Blame the Parents?

How Financial Incentives Affect Labor Supply and Job Quality for Young Adults*

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Abstract

Young adults entering the labor force typically have little access to unemployment insurance or other formal insurance mechanisms. We study the role of parental support as an informal insurance mechanism and its effects on labor market entrants. Specifically, we analyze the labor supply and job quality responses of Belgian young adults to parental job displacements. Our estimates correct for unobserved heterogeneity by using the timing of parental shocks before and after labor market entry. We find that a child whose parent loses a job prior to the child's labor market entry is, on average, induced to work 9 percent more in the 3 years following labor market entry than a child whose parents lose a job after the child's entry (where labor market entry is defined as the end of the child's full-time education). This effect is concentrated on the extensive margin and decreases in magnitude over time. We find no evidence that parental support affects the quality of the initial job that entrants find.

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1 Introduction

Labor force participants with more financial resources can afford to stay unemployed longer without sacrificing consumption. Indeed, many papers have shown that more generous unemployment insurance (UI) leads to increased unemployment spells. UI benefit generosity also has important effects on the quality of subsequent jobs (Nekoei and Weber (2015)). However, relatively little is known about the effects of other, non-UI, financial support on labor market outcomes. This topic is especially relevant for labor market entrants, who have elevated unemployment rates, typically do not have access to UI, and may rely on parental support, if available, at the beginning of their careers. In this paper, we use a new identification strategy using the timing of parental job displacements to identify the effect of parental support on children's initial job search and labor market outcomes.

We find that parental support does matter for young adults' job search decisions. Young adults with less support at the time of labor market entry find a job quicker. As a consequence, they work 9 percent more in the first 12 quarters of their career. However, we find no evidence that parental insurance affects the average quality of the job found by young workers. The increase in the amount of days worked appears to be the result of an increase in job search effort and not a decrease in job acceptance's standards. Furthermore, we find that the effects of parental support at the time of labor market entry on the child's labor supply fall over the four years after entry. Lastly, the effect is concentrated on low-skilled entrants from lower income families. Our results are consistent with models in which increased financial resources, in this case due to parental support, allow the unemployed to enjoy more leisure before beginning a job.

Before describing our empirical strategy, we discuss the important ways in which our setting differs from existing studies of the effects of financial support on labor market outcomes. We study young adults, whose unemployment is a key policy topic in most European countries, while the literature on the effects of UI has typically used changes in benefits to experienced and older workers for identification.[1] Therefore, the estimates of the unemployment duration and wage responses to UI may not be applicable to this group. The incentives of young adults to find jobs differ in several ways from those of experienced workers. First, labor market entrants have greater human capital accumulation incentives than older workers, and this may reduce short-run labor supply elasticities (e.g. Keane and Rogerson (2012)). Second, labor market entrants may have more financial support from parents but fewer pre-existing assets than older workers, which affects their relative disutility from unemployment. Third, family support may be associated with a different set of obligations and expectations than government financial transfers, which may change the magnitude of its

moral hazard effects relative to UI.

Our estimates demonstrate that family support can have similar unemployment duration effects as other financial transfers. Furthermore, since we fail to find a large effect on wages or wage growth, it seems that, at least for those induced to find work by the shocks, the intensity of their initial work experience does not meaningfully affect the rate of human capital accumulation. Given a lack of human capital accumulation motive, it may not be surprising that this population is responsive to parental support. Furthermore, we find evidence of lower effects of a parental shock for older entrants, who presumably have some college education. This result is consistent with both a stronger human-capital accumulation motive and the presence of increased family insurance for this group.

We now discuss our empirical strategy. The main challenge to identification in our setup is that households experiencing a job-loss shock plausibly differ from other families in a variety of unobservable ways. Simple comparisons between children whose parents suffer from a shock and children whose parents do not, are therefore likely to produce biased estimates, even if one controls for a large set of observables. Our empirical strategy avoids this "unobserved heterogeneity" bias by only using data on families who experience a job-loss shock around the time of the child's entry into the labor force. Our identifying assumption, whose plausibility we extensively document, is that parents experiencing an involuntary job-loss shock shortly before the child's entry into the labor force do not differ in systematic ways from parents suffering from such a shock shortly after entry. We can therefore identify the causal effect of parental income shocks on initial labor market outcomes by comparing children whose parents suffer from a shock shortly before entry (*treatment group*) with children whose parents experience the shock at a later time and whose initial job search behavior is not affected by a reduction in parental transfers (*control group*). This research design allows us to isolate the effects of job search decisions in the early stages of a career from other medium- and long-term effects of parental job-loss.

We implement our research design using administrative data on Belgian residents. Between 2004 and 2008, we observe over five thousand children whose parents experience the loss of a stable, full-time job in a three year window around the time of the child's entry into the labor force. We first show that the parental job-loss shocks that we identify lead to large and persistent income losses. More than 45% of parents are still unemployed 12 quarters after the shock, with both annual compensation and total days worked remaining at less than 50% of their pre-shock level. This drop in labor income and labor supply persists, without further recovery, for at least 6 years. Furthermore, we find no evidence that spousal earnings adjust in any

[1] E.g. Schmieder, von Wachter and Bender (2016), Krueger and Meyer, 2002, Chetty, 2008, Lalive, Ours and Zweimller, 2006, Nekoei and Weber (2015), Card, Chetty and Weber (2007), Lalive (2007) and van Ours and Vodopivec (2008)).

meaningful way to this change. Lastly, we find that social insurance only partially compensates for the loss in labor income, reducing the overall loss income to approximately 35% in the first year.

Next, turning our attention to the validity of the identifying assumption, we show that parents who experience a shock before their child's entry into the labor force do not systematically differ from parents who suffer from such a shock after entry. After adding appropriate controls, we find minor and mostly insignificant differences between the two groups in demographics and labor market outcomes up to 10 years prior to the shock. A related threat to our identification strategy, discussed extensively in Hilger (2016), is that the parents being laid-off in different years vary in unobservable characteristics. To address this concern, we include year of parental job-loss fixed effects in our specifications. Accounting for this unobserved heterogeneity increases our estimates of the causal effect of a parental job-loss.

We also show that the distribution of shocks around the time of entry is continuous, providing evidence against the existence of self-selection between the treatment and control group. Such voluntary selection would create legitimate concerns about the validity of our research design. For example, as a result of reduced financial needs, parents could choose to quit their job after the child's entry into the labor force. However, given the absence of bunching around the time of entry, this is not a problem in our data. Another concern with our identification strategy is that parental income shocks might induce children to alter the timing of entry into the labor force. However, we find no effect of job loss shocks on the timing of entry: the average child's age at entry for members of the treatment (before) and control (after) groups is statistically indistinguishable (the averages are within less than 0.01 year of each other).

Having established the plausibility of our identifying assumption, we implement our empirical strategy by comparing the labor market outcomes of children whose parents experience a job-loss shock shortly before entry with the outcomes of children whose parents experience a shock after entry. First, we find that children whose parents lose a job in the year prior to entry, on average, work 9% (39 days) more in the first 3 years of their career. Second, the increase in labor supply is largest in the first two years. Third, the increase in labor supply is accounted for by responses along the extensive margin, with a significant increase in average tenure. By contrast, there is no evidence of an increase in days or hours worked per quarter at continuing jobs. Fourth, we find no evidence of a decrease in job quality as measured by wage growth, employer size or industry. Sixth, we do not find evidence of changes in children's decisions to move out of the parental home. Finally, the labor supply response is largest for children without a college education and with parents in the bottom one-third of the wage distribution.

Overall, our results are best explained by a simple model of job search with a constant reservation wage. Labor market entrants who experience a decrease in parental support before entry increase their search effort. As a consequence, their job finding rate is initially higher. This effect progressively fades out as members of

the treatment group, whose parents experience a shock after entry, also face a decrease in parental support and increase their job search effort.[2] We find no support for theories in which labor supply responses are partially the result of variations in the quality of jobs that unemployed workers seek or accept. In particular, we find no evidence that the increase in job finding rates that we document is the result of a lower reservation wage.

The closest paper to this one is Hilger (2016). Hilger uses a similar and concurrently developed identification strategy to study the effects of parental shocks on college attendance in the US. He finds a statistically significant, albeit small, effect of parental income shocks on children's college attendance in the US. Our studies differ due to the setting, outcomes, and sample sizes. First, we focus on youth employment outcomes in Belgium. This allows us to study whether family support has similar effects on labor supply to financial incentives such as UI. We look at a variety of outcomes including days worked, salary, industry wage levels, and wage growth. In contrast, Hilger's main outcome variable is college attendance. College in the US is typically expensive (although need-based financial aid can be generous for certain families). On the other hand, college tuition in Belgium is highly subsidized for everyone, with tuition fees lower than €1,000 per year. Therefore, the small effects of layoffs on college attendance in the US serve as an upper bound on the effect in Belgium. Hilger does look at earnings related variables, but does so mainly in the context of the tradeoff between schooling and work, whereas we condition our estimates on a child's entry into the labor market. Lastly, Hilger's dataset contains many more layoff events, allowing him to have more precision in estimating heterogeneous treatment effects.

Our results relate to a large literature on the relative costs and benefits of social insurance. One potentially important benefit of unemployment insurance is that it allows unemployed individuals to look longer for better matching and riskier jobs (e.g. Acemoglu and Shimer, 1999, 2000, Acemoglu, 2001). This mechanism leads to higher optimal unemployment benefits than implied by simpler models, which do not take into account the productivity gains that can result from a prolonged job search. However, most empirical studies have failed to find evidence that UI leads to better matches or higher paying jobs.[3] A possible reason for this negative result is that existing studies focus on experienced workers, for whom the match quality channel might be less important because of their established labor market credentials.[4] Our research design allows us to study these effects on a much younger population — workers who are entering the labor force. Previous research has found that macro-economic conditions at the time of graduation have long-lasting effects on

[2]The lack of long-term effects is in line with von Wachter and Bender (2006), who find that wage losses from early-career job displacements drop to zero after 5 years.

[3]This is the case for Card, Chetty and Weber (2007), Lalive (2007) and van Ours and Vodopivec (2008). One notable exception is Nekoei and Weber (2015).

[4]One paper that does study young workers is Kaplan (2012). He uses a structural model to show that parental insurance results in a 5% higher wage for young adults at age 23.

workers' entire career outcomes (Kahn, 2010, Oreopoulos, von Wachter and Heisz, 2012, and Oyer, 2008), creating at least suggestive evidence that the "job-quality" channel might be more relevant for first-time job-seekers.[5] However, we find no evidence that reduced family support induces workers to find higher paying or more risky jobs.

Our paper also contributes to a large literature on the importance of family insurance. Most of this research studies spousal insurance rather than child-parent insurance. For example, Blundell, Pistaferri and Saporta-Eksten (2014) show that 25% of married males' labor income shocks are insured by the labor supply response of their spouses. Furthermore, they show that the importance of this insurance channel depends on the level of self-insurance provided by asset accumulation.[6] We show that, at least for families with young adults, parental income shocks can also affect children's employment outcomes at the beginning of their career and that the labor supply of the young adult can also act as a form of insurance in the household.

Our work is also related to the literature on the effects of parental income on children's short- and long- term outcomes (Gertler, Levine and Ames, 2004, Currie, 2009, Dahl and Lochner, 2012, Hoynes, Schanzenbach and Almond, 2012). This literature finds that changes in family resources early in a child's life have large long-run effects. We find no evidence that changes in family resources have similar effects when the child has grown up. Finally, our paper complements the findings of Kaplan (2012) who shows that parents provide insurance to their children during the first years of their career by allowing them to move back into the family home during jobless episodes.

The rest of this paper proceeds as follows. In Section 2, we provide necessary information on the Belgian institutional setting and the data used for this project. We discuss our sample selection procedure in section 3. Section 4 presents descriptive statistics on children's entry into the labor force as well as evidence on the magnitude of parental income shocks. Section 5 describes our identification strategy as well as the theoretical predictions that we test in our analysis. Section 6 presents tests that establish the credibility of our research design. Finally, section 7 contains the empirical results on the effects of parental shocks on child labor market outcomes and section 8 concludes.

[5] See also Gervais et al. (2014), who model the trial-and-error process by which young workers sample jobs in the first years of their career in order to discover the occupation in which they are the most productive.

[6] Other papers that study the importance of spousal insurance include Cullen and Gruber (2000), Stephens (2001), Hyslop (2001) and Tella and MacCulloch (2002). Blundell and Macurdy (1999) review the traditional modeling approaches in this field. There is also a large literature on child labor in developing countries, which can be thought of as a form of family insurance. For a review, see Edmonds, 2008.

2 Data and Institutional Setting

2.1 Institutional Background on Children

Our research design requires knowledge of the time at which children put an end to their regular full-time education and enter the labor market. This is made possible, in our set-up, by a combination of institutional characteristics of the Belgian social security system. This section provides the necessary institutional background required to understand how we identify the timing of labor market entry. It also provides general information on Belgium's education system that will prove useful in understanding our results.

Secondary and tertiary education. In Belgium, school is compulsory until the age of 18, at which point students are expected to have completed high school. However, high repetition rates lead a large share of students to complete high school with one or more years of delay or to enter the labor market without completing the entire high school curriculum.[7] Between 12 and 18, high school students are progressively sorted in technical, professional or general programs. Technical and professional education is usuallys associated with lower-ability students who typically enter the labor market directly out of high school or enroll in shorter tertiary programs with a vocational focus. By contrast, students who have followed the general high school program most often try to acquire a regular tertiary education.[8]

There are two types of higher education institutions in Belgium : universities and colleges. Universities provide a more academic education lasting 4 to 5 years and usually leading to higher paying jobs. Colleges provide a more vocational and technical training in programs lasting 3 to 5 years. With a few exceptions, the only admission condition to both types of education is a high school degree of any sort: institutions are not allowed to set other admission standards. As a consequence, while a large share of each cohort registers for higher education, initial success rates are low.[9] Overall, this translates into a graduation rate from tertiary education of 44% in the 30-34 age group, with approximately equal share of college and university graduates (OECD, 2014).

Family Allowances. Family allowances (also called child benefits) are transfers from the state to families with children. Family allowances in Belgium consist of automatic monthly cash payments to parents of dependent children under the age of 25. Eligibility for family allowances is unconditional until the child

[7] As a consequence, in 2013, only 83.1% of 20-24 year olds in Belgium had an upper-secondary degree, a number that is nonetheless 2% higher than the EU average (source: Eurostat).

[8] Available data from the Dutch-speaking part of the country indicate that 45% (55%) of high school graduates obtain a general (technical or professional) degree with 87.3% (46.7%) of general (technical or professional) high school graduates enrolling in higher education (this can be deducted from Table 2 of Declercq and Verboven, 2014). Given that education systems are administered separately by Belgium's 3 linguistic communities (Dutch, French and German), aggregate statistics for the entire country are hard to obtain. Nonetheless, since the overall structure of the education system remains similar in the 3 different communities, these numbers still provide a broadly accurate picture for the entire country.

[9] Around two-thirds of students either drop out of higher education, repeat their first year or switch to a less selective education (Declercq and Verboven, 2014).

reaches 18, the upper limit of compulsory schooling in Belgium. No child is eligible for family benefits above 25. Between the age of 18 and 25, benefits are only paid for children who are enrolled in full-time education or apprenticeships. Eligibility for family allowances after 18 also requires students to work less than 240 hours per trimester (half the normal full-time work hours), except during the summer when there is no limit on student work. Moreover, and this is an important feature in our set-up, students who finish their full-time education are usually eligible for up to 9 additional months of family benefits, provided that (i) they have completed high school or have obtained a higher education degree, (ii) they are registered with the local public employment agency and (iii) they earn less than €520 per month (in 2014).[10] Since eligibility for family allowances requires the continuation of full-time education, our sample selection will primarily rely on family allowances' payment data to identify the timing of children's entry into the labor force.[11]

Family allowances are not generally means-tested, although payments do increase with a child's age and the number of children in the household. Baseline benefits are also increased for families with unemployed, retired, sick or single working parents. Monthly payments for full-time students 18 years old or above range from €118 for one-child families without increased benefits to €401 per child for orphaned children.[12] Therefore, family allowances typically represent a small but significant share of the household budget. Lastly, family benefits do not cover the full cost of maintaining a child. While there is no official estimate of child-rearing costs, welfare benefits paid by the Belgium's residual social safety net provide a good comparison point: as of 2013, these benefits stood at €545 per month for dependent members of a household.[13]

Unemployment assistance for new labor market entrants. As is the case in most countries, access to regular unemployment benefits in Belgium requires a minimum level of work experience. However, new labor market entrants in Belgium are eligible for special unemployment benefits ("*allocations d'attente*") after a (usually) 9-month waiting period.[14] During the waiting period, unemployed individuals are expected

[10] The same limit applies to children in an apprenticeship.

[11] People prove their allowance status in the following manner. During the September of the year in which a child turns 18, parents receive a form regarding their child's student status. This form requires a registration document from a college or university and is validated by December. Parents receive payments retroactively (from September) to next September unless a child's status changes. Second, students cannot work for more than 240 hours per semester (except in the summer prior to the last year of schooling). Students who do work more than this amount lose their benefit eligibility.

[12] Specifically, baseline monthly payments (in 2014 Euros) are 90, 167 and 249 for the first, second and third (or more) child respectively (all amounts have been rounded to the nearest integer). The baseline amounts are higher (varying from 136 to 272) for unemployed, retired, sick or single working parents earning less than €2,310 per month (2,386 for two-parents households). Baseline amounts are further increased by €24 per month for kids above 12 and by 28 for kids above 18 in families receiving normal benefits. Appendix A.2 discusses other details regarding family allowance payment amounts.

[13] Another useful point of comparison is the method most commonly used by civil courts to determine child support payments (*Méthode Rénard*). According to this method, the cost of maintaining an 18 year old child in high school is equal to 27% of total family income. For a single-earner family, using the above-mentioned average net-of-tax labor income of €2,050, this method implies a monthly cost of around €553.

[14] This waiting period is extended to 12 months for graduates over the age of 25. The waiting period is also shortened by the number of days previously worked. For example, a young graduate who worked on a regular employment contract for 2 months during his higher education, would be eligible for the "*allocations d'attente*" 7 months after graduation. By contrast, the waiting period would not be extended if the graduate worked part-time during these 7 months, since all days (whether employed or unemployed) are taken into account during the waiting period. Note, however, that Belgian students often work

to rely on the financial support of their parents, who have the legal obligation to support them and who, as explained above, continue to receive family allowances under certain conditions. Beneficiaries are also required to stay continuously registered as active job-seekers with the public employment agency during the waiting period in order to later benefit from "*allocations d'attente*". During the time covered by our data, these special benefits were paid, without time limit, at the monthly flat rates of €425 for dependent children and €817 (€493) for individuals above 21 (between 18 and 20) living alone.

Labor market entrants who are not eligible for "*allocations d'attente*", as well as children of parents who cannot provide them with sufficient support, can turn to the residual social safety net which provides welfare payments to individuals who have no other sources of revenue. In 2014, maximum monthly payments are €545 for a dependent, €817 for singles and €1,090 for heads of households. These payments are fully means-tested: except for a small exempt amount,[15] welfare agencies only pay the difference between the maximum amount and all other financial resources of the beneficiary. For children still living with their parents, this includes both their own as well as their parents' income. Therefore, as long as they do not formally leave the parental home, new labor market entrants are eligible for welfare payments only if they live in very low income families.

In summary, new labor market entrants in Belgium are eligible for some public assistance. However, during their studies and first 9 months of unemployment, young job-seekers are expected to rely primarily on the financial help of their parents. This motivates our focus on parental income shocks throughout the rest of this paper.

2.2 Institutional Background on Parental Shocks

To implement our research design, we need to correctly identify parental income shocks resulting from the loss of a stable full-time job. This section describes how the Belgian Social Security data allows us to identify involuntary job displacements.

Unemployment insurance for experienced workers. Experienced workers who suffer from an involuntary job loss are eligible for unemployment benefits without any time limit, a unique feature of Belgium's unemployment insurance. Benefits are not available for workers who quit. Benefits are computed as a percentage of the last job's gross salary, subject to minimum and maximum amounts that depend on individual characteristics. The generosity of employment benefits decreases with the duration of unemployment because of

under a special contract with reduced social security contributions: days worked under such contract are not taken into account to shorten the waiting period. Eligibility for special unemployment benefits also requires the unemployed to have obtained a regular high school diploma or a lower-secondary diploma from a technical high school.

[15] €155 per month for a dependent.

lower replacement rates as well as lower minima and maxima. The evolution of benefits over time depends on the family situation with heads of household entitled to the highest benefits followed by single job-seekers and finally non-head members of a households who receive the lowest benefits. For example, during the period covered by our data, newly unemployed heads of household received between €1,135 and €1,603 while non-head household members were initially subject to the same maximum but benefited from a much lower minimum of €715. The evolution of unemployment benefits over the unemployment spell also depends on the beneficiary's employment history: workers with a longer employment history benefit from a slower decline in replacement rates over time.[16]

As a result of those rules, OECD data indicates that, in 2010, net replacement rates during the first 6 months of unemployment varied between 73% and 84% for workers who have lost a minimum wage job and between 42% to 62% for workers at 150% of the average wage. After 60 months of unemployment, these replacement rates dropped to between 64% and 80% for minimum wage workers and 37% and 56% for workers at 150% of the average wage. Overall, while Belgium's unemployment insurance system appears generous compared to the US, it does not come close to fully insuring workers against the risk of unemployment. At all levels of the wage distribution, unemployment - in particular, long-term unemployment - still translates into a substantial loss of income.

