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Trends in earnings inequality and earnings instability among U.S. couples: How important is assortative matching?[☆]

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ABSTRACT

We examine changes in inequality and instability of the combined earnings of married couples over the 1980–2009 period using Social Security earnings data matched to Survey of Income and Program Participation panels. Relative to male earnings inequality, the inequality of couples' earnings is both lower in levels and rises by a smaller amount. We also find that couples' earnings instability is lower in levels compared to male earnings instability and actually declines in these data. While wives' earnings played an important role in dampening the rise in inequality and year-to-year variation in resources at the family level, we find that marital sorting and coordination of labor supply decisions at the family level played a minor role. Comparing actual couples to randomly paired simulated couples, we find very similar trends in earnings inequality and instability.

1. Introduction

The U.S. labor market experienced a tremendous rise in male earnings inequality over the past four decades.[1] Not only did cross-sectional earnings inequality increase, over the early part of this period the within-person variability of earnings increased as well.[2] The same period saw a large increase in employment and earnings of women, with particularly dramatic changes for married women. These concurrent trends raise the question of the extent to which changes in wives' earnings contributed to growth in the inequality and instability of family earnings. Positive assortative matching is one reason to think it might, and so a related question is whether positive assortative matching of couples has increased and has contributed to the rise in family earnings inequality.

A number of papers have examined these questions. Cancian et al. (1993), Cancian and Reed (1998), Hyslop (2001), Devereux (2004), and Pencavel (2006) find that wives' earnings have had an equalizing impact on the distribution of family earnings. Pencavel (2006) and Hyslop (2001) additionally consider the role of positive assortative matching, with Pencavel (2006) finding that the covariance of husbands' and wives' earnings did not contribute much to the rise in family earnings inequality while Hyslop (2001) finds it had a somewhat larger role. Recent papers by Eika et al. (2014) and Greenwood et al. (2015), which focus on couples matching on education, also reach the conclusion that positive assortative matching played a minor role in the rise in household income inequality.

In this paper we examine these two questions—the impact of wives' earnings on couples' earnings inequality, and the contribution of positive assortative matching—considering both the level and the rise in couples' earnings inequality. We do so using the Survey of Income and Program Participation linked to Social Security earnings records (SIPP-SSA). Our paper makes two primary contributions. The first is to provide evidence on family earnings dynamics based on administrative earnings records, in keeping with recent papers in the literature on individual earnings inequality that use administrative data sets to reconsider earlier findings based on survey data (Daly et al., 2016; Guvenen et al., 2014; Kopczuk et al., 2010; Sabelhaus and Song, 2010).

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[1] See, for example, survey articles Autor and Katz (1999) and Autor et al. (2008). More recent papers documenting inequality trends include Blundell et al. (2008), and Heathcote et al. (2010a).

[2] Gottschalk and Moffitt (1994) first documented the rise in the within-person variability of earnings, referred to in the literature as "earnings instability." Other papers using alternative data sets and methods generally confirmed Gottschalk and Moffitt (1994)'s basic findings: earnings instability increased dramatically during the 1970s and reached a peak during the 1982 recession but since that period stabilized to the level observed prior to 1982—see, for example, Cameron and Tracy (1998), Haider (2001), Kopczuk et al. (2010) and Dahl et al. (2008). However Dynan et al. (2012) and Shin and Solon (2011) find that earnings instability rose in the PSID in the 1990s and the 2000s.

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Our second contribution is to bring to bear a simple intuitive method based on resampling to investigate the role of covariance of couples' earnings. Earnings of spouses may be positively correlated because of positive assortative matching on characteristics such as education and age (Mare, 1991; Pencavel, 1988). Earnings of spouses may also covary due to coordinated labor supply decisions. For example, an increase in husband's wage may reduce wive's hours if there is a large income effect. Families may also have one spouse specialize in the market and the other in the home when young children are present, if time at home for husband and wife are substitutes at this stage of the life cycle (Lundberg, 1988).

Wives may also increase labor supply temporarily to compensate for husbands' job loss—a pattern known as the "added worker effect"(Lundberg, 1985; Stephens, 2002). Such adjustments imply a negative correlation between husbands' and wives' earnings that may affect both transitory and permanent variances.

To gauge the importance of matching and joint labor supply decisions, we build counterfactual earnings inequality and instability measures by drawing random matches of married men and married women and constructing the same measures using their combined earnings. If earnings inequality and instability measures for the randomly rematched couples differ substantially from those of actual couples, this would point to an important role for matching and/or joint labor supply decisions.

Our findings are as follows. Inequality in the combined earnings of couples is lower than inequality of husbands' earnings, and grew at a slower rate, indicating that wives' earnings had an equalizing impact on both the level and growth of family earnings inequality. Similarly, earnings instability is lower for couples and actually fell over time in the SIPP-SSA data while husbands' earnings instability rose slightly. We find that coordination of spouses' labor supply decisions and positive assortative matching on net played a minimal role in determining overall earnings inequality and earnings instability among couples. We find similar trends for actual and simulated couples, suggesting that who is married to whom is relatively unimportant for the evolution of couples' inequality and instability in the U.S.

Our findings on the equalizing impact of wives' earnings is similar to Cancian and Reed (1998), Devereux (2004), and Pencavel (2006) who study cross-sectional earnings inequality. Our panel data, however, allow us to examine earnings instability as well as the inequality of permanent earnings. The minor role we attribute to the covariance of couples' earnings in explaining inequality growth is in line with Pencavel (2006). Our conclusion differs somewhat from Hyslop (2001) who finds a larger role of covariance of earnings. One important way in which our analysis differs from Hyslop (2001) is that we base our findings on a more inclusive sample—rather than selecting on couples who are continuously employed, we require that husbands be continuously employed but include couples whether or not wives have positive earnings. When we select on continuously working couples to follow Hyslop (2001), we similarly find that the covariance of couples' earnings plays a larger role. This suggests that an important reason for the low correlation of couples' earnings is wives' entry and exit decisions.

These results refer to the net effect of positive assortative matching and offsetting labor supply. Our paper also attempts to disentangle the two effects. Consistent with Eika et al. (2014) and Greenwood et al. (2015), we find that positive assortative matching based on observable characteristics such as education and age contributed little to couples' earnings inequality growth. While it is difficult to distinguish between the effects of changes in offsetting labor supply and changes in positive assortative matching on unobservable characteristics in our data, some further analysis using wages in the PSID suggests that sorting, even including unobservables, played a relatively minor role for couples' earnings inequality growth.

The rest of the paper is structured as follows. Section 2 discusses the methodology while Section 3 describes our data set and samples used. Section 4 describes earnings inequality and instability trends for individuals and couples. Section 5 compares inequality and instability measures across actual and simulated couples to examine the importance of spousal matching and family labor supply decisions. Section 6 examines the robustness of our results by applying the same methods to an alternative data set, using different inequality measures, altering our sample restrictions, and using additional background variables to check whether there is substantial assortative matching on characteristics other than age and education. Section 7 summarizes our findings.

2. Methodology

To help describe our basic approach, we begin with the following statistical model:

$$\log y_{it} = X_{it}'\beta_t + \epsilon_{it}$$
$$\epsilon_{it} = p_t^{\mu}\mu_{it} + p_t^{v}v_{it}, \tag{1}$$

where $\log y_{it}$ denotes individual i's log annual earnings and X_{it} denotes observed characteristics. Residual earnings, ϵ_{it}, are assumed to consist of a permanent component, μ_{it}, and a transitory component, v_{it}, which is assumed to be independent of μ_{it}. The term p_t^{μ} represents factor-loading on the person-specific permanent component, such as time-varying returns to individual skills or human capital. Similarly, the term p_t^{v} reflects factor-loading on the person-specific transitory component. The transitory component, v_{it}, may comprise purely transitory i.i.d. shocks and/or a (short-lived) serially correlated transitory process. The permanent component, μ_{it}, may comprise a factor that is completely fixed and/or the cumulated effects of long-lived shocks.[3]

In the data, much of the variation in individual earnings is due to the variation in ϵ_{it}. Understanding the cross-sectional variation of ϵ_{it} is, therefore, important for understanding the cross-sectional variation of earnings, $\log y_{it}$. In the following, we refer to the cross-sectional variance of residual earnings, ϵ_{it}, as "earnings inequality." We run a pooled regression of individual log earnings on year dummies to control for aggregate trends in earnings, and a polynomial in age to control for predictable life-cycle effects. Our measure of inequality, therefore, will reflect earnings inequality due to idiosyncratic individual labor market shocks as well as earnings inequality due to differential returns to observable characteristics among individuals of the same age.

To gauge the importance of permanent versus transitory components of earnings inequality we follow the methodology of Kopczuk et al. (2010). In particular, we average ϵ_{it} over a five-year window and denote that average as $\bar{\epsilon}_{it} = \sum_{j=t-2}^{j=t+2} \epsilon_{ij}$. As in Kopczuk et al. (2010), we refer to the cross-sectional variance of $\bar{\epsilon}_{it}$, $\text{var}^i(\bar{\epsilon}_{it})$, as the "permanent variance" at time t, and the cross-sectional variance of $\epsilon_{it} - \bar{\epsilon}_{it}$ as the "transitory variance" at t. To interpret the measures, consider the case when μ_{it} is a time-invariant person-specific effect μ_i, the factor-loadings p_t^{μ} and p_t^{v} are constant, and v_{it} is an i.i.d. shock. The variance of $\bar{\epsilon}_{it}$ will then come close to the variance of the permanent component, μ_i, provided that a five-year average of the transitory shocks v_{it} has negligible variance. In a more general case, when the permanent component is modeled as a random walk or a highly persistent process, the variance of $\epsilon_{it} - \bar{\epsilon}_{it}$ may contain the contribution of both permanent and transitory shocks, as also noted by Kopczuk et al. (2010). However, $\bar{\epsilon}_{it}$ will put a larger weight on shocks to the permanent component, more so if the averaging window is larger.[4] In general, events

[3] The permanent component captures both idiosyncratic earnings differences due to time-invariant factors such as formal education, and/or time-varying personal attributes that affect individual earnings for an extended period of time (e.g., match effects that may vary due to firm-specific productivity shocks). The permanent component is normally modeled as a person-specific fixed effect (i.e. $\mu_{it} = \mu_i$ for all periods), or, more generally, as a sum of the fixed effect ($\tilde{\mu}_i$) and a highly persistent component (e.g., a random walk: $\mu_{it} = \mu_{it-1} + \xi_{it}$).

