Intergenerational Transfers, Wealth, and Job Search Behaviour

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Abstract

This paper analyzes jointly the effects of *own* wealth and *parental* wealth on job search behaviour. To study the role of individual wealth we exploit the quasi-random timing of the 2008 economic stimulus payments in the US. We find that the increase in liquid wealth lowered job finding rates and increased reemployment wages, especially for lower wage and younger individuals. To study the role of parental wealth we use data from the 1979 National Survey of Youth, as well as its follow-up child and young adult survey. First, we find that parental inter-vivos transfers depend on both the (adult) child's employment status and the income of the parents. Second, we estimate the effect of parental income on job search behaviour. In the cross-section, we find that the correlation between parental income and job search behaviour is different from the exogenous wealth shock: richer parents tend to be associated with higher job finding rates as well as higher reemployment wages, even after controlling for a rich set of characteristics. However, when estimating the effect of a job loss of a mother on the job search behaviour of her (adult) children we do find a positive effect on the job finding hazard and a negative effect on the occupational rank of the new job. This effect is stronger for individuals with deceased or absent fathers. We link these findings to theoretical predictions from the job search literature and argue that they highlight the importance of intergenerational insurance for job search behaviour.

Keywords: Job search, Wealth, Inter-vivos transfers, Intergenerational insurance

JEL: E21, E24, J62, J64

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1 Introduction

The role of wealth in job search behaviour has become of increasing interest in labour economics. When coupled with the realistic assumptions of decreasing absolute risk aversion (DARA) and a constraint on borrowing, standard theories of job search – including both the random search and directed search frameworks – predict that wealth should affect a job seeker's optimal decision in terms of search effort, target wage and reservation wage.[1] In this paper we investigate how findings of the effects of individual wealth on job search may generalize to *parental* wealth. Noting that much of an individuals career trajectory is determined in early-life, and that many individuals at this point still receive support from their parents, we suggest that findings on the importance of individual wealth may also apply to family wealth. However, to further our knowledge on the link between parental wealth and job search decisions of (adult) children, more empirical research is needed. In particular, two key links must be established: (i) how does individual wealth affect job search? And (ii) how is wealth transferred across generations. In this paper we contribute new knowledge into both of these questions, as well as novel findings on the direct impact of parental income on children's job search behaviour.

To analyze the effect of individual wealth on job search we make use of the quasi-random allocation in the timing of the 2008 US stimulus payments, which were paid out to most US households as a means of averting the impeding recession. These transfers were largely based on the last two digits of an individual's social security number, and hence represent a plausibly exogenous wealth variation, which we use to analyze the effects on recipients' job search behaviour. To do so, we use data from the 2008 Survey of Income and Program Participation (SIPP), which is a nationally representative survey. The broad reach of the stimulus payments, as well as our dataset, allows us to analyze effects of wealth on job search in a more theoretically relevant setting than previous empirical literature. Earlier work on effects of unconditional wealth transfers on job search (i.e. not state-dependent transfers, such as unemployment insurance) has typically relied on variation in severance payments (Card et al. 2007, Chetty 2008). We believe that studying a broader wealth shock provides important new insights, as receivers of severance payments belong to a select group that are likely to be further from their borrowing constraint, and whose job search behaviour is therefore theoretically less likely to be impacted by added liquidity. Our main results regard the job search behaviour of those who were unemployed when receiving their stimulus payments: we find that the contemporaneous effect of the liquidity injection was a fall in the job finding rate of around two percentage points, and that this effect was larger for groups that we expect to be closer to their borrowing constraint – younger and lower earning individuals. We also investigate how the added wealth affected the match quality at the subsequent employer. Here the results are less clear, but suggest that those who found a job in proximity to their transfer tended to find work in occupations associated with higher wages on average, and tended to stay with the new firm longer. All-in-all these findings are in line with the prediction of the directed search framework and suggest that wealth can have important career consequences, in particular for young and low-wage individuals.

Next, we turn to the analysis of intergenerational wealth transfers. We note that the theoretical predictions of the effects of parental wage on the child's job search behaviour crucially depend on the nature of intergenerational transfers; it is particularly important whether parental help is need-specific or unconditional. In a sense, the question we are asking is: should parents be thought of as wealth, or as insurance? This question links us to another strand of literature that analyzes the motives of parent to child wealth transfers, and asks whether these are best described as altruistic, and hence varying by the need of the child, or unconditional, as described by, for example, a 'warm glow' or 'joy-of-giving' assumption[2]. Answering

[1] In the random search literature: Danforth (1979) show that reservation wages are increasing in wealth; Lentz and Tranæs (2005a), Chetty (2008) and Lentz (2009) analyze interactions between wealth and search intensity. In the directed search literature: Griffy (2021), Eeckhout and Sepahsalari (2021) and Chaumont and Shi (2022) analyze the tradeoff between higher job finding and higher wages.

[2] Many papers investigate the motivation behind gifts within the family. See, for example, Becker (1974), Altonji et al. 1997

this question empirically has proved difficult, as data on inter-vivos transfers from parents to children are scarce. The most concrete contribution of this section is to add to this knowledge by analyzing a dataset that has so far been overlooked in this literature: the 1979 National Survey of Youth (NLSY79) and its follow-up Child and Young Adult sample (CNLSY79). These datasets have several desirable features: the respondents in the CNLSY79 are the children of all women in the NLSY79, which means that we have detailed longitudinal information on labour market outcomes linked across two generations. The CNLSY79 also contains information on transfers from parents to children through a set of questions that asks the child sample how large a share of their personal expenditure is covered by their parents. We find that the majority of expenditures paid by parents to their children that are above 18 years old and not in college occur when cohabiting, but parents continue to pay a significant share of expenditures even after the child has moved away from home – around 10% on average for 18-19 year olds, but then declining with age. Furthermore, we find that a small but statistically significant share of expenditures can be explained by the labour market status of both the parent and child: parents are more likely to give transfer when their income is high, and children are more likely to receive transfers when they are out of employment. These results hold both in the cross-section and when limiting the analysis to within-individual time-varying variation. Qualitatively, these results suggest that an altruism model may be appropriate in describing inter-vivos transfers, although only a smaller fraction of transfer variation can be attributed to state-dependent transfers, while a larger part appears to be unconditional.

We also make use of the detailed labour market history in the CNLSY sample to investigate directly how parental income affects the child's job search behaviour. Contrary to the theoretical prediction, we find that higher parental income is associated with a *higher* job finding rate, as well as reemployment wages. This result is robust to controlling for a host of individual characteristics, but not significant when only using within individual time-varying variation. To bring the analysis of intergenerational insurance and job search closer to an exogenous change in parental income, we also study the effect of a job loss (defined as a transition from employment to unemployment) of the mother on their child's job search behaviour. In line with theoretical predictions, we find that a job loss of the mother is associated with an 1.5 percentage point increase in the contemporaneous job finding rate of the child. Focusing on the subsample of individuals who either have deceased fathers or report having no contact with their father this effect is significantly larger and more persistent, with an employment loss of the mother being associated with more than a 3 percentage point increase in the job finding rate of the child both in the same month as the mother's job loss and the month following. These findings are consistent with a liquidity effect on job search in line with the theoretical predictions. We also analyse whether the increase in the job finding hazard following the job loss of the mother was associated with sorting into lower-paying occupations, and find that individuals who found a new job in relation to a job loss of their mother tended to do so in a lower-ranked occupation, which once again is in line with the predictions of the standard model.

Finally, we estimate the direct impact of a transfer from parent to child on job search behaviour. We find that receiving transfers is associated with worse labour outcomes; job finding rates as well as re-employment wages tend to be lower, and long-term wage effects seem to be negative as well. However, we are cautious not to interpret this correlation causally, as there is likely reverse causality whereby the person needing a transfer is subject to a worse shock, which in itself may have lasting effects on labour market outcomes. We attempt to control for such reverse causality by instrumenting transfers by transfers to siblings. Once again we find that a sibling receiving a transfer, which should correlate with the individual receiving 'family insurance', correlates with worse labour market outcomes. To the extent that this evidence can be taken as causal, this suggests that the moral hazard dimension of intergenerational insurance may be important for labour market outcomes, although we cannot rule out that the correlation is explained by other factors, such as synchronized local labour market shocks.

or Barczyk and Kredler 2021.

The paper is structured as follows. The first part – section 2 – reviews the literature on individual wealth and job search behaviour before reporting our new estimates of the causal effect of wealth on labour market outcomes using the natural experiment of the 2008 tax rebates. The second part – section 3 – reviews the literature on intergenerational transfers and job search models with family insurance before introducing the CNLSY dataset and reporting the empirical results on how intergenerational transfers interact with parent's and children's labour market outcomes. Finally, section 4 concludes with a discussion of potential avenues for future work.

2 Effect of wealth on job search behaviour: A natural experiment

In this section we analyze the effect of wealth on job search behaviour through a natural experiment – the stimulus payments (tax rebates) received by most US households following the 2008 financial crisis. The timing of these transfers, which in large were determined by social security number[3], means that the month in which an individual received their transfer was close to random, and hence makes them ideal to study wealth effects. Indeed, a multitude of research papers have exploited these tax rebates to study the effect of wealth on various economic issues such as consumption pass-through (e.g. Parker et al. 2013, Kaplan and Violante 2014, Broda and Parker 2014), the effect on earnings (Powell 2020), consumer bankruptcy (Gross et al. 2014) and subjective well-being (Lachowska 2017). However, as far as we are aware, we are the first to focus on the effects of the tax rebates on unemployed individuals, and in particular, on their job search behaviour. This fills a gap in the literature: while there has been quasi-experimental studies made to investigate the effect of unemployment insurance on hazard rates and re-employment wages (e.g. Card and Levine 2000, Lalive et al. 2006), the literature on wealth effects is more scarce and have typically only considered wealth effects through severance payments (e.g. Card et al. 2007). Since workers covered by severance payments is naturally a selected group the focus of the tax rebates in 2008, which had a large reach in eligibility, will provide an important addition to this literature.

2.1 Theoretical foundation and earlier empirical work

Empirical work

One of the contributions of this section is to add to the knowledge of the effects of insurance on individual's job search behaviour. The role of insurance, either through own accumulated wealth or government provided unemployment insurance (UI), in worker's job market outcomes has a long tradition of being studied in labour economics. A robust finding in the empirical literature is that an increase, or lengthening, of UI lowers the job finding rate. In one of the studies most relevant to our setting Card et al. (2007) use sharp cut-offs of severance payments and unemployment insurance (UI) extensions in Austria to find that a lump-sum payment equivalent to two months of income reduced the job-finding rate by 8%-12% on average, and that an extension of UI from 20 to 30 weeks lowered the job finding rate in the first 20 weeks by 5%-9%. However, they do not find any significant effect of either extended UI or severance payments on the quality of the subsequent job, as measured by the re-employment wage or job duration. Several other papers document similar relationships between UI and unemployment duration, both using cross-sectional correlations (e.g. Moffitt and Nicholson 1982, Katz and Meyer 1990) and quasi-experimental variation (e.g. Card and Levine 2000, Lalive et al. 2006). There is also some empirical evidence of the *mechanism* through which UI affects unemployment and wages: for example Marinescu and Skandalis (2021) use rich French panel data that contains detailed information on job applications and find that job search intensity goes up, and the target wage falls, when UI is nearing an end.

[3] See Powell (2020) for more detail about the arrangements of the tax rebates, and how they depended on social security number.

One of the key aims of this literature is to separate the 'moral hazard' and 'liquidity' components of the UI-caused distortions in job finding. The moral hazard effect can be seen as the insured agent inefficiently substituting search effort for leisure, as the marginal tax rate on labour is particularly high when job finding is coupled with a loss of UI. A liquidity effect, on the other hand, is active if the agent is borrowing constrained, and therefore – from a life-time income perspective – would have preferred a longer spell of unemployment, either because this would allow for more time to search for a better job, or to optimize their labour/leisure tradeoff. Chetty (2008) (henceforth Chetty) use a revealed preference framework to estimate that 60% of the increase in unemployment duration caused by UI is due to the liquidity effect rather than moral hazard. For identification Chetty relies on two types of variation: one exploits geographically differential changes in UI duration across the US, which is coupled with information on households' capability to smooth consumption, as measured by asset holdings and single- versus dual earner status. Here the finding is that 'constrained' (low-wealth, single earner) households responded more strongly to the duration increase than unconstrained household, suggesting that the liquidity effect is important. However, since both wealth and dual earner status are endogenous outcomes, and hence likely correlated with other potentially important characteristics, Chetty also use another empirical strategy, by exploiting variation in lump-sum severance payments. Lump-sum payments do not affect the marginal tax-rates and hence should only have an income effect, but no substitution effect, on the tradeoff between job search and leisure. If the severance pay is small relative to life-time income this wealth effect should be particularly relevant for credit constrained individuals. Chetty finds that job losers who receive severance payment tend to spend longer time in unemployment, and that this effect is stronger for those closer to their borrowing constraint, again suggesting that the liquidity effect is important.