Employment protection.[17] During the period covered by our data, Belgium had different employment protection rules for white- and blue-collar jobs (which each represent roughly 50% of jobs). Firing white-collar workers in Belgium did not require any formal motivation or administrative procedure. In compensation for the absence of such constraints, employers were required to provide workers with advance notice ("*préavis*") of the decision to fire them. After a 6 month trial period during which dismissal only required 7 days notice, workers were eligible to 3 additional months of notice for every 5 years of seniority. Employers who wished to immediately get rid of workers could do so but they were required to pay the entirety of the regular compensation which would have accrued to the worker if he had continued working throughout the advance notice period. Advance notice requirements for blue collar workers are usually much shorter: the maximum default is 4 months (120 days) after 20 years of seniority. In exchange for these short notices, employers were required to justify their firing decision on individual or economic grounds.

The existence of these employment protection regulations raises two questions. First, advance notice requirements imply that parents - especially those with long tenure - might have advance knowledge of their

[16]Table A1 provides a more detailed summary of the rules applicable to the computation of unemployment benefits as a function of the claimant's demographic situation and the duration of the current unemployment spell.

[17]For a more detailed summary of employment protection regulations applicable during the period covered by our data, see http://www.oecd.org/employment/emp/42745360.pdf.

dismissal before the observed job loss. As a consequence, they might already restrict financial transfers to their child during the advance notice period. In this case, children whose parents suffer from a shock after entry into the labor force would already have been treated with an higher incentive to look for a job during their initial job search episode. This would bias our estimates downward because our treatment and control groups would have been (at least partially) treated. To address this concern, we will test the robustness of our results by restricting the control group to parents who experience a shock at a more distant date from the child's entry into the labor force.

Severance payments in case of immediate dismissal create another potential problem. In such cases, dismissed workers are not suffering from an immediate shock to their available cash-on-hand since severance pay is equal to the salary they would have received during the advance-notice period. One might therefore argue that we should not expect a sudden reduction of parental transfers to the child. We address this concern directly by verifying that our results are robust to restricting the analysis to children whose parents receive no severance payment.

2.3 Data

The implementation of our research design requires administrative data to identify the child-parent relationship, the timing of children's entry into the labor force as well as parental employment history and children's labor market outcomes. In Belgium, access to this information is made possible by the *Labor Market Data Warehouse* (LMDW) of the national *Crossroad Bank for Social Security* (CBSS). Since 1998, this database aggregates data on all Belgian residents from many governmental institutions. The data used in this paper comes from the following sources: demographic information from Belgium's National Registry, family benefits' data from the family allowances administration, employment data from the central Social Security office and the pension registry, unemployment benefits history from the UI and job placements agencies as well as welfare, disability and pension benefits data from each relevant administration. For further information, A.1 provides more details about the different sources of our data.

Most of the data is available at the quarterly level since 1998 although some data is available at the monthly or yearly level and some institutions have only been added to the database in 2003. For most institutions, data for parents is available from the beginning of 1998 to the end of 2011 and data for kids from at least 12 quarters before entry until the last quarter of 2011. In addition, we have access to parents' employment history for more than 10 years before the job loss shocks.

3 Sample Selection

Sample selection proceeds in two steps starting from the universe of Belgian residents. First, we identify the universe of entrants into the labor force between 2004 and 2008 as well as the exact timing of their entry. Second, we identify the parents of the entrants and any parental job loss shocks taking place within a three year window around the child's entry into the labor force.

Step 1: Identifying Entrants. The first step of our selection process is to identify entrants into the labor force. These are children who put an end to their full-time education and either work immediately or start their career with an initial unemployment period. As described in the previous section, eligibility for family benefits after the end of compulsory schooling requires the continuation of full-time studies. Identification of entrants therefore starts with the selection of individuals who stop receiving family benefits. Since family allowance payments stop when the child reaches 25 (even if he is still a student), we restrict our sample to individuals who are younger than 25 in the first quarter for which they do not receive family benefits. This constitutes the initial universe of entrants we consider.

Family allowance payments for unemployed labor market entrants can continue for up to 9 months after graduation (i.e. during the waiting period for unemployment insurance). An individual will therefore be observed as receiving family benefits during a quarter of this period either if he is unemployed during an entire quarter or if he is partially employed but earns less than the maximum allowed amount (520 euros) in every month of the quarter. Therefore, the loss of family benefits cannot naively be used to precisely identify the exact timing of entry into the labor force. Fortunately, this problem can be addressed by using two additional data sources: the Employment Registry from the Social Security Administration and registration data from the public Job Placement Agencies.

We use the following algorithm to determine the quarter of entry Q for all entrants in our sample. We start by identifying the last quarter T for which the child is receiving family benefits. We then look in quarters $T-3$ to $T+2$ and identify the quarter of entry Q as the first of two consecutive quarters for which the child is either (i) not receiving family allowances, (ii) registered as a job-seeker with the public employment agency or (iii) working for more than two-thirds of the quarter.[18]

The rationale for this algorithm can be understood by considering different types of entry into the labor force. First, the third criteria (employment) will correctly identify the timing of entry for individuals who start working full-time directly out of school and keep their initial job for at least two quarters. Second, individuals who leave school without having found a full-time job are required to register with the public

[18]Individuals who are registered as unemployed in quarter $T-2$ and are working more than 66% of quarter $T-1$, will be considered as entering in quarter $T-2$ (even if they are not registered as unemployed at any point in $T-1$).

unemployment agency in order to keep receiving family allowances during the U.I. waiting period and to be eligible for subsequent unemployment benefits. The timing of entry for those children will therefore be correctly identified using the second criteria (registration with the public employment agency). Third, individuals who initially find a part-time (less than 66%) job (and therefore do not meet the third criteria) are also required to register with the public employment agency in order to continue receiving family benefits during the U.I. waiting period (if they make less than 520 euros per month) and be eligible for part-time unemployment benefits at the U.I. waiting period. In those cases, the second criteria will again correctly identify the timing of their entry into the labor force. Fourth, the first criteria (loss of family benefits) will also correctly identify the timing of entry for individuals who have not directly found a job and fail (for whatever reason) to register with the public employment agency. These individuals will indeed lose their family benefits as soon as they stop being full-time students.

All intermediary cases are correctly taken into account by the alternative nature of the algorithm. For example, individuals who initially find a job but fail to keep it for more than 2 quarters (and therefore become unemployed in their second quarter), are identified as entering in the first quarter after graduation, since they meet the first criteria (employment) in the first quarter and the second criteria (registration as a job-seeker) in the second.

The choice of the $T-3$ to $T+2$ observation window is motivated by the fact that the maximum UI waiting period is 9 months so that some entrants can be observed as receiving family benefits at any point during the 3 quarters following entry. The rationale for requiring that the conditions be met for at least 2 consecutive quarters is that, since students who have passed all their exams in June do not have any coursework between July and the end of September, some of them work during a large share of the summer.[19] If we only took a single quarter into account, those students who work for more than two-thirds of the summer would meet the "employment criteria" and would be identified as entering the labor market at that moment, even if they go back to school at the end of the summer and graduate in June of the next year (3 quarters later). By requiring that at least one of the criteria be met for two consecutive quarters, we avoid this problem.

Finally, it should be noted that this method will still lead, in some cases, to a slight (one quarter) mismeasurement of the timing of entry. This is the case, for example, when children enter in the second or third month of the quarter and find a full-time job immediately upon entry. Since they work during less than two-thirds of the quarter, they will only be registered as entering the labor force in the next quarter. We address this problem by excluding from the sample children whose parents suffer from a shock during the identified quarter of entry. We also test the robustness of our results to the exclusion of shocks occuring

[19] Family benefits are paid during the summer even if the child earns more than €520 in July, August or September.

within a year around the time of entry.

Step 2: Identifying Parents and Job Loss Shocks. We do not have access to a direct measure of biological filiation. Instead, we rely on the household identifier and the position of each individual in that household provided by the National Registry. Individuals who are registered as "head" or "co-head" of the child's household are identified as his parents. To avoid any endogeneity that might arise from changes in family composition as a result of the job loss shock, we identify parents based on family composition 16 quarters before the loss of family benefits (that is, before the first date at which we identify a parental job loss).

Next, we identify parents who experience the loss of a stable full-time job in a 3-year window around the child's entry into the labor force. A parent (father or mother) is identified as suffering from such job displacement in quarter T if he or she (i) has been working for the same employer for at least 12 quarters (from $T-11$ to T) and is not working for this employer in quarter $T+1$, (ii) has been registered as having a full-time job for at least 10 out of the last 12 quarters, and (iii) is registered at the end of T or $T+1$ as either (a) receiving unemployment benefits and looking for a job or (b) in a state of "*unregistered activity*" according to the socio-economic status variable provided by Belgium's *Labour Market Data Warehouse*. An individual is considered in a state of "unregistered activity" if and only if he does not appear in any of the other databases of the Belgian Social Security System, namely if he does not have a salaried job, is not registered as self-employed, is not registered as a job-seeker with the public employment agencies and is not receiving any benefit payments from the various unemployment, sickness, invalidity, workers' compensation, pension, family allowances or welfare agencies.

The motivation for our selection process is the following. First, our definition of a stable job as a job held for at least three years results from the practical consideration that this is the longest time-window for which we observe the uncensored quarterly employment history of all parents in our sample at the time of the shock.[20] Second, we only require the full-time condition (ii) to be met for 10 - rather than 12 - out of the 12 quarters prior to the shock in order to avoid excluding from our sample parents who, at some point in the last three years, have temporarily reduced their working hours for personal reasons (a possibility that benefits from public support in certain situations) or because of a temporary decrease in economic activity at their firm (Belgian regulations also allow for temporary reductions in work hours in certain such situations).

Condition (iii) is meant to restrict the sample to involuntary income losses resulting from the dismissal of the worker and exclude job separations resulting from job-to-job transitions, very short unemployment

[20]The quarterly Social Security Employment Registry data is available starting in 1998. Children in our sample enter the labor force starting in 2004 which means that - given that we select parental shocks in a 3-year time window around entry - the first parental shocks are observed in 2001. This gives us a maximum of 3 years (2001-1998) of uncensored employment history for the first parents getting a shock in our sample (we also have yearly employment history up to 10 years but this data does not allow us to compute the quarter of job loss).

periods, switches between salaried employment and self-employment, episodes of disability (due to sickness or accident) as well as retirement decisions. As a consequence, condition (iii) restricts our sample to those job separations followed by a period of insured unemployment or "unregistered inactivity" (as defined above). We include this second category in our sample to cover the case of individuals who are fired without notice by their employer and receive severance pay in compensation for their immediate dismissal. As a consequence, these individuals will not be eligible for unemployment benefits for the period covered by severance compensation (which, as we have seen in the previous section, can last for several quarters). During that period, they will therefore not appear in UI agency data and will be recorded in the residual "unregistered inactivity" category of the socio-economic status variable. Such cases of immediate dismissal are far from uncommon. Excluding them would therefore significantly reduce our sample size as well as prevent us from observing those individuals for whom the job-loss shock is arguably the most unexpected. However, the inclusion of this category raises one concern.

Individuals who voluntarily quit their job in Belgium are not initially eligible for unemployment benefits. Therefore, except if they are covered by other forms of social insurance (such as pension benefits or the residual social safety net), they will also appear in the residual "unregistered activity" category. Our procedure will therefore incorrectly identify those parents as suffering from an involuntary job loss. This might appear as a particularly relevant problem in our set-up. As their child gets a job and does not need their financial support anymore, the marginal value of additional disposable income for the parents decreases and they might find it optimal to voluntarily quit their job as a result. This would pose two problems for our empirical strategy. First, one might argue that only financially unconstrained parents would choose to voluntary leave a full-time job. If this is the case, there would be no reason to expect voluntary job quitters to reduce financial transfers to their child. Our estimation strategy would therefore fail to identify meaningful variation in the child's incentive to search for a job. Second, to the extent that parents base their decision to quit on the time at which their child finds a job (i.e. leaving their job once they do not need to support their child), this could create a selection bias between our treatment (before) and control (after) group. In such case, the control group would indeed be composed of a larger proportion of voluntary job quitters which could result in systematic (and potentially unobserved) differences with the treatment group.

Our response to these concerns will be twofold. First, we will show that the distribution of shocks around the time of entry is continuous, showing no sign of elevated levels of quits before or after entry. There is therefore no evidence that voluntary selection in the treatment or control group could bias our estimates. Second, since only involuntary job losers are initially eligible for both unemployment insurance and severance payment according to Belgian law, we will show that our results are robust to restricting our sample to parents who are observed as receiving either unemployment benefits or some form of severance

pay upon the loss of the job. In any case, if the main worry is that voluntary parental job losses do not result in increased incentives to look for a job, this would create a downward bias in our estimates which would constitute a lower-bound for the real effect of an involuntary job loss.

Finally, since some of the data sources used in the construction of the "unregistered activity" category were only added to the LMDW in 2003, we exclude from our sample children whose parents experience a job loss shock before 2003. This does not affect our main results since our benchmark estimates focus on children entering the labor force between 2004 and 2008 and whose parents experience the job loss shock in a three-year window around entry.

4 Descriptive Statistics

In this section, we discuss some important descriptive features of our dataset. After briefly discussing summary statistics, sub-section 4.1 presents a quantitative description of children's entry into the labor force. Sub-section 4.2 provides a description of parents' employment outcomes following the loss of a stable full-time job.

Privacy laws in Belgium do not allow us to work with the universe of entrants available in the Belgium's *Labour Market Data Warehouse*. However, we have been allowed to extract a sample of roughly 70% of entrants entering the Belgian labor market between 2004 and 2008 and whose parents suffer from the loss of a stable full-time job in a 3-years around entry. For comparison purposes, we have also extracted a representative sample of approximately 6.5% of all labor market entrants identified between 2004 and 2008 (i.e. we only applied the first two steps of the selection process described in the previous section without limiting the sample to children whose parents experience a job loss shock).

Table 1 presents summary statistics for the representative sample of all entrants (column I) as well as for the samples of entrants whose parents suffer from a job-loss shock in a three or one year window around the child's entry into the labor force (column II and III respectively).[21] Given the strictness of our selection process, we end up with 5,605 (2,185) entrants whose parents suffer from a shock in a 3 (1) year window around entry.

A few points are worth discussing. First, since we sometimes identify more than one entrant by family, the number of households is smaller than the number of entrants. For example, the sample of all entrants has nearly 57,000 children but fewer than 44,000 households. Second, as a result of the high rate of grade

[21] The 3 (1) year sample contain all individuals whose parents are getting a shock between -12 (-4) and +12 (+4) quarters around the quarter of the child's entry into the labor force. As explained in the previous section, we exclude children whose parents experience a shock on the exact quarter of the child's entry. We also exclude children whose parents are getting more than one job-loss shock in the time window considered (this constitutes a negligible share of all entrants).

repetition in the Belgian education system, children often enter the labor force at a later age than would be expected based on the normal duration of occupational programs. For example, fewer than 30% of children enter the labor force before 20 even though more than 50% of individuals in recent cohorts do not graduate from higher education, and would therefore be expected to graduate from high school at age 18. Third, entrants are frequently not fully employed. On average, they have a job during 8.7 of the 12 quarters after entry. During the same period, they work an average of 460 days (they would be expected to work between 700 and 800 days if they continuously held a full-time job). As will become clear in the next section, this is the result of both a high level of unemployment in Belgium and the fact that it takes several quarters after entry to reach the steady-state level of employment. Fourth, as is to be expected for new workers without experience, labor market entrants receive a much lower wage than the average worker: the average wage in our samples is between the 27^{th} and the 30^{th} percentile of the wage distribution.

Fifth, while we identify parents based on household composition data rather than direct information on filiation, the age of parents in our sample suggests that we have mostly identified biological parents. The average age for mothers at a child's birth in the sample is close to 27, which is around the average mother's age at birth as reported by official Belgian population statistics. While we do not have similar official data for fathers, the average age at birth for fathers in the sample of all entrants (29.36) is consistent with the idea that males usually partner with slightly younger women. More convincingly, Figure 1 displays the distribution of fathers' and mothers' age at birth in our data. For mothers, the figure also displays the empirical distribution of mothers' age-at-birth from official statistics based on the universe of births in Belgium in 1985 (the average year of birth for children in our sample). Both distributions are consistent with the hypothesis that we mostly identify biological parents: strikingly, the age distribution for mothers in our data is visually indistinguishable from the official birth statistics.

Finally, the job-loss groups systematically differ from the sample of all entrants. First, children in the job-loss group seem to be selected from less advantaged parts of the distribution: their parents were younger when they were born, they enter the labor force earlier (indicative of lower levels of education), they are less likely to be Belgian citizens, they work less after entry, and have a lower wage and income. Given the well-documented intergenerational correlation in employment outcomes, one should not be surprised that children of parents who suffer from a job-loss shock display lower-than-average employment outcomes.

Other differences between the job-loss group and the sample of all entrants are simply the mechanical consequences of our selection process. The job-loss group presents a higher concentration of two-parents family (the likelihood of at least one parent losing his job is higher if there are two parents in the household) and a higher concentration of single fathers among single-parents households (single mothers are more likely to be out of the labor force altogether and, as such, less likely to lose a job). Parents in the job loss group

are also much more likely to be employed 16 quarters before entry (losing a stable job in a 3-year window around entry requires one to have had a stable job in the past).

4.1 Children's Entry Into the Labor Force

Figure 2 presents a graphical summary of the transition process from full-time education to active life for children in our representative sample of all entrants. It serves first as a validation of our sample selection procedure. This figure is also interesting in its own right since it represents, to our knowledge, the first description of the transition process between full-time education and active life, at least at this level of detail.

Panel A, B and C show that we have succeeded in correctly identifying the timing of children's labor market entry. First, there is an increase in individuals having at least one job around the time of labor market entry (Panel A).[22] Moreover, while there are already around 20% of entrants who have at least one job several quarters before entry, Panel B reveals that these are overwhelmingly part-time jobs, with average days worked at a job approximately 20 per quarter (less than a third of a full-time job schedule). This result is consistent with the idea that these are mostly student jobs. In contrast, there is a clear increase in the number of days worked around the time of entry as many entrants progressively find full-time jobs. Panel C shows a jump in the number of individuals receiving unemployment benefits 3 quarters after entry, which is consistent with the regulations described in section 2.1.[23]

On a more substantive note, labor market entrants reach a stable employment level at a quick rate: both labor force participation and total days worked per job more than double between the two quarters around entry. Nearly 80% of all entrants have at least one job in the quarter following their entry. While there is still an increase in the next 3 quarters, there are only limited additional changes afterwards. The same pattern roughly holds for total days worked during the quarter (Panel B) as well as unemployment insurance (Panel C). In this last case, one initially sees unemployment benefits increase strongly as the U.I. waiting period ends (3 to 4 quarters after entry).[24] U.I. benefits receipts then peak and decrease afterward as labor market entrants progressively find a job. After 6 quarters, further decreases in unemployment are limited.

Two additional findings are worth mentioning. First, Panel C shows a small increase in receipts of welfare benefits around the time of entry as children lose their family benefits or leave the parental household and

[22] There is already a small jump in labor force participation one quarter before entry consistent with the fact, explained in the previous section, that our procedure can lead to a small (one-quarter) mismeasurement in the timing of entry.

[23] As we have seen in section 2.1, the 9-month U.I. waiting period can be shortened in some cases (e.g. if the individual has worked on a regular employment contract at some point in the past). This explains why some individuals start receiving unemployment benefits before the third quarter after entry.

[24] UI claims likely reach a peak in quarter 4 both because some entrants are only eligible for part of the 3rd quarter and may not file in time and because of a potential 1 quarter classification error.

turn to the residual safety net for help. However, due to the means-tested nature of those benefits, only a small portion of entrants receive them at any point in the sample. Second, consistent with existing literature on the returns to experience in the labor market, Panel D shows a fast increase in wages in the first years of a child's career. Before entry into the labor force, children are concentrated in the bottom part of the income distribution (the average wage percentile is less than 15), again consistent with the fact that they are mostly holding student jobs. Four quarters after entry, they are still 20 percentile points below the median. Six years after entry they have closed more than half of that gap, with an average wage percentile above 40.

4.2 The Labor Market Outcomes of Parents After the Shock

Figure 3 presents a graphical description of the consequences of the parental job-loss shocks. This figure confirms that our selection procedure correctly identifies large shocks to employment and labor earnings. It also allows us to quantify the magnitude of income losses suffered by parents.

In line with our definition of the job-loss shock, Panel A and C show that all parents have a job and none of them are registered as unemployed or in a state of "unregistered inactivity" in the 12 quarters before job loss. Also consistent with our sample selection procedure (which restricts our sample to jobs equivalent to more than 66% of a full-time work schedule), parents' labor supply in all 12 quarters before job loss is on average equivalent to more than 80% of the total working time for a full-time (100%) worker.

By contrast, both labor supply and income experience a sharp drop around the time of the job-loss shock with parents working on average less than 20% in the quarter following job loss (panel B) while nearly 60% are receiving unemployment benefits and 20% are in a state of "unregistered inactivity" (Panel C).[25] Consistent with the idea that individuals in the "unregistered activity" category are mostly workers who have been dismissed without notice, and are not eligible for unemployment benefits during the period covered by severance pay, the proportion of parents in this category quickly drops over the next two quarters as they either find a job or become eligible for unemployment insurance. Three quarters after entry, less than 10% of parents are still classified in a state of "unregistered inactivity"[26].

Perhaps the most striking feature of these graphs is the persistence of the income shock. The rate of unemployment decreases slowly after the shock and the employment rate never comes close to a full recovery. Three to six years after the shock, only about 50% of parents have a job in any given quarter (panel A) and

[25]The share of individuals receiving UI benefits or in a state of "unregistered inactivity" do not sum up to one in any given quarter, since we require to be in one of those states either in the quarter of job loss or the next.

[26]Figure A4 also shows that, in the vast majority of cases, we are not incorrectly confusing job-loss shocks with voluntary transitions to retirement or self-employment or with sickness shocks (which are better insured in Belgium). Even five years after the shock, only about 20% of those we identify as suffering from an involuntary job-loss shock are either retired, registered as self-employed or receiving sickness benefits.

the average labor supply is still at less than 40% of that of a full-time worker (panel B). Moreover, even those parents who do find a job suffer from a substantial long term decrease in their wage. Panel D shows that the average wage percentile was close to the median for the rest of the salaried population during most of the 12 quarters before the shock. However, after the job displacement, the average wage percentile drops by nearly 10 percentage points and never recovers. Therefore, even parents who do find a job experience a significant pay cut.

