Erasmus University Rotterdam

Erasmus School of Economics

Master Thesis Policy Economics

The Effects of Parental Job Displacement on the Educational Outcomes of Children in the Context of High-Stakes Tests

Author
H.E. Goris [1][2]

Supervisor
Prof. dr. Dinand Webbink

11th December 2021

Abstract

This study investigates the effects of parental job displacement on the educational outcomes of children in the context of Dutch high-stakes test. Exploiting the variation in the timing of job displacement around the test date, this study finds that children whose parents are displaced before the test date obtain lower language and total test scores, receive lower track placement recommendations and have a lower probability of attending pre-university tracks. Heterogeneity analysis shows that these effects are driven by paternal job displacement.

[1]The author is grateful for his experience as an intern at the Dutch Bureau of Economic Policy Analysis (CPB) and the access to confidential micro-data provided by the Dutch Bureau of Statistics (CBS). Both are fundamental to this thesis.

[2]Student number: 388660

Contents

1 Introduction

Does parental job displacement affect the educational outcomes of the child? The answer to this question is important for both the equity and efficiency of education. If displacement hampers the human capital accumulation of children, it could have potential lifetime consequences. Such effects could indicate that the societal costs of job displacement are understated. Moreover, if job displacement is unevenly dispersed across sub-populations, it could lead to increasing inequality. It is therefore relevant to understand the mechanisms through which job losses may affect intergenerational human capital. This paper will examine the question whether high-stakes testing is a mechanism through which job displacement may affect the educational outcomes of the child.

To answer this question, this paper will analyse the effect of parental job losses on the educational outcomes of their children using Dutch administrative data. For this research, firm closures and mass lay-off events are deduced from employment spell data. Data on displaced workers are combined with the educational records of any children that sat for the test around the time of the displacement. The estimation approach relies on a comparison of groups of children whose parents were displaced just before and just after they sat for the test.

By applying a model that compares education outcomes for children whose parents were displaced before the test with children whose parents were displaced after the test, the main empirical results suggest that parental job displacement exerts a negative effect on short- and long-run educational outcomes of the child. In the short-run, pre-test parental job displacement negatively affects test performance and the subjective teacher assessment of the child. In the long-run, pre-test parental job displacement decreases subsequent enrolment levels in pre-university tracks and increases enrolment levels in lower vocational tracks. The results are driven by paternal job displacement, as maternal job displacement events exert an insignificant negative effect.

This thesis exploits variation in the timing of job displacement around the test date. However, it cannot be precluded that the results are driven by unobserved heterogeneity between the treatment and control groups instead of the job displacement. The group of children whose parents are displaced pre-test are of a lower socio-economic status on average.

This research seeks to contribute to existing literature on the effects of job displacement on educational outcomes of children by analysing the relative timing of job displacement around an high-stakes test. Earlier papers already suggest a link between the timing of certain family affairs on educational outcomes of children. For instance, Pan and Ost (2014) exploit variations in the timing of job loss, by comparing job losses of parents for the ages of children between 15 to 17 years against 21 to 23 years. Stans (2020) also builds upon the relative timing of events around the same high-stakes test, using grand-parental death as treatment.

The rest of the paper is structured as follows. Section 2 discusses the relevant background in three parts. Part 2.1 focusses on the theoretical background of human capital investment and the potential effects of job displacement of parents. Part 2.2 provides a brief overview of the empirical literature on the effects of parental job displacement on educational outcomes. Part 2.3 explains the institutional background of the Dutch education system. Section 3 provides a description of the method and data used for this paper and concludes with a discussion on the internal validity. Section 4 presents and interprets the empirical results. Section 5 ends with concluding remarks, policy implications and suggestions for future research.

2 Background

The first part of the following section elaborates on the theoretical framework of human capital and the mechanisms through which job displacement may affect human capital accumulation. Second, it discusses empirical work on the effects of job displacement on educational outcomes. The last part of this section illustrates the institutional background of the Dutch social security and education system.

2.1 Theoretical Background

There are several mechanisms through which shocks at the parental level, such as job displacement, may affect children[1]. The effects of parental economic distress on the development of children can be broadly categorised in economic and psychological factors (Kalil 2013). However, these factors do not operate in strict isolation. The development of children should be viewed as the result of a complex interplay between economic inputs and behavioural, emotional and developmental processes. The section below discusses the theoretical framework of human capital investment, the event of job displacement and several mechanisms through which job displacement may affect child development.

2.1.1 Human Capital Investment

Models of human capital investment were first introduced by Becker (1994). These type of models revolve around utility-maximising parents that care about themselves and their children. In these models, individuals may forego consumption for productivity-increasing investments in themselves or their children. This framework has been expanded upon since its inception. First, researchers tend to take a more granular view of human capital by distinguishing between categories of human capital, such as cognitive and non-cognitive skills, and by distinguishing different (non-)cognitive skills from another (Kautz et al. 2014). Second, researchers not only acknowledge time and resources as investment inputs, but also take into account an investment-enhancing effect of the already existing stock of human capital, which in short comes down to the stylised fact that "skills beget skills".

Cunha and Heckman (2007, 2008) identify two separate mechanisms that lend support to this view. First, the self-productivity effect. This suggests that a higher stock of skills in one period raises the stock of skills in the next period. For example, superior non-cognitive skills (such as grit, self-determination and resilience or the absence of emotional instability) enable an individual to develop more efficiently for constant investment inputs. The second aspect is dynamic complementarity, meaning that a higher stock of skills in one period raises the returns to skills and formation thereof in the next period. For example, it may be easier to educate a child in history or geography if it has better comprehension skills. Both these features serve as multipliers and help us understand why "skills beget skills". These elements serve the view that, ceteris paribus, human capital investments early in life have higher pay-offs than human capital investments later in life.

Events that affect the inputs to human capital accumulation have, by virtue of the mechanisms of that process, the potential to cause long-lasting effects. The focus of this paper is whether job displacement, through high-stakes testing as a mechanism, can affect the inputs and thereby the outcomes of human capital accumulation processes.

[1] For a more general view on the relation between family background and child outcomes, see Björklund and Salvanes (2011).

2.1.2 Job Displacement Effects & Mechanisms

In this section, I discuss theoretical work on the mechanisms through which job displacement may affect the development of the child. Brand and Simon Thomas (2014) notes that the event of job displacement consists of several elements ranging from notification, anticipation, dismissal, unemployment to possible job search, retraining and reemployment. Each of the elements in this chain may affect the child through changes in resources invested in the child and psychological distress. In the remainder of this section, these mechanisms will be discussed more closely.

Resources & use of time

Two conditions have to be fulfilled in order for job displacement to affect children via the investment channel. Additionally, the expected direction of the effect (whether job losses positively or negatively affect the child) depends on how these conditions are fulfilled.

First, job loss should change parental income, leisure time or both. In environments without any social safety net, parents may be confronted with a trade-off between hours worked and income. Job loss in such an environment increases available time and decreases available income to invest in the child. In environments where the social safety net completely offsets the loss in income, job loss increases available time and does not affect available income to invest in the child. Dependent on the replacement rate (the share of lost income that is replaced by social benefits) an environment may be characterised as somewhere in between these two extremes. Furthermore, it may also be the case that parental job losses trigger changes in third-party investments in the child through countervailing measures. For example, it may be government or school policy to dedicate more time, money or other resources directly to children who experience parental job losses.

Second, changes in income, leisure time or third-party assistance should map into changes in human capital investments in the child. If the parental preference for (non-)monetary investment in the child is independent of the available (non-)monetary resources, changes in the income or leisure time will not affect the development trajectory of the child. This may be the case if the parent fully devotes the increase in available time to other activities (job searches for example) and the loss in parental income is fully offset through the safety net. Similarly, it may be the case that on the margin, income and time is spent on other activities so that changes on the margin do not affect income and time devoted to the child. Ceteris paribus, parents with a higher-income job are expected to spend less time on the child as their opportunity costs are higher.

The direction of the effect of parental job loss depends on the sum of changes in investments. Changes in investments are composed of changes in both the productivity and size of (non-)monetary investments. If monetary investments are more productive than non-monetary investments, job losses resulting in a decrease in income and an increase in time available may have a negative net effect on child development. Conversely, if non-monetary investments are more productive than monetary investments, job losses may result in a net positive effect on child development.

