of a large (25 percent) one-year drop in wage and salary income among male heads. In all charts, I label pairs of years according to the second (later) year. Lines in the chart are linear interpolations between data points.

Over the 1970s and the first half of the 1980s, the CPS and PSID trends align fairly well, with the CPS estimates consistently higher than those of the PSID. However, the CPS shows increasing instability while the PSID trend over this period is more ambiguous. Beginning in the mid-1980s, the CPS trend clearly departs from the PSID and SIPP estimates and rises steadily. From 1970 to 1996, the risk of a drop in wage and salary income actually falls from 10 percent to 9 percent in the PSID, but in the CPS it rises from 10 percent to 16 percent. From 1986 to 2007, instability in the CPS increases from 13 percent to 19 percent, but the SIPP shows a decline from 12 percent to 9 percent.
Notably, the trends and levels for the PSID and SIPP align very closely for the years where they overlap (1986-1995).

If the PSID and SIPP trends are considered as a single series, the implication is that wage and salary instability has largely followed a cyclical pattern, rising with contractions in the economy and falling during expansions. Instability estimates for 1970 and 2007 are basically the same. This conclusion challenges the common perception that the U.S. economy has undergone a "great risk shift" in recent years.

On the other hand, in the CPS, the risk of an income drop more than doubles over forty years. What accounts for this stark difference? Figure 1b suggests a possible answer, showing rates of imputation for each survey for each pair of years over which an income drop is measured. The high imputation rates in the SIPP and CPS stand out against the low rates in the PSID. Furthermore, from 1988 to 2002, imputation increased dramatically in both the CPS and SIPP, so that roughly 40 percent of heads in my early 2000s samples had wage and salary data that was wholly or partly imputed in one or both of the years over which drops are measured.

There are, however, three potentially important differences between the SIPP and CPS when it comes to imputations. First, in the SIPP, imputation became much less common after 2002, while it declined more slowly in the CPS. Second, all cases in the CPS with imputations consist predominantly of imputed data, while the share of male heads with at least 75 percent of their wages and salary imputed is low in the SIPP—around 10 percent. This difference reflects the fact that in the CPS, wage and salary income is recorded in a single variable, so if this variable is imputed, the entire amount is imputed. On the other hand, in the SIPP, wage and salary income is the sum of two variables, each measured twelve times. Having imputed values in one or both variables in one month or a small number of months is unlikely to translate into a large share of annual wage and salary income being imputed. Third, the share of cases with at least 75 percent of wage and salary income imputed is stable over the SIPP panels and waves, while it rises steadily in the CPS through the early 2000s. All in all, these patterns point to a greater likelihood that whatever bias is present in the estimates in Figure 1a, the SIPP trend is less likely to be biased than the trend for the CPS.

Figure 1c presents the same trends as Figure 1a after excluding men with wage and salary estimates dominated by imputations, and it goes a long way toward reconciling the three surveys' results. When looking only at male heads with less than 75 percent of their data imputed (or, in the PSID, none of it imputed), the trends in instability across the three surveys is completely consistent through the early 2000s. Instability hovered around 10 percent throughout the 1980s and 1990s.

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23 The CPS series has a break in 1987 to reflect the change in imputation procedures.
24 Computing the fraction of cases with imputed incomes prior to the 1976 survey in the CPS is not possible because allocation flags refer to a person's entire family rather than to individual income. The choice of a 75 percent threshold is arbitrary, but excludes only cases that are clearly dominated by imputed data. Among cases excluded based on this criterion, the vast majority have their entire earnings imputed.
From 2002 to 2007, the CPS trend still departs from that in the SIPP. Instability falls notably in the SIPP but not in the CPS. The fall in instability shown in the SIPP is consistent with the cyclical pattern in all three datasets in previous years, however the absence of this decline in the CPS is consistent with the idea of a jobless recovery over the aughts.

Figure 1c also shows three other trends—instability over two years in the PSID and in the SIPP and year-to-year instability in August-to-December wage and salary income in the SIPP. The SIPP trends mirror each other but the August-to-December trend extends to 2009. It shows an increase in instability from 2007 to 2009 that is of similar magnitude to the CPS increase. From 2002 to 2006, instability declines modestly in the PSID, but not nearly as much as in the SIPP, however, the PSID and SIPP data points for the two-year estimates do not overlap much, so it is difficult to compare the two series. Levels are similar for the PSID and the SIPP except in 2006. On the other hand, the between 1998 and 2006, the increase in instability is very similar for the two-year PSID and one-year CPS estimates.