[4] Note, however, that there is a tradeoff in selecting a wider window—the wider window will be more informative on the rise of inequality due to permanent or more persistent shocks but it also entails selecting a sample of more stable couples which is likely to be less representative of the overall population of U.S. families.

such as brief unemployment spells, overtime and bonuses will contribute mostly to our measure of the transitory variance whereas changes in earnings due to job mobility, job displacement, disability, and changes in skill prices will be mostly reflected in our measure of the permanent variance. We should add that the estimated variances do not necessarily reflect risk alone, as some of the couples' earnings changes are likely anticipated and some are insured, e.g., through inter-family transfers.

We apply the same statistical model to couples indexed by c:

$$\log y_{ct} = X'_{ct}\beta_t + \epsilon_{ct}$$
$$\epsilon_{ct} = p^{\mu}_t \mu_{ct} + p^{v}_t v_{ct}. \tag{2}$$

The residual variance of couples' earnings will reflect the variances of the husband's and wife's permanent and transitory components as well as the covariances between their permanent and transitory components. We run a regression of log of couples' earnings, $\log y_{ct}$, on a polynomial in husband's age and year dummies. Our residual, ϵ_{ct}, is therefore a combination of residual earnings of the husband and wife. As with our previous measures, we use the variance of ϵ_{ct} averaged over a five-year window to measure the permanent variance of couples' earnings, while the variance of $\epsilon_{ct} - \bar{\epsilon}_{ct}$ measures the transitory variance at time t.

The permanent variance of couples' earnings, call it $\psi_{\bar{\epsilon}_{ct}}$, will include the effects of combining income draws from the husbands' and wives' individual earnings distributions as well as the effects of spousal matching and coordinated labor supply. We expect positive assortative matching to raise inequality. In contrast, coordinated labor supply is expected to reduce inequality as women may increase work to compensate for husband's job loss or specialize in home production while husbands specialize in working outside the home. To gauge the importance of matching and joint labor supply decisions, we build counterfactual permanent variances by drawing random matches of married men and married women within each five–year window and constructing the same measures using their combined earnings. Using this method, we effectively set the covariance of husbands' and wives' earnings to zero. We refer to these rematched couples as "unconditionally swapped." The difference between inequality measures of actual couples and those of unconditionally swapped couples will indicate the combined importance of couple-specific matching and joint labor supply behavior.

To try to separate the effects of joint labor supply decisions from marital sorting, we build a second counterfactual permanent variance by grouping couples based on the ages and educations of the husband and wife, (in addition to year) and randomly matching couples within those groups.[5] We refer to these rematched couples as "conditionally swapped." If education and age fully captured the positive assortative matching of spouses, the difference between conditionally swapped and unconditionally swapped couples would reflect assortative matching, and we would expect the conditionally swapped couples' earnings variance to be higher than the unconditionally swapped couples' earnings variance. Under the same assumption, the difference between the actual variance and the conditionally swapped counterfactual variance of couples' earnings would isolate the role of offsetting joint labor supply, and we would expect actual couples' earnings variance to be lower than the conditionally swapped couples' earnings variance.

We construct analogous measures for the transitory variance which, under the same conditions, would allow us to similarly consider the roles of matching and joint labor supply on earnings instability. We explore the extent to which education and age adequately capture assortative matching in Section 6.

3. SIPP-SSA matched data

3.1. Base sample

The SIPP is a series of nationally representative U.S. panel data sets, with sample sizes ranging from about 14,000 to 52,000 households per panel. Each of the panels we use collects information in 8–16 four-month waves. Our sample of individuals is drawn from respondents to the 1984, 1990–1993, 1996, 2001, 2004, and 2008 SIPP panels who provided the information needed to validate matches to Social Security Administration (SSA) earnings records. For these individuals, we have annual earnings for 1978–2011 based on summaries of earnings on jobs recorded in SSA's Master Earnings File (MEF). The primary source of the MEF earnings information is W-2 records, but self-employment earnings are also included. We include employees' contributions to deferred compensation plans as part of our earnings measure. We obtain marital histories and educational attainment from data collected in the SIPP. Age and gender are based on administrative records from SSA sources.[6]

We present estimates of earnings inequality among men as a point of comparison for our findings on couples, so here we describe our selection rules for both groups:

Men

Our base sample includes all matched male SIPP respondents in any years in which they are aged 25–59. While detailed survey information on employment and earnings is collected for each individual only over the relatively short window of their SIPP panel, from the administrative records we have annual earnings for each year between 1978 and 2011. Thus for someone who was 50 when interviewed in the 1990 SIPP panel, we use earnings for 1978–1999, while for someone who was 20 in 1990 we use earnings for 1995–2011. In total we have about 1.8 million observations from our 5-year earnings panels for this sample, or on average about 63,000 observations per year.

Couples

Our primary analysis focuses on couples and so conditions on marital status. We only use earnings for husbands and wives in years in which we can determine whether or not they are married to each other and the husband is aged 25–59. Marital histories collected in the second wave of each SIPP panel, along with updates from changes in later waves, give us marital status information for years leading up to and during the SIPP panel in which a couple was sampled. While we have earnings for years after the earlier panels are over, we do not know marital status for those years. Thus when we condition on marital status we have much smaller samples at the end of our period than at the beginning because in later years we can only use the most recent panel(s). For example, in 2004–2007 we can only identify married couples if they are members of the

[5] More precisely, we define 12 education classifications for the couple based on cross-classification of five education groups for the husbands and wives (less than high school, high school graduate, some college, bachelor's degree, advanced degree). We collapse smaller off-diagonal cells to reduce the 20 off-diagonal cells to 7: 1) one spouse is a high-school graduate and the other did not finish high school; 2) women with at most a high-school education married to men who have at least some college; 3) women with a college degree married to men with at most some college; 4) women with an advanced degree married to men with a bachelor's degree; 5) men with an advanced degree married to women with a bachelor's degree; 6) women with some college married to men with at most a high-school education; 7) women with some college married to men with at least a bachelor's degree. We further define 3 age groups for husbands (25–34, 35–44, 45–59), and 3 relative-age groups for wives: 1) wife is 3 or more years younger than husband; 2) wife is very close in age to husband (2 years younger-1 year older); 3) wife is more than 1 year older than husband. Overall, this results in 108 (12 × 9) groups for each year. We do not have current state of residence except during a sample couple's SIPP panel, so we are limited to using age, education, and year to do the rematching. We create 400 such simulations. For each simulation, we take the set of husbands from couples meeting the selection criteria and randomly assign a pseudo-wife by sampling from among the wives from the available set of couples. For the conditional resampling, we stratify as described above.

[6] The results presented here are based on confidential data from Version 6.0 of the SIPP Gold Standard File. Because our sample pools data from several SIPP panel samples we do not use SIPP survey weights in our analysis, so the results cannot be assumed to be nationally representative. External researchers can access related data through the public-use SIPP Synthetic Beta (SSB) files, and Census will validate results obtained from the SSB on the internal, confidential version of these data (the Completed Gold Standard Files). For more information, please visit https://www.census.gov/programs-surveys/sipp/guidance/sipp-synthetic-beta-data-product.html. The U.S. Census Bureau also supports external researchers' use of some of these data through the Research Data Center network (www.census.gov/ces/rdcresearch).

Table 1
Sample means, base and current samples, SIPP-SSA.

Base sample	Men	Husbands	Wives	Couples
Age	41.1	42.4	40.4	
Education shares				
High school or less	0.406	0.355	0.382	
Some college	0.310	0.317	0.332	
College graduates	0.284	0.328	0.286	
Earnings ($2014)	58,428	63,730	24,435	88,165
Log earnings[a]	10.743	10.853	–	11.215
Number of observations	1,882,100	721,100	721,100	721,100
Number of persons	131,300	73,600	73,600	73,600
Current sample				
Age	41.0	42.4	40.4	
Share married	0.732	1.00	1.00	1.00
Education shares				
High school or less	0.397	0.392	0.422	
Some college	0.304	0.297	0.311	
College graduates	0.299	0.311	0.268	
Earnings ($2014)	58,081	63,124	24,397	87,521
Log earnings[a]	10.741	10.843	–	11.207
Number of person/years	325,800	210,900	210,900	210,900
Number of persons	84,000	55,900	55,900	55,900

Notes: Number of observations is the total count of observations in the 5-year windows used in our estimates, rounded to the nearest 100. For comparability, means are calculated within each year, and then averaged over the years in common to both current and base samples (i.e., we exclude 1980, 1985, and 1986 in calculations). [a]Mean log earnings are not defined for wives since zero wife earnings in levels are allowed.

2004 or 2008 SIPP panels, while in 1978 we can in principle use data on any matched person born between 1919 and 1953 from any of our SIPP panels as long as they provided a marital history. In total, we have about 721,100 observations from 5-year earnings panels for this sample, with the number of couples contributing to estimates for a particular year ranging from roughly 7500 to 37,000.

Our estimates for couples require linked earnings histories for both members of a couple, which are available only if they are members of the same household in the second wave of their SIPP panel. For a sample member who had a marriage that ended before the panel started, we have earnings for that sample member and know in which prior years they were married, but we do not have any information on their former spouse. This also means that long-duration marriages will tend to be over-represented in our base sample.

3.2. Current sample

To examine the sensitivity of our results to this over-representation of long-duration marriages, we also estimate trends in couples' earnings inequality and instability using a subset of our base sample in which we include only couples from the most current SIPP panel(s) and earnings data only from years during the panel and the five years leading up to the start of the panel. Given our use of five-year averages in defining inequality and instability, a five-year earnings history is already a minimum requirement. In discussion of our results we refer to this as the current sample. In total, we have about 325,800 observations from 5-year earnings panels for this sample, with the number of couples contributing to estimates for a particular year ranging from roughly 2100 to 16,000.

The top panel of Table 1 presents summary statistics for our base samples and the bottom panel presents statistics for the current sample. The timing of the SIPP panels means that the current sample does not provide estimates for 5-year panels centered on years 1980 and 1985–1986. Comparison of the means shows that the two samples are similar. In Section 6 we present estimates that show they also produce very similar estimates of inequality and instability.