Earlier literature has identified several parameters that matter for the level and productivity of time and monetary investments. First, the age of the child matters as investments earlier in the life tend to have higher pay-offs (Cunha and Heckman 2007, 2008). Second, the higher the education of the parents, the more time is spent on the child. This result holds across countries[2],

[2] Guryan, Hurst and Kearney (2008) study Canada, Chile, France, Germany, Italy, the Netherlands, Norway,

gender and (un)employed subgroups and across basic child care, educational activities, recreational and travel related activities (Guryan, Hurst and Kearney 2008). However, this result is inconsistent with the above-mentioned opportunity cost explanation. Namely, this would imply that time spent with children is not determined by opportunity costs and that there are other differences between lower- and higher-educated parents that determine time spent with children.

Distress

The psychological and emotional stress that job displacement puts on families has several effects. It disrupts routines, may strain relationships and impede - directly or indirectly - parents' ability to accommodate the child's social, emotional and intellectual development. Both incidental shock effects arising from job displacement and structural effects arising from being unemployed could impede developmental progress through this channel.

McLoyd and Wilson (1990) note that "deterioration in the parent's psychological functioning in the context of economic loss or poverty may become a communicable social phenomenon to the extent that the child imitates the symptomatic effect and behaviour of the parent".

The important role of parents' stress on a child's educational performance lends support from the research by Ananat et al. (2011), who find that state-wide waves of job losses negatively affect academic performance in such states. Their result outsizes the effects found in studies that only look at the effects of children of displaced parents, suggesting that parental job displacement also spills over to peers. In other words, the societal costs associated with displacement are higher than the costs bourne by the displaced individual. The authors tested for four mechanisms and found that changes in emotional stress are the most consistent explanation for the effects. It may thus even be the case that psychological functioning is not only communicable between parent and child, but also between peers.

In the extreme case, distress may even cause negligence of and disengagement with the child. Moreover, the effects of distress are influenced by the family structure. For example, the risk of displacement affecting the child is heightened in single-parent households because there is no additional parent to respond to the needs of the child. Displacement seems to be particularly damaging for children if it causes maternal depression (Brand and Simon Thomas 2014), indicating that fathers and mothers are not perfect substitutes with regards to the needs of their children.

The effects of distress are not always negative. On the one hand, studies show that parental job displacement may diminish motivation and lead to disengagement from the task at hands. On the other hand, there is also the possibility that children can use the negative experiences of their parents for the better as it may improve future resilience or serve as an example how not to progress in life. However, this possibility may only be reserved for children that are able to demonstrate resilience in the face of hardship.

Palestine, Slovenia, South Africa, the United Kingdom and the United States.

2.2 Empirical Background

There is a growing body of literature that identifies intergenerational spillovers of job displacement[3]. Two things must be noted about the state of the literature. First, most of the empirical work is based on data from the USA and Canada. This troubles interpretation to the extent that there are differences in public policies, labour market, education and culture. Second, most of the empirical work studies human capital by approximation. For instance, retention or drop-out rates are commonly used outcomes. It must be noted that these are merely imperfect proxies of the more latent factor human capital. Notwithstanding these caveats, the section below reviews earlier empirical literature on the effects on educational outcomes in this section. For a more schematic overview of the literature I discuss, please see table 2.1.

Studying Canadian adolescents, Oreopoulos, Page and Stevens (2008) identify a negative correlation between paternal job loss and their son's subsequent earnings and welfare receipt. In an effort to investigate a potential explanation for that result, Coelli (2011) finds similar results and expands by estimating that parental job loss for children aged 16 to 18 is associated with a 10 percentage point decrease in post-secondary education enrolment rates. The author suggests that reduced parental income may impose a binding constraint on enrolment decisions. These results from Canada seem consistent across North-America. Kalil and Wightman (2011) compare US parents who experienced involuntary job loss against those who have not. For white and black middle-income families respectively, involuntary job loss is associated with a 7 to 26 percentage points lower college enrolment rate and a 5 to 32 percentage point lower high school graduation rate. However, involuntary job loss is not plausibly exogenous as it may be related to parental characteristics that also affect their child's development. Pan and Ost (2014) address this endogeneity concern by comparing parents who lost jobs when the child was 15 to 17 years old against parents who lost jobs when the child was 21 to 23 years old. These groups do not significantly differ in observable characteristics other than the timing of job loss and the age of the father. The authors report that parental job loss at 15 to 17 years is associated with 10 percentage point decrease in college enrollment rates, consistent with earlier studies that built upon more restrictive identifying assumptions.

Kalil and Ziol-Guest (2008) find self-reported paternal "employment separation" to negatively affect grade retention while no effect is found for maternal variant. These results are driven by families where the mother earns more than the father. The authors speculate that cultural norms and the identity and self-image of the father play an important role, as has been found in other literature. Stevens and Schaller (2011) also study grade retention but narrow job displacement down to those likely to be driven by broader labour market conditions instead of individual covariates that may both affect retention and employment separation probabilities. They find that parental job loss increases retention probability by 0.8 percentage points or 15% and do not find a significant distinction between double-parent (assumed to be fathers) and single-parent households (mothers).

Hilger (2016) finds that while lay-offs reduce family income to a large magnitude, the effects on college enrolment, quality and early career earnings are small. These effects are consistent with the explanation that parents do not spend much on college out of their marginal income. The author uses tax returns and therefore cannot investigate channels other than income.

[3]See for example J. M. Lindo (2011), Liu and Zhao (2014) and Bubonya, Cobb-Clark and Wooden (2017) for health-related spill-overs and Rege, Skardhamar et al. (2019) for crime-related spill-overs.

Rege, Telle and Votruba (2011) study the effects of parental job loss induced by plant closures as captured by Norwegian administrative data from 2003 to 2007. More specifically, they estimate the effect of job loss one to three years before the child's 10th grade on the 10th grade GPA, this corresponds to an age of 16. They find that paternal job loss has a significant negative effect while maternal job loss has insignificant positive effect. In their preferred specification, paternal job loss is associated with a 6.3% SD decrease in 10th grade GPA. The effect-size is larger for fathers with below-median earnings before job loss. Different sub-samples and placebo tests suggest that subsequent income, marital & employment status, household division of tasks and relocation appear to be unimportant factors in explaining the results. More specifically, the placebo tests return similar results across both fathers and mothers, while only paternal job loss has a significant effect on educational performance. The authors hypothesise that the heterogeneous effects across parents are driven by unobserved and differential effects on mental health and distress. It must be noted that these results may be sensitive to generosity of the Norwegian welfare state. The absence of social security may open subsequent earnings and employment status as paths through which job loss may affect educational performance.

Ruiz-Valenzuela (2020) points out a challenge in using mass lay-offs as an identification strategy. Downsizing or closure, both at the firm or at the establishment level, may be correlated with worker characteristics that in turn may affect child outcomes. It should be appreciated that this is not merely a theoretical option, as Card, Heining and Kline (2013) and Hilger (2016) show that worker characteristics differ across firms that do and do not struggle. Ruiz-Valenzuela (2020) addresses this challenge by using repeated observations of the same child. The author merges parental data, collected through a small-scale survey, with their child's educational performance, primarily yearly GPA, to study the effects of job loss in the context of the Great Recession. In order to compare the results with earlier studies, the author also uses cross-sectional data and a value-added specification. She finds that the estimates in her preferred specification are smaller in magnitude[4] as compared to the two alternative strategies and suggests that the inability to control for covariates of job loss and educational performance may result in an overestimation of the effect of job loss.

Two conclusions can be drawn on the effects of job displacement. First, the effects of paternal job loss are consistently negative on the child's educational outcomes. Second, the effects of maternal job loss do not always seem to affect the child in observable outcomes. The main effect of interest of this paper is whether experiencing parental job displacement before the test decreases test performance. Given these empirical results, this paper also looks into whether the effects differ for maternal and paternal job displacement.

With respect to the state of the literature, two points can be made. First, the literature relating job loss to intergenerational education outcomes studies adolescent youth more than children of a younger age. Second, the outcomes that are studied in the literature on intergenerational effects of parental job loss are skewed towards "hard" measures such as GPA, education level or income whereas "soft" measures such as socio-emotional development attract less attention[5].

[4] Paternal job loss causes a 15% SD decrease in the yearly GPA in the fixed-effects model as compared to 25% SD and 26% SD for the cross-sectional and value-added specifications respectively.