Theory

To inform our analysis of the role of wealth in job search behaviour we draw on theoretical insights from job search theory. A number of papers have analyzed the finding that wealth impacts job search behaviour through random search models where agents have some combination of a search effort decision, curved utility (risk-aversion), and a savings decision subject to a borrowing constraint. Common to all of these papers is that missing markets for credit (self-insurance) and private unemployment insurance creates a role for the government to provide unemployment insurance that allows agents to smooth their consumption. Lentz (2009) use such a framework to study optimal UI policy, finding that it is very sensitive to both the subjective discount rate, and the interest rate. In this model the wage offer distribution is degenerate, so there is no relevant impact of wealth on the quality of the new match. The government's tradeoff is therefore only to provide unemployment insurance, which allows for better consumption smoothing, at the expense of distorting agents' search effort motives. Similar models are analyzed in Lentz and Tranæs (2005a), Card et al. (2007) and Chetty (2008). Lise (2013) extends this framework by incorporating on-the-job search, which endogenously creates a large wealth dispersion, as workers on different parts of the wage ladder have vastly different optimal savings behaviour.

A more recent literature has incorporated the empirical positive correlation between wealth and unemployment duration into directed search models. Here the correlation has a natural interpretation as wealthier individuals may be more willing to search for higher-paying jobs despite a lower job-finding rate, thus generating a positive correlation between wealth and both re-employment wages and unemployment duration without the need of either a reservation wage choice or a search effort choice. One challenge for the directed search literature to explain is that there is a well-established negative correlation between wages and unemployment duration; high-wage individuals tend to find a job faster (e.g. Van den Berg and Van Ours (1996)). The canonical directed search model is unable to explain this *negative duration dependence* as higher-wage postings always attract more applicants and thus should be associated with lower hazard rate from unemployment. Eeckhout and Sepahsalari (2021) shows how introducing a savings decision and decreasing absolute risk aversion into the directed search model can reconcile this – as unemployed workers run down their savings they become increasingly likely to apply for lower-paying jobs with higher job finding probability. Thus, this model can generate both a positive association between wealth and job finding and a negative correlation between unemployment duration and re-employment wages.

Griffy (2021) also studies a directed search model with a savings decision and a borrowing constraint, but extends the framework to allow for endogenous human capital formation in the spirit of Ben-Porath (1967), where agents face a tradeoff between labour earnings and human capital investment. The model is used to analyse the impact of initial conditions at labour market entry on life-time earnings. It is shown that feedback effects between directed search and human capital investment creates a stronger link between initial wealth and life-time earnings than what earlier literature, notably Huggett et al. (2011), has suggested. The reason for this is that, in a directed search model with risk aversion, borrowing constraints and a savings decision, a job separation is particularly costly for a low-wealth individual, as they choose to search for lower-paying jobs with higher job finding probability. This means that upon labour market entry a low-wealth individual devotes more of their resources to building up precautionary savings, rather than to human capital accumulation, which has long-lasting effects on life-time income. An interesting extension to this framework, which is not done in this paper, would be to consider how differences in *parental wealth* upon labour market entry affect life-cycle earnings.

The mechanism through which wealth affects income in the aforementioned papers is through the borrowing constraint. Much like it is the borrowing constraint that generates precautionary savings in the Bewley (1977), Huggett (1993), Aiyagari (1994) class of incomplete market models, as low-wealth individuals are unable to smooth consumption when receiving a negative income shock, it is the borrowing constraint that gives low-wealth individuals a *precautionary job search motive* in directed search models with risk aversion. Herkenhoff (2019) develops a model in this class that directly hones in on this mechanism. Noting that access to non-secured debt (e.g. credit card debt) has increased sharply, Herkenhoff builds a directed search model that explicitly models unsecured borrowing and a default decision. Analysing the impact of unsecured credit over the business cycle Herkenhoff finds that increasing access to credit coupled with the end of a recession leads to a slower recovery, as the increase in credit access causes individuals to search for higher-paying, but harder-to-find, jobs.

2.2 Institutional background

The wealth variation used in this paper will come from the tax rebates that were paid out as unconditional cash transfers (check in the mail or wire transfer) in the US in 2008 as part of a stimulus program. The stimulus program was named 'The Economic Stimulus Act of 2008' and was passed by the senate in February 2008 and signed into law by President Bush in the same month. A large part of the stimulus package – which was designed to avert a feared recession – took the form of direct economic stimulus payments (ESPs) to individuals through tax rebates. Any person who filed a 2007 income of at least $3000 in 2007 were eligible for a tax rebate, which amounted to at least $300 per individual or $600 for a married couple filing jointly, even if this amount was below the household's tax liability, and then equal to the entirety of tax liabilities up to a cap of $600 per individual or $1,200 per couple. Rebates were gradually phased out for individuals earning above $75,000, or couples earning above $150,000, at a rate of 5% of income above this threshold.

The first stimulus payments were made on the 28th of April 2008, and the rest were scheduled between April and July. The timing of the transfers were based on two factors: the last two digits of the recipient's social security number and whether the recipient reported a bank routing number in their 2007 tax return, which determined whether the rebate was received via electronic transfer or by a check in the mail. Although all payments were scheduled between April-July, in the data we also observe individuals receiving their payments in August-December, Powell (2020) hypothesize that this is because these individuals filed their

2007 tax returns late.

2.3 Data description

To analyse the effect of the tax rebates on job search behaviour, we use the 2008 panel of the Survey of Income and Program Participation (SIPP). SIPP is a panel survey representative of the US noninstitutionalized population where just over 42,000 households were interviewed every 4 months for a maximum of 16 rounds – making the interview dates span from September 2008 to December 2013. Our key variables of interest are the employment, earnings and tax rebate status of individuals: respondents provided earnings at the monthly level and employment at weekly level going back to May 2008 and were asked specifically about the 2008 tax rebates – which month they were received and what they amounted to – making SIPP an ideal sample to analyse the labour market effects of the stimulus payments.

In the data we observe stimulus payments between April 2008 and December 2008. Table 1 reports some summary statistics; the first column uses the full sample, which includes those who never received a transfer, whereas the rest of the columns report the sample split by which month transfers were received. Clearly, and as mentioned previously, there is some nonrandom variation in individual characteristics depending on which month the transfer was received. For this reason, our preferred specification uses individual level fixed effects to account for unobserved heterogeneity. Using individual fixed effects, which has been the norm in the studies using this variation, also has the advantage of controlling for nonrandom attrition in the sample, although this is less of an issue as all rebate payments occurred in the first two waves. The selection issue mainly arises from transfers that were received after July, as transfers in this period were not based on social security number. However, since transfers between April-July should be close to randomly allocated we also estimate models on data restricted to people who receive transfers in these months without including individual fixed effects. We make few restrictions on the data, although we run through a number of different specifications as robustness checks, and to elicit information on which sub-groups seem to have been most affected by the transfers.

2.4 Results

Our main objectives are to investigate the effect of the tax rebates on the job finding rate and re-employment wage rate of individuals. We also report results of some other labour market outcomes such as job destruction rates and job-to-job switches. To do so we estimate a linear model, which allows for both anticipation and lagged effect of transfers, while controlling for individual, age, month, and 'months from survey' fixed effects[4]. Following the literature we do not make use of the information of the size of tax rebates, as this variation is non-random and may create bias, hence we only use a dummy that takes value one if a transfer was received in a given month on the right-hand side. The model can be written as

$$Y_{i,t} = \alpha_i + \beta_0 \times Reb_{i,t+1} + \beta_1 \times Reb_{i,t} + \beta_2 \times Reb_{i,t-1} + \beta_3 \times Reb_{i,t-2} + \gamma \times X_{i,t} + \delta_t + \epsilon_{i,t}, \quad (1)$$

where $Y_{i,t}$ is the outcome variable of interest. β-coefficients, which are estimated using ordinary least squares, denote the forward-lagged, twice lagged and contemporaneous effects of tax rebates (denoted $Reb_{i,t}$). α_i, δ_t

[4]Months from survey is a variable that measures how far the interview month is from the observation month. Controlling for this is important since it correlates both with the outcome variable – for example, reported job finding is highest in the earliest month asked about – and with the rebate timing, as the majority of transfers were made in the very earliest months of the survey and hence correlated with being far from the interview months. Failing to control for this can thus create a bias where the job finding rate appears to be greater in transfer months. Furthermore, since not all individuals were interviewed in the same month, this variable can be identified separately from month fixed effects.

Table 1: Summary statistics by month of stimulus payment.

	(1) Full Sample	(2) April	(3) May	(4) June	(5) July	(6) August-December
Labour market statistics						
Earnings	2215.59	2729.76	2706.70	2550.86	2429.88	2505.67
Wealth	240764	267730	216316	231032	215879	259923
Employed	0.66	0.75	0.75	0.74	0.73	0.72
Unemployed	0.12	0.08	0.07	0.08	0.08	0.08
UE-transition	0.30	0.30	0.32	0.32	0.31	0.31
Tenure	76.91	90.30	96.26	97.10	96.94	94.46
EU-transition	0.02	0.01	0.01	0.01	0.01	0.01
Individual characteristics						
Age	39.94	43.57	44.50	44.95	45.26	46.65
Married	0.47	0.63	0.64	0.63	0.63	0.63
Female	0.51	0.52	0.53	0.53	0.53	0.52
Months observed	37.39	44.66	45.03	44.94	44.24	44.23
Observations	87910	3418	14313	15951	10880	3707

Note: Each variable is first averaged at the individual level, and table reports the cross-individual mean of these averages.

Table 2: Distribution of number of UE-transitions per individual.

	0	1	2	3	4+
N	65,331	15,292	4,757	1,663	867

and η_j denote individual and month fixed effects respectively, and $X_{i,t}$ is a vector of time-varying individual characteristics (fixed effects for age, marital status, and years from survey).

Since our key dependent variables of interest; job finding rates and re-employment wages; are only defined when an individual transitions into employment, the individual fixed effects are only identified for individuals with more than one non- or unemployment spell. Table 2 shows the distribution of such transitions for our estimating sample. For the majority of individuals we observe zero or one transition, hence they will not contribute to the estimation. For this reason we must be careful when interpreting the results from individual fixed effects as they will only apply for the selected sample of individuals who frequently transition in and out of employment. To address this issue we also run estimations without individual fixed effects, but here focusing only on individuals who received transfers in April-July, as this should be a more randomly selected group given that transfers in this period were mainly based on social security number.

2.4.1 Effect of transfers on job finding rate

Our first results consider the effect on job finding rates of individuals. We define an unemployment spell as any spell of non-employment during which the individual reports actively searching for a job in at least one week. The job finding rate only includes individuals who transition from an unemployment spell to employment. This includes month-to month transitions, but also within-month transitions, i.e. if the individual reported being without employment for some weeks of the months after which they started a job. Individuals who were with a job, but absent with or without pay, did not count as job finders once they reappear as employed. This means that the effect noted by Powell (2020), who finds that a higher likelihood of taking an unpaid absence was one of the significant effects of transfers, will not be picked up by our estimation strategy. Using this definition of job finding we construct a binary variable *jobfind*, which takes value one if an individual transfers from an unemployment spell to employment in a month and zero is the month is part of an unemployment spell but no job was found. Using this binary variable as the dependent variable we estimate equation 1 under some different sample selections. Figure 1 reports the results for the coefficients of interest; corresponding to the tax rebate dummy as well as its lags and forward lag.

Panel A reports the result for the main specification, which uses the full sample of individuals. The pattern that emerges is consistent with a liquidity effect: receiving a tax rebate is on average associated with a 2 percentage point contemporaneous drop in the job finding rate, which subsequently fades away – consistently with the liquidity effect vanishing. There also appears to be an anticipation effect, where the job finding rate is lower in the month preceding a transfer. This could easily be rationalized in a standard search model with a savings/borrowing decision, as individuals expecting a cash payment in the following month have less motive to save and hence are effectively moved away from their borrowing constraint.

