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Spillover effects from parental unemployment on children's educational and early labour market outcomes

Evidence from Germany

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Abstract: This thesis studies the intergenerational impact of parental unemployment on children's educational and early labour market outcomes. To mitigate selection bias, the empirical strategy relies on plausibly exogenous job ends and applies entropy balancing. Using data from the German Socioeconomic Panel, I provide causal evidence that adverse and long-lasting spillover effects exist in particular for young children. However, they only materialise for paternal unemployment spells. The findings suggest that unemployment in the current generation has large indirect costs for the next generation.

Keywords: Parental unemployment, intergenerational spillover, education, human capital formation, family environment, plant closure, entropy balancing, Socioeconomic Panel

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List of Abbreviations

ATT	Average Treatment Effect on the Treated
ISCO	International Standard Classification of Occupations
ISEI	Standard International Socio-Economic Index of Occupational Status
LPM	Linear Probability Model
OECD	Organisation for Economic Cooperation and Development
OLS	Ordinary Least Squares
PISA	Programme for International Student Assessment
SOEP	Socioeconomic Panel
SUTVA	Stable Unit Treatment Value Assumption
VET	Vocational Education and Training

1 Introduction

In Germany, around 2.6 million people were registered unemployed in 2021, amounting to an unemployment rate of 6.3 percent (Federal Statistical Office of Germany, [2022](#)). It is well-documented that unemployment has negative consequences for the directly affected individual. Not only are job loss and unemployment associated with a reduction in income and earnings (see Couch and Placzek, [2010](#); Hijzen et al., [2010](#)), empirical evidence suggests that it can also deteriorate physical and mental health (see Browning and Heinesen, [2012](#); Eliason and Storrie, [2009](#); Kuhn et al., [2009](#); Marcus, [2014](#); Paul and Moser, [2009](#); Schmitz, [2011](#)) and even lead to increased risk of mortality and suicide (see Gerdtham and Johannesson, [2003](#); Milner et al., [2014](#); Sullivan and Von Wachter, [2009](#)). Much less is known about the potential externalities of unemployment on other household members.

According to Eurostat ([2022](#)), as much as 8.8 percent of all children in Germany between the ages of 0 and 17 live in households with no employed person. Given the crucial role of parental inputs in the human capital formation of children (Cunha and Heckman, [2007](#)), it is conceivable that unemployed parents are not able to optimally invest in their children's skill formation. If this were to entail lower educational achievement and inferior labour market outcomes for children of unemployed parents later in life, this would have important implications for social mobility and social inequality.

Against this background, this thesis explores spillover effects from parental unemployment on children's educational and early labour market outcomes in Germany. The aim of the research is to find out whether such spillovers exist, and if yes, in what form and how they vary across different groups. Germany is a particularly interesting case study. On the one hand side, Germany has a favourable institutional environment with a generous social security system and a public education system without tuition fees for higher education. On the other hand side, Germany's education system is nevertheless prone to reproducing socioeconomic inequalities. Illustrating this, 74 percent of children with at least one tertiary educated parent enrol into university but among children without tertiary educated parents it is only 21 percent (Hochschulbildungsreport, [2020](#)). In this

regard, focusing on unemployment offers an opportunity to examine the role of shocks rather than fixed characteristics such as the parents' educational background.

The data used in this study comes from the German Socioeconomic Panel (SOEP), a longitudinal household survey of the German population that has been running since 1984. The main identification challenge is that unemployment spells are not random but likely disproportionately affect families with characteristics that already predict worse educational and early labour market outcomes for their children to begin with. As a solution, I exploit plant closures and lay-offs as plausibly exogenous entries into involuntary unemployment. This means that I essentially compare children whose parent loses their job through plant closure or lay-off and subsequently becomes unemployed to children for whom neither parent experiences unemployment through any type of job end. Since I am interested in investigating heterogeneous effects, I construct twelve different treatment indicators that distinguish themselves in terms of the gender of the unemployed parent and the age of the child at the time of job loss.

Nonetheless, this design cannot be expected to fully randomise treatment exposure. There might still be negative selection, for instance into working at firms vulnerable to closure in the first place or into staying at closing firms until the end. Yet if the treatment and control group differ with respect to important confounding factors, the estimated treatment effect will be biased. This is why I employ the data preprocessing method known as entropy balancing (Hainmueller, 2012). The purpose of this method is to reweigh the control group such that it becomes more comparable to the treatment group, which mitigates remaining selection bias. This is done by generating a set of weights equating the mean of the distribution for pre-specified confounding factors between both groups. Afterwards, I use the preprocessed data to estimate the effect of parental unemployment on children's educational and early labour market outcomes in a linear regression model. Under the identifying assumption that there are no unobservable time-invariant or time-varying characteristics that simultaneously influence the incidence of parental unemployment due to plant closure or lay-off and children's educational and early labour market outcomes, the estimated effect can be interpreted causally.

The main finding is that adverse effects of unemployment are indeed transmitted to children in the sense that they consequently achieve lower levels of educational attainment. However, the existence of spillovers can only be detected when unemployment is experienced by the father. In contrast, maternal unemployment does not appear to have a significant impact in either direction. Besides the parent's gender, the child's age at onset of unemployment seems to matter. The spillover effect from paternal unemployment is especially strong for young children. The results suggest that children who experience paternal unemployment while they are 6 to 9 years old are 16 percentage points less likely to graduate from secondary school with a university entrance qualification and 8 percentage points less likely to earn a university degree. These estimates are significant at the 1 percent level and pass a battery of robustness checks. In particular, I show that regressing contemporaneous outcomes on placebo treatments in the future does not produce significant estimates, which lends credibility to the causal interpretation of my findings. Yet sample size limitations hamper the interpretation of estimated effects on early labour market outcomes.

This thesis contributes to the literature in multiple ways. First, it provides novel evidence for Germany on outcomes that have not been studied yet such as the transition from primary to secondary school. Second, it looks into the impact of maternal unemployment relative to paternal unemployment, which remains understudied in the literature and without consistent findings. Third, it already covers parental unemployment shocks experienced by children as young as six years old and allows the estimated effect to vary across age groups.

The rest of this thesis is structured as follows. Section 2 provides a brief overview of the current state of research in the directly related literature, while Section 3 draws on a large body of theoretical and empirical literature to identify mechanisms that can potentially mediate spillover effects from parental unemployment on children. Important features of the institutional setting in Germany are outlined in Section 4. Sections 5 and 6 explain the empirical strategy and the data, respectively. Section 7 then presents the results, which undergo robustness tests in Section 8. Finally, Section 9 discusses the results and acknowledges limitations before a conclusion is drawn in Section 10.

2 Literature Review

The topic of spillover effects from parental unemployment on child outcomes has already received much attention in the literature. Empirical studies consider a variety of child outcomes. In the United States, Schaller and Zerpa (2019) find that paternal job loss is harmful to children's physical and mental health while maternal job loss is not. Powdthavee and Vernoit (2008) show that parental job loss when the child was relatively young has a positive impact on the child's self-reported happiness in the United Kingdom. In particular, the German SOEP is a widely used data set among researchers: Angelini et al. (2018) focus on the "big five" personality traits and report that paternal unemployment makes children more conscientious and less neurotic. Similarly, Peter (2016) demonstrates that maternal job loss increases socio-behavioural problems among preschool children and decreases the locus of control among adolescents. Nikolova and Nikolaev (2021) study life satisfaction during early adulthood and only detect negative effects of parental job loss during early childhood and early adolescence, but not in the period in between. Boys appear to be more negatively affected than girls, which is further substantiated by evidence from Haisken-DeNew and Kind (2012). Findings from Siedler (2011) suggest that there is a positive relationship between growing up with unemployed parents and young people's far-right wing party affinity.

Concerning labour market outcomes, the literature does not provide consistent evidence across countries. In the United States, Hilger (2016) as well as Page et al. (2007) find no effect of parental job loss on early career earnings for specifications using firm closure as the reason for job loss. Yet in Canada, Oreopoulous (2008) demonstrates that paternal job loss through firm closure does reduce earnings for male adults by 9 percent. In Norway, Bratberg et al. (2008) do not detect effects of paternal unemployment on earnings, while Huttunen et al. (2018) only report a reduction in earnings at age 30 by 2.2 percent. They further highlight that paternal unemployment influences children's career choices; children of displaced fathers are less likely to choose the same field of study or work in the same plant as the parent.

Regarding educational outcomes, the literature largely points toward adverse effects on

parental unemployment, although the role of maternal unemployment is not clear. In the United States, Wightman (2012) shows that parental job loss due to firm closure lowers the likelihood of obtaining post-secondary education by 5 percent. Hilger (2016) only finds that paternal job loss due to plant closure decreases college enrolment rates by less than 0.5 percentage points. Stevens and Schaller (2011) report that parental unemployment increases the probability of grade retention by 15 percent. With respect to maternal job loss, Kalil and Ziol-Guest (2008) find no significant associations between mothers' employment experiences and children's academic progress even if mothers are the main earners in their household. In contrast, Brand and Simon Thomas (2014) demonstrate that among single mothers, maternal job loss leads to a 4 percentage points lower high school completion and a 6 percentage points lower college completion. According to Coelli (2011), parental unemployment in Canada reduces the probability of university enrolment by 10 percentage points. Ruiz-Valenzuela (2021) and Rege et al. (2011) study the effect of father's job loss on grade averages in Spain and Canada and find that it reduces grades by 13 and 6 percent of standard deviation, respectively. In both cases, maternal unemployment has no significant impact. On the contrary, Mörk et al. (2020) find small negative effects of maternal job loss on educational outcomes in Sweden but no indications of effects for paternal job loss. Schmidpeter (2020) considers paternal and maternal unemployment together and shows that in Austria, parental unemployment shortly prior to an important educational decision lowers a child's likelihood of earning a university degree by more than 5 percentage points.

Given the vast differences in welfare regimes and education systems across countries, I assess the external validity of the existing literature to be rather low. Therefore, there is merit in producing novel evidence for Germany. To the best of my knowledge, only two studies have explored the intergenerational impact of parental unemployment on educational and early labour market outcomes in Germany so far. However, this thesis improves upon them in terms of scope and methodology. Lindemann and Gangl (2019) only consider parental unemployment while the child is between 12 and 17 years old and restrict the sample to children who are already tracked into the academic track

of secondary school. Similarly, Müller et al. (2016) only take paternal unemployment experienced between the age of 10 and 15 into account. Yet this study covers children who experience parental unemployment between the age of 6 and 18. This makes it possible to encompass a broader range of outcome variables and in particular to explore the impact on tracking choice. Neither of the two studies narrows the type of job end down to those that can be considered most exogenous. Since my empirical strategy restricts the entry into unemployment to plant closures and lay-offs, employs entropy balancing, and conducts a placebo-in-time robustness check, the causal interpretation of my findings appears more plausible. Generally speaking, this thesis also contributes to the body of literature by differentiating between paternal and maternal unemployment and by shedding light on the heterogeneous effects across the age of the child at the time of the parental unemployment spell.

3 Theoretical Framework

Two types of theoretical models are relevant in the context of parental unemployment and children's educational and early labour market outcomes.

The first ones are models of skill formation. In the framework proposed by Cunha and Heckman (2007), human capital accumulation is modelled as a dynamic process that allows inputs to impact skill formation differently across childhood stages. Two features of the model are critical, namely self-productivity and dynamic complementarity of human capital. Self-productivity suggests that skills acquired in one period persist into future periods so that early investments are harvested over a longer time period. Dynamic complementarity embodies the idea that skills produced at one stage raise the productivity of investment at later stages, thus creating synergies. The main implication is that investments in skill formation yield higher returns the earlier in life they are made.

The second type of theoretical models relate to educational decision-making. Breen and Goldthorpe (1997) put forward a standard rational choice model, in which children and their families choose among alternative educational options on the basis of evaluations of their financial costs and subjective benefits. Each educational option is weighted by its

perceived probability of successful completion.

Using these models and a substantial body of empirical literature as a basis, I identify four channels through which parental unemployment can have spillover effects on children. The income and time channel are mainly related to models of skill formation, whereas the expectations channel is inspired by models of educational decision-making. These channels suggest different directions of the potential spillover effect, which makes it difficult to predict *ex ante* whether the effect would be positive or negative. The relative importance of each channel likely varies across children's age at the time of the unemployment spell as well as across the gender of unemployed parent.

3.1 Income channel

The most obvious way in which the effects of parental unemployment can be mediated to children is via an income channel. Empirical studies have shown that income changes during childhood have an impact on children's human capital development (see Dahl and Lochner, 2012; Hoynes et al., 2016). Unemployment can inhibit families' economic ability to invest in skill formation and to support their children in their education pursuits. First and foremost, attending university is costly, both in terms of direct costs and foregone income. But even in earlier stages of the education system, income can play a crucial role. For example, unemployment may prevent parents from choosing high-quality schools since the housing costs in the surrounding areas of those schools tend to be high and increasing (Fack and Grenet, 2010).¹ Moreover, parents may not be able to afford private tutoring or other resources considered critical for creating a safe and cognitively-enhancing learning environment, such as proper healthcare, nutritious food, or books. Given that the financial consequences for the household of a father's job loss may be more severe than of a mother's job loss, it can be expected that the income channel is more relevant for cases of paternal unemployment than for maternal unemployment (Lindemann and Gangl, 2019).²

¹Indeed, Huttunen et al. (2018) find that job displacement increases regional mobility.

²In Germany, men still remain the primary providers in many households despite the rise in dual-earner arrangements (Trappe et al., 2015).

It is important to point out that the income channel could not only operate through a reduction in transitory disposable income, but also through diminished labour market prospects and binding borrowing constraints. Kuchler and Zafar (2019) show that experiencing unemployment can evoke more pessimistic views about future employment prospects and wages. As unemployed parents adjust their expectations about lifetime earnings, they may reevaluate educational investment decisions.³ Following this logic, Schmidpeter (2020) argues that in the Austrian case already the prospect of higher financial costs of university education far ahead in the future might convince parents to opt for the vocational track of secondary school for their child, which does not typically lead to university education. This could occur even if the educational decision is taken many years before any payment would be due.

3.2 Time channel

Besides income, time spent with parents is regarded as a major input in the production function of human capital formation. However, parents face a trade-off when allocating their time, since more time spent in childcare implies less time earning an income. In this sense, joblessness reduces the opportunity cost of childcare and allows parents to spend more time with their children. This is supported by evidence from Knabe et al. (2010), who find that unemployed parents in Germany spend almost twice as much time caring for children than their employed counterparts.

Yet the extent to which unemployed parents substitute labour is likely different for mothers and fathers. For example, van der Lippe et al. (2018) report that women in European countries increase their time spent on housework during unemployment more so than men. An explanation for this lies in the gender identity hypothesis, which stipulates that individuals derive utility from complying with the social norms related to their gender identity (Akerlof and Kranton, 2000). Across cultures, women are viewed primarily as

³Coelli (2011) notes that the unexpected nature of an unemployment shock may play an important role in explaining negative effects of job loss on youth education outcomes since parents may have already planned spending commitments out of expected future earnings. According to a survey conducted in Canada among parents of children aged 13 to 18, the majority of parents relied on using future earned income to contribute to the cost of their children's post-secondary education (Shipley et al., 2003).

mothers and wives, which is why women may be more inclined to use an unemployment spell as an opportunity to fulfill more family-related duties in the household (Nikolova and Nikolaev, 2021). In contrast, men are seen as responsible for providing a living for their family. In line with this, Forret et al. (2010) show that fathers are far more likely to perceive unemployment as a defeat than mothers. As a result, fathers in unemployment might reallocate less time to childcare and focus instead on the job search.

