ESSAYS ON JOB DISPLACEMENT AND EARNINGS VOLATILITY

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ABSTRACT

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The first essay "The Effects of Job Displacement on Family Expenditures" explores the effects of a husband's job displacement on his family expenditures by using the Panel Study of Income Dynamics during 1999-2013. Even with a lasting and sizable fall in family income induced by a husband's job displacement, the households reduced their expenditures only moderately. More specifically, their expenditures fell just slightly in the periods around the job displacement occurrence, then continued to further decline two to six or more years after the job loss, but not as much as their concurrent income loss. These later declines in their expenditures were largely driven by the fall in those for housing, health care and education. The relatively small reduction in those expenditures was achieved through a reduction in wealth.

The second essay "The Evolution of Earnings Volatility During and After the Great Recession" studies how men's and women's earnings volatility evolved during and after the Great Recession, the most severe downturn of the postwar era. Using matched March Current Population Survey data, I find that earnings volatility rose considerably after the Great Recession began and, particularly for men, the volatility increase was as large as in the severe recession of the early 1980s. These findings update the evidence on the counter-cyclicality of earnings volatility. I show that such counter-cyclicality is due mainly to counter-cyclical volatility in

annual hours of work. In turn, the counter-cyclical volatility in work hours appears mainly among workers who experience unemployment.

Finally, the third essay “How Did Men’s Earnings Volatility Change during the Great Recession? A Comparison of Evidence between the PSID and CPS” also finds men’s earnings volatility increased substantially during the Great Recession by using the Panel Study of Income Dynamics (PSID). I further find that the counter-cyclicality was mainly driven by unemployed workers. All these findings are largely consistent with those of my second essay, which examines the same topic with the matched March Current Population Study (CPS). In particular, I find that the PSID and CPS generate a fairly similar evolution of men’s earnings volatility from the early 1980s through the Great Recession for the sample, excluding zero-earners and the 1 % top and bottom positive-earners. For the sample including the zero-earners and outliers, however, the PSID shows somewhat divergent results compared to the CPS, particularly for the period 1993-2004. This partly explains why previous studies have reported different trends in men’s earnings volatility during the early 2000s between the two datasets.

To my wife, Ye Won

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CHAPTER 1

The Effects of Job Displacement on Family Expenditures

1.1 Introduction

The existing literature has extensively documented the persistent effects of job displacement on a worker's subsequent earnings. Although the estimates vary depending on the data set and period, displaced workers have been found to have 10 to 30 percent less earnings than those they would have without displacement, even six or more years after displacement (Couch and Placzek 2010; Jacobson, Lalonde, and Sullivan 1993; Ruhm 1991; Stevens 1997; Topel 1990; von Wachter, Song, and Manchester 2007).

To better understand the welfare and policy implications of job displacements, further investigation of how job displacement changes people's consumption behavior is necessary given that consumption is a better indicator of people's well-being than is income. In general, displaced workers' consumption behavior cannot be inferred solely on the basis of income change because a change in income does not necessarily lead to the same or proportionate change in consumption. As predicted by life-cycle models of consumption and the permanent income hypothesis, displaced workers who are forward-looking should smooth their consumption by saving and/or borrowing. In practice, the ability of displaced workers to smooth consumption may depend on the adequacy of their savings or borrowing ability and the availability of any other insurance mechanisms to offset income loss, such as public/private transfers and labor supply adjustments of other family members (Blundell, Pistaferri, and Preston 2008; Blundell, Pistaferri, and Saporta-Eksten 2016; Dynarski and Gruber 1997).

In contrast to the substantial literature on the income effects of job displacement, there is relatively scant literature examining the link between job displacement and consumption by using micro expenditure data. The majority of studies focus on only short-term change in workers' expenditures one or two years after displacement, or, if they do track the long-term expenditure response of displaced workers, rely only on a limited number of expenditure categories, such as food and some other non-durables (Browning and Crossley 2008; Dynarski et al. 1997; Gruber 1997; Saporta-Eksten 2014; Stephens 2001).[1] Hence, there is a need to explore how displaced workers change their consumption behaviors, particularly over a longer period during which they experience persistent income loss.

To fill this research gap, the current paper studies how a husband's job displacement affects a broad set of his family expenditures over an extensive period of time from roughly two years before the displacement through six or more years after afterwards using the 1999-2013 Panel Study of Income Dynamics (PSID). Furthermore, this study tracks concurrent changes in diverse components of family wealth and income of a displaced husband that underlie the family's expenditure behaviors in order to explicitly capture the contribution of savings/assets and other family income sources that offset his earnings loss due to job displacement. This comprehensive analysis on the long-term effects of job displacement is possible mainly because of the availability of rich information on an extensive range of indicators of family expenditures and wealth, which have been biennially collected since 1999 by the PSID.

Like a number of previous studies, this study directly analyzes expenditures rather than consumption, although consumption is the concept more clearly linked to individual well-being.

[1] A more detailed review of the literature is presented in Section 1.2.

While there is little difference between the two for non-durable goods, a large difference can exist for durable goods. Simply put, a one-time expenditure on a durable such as a car could provide substantial consumption flows for multiple years. Despite this distinction, I analyze expenditures for three reasons. First, income, expenditures, and changes in assets are linked through an accounting identity, and I make use of this identity to provide additional insight into how a family with a displaced husband (hereafter referred to as a displaced family) respond to his job loss. Second, accurately converting expenditures into consumption requires much additional data and assumptions, which can lead to substantial measurement error. Third, changes in expenditures can still provide important insights into how consumption is changing.

The key findings of this paper can be summarized as follows. First, during the period 1998-2012, the annual total income of displaced families is reduced by 16 percent, on average, over the long period of time noted above (roughly two years before to six or more years after displacement) compared to what they would have without a husband's displacement. This large, persistent income loss was mainly because the husband went through a substantial loss in annual earnings due to job displacement over the same period of time, and there was no significant role of other insurance mechanisms such as spouses' labor earnings and public/private transfers in compensating the earnings loss of displaced husbands.

Second, even with a large income loss, displaced families practiced considerable expenditure smoothing for a broad set of major expenditure categories related to food, housing, transportation, health care, and education (hereafter referred to as total expenditures). In particular, they did not decrease the expenditures significantly until the year of displacement, notwithstanding the roughly 12% of their annual income loss that they concurrently had by then. Roughly two years after displacement, their expenditures began to fall significantly by five percent, and decreased further for the subsequent years, so that they had nine percent lower expenditures roughly six or more years after displacement, compared to what they would spend without displacement. This sluggish fall in the total expenditures of displaced families is not apparent in their food expenditures alone, which have been heavily relied on by a number of previous studies to represent a family's whole consumption behavior. This study, in fact, like that of Stephens (2001), finds that their food expenditures began to fall even before the occurrence of a husband's displacement and also manifested a similar magnitude of decrease as the initial one even six or more years after displacement. Unlike the food expenditures, however, the other non-food expenditures of displaced families appeared not to have this early decrease in the year of, or prior to displacement found in food expenditures; their expenditures for housing, health care and education, in particular, began to fall quite slowly, which mainly drove the entire sluggish fall in their total expenditures. The decline in their food expenditure turns out to have explained only 15% of the lagged decrease in their total expenditures that began roughly two years after displacement.

Lastly, I find that the main tool of the large expenditure smoothing of displaced families was to decrease their own wealth relative to what they would have without the displacement. They began to decrease their total wealth, even before the occurrence of displacement, and ended up with $172,000 less family wealth, on average, by roughly six or more years after displacement, than what they would have without it. This amount of wealth reduction equals 45% of the sample average of total family wealth in this study, and also covers roughly 55% of their disposable income loss during the same time. Furthermore, the speed of the decline in their family wealth was decreasing over time, which is consistent with their sluggish decrease in their total expenditures described above: A large decrease in their wealth occurred in the initial periods around the occurrence of job displacement to the extent that it roughly offset the total loss in their disposable family income during that same initial period. In turn, the annual decrease in their wealth for subsequent periods became less prominent covering only 40% of their annual disposable income loss that they had since roughly two years after displacement.

The remainder of the paper is structured as follows: Section 1.2 reviews relevant previous studies and highlights the contributions of this study to the existing literature; then, main econometric approaches and data set are described in Sections 1.3 and 1.4 respectively; Section 5 presents empirical results, and Section 6 summarizes the main findings of the paper and discusses respective implications. The conclusion is presented in Section 7.

1.2 Literature Review

As noted earlier, there are a relatively small number of studies exploring the relationship between job displacement and consumption behaviors in contrast to the extensive literature investigating the adverse effect of displacement on workers' wages or earnings.[2] In particular, many of the studies exploring the consumption effect of joblessness have focused on analyzing the short term dynamics of expenditures. Dynarski and Gruber (1997) examine the immediate family expenditure response to the year-to-year change in earnings due to a head's unemployment shock, and find evidence of considerable expenditure smoothing. Gruber (1997) documents a significant role of unemployment insurance benefit in unemployed workers' expenditure smoothing. According to his estimates, the unemployment shock to a head resulted in 6% less family food expenditures within a year, on average, compared to the previous expenditure level before the shock. Browning and Crossley (2008) find that permanently laid-off

[2] See Couch and Placzek (2010) and Kletzer (1998) for summaries of papers related to the income effects of displacement.

workers reduced their total expenditures by 4 to10% within 4 to 9 months after displacement, which represented a loss of two-year normal expenditure growth of temporarily laid-off workers**,** who were considered a control group fully insured against displacement shocks by the authors.

Stephens (2001) and Saporta-Eksten (2014) explore the long-term relationship between job displacement and nondurable expenditures. Both authors find that the households with displaced heads practiced sizable expenditure smoothing through the whole post-displacement period. However, they do not further explore the link between the expenditure smoothing and dissaving /borrowing behaviors explicitly among household with displaced heads, as I do in the current study.

Specifically, Stephens (2001) considers the long-run dynamics of family consumption resulting from a husband's earliest displacement. Unlike the current study using a broad set of expenditure data, he relies solely on food expenditure data, which was the only nondurable expenditure data available in the PSID for 1968-1992, and find a much more moderate and less volatile decrease in food expenditures of displaced husbands' families that continued for more than 6 years after displacement compared to the severe decrease in family income they experienced during the same period.

Using more extensive family nondurable expenditure data in the PSID 1999-2009, Saporta-Eksten (2014) also explores the long-term dynamics of nondurable expenditures before and after job displacement. He finds there was an overall larger decrease in nondurable expenditures before and after displacement than that in food expenditures found by Stephens (2001). But in terms of changing patterns in nondurable expenditures around displacement occurrence, his findings, are similar to those of Stephens (2001) using food expenditures alone.

Saporta-Eksten's (2014) study differs from the current study in several respects. He uses a smaller set of expenditure data excluding durable expenditures and a shorter sample period for the analysis. [3] Furthermore, the author uses only pooled OLS for the analysis without isolating the earliest displacement from other subsequent ones for each household's head. This is an important departure from the current study and that of Stephens' (2001) ; in contrast to Saporta-Eksten's (2014) study, both Stephens (2001) and I focus on the first-observed displacement of a husband and its long-term effects, and mainly use the fixed effect estimation to control unobserved time-invariant component for each household, which has been considered important for mitigating endogeneity issues in the literature (Jacobson, Lalonde, and Sullivan 1993; Stephens 2001; Stevens 1997). For that reason, Stephens' (2001) findings are more comparable to those from this study than the findings from Saporta-Eksten (2014).

The current study is also related to another strand of literature that empirically tests life-cycle theories and the permanent income hypothesis. A number of studies have used micro data to focus on exploring agents' consumption responses against well expected changes in income, and the empirical results diverge (Browning and Collado 2001; Hsieh 2003; Parker 1999; Souleles 1999). Browning and Crossley (2001) argue that such divergent results may largely stem from the differences in magnitudes of the expected income change across those studies; the greater the expected income change relative to life income, the greater its effect on people's current consumption. Accordingly, empirical studies using larger expected income variations would report more favorable results to the predictions of the standard life-cycle theories and the permanent income hypothesis.

[3] The author uses the expenditure data only for nondurables, thereby excluding from the analysis other expenditure data for major durables related to housing and vehicles.

In a more comprehensive approach related to testing life-cycle theories and the permanent income hypothesis, Blundell, Pistaferri, and Preston (2008) identified transitory and permanent income shocks from households' earnings variations by assuming a general earnings equation. They showed that permanent income shocks were much less insured than transitory ones, thereby leading to a larger change in family expenditures.

Along this line of literature, what people expect the future effects of a permanent income shock will be and how such expectation affect their consumption behaviors over a long period are interesting empirical research question, though relatively unexplored. Given that job displacement is a well-documented source of permanent income shocks, the current study provides useful information for examining those questions, through documenting long-term changes in expenditure/saving behaviors of displaced families using rich data on family expenditures and wealth.

1.3 Econometric Approach

I use a simple reduced-form approach, which has been extensively used by other relevant studies exploring the long-run effects of job displacement (Stephens 2001; Stevens 1997). The main model specification is,

$$Y_{it} = D_{it}\beta + X_{it}\gamma + \alpha_i + \delta_t + \epsilon_{it},$$

where Y_{it} is an economic outcome for family *i* in year *t*, such as total annual family income and its sub-components, total annual family expenditures and its subcomponents, and total family wealth and its sub-components.

I use both log and level specifications for the dependent variables in the analysis. On the one hand, using log dependent variables enables me to interpret the regression coefficients in terms of percentage points, which makes it easier to assess the relative impacts of job displacement and is also preferred in the literature on theoretical grounds for analysis regarding income and expenditure dependent variables (Deaton and Muellbauer 1980; Mincer 1974; Zeldes 1989). On the other hand, using level dependent variables is useful when analyzing dependent variables that are additively related, as are many of the dependent variables in my analysis. For example, I can analyze how each disaggregated sub-component contributes to the total job displacement effects on an aggregate dependent variable in dollar terms because the linear regression coefficient for the aggregate level dependent variable is merely the sum of the corresponding regression coefficients for all the aggregate variable's exclusive and exhaustive sub-components. Similarly, I can examine how job displacement affects the family budget constraint because a family's change in wealth equals the difference between income and expenditures. A further advantage of using level dependent variables is that I can include zero or negative values of the dependent variables in the regressions, which is not possible when using log dependent variables.

The vector D_{it} refers to the dummies indicating the first job displacement that occurred to the husband of a household *i* in the past, currently, or in the future at year *t*. Therefore, the coefficients for the job displacement dummies basically capture the mean log or level differences in family incomes, expenditures, or wealth of displaced heads' families from those they would have without experiencing their heads' displacement. X_{it} is a vector of time-varying variables, which is expected to capture dynamic changes in family expenditure preference; X_{it} includes logarithmic family food standard variable[4], quadratics of the head's age,[5] the number of family

[4] These are variables which were generated by the PSID from 1968 to 1992, based on the number of family members by gender and age. By using the same methods, I create the same variables for 1999-2013.

members between the ages of 0 and 5, 6 and12, 13 and17, 18 and 64, and 65 or older. α_i stands for family-specific time-invariant components affecting family behavior. A large body of literature has underscored the importance of controlling the unobserved effect because it is likely to be correlated with the probability of job displacement (Couch and Placzek 2010; Jacobson, Lalonde, and Sullivan 1993; Stevens 1997). To control the unobserved effect, I employ the fixed effect estimation method. δ_t is a vector of year dummies controlling the aggregate time effect, which may affect family economic activities globally. ϵ_{it} is the error term, which is clustered by family to obtain standard errors robust to both serial correlation and heteroskedasticity.

1.4 Data

The Panel Study of Income Dynamics (PSID) is one of the longest nationally representative panel data, and has collected a variety of socioeconomic information annually from 1968 to 1996, and biennially from 1997 on. In particular, it contains a set of variables related to employment history for household heads, from which whether and when a head experienced job displacement can be identified. Hence, the PSID have been extensively used to study the long-term effects of job displacement on family earnings and income in the literature. Although the PSID collected a wide set of diverse family income information, its documentation of family expenditure information was quite limited before 1999; family food expenditure and rents/mortgage payments were the only categories that the PSID collected for most years during this period from 1968 to 1997.

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[5] Substituting a head's potential experience for a head's age in the regressions for diverse income dependent variables also gives very comparable results

Since 1999, however, this limitation of the PSID has been alleviated significantly. The PSID began to collect data on a biennial basis after 1997, enlarging its sample size and the number of variables covered by the survey. Along these lines, it began in 1999 to collect extensive information on family expenditures for major durables and non-durables related to housing, transportation, health care, education, as well as food.[6] Moreover, diverse family wealth information has also been collected biennially since 1999, covering a broad range of family wealth indicators, such as home equity, cash in hand (checking/savings account), net values of vehicles, business or farm, other estates, stocks, annuities, and non-collateralized debts. Therefore, the PSID for 1999-2013 provides a distinctive opportunity to directly explore a topic that has heretofore received scant attention in the literature: how a husband's displacement impacts major family expenditures as well as diverse family income sources in the long-run, and, furthermore, how it changes the net values of a wide range of family savings and assets during the same time.

1.4.1 Constructing Annual Family Expenditures

In the 1999-2013 PSID, there are a number of inconsistencies in timing and time units across the extensive expenditure variables, unlike the variables related to family income and wealth. To minimize these inconsistencies, I develop a proxy for annual expenditures during the previous year of each interview year for each expenditure category.[7] I then calculate total annual family

[6] Since 2005, the PSID has encompassed broader expenditure categories, such as expenses for home repair/maintenance, furnishing, clothing, and entertainment/vacation. For consistency of analysis, those variables are not used in the main analysis of this study.

[7] The variables related to annual family income in the PSID 1999-2013 mostly refer to the year prior to each survey year. However, all the variables related to family wealth refer to the time around the interview month which is mostly between March and May for each survey year, while the reference time for expenditure variables differs widely across expenditure categories. In this study, I consider each wealth variable as a proxy for the amount of wealth accumulated by the end of the year prior to each survey year. Similarly, I construct annual expenditure variables for each expenditure category to obtain a proxy capturing, as closely as possible, what a family spent for

expenditures for food, housing, transportation, health care, and education by summing up all the corresponding sub-component expenditure categories.

The specific sub-components summed up for each category are the following: the food expenditures include the annual expenditure for food-away-from-home, as well as that for food-at-home, which also includes the annual monetary value for delivered food and food stamps. The housing expenditures are calculated by summing up annual rent or mortgage payments, home utility expenses, such as electricity and heating, home insurance premiums, and property taxes.

The transportation expenditure refers to the summation of average annual expenses for purchasing or leasing car(s), other annual car operation costs such as fueling, car repair or maintenance, car insurance premiums, and annual public transportation costs for using buses, taxis, and trains. Note that the mortgage payments within the housing expenditures and car-purchasing expenses within the transportation expenditures encompass some expenditures for durables as well as non-durables.

As for the health care expenditures, I sum the annual expenses for doctor appointments, surgery, dental treatment, hospitalization, nursing care, prescription medicine and health insurance. Lastly, the education expenditures encompass both annual school- related costs and child care costs. All the expenditures described above are deflated to 2013 dollar terms using CPI-U-RS from the Bureau of Labor Statistics.

Table 1.C1 shows descriptive statistics for annual family expenditures for food, housing, transportation, health care, and education, with all the corresponding sub-component categories for the main sample. Housing expenditures account for the largest portion of these expenditures, about $21,000 on average. The second and third largest categories are transportation and food

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the year prior to each survey year. Refer to Appendix A for a more detailed description of the procedures for generating annual expenditure variables.

expenditures, totaling $15,000 and $10,000, on average, respectively. The largest category of total annual family expenditures, encompassing food, housing, transportation, health care, and education adds up to $56,000 on average.[8]

1.4.2 Job Displacement

Since 1968, the PSID has contained a series of questions that ask household heads about the reasons why they left their previous jobs or employers, if their jobs were changed recently. [9] Based on the answers to those questions, job separation due to company closings, being laid off or fired is defined in this study as job displacement.[10] Thus, I focus on the job separations which can be considered exogenous shocks to workers. Note that I employ the conventional definition of job displacement found in the previous studies on job displacement using the PSID (Stephens 2001; Stevens 1997).

By using those questions to investigate the causes of heads' recent job separations, I identify a husband's experience of job displacement and its timing for each household in the following

[8] One issue in constructing larger expenditure categories by adding up multiple sub-components is that the greater the number of sub-components that are summed up, the worse the missing data problems become. To mitigate this missing data problem, I perform selective imputations for missing data of some variables such as home insurance, property tax, and car insurance, which have a relatively large amount of missing data. Furthermore, I do not include water/sewage expenses in housing expenditures because those expenses have a large amount of missing data, while accounting for a relatively small portion of the total expenditure for housing. Table 1.C1 also shows the statistics of the variables containing the imputed variables above, and they appear to be comparable to those of the non-imputed ones. The total expenditures shown in the bottom of Table 1.C1 are constructed by using the imputed variables to alleviate the missing data problem.

[9] More specifically, in the 1999 and 2001 waves, the PSID asked currently-working heads about the reasons for their job changes if they had worked in other jobs in the year prior to the interview year. It also asked currently-non-working heads about their most recent job, or up to two jobs that they had had in the year prior to the interview year, and recorded reasons why that job or jobs had been terminated. Since 2003, the PSID has begun to investigate up to four recent jobs which a head had had in the year prior to the interview year whether the head is currently working or not, and recorded the reasons for job separation(s) if the head no longer had the job(s) at the time of the interview.

[10] According to Boisjoly, Duncan, and Smeeding (1998), of the respondents answering that they were laid off or fired in the PSID for 1968-1992, roughly 16% had actually been fired.

way: [11] In a certain interview year, if a husband responds to these questions by stating that he had stopped working in a job he held the prior year, due to a company-closing, being laid off or fired, the husband is considered to have experienced job displacement between January of the year prior to the interview year and the interview month of the interview year (mostly from March to May). If a husband experienced multiple job displacements through multiple interview years, only the first observed job displacement is accounted for in this study, as Stephens (2001) and Stevens (1997) do. A primary reason for focusing on the first job displacement is because a household head's subsequent displacements following the head's first one were found to play a central role in generating the persistent earnings loss of displaced heads (Stevens 1997). This finding implies that the subsequent job losses may be an important consequence of the first one. Thus, this study focuses on exploring the effect of the earliest job displacement of a husband since he started to be interviewed by the PSID.

This identification strategy for the first occurrence of job displacement works well before 1997, when the PSID collected the data annually. Since 1997, however, when the PSID began to collect the data biennially for each household head, it has systemically generated certain periods (mostly between June and December of odd-numbered years between 1997 and 2013), during which it is not possible to determine whether or not the head experienced job displacement. This is because the PSID has continued to document a head's reasons for job separations only for separations occurring in the previous year or current year of each interview year, even after it began to survey biennially after 1997.

This data limitation raises several issues that should be kept in mind when the regression results are interpreted in Section 1.5 of this paper. First, the effects of job displacement occurring

[11] The sample of this study, which is discussed later, consists of only households whose head has a spouse, and for these households, the husband is considered a default head by the PSID

in the second half of odd-numbered years during the 1997-2013 period cannot be identified. Hence, the estimated effect of job displacement in this study should be regarded more as an average effect accounting only for the displacements occurring in even numbered years or in the first quarter or first half of odd-numbered years between 1997 and 2013. Second, the sample of non-displaced husbands from the period of 1999 to 2013 identified in this study would include a fraction of husbands who were actually displaced in the second half of odd-numbered years during that earlier commencing period, 1997 to 2013. This would underestimate the effect of a husband's job displacement on family expenditures, and the degree of underestimation would depend on the percentage of the misidentified non-displaced compared to the correctly-identified non-displaced.[12] Third, some instances of identifications of the first job displacement occurrence may be incorrect as they may have occurred in the second half of earlier odd-numbered years between 1997 and 2013.[13] [14] [15]

1.4.3 Sample Selection

The main sample of this study consists of households which have both a husband (defined as the household head by the PSID) and his wife, whose ages are between 25 and 65 for each

[12] In fact, the potentially misidentified group appears to account for just a small portion of the whole non-displaced group. Based on the observations in the second half of even-numbered years during 1997-2013, only 13% of heads experienced job separations, and among those separations, only 13% were due to job displacement. Hence the degree of underestimation is expected to be moderate. To further examine the degree of underestimation, I also performed a quasi-experiment, by only using the data of odd-numbered years (or even-numbered years) from the annual data set of the PSID during 1968-1997; the results show that the degree of underestimation is quite small. More detailed results are presented in Table 1.C3.

[13] These cases also seem to be minimal in number, according to the discussion in Footnote 12.

[14] As a way of mitigating the misidentification problems for displaced husbands described here, I specify the husbands who were likely to experience displacement in the second half of the odd-numbered years between 1997 and 2013. This likelihood is judged by using other information on whether the head had some unemployed periods or received unemployment benefits around that time. I then drop some of the heads' observations which come after the period when they were suspected to be displaced. Even with this further sample restrictions, I find that all the main empirical results in this study hold robustly.

[15] See Table 1.C2 for detailed numbers of the first job displacement dummies by year, which are used by this study.

sample year.[16] More specifically, for each male head (or husband) observed by the PSID at least twice during the period 1968-2013**,** I specify the earliest survey year when he reported a spouse satisfying the age criteria above. Then, from that survey year on, I include him and his family in the sample of this study unless he divorces, separates, misses/leaves the survey, or either his or his wife's age become over 65. Furthermore, I use only the households whose heads' labor earnings and total family income were positive for the whole observed period. Thus, the main sample of this study refers to relatively stable families in terms of their family structure, continuity of survey participation, and husband's attachment to the labor market. This sample stability is expected to be helpful for identifying the pure effects of the first job displacement of a husband, as Stephens (2001) discusses.

Among the households selected based on the criteria described above, only the families which were observed during the period 1999-2013 are used in the analyses because the main analysis of this study is on the various family expenditures and wealth data of the PSID during the same period. Moreover, to obtain a more consistent measure of job displacement occurrences, I drop the families which reported their male heads' displacement in the PSID waves prior to 1997, when the PSID collected data on an annual basis. As a result, the displaced husbands in the main sample of this study refer to those who were first observed to have job displacement in the 1997 PSID wave or later ones, while satisfying all the sample selection criteria listed above. Along the same lines, the non-displaced husbands in the main sample of this study indicate those

[16] I use only national representative samples, the SRC (Survey Research Center) and immigrant samples of the PSID. That is, I exclude the SEO (Survey of Economic Opportunity) sample, which oversamples low-income families, and the Latino sample, which was dropped after 1995. The SRC sample refers to the nationally representative families who were selected in 1968 and their subsequent split-off families. The immigrant sample refers to national representative immigrants who were added to the PSID since 1997 to reflect the large increase in immigrants in the U.S. The PSID provides longitudinal (and therefore time-varying) family weights accounting for both the core and immigrant samples. From these weights, I calculate two types of fixed family weight for each family: first, I select the last observed family weight within a family. Second, I average out the family weights over time within a family. Both weights lead to quite comparable results. All the results shown hereafter are ones based on the first type of family weight.

who satisfy the same selection criteria as the displaced ones, except that they had never reported the experience of job displacement.

I further drop from the main sample the observations that have missing data for any variables used in the regression analyses of this paper. Consequently, the number of total observations of the main sample is 9,924 for 2,245 families.

1.4.4 Descriptive Statistics for the Non-displaced and Displaced

Table 1.B1 presents the summary statistics for non-displaced and displaced families for the PSID waves 1999-2013. Those for the latter group are calculated by pooling all the *ex post* observations after displacement, and those for the former group are based on the observations of the households whose heads have never been displaced, or have not yet been displaced during the period 1998-2012 (based on the PSID waves 1999-2013).

Table 1.B1 shows that the displaced husbands and their spouses were, on average, slightly older than the non-displaced ones. The mean schooling years for the former were also somewhat lower than those of the latter. In addition, the displaced husbands were slightly less likely to be white. The other demographic variables, such as the number of children show fairly comparable mean values for both groups.

This study finds no significant difference in the probability of working in the manufacturing industry in a comparison of the two groups. The displaced husbands were more likely to work in blue-color jobs, however, and less likely to have a job covered by a union contract or to be a union member.

When comparing income levels, the difference between the two groups becomes more salient. The displaced husbands experienced rather large reductions in annual earnings and total family income, on average, after being displaced. Consequently, the proportion of the spouse's earnings in total family income increased considerably after a head's job displacement. A significant increase in the mean of public transfer is observed too, but the magnitude of the compensation is fairly small compared to the total income loss due to job displacement.

The next panel in Table 1.B1 shows the statistics for family expenditures. With the large reduction in family income, the displaced families also seem to have reduced various family expenditures on average, but the magnitudes of decreases were relatively moderate compared to the average income loss. In addition, considerable reductions in the average value of various sub-components of family wealth are found in the panel at the bottom of Table 1.B1. Taken together, these observations imply that much of the expenditure smoothing was executed by the displaced families and their substantial dissaving/borrowing (or decrease in active saving) played an important role in alleviating the reduction in their expenditures.

Referring only to those unconditional means, however, is not sufficient to isolate the pure effects of job displacement. To clarify the role of job displacement further, I employ the fixed effect estimation method, which controls husbands' age effects, change in age compositions of family members, year effects, and each family's time-invariant unobservable components, as discussed in Section 1.3. The regression results follow in the next section.

1.5 Results

Figure 1.A1 presents both the main findings of this paper and how they compare to previous results in the literature. Specifically, the left panel A shows the average evolution of a displaced husband's annual earnings, his family's annual total income, and food and total expenditures

during the period of 1998 to 2012 (the 1999-2013 waves of the PSID); the point estimates are in panel A of Table 1.B2 and Table 1.B3 which are later discussed. For comparison, the right panel B shows what Stephens (2001) finds by using the PSID for 1968-1992. As can be seen, all the corresponding graphs to those in panel A of Figure 1.A1, except for total family expenditure, are found in panel B of the same figure. For both panels, point *t* on the x-axis indicates the year when the first displacement occurred, while the dotted horizontal line at zero on the y-axis implies the level of income or expenditures that a husband or his family would have without displacement. Note that in the remainder of this paper, all the quantitative comparison statements for displaced families made from regression results are based on this counter-factual horizontal line, although, for the sake of brevity, I will generally not repeat this comparison criterion explicitly.

The two graphs at the bottom of panel A of Figure 1.A1 first show that a displaced husband had a sizable and persistent earnings loss during the period 1998-2012, on average, and his earnings loss also led to a considerable fall in his total family income for the same period. Specifically, a displaced husband's annual earnings began to decline by 12% at two to two and a half years before displacement (*t-2*), and further decreased from the year of, or within a half year before displacement (*t*) through one and a half to two years after displacement (*t+2*), ending up with a 36% lower level compared to what he would earn without displacement. After three and a half to four years after displacement (*t+4*), his earnings showed a recovery pattern somewhat, but still remained at a 23% lower level even five and a half or more years after displacement (*t+6 plus*). The displaced family's total annual income also showed a similar dynamic pattern, although the magnitude of decrease was smaller in general; a displaced family had 8%, 13%, 23%, 20%, and 18% less annual total income during the periods *t-2*, *t*, *t+2*, *t+4*, and *t+6 plus*, respectively, relative to what it would have without its male head's displacement. All those decreases in a displaced husband's earnings and his family income are statistically significant (see Column (1) and (3) in Panel A of Table 1.B2), and the decreasing patterns also appear comparable with those previously found by Stephens (2001) using the PSID 1968-1992 (see the graphs with square- and triangle shaped points in panel B of Figure 1.A1).[17]

The smaller percentage fall in the total income of a displaced family compared to that of a displaced husband's earnings is partly due to the fact that a husband's earnings only account for 65-70% of his total family income in the main sample of this study (composition effects). Moreover, the lesser fall might be partly induced by some increases in other family income sources, such as the spouse's earnings and public/private transfers, which offset a fraction of the earnings loss of a displaced husband (insurance effects). The two effects are difficult to disentangle from each other based solely on the results from the log dependent variable analyses shown in panel A of Figure 1.A1 or Table 1.B2. In the level dependent variable analyses presented in panel B of Table 1.B2, however, we can see how each family income source was affected by a husband's displacement and ended up being aggregated into total family income in dollar terms, so that we may compare the composition and insurance effects more clearly.

Specifically, columns (1), (2), and (3) in panel B of Table 1.B2 demonstrate the evolution of job displacement effects on husband's annual earnings, the sum of husband's and wife's annual earnings, and their total family annual income in 2013 dollar terms, respectively. Unlike the log dependent analyses seen in panel A, the level dependent variables for those columns in panel B have additive relationships with each other, as do all the corresponding regression coefficients as well. Hence, the differences in the coefficients in columns (1) and (2), and columns (2) and (3) in

[17] Note that I use the before-tax total family income, while Stephens (2001) uses the after-tax one, which was imputed by the PSID until the 1992 wave. The after-tax family income was not provided in the 1999-2013 PSID.

panel B indicate each individual effect of a husband's displacement on his wife's earnings and the other family income sources, respectively, in dollar terms. [18]

According to the results presented in panel B of Table 1.B2, the gap in the percentage fall between a displaced husband's earnings and his family income appears to be mainly due to the composition effect rather than the insurance effect. Comparing the level differences in coefficients between the columns in panel B of Table 1.B2, both a wife's earnings and other family income of a displaced husband turn out to have decreased slightly in dollar terms rather than increased during the period 1998 to 2012, although those differences are not statistically significant. This observation suggests that other family income sources played a minimal role in buffering the earnings shock of displaced husbands during the period 1998-2012.[19]

Even with such a substantial reduction in the total family income of displaced families as shown in Figure 1.A1/Table 1.B2, they decreased their annual total expenditures only moderately during the period 1998-2012, as is also seen in the graph with diamond-shaped points in panel A of Figure 1.A1.[20] In particular, a significant fall in their total expenditures appeared far later compared to that in their total income. For instance, the displaced families did not decrease their total expenditures significantly during the period between *t-2* and *t* (see column (1) in panel A of Table 1.B3 for the standard errors), despite the fact that they had a significant fall in their total annual income by 11% on average for the same period, as seen in Table 1.B2. Roughly two years after a husband's displacement (*t+2*), the total family expenditure started to

[18] Other family income sources include the earnings of family member(s) other than husband and wife, public/private transfers, interests/dividends from family assets.

[19] This observation seems inconsistent with Stephens (2002)'s finding based on the 1968-1992 PSID that a displaced husband's wife increased her labor supply significantly and persistently before and after her husband's displacement. One factor driving this inconsistency might be the difference in the growth of female labor market participation in the United States between the periods 1968-1992 and 1999-2013; while there was a strong increasing trend in the female labor force participation rate (FLFP) during the former period, the growth in FLFP stagnated substantially during the latter period (Lee 2014).

[20] As explained earlier, total expenditures include expenditures for food, housing, transportation, health care, and education.

statistically significantly decline by 5%, and further decreased with stronger statistical significance for the subsequent period, so that the displaced heads' families showed 9% less total expenditures roughly four or more years after displacement (i.e., *t+4* and *t+6 plus*), relative to what they would spend without a husband's displacement.

It is notable that this sluggish fall in total expenditures of displaced families is difficult to detect in what Stephens (2001) finds about their food expenditure behaviors alone by using the PSID 1968-1992. As seen in the graph with x-shaped points in panel B of Figure 1.A1, he finds a significant earlier drop in their food expenditures even before the occurrence of displacement (*t-2* and *t-1*). In addition, in his study, the degree of decrease in their food expenditures changed little over the whole post-displacement period, after their food expenditures had fallen by 9% in the year of displacement (*t*). Consequently, displaced families showed a flat trend in their food expenditures between the periods *t* and *t+6 plus*. Stephens (2001) regards this flat pattern as evidence of consumption smoothing of displaced families who anticipated much of their future income loss.

As for the food expenditures alone, the results of this study are comparable to those of Stephens' (2001) for displaced families. For example, the graph with x-shaped points in panel A of Figure 1.A1 (or column (2) in panel A of Table 1.B3) shows that displaced families had a significant early drop in their food expenditures over the initial period of time around the occurrence of displacement during the period 1998-2012; their food expenditures fell by 5% and 8% at *t-2* and *t*, respectively, and those declines were statistically different from zero. Moreover, even roughly six or more years after displacement (*t+6 plus*), the decrease in their food expenditures appeared to be as much as in the earlier period *t*, although the full trajectory of their food expenditures between *t* and *t+6 plus* seems not as flat as observed in Stephens' (2001) results during the corresponding period. [21]

The initial decline of food expenditures, and their leveling off thereafter suggests that the sluggish fall in total expenditures including food expenditures would be driven by other non-food expenditures. Indeed, the other regression results using log dependent variables for non-food sub-expenditures, shown in column (3)–(5) in panel A of Table 1.B3, displays a tendency for those non-food sub-expenditures to decline more slowly compared to the food expenditures. As discussed earlier regarding panel A of Table 1.B2, however, it is hard to clearly see how each sub-expenditure category contributed to the dilatory decrease in total expenditures through the log dependent variable analyses. Moreover, the log dependent variable analysis has to drop some observations with zero values for each sub-expenditures regression, which leads to different sample sizes across the sub-expenditures analyses, as shown in panel A of Table 1.B3. This constraint also impedes a fair comparison among them.