3.3. Earnings samples

We make the following additional sample restrictions in constructing our estimates of both inequality of permanent earnings and earnings

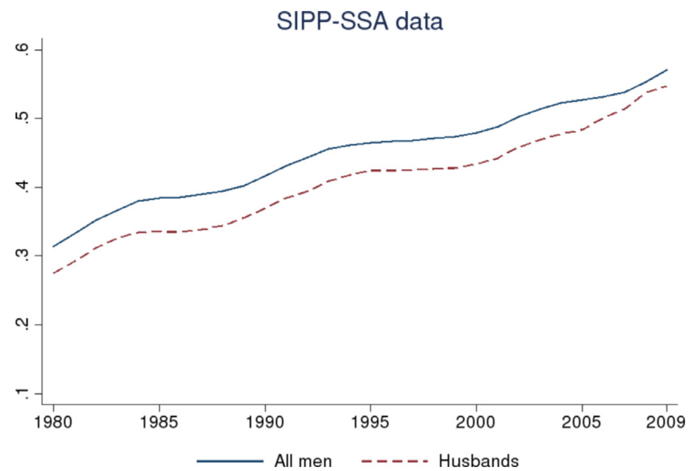

Fig. 1. Permanent variance for men, SIPP-SSA.

instability. For our primary analyses, we select men who have non-zero earnings. We minimize the effect of outliers by excluding the bottom and top 1% of earnings observations. Men in this sample have to satisfy the age and outlier conditions for all years of the window in question—so for the five-year window surrounding year *t*, men must satisfy the above conditions for the 2 years before and after *t*, as well as for year *t* itself. For our couples' samples, we begin with husbands who satisfy the above conditions. We further require that couples be continuously married to each other over the relevant window. Since our focus is on wives' contributions to couples' earnings through both wages and labor supply, we include wives who have zero earnings. This restriction is typically used in models that assume full-time working males and females making labor supply decisions at both the extensive and intensive margins—see, for example, Attanasio et al. (2008) and Heathcote et al. (2010b). We explore sensitivity of our results to alternative restrictions on female and male labor supply in Section 6.

4. Trends in earnings inequality and instability

4.1. Earnings of men and husbands

Fig. 1 shows the well-documented rise in the permanent variance of male earnings. The figure reports the variance of 5-year averages centered on years 1980 through 2009, based on earnings data from years 1978–2011. The variance increased by 82% over the period 1980–2009 for all men and almost doubled for husbands (see top panel of Table 2). Estimates for all men are useful as a check for consistency with others' findings, but trends among husbands are of particular interest here because their earnings directly contribute to couples' earnings.

Fig. 2 shows transitory earnings variance among all men and among husbands. There is cyclical variation in male earnings instability in the SIPP-SSA data but little trend. Over the period 1980–2009, earnings instability rose by only 3.5% for all men and by 13.9% among husbands (see bottom panel of Table 2). Similar patterns are reported by Kopczuk et al. (2010) and Dahl et al. (2008) who also use SSA earnings data. However Dynan et al. (2012) and Shin and Solon (2011) find that earnings instability rose in the PSID in the 1990s and the 2000s.[7]

4.2. Couples' earnings

How do inequality and instability of couples' earnings compare to those for husbands? The top panel of Table 2 gives a comparison of

[7] Comparing across four data sets (including the SIPP), Celik et al. (2012) find that the recent rise in volatility found in the PSID is somewhat anomalous. Monti and Gathright (2013), on the other hand, find an increase in transitory earnings variation for men in the 1990s based on SIPP-collected earnings data, but a decrease for the same individuals when using the linked SIPP-SSA earnings data for the same years.

Table 2
Variances of log earnings, base sample, SIPP-SSA.

	1980	1990	2000	2009	Δ (2009–1980), %
	Permanent earnings (5-year averages)				
Men	0.314	0.416	0.479	0.571	82.1
	[0.307,0.319]	[0.410,0.422]	[0.472,0.485]	[0.563,0.579]	[77.9,86.4]
Husbands	0.275	0.369	0.434	0.547	99.0
	[0.268,0.282]	[0.362,0.376]	[0.423,0.444]	[0.525,0.568]	[90.0,108.3]
Couples	0.245	0.302	0.347	0.415	69.8
	[0.239,0.251]	[0.296,0.308]	[0.338,0.356]	[0.400,0.432]	[62.6,78.0]
	Transitory earnings (deviations from 5-year averages)				
Men	0.120	0.115	0.111	0.124	3.5
	[0.117,0.122]	[0.113,0.117]	[0.109,0.113]	[0.121,0.126]	[0.4,6.5]
Husbands	0.103	0.098	0.090	0.117	13.9
	[0.100,0.106]	[0.095,0.101]	[0.087,0.094]	[0.109,0.125]	[5.7,22.6]
Couples	0.077	0.058	0.051	0.058	–24.3
	[0.074,0.079]	[0.057,0.060]	[0.049,0.054]	[0.054,0.063]	[–30.5,–18.3]

Notes: 95% confidence intervals are based on 400 bootstrapped samples. To construct a 95% confidence interval, we use the 10th and 390th order statistics of the bootstrapped distributions. For the column reporting percent changes, we take the percentage change between 1980 and 2009 for each simulation, and then use percentiles from the distribution of changes as the bounds on the confidence intervals.

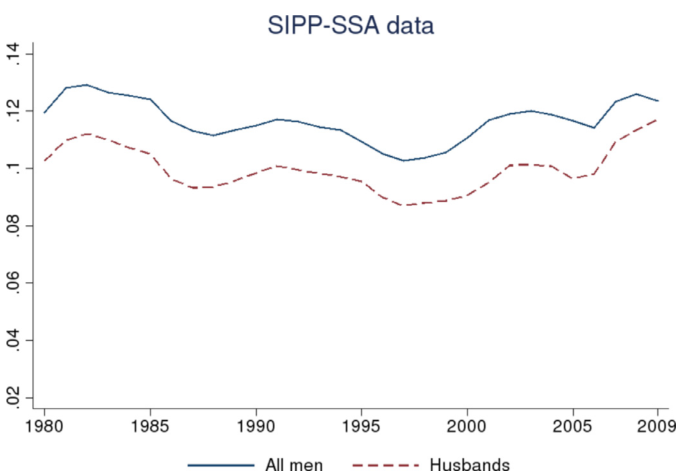

Fig. 2. Transitory variance for men, SIPP-SSA.

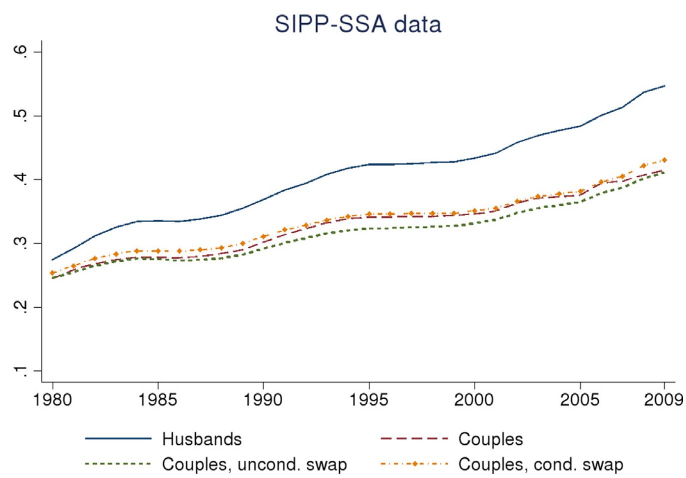

Fig. 3. Permanent variance for actual and rematched couples, SIPP-SSA.

the trends in the permanent variances. Inequality of couples' earnings is both lower in levels and has increased by a smaller amount than inequality for men. Over the period of our analysis, the permanent variance of couples' earnings rose by about 70% while the permanent variance of husbands' earnings almost doubled.

The trend in husbands' earnings inequality provides one benchmark for considering how important wives' earnings are in determining trends in inequality: if either wives had zero earnings so their share was always zero or if wives' earnings were not zero but perfectly positively correlated with husbands' earnings—for example, their earnings were a constant multiple of husband's earnings in each year and for each couple—couples' and husbands' inequality would coincide.[8]

The bottom panel of Table 2 presents estimates of the instability of couples' combined earnings along with instability of husbands' earnings. Couples' earnings have lower levels of instability than the earnings of husbands alone. The SIPP-SSA estimates suggest that instability of couples earnings has actually *fallen* since 1980.

Comparison of male earnings with couples' earnings suggests that wives have played a significant role not only in mitigating the rise of permanent earnings inequality but also in smoothing over earnings instability at the family level. In the next section we explore to what extent coordinated labor supply decisions and positive assortative matching contributed to, or possibly hindered, this outcome.

5. The impact of coordination and matching on couples' earnings inequality and instability

To gauge the importance of marital sorting and coordination, we now turn to comparing earnings instability and inequality measures for actual couples to our counterfactual estimates based on randomly matched couples as described in Section 2.

Our unconditional matching exercise, which sets the correlation between spousal earnings to zero, provides another benchmark for evaluating the importance of wives' earnings for the trends in couples' earnings inequality. This benchmark incorporates the potentially equalizing effects of wives' earnings on couples' earnings inequality because wives may have nonzero earnings but it zeros out the effects of coordinated labor supply and assortative matching that may contribute to shaping the trends in couples' earnings inequality. As we will show below, the correlation of spousal earnings is positive but far from perfect. As a result, the actual variance of couples' earnings lies somewhere in-between the variance of husbands' earnings and the variance of the unconditionally matched couples' earnings but is far closer to the latter.

Trends in variances

Fig. 3 illustrates the permanent variance of actual couples' earnings along with estimates based on the combined earnings of rematched

[8] This can be readily seen for the variance of log earnings by letting husbands' share in total earnings equal s for each couple c and year t. Then $\text{var}[\log y_{ct}] = \text{var}[\log(y^m_{ct}/s)] = \text{var}[\log(y^m_{ct})]$.

Table 3
Variances of log earnings for rematched couples, base sample, SIPP-SSA.