Figures 2a through 2c provide analogous results when expanding the sample to men who are spouses or cohabiting partners of the head and expanding the earnings measure to incorporate self-employment income. The figures generally mirror those for male heads' wage and salary income, with instability levels slightly higher but trends no different.

Female Earnings

Trends in and levels of economic instability across the three surveys are even more consistent for women than for men, once imputations are accounted for. In Figure 3a, the PSID and SIPP results again line up very well, showing a secular decline in instability. The CPS estimates again diverge, showing a small increase over time. Figure 3c, however, shows that when women with imputed earnings are excluded, the three surveys show very similar levels, and all show declines in instability, flattening out in the last 15 years. The flattening out is more apparent in the two-year-drop estimates.

Household Income

Finally we come to household income instability. Figure 4a shows the familiar rise in instability in the CPS, but this time, the PSID also shows an increase. In contrast, the SIPP estimates show little secular change.

Figure 4b displays imputation rates. The SIPP and CPS have remarkably high shares of cases with at least some imputed components. In the SIPP, it approaches 90 percent and nearly reaches 80 percent in the CPS. However, when focusing on cases where 75 percent of income is imputed, the rates are far lower, ranging from 10 to 20 percent in the SIPP and 20 to 40 percent in the CPS. Imputation nevertheless increases over time in the CPS but not in the SIPP.

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25 Interestingly, the increase is larger when the value of employer-provided health insurance is added to earnings. Results available upon request.
Excluding cases dominated by imputation brings the CPS levels closer to the SIPP ones, as shown in Figure 4c, but the CPS estimates still show an increase over time. The PSID estimates also increase over time, particularly when the two-year estimates are incorporated, though they again have the cyclicality that the CPS estimates lack. The SIPP estimates are also cyclical but show no secular increase through 2007. The August to December estimates show a bigger increase from 2007 to 2009 than the CPS estimates do.

Discussion and Conclusion

My review of work to date on economic instability characterized the research as concluding that male earnings instability rose in the 1970s and has remained at those higher levels thereafter and that family and household income instability has risen steadily since the early 1970s. Of the research that specifically looks at the risk of a large short-term earnings or income drop, most studies find increases over time.\(^26\) The exceptions to this conclusion have come from studies using the SIPP, often linked to administrative data on earnings.\(^27\) Researchers have not offered compelling reasons for why the SIPP results differ from the existing CPS and PSID results, but to the extent that they have acknowledged the lack of correspondence, they have not challenged it.

The analyses here suggest that when imputations are addressed adequately, the three major panel household surveys show the same trends in male and female earnings instability through the early 2000s (and through the late 2000s for female earnings). There was little to no secular increase in instability among men, and declining instability among women. The CPS indicates rising instability among men since the early 2000s, but there is no evidence of an increase in the SIPP. Contrary to the CPS estimates, the SIPP ones display the cyclical pattern that shows up in earlier business cycles. The PSID estimates suggest that instability among men may have risen after 2000.

The differences between the three surveys' estimates of male instability after the early 2000s should not be overstated. For all three surveys, whether looking at male heads' wage and salary income or the earnings of male heads and partners, the risk of a 25 percent drop ranged between 8 and 13 percent in 2007. That compares with 7 to 9 percent during the late 1970s and 9 to 10 percent in 1989, comparable points in the business cycle.

In 2009, the estimates for men ranged from 12 to 17 percent. In the CPS, the risk of a large drop was four percentage points higher in 2009 than in 1982, the height of instability in the early 1980s recession. In the SIPP, the risk was no higher in 2009 than at the height of the 1990 or 2001 recessions.

\(^{26}\) Dynan, Elmendorf, and Sichel (2007, 2008); Gosselin (2008); Gosselin and Zimmerman (2008); Hacker and Jacobs (2008); Hacker et al. (2010a, 2010b); and Hertz (2007).

\(^{27}\) Acs, Loprest, and Nichols (2009); Dahl and Schwabish (2009); Dahl, Schwabish, and DeLeire (2008); Dahl, DeLeire, and Mok (2010). Acs et al., however, find an increase in the risk of a 75% income drop between four-month periods separated by a year.
To put these differences in context, we can turn to trends in unemployment. As Figure 5a shows, unemployment tracks the instability estimates fairly closely. Because of this correspondence, the unemployment rate is in many ways a more attractive indicator of male instability than the measures here. Chief among these attractions is that we have unemployment measures going back to 1929. Figure 5b dramatically puts recent instability trends in historical perspective. The Great Recession's impact has been far less than that of the Great Depression, when instability soared. Instability has been higher from roughly 1975 to the present than it was from roughly 1945 to 1965, but not dramatically or consistently so.