Panel B reports the result from the specification that does not condition on individual level fixed effects and only uses the subsample of individuals who received their transfers from April to July, when payments were mainly based on social security number and hence more randomly allocated. These estimates show a similar pattern to the main specification – which serves as a robustness check on the results. It appears that the anticipation effect is smaller and that the persistence of the shock is lower in this sample, suggesting that these effects may not be as robust as the contemporaneous effect of the transfer.

Panels C-E hone in on subsamples of the population whose job search behaviour theoretically should be

Table 3: Effect of transfer on job finding rate relative to baseline rates.

	Panel A	Panel B	Panel C	Panel D	Panel E
Job finding rate	8.37%	13.13%	10.81%	11.80%	14.07%
Monthly income	$1,794	$2,194	$1,542	$1,145	$1,001
Transfer amount	$905	$917	$954	$928	$996
β_1-coefficent	-1.96%	-2.56%	-3.92%	-3.16%	-5.15%
Relative effect	-23.5%	-19.5%	-36.2%	-26.8%	-36.6%

Note:- Data from 2008 SIPP. Results control for month, months from interview and age fixed effects. Standard errors are clustered at the individual level. Regressions are weighted using the SIPP sampling weights.

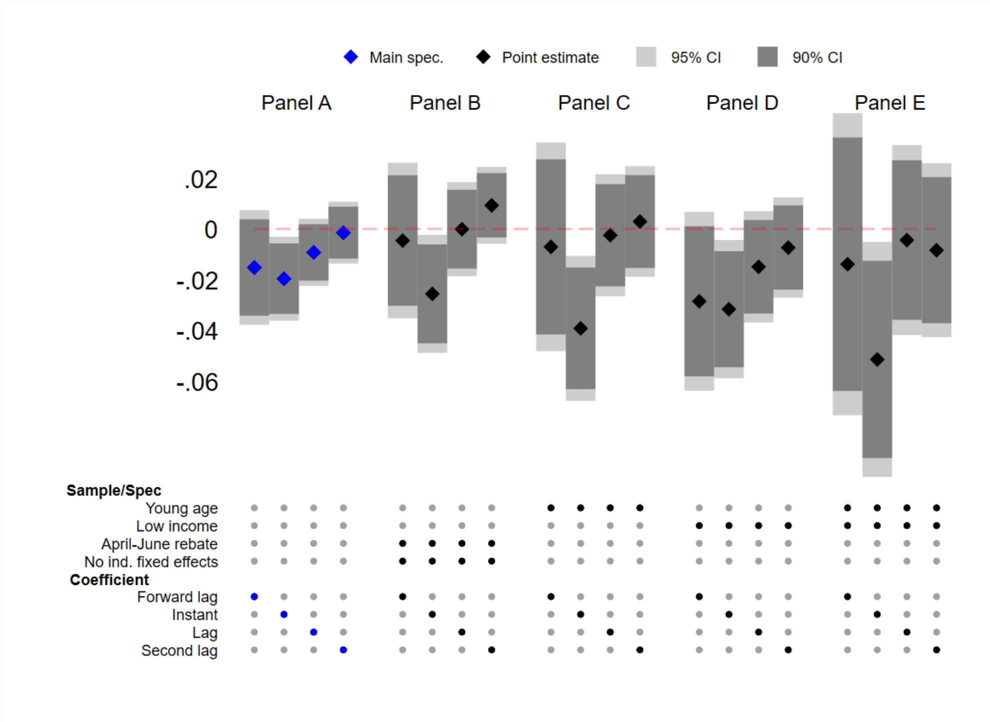
more affected by the rebate. In Panel C the sample is restricted to only include young individuals, who were between age 18 and 35 in 2008. Since these individuals tend to have lower wealth and are more likely to be borrowing constrained we would expect the transfers to have a larger impact for this group, and indeed that is also what we find, with the contemporaneous effect of the transfer being associated with a 4 percentage point fall in the job finding rate for this subgroup. Since these individuals are the ones that are most likely to receive support from home this suggests that differences in family support in this group could lead to significant differences in job search behaviour, although we unfortunately cannot test this hypothesis in the SIPP data as it does not contain any information on family background. Panel D focuses on another group which we expect to be more liquidity constrained: low-wage individuals. We define this group by running a regression of log monthly earnings on individual fixed effects as well as controls for month and age, and define a person as 'low-wage' if their individual intercept coefficient falls below the median. Once again the result is consistent with the theory; we find that low-wage individuals responded stronger to the transfer than the full sample average. Finally, panel D looks at the intersection of young and low-wage individuals. We find that this is the group that responds strongest to the rebates, although the smaller sample size means that this finding should be treated with some caution.

All-in-all there is a robust finding of a negative contemporaneous effect of transfers on the job finding hazard. Our results are broadly the findings of Card et al. (2007), who use a regression discontinuity design exploiting a sharp cut-off in Austrian severance payments, which only applied to workers who spent 36 months in employment, to estimate a 8%-12% average fall in the job finding rate during the 20 week period following a lump-sum payment equal to two months salary. In our case, transfers are typically equal to between 30%-50% of an individual's average monthly salary and we estimate a drop in the job finding rate between 2 to 5 percentage points. In table 3 we report the exact coefficients for the contemporaneous estimated effects for each of the estimation samples A-E in figure 1 together with their baseline job finding rate, average monthly income in years 2008-2010, and average tax rebate size conditional on receiving a rebate. Although our methodologies are not directly comparable, job finding seems to respond stronger to the liquidity injection in our estimates relative to Card et al. (2007). In our estimation the relative *contemporaneous* effect (which is different from Card et al. (2007) who report average effects over a 20 week period) is associated with a 19.5%-36.6% relative fall in the job finding rate, depending on the sample and methodology used. We suggest two explanations for the discrepancy in our results. First, the results here are estimated using a representative sample of the US population, which is likely to contain more individuals close to their borrowing constraint relative to those identifying the results in Card et al. (2007), which are workers who have been employed in proximity to 36 months. Second, we may expect that unemployed workers in the US are more financially constrained than in Austria, in the sense that a smaller welfare state means that the consequences of running down one's assets are greater.

2.4.2 Effects of transfers on re-employment wages and duration of next job

Next, we consider the effect of transfers on re-employment wages. To best capture wages in the job that a worker enters we use as dependent variable the average wage in the occupation associated with a UE-

Figure 1: Estimated effect of tax rebate on job finding rates.



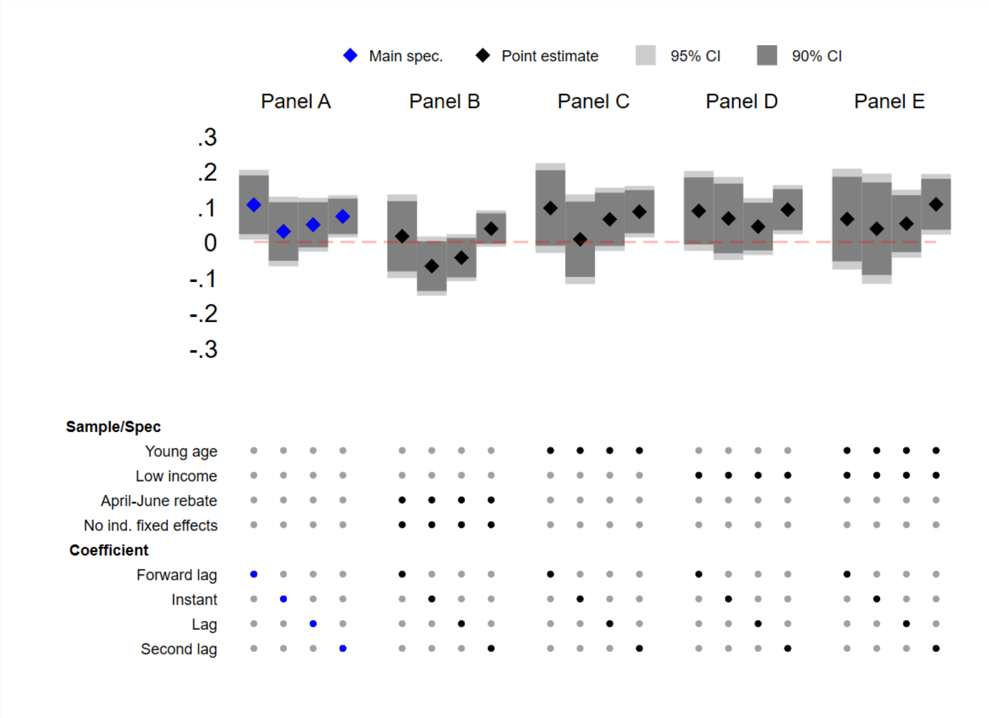
Note:- Data from 2008 SIPP. Each point represent the coefficient corresponding to the indicated variable and sample selection. Results control for month, months from interview and age fixed effects. Standard errors are clustered at the individual level. Regressions are weighted using the SIPP sampling weights.

transition. We classify occupations into 21 'major' groups, as given by their two-digit disaggregation in the 2000 Census Occupational Classification. The results are given in figure 2a. Our preferred specification once again uses the full panel and individual fixed effects. The findings are consistent both with an increase in the reservation wage of job finders, or with a directed search model where the now less liquidity constrained individual applies for a higher wage job with lower job finding probability: the selected group that find a job in the same month as receiving a transfer tended to find work in an occupation associated with a higher wage. The effect appears to be fairly equally spread around the transfer timing, with anticipation effects and lagged effects being of the same magnitude as the contemporaneous effect. Although not clearly statistically significant, the effect is sizable; in the preferred specification the re-employment wage is 0.05-0.1 log points higher in proximity to a transfer month than otherwise. Interestingly, when focusing on the plausibly random sample and dropping the individual fixed effects the result changes sign, with receiving a transfer now being associated with a *lower* re-employment wage. When interpreting this coefficient it is important to notice that, while the timing of the rebate is mostly random, re-employment wages are only defined for job finders, which is a selected group. Yet, we cannot rationalize this finding by looking at how the effect of job finding affects different groups: we find that the negative effect of transfers on job finding is stronger for low-wage individuals, hence the group of job finders in a transfer month should be positively selected by income and hence, if anything, bias the results in a positive direction. We also do not find any heterogeneous effects; younger and lower-wage individuals see similar increases in their reemployment wage as other groups.

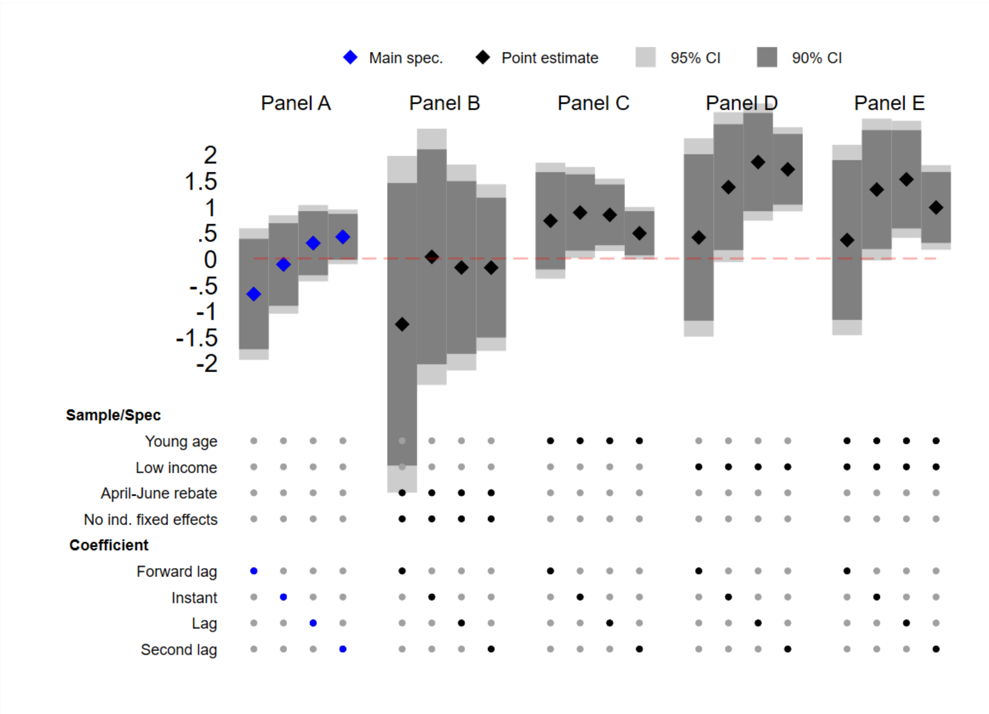
Since a successful job match is not only captured by the wage rate at the new job we also look at an alternative measure of match quality: duration of the next job. One would expect a better match to have a longer duration (see e.g. Jovanovic 1979). We thus also estimate equation 1 using duration (in months) of the new match as a dependent variable. The results are reported in figure 2b. We cannot establish any significant effects in the main sample. However, for the young and lower-wage individuals we do find a significant and positive relationship: matches that were formed in close proximity to the transfer were

Figure 2: Estimated effect of tax rebate on match quality of next job.