	1980	1990	2000	2009	Δ (2009–1980), %
	Permanent earnings (5-year averages)				
Couples	0.245	0.302	0.347	0.415	69.8
	[0.239,0.251]	[0.296,0.308]	[0.338,0.356]	[0.400,0.432]	[62.6,78.0]
Couples, cond. swap	0.253	0.311	0.351	0.430	69.9
	[0.248,0.259]	[0.304,0.318]	[0.342,0.360]	[0.414,0.447]	[62.9,78.1]
Couples, uncond. swap	0.246	0.292	0.331	0.411	66.9
	[0.240,0.251]	[0.286,0.298]	[0.323,0.340]	[0.393,0.428]	[59.3,74.5]
	Transitory Earnings (deviations from 5-year averages)				
Couples	0.077	0.058	0.051	0.058	−24.3
	[0.074,0.079]	[0.057,0.060]	[0.049,0.054]	[0.054,0.063]	[−30.5,−18.3]
Couples, cond. swap	0.077	0.059	0.052	0.063	−18.7
	[0.075,0.080]	[0.058,0.061]	[0.050,0.054]	[0.059,0.067]	[−24.9,−12.1]
Couples, uncond. swap	0.078	0.060	0.054	0.064	−17.8
	[0.076,0.080]	[0.059,0.062]	[0.051,0.056]	[0.059,0.069]	[−24.6,−11.4]

Notes: Confidence intervals for variance estimates for actual couples are based on 400 bootstrapped samples. The conditionally and unconditionally swapped estimates are based on averages across 400 simulations. To construct a 95% confidence interval, we use the 10th and 390th order statistics of the simulated distributions. For the column reporting percent changes, we take the percentage change between 1980 and 2009 for each simulation, and then use percentiles from the distribution of changes as the bounds on the confidence intervals.

couples. Table 3 provides estimates for selected years. We find that relative to unconditionally matched couples, conditionally matched couples have somewhat higher variance of combined earnings, reflecting positive assortative matching on education and age. Relative to the conditionally matched couples, actual couples have slightly lower variance of earnings which is consistent with coordinated offsetting labor supply behavior. That is, comparing couples within a group defined by the ages and education levels of both spouses, husbands with relatively high earnings tend to have wives with relatively low earnings. But this pattern is not particularly strong. A striking feature of these graphs is that relative to the gap between husbands' earnings variance and couples' earnings variance, the differences between the three lines of couples' earnings are very small.

This statement also holds for the differential rise in inequality over time. The gap between the rise in husbands' earnings inequality and all three measures of couples' earnings inequality is much larger than the differences in increases observed among the three couples' measures. Table 3 shows that actual couples' earnings variance increased by about 70% from 1980 to 2009. If we randomly match couples, thereby shutting down both positive assortative matching and joint labor supply behavior, couples' earnings variance increases by about 67%, thus lowering the increase by 3 percentage points relative to actual couples. Couples' earnings variance rose by about 70% for conditionally matched couples as well. Under the assumption that education and age capture assortative matching, this would suggest that sorting had a small role while coordinated labor supply had no role in the rise couples' earnings variances. If sorting on unobservable characteristics in addition to education and age is also important, it may be the case that changes in the effects of coordinated labor supply exactly offset the increases in the effects of assortative matching on unobservables. In any case, the small role we attribute to positive assortative matching on education and age is consistent with Eika et al. (2014) and Greenwood et al. (2015) that report that changes in assortative matching played a minor role in the rise in household income inequality.

In Figs. 4 and 5 we repeat the comparison in Fig. 3 but split the sample between couples in which the husband has a bachelor's degree and those in which he does not.[9] For both groups, we find a pattern similar to the overall results in that wives have an equalizing influence on the level and change in family earnings inequality. That is, couples' earnings variance is lower and rises by a smaller amount than husbands' earnings

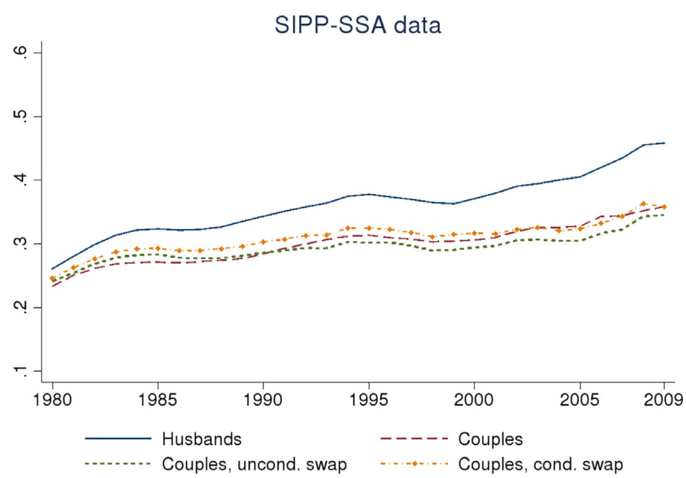

Fig. 4. Permanent variance, couples with non-college-graduate husband, SIPP-SSA.

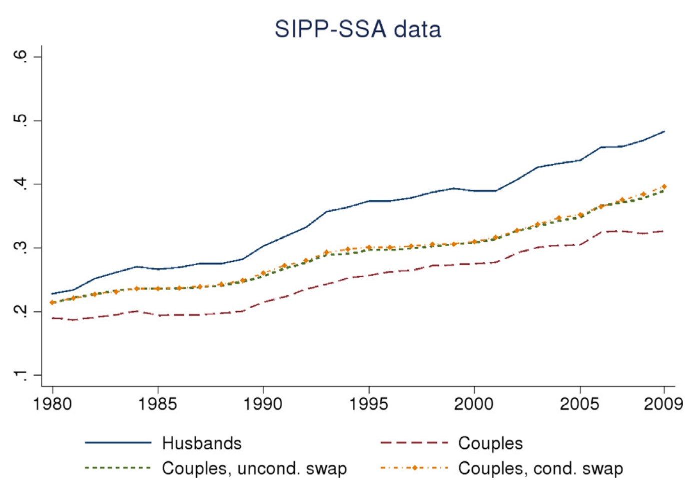

Fig. 5. Permanent variance, couples with college-graduate husband, SIPP-SSA.

variance. However, Fig. 5 shows one notably different pattern: among more educated couples the variance of earnings for actual couples is lower than that for even unconditionally rematched couples, indicating strong offsetting labor supply behavior. In other words, once selecting

[9] Here, sampling for the unconditionally rematched couples does condition on whether or not the husband is a college graduate, so it incorporates part of the effects of positive assortative matching. Estimates for selected years appear in Appendix Table A.1.

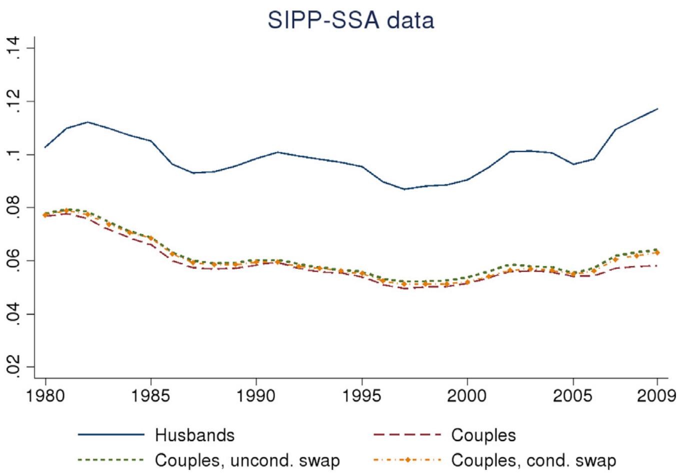

Fig. 6. Transitory variance for actual and rematched couples, SIPP-SSA.

on educated couples, we find a diminished role for sorting and a magnified role for offsetting labor supply behavior.[10]

Again, matching and joint labor supply behavior together contribute little to the rising trend in the permanent variance of couples' earnings.

Fig. 6 provides the same comparison between actual and rematched couples for earnings instability. The gap between male earnings instability and couples' earnings instability is even more pronounced relative to the very minor differences between the actual and randomly matched couples.

Trends in the coefficient of variation

We have so far focused exclusively on the variance of log earnings as our measure of permanent earnings inequality. While doing so makes our results comparable to many of the previous studies, a potential drawback is that the variance of log couples' earnings does not allow for an explicit decomposition into the components such as the variance of husbands' log earnings, the variance of wives' log earnings and the covariance between spousal earnings. The coefficient of variation (CV) is an alternative measure of inequality of couples' earnings in *levels* that can provide such a decomposition.

Letting y denote the level of earnings and $\bar{y}$ the cross-sectional mean of earnings, the square of the CV for couples can be decomposed in the following way:

$$\mathrm{CV}^2(y_{ct}) = \frac{\mathrm{var}(y_{ct})}{(\bar{y}_t)^2} = \frac{\mathrm{var}(y_{ct}^m) + \mathrm{var}(y_{ct}^f) + 2\mathrm{cov}(y_{ct}^m, y_{ct}^f)}{(\bar{y}_t^m + \bar{y}_t^f)^2}$$
$$= \frac{(\bar{y}_t^m)^2}{(\bar{y}_t^m + \bar{y}_t^f)^2}\mathrm{CV}^2(y_{ct}^m) + \frac{(\bar{y}_t^f)^2}{(\bar{y}_t^m + \bar{y}_t^f)^2}\mathrm{CV}^2(y_{ct}^f) + \frac{2\mathrm{cov}(y_{ct}^m, y_{ct}^f)}{(\bar{y}_t^m + \bar{y}_t^f)^2}.$$

The first two terms in the decomposition are the squared CVs for husbands and wives, in each case weighted by their respective squared earnings shares, while the last term involves the covariance of spouses' permanent earnings. The covariance term incorporates the effect of positive assortative matching and offsetting labor supply behavior. When we unconditionally rematch couples, we effectively set this covariance term to zero while having no effect on the CVs for husbands and wives.

Table 4 reports the coefficient of variation in earnings for husbands, wives, actual couples and rematched couples. While the differences between husbands' and couples' inequality in levels are more modest when using CVs than when using the variance of log earnings, we find that inequality is lower and rises more slowly for couples than among husbands using this measure as well—growing 49% for husbands and 42% for couples. Wives' earnings have an equalizing impact on couples' earnings inequality. The CV of conditionally matched couples is higher than the CV of unconditionally matched couples, and the CV of actual couples is lower than that of the conditionally matched couples indicating offsetting labor supply. The CV of actual couples rises 42% while the CV of unconditionally matched couples rises 36%, leading to a gap of 6 percentage points. While the results using the coefficient of variation lead to a somewhat larger role for sorting and coordinated labor supply, we again find that these differences are small relative to the rise in earnings inequality.

Fig. 7 plots 1980–2009 trends in the variables that factor into the decomposition: couples', husbands' and wives' CVs in the top two panels; the share of earnings for wives and the correlation between spouses' earnings in the bottom two panels. As illustrated in the bottom right panel, the correlation of spousal earnings for unconditionally matched couples is zero, while the conditional rematching results in a positive correlation because it reproduces the positive assortative matching on age and education that exists among actual couples. For most of our period, the conditionally rematched correlation lies above the actual correlation, as would be expected if spouses coordinate labor supply. It is interesting to note, however, that the correlation of actual couples catches up over time suggesting that the offsetting labor supply behavior of couples weakened over this period. This is consistent with papers that have noted that married women's labor supply has become less responsive to husband's wages (see, for example, Juhn and Murphy, 1997 and Blau and Kahn, 2007).

