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The Long-Term Labor Market Effects of Parental Unemployment*

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Abstract

I investigate the impact of parental unemployment on children's educational attainment and long-run labor market outcomes in Austria. I find that parental unemployment shortly before an important educational decision by parents for their children lowers a child's probability of holding a university degree by more than 5 percentage points. I do not find that income is affected at the beginning of a child's labor market career along the distribution but find a gradual deterioration later on. A substantial share of these long-term losses can be explained by the lower parental investment decision. My results emphasize the intergenerational and long-lasting consequences of parental unemployment.

JEL Codes: C21, I23, J13, J31, J64

Keywords: Parental Unemployment, Intergenerational Effects, Long-Term Effects, Unemployment, Wage Loss, Decomposition

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1 Introduction

High risk of a job loss has become a normal feature of labor markets in developed economies and many displaced workers have children. In the United States alone, it is estimated that one out of nine children, or 8.1 million children in total, had at least one unemployed parent during the most recent recession (Lovell and Isaacs, 2010). Similarly, 10% of all children in Europe between the age of 0 and 17 live in household with no employed person (Eurostat, 2018). It is well-established that a job loss has uniformly negative effects on individuals, such as large reductions in income and earnings, deteriorating health, and increasing mortality risk.¹ These consequences, however, are not restricted to the displaced individuals alone, but are transmitted to their children as well. Parental unemployment can decrease the well-being and school performance of children (e.g. Rege, Telle and Votruba (2011) and Powdthavee and Vernoit (2013)).

The analysis in this paper investigates how parents' displacement might lead to lower educational (human capital) investments in children. Unemployed parents might not be able to optimally invest in their children's human capital as a consequence of lower income, diminished labor market prospects, and binding borrowing constraints. Consequently, parental unemployment can lead to lower educational achievement, inferior labor market outcomes, and lower life-time earnings of their children. In turn, this could translate into an increase in income inequality in the future. In this paper, I look at the consequences of parental unemployment on the educational achievement of children and their long-run labor market outcomes in Austria. Obtaining causal estimates is, however, difficult for at least four reasons.

First, parental unemployment might be correlated with the displaced parents' investment decisions into their children. Highly productive workers might have a higher probability of holding higher paying jobs and be employed by more successful firms (Abowd, McKinney and Schmutte, 2017). This shields these workers from job loss and gives them the opportunity to invest more into their children. This implies that comparing children of unemployed parents to those whose parents did not experience a job loss is likely to overstate the impact; see also Hilger (2016) and Fradkin, Panier and Tojerow (2019).²

Second, even when one is able to find a suitable control group the effect likely depends on the timing of the unemployment spell in a child's life. On the one side, parental unemployment later in a child's life-cycle can have no or only marginal effects on children's long-term outcome if most important investment decisions have already been

¹See, for example, Jacobsen, LaLonde and Sullivan (1993); Neal (1995); Stevens (1997); Hijzen, Upward and Wright (2010); Couch and Placzek (2010); Schmidpeter (2018) for the impact on income and earnings and Sullivan and von Wachter (2009); Eliason and Storie (2009); Kuhn, Lalive and Zweimüller (2009); Browning and Heinesen (2012) for the impact on health and mortality.

²Another strategy is to use plant-closures as quasi experiment for unemployment. There is reasonable doubt, however, that this strategy solves the sorting problem when comparing children of displaced to those of non-displaced workers.

made. On the other side, even short-term losses and constraints can affect parental choices when the job loss happened around crucial educational investment decisions. Pooling children of different ages together when estimating the effect of parental unemployment can mask significant differences. Therefore it is important to define an objective timing of crucial educational investment during the life-cycle of a child.

Third, measuring the impact of parental unemployment on children’s labor market outcomes within a relatively short time period after the job loss or at a young age might not be a good approximation for the effect on life-time outcomes (see, for example, [Björklund \(1993\)](#), [Böhlmark and Lindquist \(2006\)](#), and [Haider and Solon \(2006\)](#)). It is possible that parental unemployment early during childhood leads to lower investment into children, lower educational attainment, and as a consequence to an earlier entry into the labor market by the child. Thus, using outcomes measured early during the life-cycle, one would compare children with relatively more labor market experience to those with less experience but higher education. This can lead to incorrect conclusions if the benefits of attending higher education materialize later during the child’s working life.

Fourth, it is crucial for a well-defined policy debate to pin-down potential channels through which parental unemployment can affect children’s outcomes. This task is particularly difficult when the job loss happened relatively early during a child’s life-cycle. The total effect can be driven by changes in monetary-related investments into the child, for example, schooling choices. It can also be driven by non-monetary related investments, such as time spent with the child. Even if parents as a response to unemployment spend more time with their children, this adjustment can have larger or smaller effects depending on the child’s age when parents experience unemployment. These differences can occur even within a relatively small age range.³ Hence, the estimated effect would not only measure the incidence of different educational choices but also different productivity of parental inputs.

In this paper, I study the effect of parental unemployment around important human capital investment decisions in the child on the distribution of long-term labor market outcomes. Using administrative data for Austria, I exploit the tracking of children in the Austrian school system at age 10 and compare children of parents who had an unemployment spell around the track choice. The early track choice constitutes an important educational investment decision in the life of a child as students are required to choose between a higher track which emphasizes more general education with the possibility to attend university or a lower track which provides basic education and prepares students for vocational training. Which track is chosen depends on the parental decision and in-

³For example, the baseline estimates in [Del Bono, Francesconi, Kelly and Sacker \(2016\)](#) indicate that maternal educational time spent with the child at age 3 is almost 3 times as productive as educational time spent at age 5, while maternal recreational time spent with the child at a younger age of the child is twice as productive.

put, among other things. Parents who choose the higher track for their child with the possibility to enter university are more likely committed to financially supporting their children in the future both directly through payment of university costs and indirectly by supporting their children during the time they delay entering the labor market. Facing these prospects, an income shock around the track choice can crucially affect the parental investment decision.⁴

Experiencing unemployment will affect parents' educational investment decisions by affecting the ability to save and through their long-run expectations of their own life time earnings. Specifically those who experience a sudden unemployment spell may be more likely to have more pessimistic views about future employment prospects and wages (Kuchler and Zafar, 2018; Rabe and Schmidpeter, 2018). If unemployment insurance systems such as those that are common in the U.S. and Austria do only replace a small fraction of the displaced worker's earnings, both of these effects – expectations about employment stability and life time earnings - and the inability to save combined with the need to dip into savings while unemployed will come into play in parents' educational investment decisions.⁵

It is also possible that a sudden unemployment spell at the timing of an educational investment decision deters parents from choosing high-quality but also more expensive schools. The costs considered by the parents when deciding which school to choose might not only contain fees but likely depend on other factors, too. For example, areas with high and improving school quality not only see an increase in student performance but also experience higher house prices (e.g. Black, 1999; Gibbons and Machin, 2006; Kane, Riegg and Staiger, 2006; Cellini, Ferreira and Rothstein, 2010; Fack and Grenet, 2010; Hussain, 2017). Thus, even when families receive financial support for direct school costs such as fees, a sudden income shock around an important educational decision can prevent parents from moving to more expensive areas with higher school quality and reduce the amount parents spend to help their children obtain their educational goals in other ways.

I first show that an income shock as a result of parental unemployment shortly before the child's track choice significantly lowers a child's likelihood of obtaining a university degree compared to unemployment after the choice was made. This difference is unlikely explained by differential access to family resources. I then investigate the long-term impacts of this sub-optimal investment decision on the long-term labor market outcomes of children. While I do not find any effect on annual income at age 29-31, my results

⁴There is no well established student loan system available in Austria and most students rely, at least partly, on parental financing during study. Parents are also required by law to support their children financially for housing and food among other expenses deemed necessary during university. In Section 2, I provide an overview of the Austrian education system.

⁵Austria's unemployment insurance system is less generous than in most other European countries. The benefit replacement rate is 55% of net earnings, but it is subject to a maximum and minimum benefit level and other adjustments. Regardless of the personal circumstances, the maximum replacement rate is capped at 80% of net earnings; see the discussion in Nekoei and Weber (2017) for more details.

indicate a gradual deterioration at the upper part of the distribution later on. At age 35-37, an age where the estimated impact is a good approximation for life-time effects, children between the 60th and 90th percentile whose parents had an unemployment spell shortly before the educational track decision earn between 1500 Euros and 3500 Euros (between \$1,700 and \$3,900) less per year. Assuming that these effects persist until the end of an individual's working life and ignoring the impact on retirement benefits, my estimates imply at age 35 a net present value of lower future income between 20,000 Euros to 65,000 Euros (\$22,000 to \$72,000). Applying a decomposition analysis, I find that up to 45% of these losses can be explained by lower educational achievement of these children.

I also look at the effect on non-monetary labor market outcomes such as days employed and unemployed. I find that parental displacement at a crucial educational decision induces individuals to work between 50 to 75 days more per year at age 29-31. This effect fades out later on with no significant impact on days worked at age 35-37. My estimation for days in unemployment reveals the opposite effect. Early parental unemployment has no effect on children's days spent in unemployment at a younger age, but increases them by around 50 days per year at age 35-37 for unemployment prone individuals.

My findings show that parents do not necessarily behave in a (long-term) forward looking manner and that the timing of an adverse income shock can play an important role in determining parental investment decisions. Parental unemployment at the time of crucial educational decisions results in children more likely being tracked into technical or vocational training which require fewer years of education. Children in these tracks therefore enter the labor market earlier and have more time to accumulate experience relative to children who attend university. This dampens the impact of lower educational attainment on income during the early working life. As children with higher education start to accumulate sufficient labor market experience, this trade off weakens and the effect of parental unemployment at the time of the educational investment decision starts to come into full effect.

My work contributes to the recent literature on the impact of parental unemployment on children's labor market outcomes in several important ways. Like [Hilger \(2016\)](#) and [Fradkin et al. \(2019\)](#), I exploit the timing of the shock to identify the effect of parental unemployment on children's outcomes. I use the existence of a crucial educational investment decision (track choice), however, to define the exact timing. Then, I compare the outcome of children whose parents experienced unemployment at the time the educational tracking decision was made (treatment group) to the outcome of children whose parents experienced unemployment shortly after the tracking decision (control group) was made.⁶ Under the assumption that children in both my treatment and control group

⁶[Fradkin et al. \(2019\)](#) estimates the effect of parental unemployment around labor market entry, an age where arguably most parental investment decisions have already been made.

are at the same development stage and exposed to the same shock to family income, this allows me to capture the impact of a sudden drop in income and constraints at crucial educational investment decisions. This is an important policy parameter which has been not explicitly accounted for in the existing literature (e.g. [Oreopoulos, Page and Huff Stevens, 2008](#); [Hilger, 2016](#); [Huttunen and Riukula, 2019](#)).

With my data at hand I am also able to evaluate the impacts of parental unemployment on children’s labor market outcomes up until the age of 37. This is longer than in any previous work on this topic. For example, [Hilger \(2016\)](#) uses income at age 25, [Huttunen and Riukula \(2019\)](#) measure income at the year the child turns 30, and [Fradkin et al. \(2019\)](#) uses the quarterly income around the lay-off for individuals of age 18 to 28. [Oreopoulos et al. \(2008\)](#) take a five year earnings average when individuals in their sample are between 25 and 33 years old. My results show that concentrating on younger age groups is likely to understate the long-term costs of unemployment. The estimated long-term negative impact presented in this work is likely to persist until the end of a child’s working life and therefore are a good approximation for life-time effects.

I also concentrate on the distribution rather than mean effects with the aim to investigate potentially heterogeneous impacts of parental unemployment on children’s outcomes. My findings indicate that parental investment affects children differently at different parts of the outcome distribution. Understanding the distributional impact helps to identify vulnerable groups. This may be especially interesting for policy makers as they might be concerned about the impact of parental unemployment at certain parts of the distribution.

This paper also contributes to a to the discussion on the impact of parental income, wealth, and credit constraints during childhood on human capital investment and possible long term-impacts. Income has become increasingly important in explaining investments in children and their achievements.⁷ At the same time, binding borrowing constraints can negatively affect children’s educational achievement, for example, when occurring at important times (e.g. [Brown, Scholz and Seshadri, 2012](#), and [Caucutt, Lochner and Park, 2017](#)).⁸

⁷[Løken \(2010\)](#); [Milligan and Stabile \(2011\)](#); [Dahl and Lochner \(2012\)](#); [Løken, Mogstad and Wiswall \(2012\)](#); [Jones, Milligan and Stabile \(2015\)](#) find in general a positive effect of variations in family income on child investment and achievement using either an instrumental variable approach or parental displacement. [Carneiro and Ginja \(2016\)](#) find that parental investments react to permanent but not to transitory income shocks.

⁸[Carneiro and Heckman \(2002\)](#) do not find evidence that family borrowing constraints affect post-secondary education using a simpler model than [Brown et al. \(2012\)](#). [Bulman, Fairlie, Goodman and Isen \(2016\)](#) find only modest effects of changes in parental resources on college attendance for younger children using lottery wins as instrument, especially for low SES households. They interpret their findings that households do not face borrowing constraints. [Lochner and Monge-Naranjo \(2012\)](#) provide an overview over the current literature on the impact of credit constraints on the accumulation of human capital.

In this paper, I provide indirect evidence that not only parental income per se matters in explaining children’s achievements but also the timing and likely associated (short-term) constraints. These channels are also highlighted in the model of [Caucutt and Lochner \(2017\)](#). My results show that a drop in income at an important parental investment decision can negatively affect the educational attainment of the child compared to situations where the investment has already been made.

It should be emphasized that this paper neither intends nor is able to evaluate the consequences of early tracking as, for example, [Hanushek and Wößmann \(2006\)](#) and [Dustmann, Puhani and Schönberg \(2017\)](#). [Hanushek and Wößmann \(2006\)](#) find that early tracking increases educational inequality comparing outcomes between primary and secondary school across different international systems. [Dustmann et al. \(2017\)](#) do not find evidence for Germany that early tracking affects long-term outcomes. They attribute this to the possibility of later track-reversal facilitated by the German school system. The difference between the German education system and the Austrian System studied here is that the German Education system offers three tracks: a low track, middle track, and higher track. This makes it easier for those attending the middle track to reverse their choice upward later on. The Austrian Education system offered only a lower and a higher track during the period studied here, making it comparably more difficult to reverse the choice later on. In my work, I use the track choice as an important parental decision in order to compare the consequences of parental unemployment on children at similar development stages.

This paper proceeds by first describing the Austrian Education System in the next section. In Section 3, I describe the data and how I construct my treatment and control group. The impact of parental unemployment on family resources and children’s educational attainment is discussed in Section 4. My empirical approach is outlined in Section 5. Section 6 presents the main findings of the paper. In Section 7, I discuss underlying channels and present results from my decomposition analysis. Section 8 concludes.

2 The Austrian Education System ⁹

Compulsory schooling in Austria starts at age 6 and lasts until the age of 15. Enrollment is based on a cut-off date. All children born before the 1st of September must enroll by the age of six, while for children born after this date enrollment is optional and depends on their cognitive and non-cognitive development.¹⁰ In addition, students might

⁹This section draws heavily on [Schneeweis and Zweimüller \(2014\)](#).