(a) Occupation-specific re-employment wage.



(b) Job duration (months).



Note:- Data from 2008 SIPP. Each point represent the coefficient corresponding to the indicated variable and sample selection. Results control for month, months from interview and age fixed effects. Standard errors are clustered at the individual level. Regressions are weighted using the SIPP sampling weights.

associated with between 0.5-2 months longer duration of tenure.

2.4.3 Effect of transfers on job transitions

We also consider some auxiliary outcomes where liquidity may affect individuals' labour market outcomes. In particular, we consider the effect of wealth on career, or job, changes. Since changing careers is a risky decision – for example through layoff rates being higher for low-tenured workers (see e.g. Marinescu 2009) – we hypothesize that a wealth injection, insofar as it changes the risk preferences of individuals may affect this dimension. We consider two outcome variables where this effect may appear in the data: job destruction into unemployment and job-to-job moves. Both of these measures are readily available from the data. For moves into unemployment we separate the effects into moves to unemployment and moves into non-employment, as wealth may also affect the decision to take an unpaid leave or to retire, which should result into a move into non-employment. For job-to-job switches we follow the same methodology as Menzio et al. (2016), who also calculate job-to-job transitions from SIPP data, and define it as any move from one employer to another without a gap in between.

We find no significant effect of the stimulus transfers on job-to-job transitions. If anything point estimates suggest a marginal negative effect, although these results are highly insignificant. For transitions to non- or unemployment we do find an effect, although this seems to be mainly driven by transitions to non-employment, suggesting that the effect on the decision to retire or take unpaid leave are stronger than that for transitions to a new career via an unemployment spell. The full results are found in the appendix; figure D1 displays the results for job-to-job transitions and figure D2 for job destruction.

2.4.4 Concluding remarks

To summarize this section, we find suggestive evidence that the liquidity injection of the 2008 stimulus payments affected job searchers in a consistent way to the predictions of the standard theoretical job search frameworks. Our finding that the job finding rate responded negatively to the transfers was qualitatively similar to other research using quasi-experimental variation in liquidity, notably Card et al. (2007), although our results were stronger in magnitude. We also find some evidence of an improvement in the match quality among those who found a new job in relation to their transfers, although these results are less robust to changes in the econometric specification and sampling. We also find that the effect sizes – both in terms of job finding and in terms of duration of the next job – were stronger for young individuals. This serves as further motivation for the analysis of intergenerational insurance and job search behaviour, as many young individuals still receive support from home at this point in life.

3 Intergenerational insurance and job search behaviour

In this section we use data from two of the US 'National Surveys of Youth' to investigate the effect of parental wealth on job search behaviour. In particular, we are interested in (i) whether richer background individuals are more likely to financial help from their parents when facing a negative labour market shock, (ii) whether parental income changes the job search behaviour of a child, and (iii) whether transfers from parents to adult children have the same effect on the child's job search behaviour as the wealth effects estimated in the previous section. Before turning to the empirical analysis, we summarize the earlier literature on family insurance and job search.

3.1 Earlier literature

The role of family insurance for labour market outcomes

The role of family background has received some attention in studies of savings and job search over the life-cycle, although not as much as government-provided insurance (UI) or self-insurance through precautionary savings. Kaplan (2012) uses data from NLSY97 to reveal that young men often respond to adverse labour market shocks by moving home, and that those with opportunity to move home are less scarred by job losses early in their career. These findings are incorporated into a structural model where young agents choose their level of savings, whether to move home, and face stochastic job offers, which they choose whether to accept or to reject. The model can rationalize the empirical findings and also explain the low precautionary savings behaviour of young, low-skilled workers, as the family-provided insurance replaces the need for self-insurance through savings.

Unlike this paper and Kaplan (2012), the largest literature on the role of family insurance on labour supply has not considered intergenerational insurance from parent to child, but instead the role of spousal insurance. Blundell et al. (2016) find that endogenous responses of spousal labour supply is an important factor that enables individuals to smooth consumption in the presence of earnings risk. Blundell et al. (2018) and Wu and Krueger (2021) extend this framework to analyse the labour supply choices of families with children and the these interact with child-specific grants and progressive income taxation. While these papers analyse spousal insurance through a labour supply response they do not use a search-and-matching framework and hence do not analyse the effects of spousal insurance on job search behaviour. There is a literature that also considers this dimension; Guler et al. (2012) analyse theoretically the job search choice of a couple, finding that, under the assumption of concave joint utility in income, joint search generates similar behaviour as an increase in wealth – on average unemployment spells tend to be longer as couples where one is employed can afford to be more selective in their search. In a calibrated version of the model the authors find that couples who search jointly therefore have between 1%-2% higher life-time income than single households. Flabbi and Mabli (2018) extends on this analysis to allow for fertility decision, on-the-job search, labour supply and gender heterogeneity. One of the aims of this section is to build towards a joint theory of parental insurance and job search behaviour, which would fill a gap in this literature.

Altruism and inter-vivos transfers: theory and previous empirical findings

Another contribution of this section is to shed more light on the motivations behind transfers from parents to children. For our application this will be an important factor when choosing whether to think of parental transfers as state-dependent insurance or as unconditional transfers, which will have implications on the search model's prediction of the effect of parental wealth on children's job search behaviour. This links us to another literature that investigates the motives behind parent to child transfers. Two competing theories are particularly relevant to our setting. If transfers are motivated by a 'joy of giving' assumption, they should be independent of the labour market status of the child, and hence parental wealth enters the child's decision in a similar way to own wealth. On the other hand, if transfers are described by an altruism model, they should act more as insurance, which for example may mean that the child only receives family support if they are unemployed. This would be the outcome of the standard static setting of the altruism model, as in Becker (1974), who find that altruistic parents choosing how much money to share with their child should transfer enough to equate the marginal utility of own consumption with the weighted marginal utility of consumption of the child. The weight on the child's marginal utility is determined by an 'altruism parameter', which measures how much the parent cares about their child's utility relative to their own. Three simple testable implication arise from this model: transfers should be (i) increasing in the income of the parents, (ii) decreasing in the income of the child, and (iii) conditional on the parent giving a positive amount of money, a reallocation of wealth from child to parent should be exactly offset by an increase in the transfer. In a dynamic setting the problem becomes more complicated, as strategic interactions arise if there is lack of commitment from the child's side. In particular, a *samaritan's dilemma* may arise where the child consumes too much, and saves too little, in earlier periods as they trust that the altruism of their parents will guarantee help in later period. Internalizing this, parents will backload their transfers as much as possible, but may still give transfers in earlier period in the case where their child is severely liquidity constrained, and hence have a large marginal utility of consumption[5].

Testing the appropriateness of the altruism model is cumbersome since data on transfers from parents to adult children are scarce. The 1988 and 2013 waves of the cross-generational survey 'Panel Study of Income Dynamics' (PSID) contains information on gifts, loans and support from parents in the preceding year and have been used extensively in research (Altonji et al. 1996, Altonji et al. 1997, Schoeni 1997, Wiemers and Park 2021). Another set of papers use data from the Health and Retirement Survey (HRS). Depending on the wave, the HRS asks respondents if they gave any transfers above $500 to their children or parents, and if so how much these amounted to. The HRS has been used to study uneven transfer between siblings (McGarry and Schoeni 1995), investment in children's education (Brown et al. 2011), dynamic aspects of family transfers (McGarry 2016), and the relative sizes of inter-vivos transfers and bequests Barczyk et al. (2019). Finally, the 1997 NLSY survey asks respondents about financial transfers in the past year as well as co-residence. NLSY97 has, for example, been used to study the impact of parental transfers on part-time work during college (Kalenkoski and Pabilonia 2010). However, as far as we are aware, no papers have previously used the transfer information in the CNLSY79 dataset, which we believe have some useful unique properties, as we outline in more detail below.

3.2 Data description

We use data from two of the National Longitudinal Surveys of Youth: the 1979 sample (NLSY79) and the children and young adult sample (CNLSY79). NLSY79 is a longitudinal survey that in 1979 started interviewing a sample of 12,686 young individuals, born between 1957-1964, who have since been interviewed annually until 1994 and after that biennially. CNLSY79 is a follow-up survey that interviews all the biological children of the women in the NLSY79 sample starting from the age of 12, thus allowing for intergenerational comparisons. Excluding individuals born later than 1997 (so that each person is interviewed at least once after their 18th birthday) the CNLSY79 sample consists of 7,934 unique individuals, born between the years 1971-1997. Interviews occurred biennially starting (at earliest) in 1994 and with the latest round of interviews being in 2016. CNLSY79 contains detailed labour market information; each interview object is asked to list up to 5 jobs that they have held since the last interview date, along with information such as start/end date, occupation, wage etc. Unfortunately, since the second survey wave in 1996, respondents were not asked about whether they were actively searching for jobs in between employment spells, hence

[5]See Barczyk and Kredler (2021) for an in-depth analysis of the altruism model in a dynamic setting.

we cannot separate non-employment spells from unemployment spells in this data. Since the two surveys occur concurrently we can couple the labour market information of youths with detailed labour market information and other characteristics of their mothers, for whom we observe labour market status at a weekly frequency. We also observe some information about their father's labour market outcomes and other characteristics, as the mother answers a number of questions about their spouse such as how many weeks they worked last year, as well as their occupation and earnings.

Apart from labour market outcomes our main object of interest is inter-vivos or in-kind transfers from parents to their children. The data does contain some information on transfers from parent to child, although this is more scarce. To infer information on family transfers we make use of the responses to the following survey questions:

- During [last year], did anyone [(other than your spouse/partner)] pay part of your living expenses?
- Does this person live (here in this household/in your home)?
- What is this person's relationship to you?
- About how much of your living expenses did this person cover?

Since these questions refer to yearly averages we only have information on transfers for every second year, making it harder to interpret results for shorter unemployment spells. This caveat should be taken into account when considering the analysis to come.

Relative to previously used datasets the CNLSY79 survey has two advantages. First, the intergenerational structure means that there is detailed information about both the givers of transfers as well as the receivers, this is a feature that only the PSID has among the previously mentioned datasets, but here there is a limited panel dimension to the transfers as only two waves contains detailed transfer information. Second, the phrasing of the question, which refers to 'the share of living expenses paid for' rather than pure cash transfers can be a strength or a downside depending on the question of interest. Although being less precisely asked it is possible that many transfers from parents to children are in-kind, for example by buying things for ones child, rather than direct inter-vivos transfers. Hence, this question may pick up a broader range of transfers. Since the CNLSY survey also contains information on co-habitance, just as NLSY97, it is also possible to separately identify in-kind transfers through cohabitance, which has been deemed perhaps the most important form of in-kind transfers (Johnson 2013).

Another advantage of the NLSY surveys is that we can control for a very rich set of covariates; apart from information on labour market outcomes, education, race etc. of both parents and children the NLSY surveys also include tests on cognitive ability. The mother sample undertook the Armed Services Vocational Aptitude Battery tests, and in the youth sample each respondent undertook the Peabody Individual Achievement Test (PIAT) tests for maths and reading comprehension. In the estimations that follow we proxy the mother's cognitive ability by their approximate Armed Forces Qualifications Test (AFQT) percentile score, which is derived from their ASVAB test scores, and the youths' cognitive ability by the average of their maths and reading PIAT scores.

3.3 Results

The aim of this section is to investigate whether family background affects labour market outcomes either through an insurance effect or through a wealth mechanism. In particular, we investigate whether wealthier background individuals – in line with predictions from directed search models – apply for higher paying jobs with lower job finding probabilities. Armed with data from the cross-generational NLSY surveys we approach this question in three ways.

Table 4: CNLSY79 summary statistics

	Mean	Min	Max	Individuals	N
Job finding rate	0.13	0	1	5,213	29,908
Mother's hh income	56,406.59	0	922,631	6,109	26,217
Mother's hh wealth	161,338	-2,489,667	5,526,252	5,339	12,874
Siblings	1.69	0	7	6,921	73,306
Age	26.92	18	47	6,921	73,306
Years observed	13.68	1	23	6,921	73,306

First, we investigate whether transfers from parents to children correlate with employment status and parental income as described by an altruism model, i.e. whether wealthier parents are more likely to pay for part of their children's living expenses, and whether these transfers are larger when the child is unemployed.

