When Parents Work from Home *

Pascal Achard†
Michele Belot‡
Arnaud Chevalier§

April 3, 2025

Abstract

This paper estimates the causal effect of parental right to work from home (WfH) on their children's educational attainment. Using administrative data from the Netherlands and firm-specific information on collective agreements, we find that children whose parents gain the right to WfH improve their scores at a high- stake exam by 0.09 of a standard deviation. This results in a 4 percentage points upswing in qualifying for a general or academic track in secondary school. Additionally, we link the collective agreement data to the labor force survey, and find that change in WfH policies are associated with a 15 percentage points increase in WfH propensity, but no change in other labor market outcomes. These results highlight the large potential benefits of remote work in supporting families and their children.

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

*We thank Astrid Ruland, Anatole Chesson and Vaios Tryantafillou for excellent research assistance. We also thank the audiences at various presentations for their interest and comments.
†CREST. Email: Pascal.ACHARD@ensae.fr
‡Cornell University, CEPR and IZA. Email: mb2693@cornell.edu
§Royal Holloway University of London, CES-Ifo and IZA. Email: arnaud.chevalier@rhul.ac.uk

1 Introduction

Remote work has become a prevalent phenomenon in the labor market, with about 25% of days worked from home in the US in 2024 (Buckman et al., 2025).[1]. However, recent backlash over its impact on productivity have resulted in companies, such as Amazon, Disney or Goldman Sachs, and the federal government, changing their policies back to a strict 5-day in office policy for federal workers. So far, the literature has mostly described the demand for remote working and investigated its impact on workers' productivity. The economic impact of Wfh is however potentially much larger than its direct impact on workers' productivity.[2] To date, very few studies have examined these other economically relevant outcomes. Recent work by Angelici and Profeta (2024); Bellmann and Hübler (2021); Costi et al. (2024); Goux and Maurin (2024) provides conflicting evidence on the effect of Wfh on workers' health or well-being. In this paper, we expand the literature on the causal effect of WfH by investigating potential externalities to other family members. Specifically, we focus on the impact of homeworking on children's educational performance.

WfH is viewed by workers as a positive amenity; Mas and Pallais (2017); Barrero et al. (2021), or Aksoy et al. (2023) estimate workers willingness to pay for teleworking at between 5% to 8% of their wage, as they value reduced commuting and better work-life balance, especially among parents and women.[3] Wfh is often mentioned as a 'family-friendly' policy[4] and evidence from time-use surveys shows that teleworkers spend more time with family (Giménez-Nadal et al., 2018; Pabilonia and Vernon, 2022; Aksoy et al.,

[1] See also (Bick et al., 2023; Chen et al., 2023) for a discussion of recent trends in Wfh

[2] 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; a point stressed in (Barrero et al., 2023)

[3] or Cullen et al. (2025) estimate a willingness to pay as high as 25% for IT workers.

[4] 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), or (**?**) for evidence that Wfh is associated with reductions in the gender wage gap

2023). Increased parental interactions 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) or tutoring. Additionally, working from home allows parents better monitoring of their children's effort towards their studies.

Evaluating the impact of Wfh (on any outcome of interest) is challenging, as employees self-select into work arrangements that suit their needs and those of their families. In this study, we leverage plausibly exogenous changes to Collective Labor Agreements (*Collectieve Arbeidsovereenkomst*, CLA) signed between workers' representative and firms (or employers' organisations) in the Netherlands, which is the country in Europe with the highest share of workers teleworking at least some days (52%). 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 the two decades prior to the COVID-19 pandemic.[5] To account for selection into 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 have formal provisions for WfH in their CLA.

Having identified a set of treated and control firms, we use the national employer/employee register, and select employees already in place at least one year before the implementation of the agreement, who unexpectedly gain more flexibility in their work schedule; this is our treatment group. The control group consists of people working in matched firms that did not adopt WfH provisions in their CLA. We then restrict the sample to parents of children aged 8 to 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

[5]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.

CLA. To alleviate these concerns, we take advantage of a feature of the Dutch educational system: test-based school tracking. During their last year of primary school - age 12 - most Dutch children sit a national standardized test (CITO) which largely determines the track they will be allowed to attend in secondary education. Thus, it is a very salient high-stake exam. Since parents 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 sit the test, the allocation of treatment to the child is as good as random.

Our identification comes from comparing the CITO scores 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 matched to our control 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 existing practices regarding WfH and that they 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. Looking at these oucomes together with educational outcomes of the children helps us getting a broader picture of how family-friendly policies affect families.

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 experience no change in key labour market outcomes such as hours or wages, and have similar mobility patterns as other parents. There appears to be no 'economic' cost to the parents or firms and large gains to the children when their parents are able to work from home.

To our knowledge, this is the first paper presenting causal evidence on the impact of WfH arrangements on children's outcomes.[6] 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 and at critical ages.

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

[6] 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.

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).[7] 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 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

[7]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.

