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#866

Imprint

Ruhr Economic Papers

Published by

RWI – Leibniz-Institut für Wirtschaftsforschung
Hohenzollernstr. 1-3, 45128 Essen, Germany

Ruhr-Universität Bochum (RUB), Department of Economics
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Editors

Editorial Office

Ruhr Economic Papers #866

Responsible Editor: Ronald Bachmann

ISSN 1864-4872 (online) – ISBN 978-3-96973-003-4

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Bibliografische Informationen der Deutschen Nationalbibliothek

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie; detailed bibliographic data are available on the Internet at **http://dnb.dnb.de**

RWI is funded by the Federal Government and the federal state of North Rhine-Westphalia.

http://dx.doi.org/10.4419/96973003
ISSN 1864-4872 (online)
ISBN 978-3-96973-003-4

Bernhard Schmidpeter[1]

The Long-Term Labor Market Effects of Parental Unemployment

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 I 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 longlasting consequences of parental unemployment.

JEL-Code: C21, I23, J13, J31, J64

Keywords: Parental unemployment; intergenerational effects; long-term effects; unemployment; wage loss; decomposition

September 2020

1 Bernhard Schmidpeter, RWI, Christian Doppler Laboratory, JKU Linz and IZA Bonn. *– I am grateful to Anders Björklund, Andrey Fradkin, Martin Halla, Nate Hilger, Matthew Lindquist, Matthias Parey, Anne Polivka, Mari Rege, Rudolf Winter-Ebmer, and seminar participants at SOFI Stockholm, the University of Stavanger, IAB Nuremberg, FBK-IRVAPP, the Maastricht University Economics Workshop, the Microeconomics Workshop in Bolzano, and the Econometric Society Winter Meeting in Naples for discussions and valuable suggestions. All remaining errors are my sole responsibility. Support by the ESRC Centre on Micro-Social Change at ISER (Grant RES-518-28-001) and the Upjohn Institute Early Career Research Award are gratefully acknowledged.* *- All correspondence to: Bernhard Schmidpeter, RWI, Hohenzollernstr. 1-3, 45128 Essen, Germany, e-mail: bernhard.schmidpeter@rwi-essen.de*

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 percent of all children in Europe between the ages of 0 and 17 live in households 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 risk of mortality.[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 lifetime earnings for 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 the children's 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' decisions about investment in 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). Such circumstances shield these workers from job loss and give them the opportunity to invest more in 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 hand, parental unemployment later in a child's life cycle can have no effect or only marginal effects on

[1] See, 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.

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

the child's long-term outcome if most important investment decisions have already been made. On the other hand, even short-term losses and constraints can affect parental choices when the job loss happens around the time of 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 the 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 properly reflect longterm impacts. It is possible that parental unemployment during early childhood leads to lower investment in 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 who have 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 in a child's life cycle. The total effect can be driven by changes in monetary-related investments in the child—for example, schooling choices. It can also be driven by nonmonetary-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 the different productivity of parental inputs.

In this paper, I study the effect of parental unemployment happening around the time of important human capital investment decisions for 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 time of 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, and a lower track, which provides basic education and prepares students for vocational training. Which track is

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

chosen depends on the parental decision and input, among other things. Parents who choose the higher track for their child, which carries the possibility of entering 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 time of the track choice can crucially affect the parental investment decision.[4]

Experiencing unemployment will affect parents' educational investment decisions by affecting their ability to save and through their long-run expectations of their own lifetime 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 United States and Austria do only replace a small fraction of the displaced worker's earnings, both of these effects—1) expectations about employment stability and lifetime earnings and 2) 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, Fereirra 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 find 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. An explanation for my finding is the timing of an adverse income shock together with parents' lower expectations about

[4] There is no well-established student loan system available in Austria, and most students rely, at least partly, on parental financing during their time of 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.

[5] Austria's unemployment insurance system is less generous than in most other European countries. The benefit replacement rate is 55 percent 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 percent of net earnings; see the discussion in Nekoei and Weber (2017) for more details.

future labor market outcomes leads to lower parental investment. I show that my results are driven by children of parents with a lower predicted probability of recovering from the unemployment shock.

I then investigate the long-term impacts of this suboptimal investment decision on the long-term labor market outcomes of children. While I do not find any effect on annual income at ages 29–31, my results indicate a gradual deterioration at the upper part of the distribution later on. At ages 35–37 children between the 60th and 90th percentile whose parents had an unemployment spell shortly before the educational track decision earn between 1,500 and 3,500 euros (between $1,700 and $3,900) less per year. Under the arguably strong assumption that these effects persist until the end of an individual's working life and approximate lifetime effects (Björklund, 1993; Böhlmark and Lindquist, 2006; Haider and Solon, 2006), and ignoring the impact on retirement benefits, my estimates imply at age 35 a net present value of lower future income of between 20,000 and 65,000 euros ($22,000 to $72,000). Applying a decomposition analysis, I find that up to 45 percent of these losses can be explained by lower educational achievement of these children.

I also look at the effect on nonmonetary labor market outcomes, such as days employed and unemployed. I find that parental displacement at the time of a crucial educational decision induces individuals to work between 50 and 75 days more per year at ages 29–31. This effect fades out later on: there is no significant impact on days worked at ages 35–37. My estimation of days spent 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 a year at ages 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 being more likely to be tracked into technical or vocational training, which requires 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. Similar as in 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 (the treatment group) to the outcome of children whose parents experienced unemployment shortly after the tracking decision was made (the control group).[6] Under the assumption that children in both my treatment and control groups 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 on crucial educational investment decisions. This is an important policy parameter, which has not been 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) use the quarterly income around the layoff for individuals aged 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 can understate the long-term costs of unemployment.

I also concentrate on the distribution rather than mean effects, with the aim of investigating 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 identify vulnerable groups. This may be especially interesting for policymakers, as they might be concerned about the impact of parental unemployment at certain parts of the distribution.

This paper also contributes 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

[6] Fradkin et al. (2019) estimate the effect of parental unemployment around the time of labor market entry, an age at which, arguably, most parental investment decisions have already been made.

[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. Lovenheim (2011) and Lovenheim and Reynolds (2013) find that an increase in housing wealth increases college enrollment and affects college choice; in particular for lower-resources families.

at important times (e.g. Brown, Scholz and Seshadri, 2012, and Caucutt, Lochner and Park, 2017).[8]

In this paper, I provide indirect evidence that not only does parental income per se matter in explaining children's achievements but also the timing and likely associated (short-term) constraints matter. These channels are also highlighted in the model of Caucutt and Lochner (2017). My results show that a drop in income at the time of an important parental investment decision can negatively affect the educational attainment of the child compared to situations in which 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 do, for example, Hanushek and Wößmann (2006) and Dustmann, Puhani and Schönberg (2017). Hanushek and Wößmann (2006) find that early tracking increases educational inequality; they compare outcomes between primary and secondary school across different international systems. Dustmann et al. (2017) do not find evidence in 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: 1) a low track, 2) a middle track, and 3) a higher track. This makes it easier for those attending the middle track to revise 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.

[8] Carneiro and Heckman (2002) do not find evidence that family borrowing constraints affect postsecondary 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 an instrument, especially for low-SES households. They interpret their findings as indicating that households do not face borrowing constraints. Lochner and Monge-Naranjo (2012) provide an overview of the current literature on the impact of credit constraints on the accumulation of human capital.

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 cutoff date. All children born before the first 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 noncognitive development.[10] In addition, students might 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 one through four).[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 four. At the end of grade four—age 10 for most students—there are two types of compulsory education offered: 1) a general track which provides general education and concludes with a university entrance exam (Matura) and 2) 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 of German writing, reading, and mathematics. If these requirements are not met, students can sit for 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 percent of a student's likelihood of taking the higher, general track is influenced by the parents and their socio-economic background. 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 percent of students choose the higher, general track in grade five (Schneeweis and Zweimüller, 2014; Statistik Austria, 2018).

Early track choice might be reversed at grade nine, when students can revise their first decision and choose to change from the specialist to the general track, given strong school performance. Switching tracks in Austria occurs in only a minority of cases, however: around 30 percent 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) (Schneeweis and Zweimüller, 2014; Statistik Austria, 2018). Around 80 percent of stu-

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

[10] The 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 have fulfilled the requirements.

[11] The share of students who have to repeat a grade is in general low in Austria. On average, around 2 percent of students repeat grades two through four, 3 percent grades five through eight, and 4 percent of students repeat grade nine (Schneeweis and Zweimüller, 2014). Recent statistics for the year 2015–2016 put the retention rate during primary school even as low as 0.6 percent (Statistik Austria, 2017).

[12] In 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.

dents who switch from the specialized to the higher track attend higher vocational schools which provide professional training and conclude with a university entrance qualification. The remaining 20 percent attend higher general (academic) schools.[13] Students attending higher vocational schools have in general a substantially lower graduation rate compared to students in higher general schools. Around 58 percent of students in higher vocational schools graduate with a university entrance exam compared to 75 percent of students attending higher general schools. Students who successfully graduate from higher vocational schools are also less likely to attend university. Only 55 percent of students from higher vocational schools choose to study within 3 years after graduation compared to 85 percent of students graduating from higher general schools (Statistik Austria, 2017).

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 specified time period. This period is in general set to four years for a bachelor's degree and an additional three years to obtain a master's degree. Only around 50 percent of all students manage to graduate during this specified time period, however. After exceeding these limits, students must pay university fees of 766 euros ($850) a year.[14] 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 United States or the United Kingdom, 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 at university are wages from (part-time) employment and parental support. In 2014, around 86 percent of students in Austria held a job, and around 50 percent of all students received additional income support from their parents.[15,16] Income from employment is, however, relatively low, as 45 percent of employed students earned less than 400 euros per month, and almost 70 percent earned less than 750 euros ($840) per month. In comparison, the average monthly earnings of an individual aged 20 to 29 in Austria is 1,800 euros ($2,000).