Second, we investigate whether intergenerational insurance has implications for job search behaviour by estimating the effect of parental income on the job finding probability and re-employment wages of youths, controlling as best possible for observable characteristics. The rich set of covariates increases confidence that we can identify the effect of family income or wealth separately from other determinants of job search behaviour. However, as there is likely still some unobserved heterogeneity that both affects job search and is correlated with family income, we also use the panel structure of the data to exploit within-individual variation in parental income to investigate whether individuals change their job search behaviour based on time-specific family resources.

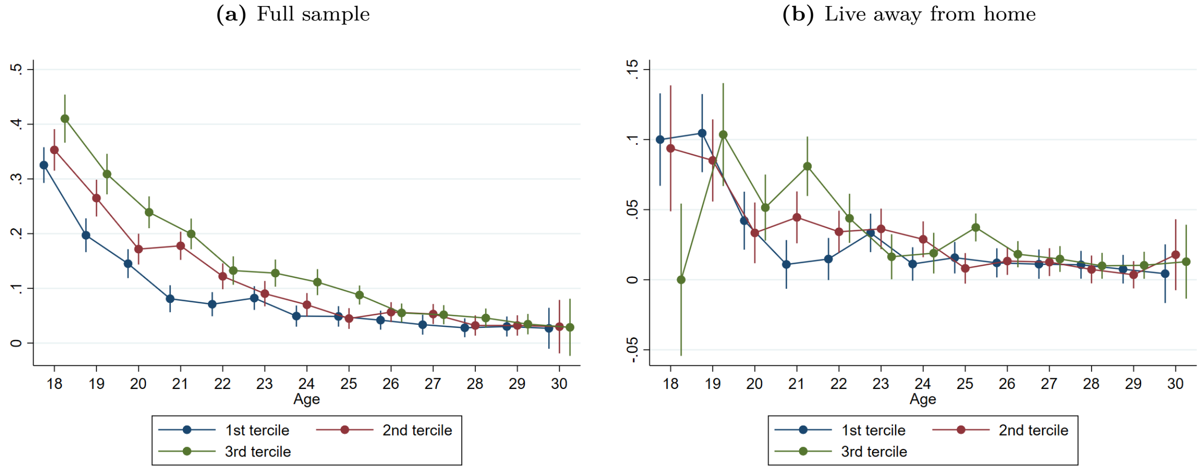
Third, we attempt to measure directly the impact of transfers from parent to child on job search behaviour. A reverse causality issue makes this estimation difficult: even if receiving transfers helps the receiver search for a higher-paying job, having lack of success in the labour market is likely associated with receiving more help from parents, hence disentangling the effect of transfers on job search behaviour from the reverse causality is difficult. We attempt to solve this issue by considering transfers in earlier periods and transfers to siblings, however, we cannot rule out that reverse causality cause bias in these settings as well.

We make some restrictions on the sample. Since the object of interest is not a college decision, or indeed help from family while in college, we exclude individuals who are either in college, or are yet to complete their first college spell. A small number of individuals report having a college degree despite never having reported attending college. To avoid college transfers to these individuals we omit any person with a college degree below the age of 25 from the sample. We also discard individuals serving in the military, those under the age of 18, and those who are never observed in paid employment. Table 4 provides some descriptive statistics of the estimating sample at the annual level. Note that some information, such as the transfer information, is only available biennially, hence the actual estimating samples are often smaller than that in table 4.

3.3.1 Effect of employment status and parents' income on inter-vivos transfers

We first summarize how common it is for parents to pay part of their children's expenses, how transfers vary with age, and whether they depend on the income of the parents. The share of living expenses paid is reported as a multiple choice question with alternatives 'less than 1/4', 'At least 1/4 but less than 1/2', 'At least 1/2 but less than 3/4' and '3/4 or more'; we re-code these to numerical values as the middle points 12.5%, 37.5%, 62.5% and 87.5%. Those who reported not receiving any help for living expenditures, and those who do not report that it was either their mother or father that paid part of expenses get imputed a value of 0%.

Figure 3: Share of living expenses paid by age and tercile of mother's household income.



Notes: Data from NLSY79 and CNLSY79. Mother's household income terciles refers to the tercile bin of the mother's average household income across all waves in the CNLSY79.

As a first visualization of the data, we plot the size of transfers by age, split by the income tercile of the mother's household, which is observed directly from the NLSY79 sample. We calculate the mother's mean household income across all waves that the respondents are observed above the age of 18, and split into three equal-sized bins. Figure 3a displays the results. It is clear that the share of expenses paid is rapidly decreasing in age – on average 18 year olds have between 30%-40% of their living expenses paid by their parents, but this share declines steadily to around 5% at age 26, where it stabilizes until age 30. Up until age 25 richer-background individuals see a higher share of their living expenses paid, but after this age the shares are similar across groups. Although the intergenerational transfers seem to fade out at a relatively young age it is worthwhile to note that the findings of section 2 suggest that it is the young and low-wealth individuals that see the largest impact of liquidity on job search, hence the group for which we do observe intergenerational transfers is the one where we may expect such transfers to have the largest impact on the receivers' job search behaviour and career choice.

To investigate how much the transfers are accounted for by individuals living with their parents, which Kaplan (2012) notes to be an important factor in household insurance, we repeat the analysis excluding those who reported living in the same household as the person paying part of their living expenses, as well as household who (regardless of transfer status) live with their parents[6]. Figure 3b displays the results. It is clear that cohabitation explains a large share of inter-vivos transfers, especially for individuals aged between 18-20, where transfers to independent youths are only around 1/3 of the size relative to the overall average.

Next, we investigate whether the share of expenditure paid for by parents correlates with labour market indicators. We ask the following questions: (i) Are individuals more likely to receive transfers when they are unemployed? and (ii) Are parents more likely to give transfers when their income is higher? We choose to focus on parents' income rather than wealth since – despite wealth being the theoretically more relevant dimension – income is typically more precisely measured. Results using wealth instead of income are reported in appendix A. To address the two questions we run regressions with transfer size as the dependent variable. We use three different specifications, each using different variation in the data. First is an OLS regression, which uses both cross-sectional and within-individual variation. Here we control for a battery of household characteristics, in an attempt to avoid omitted variable bias if household characteristics determine transfer sizes in a way that is correlated to, but not caused by, family income or employment

[6]We observe whether youths cohabitate with their parents at the interview date, which is in the calendar year after which the transfer questions refer to, so it is possible that some youths with zero transfer reported remain in this sample despite living at home in the relevant period, if they only recently moved out.

Table 5: Regressions of annual employment share of youth and income of mother's household on the share of child's living expenses paid for in a year.

	Full Sample			Live away from home		
	(1)	(2)	(3)	(4)	(5)	(6)
Youth employment share	-0.0580***	-0.0515***	-0.0434***	-0.0188***	-0.0170***	-0.0139***
	(0.005)	(0.006)	(0.007)	(0.003)	(0.004)	(0.005)
Log(Mother's hh income)	0.0100***	0.00601*	0.00721**	0.00371***	0.00226	0.00205
	(0.002)	(0.003)	(0.003)	(0.001)	(0.002)	(0.002)
N	14378	14366	13469	9806	9576	8651
R^2	0.116	0.340	0.484	0.032	0.275	0.433
Individual fixed effects	No	No	Yes	No	No	Yes
Mother fixed effects	No	Yes	No	No	Yes	No
Age & year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time-invariant controls	Yes	No	No	Yes	No	No

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors in parentheses are robust for heteroskedasticity and clustered at the individual level. Time-invariant controls: quadratics in ability of youth and mother, education of youth and mother (four groups), race and gender of youth. Age fixed effects refer to age of both mother and youth.

status. We thus control for the education level of the mother and youth (high school dropout, high-school graduate, some college and college graduate), race of the youth (white, hispanic or black), quadratics in cognitive test scores of mothers and youths, age fixed effects, and gender. Nonetheless, there may still be unobserved characteristics that create bias in the estimates. For this reason we also run specifications where we condition on mother, or individual level, fixed effects. Using mother fixed effects means that we do not consider between family variation, but only time-varying and *within-family* variation, i.e. differences in transfers to siblings depending on their income or employment status. Individual-level effects only consider within-individual, time-varying variation. Letting (2) be the cross-sectional regression, (3) be the family fixed effects specification and (4) be the individual fixed-effects specification we can write the models as

$$\text{Exp. Share}_{i,t} = \alpha + \gamma^Y \times \text{Emp. share}_{i,t} + \gamma^M \times \log Inc_{m(i),t} + \eta X_i + \delta_t + \epsilon_{i,t} \tag{2}$$

$$\text{Exp. Share}_{i,t} = \alpha_{m(i)} + \gamma^Y \times \text{Emp. share}_{i,t} + \gamma^M \times \log Inc_{m(i),t} + \delta_t + \epsilon_{i,t} \tag{3}$$

$$\text{Exp. Share}_{i,t} = \alpha_i + \gamma^Y \times \text{Emp. share}_{i,t} + \gamma^M \times \log Inc_{m(i),t} + \delta_t + \epsilon_{i,t}, \tag{4}$$

where i denotes an individual, t a year, Emp. share$_{i,t}$ the fraction of the year that the individual was employed, $\log Inc_{M_{i,t}}$ the log of mother's annual household income, and $X_{i,t}$ is the vector of control variables. The coefficients of interest are γ^Y and γ^M, which will inform us of how transfer sizes correlate with the employment status of the youth and the income of its parents.

Table 5 reports the results. Columns 1 to 3 uses the full sample, including individuals who live at home, whereas columns 4 to 6 only use the sample of individuals who live away from home. Both the cross-sectional and time variation imply an effect of the youths employment status on transfers. The results indicate that a youth who spends the entire year unemployed receives 6 percentage points higher share of living expenses paid for in the cross section and 1-2 percentage points more when using either family-specific or individual-specific variation. There is also evidence that richer-background youths are more likely to receive transfers in the cross-section, although this correlation is not statistically significant for the subsample that lives away from home. Qualitatively, these results support an altruism model, as transfers depend positively on the need of the receiver. Whether the altruism effect is quantitatively important is a more difficult question, and may benefit from placing these estimates in a structural model.

Table 6: Regressions of income of mother's household on youth's annual average job finding rate and log re-employment wage.

	Job finding rate		Log(Re-employment wage)	
	(1)	(2)	(3)	(4)
Log(Mother's hh income)	0.00924***	0.000976	0.0549***	0.0132
	(0.002)	(0.003)	(0.011)	(0.029)
Individual fixed effects	No	Yes	No	Yes
Age controls	Yes	Yes	Yes	Yes
Time-invariant controls	Yes	No	Yes	No
N	9539	8657	3374	1540
R^2	0.030	0.382	0.090	0.653

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors in parentheses are robust for heteroskedasticity and clustered at the individual level. Time-invariant controls: quadratics in ability of youth and mother, education of youth and mother (four groups), race and gender of youth. Age fixed effects refer to age of both mother and youth.

To put these results in perspective, we compare them to the results of McGarry (2016), who also investigates the dynamic aspects of inter-vivos transfers, but using data from the Health and Retirement Survey. The finding in McGarry (2016) that relates most closely to ours is the finding that a $10,000 increase in the income of the child is associated with a one percentage point fall in the probability of receiving a transfer. In appendix B:, we run the same regression as McGarry (2016) and find that this results holds up almost exactly using the CNLSY dataset. That this result is so similar using two distinct datasets adds confidence to the accuracy of the estimate of this elasticity, and suggest that the transfer information in the CNLSY79 sample is roughly consistent with that in the HRS.

3.3.2 Effect of parental income on job search behaviour

Next we turn to the effects of family background and transfers on job search behaviour. The goal is to study whether individuals with higher-income parents, who should receive better 'family insurance' following a job loss, have lower job finding rates and higher re-employment wages, as a directed search framework would predict. To do so we make use of the detailed employment history information in the CNLSY79 to construct monthly job finding rates as well as re-employment wages, which are defined as the occupation-specific wage rate at the job associated with a job finding. Since information on the mother's income and transfers are at a biennial frequency we aggregate job finding rates and re-employment rates to their annual averages. Using these as dependent variables we estimate models similar to 2 and 4, although without the youth's employment share as an independent variable. The regressor of interest is the log of the mother's contemporaneous household income.

Table 6 reports the results. We find that the job finding hazard goes against the directed search intuition; both in the OLS and fixed effects specifications there is a positive correlation between family income and the job finding rate, although only in the OLS specification is this relationship statistically significant. Re-employment wages, on the other hand, go in the expected direction, with higher family income being associated with a higher re-employment wage. We also consider regressions where we compare the role of parental income to other forms of self-insurance that have been emphasized in the literature, including spousal income and credit card debt. These results are reported in appendix C.