To mitigate the limitations of the log dependent variable analysis described above, I put the corresponding results of the level dependent variable analyses in the lower panel B of Table 1.B3, as I do in Table 1.B2. Specifically, columns (1)-(5) in panel B of Table 1.B3 show the level effects of a husband's job displacement in 2013 dollar terms on total annual family expenditures and its exhaustive sub-components, such as annual family expenditures for food, housing, transportation, and health care/education, in that order. Correspondingly, I also depict five panels A through E in Figure 1.A2 to visualize each result in columns (1)-(5) in panel B of Table 1.B3, with 95% confidence intervals. As the measure of total family expenditures is the summation of

[21] The smaller decreases in food expenditures of displaced families between *t+2* and *t+4* compared to the previous period appeared to be partly affected by a significant increase in their food stamp usage since *t+2*, during the period 1998-2012. Based on my replication of Stephens (2001) using the 1968-1992 PSID, however, no significant role of food stamp usage is found in food expenditure smoothing for displaced families, during the period 1967-1991.

the other sub expenditures above, each coefficient of job displacement dummies in column (1) of panel B is also the simple summation of the corresponding coefficients of the other sub-expenditure regressions from column (2) through (5) of panel B. Thus, it is possible to compare each sub-expenditures' contributions directly from panel B of Table 1.B3, as can similarly be done in panel B of Table 1.B2.

Overall, panel B of Table 1.B3 shows comparable dynamic patterns for each sub-expenditure as well as total expenditures of displaced families with what we see in upper panel A of the same table. For example, column (2) in panel B of Table 1.B3 (or panel B in Figure 1.A2) shows an early significant fall in displaced families' food expenditures as does the same column in upper panel A; for both early periods *t-2* and *t*, displaced families statistically significantly reduced their food expenditures by \$800 and \$900, respectively. When it comes to the total expenditures for the same period (column (1) in panel B of Table 1.B3), however, the declines appear not to be statistically different from zero, which is also consistent with the results shown in the same column of the upper panel. Referring to the corresponding estimates in columns (3) through (5) in panel B of Table 1.B3 (or panels C, D, and E in Figure 1.A2), the statistically insignificant fall in total expenditures at *t-2* and *t*, turns out to have been mainly because displaced families did not have a significant fall in the other non-food sub-expenditures during the same periods, for housing, transportation, health care, and education.[22]

From *t+2* through *t+6 plus* in panel B of Table 1.B3 (or in Figure 1.A2), in contrast, displaced families began to significantly decrease their total expenditures by \$3,000-\$6,000, and in particular, this lagged fall in their total expenditures were mainly due to the sluggish

[22] For *t-2*, the displaced families' expenditures for transportation and health/education of displaced families increased rather than decreased point estimate-wise. This pattern suggests that the main purpose of the earlier decrease in their food expenditures might be to adjust the expenditure share across multiple expenditure categories in response to the increased risk of future income loss.

downswing in their expenditures for housing, health care, and education during the same time. Specifically, at *t+2*, displaced families' total expenditures decreased by $3,000, and the decline in their sub-expenditures for housing and health care/ education mostly accounted for the reduction in total expenditures. For the subsequent periods, *t+4* and *t+6 plus*, displaced families spent $6,000 less on total expenditures, on average, and their sub-expenditures for housing, and health care/education accounted for 50-30% and 30-50% of the total decrease in their total expenditures, respectively. It is notable that only 15% of the entire decline in their total expenditures from *t+2* through *t+6 plus* can be attributable to the fall in their food expenditures, which a number of previous studies have relied on heavily to proxy people's consumption behavior.

These dollar amounts of declines in total expenditures of displaced families shown in column (1) in panel B of Table 1.B3 can be further compared to their family income losses in dollar terms for each corresponding period previously shown in panel B of Table 1.B2. The comparison illustrates a large gap between the two: On the one hand, for example, displaced families had had $30,000 lower total income (before tax) annually, on average, over roughly eight years for the post-displacement period (i.e., from *t* through *t+6 plus*).[23] This amount nearly equals the loss in their disposable family income of $24,000.[24] On the other hand, they decreased their annual total expenditures only by $4,000, on average, for the same post-displacement period, based on column (1) in panel B of Table 1.B3. Therefore, the average gap between their disposable income loss and total expenditures decline appeared to be about $20,000 in an accounting sense.

[23] In this study, 8.7 years are the conditional sample mean of the maximum number of years that passed since the husband's first displacement, among the husbands having been first displaced roughly six or more years ago.

[24] The average tax rate for the sample of this study is 20 percent based on NBER TAXSIM9, which is a tax calculating program provided by NBER (http://users.nber.org/~taxsim/taxsim-calc9/index.html),

This gap can be explained either by a fall in the other sub-expenditures which are not included in the measure of total family expenditures of this study, or by their self-funding activities through reducing the net value of their own family wealth. While the former factor cannot be analyzed because of the lack of other expenditure information in the 1999-2013 PSID, the latter factor, self-funding behaviors of displaced families, can be explored by using extensive family wealth data in the same data set.

Table 1.B4 shows in detail how a husband's job displacement affected various components of his family wealth from *t-2* through *t+6 plus*. As in the previous analyses for the income/expenditures effects of job displacement, all the interpretations of the coefficients for job displacement dummies will be based on counter-factual wealth level which a family would have without its husband's displacement. But, for brevity, I will also frequently omit to explicitly mention the comparison criterion when describing the following results.

Column (1) in Table 1.B4 first shows the evolution of total family wealth level of displaced families in 2013 dollar terms. The displaced families appeared to considerably reduce their own wealth for their expenditures smoothing: Their total family wealth declined by $172,000 by the end of *t+6 plus*, on average, compared to what they would have without displacement, and this amount of reduction accounts for 45% of the average total family wealth of families in this sample. More specifically, they decreased the net value of their total family wealth by $11,000 annually, on average, from the end of *t-2* through the end of *t+6 plus*.[25] This average annual decline in their total family wealth roughly covers 55% of the unexplained gap of $20,000 between their disposable income loss and expenditures fall from *t* through *t+6 plus*, as previously computed based on the results in Table 1.B2 and Table 1.B3.

[25] The difference in family wealth effects of job displacement between *t-2* and *t+6 plus* based on column (1) in Table 1.B4 is -$113,921, whose standard error is 45,538.

Moreover, the decreasing pattern in their total family wealth is consistent with the sluggish fall in their total family expenditures discussed in Table 1.B3; displaced families decreased their total family wealth more intensively during the initial periods before the end of *t*, than they did during the latter periods after the end of *t*. Specifically, they had already reduced their total wealth by \$58,000 at the end of *t-2*, and further decreased it by \$29,000 by the end of t point estimate-wise, and thus their total family wealth was \$87,000 less at the end of t, on average, compared to what they would have without displacement. Hence, of the total average reduction in their total family wealth of \$172,000 by the end of *t+6 plus*, roughly 50% took place by the end of *t*.

Notably, this early decrease in their total family wealth appears to have covered most of their family income loss which they had during the same period. Between the ends of *t-2* and *t*, for instance, they decreased their total wealth by \$14,500 per year on average, point estimate-wise, according to column (1) in Table 1.B4, and this decline roughly equals the loss in their *disposable* annual total income of \$15,000 at t, which is calculated based on the results in column (3) in panel B of Table 1.B2.[26] This observation suggests that not only the expenditures included in this study, but also other expenditures not covered by this study were likely reduced only slightly by displaced families during the initial period around their husband's job displacement occurrence.

In addition, the speed of the decline in total family wealth of displaced families decelerated for the subsequent periods after *t*, and this slowdown also corroborates the finding of their dilatory reduction in total expenditures. Specifically, for about eight years, between the ends of *t*

[26] The difference in family wealth effects of job displacement between *t-2* and *t* based on column (1) in Table 1.B4 is -\$28,716, whose standard error is 23,114.

and *t+6 plus*, they decreased their total family wealth by $10,000 per year, on average.[27] This amount covers only 40% of the average annual loss of $27,000 in the total disposable family income that they had had since *t+2*.[28] Inversely, this computation implies that the other 60% of the disposable income loss of displaced families was offset, in an accounting sense, through a decrease in their expenditures since *t+2*. This conjecture is in keeping with the previous finding that displaced families decreased their total expenditures relatively slowly; a significant fall in their total expenditures had begun to appear in *t+2*.[29]

Columns (2) through (8) in Table 1.B4 further present the individual regression results for each sub-component of total family wealth, such as the net value of housing, vehicles, cash in hand (checking/saving), stocks, business/farm, individual retirement account (IRA), and other assets, in 2013 dollar terms.[30] As in panel B of Table 1.B3, all the coefficients in column (1) of Table 1.B4 are the mere summation of the corresponding ones in column (2) through (8) in the same table. Hence, we can compare each sub-component's contribution to the decrease in total family wealth for each period.

As for the early decrease in total family wealth by the end of *t*, for example, its decrease in displaced families was largely driven by falls in the (net) values of their home equity, stocks, and family business/farm. In particular, the early reductions in the net values of their stocks and business/farm were so large that the average dollar amounts of both declines by the end of *t* were up to 50- 60% of each corresponding mean dollar value of the entire sample in this study. In turn,

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[27] The difference in family wealth effects of job displacement between *t* and *t+6 plus* based on column (1) in Table 1.B4 is -$85,205, whose standard error is 36,103.

[28] It is calculated based on the estimates in panel B of Table 1.B2

[29] This further implies that the total expenditures of this study related to food, housing, transportation, health care and education have relatively low income elasticity compared to the other expenditures that are not covered by this study: as seen earlier, displaced families decreased their expenditures for food, housing, transportation, health care and education only by $5,000 per year, on average, from *t+2* through *t+6 plus*. This accounts for less than 30% of $17,000, which is the average difference between disposable income loss and reduction in their family wealth.

[30] The vehicles include cars, trucks, a motor home, a trailer, or a boat, while the other assets include the net values of other estate and non-collateralized debts

the decrease in the net values of their vehicle and checking/saving accounts was relatively moderate by the end of *t*, while the net values of their IRA appeared to even increase during the same time, although the estimate for the IRA increase shows relatively weak statistical significance.

These decreasing patterns across the subcomponents of family wealth, however, had changed since the end of *t*. On the one hand, specifically, the further decrease in total family wealth of displaced families was largely driven by the additional fall in the net values of their checking/saving accounts, stocks and IRA taking place since the end of *t*. In particular, the net values of their checking/saving accounts and IRA decreased so prominently from the end of *t* through the end of *t+6 plus* relative to the preceding period, that their contributions to the entire decrease in their total family wealth at the end of *t+6 plus* became much larger. On the other hand, in contrast, there was no further significant decrease in the (net) values of their home equity and business/farm from the end of *t* through the end of *t+6 plus*, although both of them played a central role in the early decrease in their total family wealth by the end of *t*. As a result, the contribution of the decline in the (net) values of their home equity and business/farm to the decrease in their total family wealth became much smaller at the end of *t+6 plus* than the earlier period.

The relatively early decline by the end of *t* in the (net) values of home equity, stocks, and business/farm of displaced families can be viewed as their early downsizing of the share of relatively illiquid/risky assets in response to their increased future income risk, by putting less money into them than they would have done without the displacement. However, given that their housing expenditures continued to decrease over time as seen in column (3) of Table 1.B3, the early decrease in their home equity and its subsequent flat path through *t+6 plus* appears somewhat puzzling. Through additional analyses on the change in their home price, which is the summation of home equity and debt for home, and the probability of their moving to a lower-/higher priced home before and after the displacement, I find that their home price began to decline at *t-2* as their home equity did, but also continued to decrease over time through *t+6 plus*, as opposed to the non-decreasing trend in their home equity during the same time.[31] Moreover, such patterns in their home price were partly due to a lower likelihood of a move to higher-priced homes by *t*, and a higher likelihood of a move to lower-priced homes after *t* through *t+6 plus*, relative to their counterparts who had not experienced job displacement.[32] These findings partly explain the seemingly inconsistent pattern of changes in home equity and housing expenditures of displaced families: The displaced families became more likely to move to lower-priced homes with lower-home debt over time after displacement, than they would have done without displacement. As a result, their average home price kept going down over time, which made the fall in their average housing expenditures (mainly the mortgage payments) more salient as time lapsed, whereas their home equity did not further decrease because their home debt also decreased commensurate with the drop in home price.

1.6 Summary and Discussion

The results of this study translate into two main findings: First, the households with displaced husbands practiced considerable expenditure smoothing mainly by reducing a large share of their own wealth during the period 1998-2012. Second, the total expenditures of displaced families fell sluggishly, although there began to be a significant fall in total family income much earlier,

[31] Refer to Table 1.C4 for detailed regression results.
[32] Refer to Table 1.C5-Table 1.C8 for detailed regression results.

even before the occurrence of displacement. In this section, I briefly review the main results related to each finding, and then further discuss their respective implications.

1.6.1 Displaced Families' Expenditure Smoothing by Reducing Their Own Wealth

This study shows that during the period 1998-2012, a displaced family had18% less total annual income, on average, over six or more years after a husband's displacement (i.e., from t to *t+6 plus*), relative to what it would have without a husband's displacement. For the same time, despite the substantial loss in family income, the displaced family decreased its annual total expenditures only by 7%, on average, which total expenditures encompass a broad set of major expenditure categories related to food, housing, transportation, health care, and education. As a result of this large expenditure smoothing, displaced families ended up with $172,000 less family wealth, on average, at the end of *t+6 plus,* which accounts for 45% of the average net value of family wealth in the whole sample of this study. Particularly for the post-displacement period from the end of *t* through the end of *t+6 plus*, for example, families decreased their total wealth by $11,000 annually, on average, and this amount roughly offset 55% of the loss of its disposable annual income during the same time.

These findings of this study are consistent, overall, with Stephens (2001), in that a relatively moderate fall in expenditures of displaced families was observed. Specifically, Stephens (2001), by using the 1968-1992 PSID, finds a moderate fall in food expenditures over a long period for displaced families, compared to the large income loss they persistently had. The author views the finding as evidence of consumption smoothing of displaced families. Along the same line, the current study reexamines the long-term expenditure behaviors of displaced families by using richer information about their family expenditures, and the results largely confirm Stephens' (2001) argument: even with a broad set of expenditures categories including housing, transportation, health care, and education, as well as food, displaced families turned out to practice considerable expenditure smoothing during the post-displacement period. This finding of the current study again calls attention to the importance of investigating the expenditure effects of job displacement for assessing the true welfare loss caused by job displacement. As Stephens (2001) argues, simply referring to the income loss induced by displacement is likely to overestimate the actual long-run impact of job displacement on people's welfare.

Through further analyses on wealth change of displaced families, however, this study highlights the fact that such a large expenditure smoothing of displaced families was enabled at the cost of a large share of their family wealth that they would have without displacement. This finding invites consideration of two scenarios where a husband's job displacement could severely damage a family's welfare: first, a family with a low level of wealth, thus presumably also having a relatively tight borrowing constraint, would have to decrease their family expenditures more drastically; and, second, even a family with a relatively high level of wealth likely becomes more financially vulnerable over time once it experiences a husband's job displacement, because a displaced family tends to have an early large depletion of, and gradual subsequent declines in its family wealth, as seen in Table 1.B4.

This study further suggests that the existing public assistance program did not play a substantial role in mitigating the two problems described above in Table 1.B2. In the Table 1.C9, an extended version of panel B in Table 1.B2, which presents the level job displacement effects for each sub-component of family income, it is observed that there was statistically significant increases in public transfers in dollar terms for displaced families, on average, at *t*, *t+2*, and *t+4*, but the magnitudes of the increases were trivial. In addition, a large part of the increase was concentrated on the initial periods around the displacement occurrence at *t*, and thus displaced families were likely to have less public transfer over time after displacement.

1.6.2 The Sluggish Fall in Displaced Families' Expenditures

Another noteworthy finding of this paper is that the decrease in displaced families' expenditures became more salient over time. In fact, they did not decrease the total family expenditures significantly during the initial period around displacement, although their total income was significantly lowered by 12%, during the same time. A significant fall in total expenditures of 5% began to appear roughly two years after displacement (*t+2*), and the total expenditures decreased further point estimate-wise for the subsequent years, resulting in 9% less annual total expenditures from *t+4* through *t+6 plus*. I further show that this lagged fall would appear in general even with a broader measure of family total expenditures than that of the current study, by tracking their decreasing patterns of total family wealth and comparing it with their concurrent income loss in an accounting sense.[33]

This sluggish fall in displaced families' total expenditures is a novel finding, which is not found in their food expenditure behavior alone, as shown in both the current study and Stephens (2001) (see Figure 1.A1). Furthermore, the interpretation of the lagged fall in total expenditures of displaced families seems more complex than that of the flatter trend in their food expenditures with an early significant fall, under classical assumptions of standard life cycle theories/permanent income hypothesis. Why, for example, did the displaced families not begin to reduce their total expenditures earlier so as to make their consumption path more even, even

[33] Specifically, they appeared to decrease their wealth level enough to roughly cover the entire income loss they had by t, whereas the decrease in their wealth for subsequent periods only covered 30% of the income loss that they had during the same time.

though a large number of them appeared to expect future income loss and prepare for it in advance by reducing some part of their expenditures and wealth even before displacement?

There are at least two possible explanations for this lagged fall that would answer this question. First, the lagged decrease in their total expenditures could be attributable to the difference in adjustment costs across sub-expenditure categories of the total expenditures. For example, the food expenditures can be adjusted relatively easily while the expenditures for housing generally require more time and money costs for adjustment. Health care and education expenses are also relatively difficult to adjust if some family members are getting ongoing treatments or already enrolled in schools. This different adjustment costs can lead to a large difference in the timing of change across food expenditures and the other sub-expenditures shown in Table 1.B3. Thus, no matter how precisely the displaced families expect the future income loss due to their heads' displacement, they might optimally choose to postpone a decrease in expenditures for some categories with high adjustment costs in order to minimize it, which lead to a lagged decrease in their total expenditures.

The second possible explanation is that their expectation of persistent (or permanent) income loss due to job displacement might change systematically over time. In other words, they might underestimate the persistent effect of job displacement on their earnings at initial stages, on average (from an *ex post* perspective), yet continue to update their expectations as time lapses, which leads to a further decrease in their total expenditures.

There are a couple of factors which can cause such systematic expectation errors on the income effect of job displacement. First, a large economic shock could change people's expectations. The sample years of this study, 1998-2012, include two recession periods: 2002-2003 and 2008-2009, the latter a particularly severe recession that has been termed as the Great

Recession (Elsby et al. 2011). Therefore, the families in the main sample of this study were likely to be affected by such a large economic shock and might, as a result, change their future expectations.

Second, undergoing typical processes of persistent earnings losses after the first job displacement may necessarily entail people's expectation adjustments. For example, Stevens (1997) finds that multiple job losses following the first job loss account for much of the persistent reductions in earnings due to job displacement. The experience of such subsequent job losses would lower people's expectations of their future income paths gradually, which, in turn, would lead to gradual falls in their expenditures over time.

Table 1.C10 shows some evidence that both factors above, the Great Recession shock and multiple job losses, played a role in generating the lagged decrease in total family expenditures of displaced heads. Specifically, column (1) in Table 1.C10 refers to the effect of displacement on total family expenditures which is the same as column (1) in Table 1.B3. Columns (2) and (3) in Table 1.C10 show how the Great Recession and subsequent job displacements, respectively, affected their total expenditures during the post-first-displacement period. Overall, both factors appeared to affect the lagged reduction in their total expenditures point estimate-wise; in particular, the Great Recession made a relatively significant contribution to the decrease in their total expenditures at *t+4*, while the subsequent job losses of their heads appeared to play a statistically significant role in further decreasing their total expenditures, in the current year of or a half year before the occurrence of the subsequent job losses. When I include all the interaction terms for the Great Recession and the heads' subsequent job losses in my original model (column (4)), it can be observed that the lagged decrease in their total expenditures due to heads' first displacement becomes smaller in terms of both point estimate and statistical significance.

This suggests that displaced families might change their future expectation of life family income while experiencing unexpected additional shocks after their heads' first displacement.

1.7 Conclusion

During the period 1998-2012, families with a displaced husband practiced substantial expenditure smoothing over a long period from roughly two years before through six or more years after displacement, notwithstanding the sizable persistent income loss that they experienced over the same period of time. In particular, their expenditures decreased only slightly in the initial periods around the year of displacement, but the decline in their expenditures became more salient as time went on. It is noteworthy that this large expenditure smoothing was enabled mainly by a considerable decrease in savings/assets of displaced families. For about six or more years since the year of displacement, the average annual decrease in their wealth covers roughly 55% of the average annual loss in their disposable income. In turn, the average decline in their annual expenditures for food, housing, transportation, health care and education explains about 20% of the average annual loss in their disposable income during the same time.

APPENDICES

APPENDIX A

FIGURES FOR CHAPTER 1

Figure 1.A1 The Effects of Husband's Job Displacement on Annual Earnings, Family Income, and Expenditures

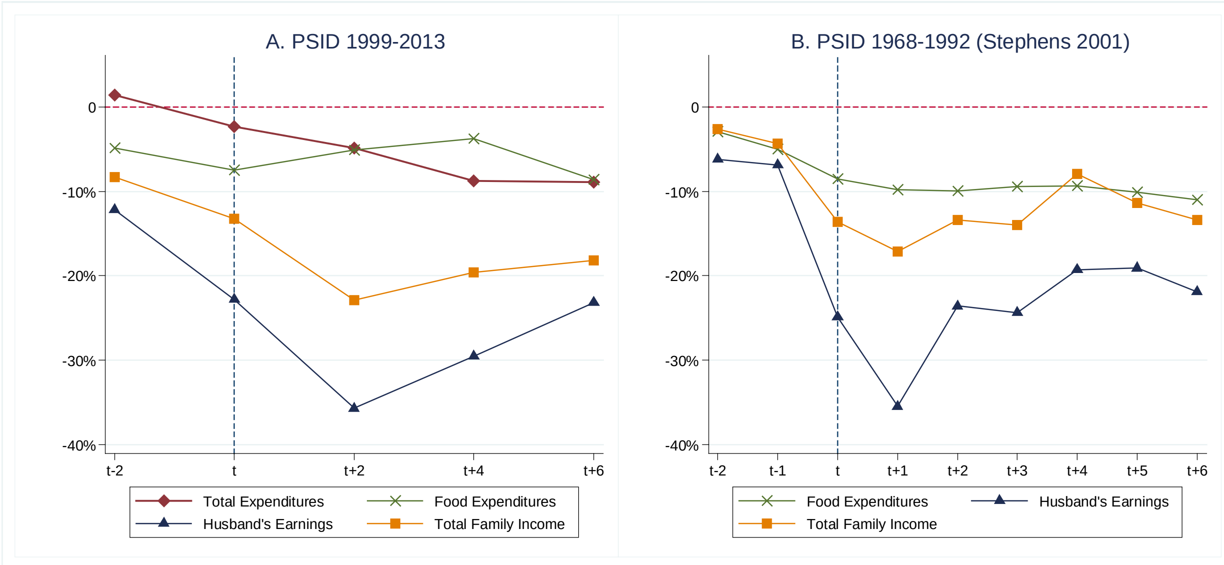
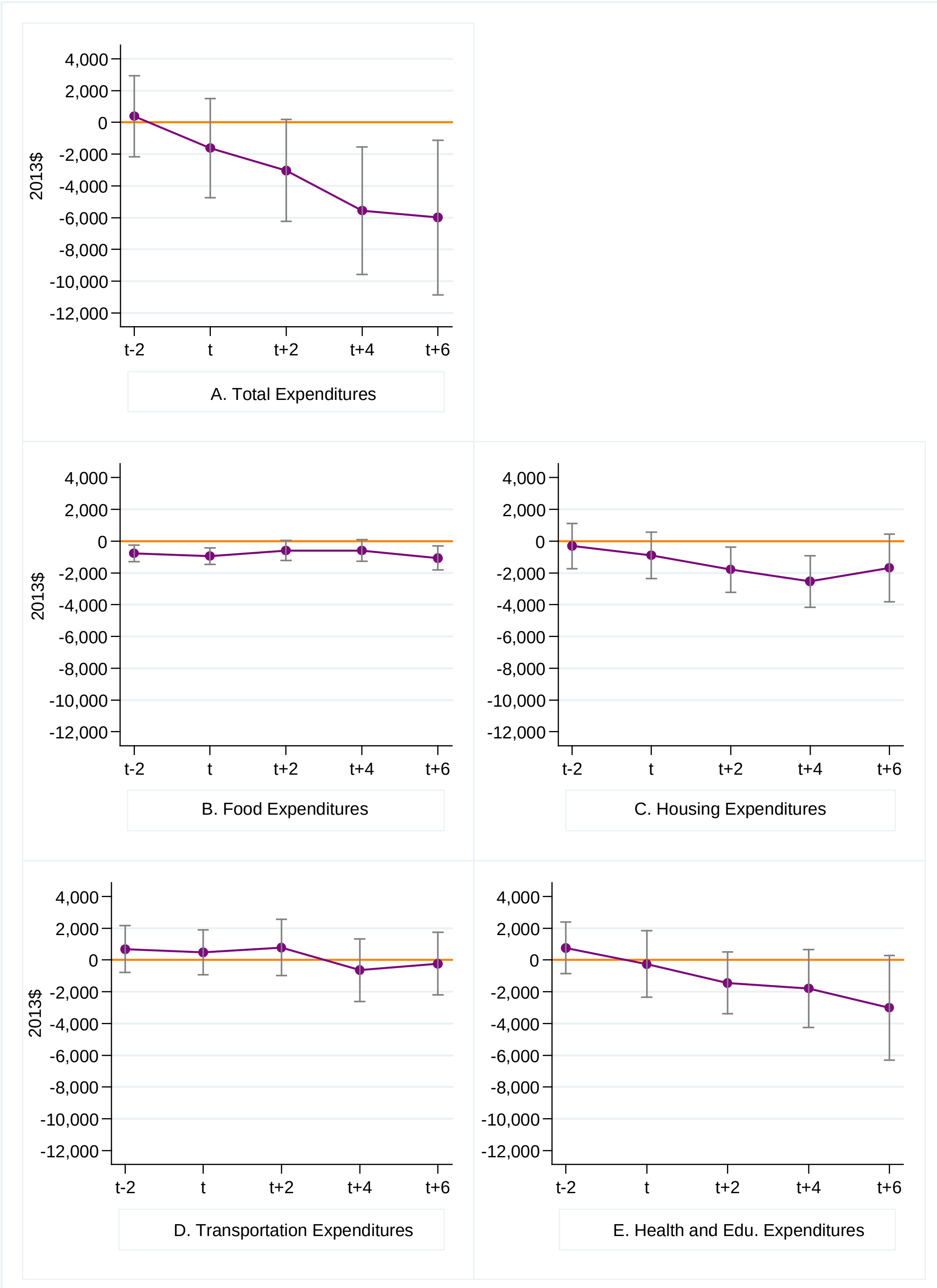


Figure 1.A2 The Effects of Husband's Job Displacement on Family Expenditures in Dollar Terms



APPENDIX B

TABLES FOR CHAPTER 1

Table 1.B1 Descriptive Statistics for Non-displaced and Displaced Families (PSID 1999-2013)

	Non-displaced 1999-2013		Displaced 1999-2013	
Variables	Mean(SD)	N	Mean(SD)	N
Head's Age	42.74	8,561	44.07	1,363
	(10.04)		(9.77)	
Head's Education (Years)	14.25	8,470	14.03	1,355
	(2.70)		(2.64)	
Wife's Age	41.12	8,561	42.57	1,363
	(9.87)		(9.55)	
Wife's Education (Years)	14.39	8,473	14.04	1,338
	(2.50)		(2.76)	
Percentage of White Heads	0.86	8,561	0.80	1,363
	(0.35)		(0.40)	
Number of Children (0-17)	1.23	8,561	1.33	1,363
	(1.20)		(1.27)	
Number of Young Children (0-5)	0.42	8,561	0.40	1,363
	(0.71)		(0.68)	
Percentage of Manufacturing Industry	0.19	8,561	0.20	1,363
	(0.39)		(0.40)	
Percentage of Blue-Collar Workers	0.31	8,561	0.41	1,363
	(0.46)		(0.49)	
Percentage of Jobs Covered by Union Contract	0.14	8,561	0.08	1,363
	(0.34)		(0.26)	
Percentage of Union Members	0.12	8,561	0.07	1,363
	(0.33)		(0.25)	
Family Annual Income (2013$)	133,159	8,561	103,650	1,363
	(180,928)		(75,160)	
Head's Earnings	86,846	8,561	64,291	1,363
	(155,902)		(60,857)	
Wife's Earnings	32,131	8,561	28,110	1,363
	(36,682)		(31,661)	
Public Transfer	983	8,561	3,022	1,363
	(3,973)		(6,794)	
Private Transfer	3,000	8,561	2,224	1,363
	(16,454)		(10,044)	
Other Income	10,200	8,561	6,002	1,363
	(42,905)		(17,176)	
Total Annual Expenditure (2013$)	56,683	8,561	52,358	1,363
	(31,025)		(26,299)	
Food	10,482	8,561	10,016	1,363
	(5,269)		(4,614)	
Housing	21,723	8,561	19,843	1,363
	(16,699)		(12,901)	
Transportation	16,234	8,561	14,684	1,363
	(12,746)		(11,088)	
Health Care	3,861	8,561	3,929	1,363
	(4,900)		(4,009)	
Education	4,383	8,561	3,886	1,363
	(9,842)		(9,210)	

Table 1.B1 (cont'd)

	Non-displaced 1999-2013		Displaced 1999-2013	
Variables	Mean(SD)	N	Mean(SD)	N
Total Family Wealth (2013$)	396,807	8,561	262,111	1,363
	(1,004,994)		(507,842)	
Home Equity	123,594	8,561	99,337	1,363
	(190,764)		(181,102)	
Total Vehicle(s) Value	22,393	8,561	16,204	1,363
	(27,246)		(17,150)	
Checking/Saving	32,764	8,561	20,649	1,363
	(109,175)		(61,321)	
Stock	55,685	8,561	33,396	1,363
	(311,796)		(130,543)	
Business/Farm	66,722	8,561	15,873	1,363
	(558,822)		(153,372)	
IRA	55,198	8,561	50,089	1,363
	(160,614)		(152,592)	
Other Wealth	40,452	8,561	26,563	1,363
	(277,948)		(170,223)	

Table 1.B2 The Effects of Husband's Job Displacement on Family Income (PSID 1999-2013)

Job Displacement	(1) Husband's Earnings	(2) Husband's and Wife's Earnings	(3) Total Family Income
		A. Log Dependent Variable	
2 - 2.5 years before (*t-2*)	-0.122** (0.051)	-0.071** (0.031)	-0.083*** (0.023)
Current year - 0.5 year before (*t*)	-0.228*** (0.056)	-0.180*** (0.044)	-0.132*** (0.033)
1.5 - 2 years after (*t+2*)	-0.357*** (0.066)	-0.256*** (0.043)	-0.229*** (0.034)
3.5 - 4 years after (*t+4*)	-0.295*** (0.074)	-0.214*** (0.053)	-0.196*** (0.044)
5.5+ years after (*t+6 plus*)	-0.232*** (0.073)	-0.188*** (0.053)	-0.182*** (0.044)
Observations	9,924	9,924	9,924
Number of Families	2,245	2,245	2,245
		B. Level Dependent Variable (2013$)	
2 - 2.5 years before (*t-2*)	-13,340*** (3,962)	-13,929*** (4,149)	-16,959*** (4,886)
Current year - 0.5 year before (*t*)	-17,385*** (6,024)	-19,080*** (6,287)	-18,197*** (6,931)
1.5 - 2 years after (*t+2*)	-29,475*** (5,786)	-32,502*** (6,103)	-35,198*** (6,630)
3.5 - 4 years after (*t+4*)	-27,761*** (6,885)	-30,753*** (7,587)	-32,872*** (7,967)
5.5+ years after (*t+6 plus*)	-28,863*** (9,591)	-33,444*** (10,249)	-35,105*** (10,561)
Mean (SD)	83,777 (146,838)	115,361 (152,815)	129,144 (170,737)
Observations	9,924	9,924	9,924
Number of Families	2,245	2,245	2,245

Note: Robust standard errors are presented in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 1.B3 The Effects of Husband's Job Displacement on Family Expenditures (PSID 1999-2013)

	(1) Total Expenditure	(2) Food	(3) Housing	(4) Transportation	(5) Health+Edu
Job Displacement					
			A. Log Dependent Variable		
2 - 2.5 years before	0.014	-0.049*	0.020	0.041	0.141
(*t-2*)	(0.022)	(0.029)	(0.032)	(0.052)	(0.090)
Current year -	-0.023	-0.075**	-0.006	0.019	-0.019
0.5 year before (*t*)	(0.028)	(0.030)	(0.036)	(0.055)	(0.117)
1.5 - 2 years after	-0.049*	-0.051	-0.061	0.064	-0.140
(*t+2*)	(0.028)	(0.035)	(0.038)	(0.057)	(0.117)
3.5 - 4 years after	-0.087**	-0.037	-0.063	-0.062	-0.105
(*t+4*)	(0.034)	(0.034)	(0.044)	(0.067)	(0.138)
5.5+ years after	-0.089**	-0.086**	-0.005	-0.033	-0.306**
(*t+6 plus*)	(0.036)	(0.036)	(0.055)	(0.069)	(0.144)
Observations	9,924	9,922	9,825	9,817	9,687
Number of Families	2,245	2,245	2,242	2,245	2,232
			B. Level Dependent Variable (2013$)		
2 - 2.5 years before	378	-776***	-303	690	767
(*t-2*)	(1,299)	(263)	(727)	(751)	(827)
Current year -	-1,622	-943***	-892	470	-257
0.5 year before (*t*)	(1,593)	(269)	(751)	(719)	(1,068)
1.5 - 2 years after	-3,033*	-583*	-1,797**	788	-1,441
(*t+2*)	(1,637)	(324)	(727)	(901)	(992)
3.5 - 4 years after	-5,571***	-590*	-2,538***	-645	-1,798
(*t+4*)	(2,046)	(347)	(823)	(1,005)	(1,253)
5.5+ years after	-5,984**	-1,057***	-1,691	-225	-3,011*
(*t+6 plus*)	(2,482)	(390)	(1,085)	(1,002)	(1,677)
Mean	56,095	10,419	21,467	16,023	8,186
(SD)	(30,460)	(5,187)	(16,247)	(12,544)	(11,346)
Observations	9,924	9,924	9,924	9,924	9,924
Number of Families	2,245	2,245	2,245	2,245	2,245

Note: Robust standard errors are presented in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 1.B4 The Effects of Husband's Job Displacement on Family Wealth in 2013$ Terms (PSID 1999-2013)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Family Wealth	Home Equity	Total Vehicle(s) Value	Checking /Saving	Stocks	Business/Farm	IRA	Other
Job Displacement								
2 - 2.5 years before (*t-2*)	-58,218**	-19,198**	1	-11,168***	-7,024	-32,225**	13,709	-2,314
	(27,639)	(7,601)	(1,495)	(3,881)	(14,748)	(13,765)	(10,947)	(12,918)
Current year - 0.5 year before (*t*)	-86,934***	-21,497**	-2,186*	-9,083*	-25,988**	-34,659***	18,914	-12,435
	(27,117)	(10,181)	(1,252)	(4,955)	(13,132)	(12,620)	(12,097)	(10,879)
1.5 - 2 years after (*t+2*)	-121,837***	-15,720	-1,574	-18,976***	-22,302	-36,480**	-6,832	-19,952
	(31,696)	(10,616)	(1,417)	(4,620)	(13,995)	(16,612)	(9,673)	(12,211)
3.5 - 4 years after (*t+4*)	-118,482***	-16,283	-4,918***	-23,147***	-29,680*	-35,764**	6,458	-15,148
	(37,534)	(10,752)	(1,633)	(5,699)	(15,947)	(18,217)	(16,115)	(13,429)
5.5+ years after (*t+6 plus*)	-172,139***	-20,759	-5,467**	-28,830***	-49,890**	-39,312	-12,350	-15,530
	(48,725)	(13,398)	(2,136)	(7,350)	(21,570)	(25,502)	(15,368)	(17,748)
Mean	378,480	120,294	21,551	31,116	52,652	59,803	54,503	38,562
(SD)	(953,822)	(189,652)	(26,188)	(104,047)	(293,877)	(522,772)	(159,548)	(265,904)
Observations	9,924	9,924	9,924	9,924	9,924	9,924	9,924	9,924
Number of Families	2,245	2,245	2,245	2,245	2,245	2,245	2,245	2,245