¹⁰The official requirements are that the child has the capabilities of following and processing the material taught during first grade. The assessment is conducted by the school, if necessary with help from trained psychologists and physicians. It is also possible that attendance is postponed by one year if the student is not deemed to fulfill the requirements.

be required to repeat a grade if test scores are not sufficiently high, but the likelihood of grade retention is relatively low during primary school (grades 1-4).¹¹

An interesting feature of the Austrian education system is its early tracking of students at the end of primary school which goes through grade 4. At the end of grade 4 - age 10 for most students- there are two different types of compulsory education offered: a general track which provides general education and concludes with a university entrance exam (Matura), and a more specialized track which provides basic education and prepares the student for vocational training and education. Admission to the general track requires an average of at least good in the three core subjects German writing, reading, and mathematics. If these requirements are not met, students can sit an entrance qualification exam.¹²

In addition to a student's school performance, track choice depends strongly on the parents. [Bruneforth, Weber and Bacher \(2012\)](#) estimate that 60% to 70% of a student's likelihood of taking the higher, general track is explained by the parental decision. Hence, it is possible that even if a student meets all objective requirements to attend the general track parents may choose the specialized one for their child. Around 30% to 35% of all students choose the higher, general track in grade 5 ([Schneeweis and Zweimüller, 2014](#); [Statistik Austria, 2018](#)).

Early track choice might be reversed at grade 9, when students can revise their first decision and can choose to change from the specialist to the general track, depending on school performance. Switching tracks is relatively rare in Austria, however. Only 35% of students who chose the lower, specialized track at first switch to the higher one which allows them to sit the university entrance exam (Matura). In comparison, 96% of students who chose the higher track at first choice take the exam. These shares have been remarkably stable over the past years (see, for example, [Schneeweis and Zweimüller \(2014\)](#) and [Statistik Austria \(2018\)](#)).

Students with Matura can enroll in a university or technical college (Fachhochschule). Except for a small administration fee of 40 Euros per year (roughly \$45) attending university is free if the student graduates within a pre-specified time period. This period is in general set to 4 years for a bachelor's degree and to 3 years to obtain a master's degree. Only around 50% of all students manage to graduate during this pre-specified time period, however. After exceeding these limits, students need to pay university fees of

¹¹The share of students who have to repeat a grade is in general low in Austria. On average around 2% of students repeat grade 2-4, 3% grade 5-8, and 4% of students repeat grade 9 ([Schneeweis and Zweimüller, 2014](#)). Recent statistics for the year 2015/16 put the retention rate during primary school even as low as 0.6% ([Statistik Austria, 2017](#)).

¹²In some cases, students might be admitted to the higher track even if the requirements are not entirely met. For example, this is the case if the obtained grade in one of the three subjects is satisfactory and the student shows in general a good performance in school.

766 Euros (\$850) per year.¹³ Fachhochschulen are independent in their decision to charge fees up to a maximum of 728 Euros (\$812) per year which is done by most schools.

Unlike in the US or the UK, where school loans can be used to cover room and board in addition to university registration fees, Austria does not have an extensive student loan system. Students main sources of income while in university are wages from (part-time) employment and parental support. In 2014, around 86% of students in Austria held a job and around 50% of all students received additional income support from their parents.^{14,15} Income from employment is, however, relatively low with 45% of employed students earning less than 400 Euros per month and almost 70% earned less than 750 Euros (\$840) per month. In comparison, the average monthly earnings of an individual age 20 to 29 in Austria is 1,800 Euros (\$2,000).

Given the low earnings students earn for working, a median duration of 4 years to graduate with a bachelor's degree, and an estimated lower bound of students' living expenses of 9,900 Euros (\$11,000) per year, support of students by their parents might impose a non-negligible financial burden on families.¹⁶ But even with income from work and parental support, almost 60% of students state that the monthly amount they receive is in general not sufficient to cover all costs (Hajek et al., 2014). Hence, besides facing the prospects of continuous, regular financial support parents are also the ones who provide additional funding in case of emergencies. A sudden and long lasting drop in income at the time of the track choice can therefore deter parents from choosing the higher general, more expensive education track. Unemployment can reduce parents currently ability to save. It can also lead parents to adjust their expectations about future employment and wages downward (Kuchler and Zafar, 2018; Rabe and Schmidpeter, 2018). These factors combined may lower parents ability and willingness to invest in their children's current education. Parents might also try to limit their future responsibilities by choosing the lower educational track as they are required by law to pay their child maintenance during studies. I discuss this and alternative explanation further in Section 6.

While the track choice at age 10 is common in Austrian and Germany, similar crucial educational investment decisions exist in other countries.¹⁷ In most OECD countries, tracking of students is common by the age of 16 when parents and students need to choose

¹³The median study duration is 4 years for a bachelor's degree and almost 3 years for a master's degree (Statistik Austria, 2019). Students outside the European Economic Area are in general subjected to a maximum fee of around 1,490 Euros per year regardless of the study duration.

¹⁴This and the following figures were taken from the survey presented in Hajek, Kasper and Freidl (2014).

¹⁵In an international comparison the share of working students is relatively high. For example, in the U.S. around 43% of full-time undergraduate students worked in 2017, down from 50% in 2005 (McFarland, Hussar, Zhang, Wand, Wang, Hein, Diliberti, Forrest Cataldi, Bullock Mann and Barner, 2019).

¹⁶The costs of living were taken from the recommendations of the Business University of Vienna, <https://www.wu.ac.at/studium/services-fuer-studieninteressierte/master/internationale-studierende/allgemeines-leben-in-wien/lebenshaltungskosten/>

¹⁷There are several other European countries with relative early tracking. For example, the Czech Republic and Hungary track at age 11, and Belgium and the Netherlands track around age 12 (OECD, 2004).

the educational track (OECD, 2004). In addition, mirroring the setting in Austria, most European countries either do not have a well-established student loan system and/or additional financial support from parents is likely necessary to study.¹⁸ Thus, similar to Austria, a sudden income shock at the track choice can affect the parental decision when facing the possibility to commit to prolonged financial support in the future.

3 Data and Selection Process

The analysis is based on two different data sets: the Austrian Social Security Database (ASSD) and the Register for Family Allowances. The ASSD is a matched firm-workers database covering all private sector workers in Austria. It contains detailed information about demographic characteristics, daily labor market states, and yearly earnings until the end of 2016. Zweimüller, Winter-Ebmer, Lalive, Kuhn, Wuellrich, Ruf and Büchi (2009) provide an extensive description of the ASSD.

The Register for Family Allowances contains information about child benefit payments, the person who is granted the benefits, and the child for whom the benefits are paid. Furthermore, in case a spouse is indicated at application, information on her/him is supplied. This enables me to link parents with children. The unique person identifier, available in all data sets, allows me to link parents and children to their work history and demographic characteristics.

Unfortunately, the data contain only information about the qualification received and do not allow me to track the different educational institutions an individual has attended or individual school performance. The numbers presented in the previous section, however, suggest that school choice at age 10 is closely correlated to the final degree a person received. Therefore the final degree obtained constitutes a good proxy for parental educational investment decisions made at age 10 of the child.

3.1 Treatment and Control Group

From the combined data, I first select all parents who had at least one child born between 1975 and 1979. Concentrating on the early years available in the data enables me to follow children of unemployed parents until the age of 37, an age where individuals are fairly established in the labor market and observed effects are a good approximation for the

¹⁸Grant and loan systems differ substantially by country. For example, students in the Netherlands can borrow up to 800 Euros per month excluding tuition fees. The exact amount depends on parental income and the own employment situation, and some students might not be eligible at all. (<https://duo.nl/particulier/student-finance/amounts.jsp>). Germany has a more complex grant and loan system, and eligibility depends on numerous criteria. Loans which are available to all students regardless of parental income ("Bildungskredit") only allow to borrow up to 7,200 Euros which is likely not enough to cover all costs (https://www.bva.bund.de/DE/Services/Buerger/Schule-Ausbildung-Studium/Bildungskredit/Antrag/Bildungskredit-Hintergrund/bildungskredit-was-bietet_node.html).

impact on life-time outcomes. I then select all families where the *main* earner in the family had an unemployment spell between 1987 and 1989. The main earner is defined as the person, either mother or father, with the highest average income over the three years prior to the year of interest.

From the obtained sample, I select all families with at least one child age 10 or 12 at the time of parent’s unemployment. I exclude children who were 11 years old at the time of the parental unemployment spell to overcome potential problems with grade retention or repetition at time of the track choice or late school enrollment.¹⁹ In order to account for the schooling entry cut-off in a given year, I also exclude all children born after the 1st of September from my analysis. This is to minimize ambiguity of the age when the track choice was made for children born after the 1st of September.

From this sample, I drop all families where the main earner had less than 360 employment days in the year prior to the unemployment spell. Requiring a minimum labor market attachment of the main earner allows me to capture a sudden drop in family income as a result of unemployment and is common in the literature which studies the effects of unemployment (e.g. [Jacobsen et al. \(1993\)](#)). Applying all restrictions, my final sample consists of 3,767 children.²⁰ Children whose parents had an unemployment spell at the time of the track choice, that is at the time the child was 10, are considered as treated, while children whose parents had an unemployment spell at age 12 are considered as controls.

Table 1 provides summary statistics for my estimation sample by treatment status. In general, the difference in parental characteristics between treatment and control group are very small. Main earners in both groups have similar average labor market experience and earnings, although parents in my treatment group tend to have slightly more unemployment days prior to the considered spell. They also tend to have slightly younger spouses at birth of the study child and are also more likely to be employed in the construction and tourism sector previous to the reference year. Although the years 1987 to 1989 were marked by stable economic conditions, the observed differences might be the result of different sectoral shocks in the economy. To account for this, I control not only for sector but also year effects in my analysis.

[Table 1]

¹⁹Students at age 11 do not comprise a “pure” control group. Extending the age range at the time of parental unemployment delivers qualitatively similar results as those presented here. The results for the extended control group can be found in Appendix A.

²⁰The estimation results not requiring any labor market attachment before the unemployment spells are very similar to the ones presented here and can be found in Appendix A.

3.2 Long-Term Child Outcomes

My outcomes of interest are children’s yearly days of employment and unemployment as well as their annual income. In order to derive policy implications, it is important to evaluate the effect of parental unemployment over a long-time period. As pointed out above, previous studies examining outcomes early in a child’s life cycle certainly is important. But focusing only on early outcomes can fail to reveal long term consequences. This study examines both short-term and long-term outcomes and illustrates the importance of examining both.

I measure all the outcomes of interest when the child are older - between the ages of 29 and 37. The lower age bound is chosen to allow for sufficient time for students to finish higher education. Although this threshold is somewhat arbitrary, the difference in labor force participation rate between university graduates and non-graduates before age 29 is substantial. At age 20, regardless of whether children are in my treatment or control group, I observe a 28 percentage points (pp) lower labor force participation rate for university graduates compared to non-graduates in my data. A comparable difference remains until children reach the age of 29.²¹ At the upper bound at age 37, individuals are fairly established in the labor market. I divide my data into 3-year intervals and average the outcomes within these intervals. This allows me to capture labor market performances at three different points in a child’s life cycle. Outcomes at age 29-31 can be thought of as an early stage where individuals are still not well established in the labor market, especially for individuals who attend university. At age 32-34, individuals normally have obtained sufficient amount of labor market experience and are probably in their career jobs. It is likely that the impact of parental unemployment will start to materialize when individuals reach this age. Differences observed between the treatment and control group at age 35-37 are likely to be a good approximation for differences in life time earnings and attachment to the labor market (Björklund, 1993; Böhlmark and Lindquist, 2006; Haider and Solon, 2006). Hence, effects found at this age range are likely to be persist until the end of people’s working life.²²

The lower part of Table 1 contains summary statistics for long-term wages and educational attainment of children in my sample. I do not find any statistical significant differences in mean log average earnings at age 35-37, but this measure masks important differences later in life. Looking at the distribution, I find that children in my treatment group have in general lower income along the entire distribution but that the effect of early parental unemployment is more pronounced at the upper part of the distribution. For example, at the 70th and 90th percentile, I estimate significant differences of 7 log-points. There are also large differences in the probability of having long-term unemployment

²¹In Appendix A, I provide estimates for outcomes before age 29.

²²The effects are more pronounced using a 2-years average of the outcomes and show a even clearer deterioration in the labor market performance with age. Detailed estimates can be found in Appendix A.

spells. For example, children in my treatment group have a 2.5 pp higher likelihood of having at least 90 days on average over the 3-year period. These estimates indicated that parental unemployment at important educational decisions points for their children could contribute to long-term earnings inequality in their children’s generation.

What also is striking is the difference in educational attainment of children by treatment status. Children whose parents were displaced at the time of their track choice have around 5 pp lower probability of obtaining a university degree and this difference is statistically significant at a 1% level.²³ Having higher levels of education is arguably more important at the upper part of the wage distribution. These unadjusted results for labor market outcomes and educational attainment indicate that parental unemployment at a time of important educational decisions could contribute to long-term earnings inequality in the next generation and long-term effects on their children

4 Parental Unemployment, Family Income, and Children’s Educational Attainment

4.1 Impact on Family Income

My main hypothesis is that parental unemployment at important parental educational decisions and not only income loss per se is an important determinant in explaining differences in children’s long term outcomes. I first investigate the effect of unemployment on family income in order to establish that families do suffer a loss of income when a parent becomes unemployed. If the timing of the income shock is an important determinant of observing differences in children’s educational achievement and not only the drop in income, I should not find any differences in the income paths between parents in my treatment and control group.

I evaluate the impact of an unemployment spell on yearly log parental income ω from 3 years prior until 5 years after the unemployment spell of a parent in the family by calculating the counterfactuals $E[\omega(1)]$ and $E[\omega(0)]$. Under unconfoundedness and sufficient overlap in the outcome between treatment and control group, these quantities can be obtained by separate linear regression in the two sub-population; with $T = 1$ (a parent in the family experiences unemployment at the time of the educational decision point for the child) and $T = 0$ (parent in the family experiences unemployment after the time of the educational decision for the child) . Figure 1 plots the estimates together with 95% confidence intervals. Panel a depicts the income path for the main earner of

²³Although the Austrian education system distinguishes between numerous higher education institutions, such as universities and Fachhochschulen, I refer to degrees from higher education institution collectively as university degree in the following.

the household and Panel b the path of total family income, the yearly income of the main person and the spouse.

[Figure 1]

Figure 1 reveals two striking features. First, an unemployment spell is associated with a sharp fall in both individual and family income by around 35 log points at the year of the unemployment spell. Both family and individual income slowly recovers over the following years, but it takes more than 4 years to reach pre-unemployment levels. The small differences between both measures and their similar path show that spouses in both groups react only weakly to unemployment of the main earner; see also, for example, [Blundell, Pistaferri and Saporta-Eksten \(2016\)](#), [Halla, Schmieder and Weber \(2018\)](#), and [Haan and Prowse \(2019\)](#) for recent work on this topic.

Second, the estimated counterfactual differences in the income paths between those parents who experienced unemployment at the educational decision time and those who experienced unemployment after the decision time are small and statistically insignificant. Hence, children in both my treatment and control group are likely confronted with the same effect on family resources. Therefore, a more severe or differential drop in income between those two groups alone is unlikely to explain the observed differences in children’s educational achievement. It is more likely that the short-term financial constraints caused by unemployment at the time of parental educational investment decision, lower expectations about future employment of the parents, and the prospects of future support payments if the child attends university leads parents in my treatment group to choose the lower educational track. In addition, the virtually non-existent difference in income between the two groups also supports my identification strategy and shows that children in both my treatment and control group likely have the same access to resources before and after the parental unemployment spell.