While the general expectation is that increased time spent with parents is conducive to a child's cognitive and psychological development and thus enhances their outcomes later in life, this positive effect can depend on the quality of the care as well as the timing. In particular, younger children likely benefit more from interactions with their parents relative to older children. These differences can be strong even within a relatively small age range as demonstrated by Del Bono et al. (2016), who conclude that maternal educational time spent with the child at age three is almost three times as productive as educational time spent at age five.

3.3 Family stress channel

Parental unemployment may deteriorate the family environment in which children grow up in a number of ways and thereby impair their development over the life course. There is substantial evidence that unemployment depresses mental health and lowers life satisfaction (see Browning and Heinesen, 2012; Kuhn et al., 2009; Powdthavee and Vignoles, 2008; Kassenboehmer and Haisken-DeNew, 2009). This seems to be especially the case for men, who typically face the threat of not complying with the male breadwinner role (Paul and Moser, 2009). The psychological consequences of unemployment can even be transmitted to partners (Marcus, 2013) and children (Schaller and Zerpa, 2019). Consequently, unemployment can disrupt the structure of family relations and permanently scar children. Poor mental health or high levels of stress might compromise a parent's ability to provide the adequate emotional support or a stable learning environment for their child. Unemployment is also associated with an increased likelihood of divorce (see Charles and Stephens, 2004; Doiron and Mendolia, 2012; Eliason, 2012).

3.4 Expectations channel

As mentioned above, educational decision-making can be modeled as a rational choice between different educational options on the basis of their costs and benefits as well as of the perceived probability of successfully completing them. In this context, parental unemployment can influence educational decision-making by changing expectations about the returns to education and about the likelihood of success. Previous literature offers two different perspectives on the possible direction of this effect.

On the one hand side, scholars promote the view that adverse life events such as parental unemployment can foster future resilience in children and thereby have a positive effect on their educational and labour market outcomes. Under the impression of their parents' negative experience, children might develop coping mechanisms to minimise their own risk of falling into unemployment in the future. It is plausible to assume that the nature of the coping mechanism might depend on the unemployed parent's educational background. If the parent only had low educational attainment, the child might form the expectation that returns to education are high and thus be encouraged to pursue higher education. As argued by Seery (2011), children of unemployed parents might become more motivated to work hard in school and develop higher educational ambitions. Whether this effect materialises may depend on the child's age; as older children start developing abstract thinking and are more aware of the consequences of their decisions, they may be more likely to develop resilience in response to unemployment (Cole et al., 2005; Piaget, 1971). Furthermore, the argument is brought forward that girls are especially effective in coping with adverse life events early in life since they typically display higher levels of inhibitory controls than boys at the same age (Matthews et al., 2009).

On the other hand side, parental unemployment may lead children to lower their educational aspirations. Because of their parents' labour market experience, children may develop overly pessimistic expectations about their own chances of success, resulting in lower motivation and disengagement from school. For example, Peter (2016) shows that children experiencing maternal job loss in Germany exhibit a lower belief in self-determination. The exposure to unemployment risk and the implied consequences may produce a so-called

“scaring effect” (Knabe and Rätzel, 2011) and lead affected children to become more risk-averse. Against this background, investments in education may appear too risky to youth who have experienced parental unemployment (Coelli, 2011). Again, this effect is more likely to manifest itself among older children.

4 Institutional Setting

In many ways, the German institutional environment may not appear particularly vulnerable to intergenerational effects of unemployment. Compared to Anglo-Saxon countries, Germany has a generous unemployment benefit system. For at least one year, the earnings replacement rate is 67 percent for workers with children. Reduced benefits are offered afterwards. On top of this, tertiary education is offered free of charge, and the education system is dominated by public institutions, with private schools and universities only playing a negligible role. Schools and universities are both state-financed, for the most part based on student headcounts, which generates a relatively large equality in the endowment and quality across educational institutions (Dustmann et al., 2017). Against this background, the relevance of financial constraints for children’s educational attainment should be lower than in other countries.

At the same time however, the German education system is characterised by a high degree of stratification, which refers to the separation of students into horizontal branches with different academic orientation.⁴ Compulsory schooling starts at the age of six and after completion of the four-year primary school, children are sorted into one of the three secondary school tracks.⁵ This implies that for most children, the tracking choice is made around the age of ten, which is considerably earlier than in other countries with a tracking system.⁶ The transition from primary to secondary school is determined by

⁴It is important to note that the responsibility for the education system does not fall under the jurisdiction of the federal government but under that of the German states, which is why there are certain differences across states. Nonetheless, the general structure of the education system is uniform (Biewen and Tapalaga, 2017).

⁵Exceptions are Berlin and Brandenburg, where primary school lasts six years.

⁶On average, tracking takes place at age fourteen in Organisation for Economic Cooperation and Development (OECD) countries (OECD, 2014).

recommendations from primary school teachers based on school performance, which can be overridden by parents in most of the states (Dustmann et al., 2017).

The secondary school tracks follow a hierarchical order and only the highest track (*Gymnasium*) leads directly to the university entrance qualification (*Abitur*) obtained after eight or nine years. In contrast, the intermediate (*Realschule*) and the lower track (*Hauptschule*) aim to prepare children for an apprenticeship in blue-collar or white-collar occupations. Early track choice may be reversed at a later stage during the education cycle. In principle, switching tracks is possible in any year throughout lower secondary school, although this is uncommon. Instead, students with strong performance tend to move up to a higher track upon completion of lower secondary education - after five years in the lower track and six years in the intermediate track - and continue their education at a different institution (Dustmann et al., 2017). Moreover, students have the option to attend a specialised vocational high school, upon completion of which they obtain the entrance qualification for universities of applied sciences or in a restricted field of study for universities (*Fachabitur*). The share of students graduating with a university entrance qualification has continuously increased over the past years: 41.2 percent of the total age-specific population had obtained *Abitur* in 2015, up from 22.9 percent in 1990 (Bundesministerium für Bildung und Forschung, 2021).⁷

A distinct feature of the German education system is its well-developed system of vocational education and training (VET), which is offered after completion of lower secondary school. Its main characteristic is its strong dual component, integrating learning at vocational schools and workplace training. Typically, apprentices in the dual system spend three days per week in a training firm and two days at school. Apprentices earn a small salary, which is however considerably lower than that of regular employees (Biewen and Tapalaga, 2017). After the apprenticeship, many apprentices continue working at their training firm. In this respect, the VET system is praised for providing a smooth entry into the labour market and is often used to explain the exceptionally low levels of youth unemployment in Germany (OECD, 2014). Lindemann and Gangl (2019) therefore accurately describe

⁷Considering both *Abitur* and *Fachabitur*, the shares are 31.4 and 53.0 percent in 1990 and 2015 respectively (Bundesministerium für Bildung und Forschung, 2021).

this system as a “low-cost and low-risk” pathway, potentially “diverting students from lower socioeconomic backgrounds away from tertiary education”. Consequently, the level of tertiary attainment in Germany remains low and the tertiary attainment gap between Germany and other OECD countries is widening (OECD, 2022). In 2018, only 32 percent of 25 to 34 year-olds in Germany had attained a tertiary degree, compared to an OECD average of 44 percent. In spite of the strengths of the VET system, the earnings advantage for workers with tertiary education is large. The wage premium of full-time employed tertiary graduates relative to upper secondary graduates amounted to 69 percent in 2017, above the OECD average of 57 percent (OECD, 2020).

Although there are no tuition fees, tertiary education still requires expenses and represents opportunity costs. In Germany, parents are obliged by law to provide financial support to their children up to their first professionally qualifying degree to the extent that their own financial resources allow them to do so. In case of insufficient financial resources, the state contributes to the financing via the student allowance system (*Bafög*). This is divided into a grant component and an interest-free loan component, where both components have equal shares. According to the 21st Social Survey carried out on behalf of Deutsches Studentenwerk, the average student in 2016 received 52 percent of their income from their parents and only 13 percent from *Bafög* support. The remainder was made up of own employment and other sources of income. Since its introduction in 1971, the role of *Bafög* in student financing has gradually decreased and the rate of supported students was only 18 percent in 2016 (Dohmen et al., 2021). An overview of the German education system is provided in Appendix A.1.

The OECD argues that the system-level policies present in Germany such as tracking can hinder equity, and suggests that reducing the impact of socioeconomic background on student outcomes is a key policy issue for Germany (OECD, 2020). In Programme for International Student Assessment (PISA) 2018, advantaged students outperformed disadvantaged students in reading by 113 score points in Germany, whereas the mean performance gap was only 89 score points across OECD countries. Similarly, socioeconomic status was a strong predictor of performance in mathematics; it explained 18 percent of

the variation in mathematics performance in PISA 2018, compared to 14 percent at the OECD average (OECD, 2019).

5 Empirical Strategy

5.1 Identification strategy

As first formalised by Rubin (1974), estimating causal treatment effects essentially consists of a comparison between two potential outcomes, namely the outcome if a unit receives the treatment, and the outcome if the same unit does not receive the treatment. Yet in practice, only one of the potential outcomes can be observed for each unit, whereas the other outcome remains counterfactual. This is commonly referred to as “the fundamental problem of causal inference” (Holland, 1986). Researchers address this problem by finding a control group that is as similar as possible to the treatment group with regard to variables that are related both to the treatment exposure and the outcome in question. In experimental studies, this can be achieved by randomising the assignment of units to the treatment and control group. In the typical setting of observational studies however, treatment is not allocated randomly and so the two groups look very different in terms of the distribution of covariates. Comparing them would not produce reliable estimates owing to confounding bias.

In relation to the question of interest in this thesis, the main identification challenge is that unemployment does not occur randomly but is likely correlated with important determinants of children’s educational and early labour market outcomes. For instance, highly educated workers may face a lower risk of displacement but may also spend time with children in a way that is more favourable to their skill formation. In order to overcome this challenge, my identification strategy relies on types of involuntary job loss that are plausibly exogenous. Clearly, this is most likely the case for displacements through plant closures because plant closures affect all workers of a firm, irrespective of any of their behaviours that could in turn be associated with child outcomes. Hence, exploiting plant closures is a widely used approach in the literature investigating consequences of

unemployment (see Brand and Simon Thomas, 2014; Bratberg et al., 2008; Kassenboehmer and Haisken-DeNew, 2009; Marcus, 2013; Mörk et al., 2020; Nikolova and Nikolaev, 2021; Peter, 2016; Rege et al., 2011). Yet displacements through firm closure do not occur frequently, which makes it statistically challenging to focus exclusively on plant closures when using survey data. Therefore, the main specification combines plant closures and dismissals by employers as potential entries into unemployment, whereas plant closures as a distinct type of involuntary job loss are used to investigate the robustness of results. This approach is not uncommon in the literature (see Coelli, 2011; Kalil and Ziol-Guest, 2008; Peter, 2016; Schaller and Zerpa, 2019; Schmidpeter, 2020). In particular, Peter (2016) argues that concerns about endogeneity should not be overestimated in light of the strict employment protection against individual dismissal provided by the German law. In this sense, dismissals by employers likely occur for operational reasons rather than for behavioural ones. Furthermore, Marcus (2013) shows with the same SOEP data that results from combining plant closures with lay-offs are similar to the ones obtained from restricting the entry into unemployment to plant closures only.

My identification of causal treatment effects therefore makes use of a comparison between children whose parent experienced unemployment due to plant closure or lay-off during the observation period (treatment group) with children where neither parent experienced unemployment of any kind during the observation period (control group). Details about the construction of the treatment and control group are provided in Section 6.2. An alternative option would have been to exploit the timing of parental unemployment and study children who both experienced parental unemployment but before and after a given point in time (see Huttunen et al., 2018; Pan and Ost, 2014; Schmidpeter, 2020). The underlying reasoning behind this approach is that it is clearly random whether a parent loses their job when the child is 10 or 12 years old. Still, this option was discarded as it was considered unsuitable for the research purpose of this thesis.⁸

⁸The unsuitability stems from a number of factors: On the one hand side, one could select a point in time as a threshold that lies around the realisation of the outcome of interest as in Pan and Ost (2014) (e.g. consider unemployment shocks in the treatment group between the age of 6 to 9, study the transition to secondary school around the age of 10, and consider unemployment shocks in the control group between the age of 12 and 14). However, considering the short period of time between the unemployment spell and the realisation of the outcome, this research design would only capture short-term effects of parental

Nonetheless, concerns about non-random selection into treatment remain. There might be negative selection into working at closing workplaces in the first place as unsuccessful firms might employ workers that have lower productivity and lower qualifications on average. Even more, there might be negative selection into staying at a closing firm until it goes out of business. Plant closures can usually be foreseen and therefore parents with certain characteristics might manage to find new employment seamlessly. In contrast, workers with highly firm-specific skills might be less flexible in changing their employer (Kassenboehmer and Haisken-DeNew, 2009). In order to mitigate concerns about selection bias, I follow Marcus (2013) and apply entropy balancing.

Entropy balancing is a data preprocessing method that aims to achieve covariate balance by reweighting the control group in such a way that it becomes more comparable to the treatment group. In this regard, entropy balancing fulfills the same purpose as commonly-used propensity score methods but the approach taken by entropy balancing has a number of key advantages, which is why it is the preferred choice in this thesis. Propensity score methods originate from the seminal work of Rosenbaum and Rubin (1983) who propose the propensity score as a measure of the probability of exposure to treatment conditional on observed covariates. The underlying idea is that control units with a higher probability of exposure to treatment are more similar to treatment units and should therefore receive a larger weight. Propensity score methods first estimate the propensity score from the data and then derive a weighting vector based on the propensity score. Different methods exist as to how the weights are linked to the propensity score; either

unemployment and any long-term effects would escape the analysis. On the other hand side, one could allow for more time to elapse between observing parental job loss and measuring child outcomes (e.g. consider unemployment shocks in the treatment group between the age of 6 to 9, study early labour market outcomes at the age of 30, and consider unemployment shocks in the control group after the age of 30). The downside to this option would be twofold: First, panel attrition over a long period of time would reduce the pool of subjects in the control group considerably. Secondly, it may not necessarily be random anymore whether a parent experiences job loss when the child is aged 8 or 32 because in the mean-time, time-varying covariates could have changed in a meaningful way that influenced both the likelihood of job loss and children's outcomes. Schmidpeter (2020) takes another route and studies children whose parents had an unemployment spell around the time of the track choice in the Austrian school system at the age of 10 and then examines the long-term consequences on children's labour market outcomes in their 30s. This implies that the unemployment shock both in the treatment and the control group occurs before the realisation of the outcome. Hence, the estimated impact would only reflect the effect channeled through the timing of the unemployment spell rather than the entire effect of unemployment.

they depend directly on propensity score values as in *propensity score weighting* (Robins et al., 1994) or they depend on propensity score distances to treatment observations as in *propensity score matching* (Rosenbaum and Rubin, 1985). The main shortcoming of propensity score methods is that the success of any method hinges on the quality of the estimated propensity score. In practice, researchers often manually iterate between tweaking the propensity score model and checking the covariate balance until they attain a satisfactory solution, which has been criticised as the “propensity score tautology” (Imai et al., 2008). Not only is this cyclical process tedious, it does not even guarantee eventually finding a satisfactory solution that jointly balances all covariates, especially in small samples. Previous research shows that misspecified propensity score models can lead to substantial bias in the subsequent estimation of treatment effects (see Diamond and Sekhon, 2013; Drake, 1993; Smith and Todd, 2001).