Note: Robust standard errors are presented in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

APPENDIX C

SUPPLEMENTAL TABLES FOR CHAPTER 1

Table 1.C1 Summary Statistics for Family Expenditures (PSID 1999-2013)

Annual Expenditure Variables (2013$)	PSID 1999-2013 Mean(SD)	N	# of Zeros	Cond. Mean for Non-zeros (SD)	# of Missing
Food	10,419 (5,187)	9,924	2	10,420 (5,185)	0
Food-at-home	7,398 (3,793)	9,924	44	7,424 (3,773)	0
Food-away-from-home	2,906 (2,875)	9,924	414	3,037 (2,871)	0
Food Stamps	115 (819)	9,924	9,583	3,970 (2,823)	0
Housing	21,502 (16,596)	8,945	99	21,706 (16,541)	979
Rent	1,757 (5,146)	9,924	8,268	12,027 (7,599)	0
Mortgage Payments	13,353 (14,270)	9,924	2,785	18,419 (13,695)	0
Home Utility	3,178 (2,099)	9,603	198	3,240 (2,072)	321
Electricity	1,513 (1,100)	9,924	895	1,652 (1,044)	0
Heating	1,136 (1,210)	9,924	2,193	1,440 (1,191)	0
Home Utility	2,648 (1,594)	9,924	203	2,699 (1,566)	0
Home Insurance	825 (891)	9,119	1,944	1,016 (885)	805
Property Tax	2,905 (3,472)	9,472	1,893	3,518 (3,528)	452
Housing (w/ imputed subcomponents)	21,467 (16,247)	9,924	99	21,651 (16,194)	0
Home Insurance w/ imputation	831 (865)	9,924	1,944	1,005 (854)	0
Property Tax w/ imputation	2,878 (3,419)	9,924	1,928	3,468 (3,470)	0
Transportation	15,187 (11,926)	9,396	107	15,368 (11,880)	528
Total Car Expenditure	14,856 (11,691)	9,396	201	15,182 (11,608)	528
For Buying Car#1	3,476 (6,230)	9,924	5,163	7,328 (7,321)	0
For Buying Car#2	1,080 (2,951)	9,924	7,667	4,964 (4,556)	0
For Buying Car#3	212 (1,167)	9,924	9,182	2,829 (3,286)	0
For Leasing Car#1	400 (1,766)	9,924	9,391	7,060 (2,832)	0
For Leasing Car#2	111 (942)	9,924	9,777	7,064 (2,742)	0
For Leasing Car#3	11 (311)	9,924	9,911	7,226 (3,268)	0

Table 1.C1 (cont'd)

Annual Expenditure Variables (2013$)	PSID 1999-2013 Mean(SD)	N	# of Zeros	Cond. Mean for Non-zeros (SD)	# of Missing
For Additional Car(s)	2,336 (4,341)	9,924	6,374	6,666 (4,990)	0
Car Operation	7,205 (6,545)	9,396	203	7,365 (6,528)	528
Car Insurance	2,130 (2,805)	9,396	268	2,190 (2,821)	528
Gasoline/Fuel	2,997 (2,398)	9,924	247	3,074 (2,380)	0
Car Repair	2,007 (4,752)	9,924	4,990	4,064 (6,112)	0
Car Parking/Pooling	84 (531)	9,924	9,017	876 (1,494)	0
Public Transportation	331 (2,242)	9,924	8,851	2,864 (6,020)	0
Bus/Train	101 (566)	9,924	9,276	1,406 (1,627)	0
Taxi	25 (213)	9,924	9,636	824 (924)	0
Other	206 (2,112)	9,924	9,550	5,129 (9,280)	0
Transportation (w/ imputed car-insurance)	16,023 (12,544)	9,924	107	16,204 (12,498)	0
Car Expenses (w/ imputed car-insurance)	15,692 (12,276)	9,924	201	16,018 (12,190)	0
Car Operation (w/ imputed car-insurance	8,065 (7,323)	9,924	203	8,234 (7,305)	0
Car Insurance w/ imputation	2,977 (4,302)	9,924	287	3,061 (4,333)	0
Education	4,315 (9,760)	9,924	4,258	7,636 (11,967)	0
Child Care	1,112 (3,453)	9,924	7,555	5,085 (5,858)	0
School	3,203 (9,255)	9,924	5,532	7,109 (12,741)	0
Health Care	3,870 (4,789)	9,924	406	4,026 (4,819)	0
Doctor/Surgery/Dental	809 (1,370)	9,924	1,463	938 (1,434)	0
Prescription/Drugs	376 (765)	9,924	1,427	436 (807)	0
Hospital/Nursing Home	370 (1,519)	9,924	6,512	1,120 (2,480)	0
Health Insurance	2,316 (3,716)	9,924	2,321	3,018 (3,985)	0
Total (Food, Housing, Transportation, Health, & Education)	56,095 (30,460)	9,924	0	56,095 (30,460)	0

Table 1.C2 The Number of the Job Displacement Dummies (PSID 1999-2013)

	Survey Year								
	1999	2001	2003	2005	2007	2009	2011	2013	Total
1st Job Displacement Dummy									
2 - 2.5 years before (*t-2*)	24	35	25	45	58	56	43	0	286
Current year - 0.5 year before (*t*)	22	31	39	30	48	87	66	44	367
1.5 - 2 years after (*t+2*)	29	22	27	36	27	41	80	61	323
3.5 - 4 years after (*t+4*)	0	24	21	23	32	30	43	73	246
5.5+ years after (*t+6 plus*)	0	0	19	32	53	82	107	134	427

Table 1.C3 The Effects of Husband's Job Displacement on Family Food Expenditure (PSID 1968-1997)

	Dependent Variables: Log Annual Food Expenditure				
	(1)	(2)	(3)	(4)	(5)
Job Displacement Dummies	All years	Odd-numbered years	Odd-numbered years	Even-numbered years	Even-numbered years
1-2 year(s) before (2 years before for (2)-(5))	-0.046*** (0.014)	-0.069*** (0.024)	-0.057** (0.024)	-0.037 (0.027)	-0.035 (0.027)
Current year or 1 year after (Current year for (2)-(5))	-0.074*** (0.015)	-0.084*** (0.025)	-0.079*** (0.024)	-0.078*** (0.025)	-0.075*** (0.025)
2-3 years after (2 years after for (2)-(5))	-0.078*** (0.016)	-0.090*** (0.026)	-0.085*** (0.026)	-0.114*** (0.030)	-0.112*** (0.029)
4-5 years after (4 years after for (2)-(5))	-0.081*** (0.017)	-0.098*** (0.027)	-0.093*** (0.026)	-0.073** (0.029)	-0.069** (0.029)
6+ years after	-0.077*** (0.019)	-0.085*** (0.028)	-0.080*** (0.027)	-0.084*** (0.030)	-0.077*** (0.029)
Dropping all the heads displaced in even-numbered years		Yes	No		
Including all the heads displaced in even-numbered years and assuming they're not displaced		No	Yes		
Dropping all the heads displaced in odd-numbered years				Yes	No
Including all the heads displaced in odd-numbered years and assuming they're not displaced				No	Yes
Observations	39,595	16,339	19,928	15,927	19,667
Number of Families	4,028	3,403	4,003	3,210	3,848

Note: Robust standard errors are presented in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 1.C4 The Effects of Husband's Job Displacement on Home Equity, Home Price, and Housing Expenditures in 2013$ Terms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Home Equity	Home Price	Housing Expenditures	Rent	Mortgage Payments	Electricity	Heating	Property Tax	Home Insurance
Job Displacement									
2 - 2.5 years before (*t-2*)	-19,198**	-19,019**	-303	-116	-239	49	24	-36	15
	(7,601)	(9,032)	(727)	(277)	(746)	(61)	(76)	(124)	(50)
Current year - 0.5 year before (*t*)	-21,497**	-27,698**	-892	-165	-663	1	102	-184	17
	(10,181)	(12,556)	(751)	(313)	(751)	(67)	(159)	(141)	(85)
1.5 - 2 years after (*t+2*)	-15,720	-37,123***	-1,797**	232	-1,511**	32	-38	-408***	-103*
	(10,616)	(13,077)	(727)	(346)	(730)	(74)	(96)	(141)	(59)
3.5 - 4 years after (*t+4*)	-16,283	-44,247***	-2,538***	261	-2,211***	13	50	-529***	-121*
	(10,752)	(13,092)	(823)	(396)	(850)	(92)	(109)	(166)	(73)
5.5+ years after (*t+6 plus*)	-20,759	-49,490***	-1,691	580	-1,621	32	-4	-471*	-208**
	(13,398)	(17,303)	(1,085)	(527)	(1,017)	(96)	(119)	(258)	(84)
Mean	120,294	248,141	21,467	1,757	13,353	1,513	1,136	2,878	830.8
(SD)	(189,652)	(261,710)	(16,247)	(5,146)	(14,270)	(1,100)	(1,210)	(3,419)	(864.9)
Observations	9,924	9,924	9,924	9,924	9,924	9,924	9,924	9,924	9,924
Number of Families	2,245	2,245	2,245	2,245	2,245	2,245	2,245	2,245	2,245

Note: Robust standard errors are presented in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 1.C5 The Probability of A Displaced Family's Living in a Different, Low-Priced House Compared to *t-4*

Job Displacement	(1) 1:Live in a different, lower-priced house at *t-2*, compared to *t-4*	(2) 1:Live in a different, lower-priced house at *t*, compared to *t-4*	(3) 1:Live in a different, lower-priced house at *t+2*, compared to *t-4*	(4) 1:Live in a different, lower-priced house at *t+4*, compared to *t-4*	(5) 1:Live in a different, lower-priced house at *t+6*, compared to *t-4*
Job Displacement Occurred at *t*	-0.022 (0.018)	0.025 (0.029)	0.029 (0.042)	0.116** (0.057)	0.207** (0.086)
Observations	8,774	6,917	5,236	3,893	2,821
Number of Families	2,241	1,972	1,647	1,353	1,116

Note 1: Table 1.C5 is based on Table 1.C6.
Note 2: Robust standard errors are presented in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 1.C6 The Probability of A Displaced Family's Living a Different, Low-Priced House

Job Displacement	(1) 1:Live in a different, lower-priced house compared to 2 years ago	(2) 1:Live in a different, lower-priced house compared to 4 years ago	(3) 1:Live in a different, lower-priced house compared to 6 years ago	(4) 1:Live in a different, lower-priced house compared to 8 years ago	(5) 1:Live in a different, lower-priced house compared to 10 years ago
2 - 2.5 years before (*t-2*)	-0.022 (0.018)	0.005 (0.028)	-0.004 (0.033)	0.049 (0.041)	0.110* (0.059)
Current year - 0.5 year before (*t*)	-0.008 (0.019)	0.025 (0.029)	0.034 (0.037)	0.069 (0.049)	0.109* (0.060)
1.5 - 2 years after (*t+2*)	0.010 (0.022)	0.035 (0.036)	0.029 (0.042)	0.125** (0.055)	0.162** (0.074)
3.5 - 4 years after (*t+4*)	0.008 (0.025)	0.029 (0.038)	0.060 (0.049)	0.116** (0.057)	0.197** (0.079)
5.5+ years after (*t+6* plus)	-0.003 (0.025)	0.046 (0.045)	0.056 (0.062)	0.114 (0.071)	0.207** (0.086)
Observations	8,774	6,917	5,236	3,893	2,821
Number of Families	2,241	1,972	1,647	1,353	1,116

Note: Robust standard errors are presented in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 1.C7 The Probability of A Displaced Family's Living a Different, Higher-Priced House, Compared to *t-4*

Job Displacement	(1) 1:Live in a different, higher-priced house at *t-2*, compared to *t-4*	(2) 1:Live in a different, higher-priced house at *t*, compared to *t-4*	(3) 1:Live in a different, higher-priced house at *t+2*, compared to *t-4*	(4) 1:Live in a different, higher-priced house at *t+4*, compared to *t-4*	(5) 1:Live in a different, higher-priced house at *t+6*, compared to *t-4*
Job Displacement Occurred at *t*	-0.007	-0.084**	-0.088	-0.124	-0.289**
	(0.028)	(0.041)	(0.062)	(0.097)	(0.129)
Observations	8,774	6,917	5,236	3,893	2,821
Number of Families	2,241	1,972	1,647	1,353	1,116

Note1: Table 1.C7 is based on Table 1.C8.
Note2: Robust standard errors are presented in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 1.C8 The Probability of A Displaced Family's Living a Different, Higher-Priced House

Job Displacement	(1) 1:Live in a different, higher-priced house compared to 2 years ago	(2) 1:Live in a different, higher-priced house compared to 4 years ago	(3) 1:Live in a different, higher-priced house compared to 6 years ago	(4) 1:Live in a different, higher-priced house compared to 8 years ago	(5) 1:Live in a different, higher-priced house compared to 10 years ago
2 - 2.5 years before	-0.007	-0.052	-0.019	-0.053	-0.072
(*t-2*)	(0.028)	(0.040)	(0.047)	(0.067)	(0.089)
Current year -	-0.037	-0.084**	-0.126**	-0.131*	-0.182*
0.5 year before (*t*)	(0.023)	(0.041)	(0.056)	(0.073)	(0.095)
1.5 - 2 years after	-0.021	-0.091**	-0.088	-0.190**	-0.197*
(*t+2*)	(0.027)	(0.045)	(0.062)	(0.085)	(0.103)
3.5 - 4 years after	-0.003	-0.026	-0.083	-0.124	-0.251**
(*t+4*)	(0.035)	(0.055)	(0.077)	(0.097)	(0.121)
5.5+ years after	0.020	-0.019	-0.052	-0.151	-0.289**
(*t+6 plus*)	(0.031)	(0.058)	(0.085)	(0.109)	(0.129)
Observations	8,774	6,917	5,236	3,893	2,821
Number of Families	2,241	1,972	1,647	1,353	1,116

Note: Robust standard errors are presented in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 1.C9 The Effects of Husband's Job Displacement on Sub-components of Family Income in 2013$ Terms

	(1) Husband's Earnings	(2) Wife's Earnings	(3) Public Transfer	(4) Private Transfer	(5) Other Family Income	(6) Total Family Income
2 - 2.5 years before	-13,340***	-590	-1	-707	-2,322	-16,959***
(*t-2*)	(3,962)	(1,799)	(414)	(627)	(1,592)	(4,886)
Current year -	-17,385***	-1,695	2,099***	-535	-681	-18,197***
0.5 year before (*t*)	(6,024)	(1,989)	(574)	(918)	(1,691)	(6,931)
1.5 - 2 years after	-29,475***	-3,027	797	-593	-2,899*	-35,198***
(*t+2*)	(5,786)	(1,929)	(518)	(789)	(1,594)	(6,630)
3.5 - 4 years after	-27,761***	-2,992	1,066**	-935	-2,250	-32,872***
(*t+4*)	(6,885)	(2,399)	(540)	(884)	(1,694)	(7,967)
5.5+ years after	-28,863***	-4,582	1,238**	-1,087	-1,812	-35,105***
(*t+6 plus*)	(9,591)	(2,909)	(588)	(1,071)	(2,129)	(10,561)
Mean	83,777	31,584	1,260	2,894	9,629	129,144
(SD)	(146,838)	(36,065)	(4,517)	(15,738)	(40,404)	(170,737)
Observations	9,924	9,924	9,924	9,924	9,924	9,924
Number of Families	2,245	2,245	2,245	2,245	2,245	2,245

Note: Robust standard errors are presented in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Table 1.C10 The Effects of the Great Recession and Multiple Job Losses on Total Family Expenditures of Displaced Husbands

Dummy Variables	(1) Log Total Expenditures	(2) Log Total Expenditures	(3) Log Total Expenditures	(4) Log Total Expenditures
1st Job Displacement				
2 - 2.5 years before 1st JD	0.014	0.013	0.012	0.012
(*t-2*)	(0.022)	(0.022)	(0.022)	(0.022)
Current year of 0.5 year	-0.023	-0.023	-0.025	-0.025
before 1st JD (*t*)	(0.028)	(0.028)	(0.028)	(0.028)
1.5 - 2 years after 1st JD	-0.049*	-0.061*	-0.043	-0.056*
(*t+2*)	(0.028)	(0.031)	(0.028)	(0.031)
3.5 - 4 years after 1st JD	-0.087**	-0.066*	-0.075**	-0.054
(*t+4*)	(0.034)	(0.036)	(0.035)	(0.037)
5.5+ years after 1st JD	-0.089**	-0.086**	-0.067*	-0.066*
(*t+6 plus*)	(0.036)	(0.038)	(0.038)	(0.040)
The Great Recession (GR)				
GR in 1.5 - 2 years after 1st JD		0.033		0.037
		(0.037)		(0.038)
GR in 3.5 - 4 years after 1st JD		-0.071		-0.070
		(0.049)		(0.048)
GR in 5.5 + years after 1st JD		-0.010		-0.005
		(0.025)		(0.024)
Multiple Job Losses				
Current year of - 0.5 year			-0.063**	-0.064**
before 2nd/3rd JD			(0.030)	(0.030)
1.5 - 2 years after 2nd/3rd JD			-0.046	-0.048
			(0.045)	(0.044)
3.5 - 4 years after 2nd/3rd JD			-0.039	-0.040
			(0.038)	(0.038)
5.5+ years after 2nd/3rd JD			-0.097*	-0.095*
			(0.054)	(0.054)
Observations	9,924	9,924	9,924	9,924
Number of Families	2,245	2,245	2,245	2,245

Note: Robust standard errors are presented in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

APPENDIX D

ADDITIONS FOR CHAPTER 1

Constructing the Variables for Annual Family Expenditures

Most of the questions related to annual family income in the PSID have referred to the previous year of each survey year. But the time frames of questions related to expenditures are varied across expenditure categories; for some expenditures such as for health care and education, for example, the PSID has asked about the amount of expenditures during the two years prior to the interview year, while for many other expenditures related to food, housing, etc, it has referred the previous month or just an unspecified recent period around the interview month, which was mostly between March and May for each interview year. There are also some expenditure categories, such as for car-purchase and lease, which refer to the whole period from two years before the interview year through the interview month. Thus, in general, the time frame of annualized expenditures in the PSID would match that of the annual income, with some errors. More specific methods that I use to construct annualized expenditure amounts for each expenditure category are as follows:

Food Expenditures: The annual food expenditures in this study include food-at-home, food delivery, food-away-from-home, and food stamps. Specifically, I use the following questions in the 1999-2013 PSID to calculate annualized family expenditures for food-at-home, food delivery, and food-away-from-home: “How much do you (and everyone else in your family) spend on food that you use at home in an average week?”, “How much do you spend on that food delivered to the door?”, “How much do you (and everyone else in your family) spend eating out?”. As for food stamps, there are two different questions in the 1999-2013 PSID, “How much did you receive in food stamp benefits in the previous year?” and “How much did you receive in food stamp benefits last month?” It turns out both measures for food stamps generate comparable regression results in this paper, although I choose to use the former one because my primary purpose is to proxy the annual expenditures for the year prior to each interview year as much as possible to more correctly match the time frame of annual income information in the PSID.

Housing Expenditures: The annual housing expenditures consist of rent/mortgage payments, home utility expenses for electricity and heating, home insurance premiums, and property tax. Specifically, annualized payments for rent and mortgage are calculated based on the following questions, “About how much rent do you pay a month?” and “How much are your monthly mortgage payments?”, respectively. Home utility expenditures for electricity and heating refer to the questions, “How much do you (and your family living there) usually pay for electricity per month on average?” and “how much do you (and your family living there) usually pay for gas or other types of heating fuel per month on average?” Lastly, the home insurance premium was calculated from the question “How much is your total yearly homeowner's insurance premium?”, and property tax from the question “About how much are your total yearly property taxes, including city, county, and school taxes?”

Transportation Expenditures: The annual transportation expenditures encompasses the expenses for purchasing/leasing car(s), car-operation costs such as car insurance, car repair, fueling, and parking/pulling, and the expenses for using public transportation such as buses, trains, taxi.

Specifically, the 1999-2013 PSID has asked about detailed expenditures for up to three cars which had been newly purchased or leased since the January two years before each interview year. For example, the questions for newly-purchased car(s) cover total purchase price, trade-in dollar amounts if any, and down/loan payments if any. Using those questions, I calculate average annual expenses for each car newly purchased since January two years before the interview year. Similarly, the questions for newly-leased car(s) cover the dollar amount of initial outlay and lease payments; using those questions, I also calculate average annual expenses for each car newly leased since January two years before the interview year. The 1999-2013 PSID also asked about additional purchase/lease expenditures for additional cars which are not covered by the questions above. Combining all this information, I proxy average annual dollar amounts that a family has paid for buying/leasing car(s) for the last two years of the interview year.

I obtain the information on annual car insurance premium from the following question: "How much do you (and your family living there) pay for car insurance for (all of) your vehicle(s) per year?" In turn, to calculate other annualized car-operation costs such as for car repair/gasoline/car parking and pooling, I refer to the following question: "In [LAST MONTH], how much did (you/your family living there) pay for each of these transportation related expenses [a. Car repairs or maintenance b. Gasoline, c. Parking and car pooling]?" Similarly, to construct annualized public transportation costs for bus, train, taxi, etc, I refer to the following question: "In [LAST MONTH], how much did (you/your family living there) pay for each of these transportation related expenses [a. Bus fares and train fares b.Taxicabs c.Other transportation costs]?"

Health Care Expenditures: The annual family expenditures for health care include the expenses for doctor appointments, surgery, dental treatment, nursing care, hospitalization, prescription medicine and health insurance. Each component of them is calculated based on the following questions respectively: "About how much did you pay out-of-pocket for doctors, outpatient surgery, dental bills in the last two years combined?"; "About how much did you pay out-of-pocket for nursing home and hospital bills in the last two years combined?"; "About how much did you pay out-of-pocket for prescriptions, in-home medical care, special facilities, and other services in 1997 and 1998 combined?"; "Altogether, how much did [you/your family] pay for health insurance premiums, in the last two years combined, for (all of) the health insurance or health care coverage(s) you just mentioned? Please include amounts that you had automatically deducted from your pay, as well as amounts you paid directly."

Education Expenditures: The annual family expenditures for education consist of school-related costs and child care costs. Each component of them refers to the following questions, respectively: "In the last year, how much in total were these expenses such as [a. Purchase or rental of books, supplies, uniforms, or equipment including computers and software; b. Tuition or tutoring (not including any amounts already mentioned for day care or nursery school); c. Room and board for a family member who is away at school]?"; "In the last year, were there any other school-related expenses not already covered in the previous question?"; "How much did you (and your family living there) pay for child care last year?"

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CHAPTER 2

The Evolution of Earnings Volatility During and After the Great Recession[1]

2.1 Introduction

In a world with incomplete financial markets, securing a stable income path is important for individuals' welfare. Accordingly, the topic of changes in earnings volatility has received continual attention from various researchers since the seminal work by Gottschalk and Moffitt (1994).

A great deal of literature has documented changes in men's earnings volatility from the 1970s through the mid-2000s. The consensus is that men's earnings volatility increased substantially over the 1970s, reached its peak in the early 1980s, and decreased somewhat but still remained at a higher level than in the 1970s through the mid-2000s. It is still a matter of controversy whether the upward trend resumed around 2000 (Cameron and Tracy 1998; Celik et al. 2012; Congressional Budget Office 2007; Dynan, Elmendorf, and Sichel 2012; Haider 2001; Moffitt and Gottschalk 2002, 2012; Shin and Solon 2011). A smaller literature on women's earnings volatility has found trends entirely different from men's. Women's earnings volatility is greater than men's, but it trended steadily downward from the 1970s until prior to the Great Recession (Dynan, Elmendorf, and Sichel 2012; Ziliak, Hardy, and Bollinger 2011).[2]

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[1] This chapter was published in *Industrial Relations* in 2016

[2] See Dynan et al. (2012) for a more detailed summary of the literature on earnings volatility.

This paper explores the evolution of earnings volatility during and after the Great Recession, which has been described as “the deepest downturn in the postwar era” (Elsby et al. 2011). Using longitudinally matched Current Population Survey data for 1979-2012, I update the evidence on men's and women's earnings volatility to include the Great Recession and its aftermath,[3] and I then explore the main factors underlying the identified patterns. Although previous studies of earlier periods have documented counter-cyclicality in earnings volatility (Dynan, Elmendorf, and Sichel 2012; Shin and Solon 2011; Ziliak, Hardy, and Bollinger 2011), the present study goes further in examining the sources of that counter-cyclicality.

The first main finding of this paper is that men’s earnings volatility increased significantly during the Great Recession, to such an extent that the rise was comparable to the one that occurred during the severe recession of the early 1980s. Women’s earnings volatility also increased after the Great Recession began, even though it had shown a strong downward trend prior to that time. These patterns are evident across most subgroups defined by age or education, for both men and women.

I go on to show that the counter-cyclicality of earnings volatility is due mainly to counter-cyclicality in the volatility of annual work hours, rather than hourly wages. Moreover, the counter-cyclical volatility in work hours appears mainly among those workers who experienced unemployment. Thus, the greater volatility in earnings paths during recessions stems mainly from the annual hours reductions associated with increased unemployment.

The analysis begins in the next section with an explanation of the data source, sample selection criteria, and measures used for earnings volatility. The empirical results and conclusion will follow in the later sections.

[3] Dynan et al. (2012) and Ziliak et al. (2011) cover the periods up to 2008, and Celik et al. (2012) and Hardy and Ziliak (2014) until 2009. To my knowledge, this is the first paper documenting the evolution of earnings volatility in the United States during and after the Great Recession.

2.2 Data

This study uses matched March Current Population Survey (CPS) data from 1979–2012. In the following section, the appropriateness of the data set is briefly discussed. I then describe the sample selection criteria and three measures for earnings volatility that this study mainly uses.

2.2.1 Matched CPS

From the March CPS data, I construct thirty one 2-year short panel data sets.[4] March CPS provides the most recent annual earnings information of all datasets available at this point. Hence, it is appropriate for tracking the recent evolution of earnings volatility since the start of the Great Recession. In addition, it provides a large sample size and diverse demographic information. Therefore, it enables us to obtain more reliable estimates on earnings volatility for various subgroups as well as for the whole sample.

A well-known problem associated with using the matched March CPS data is that a portion of the sample is typically lost from the matching process from year to year. This is because of the particular survey design of CPS: its sampling is based on housing units, not on households. Therefore, it does not track interviewees who relocate between interview periods, and thus a certain portion of individuals in the March CPS fails to be matched each time.

The main focus of this study, however, is looking at the evolution of earnings volatility over time, not the level of that volatility. Therefore, if no systematic error occurs in matching March CPS across the sample years, the matched March CPS can be expected to provide reliable information with respect to earnings volatility dynamics. In the literature estimating the

[4] Matching March CPS over 1984-1985 and 1994-1995 is not possible because of changes in the survey design. I followed Madrian and Lefgren (2000)'s guidelines for matching the March CPS. The detailed procedure for matching is presented in Appendix 1.

evolution of earnings volatility from the matched March CPS, no specific evidence of systematic errors in matching March CPS has been reported for the periods prior to the Great Recession (Cameron and Tracy 1998; Celik et al. 2012; Ziliak, Hardy, and Bollinger 2011). Even during and after the Great Recession, the matching process does not seem to have been affected significantly by the shock from the Great Recession.[5] Thus, the matched March CPS is considered a legitimate dataset to explore the evolution in earnings volatility for a long period including the Great Recession.

2.2.2 Sample Selection

I use two types of samples to explore the change in earnings volatility after the Great Recession. The first one, Sample A, consists of civilian workers ages 25 to 59[6] who worked for at least 1 week with positive earnings in the year previous to the survey year. I exclude students and the self-employed from the sample. Thus, I focus on workers with strong labor attachment and little influence on decisions regarding their own wages. To prevent outliers from generating misleading results, I exclude those who had the top and bottom 1% of annual earnings. Along the same lines, individuals with imputed earnings data are excluded.[7] These restrictions are almost the same as those imposed by Shin and Solon (2011) and Celik et al. (2012). Therefore, results from Sample A will be comparable to theirs.

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[5] There was no serious change in the percentage of movers among March CPS interviewees since the Great Recession. See Appendix 1 for a detailed discussion.

[6] The primary purpose of this age restriction is to make the sample comparable to those used by previous papers. When I redid my analyses for a sample ages 30 to 54, the results were qualitatively similar to those reported here.

[7] The portion of imputed data due to non-reporting has trended upward in the CPS. Hence dropping imputed data may cause sample composition bias over the sample period. Cameron and Tracy (1998) address this issue in their appendix (see A-5). They suggest some evidence that people missing earnings reports tend to have larger earnings volatility than those who do not. Accordingly, there is a possibility that earnings volatility would be especially underestimated for the recent periods. In that case, the increased earnings volatility that I measure during and after the Great Recession may understate the increase that actually occurred.

One caution about using Sample A, however, is that I drop all workers whose annual earnings were zero (zero-earners).[8] This may generate seriously misleading interpretations of the results, especially during the Great Recession, which was characterized by a remarkably high unemployment rate that persisted for a long time (Elsby et al. 2011).

Accordingly, I have created a second sample, Sample B, which is the same as Sample A except that it includes all zero-earners and other outliers, i.e., the top and bottom 1% of all earners.[9] Figure 2.A1 presents the men's and women's proportion of zero-earners for each year in Sample B. It shows that the men's fraction of zero-earners displays a relatively large upsurge from 2009 to 2011, even relative to the continuous upward trend seen from 1979 on. Also, we observe that a sudden upsurge also occurs in the women's zero-earner fraction from 2009 to 2011, which is distinct from its relatively flat pattern in the preceding period since 2003. Hence, it seems valuable to consider Sample B together with Sample A. By looking at both together, we should be able to understand more accurately the overall change in men's and women's earnings volatility since the beginning of the Great Recession.

I use a total of thirty one 2-year short panel data sets from 1979-2012. For Sample B, observations of men and women total about 240,000 and 300,000, respectively, which means about 7700 and 9700 observations, respectively, per each panel dataset.

[8] Note that zero-earners include individuals who are not in the labor force.

[9] The March CPS has maintained the same top-coding policy since 1995. Prior to 1995, however, there had been several changes in the policy. Larrimore et al. (2008) got access to internal earnings data corresponding to each top-coded earner in the March CPS before 1995, and generated new top-coded earnings series following the post-1995 top-coding policy from 1976 onward. I incorporate the work of Larrimore et al. (2008) to correct for the inconsistency of top-coding policy over the sample period.

2.2.3 Measuring Earnings Volatility

I use three main approaches for measuring earnings volatility. The first is the standard deviation of the difference in log real earnings residuals over two adjacent years. This has been used as a standard measure for earnings volatility by various researchers since Dynarski and Gruber (1997) (henceforth denoted as DG). The following is the specific formula for DG's measurement:

$$\sqrt{\mathrm{Var}\left(\widetilde{\log(Y_{it}) - \log}(Y_{it-1})\right)}$$

Y_{it} indicates real earnings for individual i at year t. $\left(\widetilde{\log(Y_{it}) - \log}(Y_{it-1})\right)$ is the residual obtained by regression of $(\log(Y_{it}) - \log(Y_{it-1}))$ on quadratics of age for year $t-1$, which is expected to eliminate the life-cycle effect, cohort effect and year effect from individual log earnings. Shin and Solon (2011) show that this measure not only captures well the size of transitory and persistent earnings shock, it also is much less affected by the evolution of the return to individuals' time-invariant characteristics. It is also relatively transparent and less dependent on parametric assumptions because it is not derived from an arbitrary complicated earnings model.[10]

One weakness of DG, however, is that it does not account for zero-earners when estimating earnings volatility because it is based on log earnings. Thus, it cannot be applied to Sample B. As discussed in the previous section, zero-earners seem to increase substantially during the Great Recession, so I need to employ another measure, which can be applied to Sample B that encompasses all zero-earners.

As an alternative measure for both Samples A and B, I employ the standard deviation of the arc percentage change in real earnings, suggested by Dynan, Elmendorf, and Sichel (2012)

[10] See Baker and Solon (2003) and Shin and Solon (2011) for related discussions.

(hereafter DES) for estimating earnings volatility. The specific formula that DES introduced is as follows:

$$\sqrt{\mathrm{Var}\left\{100 * \frac{(\widetilde{Y_{it} - Y_{it-1}})}{\bar{Y}_i}\right\}}$$

Y_{it} denotes real earnings (not log real earnings) for individual i in year t, and $\bar{Y}_i$ denotes the average of Y_{it} and Y_{it-1}.[11] $\frac{(\widetilde{Y_{it}-Y_{it-1}})}{\bar{Y}_i}$ refers to the residual from the regression of $\frac{(Y_{it}-Y_{it-1})}{\bar{Y}_i}$ on quadratics of age for year $t-1$, which is expected to remove the life-cycle effect, cohort effect, and year effect. The advantage of using the DES measurement is that its interpretation is easier and more intuitive than DG, and it can also deal with zero earners. Furthermore, its construction causes it to be bounded below and above by -200 and +200 respectively.[12]

I use one further alternative method, beyond the two measures just described: I construct quantile distributions for the tenth, twenty-fifth, fiftieth, seventy-fifth, and ninetieth sample percentiles of the age-adjusted arc percent change in real earnings for each sample year. This measure can be applied to both Samples A and B because it uses arc percent change in real earnings instead of log real earnings. This method's main rationale for estimation of earnings volatility was suggested by Shin and Solon (2011) (hereafter SS).[13] This rationale is to take a more complete look at the distribution of earnings changes, in order to capture details that might escape an examination of second-moment statistics such as in DG or DES. For example, I can consider the difference between the 90th and 10th percentiles (say, p90-p10) as something of a counterpart to the variance of changes in earnings, and decompose it into p90-p50 and p50-p10

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[11] If both of Y_{it} and Y_{it-1} are zero, I let $\frac{(Y_{it}-Y_{it-1})}{\bar{Y}_i}$ equal zero.

[12] In other words, no matter how much one's earnings change, the measured earnings volatility for the individual by DES cannot exceed 200 in terms of absolute magnitude. This might alleviate potential problems from outliers.

[13] Shin and Solon (2011) looked at the quantile distributions for change in log real earnings and a rescaled difference in real earnings, instead of the age-adjusted arc percent change in real earnings.

to check the respective contributions of changes in p90-p50 and p50-p10 to the change in p90-p10.[14]

The three measures explained above complement each other, and thus help to generate more robust empirical results. One caution is that, as other researchers have pointed out in the literature, the measures cannot be taken directly as the amount of risk faced by individual workers. This is because those measures themselves do not tell how much earnings volatility has been expected and insured by individuals before it is realized. If people do well in dealing in advance with increased earnings volatility, then that volatility will not be so serious a problem for them. Hence, one must be cautious about deriving welfare implications from the estimated change in earnings volatility.

2.3 Results

This section presents how earnings volatility evolved during and after the Great Recession by gender, age, and education level in the United States. I then discuss how unemployment shocks during recessions have related to the counter-cyclical behaviors of earnings volatility.

2.3.1 Men's and Women's Earnings Volatility during and after the Great Recession

[14] Admittedly, p90-p10 is not exactly comparable with variance measures such as DG and DES, because p90-p10 is a simple difference between arbitrarily selected percentiles, while DG and DES are a type of synthetic statistic summarizing how much the distribution of earnings changes spreads, although their standard weighting schemes might also be regarded as arbitrary. Accordingly, we cannot say which measure is superior, so looking at all of them together and finding common patterns across various measures will be a reasonable approach to determining the evolution of earnings volatility.

Figure 2.A2 shows the evolution of men's earnings volatility over 1979–2011, measured by DG and DES.[15,16] Each point in the graphs indicates the earnings volatility between the corresponding year in the horizontal axis and the previous year.

According to the results from DG for Sample A (the graph with triangle points), men's earnings volatility over 2008-2009 was about 0.45. This represented an increase of about 18% over the figure of 0.38 reported for the previous period, 2007-2008. This seems to have been an extraordinary upsurge, because earnings volatility appeared quite stable from 1993-1994 to 2007-2008, ranging only from 0.36 to 0.39. Earnings volatilities over 2009-2010 and 2010-2011 decreased to about 0.42 and 0.41, respectively, but they were still at higher levels than in the preceding stable period, 1993-1994 to 2007-2008.