While the correlation of spousal earnings is positive and rising somewhat for actual and conditionally rematched couples, it remains small. Cancian and Reed (1998) show that in the case of the coefficient of variation, wives' earnings have an equalizing impact on couples' earnings when $r\mathrm{CV}_f < \mathrm{CV}_m$, where CV_f and CV_m refer to the coefficient of variation for wives' and husbands' earnings respectively and r refers to the correlation of wives' and husbands' earnings. While the CV for wives is large relative to that for husbands in our estimates, the correlation is small enough so that the condition $r\mathrm{CV}_f < \mathrm{CV}_m$ holds, and wives' earnings have an equalizing impact on couples' earnings inequality.

Table 5 shows the contribution of each component of the decomposition to the change in the squared coefficient of variation. Changes in the husbands' contribution accounted for 44% of the increase while changes in the wives' contribution accounted for 39%. Changes in the covariance term accounted for 16% of the increase. The table shows that despite the fact that wives' CV did not rise at all over this period, wives' contribution rose due to their rising share in couples' earnings.[11] Comparing the change in the covariance contribution to the squared CV for actual and conditionally swapped couples, two-thirds of the increase (=0.023/0.034) is accounted for by positive assortative matching on education and age.

In summary, we find that wives' earnings play an important role both in dampening the cross-sectional inequality of resources for married couples, and in offsetting transitory shocks to those resources. This is the case because the earnings of spouses are not strongly positively correlated. We also find that the covariance of couples' earnings that arises due to positive assortative matching and coordination in labor supply has relatively minor effects.

[10] We have also examined 90/50 and 50/10 earnings ratios for actual and rematched couples as an alternative way of capturing whether patterns differ by level of family resources. Findings from the SIPP-SSA and PSID were consistent with a greater role for coordination of labor supply among families with more resources. That is, the 90/50 ratio is larger (about the same) for conditionally rematched couples than for actual couples in SIPP-SSA (PSID) data, but that does not hold for the 50/10 ratio (the actual ratio is above the ratio for conditionally rematched couples in both data sets).

[11] The flat trend of wives' earnings inequality appears to be due to two offsetting trends. While the squared CV for wives with positive earnings increased 40.5% over this period, the share of wives with zero earnings decreased, resulting in little change among wives overall for our base sample, and even a slight decline when we include couples in which the husband has no earnings.

Table 4
Coefficient of variation, base sample, SIPP-SSA.

	1980	1990	2000	2009	Δ (2009–1980), %
Men	0.473	0.562	0.665	0.718	51.6
	[0.470,0.477]	[0.558,0.566]	[0.660,0.670]	[0.712,0.724]	[49.9,53.2]
Husbands	0.462	0.513	0.624	0.689	49.0
	[0.458,0.466]	[0.509,0.517]	[0.616,0.632]	[0.673,0.705]	[45.4,52.6]
Wives	1.274	1.064	1.150	1.259	−1.2
	[1.256,1.295]	[1.043,1.088]	[1.091,1.209	[1.130,1.416]	[−11.5,11.5]
Couples	0.450	0.481	0.577	0.641	42.4
	[0.446,0.455]	[0.476,0.487]	[0.563,0.592]	[0.608,0.680]	[35.2,51.6]
Couples, cond. swap	0.463	0.492	0.582	0.642	38.6
	[0.459,0.468]	[0.487,0.497]	[0.569,0.597]	[0.616,0.676]	[26.1,31.5]
Couples, uncond. swap	0.451	0.472	0.557	0.615	36.4
	[0.446,0.456]	[0.466,0.477]	[0.545,0.570]	[0.587,0.649]	[24.6,30.3]

Notes: See notes to Table 3.

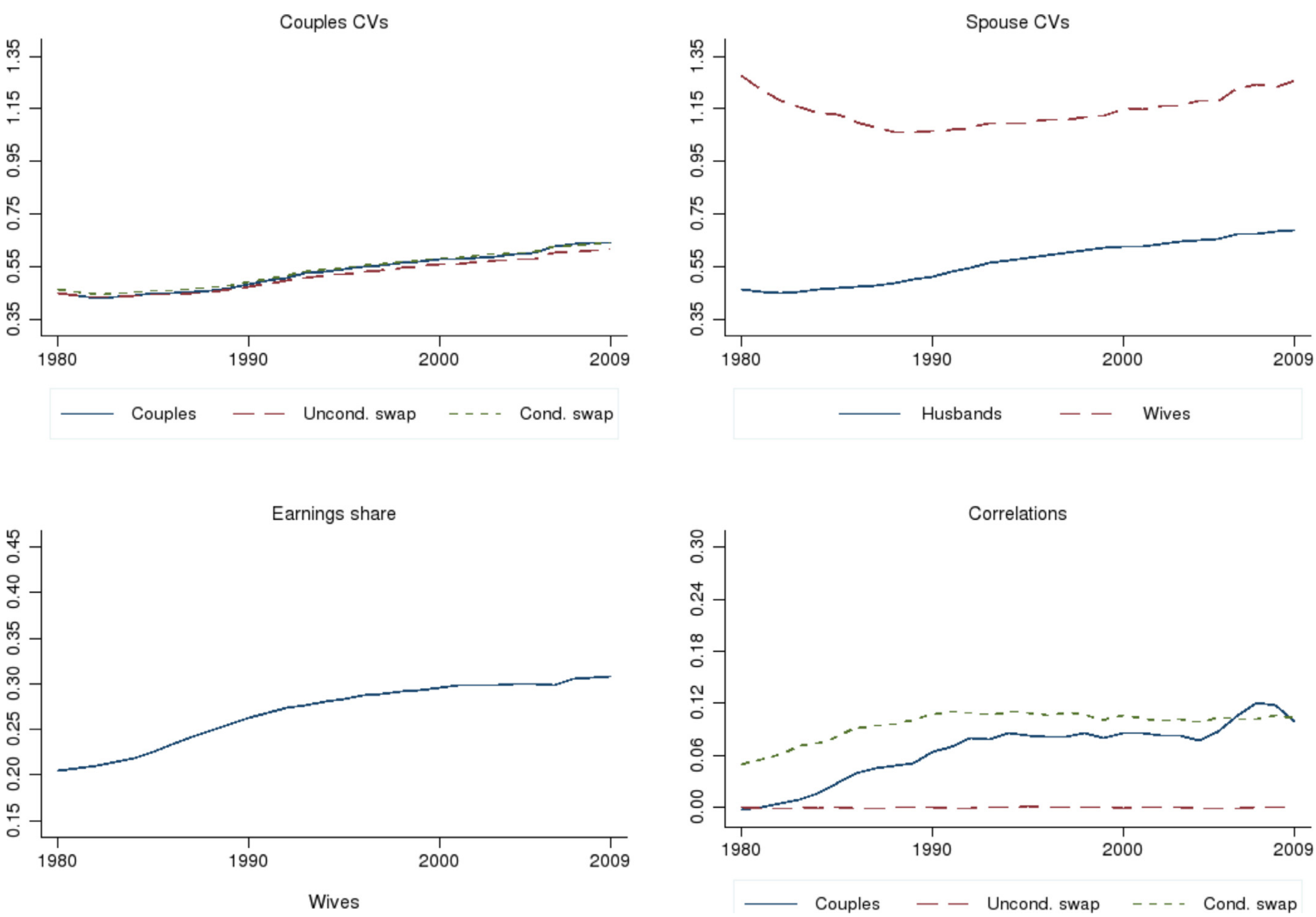

Fig. 7. Factors affecting couples CVs, base sample, SIPP-SSA.

6. Robustness

In Section 5 we found that changes in positive assortative matching and coordinated labor supply responses played a minor role in the rising inequality of couples' earnings. Here we examine the robustness of our results to alternative choices of our sample: using only current SIPP panels; using an alternative data set (the PSID); and changing our sample restrictions for zero earnings.

6.1. Using only current SIPP panel members

As mentioned above, pooling samples from current and later panels gives longer married couples a higher probability of being included in our sample. As a check on whether that has important effects, we reproduce our primary results using a more restricted sample that includes only members of the most current panel, using data from more than one panel only in the few years in which data collection for two panels overlapped. For example, for linked members of the 1984 SIPP panel, we use earnings data from 1979–1984, plus data for years 1985 and 1986 if they remained in the panel to its end in 1986. That means that, for this restricted sample, our first 5-year average involves earnings from 1979–1983 with midpoint of 1981. We then have a gap between the end of the 1984 panel data and the next linked panel in 1990, resulting in a gap in our estimates between 1984 and 1987. For estimates in years 2005–2009, only the current (2008) panel contributes to the base sample, so estimates from our base and current-panel samples are identical for those years.

Table 5
Decomposition of couples' CV, base sample, SIPP-SSA.

Terms invariant to matching	1980	2009	Δ (2009–1980)	Share
Husbands' CV squared	0.213	0.475	0.261	
Wives' CV squared	1.623	1.585	−0.038	
Wives' share of earnings, squared	0.042	0.095	0.053	
Husbands' share of earnings, squared	0.632	0.479	−0.153	
Actual couples				
Couples' CV squared	0.203	0.411	0.208	
Contributions:				
Husbands'	0.135	0.227	0.092	44%
Wives'	0.068	0.150	0.082	39%
Covariance term	−0.001	0.033	0.034	16%
Conditionally swapped couples				
Couples' CV squared	0.214	0.412	0.198	
Contributions:				
Husbands'	0.135	0.227	0.092	47%
Wives'	0.068	0.150	0.082	42%
Covariance term	0.012	0.035	0.023	12%
Unconditionally swapped couples				
Couples' CV squared	0.203	0.378	0.175	
Contributions:				
Husbands'	0.135	0.227	0.092	53%
Wives'	0.068	0.150	0.082	47%
Covariance term	0	0	0	n/a

Notes: The figures in the last column are shares of the change in the couples' CV squared reported in each panel.

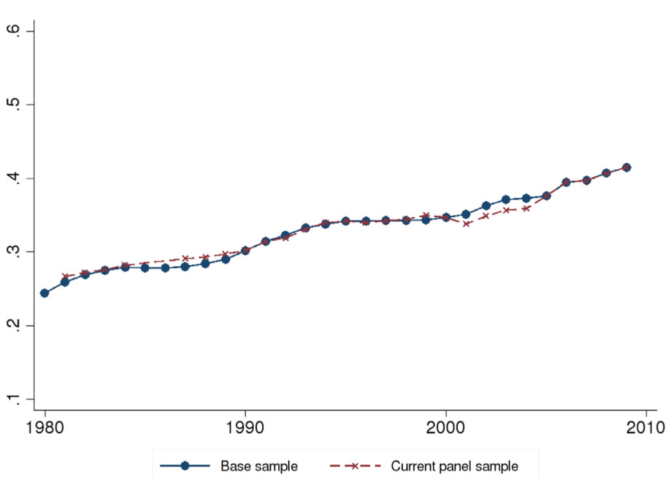

Fig. 8. Permanent variances for couples, base and current-panel samples, SIPP-SSA.