Given the low earnings students make for working, a median duration of four 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

[13] Both school types lead to a university entrance qualification. A student may find it easier to switch from the specialized track to higher vocational schools. Attending a higher general school may require, besides sufficiently good grades, that a student also passes additional entry exams if the respective subjects were not or not as extensively taught in the lower track.

[14] The median study duration is four years for a bachelor's degree and almost three years for a master's degree (Statistik Austria, 2019*a*). 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.

[15] These and the following figures were taken from the survey presented in Hajek, Kasper and Freidl (2014).

[16] In an international comparison, the share of working students is relatively high. For example, in the United States, around 43 percent of full-time undergraduate students worked in 2017, down from 50 percent in 2005 (McFarland, Hussar, Zhang, Wand, Wang, Hein, Diliberti, Forrest Cataldi, Bullock Mann and Barmer, 2019).

impose a nonnegligible financial burden on families.[17] But even with income from work and parental support, almost 60 percent of students state that the monthly amount they receive is 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 education track, which is more expensive. 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 for their child's maintenance during studies. I discuss this and alternative explanations further in Section 6.

While deciding on the track choice at age 10 is common in Austria and Germany, similar crucial educational investment decisions exist in other countries.[18] In most OECD countries, tracking of students is common by the age of 16, when parents and students need to choose the educational track they will follow (OECD, 2004). In addition, mirroring the setting in Austria, most European countries either do not have a well-established student loan system, or additional financial support from parents is likely necessary to study.[19] Thus, similarly as in Austria, a sudden income shock at the track choice can affect the parental decision when facing the possibility of committing 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-worker

[17] The 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/

[18] There 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 at around age 12 (OECD, 2004).

[19] Grant 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 their 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 students 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).

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 Groups

From the combined data, I first select all parents who had at least one child born between 1975 and 1979. This selection enables me to follow children of unemployed parents until the age of 37, an age at which individuals are fairly well established in the labor market and observed effects are a good approximation for the impact on lifetime outcomes. I then select all families in which 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 aged 10 or 12 at the time of the 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 the time of the track choice or late school enrollment.[20] In order to account for the schooling entry cutoff in a given year, I also exclude all children born after the first of September from my analysis. This is to minimize ambiguity in the age at which the track choice was made for children born after September 1.

From this sample, I drop all families in which 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

[20]Students at age 11 do not make up 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 the Appendix.

income as a result of unemployment and is common in the literature that studies the effects of unemployment (e.g. Jacobsen et al. (1993)). Applying all restrictions, my final sample consists of 3,767 children.[21] 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. Parents in my treatment group 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 before 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 for year effects in my analysis.

[Table 1]

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 are 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 children 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 the labor force participation rate between university graduates and nongraduates before age 29 is substantial. At age 20, regardless of whether children are in my treatment or control group, I observe a 28 percentage point lower labor force participation rate for university graduates compared to nongraduates in my data. A comparable difference remains until children reach the age of 29.[22] At the upper bound at age 37, individuals

[21] The estimation results that do not require any labor market attachment before the unemployment spells are very similar to the ones presented here and can be found in the Appendix.

[22] In the Appendix, I provide estimates for outcomes before age 29.

are fairly well established in the labor market. I divide my data into three-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 ages 29–31 can be thought of as an early stage, in which individuals are still not well established in the labor market, especially for individuals who attend university. At ages 32–34, individuals normally have obtained a 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 groups at ages 35-37 might be good approximations for differences in lifetime earnings and attachment to the labor market (Björklund, 1993; Böhlmark and Lindquist, 2006; Haider and Solon, 2006). Hence, effects found at this age range may persist until the end of people's working lives.[23]

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 statistically significant differences in mean log average earnings at ages 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 percentiles, I estimate significant differences of 7 log points. There are also large differences in the probability of having long-term unemployment spells. For example, children in my treatment group have a 2.5 percentage point higher likelihood of having at least a 90-day unemployment spell over the three-year period. These estimates indicate that parental unemployment at important educational decision 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 a 5 percentage point lower probability of obtaining a university degree, and this difference is statistically significant at a 1% level.[24] Having higher levels of education is arguably more important at the upper end 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 the parents' children.

[23]The effects are more pronounced using a two-year average of the outcomes and show a even clearer deterioration in the labor market performance with age. Detailed estimates can be found in the Appendix.

[24]Although the Austrian educational system distinguishes between numerous types of higher education institutions, such as universities and Fachhochschulen, I refer to degrees from higher education institutions collectively as "university degrees" in the following paragraphs.

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

4.1 Impact on Family Income

My main hypothesis is that parental unemployment at the time of important parental educational decisions, 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, and not only the drop in income, is an important determinant of observing differences in children's educational achievement, I should not find any differences in the income paths between parents in my treatment group and those in my control group.

I evaluate the impact of an unemployment spell on yearly log parental income ω from three years prior until five 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 the treatment and control groups, these quantities can be obtained by separate linear regression in the two subpopulations, with $T = 1$ (a parent in the family experiences unemployment at the time of the educational decision point for the child) and $T = 0$ (a 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 the household, and Panel B the path of total family income—the yearly income of the main earner 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 recover over the following years, but even after four years income lies still below preunemployment levels on average. The small differences between both measures and their similar paths 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 my control groups 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 the parental educational investment decision, lower expectations about future employment of the parents, and the prospects of future support payments if the child attends university, all lead parents in my treatment group to choose the lower educational track. In addition, the virtually nonexistent difference in income between the two groups supports my identification strategy and shows that children in both my treatment group and my 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 my control groups face similar income constraints, I investigate how parental unemployment around the time of the track choice affects the educational achievement of children. I estimate the average treatment effect of having obtained a university degree similarly to the way in which the income effects were estimated in the previous section; however, I now use a logit outcome model. 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 the time of the educational track choice compared to a later time lowers the child's probability of obtaining a university degree from around 30% to 25%, which corresponds to a treatment effect of around 5 percentage points.[25] This estimate is close to the observed raw differences of 5.84 percentage points 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 reemployment 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

[25] Coelli (2011) finds that parental job loss when the child is aged 16–18 lowers the probability of the child's enrolling in postsecondary education by around 10 percentage points, 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 jobs. As argued above, this might introduce bias in the estimation and can overstate the impact of parental unemployment.

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 for their child's maintenance during studies.[26] Overall, facing the possibilities of providing support to their children for a prolonged period of time, coupled 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 subsamples 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 in which 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 constrained in terms of future available family resources.[27]

Third, I consider the effect on families in which the main parent is eligible for benefit duration of 30 weeks, or roughly 7.5 months. Eligibility for 30 weeks constitutes an extension of 10 weeks 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 constrained. Furthermore, given the longer time parents are eligible for benefits, they might be more selective during the job search, searching for jobs that have 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 that child attends university.

Fourth, I also differentiate by parental preunemployment average income. Similar to individuals with longer benefit eligibility, parents with higher preunemployment earnings might have more resources to smooth the sudden shock. They might also have more resources to look for better reemployment and higher wages. Correspondingly, families

[26] By law, the maintenance payment can be quite substantial, 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.

[27] I define all households as single-parent households if no spouse was indicated during application for child-care benefits.

with lower preunemployment income might have fewer possibilities to adjust to the shock. I consider all children as coming from lower-income families if the average yearly wage of the main earner three years prior to the unemployment spell does not exceed 18,000 euros. In contrast, children of families with an average yearly income of at least 30,000 euros per year are considered high-income families.[28]

One striking feature of my estimations for the two-parent and single-parent samples, reported in the second and third rows of Table 2, is the estimated likelihood of holding a university degree under both treatment and control 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 the treatment group or the 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.[29]

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 university. For two-parent families and single-parent families, I estimate a treatment effect of –5.03 percentage points and –4.49 percentage points, respectively. Both of these estimates are very similar to the ones obtained from my entire sample. These results show that although the propensity to obtain a university degree differs whether a second parent is present or not, both types of families react in a similar way to an unemployment spell at the track choice.

My results are also similar to my estimates using the entire sample when concentrating on the extended benefit sample (row four) or when differentiating by preunemployment parental income (rows five and six). The similarity in size of the treatment effect across different sample specifications implies hat the timing of an adverse income shock together with lower expectations about future labor market outcomes can play an important role in determining parental investment decisions.

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

[29] I also investigate how results are affected when restricting the sample to individuals with preunemployment firm tenure of at least three years. For these individuals job loss may occur arguably unanticipated. They may also be less inclined to change their labor market career as a response to children's early school performance. The obtained results are similar to the ones for high parental preunemployment income.

To explore this hypothesis further, I form a prediction of the probability that the main earner attains the preunemployment income level within five years after the unemployment spell. To avoid overfitting when predicting the probabilities, I apply the method suggested in Abadie, Chingos and West (2018). Then, I split the sample into two groups of parents with above and below median predicted probabilities of reaching preunemployment income level. The results, presented in Appendix B, show that the lower educational attainment of children in my treatment group is driven by those of parents with lower predicted probability of recovering from the unemployment shock over an extended time period.

In sum, the estimates support my hypothesis that a sudden income shock coupled with lower expectations about the future can reduce parental commitment and lower investment. They also show that parents do not necessarily behave in a (long-term) forward-looking manner.