One interesting benchmark period would be the early 1980s, when there was another severe recession. A large upswing also occurred in men's earnings volatility for 1981-1982, followed by even higher earnings volatility for 1982-1983, which was the peak of the early 1980s recession period. That was almost the same level as the peak in earnings volatility during the Great Recession. After 1984, men's earnings volatility started to diminish. Except for the early 1990s, it tended to stabilize at around 0.38 to 0.40, from the mid-1990s up to 2007-2008.[17] Thus, men's

[15] CPS oversampled for certain states, in order to obtain reliable statistics by state, and generated individual weights to adjust for oversampling. One main purpose of this paper is to estimate population values for earnings volatility, so applying weights to estimation seems to be a reasonable procedure to obtain estimates close to the population value (Solon, Haider, and Wooldridge 2013). I use basic weights rather than March Supplement weights, because March Supplement weights are known to account for the newly added sample since 2002, which cannot be matched. To make the basic weights applicable to the matched March CPS across two adjacent years, I make a new weight for each observation by simply averaging the CPS individual basic weights over 2 years. Note that I use the same weights for the rest of the analysis in this paper. I also do all analysis without using the weights, although the results are not displayed in the paper. Those results are almost the same qualitatively as those estimated with the weights.

[16] Due to the large sample size, all the estimates have relatively small standard errors. Specific estimates corresponding to Figure 2 are presented in Table 2.C1 with asymptotic standard errors.

[17] The overall evolution pattern of men's earnings volatility before the Great Recession appears consistent with Celik et al. (2012), whose work suggests the most comparable results. The magnitudes of my estimates are somewhat lower than theirs, because my sample selection differs slightly. However, they show almost the same pattern in terms of how men's earnings volatility changed from 1979 to 2009.

earnings volatility during the Great Recession returned to the previous historic high observed a quarter-century earlier.

The graph with circle points in Figure 2.A2 is the estimation result by DES for the same sample, Sample A. As we see, the evolution patterns measured by DG and DES for Sample A are fairly close to each other, although their scales differ. Therefore, it can be said that the two different measures, DG and DES, tell almost the same story for Sample A about the growth of men's earnings volatility in the Great Recession.

On the other hand, the graph with square points is the result measured by DES for Sample B, which adds zero-earners and other outliers to Sample A. Compared with the circle points estimated by DES for Sample A, it shows a substantial difference. First of all, it is much higher overall, by about 37% to 72% across the sample years. Furthermore, earnings volatility for Sample B shows an upward trend, while that for Sample A does not. As a result of this upward trend, the highest level of earnings volatility in Sample B after the Great Recession is much higher than that experienced during the recession of the early 1980s. This implies that adding the 1% top and bottom earners (outliers) back to Sample A, and additionally including zero-earners, leads to a significant change in the estimated evolution of men's earnings volatility. It actually turns out that the zero-earners in Sample B are the main factor driving the changes: if I apply DES to Sample B excluding the 1% top-earners, I obtain almost the same pattern but just a bit lower one in terms of magnitude than that with the square points in Figure 2.

The stark difference between the results measured by DES for Samples A and B should be interpreted cautiously, because it seems closely related to particular characteristics of the DES measure and the fact that the zero-earners' fraction increased continuously from 1979 to 2011, as displayed in Figure 1. Specifically, in two cases, earnings volatility measured by DES increases with respect to zero-earners: zero-earnings in the first year but positive earnings in the following year, and vice versa. For both, the magnitude of arc percent change in real earnings measured by DES is always 200, the maximum absolute value that DES can generate for any individual.[18] In other words, DES for Sample B puts the largest weight on zero-earners' earnings change, which is surely an arbitrary way to account for zero-earners. Hence, the overall much higher level of earnings volatility measured by DES for Sample B compared to that for Sample A should be due largely to DES's unique approach. In addition, this approach should be determinative in generating the upward trend of earnings volatility for Sample B, because the proportion of male zero-earners rose continuously over 1979-2011, as seen in Figure 1.

Thus, it might not be fair to take literally the difference in magnitude between DES for Samples A and B as shown in Figure 2. However, for studying Sample B's own evolution of earnings volatility, DES still has some validity, if the matching process for the matched March CPS has no systemic errors, as discussed in Section 2.2.1. Therefore, I can summarize the results from Figure 2.A2 as follows: After the Great Recession, men's earnings volatility increased substantially for both Samples A and B, and the magnitude in the increase was comparable to that during the severe recession of the early 1980s. Particularly for Sample B, the increase during the Great Recession was so intense that the level of earnings volatility became even higher than that during any other period from 1979 to 2008.[19]

[18] A hypothetical extreme example would be that of a worker who earned $0 in one year and $1 the next year. In that case his arc percent change in annual earnings would be calculated at 200, the maximum value that can be generated by DES. Thus, it can be controversial whether it is justifiable to give the largest weight to the earnings change for every zero-earner. DG has a potential problem similar to DES (even though it does not deal with zero-earners): it gives the same weight to an individual whose annual earnings changed from $1 to $2 as to an individual whose annual earnings changed from $10,000 to $20,000. One difference here is that DG is applied to a restricted sample, Sample A, which excludes zero-earners and 1 percent top- and bottom-earners to mitigate such problems.

[19] The remaining persistently high level of earnings volatility for Sample B after the initial rise at 2008-2009, which seems different from what the earnings volatility for Sample A shows, implies that in a significant number of cases, positive-earners became zero-earners and vice versa during and after the Great Recession.

For the next step, I apply the third approach, which I have labeled SS, to both Samples A and B. As I explained in Section 2.2.3, the key idea of SS is to see the whole major quantile distribution of the age-adjusted arc percent change in real earnings along the sample years underlying the variance-term measure, DES. By doing this, a more detailed picture can be drawn by capturing distributional transformation of annual change in earnings over the entire sample years.

Figure 2.A3 shows the results of applying SS to Samples A and B. The upper panels in Figure 2.A3 display the evolution of the 10th, 25th, 50th, 75th, and 90th percentiles of the age-adjusted arc percent change in real earnings from 1979-2011, while the lower panels exhibit the difference between the 90th and 10th percentiles (p90-p10), the 90th and 50th percentiles (p90-p50), and the 50th and 10th percentiles (p50-p10). These lower panels provide, in effect, summary results for the upper panels.

The overall evolution patterns for the percentiles displayed in the upper panels look similar across Samples A and B although the magnitudes of changes differ across the samples. In the beginning of the Great Recession, i.e., the period from 2008-2009, the 10th percentile decreased substantially for both samples, but more drastically in Sample B. This implies that the increase in men's earnings volatility after the start of the Great Recession was driven mainly by the more frequent incidence of extreme negative change in real earnings for both Samples A and B. The more severe decrease in the 10th percentile in Sample B than in Sample A indicates a relatively large number of male workers became zero-earners during the Great Recession. It is noteworthy that during the early 1980s, another severe recession period, the 10th percentile also decreased significantly, but not so much as during the Great Recession.

How the substantial decrease in the 10th percentile affected earnings volatility as a whole during the Great Recession can be observed more clearly in the lower panels. For instance, a large increase in p50-p10 occurred for both samples during the Great Recession, and this was the main driver of the increase in p90-p10 in that period. If we think of p90-p10 as a statistic summarizing the dispersion of change in earnings, corresponding to variance measures such as DG and DES, it can be argued that a large portion of men became at risk of experiencing severe loss of earnings after the Great Recession began, and that this contributed importantly to the overall increase in earnings volatility during that recession.

It is interesting to note that we can find similar patterns in other recession periods. A relatively strong counter-cyclicality of p50-p10 and a relatively weak pro-cyclicality of p90-p50 are found in both samples and, as a result, overall we observe counter-cyclicality of p90-p10 throughout the whole sample period. Note that the stronger counter-cyclicality of p50-p10 is observed in both severe recession periods, the early 1980s and the Great Recession, and this led to a relatively large increase in p90-p10 for both samples. Especially for Sample B, the increase in p50-p10 in the Great Recession was historic, resulting in a noteworthy upsurge in p90-p10 during that period.

To sum up, we observe that in the Great Recession, men's earnings volatility increased to a large extent even without the inclusion of zero-earners in the sample, and the extent of this increase was at least as large as the one that occurred during the early 1980s recession, which witnessed the largest increase in men's earnings volatility prior to 2008. If we take into account the large increase in zero-earners since 2008 in the analysis, the level of men's earnings volatility after the Great Recession seems to have reached its highest level since the 1970s. Additionally, through a quantile-distribution analysis of the age-adjusted arc percent change in real earnings, we observe that a larger portion of male workers had to suffer extreme reduction in their earnings during the Great Recession, and this seems to have played a crucial role in drastically boosting men's earnings volatility after 2008. The increase in frequency of severe earnings loss appeared to be a common factor driving the rise in men's earnings volatility during other recessions as well as the Great Recession.

Figure 2.A4 documents the evolution of women's earnings volatility as measured by DG and DES for Sample A and by DES for Sample B.[20] The main distinctive feature of this graph is that women's earnings volatility showed a continuous downward secular trend over the 1979-2011 period. This differs completely from what men's earnings volatility showed in Figure 2, although the level of women's earnings volatility had been always higher than that of men, even with the continuous downward trend. Hence, we should consider this downward trend when evaluating the extent of the change in women's earnings volatility during the Great Recession. Another relevant aspect to consider is that the female zero-earners' fraction has typically been higher than the men's. In Figure 2.A1 we find that the women's zero-earners fraction ranged from 26% to 40% while the men's ranged from 7% to 18% over the period 1979-2008. In this respect, Sample B plays a more crucial role in analyzing women's earnings volatility than in analyzing men's.

As in the analysis for men, in Figure 4, DG and DES both give us the same story for women's earnings volatility in Sample A. We observed that men's earnings volatility for Sample A went up substantially at the beginning of the Great Recession, i.e. in 2008-2009, but by contrast, women's earning volatility for Sample A decreased over that same period. However, when viewed in light of Sample B, women's earnings volatility started to track upwards slightly from 2008 to 2009, and the magnitude of the increase should be regarded as more than first

[20] Due to the large sample size, all the estimates have relatively small standard errors. Specific estimates corresponding to Figure 2.A4 are presented in Table 2.C4 with asymptotic standard errors.

appears if we consider the continuous downward trend in women's earnings volatility prior to the Great Recession. As discussed above, bearing in mind that Sample B better represents the whole female population than does Sample A, it seems fair to argue that women also had to go through a significant increase in earnings volatility during the Great Recession.

Figure 2.A5 shows the results from applying SS to women in Samples A and B. In that graph we see in more detail the distributional change in arc percent change in females' earning over the sample years. The main features of Figure 2.A5 may be summarized as follows: first, we observe a large distributional change from Sample A to Sample B, even compared with the one evident for men (see Figure 3). This implies that zero-earners have a more important role for women's earnings volatility than for men's, as previously anticipated. To be more specific, the gap between p90 and p10 becomes much bigger in Sample B than in Sample A; this is well summarized in the evolution of p90-p10 in the lower panels.[21]

The second noteworthy feature in Figure 2.A5 is that strong counter-cyclicality for p50-p10 and weak pro-cyclicality for p90-p50 are observed, as they were in the analysis for men (see Figure 3); this appears more clearly in Sample B. As a result, a relatively clear counter-cyclicality for p90-p10 is found in Sample B, even considering its downward trend. We note that p90-p10 for Sample B rose after 2008-2009, when the Great Recession started; this result is consistent with what DES shows us about women's earnings volatility in Figure 4.

To summarize, if it is considered that Sample B is more appropriate for the analysis of women's earnings volatility, it can be concluded that women's earnings volatility also increased

[21] A notable aspect is that the gap between p75 and p25 for females in Sample B had been relatively small over the whole period, compared to that for males in the same sample (see Figure 3); this means that a relatively large portion of women experienced little earnings change over 1979-2011, compared to men. Thus, we see that the continuous reduction in women's earnings volatility over the sample period had been driven mostly by the decrease in the probability that women would experience severe earnings change; this is likely to be associated closely with the fact that the zero-earners' fraction for women decreased considerably during the same period (see Figure 1).

during the Great Recession, even though it displayed a continuous downward trend prior to that time. The main factor behind the rise seems to be the strong counter-cyclicality of p50-p10 for arc percent change in real earnings, as observed in the analysis for men. This means that during the Great Recession, women also became more likely to experience severe real earnings loss, and that was the main reason why women's earnings path also became more volatile during the Great Recession.

2.3.2 Subgroup Analysis by Education and Age

This section presents the results when earnings volatility evolution is disaggregated by education and age subgroups. I have used all three measures—DG, DES, and SS—to estimate earnings volatility for each subgroup in both Samples A and B, but for brevity I mainly display results measured by DES for subgroups in sample B, which contains the zero-earners group. Other subgroup analysis results appear in the appendix.[22]

The upper panel of Figure 2.A6 shows the change in men's earnings volatility by three education subgroups: those with high school diplomas or less, with some college, and with college degrees. On top of that, the lower panel of Figure 2.A6 shows the analysis for male age subgroups: ages 25-34, 35-49, and 50-59.[23,24]

Within each subgroup analysis for education and age, we find some level-differences across subgroups. For instance, in the education subgroup analysis, we observe that the overall level of earnings volatility is inversely related to education level: the less-educated had overall higher earnings volatility throughout the sample period than did those with more education. On the

[22] Refer to Figure 2.B2-Figure 2.B5.

[23] Specific estimates corresponding to Figure 2.A6 are presented in Table 2.C5 with asymptotic standard errors.

[24] I also have performed additional subgroup analyses for four education- and seven age-subgroups, and the results are almost the same qualitatively. Please refer to Figure 2.B6-Figure 2.B11

other hand, in the age subgroup analysis, we see that the overall level of earnings volatility is higher for the younger (25-34) and older (50-59) men than the middle-aged (35-49) ones.[25]

However, in terms of how earnings volatility evolved during and after the Great Recession, all subgroups show a story similar to what we previously saw for the aggregated sample. Across all education or age subgroups, men's earnings volatility increased substantially after the Great Recession began, at which time it reached a historic high over the entire sample period. Furthermore, this increased level of earnings volatility was sustained across all the subgroups throughout the Great Recession, as was also observed in the aggregated sample.

It is noteworthy that the main factor driving the subgroup patterns above also turns out to be quite similar to that for the aggregated sample. In an additional analysis where I apply SS to all subgroups,[26] we observe a substantial increase in frequency of severe earnings loss for every male subgroup in the Great Recession, and this was true for other previous recessions, too. This implies that intensive negative change in earnings should be considered a universal factor affecting all male subgroups by education or age, which plays a crucial role in generating the counter-cyclical behavior of men's earnings volatility.

Figure 2.A7 displays the results for female subgroup analyses by education (the upper panel) and age (the lower panel).[27,28] We observe a similar pattern with men's education subgroups that overall earnings volatility increases with a decrease in education. But as for women's age subgroups, we find a bit different patterns from men's. Note that in the case of men, the middle-

[25] These level differences seem to be closely related to the differences in working-hours volatility across the male subgroups, considering that working-hours volatility considerably accounts for overall earnings volatility, as we will see in Section 2.3.3. For example, less skilled younger or older male workers are likely to have less job security compared to high skilled and middle-aged ones. Also, general differences in the level of earnings across the male subgroups would be another factor causing such level differences in earnings volatility.

[26] See Figure 2.B2.

[27] Specific estimates corresponding to Figure 2.A6 are presented in Table 2.C6 with asymptotic standard errors.

[28] I also have performed additional subgroup analyses for four education- and seven age-subgroups, and the results are almost the same qualitatively. Please refer to Figure 2.B12-Figure 2.B17

aged men show relatively low earnings volatility compared to the younger and older. In the case of women, however, the overall level of women's earnings volatility tends to decrease with age. It might reflect generally different labor participation patterns between men and women. For example, women tend to enter and leave the labor market more frequently than men over the whole life cycle, and women's entrances and exits also tend to be frequent even in middle age; this differs from typical men's labor-market behavior.[29]

Even with some level differences of earnings volatility across the female subgroups by education and age, they generally show a consistent evolution pattern of earnings volatility, which is similar to that for the female aggregated sample. Except for the female college graduate group and the 25-34 age group, all other female education and age subgroups display an upsurge in earnings volatility after the Great Recession began. The main factor driving the rise was the increase in severe negative change in earnings,[30] as we saw previously in the analysis for the female aggregated sample. It also turns out to be a main factor raising earnings volatility during earlier recessions. This implies, as in the men's subgroup analyses, that severe earnings loss was a prevalent and important factor underlying the counter-cyclical behavior of women's earnings volatility across most female subgroups by education or age.

To sum up, the increasing patterns of earnings volatility we see in the whole aggregated sample during the Great Recession also appear generally in various male and female subgroups. Furthermore, the increased incidence of severe negative change in earnings appears to be a common factor producing the counter-cyclical volatility in earnings across most male and female subgroups, as in the analysis for the aggregated sample. Particularly during the Great Recession, we observe a historic upswing in the frequency of large earnings loss for most male and female

[29] To be more specific, women's work hours tend to become more volatile than men's depending on changes in family compositions, such as marriage, pregnancy, and child care.
[30] See Figure 2.B3.

subgroups, so it led to a historic increase in men's and women's earnings volatility for the aggregated group as well as for each subgroup.

2.3.3 Unemployment, Work Hours, and Earnings Volatility in the Great Recession.

So far, we have found that male and female workers experienced a substantial upsurge in earnings volatility during the Great Recession, even though the increase for women was not so great as that for men. A historic increase in the frequency of severe earnings loss was found to be a significant factor in the rise in men's and women's earnings volatility during the Great Recession. Moreover, the increased tendency to experience large earnings loss seems to largely account for the rises in earnings volatility during previous recessions. Through the subgroup analysis in the previous section, we saw that these patterns were also generally observed across most subgroups by education and age for both men and women.

Then, a natural question arises: what is the fundamental driving force that caused the substantial increases in the frequency of severe loss in earnings during recessions including the Great Recession? When we consider that the Great Recession was characterized mainly by an extraordinarily high unemployment rate that persisted for a relatively long time, studying the relationship between unemployment and earnings volatility appears to be a sensible starting point to deal with this question.

In the March CPS, a survey question asks whether an interviewee looked for a job or was laid off during nonworking weeks in the year prior to the survey. By using the answers to this question, I calculate the yearly portion of individuals who had worked for less than or equal to 49 weeks during the preceding year and who had experienced unemployed status in their non-working periods. Figure 2.A8 shows the results for men and women from 1979 to 2011. In that graph, the counter-cyclicality of the men's and women's unemployment rate is easily recognizable for both Samples A and B. We observe especially that the percentage of male workers who experienced unemployment went up more substantially after the Great Recession than in any other recession periods. It suggests that a high positive correlation might exist between unemployment and earnings volatility.

To look at how closely the experience of unemployment relates to men's and women's earnings volatility during the Great Recession, I re-estimated men's and women's earnings volatility after excluding from the matched samples individuals who had experienced unemployment. Figure 2.A9 summarizes the results; the upper panel displays results for men, the lower panel for women. For both upper and lower panels, the line with X-shaped points denotes earnings volatility measured by DES for Sample A, and the line with triangle points stands for earnings volatility measured by DES for Sample A excluding workers who experienced unemployment during the 2 years in the reporting period[31] (for convenience, I call this "Sample A without the unemployed"). Similarly, for both the upper and lower panels, the line with circle points denotes earnings volatility measured by DES for Sample B, and the line with square points stands excludes the unemployed.

A noteworthy feature in the upper panel of Figure 2.A9 is that a large portion of the increase in men's earnings volatility after the Great Recession in the original samples largely disappears in the samples without the unemployed. Moreover, while a significant counter-cyclicality of men's earnings volatility over the whole sample period is observed in the original samples, this diminishes substantially for samples that exclude the unemployed.

[31] According to the discussion above, "the individuals having experienced unemployment during the consecutive 2 years" are defined as those who worked less than or equal to 49 weeks, and were laid off or looked for jobs in the nonworking period in either the first year or second year.

In the lower panel of Figure 2.A9, we observe similar patterns for women. If I remove the unemployed from the original Sample B, the increase in women's earnings volatility during the Great Recession disappears, and the continuing downward trend in volatility becomes more evident. As for women in Sample A, similarly we see that excluding the unemployed makes the downward trend of earnings volatility more obvious during the recession periods. That pattern is especially evident in the Great Recession.

The observations above suggest that the intense increase in earnings volatility after the Great Recession likely reflected the severity of the unemployment shock during the Great Recession. In addition, the earnings shock from unemployment seems to have been a common influence driving the rise in earnings volatility during preceding recessions.

Considering that earnings data are a mixture of market-wage and hours-worked information, the above analysis implies that a large source of the increase in earnings volatility during the Great Recession might be hours-worked volatility, rather than market-wage volatility. To explicitly investigate this point, I decompose earnings volatility into the variance of change in log annual worked hours, the variance of change in log hourly wage, and the covariance of the two terms. The specific decomposition formula is as follows:

$$\mathrm{Var}(\Delta\log(earnings)) = \mathrm{Var}(\Delta\log(hours)) + \mathrm{Var}(\Delta\log(wage))$$
$$+2\mathrm{Cov}(\Delta\log(hours), \Delta\log(wage))$$

Here *earnings* means annual real earnings, *hours* indicates total annual working hours, and *wage* implies hourly wage, calculated from the March CPS by dividing *earnings* by *hours*. $\Delta\log(\cdot)$ refers to year-to-year change in log terms, and $\Delta\log(earnings)$, $\Delta\log(hours)$, $\Delta\log(wage)$ were all age-adjusted for the analysis. Note that $\mathrm{Var}(\Delta\log(earnings))$ is the same measure as DG, except that it is expressed in terms of the variance rather than the standard deviation. Because it uses logarithms, it can be applied to only Sample A, whose earnings are all positive.

Figure 2.A10 presents results from this decomposition analysis for men (upper panel) and women (lower panel) in Sample A. For both the upper and lower panels, the upper line with square points represents the variance version of earnings volatility previously measured by DG for sample A. Lines below with circle points, diamond points, and x-shaped points are graphs for $\mathrm{Var}(\Delta \log(hours))$, $\mathrm{Var}(\Delta \log(wage))$, and $2\mathrm{Cov}(\Delta \log(hours), \Delta \log(wage))$, respectively. If we add up all of these, the upper graph with square points will be obtained. Note that the consistent negativity of the covariance terms is due largely to the innate negative correlation between total annual working hours and calculated hourly wage (= annual earnings / total annual working hours).

Noteworthy in the upper panel of Figure 2.A10 is that, among men, the counter-cyclicality of $\mathrm{Var}(\Delta \log(hours))$ has been more intense and evident than that of $\mathrm{Var}(\Delta \log(wage))$. Thus, the counter-cyclicality of men's earnings volatility observed for all sample years seems to have come mainly from counter-cyclical annual work hours volatility. Particularly for the Great Recession period, the upsurge in work hours volatility ($\mathrm{Var}(\Delta \log(hours))$) was more severe than in any prior recession period. Therefore, it can be argued that the drastic increase in work hours volatility had a more important role in raising men's earnings volatility in Sample A during the Great Recession than it did in any other recession.

The lower panel of Figure 2.A10 depicts the decomposition results for women in Sample A. As seen previously, women's earnings volatility continuously decreased, in contrast to men's. An interesting aspect in the lower panel of Figure 2.A10 is that the downward trend of women's earnings volatility has been driven largely by a decrease in women's work-hours volatility.

Moreover, the strong counter-cyclicality of women's earnings volatility during the early 1980s and 1990s and the relatively weak counter-cyclicality during the early 2000s and the Great Recession also seem mainly dependent on how counter-cyclical women's work-hours volatility was for each corresponding time.

Overall, Figure 2.A10 tells us that the evolution of earnings volatility can be accounted for largely by work-hours volatility ($\mathrm{Var}(\Delta \log(hours))$) rather than hourly wage volatility ($\mathrm{Var}(\Delta \log(wage))$). In particular, a large percentage of men experienced significant changes in their work hours in the Great Recession, compared to previous years, and this led them to experience large variations in their annual earnings during this period.

Connecting the results above with the previous unemployment analysis in Figure 2.A9, we can come up with a more complete explanation for the counter-cyclicality of earnings volatility over the sample period, as well as the substantial increase in earnings volatility during the Great Recession. The increase in earnings volatility during recessions seems driven mainly by an increase in work-hours volatility, and that increase appears to come largely from unemployment shocks. Figure 2.A11 corroborates this link: I decompose the variance of change in log worked hours (work-hours volatility) into two components, one from individuals who experienced unemployment, the other from those who did not.[32] As is seen in the picture, the counter-cyclicality of the variance of the change in log worked hours is largely due to the group that experienced unemployment. Especially in the Great Recession, a historic unemployment shock occurred—in other words, there was an unprecedented chance to experience reduced working hours—and this facilitated the momentous upsurge in men's earnings volatility. It also substantially slowed the downward trend of women's earnings volatility.

[32] See Appendix 2 for the detailed decomposition method.

2.4 Conclusion

Men's and women's earnings volatility increased substantially during the Great Recession. Especially for men, the increase was at least as great as it had been during the last severe recession, in the early 1980s. The rise in earnings volatility was largely due to a historic increase in the probability of a reduction in annual earnings. Furthermore, the increase in the frequency of severe earnings loss was mainly due to intensive decreases in hours worked among the workers experiencing unemployment during the Great Recession. This empirical pattern is also found to be a common main factor driving the increase in earnings volatility during previous recessions. Accordingly, it seems that unemployment shocks have played a central role in generating the strong counter-cyclical behavior of earnings volatility over the last three decades.

As discussed in Section 2.2.3, the increase in earnings volatility cannot be equated directly with increased risk or uncertainty if individuals insure against earnings fluctuations in advance. For the substantial rise in earnings volatility during recessions including the Great Recession, however, an unwanted decrease in hours worked due to unemployment appears to have played a crucial part. This implies that individuals were likely to go through an unexpected decrease in their earnings during recessions. Particularly for the Great Recession, there was a historic increase in unemployment, and it persisted for a long time. Moreover, the unemployment shock hit more severely the less-skilled and younger male workers (Elsby et al. 2011), who were less likely to have enough savings or credit for a buffer against earnings shocks. Therefore, the findings of this paper indicate a possibility that the substantial rise in earnings volatility during the Great Recession would have caused a severe loss in social welfare.

Additional studies, however, are still required to clarify the welfare implications of the substantial rise in earnings volatility during the Great Recession. For instance, we need to investigate how the increase in individual earnings volatility affected family income volatility. This paper shows that there was a historic upsurge in men's earnings volatility and a significant rise in women's during the Great Recession. As earnings account for a large share of total family income generally (Dynan, Elmendorf, and Sichel 2012), we can be confident that the increased earnings volatility for both men and women translated into increased family income volatility as well.

However, a remaining empirical question is the extent to which the increase in earnings volatility was alleviated through various family income adjustments during the Great Recession. For example, a negative earnings shock to an individual could be mitigated by family labor supply adjustment, reductions in tax rates, governmental income transfer programs, and private transfers from relatives or friends (Dynarski and Gruber. 1997; Stephens, 2002).

Another relevant question is how the increase in individual earnings volatility affected family consumption in the Great Recession. Family consumption responses to earnings changes depend on saving and borrowing behavior as well as the family income adjustments already discussed. Hence, studying the consumption consequences of earnings changes will require data on consumption as well as family income (Blundell, Pistaferri, and Preston 2008; Dynarski and Gruber. 1997; Gorbachev 2014).

Therefore, investigating the dynamics of both family income changes and family consumption responses in the Great Recession is a natural extension of the research reported here. Implementing that extension is likely to require an alternative data set with richer information on family income and consumption, probably the Panel Study of Income Dynamics. I intend to pursue this line of inquiry in my own future research.

APPENDICES

APPENDIX A

FIGURES FOR CHAPTER 2

Figure 2.A1 Fraction of Zero-earners in Matched March CPS

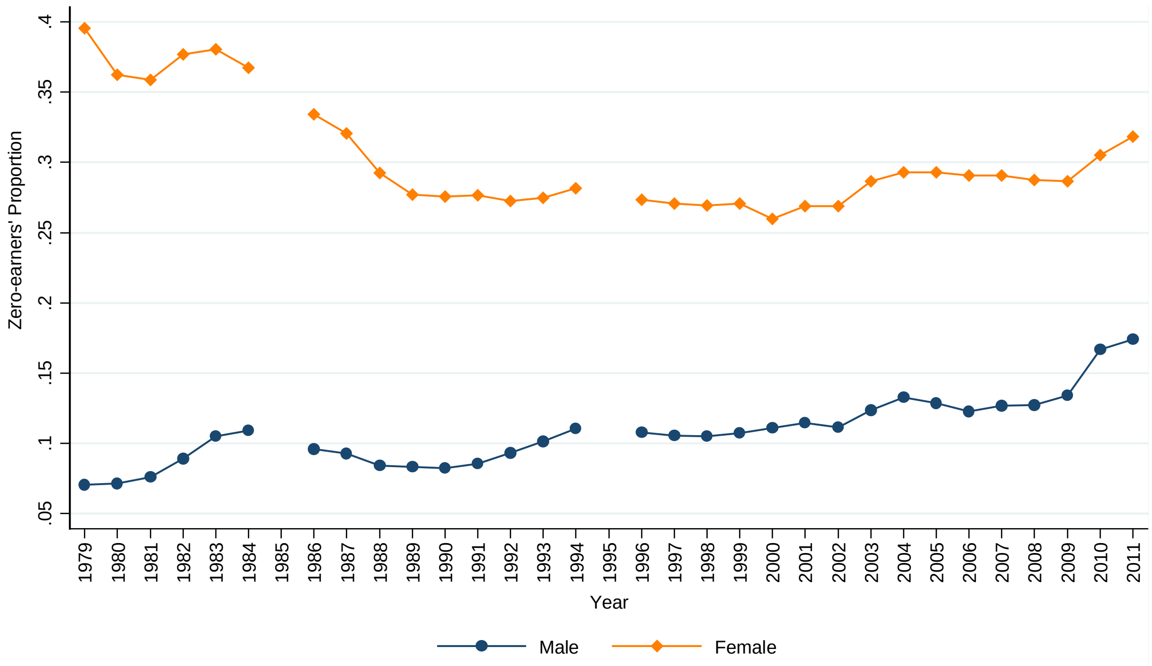
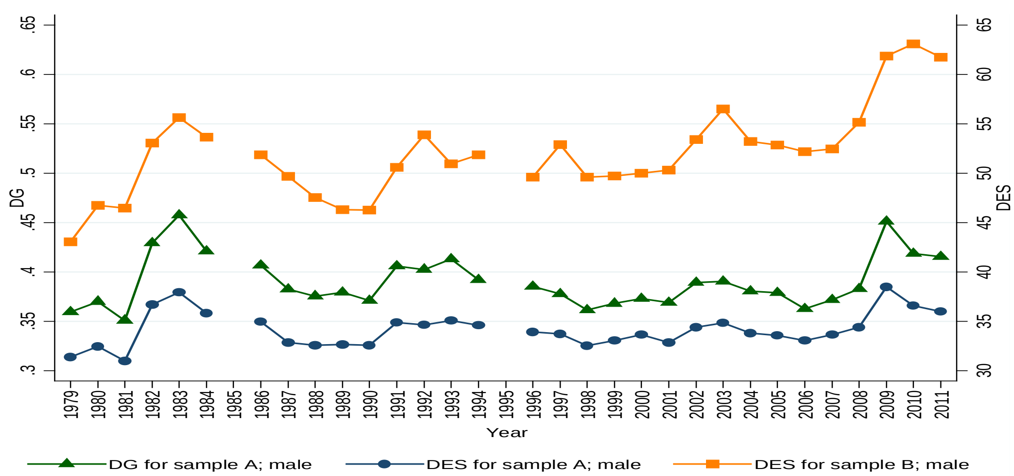
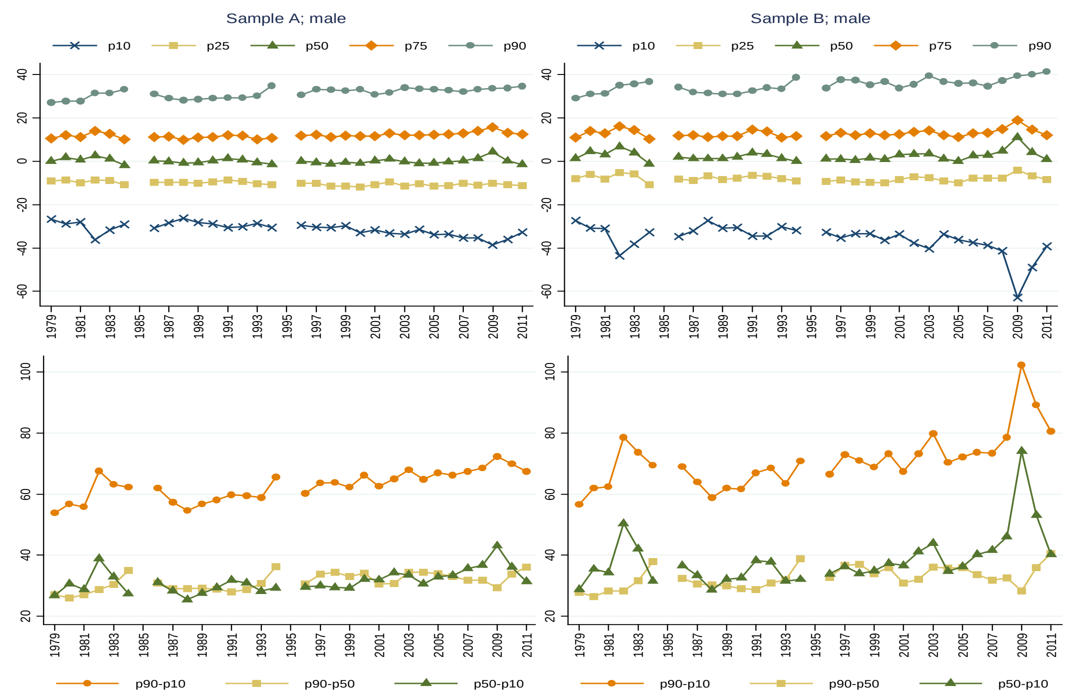


Figure 2.A2 Men's Earnings Volatility (DG and DES; Ages 25-59)



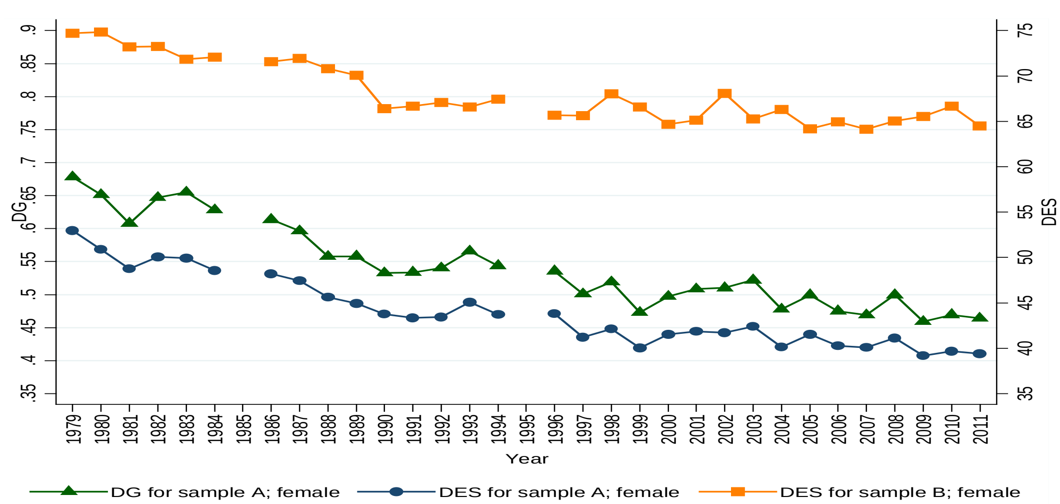
Notes: DG refers to the standard deviation of difference in log real earnings residuals over two adjacent years (see Section 2.2.3). DES refers to the standard deviation of age-adjusted arc percent change in real earnings (see Section 2.2.3). Sample A consists of civilian workers, ages 25-59, having worked at least a week with positive earnings in the previous year of the survey year, not self-employed, not students, and with no imputed earnings data. Furthermore, Sample A excludes the top and bottom 1 percent earners among individuals who satisfy the prelisted criteria. Sample B includes zero-earners and the top and bottom 1percent earners in addition to those in Sample A.

