When Parents Work from Home *

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Abstract

Using administrative data from the Netherlands and information on firm-specific labor agreements, this paper estimates the causal effect of parental work from home (WfH) rights on their children's educational attainment. While we find no evidence that WfH rights affect parents' labor market outcomes, they improve children's exam scores by 0.09 standard deviations at a high stake exam. Linking collective agreements to labor force survey data, we show that WfH policy changes increase WfH propensity by 15 percentage points—nearly doubling it. These results highlight the large potential benefits of remote work for supporting families and their children.

Keywords: Working from Home, Test scores, Work-life balance, Remote Work, Teleworking, Work Flexibility
JEL Codes: I20, J13, J22

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1 Introduction

While there is some backlash against remote working, the current expectation is that remote work will remain a permanent feature of modern labor markets (Bick et al., 2023; Chen et al., 2023). In 2022, 58% of US workers had the opportunity to work from home either part-time or full time (McKinsey, 2022) and by 2023, 28% of days were worked from home in the US (Barrero et al., 2023), with little sign of a complete return to the office. While the literature has mostly described this recent trends and investigated issues of workers productivity, this paper focuses on the externality on the children of parents working from home.

Many studies on WfH to date are descriptive and cannot be interpreted causally as employees self-select into work arrangements that suit their needs and those of their families. What is clear is that WfH is viewed by workers as a positive amenity. Eliciting willingness to pay Aksoy et al. (2023) or Cullen et al. (2025) estimates that workers value teleworking at between 5% to 25% of their wage, as they value reduced commuting and better work-life balance.

The backlash against WfH is about its impact on productivity, promotion prospect and wellbeing of isolated workers. Studies based on firm-specific randomized controlled trials (Bloom et al., 2015; Angelici and Profeta, 2024; Atkin et al., 2023) or COVID-19 related natural experiments (Choudhury et al., 2021; Gibbs et al., 2023; Emanuel and Harrington, 2024) report mixed evidence ranging from -20% to +10%, highlighting that any effect on workers' productivity might be firm or occupation specific, and partially driven by workers' selection. Few studies have estimated the causal effect of WfH on health and well-being (Angelici and Profeta, 2024; Bellmann and Hübler, 2021; Costi et al., 2024; Goux and Maurin, 2024) again providing mixed evidence on the effect of Wfh.

We add to this literature on the causal effect of WfH by investigating its effects, not on the workers themselves but on their family members, specifically their children. Our focus in on the Netherlands, the European country with the highest share of workers teleworking at least some days (52%). Work from home is often described as a "family-friendly pol-

icy", because of its potential to help in achieving work-life balance, especially for parents.[1] Increased organizational flexibility by definition reduces constraints to the allocation of time between work and family. Descriptive studies based on time-use or surveys have documented that teleworkers spend more time on leisure and household production activities, and more time with family on days they work from home (Giménez-Nadal et al., 2018; Pabilonia and Vernon, 2022; Aksoy et al., 2023). Increased parental interactions would benefit children directly, via improved cognitive development (Gupta and Simonsen, 2010; Bernal and Keane, 2011; Fiorini and Keane, 2014; Gupta and Simonsen, 2016; Fort et al., 2020) and thereby educational outcomes. Additionally, working from home might allow parents to supervise their children at lower costs; for example checking that they are back home and doing their homework, while still working (Aparicio Fenoll, 2022), or directly tutoring them. This paper focuses on the externalities of a family friendly policy like remote working on other family members, assessing whether having parents working from home has an impact on their children's educational achievements.

We leverage exogenous variations in the introduction of Wfh rights in Collective Labor Agreements (*Collectieve Arbeidsovereenkomst*) signed between workers' representative and firms (or employers' organisations) in the Netherlands. Collective Labor Agreements (CLA) codify the rights of workers, including where working hours can be conducted. The identification strategy exploits firm-level variations in the introduction of formal WfH arrangements in CLA in the Netherlands, in the two decades prior to the COVID-19 pandemic.[2] To account for selection of treatment, firms formally granting their workers the right to work from home are matched to firms in the same sector with similar characteristics that do not formally recognize Wfh in their CLA. Having identified a set of treated and control firms, we use the national employer/employee register, and select

[1] WfH is often mentioned in the context of gender disparities in the labor market, possibly as an effective way to reduce gender disparities in the labor market (Mas and Pallais, 2017; Nagler et al., 2024).

[2] Our analysis focuses on the pre-pandemic period to avoid conflicting changes in WfH arrangements with other effects of the pandemic, especially the closing of schools and the move to on-line teaching.

employees already in place at least one year before the implementation of the agreement, who unexpectedly gain more flexibility over their work schedule; this is our treatment group. The control group consists of people working in matched firms, which did not implement such clause. We then restrict the sample to parents of children between the ages of 8 and 18. There might still be remaining concerns that treated parents differ in unobservable characteristics that might be related to their preference for supporting their child's education. For example, parents with a greater preference for investing in their children might have encouraged the firm to include Wfh agreements in the renewed CLA. To alleviate those, we take advantage of a feature of the Dutch educational system: test-based school tracking. During their last year of primary school - age 12 - Dutch children sit a national standardized test (CITO) which largely determines the track that children will be allow to attend in secondary education. It is thus a very salient, high-stake exam. Since workers are unlikely to influence the exact timing of any change to CLA, and even less whether the implementation takes place before or after their child had to set, the allocation to treatment to the child is as good as random.

Our identification comes from comparing the CITO score of children that were young enough to benefit from additional parental time investments when WfH was granted, with those of children whose parents also were granted WfH but were older and had already sat the CITO. We compute the same difference in untreated firms to account for any age specific time effects, and implement a difference in difference strategy. This identification strategy allows us to estimate the Intention to Treat of being granted the right to work from home on children's educational attainment.

We complement the analysis by linking the Collective Labor Agreements data to the Labor Force Survey. This allows us to assess that changes in collective agreements do not just formalise previous behaviours regarding Wfh and do actually increase the propensity of treated parents to work from home. Following a similar identification strategy, we can also estimate whether treated parents differ in their labour market outcomes, and thus potential suffer from trade-off of a family friendly policy.

We find that children whose parents gained the right to work from home score an additional 9% of a standard deviation at the CITO, both in math and Dutch. While there is no effect for high achievers, pupils in the middle of the distribution become 4 percentage points more likely to score higher than the threshold that would place them in the vocational track. There is little heterogeneity in the size of these effects. Following the revised CLA, workers become 15 percentage points more likely to report working from home. The change in CLA does not solely reflect de-facto arrangements but real behavioural changes regarding work arrangement, confirming the plausibility of our identification strategy. Parents working in firms that granted WfH have no negative labour market outcomes in term of hours or wages, and have similar mobility patterns as other parents. There appears to be no cost to the parents or firms and large gains to the children when their parents work from home.

To our knowledge, this is the first paper presenting causal evidence on the impact of Wfh arrangements on children's outcomes.[3] There is a relatively large literature on the impact of parental leave policies on parental careers (see Kleven et al., 2020, for a review), and a growing but smaller literature on the impact of these policies on children's developmental outcomes (Ginja et al., 2020; Powell et al., n.d.), but there is almost none on the impact of work flexibility arrangements on children's educational attainment. Work from Home arrangements are important to examine because they are likely to be relevant over a much longer period than parental leave policies, and offer parents some flexibility to invest in the education of their children, at the margin, at some critical ages.

[3]Persson and Rossin-Slater (2024) use a Swedish reform increasing access to workplace flexibility to fathers following the birth of a child and estimate that increasing paternal work flexibility reduces maternal health complications and improve the mother's wellbeing.

2 Related Literature

Previous studies have examined the *causal* impact of Wfh on workers' outcomes. Angelici and Profeta (2024); Atkin et al. (2023); Bloom et al. (2015); Choudhury et al. (2021) all rely on randomised control trials in single firms, whereby workers are randomly allocated to work at home or in the office for a period of time. Despite the similarity in design, their findings on workers' productivity differ substantially. In Bloom et al. (2015) call center workers at a large Chinese travel agency randomized to work from home for 9 months saw their productivity increase by 13% with 2/3rd of the increase driven by increased working time. Similarly, Angelici and Profeta (2024) and Choudhury et al. (2021) estimate increase in productivity of up to 4% for workers allocated to an hybrid work model in Italy and the U.S. respectively. On the contrary, Atkin et al. (2023) find that Indian employees conducting data entry are 18% less productive when randomized to work from home, with 1/3rd of the effect driven by reduced learning. The differences in outcome could be driven by the type of tasks (routine vs creative), organization (team vs single) or intensity of the working from home (hybrid vs full time).[4] Yang et al. (2022) and (Gibbs et al., 2023) point to reduced collaborations driven by higher communication and coordination costs, as major factors in the drop of productivity when working from home.

Another set of studies (Gibbs et al., 2023; Emanuel and Harrington, 2024), rely on natural experiments driven by the COVID-19 pandemic and mandates to work from home. While these estimates could be biased by factors specific to the pandemic, both studies report productivity drop for workers driven to work from home compared to those that were already teleworking, ranging from -19% to -4%, highlighting that homeworkers are negatively selected.