We conclude that, even after controlling for a rich set of covariates, richer-background individuals tend to have higher job finding rates and higher re-employment wages. That these two objects move together is not in itself a surprising finding; it is well-established in the literature that individual job search behaviour exhibits *negative duration dependence*, i.e. a negative correlation between unemployment duration and re-employment wages. Our results contributes to the understanding of this finding by noting that there is one covariate that predicts which individuals fall into the high wage, high job-finding probability group – those with richer parents. A natural interpretation for this finding is that the effect of richer parents on job search behaviour is two-fold: a wealth (or insurance) effect contributes to a lower job finding rates and higher re-employment wages, in line with the literature on severance pay and unemployment insurance, while perhaps a network effect, or some unobserved inheritance effect explains the higher job finding rate, counteracting the wealth/insurance effect.

To remove the bias from these types of unobservable household characteristics, we would ideally want to study a situation where the income of the parents falls for an exogenous, and unpredictable, reason. To this end we make use of the monthly labour market information on both the mother and the child to estimate whether a change in the mother's income is associated with a changes in the job search behaviour of the child. As a wealth 'shock' on the mother side we use a transition from employment to unemployment (an EU-transition). We cannot establish that such a transition is unexpected or unwanted, but constraining ourselves to EU-transitions at least means that the mother reported searching for a job after the job loss, which should mean that retirement decisions or a choice of taking unpaid leave, both of which could be associated with a comfortable financial situation, are not included. Thus, an EU-transition on the mother side provides a suitable proxy for a adverse liquidity event in the parental generation. In the full sample we observe 2,291 EU-transitions for mothers. To estimate the effect of such an event on the job search behaviour of the child we estimate the same model as in section 2 (equation 1), allowing for one forward lag and three lags as well as the contemporaneous effect of the mother's EU-transition. As dependent variables we use both a dummy for job finding and the log of the occupation-specific mean wage associated to the job find. A downside of the detailed employment history measures is that we only observe the monthly employment status of the mother – but not of the father. For this reason we also limit our analysis to the subsample of individuals who report either having a deceased father or who report that they have no contact with their father[7]. Around 9% of the sample fall into this subcategory, which is denoted with an 'absent father' marker in the results figures below. As in section 2 we run both a specification that includes individual, month and year fixed-effects as well as one without individual fixed effects that instead controls for a host of individual characteristics (age of mother and child, cognitive ability of mother/child, education and income of mother, own education and income, gender, race). We also follow exactly the methodology of section 2 and estimate the effects focusing on a 'low-income' sample, which is defined as having a below-median individual fixed effects in a standard mincer equation controlling for age and month dummies.

The results are reported in figure 4, where panels D-F report the results for the subset with absent fathers. In both the fixed effects (figure 4a, panel A) and OLS (4a, panel B) specification we observe a spike in the child's job finding rate of approximately 2 percentage points in the same month the mother's EU transition. While this is in line with the theoretical prediction the effect disappears immediately after the first month, which casts some doubt on the robustness of the result – we would expect a persistent effect, as the mother's job loss should be associated with a persistence loss in family income rather than a sudden negative wealth shock. We do not find evidence of a stronger effect for low-wage individuals (figure 4a, panel C). Focusing on the subsample without contact with their fathers the effects are much larger, albeit not statistically significant except for the contemporaneous effect in the specification without individual fixed effects. For this subsample all specifications suggest a contemporaneous effect of an increase in the job finding rate of around five percentage points, and that this effects stays at this level even the month after the mother's EU-transition. The baseline job finding rate is very similar across the two samples, so this effect is larger both in absolute as well as relative terms.

In terms of reemployment wages we do not find clear evidence that the uptick in the job finding rate was associated with individuals sorting into occupations associated with lower average wages. These results

[7]Specifically, those who report that they never see their father in response to the survey question: *"About how often do you see your father?"*

are reported in figure 4b, and although the majority of the points estimates are negative many are not, and none of the results are statistically significant. Note that the confidence intervals are typically rather large, spanning around 0.1 log points in the full sample and at least 0.2 log points in the subsample with absent fathers, hence we cannot rule out that an economically important effect exists that we cannot pick up due to lack of power in the estimation.

In a final analysis, we look at the effect of the mother's job loss on the probability of the child moving up or down the 'occupational ladder'. The reason for considering this dimension is that a higher propensity to move to a lower-wage occupation in recession has been linked to much of the scarring effects of recessions (Huckfeldt 2022), hence this dimension provides important insight into the role of the scarring effects of loss of intergenerational insurance. Thus, we estimate equation 1 using two alternative dependent variables. First is a dummy that takes on value one if the individual finds a new job in a lower-paying occupation, and zero if they find a new job in the same occupation as previously, or in a higher-paying one. The results of this regression are reported in figure 5a. As the theory would predict, we do find a positive effect. In proximity to a job loss of the mother, and conditional on the youth finding a new job, the new job is more likely to be in a lower-ranked occupation. This effect is once again stronger for individuals with absent fathers. Figure 5b reports the result for the probability of switching to a higher-ranked occupation. Here the findings are less clear but most results, in particular for those with absent fathers, indicate a negative effect, as theory would predict.

3.3.3 Effect of transfers on labour market outcomes

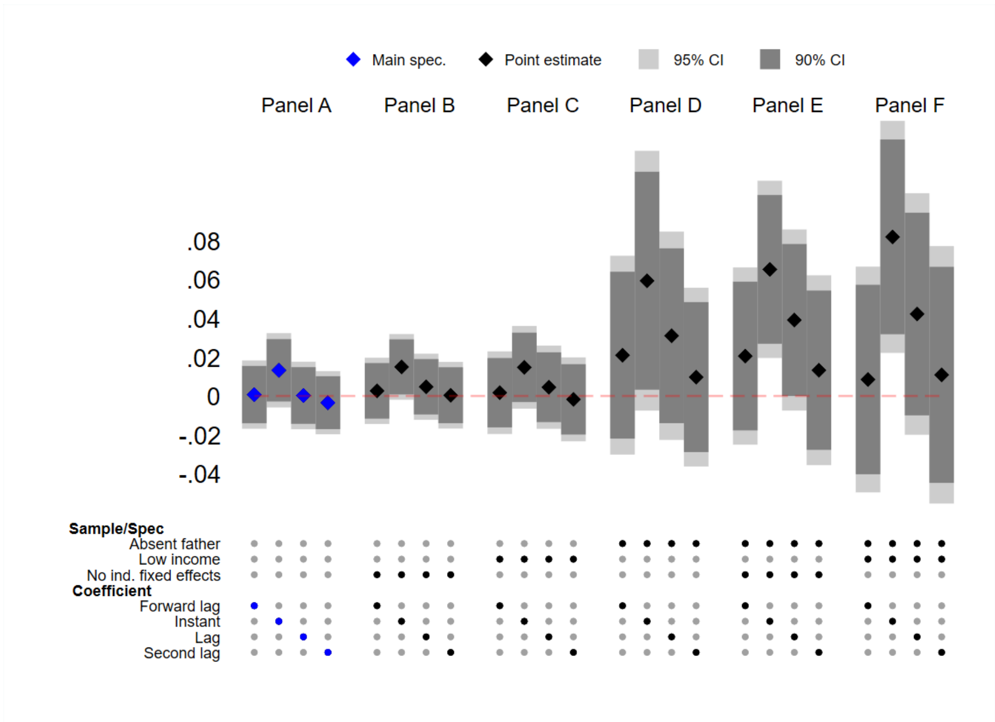
In this section we investigate the impact of transfers from parents to children on the child's job search behaviour. Our goal is to estimate directly the causal effect of inter-vivos transfers on job search behaviour. However, an endogeneity issue makes this measurement difficult: there is likely reverse causality, with those who are struggling to find a job being more likely to receive support from home, which means that estimates are downward biased. We consider two instrumental variables to deal with this endogeneity; lagged transfers and transfers to siblings, but ultimately conclude that the exclusion restriction is unlikely to hold, hence IV estimates will be biased. The reason for believing that the exclusion restriction does not hold is that, although the instruments are highly significant predictors of the endogenous independent variable, the correlation in the first-stage regression is typically small. Furthermore, regressing the outcomes of interest directly on the earlier transfers or sibling transfers provides estimates of similar magnitude to regressions on direct transfers. Taken together this implies that, if the exclusion restriction holds and the only effect of earlier transfers or sibling transfers on the outcome variable is through their effect on current transfer, the effect sizes must be enormous, which is indeed what the IV regression results show.

Since we deem the exclusion restriction unlikely to hold we do not report any results from the IV regressions. Instead, table 7 shows the results from regressions on the outcome variables (job finding in columns 1-3 and re-employment wage in columns 4-6), using current transfer, previous transfer and sibling transfers as independent variables in turn (along with the standard controls). The results suggest that receiving transfers, in the current period or in a previous one, as well as having a sibling receive transfers, is associated with a lower job finding rate and lower re-employment wages on average.

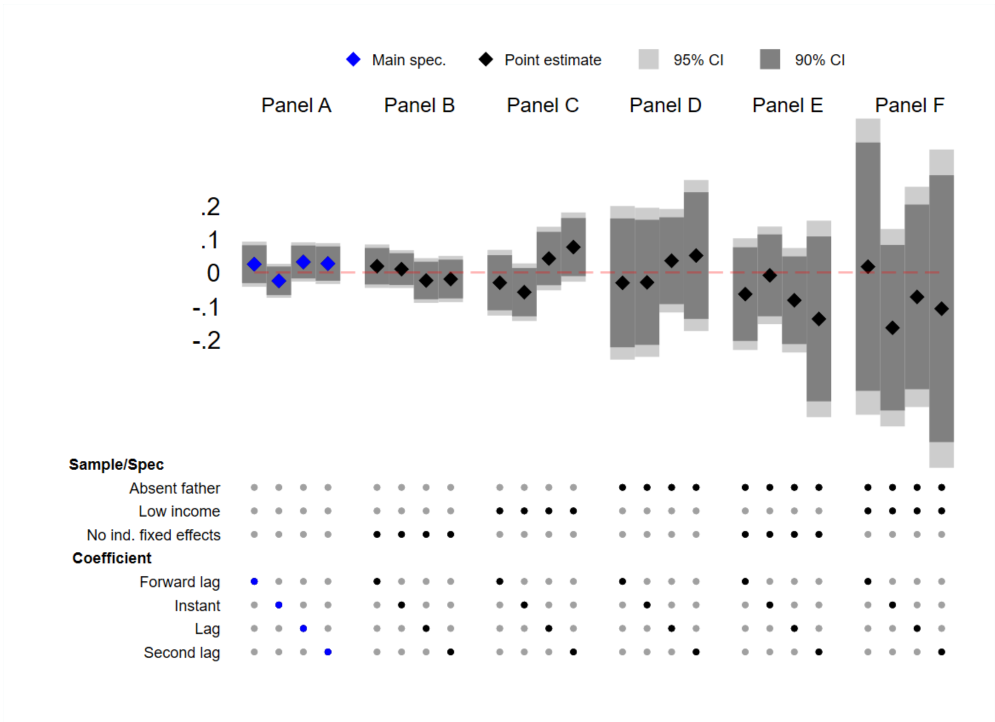
We offer two potential explanations for this result. First, it may be the case that the reverse causality issue is not only a problem for current transfers, but also for the other independent variables. For earlier transfers, the negative shock that caused the individual to receive a transfer in the past may have lingering effects that lowers their job finding rate and re-employment wages even in later years. For sibling transfers there may be local labour market shocks that had negative effects on job finding and re-employment wages for all siblings, which means that the whole sibship is more likely to receive support from their parents. Second, it may be the case that the 'moral hazard' effect of family insurance dominates the liquidity effect:

Figure 4: Estimated effect of mother EU transition on child's job finding rate and reemployment wage.