Figure 2.A3 Men's Earnings Volatility (SS; Ages 25-59)



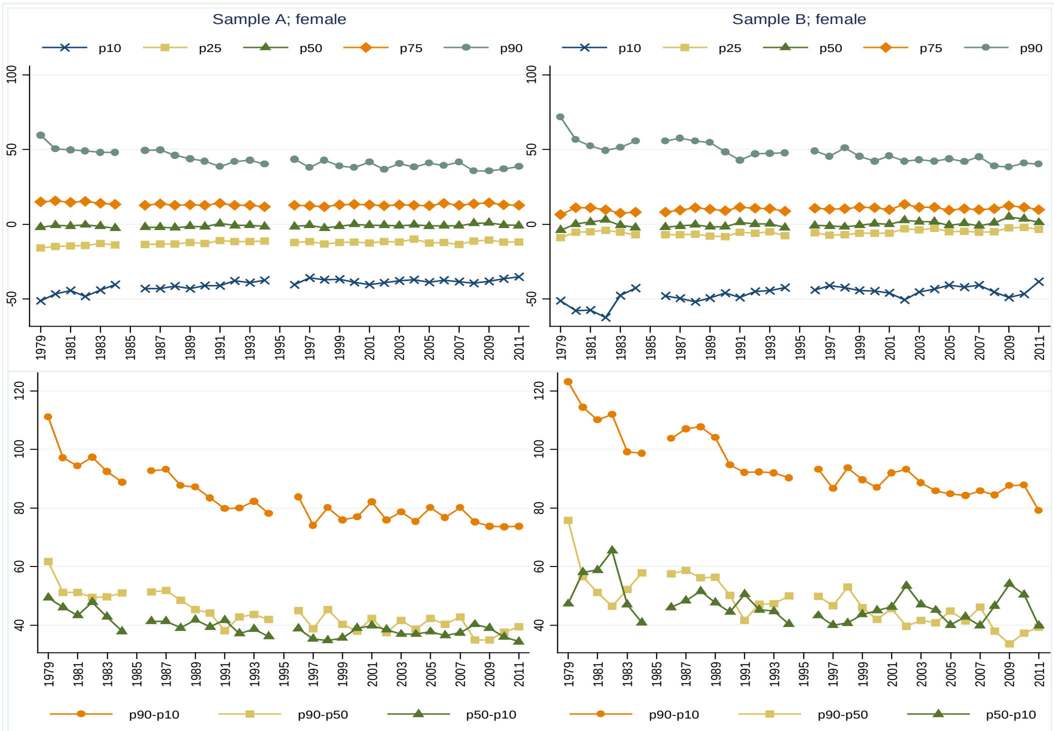
Notes: SS refers to the method of looking at the evolution of major quantiles for age-adjusted arc percent change in real earnings (see Section 2.2.3). Sample A consists of civilian workers, ages 25-59, having worked at least a week with positive earnings in the previous year of the survey year, not self-employed, not students, and with no imputed earnings data. Furthermore, Sample A excludes the top and bottom 1percent earners among individuals who satisfy the prelisted criteria. Sample B includes zero-earners and the top and bottom 1percent earners in addition to those in Sample A.

Figure 2.A4 Women's Earnings Volatility (DES & DG; Ages 25-59)



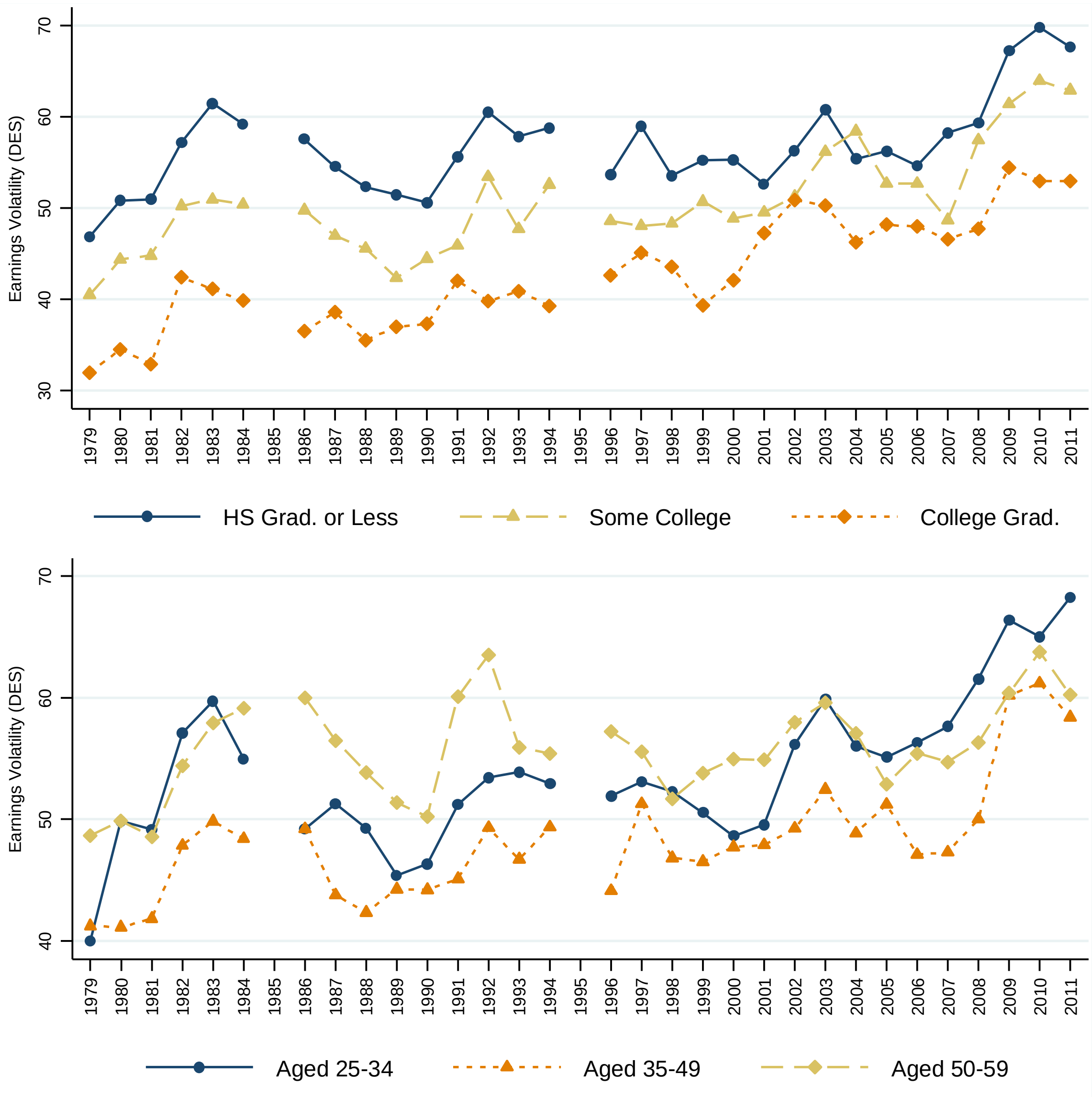
Notes: DG refers to the standard deviation of difference in log real earnings residuals over two adjacent years (see Section 2.2.3). DES refers to the standard deviation of age-adjusted arc percent change in real earnings (see Section 2.2.3). Sample A consists of civilian workers, ages 25-59, having worked at least a week with positive earnings in the previous year of the survey year, not self-employed, not students, and with no imputed earnings data. Furthermore, Sample A excludes the top and bottom 1 percent earners among individuals who satisfy the prelisted criteria. Sample B includes zero-earners and the top and bottom 1percent earners in addition to those in Sample A.

Figure 2.A5 Women's Earnings Volatility (SS; Ages 25-59)



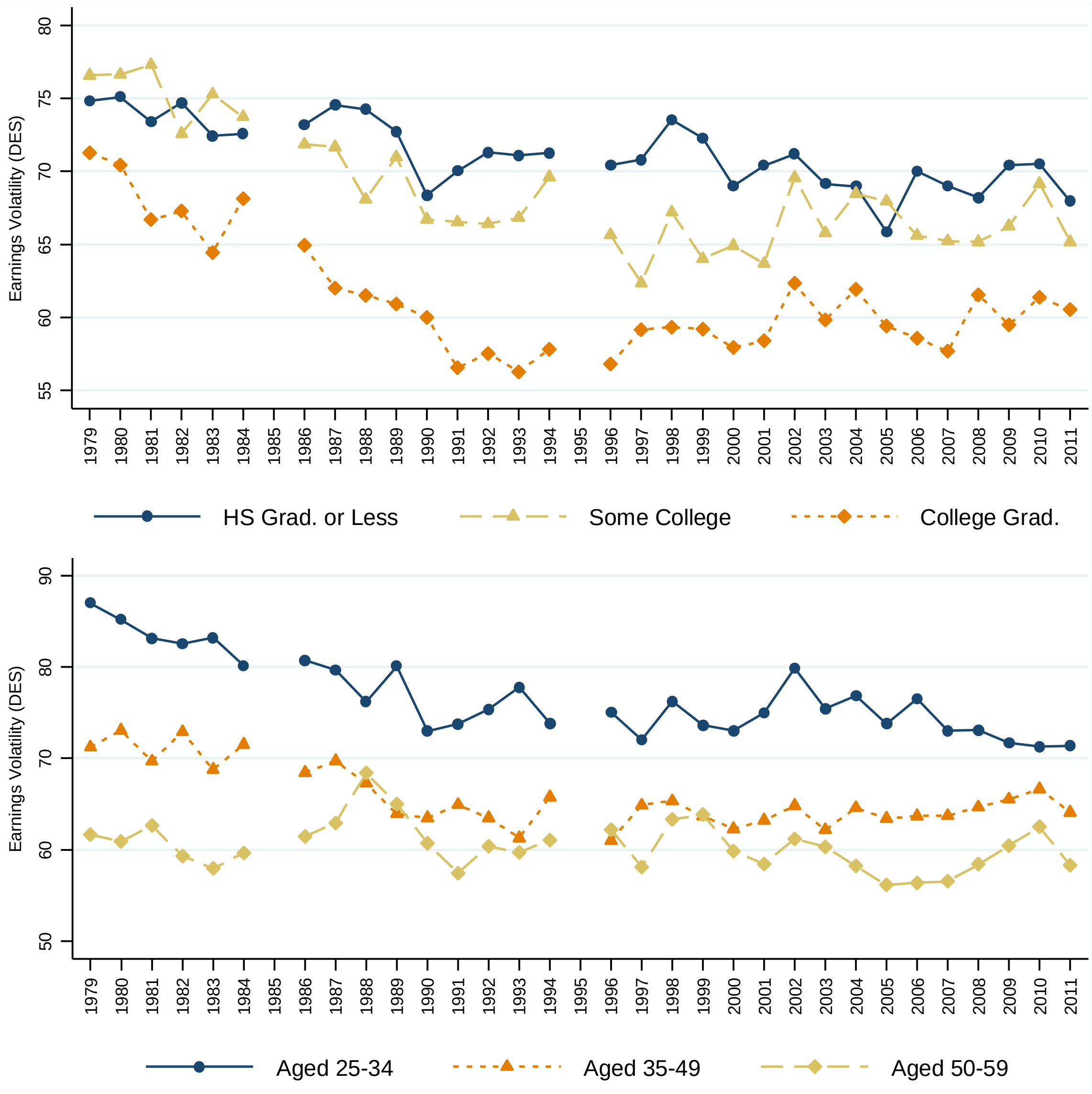
Notes: SS refers to the method of looking at the evolution of major quantiles for age-adjusted arc percent change in real earnings (see Section 2.2.3). Sample A consists of civilian workers, ages 25-59, having worked at least a week with positive earnings in the previous year of the survey year, not self-employed, not students, and with no imputed earnings data. Furthermore, Sample A excludes the top and bottom 1percent earners among individuals who satisfy the prelisted criteria. Sample B includes zero-earners and the top and bottom 1percent earners in addition to those in Sample A.

Figure 2.A6 Men's Earnings Volatility by Education and Age (DES for Sample B)



Notes: DES refers to the standard deviation of the age-adjusted arc percent change in real earnings (see Section 2.2.3). Sample B includes zero-earners and the top and bottom 1percent earners in addition to those in Sample A; Sample A consists of civilian workers, ages 25-59, having worked at least a week with positive earnings in the previous year of the survey year, not self-employed, not students, and with no imputed earnings data. Furthermore, Sample A excludes the top and bottom 1percent earners among individuals who satisfy the prelisted criteria.

Figure 2.A7 Women’s Earnings Volatility by Education and Age (DES for Sample B)



Notes: DES refers to the standard deviation of the age-adjusted arc percent change in real earnings (see Section 2.2.3). Sample B includes zero-earners and the top and bottom 1percent earners in addition to those in Sample A; Sample A consists of civilian workers, ages 25-59, having worked at least a week with positive earnings in the previous year of the survey year, not self-employed, not students, and with no imputed earnings data. Furthermore, Sample A excludes the top and bottom 1percent earners among individuals who satisfy the prelisted criteria.

Figure 2.A8 The Yearly Fraction of Male and Female Workers Having Experienced Unemployment

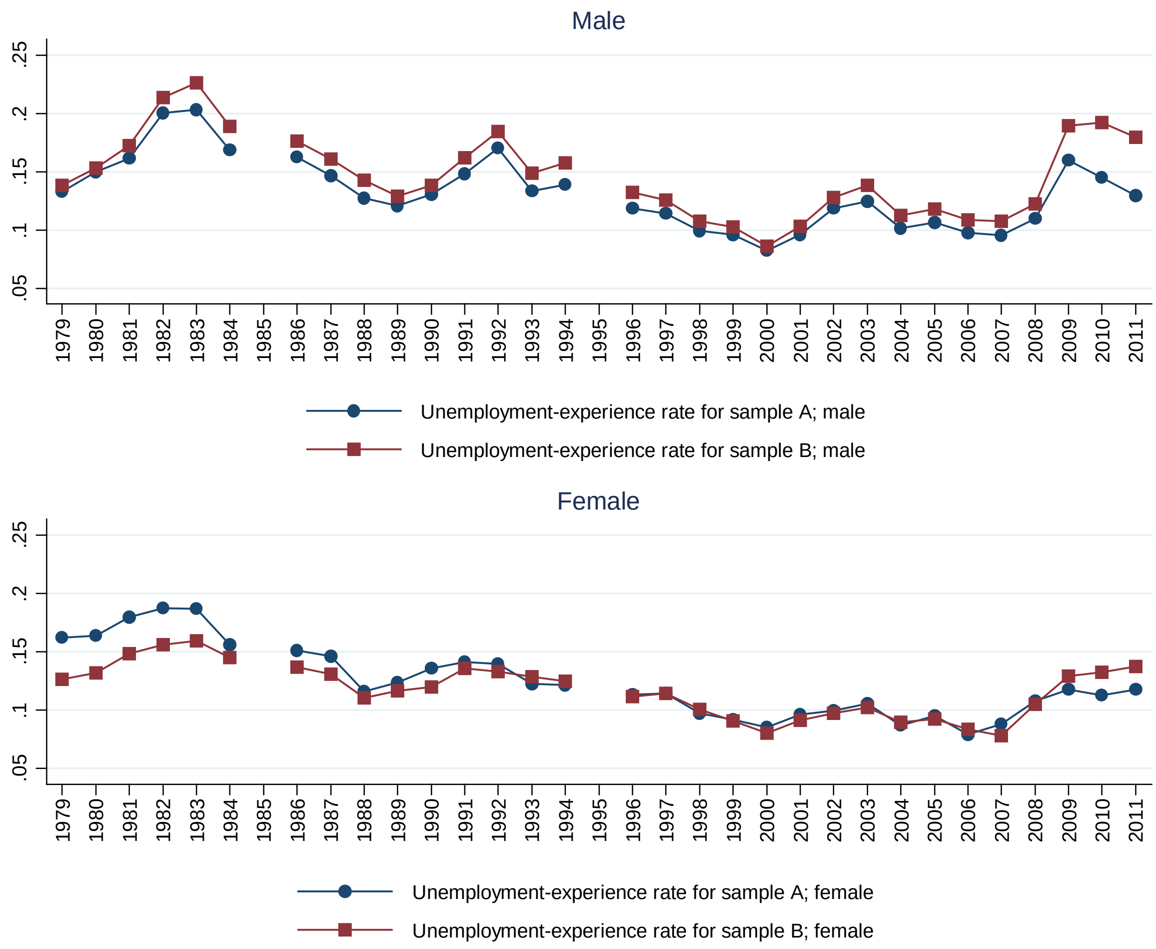
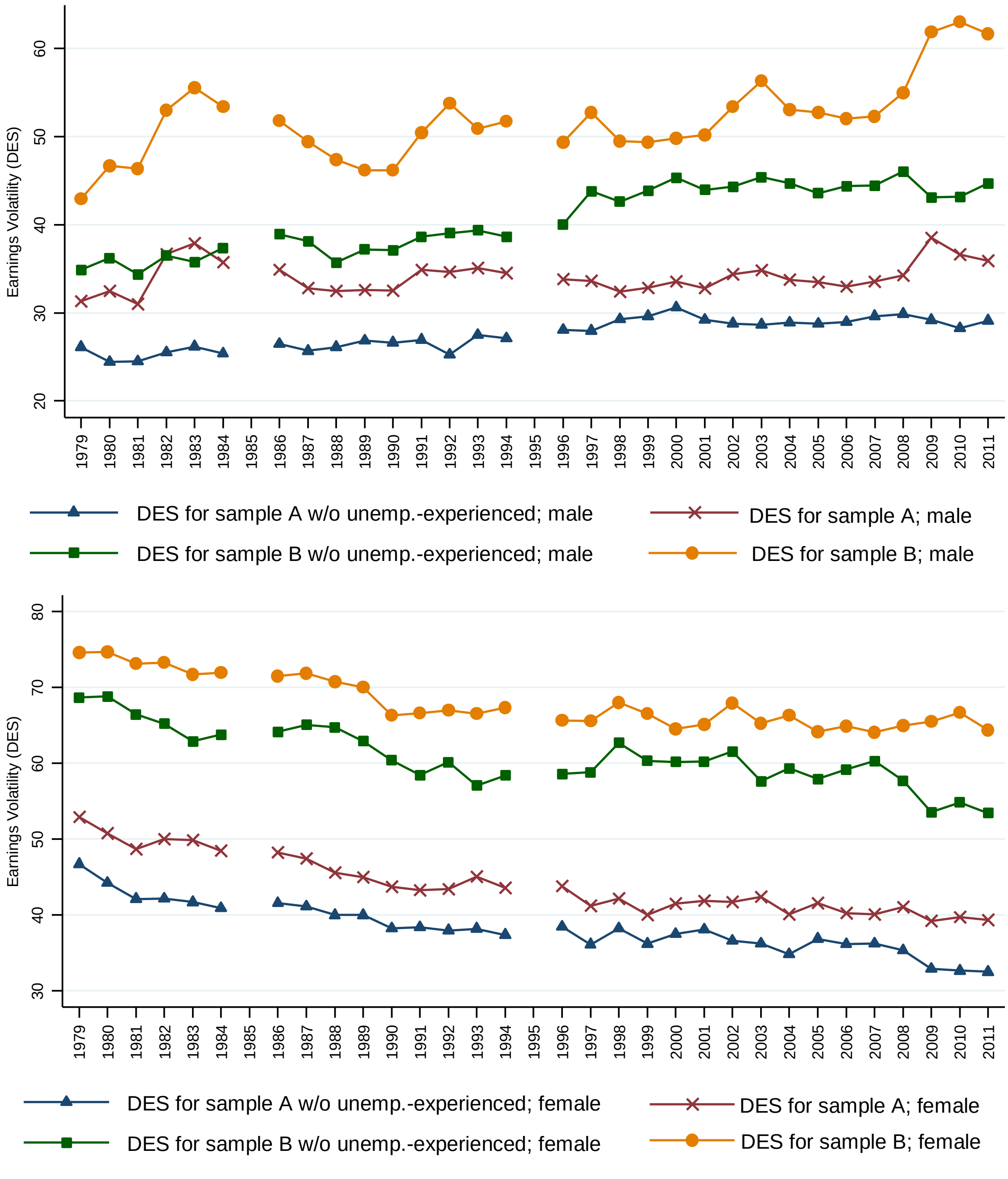
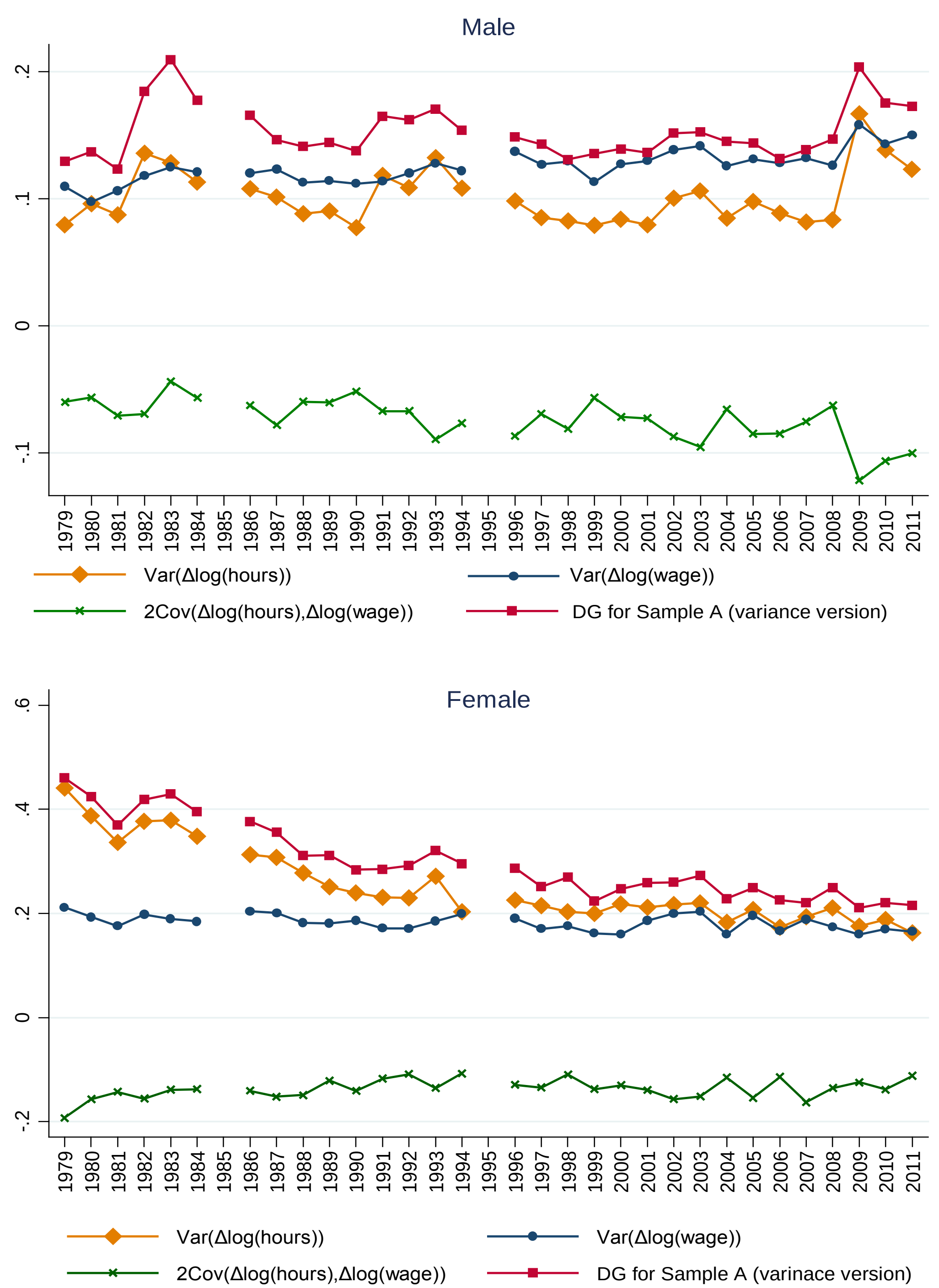


Figure 2.A9 The Effect of Unemployment on Men’s and Women’s Earnings Volatility



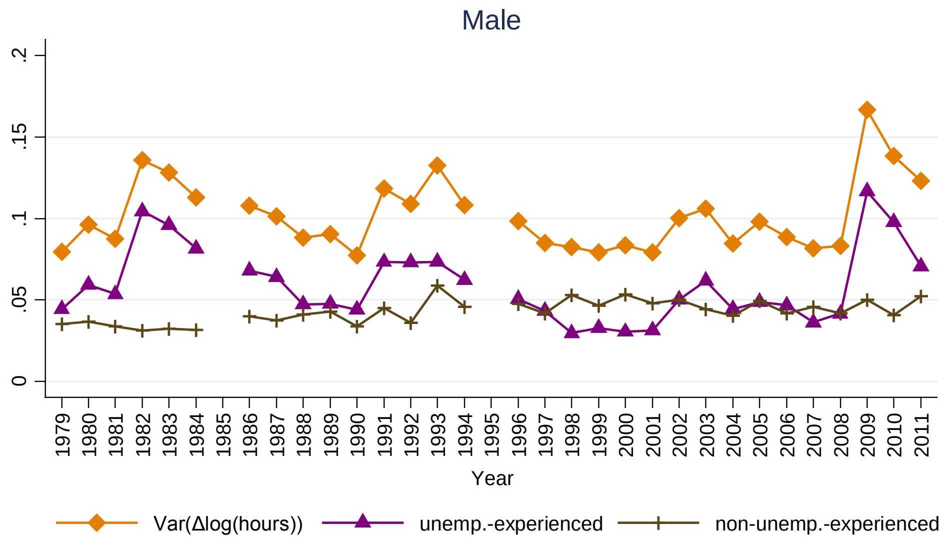
Notes: DES refers to the standard deviation of the age-adjusted arc percent change in real earnings (see Section 2.2.3). Sample A consists of civilian workers, ages 25-59, having worked at least a week with positive earnings in the previous year of the survey year, not self-employed, not students, and with no imputed earnings data. Furthermore, Sample A excludes the top and bottom 1percent earners among individuals who satisfy the prelisted criteria. Sample B includes zero-earners and the top and bottom 1percent earners in addition to those in Sample A.

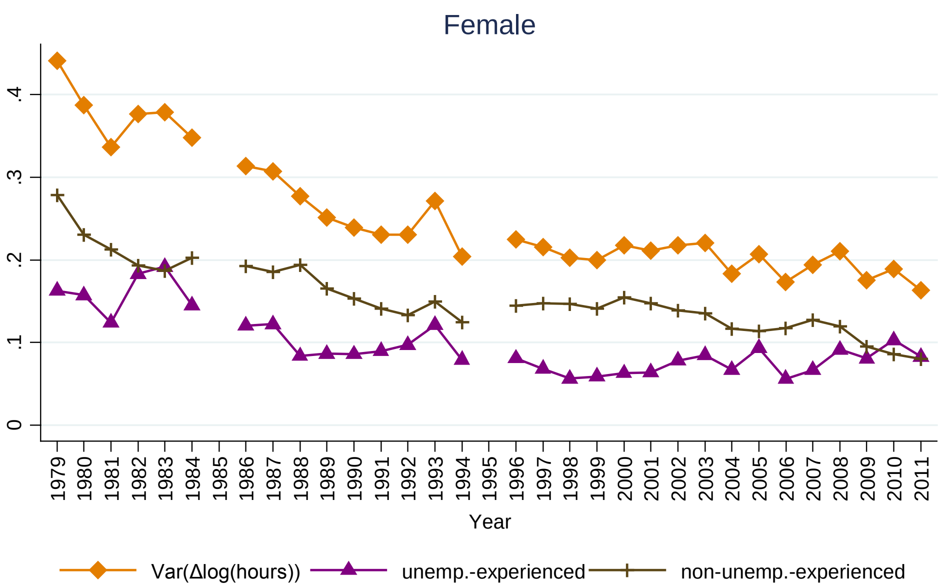
Figure 2.A10 Decomposition of DG for Men and Women in Sample A



Notes: DG means the standard deviation of difference in log real earnings residuals over two adjacent years (see Section 2.2.3.) Sample A consists of civilian workers, ages 25-59, having worked at least a week with positive earnings in the previous year of the survey year, not self-employed, not students, and with no imputed earnings data. Furthermore, Sample A excludes the top and bottom 1percent earners among individuals who satisfy the prelisted criteria.

Figure 2.A11 Decomposition of Var(Δlog(*hours*)) for Men and Women in Sample A





Note : Sample A consists of civilian workers, ages 25-59, having worked at least a week with positive earnings in the previous year of the survey year, not self-employed, not students, and with no imputed earnings data. Furthermore, Sample A excludes the top and bottom 1 percent earners among individuals who satisfy the prelisted criteria.

APPENDIX B

SUPPLEMENTAL FIGURES FOR CHAPTER 2

Figure 2.B1 Yearly Match Rate for Matched March CPS

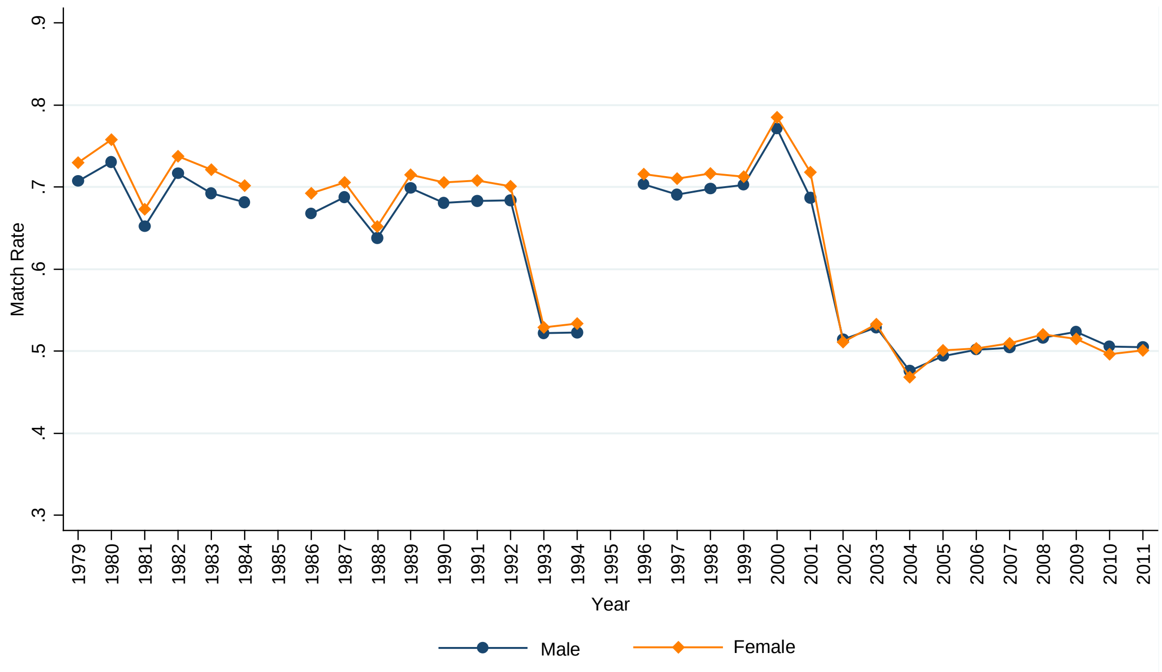


Figure 2.B2 Men's Earnings Volatility by Education and Age (SS for A &B)

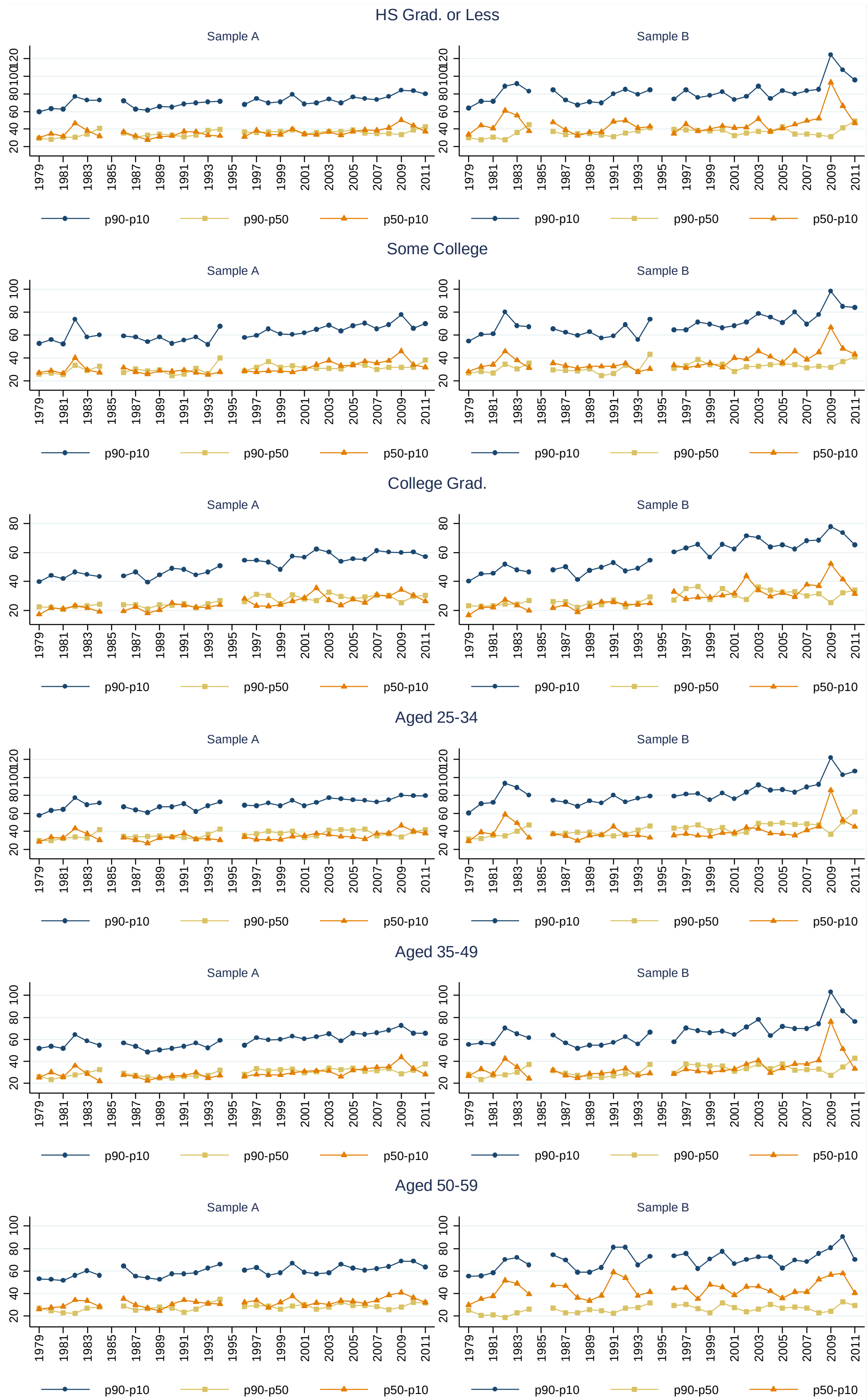


Figure 2.B3 Women's Earnings Volatility by Education and Age (SS for A &B)

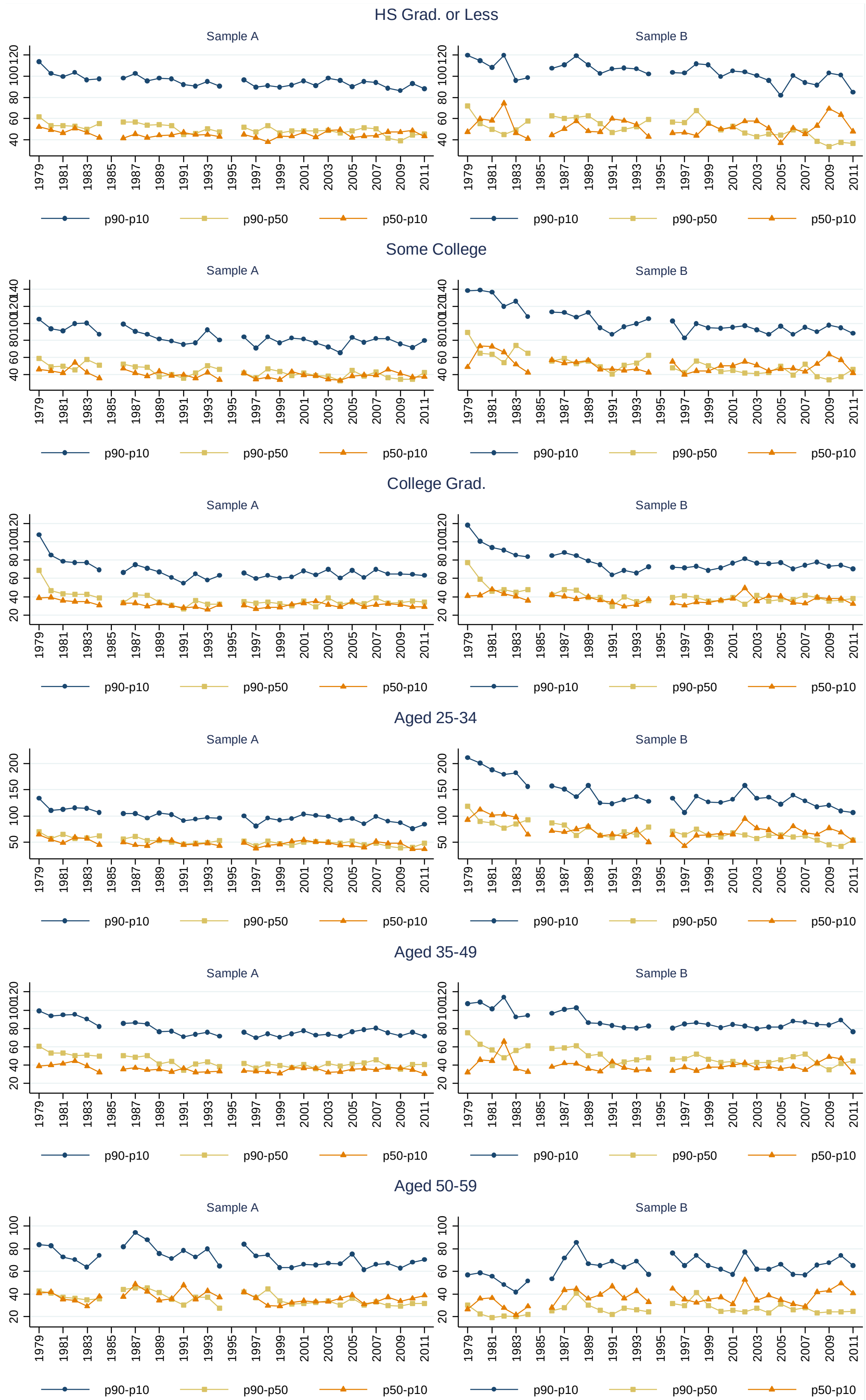


Figure 2.B4 Men's Earnings Volatility by Education and Age (DES for A)