Against this background, Hainmueller (2012) developed entropy balancing as a novel method to overcome the drawbacks of existing propensity score methods. Entropy balancing does not take the detour via the propensity score and instead directly incorporates covariate balance into the weight function that is applied to the control units. After the researcher has imposed a set of balance constraints on the sample moments of the covariate distributions, the entropy balancing scheme computes the set of weights that satisfies the prespecified constraints but at the same times remains as close as possible in an entropy sense to the set of uniform base weights. As a result, the reweighted control and treatment group match exactly on the prespecified moments. Firstly, this procedure ensures covariate balance between treatment and control samples by construction and obviates the need for continual balance checking. Secondly, it is more effective because it always improves on the covariate balance reached by conventional preprocessing methods and also works in small samples. Thirdly, entropy balancing retains more information from the raw data in the preprocessed data since the weights are kept as close as possible to the base weights and are allowed to vary smoothly across units. In contrast, propensity score matching methods tend to suffer from a loss of information as control units are either matched (with a weight of one) or discarded (with a weight of zero).

5.2 Econometric specification

The estimation strategy can be divided into two distinct steps, the reweighting step and the regression step. The reweighting step constitutes the first step, in which the weights from the entropy balancing scheme are obtained. In the second step, children's educational and early labour market outcomes are regressed on the treatment indicator while incorporating the weighting vector obtained from the first step.

The reweighting step is implemented using the *ebalance* package in Stata (Hainmueller and Xu, 2013). First of all, I define a battery of conditioning variables which are correlated with both the treatment assignment and the outcome of interest. Importantly, all conditioning variables are measured at baseline, i.e. prior to the treatment. The conditioning variables are described extensively in Section 6.4. Then, I specify the set of balance constraints which equate the moments of the covariate distribution between the treatment and the reweighted control group.⁹ It is possible to impose constraints on the mean (the first moment), the variance (the second moment), and the skewness (the third moment), however for simplicity I only impose covariate balance with regard to the mean. Given the survey nature of the data, the SOEP survey weights are used as base weights both for the treatment and control units in order ensure representativeness of the German population. As outlined in Hainmueller (2012), the set of unit weights $W = [w_i, \dots, W_{n_o}]^\top$ is determined by the following entropy balancing scheme that minimises the entropy distance metric H between W and the vector of base weights $Q = [q_i, \dots, q_{n_o}]^\top$:

$$\min_{w_i} H(w) = \sum_{\{i|D=0\}} w_i \log(w_i/q_i) \quad (1)$$

subject to the balance constraints,

$$\sum_{\{i|D=0\}} w_i c_{ri}(X_i) = m_r \quad \text{with} \quad r = 1 \quad (2)$$

⁹The optimisation algorithm is adjusted so as to increase the maximum number of iterations from 20 to 30 and increase the tolerance criteria from 0.015 to 0.04. The tolerance number can be understood as the maximum deviation from the moment conditions across all conditioning variables; convergence is declared provided that the maximum deviation remains below the tolerated level. These adjustments can be considered quite minimal as compared for example to Marcus (2013).

the normalising constraint,

$$\sum_{\{i|D=0\}} w_i = 1 \quad (3)$$

and the non-negativity constraint

$$w_i \geq 0 \quad \text{for all } i \text{ such that } D = 0 \quad (4)$$

where $D_i \in \{1, 0\}$ refers to the binary treatment that each unit i is exposed to. It takes the value 1 if unit i received the active treatment and the value 0 if it received the control treatment. X_i is the vector of conditioning variables for unit i , m_r contains the r -th order moment of a specific conditioning variable for the treatment group (here: only the first moment) so that $c_{ri}(X_i) = m_r$ describes the set of balance constraints imposed on the covariate moments of the reweighted control group. The results from the reweighting step are presented and discussed in Section 6.5.

The regression step is performed afterwards. The weights obtained from the entropy balancing are compatible with almost any standard estimator for subsequent estimation of treatment effects. In fact, the estimates generated from the preprocessed data exhibit lower model dependency relative to estimates from unadjusted data. The reason for this is that entropy balancing orthogonalises the treatment indicator with respect to the conditioning variables, which implies that the treatment becomes mean-independent of all conditioning variables.

The educational and early labour market outcomes considered in this thesis can only be observed in the post-treatment period, which complicates the model choice. For instance, estimators relying precisely on the comparison between pre- and post-treatment observations such as difference-in-difference or event studies are not suitable in the present case. Hence, I follow the majority of the related literature (see Bratberg et al., 2008; Coelli, 2011; Huttunen et al., 2018; Lindemann and Gangl, 2019; Nikolova and Nikolaev, 2021; Oreopoulos et al., 2008; Powdthavee and Vignoles, 2008; Rege et al., 2011)¹⁰ and

¹⁰Apart from this, fixed or random effects models for continuous outcomes and logit or probit models for binary outcomes find much attention in the literature (see Bratberg et al., 2008; Haisken-DeNew and

model the relationship between parental unemployment and children’s educational and early labour market outcomes using a linear regression model:

$$Y_{it} = \alpha + \beta U_i^p + \gamma X_i + \varepsilon_{it} \quad (5)$$

Y_{it} represents the outcome in question of child i measured at time t , α is the constant and ε_{it} the error term. The binary treatment indicator U_i^p captures whether child i experienced parental unemployment, which makes β the main coefficient of interest. All conditioning variables from the reweighting step are included as control variables in the regression, reflected by the vector X_i . As the treatment indicator is mean-independent of all conditioning variables after entropy balancing is implemented, this does not alter the point estimate of the treatment effect as such (Marcus, 2013). Yet it is still recommended in order to improve precision; including control variables reduces unexplained variance in the outcome and thereby decreases the standard errors of the treatment effect (Hainmueller, 2012). The battery of control variables is theoretically composed of both time-invariant and time-varying covariates, however all of the time-varying covariates are potentially endogenous and therefore measured at baseline. Section 6.4 further elaborates on this issue. Standard errors are clustered at the level of the child. Equation 5 is estimated using pooled ordinary least squares (OLS) adjusted by the entropy balancing weights, which corresponds to the special case of a linear probability model (LPM) in cases where the outcome variable is binary.¹¹ Having said that, data preprocessing methods like entropy balancing allow the researcher to relax the assumption of a linear relationship between the outcome and covariates (see Peter, 2016; McMullin and Schonberger, 2022).

The obtained estimate $\hat{\beta}$ can be interpreted as the average treatment effect on the treated (ATT), i.e. the effect of the treatment for those observations that actually received the treatment. Stated formally, the ATT is:

Kind, 2012; Nikolova and Nikolaev, 2021; Powdthavee and Vignoles, 2008; Wightman, 2012). However, the estimations in these studies rely on unadjusted rather than preprocessed data. In Stata, these models are not compatible with preprocessed data, either because it is not possible to include weights at all, or because it is not possible to include weights in combination with the appropriate standard errors.

¹¹Other authors refrain from using the terms OLS and LPM in the context of regressions based on preprocessed data, but I would like to make it clear which model I use to analyse the preprocessed data.

$$\tau_{ATT} = E[Y(1)|EB(X), D = 1] - E[Y(0)|EB(X), D = 1] \quad (6)$$

As such, it represents the difference between the expected outcome for treated units that actually receive the treatment and the expected outcome for treated units if they had not received the treatment. $EB(X)$ refers to the entropy balancing weights, which simply equal the base weights (here: unmodified SOEP survey weights) in the case of the treatment observations.

5.3 Assumptions

According to Rosenbaum and Rubin (1983), causal inference in observational settings essentially hinges on two assumptions, namely the “strong ignorability assumption” and the “overlap assumption”. The strong ignorability assumption¹² ($Y(0), Y(1) \perp D|X$) states that the exposure to treatment is independent given a set of observable characteristics. Intuitively, this condition can only be fulfilled provided that the set of observable characteristics contains all confounding factors and can therefore fully eliminate selection bias. This means that there are no time-invariant or time-varying unobservable variables that simultaneously influence parental unemployment due to plant closure or dismissal and children’s educational and early labour market outcomes. Although this assumption cannot be directly tested, it can be plausibly assumed that it holds especially in the context of entropy balancing. To the extent that the unobserved covariates are correlated with the observed covariates, entropy balancing also matches the treatment and control group on the sample moments of the unobserved covariates. Thus, there is only reason for concern about unobserved covariates that are unrelated to observed covariates (Stuart, 2010).

The overlap assumption ($0 < P(D = 1|X) < 1$), also known as “common support” assumption, requires that all units have a non-zero probability of assignment to each treatment condition (D’Amour et al., 2021). In other words, there are treated and control

¹²Depending on the context, this condition can also be referred to as the “unconfoundedness” assumption, “selection on observables” assumption, “conditional independence” assumption, or no “omitted variable bias” assumption.

observations across the entire domain of each covariate so that covariates cannot perfectly predict treatment assignment. Across different specifications in my analysis, the share of observations that were assigned an entropy balancing weight of zero is only around one percent on average. In the sense that the entropy balancing weight can be used as a proxy for the propensity score, I therefore conclude that there is sufficient overlap.

On top of this, the so-called “Stable Unit Treatment Value Assumption” (SUTVA) needs to hold. Rubin(1980) defines SUTVA as follows: “The potential outcomes for any unit do not vary with the treatments assigned to other units, and, for each unit, there are no different forms or versions of each treatment level, which lead to different potential outcomes.” According to the first component of the condition, units must not interfere with one another so that treatment effects could spill over from the treatment group to the control group. In general, it is not difficult to argue that this condition holds in the given context since it would be inconceivable how parental unemployment would affect other children that the unemployed parent is neither related to or nor cohabiting with.¹³ The second component of the condition stipulates that for each unit, there is only a single version of each treatment level. This component can be considered fulfilled because aside from a trivial number of exceptions, each child is only exposed to one parental unemployment spell during the observation period.

6 Data

This paper uses data from the German SOEP (SOEP-Core, EU-Edition, version 37), a large-scale household panel survey that is designed to be representative of the entire population residing in Germany. Since 1984, it collects longitudinal information for example on respondents’ demographics, living conditions, family composition, or education and employment in annual waves (Wagner et al., 2007). It was expanded in 1990 on the

¹³Yet to be specific, there is technically a small room for bias in cases where the family history is more complicated. For example, if a parent moves out of an original SOEP household to a new household with existing children and becomes unemployed, both the children in the old and in the new household might potentially be affected in different ways, even if the children in the new household are part of the control and not the treatment group because the unemployed individual is not their parent.

occasion of the German reunification and a number of refresher samples were added at infrequent intervals over the years to counteract panel attrition and increase the overall sample size. The most recent version covering all waves from 1984 to 2020 comprises around 35,000 individuals living in almost 20,000 households (German Institute for Economic Research, 2022). Apart from the household questionnaire filled out by the household head, the SOEP surveys all members of each household over the age of 17 in an individual questionnaire. Information about children under the age 17 is provided by the mother via mother-child instruments and youth questionnaires starting from the age of 12 were later introduced in the 2010s (SOEP Companion, 2022).

There are several features that make the SOEP an attractive data source for the present research purpose: Firstly, the SOEP continues to track all individual household members even if they move out of the household for reasons such as nest-leaving. This allows me to match offspring and the evolution of their outcomes with their parents' employment conditions during childhood and adolescence. Secondly, the longitudinal nature of the data and the long time period make it possible to study long-term effects of unemployment in the first place. Thirdly, reporting errors are minimised since information on the parental employment history is not gathered from children retrospectively but instead comes from parents' contemporaneous answers. Lastly, the SOEP is a particularly rich survey and the level of detail provided both at the individual and at the household level enables me to use a wide range of variables. However, although the SOEP is characterised by a relatively large sample size compared to other household panel surveys, panel attrition and non-response pose challenges to the empirical analysis.¹⁴ Yet using German administrative data as an alternative is not feasible as it is prohibited to link parents and children in this type of data. Therefore, administrative data is unsuitable for intergenerational research questions in Germany so that the SOEP appears to be the best possible data source.

¹⁴Panel attrition does however not appear to bias the ATT estimates. Hypothetically, one could suppose that parental unemployment might affect the probability to exit the panel. Refuting this claim, the number of person-year observations does not differ significantly by treatment status.

6.1 Sample selection

The working sample is constructed as follows: An individual tracking file that contains all individuals who have ever lived in a household that has participated in the SOEP serves as a starting point. The initial dataset is then expanded by merging all research-relevant variables on the family composition, education, employment, and income via the individual and household identifiers. In the next step, parent identifiers are used to match children with their parents and link parental information to their children in each survey year.¹⁵ Only individuals that could be matched to at least one of their parents are kept in the sample.

The working sample is further restricted in line with the sample selection criteria from the related literature. Since I am interested in studying paternal and maternal unemployment both separately and pooled, I create several different samples where the sample selection criteria are applied to the father, mother, or both parents respectively. In order to reflect the working-age population, children whose parent's age is not in the range of 18 to 67 at baseline are dropped from the sample. Furthermore, the sample is limited to parents working as employees in the private sector so that children whose parent is self-employed or a civil servant at baseline are equally dropped. Other common selection criteria relate to the parent's tenure, firm size, as well as parental marriage status and cohabitation between child and parent. But given the challenge of maintaining a decent sample size, these criteria are not applied directly in the present analysis. The variable capturing tenure at the firm has too many missing values, which is why it is not considered any further. The marriage status of the parents and cohabitation of child and parent are still accounted for to the extent that corresponding measures are later included as conditioning and control variables.

The sample size is not the same in each specification. In general, the total sample comprises at least 750 children and can reach up to more than 3000 children. In the majority of cases, the treatment group makes up for around 10 to 20 percent of the total sample. Depending

¹⁵The parent identifiers *mnr* and *fnr* are designed to identify the social parents that live in the household when the child is surveyed for the first time. These do not necessarily correspond to the biological parents.

on the considered outcome variable, the sample size and accordingly the number of treated subjects can however be lower.¹⁶ The final working sample contains children born between 1979 and 2001.

6.2 Treatment and control group

This study makes use of twelve different treatment and control group pairs, which can be differentiated by the directly affected parent and the age of the indirectly affected child at the time of treatment.