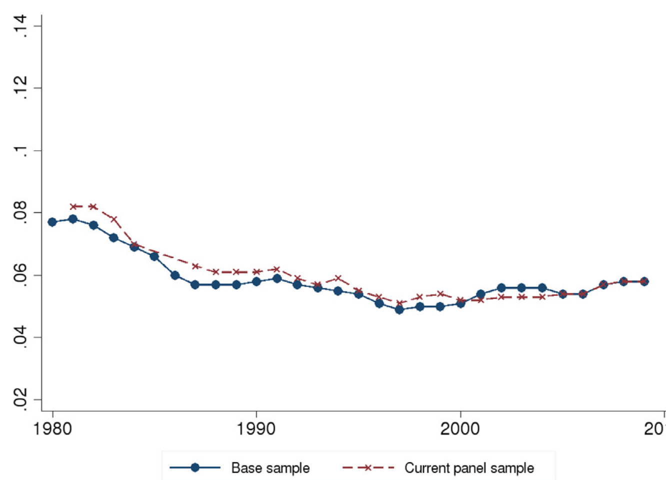

Fig. 9. Transitory variances for couples, base and current-panel samples, SIPP-SSA.

Fig. 8 plots estimates of permanent log earnings variances for our current and base samples, and shows that they are very similar. Fig. 9 shows that the transitory variances are also very similar. This suggests that once we condition on a couple being married for at least five years, there are no substantial differences in current earnings characteristics between those that remain married after that point and those that do not. Table 6 reports variances of log earnings for husbands, actual couples, and rematched couples. The percent changes reported differ slightly from our base sample results because the first year is 1981 rather than 1980. However, our main conclusions that wives' earnings had an equalizing impact and that positive assortative matching and joint labor supply had a minimal role in contributing to the rising inequality trend are robust to using this more restricted sample.

6.2. Results using data from the PSID

The PSID has been used in numerous empirical papers to document trends in earnings inequality and instability in the U.S. Given its importance in this literature, results from the PSID provide an important point of comparison for our findings from the less familiar SIPP-SSA administrative earnings data. The PSID was initiated in 1968, interviewing a sample of about 3000 families representative of the U.S. population (the SRC sample) and a sample of about 2000 low-income families (the Survey of Economic Opportunity sample). The PSID has followed the original families and their offspring over time, collecting information on earnings, marital status, and a number of other topics. Interviews were conducted annually up to 1997 and have been conducted biennially since then. We use information for the SRC sample for the same years (1978–2011) and the same sample selection rules for comparison to our SIPP-SSA results.[12]

The top panel of Table 7 reports results on the permanent variance of earnings using the PSID while the bottom panel reports results on earnings instability. As Table 7 shows, the level of permanent variance is lower in the PSID than in the SIPP-SSA, but similarly shows a rising

[12] After 1997 the PSID switched to biennial reporting which makes comparability difficult. In addition, the PSID went through a major overhaul between the 1993 and 1994 surveys, switching to computer-assisted telephone interviewing, automated editing of data, and changing the income questions. Thus, the changes encompassing these years have to be interpreted with caution. See discussion in Dynan et al. (2012).

Table 6
Variances of log earnings, current sample, SIPP-SSA.

	1981	1990	2000	2009	Δ (2009–1981), %
	Permanent earnings (5-year averages)				
Men	0.327	0.408	0.457	0.578	77.1
	[0.302,0.352]	[0.399,0.418]	[0.438,0.474]	[0.560,0.596]	[63.2,92.6]
Husbands	0.294	0.374	0.423	0.547	86.3
	[0.267,0.322]	[0.364,0.384]	[0.397,0.448]	[0.528,0.568]	[67.6,105.7]
Couples	0.267	0.302	0.347	0.415	56.1
	[0.244,0.293]	[0.293,0.310]	[0.326,0.367]	[0.401,0.430]	[40.0,72.8]
Couples, cond. swap	0.266	0.313	0.351	0.430	61.9
	[0.248,0.290]	[0.304,0.323]	[0.333,0.372]	[0.413,0.449]	[48.4,75.8]
Couples, uncond. swap	0.257	0.295	0.327	0.410	59.7
	[0.238,0.279]	[0.287,0.304]	[0.309,0.348]	[0.394,0.427]	[44.9,73.8]
	Transitory earnings (deviations from 5-year averages)				
Men	0.126	0.114	0.104	0.130	3.5
	[0.115,0.137]	[0.111,0.118]	[0.098,0.111]	[0.123,0.136]	[−6.2,15.1]
Husbands	0.114	0.102	0.087	0.117	3.6
	[0.101,0.125]	[0.097,0.106]	[0.080,0.094]	[0.110,0.124]	[−8.5,18.5]
Couples	0.082	0.061	0.052	0.058	−28.5
	[0.073,0.090]	[0.058,0.063]	[0.047,0.056]	[0.054,0.062]	[−37.2,−18.4]
Couples, cond. swap	0.081	0.061	0.052	0.063	−22.0
	[0.073,0.090]	[0.059,0.064]	[0.047,0.056]	[0.059,0.068]	[−31.9,−10.7]
Couples, uncond. swap	0.081	0.062	0.053	0.064	−20.2
	[0.071,0.089]	[0.059,0.064]	[0.049,0.057]	[0.060,0.068]	[−31.2,−8.3]

Notes: 95% confidence intervals for variance estimates for men, husbands, and actual couples are based on 400 bootstrapped samples. The conditionally and unconditionally swapped estimates are based on averages across 400 simulations. To construct a 95% confidence interval, we use the 10th and 390th order statistics of the simulated distributions. For the column reporting percent changes, we take the percentage change between 1981 and 2009 for each simulation, and then use percentiles from the distribution of changes as the bounds on the confidence intervals.

Table 7
Variances of log earnings, PSID.

	1980	1990	2001	2009	Δ (2009–1980), %
	Panel A: Permanent earnings				
Men	0.193	0.274	0.319	0.405	110.2
	[0.175,0.207]	[0.256,0.295]	[0.295,0.345]	[0.370,0.435]	[87.8,187.2]
Husbands	0.185	0.236	0.282	0.353	90.7
	[0.166,0.203]	[0.217,0.265]	[0.261,0.312]	[0.323,0.386]	[67.5,124.8]
Couples	0.177	0.208	0.227	0.292	64.5
	[0.160,0.194]	[0.192,0.236]	[0.210,0.243]	[0.266,0.324]	[44.9,95.2]
Couples, cond. swap	0.174	0.207	0.234	0.286	65.2
(baseline: age and educ.)	[0.159,0.191]	[0.191,0.233]	[0.218,0.251]	[0.265,0.311]	[46.3,88.7]
Couples, cond. swap	0.177	0.207	0.226	0.283	60.5
(baseline and region born)	[0.159,0.199]	[0.185,0.224]	[0.207,0.249]	[0.257,0.310]	[38.2,81.5]
Couples, cond. swap	0.173	0.205	0.233	0.273	58.2
(baseline and parental educ.)	[0.156,0.199]	[0.187,0.224]	[0.215,0.253]	[0.249,0.301]	[33.6,82.3]
Couples, cond. swap	0.176	0.208	0.236	0.289	64.0
(baseline and religion)	[0.156,0.195]	[0.189,0.226]	[0.216,0.254]	[0.270,0.312]	[44.9,91.2]
Couples, uncond. swap	0.172	0.202	0.228	0.276	61.2
	[0.157,0.190]	[0.186,0.222]	[0.212,0.248]	[0.255,0.300]	[41.3,86.0]
	Panel B: Transitory earnings				
Men	0.037	0.043	0.079	0.088	134.5
	[0.031,0.045]	[0.036,0.053]	[0.069,0.091]	[0.076,0.102]	[87.5,187.2]
Husbands	0.031	0.036	0.066	0.070	123.0
	[0.023,0.039]	[0.029,0.044]	[0.055,0.076]	[0.058,0.082]	[67.6,224.3]
Couples	0.025	0.027	0.039	0.040	64.3
	[0.023,0.039]	[0.021,0.029]	[0.034,0.045]	[0.035,0.048]	[37.1,118.1]
Couples, cond. swap	0.026	0.027	0.041	0.042	61.7
(baseline: age and educ.)	[0.021,0.032]	[0.023,0.033]	[0.035,0.047]	[0.036,0.048]	[25.5,109.1]
Couples, cond. swap	0.026	0.025	0.039	0.041	60.46
(baseline and region born)	[0.020,0.032]	[0.020,0.029]	[0.034,0.047]	[0.034,0.048]	[14.78,100.23]
Couples, cond. swap	0.025	0.026	0.041	0.039	59.5
(baseline and parental educ.)	[0.020,0.031]	[0.021,0.030]	[0.034,0.050]	[0.033,0.047]	[22.1,104.6]
Couples, cond. swap	0.025	0.027	0.040	0.041	60.35
(baseline and religion)	[0.021,0.031]	[0.022,0.032]	[0.034,0.045]	[0.034,0.048]	[24.28,104.00]
Couples, uncond. swap	0.026	0.028	0.041	0.041	58.0
	[0.021,0.033]	[0.023,0.032]	[0.035,0.048]	[0.036,0.048]	[15.8,102.1]

Notes: See notes to Table 3.

Table 8
Coefficient of variation using alternative exclusion restrictions, SIPP-SSA.