[5] This relates to the more general point made by Akerlof (2020)

Table 2.1: Earlier literature on the inter- and intragenerational effects of job displacement

Study	Data	Treatment	Child characteristics	Method	Outcome	Result
Bratberg, Nilsen and Vaage (2008)	Norwegian register data	Paternal job loss due to downsizing or closure	Children aged 12-15	Fixed effects regression	Earnings after 15 years	Fathers experience decreased earnings and employment; No aggregate nor disaggregrate effects on earnings of children
Kalil and Ziol-Guest (2008)	US survey data	Self-reported involuntary job loss	Children aged 5-17	OLS regression	Grade repetition	No effects for maternal job loss, negative effects for paternal job loss if the father earns less than mother. Authors hypothesize that family dynamics outweigh family income as an explanation.
Oreopoulos, Page and Stevens (2008)	Canadian register data	Paternal job loss due to closure	Sons aged 10-15	Fixed effects regression	Earnings, UI & benefits at age 25-32	Average adult earnings reduced by 9%; larger effects for low income
Page, Stevens and J. Lindo (2009)	US repeated survey data	Self-reported job loss due to downsizing and closure	Children aged 15 and younger	Fixed effects regression	Education, income, earnings & unemployment	Income shocks have negligible average effect on children's socio-economic outcomes, effects are negative for relatively young and disadvantaged children.
Rege, Telle and Votruba (2011)	Norwegian register data	Paternal job loss due to downsizing	Children aged 13-15	Pooled OLS regression	GPA at age 16	6% decrease due to paternal job loss; insignificant increase due to maternal job loss.
Coelli (2011)	Canadian survey data	Job loss due to self-reported redundancy or business failure	Children aged 16-18	Linear probability model	Post-secondary school enrolment at age 16-29	Enrolment probability reduced by 10 percentage points. Larger effects for higher pre-displacement income

Stevens and Schaller (2011)	repeated US survey data	Household head (predominantly male) job loss due to self-reported redundancy, discharge or business failure	Children aged 5-19	Fixed effects regression	Grade retention 1 year after job loss	Retention probability increased by 15 percentage points. Larger effects for higher pre-displacement income
Kalil and Wightman (2011)	US repeated survey data	Household head (predominantly male) job loss due to self-reported redundancy, discharge or business failure	Black and white low- and middle-class children	OLS regression	College enrolment probability	Enrolment probability reduced by 7 pp (middle-class) and 8 pp (lower class) white children, by 26 pp (middle-class) and 1 pp (lower class; not statistically different from the white lower class reducation) black children children.
Ananat et al. (2011)	US repeated survey (education outcomes) and register (aggregate job loss) data	State-wide job loss as % of the working-age population	Children aged 8-14	Instrumental variable	Next year GPA	Decreases in fourth- and eight-grade math and reading scores. Effects for children with displaced parents are 34x larger than for children without displaced parents suggesting spill-overs.
Gregg, Macmillan and Nasim (2012)	British cohort study	Combination of change in year-on-year paternal employment status and employment in heavily contracting industry	Children aged 11-13	Fixed effects regression	Age 16 GPA, age 30-34 earnings	Large decrease in GPA; insignificant 6 pp decrease in earnings.
Pan and Ost (2014)	US repeated survey data	Variation in child age at job loss	Children aged 15-17 or 21-23	Linear probability model	College enrolment	Parental job loss between ages 15-17 decreases enrolment probability by 10 percentage points
Hilger (2016)	US register data	Job loss as identified by UI take-up as compared to same-firm "survival"	Children aged 12-29	Differences-in-differences	College enrolment at 18-22, earnings at 25	Small significant reductions in adolescent outcomes.

Peter (2016)	German repeated survey data	Parental job loss due to plant closure or self-reported dismissal	Children aged 5-6; Adolescents aged 17	Propensity score matching	Non-cognitive skills	0.5 SD increase in socio-behavioural problems at age 5-6; 0.26 SD decrease in adolescent's self-determination beliefs.
Filiz (2016)	US repeated survey data	Maternal job loss due to plant closure	Children aged 5-14	Fixed effects model	Test scores	Decreases in math and reading scores for single-mom children; no effect for married mothers.
Nikolova and Nikolaev (2018)	German repeated survey data	Parental job loss due to plant closure	Children aged 0-5; 6-10 and 11-15	Fixed effect model	Life satisfaction at age 18-31	Negative effects for all children aged 0-5; positive effects for daughters aged 5-11; negative effects for sons aged 11-15.
Di Maio and Nisticò (2019)	Palestinian repeated survey data	Parental job loss induced by Second Intifada conflict intensity	Children aged 10-17	Instrumental variable	School dropout probability	Dropout probability increased by 9 percentage points. Household income appears to drive the results.
Ruiz-Valenzuela (2020)	Spanish survey data (labour market) & administrative data (education)	Self-reported paternal involuntary job loss	Children aged 3-16	Individual fixed effects model	GPA	15 % decrease for paternal job loss, no effect for maternal job loss.

2.3 Institutional Background

The section below discusses the Dutch social security system in the event of job displacement, followed by a discussion of the Dutch educational system in general. Afterwards, this section elaborates on the transition from primary to secondary school and the role of high-stakes standardised testing in the Netherlands. This section ends with a discussion of some relevant reforms and a discussion of Stans (2020) as it studies the effects of the death of a grandparent in the context of the same high-stakes test.

2.3.1 Social Security

If Dutch workers are displaced from their jobs due to firm closure or downsizing, they become eligible for unemployment insurance benefits under the Dutch *Werkloosheidswet*.

The replacement rate of previous wages is 75% in the first two months and 70% in the months thereafter. Moreover, there is an absolute cap to what is replaced. In other words, workers lose a minimum of 25% of their previous wage upon displacement. The duration of unemployment insurance depends on the labour history of the person concerned. Each year of work in the Netherlands allows for an extra month of unemployment insurance with a minimum of 3 and a maximum of 38 months. In addition to the right to replacement of previously earned wages, there also is a duty to apply for new employment via the unemployment agency UWV.

Dutch social security therefore does not completely offset the loss in income caused by job displacement. More specifically, displaced individuals see their income reduced by a minimum of 25% in the first 2 months and 30% thereafter. The reduction in income due to displacement can be offset by spending out of savings, so that it is not necessarily the case that displaced individuals have to amend their spending. Moreover, the displaced gain free time of which some has to be dedicated to job searches. This means that displaced parents have less income and more time to spent on their children.

2.3.2 Education System

The educational system in the Netherlands can be broadly categorised in four periods. Pre-school, primary schools, secondary schools and post-secondary education.

When children turn 5 years, they are required by law to be enrolled in education. Before that age, children typically attend pre-school. In the transition from primary to secondary school, typically at the age of 12, students are sorted into different tracks. The tracks can roughly be distinguished into three classes: lower pre-vocational education (4 years; VMBO in Dutch), higher pre-vocational education (5 years; HAVO in Dutch) and pre-university education (6 years; VWO in Dutch). These tracks are meant as preparation for the post-secondary education, which is either vocational (MBO and HBO respectively) or academic (WO).

Whether a school is public or has a particular denomination (religious or otherwise) does not matter for the purposes of this paper. This holds for both primary and secondary schools. Schools are all financed by the state and held to the same standards in terms of teacher certification. However, there is some room for discretion in terms of curriculum and methods resulting from the constitutionally protected liberty of education. Schools do vary in terms of teacher quality because teachers sort into different schools depending on its characteristics. Bonhomme, Jolivet and Leuven (2016) demonstrate that primary school teachers prefer not to work in schools with high shares of disadvantaged students, large classes and large overhead departments. Rather, they prefer to work in schools with higher average student performance, higher average staff age and higher shares of female teachers.

The early selection of students into different educational tracks, a practice known as *tracking*, is a particular characteristic of the Dutch education system. Although other education systems also separate students into different tracks, Dutch students are separated at a relatively early stage in their life, around the age of 12.

Tracking homogenises the student population of a certain track. Therefore the transmission of knowledge is more efficient, as the teacher has less differences in ability to take into account. Conversely, tracking may increase inequality. If the student is not tracked according to its true ability because of measurement error, biases or events that temporarily depress the students' performance (such as parental job displacement or grandparental death) the student may be misassigned into a certain track. Assignment into a certain track is consequential in terms of the school environment, peers and teachers a student is exposed to. Initial misassignment has the potential to incur a negative penalty on the students development.