4.2 Impact on Children’s Educational Attainment

Having established that families in both my treatment and control group face similar income constraints, I investigate how parental unemployment around the track choice affects the educational achievement of children. I estimate the average treatment effect of having obtained a university degree similar to how the income effects were estimated in the previous section. I use now a logit outcome models, however. The results are presented in Table 2 together with bootstrapped 95% Confidence Intervals.

[Table 2]

The first row of Table 2 shows the estimation results for my total sample. Parental displacement at time of the educational track choice compared to later lowers the child's probability of obtaining a university degree from around 30% to 25% which corresponds to a treatment effect of around 5 pp.²⁴ This estimate is close to the observed raw differences of 5.84 pp reported in Table 1 when not controlling for any covariates. The similar results give reassurance in my identification strategy. Including covariates in my analysis only marginally lowers the estimated treatment effect.

One explanation for my finding is that unemployed parents adjust their expectations about their future re-employment probabilities and labor market outcomes downward and therefore also lower their current investments. For example, Kuchler and Zafar (2018) show that personal unemployment experiences lead to an overly pessimistic view about future unemployment rates. Related to the finding in Kuchler and Zafar (2018), Rabe and Schmidpeter (2018) show that unexpected and negative changes to individuals' information sets can drastically lower expectations about their future financial situation. Parents might also try to limit their future responsibilities by choosing the lower educational track as they are required by law to pay their child maintenance during studies.²⁵ Overall, facing the possibilities of providing support to their children for a prolonged period of time with likely lower expectations about future job possibilities can lead parents to choose the lower educational track, even though the actual decision to attend university still lies far ahead.

The next rows present the estimation results for four different sub-samples chosen in such a way that the estimates should be a proxy for different magnitudes of credit constraints, family resources, and shocks to parental labor market careers. I first consider families where a spouse was present. These families might be more flexible in coping with unemployment of the main earner, for example, by increasing the labor supply of the spouse. Parental investment might also be higher when both parents are present.

Second, I investigate the effect on children in single parent households (Single). These children are likely the most constraint in terms of future available family resources.²⁶

²⁴Coelli (2011) finds that parental job loss at age 16-18 lowers the probability of enrolling in post-secondary education by around 10 pp using data for Canada. He compares the outcome of children whose parents were displaced to the outcome of children whose parents did not lose their job. As argued above, this might introduce bias in the estimation and can overstate the impact of parental unemployment.

²⁵By law, the maintenance payment can be quite substantial and can be up to 22% of the net yearly income of the person liable for maintenance. To avoid excessive maintenance payment, the maximum monthly amount is capped at 1,450 Euros. The exact amount payable depends on numerous factors such as the number and age of maintenance eligible children and their income.

²⁶I define all households as single parent household if no spouse was indicated during application for child care benefits.

Third, I consider the effect on families where the main parent is eligible for benefit duration of 30 weeks or roughly 7.5 months. Eligibility for 30 weeks constitutes an extension of a 10 week over the usual benefit duration of 20 weeks. An individual is eligible for the extension if, during the three years preceding the current unemployment spell, the person did not claim benefits for more than 20 weeks and was employed for at least 152 weeks. Because these parents have had more stable employment in the past, families in this group should be less credit constraint. Furthermore, given the longer time parents are eligible for benefits they might be more selective during the job search; searching for jobs with higher wages, that provide better promotion potential, that offer more job stability or that better utilize their skills (Nekoei and Weber, 2017, for example,). Obtaining higher paying and more stable jobs might lower the parents' concerns about providing future support payments to the child when attending university.

Fourth, I also differentiate by parental pre-unemployment average income. Similar to individuals with longer benefit eligibility, parents with higher pre-unemployment earnings might have more resources to smooth the sudden shock. They might also have more resources to look for better re-employment and higher wages. Likewise, families with lower pre-unemployment income might have fewer possibilities to adjust to the shock. I consider all children as coming from lower income families when the average yearly wage of the main earner three years prior to the unemployment spell does not exceed 18,000 Euros. Likewise, children of families with an average yearly income of at least 30,000 Euros per year are consider high income families.²⁷

One striking feature of my estimations for the two parent and single parent sample, reported in the second and third row of Table 2, is the estimated likelihood of holding a university degree under both treatment and control for these two groups compared to the entire sample. Children of Two-Parent families are in general more likely to obtain a university degree regardless of whether they are in the treatment or control group. However, children in two parent households with a parent experiencing an unemployment spell before the track choice are still less likely to receive a university degree than children whose parent had an unemployment spell after the track choice was made . Similarly, the estimates for the single parent sample indicate that the probability of holding a university degree is only 19% when the parent had an early unemployment spell at the educational decision point and around 24% when the unemployment spell occurred after the child's track choice.

The results for those children whose parents experienced a spell of unemployment at the educational decision point likely reflect lower family resources. In turn these tighter financial constraints likely lead to a lower probability of these children attending

²⁷It also would be interesting to see how parental contributions toward students' finances is correlated with parental income as in Hilger (2016). Unfortunately, my data does not contain any information about transfers from the parents toward the student.

university. For both two parent families and single parent families, I estimate a treatment effect of -5.03 pp and -4.49 pp respectively. Both of these estimates are very similar to the one obtained for my entire sample. These results show that although the propensities to obtain a university degree differ if a second parent is present or not, both types of families react in a similar way to an unemployment spell at the track choice.²⁸

My results are also very similar to my estimates using the entire sample when concentrating on the extended benefit sample (row four) or when differentiating by pre-unemployment parental income (rows five and six). The similarity in size of the treatment effect across different sample specifications implies that parents do not necessarily behave in a (long-term) forward looking manner and that the timing of an adverse income shock can play an important role in determining parental investment decisions.

One concern is that my estimates are picking up something else in my data and do not reflect the effect of parental unemployment around times of important education decisions. To investigate this possibility I estimate the impact of parental unemployment a significant period before the actual track choices are made. Specifically, children at age 6 when they first enroll in school and whose parents experience an unemployment spell at this date are considered as treated, whereas children age 4 whose parents had an unemployment spell before school enrollment are considered in the control group.²⁹

The last row of Table 2 presents the estimation results for this sample. Parental unemployment when the child starts school has a very small negative and insignificant effect on obtaining a university degree. These results support my hypothesis that parental unemployment at an important educational decision milestones rather than parental unemployment per se can explain the negative impact on children’s educational attainment that I estimated.

A confounding factor for my findings might be that children of unemployed parents do not achieve the required grade point average in order to advance to the general education track. For example, [Rege et al. \(2011\)](#) show that parental displacement has a negative effect on children’s school performance using data for Norway, while [Gregg, Macmillan and Nasim \(2012\)](#) find a negative impact of parents’ displacement on grades of children for the UK. One explanation for this relationship might be lower financial

²⁸I also estimated the impact by conditioning on the duration of the unemployment spell and post unemployment wage growth. While these estimates certainly do not constitute causal impacts and the results should be interpreted with care, I do not find evidence that children of parents where wages recover faster are more likely to hold a university degree than those of parents with a slower recovery rates. The likelihood of holding a university degree is, however, affected by the length of the unemployment spell with no significant treatment effect for very short durations and a strong effect for longer ones. This supports my hypothesis that a sudden income shocks coupled with lower expectations about the future can reduce parental commitment and lower investment.

²⁹The results for this group are not sensitive if children aged 4 or aged 5 are used in the control group. Arguably, parental investment at age 5 and age 6 should have more similar effects compared to age 4, but to remain consistent with my main specification I choose a 2 years difference between treatment and control group.

resources to spend on tutoring or other educational material for children. Another explanation might be that distress caused by unemployment lead to lower parental investment in general.

While I cannot test and rule out this explanation with my data, I argue that the results in Table 2 do not point toward it as the sole explanation. First, if the negative relationship between parental unemployment and children’s GPAs is driven only by lower financial resources, then one would expect that children of less constraint families would be better able to cope with the negative impact of parental unemployment and to achieve higher educational attainment with the possibility to reverse the track choice later one.³⁰ Hence, one would expect a substantially lower treatment effect for this group compared to more constraint families. My results in Table 2 do not show any large difference in educational attainment between children of constraint and unconstrained families, however.

Second, if mental distress is solely explaining the difference in children’s GPAs and in turn the lower likelihood of attending university, then one would likely expect the effects to be stronger for single parent families compared to two parent families. Single parents might not have readily a partner or social interactions available, two important factors to deal with high levels of stress Schmidpeter (e.g. 2019). Therefore, one would expect larger effects for children of single parent families. Again, my results in Table 2 do not indicate that this is the case.

In addition to the aforementioned reasons, the track admission process offers some flexibility for teachers. Students can attend the higher, general track even if the requirements are not entirely met. This is the case if they showed in general a good performance at school and admission to the higher track is supported by the teacher. Thus, students might be able to attend the higher track even when their GPAs fall shortly before the track choice. In addition, the track choice also depends strongly on the decision of the parents. Around 60% of a student’s probability of taking the higher, general track is explained by parental choice, even after adjusting for students’ grades and other characteristics (Bruneforth et al., 2012); see the discussed in Section 2.

5 Quantile Treatment Effects

The summary statistics presented in Section 3 suggest that it is important to look at the effect of parental unemployment along the outcome distributions. As some of my labor market outcomes of interests are discrete, such as employment and unemployment days, applying the standard quantile regression approach is not a valid strategy. Instead, I

³⁰As already mentioned in the introduction, numerous papers such as Løken (2010); Milligan and Stabile (2011); Dahl and Lochner (2012); Løken et al. (2012); Jones et al. (2015) find in general a positive effect of income on children’s achievement.

will make use of the so-called distribution regression (Foresi and Peracchi, 1995; Chernozhukov, Fernandez-Val and Melly, 2013). This flexible approach enables me to model any type of outcome, such as discrete, continuous, and mixed, and allows the covariates to have a different impact on the outcome at different points of the distribution. The idea and further developments have been used in numerous studies in recent years. For example, Callaway and Huang (2018) evaluate inter-generational income mobility, Kolodziej and Garcia-Gomez (2017) retirement on mental health, and Delgado, Garcia-Suarez and Sant’Anna (2017) the effect of unemployment insurance benefits on unemployment duration. In the following, I explain my estimation procedure in more detail.

The parameter of interest is the quantile treatment effect (QTE) which allows me to obtain the effect of parental unemployment at crucial stages on the distribution of long-term child outcomes. Let Y be the outcome of interest and T denotes the treatment indicator with $T = 1$ for parental unemployment at the important education decision (track choice). For some $\tau \in (0, 1)$, the QTE can be formally defined as

$$\Delta(\tau) = Q_1(\tau) - Q_0(\tau) \quad (1)$$

where $Q_t(\tau)$ is the counterfactual quantile defined as $\inf\{y : F_t(y) \geq \tau\}$ for $t \in \{0, 1\}$. Assuming unconfoundedness and overlap (see, for example, Imbens and Wooldridge (2009)), each counterfactual outcome in Equation (1) can be derived from a corresponding distribution function. I do so by proceeding in two steps. First, I estimate the counterfactual distribution $F_t(y)$ as

$$\hat{F}_t(y) = \frac{1}{N} \sum_{i=1}^N \hat{F}_{Y|X,T=t}(y|t, X_i) \quad (2)$$

In my estimation, I parametrize $F_{Y|X,T=t}(y|t, X_i) = \Lambda(X'_{i,T=t}\beta(y))$ where $\Lambda(\bullet)$ is the logistic function and $X_{i,T=t}$ are the observed covariates for group $T = t$. Hence, $\hat{F}_{Y|X,T=t}(y|t, X_i)$ is estimated by separate logistic regression in the subpopulation with $T = 1$ and $T = 0$. In order to avoid that my results depend on the chosen grid of thresholds y , I estimate (2) for every observed outcome value.

In a second step, I invert $\hat{F}_1(y)$ and $\hat{F}_0(y)$ to obtain the quantities necessary to calculate $\Delta(\tau)$. Inference is based on the Bayesian bootstrap with 999 replications taking clustering on the family level into account (Chernozhukov, Fernandez-Val, Melly and Wüthrich, 2017).³¹

³¹Compared to the nonparametric bootstrap, the Bayesian bootstrap is better in dealing with small cell sizes; see also Chernozhukov et al. (2017).

6 Parental Unemployment and Children’s Long-Term Labor Market Outcomes

In this section, I present my main estimates of the effects of parental unemployment on children’s long term labor market outcomes. Looking at the entire distributional impact, will help to uncover potential heterogeneity in the impact which would be masked by concentrating on just mean effects. The main results are presented in Figure 2.

[Figure 2]

Effect on Days of Employment: The first row of Figure 2 presents the impact of parental unemployment at the track choice on the distribution of average yearly employment days for my three age groups reflecting three different career stages together with 90% confidence intervals.³² Interestingly, I find that parental unemployment affects employment days only for individuals between the 15th and 65th percentile while the rest of the distribution is unaffected.³³ These individuals can be considered as rather “loosely” attached to the labor market.

The effect is strong and significant during the early career stage between the 20th and the 40th percentile. The average yearly increases in days worked is between 22 to 42 days. As can be seen from the figure, this effect slowly fades out as children establish themselves in the labor market. At age 32 to 34, parental unemployment at the track choice is associated with an 8 to 38 days increase in average employment days between the 20th and the 40th percentile. The estimates are only significant different from zero at the lower percentiles, however. At age 34-37, the estimated effect is substantially smaller compared to earlier stages in children’s post school work life and are not statistically different from zero.

The results presented here extend the findings of [Fradkin et al. \(2019\)](#). They find that parental unemployment shortly before the end date of a child’s full-time education increases children’s days worked over the next three years compared to a child whose parents lost their job shortly after the end of the child’s full-time education, but the effect disappears within the next four years. My results show that parental unemployment at crucial educational decision points for children increases the labor supply of these children among those who are loosely attached to the labor market at the beginning of their career. My results indicate that this effect is due to children whose parents experienced

³²Remember that I use the ages 29-31 to define an early stage in an individual’s labor market career and ages 32-34 the stage when individuals most likely are in their career jobs. At ages 35-37 my results are a good approximation for life-time effects; see 3.

³³The treatment effect below the 15th and above the 65th percentile is always estimated to be 0 and therefore not depicted in the graph.

unemployment close to a crucial educational decision point likely being pushed into more vocational oriented education and training which leads to an earlier start in the labor market. As individuals with higher levels of education start to establish themselves in the labor market these early differences slowly disappear.

Effect on Days of Unemployment: Another interesting outcome is the distributional effect of children’s average days spent in unemployment. The second row of Figure 2 plots the estimated treatment effect together with 90% confidence intervals for the three specific age ranges.³⁴ The results reveal two interesting points. First, the estimated treatment effect exhibits substantial heterogeneity along the distribution of unemployment days. It is concentrated at the upper part, also among children with more unemployment days. Parental unemployment at the track choice transmits itself to children by causing these children to experience more unemployment themselves. Second, my results indicate an increasing effect over a child’s working life. At age 29-31, my estimates are largely insignificant and small over most of parts of the unemployment days distribution, with exception of after the 90th percentile. At older ages, the treatment effect is almost monotonically increasing as the quartile cutoffs increase. When children of parents who had an unemployment close to the time of educational decisions are established in the labor market at age 35-37, my estimates show a strong and significant treatment effect on the number of days they are unemployed. Around the 80th percentile, parental displacement at the track choice is associated with a significant increase of 12 days, at the 95th percentile there is a significant increase of 50 days for those age 35-37.