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 educational decisions. To investigate this possibility, I estimate the impact of parental unemployment for a significant period of time before the actual track choices are made. Specifically, children whose parents experience an unemployment spell when the children first enroll in school at age six are considered to be in the treatment group, whereas children whose parents had an unemployment spell before school enrollment, when the children were four, are considered to be in the control group.[30]

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 the child's obtaining a university degree. This result supports my hypothesis that parental unemployment at an important educational decision milestone, 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 in the United Kingdom. One explanation for this relationship might be lower financial resources to spend on tutoring or other educational material for children. An-

[30] The results for this group are not sensitive if children aged four or aged five are used in the control group. Arguably, parental investment at age five and age six should have more similar effects compared to age four, but to remain consistent with my main specification I chose a two-year difference between treatment and control groups.

other explanation might be that distress caused by unemployment leads 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 constrained 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 on.[31] Hence, one would expect a substantially lower treatment effect for this group compared to more constrained families. My results in Table 2 do not show any large difference in educational attainment between children of constrained and unconstrained families, however.

Second, if mental distress is the sole factor in explaining the differences in children's GPAs, and in turn the lower likelihood of their 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 readily have a partner or social interactions available, two important factors to deal with high levels of stress (e.g. Schmidpeter, 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 to teachers. Students can attend the higher, general track even if the requirements are not entirely met. This is the case if students showed in general good performance at school and admission to the higher track is supported by the teacher. Thus, students may 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 parents and their socio-economic background, even after adjusting for students' grades and other characteristics (Bruneforth et al., 2012); see the discussion 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

[31] As 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) in general find 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. This idea, along with further developments, has been used in numerous studies in recent years. For example, Callaway and Huang (2018) evaluate intergenerational 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 paragraphs, 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 denote 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

$$\Delta(\tau) = Q_1(\tau) - Q_0(\tau) \tag{1}$$

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

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

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

In a second step, I invert $\widehat{F_1}(y)$ and $\widehat{F_0}(y)$ 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).[32]

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

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

6.1 Impact on 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 just on 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.[33] Interestingly, I find that parental unemployment affects employment days only for individuals between the 15th and 65th percentiles, while the rest of the distribution is unaffected.[34] These individuals can be considered as being rather loosely attached to the labor market.

The effect is strong and significant during the early career stage between the 20th and the 40th percentiles. The average yearly increases in days worked is between 22 and 42 days. As can be seen from the figure, this effect slowly fades out as children establish themselves in the labor market. At ages 32 to 34, parental unemployment at the track choice is associated with an 8-to-38-day increase in average employment days between the 20th and the 40th percentiles. The estimates are only significantly different from zero at the lower percentiles, however. At ages 34–37, the estimated effect is substantially smaller compared to earlier stages in children's postschool 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 children whose parents lost their jobs 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

[33] Remember 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 at which individuals most likely are in their career jobs. At ages 35–37, my results are a good approximation for lifetime effects; see 3.

[34] The treatment effect below the 15th and above the 65th percentiles is always estimated to be 0 and therefore not depicted in the graph.

careers. My results indicate that this effect is due to children whose parents experienced unemployment close to a crucial educational decision point likely being pushed into more vocationally oriented education and training, leading 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.[35] The results reveal two interesting points. First, the estimated treatment effect exhibits substantial heterogeneity along the distribution of unemployment days. It is concentrated in 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 ages 29–31, my estimates are largely insignificant and small over most parts of the unemployment days distribution, the exception being after the 90th percentile. At older ages, the treatment effect increases almost monotonically as the quartile cutoffs increase. At the age at which children of parents who suffered an unemployment event close to the time of educational decisions are established in the labor market, ages 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 aged 35–37.

Oreopoulos et al. (2008) find that children of displaced fathers have a higher likelihood of receiving unemployment benefits later in 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 concern is that the results might be partially driven by selection of fathers into firms that close. I show that even when accounting for potential selection into firms that close, 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 more prone to becoming unemployed themselves.

Effect on Income: The last row in Figure 2 presents 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 ages 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

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

obtaining a university degree, and therefore I am likely comparing lower-educated children with relatively more labor market experience to children with higher educational attainment but relatively less labor market experience at this age range. In the Appendix, I provide additional evidence of this interpretation and show that parental unemployment at the track choice has a *positive* effect on income at the ages of 20 to 23, a time of life 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 becomes noticeable when looking at the income when the children are ages 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. In 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 1,000 and 3,000 euros ($1,200 to $ 3,500), 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 ages 35–37, one can see that the magnitude of the income loss is amplified for the greater part of the distribution above the median. For example, my estimates suggest an average yearly income loss, at ages 32–34, of 1,600 euros ($1,800) compared to 1,100 euros ($1,200) around the 60th percentile; of 2,600 euros ($2,900) compared to 900 euros ($1,000) around the 80th percentile; and of 3,500 euros ($3,900) compared to 3,000 euros 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. Under the arguably strong assumption that my estimates at ages 35–37 reflect lifetime effects (Björklund, 1993; Böhlmark and Lindquist, 2006; Haider and Solon, 2006) and persist until the end of the child's working life, and assuming an individual retires at the 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 by between 20,000 and 65,000 euros (or between $22,000 and $72,000). This simple calculation ignores the impact on retirement benefits as a consequence of lower labor income and is therefore likely a lower bound for the true effect.[36] My results also indicate that there is no persistent effect

[36] The calculation is based on the assumption that the estimated income losses persist until the age of 60 and the individual retires thereafter. It is also assumed that retirement benefits are not affected by lower income. As the calculation of retirement benefits is 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%.

for individuals below the median with an insignificantly estimated treatment effect close to 0. The observed strong heterogeneity 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 United States decreases age-adjusted income by around 9%, while my results indicate a loss of around 6% to 7% at ages 35–37 for children in the upper half of the distribution, and no effect for those in 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.[37]

The different outcomes in previous studies might reflect the different identification strategies and as a result the 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 outcomes. My identification exposes children to the same experience and to a similar impact on family income during childhood, and it avoids inherent biases that may be embodied in the comparison of children of displaced versus nondisplaced parents. For example, Kramarz and Skans (2014) show that many children find their first job in their parents' establishments. Children benefit from the parental network by having better labor market outcomes and higher wages a few years after labor market entry. Parental unemployment weakens these network effects, however (Huttunen and Riukula, 2019). Under my identification assumption, children in both the treatment and control groups should be exposed to the same environment. Hence, my results on the impact of parental unemployment are *net* of these effects.[38]

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 fewer years of schooling, these students enter the labor market at a different time and perhaps under different economic conditions compared to children in my control group. Previous research

[37]When concentrating on mean effects, I find that children in my treatment group earn an insignificant 6 euros more per year compared to children in my control group at age 29-31. These small effect sizes at younger ages are in line with the findings of Hilger (2016) and Fradkin et al. (2019), and gives reassurance in my identification strategy.

[38]This 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.

has shown that, especially among college graduates, the circumstance of first becoming unemployed during an economic downturn leads to lower lifetime earnings and inferior employment (Kahn, 2010; 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, however, were marked by stable economic growth.[39] 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, although 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 findings in the literature on initial conditions.[40] 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 careers, 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 careers 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 subsection, I present additional results and investigate the robustness of my estimated effects.

6.2 Heterogeneous Results & Robustness

In this subsection, I provide heterogeneous results for interesting subgroups, mirroring the estimates for educational attainment 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 the age of starting school are not affected.

[39] The outcome measured at ages 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%.

[40] For example, Cockx and Ghirelli (2016) show that hourly wages of lower-educated individuals are 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.

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 Extended Benefits sample. These parents can be regarded as highly productive and attached to the labor market. They may have better information and are better at 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 sample.

To gauge the robustness of my results, I also consider the long-run effects for children of parents displaced from their jobs at around the time of their children's school enrollment (age six) compared to earlier (age four). 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 disproportionally affect younger children when parents spend more time with their children as a response to the unemployment spell. Likewise, there might also be negative long-term impacts if parents decreased the time spent with their children when they were young as a response to the unemployment spell.[41]

Figure 3 depicts the results for my three different subsamples. Looking at the results for the two-parent sample presented in the first row, one can see that the estimates for this subsample 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 ages 29–31, the estimated treatment effect is mainly concentrated around zero along the income distribution, with wide confidence intervals. When children are fairly established in the labor market at ages 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 it is large and increasing above the 60th percentile. The estimated magnitude of the yearly loss is comparable to the estimates obtained using the entire sample. Children in my treatment group around the 60th percentile earn around 1,100 euros less, compared to 1,600 euros less 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.

[41] For brevity's sake, I only report the initial and long-term estimates, and only for yearly income. The complete set of estimates is available upon request.

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

However, 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: effects are around zero over most parts of the distribution. Later, when the child is well established, I find strong negative effects in 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 from that of the figures presented in the previous panels. Children whose parents had an unemployment spell before enrollment at age four are now in the control group, and children with parents who had an unemployment spell after school enrollment at age six constitute the treatment group.

Looking at the results for children whose parents experienced a 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; this holds true for both early and long-term labor market outcomes. Second, instead of a downward 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 in 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 subsample analysis show that my main estimates are not driven by children from two-parent households or by families with longer benefit eligibility. The obtained quantile treatment effects are remarkably similar both in magnitude and shape for my subpopulation 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- or long-term income. This is in line with the estimates for educational attainment discussed in Section 4.[42]

7 Decomposition of the Treatment Effect

In light of the 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 indirectly affect 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 wages and lower parental investment due to unemployment at the track choice, I decompose my treatment effect.