Underperforming students are at risk of redoing a year in primary education or shifting into a lower track in secondary education. Overperforming students may shift into a higher track. However, it is also common for these students to stack different tracks on top of one another. In practice, this means that a student graduates from HAVO and subsequently enrols into VWO for the last (two) year(s). CPB (2017) show that typically around 80% of students starting a particular track do in fact finish it, with the fraction being somewhat lower for disadvantaged students. Stacking is also a practice in post-secondary education. For example, HBO students sometimes enrol for an academic (master) degree after they graduate or pass the first year.

2.3.3 Transition from Primary to Secondary Education

The transition and assessment of students from primary into secondary education rests on two pillars. First, there is a standardised test. Second, the primary school teachers provide a subjective advice on the track recommendation. Secondary schools take into account these two signals into account when considering whether to place a certain student into a certain track that they offer.

The standardised test has the potential to correct for biases in the teacher recommendation, on the assumption that the test is a more objective measure. Conversely, the teacher recommendation has the potential to complement the standardised test. The teacher has the discretion to take into account other factors than test scores when advising on the educational track the student should attend, such as behaviour and motivation.

Before 2015, the test was administered in February, before the teacher recommendation. If job displacement decreases test performance only, but the teacher relies on test performance to observe a child's ability, the negative effects of job displacement can still feed into the track recommendation.

From 2015 onwards, the test was administered in April, after the teacher recommendation. The teacher advice could still be amended if the test results suggests that the teacher underestimated the students' ability. In other words, if the test results are better than the teacher advice, the track advice can be adjusted upwards. The reverse does not hold true however. This limits the role that the test plays to a "second chance" with nothing but upside potential. Moreover, you have to actively apply for a revision. Merely those students and parents that received any advice below VWO and are not satisfied with the advice have an incentive to perform well on the test. This renders the results less comparable across students because the stakes differ from student to student. Differences in stakes or incentives impact test performance and do so more for males than females (Schlosser, Neeman and Attali 2019). Part of the 2015 reform was the implementation of the rule that secondary schools are only allowed to accept or reject students on the basis of teacher advice.

Attaining a better result than the school advice suggests grants the right to reconsideration, not to adjustment. Adjustment is within the discretion of the primary school. Swart, Van den Berge and Visser (2019) demonstrate that students with right to reconsideration are disproportionately from low income and educational parental backgrounds. Conditional upon having the right to reconsideration of the school advice, only one-third of the students receive an upwards adjustment. Those with high-income and migration backgrounds have a higher-than-average probability of upwards adjustment whereas children from low educational backgrounds have a lower-than-average probability of upwards adjustment (Swart, Van den Berge and Visser 2019).

The main effect of interest for this research is whether experiencing parental job displacement before the test decreases test performance. Given the information above, this paper also looks into whether the effects differ for those children that made the test after 2015 and whether there are different effects for for male and female children.

Existing literature demonstrates structural differences in how different subgroups score on the standardised test and how they are assessed according to the teacher recommendation. Feron, Schils and Weel (2016) and Timmermans, de Boer and van der Werf (2016) both find that boys and students from lower socio-economic backgrounds are less likely to receive a teacher recommendation that is higher than the test score. Swart, Van den Berge and Visser (2019) find that students with parents whose incomes or education levels are below average also are less likely to receive a teacher recommendation that is higher than the test score.

There are different explanations for these discrepancies. First, the standardised test is a snapshot while the teacher recommendation may take into account other relevant factors. The metaphors used in Dutch education are that of a photo (the test) and a video (the recommendation). There may be differences in how different subgroups perform on these factors. For example, it may be the case that children from higher socio-economic backgrounds are "taught to the test" relatively more than others. Also, teachers may deem parents with higher socio-economic status, educational or income levels better fit at supporting their children when placed into higher tracks. Furthermore, these parents may also be more able in convincing the teacher to give a certain recommendation independent of their ability to support their children.

Second, it may be the case that teachers' expectations differ systematically between different subgroups and that this feeds into the assessment. These expectations may be formed by certain characteristics of the students. Timmermans, de Boer and van der Werf (2016) find that, after controlling for performance and background characteristics, teachers give higher recommendations for those students that they perceive to have higher self-confidence and positive work habits. The authors find that teachers systematically perceive girls to have a better work habit. Moreover, expectations may also be formed by stereotypes. For example, teachers may have lower expectations when it comes to educational performance of those of lower socio-economic status.

Dronkers and Korthals (2016) note that parents can influence track placement indirectly through different strategies. First, parents may help the student prepare for the standardised test so that the performance is increased. Second, parents may try to influence the teacher in order to adjust its advice upwards. Third, parents may apply for different secondary schools up to the moment their child is accepted in the preferred track.

Stans (2020) studies the effects of distress in the context of the same high-stakes test as in this paper. By comparing groups of children that lost their parent just before (treatment) and just after (control) the high-stakes test, the author establishes three results. First, treatment leads to decreased performance on the standardised test. Second, treatment leads to a lower track advice, possibly through classroom behaviour. Third, treatment leads to a higher probability of attendance of and graduation the VMBO track. Moreover, the possibility to resit for the test or switch tracks during secondary education does not completely make up for the initial setback caused by family distress. If the overall effect of job displacement is similar as the overall effect caused by distress caused by the loss of a grandparent, these findings should be consistent with the results of this paper. Stans (2020) does not find any heterogeneous effects across socio-economic background.

3 Empirical Strategy

The first part of this section discusses the method through which this research estimates the effect of displacement on educational outcomes. Second, this section provides an overview of the sources of the data used and elaborates on how the sample fit for estimation is constructed. The third part of this section discusses how job displacement events are identified. The final part concludes with a discussion on the internal validity of this paper's method.

3.1 Method

Estimating the causal effect of parental job loss on the educational outcomes of children is demanding. A comparison between the outcomes of children whose parents were and were not displaced from their jobs would be adequate if job displacement is randomly assigned over parents. This is not the case. Unobservable factors, such as intelligence and resilience, may simultaneously determine the likelihood of parental job displacement and the educational performance of their children. A naive comparison would therefore lead to omitted variable bias.

Repeated observations of educational performance for the same individual are unavailable[1]. This prevents the comparison of the development over time of displaced against non-displaced individuals. Instead, this paper uses single observations of educational outcomes.

The timing of job displacement relative to the standardised test gives the opportunity to overcome these problems. By comparing parents who were displaced from their jobs before their children took the standardised test against parents who were displaced after the test, the question becomes whether the *timing*, and not the *occurrence*, of job displacement is independent from aforementioned unobservable factors. If so, the comparison yields a causal effect of parental job loss on educational outcomes of children.

Therefore, this research relies on a comparison between children whose parents were displaced just before and just after they made their tests in order to credibly estimate the causal effect of job displacement on educational outcomes. Concerns about possible non-randomness of the relative timing of job displacement are addressed in the section on internal validity.

3.1.1 Estimation Approach

This section briefly explains the logic of my estimation approach by means of a potential outcomes framework. Suppose a researcher wants to estimate the effect of job displacement on educational outcome Y of the child of individual i, the researcher ideally compares two states of the world. One state in which individual i is confronted with job displacement (the "treatment" state of the world denoted by T) and another in which individual i is spared from displacement (the "control" state of the world denoted by C).

The (causal) treatment effect of job displacement is equal to the difference in outcomes in the two states of the world ($Y_i^T - Y_i^C$). However, it is impossible to observe these two states of the world simultaneously for the same individual. To circumvent this problem, the researcher compares two groups of individuals that are, on average, comparable to one another except for the treatment. Comparing the average outcomes for both groups then yields an average treatment effect $E[Y_i^T - Y_i^C]$.

[1]This may change in the near future as efforts are currently made by the CBS to make school-level databases (*leerlingvolgsystemen*) fit to link with other micro-data held by the CBS.

In an ideal experiment, individuals get assigned over two groups at random to ensure comparability after which one group is subjected to treatment. This ideal may be approximated through natural experiments. If there would be an assignment rule with a clear cut-off that separates otherwise large and comparable groups of individuals into displaced and non-displaced groups (for example, a firm-wide performance ranking whereby all those performing under a certain level are displaced from their jobs), this rule could be exploited to estimate the effect of job displacement on the educational performance of children. However, no such rule is available given the available data.

Instead, this paper exploits the relative timing of job displacement. While there are no non-displaced parents in the sample, and thus everyone is treated with job displacement, some parents are displaced in the period after the child sat for the standardised test. Their children serve as the control group of this research, as by virtue of chronology it is impossible that the displacement affected their test results. Their outcomes, $E[Y_i^C]$, are compared with the outcomes of children whose parents were displaced before their child made test, $E[Y_i^T]$.