Oreopoulos et al. (2008) find that children of displaced fathers have a higher likelihood of receiving unemployment benefits later in their life. In their analysis, Oreopoulos et al. (2008) use firm closures to instrument for father’s unemployment. As pointed out by Hilger (2016) and discussed in the introduction, one concerned is that the results might be partially driven by selection of fathers into closing firms. I show that even when accounting for potential selection into closing firms experiencing a parental unemployment spell around important educational decision points can have long lasting consequences for children. My results indicate that the unemployment effects are largely concentrated among children prone to becoming unemployed and materialize late during the child’s life-cycle.

Effect on Income: The last row in Figure 2 present the estimation results using average yearly wages as the outcome. Parental unemployment at the time of the child’s track choice has no effect on average income at the beginning of the child’s labor market career (when children are age 29-31). The estimates are rather small and not statistically significant. As shown in Section 4, my treatment leads to a lower probability of children obtaining a university degree and therefore I am likely comparing lower educated children

³⁴The treatment effect below the 60th percentile is always estimated to be 0 and therefore not depicted in the graph.

with relatively more labor market experience to children with higher educational attainment but relatively lower labor market experience at this age range. In Appendix A, I provide additional evidence of this interpretation and show that parental unemployment at the track choice has a *positive* effect on income at the age of 20 to 23, an age when most university students have only a loose labor market attachment, but those with lower levels of education will be more firmly attached to the labor market.

The negative effects of parental unemployment at important educational milestones for their children become noticeable when looking at the income when the children are age 32-34. At this age regardless of their educational attainment all of the children should be relatively well established in the labor market. For this age group, one can observe an almost uniform downward shift in income along the distribution. From the median onward, the drop in income for the treatment group is particularly large. At the upper half of the distribution, having an unemployed parent shortly before the track choice compared to afterward is associated with an average annual income loss of between 1000 and 3000 Euros (\$1200 to \$ 3500) or a drop of roughly 6%. The considerable impact of parental unemployment at the track choice can also be seen when comparing the income loss to the average annual income of 31,000 Euros for workers in the age range between 30 and 39 years.

Looking at the estimated treatment effect at age 35-37, an age where the estimated impact is likely a good approximation for life-time effects, one can see that the magnitude of the income loss is amplified for most part of the distribution above the median. For example, my estimates suggest an average yearly income loss of 1,600 Euros (\$1,800) around the 60th percentile compared to 1,100 Euros (\$1,200) at age 32-34, of 2,600 Euros (\$2,900) compared to 900 Euros (\$1,000) at age 32-34 around the 80th percentile, and 3,500 Euros (\$3,900) compared to 3,00 Euros at age 32-34 around the 90th percentile. As a university degree is arguably more valuable at higher wages, my results imply that lower educational attainment can have long lasting consequences for children with higher income. Assuming that my estimates at age 35-37 indeed persist until the end of the child's working life and assuming an individual retires age of 60 years, parental unemployment at times of important educational decisions reduces the net present value of children's future income at age 35 between 20,000 Euros and 65,000 Euros (between \$22,000 and \$72,000). This simple calculation ignores the impact on retirement benefits as consequences of lower labor income and is therefore likely a lower bound for the true effect.³⁵ My results also indicate that there is no persistent effect for individuals below the median with an insignificantly estimated treatment effect close to 0. The observed strong heterogeneity

³⁵The calculation is based on the assumption that the estimated income losses persist until the age of 60 and individual retires thereafter. It is also assumed that retirement benefits are not affected by lower income. As the calculation of retirement benefits are based on the best years of an individual's working life, my calculation likely understates the impact of parental unemployment. The yearly discount rate was set to 3%.

along the income distribution highlights the importance of considering statistics beyond the mean.

While only roughly comparable, my estimates of the long-run effects of parental unemployment on income at the upper part of the distribution are lower than the mean effects found by [Oreopoulos et al. \(2008\)](#), but higher compared to those in [Huttunen and Riukula \(2019\)](#). [Oreopoulos et al. \(2008\)](#) find that paternal displacement in the U.S. decreases age-adjusted income by around 9% while my results indicate a loss of around 6% to 7% at age 35-37 for children at the upper half of the distribution and no effect at the lower half. Using administrative data for Finland and involuntary job displacement as treatment, [Huttunen and Riukula \(2019\)](#) find a negative impact of around 600 Euros on annual earnings measured at age 30. Using a modified difference-in-difference strategy to estimate the effect of parental unemployment, [Hilger \(2016\)](#) and [Fradkin et al. \(2019\)](#) do find very small effects on log-earnings and salary.³⁶

The different outcomes in previous studies might reflect the different identification strategies and as a result different implications of the estimated results. In my analysis, I exploit the timing of the parental unemployment shock and identify the importance of constraints at important educational decision points on children’s educational attainment and future labor market outcome. My identification exposes children to the same experience and to a similar impact on family income during childhood, and avoids inherent biases that may be embodied in the comparison of children of displaced versus non-displaced parents. For example, [Kramarz and Skans \(2014\)](#) show that many children find their first job in parent’s establishments. Children benefit from the parental network by having better labor market outcomes and higher wages few years after labor market entry. Parental unemployment weakens these network effects, however ([Huttunen and Riukula, 2019](#)). Under my identification assumption, children in both treatment and control group should be exposed to the same environment. Hence, my results of the impact of parental unemployment are *net* of these effects.³⁷

An alternative explanation for my findings might be that adverse economic conditions when individuals start their labor market career can explain the estimated wage loss I observe. As children whose parents experience unemployment at the time of important educational decisions are encouraged to follow a track that involves less years of schooling these students enter the labor market at a different time and maybe different economic conditions compared to children in my control group. Previous research has shown that, especially among college graduates first becoming unemployed during an economic downturn leads to lower life-time earnings and inferior employment ([Kahn, 2010](#);

³⁶[Hilger \(2016\)](#) also emphasizes the role of parental selection into closing firms and shows that ignoring this issue can lead to severely biased estimates.

³⁷This argument does not hold if the timing of the unemployment spell affects the network of parents differently. The results reported in [Huttunen and Riukula \(2019\)](#) do not indicate that this is the case.

Oreopoulos, von Wachter and Heisz, 2012; Altonji, Kahn and Speer, 2016). In contrast, lower skilled individuals seem to be less affected by initial labor market conditions and what effect there is is less persistent (Genda, Kondo and Ohta, 2010; Cockx and Ghirelli, 2016; Speer, 2016)).

The years during which I measure my long-term outcomes were marked by stable economic growth, however.³⁸ In addition, my results indicate that parental unemployment close to important educational decisions for their children leads to lower educational achievement and income losses in the long-run for children, while I do not find any significant differences at the beginning of the labor market career for these children. I also find that days of employment are higher for individuals in my treatment group, contrary to the finding in the literature on initial conditions.³⁹ Overall, initial conditions are unlikely to explain my results.

The results in this section show a negative impact of parental unemployment at their children’s important educational milestones on the long-term labor market outcomes of these children. Looking at the distributional effect rather than just the mean reveals substantial heterogeneity. Unemployment prone children and those at the upper half of the income distribution are particularly affected in the long-run. I do not find any significant effects at the beginning of their labor market career, however. One explanation for the differential impact early and late in these children’s careers is that the impact of lower educational achievement is partially dampened by an increase in labor market experience at the beginning of the career of children in my treatment group. The children in the treatment group were encouraged to pursue lower levels of education and technical training. As this trade-off between time in school and time in the labor market becomes weaker over the working life of these children, the effects of parental unemployment start to materialize. I investigate this explanation further in Section 7. In the next section, I present additional results and investigate the robustness of my estimated effects.

6.1 Heterogenous Results & Robustness

In this section, I provide heterogeneous results for interesting subgroups, mirroring the estimates for educational attainment in in Section 4. I also provide evidence of the robustness of my estimates by showing that the labor market outcomes of children whose parents had an unemployment spell around school starting age are not affected.

First, I estimate the differential impact for children of two parent households compared to children of single parent households. Second, I estimate the effects for my

³⁸The outcome measured at age 35 to 37 in my sample corresponds to the years 2010 to 2016. The average real quarterly growth rate in the GDP during this time period was around 0.4%.

³⁹For example, Cockx and Ghirelli (2016) show that hourly wage of lower educated individuals is hardly affected by taking up employment in adverse conditions but working time is. Although I do not have hours worked in my data, my findings do not point toward this interpretation.

Extended Benefits sample. These parents can be regarded as highly productive and attached to the labor market. They might have better information and are better in guiding their children in choosing a “safe field” of work later on (see [Huttunen and Riukula, 2019](#)). If this is the case, one would expect to find lower estimates for this sample compared to the results presented for the entire one.

To gauge the robustness of my results, I also consider the long-run effects for children of parents displaced around their children’s school enrollment (age 6) compared to earlier (age 4). If my baseline results for children’s long term labor market outcomes are driven by unobserved components and not the parental unemployment spell at an important educational milestone then one would expect to find comparable patterns for this subgroup, that is a flat impact at the lower part of the income distribution and increasingly strong and negative effects at the upper part of the distribution. Notice, however, that the direction and magnitude of the overall effect is not entirely clear a priori for this group. As parental investment is more valuable at a younger age range ([Del Bono et al., 2016](#)), it might dis-proportionally affect younger children when parents spent more time with their children as a response to the unemployment spell. Likewise, they might also be negative long-term impacts if parents decrease the time spent with their children when they were young as response to the unemployment spell.⁴⁰

Figure 3 depicts the results for my three different sub-samples. Looking at the results for the two parent sample presented in the first row, one can see that the estimates for this sub-sample are remarkably similar to the ones obtained for my entire sample. The confidence intervals are slightly wider, however. This is not surprising as approximately 30% of the sample was excluded when I restricted the sample to children from two parent households. At age 29-31, the estimated treatment effect is mainly concentrated around zero along the income distribution with very wide confidence intervals. When children are fairly established in the labor market at age 35-37, however, I find a similar pattern as before. The estimated effect at the lower part of the distribution is rather small and not statistically significant but large and increasing above the 60th percentile. The estimated magnitude of the yearly loss is comparable to the ones obtained using the entire sample. Children in my treatment group around the 60th percentile earn now around 1,100 Euros less compared to 1,600 Euros for the full sample and those at the 80th percentile earn around 3,000 Euros less compared to 2,400 Euros for the entire sample. The estimates do not indicate that two parent families are better in dealing with unemployment at important milestones.

The second row in Figure 3 shows the results for my Extended Benefits sample. As discussed in Section 4, unemployed parents in this sample are eligible for 30 weeks of benefits rather than 20 weeks. This might give them more opportunities to look for

⁴⁰For brevity, I only report the initial and long-term estimates, and only for yearly income. The complete set of estimates is available upon request.

better reemployment options. With more opportunities to look for better employment the effects of being unemployed could be muted and, hence, these parents might be willing to invest more in their children when they become unemployed.

The estimation results do not support this hypothesis. I find a similar pattern along the income distribution for the restricted sample of parents who are eligible for 30 weeks of benefits as I did for my entire sample. For the restricted sample, at the beginning of a child's labor market career there is no impact with effects around zero over most part of the distribution. Later, when the child is well established I find strong negative effects at the upper part of the distribution. The estimated impact is remarkably similar to the baseline estimates in both pattern and magnitude.

The last row in the Figure presents my estimation results for children whose parents were displaced around school enrollment age. Notice that in order to accommodate the wider confidence intervals, the scale is different than in the figures presented in the previous panels. Children whose parent had an unemployment spell before enrollment at age 4 are now in the control group, and children with parents after the school enrollment at age 6 constitute the treatment group.

Looking at the results for children whose parents experienced as spell of unemployment at the time they were enrolled in school, two features become apparent. First, almost all of my estimated effects along the income distribution are not statistically significantly different from zero for both early and long-term labor market outcomes. Second, instead of a down-ward shift in the estimated effect when children were more stable in the labor market, as was observed for my baseline results, these estimates indicate that parental unemployment at enrollment age has a weakly positive (but not significant) effect on long-term earnings. This might be the result of changes in early childhood parental investment into the child. The results for this sample support my hypothesis that my main results are due to parental unemployment at times of important educational decisions and not the result of parental unemployment per se.

[Figure 3]

The results from my sub-sample analysis show that my main estimates are not driven by children from two parent household or of families with longer benefit eligibility. The obtained quantile treatment effects are remarkably similar both in magnitude and shape for my sub population analysis as those obtained for the entire sample. In addition, when considering children whose parents had an unemployment spell before the important educational decision milestone I do not find any effects on either short and long-term income. This is in line with the estimates for educational attainment discussed in Section 4.⁴¹

⁴¹Despite my best efforts to account for selection into unemployment, being able to include a rich set of control variables, and the reassuring outcome from my robustness checks, there might be still some

7 Decomposition of the Treatment Effect

In light of my discussion above, it is interesting to see how much of my total treatment effect can be traced back to parents willingness to invest in their children as embodied by the educational tracking decision and how much can be explained by other mechanisms. Education is an important determinant of later labor market success but parental unemployment can also affect indirectly other aspects in a child's life, for example, through divorce (Rege, Telle and Votruba, 2007; Eliason, 2012), health problems (Sullivan and von Wachter, 2009; Eliason and Storie, 2009; Browning and Heinesen, 2012), and future fertility (Del Bono, Weber and Winter-Ebmer, 2012; Huttunen and Kellokumpu, 2016). Parental unemployment also could lead parents to encourage different amounts of risk taking by their children or induce their children to select "safer" occupations (Choi, Kariv, Müller and Silverman, 2014; Cole, Paulson and Shastry, 2014; Black, Devereux, Lundborg and Majlesi, 2015; Huttunen and Riukula, 2019). To shed more light on the connection between lower parental investment due to unemployment at the track choice and wages, I decompose my treatment effect.

7.1 Decomposition of Quantile Treatment Effects

Before I present the results of my decomposition analysis, I briefly discuss my approach. Denote by E the educational attainment of the child, i.e. if she has obtained a university degree ($E = 1$) or not ($E = 0$). Define by $E(t)$ the potential educational achievement and rewrite the potential outcome as a function of treatment and potential education $Y(t) = Y(t, E(t))$. In order to evaluate the impact of forgone education as a result of early parental displacement on my outcomes, I can decompose $\Delta(\tau)$ into two parts

$$\begin{aligned} \Delta(\tau) &= Q_1(\tau) - Q_0(\tau) \\ &= \underbrace{Q_{(1,E(1))}(\tau) - Q_{(1,E(0))}(\tau)}_{\gamma(\tau): \text{Effect due to Changes in Educational Attainment}} + \underbrace{Q_{(1,E(0))}(\tau) - Q_{(0,E(0))}(\tau)}_{\theta(\tau): \text{Residual Effect}} \end{aligned} \quad (3)$$

At a given τ , $\gamma(\tau)$ captures the effect of changes in educational attainment due to unemployment at the important milestone compared to unemployment after the decision has been made on the distribution of labor market outcomes. By holding the treatment status constant and varying educational attainment of the child, it allows me to quantify the importance of foregone education along the outcome distributions as possible mech-

concern about omitted variable bias. In Appendix A.5, I present estimates from a model without including *any* control variables. The results are virtually identical to the ones presented here in terms of magnitude and significance. This gives me reassurance that any remaining bias is likely small in my analysis.

anism. If early displacement around the track choice indeed has long-term effects on the labor force performance of children, $\gamma(\tau)$ will be negative. $\theta(\tau)$ is the residual effect and contains all other possible channels not captured by E .