While the debate on the productivity of teleworking rages on, its longer-run impact on workers and firms are less known. Bloom et al. (2024) randomised IT workers to hybrid

[4]Dutcher (2012), using a lab experiment, reports productivity gains but only for creative tasks, and Wfh is associated with a drop in productivity for "dull" tasks.

or office work for 6 months and Choudhury et al. (2024) uses a natural experiment which daily randomised workers to the office during the pandemic. Both studies report that employee well-being improved at no productivity costs, and that in the case of Bloom et al. (2024) the effect is long lasting, resulting in a 30% drop in employees leaving the firm.

More similar to our identification strategy is Goux and Maurin (2024) who estimate the effect of a reform giving the right to work from home in France, and compare workers in firms that signed WfH agreement and those that did not. Since firms adopting WfH might differ, or attract selected group of workers, they rely on a triple difference in difference strategy, based on comparing low-level employees, who were unlikely to use WfH and mid-level employees who used it more. They estimate that mid-level workers experienced a deterioration of their self-reported health, and that a 10 percentage point increase in the proportion of workers working from home results in a 5 percentage point increase in the proportion of workers suffering from chronic disease. They also do not find effects on labor market outcomes (wages, hours worked, career progression).

We improve on this identification strategy by matching treated and control firms within the same industrial sector to make firms and their workers close substitute observationally. Since we are primarily interested on the effect on the workers' children, for which we can observe the outcome of interest only at a specific age, our identification also rely on the age of children when the reform affecting their parents is implemented. This timing is more likely to be exogenous. In additional robustness checks, we assess heterogenity in the effect, separating parents with a high or low probability to work from home, based on their occupation.

3 The Dutch Context

3.1 Regulation of Working from Home in the Netherlands

In the Netherlands, work from home arrangements are regulated by collective labour agreements between employer organizations and unions. There is no national agreement.

Around 80% of employees in the Netherlands are covered by a collective agreement (Dutch Ministry of Social Affairs and Employment, SZW). These agreements lay out labour conditions for all employees, such as wages, payment for extra work, working hours, probation period, pension, childcare and, relevant for this study, work from home arrangements. The provisions in a CLA are often more favourable than those prescribed by law, but they may not contradict the law. There are two types of collective agreements: sectoral collective agreements and firm-specific collective agreements. CLA are regularly revised. When an agreement is altered it is centrally deposited. Since our identification strategy relies on matching treated and control firms within sector, we focus solely on company collective agreements.

3.2 Education and CITO Test scores

Primary education in the Netherlands consists of 8 years of comprehensive schooling (starting at the age of 4). In February of their last year of primary school, at age 12, 90% pupils take the CITO exam, which consists of a multiple choice questionnaire testing their competences in Dutch, Math, World orientation (Geography, History, Biology) and Study Skills. The test results usually inform recommendations of the appropriate secondary education track. There are three possible tracks: preparatory vocational secondary education (vmbo), senior general secondary education (havo) or university preparatory education (vwo). Vmbo is a vocational track, havo leads to colleges of applied sciences and only those completing university preparatory education are allowed to enroll at University. While it is possible to change track during secondary education, the CITO scores largely determine the educational path of children, making it a very high-stake exam.

The test is set and marked externally by a private company, not by the child's teacher. CITO scores are normalised and scaled between 501 and 550. A score below 536 leads to recommendation of secondary vocational education (vmbo), one between 537 and 544 to a recommendation of general secondary education (havo) and one above 545 to the academic secondary education (vwo). As well as the overall CITO test score for the years

2006 to 2019, for the majority of students we have grades separately in Math and Dutch, which is normalised by subject and year.

Prior to student sitting the CITO, teachers recommend a secondary track. Following the test, and if their initial evaluation differs from the track determined by the CITO score, they need to reconsider it and provide a final recommendation (Timmermans et al., 2023). Our data includes the initial teacher's track recommendation only.

4 Data

We compiled a dataset on relevant elements of collective labor agreements using records stored by a commercial Dutch company called *XpertHR*. Those records were linked using firms identifier to the National Employer-Employee database held by the Dutch National Statistical Office (CBS). We link children who passed the CITO to their parents, and to the employer-employee database to create our main database.[5]

4.1 Collective Labour Agreements

XpertHR is a private company that has maintained records of all collective labor agreements (CLAs) in the Netherlands since January 1990. As of July 2020, the database contained 14,461 records, covering both sector-wide and firm-specific agreements. Multiple records may correspond to the same CLA if they were amended over time, leading to repeated entries. Among these, 1,051 records include references to "Afspraken" or "thuis/telewerken" i.e. Teleworking arrangements.

Each record specifies the dates when rights were granted or modified, allowing us to determine when firms formally established work-from-home (WfH) policies. Out of the 1,051 records that mention teleworking, we were able to manually match the firm's name

[5]Additional information on the datasets and variables used can be found in appendix A.1. Appendix A.2 details the steps from extracting information from XpertHR to merging it with CBS data.

to its national business register number (Kamer van Koophandel, KvK) for 513 entries. CLAs can apply either to a specific firm or an entire sector.

Our identification strategy relies on comparing firms that adopted WfH policies with firms in the same sector that did not. Consequently, we focus only on firm-specific CLAs - that could be matched to a firm identifier - and exclude sector-wide agreements from our analysis. Since the matched employer-employee data starts in 2006, we limit our dataset to CLAs starting et least that year. The dataset is further trimmed to December 2019 to avoid any potential effects of the COVID-19 pandemic, further reducing the sample size.

This process results in a final sample of 90 firms, of which 39 can be identified by CBS. To ensure reliable tracking of employees within firms, we include only firms that had a teleworking CLA in 2007 or later (i.e., strictly after 2006). Including 2006 CLAs would create an issue since we lack 2005 data in the matched employer-employee records, making it impossible to determine when a worker joined a firm. As a result, our final sample is further refined from 39 to 33 firms. Further information on the creation of the data is available in Annex A.2.

The identified firms are then matched to up to four firms in the same sector with the smallest Mahalonis distance in the year the treated firm altered their CLA to include the right to work from home. This leaves us with a sample of 114 firms. These firms are then matched to the employer-employee national register to identify their employees, and a national register to identify their children aged between 8 and 18.

To conduct robustness checks we also identify CLAs that provide other type of work flexibility unrelated to WfH such as rights to informal care, short-term leave to care for relatives or split parental leave. These other policies are unlikely to affect the ability of parents to invest in the human capital of their children and will be considered as placebo policies.

4.2 Administrative Registries

The children of parents working in a treated or control firm at period T-1 are identified, and we extract their scores at the end of primary school national test (Cito). We use the Dutch and Math scores only as other subjects are not taken by all pupils. The scores are normalised to the performance of the population of test sitters in that year. We also use labor market information available in the matched employer-employee data, namely annual labor earnings, number of hours worked out of which we create a variable for part-time work which takes value 1 is one works fewer than 1,500 hours a year, and, a dummy for working in the same firm as in T-1.

The last source of data we use is the Dutch Labor Force Survey. The LFS is a nationaly representative rotating panel survey. Respondents are once interviewed thoroughly, by an interviewer from Statistics Netherlands, and are subsequently contacted by phone every three months four times. In the first wave, participants are asked about their work patterns and we create a dummy variable for all participants reporting working from home at least once a week. The LFS respondents can be linked to the Employer-Employee database, which allow us to identify whether their CLA allows for teleworking, and assess the plausibility of the mechanisms by which CLAs increase the propensity to work from home. The LFS also allows us to complement analysis from matched employer-employee data and, assess whether there was any long-run impact of the WfH policy on hours worked.

5 Empirical Approach

5.1 Identification Strategy

To estimate the impact of teleworking on children's outcomes, a direct comparison between children of parents in firms that have teleworking provisions in place and those in firms that do not, is problematic as the two types of firms, and their workers, are likely to differ

in other characteristics.

One approach is to leverage the fixed age at which children take the CITO and examine different age groups among children whose parents work at firms adopting teleworking policies. Children aged 12 or older at the time of implementation have already taken the CITO, making them unaffected by the policy and a suitable control group. In contrast, parents of children under 12 may have used WfH to invest in their child's human capital, potentially influencing CITO outcomes. Comparing both groups helps account for parental selection into firms that might impact test scores. Since WfH policies are not designed to improve CITO results, treatment assignment can be considered quasi-random, driven by birth year variations and firm-level policy changes.[6] We create a narrow window around age 12 and analyze the difference in CITO results between children aged 8 to 11 and 13 to 18 at the time a firm adopted teleworking provisions in their collective agreements, with the former group being considered treated and the latter group, a control group.

However, parents of younger children might differ in other relevant characteristics. They are younger, may have different preferences for investing in their children, and are likely to have less seniority at the firm. This could influence their eligibility for remote work, their earnings, and, in turn, their children's outcomes through channels beyond teleworking. To address this challenge, we employ a Difference-in-Differences strategy, so that we account for both selection into firms that implement WfH policies via comparing children whose parents work in the same firm but due to their age at the time of implementation are either affected by the policy of not, and account for the age effect by computing the same difference in firms that did not implement the WfH policy.

Since we have a small sample of treated firms it is not possible to rely on to-be treated firms as control for firms who implemented the policy earlier, as recommended in

[6]Note that, if the CLA-sanctioned WfH agreement were to only have formalized previous teleworking behavior at the firm, the estimated effects would be lower bounds.