Panel A of Figure 4 provides further insight by displaying total (real) labor earnings by calendar year relative to job loss from 3 years before to 3 years after the shock. This figure reveals that the job displacement shock results in an average drop of yearly labor income of roughly 50% for the parent suffering from the shock (dotted line).[27] The drop in income is also persistent with no sign of recovery in following years (although this results in part from the progressive retirement decision of parents). At the same time, there is no sign that this drop in income is compensated by a significant change in spouses' labor supply, which is flat during the entire period considered (gray line). Combined with the fact that the parent suffering from the shock was usually the household's main wage earner before losing his job, this translates into a persistent drop in salaried income of roughly 35% for both parents together.

Panel B of Figure 4 presents further evidence on the total drop in earned and unearned income resulting from the shock. Data in this figure includes not only salaried income but also all major sources of replacement income provided by the Belgian government and self-employment income.[28] This graph shows that total income drops on average by 35% in the first year after the shock for the parent suffering from that shock while the total drop in income for the family is around 20% in the year following the shock. Compared with a total drop in salaried income of 50%, this implies a replacement rate close to 30% on the job-loser's income. After 3 years, total family income is still around 13% below its pre-shock level[29].

Overall, these findings point to a large drop in income for parents who suffer from a job displacement. Therefore, these shocks provide a valuable source of variation to identify the effect of reduced parental support on children's labor market outcomes at the beginning of their career.

However, Figure 3 also points to a potential problem for our identification strategy. While there is a drop in labor supply and income at the time of job loss, there is also a decrease in average hours worked and

[27] Since this graph is based on data available at the calendar year level, information for year 0 is composed of both pre- and both- shock outcomes, depending on the timing of the shock during the calendar year. This explains that the data point in year zero displays a much smaller drop in income.

[28] It includes unemployment insurance payments, financial aid provided by the residual social safety net, disability benefits, pension benefits as well as family benefits.

[29] Given the absence of recovery in labor supply during that period, as well as the decreasing path of unemployment benefits with the duration of unemployment, this partial recovery in income must be the result of households progressively switching to other, more generous, forms of social insurance (mainly, pension benefits) or receiving some form of self-employment income. See Figure A4.

wage starting four quarters before job loss. Given the existence of extended advance-notice requirements for long-tenured white collar workers, the reduction in work hours could be explained by the fact that employees who have received a firing notice either stop being asked to work overtime, make full use of their previously accumulated paid-holidays or claim to be sick and stay home (a practice that has been mentioned by several employers in the discussions surrounding this project). On the other hand, since wages in our data are computed as the ratio of total labor compensation divided by total days worked, the decrease in wages might be explained by the fact that employers stop paying bonuses or performance pay to workers whom they are considering to dismiss.

In any case, this means that parents who are identified as losing their job after their child's entry into the labor force, might have already been aware of the incoming job loss for several quarters. If this is the case, they might already have reduced transfers to their child during this period, which would bias our estimates downward.[30] As we have previously discussed, we will assess the robustness of our results to this problem by restricting the control group to parents whose job loss shock is sufficiently distant from the child's entry into the labor force.

5 Identification Strategy and Theoretical Predictions

Identification strategy. Our goal is to identify the short- and medium-term effects of variations in financial resources available to labor market entrants during their initial job search. The ideal experiment would randomly change entrants' financial resources during that period. For example, we could simply provide a random sample of entrants with an additional monthly transfer as long they have not found a job and compare their labor market outcomes with entrants who do not receive such a transfer. We approximate this ideal setting by taking advantage of the fact that new labor market entrants usually rely on financial help from their parents during both their full-time education and their initial job search at the end of their education. Therefore, one can use variation in parents' ability to support their child as a substitute for the ideal experiment that we have just outlined. To do this, we use large and sudden variations in parental income resulting from the unexpected loss of a stable full-time job around the time of the child's entry.

However, simple comparisons between children whose parents suffer from a shock and those who do not, are likely to result in biased estimates because households who suffer from a job-loss shock are likely to differ in systematic ways from those who do not. Specifically, parents who lose their job are likely to be

[30]It should be noted, however, that the problem is much less significant if parents are hand-to-mouth consumers. In this case, most of the drop in financial transfers would still take place after the actual job loss. Given that many households in Belgium have little liquid assets, this probably concerns a non-negligible part of parents in our sample.

selected from the bottom of the skill distribution. Therefore, given the well-documented intergenerational correlation in ability, their child is also more likely to have lower than average skills and to display worse labor market outcomes. We have seen in section 4 that this prediction holds true in our data. Simple comparisons with children whose parents do not lose a job would therefore confound the effect of reduced parental transfers during the initial job search with the effects of other systematic differences between the two groups. Moreover, it is unlikely that this problem could be solved by explicitly controlling for observable differences between the two groups, since many characteristics that influence labor market outcomes, such as personality traits, are unobservable.

In this paper, we overcome this challenge by studying young job-seekers whose parents suffer from an income shock shortly before their initial job search episode (our *treatment group*) and comparing them with children whose parents experience a shock shortly after (our *control group*). While it seems natural to think that there might be systematic differences between those children whose parents lose a job and those who do not, there is no reason to believe that there would be first-order differences between children whose families both experience similar income shocks in a relatively short time window. In other words, our central identifying assumption, whose plausibility we extensively document in our data, is that parents experiencing a job-loss shock shortly before their child's entry in the labor force do not differ in systematic ways from parents suffering from such a shock shortly after entry. One can therefore identify the causal effect of initial job search behavior on employment outcomes by comparing those children whose parents suffer from an income shock shortly before entry - who have an increased incentive to search for a job - with children whose parents experience the same shock at a later time and whose initial job search behavior is not affected by the loss of financial resources.

Our empirical strategy is illustrated in Figure 5. Consider two children (A and B) entering the labor market at the same age and presenting similar observable characteristics except for the fact that A's father loses his long-term full-time job three months before A's entry into the labor force while B's father, who had a similar job, is laid-off one year later (nine months after A's and B's entry into the labor force). Despite their similar background, A and B face a different financial environments at the time of entry into the labor force. Because of the incomplete income replacement provided by unemployment insurance, A's family has a lower disposable income during A's initial job search and cannot provide him with as much support as B's family can. Moreover, given that job displacements of older workers are associated with meager job finding prospects in Belgium, A's family suffers from a large drop in its discounted future income, an additional factor that might lead to reduced financial support. Our empirical strategy relies on the assumption that differences in early employment outcomes between A and B are solely the result of changes in A's behavior resulting from lower parental support in the first months of his or her initial search for a full-time job. The

main identifying assumption is that the precise timing of parental layoffs is uncorrelated with unobserved heterogeneity in children's labor market prospects. Hilger (2016) independently came up with the same design to study the effect of parental income on college attendance.

Our identifying assumption can be expressed more formally by modeling the relationship between the timing of the job loss shock and individual labor market outcomes :

$$y_{i,t_E,t_S} = \beta B_i + \epsilon_{i,t_E,t_S} \tag{1}$$

where y_{i,t_E,t_S} is the labor market outcome of interest (e.g. income in the first quarter after entry) for child i, entering in quarter t_E and suffering from a shock in quarter t_S (where t_S quarters are indexed relative to the time of entry), ϵ_{i,t_E,t_S} is an individual random effect, B_i is a dummy equal to one if the income shock takes place shortly before the child's entry into the labor force and β represents the (average) treatment effect of reduced parental transfers resulting from a job-loss shock taking place shortly before entry. For children whose parents suffer from a shock shortly before entry ($B_i = 1$), ϵ_{i,t_E,t_S} can therefore be thought of as the labor market outcome that would have been observed for child i if parental income shock were perfectly insured and therefore did not impact his or her job-search behavior. Alternatively, ϵ_{i,t_E,t_S} can be viewed as the outcome that would have been observed if the job loss shock suffered by i's parents had taken place after his initial job search episode.

Our initial identifying assumption can then be expressed as follows :

$$E\left[\epsilon_{i,t_E,t_S}|t_S > 0, t_S < T\right] = E\left[\epsilon_{i,t_E,t_S}|t_S < 0, t_S > -T\right] \quad \forall\, t_E \text{ and } \forall T > 0 \text{ sufficiently small.} \tag{2}$$

In other words, apart from the increased incentives β resulting from the parental shock, there is no (observed or unobserved) heterogeneity between children suffering from a shock before entry ($t_S < 0$) and those suffering from a shock after entry ($t_S > 0$), provided that one focuses on shocks happening in a sufficiently close time window around entry (T small).

In practice, our sample includes parental shocks that occur within three years of entry. This means that, conditional on a year of entry, parents who lost their job before a child's entry also lost their jobs in different years than parents who lost their job after the child's entry. This may lead to an additional source of unobserved heterogeneity if, for example, parents laid off during times of low unemployment have children with worse labor market prospects than parents laid off during times of high unemployment. Indeed, Hilger (2016), shows that, at least in the US, the year of layoff is an important confound when studying the effects of parental layoffs. Therefore, we include fixed effects for the year of parental shock and the child's year of entry

as controls in our preferred specifications (Appendix A.3 discusses the identification in this specification).

Although we cannot directly test our identifying assumption (equation 2), a nice feature of our research design is that we can assess its plausibility with a large array of variables. Indeed, if there were pre-existing differences between our treatment and control groups, one would expect to find significant differences between the two groups among observable characteristics that are plausibly correlated with ϵ_{i,t_E,t_S} while not being directly impacted by the child's job search effort at the beginning of his career. In particular, given the widely documented correlation between a child's labor market outcomes and his family environment, one would expect to find significant differences between parental characteristics in our treatment and control group (apart from the fact that parents in both groups suffer from a job loss shock). In the next section, we take advantage of this prediction to test the plausibility of our assumption using a wide array of parental outcomes variables.

In addition to a simple specification, we also add a series of covariates to our regression. Specifically, we add observable characteristics to equation 1 and consider the following data generating process:[31]

$$y_{i,t_E,t_S} = \beta B_i + \tau_{y_E} + \tau_{y_S} + \gamma_{A,t_E} + \Gamma X_i + \gamma_{P,t_S} + \epsilon_{i,t_E,t_S} \tag{3}$$

where τ_{y_E} is a set of dummies for the year of entry, τ_{y_S} is a set of dummies for the year of parental shock, γ_{A,t_E} is a set of dummies for the child's age at entry, X_i is a vector of demographic variables (child gender, child nationality, household composition, and parental income prior to shock) and γ_{P,t_E} is a set of dummies for the age of the parent at the time of shock. Because our dataset covers children entering the labor market between 2004 and 2008, year-of-entry dummies control for the potential effects of time-varying macro-economic conditions on the job prospects of labor market entrants. We also control for the child's age at entry because children entering at later ages typically have different labor market prospects. For example, later entrants have typically taken at least some college coursework or even graduated from college or university. The addition of demographic covariates is aimed at increasing statistical power by reducing the variance of the error term. It also provides an additional cross-check on the validity of our research design — if our identifying assumption is correct, then the addition of such covariates should have a limited impact on the estimated treatment effects.

The inclusion of parental age at the time of shock as a control variable is important for our identification. It implies that the treatment effect in equation 3 is solely estimated from variation in parental age at birth. Since we control for age-at-entry and parental age-at-shock, our estimate is identified through differences

[31] Much of this notation is borrowed from Hilger (2016).

between the labor market outcomes of children who were born when the parent suffering from the shock was younger ("treatment group") with children who were born when the parent suffering from the shock was older ("control group"). For example, children who enter the labor market at 18 and whose father loses his job at 47 years old, will be part of the treatment group if their father was in his 30^{th} year of life (47-17) when they were born and part of the control group if he was in his 29^{th} year of life at the child's birth.

Alternatively, we can include the parental age at birth in the regression. In this case, our estimate is given by the average difference, all else equal, in labor market outcomes between children entering at the same age and whose parents were of the same age when they were born but who experience the job-loss shock at a slightly different age. We later show that both estimation strategies lead to similar results.[32]

Theoretical predictions. Our research design rests on the double prediction that (i) parental income shocks result in decreased financial support to the child during the initial job search episode, (ii) reduced parental support induces children to increase their labor search effort as well as to accept lower quality jobs. Given the absence of a widely agreed framework for modeling collective family decisions,[33] it should be emphasized that these predictions are quite general and likely to hold in a vast class of models.

First, to the extent that job-loss shocks lead to a sudden reduction in parents' disposable income and discounted future income, a simple balanced-budget argument implies that parents experiencing such shock will reduce either their own consumption or the level of transfers to their children. An optimal choice will usually involve a first-order condition equating the marginal benefit of an additional euro of transfer to the child with the marginal utility of an additional euro of consumption for the parents. As long as both parents' marginal utility of consumption and the marginal benefit of additional transfers are monotonically decreasing (two common assumptions), optimality conditions will require that the burden of the reduction be shared by the two margins.

Second, children typically own few, if any, assets and have limited access to formal forms of insurance at the beginning of their career. As a result, in the absence of any behavioral reaction, decreased parental transfers should result in one-for-one reductions in the child's current consumption. By increasing the marginal value of an additional euro of disposable income, such reductions increase the relative value of having a job. Simple job search models (such as Mortensen, 1977) imply that this should result in children searching harder for a job and lowering their reservation wage. These responses should result in a higher initial job finding rate and a decrease in the average quality of jobs accepted by new labor market entrants.

[32] From the above discussion, it should be clear that it is not possible to simultaneously control for the parent's age-at-birth, parental age-at-shock and child's age at entry. Together, these three variables exactly determine the timing of shocks relative to entry; therefore, there is no remaining sources of variation to identify the parameter of interest.

[33] There is, for example, a divide between models of the family in which each family member continues to maximize individually his own utility and models of the family in which decisions are taken jointly to maximize a common welfare function.

To the extent that parental transfers do not stop completely when the child finds a job, one would also expect an increase in labor supply at the intensive margin. If the labor supply elasticity at the intensive margin is positive, reduced transfers from the family should increase the marginal value of additional labor income and therefore be associated with an increase in hours worked while employed. The same predictions hold true if children also provide financial assistance to their parents : in this case, higher transfers to parents as a result of the job-loss shock should also lead to increased labor supply at the extensive as well as intensive margin. The rest of this paper is dedicated to testing those predictions in the data.

6 Assessing the Validity of the Research Design

In this section, we present several tests assessing the validity of our identifying assumption.

A first concern is that parents might voluntarily select into the control or treatment groups. For example, as discussed in section 3, one might expect secondary earners (most of them females) to leave the labor force voluntarily when their children do not need their financial support anymore. In the spirit of the graphical density tests that are common in the regression discontinuity design literature (Imbens and Lemieux, 2008), Figure 6 displays the distribution of the number of parental shocks by quarter in our sample relative to the time of entry. There is no evidence for the bunching of shocks either before or after entry into the labor market. Moreover, apart from the quarter-to-quarter variation that is to be expected given our limited sample size, a simple linear regression provides an excellent fit for the evolution of the number of shocks over time. This graph therefore provides evidence against the possibility that voluntary selection across treatment and control groups might be a significant problem in our set-up.

The fitted line in Figure 6 also reveals a slight negative linear trend in the number of observations per quarter relative to entry for both female and male parents. This trend is explained by the fact that the probability of job-loss shocks decreases with age (at least around parents' age at the time of their child's entry into the labor force). We show that this trend is not driven by systematic differences between parents who get a shock before and after entry by testing for such differences in parental characteristics. Furthermore, as a robustness check, we will estimate our benchmark results on the restricted sample of children whose parents get a shock in the year around the child's entry into the labor force. Given that the time trend in the number of shocks is mild, this will further reduce the risk of first-order systematic differences between the treatment (before) and control (after) group.

An additional concern is that parental job loss shocks might lead certain children to enter the labor force rather than continue their education. If this is the case, our estimates would reflect the joint effects of increased job search and lower levels of human capital. Figure 7 directly addresses this concern by plotting

the average child's age-at-entry as a function of the timing of the parental shock, for each quarter relative to entry in a 3-year time window. Contrary to what would be expected if parental shocks lead to early entry, age-at-entry is not systematically related to the timing of parental shock. There is no visual or statistical difference in the average age at entry between children suffering from a parental income shock in the 12 quarters prior to entry and children who suffer from a similar shock in the 12 quarters following entry (the difference between the two groups is lower than 0.01 year and insignificant).

Finally, Table 2 further confirms the plausibility of our identifying assumption by testing for significant differences between our treatment and control group for a large array of demographic characteristics and labor market outcomes of the parents. Each row of column I (II) displays the sample average of a variable of interest in the treatment (control) group. Column III displays the simple difference between the treatment and control groups while column IV adds year-of-shock and parental age-at-shock controls. These controls are important, due to the unobserved heterogeneity caused by differing year of layoff between the treatment and control groups. In all but two cases, there is no significant difference between the treatment and control group.

The only significant differences concern job tenure at the time of job loss and gender. However, given that we are testing so many (12) parameters, we are likely to witness at least one statistically different outcome. Moreover, there is no significant difference in the number of worked or unemployed days as well as in wage or total compensation in the 10 years prior to the shock. These findings are further strengthened by Figure 8 which displays the sample average of the same variables as a function of the timing of the shock relative to entry in a 3-years time window around entry. For all four variables, as was the case for the child age-at-entry, there is little difference in parental labor market history across shocks that occurred 3 years before or after the child's entry.

Table 3 conducts a balance test for the child-related demographics. We see little systematic differences between the treatment and control groups based on age or gender, even without adding additional controls. Overall, these findings provide strong support for our identifying assumption that there are no unobserved differences between children in the treatment and control groups that affect the validity of our results once appropriate controls are added.

7 The Effects of Parental Job-loss Shocks on Child Outcomes

7.1 Main Results

Having established the plausibility of our research design, we now turn to our central results. Table 4 displays the estimated treatment effects for four employment outcomes in the 12 quarters following entry.[34] The first two rows of the table display results for the total number of days worked and the total number of quarters worked. The last two rows display results for total labor compensation and (median) wage percentile. Column I contains our results without any controls. Each subsequent column adds additional controls building up to the more complex specifications. The final column displays the average of the outcome variable in the control group for comparison purposes.

We find statistically and quantitatively significant evidence that child labor supply increases as a result of parental income shocks that precede the initial job search period. Across all specifications, we find an average increase ranging between 23 and 39 (full-time equivalent) days worked in the 12 quarters following entry. This represents an increase of 5.3% - 9% in total labor supply given a baseline of 430 days worked in the same period for the control group. Adding more controls increases our estimates, with the largest estimates occurring in columns (VI) and (VII), where we control for the unobserved heterogeneity due to the parental year of shock. Since there are good a priori reasons to think that unobserved heterogeneity due to parental year of shock is important, we prefer these larger estimates. In addition, Table A2 shows that these results are robust to using an identification strategy relying on parental age at birth rather than parental age at shock.

The second row provides further confirmation of the previous results and shows that the increase in labor supply is, at least partially, the product of a response along the extensive margin. On average, for our preferred specification (VII), children in the treatment group have a least one job in .44 of a quarter in the 3 years following entry. This corresponds approximately to a 4.8% increase compared to the baseline for the control group (9 out of 12 quarters). In line with those results, the third row shows that the treatment group displays an average increase in total labor compensation of the same magnitude: 4.3% to 7.5% (€1,663 to €2,568) compared to the baseline for the control group (€34,186). By contrast, the fourth row indicates no statistically significant change in the median wage received by workers during the first 12 months of their career. The point estimate varies in sign and never exceeds a magnitude of .34 (relative to the baseline rate of 27.12). On a daily basis, this estimate represents approximately €.75 per full day of work compared

[34]The choice of a 12 quarters window of observation is motivated by the practical reason that this is the longest period for which we observe the full post-entry employment history for all children in our sample.

to a baseline of around €89[35] or less than 1%. We can exclude effects larger than -1.6 percentile points, equivalent to €1.5, with a 95% probability. Therefore, we can reject that there is an economically significant decrease in job quality, as measured by the median wage during the first 12 quarters of a worker's career.

7.2 The Effect of Parental Shocks on the Dynamics of Labor Supply

The previous section looked at aggregate employment outcomes over the 12 quarters following entry. However, our set-up also allows us to identify the full dynamics of the effects of parental shock by simply using the same estimator on quarter-by-quarter outcomes. Let $k = t_O - t_E$ be the number of quarters since entry. For each k, we estimate the effect of experiencing a parental shock before entry using the following equation:

$$y_{i,t_k,t_E,t_S,b} = \beta_{k,b} + \tau_{YE,k} + \tau_{YS,k} + \gamma_{Age,t_E,k} + \Gamma X_i + \gamma_{P,t_s} + Q_{q,k} + \epsilon_{i,t_k,t_E,t_S,b} \tag{4}$$

In the above equation, t_k is the quarter relative to entry at which the outcome is measured, $Q_{q,k}$ is a dummy for each quarter of the calendar year and $\beta_{k,b}$ is the time-specific effect of having a shock before entry. Other variables are defined as in equation 3. The addition of a control for the quarter of the year ($Q_{q,k}$) is meant to capture seasonal patterns in labor market outcomes. We are interested in how $\beta_{k,b}$ varies with k. Importantly, we allow the coefficient on all covariates to differ for each quarter k. As we have seen in section 2.1, average labor market outcomes display changes in the first quarters after entry and there is no reason to expect covariates (e.g. age-at-entry) to have the same effect on total days worked in, for example, the 1st quarter after entry as in the 12th. Therefore, while we estimate equation 4 by pooling together observations for all k ($k = 1, 2, ..., 12$), we interact all covariates with a dummy for each quarter relative to entry k. We also cluster standard errors at the individual level to take into account the correlation between quarterly outcomes for each individual. This procedure produces point estimates that are similar to separate quarter-by-quarter regressions but slightly improves statistical power by taking advantage of the imperfect correlation between quarterly outcomes within an individual.

