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Article

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Who helps the unemployed? Workers' receipt of public and private transfers

Abstract

I use longitudinal data from the Panel Study of Income Dynamics (PSID) to measure the extent to which an unemployment spell increases the likelihood that a worker receives a cash transfer from family. I examine the prevalence of cash transfers from family, the demographic distribution of unemployed receivers, and the variation between family supported and not family supported spells. I further investigate how this informal, private assistance relates to public transfers from Unemployment Insurance using state-by-year variation in the UI program. I find that unemployment increases the probability a worker receives financial assistance from their family, inclusive of all demographic subgroups, that family cash transfer receipt is growing over time, and is weakly related to UI availability.

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1 Introduction

The effect of public cash support on the behavior and outcomes of unemployed workers has been well studied. Unemployment insurance (UI) benefits, the US program, have been assessed in terms of their relationship with consumption (Gruber, 1997; East and Kuka, 2015), labor supply of spouses (Cullen and Gruber, 2000), consumer credit (Hsu et al., 2018), poverty (Bitler and Hoynes, 2016), taxes (Anderson and Meyer, 1997; LaLumia, 2013), moral hazard and liquidity (Chetty, 2008), productivity (Acemoglu and Shimer, 2000), cyclicality (Kroft and Notowidigdo, 2016), and take-up (Blank and Card, 1991). Yet, informal, private assistance from family in the form of cash, or family transfers, is flowing to the same population of individuals eligible for UI, and the unemployed in general.

However, we know comparatively little about the prevalence and distribution of family assistance among unemployed individuals—such as the share of unemployed who receive family assistance, the characteristics of individuals who are and are not supported by family, the features of spells that are and are not supported by family, and the size of the family transfer relative to previous income. This motivates two key research questions that I answer in succession in the paper: To what extent does the receipt of family transfers vary with unemployment? And to what extent does the receipt of family transfers *during* unemployment vary with UI benefits?

I use the 1976–2013 waves of the Panel Study of Income Dynamics (PSID), which has the advantage of observing workers over long periods of time, from many birth cohorts, and many macroeconomic conditions for spells. It has annually collected measures of receipt of transfers from family and detailed labor market characteristics of the worker, including weeks of employment and unemployment in a year, the reason for a spell, and the usual hourly wage.

To address the first question, I give a brief overview of the incidence of family cash transfer receipt during key life course events to show the importance of unemployment. I then use within-person variation in unemployment and family transfer receipt to investigate to what extent an unemployment spell increases the likelihood that a worker receives a cash transfer from family. This method is akin to the study of transfer sending by McGarry (2016), as well as the empirical approach common in the literature concerning job displacement, such as the work of Stevens (1997), and the more recent work on the effects of hospitalization by Dobkin et al. (2018). As part of this, I show how the increase in probability of family transfer receipt varies among demographic groups and spell features.

To address the second question, I give a brief overview of UI benefit receipt and receivers over time. I then instrument for UI benefits using the state-by-year variation in UI program rules to investigate if more- or less-accessible UI increases or decreases family transfer receipt. The instrument is similar to that developed by Gruber (1997) and recently used in Hsu et al. (2018) and East and Kuka (2015), with additional measures of program variation.

I find that an unemployment spell is associated with a 49% increase in the likelihood of receiving a transfer from family members, off a base receipt rate of 8.0%. In dollar amounts, this is a $71 increase relative to the mean transfer receipt of $243. The increase in likelihood of receiving a transfer from family is significant for all age groups, all race groups, all education levels, and all family types. There is little variation in the likelihood or size of transfers whether the spell was associated with displacement (layoff or firm closing) or a voluntary quit.

However, family transfers are associated with spells of longer duration; the increased likelihood of receiving a transfer from family is largest for spells >26 weeks.

I further find that the relationship between informal family transfers and public UI transfers is weak. It is clear from the sample mean values that, first, UI is not randomly distributed through the unemployed population—there are strong demographic trends—and second, concurrent occurrence of UI and family transfer receipt is rare. Within my sample, just 3% of the unemployed receive both family cash transfers and UI cash transfers. The response of family transfers when I instrument for UI benefits is imprecise. The results weakly suggest that workers in states with higher replacement rates and less-strict eligibility requirements are relatively less likely to receive a transfer from their family, but this estimate is not statistically significant.

This imprecision is somewhat unexpected; the family transfer literature has previously established that the family transfer response to marginal increases in program generosity is small in general (Altonji et al., 1997; Rosenzweig and Wolpin, 1994), although expected to be larger for UI (Schoeni, 2002). However, roughly a quarter of the unemployed receive UI benefits (Vroman and Woodbury, 2014), fewer than half of the unemployed apply for UI benefits (Vroman, 2009), and there is evidence that UI's effectiveness has eroded (East and Kuka, 2015). Hence, insensitivity to UI is less surprising given the contemporary context of UI.

A note before beginning: private family assistance is often assumed to be poorly captured in survey data and rightly assumed to be endogenous, making it a difficult subject of empirical analysis. I am not able to perfectly solve or control for these issues in the context of the paper. I present discussions of data quality, heterogeneous effects, and robustness checks and note where interpretations must be cautious. However, the central contribution of this paper is to document the extent of family transfer receipt among the unemployed and show that it is not trivial. I illustrate that this is a large flow of funds to the unemployed, that it is associated with unemployment spells, and that the size and incidence of receipt are rising over time. Critically, this flow of assistance is not predicted or moderated by UI.

This paper provides evidence that more research is needed about the role of the benefits, whose prevalence I demonstrate. I will describe financial transfers between family members in Section 2, the cash receipt among the unemployed in my sample in Section 3, the empirical strategy in Section 4, results in Section 5, and robustness in Section 6. I end with a brief discussion.

2 Financial transfers between family members

It has long been established that families in the USA send cash transfers across households. The literature examining these family transfers has two principal branches. The first branch uses family transfers to understand the determinants of wealth. Given the savings and wealth predictions of the life cycle model (Ando and Modigliani, 1963), transfers between family members may contribute to, or explain the aggregate distribution of, wealth accumulation. Several studies show how transfers to children partly determine wealth among households (Gale and Scholz, 1994; Hurd and Smith, 2001; De Nardi, 2004), concluding that transfers are an important investment. The second branch examines transfers to test whether generations within families are altruistically linked, as originally posited by Becker (1974). Researchers have used the relationship between public and private benefits to give insight to family links,

as in the following works (Altonji et al., 1992; Altonji et al., 1997; Rosenzweig and Wolpin, 1994; Cox, 1990; McGarry and Schoeni, 2000), often, but not always, rejecting altruism.

Although the latter touches on transfers in the context of support and informal safety net (implicitly competing with a public safety net), it does not go so far as to adequately examine transfers as a means of smoothing or insuring against shocks. The point of view, and emphasis, is on the role of giving, rather than on the role of receiving. Little is known about the people who receive cash transfers from their family (during a shock or not), how those receivers are distributed among the population, or the impact that receipt has.

This lack of understanding of short-term assistance is in contrast to the role of cash transfers as studied by other literatures. In the context of developing economies, cash transfers, which are referred to as remittances, are widely understood to act as informal insurance mechanisms that protect against income shocks, especially in countries without robust public systems (Miller and Paulson, 2007). Recently, the role of the family as an informal private safety net has been identified in the context of housing: Wiemers (2014) documents the relationship between unemployment and family coresidence, and Kaplan (2012) demonstrates that the availability of family residential assistance acts as an insurance mechanism, changing job search and savings behavior among young men.

The research question in this paper is to what extent, in the USA, family transfer receipt varies with unemployment. With that goal, I examine cash transfers flowing to households in the year of an unemployment spell. Rather than focus on, or test, the parental motives for giving, the intertemporal changes to lifetime family transfer giving, changes to later bequests, or the relationship to long-term wealth accumulation, I examine an income shock, measure whether transfer receipt is in response to the income shocks, and describe the distribution of receipt among all those experiencing the shock.

McGarry (2016) has research aims closest to that of this paper. For her analysis, she uses a longitudinal sample of children of the respondents in the Health and Retirement Study (HRS) to examine whether parents are more likely to send a cash transfer in a year when the child is out of work. To do this, she regresses the probability of receiving a transfer on the child's employment status, controlling for child fixed effects—the same method that I will use. There are several limitations to the HRS, which motivate further analysis. First, the HRS sample is limited in age to people aged ≥50 years, and the measure of transfers is left truncated at $500. Hence, the sample of out-of-work children must have a living parent >50 years of age giving numerations of at least $500; the age of children in her sample averages 31–47 years. Second, the employment status of the children is based on parent's reporting and does not include details of whether the child is out of work and out of the labor force, or out of work and looking for employment. Analysis of transfer behavior based on the nature of the spell, or its length, is not possible. Further, the HRS does not capture whether the child sample is receiving UI benefits, and because of the less-detailed information on labor force attachment and wages, UI eligibility cannot be inferred.