	1980	1990	2000	2009	Δ (2009–1980), %
	Excluding couples in which wives have zero earnings				
Husbands	0.449	0.495	0.580	0.649	44.4
	[0.443,0.456]	[0.490,0.499]	[0.571,0.590]	[0.632,0.670]	[40.2,48.8]
Wives	0.677	0.698	0.847	0.951	40.5
	[0.653,0.711]	[0.675,0.723]	[0.791,0.909]	[0.822,1.092]	[22.2,62.7]
Couples	0.399	0.438	0.532	0.598	49.8
	[0.393,0.614]	[0.559,0.642	[0.512,0.553]	[0.430,0.446]	[39.2,40.9]
Couples, cond. swap	0.396	0.429	0.516	0.545	44.9
	[0.389,0.404]	[0.423,0.436]	[0.501,0.538]	[0.539,0.619]	[34.7,57.1]
Couples, uncond. swap	0.377	0.406	0.486	0.545	44.8
	[0.369,0.386]	[0.400,0.413]	[0.470,0.505]	[0.510,0.592]	[34.7,57.1]
	Adding couples in which husbands have zero earnings				
Husbands	0.586	0.635	0.731	0.811	38.2
	[0.582,0.591]	[0.629,0.640]	[0.722,0.741]	[0.795,0.826]	[35.2,41.1]
Wives	1.306	1.102	1.233	1.286	−1.5
	[1.288,1.324]	[1.084,1.125]	[1.163,1.309]	[1.169,1.415]	[−10.3,9.1]
Couples	0.549	0.570	0.657	0.715	30.2
	[0.544,0.554]	[0.565,0.576]	[0.641,0.675]	[0.683,0.751]	[24.7,36.7]
Couples, cond. swap	0.556	0.577	0.662	0.722	29.9
	[0.551,0.560]	[0.572,0.582]	[0.645,0.678]	[0.692,0.754]	[24.5,35.7]
Couples, uncond. swap	0.540	0.553	0.636	0.693	28.4
	[0.535,0.545]	[0.548,0.558]	[0.620,0.653]	[0.667,0.723]	[23.4,33.7]

Notes: The base sample includes couples in a 5-year panel whether or not the wife have any earnings in that interval, but excludes couples if the husband has no earnings below the first percentile or above the 99th percentile in any of those years. In the top panel of this table, estimates exclude couples in which the wife has zero earnings in any of year of the 5-year window. In the bottom panel, in addition to all members of the base sample, the estimates also include couples in which the husband has earnings below the first percentile in one or more of the years.

trend. Importantly, we find that the difference in trend between the actual and the simulated couples is also small in the PSID. Actual couples' earnings variance increased by 64.5% from 0.177 in 1980 to 0.292 in 2009. The earnings variance of randomly matched couples increased by nearly as much, rising 61.2% from 0.172 in 1980 to 0.276 in 2009.

As mentioned above, earnings instability trends in the PSID are at odds with the patterns found in the SIPP-SSA. While couples' earnings instability actually fell over the period in the SIPP-SSA, the bottom panel of Table 7 shows that all measures of earnings instability increased in the PSID. Instability of actual couples' earnings rose from 0.025 in 1980 to 0.040 in 2009. Instability of randomly matched couples increased from 0.026 in 1980 to 0.041 in 2009, indicating that changes in matching and coordinated labor supply account for little of the overall change in couples' earnings instability as well.

6.3. Excluding couples in which wives have zero earnings

Next, we explore how our results differ when we drop couples in which the wives have zero earnings in one or more years of the five-year window, following the restriction used in Hyslop (2001). Relative to the main sample, this selection limits the effects of coordinated labor supply on the evolution of inequality over time by eliminating couples in which wives enter or exit the labor market in response to shocks or predictable changes in the husband's earnings.

Table 8 shows that, as in our base sample, conditionally matched couples have more unequal incomes than unconditionally matched couples, reflecting effects of positive assortative matching on inequality. But in contrast to our base sample results, once we include only continuously working couples, earnings inequality is higher for actual than conditionally matched couples, as is the correlation between spouses' permanent earnings (shown in Appendix Fig. A.1). These findings indicate that assortative matching on factors we do not observe—such as field of degree or work experience—is also playing a role.

Not surprisingly, results for this sample are also more like those of Hyslop (2001) in that the covariance term plays a larger role in explaining both the level of and growth in permanent earnings inequality. Table 9 shows our decomposition of the squared couples' CV for this sample, which attributes 22% of the growth to the covariance. But in our more general sample of couples, the growth in covariance is less important, accounting for only 16% of the overall growth.

Table 9
Decomposition of Couples' CV, Excluding wives with zero earnings, SIPP-SSA.

Terms invariant to matching	1980	2009	Δ (2009–1980)	Share
Husbands' CV squared	0.202	0.421	0.220	
Wives' CV squared	0.458	0.904	0.446	
Wives' share of earnings, squared	0.118	0.156	0.038	
Husbands' share of earnings, squared	0.432	0.366	−0.066	
Actual couples				
Couples' CV squared	0.159	0.358	0.198	
Contributions:				
Husbands'	0.087	0.154	0.067	34%
Wives'	0.054	0.141	0.087	44%
Covariance term	0.018	0.062	0.044	22%
Conditionally swapped couples				
Couples' CV squared	0.157	0.329	0.173	
Contributions:				
Husbands'	0.087	0.154	0.067	39%
Wives'	0.054	0.141	0.087	50%
Covariance term	0.016	0.036	0.020	11%
Unconditionally swapped couples				
Couples' CV squared	0.142	0.297	0.155	
Contributions:				
Husbands'	0.087	0.154	0.067	42%
Wives'	0.054	0.141	0.087	58%
Covariance term	0	0	0	n/a

Notes: The figures in the last column are shares of the change in the couples' CV squared reported in each panel.

6.4. Adding couples in which husbands have zero earnings

We further explore robustness to inclusion of men with zero earnings in one or more years of the 5-year window, making our restrictions similar to those used in Greenwood et al. (2015). It is possible that by restricting our base sample to husbands with nonzero earnings, we understate the impact of assortative matching, particularly if husbands with zero earnings tend to be married to wives with zero or low earnings. However, as Table 8 and Appendix Fig. A.2 illustrate, we find that

Table 10
Correlation of log wages, PSID, working spouses.

Sample	Value
All working spouses	0.412
Spouses with hours worked ≥ 1400	0.426
All working spouses, cond. swap (age and educ.)	0.152
All working spouses, cond. swap (age, educ. and region born)	0.164
All working spouses, cond. swap (age, educ. and parental educ.)	0.174
All working spouses, cond. swap (age, educ. and religion)	0.179

relaxing this exclusion restriction has little effect on our conclusions. Even when we include men with zero earnings, we find little difference in inequality levels or trends between actual and randomly matched couples.

6.5. *Exploring whether age and education adequately capture assortative matching*

Our assessment of the importance of assortative matching was based on the assumption that there is little assortative matching on anything besides education level and age, and therefore the difference between inequality measures for the conditionally and unconditionally rematched couples identified sorting while the difference between conditionally rematched and actual couples identified the effects of offsetting labor supply.

However, couples may also match on factors that we do not observe in our data, such as field of degree or non-cognitive abilities. In that case, the effects of (unmeasured) assortative matching on unobservables may lead us to understate the importance of coordinated labor supply.

To assess the extent to which education and age capture assortative matching, we examine their ability to explain the correlation in spousal average wages. Arguably, average wages are more indicative of an individual's skill type than earnings, which also reflect labor supply. We calculate the correlation of average wages for continuously working heads and wives in actual and randomly matched couples in the PSID during each five-year window. The results are presented in Table 10. The correlation of wages of couples randomly matched within education and age group is equal to about 37% of the correlation of wages of actual couples (=0.152/0.412), suggesting that age and education are informative characteristics for measuring positive assortative matching.[13] The correlation of wages for this sample of continuously working spouses may still reflect labor supply if, for example, part-time workers are paid less than full-time workers for the same level of skill. To assess whether this is an important concern, we next calculate the correlation of average wages among couples in which both spouses consistently work more than 1400 hours per year over a five-year window. Doing so slightly increases the correlation from 0.41 to about 0.43, from which we conclude that the correlation is similar for full- and part-time workers, and so we do not exclude part-time workers in the following.

As a second way of checking on the adequacy of our reliance on age and education, we examine whether additional spousal background information available in the PSID—parental education, region of origin, and religious preferences—can improve our ability to explain the correlation in spousal wages. To assess the incremental importance of these factors for assortative matching, we calculate the correlation of average wages for couples randomly matched on age, education, and each of these factors.[14] Conditioning, in addition to age and education, on whether each spouse grew up in the south versus elsewhere raises the correlation of average wages from 0.152 to 0.164; whether spouses have a college-educated parent or not raises the correlation to 0.174; whether or not spouses consider themselves Protestant, another religion, or atheists raises the correlation to 0.179. Clearly, each of these factors contributes to positive assortative matching but their effects are modest when age and education have already been taken into account.

To further assess the importance of assortative matching as captured by age and education for the trends in couples' earnings inequality, we continue with the analysis of Table 7 calculating permanent and transitory variances for couples conditionally matched on age, education, and other background information. The results are in the fifth to seventh rows of Panels A and B in Table 7. The results are largely in agreement with the results of matching just on education and age which is not surprising given that the additional background variables we consider do not contribute much to the correlation in spousal wages (and so earnings) as we established above.

Finally, we calculate permanent and transitory variances of potential earnings, based on the product of observed wages and a fixed number of hours.[15] This experiment eliminates any effects of labor supply on variation in (potential) family earnings and so all variation in (potential) family earnings comes from the way in which spouses are paired—that is, from assortative matching. We use our PSID sample of continuously working spouses, and present the results in Appendix Fig. A.3. The permanent variance of potential earnings for actual couples is, on average, 40% higher than the variance for the unconditionally matched couples, whereas the permanent variance for the couples conditionally matched on age and education is about 10% higher than the permanent variance of the unconditionally matched couples. We therefore conclude that conditioning on age and education captures about 25% of the overall contribution of positive assortative matching towards the permanent variance of couples' earnings. While it appears that we understate the role of positive assortative matching to the level of couples' earnings inequality when we focus exclusively on education and age, it is worth noting that couples' earnings inequality trends under all three counterfactuals—no sorting, sorting on observables only, sorting on observables and unobservables—have very similar trends. This suggests positive assortative matching played only a minor role in contributing to couples' earnings inequality growth.

7. Conclusion

In this paper we examine trends in the variance of combined earnings of couples for husbands and wives selected in a series of SIPP panels. We examine variation in both short-term changes in earnings (instability) and in longer-term averages (inequality). We use random rematching of spouses as a way to tease out the magnitudes of the effects of positive assortative matching on observables and coordination of labor supply within families on these trends.

Comparing inequality of couples' earnings to inequality of husbands' earnings indicates that wives' earnings have muted the rise of permanent earnings inequality as well as smoothed over earnings instability at the family level. This is largely due to the fact that the correlation in earnings of husbands and wives, while rising over time, is far from perfect. We also find that the covariance in earnings of husbands and wives, arising from the combined effects of positive assortative matching and coordinated labor supply, played a minor role in rising family earnings inequality. Earnings inequality and instability trends of actual couples and those constructed for randomly rematched couples are remarkably similar.

Comparing our results to previous papers, our finding that wives' earnings have had an equalizing impact is similar to

[13] We also found that this ratio of the correlation for conditionally matched and real spouses does not have any visible trend over time (the results are not reported for brevity).