Focusing on individuals whose parents suffer from a shock in a three-year window around entry, Figure 9 plots the time-varying treatment effects ($\beta_{k,b}$'s) for days worked (Panel A) and labor compensation received (Panel B) by quarter. The dynamic pattern of the labor supply response is largely consistent with the idea that we are correctly identifying the effects of an increased labor search effort at the beginning of one's career, rather than pre-existing differences between the treatment and control group. Indeed, Panel A makes clear that average labor supply is identical for the treatment and control group in the two years preceding entry,

[35] Estimated in 2011 euros.

a moment at which a significant number of individuals already held part-time student jobs. By contrast, the treatment group starts working more around the time of entry with a maximum treatment effect of nearly 4 additional days of work in the second quarter following entry. Although there is a significant amount of quarterly variation, this effect decreases after the initial peak difference in the first two years after entry.[36] Panel B further confirms this result when looking at labor compensation, indicating an increase in total salary in the first quarters after entry.

Together with the previous results, Figure 9 paints a picture that is consistent with a simple job-search model. Children whose parents are subject to an income shock shortly before entry look harder for a job at the end of their full-time education. As a result, they find a job a little earlier and work a bit more than children who are not under the same financial pressure. As time passes, this effect diminishes as members of the control group progressively find a job.[37]

7.3 The Effect of Parental Shocks on Additional Child Outcomes

The absence of a significant response on the job quality margin, and the vindication of a simple job-search effort model, is supported by the results for additional labor market outcomes in Table 5. In both panels, Column I displays the simple average difference between the treatment and control group (equivalent to Column I of Table 4). Column II presents results for equation 3 adding a full set of controls (equivalent to Column VII of Table 4) while Column III displays the sample average for the control group.

The first 5 rows of Panel A provide a more detailed picture of the margins of adjustment resulting in the increase in total labor supply during the first 12 quarters after entry. First, there is no evidence of a response along the intensive margin: the treatment effect for the number of days worked in a given quarter at continuing jobs is close to zero and even slightly, as well as insignificantly, negative (first row). Children in the treatment group do not chose to work longer hours once they have found a job. Overall, there is also no increase in the total number of employers conditional on having at least one employer during the first two, four, and twelve quarters after entry. Therefore, the effect that we find is mostly explained by the fact that treated entrants are more likely to have a job. This is confirmed in the fifth row, which shows that there is an increase in the average tenure observed over the 12 quarter period (fifth row). In other words, the increase in the labor supply appears solely as a result of more quarters worked for the same employer. We interpret this effect as an increase in the labor force attachment of treated children.

The rest of Table 5 provides additional confirmation that the increase in labor supply is not the result of

[36]The increase in standard errors over time, resulting in a wider confidence set, is explained by the reduced number of children for which we observe the full employment history in the 24 quarters following entry

[37]See Figure A5 for the time-varying effect on wages.

a decrease in job-seekers standards of job quality. First, the sixth and seventh row of Panel A confirm that the lack of changes in observed wages does not depend on the exact method of measurement: while Table 4 focused on the median wage observed in the 12 quarters following entry, we also do not find a significant effect when looking at the first and last wages observed during this period. Panel B investigates whether the increase in labor supply might be the result of a decrease in job quality on other, more subtle, margins. Again, we do not find any significant effect although some point estimates are more imprecise and should therefore be interpreted with additional caution.

First, children whose parents suffer from an income shock before entry might be ready to trade-off the possibility of finding a job faster against the long-term prospect of higher income growth. This does not seem to be the case: the first row of panel B shows that children in the treated group do not display significantly lower wage-growth during the first three years of their career. Second, blue-collar jobs are a strong marker of low quality employment in Belgium: not only do blue-collar workers earn less and have lower education on average but, as we have previously discussed, they benefit from lower employment protection. However, parental shocks before entry do not seem to increase the share of blue-collar jobs in the first 12 quarters after entry. On the contrary, it seems to slightly decrease compared to white-collar jobs (row 2 and 3 respectively). Third, firm size has been widely documented to be correlated with higher quality, more productive jobs (Oi and Idson, 1999). Again, we fail to find evidence of a significant decrease in firm size as a result of parental shocks before entry (row 4). Fourth, young workers also do not seem to react to family shocks by self-selecting toward lower paying industries or industries with low income-growth prospects (row 5 and 6).

Overall, there is no evidence that earlier job finding causes worse match quality. These results also provide an additional validation of our research design. If higher labor supply in our treatment group simply reflected pre-existing differences with the control group, we would also expect these differences to manifest themselves in measures of job quality. The absence of such effect lends further support to the hypothesis that we are indeed correctly identifying the causal effect of increased job search effort.

Finally, Kaplan (2012) has documented that family insurance often takes the form of children moving back in the parental home during periods of unemployment in the first years of their career. Given that we observe a decrease in unemployment among the treated group, one might expect them to have a higher propensity to leave the parental household. Alternatively, the shock to family income may induce children to stay in their parents' place of residence rather than move out. Row 7 shows that pre-entry parental shocks are not associated with any decrease in the probability of living in the parental home two years after entry. However, our estimate is too imprecise to allow us to reach a definitive conclusion on the direction of this effect.

7.4 Robustness

In this section we show that our results are robust to alternative specifications of our main estimating equation including varying the job-loss sample, adding a 'super-control' group of entrants without a job-loss, and changing the definition of a job loss.

We first discuss robustness with respect to the sample window. One potential concern regarding our specification is that, even with controls, individuals whose parents lose their jobs 3 years before entry are different than those whose parents lose their job 3 years after entry. To alleviate this concern, we reduce the selection window for entrants to one year and rerun our main specifications. Table 6 displays the results of a set of specifications where we use a one year window around entry. Columns (1) - (5) progressively add additional controls to the specification, mirroring our baseline setup. The results from these regressions are both qualitatively and quantitatively similar. Parental job loss affects the labor supply but not the job quality. The only major differences between these results and our baseline results are easily explained. First, due to the lower sample size, the effect on total salary is not statistically significant in these specifications. Second, the effects on labor supply are smaller than in the baseline specifications where we include the year of parental shock. However, the treatment effect is not identified with just a one year window if we include a parental year of shock fixed effect, so we cannot replicate those specifications.

Another potential concern with our specification is that there could be anticipation effects on behalf of labor market-entrants. To alleviate these, we exclude from our specification sample those individuals who received a shock in the year before and after entry. Table 7 displays the results for this specifications discussed above. The estimated effects in these specifications are qualitatively similar but larger in magnitude and less precise. This could be due to several factors. First, there could have been anticipation effects. Second, the effect of a job-loss two years before entry could be larger than the effect of a more immediate job-loss due to the accumulated financial burden. Third, the job-loss of a parent in the year after entry could induce entrants to work more, even after entry, reducing our estimates.

A related concern is that the timing of entry could be endogenous if children drop out of college after a parental job-loss. We test whether this could be driving our results by focusing solely on the sample that enters at age 18, before college. Table A3 displays the results from this specification and demonstrates that, in our preferred specification (VI), the results are qualitatively similar. Furthermore, this subsample experiences a larger effect on days worked than the overall sample. We explore the potential causes for this heterogeneity in the next section.

Next, recall that our dataset also includes a 6.5% random sample of entrants whose parents did not experience a job-loss shock. In Table 8 we include these observations in our estimation. The addition of

these observations help us to identify the coefficients on controls unrelated to the job-loss, such as the age at entry. Furthermore, these observations increase the statical procedure of our estimates. The results from these specifications are not quantitatively or qualitatively different form the baseline specifications.

Lastly, table 9 tests the robustness of our baseline results using more narrow definitions of involuntary job-loss. As discussed in section 3, our definition of the job-loss shock includes parents who lose a stable full-time job and transition either to unemployment insurance or to a state of "unregistered activity". This second category raises the concern that our sample might contain a significant portion of voluntary job quitters. Consistent with this hypothesis, the first and second row of Table 9 indeed show that most of the effect that we identify is concentrated on individuals who transition to unemployment insurance. Moreover, the last two rows further demonstrate that the increase in labor supply is concentrated on parents who do not receive any severance pay and who, as a consequence, receive a more direct reduction in disposable income. Therefore, our benchmark estimates are probably a lower bound estimate of the real treatment effect of involuntary job-loss shocks.

7.5 Heterogeneous Effects

This section investigates how the average treatment effect that we have identified varies as a function of entrants' characteristics. Table 10 shows results for our main variables of interest, broken down along age, parental income, and gender. Each column represents a regression outcome and each row represents a coefficient type, with the coefficients on 'Before *' representing treatment effect heterogeneity. The regression specification used is analogous to the regression in column (VII) of table 4.

We first discuss the general variation in the data. Males tend to work more than females in the sample while younger entrants and those with poorer parents work less. Next, we turn to the treatment effects. The baseline treatment effects for the excluded category (female, over 19 years at entry with parents in the upper tercile of income) are not statistically significant and close to 0 in magnitude. Entrants whose parents have wages in the lowest tercile of income before job-loss experience the largest effect of parental job-loss of 35 days. Entrants with middle-wage parents also experience an economically large effect of parental job-loss but this effect is not statically significant. Another dimension of heterogeneity is also statistically significant, the age at entry. Young adults who enter at 18 or 19 years old work 33 days more as a result of a parental job-loss shock.

We interpret these results in the following manner. The families of entrants with lower parental income have fewer assets and can provide less support to their kids when they lose their job. In contrast, high income families can provide support for children even when they lose a job. Furthermore, younger entrants

are more responsive to parental shocks. This is consistent with a lower human-capital accumulation motive for these entrants. If entrants gained human capital on the job, then they would want to find a job quicker regardless of parental support, so that they could start accumulating experience faster. To the extent, that there is less human capital accumulation for lower education jobs, then the human capital accumulation motive should be lower for this subpopulation.

8 Interpretation and Conclusion

We have shown that children whose parents lose a job prior to their child's labor market entry are, on average, induced to work 9% more in the 3 years following labor market entry. We find no evidence that parental support affects the quality of the initial job that entrants find. Overall, our results are best explained by a simple model in which financial incentives to look for and accept a job impact labor market outcomes mainly through a decrease in job search effort. We find no support for theories in which employment outcomes are partially the result of changes in the characteristics of the jobs that the unemployed seek or accept.

The effect that we document is therefore best compared to changes in unemployment duration resulting from variations in the generosity of unemployment insurance benefits. That is, there is moral hazard on the part of children with regards to their parents' financial support. However, comparing our results to previous estimates of the elasticity of unemployment duration with respect to U.I. benefits is difficult for at least two reasons. First, while we do observe the magnitude of parental income shocks, we do not directly observe the level of parental transfers to the child. For example, if parents over-insure their child's consumption compared to their own (i.e. they reduce their level of support less than proportionally with the drop in income), this would imply a much larger elasticity of unemployment duration for a given reduction in parental income. Second, we do not measure the level of self-insurance by parents: higher levels of self-insurance will result in a less-than-proportional drop in parental support and would therefore imply a higher elasticity of unemployment duration for a given reduction in parental income.

Nonetheless, we can make some simplifying assumptions to compute a back-of-the-envelope comparison of our results with the rest of the literature. We make five assumptions. First, consistent with evidence shown in section 4.2, we assume that a parental shock results, on average, in a one-time reduction in parental labor income of 50%. Second, we assume that this drop is insured at 60% on the long-term.[38] Third, we consider the case of a single-earner family. The three previous assumptions imply a drop in available income of $(1 - .6) * 50\% = 20\%$. Fourth, we assume that the drop in parental income results in a proportional drop

[38]This is close to the 57% long-term net replacement rate provided by the OECD for single-earner couples at the average wage, with 2 children.

in parental transfers to the child. Finally, we need to make an assumption about the benchmark number of unemployed days for children. We do so by using the maximum total number of days worked by a full-time worker during the first twelve quarters after entry. Since a full-time quarter of work is equivalent (on average) to 66 days of work in our data, maximum labor supply over 12 quarters is 792 days. This compares to an average of 429.7 days effectively worked by the control group in our data over the same period. Therefore, 39 additional days of work for the treatment group (benchmark result, Table 4) correspond to a $\frac{39}{792-429.7} = 11\%$ decrease in unemployed days. This implies an elasticity of unemployment duration with respect to parental transfers of $11/20 = .55$, close to the benchmark estimate of .5 reported by Krueger and Meyer (2002).

However, another reading of our results can produce a much higher estimate given the much larger labor supply effect found for children of low-wage parents and children entering the labor market at the age of 18 or 19 (section 7.5). Using our highest estimated treatment effects (up to 70 additional days of work in the case of low-wage parents with entrants younger than 21), this translates into an elasticity of .95.[39] Therefore, while our results are not inconsistent with previous estimates of unemployment duration elasticity with respect to the generosity of U.I. benefits, we cannot exclude the possibility that certain categories of new workers reacting more strongly to financial job-search incentives.

Lastly, the increase in labor supply that we find is concentrated on children entering the labor market between before the age of 20 and who come from relatively poor families. Therefore, our results indicating that a decrease in unemployed days does not result in worse job quality may not be broadly applicable to other categories of new workers. In particular, the job search process of college graduates might be less sensitive to financial support from parents precisely because there are greater returns to looking longer. We leave further investigation of this heterogeneity for future work.

[39] 70 additional days of work correspond to a $\frac{70}{792-429.7} = 19\%$ decrease in unemployed days which, for a decline in transfers of 20%, implies an elasticity of $19/20 = .95$.

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Tables

Table 1: Summary Statistics

Panel A. Child characteristics and outcomes	All entrants	Job Loss Group		Panel B. Household and parental characteristics	All entrants	Job Loss Group	
		3 Years Window	1 Year Window			3 Years Window	1 Year Window
	(I)	(II)	(III)		(I)	(II)	(III)
# of entrants	56,913	5,605	2,185	*# of parents*	99,251	9,920	3,998
Males	50.2%	50.5%	49.4%	*# of households*	43,794	4,700	1,893
Belgian citizen	97.2%	95.9%	95.5%	*Two parents family*	78.8%	89.2%	89.0%
Age at Entry				*Single parent family*	21.2%	10.8%	11.0%
18-19	26.3%	33.1%	32.4%	*Single Father*	18.5%	36.1%	36.1%
20-21	28.1%	29.3%	29.5%	*Single Mother*	81.5%	63.9%	63.9%
22-23	29.0%	24.8%	25.5%	*Father age at birth*	29.36	28.18	28.16
24-25	16.5%	12.9%	12.6%		(5.62)	(5.03)	(4.96)
Labour market outcomes in 12 quarters after entry				*Mother age at birth*	26.89	25.91	26.00
Days worked	460.1	441.4	440.0		(4.65)	(4.46)	(4.45)
	(252.1)	(245.2)	(250.7)	*Number of children in parental household*			
Quarters with a job	8.70	8.64	8.61	*1*	20.1%	22.5%	20.9%
	(3.51)	(3.44)	(3.49)	*2*	41.9%	40.2%	40.3%
Total salary (in euros)	37,868	35,020	34,719	*3 or more*	38.1%	37.3%	38.9%
	(24,027)	(22,689)	(22,953)	*Working 16 Q. before entry*			
Wage Percentile	29.79	27.22	27.20	*Father*	84.8%	94.9%	95.6%
	(18.08)	(17.07)	(17.00)	*Mother*	57.8%	80.3%	83.7%

Notes: This table displays summary statistics for the samples used in the paper. All percentages represent the share of individuals in the sample that present the considered characteristic. Other statistics are averages taken over all individuals in the sample (standard deviation are in parenthesis when relevant). Column I displays statistics for a representative sample of entrants in the Belgian population between 2004 and 2008. Columns II (III) displays statistics for the sample of children whose parents experience a job-loss shock in a three (one) year(s) window around the child's entry into the labor force. Labor market outcomes for the child are measured from the quarter of entry Q to $Q+11$. *Days worked* is the sum of all full-time equivalent days worked as a salaried employee in the 12 quarters after entry (i.e. 4 hours of work on a given day are counted as half a day if the normal day of work for a full-time worker at the firm is 8 hours). *Quarters with a job* is the number of quarters during which the individual had at least one job. *Total salary* is equal to the sum of all pre-tax compensation paid, net of all (employer and employee) social security contributions. *Wage percentile* is measured in three steps. First, the wage for each job is measured each quarter by dividing total compensation by the number of days worked at that job during the quarter. Second, the wage percentile for each job and quarter is computed based on the wage distribution for all individuals in the Social Security Employment Registry for the same quarter. Third, a single wage percentile for each individual is computed as the median wage percentile for the individual for all jobs held in the 12 quarters after entry. All variables relating to family composition are measured 16 quarters before the end of family allowances payments. Father's and mother's age-at-birth refer to the age of the parent at the time of birth of the child. *Working 16 Q. before entry* is a dummy equal to one if the parent had at least one job on the 16^{th} quarter before the child's entry into the labor force.

Table 2: Balance Tests - Parental Demographics and Employment History

	Average by shock time		Before/After Difference	
	Treatment group	Control group	Simple difference	Year-of-shock and Age-at-shock controls
	(I)	(II)	(III)	(IV)
Age at shock	44.686	44.156	0.521***	
	(0.107)	(0.103)	(0.142)	
Single parent	0.100	0.097	0.002	0.000
	(0.006)	(0.006)	(0.008)	(0.011)
Head of household	0.754	0.763	-0.011	-0.022
	(0.008)	(0.008)	(0.011)	(0.014)
Number of kids	2.384	2.401	-0.021	0.022
	(0.024)	(0.026)	(0.034)	(0.044)
Female (%)	0.291	0.267	0.020*	0.028*
	(0.009)	(0.009)	(0.012)	(0.016)
Median wage in last 3 years	9.834	9.999	-0.121	-0.209
	(0.096)	(0.096)	(0.130)	(0.162)
Employer size	4.805	4.950	-0.130*	0.042
	(0.051)	(0.051)	(0.068)	(0.090)
Blue Collar job	0.665	0.658	0.004	-0.016
	(0.009)	(0.009)	(0.013)	(0.016)
Tenure (in years)	7.644	8.203	-0.557***	-0.375***
	(0.054)	(0.051)	(0.071)	(0.094)
Days worked in last 10 years (percentile)	73.869	74.178	-0.271	-0.577
	(0.267)	(0.267)	(0.360)	(0.453)
Days receiving unemployment benefits in last 10 years (percentile)	78.284	78.995	-0.699***	-0.268
	(0.154)	(0.145)	(0.202)	(0.259)
Median wage in last 10 year (percentile)	52.434	54.339	-1.716***	-0.777
	(0.468)	(0.451)	(0.624)	(0.767)
Total compensation in last 10 year (percentile)	75.369	75.974	-0.559	-0.362
	(0.258)	(0.256)	(0.347)	(0.432)

Notes: This table tests for differences in average demographic characteristics and employment history between parents in the treatment and control groups. The treatment group includes parents experiencing the loss of a stable full-time job in the 12 quarters prior to their child's entry into the labor force. The control group includes parents experiencing a similar shock in the 12 quarters after entry. Each row of Column I (II) displays the sample average of the variable of interest in the treatment (control) group. Column III displays the simple difference between the treatment and control groups while column IV presents the coefficients on a dummy equal to one for the treatment group from a regression of the variable of interest that controls for a full set of year-of-shock and age-at-shock dummies. Demographic characteristics are measured 16 quarters before the child's entry into the labor force. *Number of kids* refers to the number of children living in the parental household (including the child identified as entering the labor force). *Employer size* and *Blue Collar jobs* are measured two quarters before job loss. All other employment variables are measured in the 10 calendar years prior to the job-loss shock and, except for tenure, are expressed in percentiles of the distribution of the relevant variable in the same year for parents in our representative sample of all entrants (see Table 1, Column I). *Tenure* is measured using the unique employer identifier provided by the pension registry and is censored at a maximum of 10 years. *Median wage in last 10 years* is obtained by dividing total compensation by total days worked for each year and taking the median over the last 10 years for each parent. Standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 3: Balance Tests - Child Demographics

	Average by shock time		Before/After Difference
	Treatment group	**Control group**	**Simple difference**
	(I)	**(II)**	**(III)**
Males	0.516	0.494	0.022*
	(0.010)	(0.009)	(0.013)
Belgian citizen	0.956	0.961	-0.005
	(0.004)	(0.004)	(0.005)
18-19	0.330	0.331	-0.001
	(0.009)	(0.009)	(0.013)
20-21	0.293	0.293	0.000
	(0.009)	(0.008)	(0.012)
22-23	0.247	0.249	-0.002
	(0.008)	(0.008)	(0.012)
24-25	0.130	0.128	0.002
	(0.006)	(0.006)	(0.009)
Age at Entry	20.837	20.816	0.021
	(0.040)	(0.039)	(0.056)

Notes: This table tests for differences in average demographic characteristics young adults in the treatment and control groups. The treatment group includes children with a parent experiencing the loss of a stable full-time job in the 12 quarters prior to their child's entry into the labor force. The control group includes children with a parent experiencing a similar shock in the 12 quarters after entry. Each row of Column I (II) displays the sample average of the variable of interest in the treatment (control) group. Column III displays the simple difference between the treatment and control groups. *p<0.1; **p<0.05; ***p<0.01