Household income instability increases in the CPS and PSID over the past four decades, but it does not increase through 2007 in the SIPP. Once again, the increases in the CPS and PSID are not large: in the CPS, adults had a 12 percent risk of a large income drop in 1989 and a 16 percent risk in 2007. The SIPP shows a more dramatic increase from 2007 to 2009 than the CPS does.

So have male earnings instability and household income instability increased modestly or not at all? There are a number of reasons to favor the SIPP estimates showing no secular change. The "quadrennial" interviewing schedule and month-by-month consideration of income figures is likely to have three important advantages relative to the data collection in annual surveys. First, the problem of the incongruence of the current household composition with that during the reference period is minimized. In the CPS and PSID, the previous year's incomes are reported for the current year's household members (who may or may not have been present in the household in the previous year). In the SIPP, each month's income data is collected for the household members present in that month. Second, respondents do not have to recall incomes more than four months back in time.

And finally, as we have seen, nonresponse and imputed data are much less consequential when they apply to four months than when they apply to twelve months. Dropping incomes that are based heavily on imputation, as I have done, is an imperfect solution in that individuals with such incomes may be systematically different than other sample members. Therefore, it matters how prevalent such cases are. In the CPS, the share of male heads with mostly-imputed wage and salary income data in one of the two years over which instability is measured rises from around 20 percent in the 1970s and 1980s to 40 percent by 2003. The increase in imputation for adults' household incomes is of a similar magnitude. In contrast, just 10 to 20 percent of similar cases in the SIPP involve heavy imputation. When heavily-imputed cases are dropped, the potential for biasing CPS estimates is much greater than for the SIPP, as is the potential for biasing changes in the estimates. This possibility is all the more worrisome given that the CPS does not follow households that move.²⁸

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²⁸ Another reason to be concerned about the CPS estimates is the imprecision of matching records between years. However, the similarity of pre-2000 CPS estimates to the SIPP and PSID estimates, combined with the relatively constant match rates I obtained across years leads me to conclude that this is less of a problem than nonresponse, imputation, and household moves.
Also pointing to the superiority of the SIPP estimates, the consistency of the SIPP trends for male earnings and household income, for annual income comparisons and August-to-December income comparisons, and whether cases dominated by imputation are included or omitted is striking. The SIPP estimates consistently display cyclical patterns that follow the business cycle, unlike the CPS estimates. They do so more consistently than the PSID estimates as well. The PSID's attrition over time and administrative changes call the validity of estimates from the survey into question.

These analyses cast doubt on the conclusions of an ongoing high-profile effort to track economic security using a measure based on the risk of a large income drop. Jacob Hacker and his colleagues report that economic insecurity and instability has been rising steadily since the late 1960s and is currently at a level never before seen over this period. In contrast, I find using the SIPP—contrary to their estimates using the same dataset—that there was no secular increase in income instability from the mid-1980s to the mid-2000s and that the recent increase associated with the recession put instability at a level barely higher than the recessions of 1990 or 2001.

While the evidence retains some ambiguity, the similarity of the PSID and SIPP estimates after addressing imputations suggest that estimates from the two data sets can usefully be combined to produce time series extending from the late 1960s to 2009. Those time series generally refute the idea that economic instability has risen substantially or that there has been a "Great Risk Shift". This conclusion could be solidified if future research is able to show that the departure of the CPS trends is due to survey differences and if it finds similar results using other measures of economic instability.
Bibliography


Winship, Economic Instability


Winship, Economic Instability


Appendix: Matching March CPS Files

Matching individuals' data from one March CPS survey to the following year's is no easy task. Not only is there no complete resource available offering instruction to the data analyst, but various aspects of the data require special handling, and the result is an aggregate file with 1.5 million potential matches. The following discussion draws from Unicon Research Corporation (2006) and U.S. Census Bureau (2009).

To match records, one must first match on year (which, of course, increments by one) and month in sample (which increments by four because the second year's March interview is fourth interview since the sample address's first March interview). Then one matches on address and household identifiers. Unfortunately, the variables needed change over time, the address identifiers are not unique in some years, they are corrupted in a few files, household identifiers are not necessarily unique within addresses, and periodic sample redesigns mean that addresses cannot be matched across some pairs of years.

Finally, one matches individuals within households on their "line number" and other characteristics that uniquely identify them. Line numbers are unavailable in a few years. The more variables one uses to match individuals, the more likely it is that matches will be unique, but the more people will be improperly excluded due to response, transcription, or coding errors in one of their two interviews. The fewer variables one uses, the more matches one will get, but many of them will be non-unique, and some of the unique matches will be improper because they will link two different people who happen to have the same line number and characteristics (particularly problematic when the occupants of an address change).