De Chaisemartin and d'Haultfoeuille (2023). Moreover, we are not considering the roll out of a single policy, but firm specific policy change, which means that each firm could be considered its own unique treatment. To account for selection into treatment, as well as heterogeneity between the treated firms, we rely on a matching difference in differences, which defines a control group for each treated firm.[7]

Control firms are matched strictly on sector and year, and by closest Mahalanobis distance on firm size, share of highly educated workers, share of female workers, share of part-time female workers, share of part-time male workers, gender-specific mean wage. These variables are meant to capture characteristics of the workforce as well as the prevalence of women and the use of time flexibility. These last two categories can be relevant for how family-friendly a workplace was prior to a change in CLA. The matching is based on characteristics measured in the year before the WfH arrangements were formalized in the collective labor agreement (year *T-1*) to limit the potential selection of workers driven by WfH arrangement and thus, that the covariates are themselves affected by the treatment. For each treated firm, a control group of up to four untreated firms in the same sector with the smallest Mahalanobis.[8] In further tests we assess the robustness of the result to variations in the set of covariates used to match firms.

When identifying workers in these firms, we limit our analysis to those with at least one year of tenure at the time of the labor agreement change to reduce the likelihood that some parents joined the firm in anticipation of a forthcoming WfH policy. When identifying their children, we exclude students who took the test in year T, as it is unclear whether they were affected by the new policy.

[7]This is very similar to the identification strategy implemented in Gathmann et al. (2020) when estimating spillover effects of plant closures. A synthetic difference in differences a la Arkhangelsky et al. (2021) is not possible since the data at the child level is not a panel.

[8]Note that since we match on the year CLAs were signed, we obtain a fictional year of treatment for the control firms.

5.2 External validity and balancing

To validate our empirical strategy, we compute the following balancing tests, first comparing the treated firms to the universe of other firms, which provides us with a measure of external validity. Then, we compute the same tests on the sample of treated and matched-control firms to assess the balancing of the sample of interest. These tests are conducted at the level of firms, parents and children. Note that while we matched firms on the observable characteristics, we did not include information on parents and children. Nothing ex-ante insures that their characteristics are internally balanced.

Table 1 Panel A reports the characteristics of the treated firms against all other firms in the database. It highlights that the treated firms are substantially larger - 780 employees vs 32, have more educated workers, who are less likely to work part-time and were female employees earn almost 50% higher wages than at the average firm. Firms that are part of our experimental population are unrepresentative.

Panel B reports the characteristics of the parents of children whom CITO scores we will consider. This confirms the firm-level analysis. Parents in treated firms earn higher wages (+20% and +40% for fathers and mothers, respectively), but also work longer hours (22% and 33% for fathers and mothers, respectively).

Panel C in Table 1 reports the differences in educational attainment and recommendations between the treated children and the general population. Concomitant with their parents being positively selected in term of education and income, their children achieve 7% to 10% of a standard deviation higher grades at the CITO and are 4 percentage points more likely to qualify for one of the top two tracks.

We now assess whether the matching provided an internally valid sample. Table 2 Panel A reports the estimate of separate regressions of the matching characteristics on an indicator of treatment for the subsample of treated and matched firms. The difference is never statistically significant, indicating a balanced sample of treated and control firms. This might not be overly surprising since the matching was conducted on most of these variables.

Table 1: External Validity

Variable	Mean Non-Expe. Firms	Difference Expe. - Non-Expe.	No. of Obs
Panel A: Firms			
% Earnings Females	0.356 (0.001)	-0.076*** (0.006)	4,184,020
% Part-Time Females	0.663 (0.000)	-0.235*** (0.001)	3,014,840
% Part-Time Males	0.294 (0.000)	-0.188*** (0.000)	3,494,043
% College Educated	0.307 (0.000)	0.066*** (0.017)	4,184,105
No. of Workers	25.510 (0.597)	757.800*** (100.300)	4,184,105
No. of Workers - Females	12.040 (0.290)	316.300*** (47.960)	4,184,105
No. of Workers - Males	13.770 (0.346)	441.400*** (61.960)	4,184,105
Average Earnings - Males	38,662.6 (54.700)	12,737.200*** (1,183.400)	3,494,043
Average Earnings - Females	19,614.1 (25.830)	15,264.300*** (976.800)	3,014,840
Panel B: Parents			
Earnings - Mother	15914.1(18.87)	6569.2*** (211.0)	1,719,372
Earnings - Father	42299.4 (45.87)	8311.3*** (507.9)	1,672,342
Hours Worked - Mother	756.9 (0.51)	250.8*** (5.68)	1,719,372
Hours Worked - Father	1471.5 (0.70)	328.6*** (7.75)	1,672,342
Panel C: Children			
Z-Score Dutch	0.01 (0.001)	0.10*** (0.009)	1,623,085
Z-Score Maths	0.01 (0.001)	0.07*** (0.009)	1,623,085
Eligibility Uni	0.19 (0.001)	0.02*** (0.004)	1,720,986
Eligibility Gen. Sec. & Uni	0.50 (0.001)	0.04*** (0.005)	1,720,986

Notes:

In Panel B, we conduct balancing tests on the sample of parents. The treated and control parents are non-distinguishable in term of gender of the parent benefiting from WfH, their age, education, probability of being an immigrant or a descendant of immigrant, number of hours worked, earnings, hourly wage. Focusing on the children in the match sample (panel C) leaves us with a population of 14,331 children. We find the samples of treated and control children to be balanced in term of age, sex and number of siblings. Matching, even not conducted using parents or child characteristics is effective at eliminating differences between the treated parents and children and their untreated peers.

5.3 Event-study

Although having a balanced sample is reassuring, our strategy is not based solely on comparing children in treated and control firms but younger and older children in both types of firms. Therefeore, our identification strategy requires that pre-policy change, treated and control observations had parallel trends. We first test this assumption over various parents characteristics, and then on their children outcomes.

Pre-trends on parents First, we analyse whether parents working in firms that changed their policies regarding working from home had different labour market trends before the policy was implemented, and in its aftermath. To do so we look at the relevant labor markets outcomes for parents in a window fours years before the implementation of the policy change to three years after. For each of these years separately, we regress the outcome on a treatment dummy and basic individual controls (namely age, its square and gender). This allows to see wether the descriptive statistics support the absence of pre-trends between parents.[9]

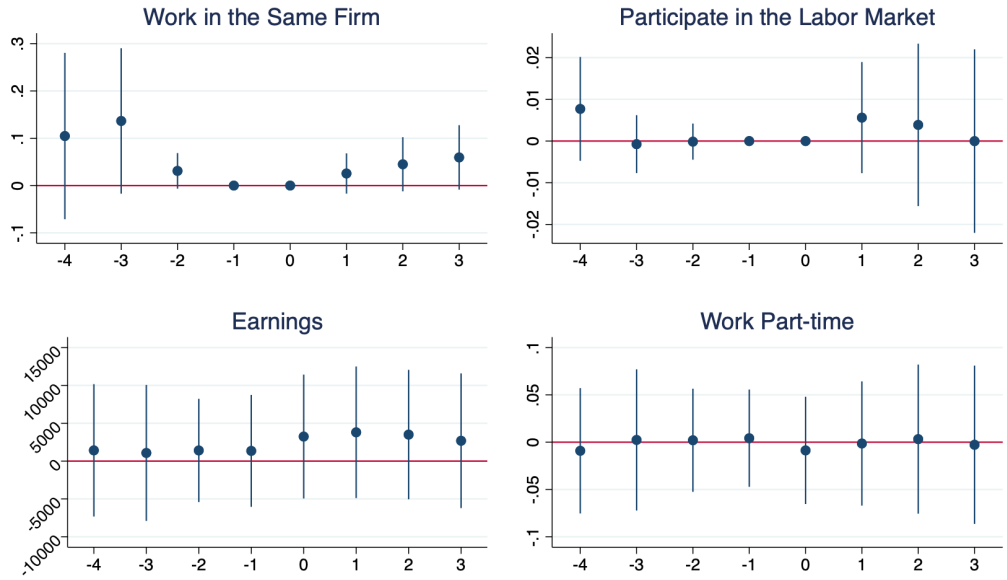
[9]Figure 1 includes parents of both younger and older children. Note that the picture remains the same when one looks only atyounger or older children.