Table 4: Main Results

	Estimated treatment effect for main outcome variables							Baseline Average
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	
Dependent variable								
Total Days Worked	23.422 ***	22.746 ***	28.023 ***	30.576 ***	27.861 ***	38.631 ***	38.763 ***	429.702
	(6.542)	(6.370)	(6.572)	(6.554)	(6.571)	(12.750)	(12.671)	(4.617)
Quarters with a job	0.245 **	0.242 **	0.316 ***	0.349 ***	0.309 ***	0.434 **	0.439 **	9.068
	(0.098)	(0.097)	(0.100)	(0.100)	(0.101)	(0.195)	(0.194)	(0.069)
Total Salary	1,663 ***	1,469 ***	2,023 ***	2,077 ***	1,810 ***	2,555 **	2,568 **	34,186
	(605)	(565)	(583)	(583)	(581)	(1,135)	(1,121)	(427)
Wage percentile	0.193	-0.116	0.242	0.314	0.285	-0.339	-0.236	27.120
	(0.469)	(0.433)	(0.448)	(0.448)	(0.444)	(0.873)	(0.857)	(0.331)
# of observations	5,605	5,605	5,605	5,605	5,532	5,605	5,532	
Controls								
Age at entry		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	
Parental age at shock			*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	
Year of entry				*Yes*	*Yes*	*Yes*	*Yes*	
Year of parental shock						*Yes*	*Yes*	
Family and Demographic controls					*Yes*		*Yes*	

Notes: The table displays our benchmark estimates for the treatment effect of a parental job-loss shock before entry on the employment outcomes of children in the 12 quarters following entry. Each entry in Columns (1) - (7) displays the coefficient on an indicator variable equal to one for entrants experiencing a parental job-loss shock in the 12 quarters before their entry into the labor force. The last Column (*Baseline Average*) displays the average of the variable for the sample of children whose parents suffer from the shock in the 12 quarters following entry. All regressions are estimated on the sample of labor market entrants whose parents experience the loss of a stable full-time job in the 12 quarters before or after entry. We exclude those children whose parents lose their job during the quarter of entry. We also exclude the (few) cases where parents get more than one shock in the three-year window around entry. *Days worked* is the sum of all full-time equivalent days worked as a salaried employee in the 12 quarters after entry. *Quarters with a job* is the number of quarters during which the individual had at least one job. *Total salary* is equal to the sum of all pre-tax compensation paid during the quarter, net of all (employer and employee) social security contributions paid during the same quarter. *Wage percentile* is measured in three steps. First, the wage for each job is measured each quarter by dividing total compensation by the number of days worked at that job during the quarter. Second, the wage percentile for each job and quarter is computed based on the wage distribution for all individuals in the Social Security Employment Registry for the current quarter. Third, a single wage percentile for each child is computed as the median wage percentile for the individual for all jobs held in the 12 quarters after entry. Note, in some cases, individuals never earn a daily wage and are excluded from row 4. All controls include a full set of dummy variables for each value of the covariate. Family and demographic controls are gender, nationality (Belgium, E.U. 15, Other U.E., Other countries, unknown), family type (married two-parents, non-married two-parents, single parent), and tercile of parental income before the job-loss. Family type is measured 16 quarters before the loss of family benefits. Standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 5: Effect on Additional Labor Market Outcomes

Panel A	**No covariates**	**With covariates**	**Baseline average**	***Panel B***	**No covariates**	**With covariates**	**Baseline average**
	(I)	(II)	(III)		(I)	(II)	(III)
Quarterly days worked at employer	*0.734*	*0.817*	*50.726*	*Quaterly wage growth (in perc.)*	*-0.110* *	*-0.027*	*1.211*
	(0.452)	*(0.895)*	*(0.321)*		*(0.054)*	*(0.107)*	*(0.039)*
# of employers	*-0.014*	*-0.082*	*2.882*	*Quarters with white collar job (%)*	*0.028* *	*0.036* *	*0.558*
	(0.053)	*(0.106)*	*(0.038)*		*(0.013)*	*(0.020)*	*(0.009)*
# of employers in first 2 Q.	*0.018*	*0.045*	*1.407*	*Quarters with blue collar job (%)*	*-0.024* *	*-0.038* *	*0.456*
	(0.022)	*(0.044)*	*(0.015)*		*(0.013)*	*(0.020)*	*(0.009)*
# of employers in first 4 Q.	*0.002*	*-0.037*	*1.809*	*First employer size*	*-0.832*	*-55.654*	*893.020*
	(0.030)	*(0.061)*	*(0.022)*		*(25.561)*	*(51.401)*	*(18.026)*
Maximum tenure	*0.328* **	*0.573* **	*7.316*	*Average industry wage (perc.)*	*0.003*	*0.163*	*28.929*
	(0.091)	*(0.179)*	*(0.064)*		*(0.238)*	*(0.468)*	*(0.168)*
First wage (percentile)	*0.345*	*1.216*	*23.518*	*Mean industry wage growth*	*0.000*	*-0.008*	*0.857*
	(0.475)	*(0.907)*	*(0.335)*		*(0.007)*	*(0.013)*	*(0.005)*
Last wage (percentile)	*0.231*	*-0.028*	*31.508*	*Live with parents 8 Q after entry*	*-0.009*	*-0.000*	*0.488*
	(0.552)	*(1.013)*	*(0.389)*		*(0.013)*	*(0.026)*	*(0.009)*

Notes: The table displays our estimates of the treatment effect of a parental job-loss shock before entry for a larger set of child outcomes after entry. All regressions are estimated on the sample of labor market entrants whose parents experience the loss of a stable full-time job in the 12 quarters before or after entry. We exclude those children whose parents lose their job on the quarter of entry or get more than one job-loss shock in the 12 quarters around entry. Columns I and II display the coefficient on an indicator variable equal to one for entrants experiencing a parental job-loss shock in the 12 quarters before entry into the labor force. Column I does not include any controls (i.e. the coefficient is equal to the difference in sample average between the treatment and control group). Column II adds controls for age-at-entry, parental age-at-shock, year of parental shock, year-of-entry and demographic controls. Column III displays the baseline average of the variable for the sample of children whose parents suffer from the shock in the 12 quarters following entry. Except when otherwise mentioned, all dependent variables are computed using data on the first 12 quarters after the child's entry in the labor force. *Quarterly days worked at employer* measures the number of days worked by quarter at continuing jobs. It is computed by (i) looking only at continuing employer-employee relationships (i.e. jobs that exist both in the previous and the next quarter) and (ii) taking the individual average over all job-quarter observations if the individual has more than one such job. *# of employers* is the total number of different employers for which an individual has worked, even for as little as an hour (variable is set to missing for individuals that have zero employers). *Maximum tenure* is the maximum number of quarters for which the individual has worked for a given employer during the 12 quarters after entry (variable is set to missing for individuals that have zero employers).*First (Last) wage* is the first (last) wage observed in the 12 quarters following entry. *Wage growth* is the average difference in wage between the current and the previous quarter in the 12 quarters following entry. Wages are expressed in percentiles and computed as explained in the notes to Table 4. *Quarters with a white (blue) collar job* is the number of quarters during which the individual had a least one white (blue) collar job. *Employer size* is the number of different workers who worked for the employer in the current quarter, as indicated by the full set of records in the social security employment registry. Industry level variables are computed by taking the sample average of each variable at the NACE 3-digit level on the representative sample of all labor market entrants between 2004 and 2008 described in column I of Table 1. *Live with parents 8 Q after entry* is an indicator variable equal to one if the child is still registered as living with his first parent in Belgium's national registry 8 quarters after entry. The first parent is the parent that was registered as head of the family 16 quarters before the child's entry into the labor force. Standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 6: Robustness: Sample Window - Only 1 Year Around Entry

	Estimated treatment effect for main outcome variables					Baseline Average
	(I)	(II)	(III)	(IV)	(V)	
Dependent variable						
Total Days Worked	24.476 **	25.358 **	26.299 **	25.562 **	24.205 **	427.067
	(10.736)	(10.420)	(10.567)	(10.487)	(10.462)	(7.812)
Quarters with a job	0.318 **	0.322 **	0.355 **	0.342 **	0.316 **	9.015
	(0.160)	(0.158)	(0.160)	(0.159)	(0.159)	(0.116)
Total Salary	1,128	1,254	1,392	1,412	1,260	34,117
	(983)	(912)	(926)	(924)	(918)	(715)
Wage percentile	-0.782	-0.696	-0.605	-0.582	-0.566	27.617
	(0.750)	(0.690)	(0.699)	(0.697)	(0.686)	(0.547)
# of observations	2,185	2,185	2,185	2,185	2,185	
Controls						
Age at entry		*Yes*	*Yes*	*Yes*	*Yes*	
Parental age at shock			*Yes*	*Yes*	*Yes*	
Year of entry				*Yes*	*Yes*	
Demographic controls					*Yes*	

Notes: The table displays estimates for the treatment effect of a parental job-loss shock before entry on the employment outcomes of children in the 12 quarters following entry. Each entry in Columns (I) - (V) displays the coefficient on an indicator variable equal to one for entrants experiencing a parental job-loss shock in the 4 quarters before their entry into the labor force. The last Column (*Baseline Average*) displays the average of the variable for the sample of children whose parents suffer from the shock in the 4 quarters following entry. All regressions are estimated on the sample of labor market entrants whose parents experience the loss of a stable full-time job in the 4 quarters before or after entry. We exclude those children whose parents lose their job during the quarter of entry. We also exclude the (few) cases where parents get more than one shock in the one-year window around entry. *Days worked* is the sum of all full-time equivalent days worked as a salaried employee in the 12 quarters after entry. *Quarters with a job* is the number of quarters during which the individual had at least one job. *Total salary* is equal to the sum of all pre-tax compensation paid during the quarter, net of all (employer and employee) social security contributions paid during the same quarter. *Wage percentile* is measured in three steps. First, the wage for each job is measured each quarter by dividing total compensation by the number of days worked at that job during the quarter. Second, the wage percentile for each job and quarter is computed based on the wage distribution for all individuals in the Social Security Employment Registry for the current quarter. Third, a single wage percentile for each child is computed as the median wage percentile for the individual for all jobs held in the 12 quarters after entry. Note, in some cases, individuals never earn a daily wage and are excluded from row 4. All controls include a full set of dummy variables for each value of the covariate. Family and demographic controls are gender, nationality (Belgium, E.U. 15, Other U.E., Other countries, unknown), family type (married two-parents, non-married two-parents, single parent), and tercile of parental income before the job-loss. Family type is measured 16 quarters before the loss of family benefits. Standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 7: Robustness: Sample Window - Exclude 1 Year Window

	Estimated treatment effect for main outcome variables							Baseline Average
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	
Dependent variable								
Total Days Worked	24.089 ***	22.507 ***	31.920 ***	36.615 ***	32.044 ***	74.207 ***	76.205 ***	431.241
	(8.201)	(8.011)	(8.483)	(8.546)	(8.610)	(26.430)	(26.250)	(5.679)
Quarters with a job	0.218 *	0.216 *	0.317 **	0.376 ***	0.314 **	0.800 **	0.826 **	9.094
	(0.123)	(0.123)	(0.130)	(0.131)	(0.132)	(0.405)	(0.403)	(0.085)
Total Salary	2,134 ***	1,757 **	2,696 ***	2,763 ***	2,400 ***	5,964 **	6,222 ***	34,191
	(763)	(716)	(759)	(766)	(766)	(2,368)	(2,335)	(529)
Wage percentile	0.851	0.330	0.880	1.115 *	1.087 *	1.974	2.214	26.763
	(0.595)	(0.552)	(0.588)	(0.594)	(0.592)	(1.828)	(1.794)	(0.411)
# of observations	3,472	3,472	3,472	3,472	3,399	3,472	3,399	
Controls								
Age at entry		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	
Parental age at shock			*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	
Year of entry				*Yes*	*Yes*	*Yes*	*Yes*	
Year of parental shock						*Yes*	*Yes*	
Family and Demographic controls					*Yes*		*Yes*	

Notes: The table displays estimates for the treatment effect of a parental job-loss shock before entry on the employment outcomes of children in the 12 quarters following entry. Each entry in Columns (I) - (VII) displays the coefficient on an indicator variable equal to one for entrants experiencing a parental job-loss shock in the 5-12 quarters before their entry into the labor force (Hence, we exclude the cases where parents get a shock in the one-year window around entry). We exclude those children whose parents lose their job in the year proceeding or subsequent to a child's entry. We also exclude the (few) cases where parents get more than one shock. *Days worked* is the sum of all full-time equivalent days worked as a salaried employee in the 12 quarters after entry. *Quarters with a job* is the number of quarters during which the individual had at least one job. *Total salary* is equal to the sum of all pre-tax compensation paid during the quarter, net of all (employer and employee) social security contributions paid during the same quarter. *Wage percentile* is measured in three steps. First, the wage for each job is measured each quarter by dividing total compensation by the number of days worked at that job during the quarter. Second, the wage percentile for each job and quarter is computed based on the wage distribution for all individuals in the Social Security Employment Registry for the current quarter. Third, a single wage percentile for each child is computed as the median wage percentile for the individual for all jobs held in the 12 quarters after entry. Note, in some cases, individuals never earn a daily wage and are excluded from row 4. All controls include a full set of dummy variables for each value of the covariate. Family and demographic controls are gender, nationality (Belgium, E.U. 15, Other U.E., Other countries, unknown), family type (married two-parents, non-married two-parents, single parent), and tercile of parental income before the job-loss. Family type is measured 16 quarters before the loss of family benefits. Standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 8: Robustness: Difference-in-Difference-in-Difference (Using Non-Shocked Entrants as Controls)

	Estimated treatment effect for main outcome variables					Baseline Average
	(I)	(II)	(III)	(IV)	(V)	
Dependent variable						
Total Days Worked	23.483 ***	23.098 ***	23.832 ***	25.497 ***	24.892 ***	449.606
	(6.785)	(6.582)	(6.498)	(6.475)	(6.476)	(0.246)
Quarters with a job	0.246 **	0.248 **	0.261 ***	0.279 ***	0.265 ***	9.143
	(0.102)	(0.101)	(0.099)	(0.099)	(0.099)	(0.004)
Total Salary	1,667 ***	1,500 **	1,560 ***	1,561 ***	1,525 ***	36,564
	(638)	(595)	(587)	(584)	(582)	(23)
Wage percentile	0.193	-0.120	-0.108	-0.083	-0.016	29.015
	(0.490)	(0.449)	(0.446)	(0.445)	(0.442)	(0.018)
# of observations	2,054,677	2,054,677	2,054,677	2,054,677	2,011,592	
Controls						
Age at entry		*Yes*	*Yes*	*Yes*	*Yes*	
Parental age at birth			*Yes*	*Yes*	*Yes*	
Year of entry				*Yes*	*Yes*	
Family and Demographic controls					*Yes*	

Notes: The table displays estimates for the treatment effect of a parental job-loss shock before entry on the employment outcomes of children in the 12 quarters following entry. In addition to entrants with a parental job-loss shock, the sample for this table includes labor market entrants whose parents did not experience a job loss event in the 12 quarters proceeding or following entry. Each entry in Columns (I) - (V) displays the coefficient on an indicator variable equal to one for entrants experiencing a parental job-loss shock in the 12 quarters before their entry into the labor force. All specifications include an indicator variable for whether an entrant received a parental shock (regardless of timing). *Days worked* is the sum of all full-time equivalent days worked as a salaried employee in the 12 quarters after entry. *Quarters with a job* is the number of quarters during which the individual had at least one job. *Total salary* is equal to the sum of all pre-tax compensation paid during the quarter, net of all (employer and employee) social security contributions paid during the same quarter. *Wage percentile* is measured in three steps. First, the wage for each job is measured each quarter by dividing total compensation by the number of days worked at that job during the quarter. Second, the wage percentile for each job and quarter is computed based on the wage distribution for all individuals in the Social Security Employment Registry for the current quarter. Third, a single wage percentile for each child is computed as the median wage percentile for the individual for all jobs held in the 12 quarters after entry. Note, in some cases, individuals never earn a daily wage and are excluded from row 4. All controls include a full set of dummy variables for each value of the covariate. Family and demographic controls are gender, nationality (Belgium, E.U. 15, Other U.E., Other countries, unknown), family type (married two-parents, non-married two-parents, single parent), and tercile of parental income before the job-loss. Family type is measured 16 quarters before the loss of family benefits. Standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 9: Robustness: Definition of Involuntary Job-loss

	Dependent variable								
	Total Days Worked		Quarters with a job		Total Salary		Wage percentile		# of obs.
	(I)		(II)		(III)		(IV)		
To Unemployment Insurance (U.I.)	*30.631*	***	*0.336*	***	*2,385*	***	*0.817*		*4,051*
	(7.706)		*(0.117)*		*(698)*		*(0.543)*		
To unregistered activity	*8.151*		*0.051*		*224*		*-1.194*		*1,554*
	(12.263)		*(0.176)*		*(1,189)*		*(0.918)*		
Receives severance pay	*11.108*		*0.034*		*469*		*-0.742*		*2,068*
	(10.857)		*(0.164)*		*(1,001)*		*(0.772)*		
To U.I., no severance pay	*37.186*	***	*0.427*	***	*3,123*	***	*1.434*	**	*2,831*
	(9.188)		*(0.138)*		*(842)*		*(0.655)*		

Notes: This table presents robustness tests for the treatment effect of a parental shock before entry on the main employment outcomes of interest. The sample used for this estimation includes children whose parent lost a job within a three year window of entry. The dependent variables are defined as in Table 4. For each dependent variable and specification, the table displays the coefficient on an indicator variable equal to one if the entrant experienced a parental job-loss shock before entry. Because of reduced sample size, the treatment effect is simply estimated as the difference between the treatment (before) and control (after) group. The first row restricts the sample to parents receiving unemployment insurance at the end of the quarter during which they lose their job or at the end of the next quarter. The second row (*unregistered inactivity*) restricts the sample to parents who are not observed as transiting to unemployment insurance in the same period after entry. The third row presents result for the sample of parents who receive at least one euro of severance pay in the job-loss quarter or the next quarter (but no unemployment insurance). The fourth row is estimated on the sample of parents who transit to unemployment insurance and do not receive any severance pay. Standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

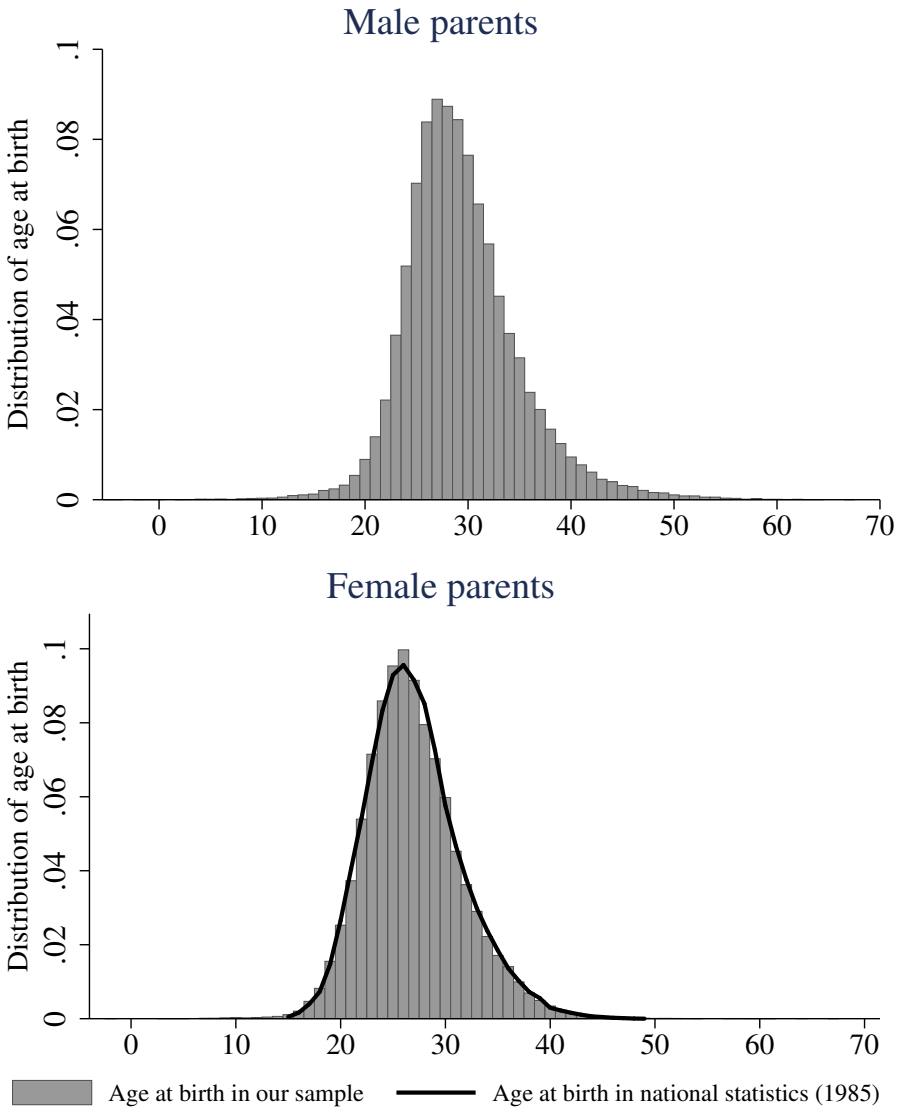
Table 10: Heterogeneous Treatment Effects: Multivariate Regressions

	Days Worked	Quarters with Job	Total Salary	Daily Salary
	(1)	(2)	(3)	(4)
Shock Before Entry	−4.914	−0.012	−187.6	0.544
	(14.315)	(0.218)	(1,291.45)	(0.998)
Male	13.793	−0.007	1,326.2	3.889***
	(9.014)	(0.137)	(813.19)	(0.631)
Low Wage Parental Job	−78.416***	−0.736***	−9,397.6***	−6.311***
	(12.914)	(0.197)	(1,165.05)	(0.9)
Medium Wage Parental Job	−53.761***	−0.411**	−6,374.3***	−4.061***
	(11.562)	(0.176)	(1,043.14)	(0.805)
Entry Age < 20	−95.149***	−0.889***	−11,414.3***	−6.435***
	(10.088)	(0.154)	(910.14)	(0.708)
Before * Male	7.147	0.013	1,401.6	1.436
	(12.776)	(0.195)	(1,152.63)	(0.895)
Before * Low Wage	35.778**	0.4	2,550.8	0.821
	(17.888)	(0.273)	(1,613.86)	(1.251)
Before * Medium Wage	25.168	0.222	1,312.8	−0.271
	(16.191)	(0.247)	(1,460.76)	(1.131)
Before * < 20	33.758**	0.391*	2,081.1	−1.028
	(14.055)	(0.214)	(1,268.05)	(0.988)
Observations	5,605	5,605	5,605	5,306