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, pupils take a standardized test, the most common is the CITO exam.[8] It 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.,

[8]Until 2015, the CITO was the sole standardized test, with 90% of students taking it. Since the introduction of alternative final tests, its participation rate has declined, and currently, between 50% and 60% of students sit for the CITO exam.

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*. These records were linked to the Dutch National Employer-Employee database, maintained by the National Statistical Office (CBS), using firm identifiers. Additionally, we connected children who took the CITO exam to their parents and integrated this information with the employer-employee database to construct our main dataset.[9]

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 to its national business register number (Kamer van Koophandel, KvK) for 513 entries.

Since we use firm identifiers, sector-level agreements cannot be identified. As a result, we adopt a matching strategy that accounts for this limitation. Specifically, when selecting control firms, we match strictly on sector to ensure that sector-wide changes in WfH

[9]Further details on the datasets and variables used are provided in Appendix A.1. Appendix A.2 details the steps from extracting information from XpertHR to merging it with CBS data.

availability impact both treated and control firms similarly. This approach also enhances comparability by ensuring that firms operate within the same sector. Therefore, our identification strategy relies on comparing firms that adopted WfH policies with firms in the same sector that did not.

Since the matched employer-employee data starts in 2006, we limit our dataset to CLAs starting at 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. The sample is limited to firms employing individuals whose children took the CITO during the relevant years. As a result, our final sample is further refined from 39 to 28 firms. Further information on the creation of the data is available in Annex A.2.

The identified firms are then matched with four firms from the same sector based on the smallest Mahalanobis distance - calculated using firm characteristics the year prior to the change in WfH arrangements (hereafter referred to as T-1). This leaves us with a sample of 111 firms matched 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. Imposing restrictions on children taking the CITO reduces the sample to 86 control firms, so 114 in total.[10]

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

[10]Picking 4 matches for 28 firms should give 112 controls. One firm could only be matched to three controls, two control firms were selected for two different treated firms. Imposing restrictions on children taking the CITO reduced the sample further.

parents to invest in the human capital of their children and will be considered as placebo policies.

4.2 Administrative Registries

We identify the children of parents employed at a treated or control firm in the year T-1 and retrieve their scores from 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 one day a week. The LFS respondents are linked to the Employer-Employee database, allowing us to assess how CLAs impact the likelihood of working 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

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

other characteristics, making this comparison meaningless.

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 driven by birth year variations and firm-level policy changes, can be considered quasi-random.[11] 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. The former group is considered treated and the latter control.

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 use a Difference-in-Differences strategy. First, we account for selection into firms that adopt WfH policies by comparing children whose parents work in treated firms but, due to their age at the time of implementation, are either affected by the policy or not. Second, we control for age-related effects by applying the same comparison to firms that did not implement a 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 De Chaisemartin and d'Haultfoeuille (2023). Moreover, we are not considering the roll

[11] 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.

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.[12]

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. Matching is based on characteristics measured in the year before WfH arrangements were formalized in the collective labor agreement (T-1). This helps minimize the risk of worker selection being influenced by WfH policies and ensures that the covariates are not 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.[13] 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.

[12]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.

[13]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 conduct two balancing tests. First, we compare treated firms to all other firms to assess external validity. Then, we repeat the tests on treated firms and their matched controls to ensure the sample is internally well-balanced. 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 not representative.

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 earnings (+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.

In Panel B, we conduct balancing tests on the sample of parents. The treated and

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: This table presents summary statistics for firms, parents and children in the Netherlands. Data come from Dutch labor market and education records covering the years 2006–2019. The statistics are obtained from regressions of the variable in each row on a dummy indicator for being part of the experiment (i.e., employed in a firm that implemented teleworking provisions in its Collective Labor Agreement or one of the firm selected as its control). Standard errors are in parenthesis. See Section 5.1 for details on sample selection. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 and 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 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.[14]

Across the four outcomes—earnings, labor market participation, part-time work, and

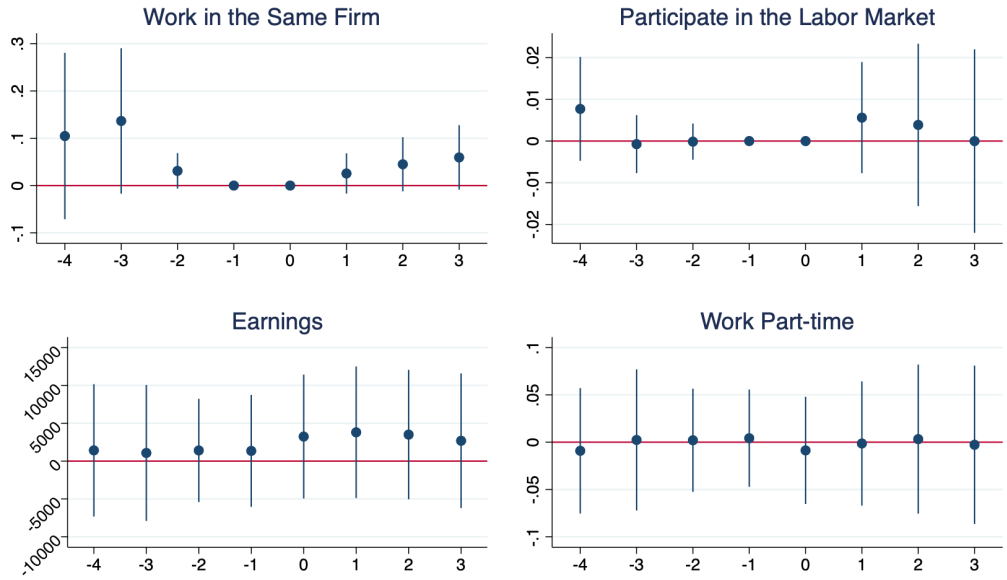
[14]Figure 1 includes parents of both younger and older children. Note that the picture remains the same when one looks only atyounger or older children.