In terms of the directly affected parent, there are three different treatment groups. Two of them consider maternal and paternal unemployment separately, meaning that either the mother or the father is affected by involuntary job loss. In the remaining group, maternal and paternal unemployment is pooled so that any parent can be affected by involuntary job loss.

To examine how the spillover effect of parental unemployment varies with the age of the child, the treatment groups are further differentiated four times by the age of the indirectly affected child at the time of treatment. The first age group considers children aged 6 to 9, the second age group children aged 10 to 14, and the third age group children aged 15 to 18. Another treatment group pools all age groups together, restricting the children's age to the range between 6 and 18. The definition of age groups is inspired both by the steps in the German schooling system¹⁷ and by the development psychology literature which stipulates that children's development occurs in stages (Piaget, 1971).

Based on this, the treatment group is composed of children whose parent enters unemployment through plant closure or lay-off at least once during the respective observation period of the age group.¹⁸ The SOEP specifically asks respondents who reported a job termination

¹⁶This is comparable to the sample size in other papers studying parental unemployment and children's outcomes using SOEP data. For example, Lindemann and Gangl (2019) have between 137 and 145 treated subjects, Marcus (2013) between 39 and 94, and Nikolova and Nikolaev (2021) between 66 and 122.

¹⁷Generally speaking, German children aged 6 to 9 are in primary school and enter secondary school at the age of 10. In secondary school, the upper stages begins at the of 16 and children usually complete secondary schooling at 18 years old.

¹⁸In principle, the parent can enter unemployment more than once during the observation period, although this is a rare event.

to choose a reason among thirteen categories, including for instance end of temporary contract, own resignation, dismissal by employer, plant closure or early retirement, which allows me to identify the type of involuntary job loss I am interested in. Before the entry into unemployment, the parent has to be in full- or part-time employment to ensure that there is a change in the initial employment status. Afterwards, the parent has to be officially registered as unemployed at the German Employment Office. This condition is included because not all individuals who lose their job necessarily find themselves in a state of unemployment with the associated consequences afterwards. For example, some might find another job shortly after the initial job loss, while others may voluntarily exit the labour force. Hence, it is important to distinguish between job loss and unemployment.¹⁹ There is no condition regarding the duration of the unemployment spell.²⁰

The control group comprises children for whom neither parent was registered unemployed during any of the years of the respective observation period of the age group. Since I impose conditions on both parents, the composition of the control group does not vary across the different treatment indicators related to the gender of the affected parent, but it does vary across those related to the different age groups.²¹ The fact that I use unemployment registration as an exclusion criterion has the implication that I cover all types of involuntary job loss so that children whose parent loses their job for a reason other than plant closure or lay-off are neither included in the treatment nor in the control group.²² By choosing to define the control group based on the same observation period as the corresponding treatment group, I technically allow children in the control group to experience parental unemployment at a point in time outside the observation period.

¹⁹In this way, this paper is different from other papers in the literature (e.g. Mörk et al., 2020) that study the effects of job loss rather than unemployment as such.

²⁰Consequently, the estimated treatment effects reflect the effect for the average duration of treatment. This implies that the focus in this analysis is on the extensive margin of unemployment rather than the intensive margin.

²¹Note that the size of the control group for the treatment indicators that use the same age group but differ with regard to which parent is displaced is still not exactly the same because I use different samples in each analysis and the sample selection criteria are only applied to the parent in question. For example, men are more inclined to be self-employed than women, so this restriction reduces the samples for paternal unemployment to a greater extent than those for maternal unemployment.

²²Apart from this, it entails that I do not strictly require parents in the control group to be continuously employed; they may also be continuously engaged in the household without formal employment since homemakers are not registered as unemployed.

Even though this point in time could precede the realisation of the considered outcome, this design should not introduce bias and is applied in other similar studies such as in Mörk et al. (2020). The reason for this is that entropy balancing allows me to assume that treated and control units have the same probability of exposure to treatment.²³

6.3 Outcome variables

The impact of parental unemployment on children is measured considering a wide range of different outcomes, which can be divided into educational and early labour market outcomes.

The set of educational outcomes comprises four different types of outcomes. A measure of school performance is provided by the grade average on the report card in German, maths, and the first foreign language. The data originates from the youth questionnaire for first-time respondents at age 17.²⁴ In the German grading system, the grading scale ranges from 1 to 6 but the constructed discrete variable reverses the scale so that 6 reflects the highest grade. The first educational transition taken into account concerns the track choice after the primary school cycle around the age of 10. As outlined in Section 4, children can either attend *Hauptschule*, *Realschule*, or *Gymnasium*. *Hauptschule* and *Realschule* together form the vocational track, whereas *Gymnasium* offers the academic track. Accordingly, a binary variable is created for each of the two tracks that takes the value 1 if the child attends this track and the value 0 if it does not. Due to the possibility of track reversibility, it is important to examine as well with which type of diploma a child completes the secondary school cycle with. For this purpose, two binary variables indicate whether a child graduates from *Hauptschule* or *Realschule*, or with a university entrance qualification (*Abitur* or *Fachabitur*) from *Gymnasium*. The age at graduation varies across secondary school types but typically lies between 16 and 19. Regarding

²³Still, I make sure that this does not create bias by requiring in a robustness check that the restrictions for the control group are met in all years while the children are aged 6 to 18. This is the control group that corresponds to the treatment indicators that pool all age groups together. I do not use this control group across all models already in the main analysis because this enables me to keep children in the control group that only enter the panel after the age of 6.

²⁴Information on grades is also provided in other SOEP questionnaires at younger ages, however these did not have a sufficient number of observations.

the educational transition after the secondary school cycle, I use three binary variables to study whether school-leavers earn a vocational training certificate, a tertiary degree, or neither.²⁵ Even though the SOEP data does provide information at a more granular level, for example with regard to whether an individual attends a university or university of applied sciences, or whether the highest obtained tertiary degree is a Bachelor's or a Master's degree, I do not make any differentiation of such kind because there are not enough observations available for each sub-group. In spite of the important implications of these different nuances of educational attainment from a social inequality perspective, this analysis can hence only capture the effects parental unemployment potentially has on broad categories of educational attainment.

All early labour market outcomes are measured at age 30. This choice is motivated by two factors: On the one hand side, there is a strong case for choosing a relatively high age at which individuals are already well-established in the labour market and their current income is a good indicator for their lifetime income. Since individuals with high educational attainment naturally spend more years in education and therefore enter the labour market at a later age than their peers with lower educational attainment, the earnings differential between individuals of the same age does not necessarily take its full effect at an early career stage, but increases with years of experience. On the other hand side, with each year of age the probability decreases that a SOEP respondent remains in the panel and can still be observed. Thus, the age should not be too high either so as to maintain a useful sample size. In light of this trade-off, measuring outcomes at age 30 appears to be a reasonable choice, although this means it is not possible to make inferences about long-term or lifetime outcomes.

The set of early labour market outcomes consists of three types of outcomes. Labour earnings are measured using a continuous variable capturing annual gross labour earnings. This variable is log-transformed. Own unemployment experience is measured using a continuous variable that reports the total length of unemployment in a respondent's career

²⁵In cases where an individual completed a vocational training as well as tertiary education, I do not only count the higher qualification but instead assign the value 1 for both the vocational training and the tertiary degree dummy.

up to the point of the interview. The length of time is given in years with months in decimal form. Lastly, socioeconomic status is reflected by a variable measuring the last reached score on the Standard International Socio-Economic Index of Occupational Status (ISEI Index). The ISEI Index was developed by Ganzeboom et al. (1992) and combines income and education to indicate the status of an occupation. The calculation is based on minimising the direct effect of education on income and maximising the indirect effect through occupation while controlling for age. The values of the discrete variable can take values in the range of 16 (corresponding to the score of cleaning personnel) and 90 (corresponding to the score of judges).²⁶

In total, the considered outcomes are observed between ages 10 and 30. As the impact of parental unemployment on children's outcomes can only be assessed provided that the unemployment spell occurred before the realisation of the outcome in question, the age group 6 to 9 is the only one that can be used to look at all educational and early labour market outcomes. Following this logic, for the age group 10 to 14, the variables related to grade average as well as secondary school attendance are not eligible, and for the age group 15 to 18 as well as the pooled age group, the variables related to secondary school graduation are additionally excluded.²⁷

6.4 Conditioning and control variables

Although I use the term “conditioning variables” in the context of the reweighting step and the term “control variables” in the context of the regression step, the two terms essentially refer to the same set of variables. The choice of conditioning and control variables is mainly motivated by economic theory; variables that can reasonably be expected to influence parental unemployment incidence as well as children's educational and early labour market

²⁶The selected variable in the SOEP derives the ISEI values from the International Standard Classification of Occupations (ISCO) in its third iteration known as ISCO-88. Although the more recent ISCO-08 has already been in use since 2008, this variable was preferred because it offered more observations.

²⁷Since I do not make any restrictions with respect to the age at which each educational outcome has to be measured, my reasoning is based on the standard age range corresponding to each step in the educational system. This might lead to slight inaccuracies in cases where a child completes the education cycle at an exceptionally fast pace, yet this is not a reason for concern given the extremely low frequency of such cases.

outcomes are taken into consideration.²⁸

The final selection of variables is based on a screening of variables used in similar empirical studies on parental unemployment and child outcomes (see Lindemann and Gangl, 2019; Marcus, 2013; Mörk et al., 2020; Nikolova and Nikolaev, 2021; Peter, 2016; Siedler, 2011). Thanks to the comprehensiveness of the SOEP dataset, the majority of variables used by other researchers is available. Still, not all of them are part of the final pool in order to avoid strong collinearity. The final set of variables incorporates demographic information about the child, information about the family and household, as well as information about the parent with regard to the labour market or educational background. In particular, I follow Marcus (2013) and Nikolova and Nikolaev (2021) and control for the parent’s perceived job security, which accounts for any anticipation of plant closure. A complete overview of all the conditioning and control variables together with their definitions is presented in Table 1.

A major issue in this context is endogeneity. Virtually all of the time-varying control variables could potentially be outcomes of the treatment. For instance, household disposable income could fall in response to a parental unemployment spell or parents might even get divorced. These variables would hence constitute “bad controls” and could lead to biased estimates (Angrist and Pischke, 2009). To overcome this issue, they are measured at baseline. Since no universal before and after treatment indicator exists for both the treatment and the control group, the baseline is simply constructed by taking the first year of each respective age group. This means that baseline measures refer to the age of 6 in the age group 6 to 9 and in the pooled group, to the age of 10 in the age group 10 to 14, and to the age of 15 in the age group 15 to 18.²⁹ However, this is not a viable solution for contemporaneous determinants of children’s educational and early labour market outcomes that can only be observed after treatment. For this reason, I cannot control for any variables reflecting the level of accumulated human capital in the child

²⁸Note that strictly speaking, some variables are not confounding variables per se. For example, the child’s gender is included as a predictor of the outcome rather than the treatment exposure. But since it could moderate the effect of parental unemployment, it makes sense to also include it as a conditioning variable in the reweighting step.

²⁹In cases where the treatment already occurs during the first year of the age group, the baseline is assigned to the year prior.

Table 1: Overview of the conditioning and control variables

Variable	Definition
<i>Demographic information</i>	
Female	1 = female, 0 = male
Migrant	1 = individual or parents immigrated to Germany, 0 = otherwise
Rural area	1 = lives in rural region, 0 = lives in urban region
East Germany	1 = lives in East Germany (including Berlin), 0 = lives in West Germany
<i>Family and household information</i>	
Cohabitation with parent	1 = has lived with parent for at least 10 years, 0 = otherwise
Parents married	1 = parents are both married and have matching partner identifiers (i.e. parents are married to each other), 0 = otherwise
Single parent	1 = parent has no partner identifier and is either unmarried, married but living separately, divorced, or widowed, 0 = otherwise
Number of kids in household	Number of persons in the household under the age 18
Home ownership	1 = household is owner of dwelling, 0 = household is tenant of dwelling
Dual earner	1 = other parent is employed as well, 0 = otherwise
Household disposable income	Annual household income after taxes and government transfers
Household disposable income squared	
<i>Parent information</i>	
Age	Age in years
Age squared	
Never unemployed	1 = never unemployed, 0 = ever unemployed
Employed without job change	1 = employed without job change, 0 = otherwise
Full-time work experience	Total length in years with months in decimal form of work experience in full-time employment
Full-time work experience squared	
Perceived job security	3 categories (big worries, some worries, no worries)
Socioeconomic status	Last reached score on the ISEI Index
Socioeconomic status squared	
Abitur	1 = <i>Abitur</i> or <i>Fachabitur</i> , 0 = no <i>Abitur</i> or <i>Fachabitur</i>
Vocational training	1 = vocational training, 0 = no vocational training
Tertiary education	1 = tertiary education, 0 = no tertiary education
No vocational training or tertiary education	1 = no vocational training or tertiary education, 0 = vocational training or tertiary education

itself, such as tertiary education in the regressions for early labour market outcomes.

The set of conditioning and control variables varies across the different treatment groups. For the paternal and maternal treatment groups, parental information variables are only included for the respective parent, whereas they are included for both parents in the pooled treatment group. The time-varying variables are measured at different baselines depending on which age group is considered. However, the set of conditioning and control variables is the same regardless of the outcome variable in question.

6.5 Descriptive statistics

In the following, balance tests for covariates at baseline before and after entropy balancing are discussed. Entropy balancing was conducted separately for each treatment and control group pair, but the results are illustrated using the paternal unemployment group at age 6 to 9 as an example. Balance tables after entropy balancing for some of the remaining groups are presented in Appendix A.2 to showcase that entropy balancing was effective. The results do not differ in a meaningful way across specifications.

Table 2: Balance test for covariates at baseline before entropy balancing
(Paternal unemployment, age group 6-9)

Variable	Mean treated	Mean control	Mean difference	P-value
<i>Demographic information</i>				
Female	.5207	.5143	.006431	.9071
Migrant	.2127	.1693	.04337	.2978
Rural area	.4797	.2647	.215***	.0000132
East Germany	.4169	.09984	.3171***	5.24e-19
<i>Family and household information</i>				
Cohabitation with parent	.8727	.9553	-.08256***	.0006214
Parents married	.8432	.9691	-.1259***	4.28e-09
Single parent	.0404	.0021	.0383***	5.06e-07
Number of kids in household	2.442	2.23	.2115**	.0445
Home ownership	.3135	.588	-.2744***	4.43e-07
Dual earner	.5112	.5096	.001588	.977
Household disposable income	25020	32495	-7475***	5.88e-08
Household disposable income squared	7.15e+08	1.22e+09	-5.01e+08***	.0002166
<i>Parent information</i>				
Age	35.93	37.31	-1.384**	.01563
Age squared	1323	1419	-95.83**	.0315
Never unemployed	.3583	.6488	-.2905***	4.18e-08
Employed without job change	.6718	.9002	-.2284***	7.14e-11
Full-time work experience	13.47	15.36	-1.889***	.003869
Full-time work experience squared	217.8	270.9	-53.14**	.01993
Perceived job security	9.304	4.135	5.17***	.001129
Socioeconomic status	32.74	44.16	-11.42***	9.51e-12
Socioeconomic status squared	1157	2188	-1031***	3.07e-10
Abitur	.04017	.2485	-.2084***	7.33e-06
Vocational training	.8238	.8068	.017	.6955
Tertiary education	.008231	.1909	-.1826***	.0000137
No vocational training or tertiary education	.1679	.0711	.09682***	.001039
Observations	107	1,129	1,236	

Note: This table presents means and mean differences for the set of conditioning variables before entropy balancing. All variables are measured at baseline. Observations are weighted by SOEP survey weights. The last column displays p-values for the t-test.