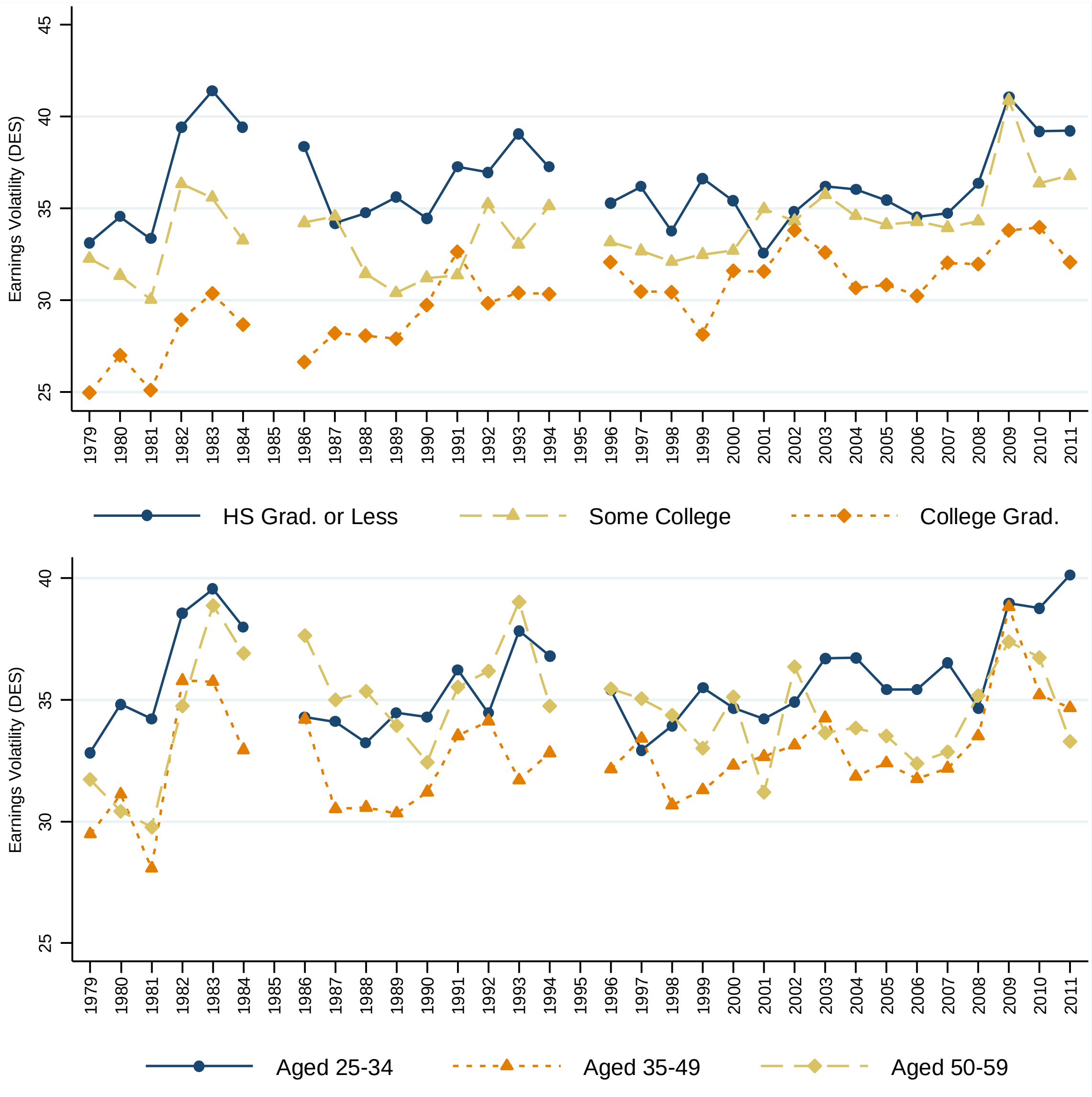


Figure 2.B5 Women's Earnings Volatility by Education and Age (DES for A)

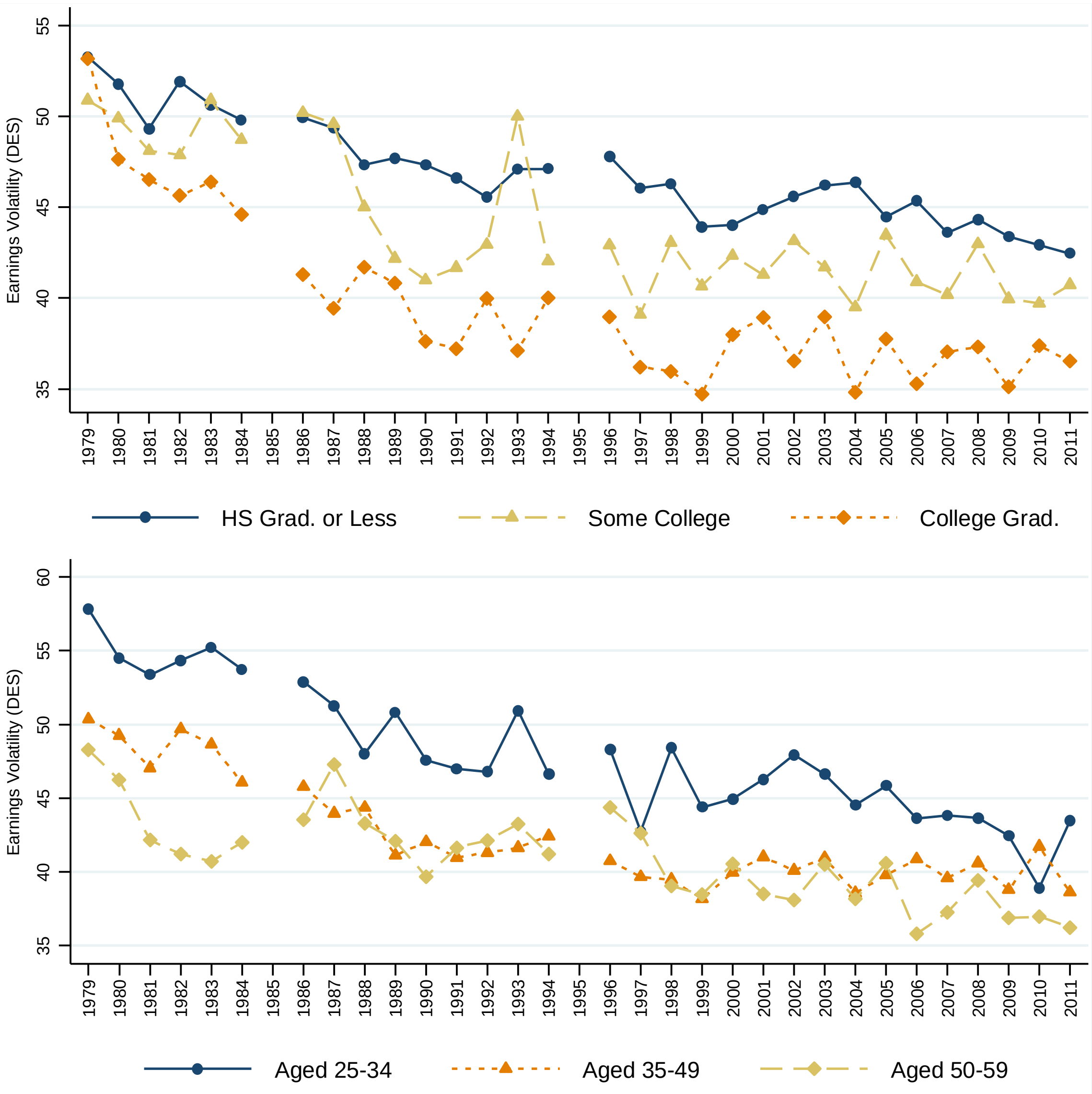


Figure 2.B6 Men's Earnings Volatility by Four Education Subgroups (DES for A)

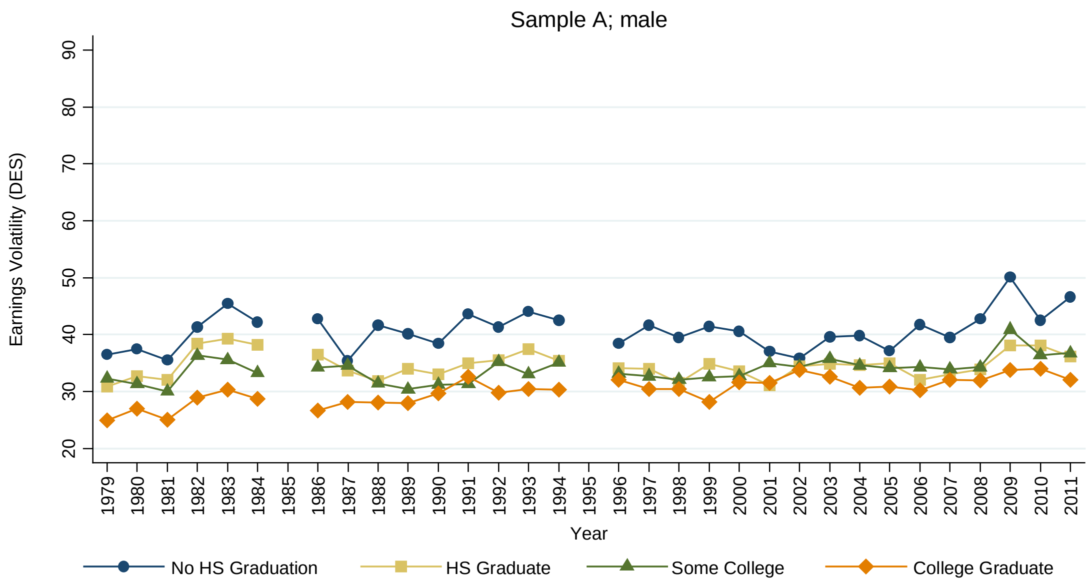


Figure 2.B7 Men's Earnings Volatility by Four Education Subgroups (DES for B)

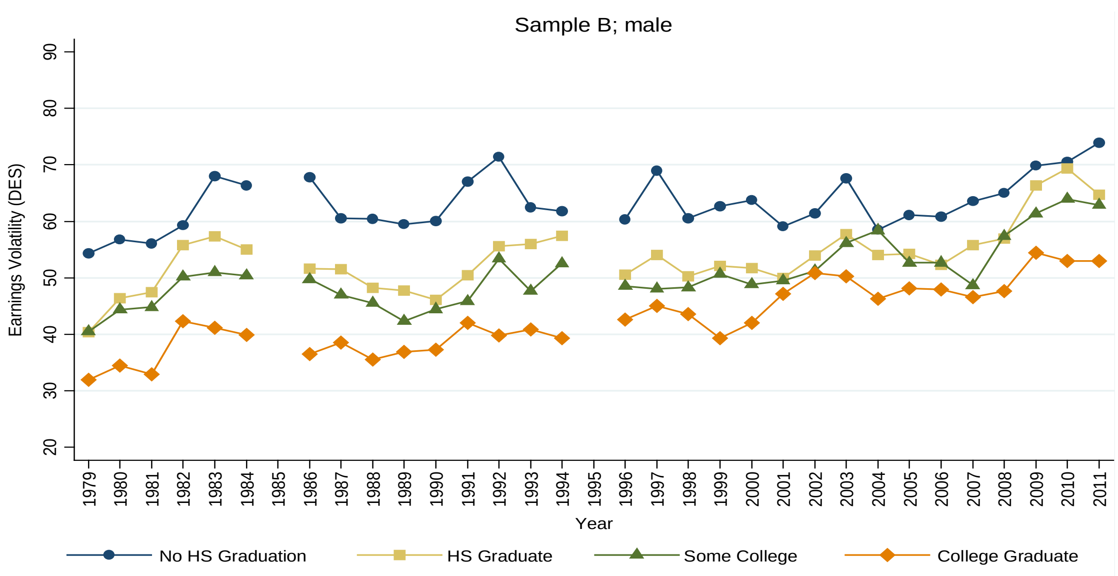


Figure 2.B8 Men's Earnings Volatility by Four Education Subgroups (SS for A & B)

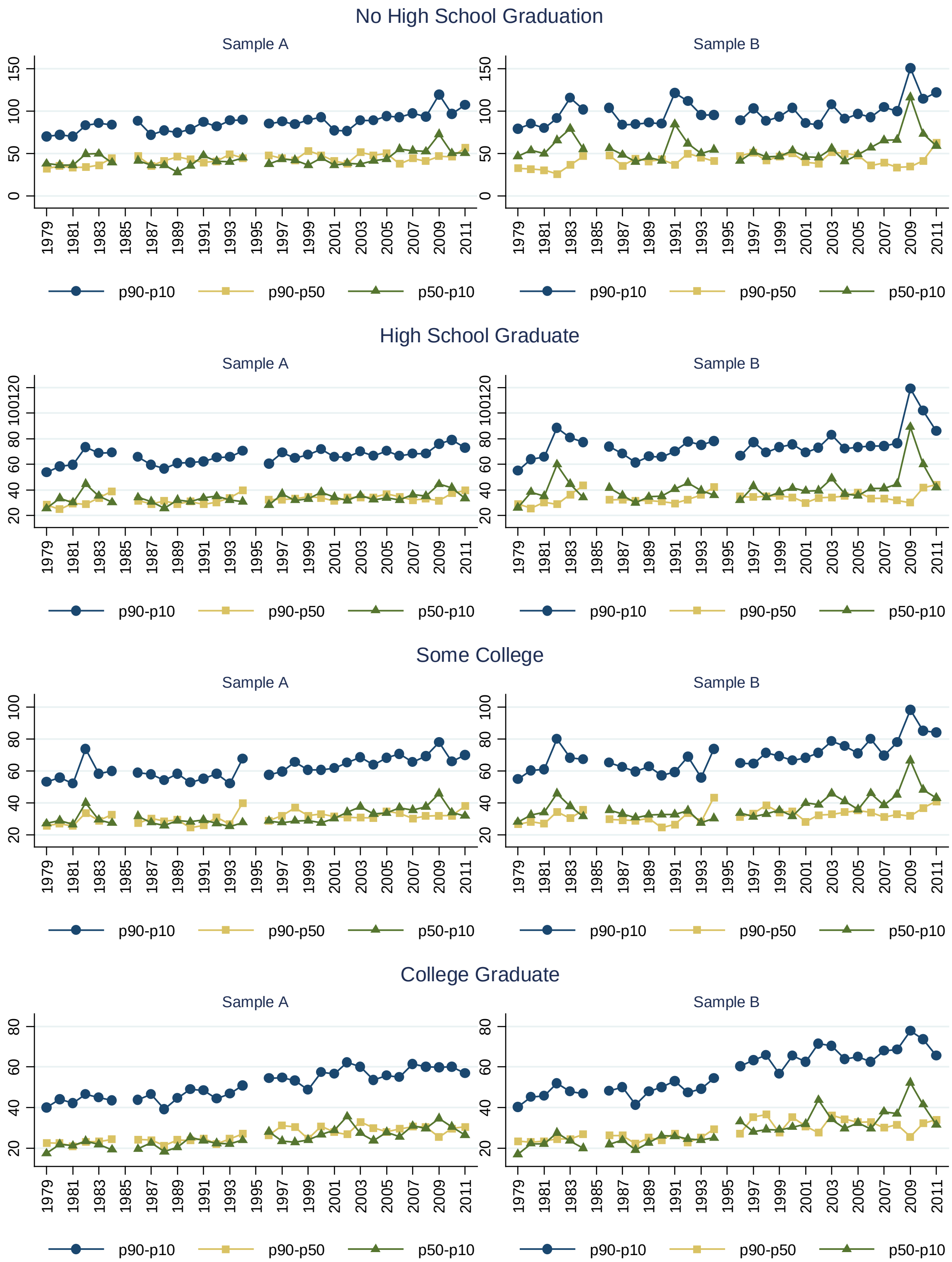


Figure 2.B9 Men’s Earnings Volatility by Seven Age Subgroups (DES for A)

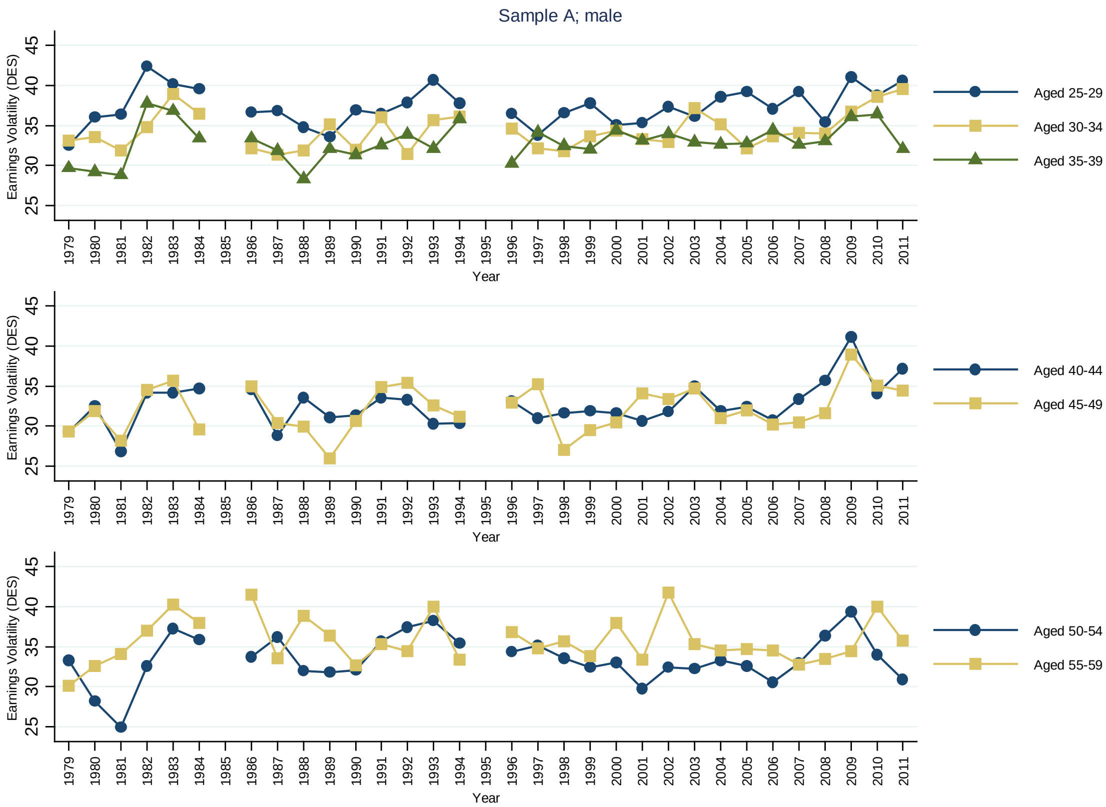


Figure 2.B10 Men's Earnings Volatility by Seven Age Subgroups (DES for B)

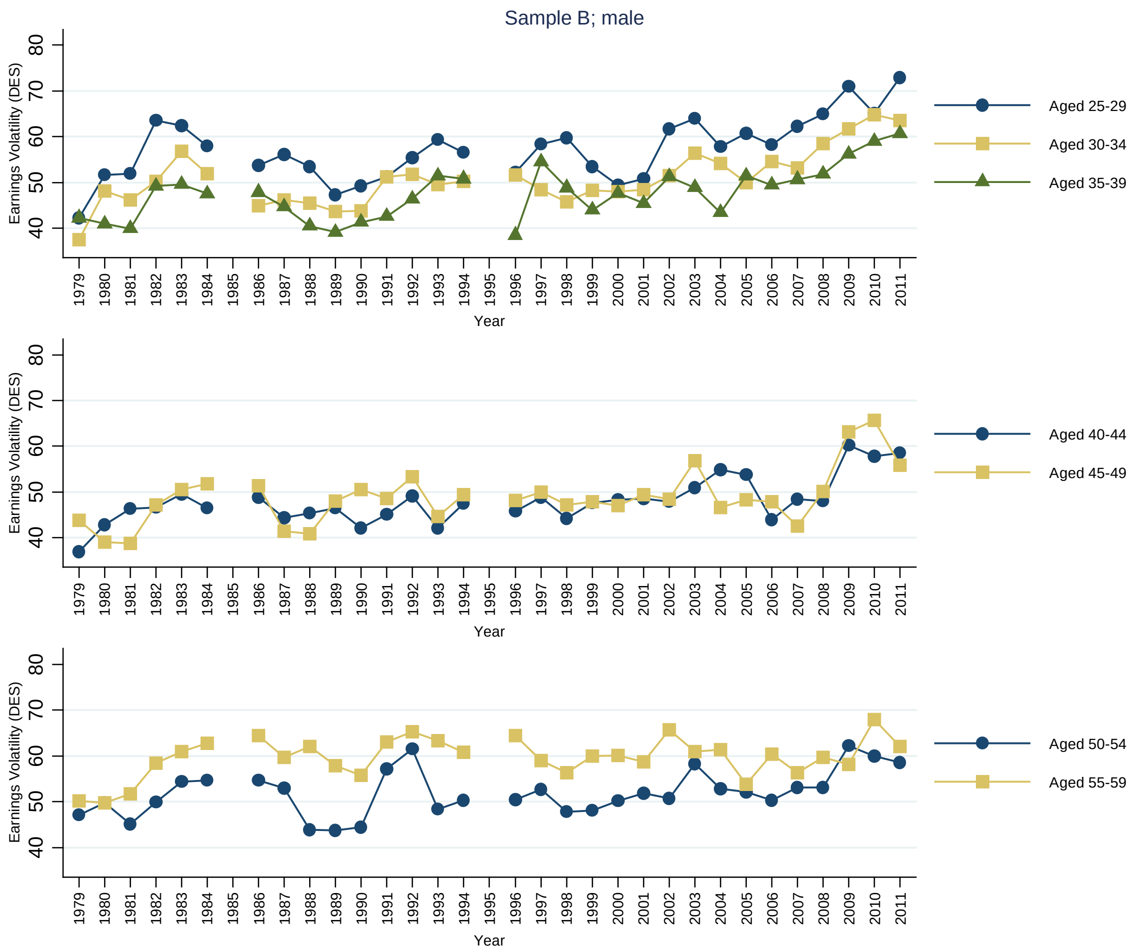


Figure 2.B11 Men's Earnings Volatility by Seven Age Subgroups (SS for A & B)

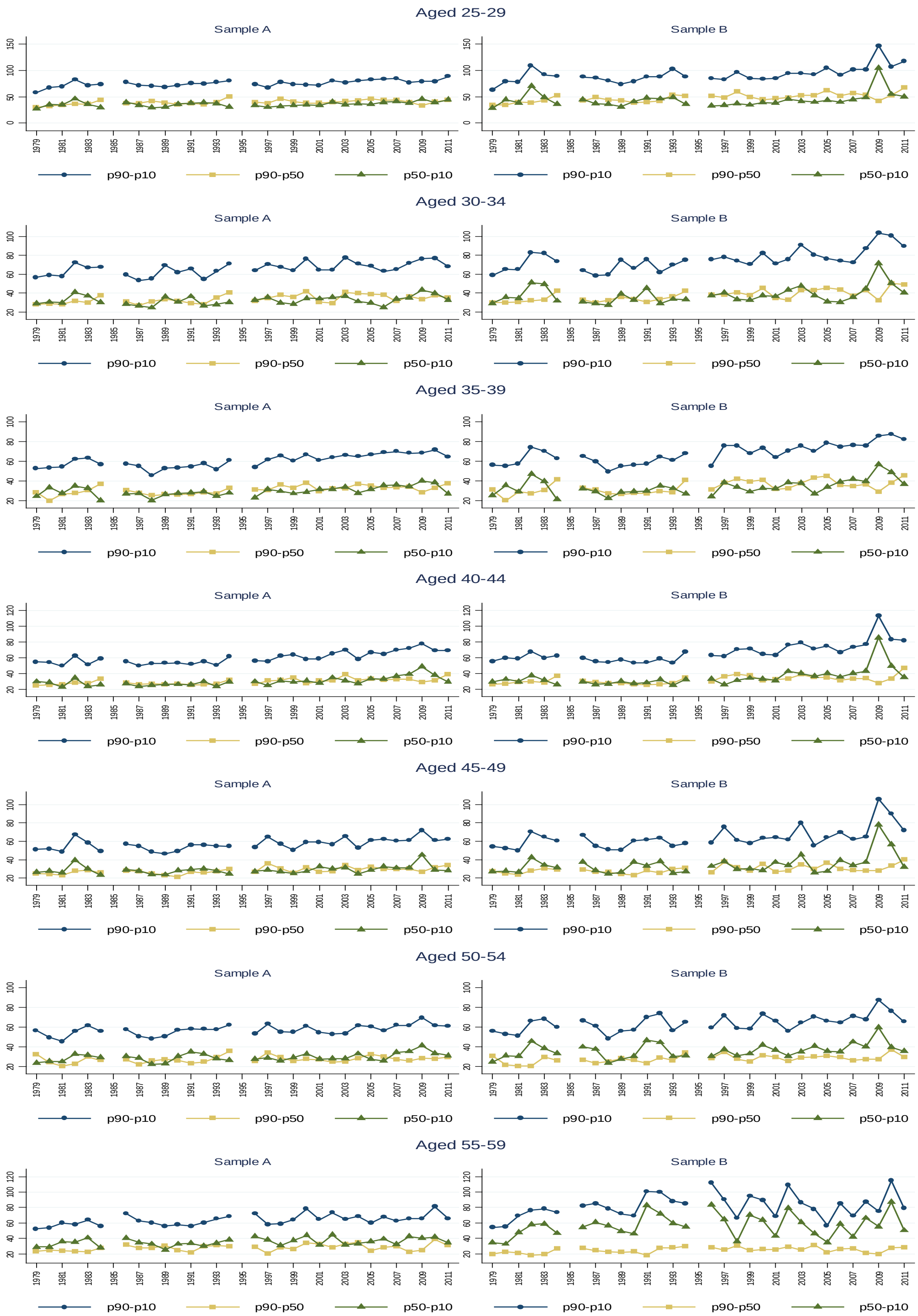


Figure 2.B12 Women's Earnings Volatility by Four Education Subgroups (DES for A)

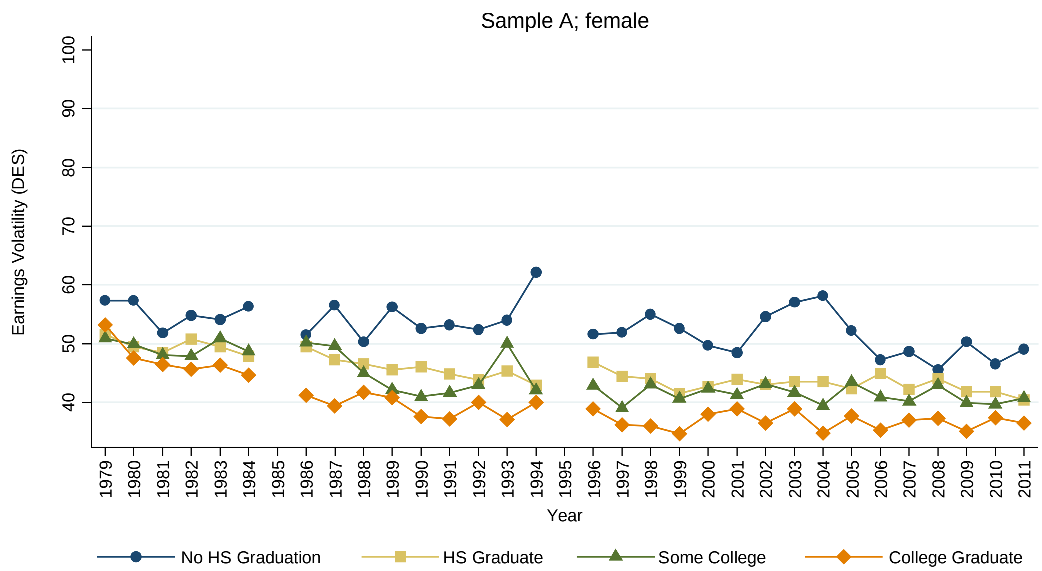


Figure 2.B13 Women's Earnings Volatility by Four Education Subgroups (DES for B)

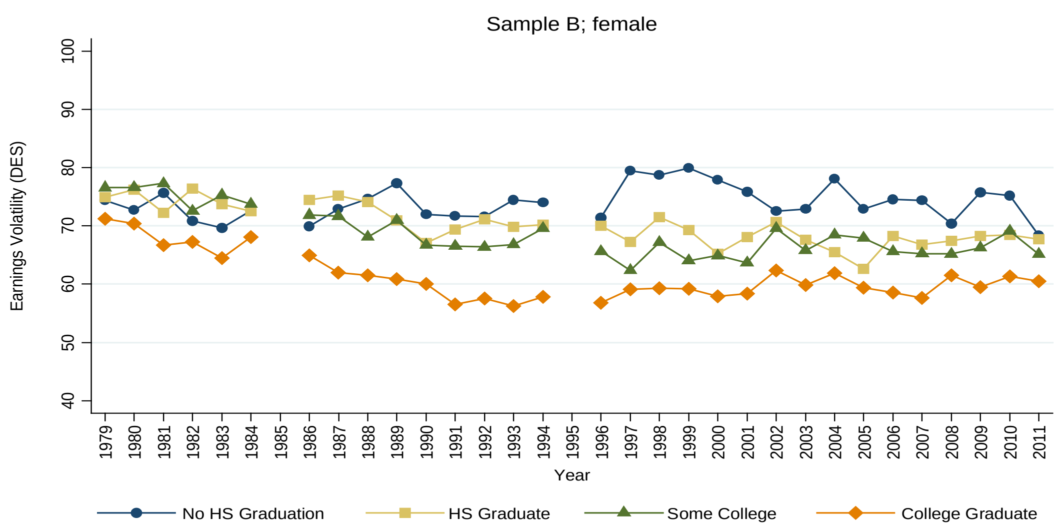


Figure 2.B14 Women's Earnings Volatility by Four Education Subgroups (SS for A & B)

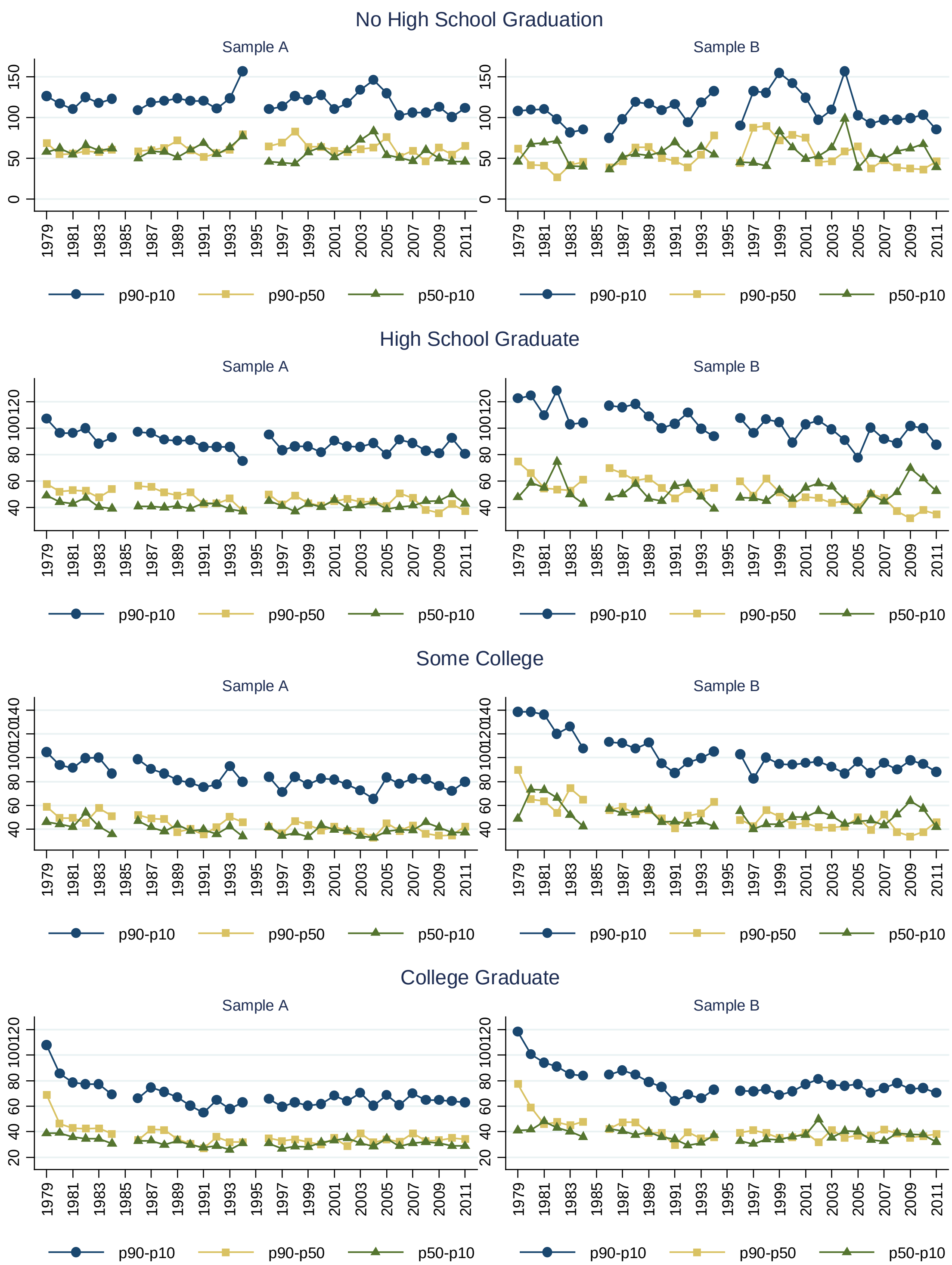


Figure 2.B15 Women's Earnings Volatility by Seven Age Subgroups (DES for A)

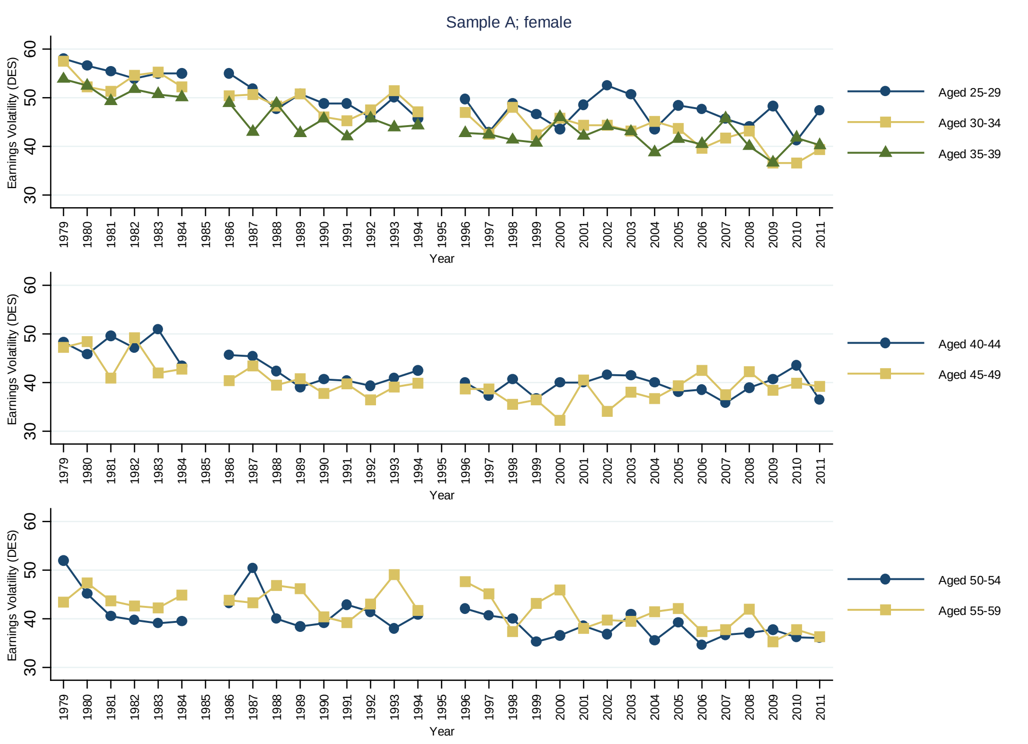


Figure 2.B16 Women's Earnings Volatility by Seven Age Subgroups (DES for B)