Table 2: Balancing - Internal Validity

Variable	Mean Control	Difference T - C
Panel A: Firms		
% Earnings Females	0.28 (0.02)	0.005 (0.05)
% Part-Time Females	0.44 (0.02)	-0.03 (0.04)
% Part-Time Males	0.11 (0.01)	-0.003 (0.02)
% College Educated	0.37 (0.02)	0.015 (0.04)
No. of Workers	725.9 (114.1)	233.6 (241.9)
No. of Workers - Females	300.7 (51.0)	112.6 (128.6)
No. of Workers - Males	425.2 (71.91)	121.0 (144.2)
Average Earnings - Males	51005.8 (1390.9)	1604.4 (2684.7)
Average Earnings - Females	34269.0 (1166.6)	2481.0 (2109.5)
No. of Obs	114	
Panel B: Parents		
Male	0.63 (0.04)	0.007 (0.07)
Age	44.89 (0.28)	0.35 (0.48)
Foreign Background	0.19 (0.02)	0.022 (0.04)
Above High School	0.61 (0.05)	0.019 (0.08)
Hours Worked	1728.7 (40.39)	-3.57 (69.62)
Hourly Wage	26.17 (1.13)	-0.30 (1.90)
Earnings	47047.8 (1975.6)	1027.4 (4043.3)
No. of Obs	14,331 (except Education: 8,626)	
Panel C: Children		
Boys	0.50 (0.01)	0.004 (0.01)
Age	11.95 (0.01)	0.007 (0.02)
No. of Siblings	1.37 (0.03)	0.038 (0.06)
No. of Obs	14,331	

Notes:

Figure 1: Pre-trend in Labour Market Outcomes



Notes:

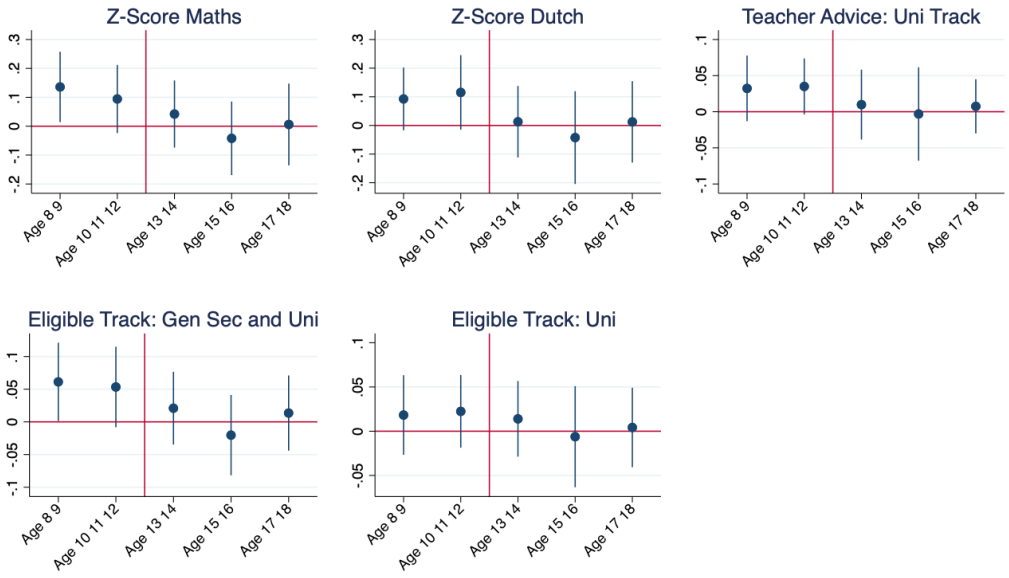
Across the four outcomes—earnings, labor market participation, part-time work, and the likelihood of staying with the same firm—we find no statistically significant differences in trends between parents in treated and control firms. This mitigates concerns about workers sorting into firms in anticipation of WfH policies and any differential income trends that could indirectly affect children's educational attainment. Prior to the policy implementation, differences in earnings are small positive and not statistically significant. Post implementation, the differences in earnings grow, i.e. workers in firms that implemented WfH received pay increase about 8% larger than those in the control firms, but these differences are not significant.[10]

Pre-trends on children Testing for the absence of pre-trends differs for children since their educational outcome is observed only once. Instead of time passing, variation comes

[10]The only notable difference is in the likelihood of remaining at the same firm three to four years before the WfH policy change. This is primarily driven by control group firms that were newly established during this period, which mechanically lowers the probability of continued employment in the control group compared to the treated group.

from the age children were when their parent's firm implemented the WfH policy. Figure 2 shows the difference in CITO test performance between children of treated and control firm employees, separately for different age groups (8–9, 10–12, 13–14, and 15–16). All estimates to the right of the policy represent the pre-trend period! These children were older than 12 the year in which the CLA granted the right to work from home. They sat the test when no provision for teleworking was in place for their parents and thus could not have benefited from these arrangements. As expected these estimates are close to zero and not statistically significant, supporting the parallel trend assumption. The outcomes for matched control children appear credible counterfactuals for the treated children.

Figure 2: Effect of Wfh on test score by age at Treatment



Notes:

Additionally, the pre-trend estimates highlight that there were little anticipation effects of the Working from Home policy nor that the change in the CLA were just a formalization of arrangements that were already informally in place (more evidence are provided in section 7). Post-treatment, there is a jump in performance for children who were less than 12-year old when the policy was implemented. This provides the first evidence that younger children might have benefited from the ability of their parents to work from home.

The estimates are imprecise, but always positive. The younger the children the longer they were exposed to the policy but there is no evidence of dosage response - maybe because parents mostly rely on WfH to invest in the human capital of their children in the year when they have to take the test.

6 Main results

We estimate equation (1) on the restricted sample of children - aged 8 to 16 at the time of the change in WfH policy - whose parents work in a treated or matched control firms.

$$y_{i(f,k),t} = \alpha + \lambda_t + \gamma X_i + \eta \text{Young}_i + \rho \text{Treated}_f + \beta \text{Young}_i \text{ x Treated}_f + \epsilon_{i(f)} \quad (1)$$

where $y_{i(f,k),t}$ if the outcome of individual i in year t whose parents work in firm f in sector k. We consider a series of outcomes: normalised CITO scores in Dutch and math, whether the score is above specific thresholds that determine tracking to the academic track (≥ 545) or the genaral secondary and academic tracks (≥ 537) as opposed to the vocational track, and the initial teacher's track recommendation.[11] To account for possible variations in the difficulty of the CITO test between years, or cohort specific effects we also include λ_t a year of test fixed effects. *Treated* refers to the firms allowing Wfh in their labor agreement and *Young* is a dummy variable taking value 1 if the child is between 8 and 12. The coefficient of interest, β, compares older and younger children in firms that implemented a Wfh policy and in untreated firms.

We estimate different versions of this specification, starting without controls and progressively adding them. In the baseline specification we include child and parental controls (X_i) namely age and gender of the child and the parent. Additionally, to capture any

[11] The data at our disposal does not allow us to check the track actually attended by the child.

remaining unobservable characteristics of the parents linked to their occupational choice, we include sector fixed effects (θ_k). This would capture parental unobserved characteristics associated with the choice of sector or the type of parents most likely to use WfH opportunities. For example, two firms might allow workers to work from home but the one in a sector where more tasks can be conducted from home should have a greater proportion of employees teleworking. Finally, to further controls for possible selection of parents in different types of firms and compare even more similar parents, our favoured specification includes a match-specific fixed effect, and compare outcomes for the children of parents in a treated firm against outcomes for children of parents in firms that were matched to it. In all specifications, standard errors are clustered at the firm level to account for potential correlations within firms.[12]

6.1 Baseline results

Table 6.1 reports the estimates on β from equation 1. Note that the estimate should be interpreted as an intention to treat since not all parents working in this firm might take advantage of the teleworking opportunities. The table is split into four panels each representing a different specification. Panel A does not include any controls, Panel B includes individual control, Panel C includes firm's sector fixed effects and Panel D includes match indicators.

The estimates are reasonably consistent over all specifications and for each outcome points towards a positive effects of working from home policies, apart from the selection to the most academic track, for which it does not seem to be effective. Across all outcomes, the estimates drop by 20% to 50% as the specifications become more restrictive, but remain statistically significant. The estimates drop when including sector fixed effects or match-specific fixed effects, highlighting the importance of comparing most similar firms

[12]Following Abadie and Spiess (2022) we also compute standard errors clustering at the match-specific level, but the difference between the two is minimal and only the first one are reported in the Tables.

Table 3: Regression Results

	(1)	(2)	(3)	(4)	(5)
	Z-Score		Teacher Rec.	Eligible Track	
	Maths	Dutch	Uni Track	Uni	General Sec. & Uni
Panel A: No Controls					
β	0.109***	0.124***	0.030	0.017	0.053***
	(0.035)	(0.035)	(0.018)	(0.018)	(0.018)
Panel B: With Controls					
β	0.102***	0.113***	0.028*	0.015	0.049***
	(0.033)	(0.030)	(0.016)	(0.017)	(0.017)
Panel C: Sector FE					
β	0.098***	0.105***	0.023	0.013	0.047***
	(0.035)	(0.030)	(0.014)	(0.017)	(0.017)
Panel D: Matching FE					
β	0.086**	0.089***	0.017	0.008	0.041**
	(0.033)	(0.033)	(0.013)	(0.017)	(0.018)
No. of Obs	14,331	14,331	10,520	14,331	14,331
Mean	0.050	0.100	0.170	0.200	0.530

Notes:

to capture the selection of parents in different types of firms. In our favoured specification - controlling for match-specific indicators, children, whose parents became eligible to work from home, improve their test scores by 9% of a standard deviation compared to peers whose parents work at very similar firms. The effect appears to be driven by improvement at the middle of the distribution, pushing score above the 537 threshold making pupils eligible for the general secondary track or above, but not above scores of 545, which would lead to recommendation to the most academic track. Commensurately, the teachers recommendation about attaining the top track is unaffected by the introduction of WfH policies. Considering that the estimated effect on having a score making a student eligible to the academic track is zero, the WfH moves an additional 12% of pupils to the general secondary track.