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

Table 2 compares covariates at baseline between children from the treatment and the control group before entropy balancing. Column 1 and 2 report the means for the treatment and control group, respectively. Column 3 shows the means difference and column 4

the p-values for the equality of means test. It is clear that there is a strong covariate imbalance in the unadjusted data. Treated children differ significantly from their control counterparts in many respects: On average, they are around 30 percentage points more likely to live in East Germany and 12 percentage points less likely to have married parents. Their household has a higher number of children and almost 7500€ less disposable income per year. Their father is six times less likely to have graduated with *Abitur* from the secondary school cycle. Gender, migration background, and surprisingly, household dual earner status, and whether the father has completed vocational training, are the only covariates for which the mean does not differ significantly across treatment and control group. These results underline that children who experience parental unemployment are negatively selected.

As demonstrated in Table 3, the means for covariates at baseline in the control group match those in the treatment group almost exactly after entropy balancing. In the preprocessed data, no significant mean differences remain. Against this background, I conclude that entropy balancing was successful and achieved sufficient covariate balance to proceed with the estimation strategy.

Table 3: Balance test for covariates at baseline after entropy balancing
(Paternal unemployment, age group 6-9)

Variable	Mean treated	Mean control	Mean difference	P-value
<i>Demographic information</i>				
Female	.5207	.5206	.0000908	.9975
Migrant	.2127	.2126	.0000313	.9989
Rural area	.4797	.4797	.0000296	.9992
East Germany	.4169	.4171	-.0001461	.9958
<i>Family and household information</i>				
Cohabitation with parent	.8727	.8726	.0001377	.9942
Parents married	.8432	.843	.0001617	.9938
Single parent	.0404	.04039	.000012	.9991
Number of kids in household	2.442	2.442	.0002262	.9974
Home ownership	.3135	.3135	.0000635	.9981
Dual earner	.5112	.5113	-.0001211	.9966
Household disposable income	25020	25024	-3.8	.9943
Household disposable income squared	7.15e+08	7.15e+08	-247649	.9956
<i>Parent information</i>				
Age	35.93	35.93	.0000159	1
Age squared	1323	1323	.005686	.9998
Never unemployed	.3583	.3583	4.44e-07	1
Employed without job change	.6718	.6717	.0000447	.9987
Full-time work experience	13.47	13.47	.002243	.9948
Full-time work experience squared	217.8	217.8	.04629	.9965
Perceived job security	9.304	9.302	.002092	.9989
Socioeconomic status	32.74	32.75	-.004513	.9931
Socioeconomic status squared	1157	1158	-.3702	.993
Abitur	.04017	.0404	-.0002301	.9836
Vocational training	.8238	.8239	-.0000488	.9982
Tertiary education	.008231	.008525	-.0002938	.9549
No vocational training or tertiary education	.1679	.1679	.0000499	.9981
Observations	107	1,129	1,236	

Note: This table presents means and mean differences for the set of conditioning variables after entropy balancing. All variables are measured at baseline. The last column displays p-values for the t-test.

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

7 Results

7.1 Main results

In total, I run a large number of regressions on the various treatment indicators and for all the different children’s educational and early labour market outcomes. However, here I focus on the models using paternal or maternal unemployment at age 6 to 9 as the treatment indicator since these models produce the most interesting results and are the only ones that cover all child outcomes. Regression output tables for the remaining models are provided in Appendix A.3.

Table 4 reports the results obtained for all educational and early labour market outcomes, where paternal unemployment at age 6 to 9 is the treatment indicator. The rows of the table contain the results for separate regressions run for each outcome variable. The specification in the first column corresponds to the linear regression model as described in Section 5.2 based on the unadjusted data without entropy balancing weights. After the implementation of entropy balancing, the same models are estimated again with the preprocessed data that is now weighted by the entropy balancing weights. This is the preferred specification; results from this specification are presented in the second column. For the most part, entropy balancing appears to reduce the bias contained in the models based on the unadjusted data. The point estimates of the treatment effect are corrected downward and so is their significance. Only in a few models does the treatment effect first become significant with entropy balancing. Overall, the results are largely consistent across the two model specifications.

Considering the results for each outcome, paternal unemployment experienced at the age of 6 to 9 increases a child’s likelihood of attending the vocational track of secondary school by 9 percentage points and decreases the likelihood of attending the academic track by 8 percentage points. The grade point average on the report card received at age 17 of affected children is 0.16 points lower than unaffected children. The results concerning the type of secondary school track are amplified when it comes to the type of school the children graduate from. Treated children are 16 percentage points more likely to graduate

Table 4: Main results - all educational and early labour market outcomes
(Paternal unemployment, age group 6-9)

	Before ebalance		After ebalance	
<i>Attend Haupt- or Realschule</i>				
Treatment indicator	0.0919	(0.0651)	0.0924*	(0.0559)
R ²	0.155		0.189	
Number of observations	6356		6240	
Number of individuals	1230		1203	
<i>Attend Gymnasium</i>				
Treatment indicator	-0.107**	(0.0504)	-0.0766*	(0.0459)
R ²	0.219		0.237	
Number of observations	6356		6240	
Number of individuals	1230		1203	
<i>Grade point average at age 17</i>				
Treatment indicator	-0.108	(0.103)	-0.161*	(0.0851)
R ²	0.0917		0.272	
Number of observations	923		904	
Number of individuals	923		904	
<i>Graduate from Haupt- or Realschule</i>				
Treatment indicator	0.257***	(0.0532)	0.159**	(0.0641)
R ²	0.241		0.230	
Number of observations	4175		4065	
Number of individuals	782		765	
<i>Graduate with Abitur</i>				
Treatment indicator	-0.252***	(0.0523)	-0.164***	(0.0628)
R ²	0.246		0.235	
Number of observations	4175		4065	
Number of individuals	782		765	
<i>Complete vocational training</i>				
Treatment indicator	0.0850	(0.0762)	0.0460	(0.0802)
R ²	0.333		0.300	
Number of observations	3775		3680	
Number of individuals	968		943	
<i>Complete tertiary education</i>				
Treatment indicator	-0.0752***	(0.0249)	-0.0826***	(0.0224)
R ²	0.224		0.195	
Number of observations	3775		3680	
Number of individuals	968		943	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	-0.0439	(0.0533)	-0.00689	(0.0592)
R ²	0.196		0.141	
Number of observations	6075		5933	
Number of individuals	1134		1109	
<i>Log labour earnings at age 30</i>				
Treatment indicator	-0.856*	(0.470)	-0.815***	(0.299)
R ²	0.175		0.569	
Number of observations	3271		3291	
Number of individuals	114		112	
<i>Own unemployment experience at age 30</i>				
Treatment indicator	3.664***	(0.777)	3.699***	(0.371)
R ²	0.428		0.838	
Number of observations	3355		3348	
Number of individuals	117		115	
<i>Socioeconomic status at age 30</i>				
Treatment indicator	-4.578	(4.862)	-4.895	(5.903)
R ²	0.339		0.825	
Number of observations	1775		1784	
Number of individuals	60		60	

Note: This table presents regression output for all educational and early labour market outcomes before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

from the vocational track of secondary school without a university entrance qualification. Conversely, they are 16 percentage points less likely to graduate from the academic track with *Abitur*. This effect is statistically significant at the 1 percent level. In contrast to these findings, paternal unemployment at the age of 6 to 9 does not seem to have a causal impact on the completion of vocational training after the secondary school cycle. Neither of the two specifications produce a significant coefficient for the treatment effect, although the direction of the estimate points toward a positive effect. In relation to the completion of tertiary education however, the impact is statistically significant at the 1 percent level. Children of fathers who become unemployed when they are 6 to 9 years old are 8 percentage points less likely to earn a university degree. Whether children complete no vocational training or no tertiary education at all remains unaffected by treatment. The models investigating the effect on early labour market outcomes do not allow reliable conclusions to be drawn in light of the small sample size. Still, the findings suggest that paternal unemployment experienced during childhood lowers labour earnings and raises unemployment experience at age 30.

In comparison with the results from maternal unemployment at the same age, it becomes evident that paternal unemployment affects children much more strongly than maternal unemployment. Table 5 displays the regression output for all models run using maternal unemployment at age 6 to 9 as the treatment indicator. Almost no coefficient is significant, suggesting that maternal unemployment at that age does not have an impact on a child's educational outcomes. The estimated effects are generally smaller in magnitude, yet their directions are the same as for paternal unemployment. An exception is the impact on the grade point average, where the estimated effect is positive, although insignificant.

Table 5: Main results - all educational and early labour market outcomes
(Maternal unemployment, age group 6-9)

	Before ebalance		After ebalance	
<i>Attend Haupt- or Realschule</i>				
Treatment indicator	0.124	(0.0765)	0.0751	(0.0686)
R ²	0.136		0.188	
Number of observations	5505		5375	
Number of individuals	1049		1021	
<i>Attend Gymnasium</i>				
Treatment indicator	-0.113	(0.0699)	-0.0929	(0.0594)
R ²	0.199		0.205	
Number of observations	5505		5375	
Number of individuals	1049		1021	
<i>Grade point average at age 17</i>				
Treatment indicator	0.109	(0.113)	0.0424	(0.100)
R ²	0.153		0.436	
Number of observations	825		803	
Number of individuals	825		803	
<i>Graduate from Haupt- or Realschule</i>				
Treatment indicator	0.0459	(0.107)	0.0927	(0.0781)
R ²	0.225		0.337	
Number of observations	4040		3909	
Number of individuals	729		711	
<i>Graduate with Abitur</i>				
Treatment indicator	-0.0552	(0.104)	-0.140*	(0.0786)
R ²	0.237		0.307	
Number of observations	4040		3909	
Number of individuals	729		711	
<i>Complete vocational training</i>				
Treatment indicator	0.212**	(0.0872)	0.0434	(0.0811)
R ²	0.298		0.308	
Number of observations	3643		3524	
Number of individuals	862		839	
<i>Complete tertiary education</i>				
Treatment indicator	0.00141	(0.0557)	-0.0213	(0.0401)
R ²	0.203		0.248	
Number of observations	3643		3524	
Number of individuals	862		839	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	-0.113**	(0.0488)	-0.0537	(0.0522)
R ²	0.213		0.164	
Number of observations	5811		5612	
Number of individuals	999		973	
<i>Log labour earnings at age 30</i>				
Treatment indicator	-0.381	(0.271)	-0.307***	(0.0875)
R ²	0.366		0.653	
Number of observations	3220		3168	
Number of individuals	108		104	
<i>Own unemployment experience at age 30</i>				
Treatment indicator	1.167	(0.754)	1.682***	(0.324)
R ²	0.357		0.817	
Number of observations	3392		3325	
Number of individuals	113		109	
<i>Socioeconomic status at age 30</i>				
Treatment indicator	14.43	(20.16)	3.279	(7.618)
R ²	0.480		0.912	
Number of observations	1668		1678	
Number of individuals	53		53	

Note: This table presents regression output for all educational and early labour market outcomes before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

7.2 Further results

Next, I would like to highlight the effect of parental unemployment on the secondary school transition of children because it appears that parental unemployment consistently has the strongest impact on this type of outcome. As explained in Section 4, whether children follow the vocational or academic track of secondary school has crucial implications for their later life outcomes. For this purpose, Table 6 reports corresponding results across different models, i.e. for paternal unemployment, maternal unemployment, and pooled paternal and maternal unemployment. For each of the three groups, models are run both in the age group 6 to 9 and in the age group 10 to 14. Results for whether children graduate from the vocational track are contained in the first two columns, whereas the last two columns contain results for whether children graduate from the academic track with a university entrance qualification. The even columns show results for the preferred specification, which is based on the preprocessed data after entropy balancing.

Across all models, children who experience parental unemployment are more likely to graduate from the vocational track and less likely to graduate from the academic track of secondary school. The size of the effect lies in the range of 15 to 25 percentage points. The only exception is the group that experiences maternal unemployment at age 6 to 9, where the effect remains insignificant even in the preferred specification. Although paternal unemployment has a stronger impact on the type of secondary school track than maternal unemployment, the adverse effect is already discernible in the group that pools paternal and maternal unemployment together. While the magnitude of the effect does not differ considerably across age groups for paternal unemployment, the adverse effect only fully materialises in the age group 10 to 14 for maternal unemployment.

Lastly, a few observations from the analysis of the remaining models in Appendix A.3 deserve mentioning. There are no consistent results regarding completion of vocational training as the estimated effects are largely insignificant and the coefficients take different signs. Nevertheless, there is some evidence from regressions that pool maternal and paternal unemployment together that parental unemployment increases the likelihood that a child completes vocational training. In contrast, the findings regarding completion

Table 6: Main results - graduation from secondary school
(All unemployment, age groups 6-9 & 10-14)

	Graduate from Haupt- or Realschule				Graduate with Abitur			
	Before ebalance		After ebalance		Before ebalance		After ebalance	
<i>Pooled unemployment, age group 6-9</i>								
Treatment indicator	0.217***	(0.0756)	0.159**	(0.0768)	-0.218***	(0.0749)	-0.174**	(0.0732)
R ²	0.301		0.366		0.307		0.342	
Number of observations	3080		2939		3080		2939	
Number of individuals	577		556		577		556	
<i>Pooled unemployment, age group 10-14</i>								
Treatment indicator	0.206***	(0.0643)	0.222***	(0.0546)	-0.193***	(0.0619)	-0.182***	(0.0530)
R ²	0.284		0.331		0.303		0.346	
Number of observations	4586		4482		4586		4482	
Number of individuals	865		847		865		847	
<i>Maternal unemployment, age group 6-9</i>								
Treatment indicator	0.0459	(0.107)	0.0927	(0.0781)	-0.0552	(0.104)	-0.140*	(0.0786)
R ²	0.225		0.337		0.237		0.307	
Number of observations	4040		3909		4040		3909	
Number of individuals	729		711		729		711	
<i>Maternal unemployment, age group 10-14</i>								
Treatment indicator	0.156*	(0.0935)	0.256***	(0.0708)	-0.138	(0.0945)	-0.231***	(0.0719)
R ²	0.175		0.255		0.180		0.244	
Number of observations	5904		5817		5904		5817	
Number of individuals	1112		1100		1112		1100	
<i>Paternal unemployment, age group 6-9</i>								
Treatment indicator	0.257***	(0.0532)	0.159**	(0.0641)	-0.252***	(0.0523)	-0.164***	(0.0628)
R ²	0.241		0.230		0.246		0.235	
Number of observations	4175		4065		4175		4065	
Number of individuals	782		765		782		765	
<i>Paternal unemployment, age group 10-14</i>								
Treatment indicator	0.103	(0.0766)	0.156**	(0.0617)	-0.125*	(0.0750)	-0.176***	(0.0568)
R ²	0.184		0.217		0.195		0.256	
Number of observations	6133		6062		6133		6062	
Number of individuals	1210		1198		1210		1198	

Note: This table presents regression output for graduation from secondary school outcomes before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

of tertiary education suggest an adverse effect of parental unemployment. This is mainly driven by paternal unemployment rather than maternal unemployment. The effect is the strongest when paternal unemployment occurs at the age of 6 to 9, not significant at the age of 10 to 14, and slightly smaller at the age of 15 to 18. While the majority of estimates from the models based on maternal unemployment remain insignificant, in a limited number of cases they even point towards a favourable effect of maternal unemployment on children's outcomes. For example, affected children appear to be less likely to complete neither vocational training nor tertiary education, especially when their mother becomes unemployed while they are younger. Similarly, the impact on the grade point average is positive throughout all models based on maternal employment, although it is not always significant. The interpretation of findings concerning early labour market outcomes is

impaired in all models owing to a small sample size. However, it appears consistent that paternal unemployment lowers labour earnings at age 30 and raises own unemployment experience at age 30.