I use the 1976–2013 waves[1] of the PSID, a longitudinal household survey that began in 1968 and followed the original sample households and their descendants over time. Family transfers in the PSID are captured through a survey question given to every head of household

1 The survey instrument was redesigned in 1976, changing how unemployment was measured. I use only post-1976 waves for consistency.

(1968–2013) and, later, to their spouses (1981–2013), asking if they received "help from relatives" in the completed calendar year and, if so, how much they received. I define a family transfer-year as any year in which the respondent's household (either the head or the spouse) reports a positive amount received. For brevity, I refer to these calendar year totals as a family transfer, although it could be multiple disbursements within a year. I assume that these family transfers exclude child support and alimony because court-ordered income sources are asked about and totaled separately in the survey.

Because the question asks for a dollar amount of "help from relatives", I refer to them as cash transfers. The PSID question does not ask for, or specify to, include the value of an in-kind gift; it is unclear whether a family member purchased something for the respondent, and whether the respondent would include the dollar value of this purchase here. However, in general, receipt of assistance is underreported and giving of assistance is overreported, and we can assume this variable does not capture the full extent of in-kind assistance. One in-kind transfer that is certainly excluded is residence. The question on help from relatives, and—for many waves of the survey—details of employment and unemployment, were *only* collected from heads or spouses. Someone who is not a head or spouse is not in the universe of this question, including anyone living with parents or relatives. The data do not support a simultaneous study of transfers and coresidence.

Among the 19- to 59-year-old heads of households and their spouses observed in the PSID from 1976 to 2013, I observe 23,405 family transfers. Of these, 47% are <$500 (nominal). In real terms, the median transfer is $1,019 and the mean is $3,020. In total, 37.7% of heads and spouses report receiving a transfer from their family at some point between the ages of 19 and 59 years. Of these receivers, more than half report only one transfer in their observed lifetime. This aligns with previous findings by McGarry (2016) and others examining transfer giving in the HRS, who find that transfers are infrequently given and repeated transfers over many years is rare.

Table 1 shows the share and size of all transfers flowing to households concurrent with life events.[2] For example, in the first row, 5.7% of all transfers reported by 19- to 59-year-old

Table 1 Share of observed family transfers concurrent with life events, heads of household and spouses aged 19–59 years, 1976–2013 PSID

	Share of transfers	Conditional transfer size in $2013
Exiting college	0.057 (0.231)	$4,015 ($7,749)
Getting married	0.038 (0.191)	$2,796 ($5,858)
Becoming separated	0.056 (0.229)	$2,565 ($4,775)
Buying a house	0.047 (0.211)	$5,795 ($12,708)
Having a child	0.054 (0.226)	$3,094 ($7,769)
Unemployed	0.236 (0.425)	$2,060 ($4,158)
None of the above	0.599 (0.490)	$3,034 ($6,479)

Notes: Data are from the 1976–2013 waves of the PSID. The sample includes 19- to 59-year-old adults who are either head of household or spouse. Family transfers are 1-year household totals (transfer reported by head, spouse, or both). A worker is unemployed if he/she reports at least 1 week of unemployment in a year. Other events are based on status changes from the previous year. Table shows mean values and standard deviations (in parentheses) of transfer receipt in the year of concurrent unemployment or year of status change.

2 These events are based on self-reported status changes between survey waves; the transfer is measured in the year of the new status.

heads of household and their spouses coincided with the year that the head or spouse exited college, and, conditional on transfer receipt at college exit, transfers averaged $4015 in 2013 dollars. Moreover, 3.8% of the observed transfers coincided with the year of first marriage, 5.6% with the year of separation or divorce, 4.7% with the year of buying a first home, and 5.4% with the year of a new child. Critical to the analysis in this paper, around one in four transfers (23.6%) were reported by individuals in the year they were unemployed and, interestingly, unemployed households had the smallest transfer amount of all observed, i.e., $2,060 compared to $5,795 for the year of a first house, which is the largest average transfer. As we do in the remainder of the paper, we define unemployment as a calendar year in which the individual reported at least 1 week of unemployment. We refer to this as a spell, although it is, in fact, a spell-year. We do not make any conditions on the 19- to 59-year-old population in terms of labor force attachment.

It is important to note that these events account for less than half of transfers, as 59.9% of transfers occur in years in which none of these life course events are observed. Not observed in the survey but potentially related to cash transfers from parents are other measures of hardship or financial strain, such as health shocks or major home repair, or other investments, such as education expenses or education saving for the child or grandchild. Nonetheless, the large share of total family transfers flowing to heads and spouses in the year of unemployment is confirmation of the relevance of my first research question: studying the relationship between family transfers and unemployment.

3 Cash support for the unemployed

UI is a joint state and federal program established in 1935 to provide temporary cash benefits to unemployed workers. Benefits are designed to replace about half of previous earnings and typically last 26 weeks; states design their own eligibility and benefit structure under the broad federal mandate to provide benefits to workers who are willing and available to work, have worked previously, and have lost their job through no fault of their own.

UI research is often framed within the trade-off between liquidity and moral hazard for searching workers (Chetty, 2008). The former includes studies of benefit generosity and the effects of UI on consumption smoothing (Gruber, 1997), household finance and mortgage default (Hsu et al., 2018), health spending (Kuka, 2016), and spousal labor supply (Cullen and Gruber, 2000). The latter includes studies of incentives on search and the macroeconomic consequences of higher or lower benefits, whether there are positive spillovers (Acemoglu and Shimer, 2000), lengthened spells with regular durations (Moffitt, 1985; Meyer, 1990; Katz and Meyer, 1990) or extended durations (Farber and Valletta, 2015; Farber et al., 2015; Kroft et al., 2016), search distortions (Krueger and Mueller, 2010), or cyclicality concerns (Kroft and Notowidigdo, 2016). A common method for identification is to use the variation both across states and within states over time in the generosity of potential benefits the worker is eligible for, thereby avoiding selection in who elects to apply for benefits. Hsu et al. (2018) demonstrate that the state variation in generosity is not related to local economic conditions.

The generosity and extent of coverage from UI and family transfers among unemployed workers is presented in Table 2. It summarizes the responses from heads and spouses in the

Table 2 Replacement rate and receipt of unemployment insurance benefits and family cash transfers among unemployed workers, heads of household and spouses aged 19–59 years, 1976–2013 PSID

	Replacement rate			Share reporting receipt			
	Potential weekly UI benefit	Reported 6-month UI benefit	Reported 6-month transfer	UI benefits	Family cash transfer	Both UI and family	Neither UI nor family
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All	0.432	0.277	0.411	0.286	0.153	0.029	0.533
Age, years							
29	0.442	0.288	0.379	0.216	0.209	0.030	0.544
39	0.430	0.258	0.413	0.326	0.129	0.032	0.514
49	0.421	0.265	0.471	0.339	0.097	0.025	0.538
59	0.423	0.319	0.568	0.376	0.075	0.019	0.531
Race							
Black	0.428	0.276	0.303	0.237	0.162	0.027	0.574
White	0.435	0.270	0.511	0.336	0.149	0.032	0.483
Gender							
Male	0.429	0.261	0.293	0.361	0.149	0.032	0.459
Female	0.436	0.307	0.545	0.205	0.157	0.025	0.613
Family							
Married	0.433	0.276	0.412	0.310	0.109	0.024	0.557
Divorced	0.430	0.259	0.378	0.293	0.187	0.041	0.479
Never married	0.432	0.304	0.432	0.213	0.253	0.034	0.499
Education							
LTHS	0.437	0.293	0.354	0.264	0.120	0.015	0.601
HS	0.440	0.285	0.334	0.298	0.139	0.027	0.536
Some college	0.433	0.268	0.439	0.299	0.171	0.037	0.492
BA or higher	0.408	0.248	0.555	0.258	0.196	0.036	0.510
White HS	0.445	0.272	0.358	0.377	0.124	0.031	0.467
Black HS	0.433	0.291	0.303	0.230	0.157	0.023	0.590
White BA	0.402	0.255	0.650	0.261	0.199	0.033	0.508
Black BA	0.419	0.218	0.187	0.255	0.194	0.043	0.508

Notes: Data are from the 1976–2013 waves of the PSID. Sample contains 19- to 59-year-old adults who are either head of household or spouse. Family transfers are 1-year household totals (transfer reported by head, spouse, or both). A worker is unemployed if he/she reports at least 1 week of unemployment in a year. Replacement rates in Column 1 are calculated based on earnings in the year preceding unemployment and the state and year of unemployment. Replacement rates in Columns 2 and 3 are based on earnings in the year preceding unemployment and the reported transfer income (from UI or family) in the year of unemployment. LTHS, Less than high school; HS, high school; BA, bachelor's degree or higher.