(a) Job finding rate



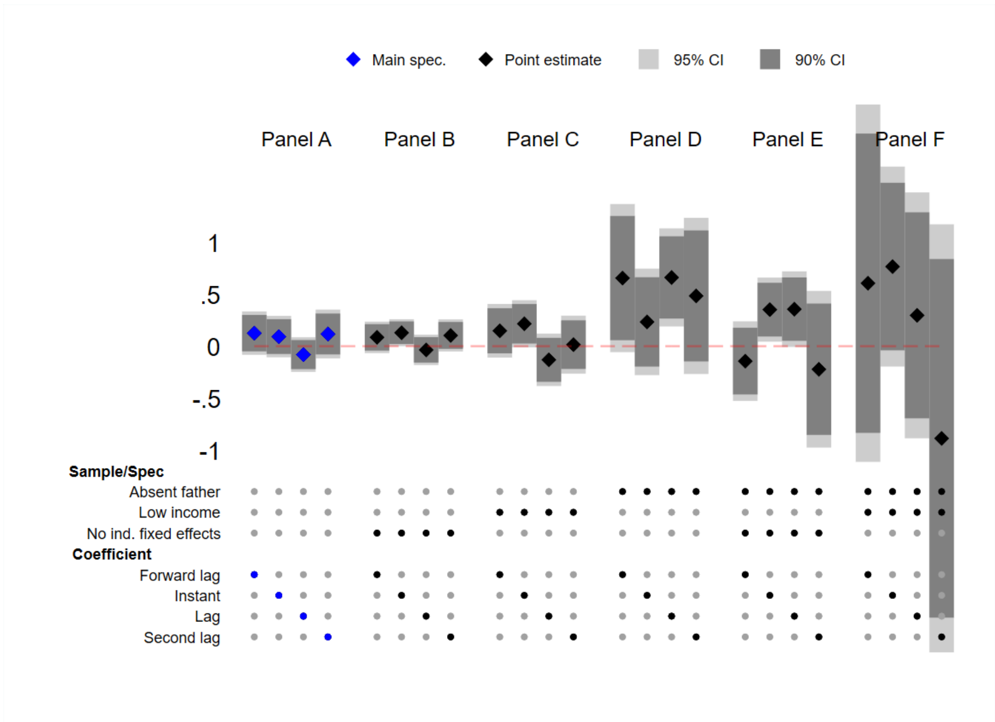
(b) Occupation specific reemployment wage



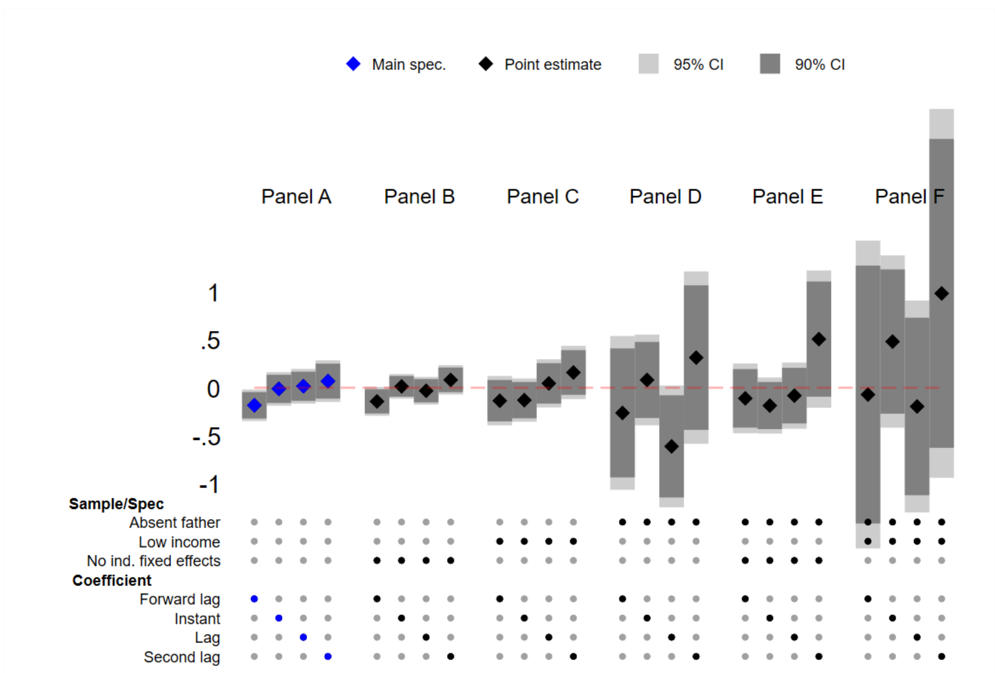
Note:- Data from NLSY79 and CNLSY79. Each point represent the coefficient corresponding to the indicated variable and sample selection. Results with individual fixed effects control for month, age, and mother age fixed effects. Results without individual fixed effects further controls for mother and child education level fixed effects, quadratics in cognitive ability of mother and child, race, and gender. Standard errors are clustered at the individual level.

Figure 5: Estimated effect of mother EU transition on child's job finding rate and reemployment wage.

(a) Prob. of lower ranked occupation



(b) Prob. of higher ranked occupation



Note:- Data from NLSY79 and CNLSY79. Each point represent the coefficient corresponding to the indicated variable and sample selection. Results with individual fixed effects control for month, age, and mother age fixed effects. Results without individual fixed effects further controls for mother and child education level fixed effects, quadratics in cognitive ability of mother and child, race, and gender. Standard errors are clustered at the individual level.

Table 7: Estimates of effects of own transfers, previous transfers and sibling transfers on youth's annual average job finding rate and log re-employment wage.

	Job finding rate			Log(Re-employment wage)		
	(1)	(2)	(3)	(4)	(5)	(6)
Transfer	-0.0395***			-0.105***		
	(0.008)			(0.029)		
Earlier transfer		-0.00413			-0.0385**	
		(0.004)			(0.015)	
Sibling transfer			-0.00737**			-0.0466***
			(0.003)			(0.014)
Individual fixed effects	No	No	No	No	No	No
Age controls	Yes	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes	Yes
Time-invariant controls	Yes	Yes	Yes	Yes	Yes	Yes
N	7974	26667	23040	3310	8527	7174
R^2	0.049	0.040	0.040	0.157	0.146	0.151

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors in parentheses are robust for heteroskedasticity and clustered at the individual level. Time-invariant controls: quadratics in ability of youth and mother, education of youth and mother (four groups), race and gender of youth. Age fixed effects refer to age of both mother and youth.

perhaps parents that pay large shares of their children's living expenses cause a drop in search intensity among their children, which could explain both lower job finding rates and re-employment wages. Trying to disentangle these potential forces is beyond the scope of this paper, but we hope that this result can be a starting point for more research into the effect of family transfers on children's job search behaviour.

4 Conclusion

The results in this paper add to previous findings of the importance of wealth effects in studies of job search behaviour. The analysis of the job search response to the stimulus payments in 2008 confirms the empirical findings from previous literature of a negative response of the job finding rate to an addition in liquidity. The analysis of the heterogeneous response of the liquidity shock also confirm a key theoretical prediction of standard job search models – that the response is stronger for individuals closer to their borrowing constraint, which we proxy by being younger and lower-wage. Whether the effect on job search was due to a shift in the tradeoff between applying for jobs with lower job finding probability but higher match quality, or whether it was due to a fall in search effort is empirically uncertain; our points estimates suggest that those who found new work in proximity to their stimulus transfer did so in higher paying occupations and had longer tenure with their new employer, although these results are not statistically significant.

We also take this finding to a new setting, by thinking about how the effects of wealth on job search and career choice can generalize to *parental* wealth. How to model the impact of parental wealth on the child's job search behaviour is theoretically ambiguous; if wealth is fully dynastic the effect of having wealthier parents should have the same implications as a pure wealth transfer – such as the 2008 stimulus payments – but if wealth transfers from parents to children are not unconditional but rather dependent on the need of the child, the wealth of parents are better analyzed as an insurance policy – such as unemployment insurance – which is known to have distinct theoretical implications on an individual's job search. The empirical findings of the paper shed some light on this ambiguity with two findings: (i) transfers from parents to children are common up to the age of 25, although more than half of these transfers are accounted for by children living at their parent's home, and (ii) the bulk of transfers are independent of the child's employment status, but a significant share of the variation in transfer size does depend on the labour market status and earnings of both the parents and the child in the expected direction; with transfers being negatively correlated to the child's employment status and positively correlated with the parents' income.

Finally, the paper investigates the connection between transfers, wealth and job search behaviour. We document three empirical facts: (i) both the job finding rate and reemployment wages are positively correlated with parental income, even after controlling for a rich set of household characteristics, (ii) following a job loss of the mother, there is an increase in the job finding rate of the child, who is also more likely to find a job in an occupation with a lower wage rank, and this effect is particularly strong for those who either have deceased or absent fathers, and (iii) receiving transfers is negatively correlated with job finding and reemployment wages,

The findings here open up interesting avenues for further research. In particular, they highlight the importance of a study of interactions between parental transfers and common labour market policies. Policies such as unemployment insurance, severance pay, and stimulus payments may affect individuals heterogeneously depending on family wealth, and may also crowd out parental transfers. A model of the labour market that takes into account parental transfers, and matches the empirical findings documented in this paper, should therefore be able to shed light on potential policy improvements in these dimensions.

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Appendix

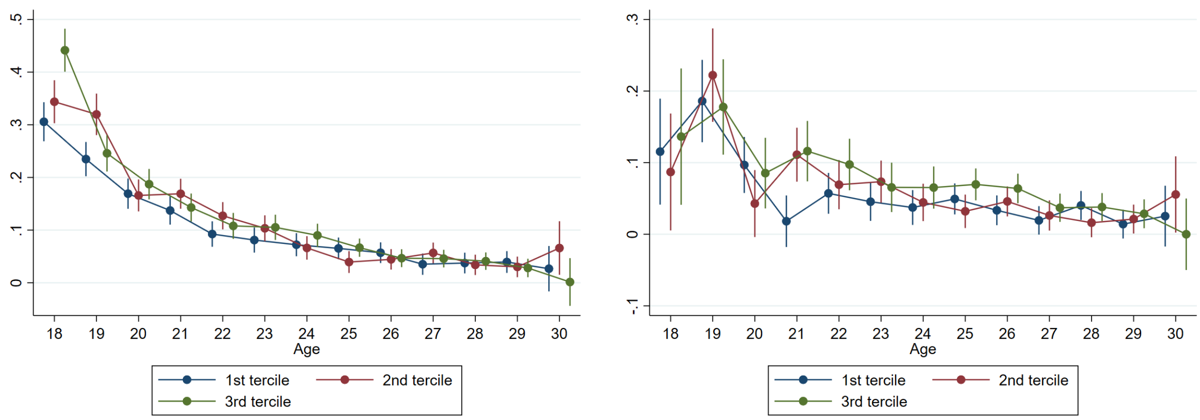
A: Effect of family wealth on transfers and job search behaviour

This appendix repeats the analysis of sections 3.3.1 and 3.3.2, but uses the mother's household wealth, rather than income, as a the key independent variable. To measure wealth we use the *'family net wealth'* variable in NLSY79, which is created by summing all asset values and subtracting all debts. Asset information in the NLSY79 is only collected at every second interview, hence this information is only available for every fourth year in the sample; for this reason sample sizes will be smaller, and in particular fixed-effect regressions, will suffer from less identifying variation. Since net wealth can take negative values a log transformation is not possible, hence we instead use the the household's percentile rank in the wealth distribution to reduce the impact of outliers in the data.

Figure A1: Share of living expenses paid by age and tercile of mother's household wealth.

(a) Full sample

(b) Live away from home



Notes: Data from NLSY79 and CNLSY79. Mother's household wealth terciles refers to the tercile bin of the mother's average household wealth across all waves in the CNLSY79.

Table A1: Regressions of annual employment share of youth and wealth of mother's household on the share of child's living expenses paid for in a year.

	Full Sample			Live away from home		
	(1)	(2)	(3)	(4)	(5)	(6)
Youth employment share	-0.0502***	-0.0389***	-0.0287**	-0.00905**	-0.0121*	-0.00392
	(0.007)	(0.009)	(0.012)	(0.004)	(0.007)	(0.009)
Mother's hh wealth percentile	-0.00426	0.0262	0.0249	0.00429	0.0147	0.0317*
	(0.009)	(0.024)	(0.024)	(0.005)	(0.017)	(0.018)
N	6541	6010	4382	4431	3779	2478
R^2	0.112	0.408	0.573	0.029	0.372	0.540
Individual fixed effects	No	No	Yes	No	No	Yes
Mother fixed effects	No	Yes	No	No	Yes	No
Age & year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time-invariant controls	Yes	No	No	Yes	No	No

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors in parentheses are robust for heteroskedasticity and clustered at the individual level. Time-invariant controls: quadratics in ability of youth and mother, education of youth and mother (four groups), race and gender of youth. Age fixed effects refer to age of both mother and youth.