It should be emphasized that the decomposition in Equation (3) only captures the direct effect of educational attainment on future labor market outcomes. There are other important determinants which are affected by parental job loss and education, such as taste for risk taking, economic rationality, and occupational choice (Choi et al., 2014; Cole et al., 2014; Black et al., 2015; Huttunen and Riukula, 2019). These channels are not captured by my estimates of $\gamma(\tau)$, but are included in the residual effect. Therefore, it is likely that my decomposition is underestimating the effect of foregone educational attainment on future labor market outcomes.

Under a so-called sequential conditional independence assumption (SCIA), the decomposition in Equation (3) can be given a causal interpretation. The SCIA is stronger than the unconfoundedness assumption imposed in the previous section and requires not only that the treatment is (conditionally) independent of the potential outcomes and attending university but also that there is no selection into attending university with respect to the outcome, once I control for treatment status and my observed covariates; see, for example, Imai, Keele and Yamamoto (2010) for a discussion of this.⁴²

There is strong support that SCIA holds in my analysis. First, exploiting the timing of unemployment rather than comparing employed to unemployed parents minimizes the issue of selection. This can also be seen by the small differences in background characteristics as reported in Table 1 and by the similar family income path pre- and post-unemployment between both groups, as shown in the previous section. Second, while the educational path of children is chosen shortly after the unemployment spell in my treatment group, labor market outcomes are determined at least 10 years after this point in time. Labor market outcomes thus cannot be perfectly foreseen either by parents or children. Although parents might anticipate that there is a higher wage gain when they choose the university track for their child, there is a certain level of uncertainty how future outcomes will materialize. Furthermore, the statistics provided in Section 2 show that, although there is the possibility to reverse track choice later on, this is not easy and only a small share of children do so. In addition, I control for a wide-range of characteristics which influence the selection into universities and employment, such as parental education level. But even if my decomposition does not provide a complete and causal description of pathways, it provides useful information about the scarring effects parents' unemployment can have on their children, and the potential for earnings

⁴²In addition, my estimation approach requires that θ is homogenous in E . The decomposition in Equation (3) is commonly referred to as mediation analysis and θ and γ are the direct and indirect effect respectively; see, for example, Huber, Lechner and Mellace (2016), Chen, Chen and Liu (2017), and Schmidpeter (2018) for recent applications in economics.

inequality to be transmitted to future generations via poor economic events experienced by a child's parents.

In order to estimate the missing counterfactual quantities in Equation (3), I first obtain the residual effect $\theta(\tau)$ by estimating $F_{Y|X,U=u,T=t}(y|t, U_i, X_i) = \Lambda(X'_{i,T=t}\beta(y) + \alpha(y)E_{i,T=t})$. This is done via logistic regressions separately for children in my treatment and control group respectively. Notice that the difference from the econometric approach outlined above is that the education dummy E is now included in the estimation. The counterfactual quantity $\hat{F}_{t,E(t')}(y)$ for $t, t' \in \{0, 1\}$ is obtained by integrating over the distribution of U and X , similar as in Equation (2). These estimates are then inverted to obtain $\hat{\theta}(\tau)$, as defined in Equation (3). In order to obtain $\hat{\gamma}(\tau)$, I subtract $\hat{\theta}(\tau)$ from my previous estimates of $\hat{\Delta}(\tau)$.

7.2 Decomposition Results

Figure 4 presents the results of my decomposition analysis. The solid line depicts the total quantile treatment effect $\Delta(\tau)$ and the dashed line my estimates for $\theta(\tau)$. The shaded region between both lines represents the estimates of $\gamma(\tau)$. For expositional reasons, I only report results for the 29-31 and 35-37 age range.

The first two rows in Figure 4 show the results of my decomposition for days of employment and unemployment. My estimates for $\gamma(\tau)$ at age 29-31 indicate that for children relatively loosely attached to the labor market lower parental investment due to unemployment at the track choice explains between 10 to 40% (or 1 to 10 days) of the total treatment effect for employment days. For example, around the median the estimated total treatment effect is 12 days out of which 4 days can explained directly by lower parental investment. I come to a similar conclusion when looking at the effect on unemployment days. There also is no strong evidence that the effects are changing over the life-time of the children

The impact of lower parental investment as a result of parental unemployment at the track choice becomes more pronounced when when looking at the decomposition results for average yearly income, presented in the third row in Figure 4. The results highlight again the largely heterogeneous impact of early parental job loss. During both, early career and later on, lower educational achievement as a response to early parental unemployment has a negative impact on yearly income at the upper half of the distribution. The impact is, however, largely negligible below the median. As conjectured, obtaining higher education is indeed more valuable at higher wage levels.

I also find that the negative consequences of lower parental investments intensify over a child's life cycle at the upper part of the income distribution. At age 29-31, percentile children in my treatment group at the 60th percentile earn 190 Euros less per year and children at the 80th percentile earn 390 Euros less per year solely due to lower parental

investment at the track choice. My estimates for $\gamma(\tau)$ increase to 350 Euros at the 60th percentile and to 900 Euros at the 80th percentile four years later. These effects are quite sizable and can explain up to 45% of the estimated total treatment effect.⁴³

The findings in this section clarify why I do not find stronger total effects at the early labor market stages and support my hypothesis. At the beginning, there is a trade-off between experience and higher education. As higher education becomes relatively more valuable at later stages of the life-cycle the impact of sub-optimal educational decisions of parents start to materialize.

[Figure 4]

As a robustness check, I decompose the estimated treatment effect for children where parental unemployment occurred at the child's school enrollment age. If not experiencing an unemployment spell at the time of important parental educational decisions for their children but other factors are driving parents' investment decision and thus my decomposition results, one would expect to find similar effects for $\gamma(\tau)$ for the school enrollment age group too. For brevity, I only report the decomposition results for yearly income in Figure 5 and only for the age ranges 29-31 and 35-37.⁴⁴

From Figure 5 one can see that my estimates of $\Delta(\tau)$ and $\theta(\tau)$ are virtually identical along the income distribution for the school enrollment age group. Potential lower educational achievement as a result of parental unemployment does not explain much in the total estimated effect for this sample and my estimates of $\gamma(\tau)$ are very close to 0. These results support my hypothesis that a substantial part of my long-run estimates can be explained by sub-optimal parental investment decisions around the schooling track choice.

[Figure 5]

8 Conclusion

In times of rising economic uncertainty it is crucial to understand the intergenerational transmission of parental unemployment. In this paper, I provide evidence that unemployment at important parental educational decisions for their children can have long-lasting and negative impacts on children. Using administrative data for Austria and using the

⁴³Remember that other important determinants of labor market success affected by education such as risk taking and decision-making quality consistent with economic rationality are not captured by my estimates of $\gamma(\tau)$.

⁴⁴The decompositions of non-monetary outcomes lead to similar conclusions and are available upon request.

early track choice at age 10 inherent in the Austrian Education system to define such an important parental educational decision point, I compare long-term labor market outcomes of children whose parents had an unemployment spell shortly before and after the track choice.

My estimates show that parental unemployment at an important decision milestone compared to afterward leads to a 5 percentage points lower probability of obtaining a university degree for affected children. I provide evidence that this difference cannot be explained by different access to family resources. A more likely explanation is that short-term constraints coupled with lower parents' expectations about their future employment possibilities affect the educational decisions. These lower employment expectations cause parents to want to reduce their future financial responsibilities. Because students pursuing university degrees are heavily dependent on financing from their parents, unemployed parents desire to reduce future financial responsibilities may induce them to select a lower educational track for their children.

The lower probability of obtaining a university degree can translate directly into worse labor market outcomes – fewer days of employment, more days of unemployment and lower income. Investigating the impact of parental displacement at important educational decisions on these labor market outcomes, my results reveal substantial heterogeneity along the outcome distribution of interest and across children's life-cycle. For example, I do not find any impact on yearly income at age 29-31, a relatively early stage in the child's life-cycle, but my results show that income gradually deteriorate afterward. At age 35-37, when children are well established in the labor market and effects have a likely persistent impact on life-time earnings, I estimate that parental unemployment at important educational decisions lowers children's yearly income by 1,500 to 3,700 Euros, or around 6 percentage points, at the upper part of the distribution. These estimates imply at age 35 a net present value of lower future income of up to 65,000 Euros. Applying a decomposition, I find that 20% to 50% of these losses can be explained by lower parental investment as a result of the unemployment spell at the educational decision milestone. In addition, I find a positive effect on days spent in unemployment and show that this effect is largely concentrated among children prone to being unemployed.

My results show that economic uncertainty occurring at important parental decision points can negatively affect the prospects of the next generation. Affected children are more susceptible to future economic shocks and they have lower life time earnings. This vulnerability to adverse economic shocks together with lower life time earnings may put additional pressure on public finances and social support program. The results of this paper also show that the effects of adverse economic shocks on parents can be translated into more earnings inequality in their children's generation. This transmission of adverse effects to unemployed individuals' children has important implications for policy makers.

While it is impossible to eliminate economic uncertainty, the results of this paper show that it is important for policy makers to keep in mind when designing policies and social security systems the large indirect costs for the next generation of unemployment of the current generation. Policies can be designed to mute the transmission of these effects across generations. For example, social security systems might be planned in such a way that they take the life-cycle status of children into account.

A more direct implication of my findings is that the provision of financial support or student loans for attending higher education might be able to mitigate the intergenerational effects of parental unemployment. This mitigation could occur if the financing for obtaining university degrees took into account families' past employment histories. Although many countries have student loan systems in place, many of these systems take into account parents current earnings and often have parents serve as guarantors of the student's loans. In several countries, such as Austria, the typical student loans are insufficient to cover students living expenses, and parents need to continue to provide for their children while they are enrolled in school. My results show that parents needing to support their children while they are enrolled in school and potentially guarantee student loans, will cause them to choose less expensive schooling tracks if these parents suffer an adverse economic shock at the time of a crucial schooling decision for their children. If, however, parents knew that their past employment histories would be taken into account when their children applied for student loans or loans covered more of students expenses, parents suffering adverse economic shocks may make different educational decisions earlier in the children's schooling. Without changes in the financing for attending schools, the results of this paper show that the negative effect of parents experiencing an adverse economic shock will be transmitted to their children by lowering these children's' educational attainment, decreasing their earnings, and increasing the instability of their employment.

Tables

Table 1: Summary Statistics by Treatment Status

	Treatment Early Unemployment $T = 1$	Control Late Unemployment $T = 0$	Raw Difference
Main Earner			
<i>Personal Characteristics</i>			
Age at Birth (Years)	27.94	28.05	-0.11
Female (%)	29.59	33.21	-3.62**
Single (%)	30.88	35.76	-4.88***
Non-Austrian (%)	5.10	5.36	-0.26
No. of Children	1.81	1.74	0.07***
University Degree (%)	3.81	4.73	-0.91
<i>Previous Labor Market Outcomes</i>			
Av. Log Income	9.93	9.93	-0.00
Av. Employment (Days)	332.43	334.12	-1.69
Av. Unemployment (Days)	13.23	11.34	1.88**
<i>Last Employed Sector (Shares)</i>			
Production	27.01	29.44	-2.43
Construction	17.84	14.69	3.15**
Commerce	18.56	19.09	-0.54
Tourism	4.54	3.38	1.15*
Traffic	3.97	3.51	0.46
Banking	6.03	6.45	-0.42
Health	4.48	5.24	-0.75
Other	17.58	18.20	-0.62
Spouse			
<i>Personal Characteristics</i>			
Age at Birth (Years)	25.94	26.57	-0.62**
Non-Austrian (%)	2.42	2.94	-0.51
University Degree (%)	2.01	2.23	-0.22
<i>Previous Labor Market Outcomes</i>			
Spouse Employed (%)	22.16	24.01	-1.85
Av. Log Income	9.41	9.46	-0.06
Children			
<i>Personal Characteristics</i>			
Female	48.36	47.31	1.06
University Degree (%)	24.83	30.67	-5.84***
<i>Log Average Income at Age 35-37</i>			
Mean	10.00	10.03	-0.03
10th Percentile	8.90	8.94	-0.04
30th Percentile	9.77	9.77	-0.00
50th Percentile	10.20	10.23	-0.03
70th Percentile	10.47	10.54	-0.07***
90th Percentile	10.83	10.90	-0.07***
<i>Unemployment at Age 35-37</i>			
Mean	29.51	23.12	6.39***
P(UE-Days ≥ 10)	25.70	23.03	2.67*
P(UE-Days ≥ 30)	18.47	20.69	2.22*
P(UE-Days ≥ 90)	8.89	11.46	2.57***
No. of Children	2,078	1,689	
No. of Families	1,940	1,566	

*, **, *** indicate a significance difference at a 10%, 5% and 1% level.

The treatment group consists of all 10-years old children and the control group consists of all 12-years old children where the main earner had an unemployment spell in the years 1987-1989. Parental log average income, employment, and unemployment were measured using the 5 years preceding the reference date.

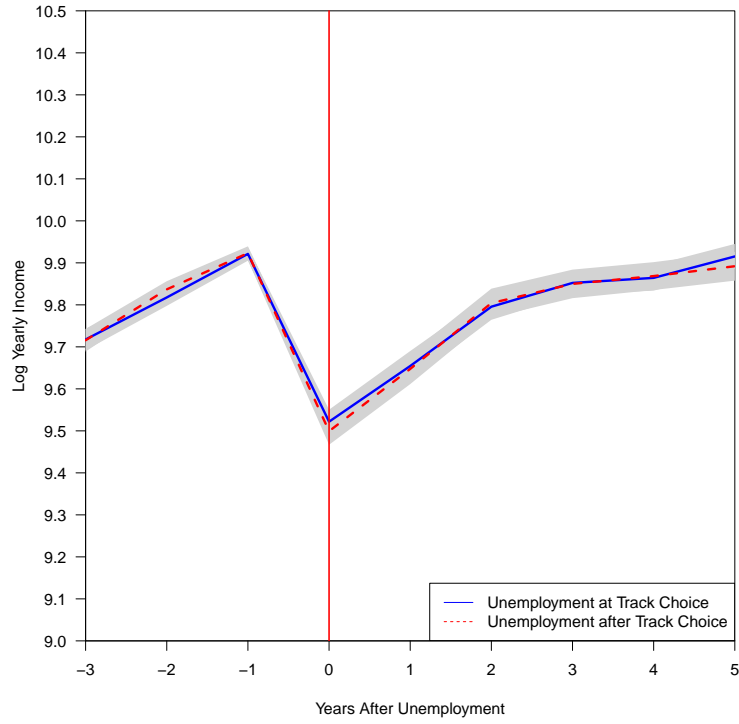
Table 2: Effect of Early Displacement on Children’s Educational Achievement

	Counterfactual $E[E(1)]$	Counterfactual $E[E(0)]$	Treatment Effect $E[E(1) - E(0)]$
Total Sample	25.11 [23.24, 27.03]	29.99 [27.90,32.02]	-4.88 [-7.43,-2.29]
Two-Parent	27.88 [25.74, 30.18]	32.91 [30.26,35.45]	-5.03 [-8.20,-1.83]
Single	19.19 [16.21, 22.36]	23.68 [20.72,26.92]	-4.49 [-8.73,-0.20]
Extended Benefits	27.21 [25.16, 29.53]	31.96 [29.61,34.76]	-4.74 [-7.91,-1.70]
Lower Income	20.70 [18.07,23.40]	26.58 [23.24,29.65]	-5.87 [-9.65,-2.00]
Higher Income	47.19 [41.99, 52.20]	52.96 [47.95,58.12]	-5.77 [-12.46,0.01]
Around School Enrollment	23.36 [19.16, 28.23]	24.05 [22.19,25.96]	-0.69 [-5.04,4.48]