8 Robustness Tests

In this section, I present the results from three robustness checks that validate the design of the identification strategy and test the sensitivity of the results from the main analysis to alternative specifications.

The first robustness test I carry out aims to check the plausibility of my empirical strategy to identify causal effects. For this purpose, I conduct placebo-in-time tests. These consist of estimating the effect of future treatment indicators on children's outcomes that are already determined before the treatment even takes place. The intuition is that the treatment can only induce a causal effect on the outcome provided that the treatment precedes the outcome in time. Therefore, my findings can only be interpreted causally if the placebo tests do not detect any significant effects from future treatment. If however the opposite is the case and future treatment does impact children's outcomes, this implies that in spite of entropy balancing there remains heterogeneity between the treatment and control group in unobserved characteristics that simultaneously influence parental unemployment incidence and children's outcomes. In this case, my identifying assumption would be violated and my findings would merely reflect correlations.

To implement the test, I use three different placebo treatment indicators reflecting different age groups of children. For the outcome variables determined around the age of 10, i.e. the type of secondary school attended, the placebo treatment indicator considers children who experienced parental unemployment between the age of 15 and 18. Treatment during the age 20 to 25 serves as a placebo for outcomes realised between 16 and 19, i.e. grade point average at 17 and type of secondary school graduated from; treatment during the ages 25 to 30 serves as a placebo for outcomes determined after the secondary school cycle, i.e. whether children complete vocational training, tertiary education, or neither. The placebo tests are not conducted for the early labour market outcomes. Except for the

Table 7: Robustness test results - placebo-in-time
(Paternal unemployment)

	Before ebalance		After ebalance	
<i>Attend Haupt- or Realschule</i>				
Placebo treatment (age 15-18)	0.00542	(0.0419)	-0.000820	(0.0394)
R ²	0.148		0.144	
Number of observations	14339		14339	
Number of individuals	3137		3135	
<i>Attend Gymnasium</i>				
Placebo treatment (age 15-18)	-0.0325	(0.0407)	-0.0272	(0.0368)
R ²	0.175		0.169	
Number of observations	14339		14339	
Number of individuals	3137		3135	
<i>Grade point average at age 17</i>				
Placebo treatment (age 20-25)	-0.0245	(0.143)	0.0103	(0.122)
R ²	0.0833		0.174	
Number of observations	1631		1509	
Number of individuals	1631		1509	
<i>Graduate from Haupt- or Realschule</i>				
Placebo treatment (age 20-25)	0.0854	(0.0728)	0.0438	(0.0547)
R ²	0.184		0.286	
Number of observations	12250		11914	
Number of individuals	1936		1825	
<i>Graduate with Abitur</i>				
Placebo treatment (age 20-25)	-0.0791	(0.0730)	-0.0249	(0.0573)
R ²	0.179		0.258	
Number of observations	12250		11914	
Number of individuals	1936		1825	
<i>Complete vocational training</i>				
Placebo treatment (age 25-30)	0.0738	(0.0832)	-0.0354	(0.0679)
R ²	0.165		0.341	
Number of observations	7542		7295	
Number of individuals	1005		946	
<i>Complete tertiary education</i>				
Placebo treatment (age 25-30)	-0.0525	(0.0603)	-0.0127	(0.0462)
R ²	0.277		0.448	
Number of observations	7542		7295	
Number of individuals	1005		946	
<i>Complete no vocational training or tertiary education</i>				
Placebo treatment (age 25-30)	-0.0238	(0.0272)	0.00445	(0.0390)
R ²	0.170		0.258	
Number of observations	10612		10201	
Number of individuals	1091		1017	

Note: This table presents regression output for the placebo-in-time robustness test before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but are not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

different observation period, the construction of the treatment indicator is the same as in the main specification. Table 7 reports the results from the placebo tests for paternal unemployment. All estimated effects are insignificant, both for the specification using unadjusted and preprocessed data. Several coefficients are however not quite close to zero,

but they are still considerably lower than in the main results. Thus, the placebo-in-time test supports the causal interpretation of my findings.

A second robustness test is implemented to examine whether the definition of entry into unemployment through plant closure or dismissal by employer can be considered exogenous. For this purpose, I restrict the possible job ends to plant closures only, which are deemed more exogenous even in the German context mentioned in Section 6.2. Table A14 presents the results for this robustness test using the treatment indicator for paternal unemployment between the age of 6 and 9, so that it can be considered a direct comparison for Table 4. The results from the robustness test and the main analysis are fairly consistent. In particular, the point estimate for the reduction of the likelihood to complete tertiary education in the preferred specification is around minus 8 percentage points at the 1 percent significance level in both cases. Similarly, the estimated treatment effects regarding graduation from the vocational or academic track of secondary school remain statistically significant at the 1 percent level and the point estimates even increase. However, the robustness test does not find significant estimates for the type of secondary school attended and the grade point average. This is not too alarming as such since the estimates were only significant at the 10 percent level in the main analysis. Neither in the main analysis nor in the robustness was there a significant treatment effect on the likelihood to complete vocational training or no vocational training or tertiary education at all. Overall, these findings add credibility to my empirical strategy.

In the third robustness test, the construction of the control group follows a stricter definition than in the main analysis. In the main analysis studying for example parental unemployment at the age of 6 to 9, the definition of the control group only requires that neither parent was registered as unemployed while the child was between 6 and 9 years old. This means that parental unemployment could occur in the control group after this observation period and still influence outcomes, but the likelihood of this event is not higher than in the treatment group since independence of treatment conditional on observable characteristics is assumed. As a robustness check, I require that neither parent was registered unemployed while the child was between 6 and 18 years old. Table A15

reports the results for this robustness check, again as a direct comparison to Table 4. The estimates are largely consistent. In all models where the estimates from the robustness test deviate from the ones in the main specification, the point estimate is larger and significant at an even lower level. If anything, this suggests that the findings from the main analysis are slightly underestimated.

9 Discussion

9.1 Interpretation

Taken together, the findings of this study amount to evidence of the existence of spillover effects from parental unemployment on children with respect to their educational attainment. Although transmissions channels derived from theory about skill formation and educational decision-making suggest different directions of the effect, it is clear that a negative effect mostly prevails. As predicted by the theory, the negative effect is driven by paternal unemployment. Paternal unemployment appears to have the strongest adverse effects when children are young at the time of the onset of unemployment. This underscores that spillover effects from parental unemployment are persistent and continue to put affected children at a disadvantage later on in life. At the same time, this supports the view of the human capital development framework developed by Cunha and Heckman (2007), according to which adverse events experienced during early childhood can result in a cumulative disadvantage over time. In this context, I observe that parental unemployment taking place when the child is aged 6 to 9 or 15 to 19 results in a reduction of the likelihood to complete tertiary education by 8 and 6 percentage points respectively, whereas the estimated effect for the age group 10-14 is close to zero and statistically insignificant. Changes in the relative importance of each theoretical transmission channel over time could deliver a potential explanation for this. The primary schooling age can be considered a decisive time period for skill formation, which is why any restriction of the family's ability to provide needed resources can hamper the child's educational trajectory. At age 10 to 14, children develop more abstract thinking, allowing them to develop coping

mechanisms against parental unemployment such that no negative impact is detected for this age group. Unemployment occurring shortly before the end of the secondary education cycle might have a stronger impact again as a loss in earnings and potentially expected earnings change the parameters that play a role in this decision-making process. In stark contrast to paternal unemployment, maternal unemployment does not appear to have consequential spillovers on children, especially when children are young at the time of unemployment. However, negative effects such as on the likelihood to graduate with a university entrance qualification emerge if children are already between 10 and 14 years old at the time of the unemployment spell. This finding is consistent with the perspective that unemployed mothers reallocate more time toward childcare relative to unemployed fathers since mother-child interactions might be more valuable for the child's development at a younger age and therefore only prevent adverse spillovers during this time. Although the existence of positive spillover effects from maternal unemployment is limited, the probability to complete neither a vocational training nor a tertiary education is reduced.

9.2 Limitations

There are a number of limitations of this thesis that need to be acknowledged. Even though my empirical strategy relies on plausibly exogenous entries into unemployment and applies entropy balancing to allay concerns about selection bias, I cannot be certain that the treatment and control group do not still differ in terms of unobserved confounding factors. In particular, I cannot incorporate measures of skill or personality traits in my analysis on the grounds of endogeneity as outlined in Section 6.4. The robustness tests reassure the validity of my empirical strategy, yet I cannot completely exclude the possibility that selection on unobservables is biasing the results. Following Nikolova and Nikolaev (2021) however, I argue that any remaining bias is likely to be small since estimates from the robustness test using only plant closures as the most exogenous possible job end did not differ much from the main estimates.

The small sample size and above all the small number of treated subjects constitute another

shortcoming of the empirical analysis. For the most part, the treatment group comprises at least 50 individuals which can still be considered adequate especially compared to the samples of similar papers. Yet especially for the early labour market outcomes the number of treated subjects was far below that, which does not produce credible results. The research process was marked by trade-offs between the maintaining a sufficient sample size and choosing the optimal research design. In particular, the small sample size meant it was not possible to investigate how adverse impacts of unemployment are transmitted to children differently across socioeconomic subgroups. This would have been interesting to explore to gain a deeper understanding of how unemployment affects children's outcomes. Furthermore, there are several issues that are not adequately addressed. Firstly, I do not account for the birth cohort of the children or the time period in which unemployment occurred. In total, my analysis covers a time period of four decades since I observe the birth cohorts between 1979 and 2001 and follow them up until early adulthood. During this time period, the institutional environment in Germany has undergone changes so that my findings may not necessarily be applicable to children that grow up in the current setting. For example, maternal unemployment spells happening today might have more similar effects to paternal unemployment spells in light of the increasing importance of female labour market participation and changing gender roles. The German unemployment benefit system has also changed during the time period as a substantive reform came into force in 2005. Years marked by economic downturn such as the global financial crisis between 2007 and 2009 were not given any special consideration either, although they may have influenced decisions about educational choices. Secondly, I do not account for any potential differences between the German states in spite of the diversified education systems.

10 Conclusion

On 1 January 2023, a reformed unemployment benefit system enters into force in Germany. The approval of the bill in parliament was preceded by months if not years of political debate. Yet what has been largely overlooked in the debate are the needs of children with

unemployed parents. In this context, I provide causal evidence on the intergenerational impact of unemployment. For this purpose, I harness data from the SOEP, one of the largest and longest running household panel surveys in the world. The selection bias inherent to studies on unemployment is addressed in two ways. The only possible job ends considered are plant closures and lay-offs, which are plausibly exogenous relative to other job ends. In spite of this, children who are affected by parental unemployment still differ from unaffected children with respect to confounders. Entropy balancing eliminates this remaining heterogeneity in observable confounders by reweighting the control group such that it matches on the means of the covariate distribution of the treatment group. As a result, the exposure to parental unemployment can be assumed to be independent conditional on the observable confounders. Using the obtained entropy balancing weights to estimate the effect of parental unemployment on children's outcomes in a linear regression model, I find that paternal unemployment experienced between the age of 6 and 9 decreases a child's probability to graduate from secondary school with a university entrance qualification by 16 percentage points and to earn a university degree by 8 percentage points.

Considering all findings, I draw the following conclusions. First, parental unemployment generates spillovers on offspring. Second, in terms of educational attainment, the nature of the spillover is predominantly negative. Third, intergenerational effects are mainly transmitted through paternal unemployment. Fourth, the magnitude of the impact is not uniform across children's age and young children appear especially vulnerable. Fifth, spillover effects can persist well into adulthood and scar children permanently.

The prevalence of intergenerational spillovers from unemployment has important policy implications. Not taking the externalities on children into account might result in underestimating the societal cost of unemployment as well as the benefit of shielding those affected from the adverse effects of unemployment. While risk of job loss is a normal feature of labour markets, policies can be designed to mute the transmission of effects. In fact, the aforementioned reform in Germany consists in part of new regulations that aim at exerting less pressure on unemployed in the initial phase of unemployment. For

instance, own assets are protected for a longer time period and the room for penalties in case of non-cooperation is limited. This can be expected to attenuate the effects mediated via income or family stress. On the contrary, characteristics of the German education system such as the early tracking age might amplify the intergenerational spillover.

However, these are only mere speculations. Exploring the role of institutional factors related to labour market regulations, the social security system, and the education system in a panel of countries could therefore be a promising avenue for future research. Most importantly, future research should focus on disentangling the role of different mechanisms that mediate intergenerational effects of unemployment so as to gain a better understanding of how children can be protected from adverse consequences of parental unemployment. Another interesting research idea would be to narrow the scope down to educational transitions during tertiary education and investigate how parental unemployment can affect young adults.