In order for this comparison to yield the causal effect of job displacement, the identifying assumption must hold. The identifying assumption is that the the timing of job displacement relative to the test is unrelated to characteristics of the child. The section on internal validity will cover the credibility of this assumption.

3.1.2 Regression Specification

The following regression specification is estimated where Y_i is set of short- and long-run educational outcomes of the child and T_i is a treatment dummy that is equal to 1 if the parents are displaced from their jobs before the standardised test date and equal to 0 if the parents are displaced from their jobs after the standardised test date. As a consequence, β_1 captures the average effect of job displacement on the educational outcomes of child i. X_i contains a set of control variables.

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \epsilon_i \tag{3.1}$$

Caveats

Initially, the estimation approach in this thesis was based on a regression discontinuity design and estimations were run accordingly. Upon closer examination however, an ordinary least squares estimation turned out to be more appropriate. This realization did not come in time as my access to CBS servers was already revoked. The results presented in the next section are therefore based on regression discontinuity specifications.

The main caveat is that the results are not strictly comparable with one another as they are run on different samples which were chosen via an automatic bandwidth selector. The bandwidth associated with each estimate are reported. The samples are not completely incomparable as the bandwidths range between 130 and 220 days total. This means that for the smallest (largest) sample, groups of children whose parents were displaced in the 65 (110) days before or after the test are compared with one another.

3.2 Data & Sample Construction

This paper is based on a dataset that is constructed from population-wide administrative records[2] of the *Centraal Bureau voor de Statistiek* (Dutch Bureau of Statistics, CBS). The Dutch Bureau of Statistics has firm-level data on bankruptcies that allows for direct and clean observation of mass lay-off events. However, this data was not accessible for me. Furthermore, such data does not allow for the observation of lay-off events due to downsizing instead of firm closure. Instead, this paper infers both downsizing & lay-off events from employment spell data.

As explained in the previous sections, researchers that are interested in the effects of job displacement on educational outcomes have to rely on surveys or administrative data. While surveys can be made to fit a wide range of purposes, they may suffer from recall bias, non-response and a possible lack of statistical power. These issues can be circumvented by use of administrative data on the condition that the data of interest is recorded.

In the Netherlands, population-wide records of educational outcomes have been kept since 2006. It is possible to observe test scores, track advice and track enrolment on a yearly basis for individuals. Leaving track enrolment aside, educational outcomes are only observed once per individual (i.e. there is only cross-sectional variation). This precludes the use of methods common in economics of education research such as value-added models. However, because track enrolment is observable on a yearly basis, it is possible to observe whether the effect of job displacement, via test performance, is consequential for track enrolment in the years after the test.

In order to construct the dataset fit for estimating the effects of job displacement, this research merges two separate datasets. First, it constructs a dataset that records the educational outcomes for all children since 2007, merged with various background characteristics of the child, the parents and the household. Second, it constructs a dataset from which can be inferred whether and when parents have been displaced from their jobs. Afterwards, these datasets are combined in order to arrive at a sample of children whose parents have been displaced from their jobs in the 365 days before or after they did the standardised test. This sample serves as the basis for the estimation approach of this paper.

For the first dataset, the test outcomes are standardised with mean 0 and standard deviation 1. Afterwards, this data is merged with background characteristics such as migration background, sex and month of birth[3]. The child data is then linked with similar parental data. These parent-child linkages also allow to observe the birth order and number of siblings of a specific child. From a household level dataset, further background data is retrieved such as the type of household (institutional, private, single-parent, double-parent, etcetera), home ownership, household income[4], primary source of household income and percentile in the household income distribution.

Moreover, the household dataset allows me to verify children do in fact live with their registered parents. If neither of the registered parents is among the household members, these observations are dropped from the dataset. If the household is not registered as a single-parent household but there is only one registered parent among the adult household members, these observations are also dropped from the dataset.

[2] See here for the access procedure to Dutch microdata (in English). See here for a catalogue of the available data (in Dutch).

[3] the exact date of birth is censored

[4] Which I adjust for inflation by using the national inflation index with base-year 2010

The dataset *Stelsel van Sociaal-statistische Bestanden* contains data on the universe of Dutch employee-employer relationships from 1999 onwards. Start & end dates and firm & worker identifiers allow for the construction of a panel with the both the absolute and full-time equivalent number of workers per firm on a monthly basis for every active Dutch firm from 1999 onwards. Van den Berge, Koot and Zweerink (2020) raise two potential problems with the employment spell data used in this paper.

First, during 2005 and 2006, the Dutch Bureau of Statistics changed the data collection process by replacing the information source, *Belastingdienst* (Dutch Fiscal Services) with the *UWV* (Dutch Labour Agency)[5]. The process was not free of error and resulted in incomplete matches between the employment spells from both sources. As such, official employment spell data between 01-01-2005 and 31-12-2006 is imputed. In order to address this problem, this research uses data from 2007 onwards.

Second, starting in 2010, the Dutch Bureau of Statistics changed the way they treated large firms with divisions operating in different markets[6]. In 2009 and preceding years, these divisions were identified with distinct identifiers. Starting in 2010, this was no longer the case. Not taking this into account would result in 'false positive' firm closure events whereas in reality the change was merely administrative. This problem is addressed in the next section.

The employment spell data is enriched with information on total yearly earnings; days worked per year and type of labour contract. Amongst others, this allows me to determine whether the employment spell was the most important (in terms of earnings[7]) or most intensive (in terms of days worked) for the individual concerned.

3.3 Job Displacement

For the purposes of this paper, job displacement is understood as involuntary and unanticipated job loss. Because the reasons why an employment spell ends are not merely involuntary, this is not sufficient to observe job displacement. It could for example be the case that an employee voluntarily terminates their labour contract. Therefore, this paper relies on a different method of isolating involuntary job losses from the employment spell data. Following common practice in the literature (see Rege, Telle and Votruba (2011) and Van den Berge, Koot and Zweerink (2020) for example), this paper relies on firm closures or mass lay-off events (hereafter referred to as displacement events).

3.3.1 Observation

In order to establish whether the firm f experienced a displacement event, this paper uses firm-level panel data with full time equivalent employment levels at the start of the month. The firm downsizing rate $\Delta L_f, m$ for firm f in month m is calculated as follows:

$$\Delta L_{f,m} = \frac{FTE_{f,m+1} - FTE_{f,m}}{FTE_{f,m}} \tag{3.2}$$

First, this research establishes whether the magnitude of the lay-off event is sufficient. The sample only includes firms for which $\Delta L_{f,m} < -30$. Job displacement events can then be categorised as mass

[5]This is known as *Operatie Walvis* in Dutch

[6]This is known as the *BEID+* classification of firms

[7]Which I adjust for inflation by using the national inflation index with base-year 2010

lay-offs, were up to 70% of the workers are retained, and closures, were all of the workers are fired. The choice for the cut-off is somewhat arbitrary. The motivation to put the cut-off at $\Delta L_{f,m} < -30$ is a trade-off between preventing endogenous downsizing events (i.e. where the displacement of parents is related to their ability) from entering into the sample on the one hand and preserving sufficient observations on the other hand. Moreover, the cut-off at $\Delta L_{f,m} < -30$ is in line with earlier Dutch job displacement literature that uses downsizing to establish displacement events (Van den Berge, Koot and Zweerink 2020).

Second, this research establishes whether worker i was in fact affected and not retained. I check whether the employment spell of worker i ended in the same month as the lay-off event took place. While unnecessary for firms that experience closure (i.e. where $\Delta L_{f,m} = -100$), this ensures that no false positives are identified among the workers of firms that experience mass lay-off but not closure (i.e. where $\Delta L_{f,m} \neq -100$ & $\Delta L_{f,m} < -30$).

Third, this research establishes whether firm f was stable in the period leading up to the displacement event. This ensures that firms that experience seasonal swings do not enter into the sample. The sample is restricted to firms that did not experience contractions in the three preceding years, defined as $\Delta L_{f,m}$ exceeding -10%.

Finally, this research checks whether worker i has a strong connection with his employment by dropping workers whose tenure is shorter than a year.

3.3.2 Patterns

Because the empirical strategy exploits the timing of job displacement around the standardised test date, patterns in the timing of job displacement are a potential threat. For example, if a firm that employs a large group of higher-educated parents, treatment and control groups may not be comparable. This section describes the necessary steps to investigate and correct for such patterns more closely. Unfortunately, CBS data protection guidelines prevent the publication of the associated figures.

Plots with the frequency of job displacement over the year show clear end-of-month and end-of-year spikes in job displacement. Because the sample consists of otherwise stable firms (i.e. that did not experience contractions larger than 10% in the past three years), it is unlikely that these are seasonal effects. The end-of-month spikes may be driven by the fact that employment spells end on the last of the month. The end-of-year spikes may be driven by administrative changes that are implemented at the end of the year. Recall that employment spells are summed by firm identifier. If the firm identifier changes, but the firm itself does not cease to exist, this may result in a false positive firm closure. Similarly, it may be the case that firms are administratively split or merged at the end of the year. On the other hand, there also seems to an end-of-year surge in mass lay-offs. It is reasonable to assume that the driving economic forces that put firms into jeopardy co-determine mass lay-offs and closures. Therefore, there may be some true and false positives among the end-of-year firm closures.

In order to correct for this, observations that fulfil the following conditions are dropped: displacement is identified, the employment spell ends at the last day of that year, the subsequent employment spell starts the first day of the next year. After visually checking the results of these corrections, it seems that they drove the spikes of job displacement throughout the sample. However, the figures still show some specific displacement events. Such outliers may bias the results. In a further effort to correct for such outliers, the job displacement statistics are decomposed by sector and employment bracket so that it can visually be checked whether there are displacement events in specific sectors that are large enough in magnitude to bias the treatment and control groups. Again, CBS guidelines prevent me from reporting these decompositions because that would infringe upon the anonymity of the businesses concerned.

The sectoral decomposition shows that there are some sectors in which there is a single displacement event that generates a very large number of displaced parents. These sectors are dropped from the sample.

3.3.3 Descriptives

Finally, this research matches the subset of displaced workers with the dataset containing child, parent and household data, conditional on the displacement taking place 365 days before or after the child made the test. If the timing of job displacement around the standardised test date is as good as random, the group whose parents were displaced before the test (treatment) should be comparable in terms of covariates to the group whose parents were displaced after the test (control group, see the right column of table 3.1 below).

The statistically significant differences between treatment and control groups indicate that the relative timing of job displacement is not as good as random across the full width of sample. In other words, the families that experience job displacement in the year before the test are not exactly comparable to the families that experience job displacement in the year after the test.

Therefore, an ordinary least squares (OLS) regression over the full width of the sample would not credibly estimate the causal effect. Instead, a comparison of families that experience job displacement closer around the test date would lead to better comparability. This is the reason why this research estimates the regression specification on a restricted sample of displacements taking place just before or after the test.

Table 3.1: Overall Descriptive Statistics

Variable	Treatment		Control	
	Mean	SD	Mean	SD
Child Covariates				
Overall Score	0.07	0.97	0.09	0.96
Math Score	0.05	0.98	0.07	0.97
Language Score	0.07	0.97	0.08	0.96
Native	0.85	0.36	0.84	0.36
Male	0.50	0.50	0.50	0.50
Age	12.03	0.47	12.02	0.47
Siblings	2.41	0.92	2.41	0.93
First-born	0.98	0.13	0.98	0.13
Household Covariates				
Single Parent Family	0.06	0.24	0.07	0.26
H ousehold Disposable Income	50658.77	47179.87	51479.36	32575.91
Home Ownership	0.86	0.35	0.86	0.35
Rental without Assistance	0.06	0.23	0.07	0.25
Rent with Assistance	0.08	0.27	0.07	0.26
Father Covariates				
Age	45.50	4.70	45.52	4.73
First Gen Mig	0.11	0.31	0.11	0.32
Employed t	0.86	0.34	0.86	0.35
Mother Covariates				
Age	43.04	4.17	43.02	4.19
First Gen Mig	0.11	0.31	0.11	0.32
Employed t	0.78	0.42	0.78	0.42
Observations	49,173		54,676	

3.4 Internal Validity

The main identifying assumption is that the timing of job displacement relative to the test is unrelated to factors that determine the child's test performance. There are several factors that add to the credibility of this assumption and therefore to the internal validity of the estimation approach of this paper.

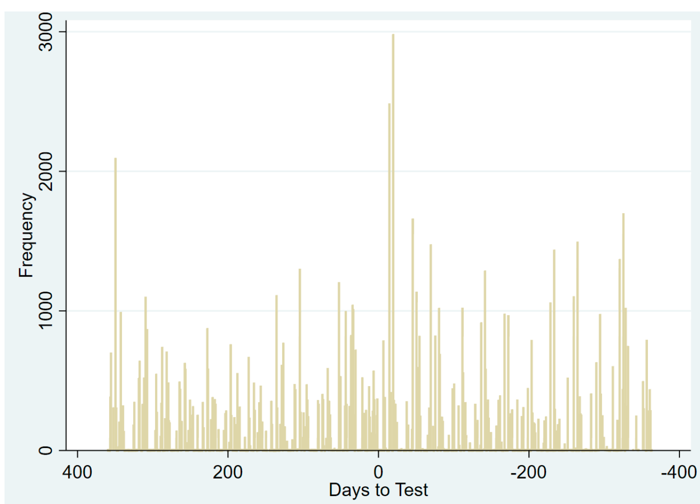
First, the displacement events that are used are determined by economic downturns and therefore plausibly exogenous to the worker. The sample is restricted to firm closures or mass lay-offs in which at least 30% of the workers are displaced. In contrast to individual dismissals, this ensures that the displacement decision is not driven by concerns about the ability of the individual worker. This adds to the internal validity because the ability of the parent may (in)directly drive test performance.

Second, it is unlikely that parents have control over the timing of the test date. The test dates are fixed on a national level. This is a safeguard protecting the validity of test results. It prevents the spread of information on question and answers among test-takers but it also strengthens the estimation approach of this paper.

Third, seasonal effects are partly controlled for. It may be the case that parents who are displaced in March are different from parents who are displaced in December. This could be the result of the concentration of displacement events in certain sectors in certain seasons combined with differences in characteristics of workers in across sectors.

This threat is addressed twofold. First, displacement events are decomposed by sector. If displacement events are concentrated either among the control or the treatment group, this sector is dropped from the sample. See section 3.3.2 for further explanation. Second, the sample is restricted to firms that are relatively stable and did not experience any month-on-month change greater than 10% in the three years preceding the displacement event. However, it may still be the case that there are (un)observed differences amongst parents displaced in different months.

The following figure shows the timing of job displacement relative to the standardised test date.



3.4.1 Balance in Covariates

Significant differences in factors unrelated to the relative timing of the displacement event may bias the results. They can be indications that the treatment and control groups are incomparable. Such factors may include background characteristics such as age, migration background, household type and household disposable income. On average, these should not differ significantly between treatment and control groups.

This paper checks whether this is the case by running a regression with these characteristics as outcomes. The results show that there are significant differences between the individuals in the treatment group and the control group. The treatment group consists of younger parents and a higher share of non-natives. Moreover, their socio-economic status is different as the treatment group has a lower household income, consists of a higher share of renters in the social sector and a lower share of home-owners. Furthermore, their main income is derived more often from social assistance and less often from profits or wages relative to the control group.

These differences indicate that assignment to treatment is not as good as random. Moreover, the socio-economic differences in particular may give rise to disparities in educational outcomes, similar to the treatment with parental job displacement. This troubles interpretation of the results as it is impossible to disentangle the relative contributions of socio-economic differences and the treatment effect to possible disparities in educational outcomes.

Table 3.2: Balancing Tests

Variable	Estimate	SE	Observations
Child Covariates			
Native	-0.116**	0.0477	103,849
Age	0.0261	0.0275	103,849
Male	−0.0132	0.0115	103,849
Siblings	0.0256	0.0358	103,849
Firstborn	0.00454	0.00330	103,849
Household Covariates			
Household Size	0.0414	0.0410	101,642
Double Parent Household	−0.00249	0.0147	101,642
Single Parent Household	−0.00367	0.0130	101,642
Disposable Household Income	−6,555**	2,621	101,642
Home Owners	−0.0752**	0.0381	101,642
Renters	−0.00148	0.0176	101,642
Social Renters	0.0715***	0.0246	101,642
Main Income Source			
Wage	−0.494***	0.128	100,481
Profits	−0.0121**	0.00599	100,481
Wealth	−0.000829	0.000622	100,481
UI	0.00416	0.00302	100,481
DI	-0.000305	0.00134	100,481
Social Assistance	0.00669**	0.00306	100,481
Father Covariates			
Age	−0.841***	0.277	102,540
Native	−0.0914**	0.0455	102,540
1st Gen	0.113**	0.0455	102,540
Mother Covariates			
Age	−0.996***	0.320	103,816
Native	−0.121***	0.0454	103,816
1st Gen	0.118**	0.0467	103,816

4 Results

This section presents the empirical results of job displacement on short and long-run educational outcomes. Thereafter, it outlines the robustness checks of these outcomes. Thereafter, it outlines the robustness checks of these outcomes. This section concludes with a discussion on the heterogeneity of results.

Every table in the remainder of this section contains estimates from specifications in which the standard errors are clustered on the firm level. If assignment to treatment is done on a cluster-by-cluster basis rather than a individual-by-individual basis, this can be a reason to cluster the standard errors as it is imposes less restrictions on the regression by allowing for correlation of the error terms within a cluster. For the purposes of this paper, assignment to treatment is done on a firm-by-firm basis, because displacement events take place on the firm level. Clusters of children whose parents work at a specific firm are collectively assigned to either control or treatment groups on the basis of the relative timing of the displacement. As such, it is appropriate to cluster the standard errors on the firm level.

4.1 Test Scores

The following table presents the treatment effects on overall, language and maths test scores respectively. Test scores have been normalized with mean 0 and standard deviation (SD) 1. For the children that experience job displacement around the test date, the job displacement event taking place before the test date decreases the result with 0.13 SD for the overall score and 0.14 SD for the language score. The estimated effect on the math score has a similar sign but is insignificant. These results indicate that parental job displacement in the 134 days preceding the standardized test exerts a negative effect on language and thereby total scores.

In magnitude, the effect on overall test score is more than four times larger than the preferred estimates of Stans (2020) who found that grandparental death in the months leading up to the test decreases overall test scores with 0.03 SD. Consistent with my estimates, the author found that the effects are more pronounced for language (-0.034 SD with $p < 0.05$) than for mathematics (-0.02 SD with $p < 0.1$).

There are different possible explanations for the disparity in effect size. First, the preferred specifications in Stans (2020) include controls for a battery of covariates while the results I presented do not include any controls, suggesting that my results are an overestimation. Second, it may be the case that distress arising from grandparental death is less pressing than the distress arising from job displacement as this event takes place one familiar degree closer to the child. Moreover, in the case of job displacement both the event and the subsequent status of being unemployed may be complementary sources of distress. Third, it may be the case that the my estimates are biased because assignment over treatment and control groups is not as good as random.

Table 4.1: Estimates of the effect of Job Displacement on Test Scores

	(1) Total	(2) Language	(3) Maths
Treatment	-0.134*	-0.140*	-0.105
	(0.0778)	(0.0761)	(0.0679)
Observations	103,849	103,849	103,849
Bandwidth	134.1	131.6	136.8
Controls	None	None	None
SE clustered on	Firm	Firm	Firm

4.2 Teacher Advice

The following table presents the treatment effects on the probability of receiving a certain track advice. Children that experience displacement before the test are 4% more likely to receive a VMBO advice, 3% more likely to receive a VMBO/HAVO advice, 1% more likely to receive a HAVO advice and 4.3% less likely to receive a VWO advice. These results indicate a clear downward shift in the track attendance.

There are two explanations for these results. First, it may be the case that the teacher advice is partly based on test performance. In 7 out of the 10 years that I include in my sample, the teacher advice is given after the test result is known. Second, it may be the case that the teachers base their recommendation on test performance but that parental job displacement may cause problematic classroom behaviour that does feed into the track recommendation. These explanations may hold simultaneously.

In magnitude, these effects are larger than those found by Stans (2020) who found that grandparental death in the months leading up to the test decreases the probability of receiving a VWO advice with 0.8% and increases the probability of receiving a VMBO advice with 1.6%.

Table 4.2: Estimates of the effect of Job Displacement on Teacher Advice

	(1) =< VMBO	(2) VMBO/HAVO	(3) HAVO	(4) HAVO/VWO	(5) VWO
Treatment	0.0312	0.0332**	−0.0528*	0.0166	−0.0456*
	(0.0381)	(0.0142)	(0.0225)	(0.0189)	(0.0262)
Observations	61,452	61,452	61,452	61,452	61,452
Bandwidth	149.8	202.6	168.0	216.4	145.1
Controls	None	None	None	None	None
SE clustered on	Firm	Firm	Firm	Firm	Firm

4.3 Track Enrolment

The effects of experiencing job displacement over the longer run are less clear and mostly insignificant with some exceptions. 6 years after making the test, those who experienced parental job displacement have a 7% lower probability of attending VWO, a 5% lower probability of attending HBO and a 12% higher probability of attending MBO. This is an indication that the combination of a lower track advice and test score may have persistent effects. In other words, experiencing job displacement or other distressing events just before a high-stakes test could create a path-dependency.

However, if that were the case we would expect to see consistent results in the 4 and 6 years after the transition into secondary school. While the signs of the estimates are consistent with the explanation given above, we would also expect to see significantly affected probabilities in the 4 years after the transition year, which we do not.

In magnitude, these effects are larger than those found by Stans (2020) who found that grandparental death in the months leading up to the test decreases the probability of VWO track attendance with 1% and increases the probability of VMBO track attendance with 1% four years after the transition to secondary school.

Table 4.3: Estimates of the effect of Job Displacement on Long Run Educational Track Enrolment

	(1) =< VMBO	(2) HAVO	(3) VWO	(4) MBO	(5) HBO	(6) WO
+ 4 years:	0.0743	−0.0366	−0.0403	0.00410	−0.000672	−0.000353
Obs: 85,147	(0.0464)	(0.0225)	(0.0302)	(0.00278)	(0.000704)	(0.000300)
+ 6 years:	0.00136	−0.0107	−0.0664**	0.118**	−0.0479**	−0.000432
Obs: 62,771	(0.00275)	(0.0157)	(0.0325)	(0.0574)	(0.0241)	(0.000538)
+ 8 years:	−0.00191	−0.00371	−0.0116*	0.0825	−0.0327	−0.0322
Obs: 43,443	(0.00127)	(0.00552)	(0.00616)	(0.0691)	(0.0551)	(0.0309)
+ 10 years:	0.00221	0.00290	0.00500	0.0556	−0.0503	−0.0133
Obs: 18,321	(0.00196)	(0.00395)	(0.00449)	(0.0486)	(0.0354)	(0.0495)
Controls	None	None	None	None	None	None
SE clustered on	Firm	Firm	Firm	Firm	Firm	Firm

4.4 Alternative Specifications

In this section I present the results of running the regression with alternative specifications. First, I present the results split up according to the gender of the displaced parent. Second, I present outcomes for a range of different specifications to see whether the results are robust.

4.4.1 Parental Gender Heterogeneity

The following table presents the result of the regression across sub-samples split-up according to parental gender. While the signs of the estimates are similar, the results indicate that paternal job displacement exerts a larger significant effect on test performance while maternal job displacement exerts a smaller insignificant effect. The results for teacher advice are similar. Essentially, this means that the combined effects of increases in leisure time, decreases in income and increased psychological distress cancel each other out in the case of mothers, but not in the case of fathers. This is consistent with the literature and there are different explanations possible. For example, there may be increased distress associated with paternal displacement as compared to maternal displacement due to societal norms. It may also be the case that the decrease in income is not as steep or better cushioned by an increase in time spent with the child for women than for men.

Table 4.4: Estimates of the effect of Job Displacement on Test Scores

	(1) Total	(2) Language	(3) Maths
Fathers	-0.196^{**}	-0.201^{**}	-0.151^{*}
	(0.0883)	(0.0870)	(0.0787)
Observations	49,171	49,171	49,171
Bandwidth	133.0	133.7	133.7
Mothers	-0.0871	-0.0788	-0.0815
	(0.0853)	(0.0823)	(0.0764)
Observations	54,678	54,678	54,678
Bandwidth	143.7	139.3	149.8
Controls	None	None	None
SE clustered on	Firm	Firm	Firm

Table 4.5: Estimates of the effect of Job Displacement on Teacher Advice split by Parental Gender

	(1) =< VMBO	(2) VMBO/HAVO	(3) HAVO	(4) HAVO/VWO	(5) VWO
Fathers	0.0879^{**}	0.0397^{**}	-0.0408^{**}	0.0120	-0.719^{**}
	(0.0431)	(0.0195)	(0.0220)	(0.0208)	(0.0300)
Observations	29,111	29,111	29,111	29,111	29,111
Bandwidth	140.2	177.8	197.0	189.1	141.5
Mothers	-0.0208	0.0162	-0.0472	0.0217	-0.0155
	(0.0380)	(0.0174)	(0.0317)	(0.0266)	(0.0307)
Observations	32,341	32,341	32,341	32,341	32,341
Bandwidth	193.5	219.0	159.8	183.5	169.3
Controls	None	None	None	None	None
SE clustered on	Firm	Firm	Firm	Firm	Firm

4.4.2 Robustness

The next page contains two tables with the effects of job displacement on test scores and on the teacher advice in different specifications. The results on test scores are robust to the inclusion of different sets of fixed effects and clustering the standard errors by firm. However, including sectoral fixed effects renders the treatment effect insignificant and changes it sign. Moreover, including year fixed effects renders the reversed sign effect significant.