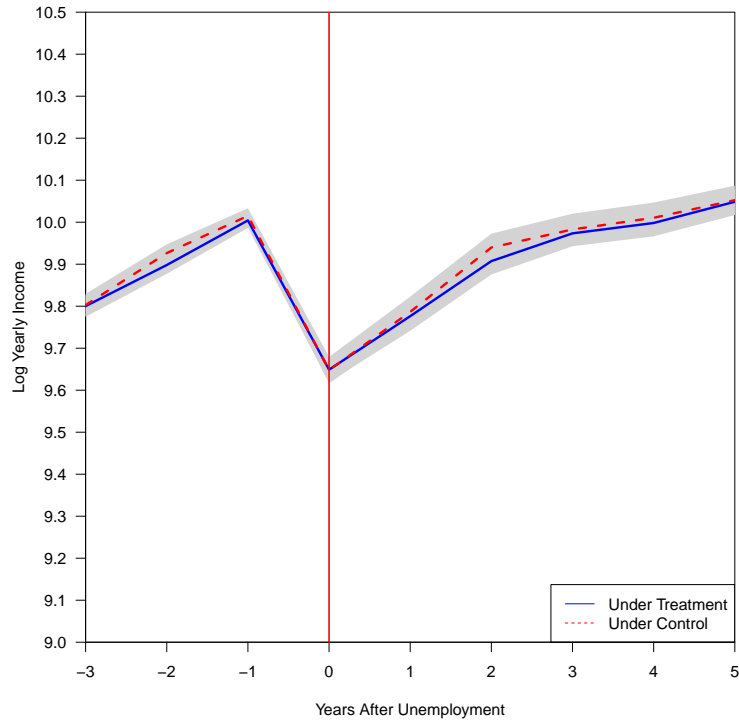
95% confidence intervals obtained from 999 bootstrap replications accounting for clustering within families are reported in brackets. The outcome E is a dummy variable which takes value one if the child holds a university degree. $E[E(1)]$ and $E[E(0)]$ are obtained by separate logistic regression in the sub-population with $T = 1$ (early unemployment) and $T = 0$ (late unemployment). The *Total Sample* consists of all 10-years old children (treated) and 12-years old children (control) born before September where the main earner had an unemployment spell in the years 1987 -1989. The *Two-Parent* includes only children of parents who specified a spouse when applying for child benefits. Likewise, the *Single* sample includes only children of parents which did not state a spouse when applying for child benefits, the *Extended Benefits* includes only children of parents where the main earner is eligible for at least 30 weeks of unemployment benefits instead of 20 weeks. An individual is eligible for the extension if, during the three years preceding the current unemployment spell, he did not claim benefits for more than 20 weeks and was employed for at least 152 weeks. The lower income sample includes all children where the average income of the main earner three years prior to the unemployment spell is at most 18,000 Euros per year. Likewise, the higher income sample includes all children where the average wage of the main earner was at least 30,000 Euros per year. The *Around School Enrollment* sample consist of all 6-years old children (treated) and 4-years old children (control) born before September where the main earner had an unemployment spell in the years 1981 -1983.

Figures

Figure 1: Effect of Unemployment on Parental Income



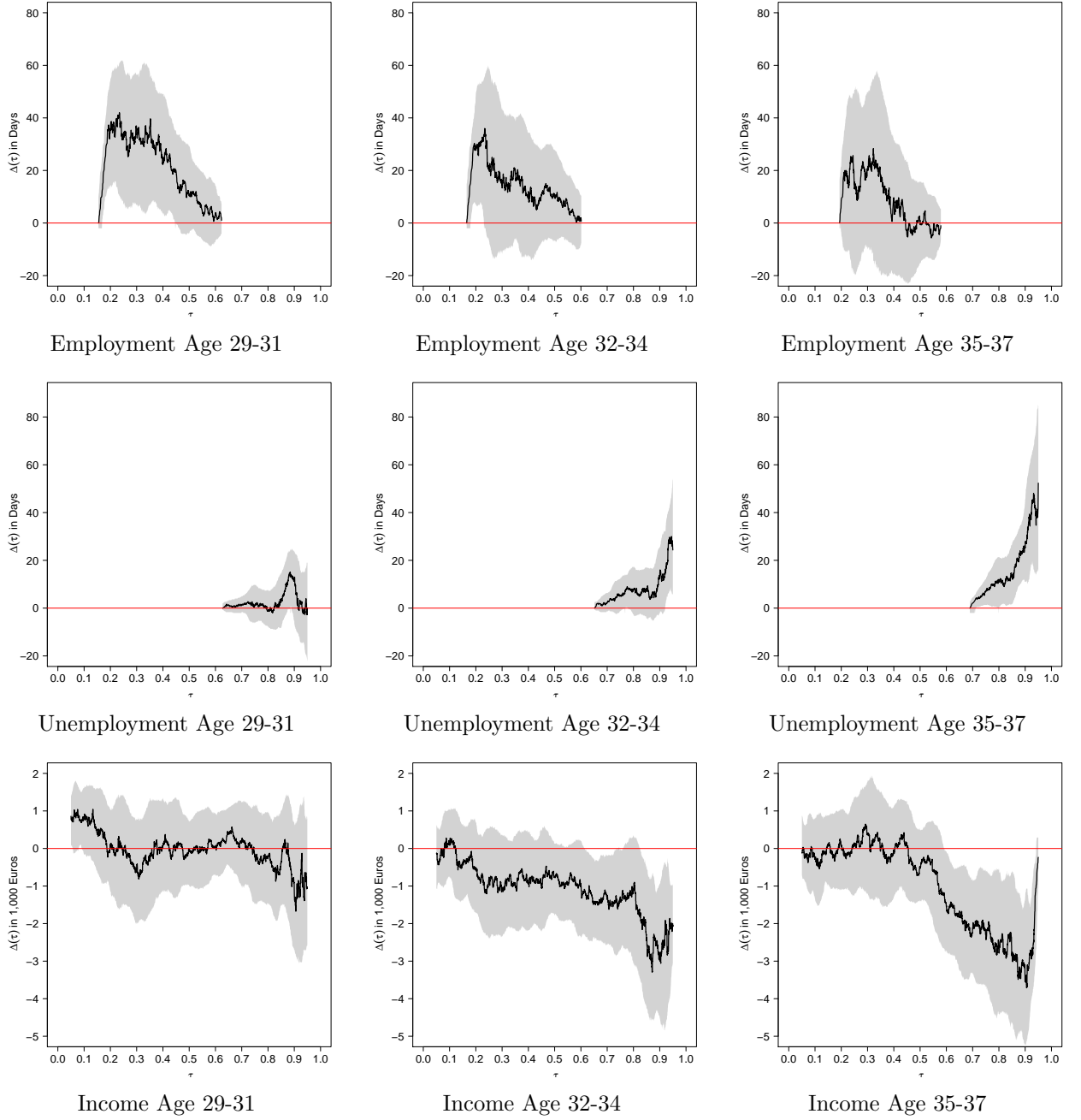
a. Income of Main Earner



a. Income of Family

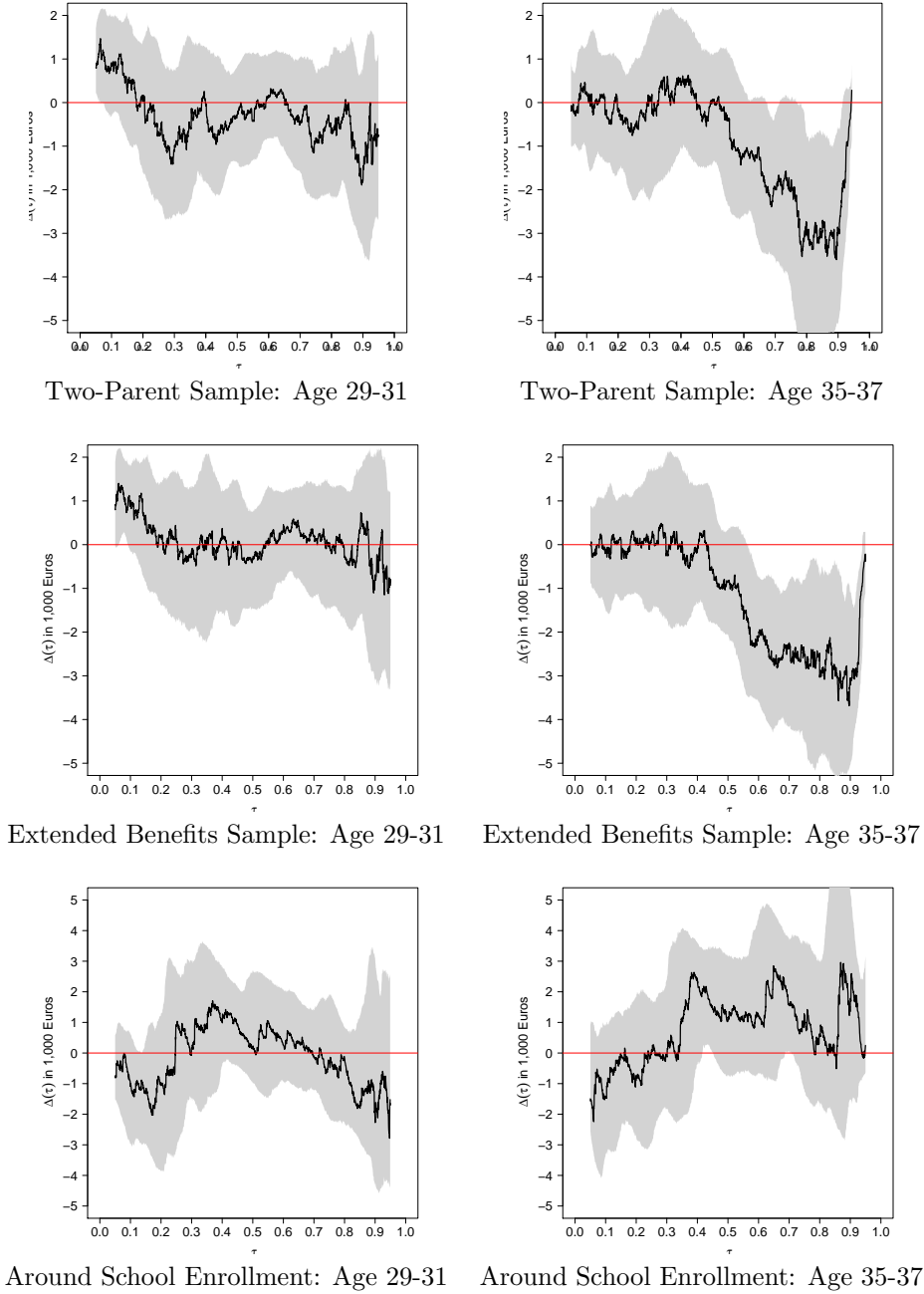
$E[\omega(1)]$ is depicted by the solid line, $E[\omega(0)]$ by the dashed line. Estimates are obtained by separate linear regression in the sub-population with $T = 1$ (early unemployment) and $T = 0$ (late unemployment). Family Income is the sum of the income of the main earner and the spouse. If the spouse did not work or there was no spouse present, spousal income was set to 0. The shaded region corresponds to a 95% confidence interval. Note that confidence intervals for $E[\omega(1)]$ and $E[\omega(0)]$ are overlapping.

Figure 2: Treatment Effect of Parental Unemployment on Children's Long-Term Outcomes



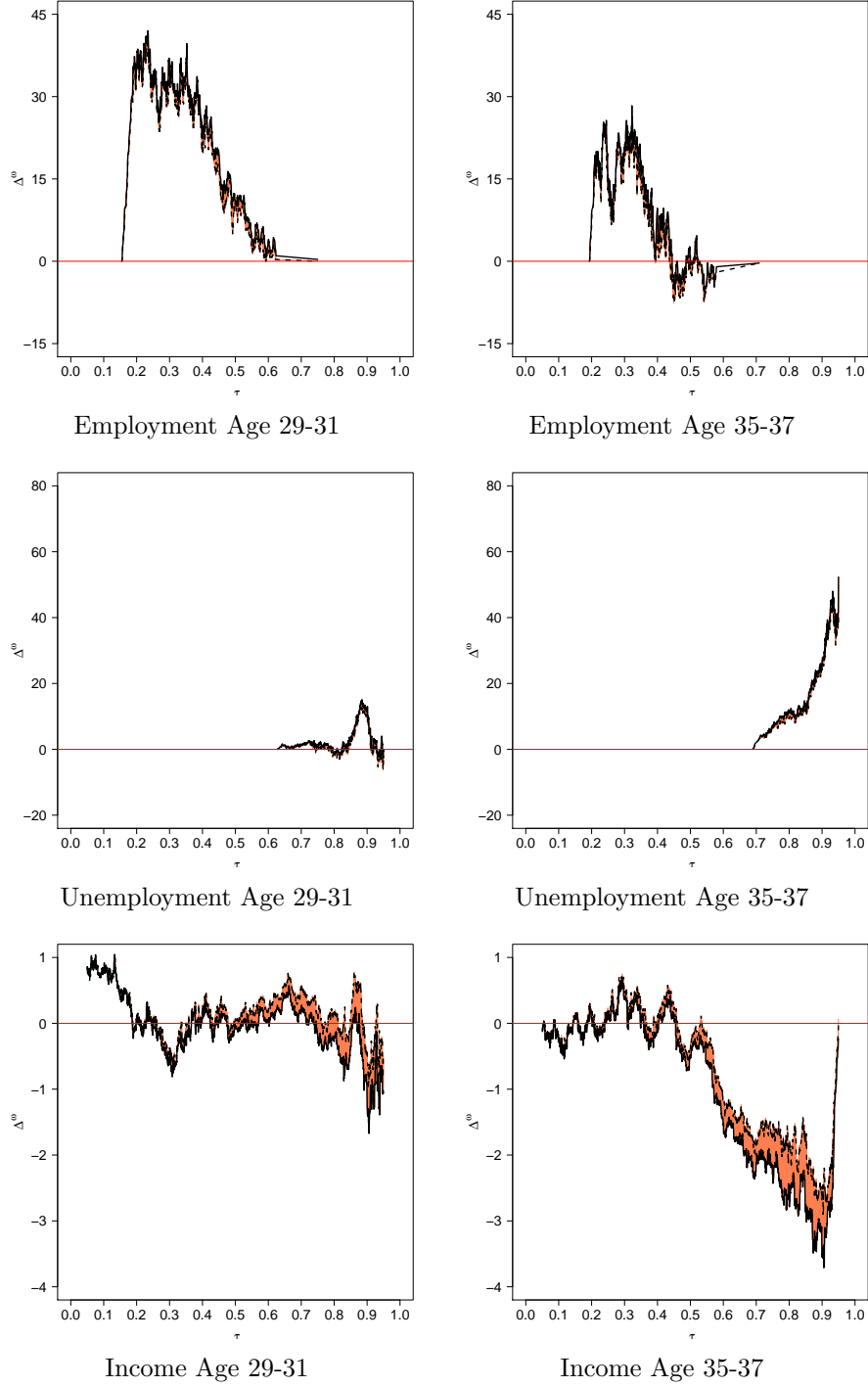
The figure presents estimates of the treatment effect $\Delta(\tau)$ and quantile $\tau \in [.05, .95]$ together with 90% confidence intervals. Outcomes are obtained by using a 3-years average of children's yearly employment and unemployment days as well as income at age 29-31, 32-34, and 35-37. The sample consists of all children who were born before September, were 10 or 12 years old in 1987-1989 and the main earner in the family had an unemployment spell during this time. The treatment group (early displaced) consists of all 10 year old children, the control group of all 12 year old children. $\Delta(\tau)$ is obtained by inverting estimated counterfactual distribution functions as outlined in Section 5. The treatment effect for employment days and $\tau < .15 \cup \tau > .65$ as well as for unemployment days and $\tau < .60$ is always estimated to be 0 and therefore not depicted in the figure. Inference is based on the Bayesian Bootstrap with 999 replications taking clustering on the family level into account.