[14] We do not explore matches conditional on all the factors at the same time due the limited cross-sectional size of our PSID sample of continuously working spouses.

[15] In this experiment, similarly to Shaw (1989), we multiply spousal wages by 2000 hours to obtain a measure of potential earnings.

Cancian and Reed (1998), while the minor role we attribute to sorting echoes the findings in Pencavel (2006), Eika et al. (2014) and Greenwood et al. (2015). Our conclusion differs somewhat from Hyslop (2001) who finds a larger contribution of the covariance of couples' earnings, but we find that an important difference is sample selection. When we select on continuously working couples as Hyslop (2001) does, we also find that the covariance of couples' earnings plays a larger role. This suggests that an important reason for the low correlation of couples' earnings is wives' entry and exit decisions.

Appendix

Table A.1
Permanent variances by male education level, SIPP-SSA.

	1980	1990	2000	2009	Δ (2009–1980), %
	Non-college graduate men/husbands				
Men	0.296	0.387	0.422	0.511	72.6
	[0.290,0.303]	[0.379,0.395]	[0.416,0.429]	[0.502,0.519]	[68.0,77.3]
Husbands	0.261	0.343	0.371	0.459	76.1
	[0.254,0.268]	[0.334,0.350]	[0.359,0.384]	[0.434,0.485]	[66.4,86.8]
Couples	0.233	0.284	0.306	0.359	54.0
	[0.226,0.240]	[0.277,0.292]	[0.296,0.315]	0.342,0.376]	[46.0,63.7]
Couples, cond. swap	0.247	0.302	0.316	0.361	46.3
	[0.238,0.256]	[0.293,0.312]	[0.300,0.333]	[0.333,0.392]	[34.1,59.7]
Couples, uncond. swap	0.242	0.286	0.295	0.345	42.7
	[0.234,0.250]	[0.276,0.296]	[0.281,0.310]	[0.317,0.375]	[30.5,55.6]
	College graduate men/husbands				
Men	0.283	0.357	0.434	0.501	76.3
	[0.273,0.295]	[0.348,0.367]	[0.422,0.447]	[0.484,0.516]	[67.8,84.6]
Husbands	0.229	0.303	0.390	0.483	111.0
	[0.217,0.241]	0.290,0.315]	[0.370,0.409]	[0.450,0.518]	[94.1,128.2]
Couples	0.190	0.215	0.275	0.327	73.1
	[0.180,0.199]	[0.206,0.223]	[0.264,0.289]	[0.306,0.351]	[58.3,89.0]
Couples, cond. swap	0.214	0.260	0.311	0.396	85.0
	[0.207,0.222]	[0.252,0.266]	[0.301,0.322]	[0.374,0.417]	[73.1,96.6]
Couples, uncond. swap	0.215	0.256	0.309	0.389	80.6
	[0.208,0.223]	[0.249,0.263]	[0.300,0.318]	[0.369,0.408]	[70.4,91.1]

Notes: See notes to Table 3.

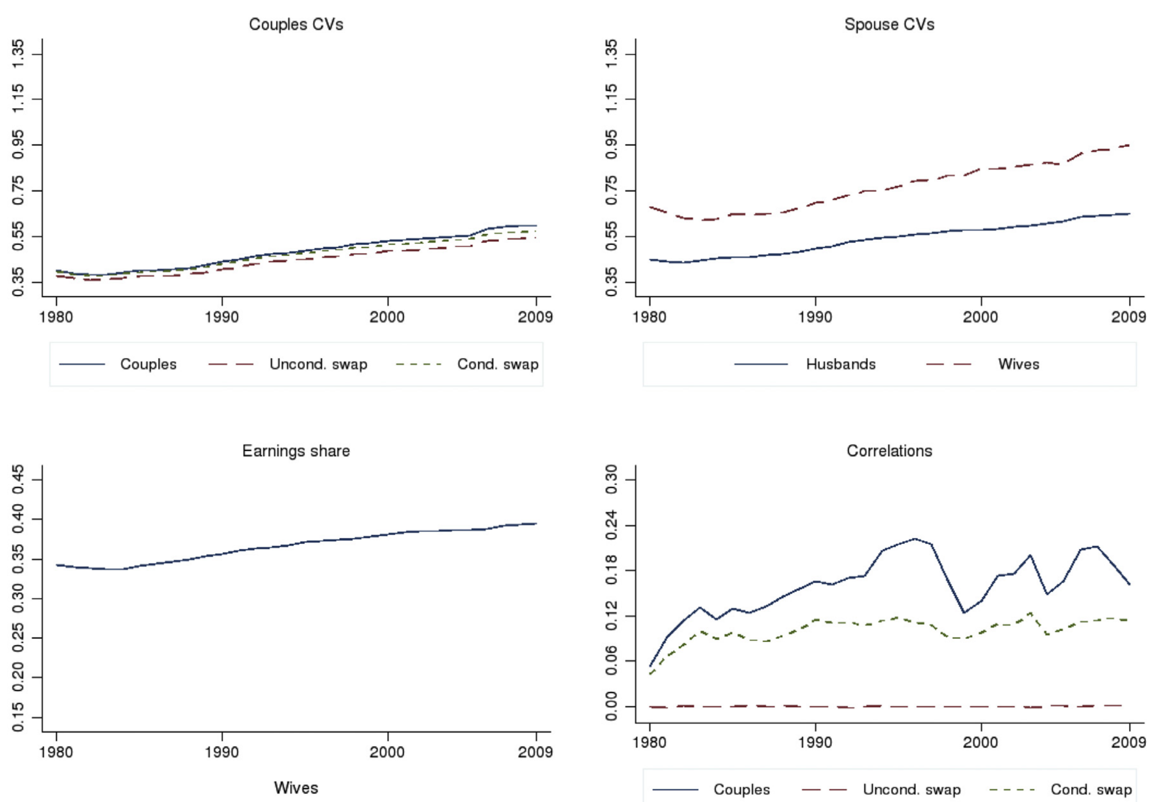

Fig. A.1. Factors affecting couples CVs, excluding wives with zero earnings, SIPP-SSA.

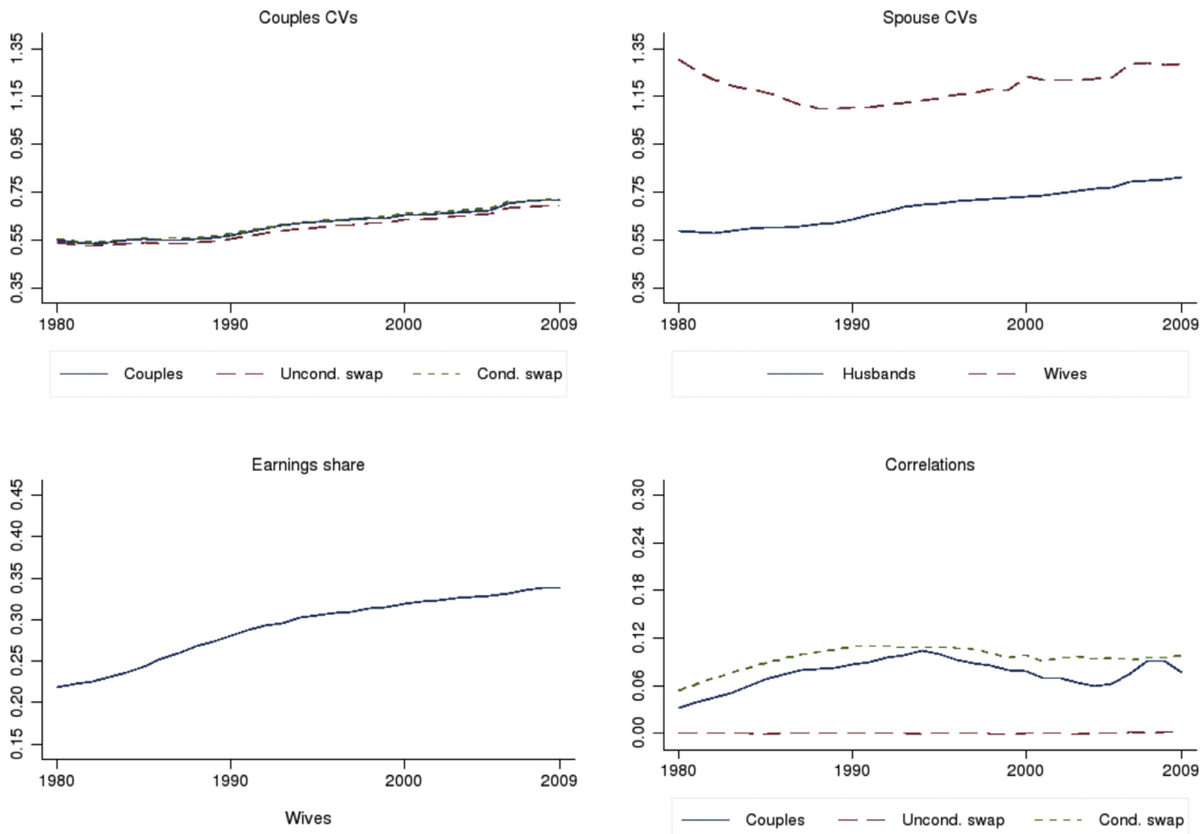

Fig. A.2. Factors affecting couples CVs, including husbands with zero earnings, SIPP-SSA.

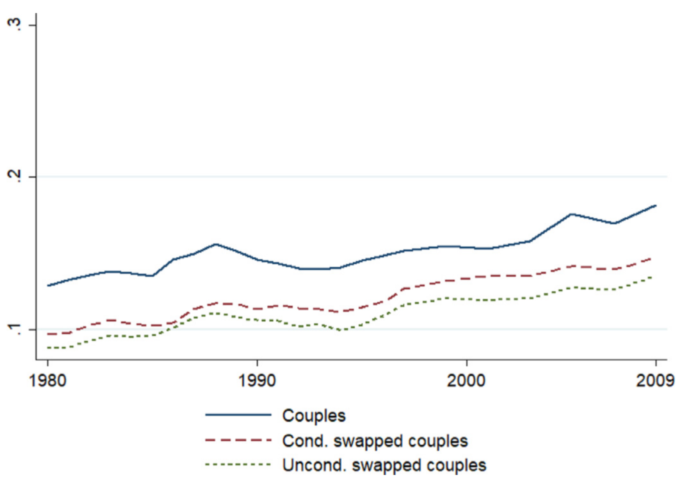

Fig. A.3. Permanent variances of potential earnings, PSID.

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