The Effect of Parental Income on Early Career Outcomes

(Preliminary)

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Abstract

Young adults entering the labor force typically have little access to unemployment insurance or other formal insurance mechanisms. Instead, they rely on family insurance in the form of parental support to smooth consumption. We study the labor market response of Belgian young adults to decreases in available parental support caused by parental job-loss. 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 6% more in the 3 years following labor market entry than a child whose parent loses a job after the child's entry. This effect is concentrated on the extensive margin and disappears within four years after entry. We find no evidence that parental support affects the quality of the initial job that entrants find.

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1 Introduction

There is growing evidence that a worker's first job has long-lasting effects on her lifetime income (Kahn, 2010, Oreopoulos, von Wachter and Heisz, 2012, and Oyer, 2008). Yet young adults entering the labor force typically have no access to unemployment insurance and must rely on parental support, if it exists, to smooth consumption. Therefore, those with little parental support might be induced to find a job faster and to inefficiently accept lower quality jobs, leading to lower career earnings. In this paper, we study the importance of this mechanism using a novel identification strategy that relies on the precise timing of parental job-loss around the time of the child's labor market entry.

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 and are consequently induced to work 6% more in the first 12 quarters of their career. This effect takes place primarily on the extensive margin for male entrants who do not complete college and whose parental income is in the bottom two-thirds of the wage distribution. However, the decision to find a job quicker has no long run effects on the child's career. The effect of parental support on the child's labor supply fades out within four years after entry. Furthermore, we find no evidence that parental insurance affects the quality of the first job found by young workers.

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 (un)observable 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 identification 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 main identifying assumption, whose plausibility we extensively document, is that parents experiencing a 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.

The setting of this paper is Belgium, where we have access to administrative data from the social security system. Our sample consists of over five thousand Belgian children whose parents experience the loss of stable full-time job in a three-years window around the time of their 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 income and labor supply persists, without further recovery, for at least 6 years.

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. We find no significant difference between the two groups in demographics and labor market outcomes up to 10 years prior to the shock. We also show that the distribution of shocks around the time of entry is continuous with no evidence of bunching either before or after entry. For example, parents could choose to quit their job after the child's entry into the labor force, as a result of reduced financial needs. Such voluntary selection in the control and treatment group would create legitimate concerns about the validity our research design. 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 their entry into the labor force. In particular, Hilger (2014) finds a significant, albeit small, effect of parental income shocks on children's college attendance in the US. However, we find no effect of job loss shock on the timing of entry: the average child's age at entry for members of the control (before) and treatment (after) groups are statistically indistinguishable. The difference between our results and Hilger's likely reflects institutional differences between the US and Belgian higher education systems. In Belgium, college tuition is highly subsidized (with tuition fees lower than 1,000 euros per year), while in the US college tuition is expensive, especially for middle-class families.

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 those whose parents experience such a shock after entry. First, consistently with theoretical predictions, we find that children whose parents lose a job in the year prior to entry, work on average 6% more (24 days) in the first 3 years of their career. Second, we find that the increase in labor supply is largest in the first two years and fades out completely within 4 years. Third, the increase in labor supply is entirely accounted for by responses along the extensive margin. In the first 3 years of their career, members of the control group have at least one job in .31 more quarters. By contrast, there is no evidence of an increase in days or hours worked per quarter at continuing jobs. Fourth, looking at the number of employers per worker, we find that children whose parents suffer from a shock before entry, have 5.2% more employers in the first two quarters after entry. However, this effect is temporary and the point estimates become close to zero and insignificant as early as four quarters after entry. Fifth, we find no evidence of a decrease in job quality as measured by wage growth, employer size or blue/white-collar status. 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 concentrated on male children without college education and with parents in the bottom two-thirds of the wage distribution.

Overall, our results are best explained by the canonical job-search model of Mortensen (1977). Labor market entrants who experience a decrease in parental support before entry increase their search efforts. 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[1]. We find no support for more elaborate theories in which labor supply responses are partially the result of variations in the quality of jobs that unemployed workers seek or accept.

Our findings align with previous empirical evidence on the effects of social insurance, which has found that higher unemployment benefits are associated with an increased duration of unemployment (e.g. Krueger and Meyer, 2002, Chetty, 2008 and Lalive, Ours and Zweimller, 2006). Also consistent with our results, previous empirical research has found small labor supply responses on the intensive margins (Meghir and Phillips, 2010). We provide a new and independent confirmation of those results by using a novel source of variation in job search incentives (parental shocks). However, unlike most prior literature, we study individuals who join the labor force for the first time, rather than experienced workers.

However, our results stand in contrast with a large body of work that has found higher labor participation elasticities among women (e.g. (Meghir and Phillips, 2010)). We find that women's labor participation elasticities may now be below that of males, at least in the early stages of their career.This difference might be explained by the fact that higher elasticities have been found mostly for married women, especially those with children. However, the labor supply response that we measure happens almost exclusively before the age of 23, an age when marriage and pregnancy rates are still low in Belgium.[2]

Our results also 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 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 the negative result in the literature is that existing studies focus on experienced workers, for whom the

[1]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.

[2]More recent studies point to rapidly decreasing elasticities among single women (Bishop, Heim and Mihaly, 2009).

[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 (2014).

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 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. However, we find no evidence that reduced family support induces workers to find higher paying, riskier or more stable jobs.

Our paper also contributes to a large literature on the importance of family insurance through the joint labor supply decisions of household members. Most of this literature studies spousal insurance rather than child-parent insurance. For example, Blundell et al. (2014) show that 25% of married males' labor income shocks are insured by the labor supply response of their spouses.[5] Furthermore, they show that the importance of this insurance channel depends on the level of household self-insurance through asset accumulation. 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.

The rest of this paper proceeds as follows. In Section 2, we provide necessary information on the Belgian institutional setting. We then discuss the different sources of administrative data used in this project in section 2.3 and 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 then describes our identification strategy as well as the theoretical predictions that we put to test in our analysis. Section 6 presents tests that establish the credibility of our research design. Section 7 finally contains the empirical results on the effects of parental shocks on child labor market outcomes. The last section concludes.

2 Institutional setting

2.1 Institutional background on children

Our research design requires the identification 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

[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.

[5] Other papers that study the importance of spousal insurance include Cullen and Gruber (2000), Stephens Jr. (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 (for a review, see Edmonds, 2008).

of institutional characteristics of the Belgian social security system. This section provides the necessary institutional background 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 enter the labor market without completing the entire high school curriculum.[6] Between 12 and 18, high school students are progressively sorted in a technical, professional or general program. Technical and professional education is usually 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 usually try to acquire a regular tertiary education.[7].

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 usually 3 years (as long as 5 in more prestigious institutions). With a few exceptions, the only admission condition to higher education is a high school degree of any sort: institutions are not allowed to set other admission standards. As a consequence, while a very large share of each cohort registers for higher education, initial success rates are low[8]. Overall, this translates into a graduation rate from tertiary education of 44% in the 30-34 age group, of which slighly less than 50% graduate from college (OECD, 2014).

Family Allowances. Family allowances in Belgium consists in automatic monthly cash payments to parents of dependent children under the age of 25. Eligibility for family allowances is unconditional until the child 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 actively enrolled in full-time education or apprenticeships. Eligibility for family allowances after 18 also requires students to work less than 240 hours by 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 put an end to their full-time education are usually eligible for up to 9 additional months of family

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

[7]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 come by. Since the overall structure of the education system remains very similar in the 3 different communities, these numbers still provide a broadly accurate picture for the entire country.

[8]Around two-thirds of students either drop out of higher education, repeat their first year or switch to a less selective education

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)[9]. Since eligibility for family allowances requires the continuation of full-time education, our sample selection will primarily rely on family allowances' payment data in order to identify the timing of children's entry into the labor force.

Family allowances are not generally means-tested although baseline payments are increased based on the 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 the rare case of orphan children with no or a single surviving parent[10]. In practice, 84.1% of children were receiving regular benefits in 2012, there was on average 1.72 children by family and the average monthly payments was around 173€[12]. This compares with an average gross monthly salary of around 3,350€ corresponding to roughly 2,050€ (2,350€) after-tax for a two-earner (one-earner) family with two dependent children[13] For most parents, family allowance therefore represent a small but significant share of the household budget. However, family benefits are usually far from covering the full cost of maintaining a child. While there is no official estimates of such cost, 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[14].

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 eligibible for special unemployment benefits ("*allocations d'attente*") after a (usually) 9-month waiting period[15]. During the waiting period, unemployed individuals are expected

[9]The same limit applies to children in apprenticeship

[10]Specifically, baseline monthly payments (in 2014 euros) are 90, 167 and 249 for the first, second and third (or more) child respectively[11]. 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) . 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). Baseline amounts are further increased by 24€ per month for kids above 12 and by 28 above 18 for families receiving normal baseline benefits. For families with more than one child or 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 since they are eligible for the higher benefits.

[12]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.

[13]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.

[14]Another useful point of comparison is the method most commonly used by civil courts to determine child support payments (*mthode 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€.

[15]This waiting period is extended to 12 months above the age of 25. The waiting period is also shortened by the number of

to rely on the financial support of their parents, who have the legal obligation to support them and who, under certain conditions, continue to receive family allowances.

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. During the time covered by our data, these 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. Beneficiaries were also required to have been continuously registered as active job-seekers with the public employment agency during the waiting period.

Labor market entrants who do not meet any of those conditions can turn to the residual social safety net which provides welfare payments to individuals who have no other resources. 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[16], 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 not left without any public assistance. However, at least during their studies and the first 9 months of unemployment, young job-seekers are expected to rely on the financial help of their parents. This paper leverages this feature by using large variations in parental income to identify the effects of job search incentives on early labor market outcomes.

2.2 Institutional background on parental shocks

Implementation of our research design requires the correct identification of parental income shocks resulting from the loss of a stable full-time job. This section provides the necessary institutional background about involuntary job diplacements in Belgium.

Unemployment insurance for experienced workers. Experienced Belgian 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 computed as a percentage of the last job's gross salary,

days previously worked. Therefore, a young graduate who has worked on a regular employment contract for 2 month during his higher education, would start receiving unemployment benefits 7 month after graduation if he still has not found a job by that time. However, the waiting period would not be extended if the graduate partially worked during these 7 months, since all days (whether employed or unemployed) are taken into account during the waiting period. Note, however, that students jobs in Belgium are often undertaken under a special student contract with reduced social security contributions: days worked under such contract are not taken into account to shorten the waiting period.

[16] 155€ per month for a dependent.

subject to a guaranteed minimum and a maximum amount that depend on individual characteristics. The generosity of employment benefits decreases with the duration of unemployment because of lower replacement rates as well as lower minimum and maximum benefits. Specifically, the level and evolution of benefits over time depend on the family situation of the beneficiary with heads of household receiving higher benefits followed by single job-seekers who receive a lower amounts and non-head member of a households who benefit from 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-heads members of a household 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 unemployment benefits over time.

While complete rules cannot be detailed here because of their complexity[17], OECD data indicates that, in 2010, net replacement rates during the first 6 month of unemployment varied between 73% and 84% for minimum wage workers and between 42% to 62% for workers at 150% of the average wage. In the long term (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 very generous compared to the US, it is nonetheless far from insuring completely 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[18] During the period covered by our data, Belgium had very different employment protection rules for white- and blue-collar jobs (which each represents roughly 50% of jobs). Contrary to the situation in many European countries, firing white-collar workers in Belgium did not require any formal motivation or administrative procedure: employers could therefore get rid of white-collar workers without demonstrating a decrease in economic activity or insufficient performance by the worker. Workers also had no automatic right to monetary compensation in case of involuntary job separation. There was also no general administrative control of individual firing decisions. 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 months trial-period during which dismissal only required a 7 days notice, workers were eligible to 3 additional months of notice for every 5 years of seniority. This period could only be shortened with the agreement of the worker, for example if he found a new job before the end of the notice period.

[17] 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

[18] 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..

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 advanced notice period. For example, if the worker was entitled to a 6-months advanced notice, his or her employer would have to pay the equivalent of 6 months of salary in order to get rid of him or her immediately (this is usually made in one lumpsum payment). Such costly dismissals were nonetheless common in professions that rely heavily on the good faith of the employee in carrying his or her duties. This was the case, for example, for sensitive legal jobs where a badly intentioned employee can impose severe damages to the firm. In such cases, unemployed workers were not eligible for unemployment benefits during the period covered by those payment.

Rules applicable to blue-collar workers differ in two ways that are relevant for our investigation.[19] First, advanced notice requirements are usually much shorter: the maximum default is 4 months (120 days) after 20 years of seniority. Sectoral agreements with labor unions have somewhat increased these requirements in certain sectors but they remain much lower than for white-collar workers for the overwhelming majority of blue-collar jobs (only a few industries have firing notice requirement above 4 months even for workers with more than 20 years of seniority)[20]. In exchange for these short notices, employers were required to motivate their firing decision on individual or economic grounds. Failure to provide such a motivation, which is tightly controlled by the courts, resulted in a 6 months penalty in favor of the dismissed worker (which, however, often required a costly judiciary procedure).

Overall, based on OECD's employment protection index, Belgium had an average employment protection system compared to other developed countries, with an overall index of 2.00 in 2011 compared to an average of 2.13 across all OECD countries, 2.87 in Germany and 0.26 in the US. The existence of these employment protection regulations raises two questions.

First, advanced notice requirements imply that parents -especially those with long tenure - might have advance knowledge of their dismissal before the actual job loss. As a consequence, they might already restrict financial transfers to their child during the advanced notice period. In this case, children whose parents suffer from a shock after their entry into the labor force would already have been treated with an higher incentive to look for a job during their initial job search period. This would bias downward our estimates which rely

[20]Since 1999, the default schedule for advance-notice requirements among blue-collar workers was the following: 35 days between 6 month and 5 years of tenure, 42 days between 5 and 10 years, 56 between 10 and 15, 84 between 15 and 20, 112 above 20 years (national collective agreement #75 of Belgium's *Conseil National du Travail*.

[20]In addition, employment protection rules for all workers do not preclude immediate termination without compensation in case of grave misconduct ("*faute grave*"). Certain specific rules also apply to collective dismissals, in the form of additional procedures to be followed as well as, in some situations, additional severance pay. Finally, analogous to "wrongful-discharge" rules in the US, grossly unfair dismissals ("*licenciements abusifs*", such as, for example, dismissals based on racial or sexual discrimination) can in some cases, give rise to additional monetary compensation. Specialized attorneys which we contacted for this project all agreed that such compensation remains a rare event.

on comparing children whose parents experience a shock before the child's entry into the labor force with children whose parents suffer from a shock after entry. 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.

Second, financial compensation in case of job loss reduces the income losses resulting from an involuntary job dismissal. For example, a long-term employee could receive the equivalent of several months (sometimes more than a year) of earnings if her employer decides to ask her to leave the firm immediately rather than stay at her job during the advanced notice period. In such case, an employee who finds a new job at the end of the period covered by those payments would incur no loss ; he would even experience an income gain if he found a job before the end of this period (since he would receive compensation from his new job while receiving severance payments from his old job covering the same days of work). We address this concern directly by verifying that our results are robust to restricting the analysis to children whose parents receive no severance payment. In any case, this problem would also bias our estimates downward and our estimates would therefore represent a lower-bound of the true effect of parental income shocks.

2.3 Data

Implementation of our research design requires a large amount of administrative data in order to identify children-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 the first years of their career. Acces to such extensive data is made possible in Belgium by the *Labor Market Data-warehouse* (LMDW). Since 1998, this database aggregates data on all Belgian residents from a large number of governmental institutions. Merging of the different databases is made possible by the existence of a unique national identifier used consistently by all public institution in Belgium. We have been able to access data from a large number of institutions : demographic information from Belgium's national regitry, payment data from the family allowances administration, employment data from the central social security office and the pension registry, unemployment benefits history from ,

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 institution, data for parents is available from the beginning of 1998 to the end of 2011 and data for kids from 16 quarters before their entry into the labor force until the last quarter of 2011. In addition, we have access to parent's employment history for more than 10 years before the job loss shocks used in this papers. Appendix provide more details below about the different sources of our data.

National registry. National registry data allows us to identify children-parents relationships as well as several household characteristics. Belgium's national registry provide 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 to be registered by law and each change is followed by the personal visit of a member of the police forces. We also extract personal information on sex, age, and citizenship for both parents and kids as well as information of 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 us with 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 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 total labor income earnings at the end of the year. This data is therefore subject to extensive verifications and subject to 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 jobs 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 government employees and civil servants[21]. 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 system ("*SIGEDIS*"). It contains yearly data on sick and unemployed days as well as, for each employer-employee relationship, yearly worked days, total salary and basic job information (such as blue- and white- collar jobs). This database covers all

[21] 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.

private-sector salaried workers as well as contractual sectors from the public sector (i.e. it does not include tenured civil servants and self-employed individuals). This is however not a significant limitation to the extent that tenured civil servants are rarely, if ever, dismissed and will therefore seldom be subject to an job loss shock. The advantage of this database is that it contains at least 10 years of employment history before the job loss shock for every parent in our sample while the Social Security Registry only provides up to 4 years of history for the earliest shocks in our data (2002). We use this additional employment data to test the credibility of our identifying assumption.

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

Public Employment 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 throughout the entire 9-month waiting period during which they are not yet eligible for benefits (see previous section). We extract a monthly dummy variable indicating whether the individual has been registered with the agency in each month of our sample: this information is used to identified the timing of the child's entry into the labor force.

Social Security Agency for the Self-Employed. The national social security agency for self-employed workers (*INASTI: Institut National d'Assurance Sociale pour Travailleurs Indpendants*") provides information on individuals who are officially registered as self-employed. This data is used to determine social security contributions and tax payments and is therefore also subject to controls by the administration (although there is little disagreement that fraud is much more prevalent among self-employed individuals). We extract from this database a dummy indicating whether children are registered as self-employed during each quarter in our sample.

Pension and sickness databases. The pension database from Belgium's National Pension Office ("*Office National Des Pensions*") provides us with information on each public pension payment received by retired workers. We use this information to verify that the job-loss shock we identify 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 allows us to verify that the parental income shocks in our sample are not the result of a sickness episode.

Socio-economic status. In addition to variables that are directly extracted from each institutions'

database, the Labor Market Datawarehouse also contains variables that summarizes the information contained in all the available databases. In particular, we use the "socio-economic status" variables that provides information on the situation of the individual at the end of the quarter (employed, insured unemployed, uninsured unemployed, sick, retired, dependent inactive children,...). This information is used in the identification of job loss shocks to differentiate involuntary job separations leading to unemployment from other types of job separations.

3 Sample Selection

Our sample selection proceeds in three steps. 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. Finally, we identify parents suffering from a job loss shock in a 3-years window around their kid's entry into the labor force.

Identifying entrants. The first step of our selection process is to identify entrants into the labor force that is, children who put an end to their full-time education and either work immediately or start their career by an initial unemployment period. As we have seen in the previous section, eligibility for family benefits after the end of the compulsory schooling age is conditioned on the continuation of full-time studies. Our identification of entrants into the labor force therefore starts with the selection of all individuals who stop receiving family benefits between 2004 and 2008. Since family allowances stop at 25 years old even if the child is still a student, we restrict our sample to individuals who are younger than 25 in the first quarter for which they receive no family benefits. This constitutes the initial universe of entrants we consider.

However, as we have seen in the previous section, family allowance payments for unemployed labor market entrants can continue for as much as 9-month after the end of full-time studies (i.e. during the waiting period for unemployment insurance). An individual will therefore be observed as still receiving some family benefits during the waiting period either if he is unemployed during the entire quarter or if he is partially employed but earns less than the maximum allowed amount (520 euros in 2014) in every months of the quarter. One can thus not simply use the loss of family benefits to identify the exact timing of entry into the labor force. Fortunately, this problem can be addressed by using two additional sources of data at our disposal: namely, the employment registry and registration data from the regional public employment agencies.

Specifically, we use the following algorithm to determine the quarter of entry Q. We first identify the first quarter T for which the child is not receiving family benefits. We then look in quarters $T-4$ to T 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. Note that these are alternative conditions: individuals who are registered as unemployed in quarter T and working more than 66% of quarter $T+1$, will be considered as entering in quarter T (even if they are not registered as unemployed at any point in T+1).

The rationale for this algorithm is better understood by considering the different possible 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 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 unemployment agency in order to keep receiving family allowances during the U.I. waiting period and to be subsequently eligible for 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. 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), will be identified as entering in the first quarter after they graduate, since they meet the first criteria (employment) in the first quarter and the second criteria (registration as a job-seeker) in the second.

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, a significant portion of them might work during a large share of the summer between two academic years[22]. If we only took a single quarter into account, Those students who work for more than 2/3 of the summer would therefore meet the "employment criteria" and would be identified as entering the labor market at that point, 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 requering that at least one of the be met for two consecutive quarters, we avoid this problem. The choice of the T-4 to T period is motivated by the fact that the maximum UI

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

waiting period is 9 month so that some entrants can be observed as receiving family benefits at any point during the quarter for 3 quarters following entry and will therefore only be observed as not receiving family in the fourth quarter following entry.

Finally, it should be noted that this method will still lead, in some cases, to a slight (one quarter) mis-measurement in the timing of entry. This is the case, for example, for children's who 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 that quarter, they will only be registered as entering in the next quarter. We address this problem by excluding from both our treatment and control groups, children whose parent suffer from a shock during the estimated quarter of entry. We also test the robustness of our results to excluding a larger set of quarters adjacent to the estimated quarter of entry.

Identifying parents. We do not have access to a direct measure of filiation that would allow us to identify each child's parents. Instead, we rely on national registry data providing a unique household identifier as well as with the position of each individual in that household. Parents are therefore identified as the members of the child's household who are registered as "head" or "co-head". While this procedure will indeed select the child's parents in traditional households, it might also identify other figures such as the new partner of the parent who has main custody of the child in case of separated parents. Other relatives (such as brothers, sisters, grand-parents) will also be selected in cases where children do not live with their parents. However, this is not, in itself, a treat to our identification strategy to the extent that the individuals identified as parents are actually those supporting (even partially) the child. Since we focus our attention on "parents" who are not only registered as head or co-head but who have also had, at the time of the income shock, a full-time job for an extended period of time, this condition is likely to be met in our data.

In order 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 the end of the child's family allowances payments (that is, before the first date at which we identify a parental job loss).

Identifying job loss shocks. The last step in our selection process is to identify parents who are suffering from the loss of a stable full-time job in a 3-years window around the time of their child's entry into the labor force. A parent (father or mother) is identify as suffering from such a job loss in quarter T if (i) he or she 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 $T+1$ anymore, (ii) he or she has been registered has having a full-time job contract for at least 10 out of the last 12 quarters, (iii) he is registered at the end of T or $T+1$ as either receiving unemployment benefits and looking for a job or being in a unregistered activity according to the socio-economic status variable provided by Belgium's *Labour Market Datawarehouse*. 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 family allowances or benefit payments from the various unemployment, sickness, invalidity, workers' compensation, pension or welfare agencies.

The motivation for such a selection process is as follows. 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 employment history of all parents in our sample at the time of the shock[23]. Second, we only require the full-time condition (ii) to be met for 10 - rather than 12 - out of 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 our sample to involuntary income losses resulting from the dismissal of the worker and exclude job separations resulting from job-to-job transitions, extremely short unemployment 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, as a compensation for their immediate dismissal, severance compensation equal to the total salary to which they would have been entitled during the period covered by the legal advanced-notice requirement. As a consequence, these individuals will not be eligible for unemployment benefits for the entire duration of 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 therefore be recorded in the residual "unregistered inactivity" category of the socio-economic status variable. As we will show below, such cases of immediate dismissal are far from uncommon. Excluding those cases 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 : we therefore include this category in our definition of job loss shocks. However, this inclusion raises two questions.

First, individuals who voluntarily quit their job in Belgium are not initially eligible for unemployment

[23]The 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-years time window around entry - the first parental shocks are observed in 2001. This gives us a maximum of 3 years (2001-1998) of uncensored parental history for the first parents getting a shock in our sample.

benefits. Therefore, except if they are eligible for other types of social insurance (such as pension benefits or the residual social safety net), they will also appear initially in the residual "unregistered activity" category of the socio-economic status variable. 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: it is indeed possible that some parents could decide to voluntary leave the labor force following the entry of their child into the labor force. 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. In this is the case, there would be no reason to expect voluntary job quitters to reduce the level of their support to their child. Our estimation strategy would therefore not identify meaningful variation in the child's incentive to search resulting from a decrease in parental transfers. 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 families in the control group. In this cases, our estimates would therefore confuse the effect of increased job search effort by the child with the unobserved heterogeneity between, on the one hand, children from families in the treatment group (job separation before the child's entry) and, on the other hand, children from families in the control group (job separation after the child's entry).

Our main response to these criticism will be to test the robustness of our results in two different ways. First, we will show that the distribution of the number shocks around the time of entry is continuous, showing no sign of elevated levels of quits either closely before or after entry. There is therefore no evidence whatsoever that strategic timing of job loss by parents could bias our estimates. Second, since only involuntary job losers are eligible for both unemployment insurance and severance payment according to Belgian law, we will show that our results are robust to restricting our samples to parents who are observed as receiving either unemployment benefits or some form of severance pay upon job loss since eligibility for both legally requires an involuntary job separation.

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

Severance payments in case of immediate dismissal create another potential problem. In such case,

dismissed workers are not suffering from an immediate shock to their available cash on hand since their severance payment is equal to the salary they would have received during the advanced-notice period, had they been allowed to stay at their job during that time. Given that severance compensation is usually settled in one lump-sum payment shortly after dismissal, such workers might even encounter a positive shock to their liquid assets at the moment of their dismissal. As a consequence, one might argue that we should not expect a sudden reduction of parental transfers to their child.

Our response to this argument is two-fold. First, we will show that, while severance payments for immediate dismissals are quite frequent, they are usually limited in size (i.e. advance notice periods for parents suffering from immediate dismissal turn out to be rather small in our sample, resulting in relatively small severance payments). Second, we will show that the income loss shocks that we identify are very persistent leading to large long-term income losses for many parents in our sample. Therefore, while parents might not suffer from a direct reduction in available income, they do on average suffer from a very large reduction in their expected discounted income. Therefore, provided that they are forward looking, we would expect them to react immediately to the loss of a job by reducing spending today (including transfers to their child) in order to smooth the marginal utility of consumption going forward. Third, even if one does not find these arguments convincing, large severance payments will result in a downward bias in our estimates which would therefore constitute a lower-bound for the real effect of sudden parental income losses.

4 Descriptive statistics

After briefly discussing summary statistics (sub-section 4.1), sub-section 4.2 presents a quantitative description of children's entry into the labor force. In order to quantify the importance of the parental income shocks used in this paper, sub-section 4.3 provides a description of the labor market outcomes of parents in our sample following the loss of a stable full-time time job.

4.1 Summary statistics

Privacy laws in Belgium do not allow us to work with the entire universe of entrants available in the administrative data. However, in order to achieve a sufficient sample size, we have been allowed to extract a sample of roughly 70% of all entrants identified through our 3-step sample selection process. In addition, for comparison purpose, we have been able to extract a representative sample of roughly 6.5% of all labor market entrants identified between 2004 and 2008 without restricting the sample to children whose parent suffer from an income loss shock (i.e. we only apply the first two-step of the selection process).

Table 1 , panel A and B, presents summary statistics for a representative sample of all entrants (column

I) as well as for the sample of entrants whose parents suffer from job loss shock in a 3 (1) year(s) window around the child's entry into the labor force (column II and III respectively)[24].

All children are entering between 2004 and 2008. Given the strictness of our selection process, we end up with 6,845 (2,464) entrants whose parents suffer from a shock in a 3 (1) year(s) window around entry.

A few points are worth discussing. First, since we sometimes identify more than one child entering the labor market by family, the number of households in our data is inferior to the number of entrants in each sample. Second, as a result of the high repetition rate in Belgium's secondary and higher education system, children enter, on average, the labor force at a later age than would be expected based on overall graduation rates and normal cursus duration. For example, while more than 50% of individuals in recent cohorts do not graduate from higher education and would therefore be expected to graduate from higher school at age 18, less than 30% of children in all samples actually enters the labor force before 20. Third, there is much less than full employment across all samples. On average, entrants have a job between 8.5 and 9 out of the 12 quarters after entry. During that same period, they work on average between 438 and 470 days while they would have worked between 700 and 800 days if they had a full-time job during the entire 12 quarters. 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 unemployment. Fourth, as is to be expected from new workers without experience, labor market entrants receive a much lower wage than the average worker: on average, their wage is between the 27th and the 30th percentile of the wage distribution in Belgium.

Fifth, while we identify parents based on household composition data rather than direct information on filiation data, the age of parents in our sample strongly suggests that we have mostly identified biological parents. The average age for mothers in our sample of all entrants is close to 27 years old, which was the average mother's age at birth in Belgium when the children in our sample were born. While we do not have similar official stata for fathers, the average age at birth for fathers in our data (29.36) does not seem out of line and is consistent with the idea that males usually partner with slightly younger women. Figure 1 displays the distribution of fathers' and mothers' age at birth in our data ; for mothers, the figure also display the empirical distribution based on official filiation data based on the universe of births in Belgium in 1985 (the average year of birth for mothers in our data). Both distributions are largely consistent with the hypothesis that we mostly identify biological parents : strikingly, the distribution for mothers in our

[24]The 3 (1) years sample contains 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 all children whose parents are getting a shock on the exact quarter of the child's entry. Finally, we also exclude all children whose parents are getting more than one job loss shock in the time window considered (this constitutes a very small share of all entrants)

data is virtually indistinguishable from the official birth statistics in figure 1.

Finally, the job loss group presents several systematic differences with the sample of all entrants. First, for several variables, children in the job loss group seem to be selected from the 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, have a lower wage and, as a result of both, have a lower total income. Given the well-documented concentration of unemployment on less-educated individuals, one should not be surprised that children of parents who suffer from an involuntary job loss display less than average outcomes.

Other differences between the job loss group and the sample of all entrants are simply the mechanical consequence 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 is two parents in the households) 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 were also much more likely to be employed 16 quarters before entry (losing a stable job in a 3-years window around entry requires to have had such a stable job to begin with).

4.2 Children's entry into the labor force

Figure 5 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 the correct identification of the timing of labor market entry. This figure is interesting in its own right since it represents, to our knowledge, the first such description of the transition process between full-time education and active life.

Panel A, B and C show that that our identification strategy has succeeded at correctly identifying the timing of children's labor market entry. First, there is an increase in individuals having at least one job around what we identify as the time of labor market entry (Panel A)[25]. Moreover, while there are already around 20% of entrants who have at least one jobs several quarters before entry, Panel B reveals that these are overwhelmingly part-time jobs (average days worked at a job is around 20 per quarter, less than a third of a full-time job schedule), consistent with the idea that these are mostly student jobs. By 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. Further reassurance about our sample selection is provided by panel C: consistent with Belgian regulations described in section 2.1, we observe a clear jump in the number of individuals receiving

[25]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) mis-measurement in the timing of entry.

unemployment benefits 3 quarters after entry[26].

On a more substantive note, the first striking feature is the speed at which cohorts of labor market entrants reach their steady-state level of employment. Labor force participation, and total worked days per job, quickly increase around the time of entry. As seen in Panel A, nearly 80% of all entrants have at least one job in the quarter following their entry into the labor force and, while there is still a significant increase in the following quarters, there is only a very small increase from the 4th to the 24th quarters after entry. The same overall pattern hold up when looking at 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 end. U.I. benefits receipts then peak at 4 quarters after entry and quickly decreases afterward as labor market entrants progressively find a job. After 6 quarters, there is only a very limited decrease over the next 18 quarters. Overall, these findings depict a quite dynamic view of the labor market, even for a traditional European welfare state like Belgium suffering from a high rate of unemployment and with a level of employment protection largely above the U.S..

Two additional findings are worth mentioning. First, one can notice in Panel C 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 turn to the residual safety net for help. The take-up rate is however very limited, consistent with the means-tested nature of the benefits it provides. Second, consistent with existing literature on the returns to experience in the labor market, Panel D shows a very fast increase in wages over the first years after entry. Before their entry into the labor force, children are almost all concentrated in the bottom part of the income distribution (average wage percentile is less than 15), 4 quarters after entry they are still 20 percentile points below the median. Six years after entry they have closed more than half that gap with an average wage percentile above 40.

4.3 The Labor Market Outcomes of Parents After Job Loss

Figure 3 presents a graphical description of the loss of a stable full-time job by parents in our sample. This figure confirms that our selection procedure correctly identifies large negative income shocks for parents in our job loss group. It also allows us to quantify the magnitude of the income losses suffered by parents.

In line with our definition of the income 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

[26]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 that some individuals start receiving unemployment benefits before the third quarter after entry

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 of a full-time (100%) worker.

By contrast, labor supply and income both experience a sharp drop around the time of the shock with parents working on average less than 20% in the quarter following job loss while nearly 60% are receiving unemployment benefits and the remaining 20% are in a state of "unregistered inactivity" as defined in the previous section. 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 compensatory 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".

Perhaps the most striking feature of those graphs is the extreme persistence of the income shocks we identify. The rate of unemployment decreases very slowly after the shock and employment rates never come close to a full recovery. Three years after the shock, only about 50% of parents have a job (Panel A) and the average labor supply is still at less than 40% of that of a full-time worker. Moreover, even those who do find a job suffer from a large long term decrease in their hourly wage. Panel D shows that the average wage percentile was close to the median for most for the 12 quarters before job loss. After the job loss shock, the average wage percentiles for parents drops by nearly 10 percentile points and never recovers from this drop.

Figure 4 provides further insight by displaying total (real) labor earning by calendar year relative to job loss from 8 years before to 8 years after the shock. This figure reveals that the income shock we identify results in an average drop of roughly 50% of yearly labor income for parents suffering from a job loss shock[27]. This shock is also very persistent with no sign of recovery in following years (although this probably results in part from the progressive retirement decision of parents as they reach older ages). Overall, these findings point to a very large drop in income for parents who suffer from a job displacement in our data. Therefore, these shocks provide a valuable source of variation to identify the effect of reduced parental support on children's labor search effort at the beginning of career.

However, figure 3 also points to a potential problem for our identification strategy. While there is a clear drop in labor supply and income at the time of job loss, Panel B and D also reveal a marked decrease in average hours worked and hourly wage starting four quarters before job loss. Given the existence of extended advanced-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 stop being asked to work overtime,

[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 year 0 data point displays a much smaller drop in incomes.

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 discussion surrounding this project). On the other hand, since wages 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 have already decided to dismiss. In any case, this means that parents who are identified in our data as losing their job after the 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[28] 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 the level of financial resources available to labor market entrants during their initial job search episode. The ideal experiment would randomly change entrants' financial resources during their initial job search. 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 episode. Therefore, one can use variation in parents' ability to support their child as a substitute for the ideal experiment that we have just outlined. We use large and sudden variations in parental income resulting from the 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 selected from the bottom of the skill distribution. Therefore, given the well-document intergenerational correlation in ability, their child is also more likely to have lower than average skills and to display worse labor market outcomes. Simple comparisons of these children with children whose parents do not lose their job would therefore confound the effect of reduced parental transfers during the initial job search episode

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

with the effect 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 and are likely to be correlated between parents and children.

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 parent get a shock shortly after (our *control group*). The motivation for this approach is that, while it seems natural to think that there might be systematic differences between those children whose parents lose their job and those who do not, there is no reason to believe that there would be first-order differences between children who families both suffer from a similar income shock in a relatively short time window. In other words, our central identification assumption, whose plausibility we extensively document in our data, is that parents experiencing a job loss shortly before a child's entry (say, in the year prior to entry) do not differ in systematic ways from parents suffering from such a shock shortly after entry (say, in the year 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 - and therefore 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 made clearer by an example 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 (sixth month after A's and B's entry into the labor force). Despite their similar background, A and B face a very different financial environments at their time of entry into the labor force. Because of the incomplete income replacement provided by unemployment insurance, A's family suffers directly from lower disposable income and can therefore provide him or her with less financial support than B's family. Moreover, given that job displacements of older workers are associated with meager job finding prospects in Belgium, A's family A suffer from a very large drop in its discounted future income, an additional factor that might also lead to decreased family transfers. Our empirical strategy relies on the condition that differences in early employment outcomes between A and B are solely the result of changes in A's job search behavior resulting from decreased parental income in the first months of its job search. The main identifying assumption in this comparison is that the timing of a parental layoff is uncorrelated with unobserved heterogeneity in a child's labor market prospects. Hilger (2014) 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 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 initial job search period and β represent the (average) treatment effect of reduced parental transfers resulting from a job loss shock shortly before entry. For children whose parents suffer from a shock shortly before their entry into the labor force ($B_i = 1$), 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 the income shock suffered by parents had been perfectly insured and would therefore had no impact on his 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 been postponed 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 the time of entry (T small). Based on this assumption, our benchmark estimate $\hat{b}$ will simply be the average difference in the outcome variable of interest between children whose parent suffers from a shock in the year (4 quarters) prior to their entry in the labor force and children whose parent suffers from a similar shock in the year following entry:

$$\hat{b} = \bar{E}\left[y_{i,t_E,t_S}|t_S < 0, t_S \geq -4\right] - \bar{E}\left[y_{i,t_E,t_S}|t_S > 0, t_S \leq 4\right] \tag{3}$$

where $\bar{E}[.]$ is the sample average operator and time T is measured in quarters relative to entry. We chose a one year window in our benchmark specification (rather than the full 3 years of shock data in our sample) in order to minimize the risk of unobserved heterogeneity. In the rest of this paper, except where otherwise mentioned, the treatment group is therefore composed of parents experiencing an income shock 1 to 4 quarters before entry and the control group is composed of parents experiencing a similar shock 1 to 4 quarters after entry.

Although we cannot directly test identifying assumption 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 group, one would also expect to find significant differences in observable characteristics that are 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 group suffer from a job loss shock). In the following 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 our simple benchmark specification, we also test the robustness of our results to the addition of a series of covariates that appear as natural controls in our set-up. Specifically, we add observable characteristics to equation 1 and consider the following data generating process[29]:

$$y_{i,t_E,t_S} = \beta B_i + \tau_{y_E} + \gamma_{A,t_E} + \Gamma X_i + \gamma_{P,t_S} + \epsilon_{i,t_E,t_S} \tag{4}$$

where τ_{y_E} is a set of dummies for the year of entry, γ_{C,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, and household composition) and γ_{P,t_E} is a set of dummies for the age of the parent at the time of shock. Because our datasets 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 new 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 graduate from college or university while earlier entrants only have a high school education. 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 will have a limited impact on our estimated treatment effects.

The parental age at the time of the shock is the most important addition to our initial specification. The addition of this covariate implies that the treatment effect in equation 4 is solely estimated from the variation in parental age at birth. Since we control for age-at-entry and parental age-at-shock, $\hat{b}$ is practically estimated by comparing, all else equal, the labor market outcomes of children who were born when the parent suffering from the shock was one-year younger ("treatment group") to children who were born when the parent

[29]Much of this notation is borrowed from Hilger (2014)

suffering from the shock was one year 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 its 30th year of life (47-17) when they were born and part of the treatment group if he was in his 29th year of life at the child's birth.

An alternative identification strategy is to estimate $\hat{b}$ out of variation in the parent's age at shock, keeping parental age at birth. In this case, $\hat{b}$ would be obtained as 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 suffer from the job loss shock at a slightly different age. Formally, we implement the alternative identification strategy by estimating the following equation:

$$y_{i,t_E,t_S} = \beta B_i + \tau_{y_E} + \gamma_{A,t_E} + \Gamma X_i + \gamma_{P_f} + \gamma_{P_m} + \epsilon_{i,t_E,t_S} \tag{5}$$

where we have removed the parental age-at-shock ies and added both father's and mother's age-at-birth dummies (γ_{P_f} and γ_{P_m}). These two different identification provide yet another way to assess the validity of our identifying assumption by comparing if both procedures lead to similar results[30].

Theoretical predictions. Our research design rests on the double prediction that (i) parental incomes shocks shortly before entry result in decreased financial support to the children during the initial job search episode, (ii) reduced parental supports 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 the modeling of collective family decisions[31], it should be emphasized that these predictions are quite general and likely to hold under quite weak conditions.

First, to the extent that job loss shocks lead to a sudden reduction in disposable income and discounted future income, a simple balanced-budget argument will imply that parents suffering from such shock will be forced to reduce either their own consumption or the level of transfers to their children. Since financial transfers can always be alternatively affected to parental consumption, optimality conditions will usually involve a first-order condition equating the marginal benefit of an additional euro of transfer with the marginal utility of an additionnal euro of consumption for the parents. As long as both the parent's marginal utility of consumption and the marginal benefit of additional transfers are monotonically decreasing (both very common assumptions), optimality conditions will therefore require that the burden of the reduction be

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

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

shared by the two margins of adjustment.

Second, children usually have no or very little assets as well as limited access to other 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 increases the relative value of having a job. Mortensen (1977) implies that this should result in children searching more and lowerering reservation wages. 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. To the extent that parental transfers do not stop completely as soon as the child finds a job, one would also expect an increase in labor supply at the intensive margin following the loss of a job by the parents. If the labor supply elasticity at the intensive margin is positive, reduced transfers from the family should increase the marginal value of additional disposable income and should therefore be associated with an increase in hours worked while employed. The same holds true if children also provide financial assistance to their parents : in this case, increased transfers to the 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 our research design

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

A first concern is that parents might voluntarily select in our control or treatment groups. In particular, as we have discussed in section 3, one might expect second-earners (most of them females in our data) to leave the labor force voluntarily when their children does not need their financial support. In the spirit of graphical 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 in our sample relative to the time at which the child stops being eligible for family allowances[32], separately for male and female parents. In both cases, there is no evidence of bunching of shocks either before or after the end of family allowances payments. Moreover, apart from the quarter-to-quarter variation that is to be expected given our sample size, a simple linear regression provides an excellent fit for the evolution of the number of shocks over time. This graph therefore provides strong 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 each panel on figure 6 also reveals a slight negative linear trend in the number of

[32]As discussed in section ????, this represents the best approximation of the time at which children are expected to support themselves either through work or through the receipts of unemployment benefits

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 the parent's age at the time of their child's entry into the labor force). One might argue that such trend should lead us to worry that our result might be driven by systematic differences between parents' who get a shock before and after entry. We will address this concern directly below by testing for such differences using a large array of parent's characteristics. Furthermore, we will estimate all our benchmark results on the sample of children's whose parent 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 very mild, this will further reduce the risk of first-order systematic differences between the treatment (before) and control (after) group. Finally, as explained in the previous section, we will estimate our benchmark results using two different identification strategies relying, on the one hand, on variation in parents' age at the time of the shock and, on the other hand, on parents' age at birth.

An additional concerns 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 age at entry as a function of the timing of the parental shock, for each year relative to entry in a 3-years 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. Specifically, there is virtually no difference in the average age at entry for children suffering from a parental income shock in the 4 quarters prior to entry and for those who suffer from a similar shock in the 4 quarters following entry.

Finally, as discussed in the previous section, we further confirm the plausibility of our identifying assumption by testing for significant differences between our treatment and control group in an array of demographic characteristics and labor market outcomes of parents. Each row of column I (II) displays the sample average of 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 age at shock controls. In all but one case, there is no significant difference between the treatment and control group. Moreover, the addition of year and age controls do not alter this result, often changing the sign of the estimated difference.

The only significant difference concerns job tenure at the time of job loss with parents who lose their job prior to their child's entry displaying a lower level of tenure. 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 differences in the number of days worked or unemployed 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. As was the case for the child age at entry, there is very little difference in parental

labor market history across shocks that occurred 2 years before or after the child's entry.

Overall, these findings provide strong support for our identifying assumption : there is no reason to expect preexisting differences between children in our treatment (shock one year before entry) and control (shock one year after entry) group to affect the validity of our results.

7 The Effect of a Parental Job-loss on Child Outcomes

7.1 Main results

Having established the plausibility of our research design, we now turn to our central result. Table 3 displays the estimated treatment effect for four employment outcomes in the 12 quarters following entry[33]. The first two rows of the table display outcomes related to labor supply: the total number of days worked and the total number of quarters worked in the first 3 years after entry. The last two rows display results on total labor compensation and (median) wage percentile. Column I contains our benchmark results without any controls (equation 3) and each subsequent column adds additional controls building up to the more complex specifications 4 and 5 (in column V and VI). The final column of Table 3 displays the averages of the outcome variable in the control group for comparison purposes.

Across all specifications, we find statistically significant evidence that child labor supply increases as a result of parental income shocks in the quarters before entry. Across all specification, we find an average increase of approximately 25 (full-time equivalent) days worked in the 12 quarters following entry, representing an increase of roughly 6% in total labor supply given a baseline of around 427 days worked in the same period for the control group. The stability of the estimates across specifications is quite remarkable given that we add a large number of indicator variables (e.g. one dummy for each age at entry or parental age at birth) and that we use two different sources of variations to estimate our treatment effect: column 5 relies on variation on parental age at shock keeping parental age at birth constant while column 2 to 4 and column 6 uses variation in age at birth keeping parental age-at-shock constant.

The second row provides further confirmation of the previous result and shows that the increase in labor supply results, at least partially, from a response on the extensive margin. On average, children in the treatment group have a least one job in roughly an additional 1/3 of a quarter in the 3 years following entry corresponding approximately to a 3.5% increase compared to the baseline for the treatment group (9 out of 12 quarters).

[33]The choice of a 12 quarter window of observation is simply 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

In line with those results, the third row shows that the treatment group also display an average increase in total labor compensation of the same magnitude: 3.3% to 4.1% (€1,128 to €1,412 euros) compared to the baseline for the control group (€34,117). However, this effect is not significant due to much larger standard errors than for the two previous variables. By contrast, rows 4 indicates a very small decrease in the median wage received by workers during the first 12 month of our career. Our point estimate indicates that treated children's wage is on average less than .8 percentile point lower than non-treated children. On a daily basis, this represents approximately .75 euros per full day of work compared to a baseline of around €89 (estimated in 2011 euros that the baseline percentile in the last column) or less than than a 1%. We can exclude effects larger than -1.6 percentile points, equivalent to €1.5, with a 95% probability. Therefore, we can reject with strong confidence that there is an economically significant decrease in job quality, as measured by the median wage during the first 12 quarters. This also implies that the lack of a statistically significant increase in total compensation should not lead to the conclusion that there is no increase in total compensation. Rather, we read the results taken together as consistent with an increase in total labor compensation even if the large unexplained component of this variable in our data produces an imprecise estimate.

Figure 9 presents a graphical illustration of those results by displaying the average value of total days and quarter worked in the 12 quarters after entry as a function of the timing of the parental shock relative to the child's entry, for up to 3 years around entry. By contrast with previous graphs that showed no distinguishable difference in child's age at entry and parental outcomes, there is a clear jump in both total days and quarters worked for children suffering from the shock in the year prior to entry compared to those who suffer from a similar shock in the year after entry[34]

Overall, these results are consistent with a simple search-and-matching model of the labor market where job search effort (rather than changes in reservation job quality) is the main margin of adjustment for unemployed individuals looking for a job. The absence of a significant response on the job quality margin, and the vindication of a simple job-search effort model, is further confirmed by results presented in table 4 which presents results for a wider range of labor market outcomes estimated on the same sample. Column I and III display the simple average difference between the treatment and control group (equivalent column I of table Table 3) while column II and IV display the sample average for the control group.

The first 5 rows of Panel A provide a more detailed picture of the margins of adjustments resulting in the

[34]There is also a (smaller) difference between children getting a shock 3 years before entry and those getting the shock 1 or 2 years before entry. However, this is not a problem for our results since it can be explained by factors that do not after our benchmark estimates. Because of data limitations, shocks are only selected between 2003 and 2011. Therefore, we only observe shocks up to 2 years before entry for children entering in 2004. As a consequence, the sample of children whose parents get a shock 3 years before entry is, on average, composed of children who enter later in our sample (i.e.e between 2005 and 2008), at a time where average days and quarter after entry were, on average, lower. Such differences do not affect our benchmark estimates since (i) we concentrate on shocks happening in a one-year window around entry where this left-censoring selection bias is not present, (ii) in any case, we control for a full set of year-of-entry dummies.

increase in total labor supply during the 12 quarters following entry. First, there is no evidence of response along the intensive margin: the treatment for the number of days worked in a given quarter at a continuing employer is close to zero and even slightly, as well as insignificantly, negative (first row). Children in the treatment group do not seem to chose to work longer hours once they have found a job. Overall, there is also no increase in the total number of employers during the first 12 (second row) or even the first 4 quarters after entry (fourth row). These findings might at first appear surprising given that we do find an increase in total days worked over the same period. In fact, the third and fifth row reveal that the entire labor supply response is explained by a significant increase in the number of jobs in the first 2 quarters (third row) which the young workers then keep for the next 12 quarters resulting in a significant increase in average tenure observed at the end of this period (fifth row).

The rest of table 4 provides additional confirmation that the increase in labor supply is not the result of a decrease in job-seekers standard 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 Table 3 focused on the median wage observed in the 12 quarters following entry, we also do not find an effect both when looking at the first and the last wage observed during this period. The rest of table 4 investigates whether the increase in labor supply might the response of a decrease in job quality on other, more subtle, margins. Again, we do not find any significant effect on other measures of quality although some of our point estimates are quite 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 last (first) row of panel A (B) shows that children in the treated group do not seem display lower wage-growth during the first three years of their career. Second, blue (white)-collar jobs are a strong marker of low (high) quality employment in Belgium: not only do blue-collar earn less and have lower education on average but, as we have previously seen, they benefit from lower employment protection. However, parental shock before entry do not seem to result in an increase in the proportion of blue- or white-collar jobs at the beginning of careers (Panel B, row 3 and 4 respectively). Third, firm size has been widely documented to be correlated with higher quality, more productive jobs (). Again, we fail to find evidence of a significant decrease in firm size as a result of parental shocks before entry (Panel B, row 5). Fourth, young workers also do not seem to react to family shocks by self-selecting in lower paying industries or industries with low income-growth prospects (Panel B, row 6 and 7 respectively).

Overall, there is little evidence that earlier job finding causes worse match quality. These results also provides an additional validation of our research design. If higher labor supply in our treatment simply reflected pre-existing differences between the treament and control group, we would expect these differences

to also manifest themselves in other measures of job quality. The absence of such an effect further lends support to the hypothesis that we are indeed correctly identifying the effect of increased job search effort at the beginning of career.

Finally, extant literature has documented that family insurance often takes the form of children moving-back in their parents' house during unemployment episodes in the first years of their career (XXX, reference). Given that we observe a decrease in unemployment among our treated group, one might expect them to have a higher propensity to leave the parental household. Although our point estimate on the probability to leave the parental is consistent with this story (indicating a decreased of 2.3% in the probability to leave the parental households two-years of entry), our estimate is too imprecise to allow to us reach a definitive conclusion on this issue.

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

Previous results looked at aggregate employment outcomes over the 12 quarters following entry into the labor force. However, out set-up also allows us to identify the full temporal dynamic 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 the entry of a child. We estimate the effect of having a shock before entry for each k using the following equation:

$$y_{i,t_k,t_E,t_S,b} = \beta_{k,b} + \tau_{y_S,k} + \gamma_{Age,t_E,k} + \kappa_{Age,t_S,k} + \tau_{q,k} + Q_{q,k} + \epsilon_{i,t_k,t_E,t_S,b} \tag{6}$$

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. 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 also 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 fast changes in the first quarters after entry and there is, for example, no reason to expect covariates (e.g. age at entry) to have the same effect on total worked days in the 1st quarter after entry as in the 12th quarter after entry. Therefore, while we estimate equation 6 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 regression but slightly improves statistical power by taking advantage of the imperfect correlation between quarterly outcomes

Focusing on individuals whose parents suffer from a shock in a one-year window around entry, figure Figure 11 plots the time-varying treatment effects ($\beta_{k,b}$'s) for days worked during the quarter both for specification 6 (solid line) as well as for the simple quarterly difference in quarterly days worked between the control and treatment group (dashed line). As was already the case previously, the addition of the (large) set of covariates in 6 only affects marginally the point estimates. The main take-way from this table is that 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 career, rather than pre-existing differences between our treatment and control group. Indeed, the labor supply between the two groups is identical in the two years preceding entry, a moment at which a significant number of individuals already held part-time student jobs (see section). By contrast, the control group starts working more around the time of entry with a maximum treatment effect of close to 4 more days of work in the second quarter following entry. Although there is a significant amount of quarterly variation, this effect progressively decreases and, from the fourth year after entry onwards, the point estimates remain consistently close to zero[35]. Together with the previous results, figure 11 paints a picture that is perfectly 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 and, as a result, get such a job a little bit earlier than children who are not under the same financial pressure. As time passes, the effect of this initial job search effort disappear as members of the control group progressively find a job. These findings also provide an additional validation of our research design. If our results were driven by spurious heterogeneity, one would not expect the treatment effect to be concentrated in the first quarters of the child's career.

7.3 Robustness

In table 5, we test the robustness of our main results to two additional factors.

Panel A shows that our main results are not affected by the extension of the time-window of observation for parental shocks around the child's entry into the labor force. Previous estimates were limited to the sample of children whose parents experience a shock in a one-year window around entry (first row). Extending the window of observation to two-years around entry more than doubles the sample size, increasing the level of statistical significance with little effects on the magnitude of the point estimates. Increasing the window to 3-years around entry (third row) further increases the statistical significance of the results with a limited increase in the magnitude of the labor supply response. Overall, the robustness of both our qualitative and

[35]The increase in standard errors, 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

quantitative results to the addition on this large new set of observations, representing in effect the addition of a completely different sample, provides additional reassurance that our results are not solely driven by luck. Another potential issue (discussed in section 4.3), concerns the downward bias in our estimates that could result from parents being aware of their dismissal several quarters before the observed job separation. The fourth row of Panel A tests for this possibility by restricting the sample to children whose parent suffer from a job-loss shock from 8 to 5 quarters before entry (treatment) and from 5 to 8 quarters after entry (control). Results are again qualitatively similar showing that anticipation effects do not seem to be a significant problem in our set-up.

Panel B test the robustness of our results to tighter definition 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 quitter. Consistent with this hypothesis, the first and second row of table 5 indeed show that most of the effect that we identify is concentrated on individuals who transition to unemployment insurance. Moreover, the last three rows of panel B 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. One should therefore keep in mind that our benchmark estimates are probably a lower bound estimate of the real treatment effect of involuntary job-loss shocks.

7.4 Heterogeneous effects

This section investigates how the average treatment effect that we have identified varies as a function of entrants' characteristics.

Table 6 shows results for our main variables of interest, broken down along the set of observables margins that we have found to produce consistently large differences in treatment effects . Each column of table 6 presents our benchmark treatment effects estimated on a restricted sample selected on the basis of the indicated child or parent's characteristics. Due to the limited sample size, test for significant differences between the estimated treatment effects across subsamples are usually insignificant (and are therefore omitted from the table). Nonetheless, the point estimates suggest very different effects across a four margins.

First, the entirety of the increase in labor supply seems to be concentrated in the sample of male entrants: for example, the estimated treatment effect for women's days worked is small and insignificant compared to men (column I and II: 7.9 days for female compared to 41.8 for males). Second, the increase in total days worked is roughly twice as large for children who enter the labor market between 18 and 20 years old

compared to children who enter between 21 and 25 years old (column III and IV). Third, the increase in labour supply is concentrated on child whose parent's lost a low-wage job (i.e. a job in the lower two-thirds of the wage distribution) with no sign an increase in total days worked in the top third of the wage distribution (column V and VI). Fourth, among households where both parent held a salaried job at the moment of the job loss shock, the increase in total days worked is also concentrated among children whose other parent (i.e. the one that does not suffer from the shock) has a low-wage job (column VII and VIII).

Our first finding might appear puzzling given that the existing literature has traditionally found higher labor supply elasticities for women.[36] Our results and those in the literature can be reconciled because the women in our sample are typically between 18 and 23 years old — ages at which most Belgian women do not yet have children or husbands. However, the high labor supply elasticities of women have typically been explained by the fact that women take a disproportionate role in child care and that, even in families without children, men are often considered the main earners. Our results imply that womens' labor supply elasticities at the beginning of their careers have even fallen below those of males, at least for low-skilled workers for which we find a significant effect. A possible explanation for this result is that that women might have a higher level of work ethic (either socially acquired or genetically inherited) and that they therefore might display a high level of job search effort independently of financial incentives. Although highly speculative, this hypothesis is at least consistent with the fact that women in Belgium, as in most other developed countries, currently achieve higher levels of education and display a higher attachment to the labor force in the first years of their career then men. While our lack of statistical power does not allow us to further investigate this issue, the finding highlights the need for further research on this subject.

The second, third and fourth results lend themselves to at least two competing explanations. First, it might be that the effect of job search effort on early labor market outcomes only matter for younger (less educated) entrants who, on average, suffer from the highest risk of unemployment. This hypothesis would be consistent with the general finding, in the existing literature, of a higher labor supply elasticity among low-income workers. The finding of a lower treatment effect among children of low-income parents might, in this case, might only reflect the fact that children entering the labor market at an earlier age come, on average, from more disadvantaged families. However, another explanation might be that the reduction in parental transfers following a job-loss shock is lower in high income families either because those shocks are less persistent or because high income parents, who also have on average more assets, have more opportunity to (self-)insure. Table 7 attempts to discriminate between these two explanations by presenting results from multivariate regressions that interact the treatment effect variable with indicator variables for younger

[36] Although recent studies tend to find increasing elasticities for women, converging toward estimates for men.

entrants (18 to 20 years old) and low-wage parents. Unfortunately, in addition to the fact that our limited sample size prevents us from reaching statistical significance, the results are not consistent across dependent variables. While estimates for total days worked and the number of quarters with a job point to a predominant role for parental income, results for total salary point to the opposite conclusion. Therefore, we have to leave this question open for further research.

8 Interpretation and conclusion

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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	6,845	2,464	*# of parents*	99,251	12,122	4,465
Males	50.2%	50.2%	49.0%	*# of households*	43,794	5,740	2,113
Belgian citizen	97.2%	96.1%	95.5%	*Two-parents family*	78.8%	89.2%	88.9%
Age at Entry				*Single parent family*	21.2%	10.8%	11.1%
18-19	26.3%	30.9%	32.5%	*Single Father*	18.5%	35.0%	33.7%
20-21	28.1%	29.5%	29.5%	*Single Mother*	81.5%	65.0%	66.3%
22-23	29.0%	25.4%	25.3%	*Father age at birth*	29.36	28.45	28.14
24-25	16.5%	14.2%	12.7%		(5.62)	(5.25)	(4.95)
Labour market outcomes in 12 quarters after entry				*Mother age at birth*	26.89	26.14	25.97
Days worked	460.1	447.4	438.1		(4.65)	(4.60)	(4.44)
	(252.1)	(247.2)	(251.0)	*Children in household 16 quarters before entry*			
Quarters with a job	8.70	8.67	8.58	*1*	20.1%	23.1%	21.1%
	(3.51)	(3.44)	(3.51)	*2*	41.9%	40.2%	39.9%
Total salary (in euros)	37,867	35,415	34,434	*3 or more*	38.1%	36.7%	39.0%
	(24,024)	(22,778)	(22,866)	*Working 16 Q. before entry*			
Average wage percentile	29.79	27.64	27.07	*Father*	84.8%	95.2%	95.8%
	(18.08)	(17.23)	(16.98)	*Mother*	57.8%	81.8%	83.8%

Notes: This table displays summary statistics for the samples used in the paper. 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. Labour 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 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. All variables relating to family composition are measured 16 quarters before the child's entry into the labor force. Father's and mother's age at birth refer to the age of the parent at the time of birth of the entering child. *Working 16 Q. before entry* is a dummy equal to one if the parent had at least one job 16 quarters before the child's entry into the labor force. All statistics are averages taken over all individuals in the sample (standard deviation are in parenthesis when relevant).

Table 2: Balance Tests - Parental Demographics and Employment History

	Average by shock time		Before/After Difference	
	Before entry (I)	After entry (II)	Simple difference (III)	Year and age controls (IV)
Age at shock	44.515	44.332	0.183	
	(0.160)	(0.177)	(0.235)	
Single parent	0.098	0.104	-0.006	0.001
	(0.009)	(0.010)	(0.013)	(0.015)
Head of household	0.759	0.772	-0.014	-0.027
	(0.013)	(0.013)	(0.018)	(0.021)
# of kids	2.437	2.415	0.022	0.023
	(0.036)	(0.041)	(0.053)	(0.064)
Median wage in last 3 years	48.915	49.175	-0.260	0.010
	(0.752)	(0.774)	(1.066)	(1.176)
Employer size	4.777	4.896	-0.118	0.003
	(0.079)	(0.085)	(0.114)	(0.128)
Blue Collar job	0.667	0.676	-0.009	-0.035
	(0.014)	(0.015)	(0.021)	(0.023)
Tenure (in years)	7.713	8.103	-0.390***	-0.276**
	(0.083)	(0.082)	(0.115)	(0.133)
Days worked in last 10 years (percentile)	57.173	57.617	-0.443	-0.063
	(0.669)	(0.692)	(0.950)	(1.042)
Days receiving unemployment benefits in last 10 years (percentile)	63.809	64.361	-0.552	-0.647
	(0.386)	(0.417)	(0.559)	(0.624)
Median wage in last 10 year (percentile)	52.539	53.304	-0.765	-0.158
	(0.716)	(0.734)	(1.014)	(1.099)
Total compensation in last 10 year (percentile)	59.511	60.460	-0.949	-0.090
	(0.635)	(0.649)	(0.899)	(0.962)

Notes: This table tests for differences in average demographic caracteristics and employment history between parents in our treatment and control groups. The treatment group includes parents experiencing the loss of a stable full time job in the 4 quarters prior to their childs' entry into the labor force. The control group includes parents experiencing a similar shock in the 4 quarters after the child's entry. Each row of column I (II) displays the sample average of 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 coefficient on a dummy equal to one for the treatment group from a regression of the variable of interest that also controls for a full set of year-of-shock and age-at-shock dummy. 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 variable in the same year for parents in our representative sample of all entrants (see table 1). *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: Main Results

	Estimated treatment effect for main outcome variables						Baseline Average
	(I)	(II)	(III)	(IV)	(V)	(VI)	
Dependent variable							
Total Days Worked	24.476 **	25.358 **	26.299 **	25.562 **	24.242 **	25.102 **	427.067
	(10.736)	(10.420)	(10.567)	(10.487)	(10.323)	(10.444)	(7.812)
Quarters with a job	0.318 **	0.322 **	0.355 **	0.342 **	0.309 **	0.319 **	9.015
	(0.160)	(0.158)	(0.160)	(0.159)	(0.157)	(0.159)	(0.116)
Total Salary	1,128	1,254	1,392	1,412	1,223	1,371	34,117
	(983)	(912)	(926)	(924)	(908)	(918)	(715)
Wage percentile	-0.782	-0.696	-0.605	-0.582	-0.660	-0.500	27.617
	(0.750)	(0.690)	(0.699)	(0.697)	(0.691)	(0.686)	(0.547)
# of observations	2,185	2,185	2,185	2,185	2,185	2,185	
Controls							
Age at entry		Yes	Yes	Yes	Yes	Yes	
Parental age at shock			Yes	Yes		Yes	
Year of entry				Yes	Yes	Yes	
Parental age at birth					Yes		
Demographic controls						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 (I) - (VI) 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 in the sample of labor market entrants whose parents experience the loss of a stable full-time shock in the 4 quarters before or after entry. We exclude those children whose parent lose their job during the quarter of entry. We also exclude the (few) cases where both 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 contribution 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. All controls include a full set of dummy variables for each value of the covariate. Demographic controls are gender, nationality (Belgium, E.U. 15, Other U.E., Other countries, unknown) and family type (married two-parents, non-married two-parents, single parent). Family type is measured 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 4: Effect on Additional Child 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	*-0.188*	*-0.127*	*51.116*	*Quaterly wage growth (in perc.)*	*-0.114*	*-0.107*	*1.224*
	(0.811)	*(0.809)*	*(0.596)*		*(0.089)*	*(0.089)*	*(0.065)*
# of employers	*-0.026*	*-0.044*	*2.925*	*Quarters with white collar job (%)*	*0.009*	*0.013*	*0.563*
	(0.086)	*(0.086)*	*(0.063)*		*(0.020)*	*(0.016)*	*(0.015)*
# of employers in first 2 Q.	*0.075* **	*0.083* **	*1.393*	*Quarters with blue collar job (%)*	*-0.006*	*-0.011*	*0.447*
	(0.035)	*(0.036)*	*(0.026)*		*(0.020)*	*(0.016)*	*(0.015)*
# of employers in first 4 Q.	*0.011*	*0.005*	*1.801*	*First employer size*	*-11.529*	*-11.467*	*910.818*
	(0.049)	*(0.049)*	*(0.036)*		*(41.119)*	*(41.509)*	*(29.985)*
Maximum tenure	*0.368* **	*0.404* **	*7.239*	*Average industry wage (perc.)*	*0.133*	*0.245*	*28.788*
	(0.147)	*(0.145)*	*(0.108)*		*(0.386)*	*(0.381)*	*(0.282)*
First wage (percentile)	*0.730*	*0.979*	*23.046*	*Mean industry wage growth*	*-0.001*	*-0.002*	*0.858*
	(0.750)	*(0.719)*	*(0.546)*		*(0.011)*	*(0.011)*	*(0.008)*
Last wage (percentile)	*-0.778*	*-0.550*	*32.053*	*Live with parents 8 Q after entry*	*-0.025*	*-0.015*	*0.486*
	(0.895)	*(0.827)*	*(0.652)*		*(0.021)*	*(0.021)*	*(0.016)*

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 shock in the 4 quarters before or after entry. We exclude those children whose parent lose their job on the quarter of entry or get more than one job-loss shock in the 4 quarters around entry. Column I and II displays the coefficients 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. 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-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 4 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* is a measure of the average 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. *# of employers* is the total number of different employers for which an individual has worked, even for a 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 (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 3. *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 a 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 5: Robustness of Results

	Dependent variable												
	Total Days Worked		Quarters with a job		Total Salary		Wage percentile		# of obs.				
	(I)		(II)		(III)		(IV)						
Panel A. Year of parental shock relative to entry date													
One year around entry	*25.102*	**	*0.319*	**	*1,371*		*-0.500*		*2,185*				
	(10.477)		*(0.160)*		*(921)*		*(0.688)*						
Up to two years around entry	*25.257*	***	*0.285*	**	*1,552*	**	*-0.022*		*4,227*				
	(7.508)		*(0.115)*		*(658)*		*(0.502)*						
Up to three years around entry	*28.524*	***	*0.310*	***	*1,920*	***	*0.401*		*5,551*				
	(6.607)		*(0.101)*		*(580)*		*(0.443)*						
Two years before vs two years after	*20.629*	*	*0.262*		*1,170*		*-0.406*		*2,077*				
	(10.704)		*(0.162)*		*(929)*		*(0.729)*						
Panel B. By type of job-loss transition													
To Unemployment Insurance (U.I.)	*33.086*	***	*0.470*	**	*1,596*		*-0.838*		*1,602*				
	(12.628)		*(0.192)*		*(1,140)*		*(0.871)*						
To unregistered activity	*2.472*		*-0.077*		*28*		*-0.548*		*583*				
	(20.220)		*(0.282)*		*(1,928)*		*(1.462)*						
To U.I. or receives severance pay	*31.196*	***	*0.403*	**	*1,730*		*-0.584*		*1,924*				
	(11.495)		*(0.172)*		*(1,052)*		*(0.801)*						
Receives severance pay	*11.913*		*0.141*		*236*		*-1.633*		*790*				
	(18.047)		*(0.267)*		*(1,659)*		*(1.240)*						
To U.I., no severance pay	*44.691*	***	*0.587*	***	*2,777*	**	*0.143*		*1,134*				
	(14.910)		*(0.225)*		*(1,361)*		*(1.049)*						

Notes: This table presents robustness tests for the treatment effect of a parental shock before entry on the main employment outcomes of interest. Dependent variables are defined as in table 3. For each dependent variable and specification, the table displays the coefficient on an indicator variable corresponding to whether the entrant experienced a parental job-loss shock before entry. Panel A presents results estimated on different time windows of observation around entry for parental job-loss shocks. For each estimate, the regression includes controls for age-at-entry, parental age-at-shock, year-of-entry and demographic controls. For comparison purposes, the first row displays the baseline result for shocks happening one year around the child's entry into the labor force. The second and third row extends the sample to shocks taking place respectively in a 2 or 3 years window around entry. Panel B restricts the sample to different types of parental job-loss transitions based on parental observed outcome. 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 restrict the sample to parent 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*) restrict 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. The fourth (fifth) row is estimated on the sample of parents who either transit to unemployment insurance or receive severance pay (transit to unemployment insurance and do not receive severance pay). Standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 6: Heterogeneous Treatment Effects of Parental Job-loss Shock

Dependent variables	**By sex of entrant**		**By age at entry**	
	Male	**Female**	**18-20**	**21-25**
Panel A	(I)	(II)	(III)	(IV)
Total Days Worked	*41.737*** *	*7.858*	*35.161** *	*18.511*
	(15.269)	*(15.089)*	*(15.133)*	*(14.530)*
Quarters with a job	*0.452** *	*0.185*	*0.333*	*0.333*
	(0.226)	*(0.225)*	*(0.242)*	*(0.208)*
Total Salary	*2,305*	*-4*	*2,158** *	*698*
	(1,403)	*(1,378)*	*(1,178)*	*(1,406)*
# of obs.	*1,079*	*1,106*	*1,027*	*1,158*
	Wage percentile of the lost job		**Spouse's wage percentile**	
	0-66	**67-100**	**0-66**	**67-100**
Panel B	(V)	(VI)	(VII)	(VIII)
Total Days Worked	30.515**	0.728	26.293	-6.513
	(12.284)	(21.537)	(16.029)	(23.386)
Quarters with a job	0.354*	0.170	0.367	0.200
	(0.186)	(0.310)	(0.232)	(0.300)
Total Salary	1,092	526	1,219	487
	(1,082)	(2,118)	(1,426)	(2,379)
# of obs.	1,652	533	887	365

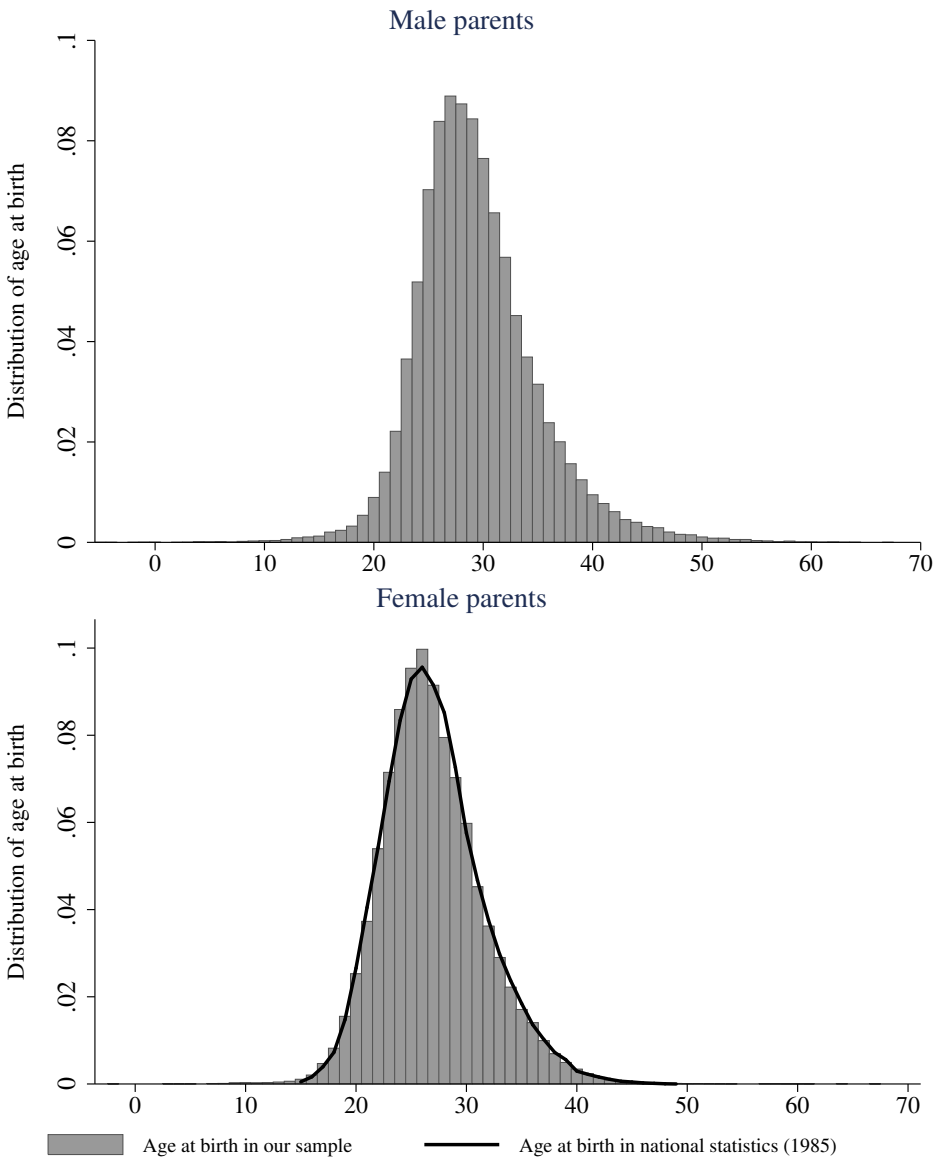
Notes: The table presents our benchmark estimates of the treatment effect of a parental job-loss shock before entry for sub-samples of entrants. For each variable of interest, the table displays the average difference between the treatment and control group in our data. Column I displays the baseline estimate for the full sample (similar to column I of table 3). Column II and III restrict the sample of male and female entrants respectively. Column III (IV) restricts the sample to children entering the labor market between 18 and 20 (21 and 25) years old. Columns VI, VII and VIII restrict the sample to children whose parent lost a job with the wage in the indicated percentile of the overall wage distribution. The wage of the lost job is measured two quarters before the job-loss shock according to the procedure described in Table 1. The same method is applied in column IX and X except that the sample is selected based on the wage of the other parent (i.e. the spouse that does not suffer from a job loss shock). In this case, the sample in naturally restricted to households were both salaried workers at the moment of the job loss shock. All regressions are estimated on the sample of labor market entrants whose parents experience the loss of a stable full-time shock in the 4 quarters before or after entry. We exclude those child whose parent lose their job on the quarter of entry or get more than one job-loss shock in the 4 quarters around entry. Dependent variables are defined as in table 3. Standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 7: Heterogeneous Treatment Effects: multivariate regressions

	Dependent variable			
	Total Days Worked	Quarters with a job	Total Salary	Wage percentile
	(I)	(II)	(III)	(IV)
Treatment effects				
Baseline treatment effect	*0.463*	*0.196*	*474.431*	*-0.059*
	(21.762)	*(0.330)*	*(1922.786)*	*(1.472)*
*Before * Low-wage job lost*	*26.739*	*0.204*	*124.764*	*-0.785*
	(25.532)	*(0.387)*	*(2255.891)*	*(1.725)*
*Before * Enters at 18, 19 or 20*	*10.445*	*-0.047*	*1502.429*	*-0.039*
	(21.932)	*(0.332)*	*(1937.801)*	*(1.483)*
Baseline estimates				
Baseline (constant)	*511.876****	*9.639****	*44714.648****	*34.770****
	(16.064)	*(0.243)*	*(1419.338)*	*(1.087)*
Low-wage job lost	*-46.425**	*-0.337*	*-4996.658***	*-3.462***
	(18.766)	*(0.284)*	*(1658.107)*	*(1.266)*
Entry between 18 and 20	*-106.850****	*-0.795***	*-1.47e+04****	*-9.846****
	(15.949)	*(0.241)*	*(1409.173)*	*(1.079)*
N	*2185*	*2185*	*2185*	*2062*

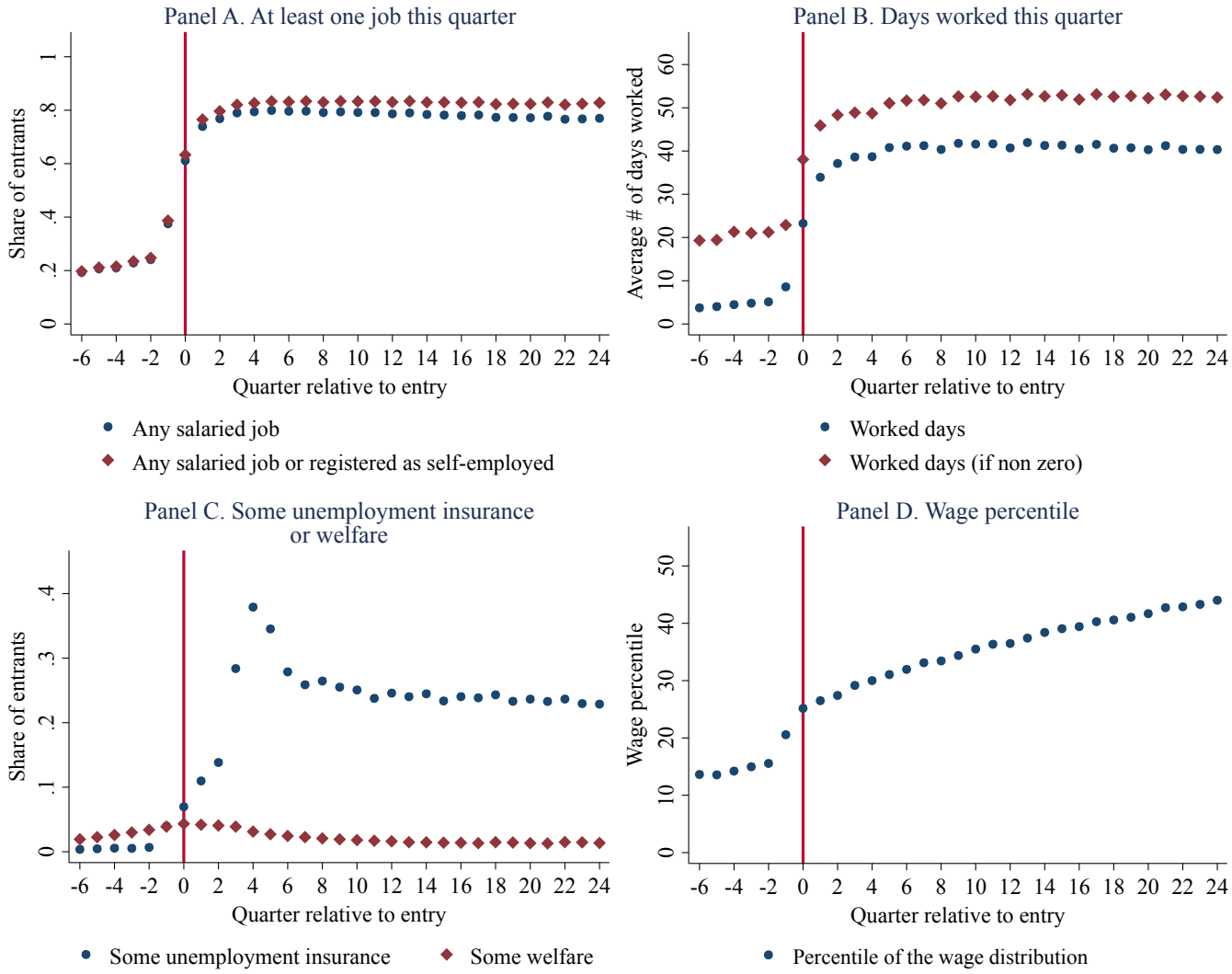
Notes: The table investigates how the treatment effect of parental job-loss before entry varies with the child's age at birth and the wage level of the parent at the lost job. For each variable of interest, the baseline treatment effect is identified for parents with children entering between 21 and 25 years old and whose parent lost a job for which they received a wage in the top third of the wage distribution. The baseline treatment is 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 (*Before*). The difference in average treatment effect for children entering earlier and parent losing a low-wage job are identified respectively by interacting the *Before* dummy with indicator variables for children entering at 18, 19 or 20 years old and parents losing a job in the bottom two thirds of the wage distribution). Regressions also include a constant (*Baseline average (constant)*), an indicator variable for all children entering between 18 and 20 years old (*Entry between 18 and 20*) and an indicator variables for children whose parent lose a job in the bottom two third of the wage distribution (*Low-wage job loss*). 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 are estimated on the sample of labor market entrants whose parents experience the loss of a stable full-time shock in the 4 quarters before or after entry. We exclude those child whose parent lose their job on the quarter of entry or get more than one job-loss shock in the 4 quarters around entry. Dependent variables are defined as in table 3. Standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01

Figure 1: Parental Age at Birth



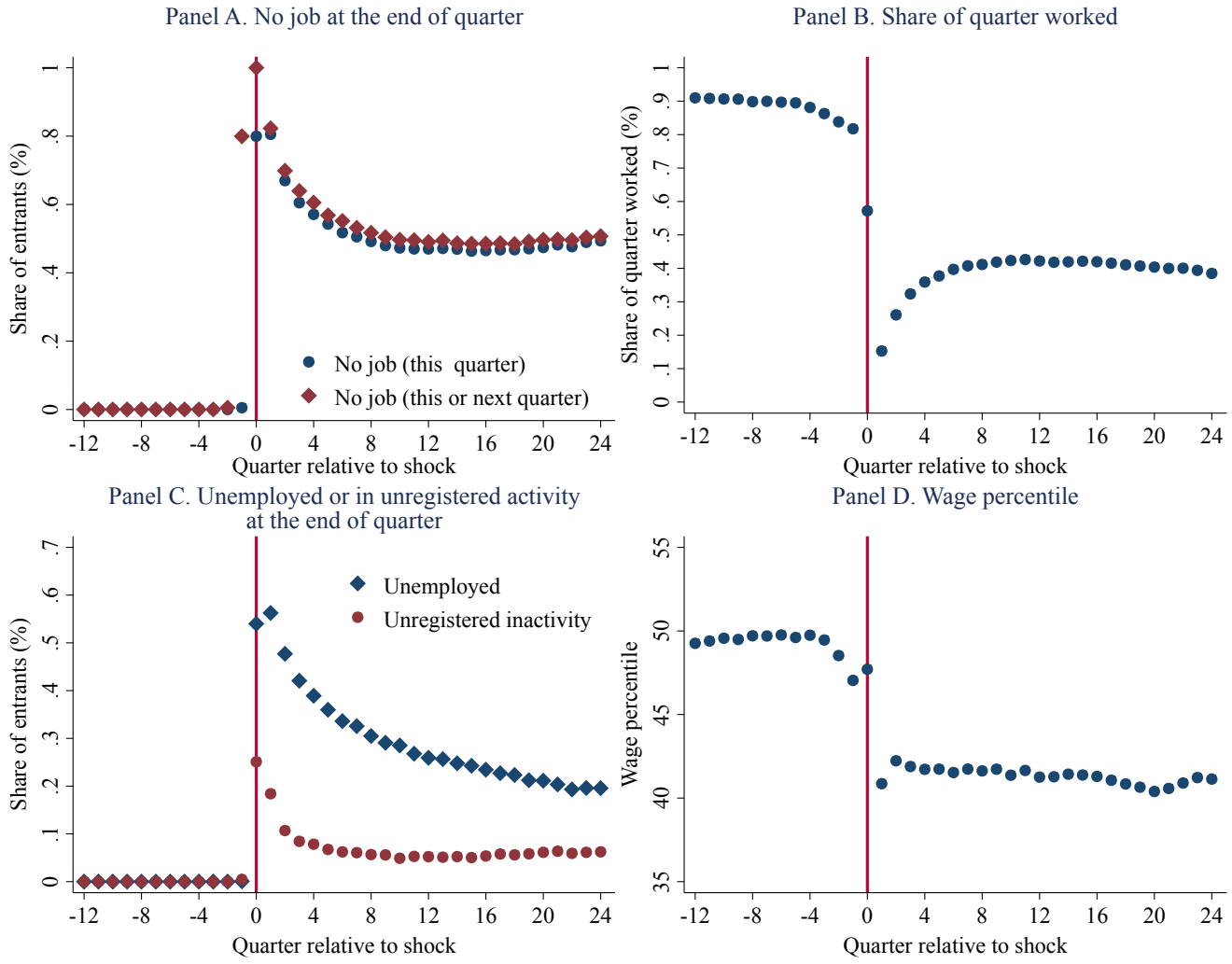
Notes: This figure displays the distribution of parental age at birth for male and female parents in our data. Age at birth is computed as the age of the parent on the December 31 of the child's year of birth. The average year of birth in our data is 1985. For comparison purposes, the graph for female 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



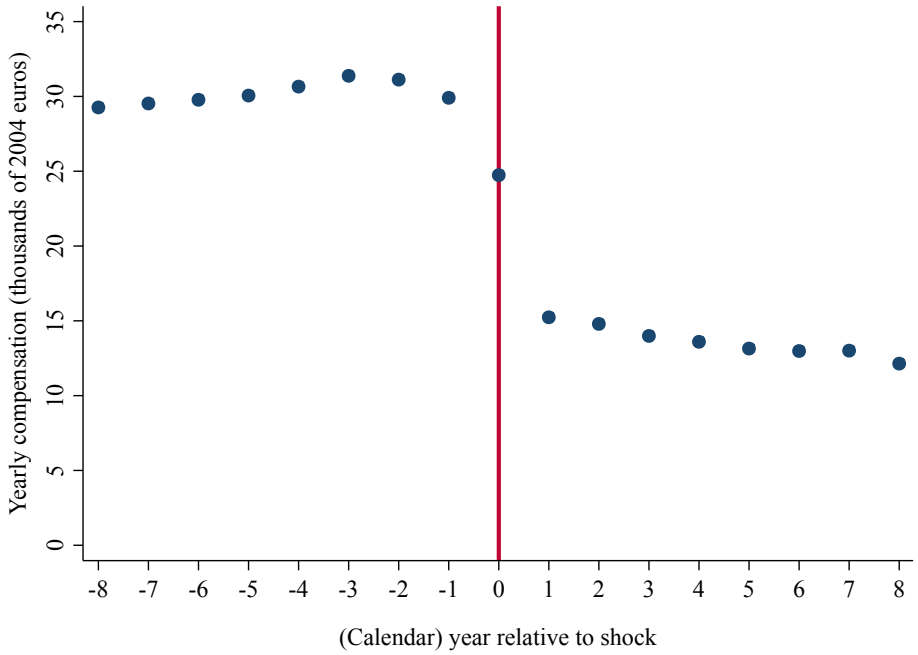
Notes: Each panel of this figure displays the evolution of a different labor market outcome variables by quarter relative to labor force entry for the pooled sample of entrants in Belgium between 2004 and 2008. Individual labor market outcomes are observed until the last quarter of 2011: Later entrants disappear from our sample at some point after the 12th quarter following entry (using a balanced sample of the earliest entrants brings similar results). 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. Individuals in Panel A are considered as having a job if they work at any point during the quarter. Days worked in Panel B represents the sum of all full-time equivalent days worked as a salaried worked 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 percentiles 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 outcome variables by quarter relative to job loss for the sample of parents suffering from the loss of a stable full-time job as defined in the sample selection section of the paper. 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. However, using a balanced sample (with parents getting the shock before 2005) brings similar, albeit noisier, results. Quarter 0 refers to the last quarter during which the parent works at the lost job (even for a single day). The share of quarters 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 refsec:parsinstetting of the paper. The wage percentile in Panel D is obtained by dividing total labor earning 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 of the individual wage percentiles for parents in our sample.

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



Notes: This figure shows the sum of all labor earnings by calendar year relative to shock for parents in our job loss group. Year zero refers to the calendar year of the job loss shock. Yearly salaried income 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 self-employment income or statutory civil servants). Yearly income is expressed in 2004 euros using Belgium's official Consumer Price Index and is winsorized at the 99th percentile. Parental shocks are observed between 2003 and 2010 and income data between 1990 and 2010. Therefore, the sample is unbalanced and most parents are not observed for the full 6 years after job loss. However, using a balanced sample of parents for which the full work history is observed brings similar, albeit more noisy, results.

Figure 5: Identification Strategy

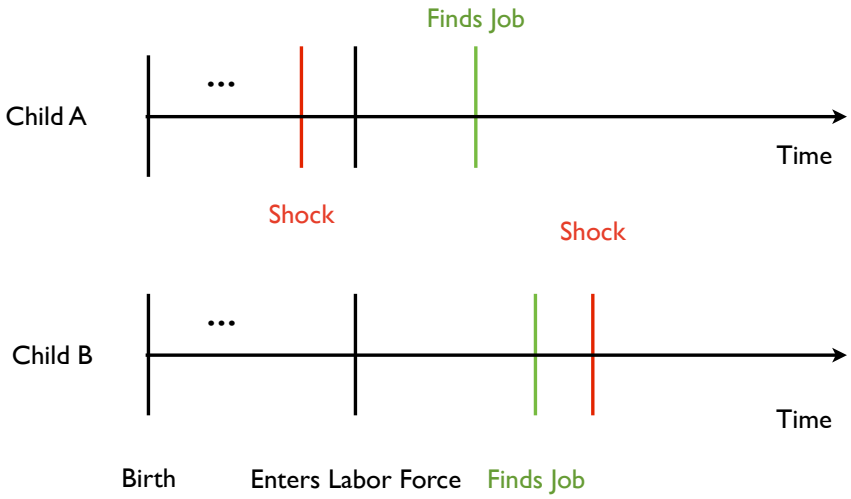
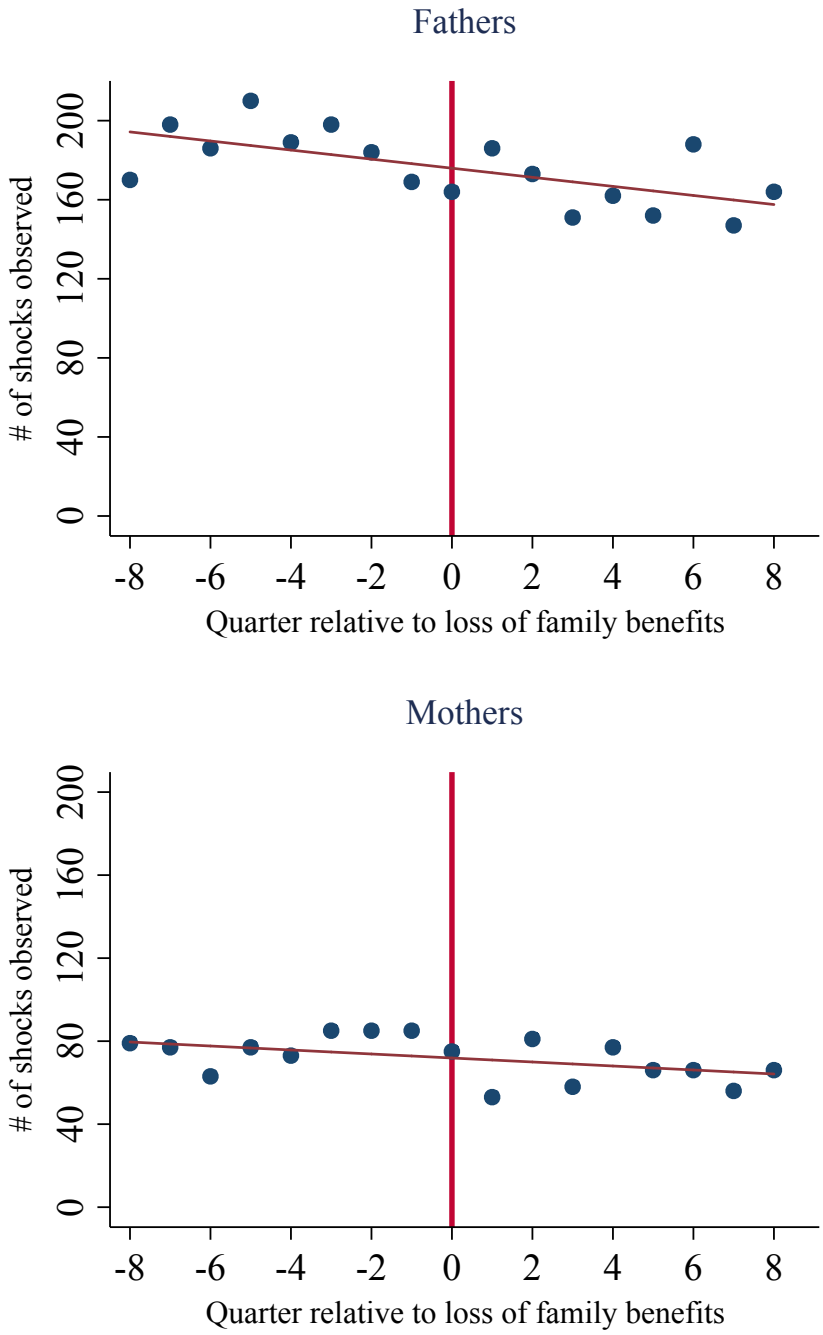
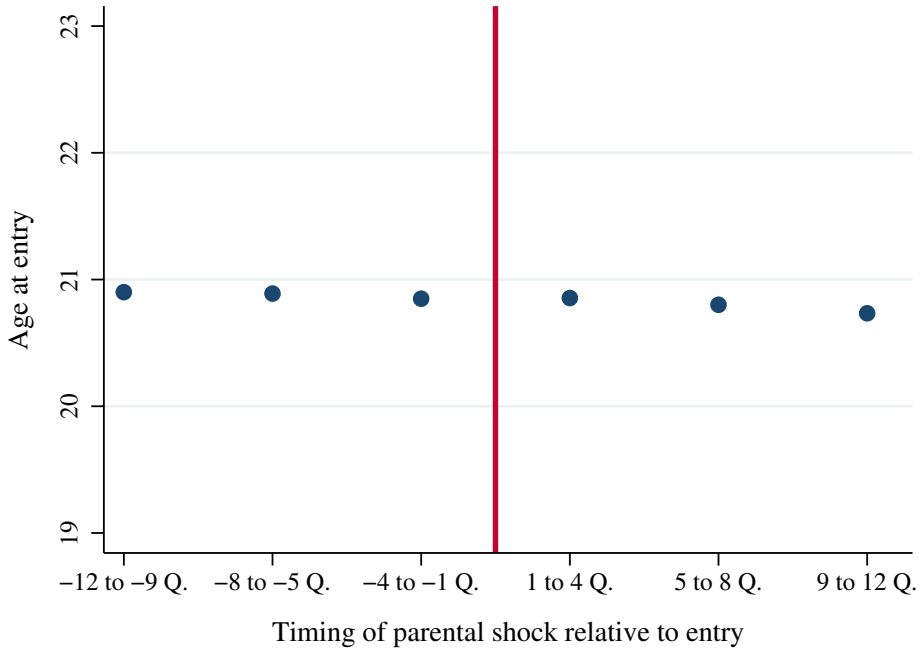


Figure 6: Distribution of Parental Shocks Over Time



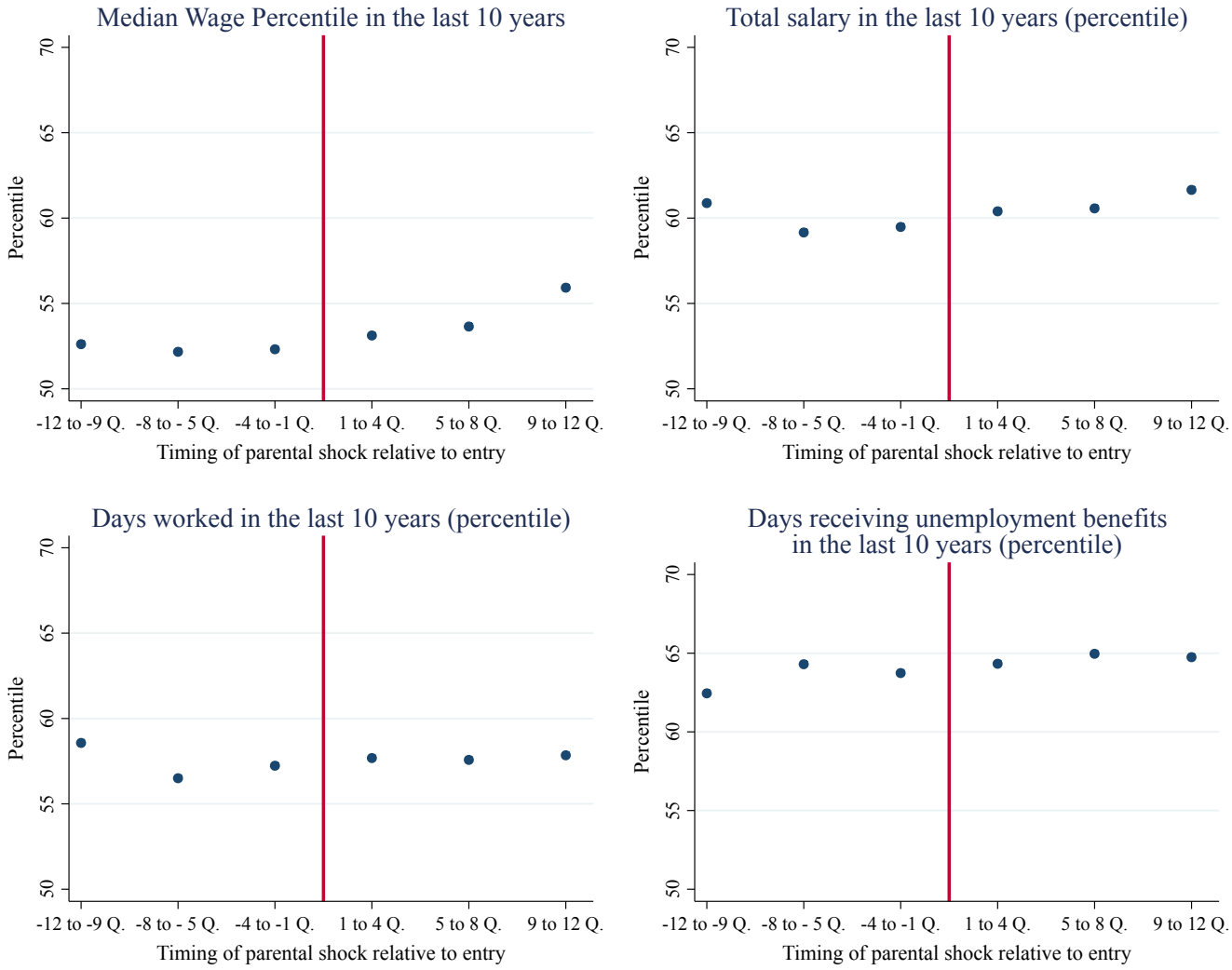
Notes: This figure displays the number of parental shocks observed in our data by time relative to the quarter at which they stop receiving family allowance for their child, separately for male and female parents. Shocks are observed between 2004 and 2011 for our sample of children entering into the labor force between 2004 and 2008. The fitted line is obtained by simple linear regression of the number of shocks observed in each quarter on the quarter relative to entry.

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



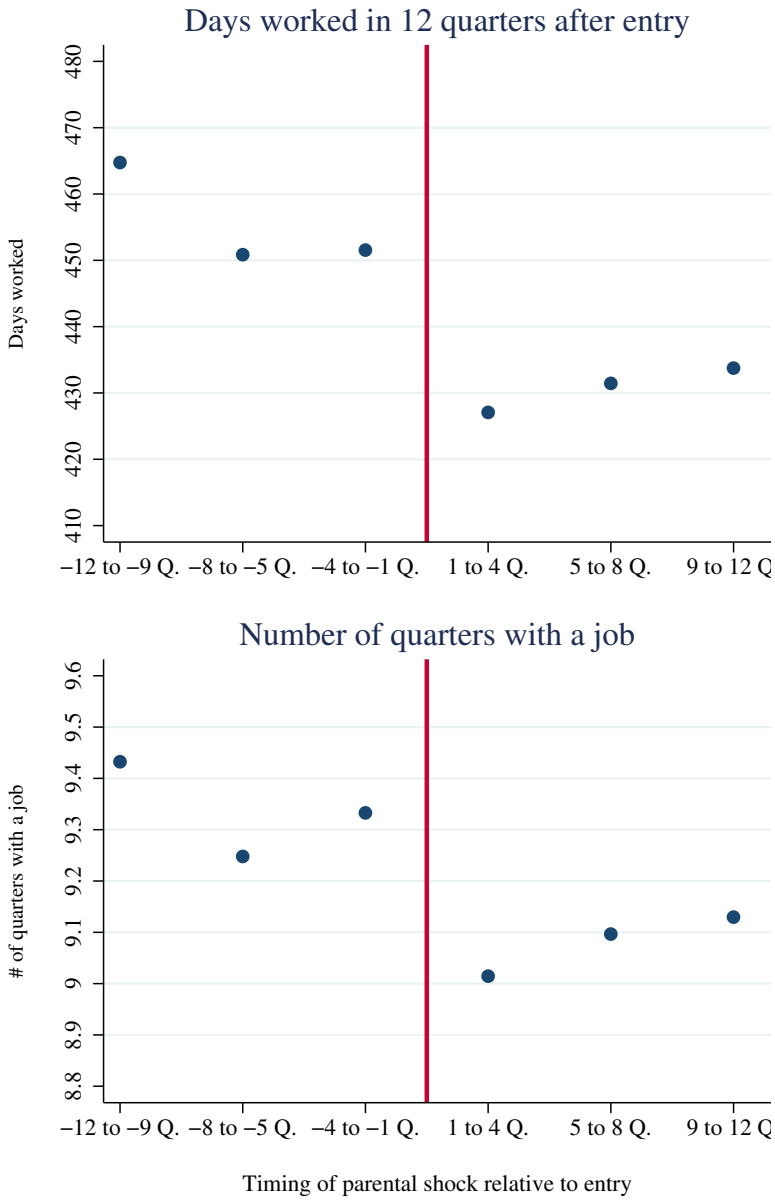
Notes: This figure displays the average children's age at entry as a function of the timing of the parental income shock relative to entry. Each point is the average age at entry for the subsample of children whose parent suffer from a parental shock in the specified time-window. Shocks are observed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

Figure 8: Balance: Parental Outcomes



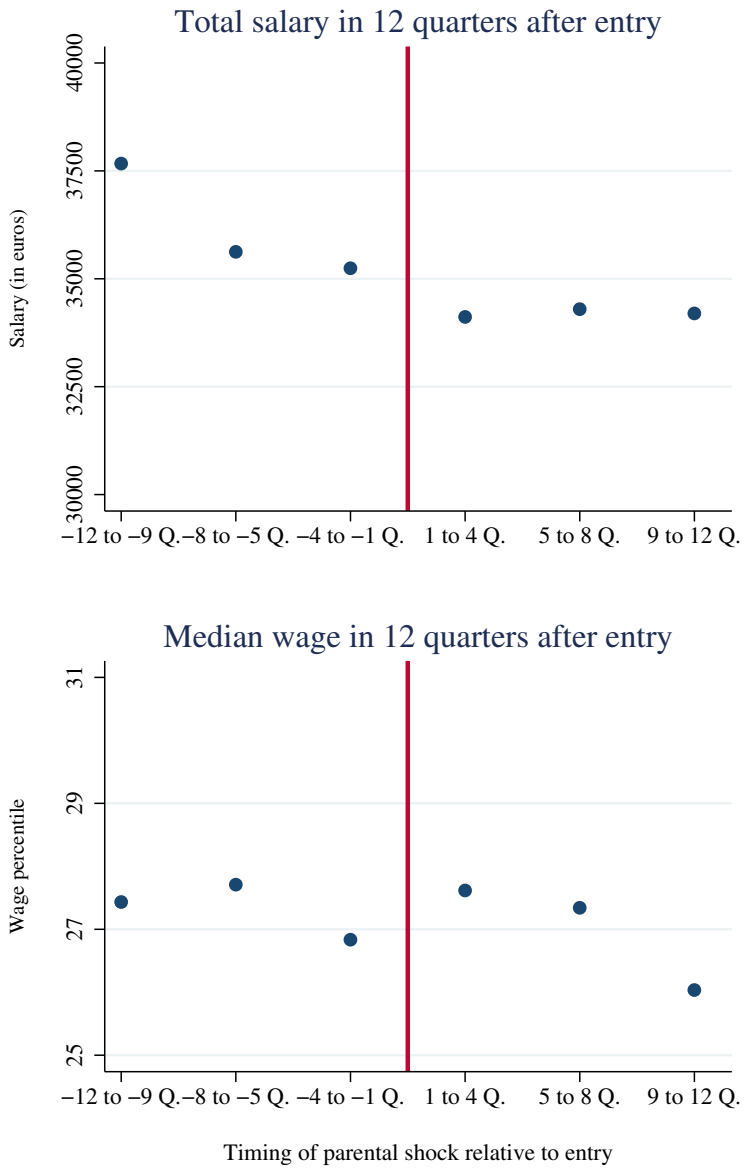
Notes: Each panel of this figure displays the average value of the corresponding variable as a function of the timing of the parental income shock relative to the child's entry into the labor force. Each point in the average age at entry for the subsample of children whose parent suffer from a parental shock in the specified time-window around entry. Shocks are observed in a 3-years window around entry, between 2004 and 2011, for the sample of children entering into the labor force between 2004 and 2008. We exclude from our sample a few cases of children whose parents experience more than one shock during the 3-years window around entry. All variables in this figure are computed using data for the 10 calendar years prior to the job loss shock Y. 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. Yearly labor income in the database is expressed in constant euros using Belgium's CPI. 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 career registry of the public pension administration for private salaried workers.

Figure 9: Days and Quarters Worked by Year of Shock Relative to Entry



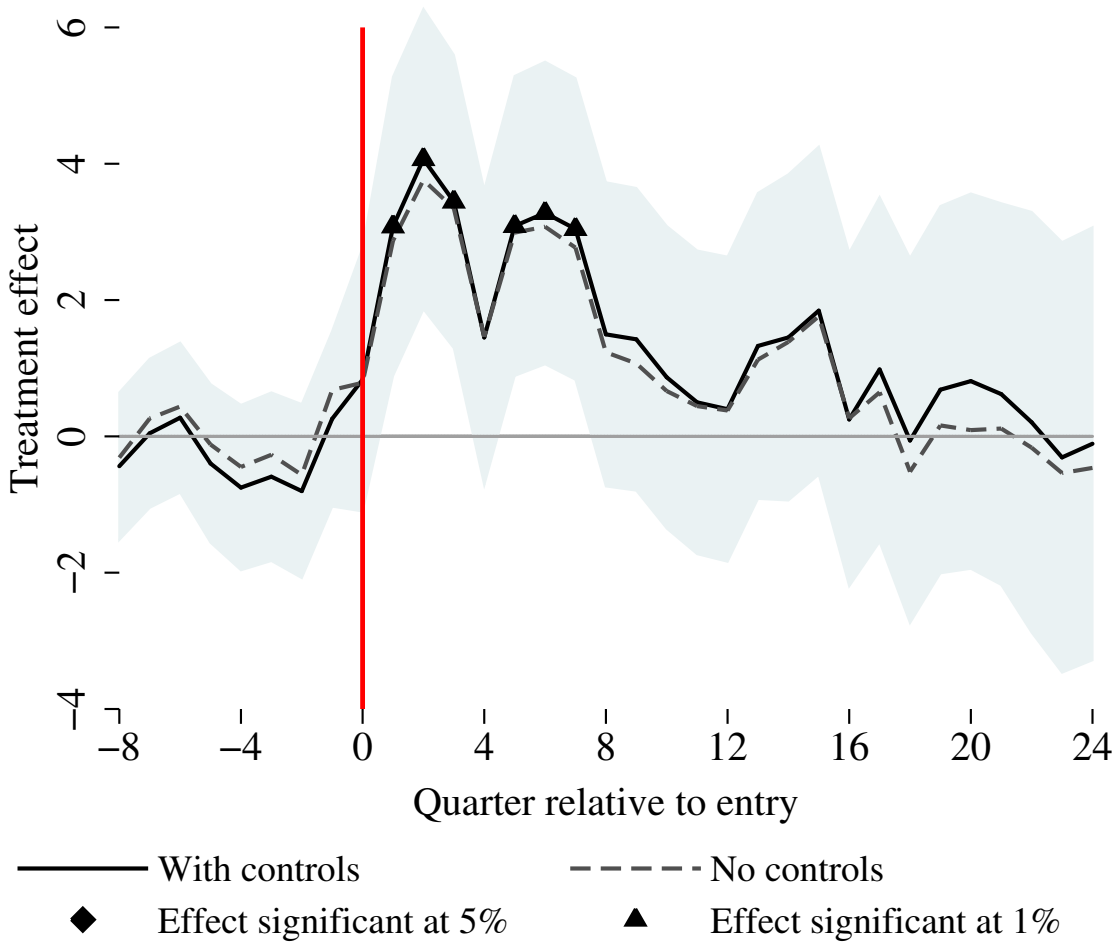
Notes: This figure displays total days and quarters worked in the 12 quarters after entry, as a function of the timing of the parental income shock relative to entry. *Days worked* is the sum of all full-time equivalent days worked as a salaried employee in the 12 quarters after entry (so, 4 hours of work on a given day is counted as half a day if the normal day of work for a full-time worker at the firm is 8 hours). *Number of quarters with a job* is the number of quarters during which the individual had at least one job (even for an hour) in the first 12 quarters following entry. Each point is the average value of the relevant variable for the subsample of children whose parent suffer from a parental shock in the specified time-window around entry. Shocks are observed in a 3-years window around entry, between 2004 and 2011, for the sample of children entering into the labor force between 2004 and 2008. We exclude from the sample a few cases of children whose parents experience more than one shock during the 3 years window around entry.

Figure 10: Total Salary and Wage by Year of Shock Relative to Entry



Notes: This figure displays total days and quarters worked in the 12 quarters after entry, as a function of the timing of the parental income shock relative to entry. Each point in the average value of the relevant variable for the subsample of children whose parent suffer from a parental shock in the specified time-window around entry. Shocks are observed in a 3-years window around entry, between 2004 and 2011, for the sample of children entering into the labor force between 2004 and 2008. We exclude from our sample a few cases of children whose parents experience more than one shock during the 3-years window around entry *Total salary* is equal to the sum of all pre-tax compensation paid during the quarter, net of all (employer and employee) social security contribution 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. All controls include a full set of dummy variables for each value of the covariate.

Figure 11: Effect of Shock on Total Days Worked (By Quarter)



Notes: This figure plots the estimated effects of parental job-loss shocks on the total number of days worked by children for each quarter relative to entry from two years before entry to three years after entry. Each point on the solid line represents the estimated coefficient for an indicator variable equal to one for children whose parents experience a job-loss shock in the 4 quarters prior to entry. The regression is estimated using the sample of children entering between 2004 and 2008 and whose parent suffer from a shock in the 4 quarters prior to (treatment group) or following (control group) the child's entry into the labor force. We observe labor market outcomes for all children from 2 years before entry up to the last quarter of 2011. Therefore, while we have a balanced sample up to 12 quarters after entry, later outcomes are only available for earlier entrants (e.g. outcomes up to 24 quarters are estimated on the subsample of individuals entering in 2004 and 2005). The solid 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 for the estimated treatment effect and markers indicate statistical significance at 1 and 5%. The dashed line plots the treatment effect estimated by taking the simple average difference between the treatment and control group (although omitted for concision, these estimates display qualitatively similar levels of statistical significance). In both cases, coefficients are estimated by pooling together all quarterly observations and interacting all regressors with a full set of dummies for each quarter relative to entry (estimates are therefore equivalent to a separate quarter-by-quarter estimation). Standard errors are clustered at the individual child level.

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 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^{th} 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)