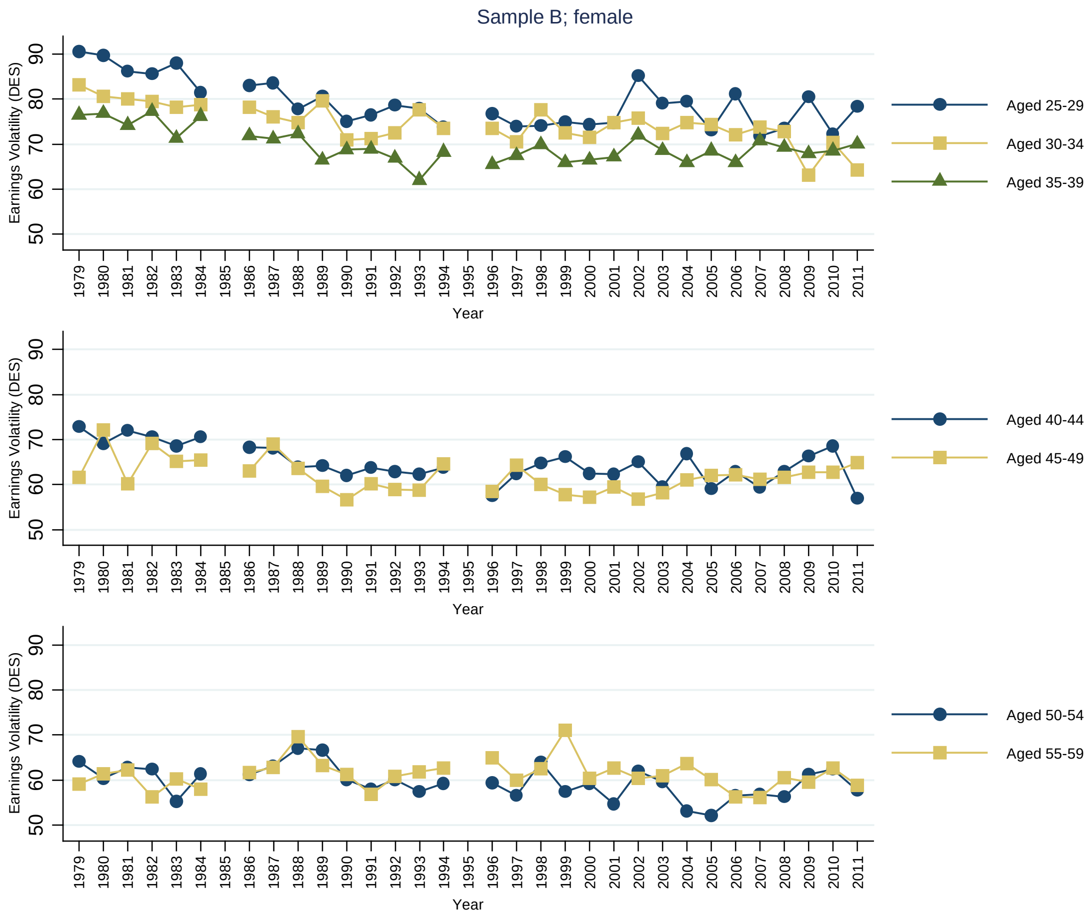
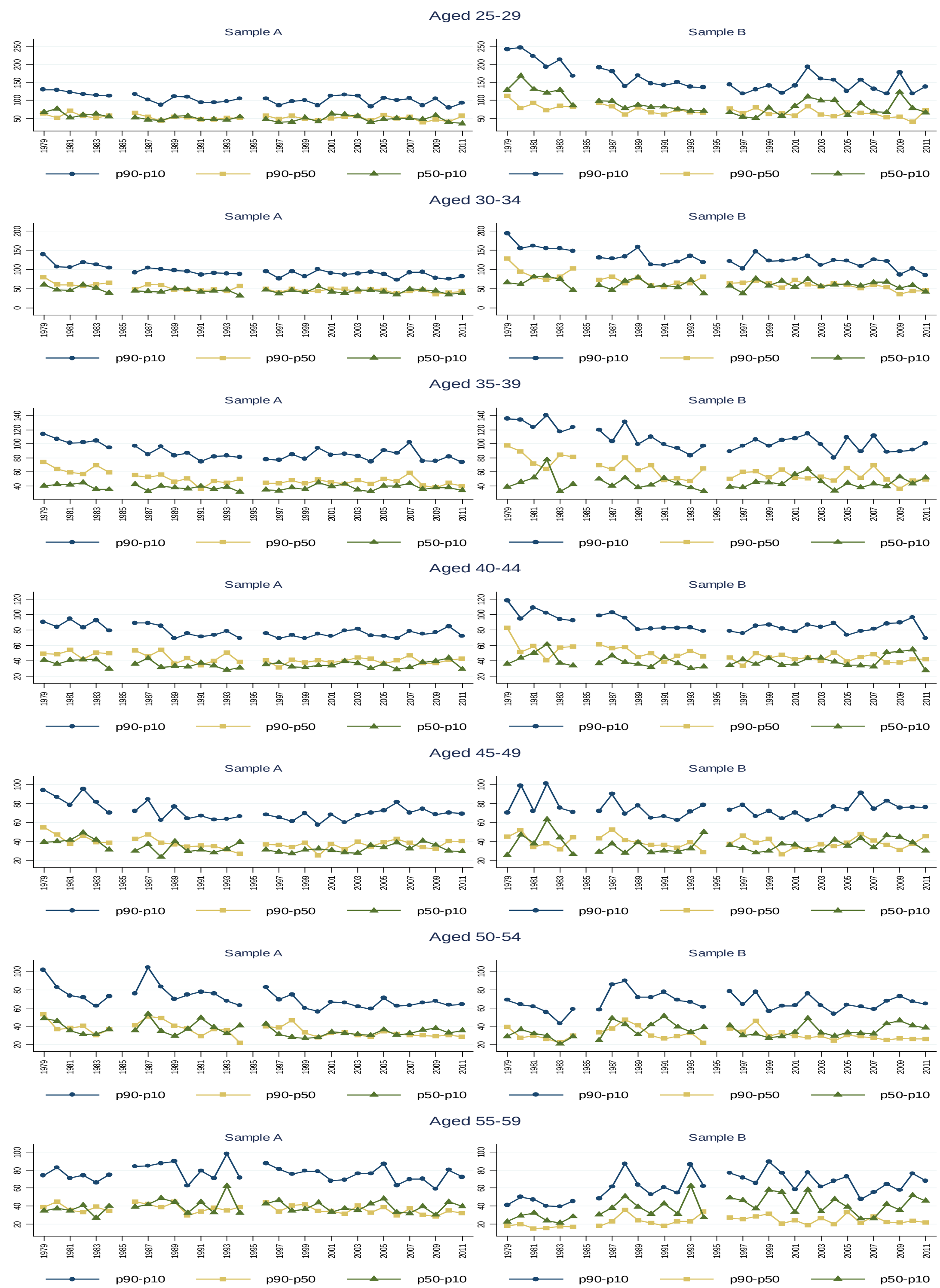


Figure 2.B17 Women's Earnings Volatility by Seven Age Subgroups (SS for A & B)



APPENDIX C

SUPPLEMENTAL TABLES FOR CHAPTER 2

Table 2.C1 Match Rate

	Male			Female		
Survey Year	Possible to be matched	Matched Number	Match Rate	Possible to be matched	Matched Number	Match Rate
1979	11,055	7,814	71%	14,048	10,255	73%
1980	13,203	9,648	73%	16,665	12,622	76%
1981	12,992	8,474	65%	16,314	10,983	67%
1982	12,180	8,732	72%	15,628	11,524	74%
1983	12,334	8,543	69%	15,527	11,203	72%
1984	12,155	8,286	68%	15,534	10,900	70%
1985	N/A	N/A	N/A	N/A	N/A	N/A
1986	12,389	8,273	67%	15,352	10,625	69%
1987	12,111	8,332	69%	15,186	10,713	71%
1988	12,345	7,872	64%	14,855	9,683	65%
1989	11,194	7,823	70%	13,863	9,910	71%
1990	12,475	8,496	68%	15,207	10,726	71%
1991	12,141	8,296	68%	14,605	10,340	71%
1992	12,453	8,515	68%	14,977	10,495	70%
1993	11,996	6,258	52%	14,862	7,868	53%
1994	10,561	5,520	52%	12,839	6,856	53%
1995	N/A	N/A	N/A	N/A	N/A	N/A
1996	9,816	6,907	70%	11,955	8,555	72%
1997	10,198	7,049	69%	12,271	8,714	71%
1998	10,289	7,182	70%	12,414	8,896	72%
1999	9,894	6,954	70%	11,877	8,460	71%
2000	9,216	7,106	77%	10,759	8,442	78%
2001	9,527	6,544	69%	11,426	8,202	72%
2002	15,473	7,963	51%	18,706	9,555	51%
2003	15,152	8,019	53%	18,629	9,934	53%
2004	14,833	7,064	48%	18,239	8,549	47%
2005	14,774	7,305	49%	18,192	9,122	50%
2006	15,591	7,823	50%	19,022	9,578	50%
2007	15,162	7,644	50%	18,471	9,417	51%
2008	15,135	7,820	52%	18,334	9,539	52%
2009	15,530	8,129	52%	18,737	9,645	51%
2010	15,349	7,766	51%	18,665	9,259	50%
2011	14,751	7,447	50%	17,710	8,878	50%

Note: “Possible to be matched” means the total number of men at risk of being matched and satisfying criteria for the main sample suggested in Section 2.2.2, i.e., whose annual earnings last year belonged to 1%-99% among male earners 25-59, not self-employed, not a student, with positive annual earnings and positive weeks worked.

Table 2.C2 Summary Statistics for Matched Men

Survey Year	Possible to be matched				Matched			
	Mean Real Annual Earnings	Mean Age	College Graduates %	Unemployed	Mean Real Annual Earnings	Mean Age	College Graduates %	Unemployed
1979	46,356	39.6	22%	11%	48,873	40.8	22%	10%
1980	46,981	39.5	22%	12%	49,453	40.5	23%	9%
1981	45,946	39.2	23%	14%	49,165	40.3	24%	12%
1982	44,686	39.3	23%	16%	47,269	40.4	24%	14%
1983	42,247	39.0	24%	20%	45,179	40.2	24%	18%
1984	41,984	39.0	24%	18%	45,249	40.2	25%	15%
1985	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
1986	43,620	39.0	24%	14%	46,387	40.1	25%	12%
1987	44,117	39.2	25%	14%	47,487	40.4	26%	12%
1988	44,174	39.0	25%	13%	47,031	40.1	26%	11%
1989	44,816	39.1	26%	11%	47,979	40.2	27%	9%
1990	44,296	39.2	25%	11%	47,812	40.4	27%	9%
1991	43,256	39.2	25%	13%	46,821	40.4	27%	11%
1992	41,504	39.3	28%	15%	44,798	40.4	29%	13%
1993	41,513	39.5	29%	14%	44,835	40.5	31%	12%
1994	41,829	39.7	30%	13%	45,640	40.7	33%	12%
1995	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
1996	44,172	40.0	29%	11%	47,640	41.1	30%	10%
1997	44,644	40.1	30%	10%	47,640	41.3	31%	9%
1998	46,685	40.3	30%	9%	50,636	41.5	32%	8%
1999	46,796	40.6	31%	8%	50,254	41.9	34%	7%
2000	48,368	41.0	32%	7%	50,796	42.0	33%	6%
2001	49,194	41.1	31%	7%	53,853	42.4	34%	6%
2002	49,939	40.9	32%	9%	53,548	42.4	35%	8%
2003	49,033	41.3	32%	10%	51,678	42.8	35%	9%
2004	48,677	41.4	33%	10%	51,840	42.7	36%	8%
2005	50,093	41.4	33%	8%	52,134	42.9	35%	8%
2006	51,446	41.7	33%	7%	55,104	43.1	35%	7%
2007	52,681	41.7	34%	7%	55,011	43.3	36%	7%
2008	51,955	41.7	35%	8%	55,017	43.1	37%	7%
2009	50,584	41.8	34%	11%	52,690	43.2	35%	10%
2010	47,106	41.8	35%	15%	49,734	43.2	37%	13%
2011	45,799	41.9	35%	14%	48,269	43.2	37%	13%

Note 1: "Possible to be matched" means the total number of men at risk of being matched and satisfying criteria for the main sample suggested in Section 2.2.2, i.e., whose annual earnings last year belonged to 1%-99% among male earners 25-59, not self-employed, not a student, with positive annual earnings and positive weeks worked.

Note 2: "Unemployed" refers to individuals who worked less than equal to 49 weeks, and were laid off or looked for jobs in the non-working period for each year.

Table 2.C3 Summary Statistics for Matched Women

	Possible to be matched				Matched			
Survey Year	Mean Real Annual Earnings	Mean Age	College Graduates %	Unemployed	Mean Real Annual Earnings	Mean Age	College Graduates %	Unemployed
1979	13,644	40.0	14%	9%	13,621	41.1	14%	8%
1980	15,200	39.9	15%	9%	15,078	40.8	16%	8%
1981	15,177	39.8	15%	10%	15,110	40.8	15%	9%
1982	14,919	39.7	16%	11%	15,092	40.7	16%	10%
1983	15,194	39.5	17%	12%	15,306	40.6	17%	11%
1984	15,884	39.6	18%	11%	16,346	40.7	18%	9%
1985	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
1986	17,332	39.2	18%	10%	17,705	40.2	19%	9%
1987	17,975	39.4	19%	10%	18,373	40.4	19%	9%
1988	19,243	39.3	20%	9%	19,601	40.3	20%	7%
1989	19,427	39.2	20%	9%	20,062	40.3	21%	7%
1990	20,143	39.4	21%	8%	20,837	40.4	22%	7%
1991	20,365	39.4	21%	9%	21,127	40.3	22%	9%
1992	20,550	39.4	24%	10%	21,472	40.4	25%	8%
1993	20,817	39.7	25%	9%	21,760	40.6	26%	8%
1994	21,508	40.0	27%	10%	22,548	40.9	29%	9%
1995	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
1996	22,079	40.0	28%	9%	23,064	41.0	30%	8%
1997	22,504	40.5	29%	8%	23,843	41.5	31%	7%
1998	22,983	40.5	29%	7%	24,222	41.5	31%	7%
1999	23,841	40.9	30%	7%	25,402	42.0	32%	6%
2000	25,353	41.3	32%	6%	25,956	42.2	33%	5%
2001	25,159	41.2	31%	6%	26,405	42.3	33%	5%
2002	25,349	40.8	33%	7%	27,484	42.4	34%	6%
2003	24,872	41.0	33%	7%	26,908	42.7	35%	7%
2004	25,512	41.1	35%	7%	28,370	42.7	38%	6%
2005	25,203	41.2	34%	7%	26,991	42.8	36%	6%
2006	26,241	41.6	35%	6%	28,559	43.3	38%	5%
2007	26,762	41.6	37%	6%	28,752	43.3	38%	5%
2008	27,343	41.7	38%	6%	29,352	43.2	40%	5%
2009	27,311	41.7	38%	8%	29,126	43.4	40%	7%
2010	26,342	41.4	39%	10%	28,469	42.9	42%	9%
2011	26,096	41.5	40%	10%	28,242	43.0	42%	9%

Note 1: “Possible to be matched” means the total number of women at risk of being matched and satisfying criteria for the main sample suggested in Section 2.2.2, i.e., whose annual earnings last year belonged to 1%~99% among female earners 25-59, not self-employed, not a student, with positive annual earnings and positive weeks worked.

Note 2: “Unemployed” refers to individuals who worked less than equal to 49 weeks, and were laid off or looked for jobs in the non-working period for each year.

Table 2.C4 Earnings Volatility for Men and Women

Year	Men			Women		
	DG for Sample A	DES for Sample A	DES for Sample B	DG for Sample A	DES for Sample A	DES for Sample B
1978-1979	0.36 (.009)	31.3 (.52)	42.9 (.79)	0.68 (.013)	52.9 (.53)	74.6 (.55)
1979-1980	0.37 (.007)	32.4 (.46)	46.7 (.70)	0.65 (.011)	50.8 (.49)	74.7 (.50)
1980-1981	0.35 (.008)	31.0 (.47)	46.3 (.76)	0.61 (.011)	48.6 (.51)	73.1 (.55)
1981-1982	0.43 (.008)	36.7 (.48)	53.0 (.69)	0.65 (.013)	50.0 (.53)	73.3 (.54)
1982-1983	0.46 (.010)	37.9 (.55)	55.6 (.73)	0.66 (.014)	49.9 (.55)	71.7 (.56)
1983-1984	0.42 (.009)	35.7 (.54)	53.4 (.77)	0.63 (.013)	48.4 (.57)	72.0 (.58)
1984-1985	N/A	N/A	N/A	N/A	N/A	N/A
1985-1986	0.41 (.009)	34.9 (.52)	51.8 (.77)	0.61 (.012)	48.2 (.54)	71.5 (.58)
1986-1987	0.38 (.010)	32.8 (.51)	49.4 (.77)	0.60 (.012)	47.4 (.51)	71.8 (.57)
1987-1988	0.38 (.009)	32.5 (.54)	47.4 (.81)	0.56 (.011)	45.6 (.52)	70.8 (.61)
1988-1989	0.38 (.009)	32.6 (.54)	46.2 (.80)	0.56 (.011)	44.9 (.53)	70.0 (.61)
1989-1990	0.37 (.008)	32.5 (.48)	46.2 (.76)	0.53 (.010)	43.7 (.50)	66.3 (.62)
1990-1991	0.41 (.009)	34.9 (.52)	50.4 (.74)	0.53 (.011)	43.3 (.54)	66.6 (.63)
1991-1992	0.40 (.009)	34.6 (.52)	53.7 (.76)	0.54 (.011)	43.4 (.54)	67.0 (.63)
1992-1993	0.41 (.011)	35.1 (.64)	50.9 (.91)	0.57 (.013)	45.0 (.64)	66.5 (.73)
1993-1994	0.39 (.010)	34.5 (.59)	51.7 (.96)	0.54 (.015)	43.6 (.69)	67.4 (.80)
1994-1995	N/A	N/A	N/A	N/A	N/A	N/A
1995-1996	0.39 (.009)	33.8 (.53)	49.4 (.84)	0.54 (.012)	43.8 (.57)	65.6 (.70)
1996-1997	0.38 (.008)	33.6 (.50)	52.7 (.83)	0.50 (.012)	41.2 (.58)	65.6 (.71)
1997-1998	0.36 (.008)	32.4 (.48)	49.5 (.84)	0.52 (.013)	42.1 (.57)	68.0 (.69)
1998-1999	0.37 (.008)	32.9 (.50)	49.4 (.85)	0.47 (.010)	40.0 (.55)	66.5 (.72)
1999-2000	0.37 (.008)	33.5 (.47)	49.8 (.85)	0.50 (.012)	41.5 (.58)	64.6 (.74)
2000-2001	0.37 (.010)	32.8 (.51)	50.2 (.90)	0.51 (.013)	41.8 (.59)	65.1 (.75)
2001-2002	0.39 (.008)	34.4 (.49)	53.4 (.80)	0.51 (.011)	41.7 (.58)	68.0 (.67)
2002-2003	0.39 (.008)	34.8 (.47)	56.3 (.79)	0.52 (.012)	42.4 (.57)	65.2 (.68)
2003-2004	0.38 (.009)	33.7 (.50)	53.1 (.86)	0.48 (.011)	40.1 (.59)	66.3 (.74)
2004-2005	0.38 (.010)	33.5 (.49)	52.7 (.85)	0.50 (.011)	41.5 (.54)	64.1 (.72)
2005-2006	0.36 (.007)	32.9 (.43)	52.0 (.82)	0.48 (.010)	40.2 (.54)	64.9 (.71)
2006-2007	0.37 (.007)	33.6 (.45)	52.3 (.82)	0.47 (.010)	40.1 (.50)	64.1 (.72)
2007-2008	0.38 (.008)	34.3 (.45)	55.0 (.79)	0.50 (.011)	41.1 (.57)	65.0 (.70)
2008-2009	0.45 (.009)	38.5 (.51)	61.9 (.71)	0.46 (.010)	39.2 (.53)	65.5 (.70)
2009-2010	0.42 (.009)	36.6 (.52)	63.0 (.79)	0.47 (.011)	39.7 (.57)	66.6 (.72)
2010-2011	0.42 (.011)	35.9 (.56)	61.6 (.83)	0.46 (.011)	39.3 (.57)	64.4 (.75)

Note: The numbers in parentheses indicate asymptotic standard errors.

Table 2.C5 Men’s Earnings Volatility by Education and Age (DES for Sample B)

	Men's Education Subgroups			Men's Age Subgroups		
Year	HS Grad. or Less	Some College	College Grad.	Age 25-34	Age 35-49	Age 50-59
1978-1979	46.83 (1.01)	40.49 (1.81)	31.97 (1.65)	39.96 (1.17)	41.21 (1.30)	48.67 (1.65)
1979-1980	50.83 (0.87)	44.36 (1.71)	34.53 (1.50)	49.87 (1.10)	41.12 (1.12)	49.85 (1.48)
1980-1981	50.96 (0.95)	44.81 (1.88)	32.92 (1.60)	49.15 (1.17)	41.83 (1.28)	48.57 (1.55)
1981-1982	57.16 (0.85)	50.21 (1.66)	42.39 (1.59)	57.09 (1.09)	47.85 (1.11)	54.39 (1.48)
1982-1983	61.47 (0.88)	50.95 (1.83)	41.13 (1.59)	59.73 (1.15)	49.83 (1.17)	57.92 (1.50)
1983-1984	59.20 (0.96)	50.40 (1.86)	39.88 (1.59)	54.94 (1.19)	48.41 (1.22)	59.14 (1.63)
1984-1985	N/A	N/A	N/A	N/A	N/A	N/A
1985-1986	57.53 (0.97)	49.77 (1.80)	36.49 (1.56)	49.21 (1.24)	49.21 (1.19)	60.01 (1.61)
1986-1987	54.51 (1.02)	46.98 (1.75)	38.57 (1.51)	51.27 (1.29)	43.77 (1.19)	56.48 (1.59)
1987-1988	52.31 (1.04)	45.55 (1.87)	35.51 (1.60)	49.26 (1.34)	42.33 (1.20)	53.86 (1.76)
1988-1989	51.46 (1.06)	42.34 (1.78)	36.95 (1.56)	45.38 (1.25)	44.25 (1.23)	51.39 (1.78)
1989-1990	50.60 (1.03)	44.45 (1.73)	37.31 (1.43)	46.35 (1.28)	44.18 (1.13)	50.23 (1.75)
1990-1991	55.64 (0.98)	45.90 (1.71)	42.02 (1.42)	51.21 (1.29)	45.10 (1.09)	60.10 (1.55)
1991-1992	60.52 (1.04)	53.39 (1.56)	39.81 (1.43)	53.42 (1.33)	49.30 (1.11)	63.52 (1.59)
1992-1993	57.81 (1.24)	47.70 (1.95)	40.90 (1.70)	53.88 (1.52)	46.70 (1.33)	55.93 (2.04)
1993-1994	58.76 (1.38)	52.57 (2.00)	39.29 (1.62)	52.94 (1.67)	49.37 (1.39)	55.39 (2.17)
1994-1995	N/A	N/A	N/A	N/A	N/A	N/A
1995-1996	53.70 (1.25)	48.58 (1.77)	42.64 (1.42)	51.94 (1.53)	44.11 (1.17)	57.23 (1.87)
1996-1997	58.98 (1.18)	48.06 (1.80)	45.10 (1.45)	53.08 (1.61)	51.27 (1.17)	55.53 (1.74)
1997-1998	53.55 (1.28)	48.31 (1.75)	43.57 (1.38)	52.29 (1.58)	46.82 (1.19)	51.70 (1.81)
1998-1999	55.22 (1.25)	50.70 (1.81)	39.31 (1.41)	50.54 (1.59)	46.52 (1.24)	53.78 (1.69)
1999-2000	55.30 (1.30)	48.85 (1.78)	42.07 (1.35)	48.63 (1.58)	47.72 (1.22)	54.93 (1.71)
2000-2001	52.61 (1.38)	49.51 (1.80)	47.22 (1.52)	49.55 (1.77)	47.89 (1.27)	54.87 (1.76)
2001-2002	56.31 (1.24)	51.24 (1.65)	50.88 (1.34)	56.14 (1.57)	49.24 (1.13)	57.97 (1.57)
2002-2003	60.76 (1.18)	56.16 (1.58)	50.28 (1.35)	59.88 (1.60)	52.46 (1.13)	59.58 (1.48)
2003-2004	55.39 (1.35)	58.40 (1.76)	46.27 (1.41)	56.03 (1.63)	48.87 (1.27)	57.05 (1.64)
2004-2005	56.22 (1.28)	52.67 (1.79)	48.16 (1.43)	55.12 (1.62)	51.23 (1.22)	52.88 (1.65)
2005-2006	54.64 (1.25)	52.68 (1.62)	47.97 (1.41)	56.32 (1.66)	47.09 (1.19)	55.43 (1.49)
2006-2007	58.23 (1.27)	48.69 (1.75)	46.56 (1.32)	57.64 (1.62)	47.29 (1.21)	54.71 (1.51)
2007-2008	59.31 (1.24)	57.42 (1.67)	47.70 (1.27)	61.54 (1.60)	50.02 (1.16)	56.33 (1.44)
2008-2009	67.22 (0.98)	61.40 (1.47)	54.45 (1.31)	66.40 (1.34)	60.17 (1.02)	60.39 (1.37)
2009-2010	69.79 (1.13)	63.93 (1.67)	52.98 (1.36)	65.02 (1.57)	61.21 (1.19)	63.78 (1.39)
2010-2011	67.62 (1.21)	62.90 (1.73)	52.95 (1.44)	68.25 (1.52)	58.41 (1.30)	60.24 (1.53)

Note: The numbers in parentheses indicate asymptotic standard errors.

Table 2.C6 Women's Earnings Volatility by Education and Age (DES for Sample B)

	Women's Education Subgroups			Women's Age Subgroups		
Year	HS Grad. or Less	Some College	College Grad.	Age 25-34	Age 35-49	Age 50-59
1978-1979	74.82 (0.67)	76.57 (1.32)	71.27 (1.44)	87.02 (0.52)	71.25 (0.96)	61.66 (1.35)
1979-1980	75.10 (0.60)	76.64 (1.18)	70.44 (1.32)	85.21 (0.55)	73.07 (0.83)	60.89 (1.26)
1980-1981	73.40 (0.66)	77.30 (1.20)	66.69 (1.53)	83.18 (0.65)	69.69 (0.95)	62.64 (1.30)
1981-1982	74.71 (0.64)	72.59 (1.31)	67.27 (1.46)	82.57 (0.66)	72.91 (0.85)	59.31 (1.36)
1982-1983	72.43 (0.70)	75.28 (1.20)	64.44 (1.43)	83.22 (0.66)	68.78 (0.91)	57.92 (1.43)
1983-1984	72.59 (0.72)	73.75 (1.35)	68.11 (1.40)	80.16 (0.76)	71.54 (0.93)	59.65 (1.40)
1984-1985	N/A	N/A	N/A	N/A	N/A	N/A
1985-1986	73.20 (0.71)	71.86 (1.27)	64.94 (1.45)	80.73 (0.74)	68.45 (0.93)	61.47 (1.46)
1986-1987	74.55 (0.69)	71.67 (1.25)	61.99 (1.46)	79.70 (0.78)	69.72 (0.91)	62.95 (1.36)
1987-1988	74.26 (0.74)	68.08 (1.44)	61.51 (1.50)	76.23 (0.90)	67.26 (0.96)	68.39 (1.45)
1988-1989	72.75 (0.76)	70.96 (1.38)	60.92 (1.44)	80.12 (0.79)	63.92 (0.99)	65.01 (1.51)
1989-1990	68.38 (0.79)	66.72 (1.35)	60.01 (1.45)	73.00 (0.93)	63.49 (0.95)	60.73 (1.50)
1990-1991	70.02 (0.78)	66.54 (1.38)	56.57 (1.49)	73.79 (0.95)	64.92 (0.95)	57.45 (1.53)
1991-1992	71.30 (0.82)	66.40 (1.35)	57.52 (1.37)	75.39 (0.93)	63.49 (0.97)	60.40 (1.54)
1992-1993	71.11 (0.98)	66.82 (1.44)	56.28 (1.57)	77.82 (1.05)	61.29 (1.11)	59.69 (1.77)
1993-1994	71.28 (1.10)	69.61 (1.59)	57.83 (1.59)	73.84 (1.28)	65.76 (1.17)	61.04 (1.95)
1994-1995	N/A	N/A	N/A	N/A	N/A	N/A
1995-1996	70.43 (0.96)	65.64 (1.44)	56.82 (1.38)	75.06 (1.08)	61.01 (1.05)	62.20 (1.61)
1996-1997	70.80 (0.97)	62.36 (1.53)	59.14 (1.37)	72.05 (1.23)	64.88 (1.02)	58.12 (1.58)
1997-1998	73.54 (0.95)	67.19 (1.38)	59.34 (1.38)	76.24 (1.10)	65.34 (1.03)	63.32 (1.55)
1998-1999	72.26 (1.01)	64.02 (1.45)	59.20 (1.39)	73.66 (1.19)	63.68 (1.07)	63.87 (1.56)
1999-2000	68.99 (1.08)	64.92 (1.50)	57.93 (1.36)	73.05 (1.28)	62.29 (1.09)	59.82 (1.55)
2000-2001	70.40 (1.08)	63.67 (1.54)	58.40 (1.35)	75.01 (1.24)	63.21 (1.08)	58.44 (1.63)
2001-2002	71.20 (1.01)	69.57 (1.30)	62.36 (1.20)	79.87 (1.03)	64.83 (1.00)	61.20 (1.41)
2002-2003	69.15 (1.02)	65.77 (1.39)	59.83 (1.20)	75.44 (1.15)	62.18 (1.01)	60.32 (1.39)
2003-2004	68.98 (1.15)	68.47 (1.51)	61.90 (1.26)	76.88 (1.24)	64.60 (1.09)	58.20 (1.53)
2004-2005	65.84 (1.17)	67.95 (1.35)	59.41 (1.22)	73.80 (1.29)	63.41 (1.05)	56.12 (1.43)
2005-2006	70.01 (1.07)	65.60 (1.44)	58.58 (1.21)	76.52 (1.21)	63.69 (1.03)	56.37 (1.42)
2006-2007	68.99 (1.13)	65.23 (1.43)	57.68 (1.21)	73.03 (1.28)	63.74 (1.06)	56.53 (1.41)
2007-2008	68.22 (1.12)	65.18 (1.44)	61.55 (1.14)	73.12 (1.28)	64.68 (1.04)	58.40 (1.34)
2008-2009	70.43 (1.06)	66.26 (1.42)	59.48 (1.18)	71.68 (1.28)	65.53 (1.04)	60.45 (1.32)
2009-2010	70.51 (1.13)	69.15 (1.51)	61.37 (1.16)	71.30 (1.33)	66.63 (1.06)	62.49 (1.37)
2010-2011	67.97 (1.23)	65.16 (1.55)	60.52 (1.20)	71.39 (1.33)	64.10 (1.17)	58.29 (1.41)

Note: The numbers in parentheses indicate asymptotic standard errors.

APPENDIX D

ADDITIONS FOR CHAPTER 2

Matching March CPS

In the Current Population Survey (CPS), an interviewee is interviewed for 4 consecutive months, once he or she enters the sample, and then has an 8-month interim without interviews. After the interim, he or she is interviewed again for 4 consecutive months, then leaves the sample. By exploiting this rotating property of CPS, two-year short panel data sets are constructed. The household identification number, house number, and line number in CPS are used mainly for matching individuals across the two adjacent years. This "naïve" matching method usually generates a number of incorrect matches, and Madrian & Lefgren (2000) suggest several methods to reduce false matches by using additional variables. I follow their recommendations and drop matched individuals if there is an inconsistency in gender, race, or age.

Theoretically, about 50% of the individuals in March Supplement of CPS for each year should be matched with data from the following year. However, as previously explained, the survey does not track interviewees who moved elsewhere, so the real matching rate tends to be lower than the full matching rate.

Table 2.C1 lists the match rate for each survey year. That rate is calculated by dividing the number of matched individuals by the total number of individuals who theoretically can be matched among those satisfying the sample selection criteria of this study.[33] From 1979 to 2001, for both male and female interviewees, the match rate ranges from 64% to 78%, usually staying around 70%, except in 1993 and 1994.[34] Since 2002, the overall match rate falls again and remains around 50% for both men and women, varying from 47% to 53% through 2011.[35]

[33] More specifically, it satisfies the criteria for the sample B. See Section 2.2.2.

[34] The match rate went down to 52-53% in 1993 and 1994 because the March CPS was redesigned substantially since 1994 and the sampling method for 1995 March CPS was significantly different from those used in other years.

[35] The decrease in the match rate since 2002 was also due to the redesign of the sampling for March CPS at that time: a considerable number of additional samples were added to more precisely estimate statistics for minority groups, but these additional samples did not have panel property, so they were not able to be matched across years. See Appendix in Celik et al. (2012).

Accordingly, it is expected that the matched sample contains a higher portion of individuals who are relatively unlikely to relocate. This will cause the matched sample to be somewhat different from the general population. In Table 2.C2 and Table 2.C3 we can see that the individuals in the matched sample tend to have higher earnings, older ages, a larger probability of being college graduates, and fewer chances to experience unemployment than those in the entire sample, which includes all individuals who could potentially be matched. Consequently, it is hard to exclude the possibility that the estimated earnings volatility might be biased in the matched March CPS sample.[36]

As discussed in Section 2.2.1, however, this study tracks the dynamics of earnings volatility. It does not focus on estimating the exact level of earnings volatility each year. Thus, if there was no systemic bias in the matching process, the matched March CPS still can be considered a legitimate data set for exploring the evolution of earnings volatility. As for the sample periods prior to the Great Recession, 1979-2007, a number of researchers argued that no serious systemic bias was found in the matching process for the March CPS. (Cameron and Tracy 1998; Celik et al. 2012; Ziliak, Hardy, and Bollinger 2011)

One relevant concern which still needs to be considered, however, is whether any serious shock influenced the matching process since 2008, due to the Great Recession. For instance, if shock from the Great Recession significantly affected the proportion of movers among the March CPS interviewees, we cannot regard the estimated earnings volatility after the Great Recession as comparable with that during preceding periods.

To check this possibility, I drew Figure 2.B1, plotting the match rate in Table 2.C1. Figure 2.B1 shows that, for both men and women, the match rate stays rather stable even after 2008-

[36] Considering that there is a positive correlation between the unemployment rate and earnings volatility, as discussed in Section 2.3.3, earnings volatility for the matched sample from CPS seems likely underestimated.

2009. Thus, it does not seem that the Great Recession caused serious change in the percentage of movers among March CPS interviewees.

Decomposition of Var(Δlog(*hours*))

$$\begin{aligned}\mathrm{Var}(\Delta\log(hours_{it})) &= \mathrm{E}((\Delta\log(hours_{it}))^2) - \{\mathrm{E}(\Delta\log(hours_{it}))\}^2 \\ &= S_{\text{unemp,t}}\mathrm{E}((\Delta\log(hours_{it}))^2 | i \in \text{unemp}) \\ &\quad + S_{\text{nounemp,t}}\mathrm{E}((\Delta\log(hours_{it}))^2 | i \in \text{nounemp}) \\ &\quad - \{\mathrm{E}(\Delta\log(hours_{it}))\}^2\end{aligned}$$

where $hours_{it}$ indicates total annual working hours of the individual *i* at year *t*; $S_{\text{j,t}}$ stands for the share of group j out of sample A at year *t*; "unemp" means individuals who had worked for less than or equal to 49 weeks, and also had experienced unemployed status in their non-working periods in the year *t*-1 or *t*; "nounemp" refers to other individuals in sample A who do not belong to "unemp" group.