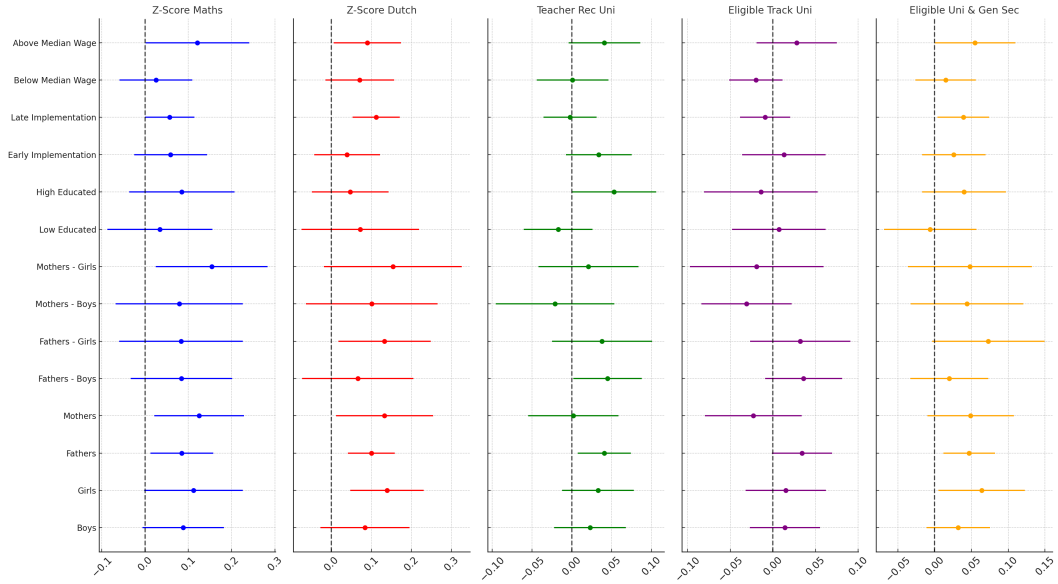
Overall, we find strong evidence that allowing parents to work for home improves the test scores of their children at a high stake exam at the end of primary school. To put this in perspective, the effect of Wfh policy is similar to the median primary school interventions in the US which has an effect size of 0.07 standard deviation on Math score and 0.1 on reading score (Kraft, 2020). Considering that our estimate is an Intention to Treat, the Treatment Effect on the Treated might be even substantially larger and can be achieved at no cost to the educational sector. In the final section, we will provide evidence on whether WfH comes at some costs to parents or firms.

6.2 Heterogeneity

We examine whether the effect varies across different dimensions, including the gender of the parent affected by the firm policy change, the gender of the child, the education level of the parent, and the period at which the right was granted. We report these estimates graphically in Figure 3 and more completely in Table B.2 in the appendix section. Each analysis of heterogeneity in the effect of WfH is conducted by splitting the sample into groups and running equation 1 for each group separately.

Golsteyn and Schils (2014) reports that boys outperform girls at the CITO by 0.2 of a standard deviation in Math and are outperformed by girls by 0.18 in Dutch. It is a priori ambiguous whether a policy allowing parents to invest more time on the education of their children will open or close any gender gap in achievement. The estimated effect of WfH on test score is significantly different from zero for both boys and girls but about 50% larger for the latter. The Wfh policy could thus reducesthe achievement gap by gender in math by about 20% but increases it by about the same level in Dutch. While large these differences are not statistically significant.

Figure 3: **Heterogeneity in the Effect of Wfh on Test Score**



Notes:

Mothers tend to spend more time with their children. A Wfh policy might have ambiguous effect on this gap. While Aksoy et al. (2023) note that fathers reallocate less of the freed-up time than mothers to care, Cowan (2024) report a closing of the gender gap in parental care among graduates. It is why the gender of the parent benefiting from WfH should matter since parents can reallocate tasks within the household so that whom ever is entitled to work from home, the parent with a comparative advantage in teaching the child can do more of it. Indeed, we do not find strong evidence of a gendered effect. The effect on test scores is about 50% larger when the mother benefits from

the WfH policy. However, this difference is not statistically significant, suggesting that fathers are as effective as mothers when engaging in activities supporting the education of their children, or that parents reallocate tasks within the households. We also do not find significant differences when considering the interactions of the child's and parent's gender, even if girls whose mothers can work from home seems to benefit the most.

As mentioned above, access to teleworking varies by occupation and seniority, so that in general more educated and higher earners have more opportunities to work from home. It is a-priori ambiguous whether this greater propensity to work from home will result in greater impact on their children's education. More educated and wealthier parents might already be investing in support activities for their children, and Wfh might not result in additional engagement or only allow them to substitute externally provided support to parental support. Alternatively, more educated parents might have a greater preference for investing in the education of their children and have greater returns to their investments, in which case, granting Wfh would result in larger educational gains for children of more educated parents. We report the estimates separately for lower educated parents (defined as vocational qualification only) and higher educated parents and reject any statistical difference between the estimates.[13]

Similarly, while the effects are substantially larger for parents earning above median wage, especially in math, the estimates are imprecise and we reject that among the sample of parents benefiting from WfH, the more educated or higher earners are able to invest more in the human capital of their children. Note that we will come back on the impact of WfH policies on the inequality of educational attainment in the final discussion.

Finally, we split the sample between early and late adopters. Early adopting firms, who granted rights to work from home before 2012 might be a selected group of firms that have employees with an especially strong preference for working for home. While in the later period, technological progress and societal understanding might have made it easier

[13]Note that education is not reported for about one third of parents.

for workers to actually work from home. In the earlier period, the effects are insignificant, suggesting that parents might not have made much use of their rights to work from home. For firms treated in the second period, the effects are similar to those of the full sample. Since the fraction of teleworking has substantially increased post COVID-19 pandemics, our estimates likely underestimate the current effect of teleworking rights on educational attainments.

Altogether, Figure 3 reveals little heterogeneity in the effect of working from home. The estimates are not very precise and none of the differences between groups of interest being statistically different from each others. The point estimates suggest that the impact is larger for girls, wealthier households and late implementers.

6.3 Robustness checks

In this section, we conduct a series of robustness checks to assess the plausibility of our identification strategy.

6.3.1 Alternative Identification

As pointed out in Section 5, an alternative approach is to leverage the fixed age at which children take the CITO. Rather than relying on matched difference in differences it compares older and younger children within treated firms, i.e. using only one dimension of the DiD. Table 4 displays the estimates for three specifications on being young enough at the time of the policy change to be able to benefit from it. With no, or just the basic set of controls these estimates are substantially larger than the DiD estimates. For example, in the model with controls the estimated effects of WfH on math and Dutch test scores are 0.17 to 0.24 respectively, compared to 0.10 to 0.11 in the DiD specification. However, when including sector fixed effects to capture unobserved characteristics of parents the estimate drop to 0.08 to 0.16 in the alternative identification strategy, similar to the 0.10 that was estimated in the DiD specification. The confirms the importance of controlling for sectors, and generally unobservable parental characteristics associated with their choice of work

sector.

The last panel presents placebo estimates under the assumption that the policy was implemented three years earlier. It compares children who were 16 to 18 at the time of the actual policy change to those who were 13 to 15. Since neither group should be affected by the policy, the expected difference between them should be zero. As anticipated, the estimates in Panel D are never statistically significant. However, their magnitude—though smaller—remains comparable to those in the other panels. This suggests the presence of differential trends in school performance among children whose parents work in treated firms. Therefore, incorporating the second difference in the DiD approach is warranted (for a comparaison see Panel D of Table 5).

Table 4: **Alternative Identification : Only Treated Firms**

	(1)	(2)	(3)	(4)	(5)
	Z-Score		Teacher Rec.	Eligible Track	
	Maths	Dutch	Uni Track	Uni	General Sec. & Uni
Panel A: No Controls					
β	0.178***	0.195***	0.0324	0.0321	0.0883***
	(0.0418)	(0.0572)	(0.0285)	(0.0206)	(0.0205)
Panel B: With Controls					
β	0.167***	0.238***	0.0419	0.0440**	0.0972***
	(0.0302)	(0.0369)	(0.0264)	(0.0173)	(0.0168)
Panel C: Sector Fixed Effects					
β	0.0763*	0.155***	-0.00320	0.00946	0.0534**
	(0.0413)	(0.0420)	(0.0220)	(0.0134)	(0.0256)
No. of Obs	3,962	3,962	2,957	3,962	3,962
Mean	0.10	0.14	0.19	0.21	0.55
Panel D: Placebo - Assuming WfH implemented at T-3					
β	0.0464	0.0693	0.0166	0.0256	0.0146
	(0.0595)	(0.0636)	(0.0173)	(0.0206)	(0.0280)
No. of Obs	2,610	2,610	1,985	2,610	2,610
Mean	0.08	0.12	0.16	0.21	0.54

Notes:

6.3.2 Alternative matching

To further test the robustness of the results to the initial matching of treated and control firms, we test alternative matching strategy. First, we randomly keep three matched control firms among the four used in the baseline strategy. Although the sample is significantly altered - reduced by more than 21% from 14,331 to 11,815 observations - there is no substantial effect on the estimates. The WfH estimate on test scores is 9% of a standard deviation, almost identical to the main specification (see Panel A of Table B.3 available in Appendix B).