For the 1968 to 2004 files, the matching variables I used included (with Unicon/CPS ASEC variable names): year (_year/H-YEAR), month in sample (mis/H-MIS), household id (_hhid/H-IDNUM or H-IDNUM1), household number (hhnum/H-HHNUM), line number (lineno/A-LINENO), sex (_male/A-SEX), age (age/A-AGE), and education (grdhi, grdcom, and grdatn/A-HGA and A-HGC). For the 2005 to 2010 files, I also matched on hhid2/H-IDNUM2 for 2005-10 (which includes hhnum/H-HHNUM within it and thus makes keeping that variable in the match redundant). As described below, matching 1973-74, 1975-76, and the files since 2004-05 required additional steps.  

In my choice of demographic variables, I confined the set to sex, age, and education as variables that could be expected to be reasonably consistent across interviews. Age could be as much as two years higher and education as much as one year higher in the second year matched (and no lower in either case). The education restriction from 1991 to 2010 uses the algorithm of Madrian and Lefgren (2000), since the variable is a categorical attainment indicator rather than representing years of schooling. The "looseness" of the

29 Note that the "household" id is actually an identifier for the housing unit, or address. "Household number" is incremented every time a new household resides at the address, with one exception. If an address is deemed a "type b" or "type c" noninterview (essentially, if the unit has become unoccupied, demolished, or converted to nonresidential use), after at least one successful interview, the household number is not incremented once the unit becomes occupied again (Nekarda, 2009).
age and education restrictions means that I include some improper matches of different people with similar education and age levels. I decided against using race as a matching variable because of concern that reporting by multiracial and Hispanic adults might not be consistent, because the race variable in the CPS changed to allow multiple categories to be selected in 2003, and because evidence in Madrian and Lefgren (2000) suggests that it adds little to matching precision beyond sex, age, and education (see their Table 3).

I require unique matches on all three variables (in the notation of Madrian and Lefgren, I use merge criterion $[a|e]$. Among the different criteria they examined, the one that I use was one of the two best at excluding people who indicated they had lived at a different address on March 1 of the previous year and one of the two worst at including people who indicated they lived at the same address. However, all of the roughly three dozen criteria examined included 92 percent or more of non-movers, while just ten excluded 20 percent or more of movers. My merge criterion errs on the side of excluding many matches inappropriately in order to appropriately exclude more matches. Evidence presented by Madrian and Lefgren suggests that the vast majority of people I exclude using my criterion reported that they lived at the same address a year earlier.30

Matching is not possible for five pairs of years. In 1972, the address identifier is incompatible with 1971 (because of changes in sampling procedures) and with 1973 (because the "random cluster" codes changed). Because of changed variable formats, 1976 cannot be matched to 1977. Matching of 1985 and 1986 is not possible due to a change in the sample design, and sample design changes prevent 1995 and 1996 from being matched.

Other pairs of years require special treatment. Many of the address identifiers from the original 1973-1975 Census tapes are corrupted, with occasional blanks in the middle of observations' values.31 While the 1974-75 match was nevertheless as successful as in other years, very few cases matched across 1973-74 or 1975-76. Therefore, after the initial match attempt, I conducted a second round of matching to link additional records from 1973 to 1974 and from 1975 to 1976. This time I matched on the two non-corrupted parts of the identifier—segment number (segnum) and serial number (sernum) rather than _hhid.

The 1975-76 match is trickier still. There is no line number in 1976, so the match must omit this variable. In addition, the household identifiers for the 1975 records must be recoded—the third-to-last digit of _hhid must be cut.32

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30 I also ensured that my CPS results were not sensitive to matching on values of sex, age, or educational attainment that were imputed in the basic monthly survey due to nonresponse.
31 To my knowledge, this is the first time this problem has been discovered. I confirmed that it exists in the Unicon data, the CPS files on NBER's website, and the files on ICPSR's website.
32 When using the Unicon data, hhid must also be substituted for _hhid in the 1976 records to match 1975 to 1976 because Unicon codes _hhid as hseq in 1976. Also, the Unicon data requires that, before conducting the secondary match, the segment number variable for 1975 be replaced by extracting a four-digit version of the segment number from _hhid. I do not know whether the same replacement is required when using the original CPS files. Between 1992 and 2010, the _hhid variable has leading zeroes in some years but not in others, so it must be coded consistently for matching. To match 1993 to 1994, the state
Line numbers are also missing from 1977 and 1978, so the match for this pair of years takes place without them.

