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Individual Demographic Transitions and Financial Hardship

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INDIVIDUAL DEMOGRAPHIC TRANSITIONS AND FINANCIAL HARDSHIP

by

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ABSTRACT

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This dissertation consists of two essays on the relationship between individual demographic transitions, major life events that alter how an individual may be categorized by demographers, and financial wellbeing. Recent literature on Social Security Disability Insurance (SSDI) has reported an absence of substitution behavior between SSDI and other social insurance programs, which is unexpected considering the observed countercyclicality of SSDI awards. In the first chapter, I decompose the increase in the SSDI enrollment rate over the period surrounding the Great Recession, finding that 54.9% of the increase in the enrollment rate can be attributed to individuals who did not previously identify as disabled. I then address the often-discarded possibility that recessions are themselves disabling, discussing evidence that the incidence of disabling conditions increased over the recessionary period.

Many changes in divorce policy have been grounded in the concern that divorce may cause financial hardship, especially among divorced women. Indeed, there is a well-documented correlation between financial hardships and divorce, but the direction of causality remains unclear: it is easy to imagine that divorce causes hardship, that hardship raises the risk of divorce, or that other factors may produce both outcomes. In the second paper, I specify a model that nests all three possibilities and can be estimated using standard limited dependent variable and simultaneous equation methods. Using instruments that have been used in prior work, I estimate the model on data from the National Longitudinal Survey of Youth 1979 Cohort. After controlling for both selection and simultaneity, the structural estimates imply a clear causal structure: I find no evidence that hardship causes divorce, but the event of divorce decreases the income/needs ratio in divorced women's households by approximately 0.32 standard deviations. However, further evidence indicates that the causal effect of the divorce itself is partially obscured by a negative association between hardship and the risk of divorce, which appears to owe to anticipatory responses in women's labor supply. Accounting for those anticipatory responses also reveals a negative structural error correlation between divorce and the income/needs ratio, suggesting some unobserved factors may produce both divorce and hardship.

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This research was conducted with restricted access to Bureau of Labor Statistics (BLS) data. The views expressed here do not necessarily reflect the views of the BLS. All errors are my own.

DECOMPOSING CYCLICAL FLUCTUATIONS IN SOCIAL SECURITY DISABILITY INSURANCE AWARDS

I. Introduction

The relationship between Social Security Disability Insurance (SSDI) awards and the business cycle has received considerable attention in the literature. The basic problem is that SSDI is designed to insure workers against permanently disabling conditions, not to insulate workers from the business cycle. This intention is reflected by the very low rate of benefit termination, which, according to the 2015 Annual Statistical Report on the Social Security Disability Insurance Program (ASRSSDI), has fallen from the 1982 retrenchment rate of 1.63% to a low of 0.72% in 2007, although it has since increased to 0.85% in 2015.

To visually approximate the magnitude of the problem, Figure 1 plots the percentage change in the number of awardees and the percentage change in annual average unemployment rate from 1987 to 2015. Contrary to the intent of the program to insure workers against the risk of permanently disabling injury, the most important determinant of the number of SSDI awards appears to be how many workers were displaced that year. Indeed, Maestas, Mullen, and Strand (2015) find that the increase in the unemployment rate over the Great Recession increased the number of SSDI applications by 6.7%. Overall, the number of awards has been growing at an average rate of 2.3% per year, spiking during recessions to as much as

18.7% following the 1990-1991 recession. Combined with the low rate of benefit termination, the SSDI program has grown to cover 10.24 million beneficiaries as of 2015, roughly 3.2% of the U.S. population (up from 1.4% at the time of the 1984 reforms), who received a total of $11.4 billion dollars in payments. While it is tempting to attribute these patterns to some form of malingering or fraud, prior work consistently finds that disability programs are appropriately targeted, and that even rejected applicants exhibit a diminished capacity for gainful employment that is entirely inconsistent with malingering (Bound 1989; Bound and Waidmann 1992; Maestas, Mullen, and Strand 2013).

Given the cyclicality of SSDI awards, a natural assumption is that there must be some substitution by workers between SSDI and other social insurance programs; however, this does not appear to be the case. In a recent paper, Mueller, Rothstein, and von Wachter (2016) find no relationship between the exhaustion of unemployment insurance (UI) benefits and the number of SSDI applications. Likewise, Autor and Duggan (2003) find no relationship between the UI replacement rate and receipt of SSDI. Contrary to expectation, these results suggest that SSDI is not being used as a substitute for UI (see Mueller, Rothstein, and von Wachter 2016 for a detailed discussion).

The apparent lack of substitution between UI and SSDI leaves the literature at something of an impasse. Our objective here is to decompose the populations enrolling in SSDI to evaluate which claims presented in the literature have empirical support as a potential cause of the observed cyclicality in SSDI awards, resolving this impasse and suggesting a way forward. To that end, this apparent lack of substitution reduces the number of testable hypotheses for the countercyclicality of SSDI awards to three. The first is that disabled individuals, who are displaced by the recession, are exiting the labor force and seeking SSDI. The second hypothesis is that Autor and Duggan's (2003) conditionally attached workers are responsible: that low-skill individuals who were previously working, and who are in generally poorer health but not necessarily disabled, upon displacement from the labor force choose to seek SSDI instead of seeking work, and are more likely to do so when the costs of seeking work are relatively high. The third possibility, which has been neglected in the SSDI literature, is that recessions are themselves disabling.

To determine which of these hypotheses are supported by observed trends in the SSDI enrollment rate, we perform Oaxaca-like decompositions of the increase in the enrollment rate during the Great Recession. This exercise eliminates two of the above possibilities. First, changes in the employment and labor force participation of initially disabled individuals only account for 7.4% of the observed increase in SSDI enrollment during the Great Recession. This finding suggests that any factor related to the labor force participation of existing disabled workers, such as the possibility of increased labor market discrimination during recessionary periods, would have limited impact on SSDI program participation. Second, examining three proxies for conditionally attached workers, we find that this population cannot substantially account for the observed increase in SSDI enrollment. Finally, we find that 54.9% of the increase in the enrollment rate can be attributed to individuals who did not previously identify as disabled. While this result could support either of the latter two hypotheses, we provide a detailed discussion of the possibility that recessions are themselves disabling.

The paper proceeds as follows. Section II provides a discussion of our data, section III presents the decomposition, section IV provides discussion, and section V concludes.

II. Data

The relatively small size of some of our populations of interest, such as disabled workers who pass through unemployment, necessitates the use of a very large sample. Our primary data source is the Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS). To provide coverage of the Great Recession and the ensuing recovery, as well as a relevant comparison period, our sample period is 2006 to 2012, which includes income and employment data from 2005 to 2011, as well as self-reported labor force participation and disability status from 2006 to 2012.

To further ensure a sufficiently large sample size in each population cell of our decomposition, we divide the sample into two subsamples: one which covers a period in which the labor market was relatively strong, and one which covers a weaker labor market. These periods are defined using national total private employment from the Bureau of Labor Statistics (BLS). The strong labor market period includes survey years 2006-2008, as by January 2005 total private employment had recovered to a pre-recession level of approximately 111 million, climbing to 116 million by December 2007[1], the official start of the Great Recession. The weak labor market period includes the remaining survey years in our sample.

In our analysis, we exploit the rotation group of the CPS ASEC. For our decomposition, we generate individual identifiers from household identifiers, age, sex, and line number, which yields a panel with two observations for each[2] individual in the rotation group. This allows us to observe which individuals in the rotation group enroll in SSDI during their observation period. We define enrollment as receiving 0 social security income in the first period and greater than 0 social security income in the second.

Since the CPS ASEC only reports Social Security income from all sources, we restrict this sample to non-institutionalized civilians aged 25-61

[1] By March 2008, when the survey is conducted, total private employment had only fallen to approximately 115.8 million.
[2] Excluding those individuals who were last surveyed in 2006, or first surveyed in 2012.

who are not widows[3]. We exclude individuals younger than 25 to avoid confounding dependent benefits and labor force non-participation from individuals still enrolled in school, and we exclude individuals over 61 to eliminate the possibility of early retirement in the event of the loss of a primary job[4]. For the purposes of our decomposition, we further exclude those individuals for whom we cannot infer labor force participation and unemployment status for the year preceding the survey, as well as those who do not self-report disability or labor force participation for the initial survey year. All those individuals for whom we observe only one year of data are also excluded, as we do not observe enrollment status for those individuals. These restrictions yield 101,563 observations for the strong labor market period, and 101,379 for the weak labor market period.

III. Decomposition

We begin with our decomposition of the change in enrollment rate. Let X_t denote a partition of the period t sample observations. Further let $X_{tj} \subset X_t$, such that $j \in \{1, \dots, k\}$, with each subset containing n_{tj} elements x_{tji}. Finally, let $n_t = \sum_{j=1}^{k} n_{tj}$ be the number of individuals in the period t sample, and ε_{tji} be an indicator variable which equals 1 if an individual reports 0 social security earnings in the previous year and greater than 0 social security

[3] While we are unable to identify those divorced spouses who are eligible for enrollment in survivor benefits, claiming this benefit requires a qualifying disability or eligibility for retirement.
[4] Including these individuals may lead us to incorrectly conclude relatively strong attachment to the labor force among SSDI enrollees.

earnings in the current year. It follows that the enrollment rate in period t is given by

$$\varepsilon_t = \sum_{j=1}^{k} \frac{n_{tj}}{n_t} \cdot \Pr\left(\varepsilon_{tji} = 1 \middle| x_{tji} \epsilon X_{tj}\right)$$

Performing an Oaxaca-like decomposition, this implies that the change in enrollment rate between an initial period 0 and a subsequent period 1 is given by

$$\varepsilon_1 - \varepsilon_0 = \sum_{j=1}^{k} \Bigg(\left[\frac{n_{1j}}{n_1} - \frac{n_{0j}}{n_0}\right] \cdot \frac{\Pr\left(\varepsilon_{1ji} = 1 \middle| x_{1ji} \epsilon X_{1j}\right) + \Pr\left(\varepsilon_{0ji} = 1 \middle| x_{0ji} \epsilon X_{0j}\right)}{2}$$

$$+ \frac{\left(\frac{n_{1j}}{n_1} + \frac{n_{0j}}{n_0}\right)}{2} \cdot \left[\Pr\left(\varepsilon_{1ji} = 1 \middle| x_{1ji} \epsilon X_{1j}\right) - \Pr\left(\varepsilon_{0ji} = 1 \middle| x_{0ji} \epsilon X_{0j}\right)\right] \Bigg)$$

The first term in the above expression is the change in enrollment attributable to the change in the relative number of people in a subset, i.e. to the change in composition. The second term is the portion attributable to the change in probability that an individual in a given subset will enroll in SSDI, i.e. to the change in rates.

We define the initial period as containing those individuals who were first observed during the strong labor market period. Likewise, the second period consists of those individuals who were first observed during the weak labor market period. We partition the individuals in each period over four characteristics. The first characteristic is imputed labor force participation status for the year preceding the first survey year in which the individual is observed. Individuals are assigned a labor force status if they report some in-universe value for at least one of wage income, unemployment duration, or weeks worked, and are designated as having participated if any of these values are greater than zero.

The other characteristics are whether an individual has reported some nonzero duration of unemployment in the year preceding their first survey, whether an individual identified as having a work-limiting disability during their first survey, and whether an individual who was identified as having participated in the labor force, when asked their labor force participation status the following March, indicated that they were no longer a participant. This partition yields ten subsets of interest, as two of the characteristics, unemployment status and labor force exit, are only relevant for individuals who were initially observed as participating in the labor market.

IV. Discussion

Table 1 presents the results of the decomposition. The total change in observed enrollment rate of 0.162 percentage points corresponds to an increase of 9.5%, which is close[5] to the 9% year-over-year increase in number of awards reported by the Social Security Administration for 2008.

Considering recent literature on the subject of enrollment in SSDI, many of the results of the decomposition are unsurprising. We focus on those results which support or conflict with our three hypotheses: that

[5] Due to the aggregation of our annual data, these two measures are not entirely comparable.

displaced disabled individuals are exiting the labor force and seeking SSDI, that conditionally attached workers are responsible, or that recession is causing disability. We proceed by hypothesis.

IV.a Displaced Disabled Workers

From Table 1, we observe that the share of initially disabled workers who do not pass through unemployment inhibits the increase in the enrollment rate by nearly 24.1%, while the corresponding increase in the share of disabled workers who do pass through unemployment (a group which we would naturally expect to have relative high enrollment rates) only accounts for 3.1% of the observed increase in the enrollment rate. Considering that the increase in the share of disabled nonparticipants in the population accounts for 28.4% of the observed increase in the enrollment rate, changes in the labor force participation decision of these individuals who initially identify as disabled accounts for only 7.4% of the observed increase in enrollment, which is not very large. Consequently, any explanation for the increase in SSDI enrollment during recessions that operates through a change in the labor force participation or employment of workers who were already disabled, such as labor market discrimination, cannot account for much of the observed increase.

IV.b Conditionally Attached Workers

Most strikingly, 97.5% of the observed increase in SSDI enrollment is attributable to labor-force non-participants. It is tempting to interpret from this that workers following a conditional application strategy played a critical role in the expansion of SSDI enrollment during the Great Recession. However, due to the requirement that SSDI applicants not engage in substantial gainful activity during the application process, all but the lowest income enrollees must withdraw from the labor force prior to enrollment.

Unfortunately, limitations[6] in our data restrict our ability to decompose by cause of job loss, which would better inform our analysis of conditional applicants. Consequently, the hypothesis we examine here is not whether workers exhibited conditional attachment during the Great Recession. Rather, we examine if the workers most susceptible to the conditional attachment mechanisms identified by Autor and Duggan, i.e. workers in the lower tail of the income distribution for whom the increasing dispersion of permanent income has made SSDI relatively more attractive, are disproportionately responsible for the observed cyclicality in SSDI enrollment.

Table 2 presents three abridged decompositions which examine these groups. Following Autor and Duggan, we use high school dropouts as a proxy for these conditionally attached workers. We further consider income-defined groups below 100% and 200% of the poverty level[7]. While the more

[6] Reason for unemployment is only reported for individuals who indicated undergoing a span of unemployment.
[7] Some may be concerned here that household income is determined by the strength of the labor market, and that imputed household income should be used in place of reported values; however, who is actually moving into and out of impoverished condition is more

pertinent issue in determining whether a worker is likely to exhibit conditional attachment is low permanent income, the Great Recession was in part characterized by a tightening of credit markets, which would have hindered efforts to smooth consumption for those who temporarily found themselves at the lower tail of the income distribution.

While each of the three groups identified is more likely than average to enroll in SSDI, we see that none of them substantially account for the change in SSDI enrollment over the Great Recession. Those below 100% and 200% of the poverty level account for 17.7% and 34.7% of the total observed change respectively, while high school dropouts inhibit the increase in SSDI enrollment by 3%, which indicates that the individuals most responsible for the increase in SSDI enrollment over the Great Recession are those with traditionally strong labor force attachment. Furthermore, while the number of impoverished people increased during the Great Recession, these individuals are much less likely to enroll in SSDI than individuals who were impoverished prior to the recession. These findings suggest that this channel is not substantially responsible for the observed increase in SSDI enrollment during the Great Recession.

One possible explanation for this is another factor discussed by Autor and Duggan: screening stringency. As we see in Figure 1, the increase in

relevant to the question at hand than who should be. Thus, we forego the use of imputed values.

SSDI enrollment during the Great Recession was mild relative to the change in unemployment. Indeed, Maestas, Mullen, and Strand (2015) find evidence of increased stringency among SSDI examiners during economic downturns. This increased stringency during the Great Recession, a constriction in the supply of benefits, would have limited the contribution of conditionally attached workers to the increase in SSDI enrollment.

IV.c Recessions as a Cause of Disability

That 54.9% of the increase in enrollment can be attributed to those members of the population who do not initially identify as disabled suggests an alternative explanation. Assuming effective screening and that self-reported disability status is an unbiased indicator of disability (see Benítez-Silva et al. 2004), these are workers for whom disability status was not a determining factor in their labor force participation decision, but who suffered a disabling condition that was sufficiently incontrovertible to be granted SSDI within our observation period. This finding suggests that the most important factor driving the increase in SSDI enrollment during the Great Recession may have been individuals becoming disabled during the recession.

Autor and Duggan largely dismiss this possibility as an explanation for increases in SSDI enrollment because the mortality rate has been decreasing over time, and yet they also note that low-mortality conditions are responsible for an increasing share of disability awards. It is therefore possible to reconcile an increased rate of disability with a decline in mortality. Furthermore, even if the decreasing trend in the mortality rate did exclude the possibility that the incidence of disabling conditions is increasing over time, this trend has since reversed for middle-aged, White, non-Hispanic workers (Case and Deaton, 2015).

Likewise, Maestas, Mullen, and Strand (2015) also disregard this possibility as an explanation for the cyclicality in SSDI awards, noting the absence of evidence for the countercyclicality of severe disability. Again, as is noted in Autor and Duggan, the share of SSDI awards for low-mortality conditions has been increasing over time, and there is substantial evidence that the incidence of these low-mortality conditions, in particular mental illness, is countercyclical. For instance, a recent study by Mehta et al. (2015) found that the incidence of major depression in the National Health and Nutrition Examination Survey increased from 2.33% in the 2005-2006 survey to 3.49% in the 2009-2011 survey. Concurrently, the incidence of other depression increased from 4.10% to 4.79%. These figures yield an increase in the incidence of all depression of 28.8% during the Great Recession. Ruhm (2015) reports the recent emergence of a countercyclical trend in deaths from accidental poisoning[8] and suicide, which supports the possibility of an increase in depression or other mental illness over the recessionary period, as well as from neoplasms. Similarly, Dávalos, Fang,

[8] A category which includes drug overdoses.

and French (2012) find that an increase in the unemployment rate is associated with an increase in the incidence of alcohol abuse and dependence. Charles and DeCicca (2008) find that worsening economic conditions increase weight gain, which is a cause of chronic musculoskeletal conditions, and reduce mental health among African American and low-skilled workers. Finally, Bradford and Lastrapes (2014) find that a 1% decrease in employment and a 1 point increase in the unemployment rate are associated with a 10% increase in the number of prescriptions for anxiety and depression drugs.

Finally, that we observe an increase in the incidence of a disabling illness during recessions is not sufficient evidence to indicate that SSDI is functioning as intended. While we have argued for the countercyclicality of depression, individuals with recession-induced illness could conceivably recover once the recession passes, and there is no compensating procyclicality in the benefit termination rate. That is, we have individuals with a potentially temporary disability applying to, and enrolling in, a largely permanent disability program. If recession-induced disability is driving enrollment in SSDI, a possible solution for preventing the substantial increases in total enrollment associated with recessions would be to flag these awards issued during recessions as “improvement expected”, which would require a medical review 6 to 18 months after the award.

V. Conclusion

Largely cyclical increases in the number of SSDI awards, combined with low rates of benefit termination, have increased the size of SSDI participation from 1.4% of the U.S. population at the time of the 1984 SSDI reforms to 3.2% of the U.S. population today. Considering the absence of substitution between SSDI and UI, we presented three hypotheses that could account for these increases. The results of our decomposition of the increase in the enrollment rate over the Great Recession and recovery conflict with explanations derived from the labor force participation decision or employment status of disabled workers, or those related to the presence of conditionally attached workers.

We further found that 54.9% of the increase in the enrollment rate is attributable to individuals who did not previously identify as disabled, and discussed evidence that the incidence of disabling illnesses increased during the recessionary period. These findings suggest that the mysteries of cyclical increases in SSDI awards and a lack of substitution between SSDI and UI may not be mysteries at all: recessions may simply be making us sick.

SIMULTANEITY AND SELECTION IN FINANCIAL HARDSHIP AND DIVORCE

I. Introduction

It is well-documented that there exists a positive correlation between financial hardship and divorce. While divorce and financial hardship are well correlated, it is often unclear what the direction of causality is, and through which channels it operates. For example, divorce may cause hardship by eliminating opportunities to share household expenses or if the spouses rush to extract the assets that were previously held jointly, hardship could cause financial arguments that culminate in divorce, and it is easy to imagine that some personality traits (e.g., addiction, poor communications skills, irresponsible financial habits) could lead to both divorces and poor outcomes in the labor market or other financial difficulty.

This paper investigates those competing hypotheses by estimating a structural model of divorce and financial hardship that nests many of the proposed channels. The empirical strategy involves both simultaneous equations methods (as suggested by Becker, Landes, and Michael (1977), Johnson and Skinner (1986), and Charles and Stephens (2004)) and corrections for selection on unobservables (as suggested by Dew, Britt, and Huston (2012), among others), and it can be estimated using standard latent variables methods.

Using data from the National Longitudinal Survey of Youth 1979 Cohort, we have four primary findings. First, we find no causal effect of income-based hardship on divorce. Second, we find that the event of divorce has a substantial negative effect on women's financial well-being as measured by an income/needs ratio. Third, we find that women who expect an elevated risk of divorce can partially mitigate the effect of the divorce on their income/needs ratio through anticipatory or predicted reactive behavior, which appears to take the form of a modest labor supply response. Finally, after correcting for this effect of the propensity to divorce on future income, we find some evidence of negative selection on unobservables. That is, that some unobserved factor may be producing both divorce and financial hardship.

In our policy extension, we further find that the negative financial consequences of divorce are substantially stronger for women who live in states where the divorce law both allows one spouse to end the marriage without the consent of the other (unilateral divorce) and does not presume that divorcing spouses share equal ownership of the family assets (non-community property states). We also find that government transfer programs, in particular Temporary Assistance for Needy Families, are effective in mitigating the negative effects of divorce on women's future income, and that the anticipatory labor supply response to an elevated divorce risk may reduce reliance on government transfer programs.

The discussion proceeds as follows. The next section reviews previous work on these issues. Section III then presents the behavioral and econometric model, and Section IV discusses the data. Section V presents the main results, Section VI presents a policy extension, and Section VII concludes with a summary and suggestions for further research.

II. Literature Review

In addition to being well-documented, the relationship between divorce and financial hardship is of substantial policy relevance. As reported by Bane and Ellwood (1986), 38% of the instances of poverty among female-headed households with children are attributable to the formation of that household through divorce. They further find that these spells of poverty tend to be longer in duration than those attributable to other sources, both for the women and for their children. A follow-up study by Stevens (1994) suggests that the consequences may have become more severe over time as well, as evidenced by their finding that the annual rate at which female-headed households exited poverty decreased over the 1980’s.

Previous work has suggested many potential explanations for the observed correlation between hardship and divorce. In their seminal paper on the subject, Becker, Landes, and Michael (1977) theorize that the probability of divorce is increasing in husbands’ earnings and decreasing in wives’ earnings because greater specialization increases the gains to marriage. They then find evidence of such an effect among the white men in the Survey of Economic Opportunity: the probability of divorce is decreasing in earnings up to an income of $40,000 in 1966 dollars. However, they caution that the direction of causality may run in both directions: that less specialization produces less stable marriages, and that those who expect their marriage to be less stable may specialize less. Evidence of this hypothesized anticipatory behavior has been found among women by Johnson and Skinner (1986), who find that women who get divorced tend to begin increasing their labor supply prior to the divorce. Note that such a response would reduce the impact of divorce on the women's finances, but it would also suggest that the raw relationship may mask or understate the true effect of divorce on those for whom it comes as a greater surprise.

Other studies present evidence that may suggest that hardship leads to divorce, though it almost always comes with the caveat that both may instead be the product of other factors. For example, Charles and Stephens (2004) find that job displacement increases the probability of divorce, but only when that job loss is due to a layoff, suggesting that the link between divorce and financial hardship may be through some omitted variable or through an information-updating mechanism. Likewise, Dew, Britt, and Huston (2012) find that even after controlling for other financial factors like income, assets, and wife's share of household income, reports of arguments about finances and financial inequity within the household are associated with an increased probability of divorce, seemingly suggesting that the statistical relationship is not entirely about financial resources. In another novel study, Francis-Tan and Mialon (2015) find that women who spent more than $20,000 on a wedding had a divorce hazard 3.5 times higher than those who spent between $5,000 and $10,000, identifying stress associated with wedding debt as a potential channel for divorce. However, it is also possible that couples who spend more on weddings may do so to compensate for lukewarm feelings about their union, or that their stress may ultimately owe more to chronic profligate spending than to a lack of income.

Two notable studies have attempted to determine causality more definitively, and each shares some features with the present paper. Smock, Manning, and Gupta (1999) use an endogenous switching regression model similar to ours, though they estimate it on a different sample consisting of women who already reported being married in the first wave (1987-1988) of the National Survey of Families and Households. They find that those who had divorced before the second wave of that survey (1992-1994) had worse financial outcomes overall than those who remained married. However, their estimates also indicate that the divorced women would have had worse financial outcomes than the other wives if they had remained married, and that their financial outcomes in divorce were better than those that would have been expected for the wives who actually chose to remain married.

The other particularly notable study is by Bedard and Deschênes (2005), who examine the causal relationship between women's financial hardship and divorce in data from the 1980 U.S. Census. They use the sex of a couple's first child as an instrument for divorce; a series of earlier sociological studies (as well as Dahl and Moretti (2008)) had shown that divorce is more likely when the first child is a daughter. Their IV estimates imply that divorce is associated with an approximately $4,000 increase in household income, and they further note that ever-divorced women have greater hours and weeks worked per year than never-divorced women. This estimate combines two responses of potential interest: that of an increased *risk* of divorce, and the corresponding share of the effect of the realization of the divorce risk. Our estimates separate these effects into an effect of the propensity to divorce and a direct effect of divorce.

III. Model

III.a. Behavioral and Structural Model

Define D to be an indicator of divorce, H to be a continuous measure of financial hardship, X to be a vector of explanatory variables that determine both D and H, Z_D to be a vector of variables that determine only D, and Z_H to be a vector of variables that determine only H. To allow for the possibility that financial hardship is affected by divorce, further let H_0 be the potential hardship in the event that an individual remains married and H_1 be the potential hardship in the event of divorce, with $\theta = H_1 - H_0$.

Consider a married individual who is deciding whether to remain that way. They may either choose to remain married and receive $U_0 =$

$U_0(X, Z_D, H_0)$, or they may become divorced and receive utility $U_1 = U_1(X, Z_D, H_1) \approx U_1(X, Z_D, H_0) + \theta \partial U_1 / \partial H$. Let c denote the cost of divorce. It follows that an individual will divorce if and only if $D^* \equiv U_1 - U_0 - c > 0$. Taking Taylor approximations of U_1 and U_0 around points x_1 and x_0, letting Δ be the differentiation operator, and using ν_1 and ν_0 to denote the effects of unobserved variables and approximation errors, we obtain

$$
\begin{aligned}
D^* = [U_1(x_1) - x_1 \cdot \Delta U_1(x_1) - U_0(x_0) + x_0 \Delta U_0(x_0) + \theta \partial U_1(x_1)/\partial H - c] \quad (1) \\
+ (X, Z_D, H_0) \cdot \Delta[U_1(x_1) - U_0(x_0)] + (\nu_1 - \nu_0) \\
= \alpha_D + X\beta_D + Z_D\gamma_D + H_0\delta + \varepsilon_D \quad (2)
\end{aligned}
$$

Note that the entire first line of equation (1) consists of constants which are subsumed into the intercept α_D. Since the effect of divorce on hardship, θ, is also subsumed into that constant, only H_0 is multiplied by the marginal utilities in the second line of equation (1)[9]. Equation (2) defines parameters (α_D,β_D,γ_D,δ) which correspond to their respective terms from equation (1), with $\varepsilon_D \equiv \nu_1 - \nu_0$.

The model for the hardship equation may also be justified as a Taylor approximation. In addition to allowing hardship to be affected by a realized divorce outcome, D, we further allow the possibility that it is affected by the latent propensity to divorce represented by D^*. For example, Johnson and Skinner (1986) show that wives who anticipate divorce are more likely to participate in the labor force, which would reduce their household's financial hardship whether or not the marriage ends in divorce. While our estimated

[9] Identification of γ_D does not require constant θ. θ may vary in X without interfering with the identification of γ_D if utility is additively separable in H and Z_D.

effect of the propensity to divorce on financial hardship may include the effects of both anticipatory and predicted reactive behavior due to the coarseness of our timing, we will refer to it as the effect of anticipatory behavior for the sake of brevity. Our hardship equation is therefore

$$H = \alpha_H + X\beta_H + Z_H\gamma_H + \theta D + \varphi D^* + \varepsilon_H \qquad (3)$$

Since unobserved variables may affect both the propensity for divorce and financial hardship, we assume

$$(\varepsilon_D, \varepsilon_H)' \sim N[0, S] \qquad (4)$$

$$S \equiv \begin{bmatrix} \sigma_D^2 & \sigma_{HD} \\ \sigma_{HD} & \sigma_H^2 \end{bmatrix} \qquad (5)$$

We are particularly interested in the off-diagonal element σ_{HD} or the associated correlation $\rho \equiv \sigma_{HD}/(\sigma_H\sigma_D)$. Since our hardship variable is the log income/needs ratio, a measure of wellbeing, a positive correlation would imply either that divorce and more comfortable household incomes just happen to occur together or that some unobserved factor is positively correlated with both the event of divorce and higher income/needs ratios. Likewise, if the correlation is negative we have a spurious relationship between divorce and hardship, such as some unobserved factor that is positively correlated with divorce but negatively correlated with the income/needs ratio.

This structural model nests several models of interest. If $\rho \neq 0$ and $\varphi = \delta = 0$, we have a typical selection model, i.e. one with no simultaneity. The model with $\varphi = 0$ and $\delta \neq 0$ is a selection model in which divorce only affects hardship through its actual occurrence, but hardship affects the propensity to divorce. Similarly, the model with $\varphi \neq 0$ and $\delta = 0$ is a selection model in which both the event of and the propensity to divorce affects hardship, but hardship has no effect on divorce. Finally, the model with $\varphi \neq 0$ and $\delta \neq 0$ is a selection model with full simultaneity, to which we now proceed.

III.b. Reduced Form Model

The structural model defined in (2) and (3) may be written in reduced form as

$$\begin{bmatrix} D^* \\ H_0 \end{bmatrix} = \begin{bmatrix} 1 & -\delta \\ -\varphi & 1 \end{bmatrix}^{-1} \begin{bmatrix} \alpha_D + X\beta_D + Z_D\gamma_D + \varepsilon_D \\ \alpha_H + X\beta_H + Z_H\gamma_H + \varepsilon_H \end{bmatrix} \tag{6}$$

Thus, we can define parameters A_j, B_j, C_j, F_j, and u_j with $j \epsilon \{H, D\}$ such that

$$D^* = A_D + XB_D + Z_DC_D + Z_HF_D + u_D \tag{7}$$
$$H = A_H + XB_H + Z_DC_H + Z_HF_H + \theta D + u_H\ , \tag{8}$$

where, e.g.,

$$B_H = (\varphi\beta_D + \beta_H)/(1 - \delta\varphi)$$

and

$$\begin{bmatrix} u_D \\ u_H \end{bmatrix} = \frac{1}{(1-\delta\varphi)} \begin{bmatrix} 1 & \delta \\ \varphi & 1 \end{bmatrix} \begin{bmatrix} \varepsilon_D \\ \varepsilon_H \end{bmatrix} \sim N[0, \Sigma] \tag{9}$$

$$\Sigma \equiv \left(\frac{1}{(1-\delta\varphi)}\right)^2 \begin{bmatrix} 1 & \delta \\ \varphi & 1 \end{bmatrix} S \begin{bmatrix} 1 & \varphi \\ \delta & 1 \end{bmatrix} \tag{10}$$

III.c. Estimation

The parameters of the reduced-form system can be consistently estimated using what Maddala (1983) calls a switching regression model. Under the normalizing assumption that $\Sigma_{11} = 1$, the coefficients in equation (7) correspond to the parameters of a probit model. In principle, one could then estimate the model through a two-step procedure, i.e., estimate a probit on divorce and use the resulting estimates to construct for each observation the inverse Mills' ratios $\lambda^{+}(x) \equiv \varphi(x)/\Phi(x)$ and $\lambda^{-}(x) \equiv -\varphi(x)/(1-\Phi(x))$, where φ is the density of the standard normal distribution, and Φ is its cumulative distribution. Since

$$E(H|X, Z_D, Z_H, D=1) = A_H + \theta + XB_H + Z_D C_H + Z_H F_H + \Sigma_{12}\lambda^{+} \quad (11)$$

$$E(H|X, Z_D, Z_H, D=0) = A_H + XB_H + Z_D C_H + Z_H F_H + \Sigma_{12}\lambda^{-} \quad (12)$$

one can then construct $\lambda = D\lambda^{+} + (1-D)\lambda^{-}$ to consistently estimate the parameters of

$$H = A_H + \theta D + XB_H + Z_D C_H + Z_H F_H + \Sigma_{12}\lambda + u_H \quad (13)$$

However, rather than following such a two-step method, we shall estimate the model through limited information maximum likelihood (LIML).

III.d. Identification

Finally, we must recover the parameters of the structural model from the estimated parameters of the reduced form model. Our estimate of θ is provided by the estimation procedure directly. Assuming just identification, the feedback parameters are given by

$$\hat{\varphi} = \hat{C}_H/\hat{C}_D \quad (14)$$

$$\hat{\delta} = \hat{F}_D/\hat{F}_H \quad (15)$$

The remaining structural parameters can be produced through similar linear combinations of the estimated reduced-form parameters, e.g.

$$\hat{\beta}_H = \hat{B}_H - \hat{B}_D\,\hat{C}_H/\hat{C}_D$$

Lastly, we must recover the structural covariance matrix through inversion of (10).

$$\hat{S} = \begin{bmatrix} 1 & -\hat{\delta} \\ -\hat{\varphi} & 1 \end{bmatrix} \hat{\Sigma} \begin{bmatrix} 1 & -\hat{\varphi} \\ -\hat{\delta} & 1 \end{bmatrix}$$
$$= \begin{bmatrix} 1 - 2\hat{\delta}\hat{\Sigma}_{12} + \hat{\delta}^2\hat{\Sigma}_{22} & \left(1 + \hat{\delta}\hat{\varphi}\right)\hat{\Sigma}_{12} - \hat{\delta}\hat{\Sigma}_{22} - \hat{\varphi} \\ \left(1 + \hat{\delta}\hat{\varphi}\right)\hat{\Sigma}_{12} - \hat{\delta}\hat{\Sigma}_{22} - \hat{\varphi} & \hat{\Sigma}_{22} - 2\hat{\varphi}\hat{\Sigma}_{12} + \hat{\varphi}^2 \end{bmatrix} \tag{16}$$

Note that under this specification, the exclusion restriction on the divorce instruments in the structural equations provides identification of φ, the effect of the propensity of divorce on hardship, but θ, the effect of the actual event of divorce (D) on hardship, can only be separately identified through functional form assumptions. This is unavoidable if we wish to measure the effect of the propensity to divorce (D^*) on hardship, and if we wish to separate out the effect of any anticipatory behavior from the reduced-form error covariance when we calculate the structural error correlation (ρ).

One alternative would be to reserve our exclusion restriction for the identification of θ by imposing the restriction $\varphi = 0$ (i.e., excluding Z_D from the reduced-form H equation). While this is the usual approach, note that in this application such a restriction excludes a potentially important response for which there is already some empirical evidence. Moreover, in practice that restriction also causes substantial changes in estimates of the structural correlation ρ, leading one to draw qualitatively different conclusions about the causal role of unobserved factors. Thus, while we shall present estimates both with and without imposing that restriction, we are more inclined to accept the insights from the unrestricted model.

Fortunately, we will find that estimates of θ are very similar regardless of whether that restriction is imposed. Moreover, the restriction makes almost no difference at all in the reduced form estimates of the other parameters. It thus appears that, in this particular application, there is very little practical cost to obtaining identification through the functional form assumptions,[10] particularly considering that it provides the benefit of allowing us to distinguish the effect of an important, otherwise unobservable channel.

IV. Data

Our primary data source is the National Longitudinal Survey of Youth, 1979 Cohort (NLSY79). The NLSY79, as the name suggests, is a longitudinal survey conducted by the U.S. Bureau of Labor Statistics on individuals who were 14-22 years of age in 1979. The longitudinal study consists of annual surveys up to 1994 and biennial surveys thereafter. These surveys cover a variety of topics, including demographics, fertility, education, labor market outcomes, and family structure. The original sample contained 12,686 respondents. The retention rate for the primary subsample remained above 90% until 1993, fell to 81% by 2000, and by 2014 has reached approximately 70%, or 79% if those respondents who are believed to be

[10] This is to be expected. Our hardship variable, the log income/needs ratio, is a log transformation of household income. Since income is widely accepted to be approximately lognormally distributed, our functional form assumption in this case is not only plausible, but in this case is empirically supported by prior evidence. This point is discussed further in the following section.

deceased are excluded. Considering that the task here is tracking a set of individuals for 35 years, 80% retention by the end of that period among the living respondents is quite high.

There are several advantages to using a longitudinal study for the study of family formation and dissolution. First, the use of a longitudinal study eliminates cohort effects, because the study is limited to a single cohort by design. Additionally, the use of a panel allows us to incorporate information about how an individual's circumstances change over time. To the extent that divorce is not an instantaneous phenomenon, we may exploit exogenous variation which occurs over a period of time, which may be more relevant for long-term decision making than the associated measure in any one period, to estimate causal effects. Finally, having access to a full panel means that we will not inadvertently exclude any highly volatile marriages which may form and dissolve between waves of other surveys.

Our subsample of interest consists of 2,598 women in the survey year in which they report becoming married for the first time since their last survey. These individuals are identified using a combination of the "number of spouses" variable for each year and the marital status variable, which unfortunately do not always agree with one another. The difference between the marital history variables appears to be that the number of spouses is the individual's highest total number of spouses within that year, whereas their marital status is their marital status as of the survey, so there exists a discrepancy in the variables if the individual was married within the year but after the survey date. For consistency with the timing of the other survey variables, if the reported total number of spouses increases from 0 to 1 in a survey year, and their marital status for that year indicates that they are married, then we assign that person to the subsample for that year. However, if the reported total number of spouses increases from 0 to 1 in a year, but their marital status does not change to married until the next year, then they are added to our subsample with the following year as their year of marriage.

We observe 40% of our sample reporting the dissolution of their first marriage within the first 30 years. While this is slightly smaller than we would expect from comparable estimates produced by Stevenson and Wolfers (2007), a member of this sample would have had to have married early for 30 years to have elapsed since the time of their first marriage. Figure 2 depicts the distribution of these divorces. As we can discern from Figure 2, the median divorce occurs after 7 years, which would suggest that the use of an 8-year interval would be appropriate. As an added benefit, an 8-year interval covers much of a typical business cycle during this period, so our hardship instrument will be less susceptible to temporary fluctuations and more representative of the overall state of the local economy inhabited by the couple under this choice of interval. We will also explore the usage of a 6-year interval, as several other studies on divorce have utilized this interval length.

Some previous research has included marriages beyond the first. We have opted to exclude these higher order marriages because their inclusion would necessitate the use of controls for the number of previous marriages in order to account for any causal effects of previous divorces. Unfortunately, any variation due to unobserved factors that produced the prior divorces would be "explained" by the number of prior marriages, which would confound our ability to measure selection on unobserved variation.

Our first dependent variable, D, equals 1 if, within 8 years of marriage (or 7 years for marriage years 1987, 1989, 1991, and 1993, for which 8 years later is not a survey year[11]), the individual changes their marital status to divorced. Our instrument for divorce, Z_D, the sex of the first child, has been used extensively as an instrument in the divorce literature (see Bedard and Deschênes (2005) and Dahl and Moretti (2008), as well as the bevy of sociological studies discussed therein). We define $Z_D = 1$ if the couple has a child and that first child is female, and 0 otherwise. Since the decision to have a child is very likely endogenous, we include an additional indicator variable which equals 1 if the couple has at least one child. Children are assigned as belonging to a couple if they are born during the marriage, or up

[11] An indicator for whether the period for an individual is seven years long was included in our regressions to control for any potential bias from uneven period lengths, but it was nowhere significant, and has been omitted from the results.

to one year before. Unfortunately, the NLSY79 documentation indicates that the fertility data for men is known to be inaccurate. Since this precludes the use of this instrument in estimating divorce and hardship equations for the male respondents, we have reluctantly omitted men from our analysis.

While this instrument has been widely used, we should acknowledge the recent critique by Hamoudi and Nobles (2014), who argue that male fetuses are more likely than female fetuses to miscarry in times of stress. However, this concern is likely to be considerably less relevant for the young women in our sample than it would be for the relatively older women studied by Hamoudi and Nobles.[12] Regardless, the sign on the estimated coefficient on the sex of the first child in our reduced-form hardship equation (C_H) is opposite to that predicted by their proposed source of bias, so if anything this would cause us to underestimate the effect of propensity to divorce on hardship.

An important question in instrumental-variable approaches is for whom this instrument is relevant, which informs the interpretation of the local average treatment effect. Table 3 presents select coefficient estimates for probit regressions of the form

$$\Pr(D = 1) = \Phi(\beta_0 + X\beta_1 + Z\beta_2) \tag{17}$$

[12] Still, in recognition of the fact that it would be particularly problematic for our identification strategy if our instrument for financial hardship predicted the sex of the first child: we checked, and it does not.

using both the 6 and 8-year intervals. In columns 2-4 we interact the sex-of-child instrument with different proxies for socioeconomic status: age at first marriage, years of education, and race respectively.

A few clear patterns emerge in Table 3. First, many well-established predictors of divorce do not predict divorce as strongly as expected under the 6-year interval, suggesting that for many of these marriages, the processes which produce divorce have often not yet completed by the end of that interval. Secondly, in columns 2-4, we observe that the effect of the sex-of-child instrument is decreasing in the proxies for socioeconomic status. This indicates the instrument is most relevant for low socioeconomic status women, which corroborates a similar finding by Bedard and Deschênes (2005). This is an important group because these women are those most likely to be on the margin of poverty.

Son preference appears to be weaker for white women or women who marry later. However, under either interval duration, the marginal effect of a female first child on the probability of divorce is nowhere significantly negative for white women, nor for any reasonable value of age at first marriage. Nor do we observe evidence of defiers by education. While we observe the most heterogeneity in the response to treatment by education, an examination of the marginal effects of sex-of-child by education category on the probability of divorce revealed no category for which we would reject the hypothesis of no defiers.

Other potential instruments for divorce were considered, including local area sex ratios by industry and occupation (see Svarer (2007)); however, none proved to be sufficiently strong in the divorce equation to function as instruments.

For our continuous hardship variable, H, we use the log income/needs ratio at the end of the 8-year period (or the 7-year period for marriage years 1987, 1989, 1991, and 1993). The income/needs ratio is constructed from total net family income by subtracting transfer payments and dividing it by the poverty level for a family of that size in that year, as defined by the U.S. Census Bureau. This measure functions as a good index of hardship as it reflects the available incoming resources per family member. Furthermore, it is sufficiently comprehensive as an index of household financial wellbeing that it accommodates most reasonable assumptions required for identification. Finally, the distribution of income is well-studied, and this transformation conforms to our functional form assumptions, which facilitates identification. One drawback of using this definition of hardship is that we are only indexing hardship by income, so we will not capture any effect of, e.g., profligate spending on divorce except possibly through the error correlation ρ.

The instrument (Z_H) we use for H is the average unemployment rate within the respondent's county of residence over the 7 or 8-year period, as reported in the Bureau of Labor Statistics Local Area Unemployment

Statistics. We match these county-level unemployment rates to individual person records in each year using the NLSY79 Geocode File. In Table 4, we repeat the relevancy exercise above, estimating via OLS equations of the form

$$H = \alpha_0 + X\alpha_1 + Z\alpha_2 + u \tag{18}$$

We again see some evidence of heterogeneity by education, with income in educated households much less responsive to changes in local economic conditions as measured by the local unemployment rate.

To further verify our distributional assumptions, Figure 3 plots the histogram of the residuals of the model in Table 4, Column 1. Apart from the lower tail outliers in the vicinity of -10, this appears to be a good, if slightly leptokurtic, approximation of the normal distribution. We will also present results in which a floor of -5 has been imposed on the log income/needs ratio, which will show that our results are robust to the presence of these outliers.

We additionally include controls for year of marriage, labor supply for both the respondent and her husband, the respondent’s tenure at her current job, the respondent’s age at this first marriage, the husband’s age[13], the respondent’s race, the couple’s family size, the respondent’s education in years and their Armed Services Vocational Aptitude Battery (ASVAB) score,

[13] Interacted with an indicator for whether the husband’s age was missing, as it is in a minority of cases.

personality indices for self-esteem and locus of control, and finally a set of dummies interacting region with urban/rural status. These independent variables all use the values from the year in which the marriage was reported. Note that since the NLSY79 is primarily focused upon tracking the respondent, we are unable to include variables for match quality, like if the respondent and her husband share the same religion. Select summary statistics are provided in Table 5.

As we can see in Table 5, the individuals in this sample are fairly representative of the cohort. The average age at first marriage among women is about 24, 88% of respondents are white, just over half of first children are girls, most have only completed high school, and about 22% of these marriages end in divorce within the first 7 or 8 years. Notably, there is a reduction in the proportion of minority respondents in the 8-year interval as compared to the 6-year interval. Inclusion in our sample requires, at a minimum, observations on both the first year of marriage and the final year of the interval. Unfortunately, 38% of Black and Hispanic oversample respondents were not surveyed in 2000, a year more likely to be included in the longer interval from first marriage.

Our identifying assumptions are that, conditional on these covariates, the local unemployment rate is uncorrelated with divorce outcomes other than through its effect on financial wellbeing, and that future household income is not influenced by the sex of the first child other than through the propensity to divorce. For example, Lundberg and Rose (2002) find that sons increase men's labor supply, but they argue that this may be operating through a decreased propensity to divorce increasing the returns to marital-specific investments, including specialization, so this effect would not violate these assumptions.

V. Results

We present the structural parameters of interest in Table 6. The four specifications vary in whether we imposed the restriction $\varphi = 0$, and whether we imposed a floor on the log income/needs ratio. Estimates for the effect of hardship on the propensity to divorce (δ) are difficult to interpret because they are effectively measured in utils, but since they are near 0 in all specifications we simply report their p-values. Since the results are substantially similar within columns (apart from differences that follow immediately from the restriction $\varphi = 0$), we shall discuss only the first set of estimates. Tables 7-10 present reduced form estimates of each specification.

The only structural parameter provided directly by this estimation procedure is θ, and we see from that estimate that the event of divorce produces on average a 0.32 standard deviation decrease in log income/needs ratio by the end of the period. The interpretation in this context is that in the absence of anticipatory behavior, the average divorced woman would see her household income/needs ratio fall to 55% of its previous level. This is roughly equivalent to the loss of a second earner. We should also note that this estimate contrasts with Bedard and Deschênes' (2005) conclusion that divorce raises women's incomes.

We proceed with our recovered structural parameters beginning with the effect of the propensity to divorce on hardship, which we have labeled φ. In the divorce equation our instrument for divorce, the sex of the first child, is strong with a chi-squared value of 11.29. Our estimated value of φ of 0.642 is significant at the 10% level. For the purposes of interpretation, consider a woman in our sample at the baseline probability of divorce of 0.22 who got married at the age of 24 and an otherwise comparable woman who got married at the age of 22. The otherwise comparable woman would have a probability of divorce of 0.25. Based upon our estimates, we would expect her to engage in some anticipatory behavior which would increase her log income/needs ratio by 6.5% in response to this 13.7% increase in her risk of divorce, which is a modest response to a worrisome increase in divorce risk.

Similarly, the additional divorce risk required to induce an anticipatory response large enough to offset the negative effect of divorce is gigantic. For example, a near tripling of the divorce risk across the baseline, from a probability of 0.11 to a probability of 0.31, would be sufficient to almost fully compensate for the event of divorce. Needless to say, there is no intuitive example that can be drawn from our parameter estimates which would correspond to so large an increase.

Bedard and Deschênes (2005) attribute their IV estimates to anticipatory labor supply responses, and our estimated anticipatory effect appears to be operating through this same channel. Figure 4 plots residuals of an OLS regression of end of period hours worked on ASVAB scores, race dummies, education, ASVAB scores, and beginning of period hours worked on the vertical axis against predicted probabilities of divorce on the horizontal axis. As we can see from the figure, the residual change in hours worked is slightly increasing in the predicted probability divorce, again suggesting a modest labor supply response.

Much of the anticipatory response may instead take the form of occupational upgrading. Table 11 presents the most common beginning of period and end of period occupational categories for women with below median and above median predicted probabilities of divorce. We observe women with low predicted probabilities of divorce moving out of the labor force, whereas those with high probabilities of divorce are entering the labor force and moving from part-time occupations into more full-time roles. This suggests that the anticipatory response consist of more than simply increasing how many hours are being worked; women with high divorce probabilities are also more likely to move to higher-paying jobs.

Our instrument for log income/needs ratio is semi-weak in the hardship equation with a chi-squared value of 5.54, but it is not at all significant in the divorce equation. More than insignificant, the marginal effect of a full point increase in the local unemployment rate is a decrease in the probability of divorce by 0.26 percentage points, or roughly 1.2% of the baseline probability of divorce in the period, and we can rule out any effect larger than 4.3% of the baseline. Consequently, we conclude that the estimated value for δ, the causal effect of financial hardship on divorce, is effectively 0. Since this is a local average treatment effect, we can infer from this result that financial strain from variation in local economic conditions does not increase the risk of divorce. This finding is consistent with that of Charles and Stephens (2004), who find that job losses due to an exogenous factor, in their case plant closings, have no effect on the probability of divorce.

Finally, we are interested to know the correlation between the structural errors. Proceeding under the assumption $\hat{\delta} = 0$, as we will for the remainder of our study, we calculate

$$\frac{\sigma_{HD}}{\sigma_H \sigma_D} = \frac{\hat{\Sigma}_{12} - \hat{\varphi}}{\sqrt{\hat{\Sigma}_{22} - 2\hat{\varphi}\hat{\Sigma}_{12} + \hat{\varphi}^2}} \approx -0.366$$

This suggests the presence of some unobserved factor which produces both divorce and financial hardship, the presence of which was masked by anticipatory behavior. While we can only speculate on the source of this negative selection, one can certainly imagine any number of stories in which personality traits which are not rewarded on the labor market would be also maladaptive in a marriage. However, we can exclude from the list of potential sources any story which involves narcissism or external loci of control, as we have included controls for these traits.

VI. Policy Extension

The above findings invite a number of policy questions. First, Voena (2015) finds that the institution of unilateral divorce laws in states where property is divided according to a community property rule, which is where all assets are divided evenly between separating spouses, is associated with increased asset accumulation and lower female labor force participation. These differences are respectively attributed to husbands insuring against the expected loss of assets, and wives receiving an increased share of household resources due to changes in intra-household bargaining from the introduction of unilateral divorce.

We would be interested to know if women also purchase this insurance, demonstrating a stronger anticipatory response in community property states to increases in divorce risk than in non-community property states, and if women who cannot be insured against this risk (either through their own actions or those of their husbands) experience worse divorce outcomes. To examine this question, we proceed as above, stratifying the sample by divorce regime. We estimate separate regressions for community property states,[14] unilateral divorce states that do not operate under a community property regime, and mutual consent divorce states that do not

[14] Except for Louisiana, all states with community property laws also allow unilateral divorce.

operate under a community property regime. These categories are assigned at their value in year of the woman's marriage according to Online Appendix F in Voena (2015). Table 12 presents the structural parameters of interest from these regressions.

While not particularly robust to the presence of outliers, we can conclude from these results that women in non-community property states that allow unilateral divorce are much more negatively affected by divorce than women in mutual consent or community property states. It also appears as though those women positively select into divorce, and although our point estimate of the effect of propensity to divorce is near 0, we cannot rule out the possibility that this estimated positive selection may be driven by meaningful levels of anticipatory behavior due to imprecision in these estimates. Thus, while we can conclude from these results that many of the divorced women in our sample who suffer the most negative financial outcomes reside in states with unilateral divorce but without equal division of property, we unfortunately lack the statistical power to explore this difference further.

Another policy question of interest is whether government transfer programs are effective in offsetting the hardship of divorce. To explore this question, Table 13 presents our structural parameters of interest for regressions using four different dependent variables: the first is our log income/needs ratio from above, the second is the log income/needs ratio with transfer payments included, the third uses log total transfer income, and the fourth uses log Aid to Families with Dependent Children (AFDC) or Temporary Assistance for Needy Families (TANF) payments. We have also attempted these estimates with the restriction $\varphi = 0$; however, as in every other case presented here, the only substantive difference is the interpretation of the structural error correlation term, which again would be effectively 0 under this restriction. The full reduced form estimates using the latter three dependent variables are presented in Tables 14-16.

From these results, it is clear that transfer programs, mainly AFDC and TANF, reduce the overall hardship from divorce, and that the anticipatory labor supply response may decrease reliance on public assistance. Furthermore, we do observe some evidence of selection, that those who are more likely to divorce tend to receive higher levels of public assistance overall, but not necessarily higher levels of AFDC. However, we cannot conclude without first identifying the source of the selection effect whether this selection effect between divorce and transfer payments is related to the previously identified selection effect between divorce and financial hardship.

VII. Conclusion

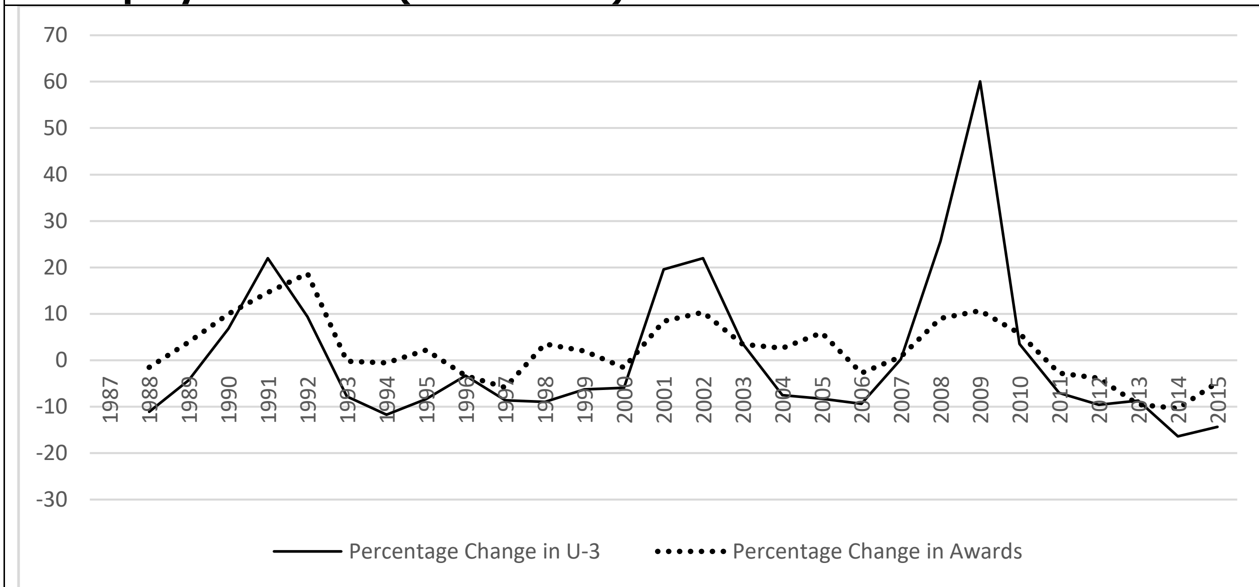
We set out to examine the direction of causality in the well-documented correlation between financial hardship and divorce. We found that financial hardship as defined as household income adjusted for the needs of a household of that size has no causal effect on the probability of divorce. We also find that divorce imposes a financial hardship on women in the form of a 0.32 standard deviation decrease in their income/needs ratio, and that after correcting for the effect of anticipatory behavior, which appears to take the form of a modest labor supply response, we find some evidence of negative selection on unobservables.

In our policy extension, we found that women in states with unilateral divorce but without equal division of property tend to experience much more negative divorce outcomes than women in other states. Additionally, we found that government transfer programs, in particular Temporary Assistance for Needy Families, are effective in alleviating the hardship from divorce, and that the anticipatory labor supply response to elevated divorce risk may reduce reliance on transfer programs.

These findings suggest a few avenues in need of future research. First, while we have concluded that there is no causal effect of financial hardship as defined by income, we have not examined the possibility that there is a causal relationship between profligacy and divorce as some of the literature suggests. Secondly, while we have identified some selection on unobservables, we are only able to exclude stories based on locus of control and self-esteem, and were unable to discern the nature of the relationship, if any, between the selection effects in our log income/needs ratio estimates and those in our transfer income estimates. Finally, while we have examined a few policy questions, we were unable to fully examine the relationship between divorce policy regime and the effect of divorce on women's wellbeing

FIGURES

Figure 1. Percentage Changes in SSDI Awards and Average Annual Unemployment Rate (1987-2015)



Sources: Bureau of Labor Statistics; Social Security Administration, Annual Statistical Report on the Social Security Disability Insurance Program (2015)

Figure 2. PDF of Divorce

Figure 3. Histogram of OLS Hardship Residuals

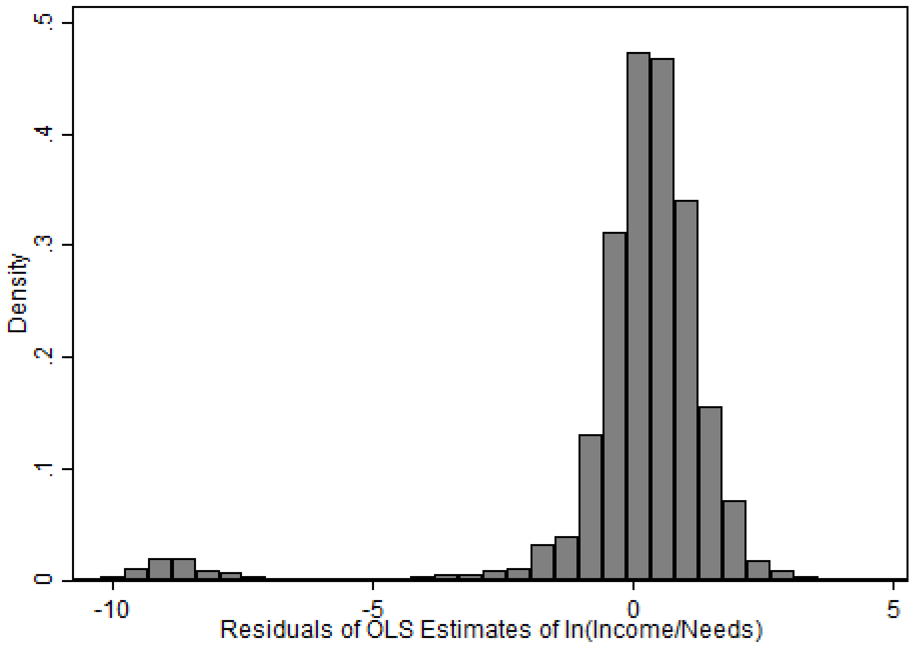
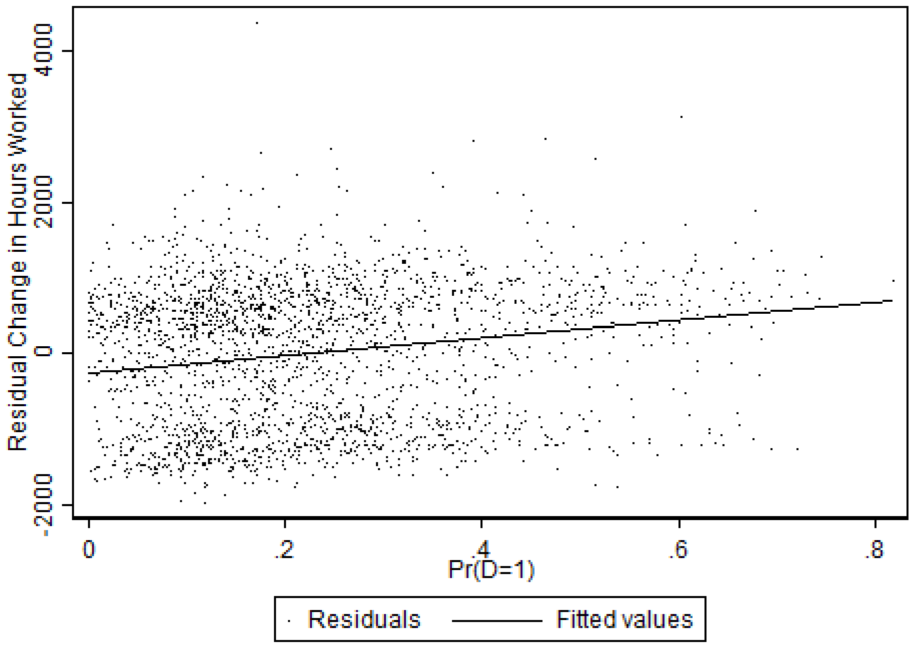


Figure 4. Plot of Residual Change in Hours Worked



TABLES

Table 1. Decomposition of Change in SSDI Enrollment Rate, 2006-2008 to 2009-2012

	Change in composition	Change in rates	Total
Total	0.066	0.096	0.162
Labor force participants	-0.037	0.041	0.004
Unemployed	0.030	0.017	0.047
Previously Disabled	0.005	0.004	0.009
Exited labor force	0.003	-0.005	-0.002
Did not exit	0.002	0.009	0.011
Non-disabled	0.025	0.013	0.038
Exited labor force	0.006	0.006	0.012
Did not exit	0.019	0.007	0.026
No unemployment	-0.066	0.024	-0.042
Previously Disabled	-0.039	0.015	-0.011
Exited labor force	-0.023	2.2×10^{-4}	-0.023
Did not exit	-0.016	0.014	0.002
Non-disabled	-0.027	0.010	-0.017
Exited labor force	-0.006	0.003	-0.003
Did not exit	-0.022	0.007	-0.015
Labor force non-participants	0.103	0.055	0.158
Previously Disabled	0.046	0.044	0.090
Non-disabled	0.057	0.011	0.068

Values presented are percentage point differences rounded to three decimal places, and have been tabulated using person weights.

Table 2. Decomposition of Change in SSDI Enrollment Rate by Income and Education

	100% of Poverty Level	200% of Poverty Level	High School Dropout
Total	0.164	0.167	0.167
Low income	0.029	0.058	-0.006
Change in Composition	0.061	0.105	-0.010
Change in Rates	-0.032	-0.048	0.004
Higher Income	0.135	0.110	0.173
Change in composition	0.021	-0.017	0.079
Change in Rates	0.114	0.126	0.094

Values presented are percentage point differences rounded to three decimal places, and have been tabulated using person weights. The discrepancy in the thousandths of a percentage point in the totals is due to rounding error.

Table 3. Probit Estimates of Divorce				
Variable	**(1)**	**(2)**	**(3)**	**(4)**
6-Year Interval				
Age	-0.036* (0.019)	-0.025 (0.020)	-0.37* (0.020)	-0.036* (0.019)
Education	-0.035 (0.023)	-0.032 (0.023)	0.007 (0.025)	-0.037 (0.023)
White	0.163* (0.091)	0.160* (0.091)	0.140 (0.091)	0.258** (0.109)
Female First Child	0.121 (0.085)	1.173** (0.488)	1.942*** (0.471)	0.338*** (0.129)
Age*Female First Child		-0.047** (0.022)		
Education*Female First Child			-0.143*** (0.036)	
White*Female First Child				-0.258* (0.144)
X^2	2.01	6.82	17.35	7.63
n	2,598	2,598	2,598	2,598
8-Year Interval				
Age	-0.049*** (0.019)	-0.039** (0.020)	-0.049*** (0.019)	-0.049*** (0.019)
Education	-0.047** (0.023)	-0.044* (0.023)	-0.016 (0.026)	-0.048** (0.023)
White	0.192** (0.086)	0.190** (0.086)	0.179** (0.086)	0.267** (0.106)
Female First Child	0.235*** (0.081)	1.015** (0.440)	1.350*** (0.454)	0.394*** (0.121)
Age*Female First Child		-0.035* (0.019)		
Education*Female First Child			-0.087** (0.035)	
White*Female First Child				-0.187 (0.139)
X^2	8.40	11.50	14.14	13.92
n	2,442	2,442	2,442	2,442

*,**, and *** denote significance at the 10%, 5%, and 1% level respectively. Standard errors are in parentheses. X^2 in the first column reports the individual X^2 statistic for Female First Child, and in columns 2-4 reports the joint significance of Female First Child and the interaction term. All regressions are weighted using sampling weights, and include the additional controls discussed in section III.

Table 4. OLS Estimates of Hardship

Variable	(1)	(2)	(3)	(4)
6-Year Interval				
Age	-0.019 (0.016)	-0.059** (0.026)	-0.019 (0.016)	-0.019 (0.016)
Education	0.137*** (0.016)	0.136*** (0.016)	0.039 (0.041)	0.137*** (0.016)
White	0.423*** (0.090)	0.441*** (0.089)	(0.434*** (0.090)	0.421** (0.202)
Average Unemployment	-0.048*** (0.017)	-0.187** (0.091)	-0.240*** (0.085)	-0.048** (0.023)
Age*Average Unemployment		0.006* (0.003)		
Education*Average Unemployment			0.015*** (0.006)	
White*Average Unemployment				0.0003 (0.030)
F	7.90	4.12	4.68	4.85
n	2,598	2,598	2,598	2,598
8-Year Interval				
Age	-0.015 (0.020)	-0.055* (0.031)	-0.015 (0.020)	0.015 (0.20)
Education	0.162*** (0.028)	0.163*** (0.028)	0.069 (0.064)	0.162*** (0.028)
White	0.502*** (0.114)	0.520*** (0.113)	0.508*** (0.113)	0.592** (0.234)
Average Unemployment	-0.029* (0.016)	-0.172** (0.087)	-0.218** (0.108)	-0.020 (0.024)
Age*Average Unemployment		0.006* (0.004)		
Education*Average Unemployment			0.015* (0.008)	
White*Average Unemployment				-0.012 (0.030)
F	3.16	2.60	2.32	1.58
n	2,442	2,442	2,442	2,442

*,**, and *** denote significance at the 10%, 5%, and 1% level respectively. Standard errors are in parentheses. *F* in the first column reports the individual F-statistic for Average Unemployment, and in columns 2-4 reports the joint significance of Average Unemployment and the interaction term. All regressions are weighted using sampling weights, and include the additional controls discussed in section III.

Table 5. Select Summary Statistics		
Variable	**6-Year Interval**	**8-Year Interval**
	Mean (Standard deviation)	
ln(Income/Needs)	0.675 (1.922)	0.881 (1.840)
Age at First Marriage	23.909 (5.061)	24.266 (5.207)
Family Size	2.740 (1.374)	2.518 (1.120)
ASVAB	46.195 (28.161)	53.857 (27.356)
Years of Education	12.958 (2.257)	13.283 (2.267)
Hours Worked in Past Year	1372.59 (856.845)	1479.56 (821.704)
Tenure at Current Job in Weeks	91.429 (135.858)	102.910 (148.605)
Weeks Worked by Husband in Past Calendar Year	45.763 (12.488)	46.104 (11.968)
Average Unemployment Rate	7.423 (2.900)	6.927 (2.585)
	Proportion	
Pr(Divorce)	0.183	0.222
Pr(Has Children)	0.720	0.764
Pr(Has Girl | Has Children)	0.512	0.522
White	0.624	0.843
Black	0.214	0.103
Hispanic	0.162	0.054
n	2,598	2,442

Table 6. Structural Parameters of Interest				
Variable	**No Floor, $\boldsymbol{\varphi \neq 0}$**	**No Floor, $\boldsymbol{\varphi = 0}$**	**With Floor, $\boldsymbol{\varphi \neq 0}$**	**With Floor, $\boldsymbol{\varphi = 0}$**
Direct Effect of Divorce: θ	-0.590** (0.240)	-0.504** (0.221)	-0.463*** (0.179)	-0.398** (0.166)
Anticipatory Effect: φ	0.642* (0.373)	0	0.426* (0.259)	0
Effect of Hardship on Divorce: δ	$p = 0.476$	$p = 0.466$	$p = 0.460$	$p = 0.447$
Structural Error Correlation: ρ	-0.366* (0.188)	-0.023 (0.074)	-0.357* (0.188)	-0.036 (0.081)
Log-likelihood	-5772	-5774	-4901	-4903

*,**, and *** denote significance at the 10%, 5%, and 1% level respectively. Standard errors are in parentheses.

Table 7. Reduced Form Estimates of $\varphi \neq 0$ Specification with No Floor		
Variable	**Divorce**	**ln(Income/Needs)**
Constant	13.102 (30.918)	38.260 (32.833)
Divorce	--	-0.590** (0.240)
Age at First Marriage	-0.049*** (0.016)	-0.022 (0.017)
Husband Age	-0.008** (0.004)	0.003 (0.004)
Black	-0.140 (0.113)	-0.696*** (0.124)
Hispanic	-0.268* (0.142)	-0.254 (0.156)
Family Size	-0.013 (0.029)	-0.066** (0.033)
ASVAB	-0.002 (0.002)	0.006*** (0.002)
Education	-0.048** (0.020)	0.157*** (0.021)
Rotter Score	0.007 (0.014)	0.004 (0.015)
Rosenberg Self-Esteem Scale	-0.010 (0.008)	0.029*** (0.009)
Husband Weeks Worked	0.003 (0.003)	0.010*** (0.003)
Hours Worked	2.97E-5 (4.42E-5)	4.20E-4*** (4.72E-5)
Weeks of Tenure	-0.001*** (3.60E-4)	1.45E-4 (2.75E-4)
Has Child	-0.913*** (0.080)	-0.645*** (0.109)
First Child Sex	0.235*** (0.070)	0.151** (0.075)
Average Unemployment Rate	-0.010 (0.013)	-0.034** (0.015)
Year	Yes	Yes
Region	Yes	Yes
$\hat{\Sigma}_{12}$	0.006 (0.131)	
$\sqrt{\hat{\Sigma}_{22}}$	1.616*** (0.023)	

*,**, and *** denote significance at the 10%, 5%, and 1% level respectively. Standard errors are in parentheses. Regression is weighted using sampling weights.

Table 8. Reduced Form Estimates of $\varphi = 0$ Specification with No Floor		
Variable	**Divorce**	**ln(Income/Needs)**
Constant	13.303 (30.914)	38.292 (32.864)
Divorce	--	-0.504** (0.221)
Age at First Marriage	-0.049*** (0.016)	-0.021 (0.017)
Husband Age	-0.008** (0.004)	0.003 (0.004)
Black	-0.140 (0.113)	-0.697*** (0.124)
Hispanic	-0.268* (0.142)	-0.255 (0.156)
Family Size	-0.013 (0.029)	-0.065** (0.033)
ASVAB	-0.002 (0.002)	0.006*** (0.002)
Education	-0.048** (0.020)	0.158*** (0.021)
Rotter Score	0.007 (0.014)	0.004 (0.015)
Rosenberg Self-Esteem Scale	-0.010 (0.008)	0.029*** (0.009)
Husband Weeks Worked	0.003 (0.003)	0.010*** (0.003)
Hours Worked	2.98E-5 (4.42E-5)	4.22E-4*** (4.73E-5)
Weeks of Tenure	-0.001*** (3.60E-4)	1.52E-4 (2.74E-4)
Has Child	-0.915*** (0.080)	-0.552*** (0.097)
First Child Sex	0.237*** (0.070)	--
Average Unemployment Rate	-0.010 (0.013)	-0.034** (0.015)
Year	Yes	Yes
Region	Yes	Yes
$\hat{\Sigma}_{12}$	-0.037 (0.120)	
$\sqrt{\hat{\Sigma}_{22}}$	1.618*** (0.023)	
*,**, and *** denote significance at the 10%, 5%, and 1% level respectively. Standard errors are in parentheses. Regression is weighted using sampling weights.		

Table 9. Reduced Form Estimates of $\varphi \neq 0$ Specification with Floor

Variable	Divorce	ln(Income/Needs)
Constant	13.160 (30.912)	32.945 (22.983)
Divorce	--	-0.463*** (0.179)
Age at First Marriage	-0.049*** (0.016)	-0.012 (0.012)
Husband Age	-0.008** (0.004)	0.004 (0.003)
Black	-0.140 (0.113)	-0.505*** (0.087)
Hispanic	-0.268* (0.142)	-0.160 (0.109)
Family Size	-0.013 (0.029)	-0.053** (0.023)
ASVAB	-0.002 (0.002)	0.005*** (0.001)
Education	-0.048** (0.020)	0.126*** (0.014)
Rotter Score	0.007 (0.014)	0.003 (0.011)
Rosenberg Self-Esteem Scale	-0.010 (0.008)	0.026*** (0.006)
Husband Weeks Worked	0.003 (0.003)	0.008*** (0.002)
Hours Worked	2.97E-5 (4.42E-5)	3.11E-4*** (3.31E-5)
Weeks of Tenure	-0.001*** (3.61E-4)	7.79E-5 (1.93E-4)
Has Child	-0.914*** (0.080)	-0.537*** (0.078)
First Child Sex	0.235*** (0.070)	0.100* (0.053)
Average Unemployment Rate	-0.010 (0.014)	-0.036*** (0.010)
Year	Yes	Yes
Region	Yes	Yes
$\hat{\Sigma}_{12}$	-0.007 (0.099)	
$\sqrt{\hat{\Sigma}_{22}}$	1.131*** (0.016)	

*,**, and *** denote significance at the 10%, 5%, and 1% level respectively. Standard errors are in parentheses. Regression is weighted using sampling weights.

Table 10. Reduced Form Estimates of $\varphi = 0$ Specification with Floor

Variable	Divorce	ln(Income/Needs)
Constant	13.256 (30.908)	32.951 (23.007)
Divorce	--	-0.398** (0.166)
Age at First Marriage	-0.049*** (0.016)	-0.011 (0.012)
Husband Age	-0.008** (0.004)	0.004 (0.003)
Black	-0.140 (0.113)	-0.506*** (0.087)
Hispanic	-0.268* (0.142)	-0.160 (0.109)
Family Size	-0.013 (0.029)	-0.052** (0.023)
ASVAB	-0.002 (0.002)	0.005*** (0.001)
Education	-0.048** (0.020)	0.127*** (0.014)
Rotter Score	0.007 (0.014)	0.003 (0.011)
Rosenberg Self-Esteem Scale	-0.010 (0.008)	0.026*** (0.006)
Husband Weeks Worked	0.003 (0.003)	0.008*** (0.002)
Hours Worked	2.98E-5 (4.42E-5)	3.12E-4*** (3.31E-5)
Weeks of Tenure	-0.001*** (3.60E-4)	8.43E-5 (1.93E-4)
Has Child	-0.916*** (0.080)	-0.474*** (0.070)
First Child Sex	0.238 (0.070)	--
Average Unemployment Rate	-0.011 (0.014)	-0.035*** (0.010)
Year	Yes	Yes
Region	Yes	Yes
$\hat{\Sigma}_{12}$	-0.041 (0.091)	
$\sqrt{\hat{\Sigma}_{22}}$	1.132*** (0.016)	

*,**, and *** denote significance at the 10%, 5%, and 1% level respectively. Standard errors are in parentheses. Regression is weighted using sampling weights.

Table 11. Occupation Categories by Divorce Probability		
Occupation Category	**Start of Period Proportions**	**End of Period Proportions**
Below Average Predicted Probability of Divorce		
None	0.120	0.214
Management	0.064	0.098
Education	0.068	0.074
Food Preparation and Service	0.027	0.031
Sales	0.074	0.055
Office Support	0.258	0.169
Production	0.058	0.041
Above Average Predicted Probability of Divorce		
None	0.290	0.239
Management	0.041	0.071
Education	0.020	0.030
Food Preparation and Service	0.116	0.058
Sales	0.119	0.074
Office Support	0.167	0.203
Production	0.064	0.071

Table 12. Structural Parameters of Interest by Divorce Regime

Variable	Community Property	Community Property with Floor	Unilateral, Non-Community Property	Unilateral, Non-Community Property with Floor	Mutual Consent, Non-Community Property	Mutual Consent, Non-Community Property with Floor
$\varphi \neq 0$						
Direct Effect of Divorce: θ	-0.712 (0.566)	-0.465 (0.366)	-2.280*** (0.333)	-1.419*** (0.424)	-0.261 (0.305)	-0.209 (0.250)
Anticipatory Effect: φ	1.593 (1.763)	1.064 (1.120)	0.180 (0.447)	-0.062 (0.298)	1.414 (0.899)	1.083 (0.691)
Structural Error Correlation: ρ	-0.681 (0.424)	-0.678 (0.424)	0.422* (0.220)	0.442** (0.223)	-0.708*** (0.216)	-0.724*** (0.207)
Log-likelihood	-1574	-1326	-1970	-1631	-2115	-1842
$\varphi = 0$						
Direct Effect of Divorce: θ	-0.584 (0.549)	-0.386 (0.362)	-2.257*** (0.336)	-1.448*** (0.363)	-0.129 (0.281)	-0.086 (0.228)
Anticipatory Effect: φ	0	0	0	0	0	0
Structural Error Correlation: ρ	-0.005 (0.187)	-0.017 (0.180)	0.497*** (0.092)	0.412** (0.167)	-0.082 (0.097)	-0.118 (0.107)
Log-likelihood	-1575	-1327	-1970	-1632	-2119	-1845
n	654		821		944	

*,**, and *** denote significance at the 10%, 5%, and 1% level respectively. Standard errors are in parentheses.

Table 13. Transfer Income Structural Parameters of Interest				
Variable	**Log Income/Needs**	**Log Income/Needs with Transfers**	**Log Transfer Income**	**Log AFDC/TANF Income**
Direct Effect of Divorce: θ	-0.590**	-0.332	0.384	0.842***
	(0.240)	(0.409)	(0.331)	(0.162)
Anticipatory Effect: φ	0.642*	0.355	-0.914	-0.189
	(0.373)	(0.270)	(0.580)	(0.315)
Structural Error Correlation: ρ	-0.366*	-0.273	0.380**	0.123
	(0.188)	(0.218)	(0.180)	(0.197)
Log-likelihood	-5772	-5003	-6895	-5699

*,**, and *** denote significance at the 10%, 5%, and 1% level respectively. Standard errors are in parentheses.

Table 14. Reduced Form Estimates with Log Income/Needs with Transfers		
Variable	**Divorce**	**ln(Income/Needs)**
Constant	9.765 (30.602)	38.203 (23.967)
Divorce	--	-0.332 (0.409)
Age at First Marriage	-0.049*** (0.016)	-0.003 (0.014)
Husband Age	-0.008** (0.004)	1.08E-4 (0.003)
Black	-0.140 (0.113)	-0.436*** (0.092)
Hispanic	-0.279** (0.141)	-0.119 (0.118)
Family Size	-0.013 (0.029)	-0.023 (0.024)
ASVAB	-0.002 (0.002)	0.004*** (0.001)
Education	-0.048** (0.020)	0.130*** (0.016)
Rotter Score	0.007 (0.014)	0.015 (0.011)
Rosenberg Self-Esteem Scale	-0.010 (0.008)	0.024*** (0.007)
Husband Weeks Worked	0.004 (0.003)	0.005** (0.002)
Hours Worked	3.49E-5 (4.39E-5)	2.37E-4*** (3.45E-5)
Weeks of Tenure	-0.001*** (3.66E-4)	2.41E-5 (2.10E-4)
Has Child	-0.914*** (0.080)	-0.432*** (0.126)
First Child Sex	0.235*** (0.070)	0.083 (0.059)
Average Unemployment Rate	--	-0.018* (0.011)
Year	Yes	Yes
Region	Yes	Yes
$\hat{\Sigma}_{12}$	0.021 (0.236)	
$\sqrt{\hat{\Sigma}_{22}}$	1.180*** (0.017)	
*,**, and *** denote significance at the 10%, 5%, and 1% level respectively. Standard errors are in parentheses. Regression is weighted using sampling weights.		

Table 15. Reduced Form Estimates with Log Transfer Income		
Variable	**Divorce**	**ln(Transfer Income)**
Constant	9.932 (30.593)	-55.752 (52.031)
Divorce	--	0.384 (0.331)
Age at First Marriage	-0.050*** (0.016)	-0.004 (0.027)
Husband Age	-0.008** (0.004)	0.003 (0.007)
Black	-0.139 (0.113)	0.246 (0.197)
Hispanic	-0.276* (0.141)	0.183 (0.246)
Family Size	-0.014 (0.029)	0.140*** (0.052)
ASVAB	-0.002 (0.002)	-0.010*** (0.003)
Education	-0.047** (0.020)	-0.139*** (0.032)
Rotter Score	0.007 (0.014)	-0.004 (0.024)
Rosenberg Self-Esteem Scale	-0.010 (0.008)	-0.024* (0.014)
Husband Weeks Worked	0.004 (0.003)	-0.016*** (0.004)
Hours Worked	3.42E-5 (4.37E-5)	-3.24E-4* (7.48E-5)
Weeks of Tenure	-0.001*** (3.60E-4)	-5.54E-4 (4.35E-4)
Has Child	-0.917*** (0.080)	0.394** (0.166)
First Child Sex	0.237*** (0.070)	-0.217* (0.119)
Average Unemployment Rate	--	0.056** (0.023)
Year	Yes	Yes
Region	Yes	Yes
$\hat{\Sigma}_{12}$	0.138 (0.177)	
$\sqrt{\hat{\Sigma}_{22}}$	2.561*** (0.037)	
*,**, and *** denote significance at the 10%, 5%, and 1% level respectively. Standard errors are in parentheses. Regression is weighted using sampling weights.		

Table 16. Reduced Form Estimates with Log AFDC Income		
Variable	**Divorce**	**ln(AFDC)**
Constant	9.862 (30.588)	-10.220 (31.866)
Divorce	--	0.842*** (0.162)
Age at First Marriage	-0.049*** (0.016)	0.025 (0.017)
Husband Age	-0.008** (0.004)	-1.53E-4 (0.004)
Black	-0.140 (0.113)	0.172 (0.120)
Hispanic	-0.279** (0.141)	0.107 (0.150)
Family Size	-0.013 (0.029)	0.114*** (0.032)
ASVAB	-0.002 (0.002)	-0.004** (0.002)
Education	-0.048** (0.020)	-0.050** (0.020)
Rotter Score	0.007 (0.014)	0.011 (0.015)
Rosenberg Self-Esteem Scale	-0.010 (0.008)	-0.013 (0.009)
Husband Weeks Worked	0.004 (0.003)	-0.012*** (0.003)
Hours Worked	3.47E-5 (4.37E-5)	-2.58E-4*** (4.58E-5)
Weeks of Tenure	-0.001*** (3.60E-4)	-3.20E-4 (2.65E-4)
Has Child	-0.915*** (0.080)	0.467*** (0.096)
First Child Sex	0.235*** (0.070)	-0.044 (0.073)
Average Unemployment Rate	--	0.041*** (0.014)
Year	Yes	Yes
Region	Yes	Yes
$\hat{\Sigma}_{12}$	0.005 (0.082)	
$\sqrt{\hat{\Sigma}_{22}}$	1.569*** (0.022)	

*,**, and *** denote significance at the 10%, 5%, and 1% level respectively. Standard errors are in parentheses. Regression is weighted using sampling weights.

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Martin Erik Meder
CURRICULUM VITAE

Fields

Primary Fields:
- Labor and Demographic Economics
- Health, Education, and Welfare

Teaching Fields:
- Econometrics
- Environmental Economics

Education

Doctor of Philosophy in Economics — May 2018
University of Wisconsin – Milwaukee, Milwaukee, WI

Bachelor of Science, Summa Cum Laude — June 2014
University of Wisconsin – Oshkosh, Oshkosh, WI
Major: Economics   Minor: Mathematics

Papers and Research

Scott Drewianka and Martin E. Meder, "Simultaneity and Selection in Financial Hardship and Divorce." 2017. (Job Market Paper)

Martin E. Meder, "Decomposing Cyclical Fluctuations in Social Security Disability Insurance Awards." 2017. (Under Submission)

Marianne Johnson and Martin E. Meder, "The Effects of Zebra Mussel (*Dreissena Polymorpha*) Infestations on Property Values: Evidence from Waupaca County, Wisconsin, USA." 2014. (Undergraduate Thesis)

Marianne Johnson, Martin E. Meder, and David Schweikhardt, "Introduction to Notes from Warren J. Samuels's Course on the Economic Role of Government." 2012. *Research in the History of Economic Thought and Methodology* 30(B): 139-149

Marianne Johnson and Martin E. Meder, Eds., "Notes from Warren J. Samuels's 1996 Course on the Economic Role of Government." 2012. *Research in the History of Economic Thought and Methodology* 30(B): 151-265

Teaching Experience

Assistant Professor
Nicholls State University — August 2018
Instructor: Principles of Microeconomics, The Economic Environment of Business

Doctoral Teaching Assistant
University of Wisconsin – Milwaukee — August 2014 – May 2018
Instructor: Economics of Employment and Labor Relations, Environmental Economics, Principles of Microeconomics, Economics of Personal Finance
Teaching Assistant: Environmental Economics, Intermediate Microeconomics, Principles of Macroeconomics, Introductory Economics

Economics Tutor
University of Wisconsin – Oshkosh
College of Business — September 2012 - June 2014
Center for Academic Resources — October 2011 - September 2012
Tutor: Economic Statistics, Econometrics, Principles of Macroeconomics, Principles of Microeconomics, and Intermediate Microeconomics.

Technology Education Coordinator
Boys and Girls Clubs of the Fox Valley — October 2010 - December 2011
Instructor: STEM (Science, Technology, Engineering, and Mathematics) enrichment programs.

Presentations
"Unemployment and the Disabled in the Great Recession"
Midwest Economic Association, Cincinnati, OH, March 2017
UW – Milwaukee Microeconomics Workshop, Milwaukee, WI, March 2017

"Effects of Aquatic Invasive Species on Home Prices"
Midwest Economic Association, Evanston, IL, March 2014

Grants, Recognition, and Scholarships
Data Access Agreement, Bureau of Labor Statistics, 2016-2018
Richard Perlman Prize for Outstanding Paper in Labor Economics, UW – Milwaukee Department of Economics, 2017
Chancellor's Graduate Student Award, UW – Milwaukee, 2014 & 2017
Chancellor's Award for Excellence, UW – Oshkosh, 2014
Outstanding Graduate Award, UW – Oshkosh Department of Economics, 2014
Outstanding Research Award, UW – Oshkosh Department of Economics, 2013
Best New Economics Major, UW – Oshkosh Department of Economics, 2012
Student/Faculty Collaborative Research Grant, UW – Oshkosh, 2012
Outstanding Contribution Award, Boys & Girls Club of the Fox Valley, 2011

Professional Activities
Discussant:
Session: "Labor Force Participation," Midwest Economic Association, Cincinnati, March 2017
Referee:
Journal of Economic Behavior & Organization

Professional Associations
American Economic Association, Midwest Economics Association, Society of Labor Economists

Media Interviews
Milwaukee Journal Sentinel, Post-Crescent (Appleton), Fox 11 News (Green Bay)

References
Dr. Scott Drewianka — Dr. Scott Adams — Dr. John Heywood