In Table B.3, we also report estimates from two alternative specifications where we change the variables treated firms are matched on. In both cases the match still needs to be fixed on years and sectors. Firm in Alternative 1 are matched on a more detailed breakdown of the parental education variable. In Alternative 2, firms are matched on fewer observations than in the baseline specification, i.e. only to firm size, share of high educated workers, share of part-time workers, average wage and ratio males/females. The estimates of WfH on test scores range from 0.086 to 0.105, again, very similar to the 0.10 found in the main specification. Despite the small number of treated firms, the main results of the impact of parental right to work from home on the educational performance of their children are largely insensitive to the choice of control firms.

Finally, the last panel in Table B.3 reports estimates of the WfH when we impose that parents work at the firm two years prior to the implementation of the policy, to assess whether parents might have selected into firms in the belief that WfH policies were soon to be implemented. The estimates of WfH on test score reaches 0.11 when imposing this restriction, indicating that announcement effects and parental selection into treated firms is limited.

6.3.3 Placebo Analysis

To assess the plausibility of the identification strategy, we conduct a series of placebo tests. First, we identify all CLAs that give workers other rights to work flexibility but are

less likely to allow parents to invest more in the education of their children. Specifically, we focus on three policies: a- the right to alter the work pattern to allow for the informal care of a family member, b- the right to take leave to provide care for a family member and c- the right to split parental leave. While, all clearly allowing workers more flexibility - and thus potentially appealing to similar type of workers as the WfH, these rights do not allow parents to spend more time with their children - specially around the age threshold of the CITO. We then reproduce our main analysis by matching firms that introduce these rights. In Table 5, we report estimates of equation 1 for each of these rights. None of these rights lead to statistically significant improvement in test scores. The estimates are small and range from -0.04 to 0.03. Only for WfH do we observe an improvement in children's academic performance lending support to the interpretation that this improvement is caused by parents being able to invest more in the education of their children, rather than the effect being driven by selection of parents.

Finally, we assume that the change in labour agreement took place in the same firms but three years prior; this is equivalent to comparing the CITO score for children aged 13 to 15 (Placebo treatment) with those aged 16 to 18 (Control group). Note, that we keep the matched firms the same as in our main analysis rather than match firms based on their characteristics in year *T-4*, so that any differences in the estimates cannot be driven by changes in the comparison group. In expectation, if the estimated effect in our main analysis are causal effects of the Wfh policy and not driven by parental selection or time effects, neither group should be affected when using this placebo treatment, as they undertook the CITO assessment before Wfh provisions were in place. We estimate Equation (1) to a modified sample to assess any effects of this fictitiously timed policy and do not find evidence of any effects. The estimated effects are small (1% to 2% of a standard deviation) - much smaller than in Table 3 and not statistically different from 0.

These placebo analyses support that the main estimates are the results of the introduction of WfH policies rather than driven by selection of workers into firms or time effects.

Table 5: Robustness - Placebos

	(1)	(2)	(3)	(4)	(5)
	Z-Score		Teacher Rec.	Eligible Track	
	Maths	Dutch	Uni Track	Uni	General Sec. & Uni
Panel A: With Controls - Informal Care					
β	-0.001	-0.027	-0.003	-0.010	0.006
	(0.052)	(0.049)	(0.027)	(0.020)	(0.027)
No. of Obs	19,818	19,818	14,508	19,818	19,818
Mean	0.17	0.20	0.19	0.23	0.58
Panel B: With Controls - Shortcare Leave					
β	0.010	-0.037	0.002	-0.004	-0.015
	(0.050)	(0.054)	(0.023)	(0.023)	(0.025)
No. of Obs	6,892	6,892	5,118	6,892	6,892
Mean	0.10	0.14	0.16	0.23	0.56
Panel C: With Controls - Split Leave					
β	-0.003	0.032	0.003	0.017	0.005
	(0.035)	(0.034)	(0.018)	(0.013)	(0.016)
No. of Obs	16,579	16,579	12,250	16,579	16,579
Mean	0.21	0.28	0.25	0.26	0.61
Panel D: Robustness - Placebos					
β	-0.009	-0.023	-0.001	0.000	-0.014
	(0.045)	(0.046)	(0.022)	(0.020)	(0.019)
No. of Obs	9,946	9,946	7,222	9,946	9,946
Mean	0.11	0.15	0.17	0.21	0.56

Notes:

6.3.4 Jackknife

Finally, we assess the sensitivity of our results to specific treated firms. This is crucial considering the small sample of treated firms that allows us to identify the effect of Wfh. We thus re-estimate equation 1 omitting one treated firm, and its associated control firms, at the time. Figure B.1 in Appendix B reports the distributions of the estimates on the effect of the policy for each outcome. The jackknife estimates are quite consistent and never statistically different from the reported estimate. Moreover, for the test scores and eligibility to General secondary and uni, where the main analysis report statistically significant effect, all jackknife estimates are also statistically significant from zero. For the teacher advise and scoring above 545 points, the main estimates were not statistically significant and the majority of jackknife estimates are also not significantly different from zero. Overall, the Jackknifes support that the main estimates are not driven by specific firms and are consistent whatever sample is constructed.

These tests confirm the plausibility and robustness of the identification strategy. The results are consistent with alteration of the identification strategy, changes to the groups of controls and all placebo tests reject that the results could have been obtained by chance or reflect time effects or selection into treatment effects. We conclude that the main evidence is strongly supportive of the causal interpretation that working from home improves the educational attainments of the children of the affected parents.

7 Mechanisms: Labour Force Survey Evidence

In this section, we explore evidence supporting the potential mechanisms by which a policy granting WfH rights might benefit the children of these workers. To do so, we link the CLA information to the Dutch Labor Force Survey via the same individual identifier used in the matched employer-employee data. We then keep respondents in treated and control firms - whether they are parents or not - who have been interviewed in the LFS. This leaves us with a sample of 3,793 observations.

7.1 Impact on teleworking practices

Our main estimates are based on an intention to treat - since we can identify firms that granted their workers the right to work but not workers who actually make use of this right. The LFS allows us to test the first stage since it includes a question on whether the respondent works from home. We recode all respondents who reported working from home at least one day a week as teleworking. Depending on the tasks conducted, workers may or may not be able to use the right to teleworking. We define teleworkable occupations as those where more than 20% of workers, in the full LFS sample, report working from home at least once a week.

CLA and teleworking We then estimate equation 1 replacing the dummy for being "Young" by a "Post-CLA" dummy and the year of CITO fixed effect by calendar year fixed effects (λ_t). Individual controls (X_i) include gender, age and age squared. Results are reported in Table 6. Working in a firm that grants the right to work from home leads to a 15 percentage points rise in reported remote working. This is a doubling of the prevalence of teleworking from 17% to 32% following the introduction of WfH policies, and substantiate that the opportunity to engage in teleworking, as outlined in the CLA, indeed contributes to an observed increase in reported remote work. This rejects that the introduction of WfH mainly acknowledged previous working pattern. Instead it alters the behaviour of employees, making it a plausible mechanism for our main findings.[14]

Teleworkable occupations and Children's educational outcomes We then assess the effect of having parents granted the right to work from home and working in a

[14]We then test whether the increase in the propensity to work from home is driven by workers in occupations that are more likely to be teleworkable. To do so, we add interaction between teleworkable occupation and the controls variables. The estimates are reported in table B.4 available in Appendix B. The estimates are very imprecise but support the assumption that there is heterogeneity in the response of workers to being allowed to work from home. In firms granting WfH rights, employees in teleworkable occupations are twice as likely to work from home as those working in less-teleworkable occupations.

Table 6: LFS - Double Difference

	(1) Teleworking	(2) Hours Worked
Treated Firms	-0.078	-2.50***
	(0.049)	(0.55)
Post-CLA	0.0086	-0.42
	(0.030)	(0.33)
Treated*Post-CLA	0.15***	1.71***
	(0.051)	(0.57)
R-Squared	0.09	0.32
No. of Obs	3,793	9,950
Mean	0.17	33.43

Notes:

teleworkable occupation, on the educational performance of their children.[15] Using the LFS and parents of children aged between 8 and 16 leaves us with only 791 observations. Table 7 reports estimates of a model similar to equation 1 including an interaction term between working in a firm granting rights to work from home and having a teleworkable occupation. While the estimates are not significant, they are indicative that the advantage of having parents able to work from home, is larger for children whose parents work in a teleworkable occupation.

CLA, teleworking - heterogeneity Finally, in Table 8 we report estimates of models that assess heterogeneity in the type of parents who work from home. For this section, we keep only parents working in treated firms - columns (1) to (4). We regress a dummy for teleworking on the "Post-CLA" dummy, year and sector fixed effects and sector specific linear time-trend. In this different estimation strategy we report again that the introduction of the policy lead to an increase in working from home. In this specification, the effect is a bit larger and reaches 22% rather than 15% estimated in the DiD specification. We then separately assess heterogeneity by education level (Graduate), gender (female)

[15] It was not possible to conduct this test using our complete sample since the linked employer-employee dataset available to us does not contain information on occupation.