Finally, due to the ASEC sample expansion in 2001 to improve estimates of low-income children without health insurance, many sample members from 2001 to 2010 are not actually interviewed in March and are not administered the ASEC in consecutive years. Their inclusion in the ASEC files creates additional non-matches and mismatches compared with earlier years. Feng (2008) notes, however, that these sample members may be excluded by matching the ASEC records to March basic monthly CPS records (those sample members who receive the basic CPS interview in March, prior to receiving the ASEC supplement). This match is straightforward for 2005-10, and I do so prior to matching ASEC supplements. I used basic monthly survey files extracted using the Census Bureau's DataFERRET software, matching to ASEC files on year, mis, _hhid, hhid2, and lineno. For 2001-04, matching the ASEC and basic monthly files appears to be impractical due to the absence of the hhid2 variable (which incorporates not only hhnum but the sample ID and serial suffix codes that are available as separate variables in the basic monthly file).

Table A1 provides matching rates and totals for each pair of years. My final sample of uniquely matched records—non-unique matches are dropped—includes nearly 890,000 adults between the ages of 21 and 60, or about 24,000 per year on average. Across years, I match an average of 62 percent of those adults who potentially could be matched. Changes in residence, attrition, and deaths make up part of the difference.

The accuracy of this and previous matching attempts could be improved by identifying errors in the March data using a process outlined by Feng (2004). The idea is to compare variable values in one month to the sample members' responses in other months. If, for instance, someone reported a different sex in the January, February, and April basic monthly surveys than in March, one would infer that the March value is an error and recode it. Whether this would profitably improve the accuracy of the matching results here, however, is questionable.

variable must be appended to _hhid in 1993, since Unicon appends it to 1994 for _hhid to produce more unique address identifiers. Presumably, the user of the original CPS files should also match on state for 1993-94.
### Table A1. Summary of Matching Process, ASEC Current Population Survey Files

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<th>Year</th>
<th>Base Sample</th>
<th>Minus non-March BMS Sample</th>
<th>Matched on</th>
<th>Address &amp; Line No. of Files on Base Minus non-Year 2 Addresses &amp; Plus Uniquely on Plus Line No., Uniquely on Plus Secndry</th>
<th>% Uniquely Matched&lt;sup&gt;a&lt;/sup&gt;</th>
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**Total**\(^b\) 1,578,075 182,739 1,505,439 1,047,162 981,669 907,244 874,503 873,579 43,793 61.0

**Average**\(^b\) 42,651 36,548 40,688 28,302 26,532 24,520 23,635 23,610 21,897 61.9

Notes: See Appendix for details. \(^a\)Percent matched is the number uniquely matched divided by the starting observations in the year with the smallest starting sample. \(^b\)Total and Average are based on the 37 pairs of years retained (excluding 1971-72, 1972-73, 1976-77, 1985-86, and 1995-96).
Figure 1a. Percent of Male Heads Experiencing One-Year Declines in Wage and Salary Income

Figure 1b. Percent of Male Heads with Imputations for Wage and Salary Income in the Year or the Previous Year
Figure 1c. Percent of Male Heads Experiencing Declines in Wage and Salary Income, Excluding Any with Imputations

CPS-Exclude Any with 75%+ Imputed in Either Year
PSID-Exclude Any with Imputations in Either Year
SIPP-Exclude Any with 75%+ Imputed in Either Year
PSID- 2-Year Estimates
SIPP- 2-Year Estimates
SIPP- August to December

Figure 2a. Percent of Men Experiencing One-Year Declines in Earnings

CPS  PSID  SIPP
Figure 2b. Percent of Men with Imputations for Earnings in the Year or the Previous Year

Figure 2c. Percent of Men Experiencing Declines in Earnings, Excluding Any with Imputations
Figure 3a. Percent of Women Experiencing One-Year Declines in Earnings

Figure 3b. Percent of Women with Imputations for Earnings in the Year or the Previous Year
Figure 3c. Percent of Women Experiencing Declines in Earnings, Excluding Any with Imputations

Figure 4a. Percent of Adults Experiencing One-Year Declines in Household Income
Figure 4b. Percent of Adults with Imputations for Household Income in the Year or the Previous Year

Figure 4c. Percent of Adults Experiencing Declines in Household Income, Excluding Any with Imputations
Figure 5a. Percent of Male Heads Experiencing One-Year Declines in Wage and Salary Income

Figure 5b. Economic Instability in Historical Perspective