Fixed effects take out the variation from the results driven by effects that are constant by year fixed by sector. Essentially, the estimate is a weighted average of year-by-year and sector-by-sector comparisons. An example of one such comparison is the differences in outcomes of children whose parents were displaced from the financial sector in 2010. This may be an indication that the results are being driven by unobservable differences in the treatment and control groups and not the effect of interest, parental job displacement.

As explained earlier, it makes sense to cluster the standard errors on the firm level. The following table demonstrates that most estimates remain significant with robust standard errors, albeit with an increased probability that the null hypothesis of no effect is wrongfully rejected. For the specification without control variables, the effect on the math test score turns insignificant. For the specifications with control variables, the effects are consistent with and without clustered standard errors, except for the specification with fixed effects for firm size classes.

Table 4.6: Effect of Job Displacement on CITO-scores and Teacher Recommendations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	Observations
Total:	−0.124***	−0.134*	−0.107***	−0.127*	0.0970*	0.0697	0.109**	0.102**	−0.0947**	0.109	103,849
	(0.0356)	(0.0778)	(0.0367)	(0.0718)	(0.0498)	(0.0508)	(0.0431)	(0.0490)	(0.0371)	(0.0790)	
Language:	−0.124***	−0.140*	−0.0980***	−0.118*	0.0690	0.0627	0.107**	0.106**	−0.106***	−0.105	103,849
	(0.0365)	(0.0761)	(0.0370)	(0.0700)	(0.0467)	(0.0523)	(0.0439)	(0.0512)	(0.0373)	(0.0791)	
Maths:	−0.0933***	−0.105	−0.0944**	−0.108*	0.0629*	0.0528	0.0730**	0.0679	−0.0724**	−0.0858	103,849
	(0.0357)	(0.0679)	(0.0366)	(0.0632)	(0.0379)	(0.0467)	(0.0353)	(0.0447)	(0.0367)	(0.0677)	
<= VMBO:	0.0398*	0.0312	0.0433**	0.0430	−0.0579***	−0.0593***	−0.0549***	−0.0528**	0.0392*	0.0292	61,428
	(0.0216)	(0.0381)	(0.0218)	(0.0358)	(0.0200)	(0.0228)	(0.0200)	(0.0221)	(0.0219)	(0.0385)	
VMBO/HAVO:	0.0328**	0.0332**	0.0144	0.0144	0.0318**	0.0326**	0.00188	0.00187	0.0298**	0.0300**	61,428
	(0.0133)	(0.0142)	(0.0133)	(0.0137)	(0.0133)	(0.0136)	(0.0112)	(0.0111)	(0.0133)	(0.0143)	
HAVO:	−0.0522***	−0.0528**	−0.0206	−0.0203	−0.0266*	−0.0265*	0.00295	0.00281	−0.0531***	−0.0513**	61,428
	(0.0191)	(0.0225)	(0.0152)	(0.0169)	(0.0146)	(0.0161)	(0.0120)	(0.0169)	(0.0189)	(0.0218)	
HAVO/VWO:	0.0172	0.0166	0.00278	0.00255	0.0515***	0.0519***	0.0341**	0.0327**	0.0190*	0.0189	61,428
	(0.0106)	(0.0189)	(0.0130)	(0.0161)	(0.0140)	(0.0170)	(0.0143)	(0.0158)	(0.0104)	(0.0180)	
VWO :	−0.0428**	−0.0456*	−0.0402**	−0.0416	−0.000955	−0.00229	0.0134	0.0136	−0.0327*	−0.0371	61,428
	(0.0178)	(0.0262)	(0.0181)	(0.0270)	(0.0167)	(0.0216)	(0.0169)	(0.0208)	(0.0184)	(0.0270)	
Controls:	none	none	year FE	year FE	sector FE	sector FE	year & sector FE	year & sector FE	size FE	size FE	
SE:	robust	clustered firm	robust	clustered firm	robust	clustered firm	robust	clustered firm	robust	clustered firm	

Results from a regression specification with parental job displacement before the standardized test as treatment.
Effects denote the change in overall, language and math standardized test scores resulting from being displaced before the standardized test took place.
Standardized test scores are normalized with mean 0 and SD 1. * 0.05, ** $p < 0.01$, *** $p < 0.001$.

5 Conclusion

Notwithstanding concerns with respect to the internal validity of the research set-up, the results indicate that parental job displacement has a negative effect on the educational performance of the child. Paternal job displacement exerts a consistent negative effect while maternal job displacement is similar in sign but insignificant. This holds for both the teacher assessment and test scores. Both these results are in line with the literature. These differences may be explained through gender-differences in social norms, income or aptitude at non-monetary investment in the child.

The spill-over effect of parental job displacement onto the child may persist through the interaction with education systems. More specifically, parental job displacement in the context of a high-stakes test can affect children that would otherwise be on the margin of being placed in a different educational track. In the Dutch system, a different track may expose the child to different expectations, a different curriculum and different peers. The combination of all these effects can set the child up for a different path in life, merely because of the exact timing of their parents' job displacement.

If parental job displacement is concentrated amongst specific populations, ethnicities, regions, sectors or socio-economic groups, it may also widen the inequality of opportunity between the displaced population and the population at large.

There are concerns about the internal validity of the estimation approach because the treatment group is socio-economically weaker than the control group. It is impossible to determine if and to what extent the effects can be attributed to treatment with job displacement or socio-economic differences. Notwithstanding these concerns, there are some implications for policy that arise out of my results.

Spill-overs of job displacement onto children may justify to (further) subsidize employment in economic downturns. The (marginal) costs of these subsidies should weighed against the (marginal) benefits in terms of foregone increases in inequality and foregone decreases in human capital investment.

Moreover, the educational system can be improved in its robustness and how it shields children from shocks. This could be by devoting special attention to children that are at risk, for example upon notification of the parents or municipal services. Another strategy of dealing with this problem is to spread-out the risk of adverse events. Instead of a single standardized test, tests could be administered on regular intervals over a longer period, so that the effects of adverse events just before a test do not weigh as heavily on the end result.

The question of interest was whether and to what extent parental job displacement around high-stakes tests influences the life of the child. Future research may tackle this question through other avenues than presented in this paper.

First, the CBS has firm-level data on bankruptcies that allows for cleaner observation of firm closures. This may be combined with closures and lay-offs inferred from employment spell-data, as is done in this paper, for a robustness check.

If this suggestion properly addresses the internal validity concerns raised in this thesis, the effects of parental job displacement in the context of high-stakes testing can be tested for broad set of outcomes, as long as they are recorded in Dutch administrative micro-data. These include medicine and (mental) healthcare uptake, criminal records, employment levels, social assistance and income. This would allow for a broader perspective on the costs of adverse events around high-stakes tests.

Second, as explained in section 4.1, the results presented are based on estimates with automatic bandwidth selector. If possible, it would have been preferred to base all the estimates on a single sample of parents that lost their jobs in the three months before and three months after the test.

Third, the estimation approach used in this thesis may be replicated thereby controlling for income and wealth losses. Differential results across parental gender may partly be driven by the differential impacts of job displacement on wealth and income. This would also shed some light on the relative contribution of gender differences other than income, such as social norms and psychological distress.

Fourth, there are some regional projects[1] in the Netherlands that collect repeated observations of different indicators for educational performance for the same individual. Conditional on sufficient and plausibly exogenous parental job displacement, it would be possible to estimate the effect of displacement with a difference-in-difference research design. If different methods reach similar conclusions, this could add to the credibility of the conclusions drawn. Moreover, it would be possible to estimate the effects on other educational outcomes than the high-stakes test.

[1] OnderwijsMonitor Limburg and educational data collected by the municipality of Amsterdam

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