Table 7: LFS - Baseline Effect - Heterogeneity Teleworkable

	(1)	(2)	(3)	(4)	(5)
	Z-Score		Teacher Rec.	Eligible Track	
	Maths	Dutch	Uni Track	Uni	General Sec. & Uni
β	0.0370	0.0959	-0.0170	-0.0731	0.0918
	(0.199)	(0.177)	(0.0913)	(0.0988)	(0.0978)
Teleworkable	0.284***	0.279***	0.145***	0.118***	0.141**
	(0.0857)	(0.0867)	(0.0475)	(0.0433)	(0.0594)
β*Teleworkable	0.154	0.176	0.0944	0.171	0.0750
	(0.303)	(0.263)	(0.168)	(0.157)	(0.149)
R-Squared	0.05	0.05	0.06	0.04	0.06
No. of Obs	791	791	564	791	791
Mean	0.21	0.24	0.21	0.25	0.59

Notes:

and whether children aged less than 16 are present in the household.

None of the interactions are statistically significant but their signs are consistent with expectations. After the WfH policy is introduced, remote work increases only slightly more for educated workers. This contrasts with the clear patterns observed among those in teleworkable occupations, suggesting that broad education categories may be imprecise indicators of telework feasibility.[16] Women are 5 percentage points more likely than men to engage in teleworking after they gain the right to work from home. The greater change in behaviour is observed for parents, who are 13 percentage points more likely than non-parents to use WfH arrangements. The last column of Table 8 reports estimates for the workers at control firms, where we do not find any impact of a placebo introduction of a WfH policy. Overall, while all workers take advantage of the introduction of WfH policies, the largest change in working behaviour is observed for parents and mothers who are 25% and 90% more likely than their peers to report working from home after the policy change.

[16]Note again, that while the more educated increase their working from home propensity as much as the less educated, they started from a higher base. It is important to stress the not all workers similarly take advantage of a WfH policy, as this will have consequences on inequality in child outcomes.

Table 8: LFS - Mechanisms - Working from Home

	(1) Baseline	(2) Educated	(3) Female	(4) Children	(5) Control firms
Post-CLA	0.220***	0.180**	0.201***	0.155**	0.001
	(0.054)	(0.077)	(0.063)	(0.065)	(0.028)
Post-CLA*Educated		0.030			
		(0.084)			
Post-CLA*Female			0.053		
			(0.086)		
Post-CLA*Children				0.133	
				(0.082)	
Intercept - Heterogeneity		0.142*	-0.064	-0.033	
		(0.081)	(0.084)	(0.079)	
R-Squared	0.10	0.13	0.10	0.11	0.08
No. of Obs	1,761	1,720	1,761	1,761	2,031
Mean	0.28	0.28	0.28	0.28	0.16

Notes:

7.2 Parental Labor Market Outcomes

By teleworking parents can improve the academic performance of their children. Without precise time use data it is not possible to fully understand the mechanisms but potential avenues is direct involvement with homework, which could reduce the working time of teleworking parents, or indirect supervision whereby parents can monitor the effort of their children. The latter might not reduce the amount of time spent working but might affect productivity. The LFS allows us to measure reported working time.

Since the question about working time is asked at each quarterly wave, the data becomes a panel with up to five observations per participants, and overall 9,950 observations. We cannot make full use of the panel structure since for most observations there is no change in CLA during the 15 months of observation.[17] Treating the data as pooled cross-sections we estimate model 1. The estimates for the parameters of interest are reported in the second column of Table 6.

[17]Note that the question on teleworking was only asked to one wave of the LFS. Thus, column (1) of Table 6 reports fewer observations than column (2).

Table 9: LFS - Mechanisms - Hours worked

	(1) Baseline	(2) Educated	(3) Female	(4) Children	(5) Control firms
Post-CLA	0.799	-1.331**	0.147	0.868	-0.306
	(0.497)	(0.669)	(0.578)	(0.581)	(0.377)
Post-CLA*Educated		3.042***			
		(0.761)			
Post-CLA*Female			1.746**		
			(0.789)		
Post-CLA*Children				0.177	
				(0.758)	
Intercept - Heterogeneity		-0.898	-7.550***	-2.004***	
		(0.735)	(0.764)	(0.739)	
R-Squared	0.29	0.31	0.29	0.29	0.35
No. of Obs	4,618	4,504	4,618	4,618	5,332
Mean	34.61	34.57	34.61	34.61	32.59

Notes:

Consistent with findings of Bloom et al. (2015), homeworking leads to a 5% increase in the number of hours worked. This increase in hours worked is solely driven by employees in teleworkable occupations who post-CLA reform increase their hours of work by 10% (see in Appendix B Table B.4, Column 2). Finally, we assess heterogeneity in the reaction to gaining the right to work from home by education level, gender and parenting status, again using the sample of workers whose firms changed their WfH policies, and using a simple before-after strategy. These estimates are reported in Table 9. Post-CLA, the less educated workers reduce their hours worked by 1.3 hours while the more educated increase them by 3. This might have to do with differences in tasks that can be completed at home, or differences in supervision allowing some groups to shirk. This gap is also reflected in the ambiguous conclusions in the literature on the productivity effect of WfH. Post-policy, women but not men report less worked hours, suggesting that WfH policies allow women to reduce their constraint on working time.

Finally, while parents work 2 hours less per week than non-parents, their post-policy increase in hours worked is similar to the one observed for non-parents. Again the final Column in 9 reports the estimates for employees at firms that did not change their CLA,

where as expected the placebo reforms have no impact on the number of hours worked.

The introduction of formal WfH policies leads to an increase in hours worked, driven by employees in teleworkable occupations who are likely to be the more educated workers. Women, but not parents, increase their hours worked more than their peers when gaining the right to work from home. Moreover, the event study reported in Figure 1 did not find any negative effects of WfH policies on earnings. Altogether, we do not find evidence that workers, especially parents reduce their hours worked and suffer from lower wage growth in order to increase their investment of the education of their children. This increase investment does not come as a cost to the firm or the parents.

8 Conclusion

Using plausibly exogenous variation in the availability of home working arrangement and administrative records of the results of a national high stake exam, we find that children whose parents become eligible for teleworking improve their score at a that exam by 9% of a standard deviation. The improvement leads to marginal students being allowed to attend higher tracks in secondary education. There is little heterogeneity in this effect by child or parents characteristics. Eligible parents increase their use of teleworking by 15 percentage points, almost doubling the number of workers reporting teleworking. Working from home does not come at a cost to the firm (no drop in hours worked) or workers (no drop in wages).

However, such a policy is not innocuous. While our sample of treated and control is balanced in terms of firms and workers characteristics, it is unrepresentative of the general population. Individuals in firms implementing working from home policies are more educated and higher earners. Even within the treated firms more educated workers are more likely to make use of teleworking, probably because they are in different occupations where tasks can more easily be completed from home. Children with such backgrounds are already performing better in education. As such working from home policies might

increase educational inequality and contribute to a decrease in educational opportunities for children of less educated parents. This would increase inter-generational correlation in education. Indeed Aparicio Fenoll (2022) similarly notes an increase in social gap attainment between children whose parents were employed in teleworkable occupations during the COVID-19 pandemic when schools were closed. As the prevalence of WfH increase, policies to support children whose parents are less likely to be teleworking - which could be approximated by educational level - should be implemented to not increase social gap in educational attainments.

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A Data Annex

A.1 CBS Data

Table A.1: Definition of the main variables

Original data - CBS		Use
Dataset	Variable	
HOOGGSTEOPLTAB	oplnivsoi2016agg4hgmetnirwo	Highest education achieved
GBAADRESOBJECTBUS		Place of living
MEDICIJNTAB	ATC4	entry G03A for contraception
	ATC4	entry M01A for anti-inflammatory
GBAPERSOONTAB	gbageneratie	Generation of immigrants
	gbaherkomstgroepering	Origin country
KINDOUDERTAB		parents-child linkage
GBAVERBINTENISPARTNERBUS		marriages and civil partnerships
POLISBUS/SPOLISBUS	(s)lnlbph	Fiscal wage
BAANSOMMENTAB	fiscloon	Fiscal wage
CITOTAB		
	citobrin_crypt	Primary school FE
	CitoStandaardScore	Cito score Total
	Citoadviesleerkrach	Advice teacher
	citozscoretaal	Cito z-score reading
	citozscorerekenenwiskunde	Cito z-score mathzematics

A.2 CONSTRUCTION OF THE DATASETS

A.2.1 Identification of Firm Names

Collective agreements were sourced from the XpertHR website. Access to the website was granted through a license that permitted automated data extraction.

The names of firms were extracted from the Title field of the agreements. These titles contained either company names or sector names. To account for minor misspellings, including missing vowels, we applied an automated spell-checking correction using Google's auto-correct functionality in a word processing environment.

Matching CLAs information to firms required to extract a firm identifier.

A.2.2 Extracting KvK Identifiers from XpertHR Data

To obtain firm identifiers (KvK numbers), we used a multi-step process:

1. We began with the cleaned list of company names derived from the XpertHR data.
2. Using a custom R script, we scraped the KvK numbers corresponding to these company names using the webiste https://www.kvk.nl/wijzigen/organisatie/. This file provides a mapping between company names and their respective KvK identifiers.
3. Ideally, the firm names in `Correctedname` and `ScrapedKVK` would be identical, allowing for a one-to-one match. However, discrepancies arose due to variations in

name formatting and non-unique matches. In some cases, a single firm name from XpertHR was linked to multiple KvK numbers, with a maximum of 20 matches per company as set by the scraping algorithm.