Notes: This table investigates how the treatment effect of parental job-loss before entry varies with the parent's age at birth and wage level of the lost job as well as the child's gender. The baseline treatment is the coefficient on an indicator variable equal to one for entrants experiencing a parental job-loss shock in the 12 quarters before their entry into the labor force (*Before*). Across specifications, the non-interacted treatment effect is for female children entering between 20 and 25 years old and whose parents lose a job for which they received a wage in the top third of the wage distribution. The heterogeneous effects are identified by interacting the *Before* dummy with indicator variables for child characteristics. The wage of the lost job is measured two quarters before the job-loss shock according to the procedure described in Table 1. All regressions include controls for year of entry, parental age at job-loss, and year of parental job-loss. We exclude those children whose parents lose their job in the quarter of entry or get more than one job-loss shock in the 12 quarters around entry. Dependent variables are defined as in Table 4. Standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01

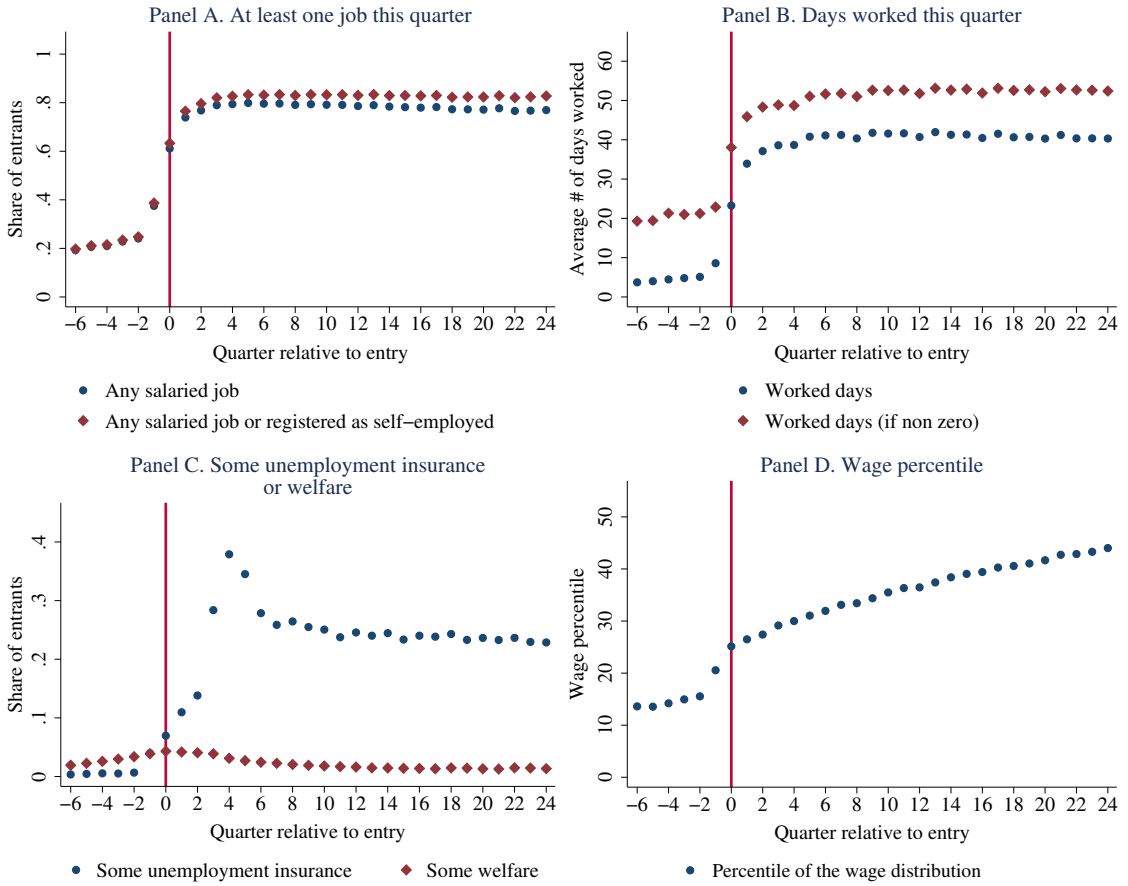
Figures

Figure 1: Parental Age at Birth



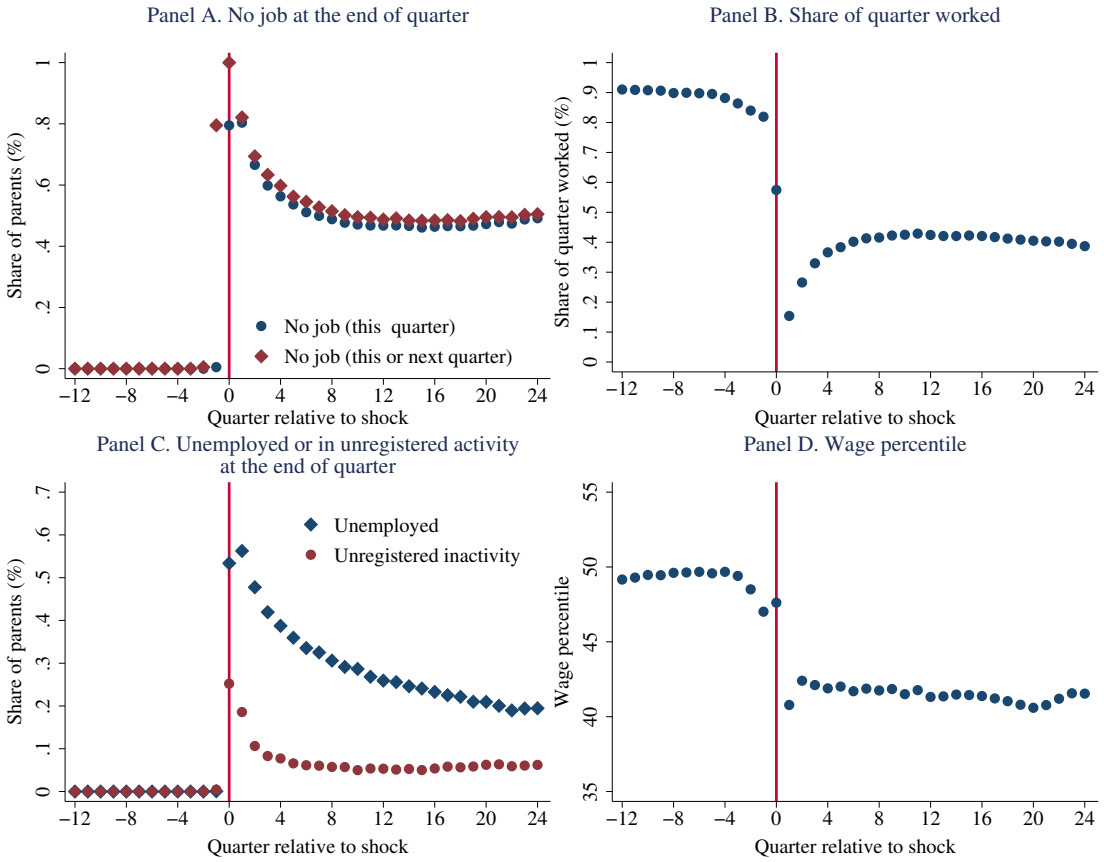
Notes: This figure displays the distribution of parental age at birth for male and female parents in our data. Age at birth is computed as the age of the parent on the 31^{th} December of the child's year of birth. The average year of birth in our data is 1985. For comparison purposes, the graph for female parents includes the distribution of parental age at birth in 1985 from Belgium's official statistics on population and birth (no such data is published for male parents). This graph is constructed by multiplying the size of the female population at each age by the rate of birth at the same age.

Figure 2: Transition from Full-time Education to Working Life



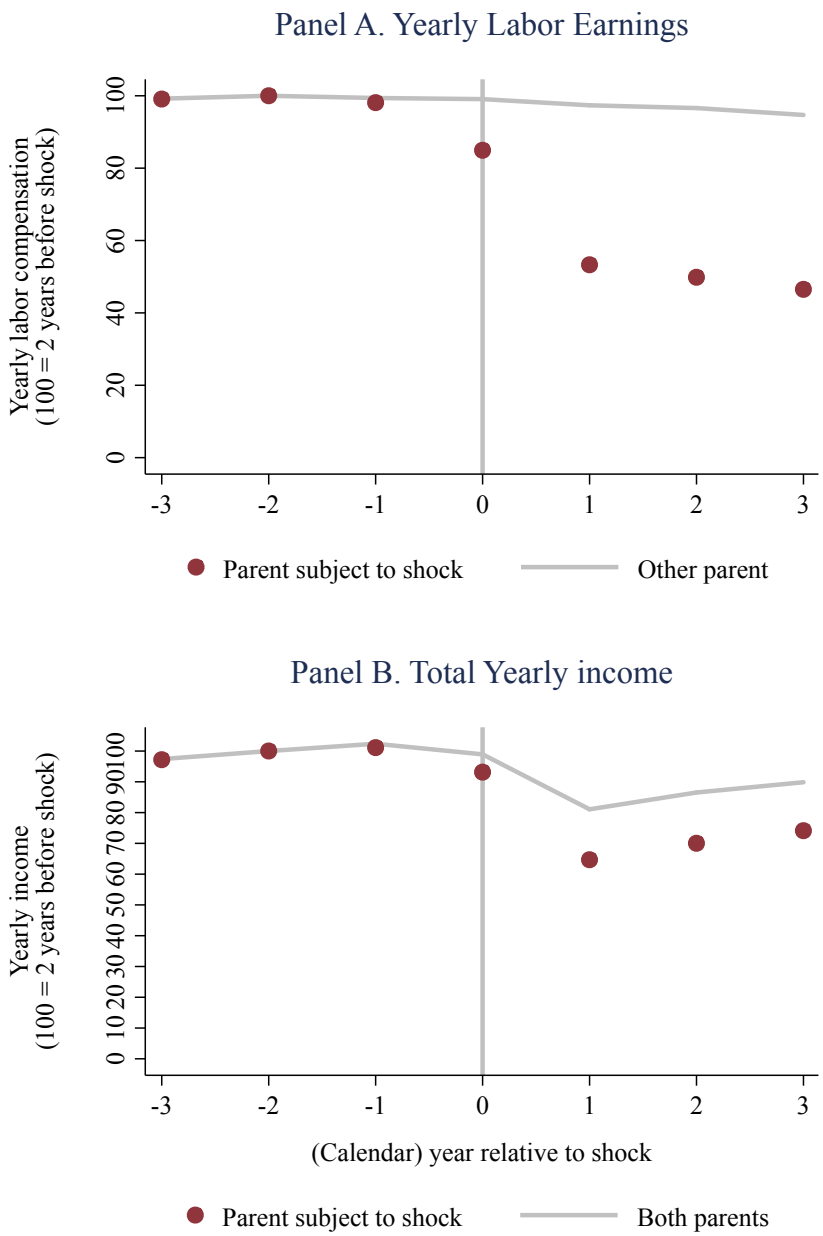
Notes: Each panel of this figure displays the evolution of a different labor market outcome by quarter relative to labor force entry for the representative sample of labor market entrants in Belgium between 2004 and 2008. Individual labor market outcomes are observed until the last quarter of 2011. As a consequence, our sample is not balanced because some outcomes are not observed for the full 24 quarters after job loss (labor market entrants progressively disappear from our sample from the 12^{th} quarter following entry. Using a balanced sample of the earliest entrants brings similar results (See Figure A2 in Appendix). Quarter 0 is defined as the first of two consecutive quarters for which the entrant is either (i) not receiving family allowances, (ii) registered as a job-seeker with the public employment agency or (iii) working for more than two thirds of the quarter as described in section 3. Individuals in Panel A are considered as having a job if they work at any point during the quarter. *Days worked* in Panel B represent the sum of all full-time equivalent days worked as a salaried worker at all jobs during the 12 quarters after entry. An individual is considered as receiving unemployment or welfare benefits in Panel C if he receives such benefits at any point during the quarter. The wage percentile is obtained by dividing total (pre-tax) labor earnings by days worked at each job and taking the median for each individual if he has more than one job during the quarter. Panel D displays the average wage percentile for individuals in our sample.

Figure 3: Description of Parental Job-loss Shocks



Notes: Each panel of this figure displays the evolution of different labor market outcomes by quarter relative to job loss for the sample of parents suffering from the loss of a stable full-time job as defined in section 3. Quarter 0 refers to the quarter of job loss as defined in the same section. Individual labor market outcomes are observed until the last quarter of 2011: therefore, this pooled sample is unbalanced because some outcomes are not observed for the full 24 quarters after job loss. Using a balanced sample brings similar, albeit noisier, results (see Figure A3 in Appendix). The *share of quarter worked* in Panel B is computed as the total amount of time worked during the quarter divided by the maximum total amount of time legally worked by a full-time worker during the same quarter. Individuals in Panel C are considered as unemployed or in unregistered activity based on the end of quarter socio-economic status variable, as explained in section 2.2. The wage percentile in Panel D is obtained by dividing total labor compensation by total days worked at each job during the quarter and taking the median for each individual if he has more than one job during the quarter. Panel D displays the average wage percentile in the sample.

Figure 4: Average Labor Earnings of Parents
by Year Relative to Job-loss Shock



Notes: Panel A of this figure shows the sum of all (pre-tax) labor compensation by calendar year relative to the shock for parents suffering from the loss of a stable full-time job as defined in section 3, in the 12 quarter around entry, as well as for their spouse (grey line). Year 0 refers to the calendar year of job loss as defined in the same section. Parental shocks are observed between 2003 and 2011 and income data between 1990 and 2011. Therefore, this pooled sample is unbalanced because some outcomes are not observed for the full 3 years after job loss. Using a balanced sample brings similar, albeit noisier, results. Yearly labor compensation data is from the Employment Registry of Belgium's public pension administration for private sector workers. This data includes all private sector salaried workers as well as contractual employees from the federal government and employees from local public authorities (i.e. it does not include labor income for the self-employed or statutory civil servants). Yearly income is normalized to 100, two years before the job loss shock and is winsorized at the 99^{th} percentile. Panel B displays data for the sum of all types of income registered in the social security database, including both labor market income and replacement income, for the parent suffering from the shock (dots), as well as for the entire household (grey line). Each point represents the average percentile of the parent (household) suffering from the shock, in the distribution of total income for all individuals (households) in the Belgian resident population.

Figure 5: Identification Strategy

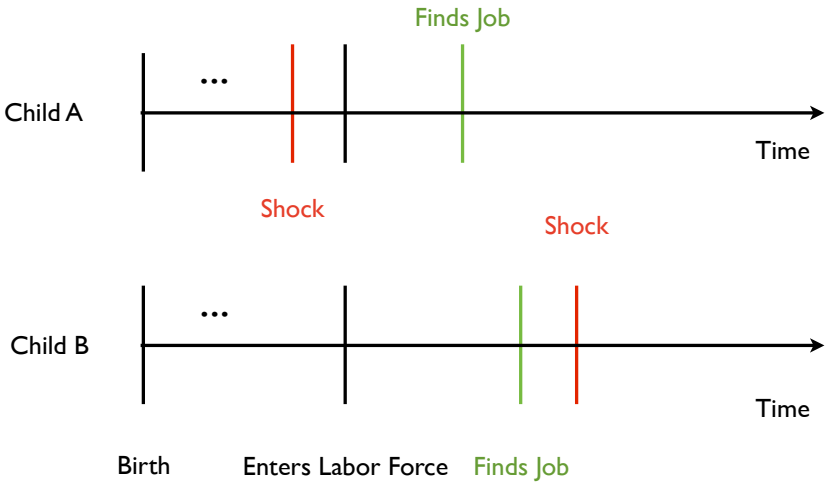
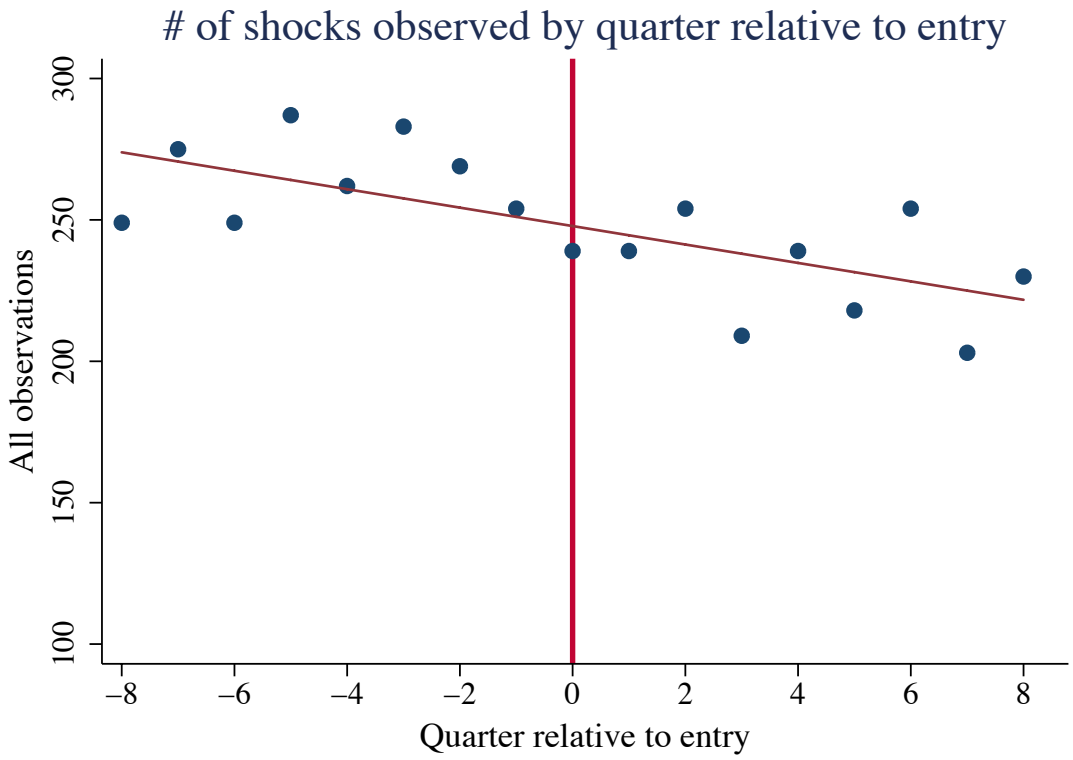
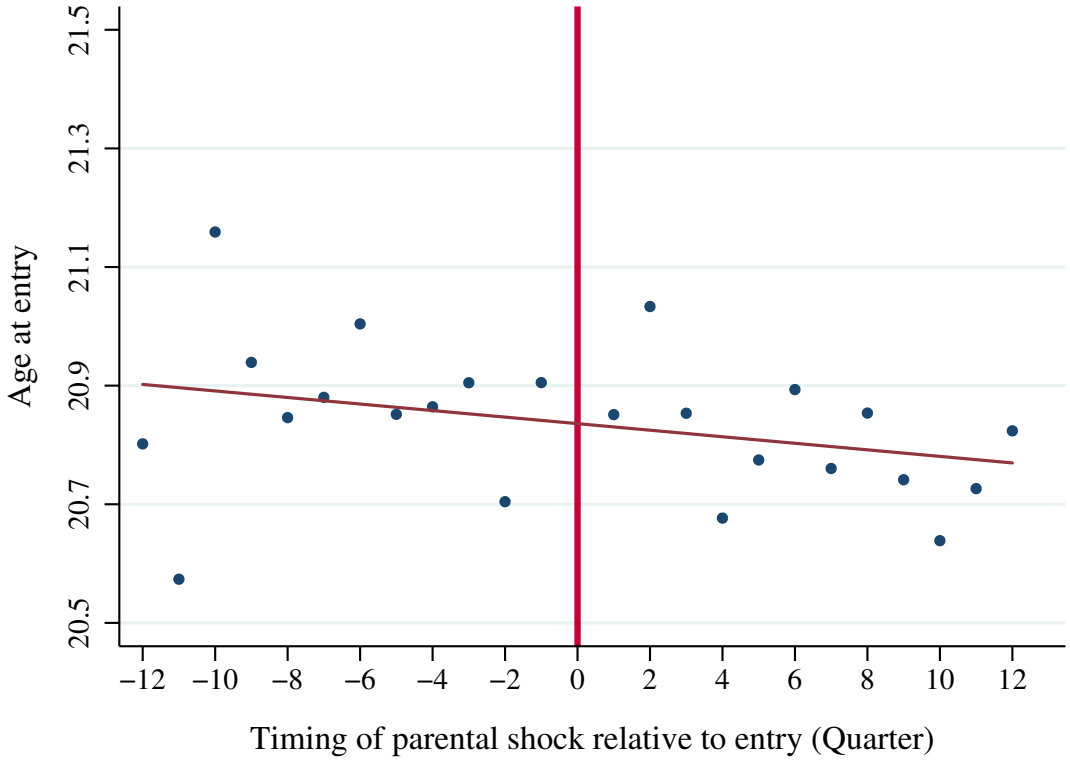


Figure 6: Distribution of the Number of Parental Shocks
by Quarter Relative to Entry



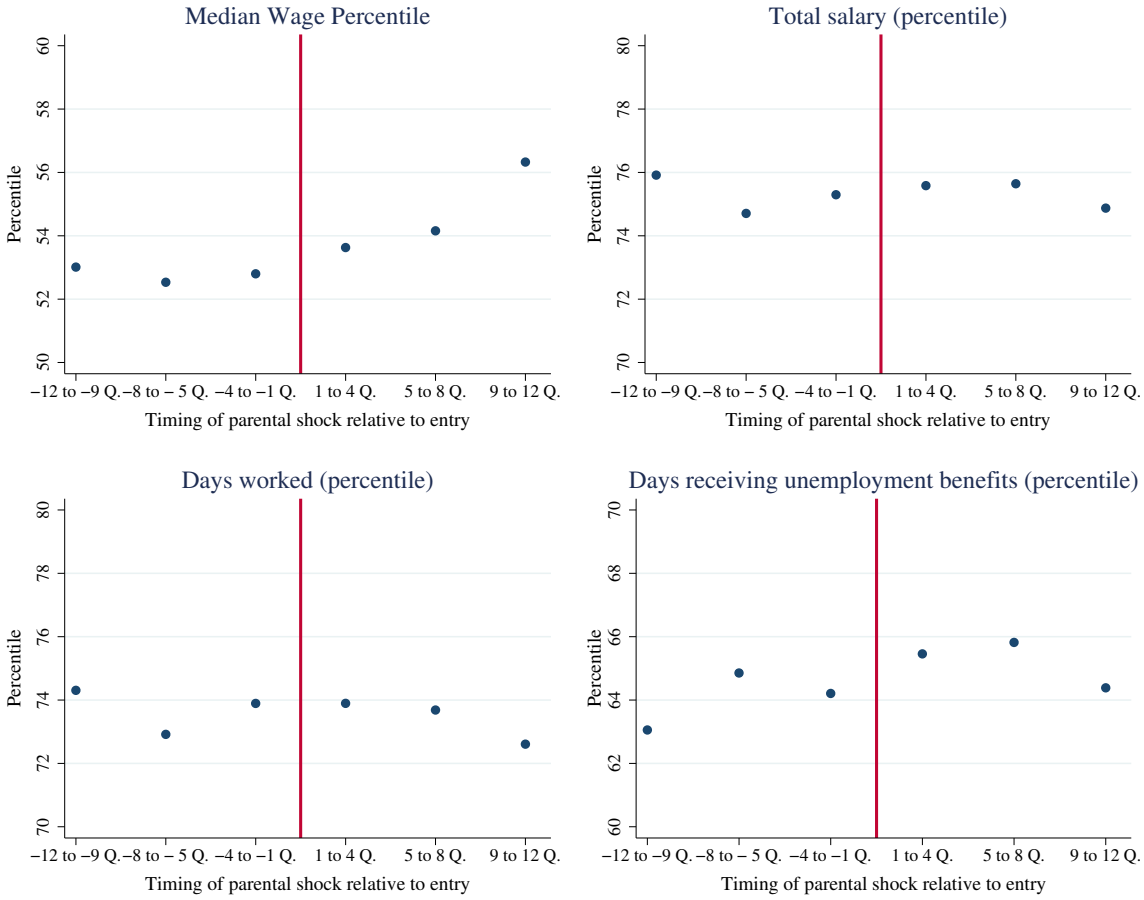
Notes: This figure displays the number of parental job loss shocks in our data relative to entry. Shocks are defined as in section 3 and are observed between 2003 and 2011 for the sample of children entering the labor force between 2004 and 2008. The fitted line is the predicted value from a simple linear regression of the number of shocks observed in each quarter on the quarter relative to entry (0 being the quarter of entry).