PSID in the years that they reported an unemployment spell of at least 1 week. There are 36,269 spells in total. The first three columns show three replacement rates. Column 1 is the potential weekly UI benefit replacement, which is the weekly benefit amount calculated from UI laws on record in the state and the year in which the worker became unemployed and his/her wages, divided by the usual weekly wage the worker earned in the year prior to becoming unemployed, which is the same generosity metric used in previous UI research. If an individual does not have sufficient earnings to be eligible for UI, the potential benefit is zero. Those workers are included in this calculation.

The potential replacement rate, however, is not easily comparable to the amount of a reported family transfer. Hence, Columns 2 and 3 report the *observed* replacement rate of assistance income, which is the total UI benefit income, or total family transfer income, in the year of unemployment, divided by the highest two quarters of earnings in the previous year, conditional on receipt. Replacement rates must be interpreted cautiously, however, as they also reflect earnings in the previous period. The final four columns, viz., Columns 4–7, give the share of the unemployed population reporting receipt of the public UI benefits, private family transfers, both, or neither, in the year in which the individual was unemployed.

Across all unemployed workers in the sample (the first row), the potential UI benefit replaces 43.2% of usual weekly earnings. Among those who claimed and received UI, the total benefits were equivalent to 27.7% of two quarters of earnings, while family transfers amounted to 41.1% of two quarters of earnings. However, UI receipt was more common than family transfer receipt; 28.6% of unemployed workers reported receiving UI, 15.3% reported a family transfer, and just 2.9% reported both, while 53.3% reported neither. The table repeats these estimates for a large number of demographic subgroups.

There are three key upshots from this table. First, the replacement rate of UI benefits, whether the potential amount (Column 1) or the conditional observed amount (Column 2), is fairly uniform across the demographic groups shown. The weekly potential replacement rates range from a low of 40.8% for workers with a college degree to a high of 44.2% for younger workers; observed 6-month replacement rates range from 24.8% for college-educated workers to 31.9% for older workers. While the former reflects the wage-based benefit calculation, the latter reflects the same plus the duration of receipt. Regardless, they are tight bands across a large number of demographic groups, groups that span poverty, access to other in-kind benefits, consumption needs, and wages and income.

Second, in contrast to the uniformity of UI, family transfer generosity has high variation across subgroups. The family transfer replacement rate for black workers is 30.3%; for white workers, it is 51.1%. This is a rather incredible difference; observed family transfers replace half of white workers' previous 6-month earnings, nearly double what they replace from UI (27.0%). The difference between genders is equally large: 29.3% for male workers and 54.5% for female workers. And interestingly, family transfer replacement increases monotonically with age and with education. Indeed, families replace (1) 37.9% of 19- to 29-year-olds' earnings compared to 56.8% of 50- to 59-year-olds' earnings and (2) 35.4% of 6-month wages for workers with less than a high school degree compared to 55.5% for workers with a bachelor's degree.

Third, and key to understanding the results that I present later in this paper, despite the relative uniformity of UI *benefits*, reported UI *receipt* rate varies widely across the demographic

groups. UI receipt is more prevalent among older workers, increasing from 21.6% for workers aged 19–29 years to 37.6% for workers aged 50–59 years. UI receipt is also much more likely among (1) white workers, at 33.6%, compared to 23.7% for black workers; and (2) among male workers, at 33.6%, compared to 20.5% for female workers. Family transfer receipt can be said to follow an opposite pattern to UI receipt, but this does not hold for all groups. Younger workers are much more likely to get help from their family than older workers, with receipt rates of 20.9% for workers aged 19–29 years and 7.5% for workers aged 50–59 years, but there are smaller differences by race (16.2% for black and 14.9% for white) and gender (14.9% for male and 15.7% for female).

It is critical to stop here to circle back to the literature. All of the numerous effects of UI previously enumerated, from consumption smoothing to lower mortgage default, have identification derived from the variation in potential UI benefit generosity—Column 1. And although there is difference in individual replacement rates *within* groups, there is little difference *across* groups in average replacement. But now consider family transfer replacement—Column 3. The difference across groups spans 25 percentage points. This begs the question, if the marginal difference of potential UI replacement rates is shown to decrease mortgage default, what does a 20-point difference between black and white family transfer replacement do?

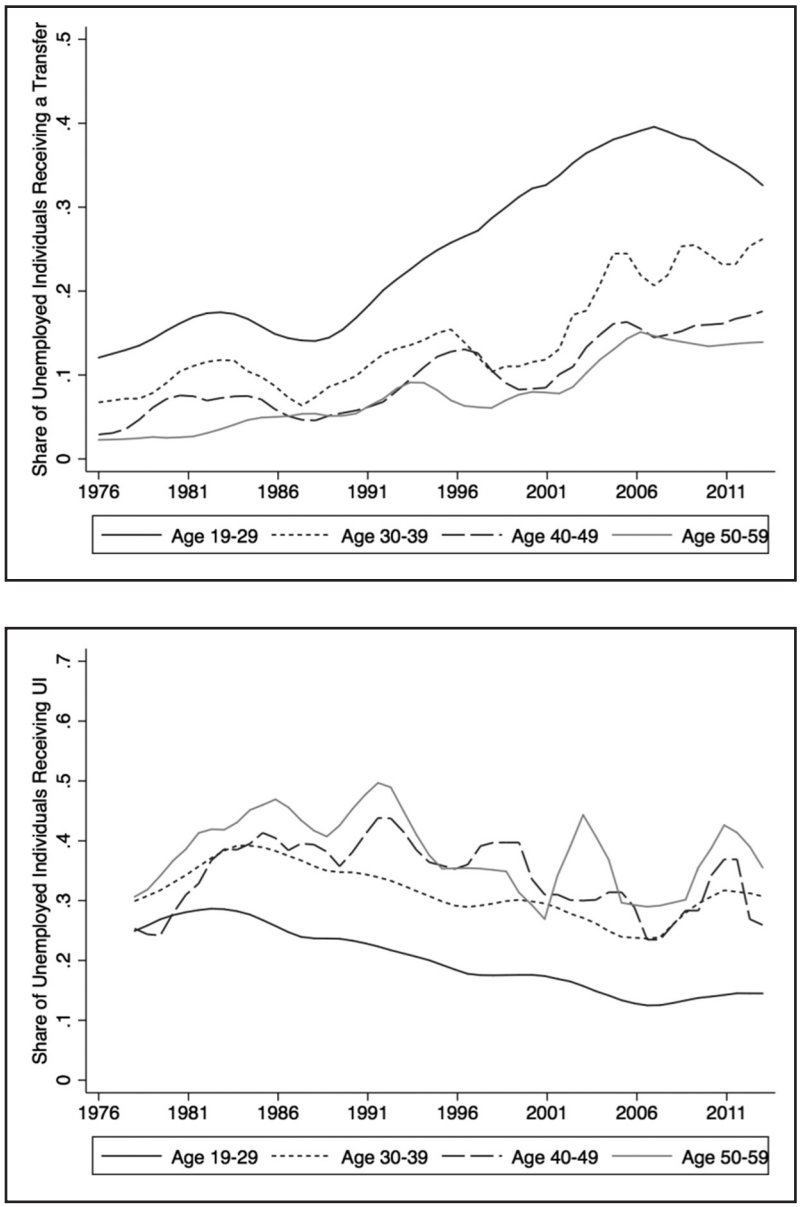
Table 2—and the inclusion of such a large number of demographic groups—illustrates the key distribution and prevalence questions that motivate the second research question in this paper, viz., studying the relationship between family transfers and public transfers from UI. As evident from Column 6, UI benefits and family transfers are mostly flowing to disparate populations. On average, across the demographic groups, just 3%–4% of the unemployed people report income from both. In addition, for nearly every group, around half of the unemployed people report receipt of neither.

Finally, the paper spans several decades. Figure 1 demonstrates graphically the context of family transfers and UI over the course of the scope of this study. The top panel shows the share of unemployed workers in the PSID sample who reported receiving a transfer from family members in the year they were unemployed. Receipt among all age groups began increasing in the latter half of the 1980s, but the increase is most stark for younger workers. By 2007, >40% of unemployed workers aged <30 years reported receiving help from family. The bottom panel shows the share of the same unemployed population reporting receipt of UI. There is some decline since the mid-1980s in the reporting of UI receipt, though again, this is most stark for workers <30 years of age, who had reported only 13% UI benefits in 2007. I will not provide an explanation of why these sources are increasing or decreasing; that is beyond the scope of the paper. However, these figures indicate that the source of assistance for unemployed workers is changing over time, and the potential effect of family transfers is more relevant in recent periods.

4 Empirical strategy

The first empirical task is to examine the relationship between unemployment and receiving a cash transfer from family and thereafter determine to what extent this relationship is causal. Using longitudinal data on an individual's receipt of cash assistance from family members and

Figure 1 Share of unemployed individuals receiving a transfer from family (top) and unemployment insurance (bottom), heads of household and spouses aged 19–59 years, 1976–2013 PSID.