4. The initial dataset contained 1,143 unique agreements, while the scraping process produced 11,385 potential matches, reflecting the multiple possible matches for certain firms.

5. A manual verification process was undertaken, where firm names from XpertHR (`Correctedname`) were compared against those from the KvK database (`Header`). The verified matches were stored.

6. In cases where a unique match was identified, incorrect records were dropped. For firms with multiple possible matches, all candidate records were retained for further manual inspection.

A.2.3 Integration with CBS Data

Once the firm-level dataset was prepared, it was uploaded onto CBS servers to be linked to employer-employee data. Since the matched employer-employee data uses a different identifier than KvK, additional steps were required to establish a connection between encrypted KvK identifiers and BEID. CBS suggested a procedure that we followed.

B Additional Results

B.1 Heterogeneity of Main Estimates

Table B.2: Heterogeneity

	(1)	(2)	(3)	(4)	(5)
	Z-Score		Teacher Rec.	Eligible Track	
	Maths	Dutch	Uni Track	Uni	General Sec. & Uni
Panel A: Boys					
β	0.088*	0.084	0.023	0.014	0.032
	(0.048)	(0.057)	(0.023)	(0.021)	(0.022)
No. of Obs	7,186	7,186	5,290	7,186	7,186
Panel B: Girls					
β	0.112*	0.139***	0.033	0.015	0.064**
	(0.058)	(0.047)	(0.023)	(0.024)	(0.030)
No. of Obs	7,145	7,145	5,230	7,145	7,145
Panel C: Fathers					
β	0.085**	0.100***	0.041**	0.034*	0.047**
	(0.037)	(0.030)	(0.017)	(0.018)	(0.018)
No. of Obs	9,027	9,027	6,603	9,027	9,027
Panel D: Mothers					
β	0.125**	0.133**	0.002	-0.023	0.049
	(0.053)	(0.062)	(0.029)	(0.029)	(0.030)
No. of Obs	5,304	5,304	3,917	5,304	5,304
Panel E: Fathers - Boys					
β	0.084	0.066	0.045**	0.036	0.020
	(0.060)	(0.071)	(0.022)	(0.023)	(0.027)
No. of Obs	4,561	4,561	3,350	4,561	4,561
Panel F: Fathers - Girls					
β	0.083	0.133**	0.038	0.032	0.073*
	(0.073)	(0.059)	(0.032)	(0.030)	(0.039)
No. of Obs	4,466	4,466	3,253	4,466	4,466
Panel G: Mothers - Boys					
β	0.079	0.101	-0.021	-0.031	0.044
	(0.075)	(0.084)	(0.038)	(0.027)	(0.039)
No. of Obs	2,625	2,625	1,940	2,625	2,625
Panel H: Mothers - Girls					
β	0.154**	0.154*	0.021	-0.019	0.048

	(0.066)	(0.088)	(0.032)	(0.040)	(0.043)
No. of Obs	2,679	2,679	1,977	2,679	2,679
Panel I: Low Educated					
β	0.034	0.072	-0.017	0.007	-0.006
	(0.062)	(0.075)	(0.022)	(0.028)	(0.032)
No. of Obs	3,294	3,294	2,466	3,294	3,294
Panel J: High Educated					
β	0.085	0.047	0.053*	-0.014	0.040
	(0.062)	(0.049)	(0.027)	(0.034)	(0.029)
No. of Obs	5,332	5,332	3,788	5,332	5,332
Panel K: Early Implementation					
β	0.059	0.039	0.034	0.013	0.026
	(0.043)	(0.042)	(0.021)	(0.025)	(0.022)
No. of Obs	7,228	7,228	5,194	7,228	7,228
Panel L: Late Implementation					
β	0.057*	0.112***	-0.002	-0.009	0.039**
	(0.029)	(0.030)	(0.017)	(0.015)	(0.018)
No. of Obs	7,103	7,103	5,326	7,103	7,103
Panel M: Below Median Wage					
β	0.025	0.071	0.001	-0.020	0.015
	(0.043)	(0.044)	(0.023)	(0.016)	(0.021)
No. of Obs	7,165	7,165	5,359	7,165	7,165
Panel N: Above Median Wage					
β	0.121*	0.090**	0.041*	0.028	0.055*
	(0.061)	(0.043)	(0.023)	(0.024)	(0.028)
No. of Obs	7,166	7,166	5,161	7,166	7,166

Notes:

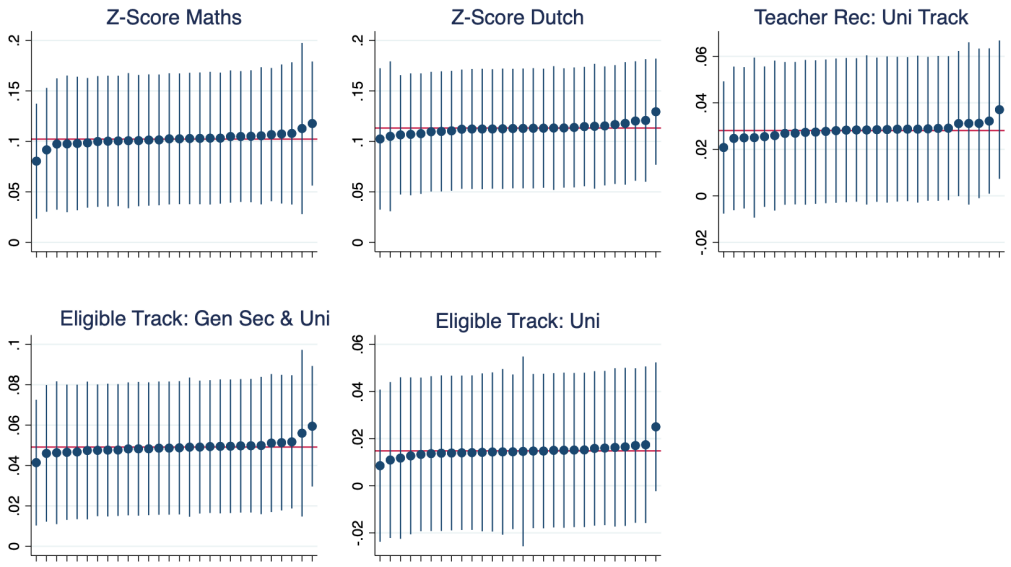
B.2 Jackknife estimates

In this section we report the estimates from Jackknife estimations whereby we estimate the model for all possible sub-sample where one treated firm, and its associated control firms have been excluded. The graphs report the point estimates, and the associated standard errors separately for the Math and Reading tests.

B.3 Alternative Matching

In this robustness check we match the treated firms, using the same set of control variables as in the main analysis but keep the best four matches, rather than the best three. As in the main analysis, the firm characteristics are measured at period *T-1* and an exact

Figure B.1: **Jackknife of the Estimates of Working from Home on Normalised Test Scores Math and Reading.**



Notes:

match on industrial sector is imposed. We then re-estimate Equation (1) with this new set of control firms.

B.4 Working from Home by Teleworkable Occupation

Table B.3: Robustness Checks: Alternative Sample and Matching Algorithms

	(1)	(2)	(3)	(4)	(5)
	Z-Score		Teacher Rec.	Eligible Track	
	Maths	Dutch	Uni Track	Uni	General Sec. & Uni
Panel A: With Controls - 3 Matches					
β	0.09***	0.093***	0.025	0.003	0.045***
	(0.034)	(0.031)	(0.018)	(0.017)	(0.017)
No. of Obs	11,815	11,815	8,718	11,815	11,815
Mean	0.05	0.10	0.16	0.20	0.53
Panel B: With Controls - Alt1					
β	0.086***	0.092***	0.039**	0.037**	0.038**
	(0.032)	(0.034)	(0.017)	(0.016)	(0.018)
No. of Obs	13,431	13,431	9,696	13,431	13,431
Mean	0.12	0.17	0.20	0.22	0.56
Panel C: With Controls - Alt2					
β	0.099***	0.105***	0.039**	0.022	0.041**
	(0.033)	(0.032)	(0.016)	(0.016)	(0.017)
No. of Obs	15,934	15,934	11,725	15,934	15,934
Mean	0.03	0.07	0.17	0.20	0.51
Panel D: Robustness - 2 Years Prior					
β	0.11***	0.112***	0.029*	0.019	0.046**
	(0.036)	(0.032)	(0.017)	(0.017)	(0.019)
No. of Obs	13,345	13,345	9,794	13,345	13,345
Mean	0.05	0.09	0.17	0.20	0.52

Notes:

Table B.4: LFS - Triple Difference - Teleworkable Occupations

	(1) Teleworking	(2) Hours Worked
Treated Firms	-0.066	-0.615
	(0.065)	(0.733)
Post-CLA	0.008	-0.424
	(0.036)	(0.407)
Treated*Post-CLA	0.101	0.029
	(0.068)	(0.767)
Teleworkable*Treated	-0.026	-3.972***
	(0.084)	(0.948)
Teleworkable*Post-CLA	-0.005	0.103
	(0.047)	(0.527)
Treated*Post-CLA*Teleworkable	0.097	3.262***
	(0.089)	(1.002)
R-Squared	0.11	0.34
No. of Obs	3,793	9,950
Mean	0.17	33.43

Notes: