Measuring the Response of the Private Safety Net to Job Separation

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Abstract

The private safety net, which consists of assistance provided to individuals from friends and family members in times of need, is difficult to measure. This is partly due to the wide range of financial and in-kind transfers that comprise the private safety net and the difficulty in identifying these transfers in existing data sets. Yet, the private safety net could play an important role in consumption smoothing, alter the incentives to accumulate precautionary savings, influence labor force participation, and change the take up rate and effectiveness of the public safety net. In this paper, I use longitudinal data from the Panel Study of Income Dynamics (PSID) to examine one specific form of the private safety net in one circumstance—the response of cash transfers from family members after a job separation. The results help establish the existence of a safety net, and begin the characterization of how sensitive it is to the size of the income shock and the generosity of public benefits.

Keywords: private safety net, intergenerational transfers, unemployment insurance

JEL: D1, H1, J65

I. Motivation

The private safety net consists of monetary and non-monetary assistance provided to individuals from friends and family members during periods of need. It is difficult to say more in detail what comprises the private safety net or what instigates its use, because it would require measuring a range of assistance that does not exist in most data sets. From paying a utility bill or rent, moving in with someone, buying groceries, giving food, to watching children, borrowing a car, or sending cash—the private set often mirrors the aims of the public safety net, but understandably lacks the formality of application and often any documentation of receipt.

For these reasons, the size, composition, and nature of the private safety net is not well documented in the economics literature and, hence, it is not known if and to what extent it could influence consumption smoothing, health outcomes, labor force participation, risk taking, or precautionary savings—behavior notably altered by the public safety net.[1] The effect could be quite large. Ignoring the value of residence, in-kind giving, time transfers, and monetary gifts from other family members, McGarry (2012) updates findings from Gale and Scholz (1994) and estimates that the yearly flow of monetary assistance from parents to non-resident children to be $65 billion in 2010 dollars. For comparison, that is roughly the same amount spent on Supplemental Nutrition Assistance Program benefits in 2010 (Department of Agriculture, 2014).

If there is a private safety net operating parallel to the public safety net, there is a critical need for documentation of the former to understand how it interacts with the latter. For example, Meyer and Sullivan (2008) find that income of welfare beneficiaries was much more volatile than their consumption, but did not endeavor to explain why there was a gap between them. Mendenhall et al. (2012) interview Earned Income Tax Credit (EITC) beneficiaries in a qualitative study and find that nearly a fourth of refund receivers use part of their tax refund to pay back family obligations. If it can be shown that private cash and in-kind transfers smooth consumption for public safety net beneficiaries, then the private safety net can be thought of as filling the holes in a porous public safety net system. On the other hand, Weimers (2011) shows that co-residence, or doubling up with family members after previously living independently, increased after job loss, but Kaplan (2012) estimates that the access to co-resident arrangements was not universally available, and for

[1] The literature on the effect of public programs on behavior and outcomes is vast. Some examples include: Almond et al. (2011) and Hoynes and Schanzenbach (2009) discuss the health effects of food stamps; Bitler et al. (2004b), Bitler et al. (2004a), and Blank (2002) measure health, marriage, poverty, and labor force participation after welfare reform; Eissa and Liebman (1996), Eissa and Hoynes (2004), and Saez (2010) document changes to labor supply as a result of the Earned Income Tax Credit; and Feldstein (1974) and Leimer and Lesnoy (1982) debate the effect of Social Security on savings behavior.

those with that option, the insurance value of co-residence was quite high, altering the savings, job search, and risk taking behavior of individuals with that insurance, alluding to the varying liquidity constraints discussed in Cox (1990). If the private safety net distributes risk and insurance across the population based on parents' income, this could exacerbate existing inequality or challenge upward mobility.

In this paper, I attempt to document one type of public and private safety net interaction. I restrict my analysis to one form of the private safety net in one particular instance: an individual's receipt of cash transfers from non-resident family members after job separation. Using the Panel Study of Income Dynamics (PSID), I measure whether individuals transitioning to unemployment receive financial help from their family, and how this private safety net response interacts with available public safety net benefits. My empirical strategy consists of two parts. The first is a study of individuals and measures the response of cash transfers to unemployment, using the longitudinal nature of the PSID to control for individual fixed effects. This section is a necessary first step; it establishes that the cash transfers respond to job separation. The second is a study of unemployment spells and analyzes whether the private safety net in that form is sensitive to the potential unemployment insurance (UI) benefit, using the multigenerational aspect of the PSID to construct an extensive vector of covariates.

The paper proceeds as follows: in section II, I discuss the relevant literature for this analysis, especially the uses of the methodology. In section III, I discuss the data, the analytical sample, and summary statistics. Sections IV and V present the two empirical models, their results, robustness, and extensions. In Section VI, I discuss the results and follow-up studies instigated by my findings.

II. Literature

The study of financial transfers from family members has long been a staple in the economics literature, but not in the specific context of a private safety net. Earlier research focused on the role of intrafamily, inter-household transfers in the accumulation of aggregate wealth and savings. Kotlikoff and Summers (1981) and Kotlikoff (1988) use transfers to critique the life cycle model of savings (Ando and Modigliani (1963); Modigliani (1975); Modigliani (1988)), which predicts that savings and wealth are accumulated for retirement, and not to transfer between generations. The question of whether individuals save in order to give transfers, and if those transfers accumulate into wealth, predicates a larger debate of why transfers are given at all. Barro (1974) and Becker (1974)

offer an altruistic motive for financial transfers between family members; parents' utility is in part of function of child's utility. Many authors reject that characterization (Altonji et al. (1992); Altonji et al. (1996); Altonji et al. (1997); Cox (1987)) in favor of an exchange motive; parents give in order to later receive.[2]

A more detailed study of parental giving became available with the Health and Retirement Survey in 1993, a longitudinal panel that surveys the population aged 50 and over. The literature is still motivated by the debate of whether transfers are a product of altruism or exchange, and the authors' goal is to build a model of parental giving. However, the richer data set offers stylized facts about transfers that suggest that they function as a private safety net provided by parents to children. Unlike bequests, they are not given equally (McGarry, 1997) and are negatively correlated with the income of the receiver (McGarry and Schoeni, 1995). Indeed, after looking over a long time period, McGarry (2012) is able to establish the correlation between transfer giving and negative events experienced by the receiver, such as job separation or divorce. Summary of these and other findings on intergenerational transfers can be found in Hurd et al. (2011).

However, findings from the HRS are limited in two ways. First, financial transfers documented in the HRS are restricted to those given from parents to children, as opposed to other family members, and are conditional on having a living parent. Hence, the population of transfer receivers studied in McGarry and Schoeni (1995), McGarry (1997), and McGarry (2012) could be different from the total population of transfer receivers. Second, the HRS has little information about the children of the respondents, relative to other data sets, and thus has less detail about income, earnings, work history, and labor force participation of receivers.

In sum, prior literature on financial transfers between family members does not examine them in the context of the private safety net and the most reliable empirical findings are based on givers, which limits transfers to those originating from older parents and provides less insight on the situation of receivers. In this paper, I directly address transfers and their function as part of the private safety net, though prior work that models the altruism and exchange motives for giving help to motivate my study and will later corroborate some of my findings.

Private family transfers and their interaction with the public safety net is also less explored. Lampman and Smeeding (1983) posit that the public safety net should displace private giving, which they use to explore the gains to "secondary beneficiaries," family members who are no longer

[2] A discussion of the possible motives for transfers in addition to altruism and exchange is found in Bianchi et al. (2010)

the basis for support, between the 1930s and 1970s. Rosenzweig and Wolpin (1994) examine parental response to wage income increases versus public welfare increases. Cox and Jakubson (1995) evaluate the counterfactual poverty rate in a world with only private and no public safety net. In both of these studies, the crowd out of private transfers by public benefits is significant but small. The most relevant prior work is Schoeni (2000), which measures how private transfers respond to UI benefits. He also finds a small displacement of private transfers, but relies on a single year of transfer data during a period of economic expansion and cannot fully account for regular transfer giving. Indeed, these three more recent papers measure if private transfers decrease with public benefits without first incorporating how transfers originated—if they are permanent sources of support or if they are one-time payments in response to an income shock. This paper, on the other hand, first characterizes the nature of transfers in the instance of job separation, establishes that they are a private safety net, and then characterizes the private-public interaction.

As a final note, the relevance of my study—the private safety net in the form of transfers—is provided by the prior research of private safety net in the form of co-residence, or family members living together in times of need. Unlike transfers, co-residence has a long empirical literature, as discussed in Bianchi et al. (2010). Studies which measure the incidence of co-residence (Cohen and Casper, 2002) establish that privacy is a normal good, co-residence is more common among low-income households. Costa (1999), Engelhardt et al. (2005), McGarry and Schoeni (2000) find that public benefits can influence co-residence decisions, and higher benefits are associated with less co-residence. Recently, Weimers (2011) and Bitler and Hoynes (2013) study co-residence after job loss, the former establishing its function as part of the private safety net. Kaplan (2012) uses as a basis for motivation that co-residence in the private safety net is not a universal option—some individuals have the option to move in with parents after job loss, but others do not. He builds a model to measure the insurance value of that option and how it alters job search and savings behavior. One goal of the analysis presented in this paper is to build an empirical foundation for a model comparable to Kaplan's, but estimating the insurance value of transfers.

III. Data

Construction of Analytical Sample

The data used in this analysis are from the Panel Study of Income Dynamics (PSID), which is annual from 1968-1996 and biannual from 1997-2011. The PSID pulls from three main sample

groups—a nationally representative sample from 1968, an oversample of low-income populations in 1968, and an immigrant sample added in 1997—and follows over time every original sample member, their descendants, and any co-resident relatives.

As noted in the introduction, this paper uses two empirical strategies. The first is an analysis of individuals over time. The only requirement necessary to construct an appropriate analytical sample is limiting the PSID to working-age adults observed for at least two consecutive time periods. The survey offers information on each family member in the household at time of survey, as well as family summary variables, but detailed information about employment status, employment history, and receipt of financial transfers, is limited to the head of household. For this reason, I limit the sample to heads of household who are 25-54 years old and observed for two time periods, which creates an analytical sample of 17,268 individuals.[3]

The second empirical strategy, however, uses unemployment spells; the unit of observation is person-year, and any longitudinal data, outside of the construction of necessary wage variables, is ignored. This means that a richer set of covariates is needed to make up for the loss of individual fixed effects. Most importantly, given that the provider of cash transfers to adults is often their parents, I need to limit my sample to individuals at time of unemployment whose parents are also in the survey, in order to control for parents' characteristics. This leaves me with a sample of 2,086 unemployment spells.

Summary Statistics

Table 1 provides summary statistics. The means for the full analytical sample are presented in column (1). In column (2), I present sample means of individuals receiving transfers at the time of transfer receipt. Because individuals are observed for a long time period, certain characteristics, such as age or marital status, could change over the course of the survey. Measuring means at the time of transfer allows for both the individual characteristics of transfer recipients, as well situational characteristics. Column (3) summarizes non-receivers.

Individuals who report receiving a transfer are, at the time of receipt, younger than their non-receiving counterparts (35.6 years old compared to 39.4) and slightly more likely to be black (18.7%

[3]There are two questions in the PSID that ask for the amount of financial assistance the head of household received from relatives who live outside the household, one included in the 1968-1993 and 2005-2011 waves, the other included in the 1993-2011 waves. The PSID does ask about spousal receipt of transfers and their employment status. Including them in the analysis, however, is not straightforward, as they could adjust labor supply and transfers based on their spouse's labor supply (Cullen and Gruber, 2000) or transfer receipt, biasing any results. However, as it is a source of private safety net, spousal inclusion should be considered in further study.

to 14%) but, somewhat surprisingly, less likely to be Hispanic (3.8% compared to 5.2%). Transfer receivers are also more likely to be never married (34.1%) or divorced (32.4%) than individuals who do not report receiving a transfer (17.1% and 19.8%, respectively), and much less likely to be married (31.4% compared to 60.7%). It is perhaps a function of being younger and unmarried, on average, that fewer transfers receivers have children (49.2%, compared to 55.7%).

Educational attainment does not vary much between individuals who do and do not receive transfers. Individuals with less than a high school degree are comparable as a share of transfer receivers and not (12% and 12.9%), as are those with a high school degree (29.3% and 31.7%, or those with a bachelors or higher (26.3% and 24.8%); only those with some college are higher receivers, at 26.1% and 19.7%. These small differences are rather remarkable, if one is thinking of the juxtaposition of the public and private safety net. The public safety net, which is often means tested, has receipt concentrated amongst lower income and therefore less educated individuals, while the private safety net, at least as measured in this instance as cash transfers, does not.

Other than marriage rates, the largest difference between individuals at time of transfer and not is employment status. Although 74% of receivers are employed, 11.7% are unemployed and 14.9% are not in the labor force. Non-receivers have much higher employment (88.6%), much lower unemployment (4.9%) and higher labor force participation, with only 7% not in the labor force.[4] Lastly, and in a way as a proxy for being older, married, and employed, non-receivers are more likely to be home-owners (61.2%) rather than renters (34.1%), while for those individuals receiving transfers, the reverse is true, with 34.9% owning a home and 54.1% renting.

IV. Analysis of Individuals

Model and Results

To establish that the private safety net responds to job separation, I regress financial transfers on unemployment, controlling for individual fixed effects.

$$T_{it} = X_{it}\beta + \gamma U_{it} + \theta_i + \mu_t + \epsilon_{it} \tag{1}$$

T_{it} is a dummy for receipt of any financial transfer by the head i in time t from a relative who does not live in the household. Transfers notably do not include receipt of cash from former spouses.

[4]The relatively high rates of employment and labor force attachment is expected, as this is a sample of prime-age adults who are heads of household.

X_{it} is a vector of observable, non-constant characteristics, including marital status, presence of children in the family, and home-ownership that change from time $t-1$ to t. U_{it} is a dummy for unemployment status of head i in time t. An individual is unemployed if she is not working but actively seeking work. In addition, I impose that unemployment must be a transition state; for U to be equal to 1, an individual must be *becoming* unemployed, and hence she was employed in the prior time period $t-1$.

There are two general trends in family financial transfers that I also need to account for. First, transfers are decreasing in age of receiver. This was shown in Table 1, but also documented in previous study of givers (McGarry and Schoeni (1995); McGarry (1997); McGarry (2012); Bianchi et al. (2010)). Younger individuals are more likely to receive transfers, but younger workers are also more likely to have spells of unemployment. The model does not differentiate between young people who receive transfers *because* they are unemployed and young people who receive transfers and *happen to be* unemployed. To control for this, I include in X_{it} linear age dummies for each age 25 to 54. Second, the share of individuals receiving transfers has been increasing over time. The mean of 7.1% is not constant over the sample period; only 2.9% of heads of household reported transfers in 1968, which increased over time and peaked, in the sample, at 11.3% in 2009. It could be that transfers are increasing due to a broader trend, such as increased economic independence of the elderly, and this trend was concurrent with periods of higher unemployment or more unemployment spells. Hence, I include in the sample μ_t, year fixed effects. The results for this model are presented in Table 2.

The effect of becoming unemployed on the receipt of transfers is shown in column (1), with a coefficient of 0.025 and significant at the 1% level. A coefficient of 0.025 is equivalent to a 2.5 percentage-point increase in receipt of transfers. Mean transfer receipt in the sample is 7.1%. Together, this implies that transitioning to unemployment causes a 35% increase in transfer receipt. Private transfers from family also respond to other negative events, such as becoming divorced, at 0.021 (or 29.5% increase), but not to positive events, such as marriage (-0.019) or home-ownership (-0.007). Again, these variables measure transition to a state—becoming divorced, getting married, or becoming a home-owner. The entrance of children to the household is associated with a small increase (0.007). The coefficients shown in column (1) of Table 2 measuring influence of various events on transfer receipt are consistent with previous studies of means of parental giving.

Robustness

My primary concern in this analysis is capturing the timing of transfer giving. The use of individual fixed effects limits the variation to within-person; a regular receiver of transfers who only happens to be unemployed should not bias the results. But I also use transfer receipt in the periods before and after becoming unemployed to test if individuals were, or became, regular recipients around the time of unemployment. To start, in column (2), I regress unemployment in one period, t, on transfer receipt in the period prior, $t-1$. The results are an interesting reversal of those in column (1). Becoming unemployed makes transfer receipt less likely in the prior (employed) period, with a -0.018 coefficient. Individuals who become unemployed are not pre-empting unemployment with financial transfers from their family. But individuals who become married or buy a home appear to be doing exactly that. The coefficients for these variables are precise and positive, at 0.092 and 0.026, respectively. Divorce, like unemployment, has a negative coefficient, meaning transfers are less likely in the prior (married) period.

Column (3) combines the results of the prior two columns through a difference model, regressing becoming unemployed in t after being employed in $t-1$ on the difference in transfers over those periods, T_{it} - T_{it-1}. Hence, column (3) is the net effect of each variable, given transfers in the prior time period and prior state. Individuals who became unemployed were less likely receive transfers before unemployment and more likely to receive after unemployment, combining to a 0.043 coefficient, compared to -0.109 for marriage, 0.056 for divorce, and -0.033 for home purchasing. The private safety net, as measured through financial transfer, emerges here as a combination of prior investment for positive events, and post-hoc response to negative ones, including unemployment.

As an additional test of the persistence of transfer giving, I also regress unemployment in one period t on transfers in the following period $t+1$ in column (4). Transfers to unemployed individuals clearly do not persist, dropping in size and significance to 0.003. Becoming unemployed does not create a permanent transfer. The results presented in the first four columns of Table 2 establish that transfers to individuals who become unemployed are a time limited response that is coincidental with the transition from employment to unemployment, but does not precede or follow it.

A second concern in this analysis involves how unemployment is defined. In equation (1), the definition of U_{it} imposed the restriction that individuals were employed in $t-1$, noted as an E - U transition. This definition means that the coefficient γ is capturing the income shock, and implies that transfers are responding to the *change* in income. It could be the case, however, that

transfers in fact respond to the *level* of income, proxied by the state of being unemployed, and the transition is irrelevant. Hence my findings, which attribute transfers to the income shock and the transition to a low-income state, would be spurious—transfers are a feature of a low-income state. To test whether transfers are responding to the shock to income or to the level of income, I rerun equation (1) with a different definition of unemployment that is not associated with a change in income. Columns (5) - (8) replicate columns (1) - (4) for individuals who remain unemployed over two periods, or U - U instead of E - U.[5] That is I regress transfer receipt in period t on unemployment in t following unemployment in $t-1$. The results show that remaining unemployed over two periods makes transfers slightly less likely (-0.006) in period t, less likely in $t-1$, and near zero in $t+1$.

There are two additional robustness checks not included in Table 2. The PSID transitions from an annual survey to biannual survey in 1997, meaning that unemployment transitions are pulling from one year prior in some years and two years prior in others. To check this, even years were removed from the years prior to 1997, imposing biannual surveys through the length of the PSID, and the results were not changed, though with less significance due to a decreased sample size. Finally, there is a concern that knowledge of potential transfer receipt during unemployment alters the work, search, or unemployment behavior of individuals. That is, even if individuals are not receiving transfers before unemployment, as shown in column (5), they could have knowledge that they will receive transfers. Outside of using individual fixed effects, this concern cannot be answered within this empirical framework. Unemployment is assumed to be exogenous or, if endogenous, not differently so between those individuals who do or do not receive financial transfers during unemployment.

The goal of the the analysis performed in this section was to establish that the private safety net response to job separation exists, and importnatly, is limited to the period and transition of becoming unemployed. This was done by using within-person variation in unemployment and transfer receipt over a long period of time, and was precisely shown. Individuals are more likely to receive financial assistance from their family after job separation, equivalently demonstrating that the private safety net responds to negative income shocks.

[5]The remaining transition to unemployment, from not in the labor force, is not tested, as the transition takes on a range of possible situations—returning to work after child birth or health problem, finishing school, getting divorced, or even leaving incarceration, for instance—with less clear assumptions of income over that period. There is also the possibility of considering individuals on temporary layoff, meaning that they are not currently working but have a job to return to. The PSID includes temporary layoff as a unique category in the employment status question after 1976. Including temporary layoff as employed or unemployed does not change the results, as they are a very small group.

Interaction effects

Taking now as given that cash transfers from family members respond to job separation, the question remains of how that private safety net responds to public safety net benefits. I explore this by interacting unemployment with an individual's potential Unemployment Insurance (UI) benefit.

$$T_{ist} = X_{ist}\beta + \gamma U_{ist} + \pi U_{ist} * UI_{ist} + \theta_i + \mu_t + \lambda_s + \epsilon_{ist} \tag{2}$$

Again, T_{ist} is dummy for transfer receipt for head i in time t, now in state s, and U_{ist} a dummy for becoming unemployed in time period t. UI_{ist} is the replacement rate of potential unemployment insurance, which is an individual's potential UI weekly benefit amount as a share of their prior weekly wage.[6] The use of potential benefits—the total amount an individual is eligible for, rather than their reported benefit income—is key. It avoids any endogeneity in an individual's decision to claim benefits, allowing the identifying variation be the generosity of benefits, rather than take up. It also adds a layer to interpretation. The replacement rate, in addition to measuring the generosity of UI benefits, also corresponds inversely to the size of the income shock after job separation. As established in the prior section, the private safety responds to the shock, rather than the low income. Hence, it iss equivalent to say that π is the response to the depth of the income shock. Both interpretations are relevant, though I will focus on public benefit generosity.

Although technically a joint federal and state program, states have considerable independence over UI, both in maintaining their UI trust fund and determining how benefits are determined. States vary in the definition of the base period (the window used to examine eligibility) as well as the eligibility requirements within that base period, some relying on the length of time worked, some relying on the amount of money earned, and others using a combination of both. In addition to the eligibility cutoff, states also differ on how benefits are calculated, as well as maximum benefits, minimum benefits, dependent allowances, and length of benefits. Use of potential UI benefits is becoming a common source of identification in the economics literature since first used in a pivotal UI study by Gruber (1997), including Cullen and Gruber (2000), Chetty (2008), Hsu et al. (2013), and Kroft and Notowidigdo (2011).

To control for any potential endogeneity in state benefits, I include state fixed effects, λ_s. In

[6]Information about wages and weeks worked from the time period prior to reported unemployment were used to construct the benefit amount, given the rules in determining eligibility in the state and year an individual became unemployed. For more detail on how to construct this variable, including an exhaustive summary of UI laws, see LaLumia (2013).

column (1) of Table 3, I replicate column (1) of Table 2, adding state fixed effects to equation (1). In column (2) of Table 3, I add the interaction term U_{ist}*UI_{ist} to the regression, evaluating equation (2). The coefficient on the interaction term is -0.011. A significant and negative estimate for π indicates that as the replacement rate of UI benefits increases, transfer receipt in any amount decreases. This suggests that on the extensive margin, more generous benefits reduces either the need or motive for private cash transfers.[7]

The key factor in this model is the identifying variation from the UI benefit, and hence the primary concern is the relationship between the unemployed and their potential benefit. γ is identified from the within-person variation in unemployment and π is identified from the state-year variation in UI generosity. In the current specification, it is assumed that the variation in benefits is independent of the individual's characteristics. It could be the case, however, that benefits are responding to the state's unemployment rate at the time an individual is unemployed, or that, through voting for certain policies or moving to a place where certain policies exist, states' UI benefits are reflective of their working population. π would thus be using individual variation in other characteristics, rather than benefits. To control for this, I included state fixed effects.

A more rigorous way of dealing with the potential endogeneity of the unemployed influencing their UI benefits, which was originally utilized in Gruber (1997), is to instrument for the potential benefit using the sample. Rather than calculating benefits based on the characteristics of the individual, the instrument calculates the benefit based on all individuals in the sample for each state in each year, and then assigns to the individual the average benefit for the state and year in which they become unemployed. The instrumented benefit amount is thus only a measure of policy variation, independent of the individual. The results from equation (2) when UI is instrumented for in this manner is shown in column (3). The interaction reduces the effect of UI generosity to a precisely estimated, near-zero coefficient (-0.001). When relying solely on the UI variation, the effect on the extensive margin of transfer giving is now zero. The differences between columns (2) and (3) suggest that for unemployed individuals, individual characteristics play a key role in transfer receipt, but public safety net generosity does not.

But there is another measure of UI generosity that is not accounted for by the replacement

[7]The term "crowd out" is specifically avoided here. My paper is a first-order analysis that looks at the private and public safety net only in the context of unemployment insurance. It is premature to attempt to refer to the relationship as "crowding out" because I am only examining cash transfers, and have no analysis or evidence on the substitutability between forms of the private safety net, such as in-kind transfers or co-residence, or the effect of other public programs that unemployed individuals might be eligible for.

rate—the duration of benefits. The standard number of weeks that n eligible individual can collect benefits is 26, though there are eight states that differ in this regard. However, during periods of high unemployment, states can choose to increase the length of benefits if their state unemployment rate is high enough, or Congress can vote to grant additional weeks. The replacement rate does not change, but the number of weeks that an individual is eligible to receive benefits can change, based on the unemployment rate in the state.[8] To test whether longer duration of benefits has an effect on transfer giving, I add to equation (2) another interacted variable:

$$T_{ist} = X_{ist}\beta + \gamma U_{ist} + \pi U_{ist} * UI_{ist} + \rho HUrate_{ist} + \tau U_{ist} * HUrate_{ist} + \theta_i + \mu_t + \lambda_s + \epsilon_{ist} \quad (3)$$

Where $HUrate_{ist}$ is a dummy variable equal to 1 if the state's unemployment rate was higher than 6.5%, and thus eligible for an automatic extension. The results, using both the potential UI benefit and the instrumented potential UI benefit, are shown in columns (4) and (5).

High unemployment in a state at time enters positively and significantly, at 0.004. Without the interaction, this suggests that simply being in a state experiencing high unemployment increases the probability of transfers. When this is interacted with the individual's unemployment, the effect is larger, 0.013, though no longer precisely measured. However, if increases in the length of UI benefits, and therefore increases in UI generosity, deterred transfer giving, the coefficient τ would be smaller, near-zero, or negative. Though not significant, the confidence interval of τ's coefficient is positive. This means that in both channels of UI variation in generosity—the replacement rate and the length of benefits—there is a non-negative effect on family transfer giving.

V. Analysis of Unemployment Spells

Model and Results

To explore and corroborate the findings of the previous section, I again regress transfer receipt on potential UI benefit, but rather than examining individuals, I look only at spells of unemployment. As discussed in section III, I ignore the longitudinal aspect of the data and limit the sample to

[8]The Extended Benefit (EB) program was established in 1970 and grants 50% federal financing of an additional 13 weeks of benefits if a state's unemployment rate exceeds 6.5% or an additional 20 weeks if the unemployment rate exceeds 8.0%. In both instances, the rate must be 110% higher than the rate in the same period of time the year before. In addition, Emergency Unemployment Compensation (EUC) is a temporary program passed by congress on eight occasions (1958, 1961, 1971, 1974, 1982, 1991, 2002, 2008) which can also extend benefits based on the state's unemployment rate for 6 to 33 weeks, and includes 100% federal financing. See Whittaker and Issacs (2014) for more details on these extensions.

person-year observations of the unemployed. This reduces the sample to 2,086 spells of heads who were employed in $t-1$ but unemployed in t.

$$T_{ist} = \alpha + X_{ist}\beta + \psi UI_i st + \mu_t + \lambda_s + \varepsilon_i \tag{4}$$

Again, T_{ist} is a dummy for receipt of transfer from unemployed individual i in state s and year t and U_{ist} is the replacement rate of potential UI benefits. Without individual fixed effects, X_{ist} is a much more comprehensive vector of covariates, including marital status, race, ethnicity, educational attainment, presence of children, as well as information about the unemployed individual's parents, their age, if they are still working, and if they live in a different state than their child.

The key determinant of the size of the UI benefit is prior wages. Though they enter each state's formula differently, they are the principle factor in calculating benefits, regardless of how they were calculated. Incorporating wages as a control in equation (4) must therefore be done flexibly, so as to not risk collinearity with the UI benefit, or soak up the variation from the state law differences. To do this, I include in the regression a 5-knot linear spline in wages, as in Cullen and Gruber (2000). The same concern applies to children in the household, which is important to include as a covariate, but is used in the UI benefit calculation in the 19 states that have dependent allowances. I add to the model a dummy for each number of children in the household, akin to a child fixed effect.

In addition, there are also the same concerns from the first regression analysis: that transfers go to younger people, who are more likely to become unemployed, and that transfer receipt is increasing over time. I also include the linear age dummies and the year fixed effects μ_t used previously. To control for any potential endogeneity in state benefits, I include in X_{ist} the unemployment in state s at time t and state fixed effects λ_s, as well as clustering standard errors by state.

The results from the OLS regression in equation (2) are presented in column (1) of Table 4. The coefficient on the UI replacement rate is small, positive, 0.015, but not significant. This estimate diverges in sign from the interaction effect measured in column (2) of Table 3, in which potential UI was negative. The differing across specifications is worrisome for the accuracy of either, as a strong result would be consistent across specifications, such as the coefficient on marriage. It is for this reason that I conclude that generosity of UI benefits has no effect on the receipt of cash assistance from family members for the newly unemployment, though I will continue to test for robustness. With the smaller sample, many of the covariate coefficients, except those for marriage and education,

are not significant. Transfers are more likely for white (0.057) and black (0.034) individuals, but less likely for Hispanics (-0.074). Transfer probability also increases with education. And although the age and income of parents is near zero, there are positive estimates on dummies for whether a parent is working (0.022) and whether the individual lives in a different state from their parents (0.009).

Robustness

As in the earlier regression in equation (2), there is concern of endogeneity in the identifying variation of UI benefits. Individual characteristics might not be independent of benefit calculation, and these same characteristics might influence transfer receipt. I again instrument for UI benefits in the same method described in the previous section. The results for that regression are shown in column (2) of Table 4, and the effect on the replacement rate coefficient is similar as before. The instrumented potential replacement rate has a coefficient much closer to zero (0.004) than the individual's potential replacement rate. This confirms that variations in generosity has little effect on the extensive margin of giving.

There are two sample concerns which were not applicable to the analysis of household heads in the previous section, but could be biasing the results in the analysis of spells. First, the sample consists of individuals at the time of unemployment whose parents are also in the survey. It is possible that there is selection into having observed parents. The PSID became a multigenerational data set as children of the original sample formed their own households. This means there are very few unemployment spells with observable parents at the beginning of the survey (0.004% of 1970 spells and 74.4% of 2011 spells).

To account for this, I tested the two groups—unemployed with parents unobserved and unemployed with parents observed—for statistically comparable means. I used four key variables describing heads of households: real wages, number of children in the household, if divorced, and state unemployment rate at time of unemployment spell. I looked at both at single-year t-tests (i.e. comparison of means in 1985) and multi-year t-tests (i.e. comparison of means 1985-2011) and considered samples statistically different at the 5% level. Both types of test show a convergence of the sample means for the two sub-groups starting in the late 1980s for certain variables, with all comparable by 1991.[9] Columns (3) and (4) replicated columns (1) and (2) of Table 3, but using

[9]There were two years, 1997 and 2003, in which the samples diverged again on at least one variable. Removing these years in later analysis did not alter the results.

only the 1992-2011 sample.

The result for the replacement rate are essentially unchanged between the two samples for both the potential benefit (0.018) and the instrumented benefit (0.005). Indeed, the only notable difference between the two samples' results is that, when restricted to later years, having a working parent becomes a better predictor of transfer receipt during unemployment spells, at 0.094. Whether this result, and that individuals are working longer in general, could explain why transfers are increasing over time, should be the subject of further study.

But that brings up further sample problems—when trying to account for selection into observable parents, I created a new selection problem by focusing on a time period when transfers, and unemployment, were occurring at different rates than in prior years. In the final columns of Table 3, I run the equation (2) without the parental covariates, and include the full sample. Again, the results for the replacement rate differ very little from the original specification. They remain insignificant, even with the larger sample; and the individually calculated potential benefit has a larger result (0.011), while the instrument has a near-zero result (0.003).

Finally, there is also the problem of accuracy in the weekly wage calculation. Within my sample of 25-54 year-old, unemployed heads of household with parents in the survey, roughly 150 of these individuals, just over 5%, had inconsistent or missing wage information, and hence a portion of the sample relied of wage information constructed differently from the rest.[10] When I remove these individuals from the sample, the results are not significantly different. The same is true for removing the even years of the sample before 1998. Indeed, through all specifications in analyzing unemployment spells, the consistent finding, or absence of finding, is that the giving financial assistance from family members has no strong relationship with UI benefit replacement rates. The generosity of the public safety net does not influence *whether* transfers are given.

Transfer amounts

The hitherto discussion has focused on the extensive margin of transfer giving. Prior work on family cash transfers and public benefits has focused on the dollar-amount reduction in giving, rather than receipt of any amount. Having first established the response to unemployment and the

[10]For most of the sample, the weekly wage was a straightforward construction—it is simple the hourly wage multiplied by the usual number of hours in a week. But there was a share of observations for which this was not possible. The most common problem was that the individual reported $0 in hourly wage, or 0 hours worked, but reported positive annual wage income as well as positive annual hours, and even positive weeks worked in a year. To get around this, I calculated an hourly wage, or hours per week, or both, based on the responses to other survey questions.

insensitivity of giving to benefit generosity, I discuss amount given here.

$$TA_{ist} = \alpha + X_{ist}\beta + \psi UI_i st + \mu_t + \lambda_s + \varepsilon_i \tag{5}$$

TA_{ist} is transfer amount given to individual i in state s at time t, which, rather than being a dichotomous 0 or 1, is the amount reported by the head of household in the survey.[11] The rest of the covariates and specifications (year fixed effects, state fixed effects, state clustered standard errors) from equation (4) are similarly used here. The results, mirroring the analytical samples used in Table 4, are presented in Table 5.

Despite the change in dependent variable from any transfer receipt to size of transfer receipt, the pattern between the individually calculated potential UI benefit (column (1)) and the instrumented potential UI benefit (column (2)), are similar to both Table 3 and Table 4. The effect of potential UI benefit on transfer amount is negative, but small and insignificant. Instrumenting for potential benefits pushes the coefficient towards zero, from -23.878 to -9.506, suggesting a 1-point increase in the replacement rate reduces the amount of transfers given by $9. This is similar for the varying analytical samples used in the remaining four columns. Benefit generosity is not a strong or clear predictor of the amount of transfers received by the unemployment. Even taking the highest predicted reduction ($43), relative to the mean ($1909), the decrease in transfer amount is 2.2%.

I extend this analysis in Table 7, substituting dollar amounts for log of transfer amounts. This requires that I limit the sample to only individuals with positive transfer receipt. Again, I regress log transfer amount on potential UI benefits. The results are similar, with a coefficient of -0.087 on potential benefits and -0.024 on instrumented benefits. Or, a 10-point increase in the replacement rate reduces the amount of transfers given by 8.7% (or 2.4%). The reliability of the results using transfer amounts is far from certain, given concerns about data quality as well as a small sample. If there is a reduction in transfer amount given in response to a more generous replacement rate, the reduction—and confidence intervals suggest it would be a reduction—is small, or near-zero.

[11] There is concern about the accuracy of reported transfer data from private givers that have been raised many times about the accuracy of reported transfer data from public givers, as documented in Anderson and Meyer (1997). I do not comment on that here, but take the dollar amounts given and ignore amounts reported to be less that $10 and cap giving at $100,000. This cap affects fewer than 50 observations.

VI. Discussion

The principle conclusions from my analysis show that the private safety net, when measured as cash assistance from non-resident family members, responds to job separation, and is not affected on the extensive margin by the generosity of the public safety net. Families that will send money to help out their unemployed relatives will do so regardless of the amount of UI that individual could receive. Despite findings in previous literature that the amount given should reduce with more generous benefits, the reduction was too small to be precisely or robustly estimated in this analysis.

This is a finding with broad possible implications, but it is important to discuss the limitations first. As mentioned in the introduction, cash assistance is only one form of the safety net, and there are others that can similarly, or dissimilarly, respond, such as co-residence or time transfers. It is not known how other forms of the safety net respond to potential public benefits, nor is it known how the private safety net substitutes between forms. The analysis in this paper did not attempt to tease other these effects, but focused specifically on cash transfers. Further study could examine other forms in a similar method, such as co-residence, to work towards characterizing the complete private safety net response.

The paper is also limited in that it does not explain why cash transfers to family members has been increasing over time. It could be due to the increase economic independence of the elderly, as shown through independent living (Engelhardt et al., 2005), or the increasing volatility of incomes (Dynan et al. (2007); Dynan et al. (2012); Gottschalk and Moffitt (1994)). Morever, there has been major changes to the composition of the public safety net (Moffitt and Scholz (2009); Ben-Shalom et al. (2011)) over the past four decades. My identification relied on the variation in benefits, which is not the same as a response to a change in benefits. Cash transfers could also respond to reforms to the public safety net.

Still, the results found in this paper are interesting in that they suggest that the private safety net is not a complement to the public safety net, but a supplement. It follows then whether in this instance of job separation, the negative effects of unemployment—such as the permanent effect on wages and health documented in Sullivan and von Wachter (2009a) and Sullivan and von Wachter (2009b), among many others—are at all abated by private transfers. Or, if the goals of the public safety net—such as consumption smoothing originally documented in Gruber (1997)—are supplanted or aided by the private safety net. It could be that the relative effectiveness of public versus private benefits gives us insight into how the public safety net could be improved.

These and other questions are outside the scope of this paper and can be the subject of future study, but the results of this paper demonstrate the need for that study.

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Table 1: Sample means of demographic, educational attainment, employment, and financial variables by transfer receipt, 1968-2011

	(1) Full Sample		(2) At time of transfer		(3) No transfer	
	Mean	SD	Mean	SD	Mean	SD
Demographics						
Age	39.2	(8.945)	35.6	(8.584)	39.4	(8.910)
White	81.2%	(0.393)	77.6%	(0.422)	81.5%	(0.390)
Black	14.3%	(0.347)	18.7%	(0.393)	14%	(0.343)
Hispanic	5.1%	(0.219)	3.8%	(0.191)	5.2%	(0.222)
Married	58.7%	(0.490)	31.4%	(0.466)	60.7%	(0.485)
Never married	18.3%	(0.382)	34.1%	(0.476)	17.1%	(0.371)
Widowed	2.3%	(0.151)	2.1%	(0.151)	2.2%	(0.151)
Divorced/Separated	20.7%	(0.400)	32.4%	(0.463)	19.8%	(0.394)
Children at home	55.2%	(0.496)	49.2%	(0.500)	55.7%	(0.495)
Educational Attainment						
Less than high school	12.9%	(0.339)	12.0%	(0.324)	12.9%	(0.340)
High school	31.5%	(0.460)	29.3%	(0.454)	31.7%	(0.460)
Some college	20.1%	(0.392)	26.1%	(0.433)	19.7%	(0.389)
BA or higher	24.9%	(0.427)	26.3%	(0.44)	24.8%	(0.432)
Employment status						
Employed	87.6%	(0.500)	74%	(0.462)	88.6%	(0.499)
Unemployed	5.4%	(0.298)	11.7%	(0.376)	4.9%	(0.290)
Not in labor force	7.1%	(0.491)	14.9%	(0.498)	7.0%	(0.488)
Financial						
Owns home	59.4%	(0.490)	34.9%	(0.476)	61.2%	(0.486)
Rents	35.4%	(0.478)	54.1%	(0.498)	34%	(0.473)
Transfer	7.1%	(0.257)	100%	-	0%	-

Notes: Table presents the mean of individuals observed in the data at the time of interviews. ··Individuals can appear in both column (2) and (3). Educational attainment does not sum to 100%, as attainment can change for an individual over the course of the survey.

Table 2: Results from panel regression with individual fixed effects of transfer receipt on unemployment, 1968-2011

Dependent var.	(1) T_{it}	(2) T_{it-1}	(3) $T_{it} - T_{it-1}$	(4) T_{it+1}	(5) T_{it}	(6) T_{it-1}	(7) $T_{it} - T_{it-1}$	(8) T_{it+1}
Unemployment	0.025***	-0.018***	0.043***	0.003				
E - U	[0.005]	[0.004]	[0.007]	[0.004]				
Unemployment					-0.006***	-0.014***	0.003	0.001
U - U					[0.002]	[0.003]	[0.004]	[0.002]
Married	-0.019***	0.092***	-0.109***	-0.010*	-0.019***	0.092***	-0.109***	-0.010*
	[0.005]	[0.008]	[0.010]	[0.005]	[0.005]	[0.008]	[0.010]	[0.005]
Divorced/Separated	0.021***	-0.036***	0.056***	0.005*	0.021***	-0.036***	0.056***	0.005*
	[0.003]	[0.003]	[0.005]	[0.003]	[0.003]	[0.003]	[0.005]	[0.003]
Owns home	-0.007***	0.026***	-0.033***	0.001	-0.007***	0.026***	-0.033***	0.001
	[0.002]	[0.002]	[0.003]	[0.002]	[0.002]	[0.002]	[0.003]	[0.002]
Children present	0.007***	0.004**	0.002	0.001	0.007***	0.004**	0.002	0.001
	[0.002]	[0.002]	[0.003]	[0.002]	[0.002]	[0.002]	[0.003]	[0.002]
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Linear age dummies	Y	Y	Y	Y	Y	Y	Y	Y
Mean of T_{it}	7.1%	7.1%	7.1%	7.1%	7.1%	7.1%	7.1%	7.1%
N	17,268	17,268	17,268	17,268	17,268	17,268	17,268	17,268

Notes: Table presents results of eight regressions. Coefficients for year effects and linear age dummies not shown. Each ··regressor is a dummy for a transition to a new state, i.e. married indicates that they were married in t but not married in *t-1*.

Table 3: Results from panel regression with individual fixed effects of transfer receipt on unemployment, with certain interacted variables, 1968-2011

Dependent var.	(1) T_{it}	(2) T_{it}	(3) T_{it}	(4) T_{it}	(5) T_{it}
Unemployment (U)	0.024***	0.028***	0.025***	0.023***	0.020***
E - U	[0.005]	[0.005]	[0.005]	[0.006]	[0.006]
U * Replacement rate		-0.011**		-0.011**	
		[0.005]		[0.005]	
U * Instrument			-0.001*		-0.001
			[0.000]		[0.000]
High state unemployment				0.004*	0.004*
				[0.002]	[0.002]
U * High state unemployment				0.013	0.013
				[0.010]	[0.010]
Married	-0.019***	-0.019***	-0.019***	-0.019***	-0.019***
	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]
Divorced/Separated	0.021***	0.021***	0.021***	0.021***	0.021***
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Owns home	-0.007***	-0.007***	-0.007***	-0.007***	-0.007***
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
Children present	0.007***	0.007***	0.007***	0.007***	0.007***
	[0.002]	[0.002]	[0.002]	[0.002]	[0.002]
Year fixed effects	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y
Linear age dummies	Y	Y	Y	Y	Y
Mean of T_{it}	7.1%	7.1%	7.1%	7.1%	7.1%
N	17,268	17,268	17,268	17,268	17,268

Notes: Table presents results of five regressions. Coefficients for year effects, state effects, and linear age dummies not shown. Each regressor is a dummy for a transition to a new state, i.e. married indicates that they were married in t but not married in *t-1*.

Table 4: Results from regression of transfer receipt on potential UI benefit and instrumented UI benefit for unemployment spells, 1968-2011.

	(1) Potential UI 1968-2011	(2) IV 1968-2011	(3) Potential UI 1992-2011	(4) IV 1992-2011	(5) Potential UI 1968-2011	(6) IV 1968-2011
UI replacement rate	0.015		0.018		0.011	
	[0.016]		[0.018]		[0.014]	
Instrument		0.004		0.005		0.003
		[0.004]		[0.004]		[0.004]
White	0.057	0.057	0.104*	0.104*	0.009	0.008
	[0.038]	[0.038]	[0.059]	[0.059]	[0.037]	[0.037]
Black	0.034	0.034	0.054	0.054	-0.000	-0.001
	[0.040]	[0.040]	[0.052]	[0.052]	[0.042]	[0.042]
Latino	-0.074	-0.074	-0.107	-0.106	-0.088**	-0.088**
	[0.071]	[0.071]	[0.064]	[0.065]	[0.035]	[0.035]
Rents	-0.044	-0.043	-0.056	-0.055	-0.033	-0.033
	[0.029]	[0.029]	[0.044]	[0.044]	[0.027]	[0.027]
Owns home	-0.017	-0.016	-0.037	-0.036	-0.016	-0.016
	[0.041]	[0.041]	[0.054]	[0.054]	[0.031]	[0.031]
Married	-0.104**	-0.104**	-0.124**	-0.124**	-0.075***	-0.075***
	[0.040]	[0.040]	[0.056]	[0.056]	[0.026]	[0.026]
Divorced/separated	-0.063	-0.063	-0.095**	-0.095**	-0.043	-0.043
	[0.039]	[0.040]	[0.045]	[0.045]	[0.027]	[0.027]
Less than high school	0.058	0.056	0.020	0.018	0.060*	0.060*
	[0.063]	[0.063]	[0.083]	[0.084]	[0.034]	[0.034]
High school	0.120*	0.120*	0.134*	0.134*	0.113***	0.112***
	[0.060]	[0.060]	[0.074]	[0.074]	[0.037]	[0.037]
Some college	0.199***	0.199***	0.204**	0.203**	0.169***	0.168***
	[0.057]	[0.057]	[0.079]	[0.079]	[0.040]	[0.040]
BA or higher	0.152**	0.151**	0.169**	0.168**	0.140***	0.139***
	[0.057]	[0.057]	[0.076]	[0.076]	[0.039]	[0.039]
Different state from parents	0.009	0.009	0.013	0.013		
	[0.031]	[0.031]	[0.048]	[0.048]		
Age youngest parent	-0.001	-0.001	-0.002	-0.002		
	[0.002]	[0.002]	[0.003]	[0.003]		
Working parent	0.022	0.022	0.094**	0.094**		
	[0.025]	[0.025]	[0.041]	[0.041]		
Parents' income	-0.000	-0.000	-0.000	-0.000		
	[0.000]	[0.000]	[0.000]	[0.000]		
Wage spline	Y	Y	Y	Y	Y	Y
Linear age dummies	Y	Y	Y	Y	Y	Y
Linear kid dummies	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
State unemployment rate	Y	Y	Y	Y	Y	Y
N	2086	2086	1151	1151	3480	3480

Notes: Table presents results of six regressions. Coefficients for the wage spline, linear age dummies, linear kid dummies, ··state fixed effects, and year effects calculated but not shown not shown. Covariates reflect status at time of interview, but potential UI benefit is based on wages in the prior (employed) time period.

Table 5: Results from regression of transfer receipt on potential UI benefit and instrumented UI benefit

	(1) Potential UI 1968-2011	(2) IV 1968-2011	(3) Potential UI 1992-2011	(4) IV 1992-2011	(5) Potential UI 1968-2011	(6) IV 1968-2011
UI replacement rate	-23.878		-43.303		-15.708	
	[27.770]		[35.645]		[23.472]	
Instrument		-9.506		-11.899		-5.501
		[6.868]		[7.962]		[5.806]
White	-21.748	-21.895	-56.849	-56.986	-282.774	-282.079
	[324.633]	[324.837]	[415.599]	[415.559]	[251.619]	[251.838]
Black	-255.528	-254.546	-365.517	-364.625	-503.987*	-502.792*
	[317.888]	[318.588]	[392.091]	[392.756]	[261.734]	[261.944]
Latino	-234.090	-235.138	-263.190	-264.720	-494.482***	-493.870***
	[248.998]	[248.437]	[278.705]	[278.306]	[152.891]	[152.972]
Rents	-77.146	-78.921	-234.944	-236.582	-129.214	-129.748
	[149.427]	[150.285]	[242.719]	[243.664]	[119.498]	[119.911]
Owns home	175.048	173.495	130.551	128.889	33.746	33.212
	[171.957]	[172.558]	[282.986]	[283.613]	[136.286]	[136.661]
Married	-543.378***	-543.475***	-718.565***	-719.076***	-346.265***	-346.243***
	[156.658]	[156.676]	[200.391]	[200.585]	[98.908]	[98.864]
Divorced/separated	-331.857**	-331.724**	-412.906*	-412.547*	-241.989**	-241.673**
	[162.312]	[162.343]	[220.911]	[220.985]	[106.893]	[106.832]
Less than high school	123.759	127.085	133.916	139.435	76.379	77.973
	[206.334]	[206.188]	[329.945]	[329.546]	[154.502]	[155.191]
High school	249.960	251.068	286.798	288.371	191.102	191.985
	[175.807]	[176.294]	[261.814]	[262.266]	[154.945]	[155.411]
Some college	428.146**	429.203**	401.451	402.630	362.028**	362.899**
	[177.910]	[178.555]	[294.375]	[294.917]	[145.656]	[146.035]
BA or higher	476.441**	477.889**	391.074	392.946	461.816***	463.183***
	[201.954]	[202.027]	[289.826]	[289.944]	[170.551]	[171.174]
Different state from parents	-50.965	-50.714	10.056	9.966		
	[140.202]	[140.130]	[171.930]	[171.833]		
Age youngest parent	20.385*	20.505*	21.498	21.690*		
	[10.822]	[10.796]	[12.882]	[12.871]		
Working parent	194.470*	194.479*	487.905***	488.449***		
	[103.626]	[103.603]	[143.736]	[143.930]		
Parents' income	0.144	0.145	-0.184	-0.182		
	[0.468]	[0.469]	[0.557]	[0.557]		
Mean TA_{it}	\$1909	\$1909	\$1909	\$1909	\$1909	\$1909
Wage spline	Y	Y	Y	Y	Y	Y
Linear age dummies	Y	Y	Y	Y	Y	Y
Linear kid dummies	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
State unemployment rate	Y	Y	Y	Y	Y	Y
N	2086	2086	1151	1151	3480	3480

Notes: Table presents results of five regressions. Coefficients for the wage spline, linear age dummies, linear kid dummies, ··state fixed effects, and year effects calculated but not shown not shown. Covariates reflect status at time of interview, but potential UI benefit is based on wages in the prior (employed) time period.

Table 6: Results from regression of log transfer amount receipt on potential UI benefit and instrumented UI benefit

	(1) Potential UI 1968-2011	(2) IV 1968-2011	(3) Potential UI 1992-2011	(4) IV 1992-2011	(5) Potential UI 1968-2011	(6) IV 1968-2011
UI replacement rate	-0.087		-0.057		-0.063	
	[0.103]		[0.096]		[0.056]	
Instrument		-0.024		-0.016		-0.016
		[0.020]		[0.020]		[0.011]
White	0.568	0.583	-0.800	-0.805	-0.772**	-0.774**
	[0.975]	[0.982]	[1.020]	[1.023]	[0.373]	[0.376]
Black	0.365	0.393	-1.434	-1.428	-1.325***	-1.321***
	[1.061]	[1.067]	[0.952]	[0.957]	[0.458]	[0.459]
Latino	0.745	0.770	0.163	0.178	-0.272	-0.269
	[0.913]	[0.912]	[0.488]	[0.489]	[0.394]	[0.397]
Rents	-0.394	-0.405	-0.208	-0.216	-0.104	-0.109
	[0.247]	[0.245]	[0.419]	[0.418]	[0.188]	[0.189]
Owns home	-0.003	0.001	-0.440	-0.434	0.168	0.167
	[0.376]	[0.376]	[0.565]	[0.566]	[0.297]	[0.297]
Married	-0.144	-0.135	0.014	0.018	-0.012	-0.013
	[0.340]	[0.340]	[0.484]	[0.485]	[0.250]	[0.250]
Divorced/separated	-0.355	-0.348	-0.269	-0.261	-0.170	-0.171
	[0.317]	[0.315]	[0.333]	[0.333]	[0.222]	[0.222]
Less than high school	-0.831	-0.820	-0.090	-0.088	-0.829	-0.822
	[0.666]	[0.658]	[1.184]	[1.184]	[0.567]	[0.567]
High school	-0.219	-0.234	0.636	0.625	-0.221	-0.228
	[0.588]	[0.581]	[1.107]	[1.108]	[0.468]	[0.466]
Some college	-0.529	-0.541	0.085	0.074	-0.133	-0.138
	[0.580]	[0.572]	[1.043]	[1.047]	[0.447]	[0.445]
BA or higher	-0.675	-0.680	-0.460	-0.454	0.243	0.243
	[0.702]	[0.697]	[1.038]	[1.035]	[0.487]	[0.486]
Different state from parents	-0.092	-0.086	0.091	0.094		
	[0.235]	[0.236]	[0.303]	[0.301]		
Age youngest parent	0.055***	0.056***	0.050*	0.051*		
	[0.019]	[0.019]	[0.028]	[0.028]		
Working parent	0.364**	0.360**	0.438*	0.434*		
	[0.171]	[0.170]	[0.234]	[0.232]		
Parents' income	0.003*	0.003*	0.005**	0.005**		
	[0.002]	[0.002]	[0.002]	[0.002]		
Wage spline	Y	Y	Y	Y	Y	Y
Linear age dummies	Y	Y	Y	Y	Y	Y
Linear kid dummies	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
State unemployment rate	Y	Y	Y	Y	Y	Y
N	351	351	230	230	494	494

Notes: Table presents results of five regressions. Coefficients for the wage spline, linear age dummies, linear kid dummies, ··state fixed effects, and year effects calculated but not shown not shown. Covariates reflect status at time of interview, but potential UI benefit is based on wages in the prior (employed) time period.