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: This table reports balancing tests between treated and control firms, parents, and children. Each column represents a separate regression with a binary treatment indicator as the dependent variable. Control firms are matched strictly on sector and year, and by Mahalanobis distance on firm characteristics. See Section 5.1 for details on sample selection. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure 1: Pre-trend in Labour Market Outcomes



Notes: This figure plots the estimated impact of parental teleworking on their own labor market situation. Each sub-figure presents results for a specific outcome. Coefficients in each time period come from a separate regression of the outcome on a treatment dummy. Standard errors are clustered at the firm level The sample consists of parents of the children included in Table 3.

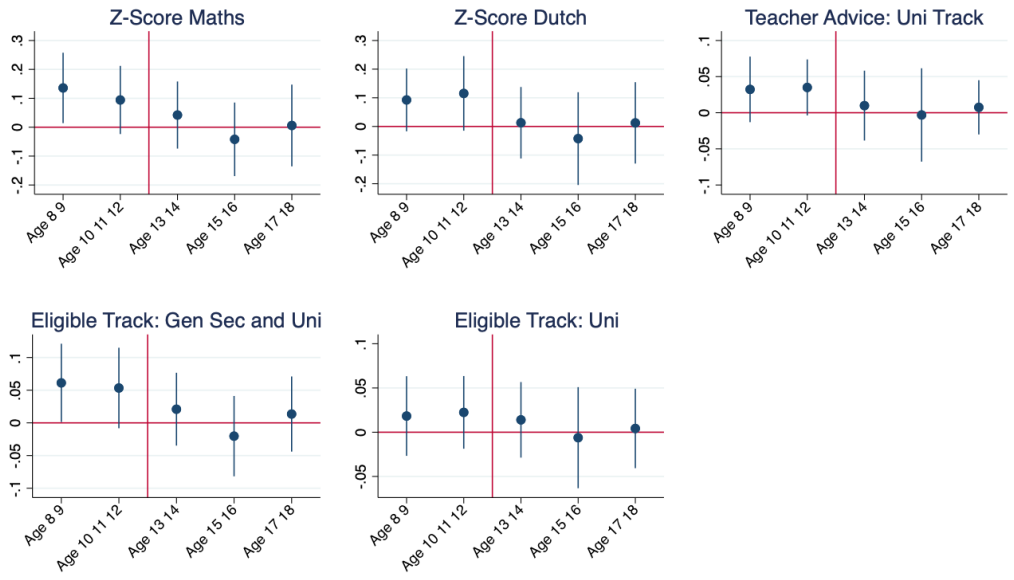
the likelihood of staying with the same firm—we find no statistically significant differences in trends between parents in treated and control firms. This mitigates concerns about workers sorting into firms in anticipation of WfH policies and any differential income trends that could indirectly affect children's educational attainment. Prior to the policy implementation, differences in earnings are small 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.[15]

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

[15]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, 15–16 and 17-18). 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: This figure plots the estimated impact of parental teleworking on their children's educational achievement. Each sub-figure presents results for a specific outcome. Coefficients in each time period come from a separate regression of the outcome on a treatment dummy. Time periods corresopnd to the age children were at the time of the change in CLA. Standard errors are clustered at the firm level. The sample consists of children included in Table 3.

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 educational placement - specifically 545 for the academic track and 537 for the general secondary and academic tracks as opposed to the vocational one - and the initial teacher's track recommendation - whether she recommends the academic track.[16] 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.

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

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 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. 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 better account for potential selection bias among parents employed in various types of firms and to enhance the comparability of parents, our favored specification includes a match-specific fixed effect. It compares outcomes for the children of parents in a treated firm against those of children whose parents work 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.[17]

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. They point 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,

[17]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: This Table reports estimates based on equation 1. Each column corresponds to a different outcome and each row to a different specification. The outcomes include the normalized CITO scores in Dutch and mathematics, a binary indicator for whether the teacher recommends the university preparatory track, and dummies for achieving a score qualifying for general secondary education or the university track. Panel A only includes CITO year fixed effects, panel B adds individual controls (age and gender of the parents and the children), panel C adds sector fixed effects and panel D replaces the sector fixed effect with a match-specific fixed effect. Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 to capture parents' selection. In our favoured specification - controlling for match-specific effects, 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 improvements in the middle of the distribution, pushing scores above the 537 threshold, which makes pupils eligible for the general secondary track or higher. However, these gains do not raise scores beyond 545, the cutoff for 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.

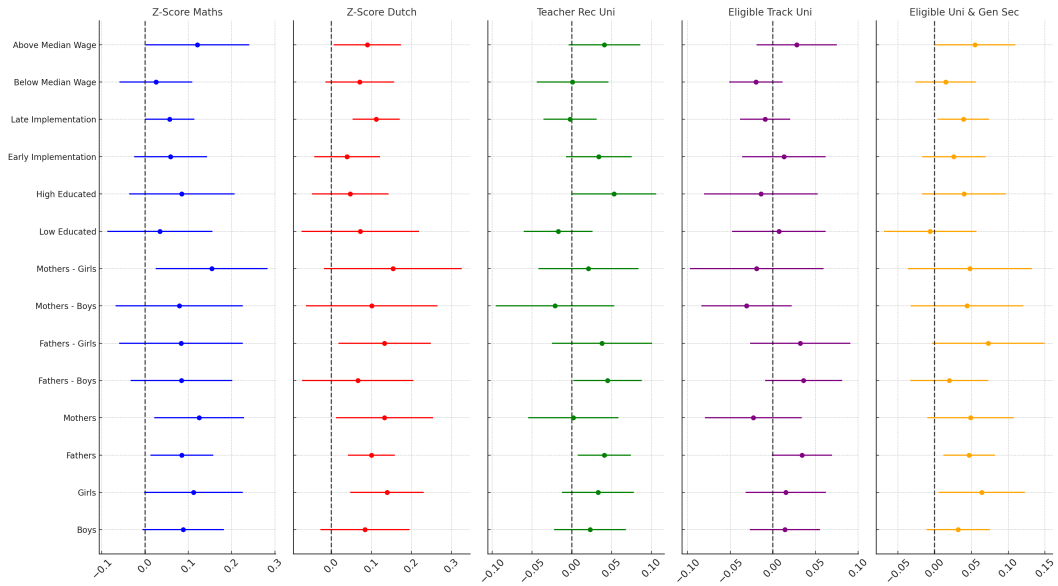
6.2 Heterogeneity

We examine whether the effect varies across different dimensions, including the gender of the parent, the gender of the child, the education level of the parent, the wage 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 reduces the 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: This figure presents heterogeneity in the estimated effect of teleworking, based on the specification in Table 3, Panel B. Each estimate comes from a separate regression. The sub-figures correspond to different outcome variables. They include the normalized CITO scores in Dutch and mathematics, a binary indicator for whether the teacher recommends the university preparatory track, and dummies for achieving a score qualifying for general secondary education or the university track. The rows represent different parental characteristics, namely the gender of the parent, the gender of the child, the education level of the parent, the wage level of the parent, and the period when the right to WfH was granted. Standard errors are clustered at the firm level.

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 unclear that 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 [18]. 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.[19]

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

[18] Guryan et al. (2008) documents that more educated parents spend more time with their children

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

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 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 are 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 some unobserved characteristics of parents - the estimates drop to 0.08 and 0.16 in the alternative identification strategy, more similar to the 0.10 that was estimated in the DiD specification. This confirms the importance of controlling for unobservable parental characteristics associated with their choice of workplace.

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, there are gains in incorporating the second difference in the DiD approach.

6.3.2 Alternative matching

To further test the robustness of the results to the initial matching strategy, we test alternative ones. 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. Firms 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 on firm size, share of high

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: This table presents estimates from an alternative identification strategy that uses only treated firms. Each column corresponds to a different outcome, including normalized CITO scores in Dutch and Mathematics, teacher recommendations for the university preparatory track, and dummies for achieving a score qualifying for general secondary or university tracks. Panel A includes CITO year fixed effects. Panel B adds individual controls (age and gender of parents and children). Panel C includes sector fixed effects. Panel D presents placebo estimates, assuming the policy was implemented three years earlier, using children aged 13–15 as the treatment group and children aged 16–18 as the control group. Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 as compared to one year in the baseline sample. This allows 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: (i) the right to alter the work pattern to allow for the informal care of a family member, (ii) the right to take leave to provide care for a family member and (iii) 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. We use the same matching algorithms as for WfH provisions. In Table 5, we report estimates of equation 1 for each of these rights. None of them leads to statistically significant improvement in test scores. The estimates are small and symmetric around zero - they 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).[20] If the estimated effect in our main analysis are causal 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.

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 WfH 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 university, 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

[20]Note, that we keep the same matched firms as in our main analysis rather than match firms based on their characteristics in year *T-4*. Any differences in the estimates cannot be driven by changes in the comparison group.

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: This table presents placebo estimates using alternative labor policies that provide flexibility but are unlikely to impact parental investment in children's education. Each column corresponds to a different outcome, including normalized CITO scores in Dutch and mathematics, teacher recommendations for the university preparatory track, and dummies for achieving a score qualifying for general secondary or university tracks. Each panel reports estimates from separate regressions based on the specification in Table 3, Panel B. Panel A considers the right to informal care, Panel B examines short-term leave for caregiving, and Panel C evaluates split parental leave policies. Panel D presents an additional placebo test assuming the work-from-home policy was implemented three years earlier. Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 identify 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 one day 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 compared to the mean of the control group. It substantiates 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.[21]

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: This table presents estimates of the effect of teleworking policies on parental work arrangements. Column (1) reports the impact on the probability of working from home, while column (2) examines total hours worked. The treatment variable indicates whether the parent was employed in a firm that introduced a teleworking provision in its Collective Labor Agreement (CLA). The Post-CLA dummy captures the period after the policy change, and the interaction term identifies the differential effect for treated firms. The sample consists of individuals working in treated and control firms who were interviewed in the Dutch Labor Force Survey (LFS). Significance levels: *** p $<$ 0.01, ** p $<$ 0.05, * p $<$ 0.10.

[21] 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 term to estimate a triple difference in difference. The estimates are reported in table B.4 available in Appendix B. Although they are imprecise, they 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.

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 teleworkable occupation, on the educational performance of their children.[22] Merging the LFS to our baseline sample leaves us with only 791 observations of chldren aged 8 to 16 at the time of a change in CLA. 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.

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: This table presents estimates of the effect of parental teleworking on children's educational outcomes, examining heterogeneity based on whether the parent's occupation is classified as teleworkable. Each column corresponds to a different outcome, including normalized CITO scores in Dutch and mathematics, teacher recommendations for the university preparatory track, and dummies for achieving a score qualifying for general secondary or university tracks. The treatment variable indicates whether the parent was employed in a firm that introduced a teleworking provision in its Collective Labor Agreement (CLA). The interaction term captures the differential effect for parents in teleworkable occupations. The sample consists of children from Table 3 whose parents were interviewed in the Dutch Labor Force Survey (LFS). Standard errors are clustered at the firm level. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.10.

[22] 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.

CLA, teleworking - heterogeneity Finally, in Table 8 we report which type of workers is more likely to work from home afetr a change in CLA. For this section, we keep only individuals 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 specification, we report again that the introduction of the policy lead to an increase in working from home. 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) and whether children aged less than 16 are present in the household.

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: This table presents estimates of the effect of teleworking policies on parental work arrangements, examining heterogeneity by education level, gender, and parental status. Columns (1)–(4) report results for treated firms, introducing interaction terms for education (college-educated), gender (female), and whether the individual has children. Column (5) reports a placebo test using only control firms. The Post-CLA dummy captures the period after the policy change, and the interaction terms measure differential effects for each subgroup. The specification includes year and sector fixed effects as well as sector-specific linear time trends. The sample consists of individuals working in treated and control firms who were interviewed in the Dutch Labor Force Survey (LFS). Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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.[23] 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.

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 and to investigate this possibility.

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.[24] Treating the data as pooled cross-sections we estimate model 1. The estimates for the parameters of interest are reported

[23] 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.

[24] 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).

in the second column of Table 6.

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: This table presents estimates of the effect of teleworking policies on total hours worked, examining heterogeneity by education level, gender, and parental status. Columns (1)–(4) report results for treated firms, introducing interaction terms for education (college-educated), gender (female), and whether the individual has children. Column (5) reports a placebo test using only control firms. The Post-CLA dummy captures the period after the policy change, and the interaction terms measure differential effects for each subgroup. The specification includes year and sector fixed effects as well as sector-specific linear time trends. The sample consists of individuals working in treated and control firms who were interviewed in the Dutch Labor Force Survey (LFS). Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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.

References

Abadie, Alberto and Jann Spiess, "Robust post-matching inference," *Journal of the American Statistical Association*, 2022, *117* (538), 983–995. 20

Aksoy, Cevat Giray, Jose Maria Barrero, Nicholas Bloom, Steven J Davis, Mathias Dolls, and Pablo Zarate, "Time savings when working from home," in "AEA Papers and Proceedings," Vol. 113 American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203 2023, pp. 597–603. 1, 23

Angelici, Marta and Paola Profeta, "Smart working: work flexibility without constraints," *Management Science*, 2024, *70* (3), 1680–1705. 1, 4

Arkhangelsky, Dmitry, Susan Athey, David A Hirshberg, Guido W Imbens, and Stefan Wager, "Synthetic difference-in-differences," *American Economic Review*, 2021, *111* (12), 4088–4118. 12

Atkin, David, Antoinette Schoar, and Sumit Shinde, "Working from home, worker sorting and development," Technical Report, National Bureau of Economic Research 2023. 1, 4, 5

Barrero, Jose Maria, Nicholas Bloom, and Steven J Davis, "Why working from home will stick," Technical Report, National Bureau of Economic Research 2021. 1

Barrero, José María, Nicholas Bloom, and Steven J Davis, "The Evolution of Work from Home-José María Barrero, Nicholas Bloom, and Steven J. Davis," 2023. 1

Bellmann, Lutz and Olaf Hübler, "Working from home, job satisfaction and work–life balance–robust or heterogeneous links?," *International journal of manpower*, 2021, *42* (3), 424–441. 1

Bernal, Raquel and Michael P Keane, "Child care choices and children's cognitive achievement: The case of single mothers," *Journal of Labor Economics*, 2011, *29* (3), 459–512. 2

Bick, Alexander, Adam Blandin, and Karel Mertens, "Work from home before and after the COVID-19 outbreak," *American Economic Journal: Macroeconomics*, 2023, *15* (4), 1–39. 1

Bloom, Nicholas, James Liang, John Roberts, and Zhichun Jenny Ying, "Does working from home work? Evidence from a Chinese experiment," *The Quarterly journal of economics*, 2015, *130* (1), 165–218. 1, 4, 36

_ , **Ruobing Han, and James Liang**, "Hybrid working from home improves retention without damaging performance," *Nature*, 2024, pp. 1–6. 5

Buckman, Shelby, Jose Maria Barrero, Nicholas Bloom, and Steven J Davis, "Measuring Work from Home," Technical Report, National Bureau of Economic Research 2025. 1

Chaisemartin, Clément De and Xavier d'Haultfoeuille, "Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey," *The Econometrics Journal*, 2023, *26* (3), C1–C30. 11

Chen, Yuting, Patricia Cortes, Gizem Kosar, Jessica Pan, and Basit Zafar, "The Impact of COVID-19 on Workers' Expectations and Preferences for Remote Work," *AEA Papers and Proceedings*, May 2023, *113*, 556–61. 1

Choudhury, Prithwiraj, Cirrus Foroughi, and Barbara Larson, "Work-from-anywhere: The productivity effects of geographic flexibility," *Strategic Management Journal*, 2021, *42* (4), 655–683. 1, 4

_ , **Tarun Khanna, Christos A Makridis, and Kyle Schirmann**, "Is hybrid work the best of both worlds? Evidence from a field experiment," *Review of Economics and Statistics*, 2024, pp. 1–24. 5

Costi, Chiara, Andrew Clark, Conchita D'Ambrosio, Anthony Lepinteur, and Giorgia Menta, "Return-to-Office Mandates, Health and Well-Being: Evidence from a Natural Experiment," Technical Report, IZA Discussion Paper 2024. 1

Cowan, Benjamin, "Time use, college attainment, and the working-from-home revolution," *Journal of Population Economics*, 2024, *37* (3), 60. 23

Cullen, Zoe B., Bobak Pakzad-Hurson, and Ricardo Perez-Truglia, "Home Sweet Home: How Much Do Employees Value Remote Work?," Technical Report, National Bureau of Economic Research 2025. 1

Dutcher, E Glenn, "The effects of telecommuting on productivity: An experimental examination. The role of dull and creative tasks," *Journal of Economic Behavior & Organization*, 2012, *84* (1), 355–363. 5

Emanuel, Natalia and Emma Harrington, "Working remotely? Selection, treatment, and the market for remote work," *American Economic Journal: Applied Economics*, 2024, *16* (4), 528–559. 1, 5

Fenoll, Ainoa Aparicio, "The uneven effect of covid school closures: Parents in teleworkable vs. non-teleworkable occupations," Technical Report, IZA Discussion Paper 2022. 38

Fiorini, Mario and Michael P Keane, "How the allocation of children's time affects cognitive and noncognitive development," *Journal of Labor Economics*, 2014, *32* (4), 787–836. 2

Fort, Margherita, Andrea Ichino, and Giulio Zanella, "Cognitive and noncognitive costs of day care at age 0–2 for children in advantaged families," *Journal of Political Economy*, 2020, *128* (1), 158–205. 2

Gathmann, Christina, Ines Helm, and Uta Schönberg, "Spillover effects of mass layoffs," *Journal of the European Economic Association*, 2020, *18* (1), 427–468. 12

Gibbs, Michael, Friederike Mengel, and Christoph Siemroth, "Work from home and productivity: Evidence from personnel and analytics data on information technology professionals," *Journal of Political Economy Microeconomics*, 2023, *1* (1), 7–41. 1, 5

Giménez-Nadal, José Ignacio, José Alberto Molina, and Jorge Velilla, "Telework, the timing of work, and instantaneous well-being: evidence from time use data," 2018. 1

Ginja, Rita, Jenny Jans, and Arizo Karimi, "Parental leave benefits, household labor supply, and children's long-run outcomes," *Journal of Labor Economics*, 2020, *38* (1), 261–320. 4

Golsteyn, Bart HH and Trudie Schils, "Gender gaps in primary school achievement: a decomposition into endowments and returns to IQ and non-cognitive factors," *Economics of Education Review*, 2014, *41*, 176–187. 23

Goux, Dominique and Eric Maurin, "Sick of Working from Home?," Technical Report, Institute of Labor Economics (IZA) 2024. 1, 5

Gupta, Nabanita Datta and Marianne Simonsen, "Non-cognitive child outcomes and universal high quality child care," *Journal of public Economics*, 2010, *94* (1-2), 30–43. 2

_ **and** _ , "Academic performance and type of early childhood care," *Economics of Education Review*, 2016, *53*, 217–229. 2

Guryan, Jonathan, Erik Hurst, and Melissa Kearney, "Parental education and parental time with children," *Journal of Economic perspectives*, 2008, *22* (3), 23–46. 24

Kleven, Henrik, Camille Landais, Johanna Posch, Andreas Steinhauer, and Josef Zweimüller, "Do family policies reduce gender inequality? Evidence from 60

years of policy experimentation," Technical Report, National Bureau of Economic Research 2020. 4

Kraft, Matthew A, "Interpreting effect sizes of education interventions," *Educational researcher*, 2020, *49* (4), 241–253. 22

Mas, Alexandre and Amanda Pallais, "Valuing alternative work arrangements," *American Economic Review*, 2017, *107* (12), 3722–3759. 1

Nagler, Markus, Johannes Rincke, and Erwin Winkler, "Working from home, commuting, and gender," *Journal of Population Economics*, 2024, *37* (3), 1–23. 1

Pabilonia, Sabrina Wulff and Victoria Vernon, "Telework, wages, and time use in the United States," *Review of Economics of the Household*, 2022, *20* (3), 687–734. 1

Persson, Petra and Maya Rossin-Slater, "When dad can stay home: fathers' workplace flexibility and maternal health," *American Economic Journal: Applied Economics*, 2024, *16* (4), 186–219. 4

Powell, David, Paul Bingley, Petter Lundborg, Stéphanie Vincent Lyk-Jensen, Robert G Valletta, Leila Bengali, Catherine van der List, Sanni Breining, Joseph Doyle, David N Figlio et al., "Parental Leave Benefits, Household Labor Supply, and Children's Long-Run Outcomes." 4

Timmermans, AC, MA Dijks, and MJ Warrens, "Are track recommendations dependent on schools and school boards? A study of trends in the level of track recommendations, number of double recommendations and reconsiderations in Dutch urban and rural areas," *British Educational Research Journal*, 2023, *49* (3), 613–647. 7

Yang, Longqi, David Holtz, Sonia Jaffe, Siddharth Suri, Shilpi Sinha, Jeffrey Weston, Connor Joyce, Neha Shah, Kevin Sherman, Brent Hecht et al., "The effects of remote work on collaboration among information workers," *Nature human behaviour*, 2022, *6* (1), 43–54. 5

A Data Annex

A.1 CBS Data

Table A.1: Definition of the main variables

Original data - CBS		Use
Dataset	Variable	
HOOGGSTEOPLTAB	oplnivsoi2021agg4hgmetnirwo	Highest education achieved
KINDOUDERTAB		parents-child linkage
POLISBUS/SPOLISBUS	(s)lnlbph	Fiscal wage
	(s)aantverlu	Number of hours worked
	(s)sect	Sector
	(s)beid	Firm id from General Business Register (ABR)
CITOTAB	citobrin_crypt	Primary school FE
	CitoStandaardScore	Cito score Total
	Citoadviesleerkrach	Advice teacher
	citozscoretaal	Cito z-score reading
	citozscorerekenenwiskunde	Cito z-score mathzemathics
EBBNW (LFS)	ebbflthuiswerk	Teleworking
	ebbpb8urenwerk	Hours worked

A.2 Construction of the Datasets

A.2.1 Extracting Information from Collective Labor Agreements

- We collected data on collective labor agreements (CLAs) from the XpertHR website, which we accessed through a licensed agreement that allowed automated data extraction.
- Since the CLAs did not have numerical identifiers, we extracted the "Title" of each agreement along with relevant clauses. In many cases, the title corresponded to the name of a firm.

A.2.2 Matching KvK Identifiers to Firm Names from CLA Data

- To link KvK identifiers to firm names, we developed a custom program that searched for KvK numbers on the website https://www.kvk.nl/wijzigen/organisatie/. This allowed us to create a mapping between firm names and their respective KvK identifiers.
- Ideally, the result from this search would be a unique mapping between a firm's name and a KvK identifier. However, discrepancies arose due to variations in name formatting and instances where multiple firms shared the same or similar names. Our scraping algorithm allowed a maximum of 20 matches per company.
- The initial dataset consisted of 1,143 unique agreements. However, the scraping process (from the website https://www.kvk.nl/wijzigen/organisatie/) yielded 11,385 potential matches, reflecting the multiple possible KvK numbers associated with certain firms.
- A manual verification process was conducted to ensure accuracy. Firm names extracted from XpertHR were cross-checked against names in the KvK database. Verified matches were stored.

A.2.3 Integration with CBS Data

- Once the firm-level dataset was finalized, it was uploaded to CBS servers for integration with employer-employee data.
- Since CBS uses a different identifier system than KvK, additional steps were needed to link the encrypted KvK identifiers to BEID. We followed a procedure recommended by CBS to establish this connection.

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

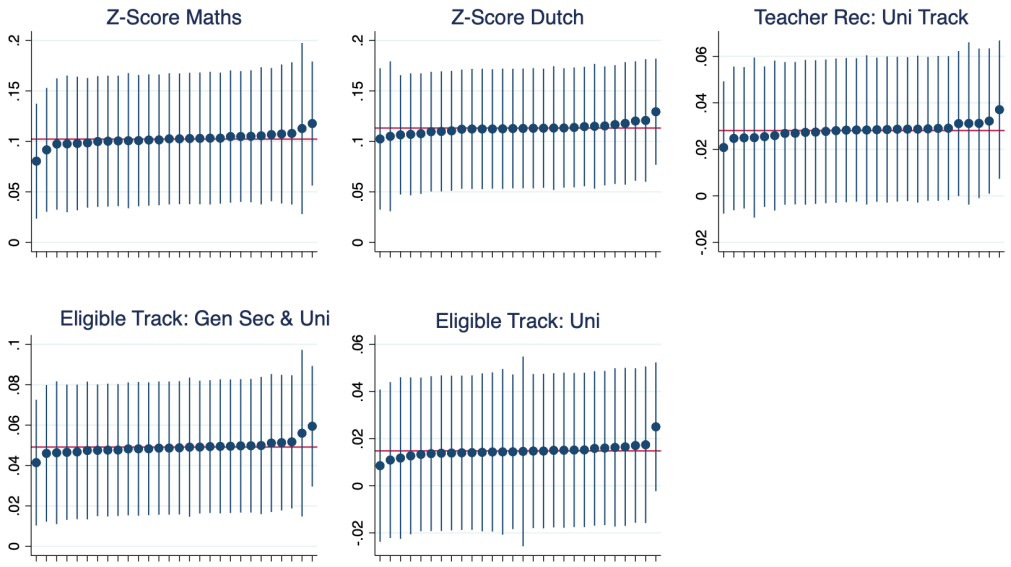
	(0.066)	(0.088)	(0.032)	(0.040)	(0.043)
No. of Obs	2,679	2,679	1,977	2,679	2,679
Panel I: Low Educated					
β	0.034	0.072	-0.017	0.007	-0.006
	(0.062)	(0.075)	(0.022)	(0.028)	(0.032)
No. of Obs	3,294	3,294	2,466	3,294	3,294
Panel J: High Educated					
β	0.085	0.047	0.053*	-0.014	0.040
	(0.062)	(0.049)	(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: This table presents estimates of the effect of parental teleworking on children's educational outcomes, examining heterogeneity by the gender of the parent, the gender of the child, the education level of the parent, the wage level of the parent, and the period when the right to telework was granted. Each column corresponds to a different outcome, including normalized CITO scores in Dutch and mathematics, teacher recommendations for the university preparatory track, and dummies for achieving a score qualifying for general secondary or university tracks. Each estimate comes from a separate regression based on the specification in Table 3, Panel B. Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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.

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



Notes: This figure presents a jackknife sensitivity analysis, assessing whether the estimated impact of parental teleworking on children's educational outcomes is driven by specific treated firms. Each point represents the estimated effect of teleworking on a given outcome, obtained by sequentially omitting one treated firm (and its matched control firms) from the sample. The outcomes include normalized CITO scores in Dutch and mathematics, teacher recommendations for the university preparatory track, and dummies for achieving a score qualifying for general secondary or university tracks. The distribution of estimates remains stable across all subsamples, suggesting that no single firm is driving the results. Standard errors are clustered at the firm level.

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 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: This table presents robustness checks assessing the sensitivity of the results to different matching strategies. Each column corresponds to a different outcome, including normalized CITO scores in Dutch and mathematics, teacher recommendations for the university preparatory track, and dummies for achieving a score qualifying for general secondary or university tracks. Panel A reports estimates when randomly keeping only three matched control firms instead of four. Panel B presents results using an alternative matching approach that incorporates a more detailed breakdown of parental education. Panel C reports estimates using a simplified matching procedure based only on firm size, the share of highly educated workers, the share of part-time workers, average wages, and the gender composition of the workforce. Panel D restricts the sample to parents who had been employed at the firm for at least two years before the policy change. All regressions are based on the specification in Table 3, Panel B, with standard errors clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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: This table presents a triple difference-in-differences analysis, examining whether the effect of teleworking policies on parental work arrangements differs based on whether the parent's occupation is classified as teleworkable. Column (1) reports the impact on the probability of working from home, while column (2) examines total hours worked. The Treated variable indicates employment in a firm that introduced a teleworking provision in its Collective Labor Agreement (CLA). The Post-CLA dummy captures the period after the policy change, and the triple interaction term (Treated $\times$ Post-CLA $\times$ Teleworkable Occupation) captures the differential effect for individuals in teleworkable occupations. The specification includes year and sector fixed effects as well as sector-specific linear time trends. The sample consists of individuals working in treated and control firms who were interviewed in the Dutch Labor Force Survey (LFS). Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.