Figure 7: Average Child Age at Entry
by Year of Parental Shock Relative to Entry



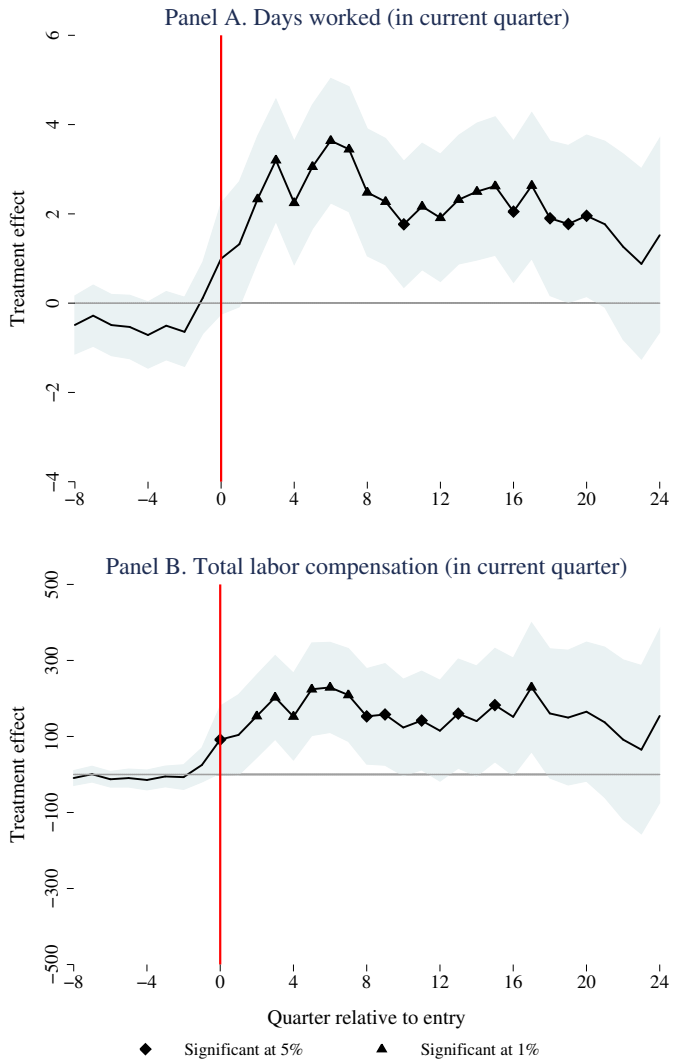
Notes: This figure displays the average child's age at entry, as a function of the timing of parental income shocks relative to entry. Each point is the average age-at-entry for the subsample of children whose parents suffer from a parental job-loss shock in the specified time window. Shocks are defined as in section 3 and are observed in a 3-years window around entry, between 2003 and 2011, for the sample of children entering the labor force between 2004 and 2008. We exclude cases of children whose parents experience more than one shock in the 3-years window around entry. None of the pairwise comparisons between averages for the different time windows considered in this graph come close to being statistically significant at conventional levels.

Figure 8: Balance Tests: Parental Employment History
by Year of Shock Relative to Entry



Notes: Each panel of this figure displays the average value of the corresponding variable, as a function of the timing of the parental job-loss shock relative to the child's entry into the labor force. Each point is the average of the indicated variable for the subsample of children whose parents suffer from a parental job-loss shock in the specified time-window around entry. Shocks are defined as in section 3 and are observed in a 3-years window around entry, between 2003 and 2011, for the sample of children entering the labor force between 2004 and 2008. We exclude cases of children whose parents experience more than one shock in the 3-years window around entry. All variables in this figure are computed using data for 10 calendar years prior to the job loss shock. The *median wage percentile* is obtained by (i) first, dividing total labor compensation for each year by the total number of days worked during the same year to obtain the daily wage for the current year, (ii) second, computing the percentile of this value in the distribution of wages for the same year, and (iii) third, computing the median wage percentile for each individual for the last 10 years. For all variables, percentiles are computed using the distribution of the relevant variable for parents whose child enter in the same year in the representative sample of all entrants. Data is from the Employment Registry of Belgium's public pension administration for private sector workers.

Figure 9: Time-varying Effect of Job Loss Shocks on Total Days Worked by Quarter



Notes: This figure plots the estimated effects of parental job-loss shocks on days worked and labor compensation by quarter from two years before to six years after entry. Each line represents the estimated coefficient for an indicator variable equal to one for children whose parents experience a job-loss shock in the 12 quarters prior to entry. The regression is estimated on the sample of children entering the labor market between 2004 and 2008 and whose parents suffer from a job loss shock in the 12 quarters prior to (treatment group) or following (control group) the child's entry into the labor force. Labor market outcomes are observed for all children from 2 years before entry up to the last quarter of 2011. Therefore, while the sample is balanced up to 12 quarters after entry, later outcomes are only available for earlier entrants (e.g. outcomes up to 24 quarters are estimated on individuals entering in 2004 and 2005). The line plots the estimated treatment effects in a regression that includes controls for the child's age-at-entry, year of entry, parental age-at-shock, year of shock, and indicator variables for each quarter of the year. The shaded area plots the 95% confidence interval and markers indicate statistical significance at 1 and 5%. Coefficients are estimated by pooling all quarterly observations and interacting regressors with dummies for each quarter relative to entry (i.e. point estimates are equivalent to separate quarter-by-quarter regressions). Standard errors are clustered at the individual level.

A Appendix

A.1 Data sources

As shown in figure A1, data for this project comes from a variety of Belgian governmental institutions. This data is merged into the *Labour Market Data Warehouse* (LMDW) using Belgium's unique personal identifier assigned to any Belgian citizen or resident. Below, we provide more information on each source of data used in this paper.

National Registry. National Registry data allows us to identify children-parents relationships as well as several household characteristics. Belgium's national registry provides basic demographic information about all Belgian citizens and residents. In particular, the place of residence, household membership and the status of each individual in the household (dependent child, head, spouse) are required by law to be recorded and each change is subject to an individual control by a member of the police forces. In addition to this information, we also extract from this database, personal information on sex, age and citizenship as well as the number of children in the household.

Family Allowances. Belgium's National Office of Family Allowances for Salaried Workers (*ONAFTS : Office National des Allocations Familiales pour Travailleurs Salariés*) provides quarterly data on family allowances payments. Specifically, we extract a dummy variable indicating, for each quarter, whether family allowances have been paid for each child in our sample.

Social Security's Employment Registry. The employment database of Belgium's National Social Security Office (*ONSS : Office National de la Securite Sociale*) contains quarterly data on each salaried employment relationship in Belgium. The database relies primarily on mandatory forms filed quarterly by employers in order to compute social security contributions and tax withholdings. It is also used by the tax administration to determine taxable labor income. This data is therefore subject to extensive verifications and little measurement error (with the obvious exception of fraudulent behavior).

The employment registry provides information on days and hours worked, salary as well as basic information on the type of job (e.g. blue- or white-collar), employment contract (regular contract, student contract, low-status job subject to specific regulations,...) and reductions in social security contributions for which the job is eligible (which can be used as a proxy for job quality since such reductions are concentrated on low quality jobs). The database also contains a unique employer identifier (which allow us to construct a measure of firm's size) as well as the employer's industry code. The Social Security Employment Registry covers all private sector employees as well as contractual public sector workers and tenured civil servants.[40] We use this database as our primary source of data on employment outcomes for children as well as to identify parental job loss shocks.

Employment Registry of the Pension Administration. We obtain additional information on parental employment history from the Employment Registry of Belgium's public Pension Administration (*SIGEDIS*). It contains yearly data on sick and unemployed days for each year as well as, for each employer-employee relationship, yearly worked

[40]Data on employees of local public authorities is technically provided by a separate database (*ONSS-APL*) which contains similar information and which we easily merge with the ONSS data to build a single database.

days, total salary and basic job information (such as blue- and white- collar jobs). This database covers all private-sector salaried workers as well as contractual employees from the public sector (i.e. it does not include tenured civil servants and self-employed individuals). This is however not a significant limitation given that tenured civil servants are rarely, if ever, dismissed and will therefore seldom be subject to a job loss shock. The advantage of this database is that it contains at least 10 years of employment history before job loss for every parent in our sample while the Social Security Employment Registry only provides up to 4 years of history for the earliest shocks in our data (2003). We use this additional employment data to test the credibility of our identifying assumption.

Unemployment Insurance Agency. Belgium's National Unemployment Insurance Agency (*ONEM - Office National de l'Emploi*) is responsible for the administration of unemployment benefits which includes the payment of benefits as well as enforcement of eligibility rules (including job search requirements). This database provides us with data on the number of days during which parents and children receive unemployment benefits for each month in our sample.

Public Job Placement Agencies. Public job assistance for the unemployed in Belgium is the responsibility of separate agencies for each of Belgium's three regions (respectively, *VDAB* for Flanders, *Actiris* for Brussels and *Forem* for Wallonia). In order to be eligible for unemployment benefits, new labor market entrants are required to be continuously registered with their respective local agency not only when they receive benefits but also throughout the entire 9-month waiting period during which they are not yet eligible for benefits after the end of their studies (see section 2.1). For each month, we extract from these databases a monthly dummy variable indicating whether the individual was registered with the agency. This information is used to identify the timing of the child's entry into the labor force.

Welfare Payments. Belgium's residual social safety net (under the authority of the *S.P.F. Intégration Sociale*) provides means-tested benefits to individuals who have no other source of income (such as jobless individuals who are not eligible for unemployment insurance). For each month, we extract a variable indicating whether each child in our sample is receiving welfare payments. This information is used to analyze the process of entry into the labor force.

Pension Benefits and Disability Insurance. The pension database of the National Pension Office (*Office National Des Pensions*) provides information on each public pension payment received by retired workers. We use this information to verify that the job-loss shocks are not the result of a voluntary retirement decision. We also use data from Belgium's short- and long-term sickness insurance databases to extract information on receipt of sickness benefits: this also allows us to verify that the parental income shocks that we use are not the result of a sickness episode.

Socio-economic Status. In addition to variables that are directly extracted from each institution's database, the *Labor Market Data Warehouse* also contains variables that summarize the information contained in all the available databases. In particular, we use the "*socio-economic status*" variable that provides summary information on the socio-economic situation of the individual at the end of the quarter (e.g. employed, insured unemployed, uninsured unemployed, sick, retired, dependent inactive children,...). This information is used in the identification of job

loss shocks in order to differentiate involuntary job separations leading to unemployment from other types of job separations (such as job-to-job transitions).

A.2 More Details on Family Benefits

The benefit amounts detailed in the text are modified for certain family situations. Orphans with a single or no surviving parent receive an even higher monthly base benefits of €346.92. Allowances are usually paid to parents but can be paid directly to orphans or children who are estranged from their parents (this remains a marginal situation). For families with more than one child or those benefiting from increased baseline payments, the supplements are €48 and €61 respectively. There is also a special regime for disabled children that concerns a small percentage of the population ($< 2.5\%$). Historically, family allowances for self-employed parents were significantly lower but since we focus on parents who are holding (and losing) a full-time salaried job, this is irrelevant in our case. In 2012, 84.1% of children were receiving regular benefits, there was on average 1.72 children by family and the average monthly payment was around €173.[41] This compares with an average gross monthly salary of around €3,350 corresponding to roughly €2,050 (€2,350) after tax for a two-earners (one-earner) family with two dependent children.[42]

A.3 Identification of Causal Effects in the Presence of Year of Layoff Fixed Effects

In this section we discuss the identification of the effect of a parental layoff before labor market entry in the presence of year of layoff heterogeneity. Suppose there are two cohorts, born in 1985 and 1986. The outcome variable, d (days worked), is observed for kids entering at age 19 and follows the following form:

$$d_{sy,by,i} = \alpha b + \gamma_{sy} + \beta_{by} + \epsilon_{sy,by,b,i} \tag{5}$$

where b is an indicator for the early shock, γ_{sy} is a shock-year fixed effect, β_{by} is a birth-year fixed effect, and ϵ_i is a child specific error.

Consider the estimator formed by the difference in means within cohort, which can be computed using a sample selected for parents having a shock within a one year window of child entry:

$$E[d_{2002,1986} - d_{2003,1986}] = \alpha + \gamma_{2002} - \gamma_{2003} \tag{6}$$

The year of shock fixed effect confounds the estimate. Now suppose that we have the 2003 and 2002 observations for the 1985 cohort. Because both observations are cases of a shock after entry, the difference in outcomes identifies the

[41] These statistics are from Belgium's family allowances administration (ONAFT). The first number comes from ONAFTS (2013), the second from ONAFT (2013) and the third is taken from ONAFTS (2011) and is expressed in 2014 euros.

[42] Source: 2012 official statistics from Belgium's Ministry of the Economy (*SPF Economie*) expressed in 2014 euros. We rely on a popular simulator from an private HR company (*Parthena*) to compute the after-tax income.

difference in year effects.

$$E[d_{2002,1985} - d_{2003,1985}] = \gamma_{2002} - \gamma_{2003} \tag{7}$$

Taking the difference of the two estimators identifies the treatment effect:

$$E(d_{2002,1986} - d_{2003,1986}) - E(d_{2002,1985} - d_{2003,1985}) = \alpha \tag{8}$$

The key to identification is that we need to observe at observations from children whose parents suffered shock in at least two years after the child's entry.

A.4 Additional Tables and Figures

Table A1: Schedule of unemployment insurance benefits for experienced workers

	Replacement rate	Minimum benefits	Maximum benefits	Average benefits	Share of beneficiaries
Head of household				1,074	33.8%
1 to 6 month	60%	1,008	1,324	1,212	5.2%
7 to 12 month	60%	1,008	1,234	1,134	2.3%
Second period	60%	1,008	1,154	1,050	26.3%
Single				973	26.2%
1 to 6 month	60%	847	1,324	1,153	3.6%
7 to 12 month	60%	847	1,234	1,125	2.7%
Second period	54%	847	1,034	924	19.9%
Dependent				806	40.1%
1 to 6 month	60%	634	1,324	1,156	10.4%
7 to 12 month	60%	634	1,234	1,126	6.9%
Second	40%	634	769	725	9.2%
Flat rate				467	15.6%
Normal		447			
Special rate		587			

Notes: This tables summarizes the rules applicable to the computation of monthly unemployment insurance benefits for experienced workers in Belgium (as of 2010). As indicated in the rows of this table, unemployment benefits are a function of the demographic situation of the benefits' claimant and the duration of the current jobless spell. The first Column presents the normal replacement rate while the second (third) Column presents the minimum (maximum) benefits that is applicable when the benefits payments computed using the replacement rate method are lower (higher) than the minimum (maximum) amount. For dependents, the *flat rate period* starts after 15 month of unemployment plus three additional months for each year of work experience. For other beneficiaries, the second period extends without specified time limit. The *special rate* applicable to dependents in the flat rate period is applicable to households for whom social insurance is the only source of revenue and only when benefits received by the other spouse total less than €769. The fourth Column presents the average benefits actually received for each category between January and April 2010. The last Column presents the proportion of beneficiaries in each category during the same period. All amounts are expressed in 2010 euros and rounded to the nearest integer. Source: Table 2-1 of Conseil Central de l'Économie (2011).

Table A2: Robustness: Effect Using Parental Age at Birth

	Estimated treatment effect for main outcome variables							Baseline Average
	(I)	(II)	(III)	(IV)	(V)	(V)	(VI)	
Dependent variable								
Total Days Worked	23.422 ***	22.746 ***	23.044 ***	25.389 ***	25.746 ***	36.197 ***	37.323 ***	429.702
	(6.542)	(6.370)	(6.351)	(6.333)	(6.364)	(12.657)	(12.611)	(4.617)
Quarters with a job	0.245 **	0.242 **	0.250 ***	0.280 ***	0.271 ***	0.401 **	0.418 **	9.068
	(0.098)	(0.097)	(0.097)	(0.097)	(0.097)	(0.194)	(0.193)	(0.069)
Total Salary	1,663 ***	1,469 ***	1,487 ***	1,542 ***	1,635 ***	2,371 **	2,473 **	34,186
	(605)	(565)	(563)	(563)	(563)	(1,126)	(1,116)	(427)
Wage percentile	0.193	-0.116	-0.108	-0.031	0.182	-0.358	-0.209	27.120
	(0.469)	(0.433)	(0.434)	(0.434)	(0.430)	(0.870)	(0.855)	(0.331)
# of observations	5,605	5,605	5,605	5,605	5,532	5,605	5,532	
Controls								
Age at entry		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	
Parental age at birth			*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	
Year of entry				*Yes*	*Yes*	*Yes*	*Yes*	
Year of parental shock						*Yes*	*Yes*	
Family and Demographic controls					*Yes*		*Yes*	

Notes: The table displays estimates for the treatment effect of a parental job-loss shock before entry on the employment outcomes of children in the 12 quarters following entry. Each entry in Columns (1) - (7) displays the coefficient on an indicator variable equal to one for entrants experiencing a parental job-loss shock in the 12 quarters before their entry into the labor force. The last Column (*Baseline Average*) displays the average of the variable for the sample of children whose parents suffer from the shock in the 12 quarters following entry. All regressions are estimated on the sample of labor market entrants whose parents experience the loss of a stable full-time job in the 12 quarters before or after entry. We exclude those children whose parents lose their job during the quarter of entry. We also exclude the (few) cases where parents get more than one shock in the one-year window around entry. *Days worked* is the sum of all full-time equivalent days worked as a salaried employee in the 12 quarters after entry. *Quarters with a job* is the number of quarters during which the individual had at least one job. *Total salary* is equal to the sum of all pre-tax compensation paid during the quarter, net of all (employer and employee) social security contributions paid during the same quarter. *Wage percentile* is measured in three steps. First, the wage for each job is measured each quarter by dividing total compensation by the number of days worked at that job during the quarter. Second, the wage percentile for each job and quarter is computed based on the wage distribution for all individuals in the Social Security Employment Registry for the current quarter. Third, a single wage percentile for each child is computed as the median wage percentile for the individual for all jobs held in the 12 quarters after entry. Note, in some cases, individuals never earn a daily wage and are excluded from row 4. All controls include a full set of dummy variables for each value of the covariate. Family and demographic controls are gender, nationality (Belgium, E.U. 15, Other U.E., Other countries, unknown), family type (married two-parents, non-married two-parents, single parent), and tercile of parental income before the job-loss. Family type is measured 16 quarters before the loss of family benefits. Standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table A3: Robustness: Effect for Entrants at Age 18

	Estimated treatment effect for main outcome variables						Baseline Average
	(I)	(II)	(III)	(IV)	(V)	(VI)	
Dependent variable							
Total Days Worked	27.880 *	36.127 **	42.830 **	41.708 **	46.093	56.678 *	338.749
	(16.529)	(17.629)	(17.447)	(17.461)	(33.130)	(32.800)	(11.801)
Quarters with a job	0.110	0.238	0.321	0.286	0.295	0.470	8.303
	(0.279)	(0.296)	(0.294)	(0.295)	(0.560)	(0.556)	(0.199)
Total Salary	1,458	2,073	2,357 *	2,208 *	2,276	3,259	23,078
	(1,248)	(1,330)	(1,325)	(1,307)	(2,519)	(2,458)	(891)
Wage percentile	-1.754 *	-1.708	-1.518	-1.296	-1.625	-0.338	21.303
	(1.013)	(1.080)	(1.083)	(1.010)	(2.066)	(1.897)	(0.718)
# of observations	822	822	822	812	822	812	
Controls							
Parental age at shock		*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	
Year of entry			*Yes*	*Yes*	*Yes*	*Yes*	
Year of parental shock					*Yes*	*Yes*	
controls						*Yes*	

Notes: The table displays estimates for the treatment effect of a parental job-loss shock before entry on the employment outcomes of children in the 12 quarters following entry. Each entry in Columns (I) - (VII) displays the coefficient on an indicator variable equal to one for entrants experiencing a parental job-loss shock in the 12 quarters before their entry into the labor force. The last column (*Baseline Average*) displays the average of the variable for the sample of children whose parents suffer from the shock in the 12 quarters following entry. All regressions are estimated on the sample of labor market entrants that entered at 18 and whose parents experience the loss of a stable full-time job in the 12 quarters before or after entry. We exclude those children whose parents lose their job during the quarter of entry. We also exclude the (few) cases where parents get more than one shock in the one-year window around entry. *Days worked* is the sum of all full-time equivalent days worked as a salaried employee in the 12 quarters after entry. *Quarters with a job* is the number of quarters during which the individual had at least one job. *Total salary* is equal to the sum of all pre-tax compensation paid during the quarter, net of all (employer and employee) social security contributions paid during the same quarter. *Wage percentile* is measured in three steps. First, the wage for each job is measured each quarter by dividing total compensation by the number of days worked at that job during the quarter. Second, the wage percentile for each job and quarter is computed based on the wage distribution for all individuals in the Social Security Employment Registry for the current quarter. Third, a single wage percentile for each child is computed as the median wage percentile for the individual for all jobs held in the 12 quarters after entry. Note, in some cases, individuals never earn a daily wage and are excluded from row 4. All controls include a full set of dummy variables for each value of the covariate. Family and demographic controls are gender, nationality (Belgium, E.U. 15, Other U.E., Other countries, unknown), family type (married two-parents, non-married two-parents, single parent), and tercile of parental income before the job-loss. Family type is measured 16 quarters before the loss of family benefits. Standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01

Figure A1: Sources of our data

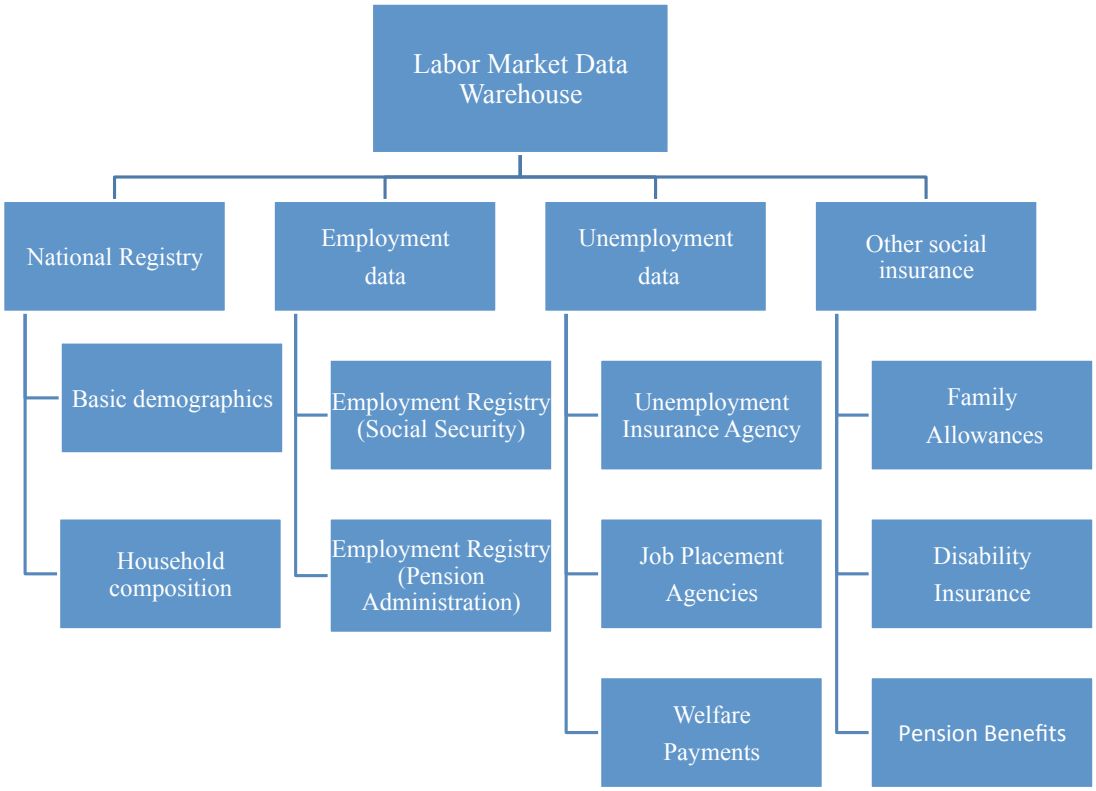
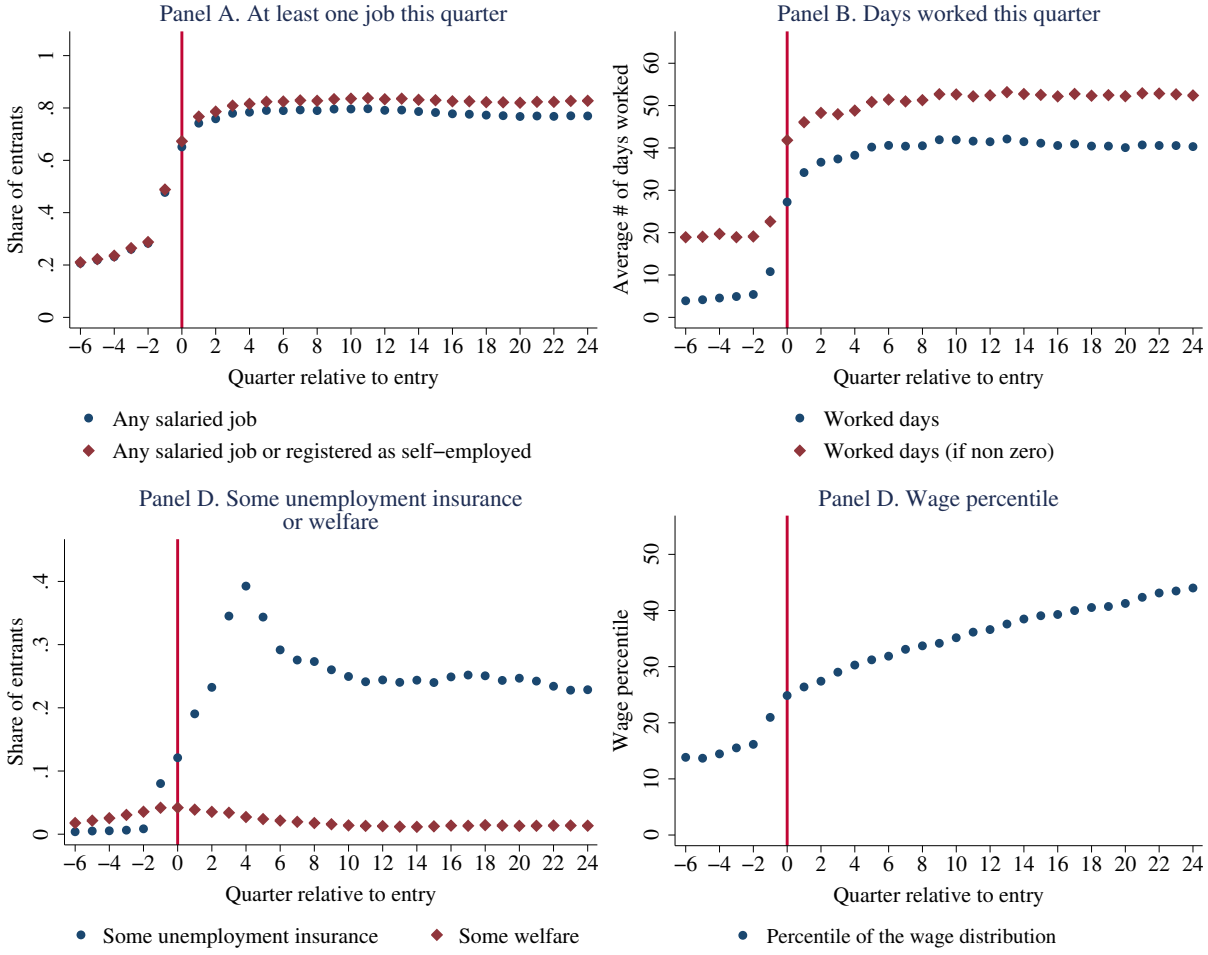
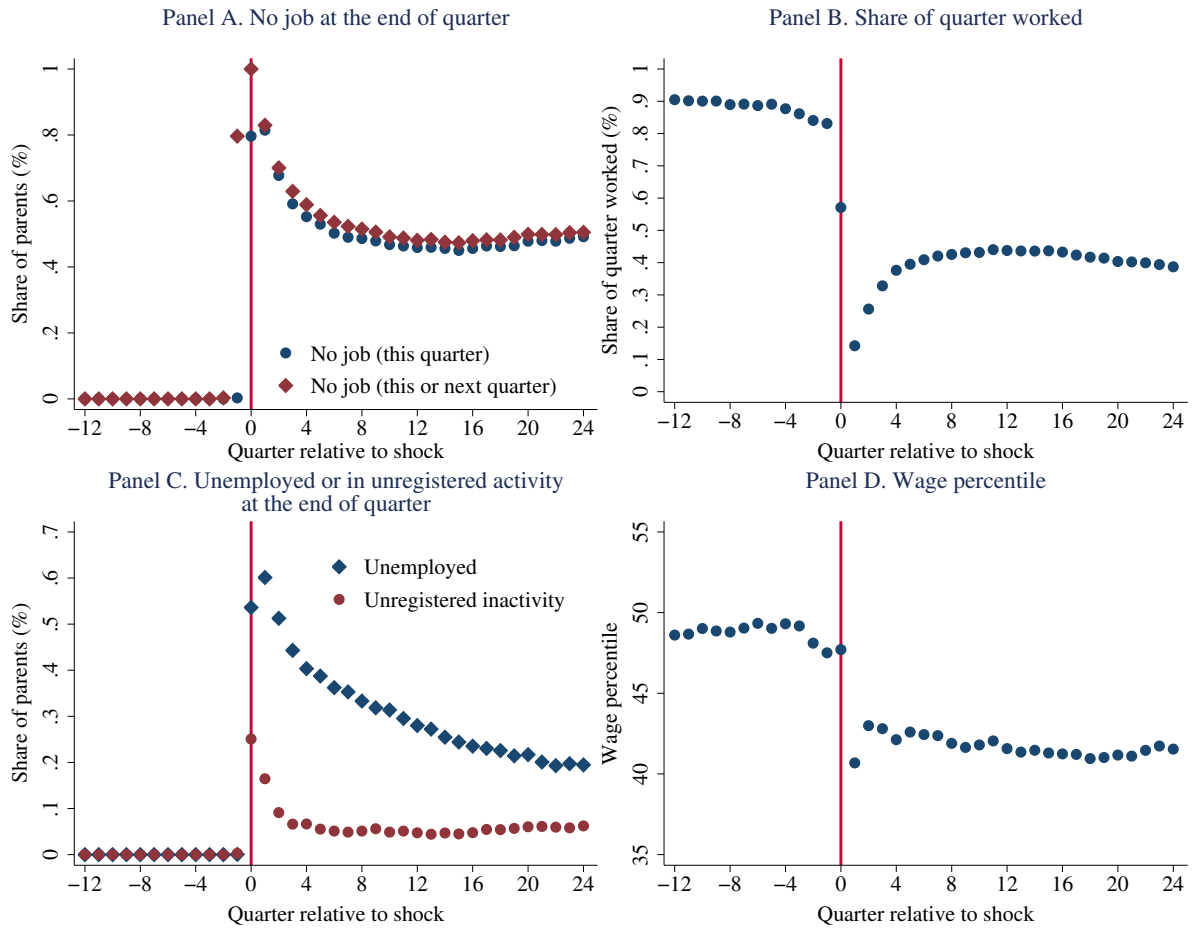


Figure A2: Transition from Full-time Education to Working Life:
Balanced Sample of Entrants (children entering in 2004 and 2005)



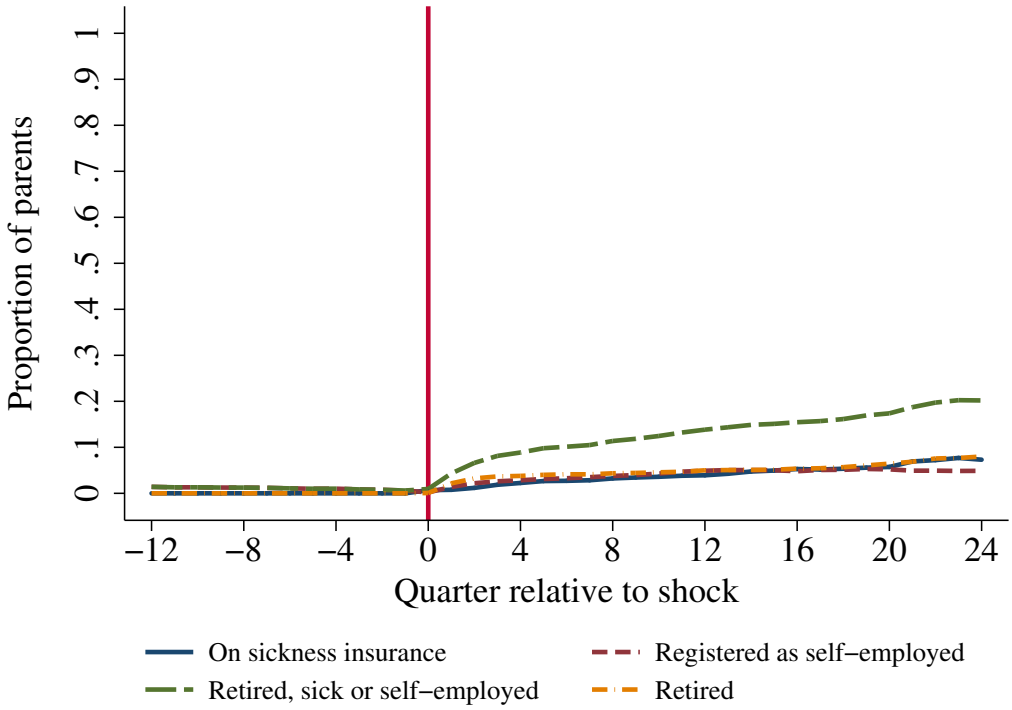
Notes: Each panel of this figure displays the evolution of a different labor market outcome variables by quarter relative to the quarter of labor force entry for the pooled sample of entrants in Belgium between 2004 and 2005. Individual labor market outcomes are observed until the last quarter of 2011 (the sample if strongly balanced across all quarters). Quarter 0 is defined as the first of two consecutive quarters for which the entrant is either (i) not receiving family allowances, (ii) registered as a job-seeker with the public employment agency or (iii) working for more than two thirds of the quarter as described in section 3. Individuals in Panel A are considered as having a job if they work at any point during the quarter. *Days worked* in Panel B represent the sum of all full-time equivalent days worked as a salaried worker at all jobs during the 12 quarters after entry. An individual is considered as receiving unemployment or welfare benefits in Panel C if he receives such benefits at any point during the quarter. The wage percentile is obtained by dividing total labor earning by days worked at each job and taking the median for each individual if he has more than one job during the quarter. Panel D displays the average of the individual wage percentile for individuals in our sample.

Figure A3: Description of Parental Job-loss Shocks :
Balanced Sample of Entrants (shocks between 2003 and 2005)



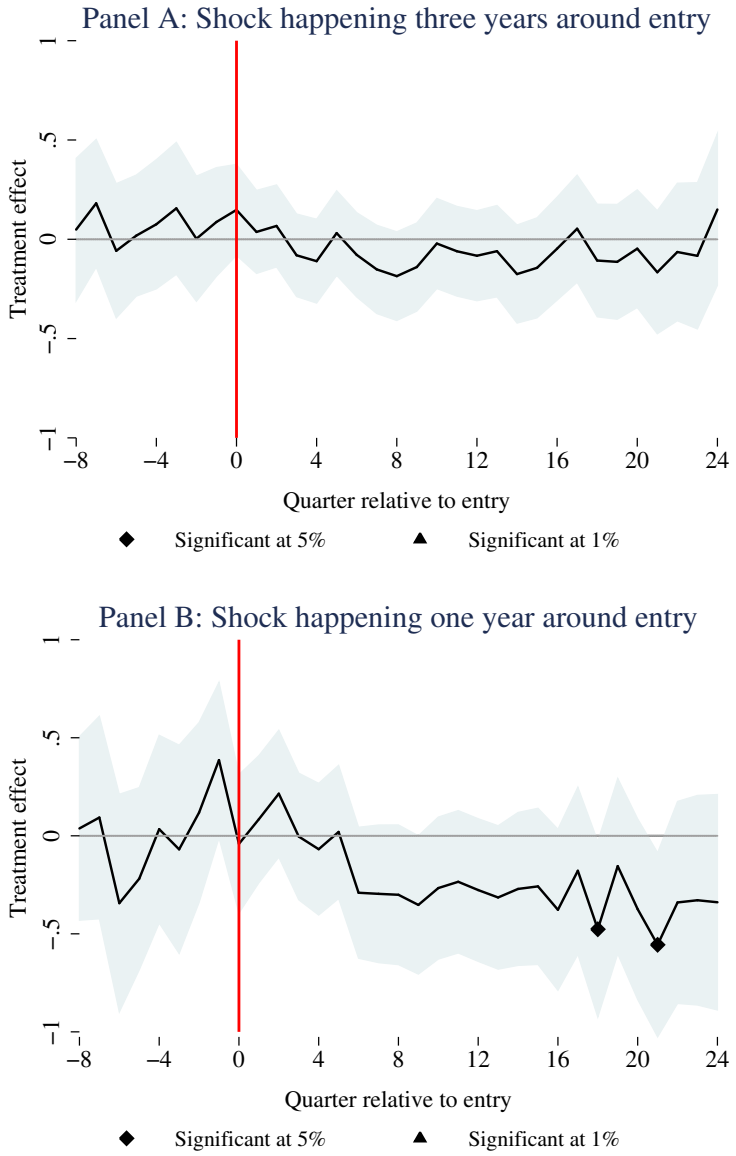
Notes: Each panel of this figure displays the evolution of different labor market outcomes by quarter relative to job loss for the sample of parents suffering from the loss of a stable full-time job as defined in section 3. Quarter 0 refers to the quarter of job loss as defined in the same section. Individual labor market outcomes are observed until the last quarter of 2011. The sample is limited to parents suffering from a shock between 2003 and 2005; therefore, the sample is balanced for the full period of observation displayed. The share of quarter worked in Panel B is computed as the total amount of time worked during the quarter divided by the total amount of time legally worked by a full-time worker during the same quarter. Individuals in panel C are considered as unemployed or in unregistered activity based on the end of quarter socio-economic status variable, as explained in section 2.2. The wage percentile in Panel D is obtained by dividing total labor compensation by total days worked at each job during the quarter and taking the median for each individual if he has more than one job during the quarter. Panel D displays the average wage percentile in the sample.

Figure A4: Percentage of Retired, Sick or Self-employment Parents by Quarter Relative to Shock



Notes: This figure displays the evolution of the percentage of retired, sick or self-employment parents by quarter relative to job loss for the sample of parents suffering from the loss of a stable full-time job as defined in section 3. Quarter 0 refers to the quarter of job loss as defined in the same section. Individual labor market outcomes are observed until the last quarter of 2011: therefore, this pooled sample is unbalanced because some outcomes are not observed for the full 24 quarters after job loss. Using a balanced sample (with parents getting the shock before 2005) brings similar, albeit noisier, results. Individuals are considered as (i) retired if they are receiving public pension benefits at the end of the quarter, (ii) sick if they are receiving sickness benefits at the end of the quarter, (iii) Self-employed if they are registered with the self-employment social security agency at the end of the quarter.

Figure A5: Time-varying Effect of Job Loss Shocks on Wages



Notes: This figure plots the estimated effects of parental job-loss shocks on wages by quarter from two years before to six years after entry. Each line of Panel A (B) represents the estimated coefficient for an indicator variable equal to one for children whose parents experience a job-loss shock in the 12 (4) quarters prior to entry. Panel A (B) present results estimated on the sample of children entering the labor market between 2004 and 2008 and whose parents suffer from a job loss shock in the 12 (4) quarters prior to (treatment group) or following (control group) the child's entry into the labor force. The wage percentile for each quarter is computed according to the method explained in the notes of Table 4. Wages are observed for all children from 2 years before entry up to the last quarter of 2011. Therefore, while the sample is balanced up to 12 quarters after entry, later outcomes are only available for earlier entrants (e.g. outcomes up to 24 quarters are estimated on individuals entering in 2004 and 2005). The line plots the estimated treatment effect in a regression that includes controls for the child's age-at-entry, parental age-at-birth, year of shock, and indicator variables for each quarter of the year. The shaded area plots the 95% confidence interval and markers indicate statistical significance at 1 and 5%. Coefficients are estimated by pooling all quarterly observations and interacting regressors with dummies for each quarter relative to entry (i.e. point estimates are equivalent to separate quarter-by-quarter regressions). Standard errors are clustered at the individual level.