B: Comparison to the HRS dataset

This appendix compares our results to those of McGarry (2016), who also studies the dynamic aspects of transfers but using data from the Health and Retirement Survey. To do so we replicate as closely as possible the regression that forms table 6 in McGarry (2016). Here the dependent variable is a dummy that takes value one if the child receives a transfer and zero otherwise. The key independent variable is the annual income of the child and a number of additional control variables are included such as the child's age, years of schooling, marital status, home ownership status, number of children, gender, race, and number of siblings. Three estimation strategies are considered: pooled OLS, as well as specifications with family fixed effects, and child fixed effects. The CNLSY data does not include the exact same set of covariates, but does allow for the estimation of a reasonably similar regression, the results of which are reported in table B1. All-in-all results are very similar, with the only significant difference between the estimates being the sign on the gender dummy which is reversed in our estimates – with males receiving larger, rather than smaller, transfers on average.

Table B1: Comparison to results in McGarry (2016), table 6.

	OLS		Family FE		Child FE	
	CNLSY79	McGarry	CNLSY79	McGarry	CNLSY79	McGarry
Child Income ($10,000s)	-0.0150***	-0.013***	-0.0155***	-0.014***	-0.0127***	-0.009***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Age	-0.0122***	-0.004***	-0.0101***	-0.004***	-0.00926***	-0.007***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.004)
Male	0.0110**	-0.011***	0.0111	-0.017***		
	(0.005)	(0.004)	(0.007)	(0.003)		
Nonwhite	-0.0114**	-0.018***				
	(0.005)	(0.005)				
Siblings	-0.0118***	-0.020***				
	(0.002)	(0.001)				
Mean dependent variable	0.123	0.139	0.123	0.139	0.123	0.139
R^2	0.069	0.089	0.332	0.30	0.482	0.39

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors in parentheses. McGarry's regression controls for a number of additional covariates, see McGarry (2016) for details.

C: Comparing mother's household income to other forms of self- or family insurance.

This appendix compares the results on the effect of the mother's household income on job search behaviour to other forms of self-insurance that have been emphasized as important in the literature: credit card debt and spousal income. The CNLSY contains some – albeit limited – information on wealth through questions about the respondent's credit card debt and, in case they own their residence, the house value and mortgage debt. Following the findings of Herkenhoff (2019) of the importance of access to credit card debt we find this dimension particularly interesting, although we cannot observe *access* to credit, merely the debt level of the individual. Still, we hypothesize that having credit card debt is associated with having low liquid wealth and hence being close to ones borrowing constraint. We also observe spousal income, which has been emphasized as an important insurance vessel in job search, although it has been noted that the correlation seemingly moves differently for men and women, with spousal income being positively correlated with the job finding hazard for men and negatively for women (Lentz and Tranæs 2005b).

Since we want to include non-married individuals and individuals with no credit card debt in the analysis, we choose a non-parametric specification. We construct four bins for each outcome variable. For credit card debt these are 'no debt', '1st debt tercile', '2nd debt tercile' and '3rd debt tercile', where debt terciles are defined within the group with positive debt. Similarly spusal income is sorted into 'unmarried' as well as three terciles, and parental income is sorted into quartiles. Using these as binary dependent variables, along with the standard set of controls, we estimate regressions on the job finding rate and re-employment wages. Since heterogeneous effects by gender has been emphasized as important, especially regarding the effect of spousal income, we estimate model separately for men and women. The results are reported in table C1. Our main result, regarding the job finding rate, is robust for controlling for these alternative insurance mechanisms: for both men and women a higher income quartile of the mother's household tends to be associated with higher job finding rates. However, we cannot distinguish any clear results for the reemployment wages in this setting, which are insignificantly different from zero for both men and women. As for spousal income, we confirm the finding of Lentz and Tranæs (2005b); for men, having a richer spouse is associated with higher job finding rates, whereas the opposite is true for women. Finally, in terms of credit card debt the evidence is mixed: for men, having credit card debt tends to be associated with both higher job finding rates and reemployment wages, while for women having debt in the first tercile is associated with significantly lower reemployment wage than having no debt at all.

Table C1: Estimated effect of mother's household income, relative to other forms of self- or family insurance.

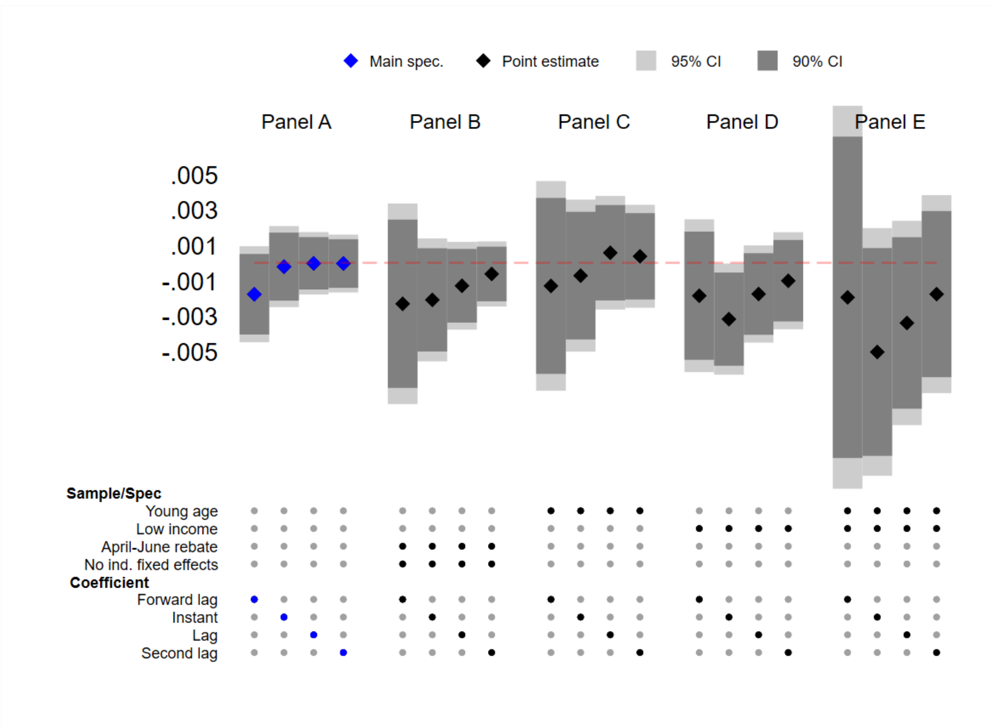
	Men		Women	
	Job find	Log(Reemp. wage)	Job Find	Log(Reemp. wage)
1st debt tercile	0.0644**	0.0111	0.0274	-0.131*
	(0.028)	(0.086)	(0.020)	(0.075)
2nd debt tercile	0.0666**	0.164*	0.0267	-0.0746
	(0.032)	(0.093)	(0.021)	(0.073)
3rd debt tercile	0.0346	0.0778	0.0234	0.0338
	(0.040)	(0.092)	(0.025)	(0.084)
2nd mother's income quartile	0.0345*	-0.0266	0.0195	-0.0911
	(0.019)	(0.097)	(0.016)	(0.101)
3rd mother's income quartile	0.0427*	-0.0289	0.0446**	-0.104
	(0.023)	(0.094)	(0.019)	(0.094)
4th mother's income quartile	0.0418	0.0417	0.0356*	0.0596
	(0.026)	(0.099)	(0.020)	(0.095)
1st spousal income tercile	0.0181	0.0970	-0.00825	-0.111
	(0.025)	(0.077)	(0.025)	(0.090)
2nd spousal income tercile	0.0605*	0.0265	-0.0170	-0.0754
	(0.035)	(0.089)	(0.023)	(0.093)
3rd spousal income tercile	0.0696*	0.216***	-0.0838***	-0.00153
	(0.040)	(0.071)	(0.020)	(0.066)
Individual fixed effects	No	No	No	No
Age controls	Yes	Yes	Yes	Yes
Time-invariant controls	Yes	Yes	Yes	Yes
N	1279	432	1476	487
R^2	0.092	0.141	0.055	0.179

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors in parentheses are robust for heteroskedasticity and clustered at the individual level. Omitted category is unmarried individuals from lowest family income quartile with no credit card debt. Time-invariant controls: quadratics in ability of youth and mother, education of youth and mother (four groups), race and gender of youth. Age fixed effects refer to age of both mother and youth.

D: Additional figures & tables

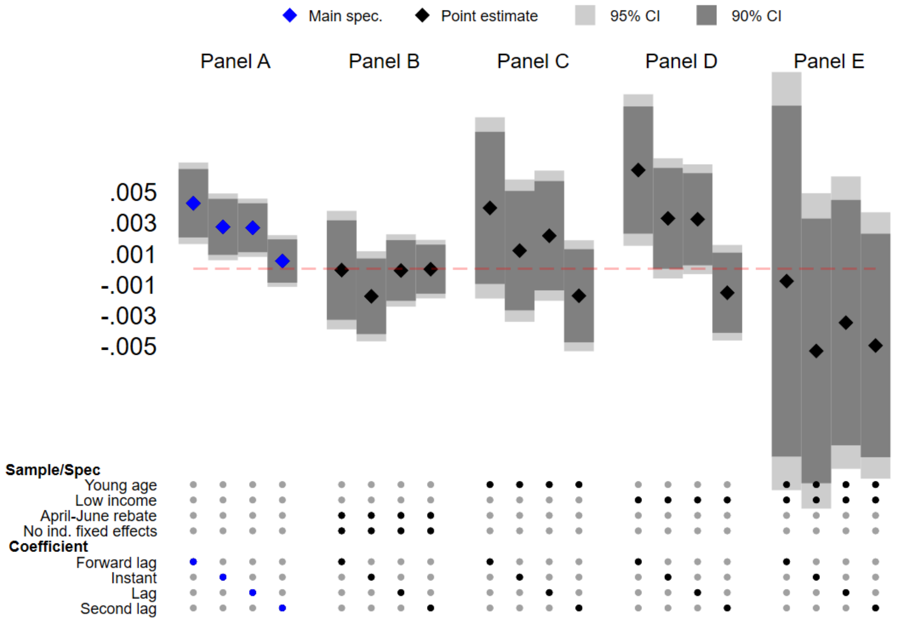
Figure D1: Estimated effect of tax rebate on job-to-job transitions.



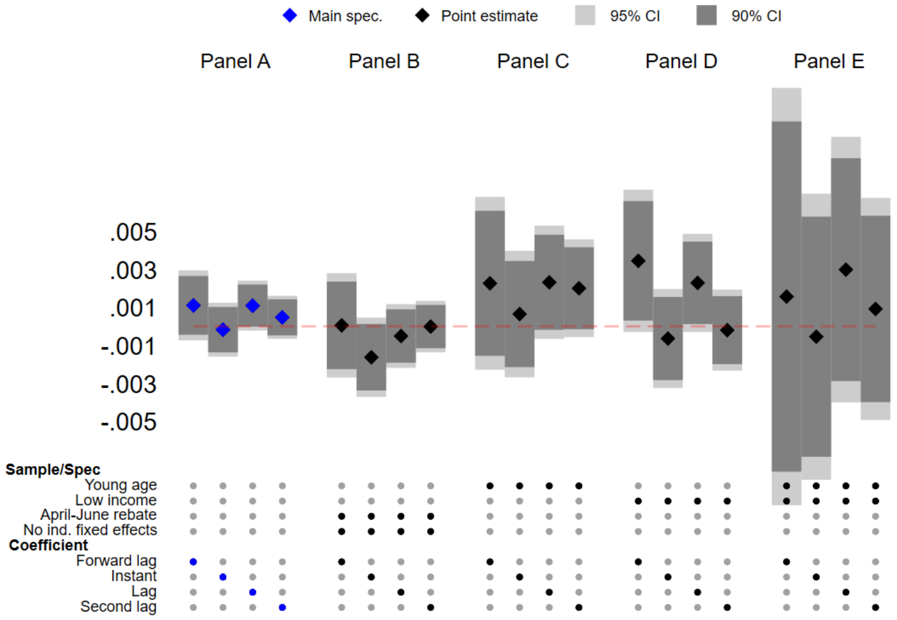
Note:- Data from 2008 SIPP. Each point represent the coefficient corresponding to the indicated variable and sample selection. Results control for month, months from interview and age fixed effects. Standard errors are clustered at the individual level. Regressions are weighted using the SIPP sampling weights.

Figure D2: Estimated effect of tax rebate on job destruction rates.

(a) Job destruction into non- or unemployment



(b) Job destruction into unemployment only



Note:- Data from 2008 SIPP. Each point represent the coefficient corresponding to the indicated variable and sample selection. Results control for month, months from interview and age fixed effects. Standard errors are clustered at the individual level. Regressions are weighted using the SIPP sampling weights.