7.1 Decomposition of Quantile Treatment Effects

Before I present the results of my decomposition analysis, let me briefly discuss my approach. Denote by E the educational attainment of the child—i.e., whether 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:

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

[42]Despite my best efforts to account for selection into unemployment, as well as being able to include a rich set of control variables and having a reassuring outcome from my robustness checks, there might still be some concern about omitted variable bias. In the Appendix, 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.

$$\underbrace{Q_{(1,E(0))}(\tau) - Q_{(0,E(0))}(\tau)}_{\theta(\tau)\text{:Residual Effect}} \tag{3}$$

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 a possible mechanism. 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 that 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 they 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.[43]

There is strong support indicating that SCIA holds true 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 postunemployment 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

[43]In addition, my estimation approach requires that θ is homogeneous 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.

they choose the university track for their child, there is a certain level of uncertainty as to how future outcomes will materialize. Furthermore, the statistics provided in Section 2 show that, although there is the possibility of reversing track choice later on, this is not easy, and only a small share of children do so. In addition, I control for a wide variety of characteristics that 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 inequality to be transmitted to future generations through adverse 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 groups, 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 $\widehat{F}_{t,E(t')}(y)$ for $t, t' \in \{0, 1\}$ is obtained by integrating over the distribution of U and X, similarly as in Equation (2). These estimates are then inverted to obtain $\widehat{\theta}(\tau)$, as defined in Equation (3). In order to obtain $\widehat{\gamma}(\tau)$, I subtract $\widehat{\theta}(\tau)$ from my previous estimates of $\widehat{\Delta}(\tau)$.

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 ranges.

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 ages 29–31 indicate that for children relatively loosely attached to the labor market, lower parental investment because of unemployment at the track choice explains between 10 and 40% (or between 1 and 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 change over the lifetime of the children.

The impact of lower parental investment as a result of parental unemployment at the track choice becomes more pronounced when one looks at the decomposition results for average yearly income, however, 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 in 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 ages 29–31, 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 because of 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.[44]

The findings in this section clarify why I do not find stronger total effects at the early labor market stages; hence, they 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 suboptimal educational decisions made by parents starts to materialize.

[Figure 4]

As a robustness check, I decompose the estimated treatment effect for children in cases for which parental unemployment occurred at the child's school enrollment age. If, instead of experiencing an unemployment spell at the time of important parental educational decisions for their children, other factors are driving parents' investment decisions (and thus my decomposition results), one would expect to find similar effects for $\gamma(\tau)$ for the school enrollment age group too. For the sake of brevity, I only report the decomposition results for yearly income in Figure 5 and only for the age ranges 29–31 and 35–37.[45]

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 suboptimal parental investment decisions around the schooling track choice.

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

[45] The decomposition of nonmonetary outcomes leads to similar conclusions; these conclusions are available upon request.

[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 parents' unemployment at the time of important parental educational decisions for their children can have long-lasting and negative impacts on the children. Using administrative data for Austria, and relying on the 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 point 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 parental 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 cycles. For example, I do not find any impact on yearly income at ages 29–31, a relatively early stage in the child's life cycle, but my results show that income gradually deteriorates afterward. At ages 35–37, when children are well established in the labor market and effects are likely to have a persistent impact on lifetime earnings, I estimate that parental unemployment at important educational decisions lowers children's yearly income by between 1,500 and 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 lifetime earnings. This vulnerability to adverse economic shocks together with lower lifetime earnings may put additional pressure on public finances and social support programs. The results of this paper also show that the effects of adverse economic shocks on parents can be translated into greater earnings inequality in their children's generation. This transmission of adverse effects to unemployed individuals' children has important implications for policymakers. While it is impossible to eliminate economic uncertainty, the results of this paper show that it is important for policymakers to keep in mind when designing policies and social security systems the large indirect costs for the next generation of unemployment in 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 only parents' current earnings, and they often have parents serve as guarantors of the students' loans. In several countries, including Austria, the typical student loans are insufficient to cover students' living expenses, and parents need to continue to provide for their children while those children are enrolled in college. My results show that parents needing to not only support their children while the children are enrolled in college, but also 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 if loans covered more of the students' expenses, parents suffering adverse economic shocks might make different educational decisions earlier in the children's schooling. Without changes in the financing for attending university, 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 those 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			
Personal Characteristics			
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
Previous Labor Market Outcomes			
Av. Log Income	9.93	9.93	−0.00
Av. Employment (Days)	332.43	334.12	−1.69
Last Employed Sector (Shares)			
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			
Personal Characteristics			
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
Previous Labor Market Outcomes			
Spouse Employed (%)	22.16	24.01	−1.85
Av. Log Income	9.41	9.46	−0.06
Children			
Personal Characteristics			
Female	48.36	47.31	1.06
University Degree (%)	24.83	30.67	−5.84***
Log Average Income at Age 35-37			
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***
Unemployment at Age 35-37			
Mean	29.51	23.12	6.39***
P(UE-Days $\geq$ 10)	25.70	23.03	2.67*
P(UE-Days $\geq$ 30)	20.69	18.47	2.22*
P(UE-Days $\geq$ 90)	11.46	8.89	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-year-old children and the control group consists of all 12-year-old children in families in which the main earner had an unemployment spell in the years 1987–1989. Parental log average income, employment, and unemployment were measured using the five years preceding the reference date.

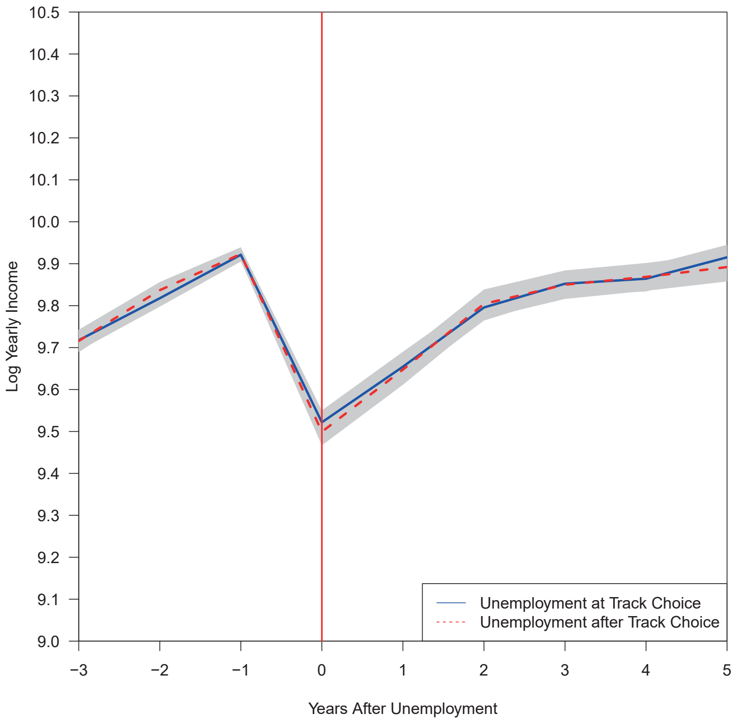
Table 2: Effect of Early Displacement on Children's Educational Achievement

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

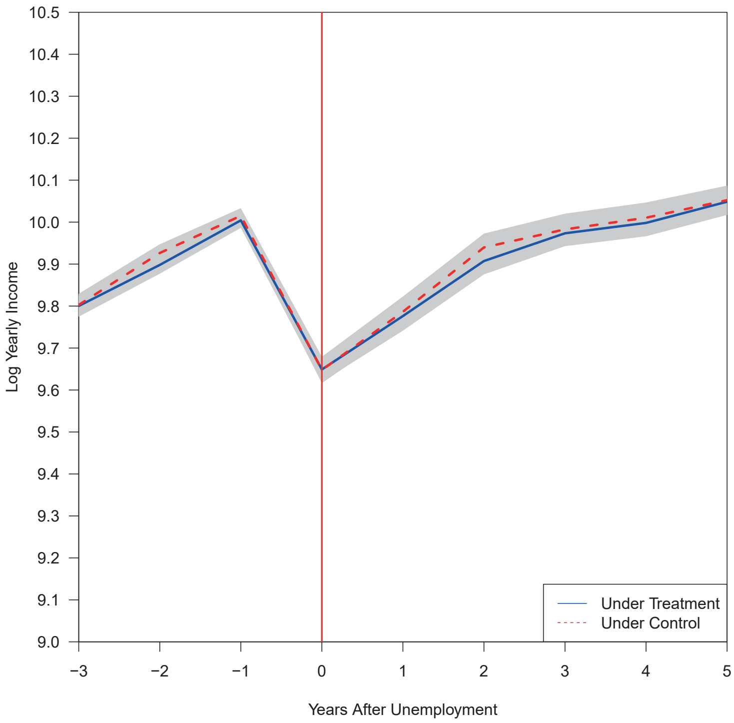
95% confidence intervals obtained from 999 bootstrap replications accounting for clustering within families are reported in brackets. The outcome E is a dummy variable which takes a value of 1 if the child holds a university degree. $E[E(1)]$ and $E[E(0)]$ are obtained by separate logistic regression in the subpopulation with $T = 1$ (early unemployment) and $T = 0$ (late unemployment). The *Total Sample* consists of all 10-year-old children (treated) and 12-year-old children (control) born before September in families in which the main earner had an unemployment spell in the years 1987–1989. The *Two-Parent* sample includes only children of parents who specified a spouse when applying for child benefits. Likewise, the *Single* sample includes only children of parents who did not state a spouse when applying for child benefits, and the *Extended Benefits* sample includes only children of parents in families in which 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 in families in which 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 in families in which the average wage of the main earner was at least 30,000 euros per year. The *Around School Enrollment* sample consist of all six-year-old children (treated) and four-year-old children (control) born before September in families in which the main earner had an unemployment spell in the years 1981–1983.