Notes: Data are from the 1976–2013 waves of the PSID. Sample comprises 19- to 59-year-old adults who are either head of household or spouse. Family transfers are 1-year household totals (transfer reported by head, spouse, or both). A worker is unemployed if he/she reports at least 1 week of unemployment in a year.

employment history, the effect of an unemployment spell experienced by individual i on transfer receipt in year t can be empirically estimated in the following manner:

$$T_{it} = X_{it}\beta + \gamma U_{it} + \theta_i + \mu_t + \epsilon_{it}, \quad (1)$$

where T_{it} is transfer receipt by the household of individual i in year t and U_{it} is a binary variable indicating that an individual experienced at least 1 week of unemployment in year t. I examine two definitions of T_{it}, a binary indicator equal to "1" if a transfer was received and the real transfer amount; I use the same linear specification for both dependent variables. Again, given

the survey's wording and format, my analysis does not include in-kind transfers or services from family. X_{it} is a vector of time-varying individual covariates that may be correlated with the receipt of private family transfers, including those presented in Table 1: marital status, presence of children in the household, home ownership, school exit, presence of a disabled individual in the household, and local unemployment rate. I also include age and age squared. The individual fixed effect, θ_i, captures all observed and unobserved time-invariant individual characteristics, and μ_t controls for calendar year fixed effects.

The independent variable of interest is U_{it}. If the unemployment shock is exogenous, γ captures the causal effect of unemployment on the probability of receiving family cash transfers, but there are several concerns regarding treating U_{it} as exogenous. First, the values of T_{it} and U_{it} are based on calendar year totals and do not capture timing within the year. The data cannot differentiate, and therefore, the model cannot detect, whether unemployment precedes a transfer or whether a transfer precedes unemployment, or even whether the transfer or the spell is singular. Hence, I also perform an event study:

$$T_{it} = X_{it}\beta + \sum_{j=-3,3} \gamma_j U_{it+j} + \theta_i + \mu_t + \epsilon_{it}. \tag{2}$$

Rather than a single-period dummy, U_{it+j} is a vector of binary variables indicating that an individual is j years since an unemployment spell, which occurs at $j = 0$. Here, γ_j captures the annual time path of family transfer receipt before and after the year of unemployment. I examine 3 years before and after the spell and drop the years beyond that window to execute the regression.

If transfers precede spells in a large or significant way, γ_{-1} should be positive, due to the temporal cutoff between calendar years. For instance, if an individual received a transfer in December and then left a job in January, those events would span γ_{-1} and γ_0. However, even if γ_{-1} is zero, we still cannot rule out that transfers precede unemployment *within* a calendar year. In addition to testing the timing of transfers, another way to think about the event study is that the empirical setup in Equation (1) assumes that any effect of unemployment is limited to the calendar year in which the spell occurred. The results of the event study allow me to justify that assumption.

The second concern about the exogeneity of U_{it} is, regardless of the timing between the transfer and spell, individuals could be compelled to quit if they knew a transfer was available. *When* they receive the money is trivial, if they *knew* they would, or could, receive money if they asked for it. For this reason, I amend Equation (1) to separately examine γ when the unemployed workers cited layoff or firm closing as the cause of the spell:

$$T_{it} = X_{it}\beta + \gamma^D U_{it}^D + \gamma^V U_{it}^V + \gamma^N U_{it}^N + \theta_i + \mu_t + \epsilon_{it}. \tag{3}$$

Here, U_{it} is divided into three mutually exclusive and exhaustive variables: U_{it}^D, which is equal to "1" if the unemployed worker indicated that the spell was due to displacement (layoff or firm closing); U_{it}^V, which is equal to "1" if the unemployed worker indicated that the spell was voluntary; and U_{it}^N, which is equal to "1" for all remaining spells for which reason was not given. Roughly 29% of spells are due to displacement, 22% are voluntary, and 49% have no reason given.[3] Neither the event study specification nor including the reason for the unemployment

3 The PSID asks for the reason why the worker was unemployed but varies in *who* is asked for the reason for unemployment. In some years, all heads of household are asked, whereas, in other years, spouses are included, and in certain years, only those heads who were unemployed at the time of the survey are asked.

spell on the right-hand side proves causality. There is so much unobserved detail in the context of unemployment that even with these steps, there are still justifiable endogeneity concerns.

Finally, a separate issue is the endogeneity introduced from ex ante sorting of individuals into riskier jobs. The previous endogeneity concerns can be thought of as being related to whether the spell is random to the worker, whereas here, the concern is whether the worker is random to the job. Insurance encourages risk-taking behavior, even when it is informally provided by family (Miller and Paulson, 2007; Kaplan, 2012). An individual who knew that family support was available to him/her could decide to take a job in a riskier firm or industry, in which the arrival rate of unemployment shocks is higher, than he/she would have otherwise chosen. I am not directly controlling for this outside of the time-invariant risk-taking attributes captured by θ_i. This limits the interpretation of my results to be representative of the observed unemployed, and not all potential spells. To further understand the incidence of family financial transfers, I also perform the analysis in Equations (1) and (3) when the U_{it} dummy variables are further divided by the spell length, i.e., short (<4 weeks), medium (4–26 weeks), and long (>26 weeks).

The second empirical task is to examine the relationship between family transfers to the unemployed and UI, as well as the extent to which it is causal, which can be empirically modeled through the addition of an interacting term, as follows:

$$T_{it} = X_{it}\beta + \gamma U_{it} + \delta U_{it} * UI_{it} + \theta_i + \mu_t + \epsilon_{it}. \quad (4)$$

UI_{it}, similar to T_{it}, could be either the amount of UI benefits received, expressed as a replacement rate of weekly benefits over previous weekly wages, or a dummy for UI receipt. This analysis will use the latter. Although it should be the case that, on the margin, more UI should result in less family transfers, in practice, as shown in Table 2, only 3% of the unemployed report both family transfer and UI receipt. Hence, the margin for benefit generosity alone to alter family transfer receipt is small. Instead, my analysis tests the relationship between UI and transfers when U_{it} is a dummy for receiving UI.[4]

Although I observe reported receipt of UI benefits in the data, the decision to claim unemployment is endogenous. Hence, for identification, I need to instrument for UI_{it}, the receipt of benefits. As previously noted, research into the effect of UI has used as exogenous variation the state-by-year differences in benefit calculation to instrument for *benefit generosity*. The instrument uses the earnings histories of every unemployed worker in a given year and calculates the average replacement rate in every state in that year. An individual's replacement is instrumented with that state average. In my sample, this varies from 32% to 55%. Yet, although Blank and Card (1991) find that UI receipt is less likely in less-generous states, variation in replacement rates is insufficient in instrumenting for *benefit receipt*. Recall from Table 2, for instance, that UI benefit generosity is constant with age, from 44.2% replacement for workers < 30 years of age to 42.3% for workers aged >50 years, but UI receipt increases, from 21.6% to 37.6%.

Hence, I will add other measures of state-by-year variation in UI policy to the sample-calculated average replacement rates in the states. Eligibility requirements—the amount of time or earnings in the period before unemployment that an individual must accrue—also varies across states and over time. Individually, the earnings test is a low bar; around 92% of the

4 For this reason, this specification does not include a main effect for UI, since an individual cannot receive UI while employed. The alternative would be

$$T_{it} = X_{it}\beta + \gamma U_{it} + \delta U_{it} * UI_{it} + \nu UI_{it} + \theta_i + \mu_t + \epsilon_{it}. \quad (5)$$

workers in my sample meet the requirements in any given state and year, and eligibility is highly correlated with earnings, which may be correlated to receipt of family transfers. I will use the same method that previous research has used for individual replacement rates and thus calculate in each state and year the share of all unemployed workers in the sample from any state who had sufficient earnings to be eligible in that state. This varies from 83% to 99%. I also add a similarly calculated average weekly benefit amount, a dummy for whether the eligibility rules were tightened in that state and year, and the average UI tax rate in the state and year. Finally, I include – in X_{it} in Equation (4) – a five-knot linear spline in weekly wages. Both eligibility and benefits are a function of previous earnings; and I want to control for them flexibly.

The replacement rate, eligibility rate, rule changes, and average tax rate instrument for whether UI was received; however, the endogeneity concerns from before still apply: who is unemployed is not exogenous.

5 Results

5.1 Main effect, unemployment, and family transfers

Table 3 provides a summary of results from Equation (1). The first two columns show the coefficient estimates when T_{it} is a dummy variable, and the final two columns show the coefficient estimates when T_{it} is the real dollar amount of transfers received, including zeros. In the first column, γ is positive and precisely estimated at 0.039. Given that the base receipt rate is 8.0%, and in this definition, T_{it} is binary and Equation (1) is linear, this is equivalent to a 3.9 percentage-point increase in the probability of receiving a transfer in the year a worker experiences an unemployment spell, or a 49% increase. When T_{it} is the dollar amount of transfer, γ is also positive and precisely estimated at $71. Given that the mean transfer amount is $243, this is equivalent to a 29% increase in transfers received.

Included in the controls are the other life events from Table 1: becoming a homeowner, getting married, getting divorced, becoming a parent, and leaving school. Also included in the controls are the corollary statuses: being a homeowner, married, divorced, a parent, a student, or disabled. The former variables identify the transfer response to the change in status, and the latter reflect the transfer response to the status. For example, transfer receipt increases in the year in which an individual buys a home (0.006), but transfer receipt is less likely among homeowners (–0.016). Table 3 also shows a positive relationship between transfers and the state unemployment rate (0.002) and a negative relationship between transfers and age (–0.012). Included in the regression but not shown are the following: age squared; dummies indicating whether the individual has a deceased parent, a working parent, or lives in the same state as a parent (none were significant); and individual and year fixed effects. Standard errors are clustered at the household level.

In the second column, I show the results from Equation (2) when U_{it} is divided into three variables: one indicating unemployment due to layoff or firm closing (grouped as "displaced"); one indicating voluntary unemployment ("voluntary"); and the third category grouped as "not specified". The three coefficient estimates, viz., 0.037, 0.047, and 0.034, are all positive and precise. The results are similar in Column 4 when T_{it} is the dollar amount of transfers. When separately estimated for displaced and voluntary unemployment, the coefficient estimates are $49, $67,

Table 3 Coefficient estimates from regression of family transfer receipt on unemployment spell, heads of household and spouses aged 19–59 years, 1976–2013 PSID

	Binary transfer receipt		Real transfer amount (in $2013)	
	(1)	(2)	(3)	(4)
Unemployment (U)	0.039*** (0.002)		70.921*** (12.284)	
U displaced		0.037*** (0.004)		49.332** (16.624)
U voluntary		0.047*** (0.004)		66.902*** (20.292)
U not specified		0.034*** (0.003)		81.649*** (17.184)
Becomes homeowner	0.006* (0.003)	0.006* (0.003)	219.624*** (35.85)	219.577*** (35.848)
Gets married	0.017*** (0.005)	0.017*** (0.005)	5.077 (39.528)	5.084 (39.535)
Gets divorced (separated)	0.004 (0.005)	0.004 (0.005)	27.743 (31.09)	27.701 (31.089)
Becomes a parent	0.004 (0.003)	0.004 (0.003)	−11.928 (28.081)	−11.911 (28.078)
Leaves school	0.071*** (0.006)	0.071*** (0.006)	394.865*** (56.53)	394.850*** (56.543)
Becomes disabled	−0.01 (0.007)	−0.01 (0.007)	−4.644 (32.211)	−5.056 (32.207)
Homeowner	−0.016*** (0.002)	−0.016*** (0.002)	1.389 (14.459)	1.3 (14.463)
Married	−0.061*** (0.004)	−0.061*** (0.004)	−83.636** (25.677)	−83.729** (25.67)
Divorced/separated	−0.013* (0.005)	−0.013* (0.005)	16.409 (31.815)	16.352 (31.813)
Has children	0.009*** (0.002)	0.009*** (0.002)	59.668*** (17.249)	59.755*** (17.247)
Student	0.050*** (0.007)	0.049*** (0.007)	234.176*** (57.261)	234.371*** (57.218)
Disabled HH member	0.027*** (0.004)	0.027*** (0.004)	53.140** (18.225)	52.949** (18.221)
State UR	0.002** (0.001)	0.002** (0.001)	9.492** (3.445)	9.553** (3.443)
Age	−0.012*** (0.001)	−0.012*** (0.001)	−34.083*** (4.792)	−34.217*** (4.794)
Other X_{it}	Y	Y	Y	Y
Individual and year fixed effects	Y	Y	Y	Y
Mean of dependent variable	8.0%	8.0%	$242.99	$242.99
N	278,794	278,794	278,794	278,794

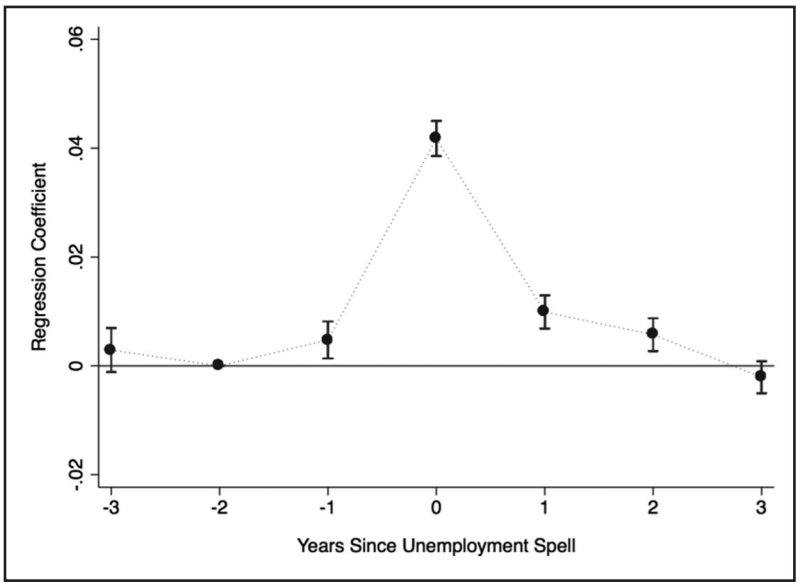
Notes: Data are from the 1976–2013 waves of the PSID. The sample comprises 19- to 59-year-old adults who are either head of household or spouse. Family transfers are 1-year household totals (transfer reported by head, spouse, or both). A worker is unemployed if he/she reports at least 1 week of unemployment in a year. Table shows coefficient estimates and standard errors in parentheses for four regressions. Covariates included but not shown are age squared; dummies indicating whether the individual has a deceased parent, a working parent, or lives in the same state as a parent; as well as the individual fixed effects and year fixed effects. Standard errors are clustered at the household level
HH, household; UR, unemployment rate.
*1%, **5%, ***10% level.

and $82, respectively. The increase in the probability of receiving a transfer in the year in which an individual becomes unemployed is not singular to any one type of employment dissolution. The results are positive and precise for all unemployed workers.

In Figure 2, I graphically present the coefficient estimates for the event study of Equation (3) when T_{it} is the binary variable.[5] There is not a significant increase in the probability of receiving a transfer from family until the year of the spell, when the coefficient spikes. It remains slightly positive in the year immediately following the spell and then falls in the years following. From this, I can conclude that, on an average, among unemployed

5 They are, beginning with $j = -3$: 0.003 (0.004), $j = -2$ omitted, $j = -1$ 0.005 (0.004), $j = 0$ 0.042 (0.003), $j = 1$ 0.009 (0.003), $j = 2$ 0.006 (0.004), $j = 3$ −0.002 (0.003).

Figure 2 Coefficients from regression of family transfers on years since unemployment, heads of household and spouses aged 19–59 years, 1976–2013 PSID.



Notes: Data are from the 1976–2013 waves of the PSID. Sample comprises 19- to 59-year-old adults who are either head of household or spouse. Family transfers are 1-year household totals (transfer reported by head, spouse, or both). A worker is unemployed if he/she reports at least 1 week of unemployment in a year. Figure shows coefficient estimates of regression of family transfer on time since unemployment; regressions include covariates, individual fixed effects, and year fixed effects.

individuals, the increased likelihood of receiving a transfer does not precede the spell, at least across calendar years.

In Figure 3, I show similar estimates, but for the years preceding and following a displaced spell (top) and voluntary spell (bottom); each type of spell—displaced, voluntary, and not specified—enters the right-hand side as an event study. For brevity, I do not show the results for "not specified" spells, as they are very similar to the main effect.[6] Here, the estimates diverge. For displaced spells (recall that this proportion is 29% of all observed spells), there is not an increase in the probability of receiving a transfer until the year of the spell, and the coefficient estimates fall in the years following. But for voluntary spells (recall that this is 22% of all observed spells), there is a significant increase in the year before a spell, as well as a spike in the year of the spell.[7]

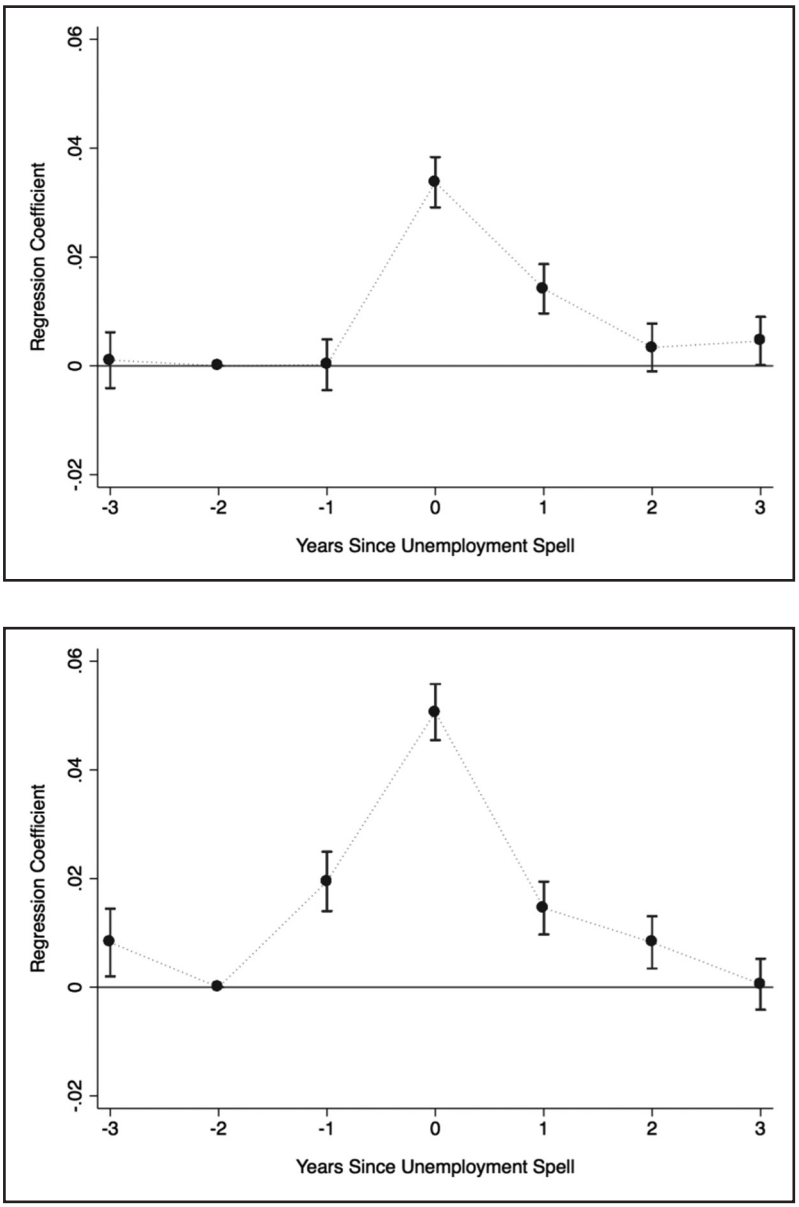
There is clearly a positive correlative relationship between being unemployed and receiving family transfers. Now, the question is how to interpret the results from the main specification and event studies in terms of causality. Based on the displaced spells, I could make a causal claim: workers indicate that the spell was due to layoff or firm closing, a classic unemployment shock; the specification includes any invariant individual attributes captured

6 Specifically:

$$T_{it} = X_{it}\beta + \sum_{j=-3,3} \gamma_j^D U_{it+j}^D + \sum_{j=-3,3} \gamma_j^V U_{it+j}^V + \sum_{j=-3,3} \gamma_j^N U_{it+j}^N + \theta_i + \mu_t + \epsilon_{it}. \quad (6)$$

7 Full coefficient estimates are as follows: for displaced spells, beginning with $j = -3$: 0.001 (0.005), $j = -2$ omitted, $j = -1$ 0.002 (0.005), $j = 0$ 0.032 (0.005), $j = 1$ 0.013 (0.004), $j = 2$ 0.002 (0.004), $j = 3$ 0.005 (0.004); for voluntary spells, the coefficients are as follows: beginning with $j = -3$: 0.007 (0.006), $j = -2$ omitted, $j = -1$ 0.018 (0.005), $j = 0$ 0.049 (0.005), $j = 1$ 0.012 (0.005), $j = 2$ 0.007 (0.005), $j = 3$ −0.001 (0.004).

Figure 3 Coefficients from regression of family transfers on time since unemployment, spell due to displacement (top) and voluntary spell (bottom), heads of household and spouses aged 19–59 years, 1976–2013 PSID.



Notes: Data are from the 1976–2013 waves of the PSID. Sample comprises 19- to 59-year-old adults who are either head of household or spouse. Family transfers are 1-year household totals (transfer reported by head, spouse, or both). A worker is unemployed if he/she reports at least 1 week of unemployment in a year. The regressions of family transfer on time since unemployment include covariates, individual fixed effects, and year fixed effects.

in θ and controls for other events that may instigate transfer as well as trends over time; transfers are not shown to precede unemployment in the previous year and the increase in the year of the spell was precise and positive. Hence, there is a causal effect of unemployment on transfer receipt. Based on the voluntary spells, I could easily refute that claim: the fullest set of controls offered by the data, regardless of individual fixed effects, cannot rule out omitted variable bias; the event study cannot speak to within-year timing of spells and transfers;

and the increase in the year before the spell was *also* positive and precise, in addition to the year of the spell. Hence, unemployment does not cause receipt of family transfers; instead, there is evidence that transfer receipt causes unemployment.

One problem with either of these interpretations is that the data say nothing about what is communicated to family members. While an individual's unemployment status is likely known or communicated to those individuals who could potentially send cash help, we cannot assume that the *reason* for the unemployment spell is equally known or shared.

However, the more accurate assessment is that I do not have the means within this empirical setting to separately identify exogenous versus endogenous family transfer responses. I present them side by side, as opposed to privileging one over the other, to be clear about this. In some cases, a member's unemployment is a random shock to both himself/herself and his/her nonresidential family; in response, the family sends cash, which he/she was not expecting or planning. In other cases, he/she may have known that they would have sent money, should unemployment occur, though the spell itself was still a shock. Or, he/she may have known that they would have sent money, so he/she instigated the spell himself/herself. Or, something else could have happened, such as a mental health shock, which caused both the transfer and the unemployment. Given what I have described, the main effect γ should be assumed to include all four of those cases.

5.2 **Heterogeneity of the main effect**

The main effect imposes no sample restriction on either the individuals included (who are all heads and spouses aged 19–59 years in the years in which they are observed) or the spell, so long as it is at least 1 week. The PSID is a rich data set that supports myriad possibilities of how to compare the main effect in a subgroup of individuals or spells.[8] I present three: demographics, spell length, and transfer receiver.

Starting with demographics, in Table 4, I rerun Equation (1), the simpler model, for demographic subgroups of the population shown in Table 3. Every subgroup has a positive and precisely estimated γ when measured through the binary T_{it}; there is no group in the population for whom unemployment is not associated with an increase in the probability of receiving a family transfer.[9] In addition, I show the subsample transfer receipt mean and the calculated percentage increase in the likelihood of receiving a transfer.

Most of the estimates of γ across the subgroups are within the standard errors of the mean effect of 0.039. Those with smaller coefficient estimates are older workers aged 40–49 years (0.023) and 50–59 years (0.015), married workers (0.027), and workers with less than a high school degree (0.022). Those with larger coefficient estimates are workers with a BA or higher (0.061). For all groups, unemployment is associated with at least a 30% increase in the probability of receiving a transfer—the highest increase observed is among college graduates, i.e.,

8 Previous presentations and journal reviews have inquired about the following: demographics, labor force attachment of worker, lagged labor force attachment of worker, presence of other workers in the household, wages, geographic proximity to parents, industry, occupation, length of spell, number of spells experienced by the worker, number of spells within the household, unemployment rate during the year of the spell, spells during recessions versus expansions, year of spell, number of siblings, birth order, number of children, ages of children, and home ownership status of parents, among others.

9 The results follow a very similar pattern when using T_{it} dollar amount; omitted for brevity and available from the author.

Table 4 Coefficient estimate of regression of family transfer receipt on unemployment, by demographic subgroup, heads of household and spouses aged 19–59 years, 1976–2013 PSID

Group	γEstimate	Mean	Percentage increase	Group	γEstimate	Mean	Percentage increase
Aged 19–29 years	0.044***	0.14	31%	Married	0.027***	0.053	51%
SE	0.004			SE	0.002		
Aged 30–39 years	0.034***	0.068	50%	Divorced	0.050***	0.108	46%
SE	0.004			SE	0.008		
Aged 40–49 years	0.023***	0.049	47%	Never married	0.054***	0.178	30%
SE	0.004			SE	0.006		
Aged 50–59 years	0.015**	0.039	38%	Less than HS	0.022***	0.072	31%
SE	0.005			SE	0.005		
Male	0.043***	0.076	57%	HS degree	0.036***	0.071	51%
SE	0.003			SE	0.003		
Female	0.035***	0.083	42%	Some college	0.039***	0.089	44%
SE	0.003			SE	0.004		
Black	0.037***	0.093	40%	BA or higher	0.061***	0.086	71%
SE	0.004			SE		0.006	
White	0.041***	0.073	56%				
SE	0.003						

Notes: Data are from the 1976–2013 waves of the PSID. Sample includes 19- to 59-year-old adults who are either head of household or spouse. Family transfers are 1-year household totals (transfer reported by head, spouse, or both). A worker is unemployed if he/she reports at least 1 week of unemployment in a year. Table shows coefficient estimates and standard errors (SEs) of 15 panel regressions. Covariates included when appropriate but not shown are marital status; disability status; home ownership; presence of children in the household; state unemployment rate; age; age squared; dummies for whether a parent is dead, working, or living in the same state; and individual and year fixed effects. Standard errors are clustered at the household level.

*1%, **5%, ***10% level.

71%. We do not explore the reason or mechanisms for these differences. Unemployment may be one of a suite of shocks or stressors that an individual experiences; an individual's family may/may not have significant resources and may/may not feel comfortable supporting an adult financially. Given that every subgroup has a positive and precisely estimated increase, the key takeaway is just how prevalent cash support from family is among demographic groups.

In Table 5, I examine family transfer receipt by length of unemployment spell, viz., short (<4 weeks), medium (4–26 weeks), and long (>26 weeks). I do this for all spells, as well by reason for the spell, and repeat the analysis when T_{it} is the real dollar amount. In interpreting the length estimates, although I refer to a single spell of a certain length, it is in fact a spell-year, and the total weeks unemployed in that year. The length could potentially encompass a single spell or multiple spells. The coefficient estimates increase with the length of unemployment; short spells have an estimate of 0.027; medium spells have an estimate of 0.035; and long spells have an estimate of 0.065. The coefficient increases with the number of weeks unemployed within each reason for unemployment, as well as for displaced, voluntary, and not specified spells. In the second two columns, the estimates for the dollar amount of family transfer increase are too small to be precise for short spells, precise for all but displaced medium spells, and the largest for the longer spell, ranging from $91 for voluntary to $122 for no reason given.

Table 5 Coefficient estimate of regression of family transfer receipt on unemployment, by length of spell, heads of household and spouses aged 19–59 years, 1976–2013 PSID

	All spells	Displaced	Voluntary	Not specified
	(1)	(2)	(3)	(4)
T_{it} = Dummy				
Short spell	0.027***	0.022**	0.049***	0.015**
SE	0.004	0.008	0.009	0.005
Medium spell	0.035***	0.032***	0.045***	0.029***
SE	0.003	0.004	0.005	0.003
Long spell	0.061***	0.059***	0.052***	0.065***
SE	0.004	0.007	0.01	0.006
T_{it} = Amount				
Short spell	44.339	–19.442	82.353	48.26
SE	24.406	31.863	50.449	34.488
Medium spell	61.758***	35.02	54.571*	78.637***
SE	14.87	20.28	23.203	22.913
Long spell	118.278***	118.122***	91.879*	122.748***
SE	19.319	28.926	41.3	26.474

Notes: Data are from the 1976–2013 waves of the PSID. Sample contains 19- to 59-year-old adults who are either head of household or spouse. Family transfers are 1-year household totals (transfer reported by head, spouse, or both). A worker is unemployed if he/she reports at least 1 week of unemployment in a year. Table shows coefficient estimates and standard errors (SEs) of four panel regressions. Covariates included but not shown are marital status; disability status; home ownership; presence of children in the household; state unemployment rate; age; age squared; dummies for whether a parent is dead, working, or living in the same state; and individual and year fixed effects. Standard errors are clustered at the household level.

*1%, **5%, ***10% level.

The length of unemployment is highly endogenous to family transfer receipt. Yet, the observation that longer unemployment spells are associated with a higher likelihood of receiving money from family has significant implications for the optimal benefit design literature and the job search literature. If the causality direction is from unemployment, and families respond to longer spells, then these findings would imply that the search length supported by UI is insufficient. If the causality is from the other direction, and family assistance enables longer spells, then these findings imply that the length of time committed to job search and matching varies with *parents*' wealth, income, and willingness to financially support a child's spell, introducing a distortion into job search, which has clear racial and intergenerational mobility implications.

Finally, in Table 6, I redefine the dependent variable. Currently, T_{it} is measured on the household level, summing the transfers that were reported by either the head or the spouse. However, the key dependent variable U_{it} is measured on the individual level, and the individual, not the household, is the basis for observation. This procedure leaves the possibility that the head became unemployed but the spouse received the transfer, or vice versa. I test three alternatives to this. First, rather than household transfers, I regress transfers to the individual on the individual's unemployment, whether they are the head or the spouse, and any transfers to the other partner are ignored. Then, I perform a similar regression but look at heads and

Table 6 Coefficient estimate of regression of family transfer receipt on unemployment, by variable construction, heads of household and spouses aged 19–59 years, 1976–2013 PSID

	Binary transfer receipt			Real transfer amount (in $2013)		
	Own	Head	Spouse	Own	Head	Spouse
	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment	0.036***	0.044***	0.006	67.603***	64.481***	1.456
SE	0.002	0.003	0.002	9.979	11.479	2.955
Other X_{it}	Y	Y	Y	Y	Y	Y
Individual and Year fixed effects	Y	Y	Y	Y	Y	Y
Mean of dependent variable	6.0%	7.3%	0.9%	$162.60	$209.62	$33.38
N	278,794	173,125	105,669	278,794	173,125	105,669

Notes: Data are from the 1976–2013 waves of the PSID. Sample comprises 19- to 59-year-old adults who are either head of household or spouse. Family transfers are 1-year household totals (transfer reported by head, spouse, or both). A worker is unemployed if he/she reports at least 1 week of unemployment in a year. Table shows coefficient estimates and standard errors (SEs) of six panel regressions. The samples vary by construction of the dependent variable (transfer to self versus transfer to household) and then receiver/unemployed worker. Covariates included but not shown are marital status; disability status; home ownership; presence of children in the household; state unemployment rate; age; age squared; dummies for whether a parent is dead, working, or living in the same state; and individual and year fixed effects. Standard errors are clustered at the household level.

*1%, **5%, ***10% level.

spouses separately. Of the 36,269 unemployment spells, 26,783 are experienced by heads and 9,486 by spouses. Note, the PSID defaults head and spouse assignment by gender; a head could be male or unmarried female, but spouses are female.

The restriction to the individual—that the unemployment spell and family transfer are experienced by the same person—shown in Columns 1 (binary) and 4 (dollar amount), does not much change the estimate compared to the main effect, at 0.036 and $68. Heads account for the majority of unemployment spells. The coefficient estimate for the heads' family transfers is positive and precise, at 0.044 and $65, but the coefficient for spouses' family transfers is not. In the next section, I will examine the relationship between family transfers and UI. The results in Table 6 establish that family transfers are going to the unemployed worker, and not the unemployed household.

5.3 **Family transfers and UI**

Table 7 provides a summary of results from Equation (4); the top panel shows the coefficient estimates when T_{it} is a dummy variable, and the bottom panel shows the coefficient estimates when T_{it} is the real dollar amount received. In the first column, UI_{it} is the reported receipt of UI among the unemployed in the survey. As noted previously, using the reported receipt is clearly endogenous, but it does inform whether UI and family cash transfers are positively or negatively correlated. The increase in the probability of receiving a family transfer is again precisely estimated at 0.047, but the interacting term is negative and precise at –0.026; an individual is

Table 7 Coefficient estimates of regressions of family transfer receipt on unemployment insurance receipt, actual and instrumented, heads of household and spouses aged 19–59 years, 1976–2013 PSID

	Observed	IV	IV	IV
	All years	All years	1976–1995	1996–2013
	(1)	(2)	(3)	(4)
Unemployment	0.047***	0.046***	0.035***	0.057**
SE	0.003	0.01	0.007	0.018
U*UI	−0.026***	−0.022	−0.013	−0.042
SE	0.004	0.028	0.021	0.050
X_{it}	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y
State × Year fixed effects	Y	Y	Y	Y
Wage spline	Y	Y	Y	Y
N	225,298	225,298	104,434	120,864
Unemployment	81.996***	126.348	104.796*	268.452
SE	16.88	81.71	42.05	171.039
U*UI	−44.413	−172.286	−199.33	−504.385
SE	23.553	233.246	120.452	480.321
X_{it}	Y	Y	Y	Y
Individual fixed effects	Y	Y	Y	Y
State × Year fixed effects	Y	Y	Y	Y
Wage spline	Y	Y	Y	Y
N	225,298	225,298	104,434	120,864

Notes: Data are from the 1976–2013 waves of the PSID. Sample comprises 19- to 59-year-old adults who are either head of household or spouse. Family transfers are 1-year household totals (transfer reported by head, spouse, or both). A worker is unemployed if he/she reports at least 1 week of unemployment in a year. Table shows coefficient estimates and standard errors (SEs) of eight regressions. Covariates included but not shown are marital status; disability status; home ownership; presence of children in the household; state unemployment rate; age; age squared; dummies for whether a parent is dead, working, or living in the same state; and individual and year fixed effects. Standard errors are clustered at the household level.

*1%, **5%, ***10% level.

more likely to receive a transfer from family in the year he/she becomes unemployed but is relatively less likely to receive one if he/she reports UI in the same year.

The next column shows the estimates when, rather than the reported receipt, UI receipt is instrumented using the average replacement rate, benefit amount, eligibility rate, indicator for stricter eligibility, and average tax rate in the state and year.[10] The coefficient estimate of γ is precisely estimated at 0.046, but the instrumental variable (IV) δ is imprecise, at –0.022. In the third and fourth columns, I perform the same IV regression from Column 2, but on the subsets of the following years: 1976–1995 and 1996–2013. My motivation for this is as follows: first, as I show in my data, the receipt rates of both UI and family transfers during this period are changing; and, second, East and Kuka (2015) find that the efficacy of UI, as measured through consumption smoothing, erodes over this time period. In the first period, the coefficient

10 The instrument performs well—the first-stage Cragg–Donaldson *F*-statistic is 154. First-stage results are omitted for brevity but available from the author.

estimate on unemployment is 0.035, and the instrumented UI receipt is negative but imprecise, at –0.013. In the second period, the main effect is larger, at 0.057, and the instrument at –0.042, still imprecise. Again, the results are similar when T_{it} is the dollar amount. The coefficient estimate on the interacted IV term is negative but imprecise. Without significant estimates, it is not possible to draw an inference from the analysis; the suggestion is that the predicted UI receipt reduces the increased likelihood of parental transfers during unemployment.

The sample of 19- to 59-year-old heads and spouses makes no condition or restriction on work history or labor force attachment—this would mean introducing selection into the sample. However, even if I were to reduce the sample to workers only, who had earnings in the year preceding the spell and indicated that the reason for the spell was displacement, making it more likely that they would be eligible for UI, there is still not a precisely estimated coefficient.

An imprecise or null result is unsatisfying in any context but somewhat surprising here. UI is the primary source of cash assistance targeted to unemployed workers in the USA. I find only weak support that if it is more generous or easy to get, it tempers the increased likelihood of receipt of cash support from family. Two points on interpretation: first, it could be that there is a preference for family support over public support, due to some combination of stigma or pride, and hence the marginal details of UI are not predictive of family behavior, because the preference is over the source and not the quantity of benefits. Second, my (lack of strong) findings are in line with those of East and Kuka (2015): UI is not as effective a program as it once was. Support for this conclusion can be found in the declining take-up in Figure 1, or the demographic variation in receipt in Table 2. UI is common among older, high school-educated white men; receipt is less about potential benefit and more about an unobserved measure of access and program knowledge.

6 Robustness

I discuss identification and causal interpretation throughout the paper and do not repeat them here. Instead, the key concern with the results I present is the reliability of the self-reported transfer data. Meyer et al. (2015) show the extent to which self-reported income data from public sources is under- and misreported in household surveys. If family transfers are underreported, the results presented here could be understating the effect of unemployment on receiving a transfer. Critically, if family transfers are underreported and reporting varies with employment status, the results presented here are biased.

Fortunately, the PSID allows for an internal validity check of transfer reporting. In addition to including a question regarding cash transfers received from family in the annual survey waves, the survey also includes questions regarding cash transfers *sent* to family. In this case, the questions are more specific, as they ask whether the outflow is part of alimony or child support, and to whom it is intended, whether a child, parent, or other family member. Edwards and Wenger (2019) link parents to children and regress parents' giving of transfers on a child's unemployment spell in the PSID in a similar regression as the main specification.[11] The parents' reported transfer sent is substituted for the child's reported transfer received; the remaining part of the specification, such as individual (child) fixed effects and covariates, is the same. They

11 Specifically, $T_{pt} = X_{ct}\beta + \gamma U_{ct} + \theta_c + \mu_t + \epsilon_{ct}$.

find that in the year of a child's unemployment, the probability of sending a transfer increases 1.8 percentage points, off of a mean of 7.0%, or a 26% increase, and a $77 increase in the amount sent, off of a mean of $329, or an increase of 23%.[12] And, as noted, when McGarry (2016) performs a similar regression using the parents with observed children in the HRS using child fixed effects, she finds a similar increase in the likelihood of receiving a transfer in the year in which a child is out of work.

In sum, separate analyses, using reported transfers sent as the dependent variable, found a similar result—a positive and precise increase—but to a smaller degree. That I would find larger results is not unexpected. The measure of family transfer receipt in the PSID, which supports the analysis in this paper, does not specify which family member it is from and could include assistance from siblings, grandparents, aunts, uncles, and cousins, in addition to, or in lieu of, parents. White and Riedmann (1992) and Eriksen and Gerstel (2002) discuss the importance of sibling relationships into adulthood, and Casper and Bryson (1998) and Casper and Bianchi (2001) describe the importance of grandparents, but none estimate cash financial flows. Given the lack of data measuring financial transfers and ties outside of the parent–child relationship, I cannot speak directly on what share, in general, of financial transfers from family come from individuals other than parents, or in particular, whether this share changes during an unemployment spell.

Another way to check the reliability of reported transfers is to use the family transfer topical module in the PSID, which was most recently fielded in 2013. The module also asks about the sending and receiving of transfers between parents and children. In the 2013 file, 65% of transfers reported in the topical module were reported in the survey wave, meaning that 35% of transfers were not reported in the core survey. This is of great concern if unemployment varies with the reporting of transfers. I find that it does slightly. The share of underreporting transfer receivers (i.e., the 35% who did not report a transfer in the core survey but *did* report a transfer in the topical module) who are unemployed is 24.9% (standard deviation [sd]: 0.43%) and the share of core-reporting transfer receivers is 28.9% (sd: 0.45%).

In this paper, I address two research questions—how family cash transfers vary with unemployment status and, further, how family cash transfers to the unemployed vary with UI. I show that there is a clear correlative relationship between being unemployed and receiving a cash transfer from family. I discuss the evidence for how these phenomena could or could not be causally related, as well as the direction in which the causality could point. Separately, I show that despite flowing to a diverse portion of the unemployed, there is no similarly clear relationship with UI benefits. Taken together, my findings, in accordance with the works of Edwards and Wenger (2019) and McGarry (2016), as well as the check from the topical module on transfers, indicate that my estimates are an upper bound of transfer receipt among unemployed individuals.

7 Discussion

Money flows to unemployed workers from UI. Previous research has used plausibly exogenous variation in benefit amount to show the effects that money has on unemployed workers, from

12 Tables 1 and 3 in the work of Edwards and Wenger (2019).

search to basic welfare. This paper shows that money also flows to unemployed workers from their family. With only limited exogenous variation from displacement, I cannot establish that this is a causal relationship; families might not know and seem not to care about the reason for the spell, and they support spells of any reason, of any length, for all demographic groups.

How important is this family source of cash to the unemployed? That question can be considered in three contexts, each a motivation for follow-on work. The first is a question of size and how family cash transfers are important relative to other sources of family support. A cash transfer is only one form of assistance; there are others ways in which family can similarly, or dissimilarly, respond to an individual's unemployment, such as coresidence, in-kind gifts, or time transfers. There is no indication in this survey or any other data that cash transfers are even the largest form of family assistance, nor is it well understood whether there is an ordering to the type of family assistance received, or whether substitution between types of assistance occurs. Hence, it is not known whether cash transfers are predictive, either positively or negatively, of receiving other forms of assistance. This paper motivates further investigation into the composition of family assistance.

The second context is a question of role and the effect that family transfers have on unemployed individuals' behavior and outcomes. I show here that UI and family transfers are flowing to separate parts of the unemployed population. If the finding that transfers are more likely to be received during unemployment is taken as evidence that some workers' labor market shocks are partially absorbed by their family, then the implications for job search, consumption smoothing, and other behaviors known to be affected by cash from public sources should be investigated. For example, Kaplan (2012) models how moving back home changes the process of job search for 19- to 21-year-old men without a college degree. This paper shows that another form of assistance from family flows to all ages and educational levels and motivates whether the same type of job search effects that Kaplan (2012) found for young men applies to the groups of receivers I document here.

The third context is a question of distribution and the effect that family transfers can have on inequality if certain groups are more, or differently, subsidized than others. The combination of differences in receipt and generosity of UI and family benefits can be significant. To illustrate this, at the bottom of Table 2, I include four race-by-education subgroups, black and white high school and college graduates. White high school graduates have the highest receipt rate of UI benefits among any group that I examine, at 37.7%; this is likely attributable in part to historically higher union membership (Vroman, 2009; Blank and Card, 1991). Black high school graduates only report 23.0% receipt rates of UI. White college graduates have a family transfer observed replacement of 65.0%. Black college graduates have replacement of only 18.7%. At both education levels, white individuals have an advantage in observed subsidy, whether more likely to receive UI or receiving more money from family. Family transfers have been shown to determine wealth and play a role in wealth inequality, shown most recently by De Nardi (2004). These disparate and disproportional flows to the unemployed motivate the study of whether family transfers play a role in labor market inequalities.

Declarations

Availability of data and materials

The data sets generated and analyzed during the current study are available in the repository of the Institute for Social Research at the University of Michigan, https://psidonline.isr.umich.edu/.

Competing interests
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