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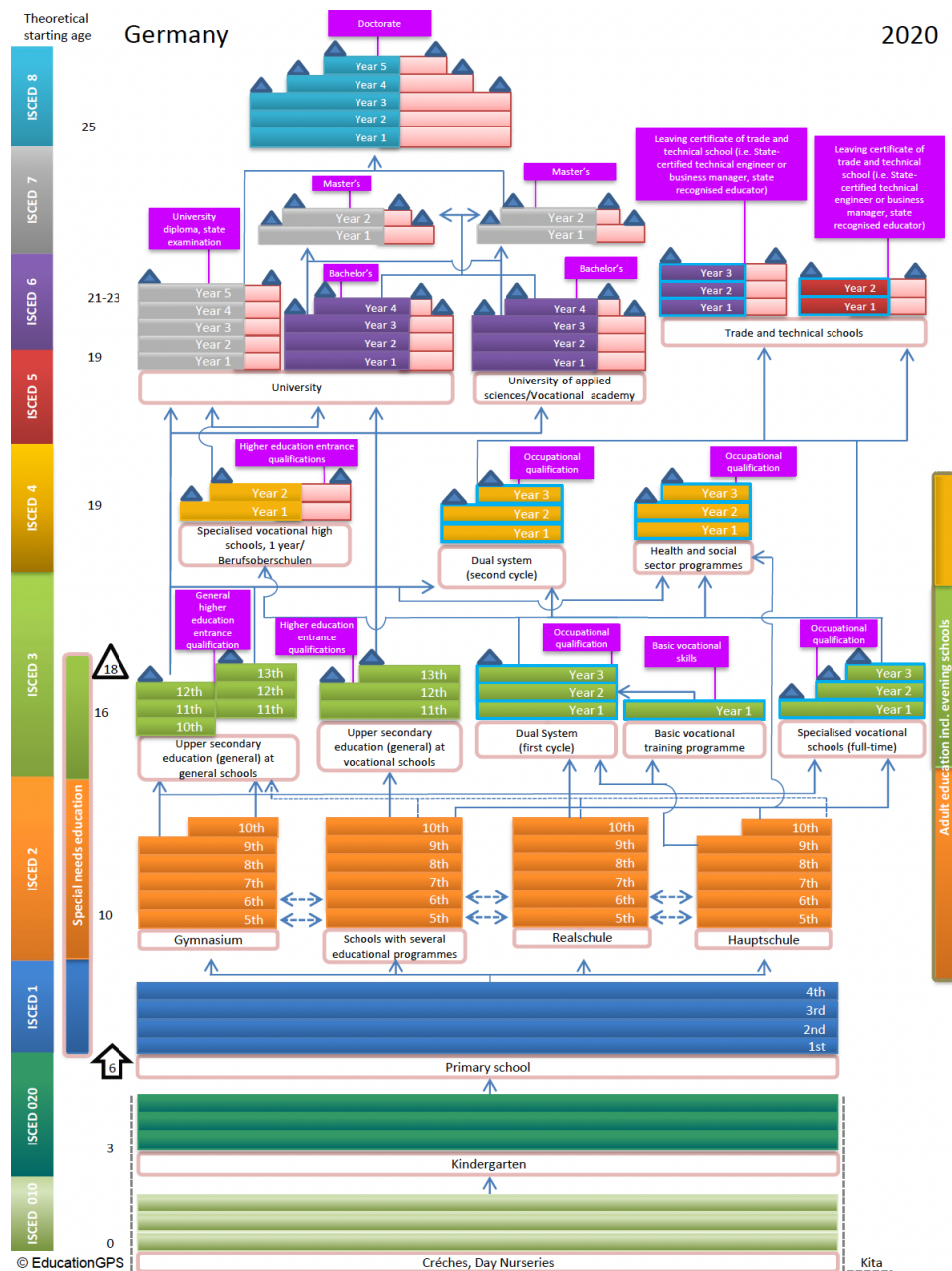
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A Appendix

A.1 Overview of the German education system



Source: OECD Education GPS, 2022.

A.2 Additional material for descriptive statistics

Table A1: Balance test for covariates at baseline after entropy balancing
(Maternal unemployment, age group 6-9)

Variable	Mean treated	Mean control	Mean difference	P-value
<i>Demographic information</i>				
Female	.5031	.503	.0000526	.9987
Migrant	.09604	.09604	1.54e-07	1
Rural area	.6215	.6214	.0000162	.9996
East Germany	.4096	.4095	.0000567	.9985
<i>Family and household information</i>				
Cohabitation with parent	.9029	.9029	-6.52e-06	.9997
Parents married	.9779	.9779	.0000276	.9976
Single parent	.01446	.01448	-.0000206	.9978
Number of kids in household	2.065	2.065	-.0001113	.9983
Home ownership	.3068	.3068	.0000104	.9997
Dual earner	.6723	.6723	.0000304	.9992
Household disposable income	26426	26426	-.6071	.9989
Household disposable income squared	7.52e+08	7.52e+08	-79092	.9976
<i>Parent information</i>				
Age	33.48	33.48	-.0000536	.9999
Age squared	1146	1146	-.002434	.9999
Never unemployed	.6322	.6321	.0000567	.9985
Employed without job change	.8902	.8901	.0001084	.9956
Full-time work experience	9.603	9.603	.0001568	.9996
Full-time work experience squared	122.7	122.7	-.0008648	.9999
Perceived job security	3.246	3.258	-.01249	.9846
Socioeconomic status	42.72	42.72	.001522	.9985
Socioeconomic status squared	1997	1997	.09915	.9988
Abitur	.1243	.1243	5.31e-06	.9998
Vocational training	.7608	.7608	.0000329	.999
Tertiary education	.1646	.1646	.0000308	.9989
No vocational training or tertiary education	.08895	.08902	-.0000632	.9972
Observations	53	977	1,030	

Note: This table presents means and mean differences for the set of conditioning variables after entropy balancing. All variables are measured at baseline. The last column displays p-values for the t-test.

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

Table A2: Balance test for covariates at baseline after entropy balancing
(Maternal unemployment, age group 15-18)

Variable	Mean treated	Mean control	Mean difference	P-value
<i>Demographic information</i>				
Female	.4718	.4718	-.0000836	.9964
Migrant	.3715	.3714	.0001175	.9947
Rural area	.3292	.3291	.0000161	.9993
East Germany	.264	.264	.0000526	.9974
<i>Family and household information</i>				
Cohabitation with parent	.945	.945	.0000154	.9985
Parents married	.8763	.8763	-.000047	.9969
Single parent	.04665	.04662	.0000246	.9975
Number of kids in household	1.794	1.794	.0000111	.9997
Home ownership	.3964	.3967	-.00026	.9885
Dual earner	.7984	.7985	-.0000594	.9968
Household disposable income	33249	33303	-53.91	.9142
Household disposable income squared	1.28e+09	1.30e+09	-1.51e+07	.7626
<i>Parent information</i>				
Age	42.04	42.05	-.002176	.9923
Age squared	1805	1805	-.1836	.9927
Never unemployed	.5422	.5424	-.0001256	.9945
Employed without job change	.6327	.6328	-.0000418	.9981
Full-time work experience	9.711	9.71	.0002936	.9992
Full-time work experience squared	161.4	161.3	.02311	.9979
Perceived job security	11.16	11.16	-.0007843	.9994
Socioeconomic status	37.92	37.93	-.005336	.9916
Socioeconomic status squared	1627	1628	-.4661	.9908
Abitur	.08477	.08501	-.0002428	.9811
Vocational training	.6233	.6233	-.0000774	.9965
Tertiary education	.1541	.1542	-.0000495	.997
No vocational training or tertiary education	.2747	.2746	.0001221	.9941
Observations	122	2,825	2,947	

Note: This table presents means and mean differences for the set of conditioning variables after entropy balancing. All variables are measured at baseline. The last column displays p-values for the t-test.

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

Table A3: Balance test for covariates at baseline after entropy balancing
(Paternal unemployment, age group 15-18)

Variable	Mean treated	Mean control	Mean difference	P-value
<i>Demographic information</i>				
Female	.515	.5151	-.0001029	.9954
Migrant	.3692	.3686	.0005937	.9725
Rural area	.4	.3997	.0003254	.9851
East Germany	.3155	.3151	.000425	.9795
<i>Family and household information</i>				
Cohabitation with parent	.885	.8851	-.0001807	.9873
Parents married	.8994	.8996	-.0001195	.9911
Single parent	.05327	.05321	.0000568	.9943
Number of kids in household	1.998	1.998	.0001544	.9965
Home ownership	.3991	.4001	-.0009671	.9559
Dual earner	.6425	.6427	-.0001497	.993
Household disposable income	29990	30087	-97	.8179
Household disposable income squared	1.03e+09	1.05e+09	-1.79e+07	.7385
<i>Parent information</i>				
Age	43.95	43.96	-.01006	.9603
Age squared	1963	1964	-.9061	.9608
Never unemployed	.2785	.2798	-.001229	.9388
Employed without job change	.6805	.6809	-.0004149	.9801
Full-time work experience	20.28	20.28	-.005767	.9811
Full-time work experience squared	457.6	457.9	-.2731	.9785
Perceived job security	9.439	9.427	.0125	.9892
Socioeconomic status	34.81	34.88	-.06516	.8787
Socioeconomic status squared	1354	1361	-6.583	.8621
Abitur	.04716	.04897	-.001808	.8127
Vocational training	.8202	.8193	.0008355	.9514
Tertiary education	.1076	.1091	-.001441	.8966
No vocational training or tertiary education	.1395	.1392	.0003114	.9799
Observations	205	2,940	3,145	

Note: This table presents means and mean differences for the set of conditioning variables after entropy balancing. All variables are measured at baseline. The last column displays p-values for the t-test.

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

A.3 Additional material for results

Table A4: Results - selected educational and early labour market outcomes
(Pooled unemployment, age group 6-18)

	Before ebalance		After ebalance	
<i>Complete vocational training</i>				
Treatment indicator	0.0691	(0.0561)	0.109*	(0.0562)
R ²	0.418		0.372	
Number of observations	2732		2594	
Number of individuals	684		656	
<i>Complete tertiary education</i>				
Treatment indicator	-0.0343	(0.0332)	-0.0624**	(0.0315)
R ²	0.207		0.342	
Number of observations	2732		2594	
Number of individuals	684		656	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	-0.0418	(0.0310)	-0.0525	(0.0361)
R ²	0.255		0.206	
Number of observations	4373		4157	
Number of individuals	794		761	
<i>Log labour earnings at age 30</i>				
Treatment indicator	0.0613	(0.147)	0.548*	(0.276)
R ²	0.535		0.512	
Number of observations	2386		2457	
Number of individuals	85		88	
<i>Own unemployment experience at age 30</i>				
Treatment indicator	0.548*	(0.276)	0.892***	(0.280)
R ²	0.512		0.663	
Number of observations	2457		2386	
Number of individuals	88		84	
<i>Socioeconomic status at age 30</i>				
Treatment indicator	15.36***	(5.445)	14.74***	(5.258)
R ²	0.816		0.971	
Number of observations	1101		1104	
Number of individuals	37		37	

Note: This table presents regression output for selected educational and early labour market outcomes before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but are not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

Table A5: Results - all educational and early labour market outcomes
(Pooled unemployment, age group 6-9)

	Before ebalance		After ebalance	
<i>Attend Haupt- or Realschule</i>				
Treatment indicator	0.125**	(0.0609)	0.0724	(0.0556)
R ²	0.146		0.187	
Number of observations	4618		4464	
Number of individuals	898		863	
<i>Attend Gymnasium</i>				
Treatment indicator	-0.0961**	(0.0479)	-0.0670	(0.0522)
R ²	0.220		0.222	
Number of observations	4618		4464	
Number of individuals	898		863	
<i>Grade point average at age 17</i>				
Treatment indicator	-0.0926	(0.0946)	-0.148**	(0.0750)
R ²	0.176		0.466	
Number of observations	671		645	
Number of individuals	671		645	
<i>Graduate from Haupt- or Realschule</i>				
Treatment indicator	0.217***	(0.0756)	0.159**	(0.0768)
R ²	0.301		0.366	
Number of observations	3080		2939	
Number of individuals	577		556	
<i>Graduate with Abitur</i>				
Treatment indicator	-0.218***	(0.0749)	-0.174**	(0.0732)
R ²	0.307		0.342	
Number of observations	3080		2939	
Number of individuals	577		556	
<i>Complete vocational training</i>				
Treatment indicator	0.142**	(0.0719)	0.123*	(0.0633)
R ²	0.410		0.446	
Number of observations	2787		2658	
Number of individuals	702		675	
<i>Complete tertiary education</i>				
Treatment indicator	-0.0738**	(0.0323)	-0.0688**	(0.0284)
R ²	0.214		0.330	
Number of observations	2787		2658	
Number of individuals	702		675	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	-0.0443	(0.0443)	-0.0633	(0.0498)
R ²	0.247		0.256	
Number of observations	4447		4240	
Number of individuals	815		783	
<i>Log labour earnings at age 30</i>				
Treatment indicator	-0.271	(0.231)	-0.383**	(0.159)
R ²	0.550		0.792	
Number of observations	2412		2359	
Number of individuals	86		82	
<i>Own unemployment experience at age 30</i>				
Treatment indicator	1.478***	(0.506)	1.285***	(0.441)
R ²	0.565		0.885	
Number of observations	2483		2415	
Number of individuals	89		85	
<i>Socioeconomic status at age 30</i>				
Treatment indicator	-4.124	(7.471)	15.88***	(5.442)
R ²	0.706		0.993	
Number of observations	1129		1135	
Number of individuals	38		38	

Note: This table presents regression output for all educational and early labour market outcomes before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but are not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

Table A6: Results - selected educational and early labour market outcomes
(Pooled unemployment, age group 10-14)

	Before ebalance		After ebalance	
<i>Grade point average at age 17</i>				
Treatment indicator	0.110	(0.0852)	-0.104	(0.0675)
R ²	0.0956		0.225	
Number of observations	1004		984	
Number of individuals	1004		984	
<i>Graduate from Haupt- or Realschule</i>				
Treatment indicator	0.206***	(0.0643)	0.222***	(0.0546)
R ²	0.284		0.331	
Number of observations	4586		4482	
Number of individuals	865		847	
<i>Graduate with Abitur</i>				
Treatment indicator	-0.193***	(0.0619)	-0.182***	(0.0530)
R ²	0.303		0.346	
Number of observations	4586		4482	
Number of individuals	865		847	
<i>Complete vocational training</i>				
Treatment indicator	-0.0575	(0.0680)	-0.0882*	(0.0450)
R ²	0.390		0.362	
Number of observations	4237		4146	
Number of individuals	1070		1049	
<i>Complete tertiary education</i>				
Treatment indicator	0.0565	(0.0580)	0.00354	(0.0286)
R ²	0.255		0.377	
Number of observations	4237		4146	
Number of individuals	1070		1049	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	-0.0121	(0.0304)	-0.00898	(0.0309)
R ²	0.250		0.217	
Number of observations	6706		6562	
Number of individuals	1243		1220	
<i>Log labour earnings at age 30</i>				
Treatment indicator	0.0179	(0.194)	0.378*	(0.195)
R ²	0.339		0.597	
Number of observations	3308		3283	
Number of individuals	122		119	
<i>Own unemployment experience at age 30</i>				
Treatment indicator	-0.425	(0.305)	-0.582	(0.354)
R ²	0.461		0.675	
Number of observations	3467		3423	
Number of individuals	127		124	
<i>Socioeconomic status at age 30</i>				
Treatment indicator	-3.870	(3.748)	0.771	(3.838)
R ²	0.687		0.777	
Number of observations	1945		1927	
Number of individuals	70		69	

Note: This table presents regression output for selected educational and early labour market outcomes before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but are not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

Table A7: Results - selected educational and early labour market outcomes
(Pooled unemployment, age group 15-18)

	Before ebalance		After ebalance	
<i>Complete vocational training</i>				
Treatment indicator	0.0640	(0.0441)	0.0521	(0.0370)
R ²	0.305		0.378	
Number of observations	6872		6783	
Number of individuals	1883		1859	
<i>Complete tertiary education</i>				
Treatment indicator	-0.0737***	(0.0263)	-0.0408**	(0.0188)
R ²	0.270		0.275	
Number of observations	6872		6783	
Number of individuals	1883		1859	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	-0.00415	(0.0311)	-0.0313	(0.0286)
R ²	0.187		0.213	
Number of observations	11080		10946	
Number of individuals	2249		2221	
<i>Log labour earnings at age 30</i>				
Treatment indicator	-0.279	(0.232)	-0.125	(0.175)
R ²	0.270		0.509	
Number of observations	4696		4707	
Number of individuals	193		190	
<i>Own unemployment experience at age 30</i>				
Treatment indicator	0.381	(0.302)	0.297	(0.317)
R ²	0.256		0.565	
Number of observations	4825		4783	
Number of individuals	195		192	
<i>Socioeconomic status at age 30</i>				
Treatment indicator	2.206	(5.823)	1.961	(4.308)
R ²	0.468		0.662	
Number of observations	2719		2695	
Number of individuals	108		106	

Note: This table presents regression output for selected educational and early labour market outcomes before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but are not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

Table A8: Results - selected educational and early labour market outcomes
(Maternal unemployment, age group 6-18)

	Before ebalance		After ebalance	
<i>Complete vocational training</i>				
Treatment indicator	0.0540	(0.109)	0.0489	(0.0699)
R ²	0.307		0.332	
Number of observations	3078		2959	
Number of individuals	750		727	
<i>Complete tertiary education</i>				
Treatment indicator	0.0466	(0.0678)	0.0159	(0.0414)
R ²	0.212		0.351	
Number of observations	3078		2959	
Number of individuals	750		727	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	-0.0831**	(0.0350)	-0.0902***	(0.0343)
R ²	0.230		0.294	
Number of observations	4962		4761	
Number of individuals	863		837	
<i>Log labour earnings at age 30</i>				
Treatment indicator	0.0599	(0.183)	0.592	(0.382)
R ²	0.393		0.384	
Number of observations	2587		2729	
Number of individuals	87		91	
<i>Own unemployment experience at age 30</i>				
Treatment indicator	0.592	(0.382)	0.787**	(0.391)
R ²	0.384		0.763	
Number of observations	2729		2658	
Number of individuals	91		87	
<i>Socioeconomic status at age 30</i>				
Treatment indicator	8.421	(7.201)	2.322	(2.876)
R ²	0.684		0.932	
Number of observations	1274		1281	
Number of individuals	40		40	

Note: This table presents regression output for selected educational and early labour market outcomes before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but are not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

Table A9: Results - selected educational and early labour market outcomes
(Maternal unemployment, age group 10-14)

	Before ebalance		After ebalance	
<i>Grade point average at age 17</i>				
Treatment indicator	0.266***	(0.0899)	0.113	(0.0832)
R ²	0.0780		0.218	
Number of observations	1284		1276	
Number of individuals	1284		1276	
<i>Graduate from Haupt- or Realschule</i>				
Treatment indicator	0.156*	(0.0935)	0.256***	(0.0708)
R ²	0.175		0.255	
Number of observations	5904		5817	
Number of individuals	1112		1100	
<i>Graduate with Abitur</i>				
Treatment indicator	-0.138	(0.0945)	-0.231***	(0.0719)
R ²	0.180		0.244	
Number of observations	5904		5817	
Number of individuals	1112		1100	
<i>Complete vocational training</i>				
Treatment indicator	-0.0234	(0.0755)	-0.0917*	(0.0531)
R ²	0.300		0.329	
Number of observations	5455		5383	
Number of individuals	1364		1351	
<i>Complete tertiary education</i>				
Treatment indicator	0.0538	(0.0600)	0.0183	(0.0331)
R ²	0.220		0.309	
Number of observations	5455		5383	
Number of individuals	1364		1351	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	-0.0171	(0.0310)	0.0113	(0.0357)
R ²	0.200		0.213	
Number of observations	8623		8511	
Number of individuals	1566		1552	
<i>Log labour earnings at age 30</i>				
Treatment indicator	-0.131	(0.265)	0.239	(0.204)
R ²	0.0773		0.650	
Number of observations	4138		4113	
Number of individuals	144		141	
<i>Own unemployment experience at age 30</i>				
Treatment indicator	0.446	(0.443)	0.394	(0.443)
R ²	0.200		0.499	
Number of observations	4398		4350	
Number of individuals	151		148	
<i>Socioeconomic status at age 30</i>				
Treatment indicator	-3.969	(6.503)	-3.011	(3.050)
R ²	0.297		0.637	
Number of observations	2535		2519	
Number of individuals	85		84	

Note: This table presents regression output for selected educational and early labour market outcomes before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but are not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

Table A10: Results - selected educational and early labour market outcomes
(Maternal unemployment, age group 15-18)

	Before ebalance		After ebalance	
<i>Complete vocational training</i>				
Treatment indicator	-0.0987	(0.0800)	-0.0548	(0.0494)
R ²	0.224		0.304	
Number of observations	8863		8857	
Number of individuals	2351		2349	
<i>Complete tertiary education</i>				
Treatment indicator	0.00612	(0.0654)	-0.0222	(0.0300)
R ²	0.267		0.243	
Number of observations	8863		8857	
Number of individuals	2351		2349	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	0.00850	(0.0390)	-0.00912	(0.0347)
R ²	0.151		0.150	
Number of observations	14297		14282	
Number of individuals	2781		2777	
<i>Log labour earnings at age 30</i>				
Treatment indicator	-0.117	(0.343)	-0.180	(0.204)
R ²	0.108		0.430	
Number of observations	5742		5839	
Number of individuals	223		222	
<i>Own unemployment experience at age 30</i>				
Treatment indicator	-0.173	(0.323)	-0.0966	(0.270)
R ²	0.154		0.431	
Number of observations	5940		5994	
Number of individuals	228		227	
<i>Socioeconomic status at age 30</i>				
Treatment indicator	12.31*	(6.790)	8.689**	(3.878)
R ²	0.417		0.691	
Number of observations	3561		3599	
Number of individuals	134		133	

Note: This table presents regression output for selected educational and early labour market outcomes before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but are not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

Table A11: Results - selected educational and early labour market outcomes
(Paternal unemployment, age group 6-18)

	Before ebalance		After ebalance	
<i>Complete vocational training</i>				
Treatment indicator	0.0110	(0.0538)	0.0854	(0.0525)
R ²	0.337		0.307	
Number of observations	3531		3438	
Number of individuals	902		878	
<i>Complete tertiary education</i>				
Treatment indicator	-0.0149	(0.0368)	-0.0343	(0.0334)
R ²	0.235		0.201	
Number of observations	3531		3438	
Number of individuals	902		878	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	-0.00592	(0.0348)	-0.0632*	(0.0352)
R ²	0.203		0.171	
Number of observations	5676		5541	
Number of individuals	1053		1029	
<i>Log labour earnings at age 30</i>				
Treatment indicator	-0.218	(0.292)	0.668*	(0.377)
R ²	0.188		0.255	
Number of observations	2949		3016	
Number of individuals	103		105	
<i>Own unemployment experience at age 30</i>				
Treatment indicator	0.668*	(0.377)	0.401	(0.426)
R ²	0.255		0.371	
Number of observations	3016		3004	
Number of individuals	105		103	
<i>Socioeconomic status at age 30</i>				
Treatment indicator	-8.109*	(4.697)	-8.267**	(3.650)
R ²	0.399		0.662	
Number of observations	1558		1564	
Number of individuals	52		52	

Note: This table presents regression output for selected educational and early labour market outcomes before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but are not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

Table A12: Results - selected educational and early labour market outcomes
(Paternal unemployment, age group 10-14)

	Before ebalance		After ebalance	
<i>Grade point average at age 17</i>				
Treatment indicator	0.0805	(0.0926)	-0.0631	(0.0707)
R ²	0.0562		0.161	
Number of observations	1410		1397	
Number of individuals	1410		1397	
<i>Graduate from Haupt- or Realschule</i>				
Treatment indicator	0.103	(0.0766)	0.156**	(0.0617)
R ²	0.184		0.217	
Number of observations	6133		6062	
Number of individuals	1210		1198	
<i>Graduate with Abitur</i>				
Treatment indicator	-0.125*	(0.0750)	-0.176***	(0.0568)
R ²	0.195		0.256	
Number of observations	6133		6062	
Number of individuals	1210		1198	
<i>Complete vocational training</i>				
Treatment indicator	-0.126*	(0.0753)	-0.0520	(0.0544)
R ²	0.343		0.409	
Number of observations	5669		5609	
Number of individuals	1508		1491	
<i>Complete tertiary education</i>				
Treatment indicator	0.0527	(0.0556)	-0.0138	(0.0256)
R ²	0.219		0.218	
Number of observations	5669		5609	
Number of individuals	1508		1491	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	0.0370	(0.0377)	0.0198	(0.0361)
R ²	0.207		0.211	
Number of observations	8997		8906	
Number of individuals	1768		1751	
<i>Log labour earnings at age 30</i>				
Treatment indicator	0.0536	(0.194)	0.104	(0.157)
R ²	0.170		0.468	
Number of observations	4354		4404	
Number of individuals	161		159	
<i>Own unemployment experience at age 30</i>				
Treatment indicator	-0.0486	(0.356)	-0.0231	(0.296)
R ²	0.257		0.347	
Number of observations	4571		4567	
Number of individuals	167		165	
<i>Socioeconomic status at age 30</i>				
Treatment indicator	-4.686	(4.361)	1.862	(2.577)
R ²	0.337		0.571	
Number of observations	2713		2705	
Number of individuals	95		94	

Note: This table presents regression output for selected educational and early labour market outcomes before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but are not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

Table A13: Results - selected educational and early labour market outcomes
(Paternal unemployment, age group 15-18)

	Before ebalance		After ebalance	
<i>Complete vocational training</i>				
Treatment indicator	0.0623	(0.0484)	0.0515	(0.0382)
R ²	0.245		0.361	
Number of observations	9004		8990	
Number of individuals	2476		2472	
<i>Complete tertiary education</i>				
Treatment indicator	-0.0897***	(0.0240)	-0.0624***	(0.0185)
R ²	0.266		0.180	
Number of observations	9004		8990	
Number of individuals	2476		2472	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	0.0233	(0.0379)	0.0177	(0.0296)
R ²	0.171		0.189	
Number of observations	14515		14495	
Number of individuals	2927		2922	
<i>Log labour earnings at age 30</i>				
Treatment indicator	-0.228	(0.275)	-0.0267	(0.183)
R ²	0.0910		0.335	
Number of observations	5822		5954	
Number of individuals	240		240	
<i>Own unemployment experience at age 30</i>				
Treatment indicator	0.504	(0.322)	0.760**	(0.316)
R ²	0.175		0.467	
Number of observations	6061		6126	
Number of individuals	246		246	
<i>Socioeconomic status at age 30</i>				
Treatment indicator	-6.399	(5.311)	-7.568*	(3.870)
R ²	0.349		0.534	
Number of observations	3446		3482	
Number of individuals	137		137	

Note: This table presents regression output for selected educational and early labour market outcomes before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but are not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

A.4 Additional material for robustness tests

Table A14: Robustness test results - entry into unemployment restricted to plant closure
(Paternal unemployment, age group 6-9)

	Before ebalance		After ebalance	
<i>Attend Haupt- or Realschule</i>				
Treatment indicator	0.149	(0.120)	-0.00319	(0.0640)
R ²	0.155		0.382	
Number of observations	5969		5861	
Number of individuals	1152		1127	
<i>Attend Gymnasium</i>				
Treatment indicator	-0.183*	(0.107)	-0.0256	(0.0670)
R ²	0.220		0.379	
Number of observations	5969		5861	
Number of individuals	1152		1127	
<i>Grade point average at age 17</i>				
Treatment indicator	0.166	(0.166)	0.134	(0.106)
R ²	0.0760		0.376	
Number of observations	864		847	
Number of individuals	864		847	
<i>Graduate from Haupt- or Realschule</i>				
Treatment indicator	0.273***	(0.102)	0.258***	(0.0911)
R ²	0.227		0.347	
Number of observations	3930		3820	
Number of individuals	736		719	
<i>Graduate with Abitur</i>				
Treatment indicator	-0.321***	(0.0841)	-0.323***	(0.0660)
R ²	0.232		0.383	
Number of observations	3930		3820	
Number of individuals	736		719	
<i>Complete vocational training</i>				
Treatment indicator	0.123	(0.140)	0.118	(0.0789)
R ²	0.339		0.451	
Number of observations	3575		3490	
Number of individuals	902		879	
<i>Complete tertiary education</i>				
Treatment indicator	-0.0451	(0.0429)	-0.0807***	(0.0296)
R ²	0.226		0.304	
Number of observations	3575		3490	
Number of individuals	902		879	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	-0.102	(0.128)	0.00905	(0.0729)
R ²	0.203		0.355	
Number of observations	5753		5623	
Number of individuals	1061		1038	

Note: This table presents regression output for the plant closure robustness test before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but are not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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

Table A15: Robustness test results - longer observation period for control group
(Paternal unemployment, age group 6-9)

	Before ebalance		After ebalance	
<i>Attend Haupt- or Realschule</i>				
Treatment indicator	0.0980	(0.0739)	0.0972	(0.0665)
R ²	0.197		0.259	
Number of observations	5016		4917	
Number of individuals	974		951	
<i>Attend Gymnasium</i>				
Treatment indicator	-0.130**	(0.0571)	-0.127**	(0.0603)
R ²	0.239		0.355	
Number of observations	5016		4917	
Number of individuals	974		951	
<i>Grade point average at age 17</i>				
Treatment indicator	-0.121	(0.120)	-0.213**	(0.0964)
R ²	0.0923		0.286	
Number of observations	721		706	
Number of individuals	721		706	
<i>Graduate from Haupt- or Realschule</i>				
Treatment indicator	0.306***	(0.0591)	0.212***	(0.0702)
R ²	0.292		0.319	
Number of observations	3271		3181	
Number of individuals	629		614	
<i>Graduate with Abitur</i>				
Treatment indicator	-0.308***	(0.0589)	-0.189***	(0.0576)
R ²	0.295		0.336	
Number of observations	3271		3181	
Number of individuals	629		614	
<i>Complete vocational training</i>				
Treatment indicator	0.0978	(0.0796)	0.132	(0.0816)
R ²	0.352		0.386	
Number of observations	2946		2870	
Number of individuals	772		751	
<i>Complete tertiary education</i>				
Treatment indicator	-0.0755***	(0.0292)	-0.0799***	(0.0280)
R ²	0.238		0.198	
Number of observations	2946		2870	
Number of individuals	772		751	
<i>Complete no vocational training or tertiary education</i>				
Treatment indicator	-0.0547	(0.0546)	-0.0763	(0.0671)
R ²	0.224		0.204	
Number of observations	4744		4633	
Number of individuals	897		876	

Note: This table presents regression output for the longer observation period for the control group robustness test before and after entropy balancing. Before entropy balancing, observations are still weighted by SOEP survey weights. Control variables are included for all specifications but are not displayed in this table. Standard errors are clustered at the level of the individual child and reported in parentheses.

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