Figures

Figure 1: Effect of Unemployment on Parental Income



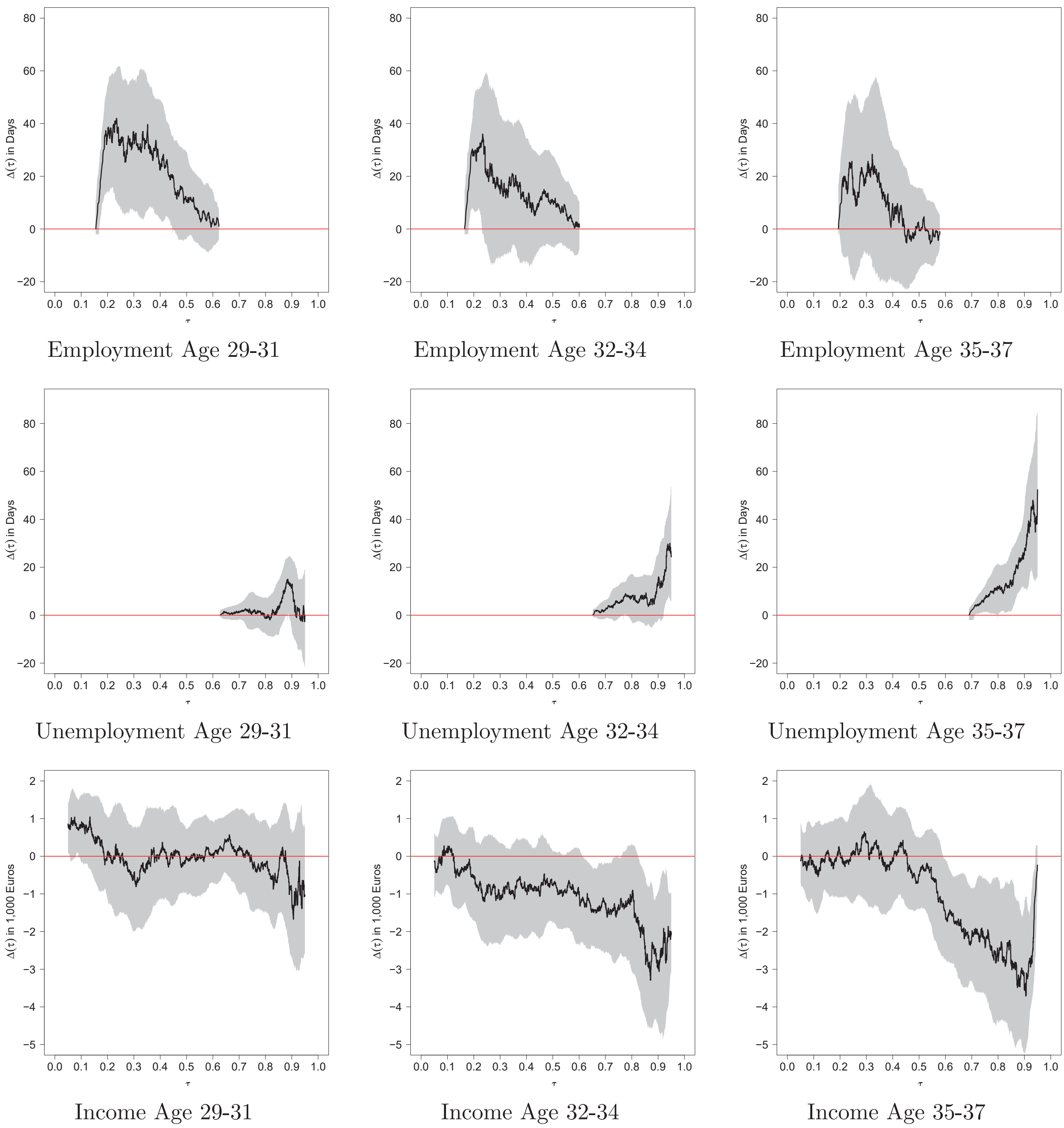
a. Income of Main Earner



b. Income of Family

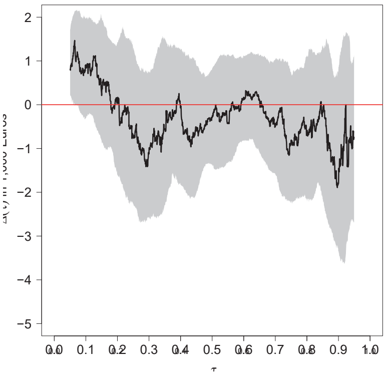
$E[\omega(1)]$ is depicted by the solid line, $E[\omega(0)]$ by the dashed line. Estimates are obtained by separate linear regression in the subpopulation with $T = 1$ (early unemployment) and $T = 0$ (late unemployment). Family Income is the sum of the income of the main earner and the spouse. If the 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

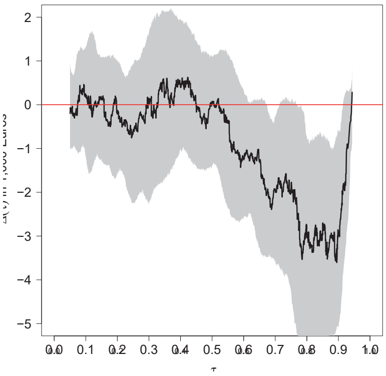


The figure presents estimates of the treatment effect $\Delta(\tau)$ and quantile $\tau \in [.05, .95]$ together with 90% confidence intervals. Outcomes are obtained by using a 3-years average of children's yearly employment and unemployment days as well as income at age 29-31, 32-34, and 35-37. The 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.

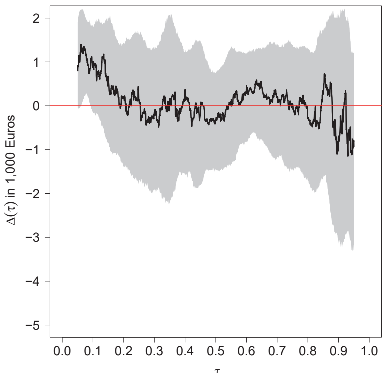
Figure 3: Heterogenous Effects of Early Parental Unemployment on Children's Long-Term Income



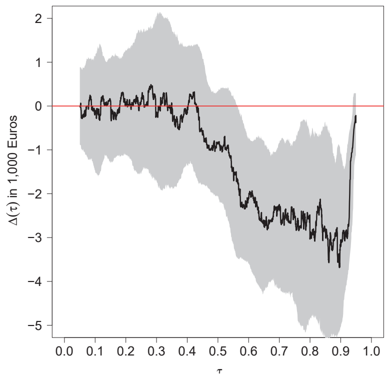
Two-Parent Sample: Age 29-31



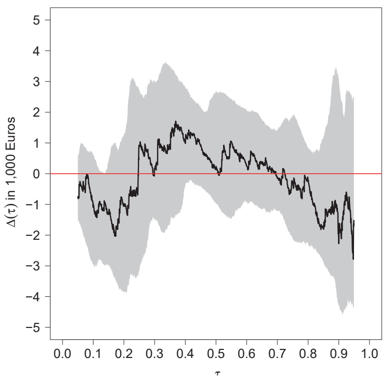
Two-Parent Sample: Age 35-37



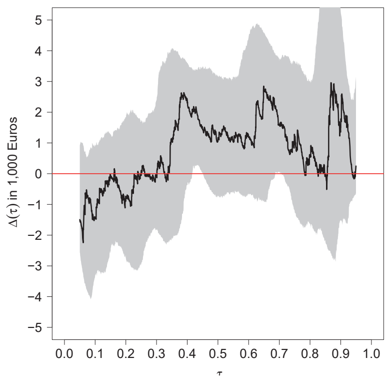
Extended Benefits Sample: Age 29-31



Extended Benefits Sample: Age 35-37



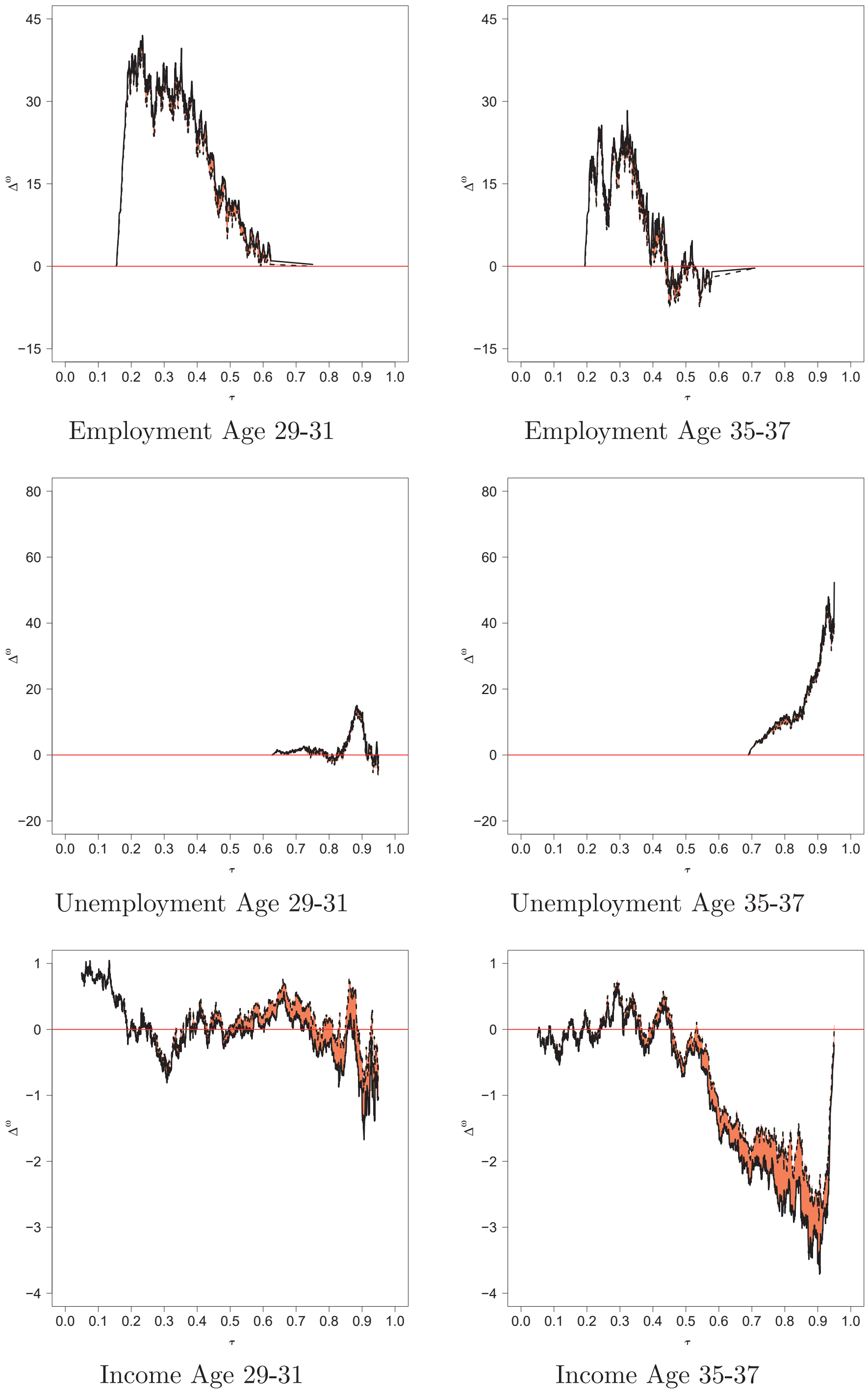
Around School Enrollment: Age 29-31



Around School Enrollment: Age 35-37

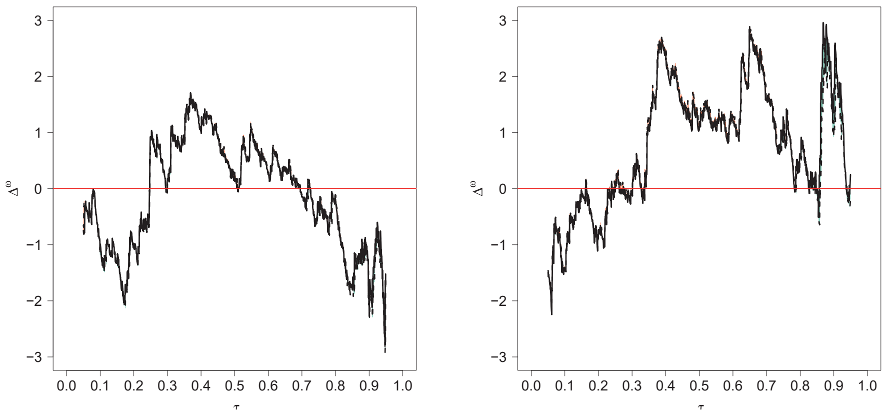
The figure presents estimates of $\Delta(\tau)$ and $\tau \in [.05, .95]$ together with 90% confidence intervals. Outcomes are obtained by using a 3-years average of children's yearly income at age 29-31, 32-34, and 35-37. The basline sample consists of all children who were born before September, were 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* sample includes only children of parents from this sample who specified a spouse when applying for child benefits. The *Extended Benefits* sample 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 E 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 ages 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



Income Age 29-31 Income Age 35-37

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 E is holding a university degree. Outcomes are obtained by using a three-year average of children's yearly employment and unemployment days as well as income at ages 29–31 and 35–37. The sample consists of all children who were born before September, were four or six years old in 1981–1983, and were in families in which the main earner in the family had an unemployment spell during this time. The treatment group consists of all six-year-old children, the control group of all four-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"

Bernhard Schmidpeter

August 18, 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 when the children were 9 or 10 now constitute the treatment group, and children with unemployed parents at age 11 or 12 are now in the control group. As in the main analysis, three-year 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 arrived at in the paper. However, the results tend to be 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 lifetime income.

A.2 Different Age Ranges for Outcomes

In this section, I present results for two-year instead of three-year averages for my outcomes. The treatment and control groups consist of all children whose parents got displaced when the children were aged 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 consequently 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 now include individuals with at least 180 days of employment. Individuals in this sample are still entitled to benefits payments, 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 ages 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 Ages

In Section 6, I argue that my first set of results, obtained at ages 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 have a very strong labor force attachment before that age, but it is also chosen somewhat arbitrarily. 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 ages 20–22, a time in which most of the estimated treatment effects are, significantly, estimates. This is likely because individuals who attend universities only have looser labor market attachment at that period of life and do not pursue full-time work. As individuals become more likely to graduate from university and transition into full-time work, the effect starts to decrease, and my estimates become more noisy with wider confidence intervals—particularly in the upper half of the distribution. The results show that even before the age of 29, one can observe a gradual decline in the labor market outcomes of children in my treatment group.

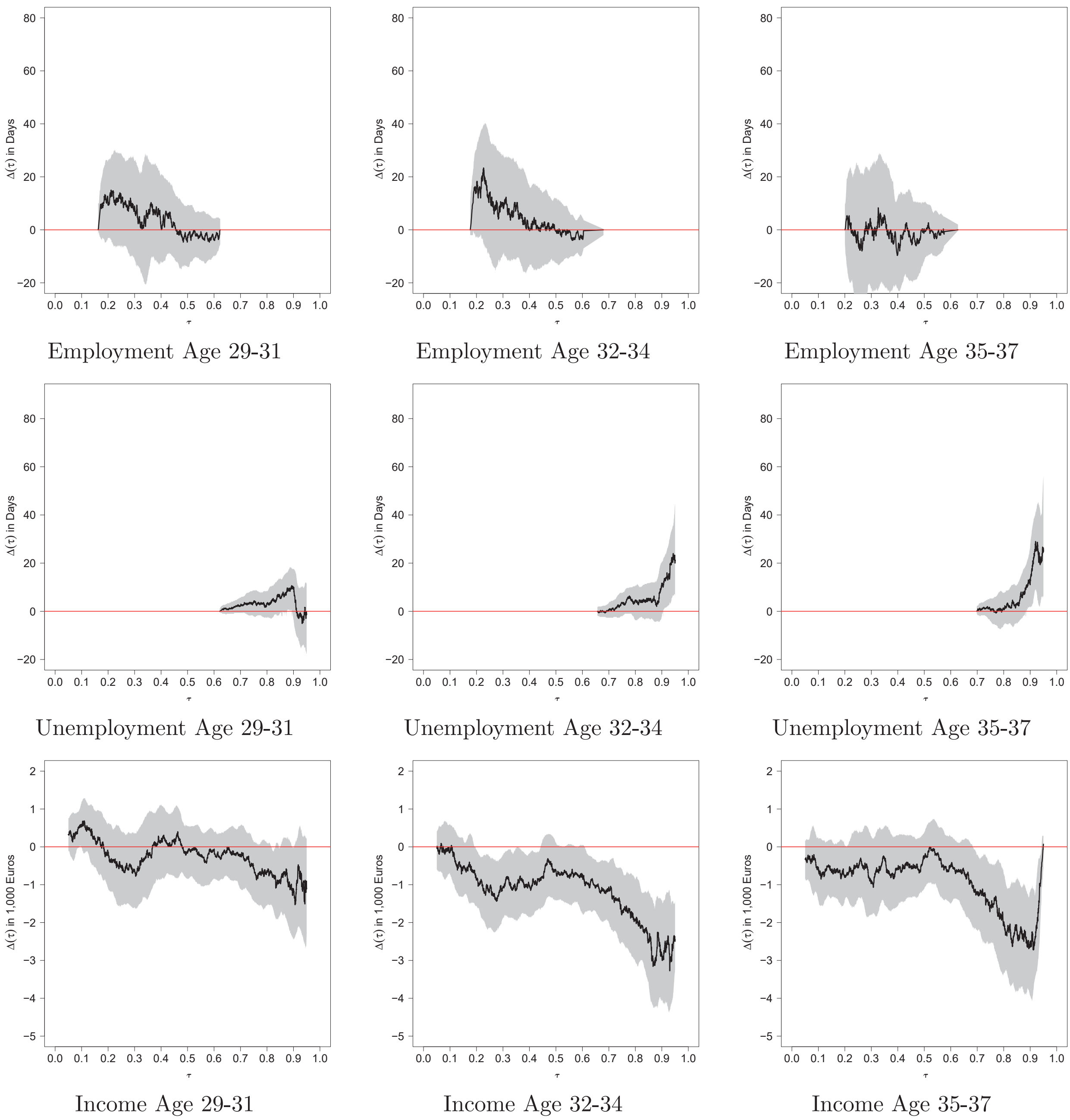
A.5 Estimates Not Including Control Variables

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

I investigate the sensitivity of my results to possible omitted variable bias by reestimating 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 concerns about omitted variable bias in my estimates.

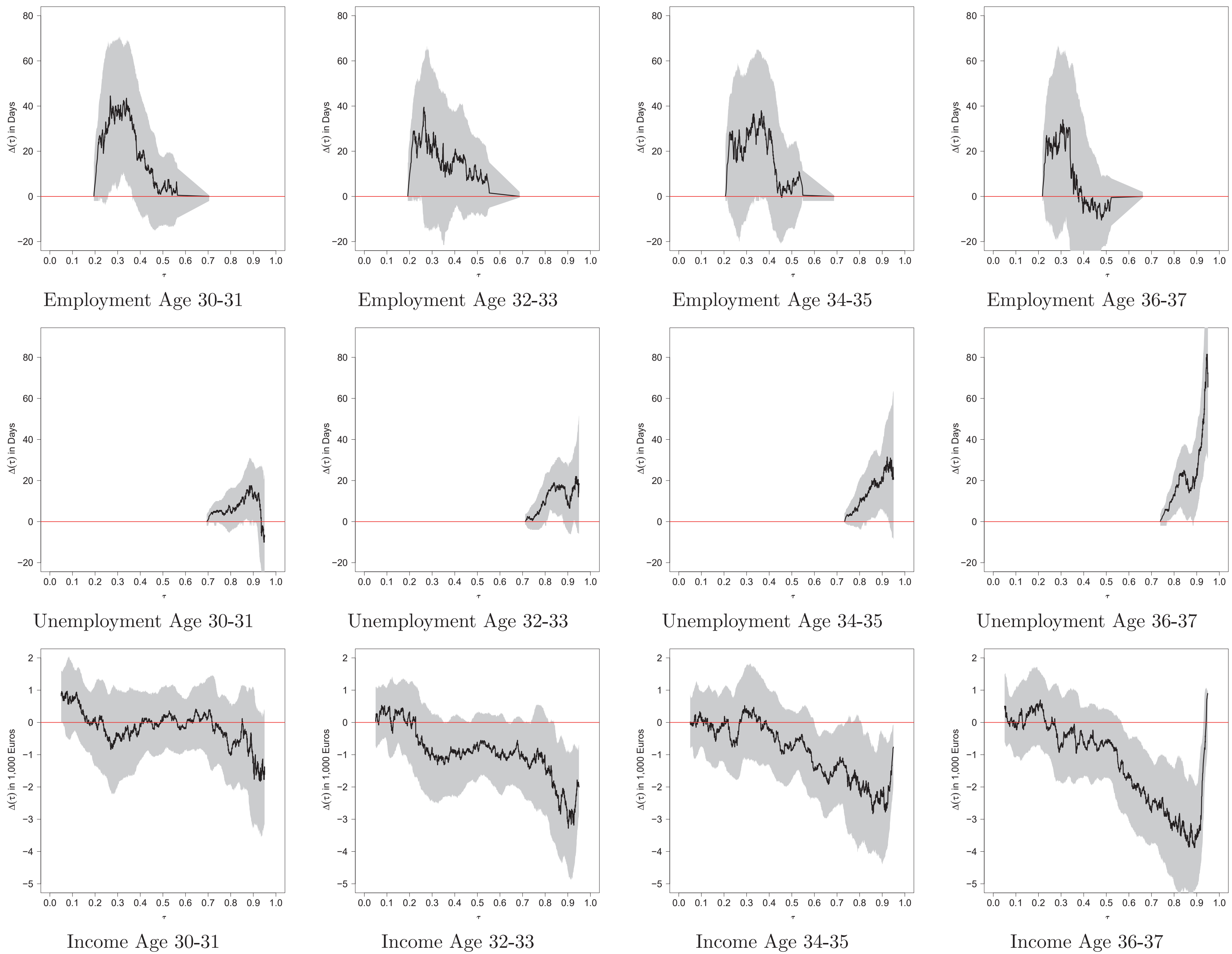
Figure A.5 presents the results when no control variables are included in the estimation. I find the same pattern as reported in the main text for all the outcomes. Reassuringly, even when not including any covariates in the estimation, my results are very similar in both magnitude and significance to 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 their labor market careers. Once children are established in the labor market and higher education becomes more important, I find strong and negative impacts in 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.

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



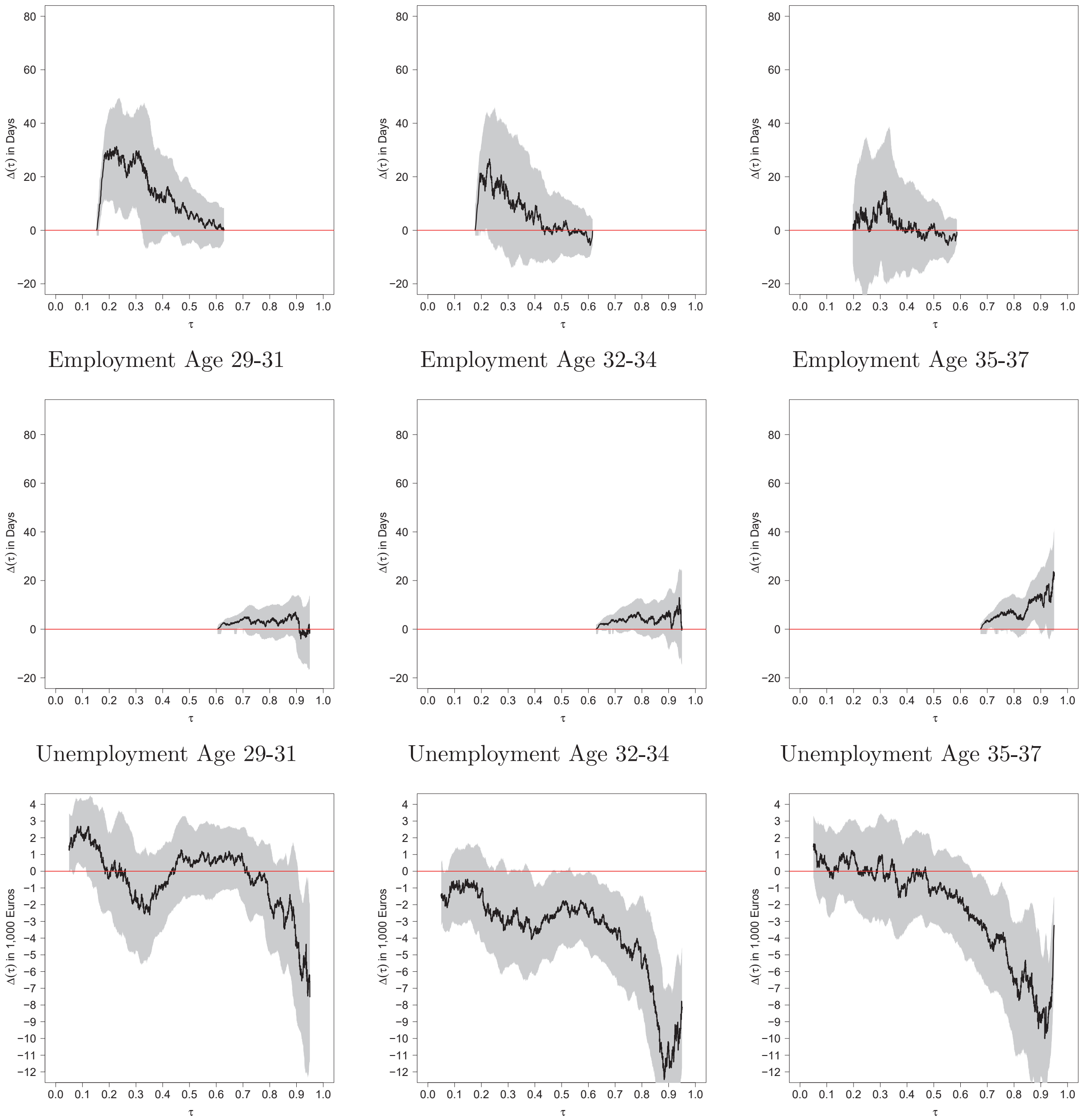
The figure presents estimates of the treatment effect $\Delta(\tau)$ and quantile $\tau \in [.05, .95]$ together with 90% confidence intervals. Outcomes are obtained by using a three-year average of children's yearly employment and unemployment days as well as income at ages 30–31, 32–33, 34–35, and 36–37. The same sample consists of all children who were between 9 and 12 years old at the time of parental unemployment. 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 into account clustering on the family level.

Figure A.2: Treatment Effect of Parental Unemployment on Children's Long-Term Outcomes—Two-Year 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 two-year average of children's yearly employment and unemployment days as well as income at ages 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 into account clustering on the family level.

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



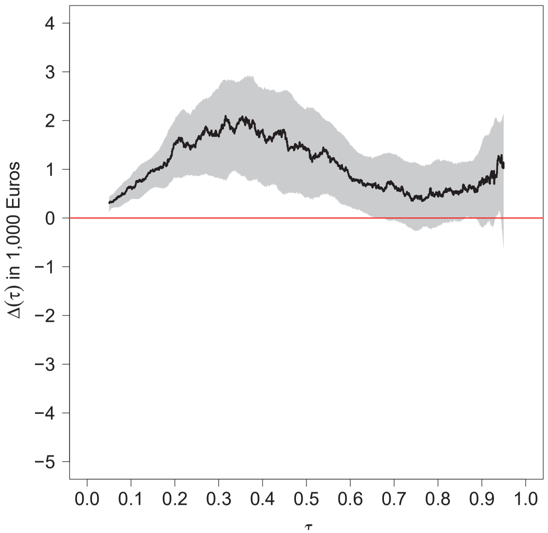
Employment Age 29-31 | Employment Age 32-34 | Employment Age 35-37

Unemployment Age 29-31 | Unemployment Age 32-34 | Unemployment Age 35-37

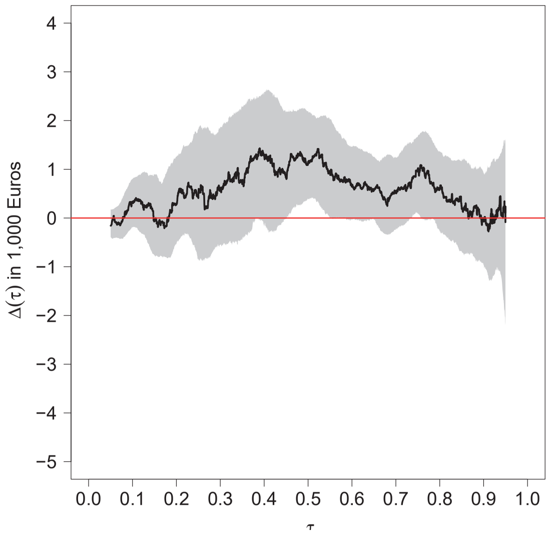
Income Age 29-31 | Income Age 32-34 | Income Age 35-37

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 three-year average of children's yearly employment and unemployment days, as well as income at ages 29–31, 32–34, and 35–37. The sample consists of all children who were born before September and were 10 or 12 years old in 1987–1989, during a time when 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 into account clustering on the family level.