Figure 3: Heterogenous Effects of Early Parental Unemployment on Children's Long-Term Income



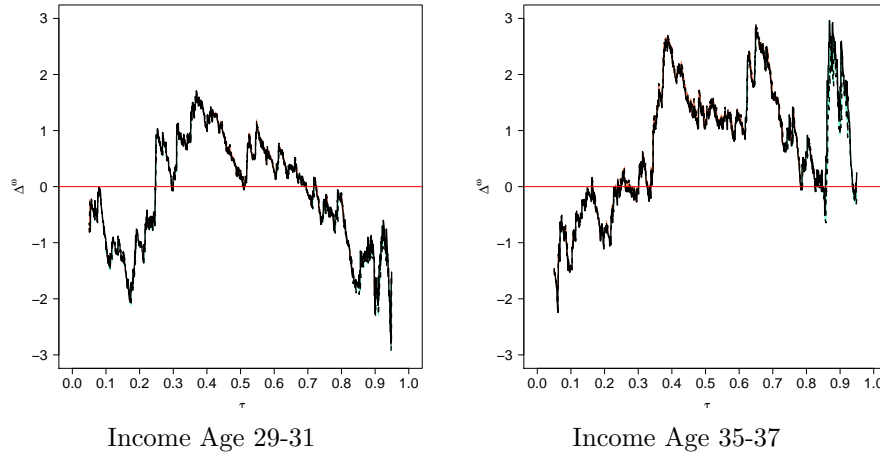
The figure presents estimates of $\Delta(\tau)$ and $\tau \in [.05, .95]$ together with 90% confidence intervals. Outcomes are obtained by using a 3-years average of children's yearly income at age 29-31, 32-34, and 35-37. The baseline sample consists of all children who were born before September, were 10 or 12 years old in 1987-1989 and the main earner in the family had an unemployment spell during this time. The *Two-Parent* includes only children of parents from this sample who specified a spouse when applying for child benefits. The *Extended Benefits* includes only children of parents where the main earner is eligible for at least 30 weeks of unemployment benefits instead of 20 weeks. The *Around School Enrollment* sample consist of all 6-years old children (treated) and 4-years old children (control) born before September where the main earner had an unemployment spell in the years 1981-1983. Notice, in order to accommodate the wider confidence intervals obtained for this sample, the scale is different to the previous figures. $\Delta(\tau)$ is obtained by inverting estimated counterfactual distribution functions as outlined in Section 5. Inference is based on the Bayesian Bootstrap with 999 replications.

Figure 4: Decomposition of the Treatment Effect $\Delta(\tau)$



The figure presents the results of the decomposition for the treatment effect $\Delta(\tau)$ and quantile $\tau \in [.05, .95]$ as outlined in Section 5. The total effect $\Delta(\tau)$ is depicted by the solid line, the direct effect $\theta(\tau)$ by the dashed line. The area between both lines corresponds to the indirect effect $\gamma(\tau)$. The mediator U is holding a university degree. Outcomes are obtained by using a 3-years average of children's yearly employment and unemployment days as well as income at age 29-31 and 35-37. The sample consists of all children who were born before September, were 10 or 12 years old in 1987 -1989 and the main earner in the family had an unemployment spell during this time. The treatment group (early displaced) consists of all 10 year old children, the control group of all 12 year old children.

Figure 5: Decomposition of Treatment Effect $\Delta(\tau)$ - Robustness



The figure presents the results of the decomposition of the treatment effect $\Delta(\tau)$ and quantile $\tau \in [.05, .95]$ as outlined in Section 5. The total effect $\Delta(\tau)$ is depicted by the solid line, the direct effect $\theta(\tau)$ by the dashed line. The area between both lines corresponds to the indirect effect $\gamma(\tau)$. The mediator U is holding a university degree. Outcomes are obtained by using a 3-years average of children's yearly employment and unemployment days as well as income at age 29-31 and 35-37. The sample consists of all children who were born before September, were 4 or 6 years old in 1987-1989 and the main earner in the family had an unemployment spell during this time. The treatment group consists of all 6 year old children, the control group of all 4 year old children. Notice: $\Delta(\tau)$ and $\gamma(\tau)$ are virtually identical for this sample and therefore hard to distinguish in the graph.

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Online Appendix for “The Long-Term Labor Market Effects of Parental Unemployment”

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February 19, 2020

This Web Appendix provides additional results not discussed in the manuscript.

A Additional Results

A.1 Extended Treatment and Control Group

This section presents the main results with an extended control group. Children whose parents had an unemployment spell at age 9 or 10 constitute now the treatment group, children with unemployed parents at age 11 and 12 are now in the control group. As in the main analysis, 3-years means of the respective variables of interest are taken as outcomes. The estimation procedure follows the same steps as described in Section 5 with 999 bootstrap replications. The results are presented in Figure A.1.

As one can see, extending the control groups does not alter the conclusion derived in the paper. The results tend to be, however, smaller in magnitude. For example, at the 90th percentile of the wage distribution the estimated income loss amounts to around 2,200 Euros compared to 3,000 Euros as reported in Section 6. Also the impact on employment and unemployment days decreases somewhat. Nevertheless, the estimated effects are still sizable, especially for my proxy of life-time income.

A.2 Different Age-Ranges for Outcomes

In this section, I present results for 2-years instead of 3-years averages for my outcomes. The treatment and control group consists of all children whose parents got displaced at age 10 and 12 respectively, as described in Section 3. Figure A.2 presents the estimated effect.

Using a shorter-time horizon over which I define the outcome does not change the results. Children of parents who were unemployed before the schooling choice have more unemployment days and lower wages in the long-run. The results presented in the Figure A.2 also highlight the deterioration in labor market outcomes with elapsed time. As one can see, unemployment days are consequentially increasing while wages are falling at the upper part of the distribution.

A.3 Less Restrictive Sample

The results reported in Section 6 are based on parents with strong attachment to the labor market. In this section, I consider the case when imposing less restrictive assumptions on employment. Instead of requiring at least 360 days of employment before the current unemployment spell I include now individuals with at least 180 days of employment. Individuals in this sample are still entitled to benefits payment but with considerably shorter duration on average. Furthermore, it is likely that unobservable characteristics play a more important role here. Figure A.3 presents the results.

From the figure, one can see that the results for employment and unemployment days are very similar to those obtained for the more restrictive sample. The effect on long-term wages for this sample is, however, substantially larger. At the upper half of the distribution, the effects at the age of 35-37 are higher compared to the ones reported in the main text. My conclusion, however, remains valid even when using a less restrictive sample criterion.

A.4 Outcomes at Younger Age

In Section 6, I argue that my first set of results obtained at age 29-31 reflects the impact on the early labor market career of the children, especially for those graduating from university. The lower bound at age 29 is motivated by the fact that a relatively low share of individuals who finally graduate from university has a very strong labor force attachment before that age, but it is also chosen somewhat arbitrary. In this section, I show that my discussions and conclusions in the main text remain valid when considering labor market outcomes before that threshold. The results for the impact on yearly income at ages 20-22, 23-25, and 26-28 can be found in Figure A.4.

My results point toward evidence that parental unemployment at an important decision milestone actually increases yearly income when the child is younger, in line with the findings of Fradkin et al. (2019). This is especially true when we look at the effect on income at age 20-22 where most of the estimated treatment effect is significantly estimates. This is likely as individuals who attend universities only have looser labor market attachment and do not pursue full-time work. As individuals are more likely to graduate from university and transit into full-time work, the effect starts to decrease and my estimates become more noisy with wider confidence intervals, in particular at the upper half of the distribution. The results show that even before the age of 29, one can observe a gradually decline in the labor market outcomes of children in my treatment group.

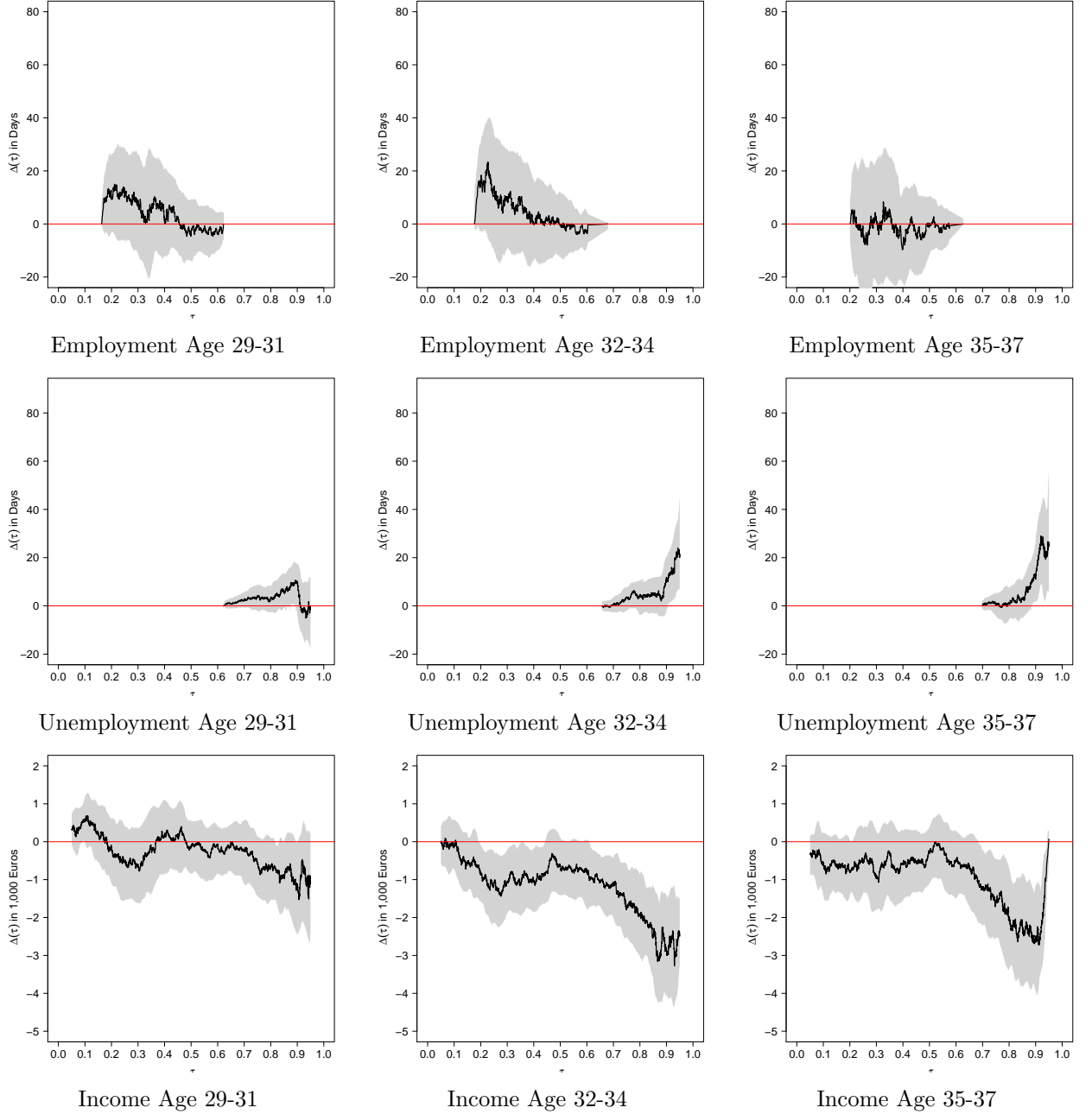
A.5 Estimates not including Control Variables

Despite my best effort to account for possible selection in my sample and the robustness checks presented in the main part of the paper, there still might be some remaining concern that my results are affected by omitted variable bias. There might also be a concern that my approach does not fully capture potential year of layoff heterogeneity (Hilger, 2016; Fradkin et al., 2019).¹ I investigate the sensitivity of my results by re-estimating my model without including *any* control variables. If the results did substantially differ from those presented in Section 6 in the main text, this would give rise to concern about omitted variable bias in my estimates.

Figure A.5 presents the results when no control variables are included in the estimation. I find the same pattern as reported in the main text for all the outcomes. Reassuringly, even when not including any covariates in the estimation, my results are very similar in both magnitude and significance to the ones from my richer model with a full set of control variables; see Figure 2) in the main text. For example, parental unemployment has no impact on children’s income at the beginning of the labor market career. Once children are established in the labor market and higher education becomes more important, I find strong and negative impacts at the upper part of the income distribution. Given these results, I am therefore confident that any type of remaining bias is likely small in magnitude in my analysis.

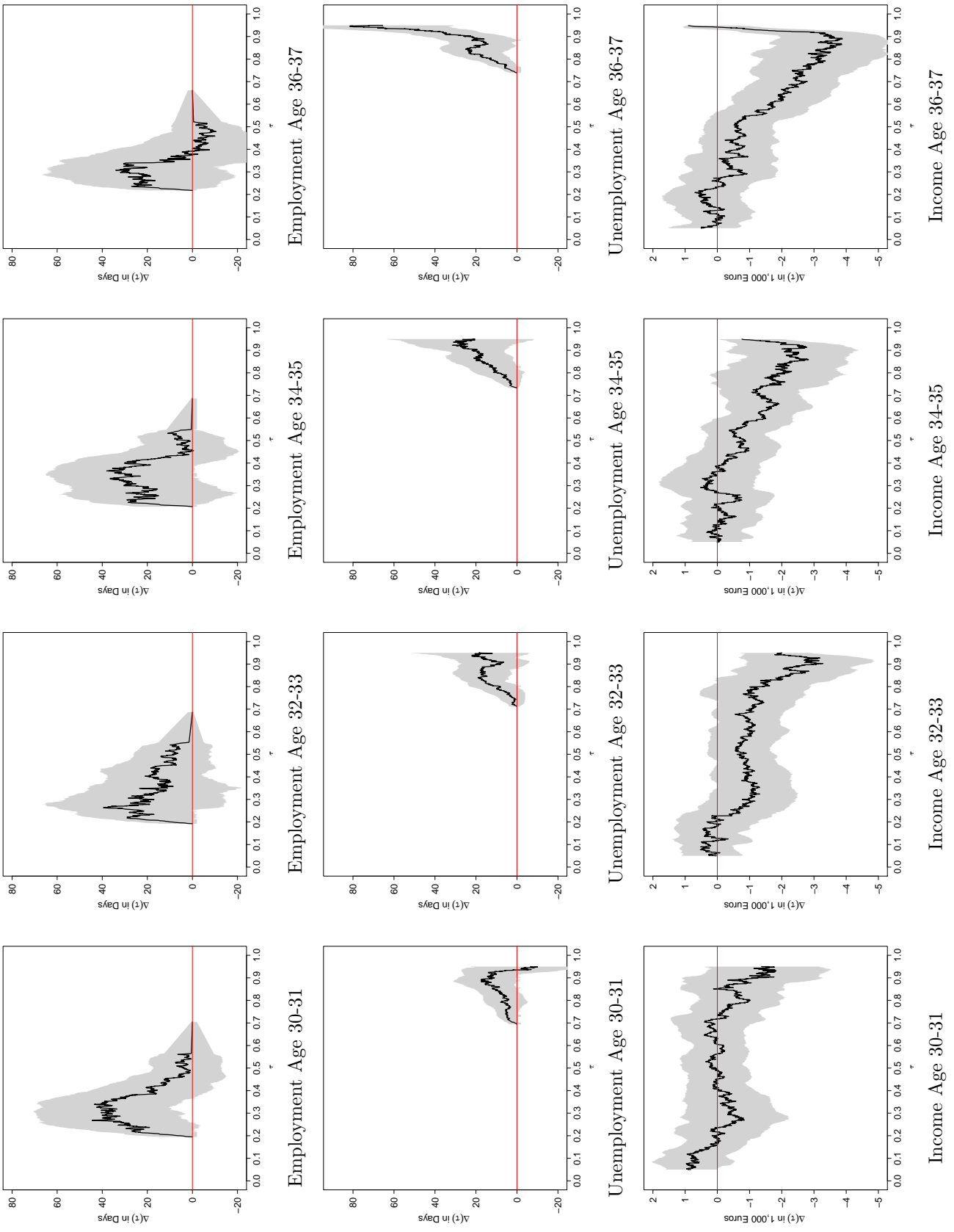
¹Hilger (2016) and Fradkin et al. (2019) discuss under what conditions year of layoff heterogeneity can bias estimates of parental job loss and propose a double difference approach to account for it. As I look on the impact of parental unemployment on the distribution of outcomes, I cannot follow their advice.