Note that $\mathrm{E}(\Delta\log(hours_{it}))$ is also practically zero because $\Delta\log(hours_{it})$ is basically the age-adjusted residual for each year *t*. Thus, the formula above is rendered to:

$$\begin{aligned}\mathrm{Var}(\Delta\log(hours_{it})) &\approx S_{\text{unemp,t}}\mathrm{E}((\Delta\log(hours_{it}))^2 | i \in \text{unemp}) \\ &\quad + S_{\text{nounemp,t}}\mathrm{E}((\Delta\log(hours_{it}))^2 | i \in \text{nounemp})\end{aligned}$$

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CHAPTER 3

How Did Men's Earnings Volatility Change during the Great Recession? A Comparison of Evidence between the PSID and CPS

3.1 Introduction

This paper explores the evolution of men's earnings volatility in the United States during the Great Recession by using the Panel Study of Income Dynamics (PSID). I further compare the findings with those of Koo (2016), who examines the same topic with the matched March Current Population Survey (CPS).1

The major motivations of this study are twofold. First, given the paucity of studies on men's earnings volatility during the Great Recession, which was the severest downturn in the post-war era (Elsby et al. 2011), I document the relevant facts that are robustly supported by the datasets PSID and the CPS, both of which have been extensively used in the literature to explore men's earnings volatility in the United States. Second, according to previous studies, the PSID generates a divergent trend in men's earnings volatility from the CPS, particularly for the period of the early 2000s (Shin and Solon 2011; Celik et al. 2012; Dynan et al. 2012). There have been no further explorations, however, of the factors driving the inconsistent results between the two

[1] Notwithstanding an extensive body of literature documenting changes in men's earnings volatility until the time prior to the Great Recession (Cameron and Tracy 1998; Celik et al. 2012; Office 2007; Dynan et al. 2012; Haider 2001; Moffitt and Gottschalk 2002; Moffitt and Gottschalk 2012; Shin and Solon 2011), there are only a limited number of studies on men's earnings volatility during the Great Recession. To my knowledge, Koo (2016) is the first study focusing on the evolution of earnings volatility during the Great Recession in the United States by using the matched March Current Population Survey (CPS).

datasets. This study not only looks at whether such inconsistency between the two datasets has persisted through the Great Recession, but also sheds light on the source of that inconsistency, by comparing the results from multiple samples across the two datasets, the PSID and the CPS.

The two main findings of this study are as follows. First, the PSID results are fairly comparable to the CPS for the evolution of men's earnings volatility during the Great Recession. Specifically, the PSID shows that there was a substantial increase in men's earnings volatility during and after the Great Recession. Furthermore, the PSID indicates that the substantial increase in men's earnings volatility during the Great Recession as well as other previous recessions was mainly driven by the increase in earnings volatility of unemployed workers. These findings are largely consistent with those of Koo (2016) from the matched March CPS.

Second, in the sample excluding zero-earners and the top and bottom 1 % of positive-earners, the PSID generates fairly comparable results with the CPS for the evolution of men's earnings volatility from 1980 to 2012. However, in the sample including those zero-earners and the outliers, the PSID generates much higher estimates for men's earnings volatility compared to what the CPS generates, particularly for the period 1993-2004. This partly explains why previous studies using the PSID report different trends from those of other studies using different datasets, such as the CPS, for the same period. Specifically the former studies document high and upward-trended men's earnings volatility between the late 1990s and mid 2000s, whereas the latter ones find relatively low and flat trends in men's earnings volatility for the same period (Shin and Solon 2011; Celik et al. 2012; Dynan et al. 2012).

In section 3.2, I explain the dataset, sample selection criteria, and measures for earnings volatility. Section 3.3 presents the main results, and the conclusion is provided in Section 3.4.

3.2 Data and Measurement

The PSID is one of the longest panel datasets which has collected various family income data and other extensive socioeconomic and demographic information on national representative samples in the U.S. Among a number of family income sources contained in the PSID, annual earnings from wage and salaries for each family head is the one which has been recorded the most consistently since 1970. Thus, I mainly use this variable to calculate men's earnings volatility over the last four decades.

One point to be considered is that the PSID has started to conduct its surveys biennially since 1997, while it had collected information annually from 1968 to 1996. Because of this inconsistency in survey frequency, I use a two-year difference in each individual annual earnings instead of a one-year difference in order to consistently track changes in earnings volatility, as Shin and Solon (2011) do. Specifically, I measure the earnings volatility for the period 1969-1971, by using the information on annual earnings contained in the 1970 and 1972 waves of the PSID. Similarly, the earnings volatility for the period 1970-1972 is measured by using the 1971 and 1973 waves of the PSID. I keep doing this by using every currently available wave of the PSID. Consequently, I use all usable two-year differences in individual annual earnings as key variables for measuring earnings volatility from 26 overlapping periods , (1969-1971, 1970-1972, 1971-1973, … ,1993-1995, and 1994-1996), and 8 non-overlapping periods, (1996-1998, 1998-2000, … , and 2010-2012) from the 1970-2013 waves of the PSID.I employ the same sample selection criteria as Koo's (2016) to maximize the comparability of this study to his. Specifically, the basic sample (hereafter, Sample A) consists of only male heads whose age ranged from 25 to 59 with positive annual earnings from wage and salaries for each year, and whose main job was neither that of a student nor self-employed. To reduce potential measurement errors, Sample A does not include ones with imputed earnings, and it further excludes the ones whose annual earnings belonged to the top and bottom 1 % of positive earnings in each year.

One primary purpose of this paper is to update changes in men's earnings volatility in the Great Recession from the PSID. Thus, excluding all zero- and low-earners from the sample would be too restrictive to fully capture the impact of the Great Recession on men's earnings variability, as Koo (2016) points out. Hence, as Koo (2016) does, I also include in analyses another sample encompassing zero-earners and the top and bottom 1 % of positive earners for each year in addition to Sample A (hereafter, Sample B).

Accordingly, I have a total of 48,800 and 56,000 observations for Sample A and B, respectively, over 40 years. In terms of average sample size per year on a two-year difference, I have 1,440 and 1,560 observations for Sample A and B, respectively. More detailed sample size and summary statistics for Sample A and B by year are presented in Table 3.B1.

I use three different approaches to robustly measure changes in earnings volatility over the last four decades, including the Great Recession. Those approaches are also employed by Koo (2016) so that I may directly compare my results with his.

The first measure I use is the standard deviation of the two-year difference in log real earnings residuals, whose usage was initiated by Dynarski and Gruber (1997) (henceforth denoted as DG). The specific formula of DG is:

$$\sqrt{\mathrm{Var}\left(\log(Y_{it}) \widetilde{-\log}(Y_{it-2})\right)}$$

, where Y_{it} stands for real earnings for individual i at year t; $\left(\log(Y_{it}) \widetilde{-\log}(Y_{it-2})\right)$ indicates the residual from a regression of $(\log(Y_{it}) - \log(Y_{it-2}))$ on quadratics of age for individual i at year t. Hence, this residual refers to two-year difference in log real earnings which is unexplained by cohort effects, life-cycle effects, and aggregate time effects.

Shin and Solon (2011) show that this measure DG captures well the variances of transitory and permanent earnings shocks while being little affected by the growth of the return to an individual's time-invariant characteristics. Moreover, it does not depend on any complicated earnings model with a number of assumptions. Thus, it is a relatively simple and transparent measure for earnings volatility.

One weakness of DG, however, is that it cannot be applicable to Sample B, including zero-earners, because it is calculated based on log real earnings. Hence, I employ another measure suggested by Dynan, Elmendorf, and Sichel (2012) (hereafter DES), which is the standard deviation of the age-adjusted arc percentage change in real earnings. The specific formula of DES is:

$$\sqrt{\mathrm{Var}\left\{100 * \frac{(Y_{it} \widetilde{- Y}_{it-2})}{\bar{Y}_i}\right\}}$$

,where Y_{it} indicates real earnings (not log real earnings) for individual i in year t; $\bar{Y}_i$ refers to the average of Y_{it} and Y_{it-2}; $\frac{(Y_{it}\widetilde{-Y_{it-2}})}{\bar{Y}_i}$ means the age-adjusted arc percentage change in real earnings, which is residual from the regression of $\frac{(Y_{it}-Y_{it-2})}{\bar{Y}_i}$ on quadratics of age for year $t-2$. Thus, $\frac{(Y_{it}\widetilde{-Y_{it-2}})}{\bar{Y}_i}$ refers to arc percentage change in real earnings which is unexplained by cohort effects, life cycle effects, and aggregate time effects for individual i in year t.

It is notable that DES has upper and lower bounds (200 and -200, respectively) by its construction, which would alleviate potential problems from outliers showing extreme changes in earnings. Compared to DG, in addition, DES has the merit of enabling easier and more intuitive interpretation, as well as being able to encompass zero-earners in analyses.

I also employ another measure applicable to both Sample A and B, which is motivated by Shin and Solon (2011) (hereafter SS): this measure constructs quantile distributions for the tenth, twenty-fifth, fiftieth, seventy-fifth, and ninetieth sample percentiles of the age-adjusted arc percentage change in real earnings, $\frac{(\widetilde{Y_{it}-Y_{it-2}})}{\bar{Y}_i}$, for each sample year. Compared to the previous measures, DG and DES, that generate one summary statistics for the dispersion of changes in earnings for each year, SS has the advantage of letting us know the whole distributional changes in earnings differences over the sample years. Moreover, it also produces alternative measures summarizing the dispersion of change in earnings for each year, such as the difference between the ninetieth and tenth percentiles, so that we may compare its evolution with the trends in other measures such as DG and DES.

3.3 Results

Figure 3.A1 shows how, in Sample A, men's earnings volatility evolved from 1971 to 2012 based on the PSID. For comparison, I put together Koo's (2016) corresponding results from the matched March CPS. The first noteworthy feature in Figure 3.A1 is that both measures, DG and DES, generate similar dynamic patterns of men's earnings volatility, even with the difference in their scales, for both the PSID and CPS. The second feature is that both the PSID and CPS datasets generate a similar evolution of men's earnings volatility during the period 1980-2012 in terms of its magnitudes as well as dynamic patterns, although the former is calculated based on a two-year difference in (log) real earnings while the latter is based on a one-year difference in them.

Specifically, men's earnings volatility for Sample A calculated from the PSID shows a dramatic increase during 1970s and hits its peak during the recession in the early 1980s. Then, in general, it stabilized at lower levels afterwards until the time prior to the Great Recession, although it showed temporary, relatively mild upsurges during two other recessions, in the early 1990s and 2000. After the Great Recession began in 2008, men's earnings volatility showed another historic upswing, and remained at relatively high levels until the early 2010s. This pattern confirms the well-documented counter-cyclical property of earnings volatility, which is consistent with Koo's (2016) findings from the CPS during the same period since 1980, as seen in the same figure.

The similar results for Sample A from both the PSID and CPS are a novel finding, because Celik et al. (2012) show that, for a comparable sample, the matched March CPS generates a different trend in men's earnings volatility during the early 2000s compared to what Shin and Solon (2011) find with the PSID. Specifically, while Shin and Solon (2011) find a resuming upward trend during the early 2000s, Celik et al. (2012) find no such evidence from the matched March CPS as well as other datasets such as SIPP (Survey of Income and Program Participation) and LEHD (Longitudinal Employer-Household Dynamics).

One main reason why I find more comparable results between the PSID and CPS than Celik et al. (2012) do is that my findings with the PSID somewhat differ from that of Shin and Solon (2011) with the same data; I find a more moderate increase in men's earnings volatility during the early 2000s' which also ended earlier than what Shin and Solon (2011) find over the same period. This difference is partly because I use a different sample selection from theirs; unlike their main sample, Sample A here excludes students and self-employed people, although Sample A is still largely comparable to Shin and Solon's sample. Moreover, I use sample weights when estimating earnings volatility in order to closely follow Koo's (2016) study for comparison, whereas Shin and Solon (2011) do not.

Another important reason for the different comparison results between this study and Celik et al. (2012) is that I use a different rule of time matching from that of Celik et al. (2012), to compare the results across the PSID and CPS. For example, Celik et al. (2012) compare the earnings volatility for 1978-1979 from the CPS with that for 1976-1978 from the PSID, that for 1979-1980 from the CPS with that for 1977-1979 from the PSID, and so on for the subsequent periods. Unlike them, I compare the earnings volatility for 1978-1979 from the CPS with that for 1977-1979 from the PSID, that for 1979-1980 from the CPS with that for 1978-1980 from the PSID, and so on for the subsequent periods.[2] The former way of time matching used by Celik et al. (2012) turns out to make the discrepancy of the results between the PSID and CPS look more salient compared to that of this study.

Figure 3.A2, in turn, shows men's earnings volatility for Sample B from PSID (circle-shaped points) and compares it with the previous result for Sample A (square-shaped points) and the corresponding results of Koo (2016) from the CPS (triangle- and diamond- shaped points). Sample B, as explained earlier, encompasses zero-earners and the 1 % top and bottom positive-earners in addition to Sample A. In general, Figure 3.A2 shows that the PSID also generates a comparable trend in men's earnings volatility for Sample B compared to the counterpart from the CPS. To be more specific, I find much higher earnings volatility with a more salient upward trend for Sample B compared to that for Sample A from the PSID over all of the sample years. Moreover, the PSID shows a substantial upsurge of men's earnings volatility during the Great Recession for Sample B, as it does for Sample A. All these features in men's earnings volatility

[2] See Figure 1 in Celik et al. (2012).

for Sample B are comparably found in the results from the CPS reported by Koo (2016) put in the same figure.

In terms of how close the estimates are to each other, in comparing the PSID and CPS for each sample, however, larger disparities are observed in Sample B than Sample A. For instance, the earnings volatility for Sample B calculated from the PSID shows overall a higher level during the period prior to 2005 compared to that calculated from the CPS, whereas we see fairly comparable results for Sample A from both datasets over the same period. In particular, men's earnings volatility for Sample B in the PSID appears to be much higher from 1993 to 2004, than that from the CPS. Accordingly, for Sample B, the PSID shows a higher level of men's earnings volatility during the mild recession in the early 2000s even compared to that during the Great Recession. This finding differs from Koo's (2016) based on the CPS; he finds an increase in men's earnings volatility for Sample B during the early 2000's, but a much moderate one compared to that during the Great Recession, which is also consistent with the results from Sample A that he and I find using the CPS and the PSID, respectively.

I employ another measure SS in Figure 3.A3 to see if all the findings in Figure 3.A2 are consistently found even with an alternative measure.[3] Specifically, I identify the tenth, twenty-fifth, fiftieth , seventy-fifth, and ninetieth percentiles of arc-percent change in men's real earnings for each year for both Sample A and B from the PSID, and then look at how those percentiles change over the last four decades for both samples. For the sake of brevity, Figure 3.A3 shows only some summary results—the differences in ninetieth and tenth (p90-p10), fiftieth and tenth (p50-p10), and ninetieth and fiftieth (p90-p50) percentiles for both samples—rather than

[3] As discussed earlier, SS tracks the whole distributional changes in earnings differences, rather than generates one statistics summarizing the dispersion of change in earnings for each sample year as DG and DES do.

displaying the full trajectories of percentile changes.[4] I also put together Koo's (2016) corresponding results from the CPS for comparison in Figure 3.A3, as I do in the previous figures.

Based on the evolution of p90-p10 in the top panels of Figure 3.A3, the PSID shows that there was an increase in men's earnings volatility during the Great Recession in Sample A, and the increase stands out more clearly in Sample B. In addition, the evolution of p90-p10 shows counter-cyclical movements along the sample period for both Sample A and B. All these observations are comparable to the findings from the PSID based on DES in Figure 3.A2.

In the lower panels of Figure 3.A3, the PSID further shows that the counter-cyclical property of men's earnings volatility was mainly driven by the counter-cyclicality of p50-p10 for both samples. In other words, men tended to go through severe earnings losses during recessions, and that was the main reason that made men's earnings paths more unstable in the Great Recession as well as in other recessions compared to other non-recession periods.

All the time trends in p90-p10, p50-p10, and p90-p50 from the PSID in Figure 3.A3 described above are largely consistent with what Koo (2016) correspondingly finds from the CPS for both Sample A and B (see the graphs with circle-shaped points in the same figure). However, it is also apparent that the discrepancy of the results between the PSID and CPS becomes more prominent for Sample B than Sample A, as seen in Figure 3.A2. For Sample B, for example, the PSID reports a larger upsurge in p50-p10 during the period 1993-2004 than the CPS does, while for Sample A, both datasets show fairly comparable results during the same period (compare panel e and f in Figure 3.A3). Accordingly, for Sample B, the PSID shows a more substantial increase in p90-p10 during the recession in the early 2000s than the CPS does.

[4] The figure for the whole change of the percentiles from 1971 to 2012 is available on request.

The fact that the PSID and the CPS shows more divergent trends in men's earnings volatility in Sample B than in Sample A is notable in two respects. First, this result is consistent with the findings of previous studies using the PSID to explore men's earnings volatility until the time prior to the Great Recession: The inclusion of zero-earners and outliers to the analysis in the PSID makes the increasing trend in men's earnings volatility more salient during the early 2000s (Shin and Solon 2011; Dynan et al. 2012). Second, this observation implies that the zero-earners and outliers added to Sample A from the PSID would have different characteristics related to higher earning volatility than the counterpart from the CPS, particularly for the 1994-2004 period.

The conjecture above is well corroborated by the results in Figure 3.A4, where I decompose men's earnings volatility for each year into that of the unemployed workers and that of not-unemployed ones.[5] Figure 3.A4 first shows that for both Sample A and B from the PSID, the counter-cyclical behavior of men's earnings volatility from the early 1980s through the Great Recession largely stemmed from the considerable increases in earnings volatility among unemployed male workers rather than not-unemployed ones. This finding is fairly consistent with what Koo (2016) finds with the CPS, as seen in the same figure. The most salient exception observed in Figure 3.A4, however, is that the substantial upswing in men's earnings volatility for Sample B during the recession in the early 2000s from the PSID was largely driven by that of non-unemployed workers. More specifically, during the period 1994-2004, the trend in "not unemployed" men's earnings volatility for Sample B from the PSID becomes much higher than the counterpart from the CPS (see panel f in Figure 3.A4). As a result, the PSID shows a historic increase in men's earnings volatility for Sample B during the early 2000s, which is the most different result of the PSID compared to those from the CPS.

[5] Unemployed workers are defined here as ones having experienced unemployment in either of two years, while not-unemployed ones indicate workers having experienced no unemployment in both years.

It is not clear why including zero-earners and outliers in addition to Sample A with the PSID leads to higher men's earnings volatility during the 1993-2004 period compared to that from the CPS. One possibility is that the zero-earners and outliers during the period 1993-2004 in the PSID were likely to contain a relatively large numbers of errors in terms of their earnings compared to those in the CPS; another possibility is that this result relates to the fact that the PSID began a severe reform in its data collection process in the mid 1990s. Although I do not pursue directly where such difference came from for that period between the PSID and CPS in this paper, it seems requisite, at least, to interpret with caution the evolution of men's earnings volatility for Sample B when using the PSID, particularly for the period 1993-2004.

3.4 Conclusion

I explore the evolution of men's earnings volatility over the last four decades including the period of the Great Recession by using the PSID. I show men's earnings volatility went up substantially after the Great Recession began, which confirms the well-documented counter-cyclicality of earnings volatility. I further find the counter-cyclicality of earnings volatility was mainly experienced by the unemployed workers. All these findings turn out to be largely comparable with what Koo (2016) finds from the CPS, although there are some discrepancies observed in the analyses for Sample B including zero-earners and outliers for the period 1993-2004. The discrepancies between the PSID and CPS imply that the zero-earners and outliers added to Sample B for the period 1993-2004 in the PSID are likely to contain a relatively large number of errors in their reported earnings. This issue needs to be more rigorously explored in future research, given that the PSID has been one of most extensively used panel datasets in the literature for numerous social/economic topics as well as the topic of earnings volatility.

APPENDICES

APPENDIX A

FIGURES FOR CHAPTER 3

Figure 3.A1 Men’s Earnings Volatility for Sample A (DG and DES)

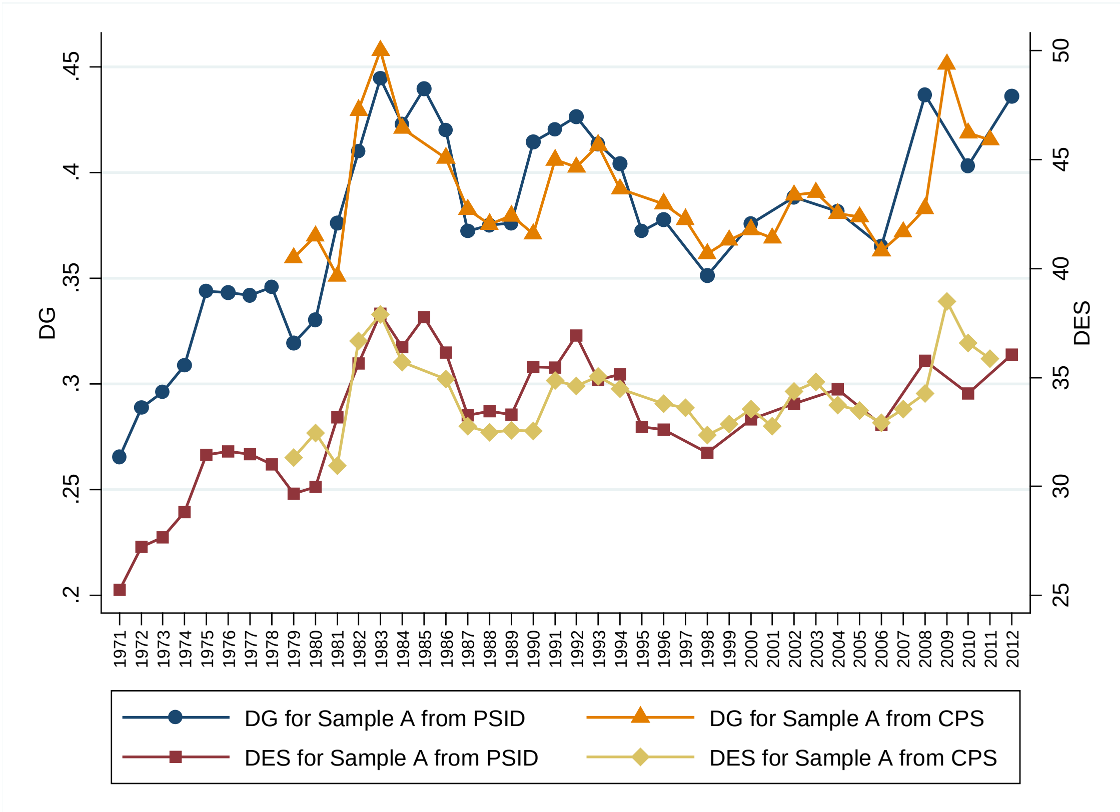


Figure 3.A2 Earnings Volatility for Sample A and B (DES)

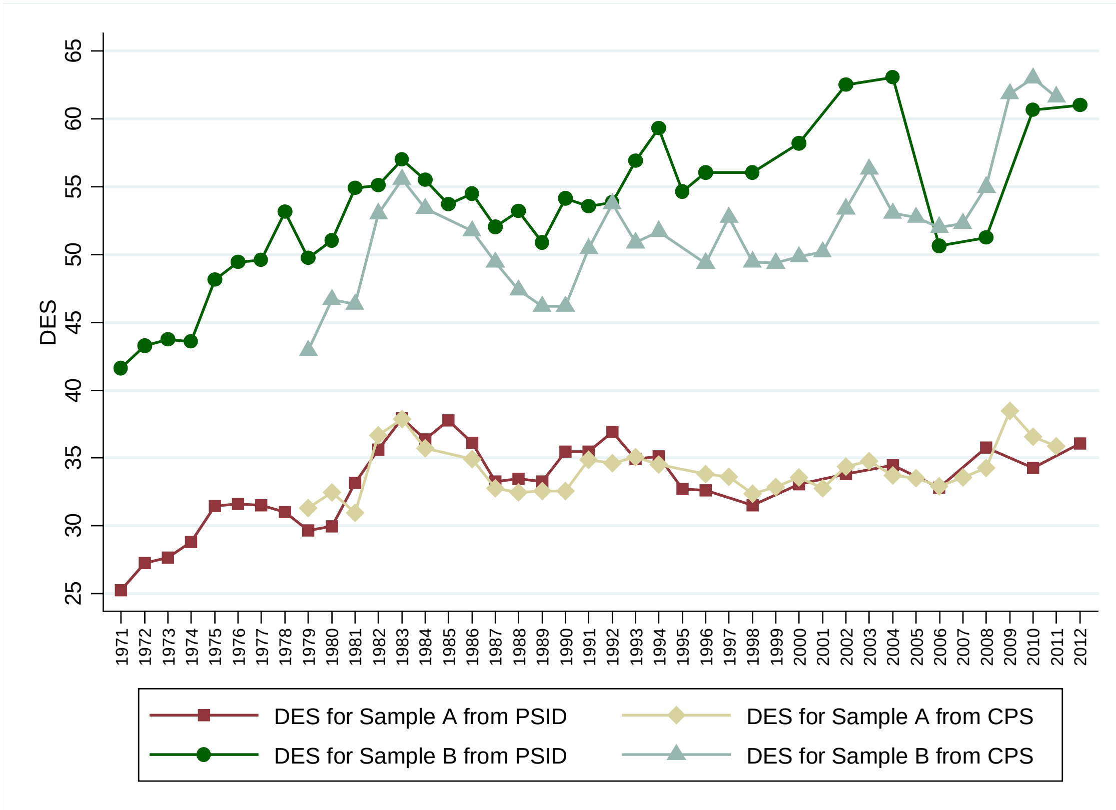


Figure 3.A3 Earnings Volatility for Sample A and B (SS)

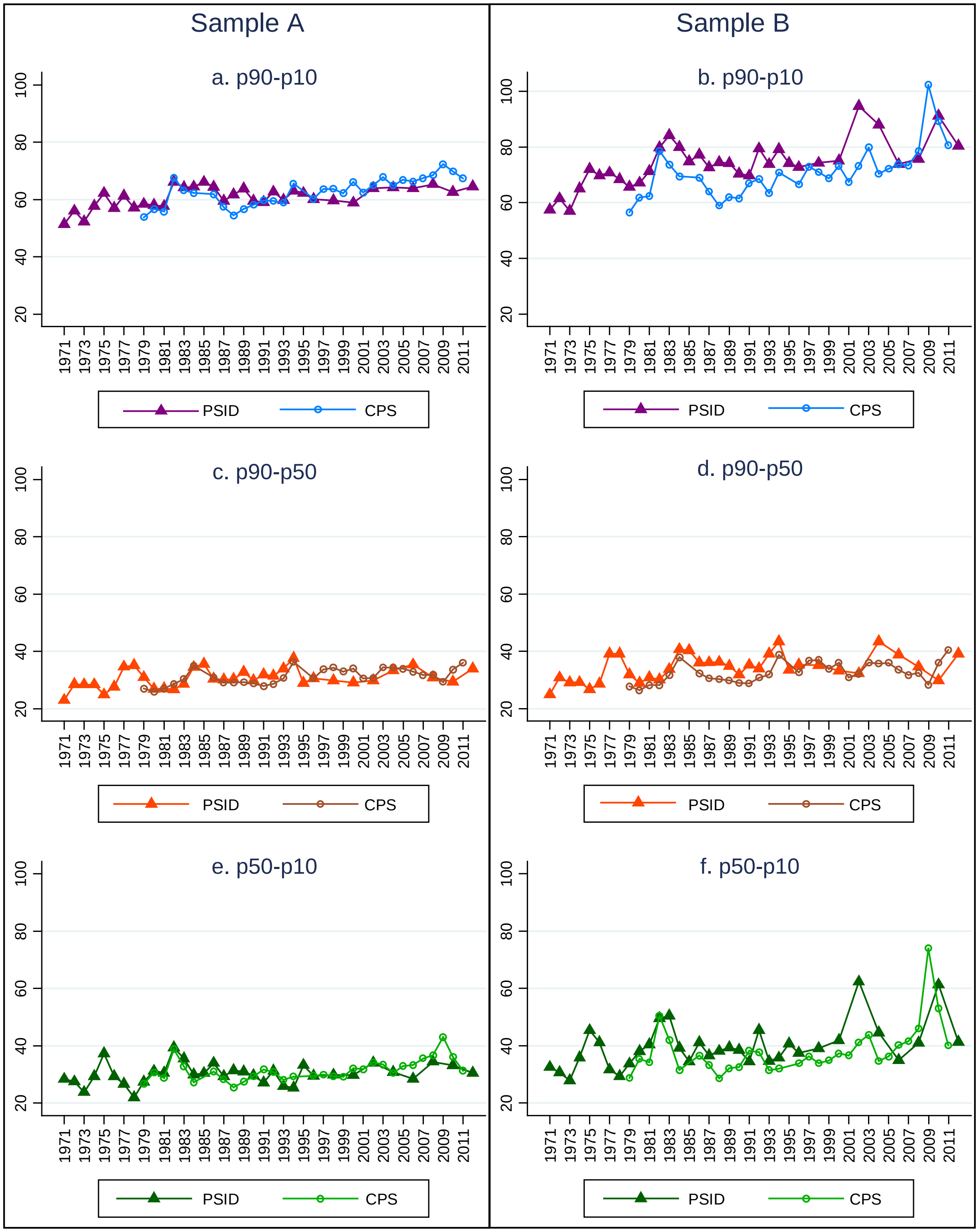
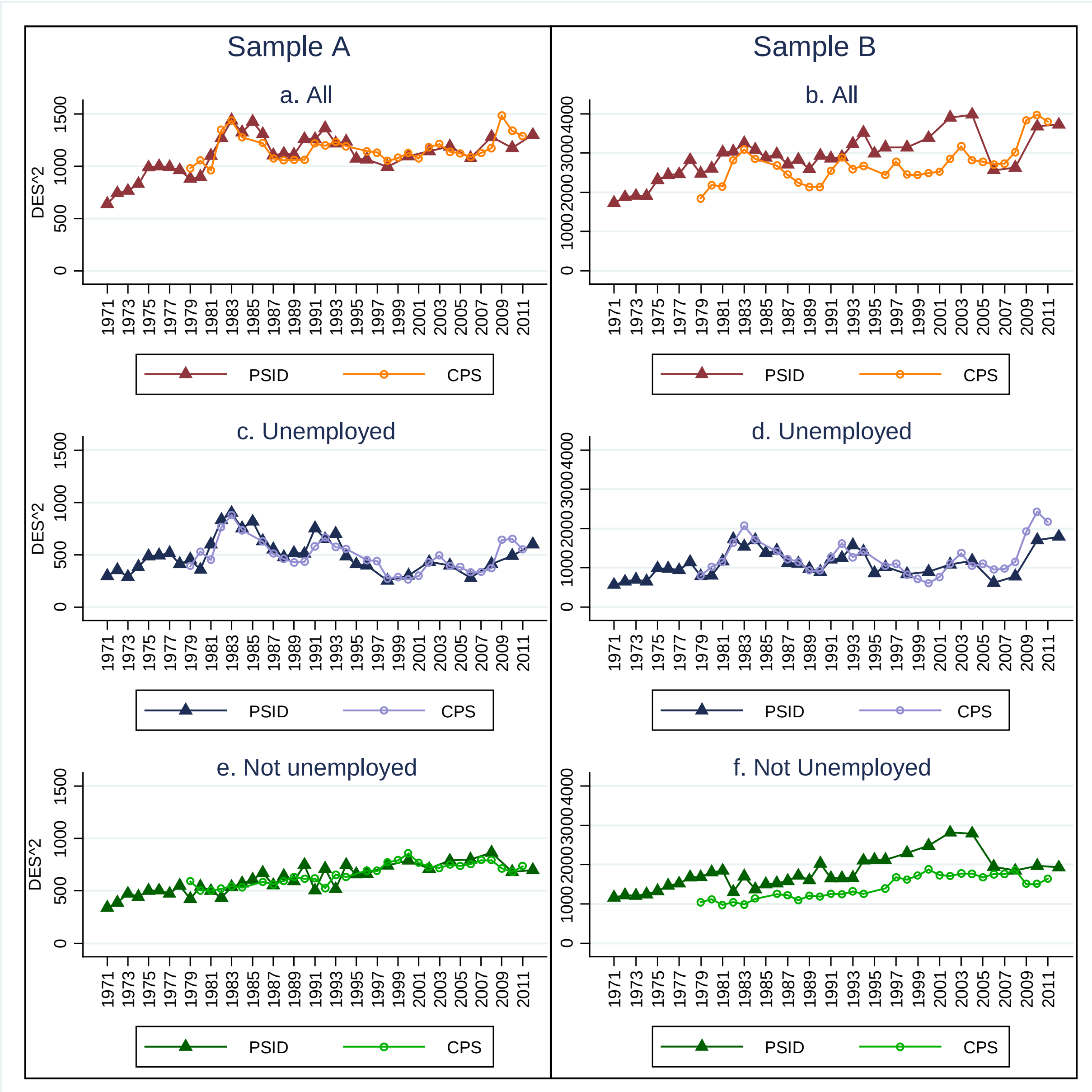


Figure 3.A4 Decomposition of Earnings Volatility: Unemployed vs. Not unemployed



APPENDIX B

SUPPLEMENTAL TABLE FOR CHAPTER 3

Table 3.B1 Descriptive Statistics for Sample A and B

	Sample A				Sample B				
Year	Number	Unemp.	Age	Earnings (2013$)	Number	Unemp.	Age	Earnings (2013$)	Fraction of Zero-earners
1971	1,005	21%	42.0	63,360	1,056	21%	42.3	60,754	3.3%
1972	1,008	22%	41.9	64,267	1,062	22%	42.1	61,818	3.0%
1973	1,044	19%	41.7	62,267	1,108	19%	42.0	59,252	4.4%
1974	1,118	22%	41.5	60,295	1,178	22%	41.8	57,643	3.6%
1975	1,136	20%	41.5	60,466	1,208	20%	41.7	57,260	4.0%
1976	1,168	23%	41.2	62,268	1,242	24%	41.6	58,905	4.4%
1977	1,203	20%	41.2	62,450	1,278	21%	41.4	59,347	3.7%
1978	1,217	18%	41.1	61,120	1,316	19%	41.4	57,345	4.4%
1979	1,267	17%	41.2	58,041	1,358	17%	41.5	55,055	4.2%
1980	1,296	20%	41.1	57,333	1,389	21%	41.4	54,135	4.6%
1981	1,329	20%	41.0	57,931	1,428	20%	41.3	54,224	5.0%
1982	1,333	26%	40.8	58,800	1,437	26%	41.2	54,792	5.2%
1983	1,324	24%	40.7	58,801	1,430	25%	41.0	54,442	5.7%
1984	1,344	24%	40.5	61,551	1,449	25%	41.0	57,281	5.0%
1985	1,359	22%	40.7	62,287	1,452	23%	41.0	58,560	4.3%
1986	1,377	19%	40.5	63,113	1,476	20%	40.8	59,039	4.9%
1987	1,405	18%	40.4	62,565	1,502	18%	40.8	58,760	4.7%
1988	1,414	17%	40.4	61,877	1,514	18%	40.7	57,924	4.9%
1989	1,437	17%	40.3	60,982	1,537	17%	40.7	56,986	5.0%
1990	1,429	16%	40.5	60,230	1,535	16%	40.8	56,224	5.4%
1991	1,455	19%	40.5	61,162	1,562	19%	41.0	57,131	5.4%
1992	1,397	17%	40.9	61,329	1,499	17%	41.2	57,308	5.7%
1993	1,382	17%	41.2	63,415	1,502	19%	41.5	58,607	6.3%
1994	1,408	15%	41.1	63,391	1,546	16%	41.4	57,978	6.9%
1995	1,593	14%	41.6	62,084	1,748	15%	42.0	57,107	6.4%
1996	1,595	13%	41.7	63,122	1,769	14%	42.1	57,685	7.5%
1998	1,628	12%	42.3	66,929	1,792	13%	42.7	61,639	6.3%
2000	1,891	13%	42.6	66,047	2,068	13%	43.0	60,570	6.7%
2002	1,912	13%	43.1	64,041	2,142	14%	43.5	57,465	9.2%
2004	1,885	11%	43.1	63,667	2,111	12%	43.7	59,408	7.4%
2006	1,954	10%	43.6	67,248	2,137	10%	43.9	61,396	7.7%
2008	1,932	9%	43.8	68,772	2,107	10%	44.2	62,838	7.1%
2010	1,832	12%	44.1	66,681	2,069	15%	44.4	58,737	10.8%
2012	1,756	12%	43.9	67,670	2,006	16%	44.4	58,834	10.5%

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