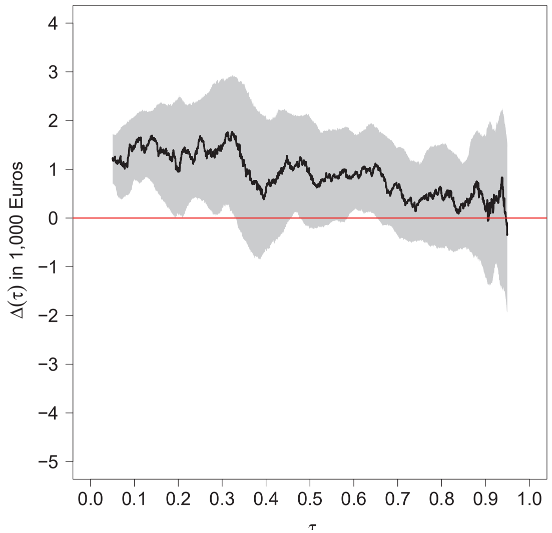
Figure A.4: Treatment Effect of Parental Unemployment on Children's Income at Ages 20-28



Income Age 20-22



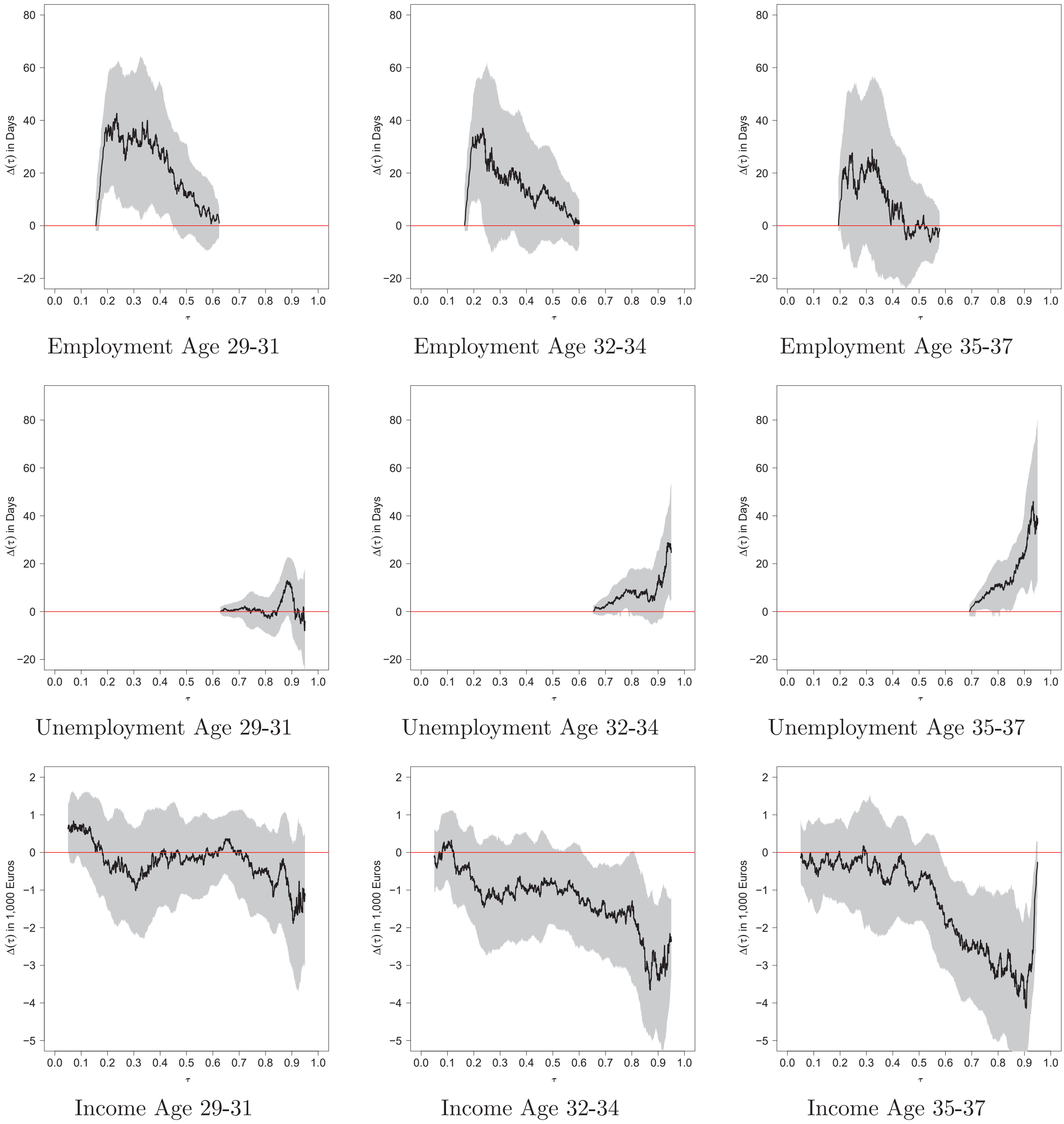
Income Age 23-25



Income Age 26-28

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 three-year average of children's yearly employment and unemployment days as well as income at ages 20–22, 23–25, and 26–28. The sample consists of all children who were born before September and were 10 or 12 years old in 1987 –1989, a time when the main earner in the family had an unemployment spell. 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 into account clustering on the family level.

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 $\tau \in [.05, .95]$ together with 90% confidence intervals *any* additional covariates. Outcomes are obtained by using a three-year average of children's yearly employment and unemployment days as well as income at ages 29–31, 32–34, and 35–37. The sample consists of all children who were born before September and were 10 or 12 years old in 1987–1989, a time when the main earner in the family had an unemployment spell. 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 into account clustering on the family level.

B Children's Educational Achievement and Parents' Predicted Labor Market Outcomes

In this section, I explore the hypothesis that the timing of the adverse income shock coupled with lower expectations of future labor market outcomes leads parents to invest less in their children. The analysis proceeds as follows: First, I predict either a) the probability that the main earner attains the preunemployment income level or b) the average postunemployment income within five years after the unemployment spell.[1] To avoid overfitting, I follow the method suggested by Abadie et al. (2018) in this step. Second, I split my sample into two groups of parents with above and below median probabilities of recovery or above and below median predicted income. Lastly, for each of these two groups I estimate the impact of parental unemployment shortly before the important educational investment decision for their children on a child's probability of holding a university degree. I also estimate the effect on children's educational attainment following similar steps using my sample of parents with an unemployment spell around the time of their child's school enrollment. The results are summarized in Table B.1.

The results in Table B.1 show that the effects discussed in Section 4 in the main part of the paper are mainly driven by children of parents who had an unemployment at the important education decision and a low probability of recovering from the income shock. For this group, I estimate a treatment effect of 8 percentage points. In comparison, I do not find any statistical different impact of parental unemployment around the track choice on parent's educational investment decision for the parents with high likelihood of recovering from the income loss. Using predicted income instead of recovering probabilities produces similar estimates.

In the last row of Table B.1 I also present results for the educational attainment of children whose parents experienced an unemployment spell around school enrollment. There is not evidence that parents adjust their investment into their children when experiencing an unemployment spell around school enrollment age. The estimates for both the high and low likelihood group are rather small. These findings are in line with the results reported in Section 4.

In sum, the results presented here confirm my hypothesis that lower parental investment is caused by the timing of the income shock coupled with lower expectations about future labor market outcomes.

[1] The results are virtually identical when including spouses' income into the prediction of the probabilities.

Table B.1: Children's Educational Achievement by Parents' Predicted Labor Market Outcomes

	Reaching Preunemployment Income Levels		Average Postunemployment Income	
	Low Predicted Likelihood	High Predicted Likelihood	Low Predicted Income	High Predicted Income
Total Sample	-8.74 [-12.75, -4.74]	-1.16 [-4.46,2.11]	-7.66 [-11.36, -4.00]	-1.59 [-4.92,1.88]
Around School Enrollment	0.11 [-6.04,8.52]	-1.51 [-6.66,5.22]	-1.47 [-8.26,6.76]	-0.44 [-5.61,6.55]

95% confidence intervals obtained from 999 bootstrap replications accounting for clustering within families are reported in brackets. Parents are divided into two groups as a function of either a) their predicted probability of reaching preunemployment income within five years after the unemployment spell or b) their average predicted post-unemployment income over the five years after the unemployment spell. To avoid overfitting, the method of Abadie et al. (2018) is used when making the prediction. The outcome E is a dummy variable which takes a value of 1 if the child holds a university degree. The treatment effects are obtained by separate logistic regression in the subpopulation with $T = 1$ (early unemployment) and $T = 0$ (late unemployment) seperately for parents in the high and low group .The *Total Sample* consists of all 10-year-old children (treated) and 12-year-old children (control) born before September in families in which the main earner had an unemployment spell in the years 1987–1989. The *Around School Enrollment* sample consist of all six-year-old children (treated) and four-year-old children (control) born before September in families in which the main earner had an unemployment spell in the years 1981–1983.

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