Figure A.1: Treatment Effect of Parental Unemployment on Children's Long-Term Outcomes - Extended Treatment & Control Group



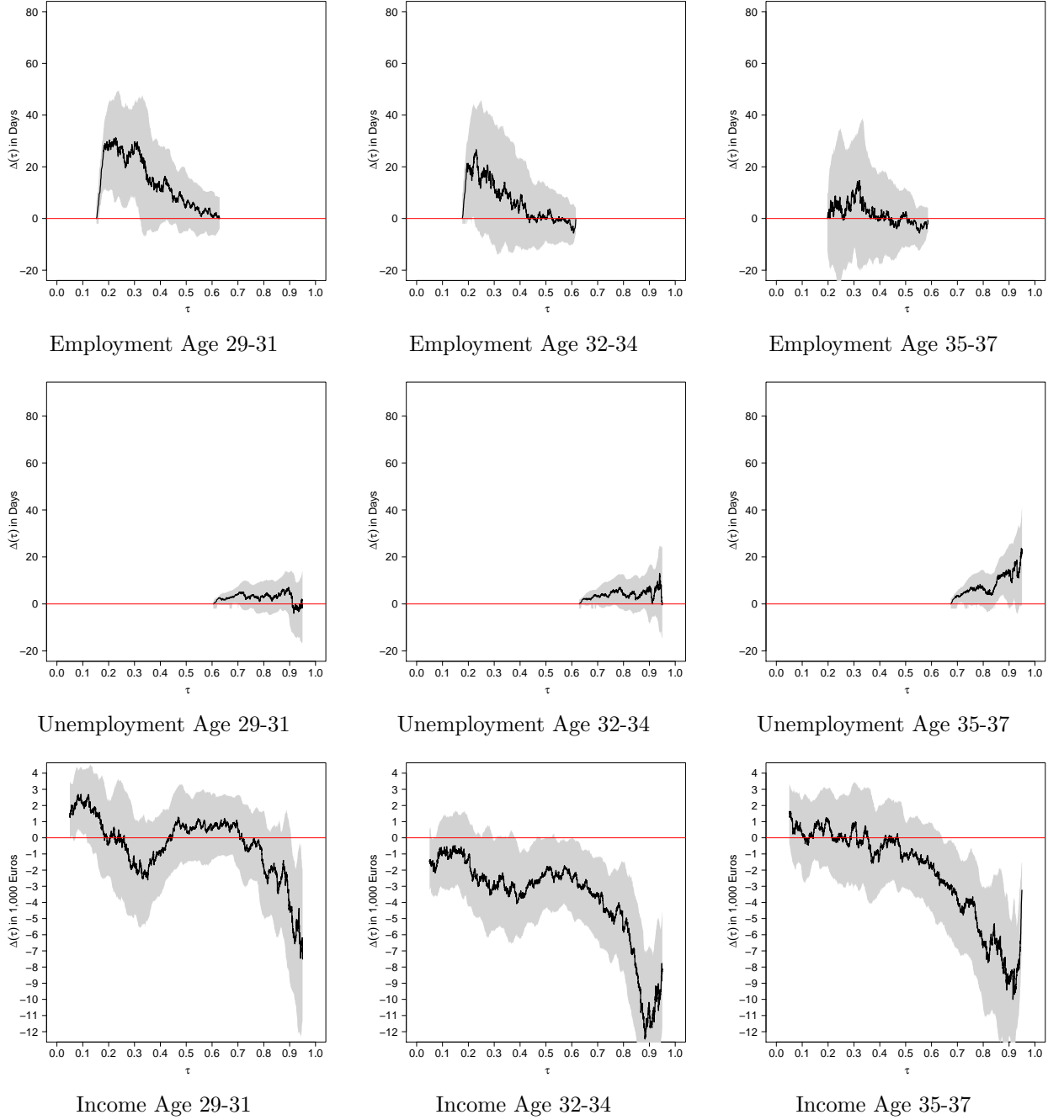
The figure presents estimates of the treatment effect $\Delta(\tau)$ and quantile $\tau \in [.05, .95]$ together with 90% confidence intervals. Outcomes are obtained by using a 3-years average of children's yearly employment and unemployment days as well as income at age 30-31, 32-33, 34-35, and 36-37. The same sample consists of all children who were at the time of parental unemployment between 9 and 12 years old. Children aged 9-10 are in the treatment group, children aged 11-12 are in the control group. $\Delta(\tau)$ is obtained by applying the Method in Section 5. Inference is based on the Bayesian Bootstrap with 999 replications taking clustering on the family level into account.

Figure A.2: Treatment Effect of Parental Unemployment on Children's Long-Term Outcomes - 2-years Averages



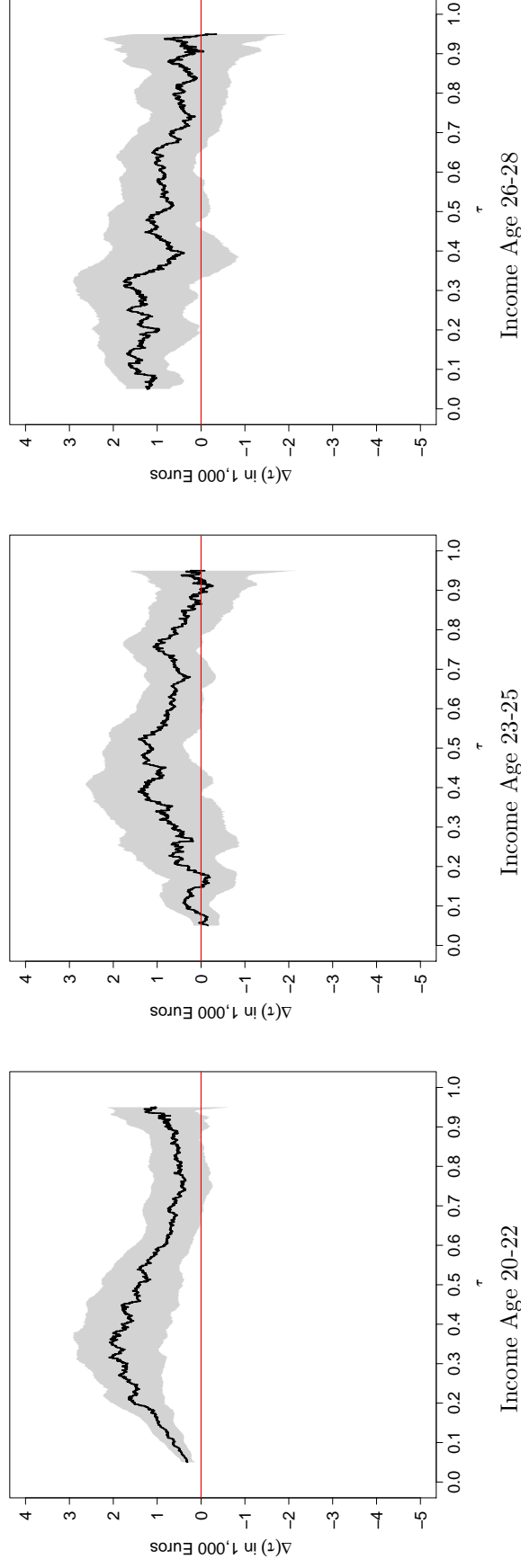
The figure presents estimates of the treatment effect $\Delta(\tau)$ and quantile $\tau \in [.05, .95]$ together with 90% confidence intervals. Outcomes are obtained by using a 2-years average of children's yearly employment and unemployment days as well as income at age 30-31, 32-33, 34-35, and 36-37. The same sample is described in Section 3. $\Delta(\tau)$ is obtained by applying the Method in Section 5. Inference is based on the Bayesian Bootstrap with 999 replications taking clustering on the family level into account.

Figure A.3: Treatment Effect of Early Parental Unemployment on Children's Long-Term Outcomes - Less Restrictive Selection



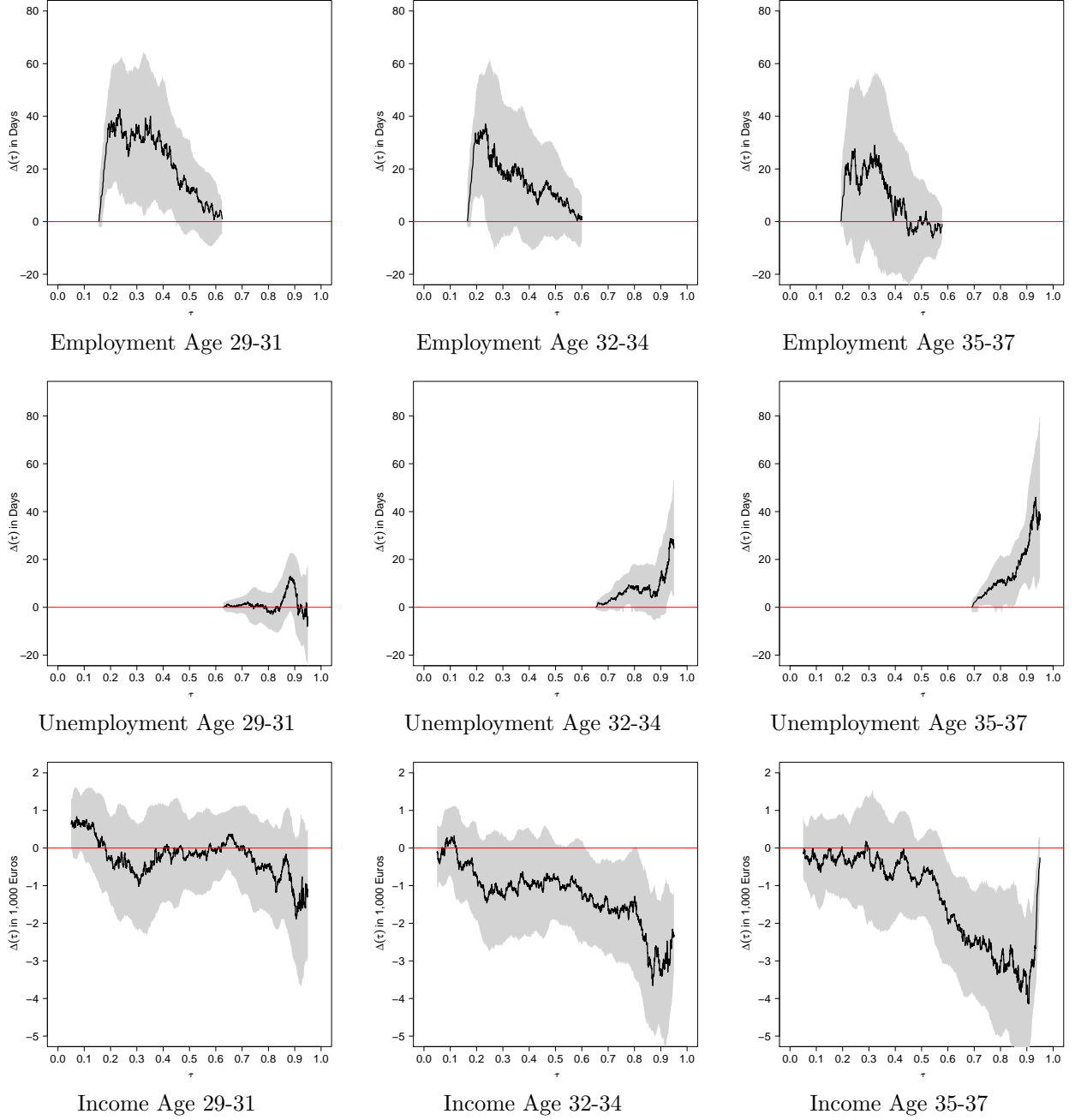
The figure presents estimates of the treatment effect $\Delta(\tau)$ and quantile $\tau \in [0.05, .95]$ together with 90% confidence intervals. Outcomes are obtained by using a 3-years average of children's yearly employment and unemployment days as well as income at age 29-31, 32-34, and 35-37. The sample consists of all children who were born before September, were 10 or 12 years old in 1987-1989 and the main earner in the family had an unemployment spell. Parents with labor market experience of less than 180 days preceding the unemployment spell are excluded. The treatment group (early displaced) consists of all 10 year old children, the control group of all 12 year old children. $\Delta(\tau)$ is obtained by inverting estimated counterfactual distribution functions as outlined in Section 5. Inference is based on the Bayesian Bootstrap with 999 replications taking clustering on the family level into account.

Figure A.4: Treatment Effect of Parental Unemployment on Children's Income at Ages 20-28



The figure presents estimates of the treatment effect $\Delta(\tau)$ for average yearly income and quantile $\tau \in [.05, .95]$ together with 90% confidence intervals. Outcomes are obtained by using a 3-years average of children's yearly employment and unemployment days as well as income at age 20-22, 23-25, and 26-28. The sample consists of all children who were born before September, were 10 or 12 years old in 1987 -1989 and the main earner in the family had an unemployment spell during this time. The treatment group (early displaced) consists of all 10 year old children, the control group of all 12 year old children. $\Delta(\tau)$ is obtained by inverting estimated counterfactual distribution functions as outlined in Section 5. Inference is based on the Bayesian Bootstrap with 999 replications taking clustering on the family level into account.

Figure A.5: Treatment Effect of Parental Unemployment on Children's Income without additional Covariates



The figure presents estimates of the treatment effect $\Delta(\tau)$ for average yearly income and quantile $\tau \in [0.05, 0.95]$ together with 90% confidence intervals *any* additional covariates. Outcomes are obtained by using a 3-years average of children's yearly employment and unemployment days as well as income at age 29-31, 32-34, and 35-37. The sample consists of all children who were born before September, were 10 or 12 years old in 1987-1989 and the main earner in the family had an unemployment spell during this time. The treatment group (early displaced) consists of all 10 year old children, the control group of all 12 year old children. $\Delta(\tau)$ is obtained by inverting estimated counterfactual distribution functions as outlined in Section 5. Inference is based on the Bayesian Bootstrap with 999 replications taking clustering on the family level into account.

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