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ABSTRACT

When Parents Work from Home*

This paper estimates the causal effect of parental right to work from home (WfH) on children's educational attainment. Using administrative data from the Netherlands and variations in firm-specific WfH policies, which generate natural experiments, we find that children whose parents gain the right to WfH improve their scores on a high-stakes exam by 9% of a standard deviation. This results in a 4 percentage points upswing in qualifying for a general or academic track in secondary school. Additionally, using the labor force survey, we find that changes in WfH policies are associated with a 17 percentage points increase in WfH propensity, but no change in hours worked or income. These results highlight the large potential benefits of remote work in supporting families and their children.

JEL Classification: I20, J13, J22

Keywords: working from home, test scores, work-life balance, remote work, teleworking, work flexibility

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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] The ability to work from home is clearly viewed by workers as a positive amenity. Recent evidence from Mas and Pallais (2017); Barrero et al. (2021); Nagler et al. (2024), or Aksoy et al. (2023) estimates the willingness of workers to pay for telework between 5% and 8% of their wage, as they value reduced commuting and improved work-life balance, especially among parents and women.[2]

If working from home improves the parents' "work-life balance", it might affect their children as well. So far, studies evaluating the impact of WfH have focused entirely on adults' outcomes. In this paper, we examine how WfH provisions affect the educational outcomes of the children. We consider the context of the Netherlands before the Covid19 pandemic and exploit plausibly exogenous variations in WfH provisions in firm-level collective agreements. The advantage of our approach is that the variation in WfH we consider resembles the switch in the modal working arrangement that occurred because of the pandemic: a switch from full-time in the office to a hybrid mode, but since the switch is firm-specific we can compute counterfactual.

Our main contribution to the literature is the focus on children, and their educational performance in particular. So far the literature has mostly examined the demand for remote working and investigated its impact on workers' productivity.[3] However, the economic impact of WfH is potentially much larger than its direct impact on workers'

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

[2]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 Harrington and Kahn (2023) for evidence that WfH is associated with reductions in the gender wage gap.

[3]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) and Emanuel and Harrington (2024).

productivity. To date, very few studies have examined these other economically relevant outcomes. Angelici and Profeta (2024); Bellmann and Hübler (2021); Costi et al. (2024); Goux and Maurin (2025) provides conflicting evidence on the effect of WfH on workers' health or well-being. To our knowledge, no analysis on the externalities to other family members of WfH arrangement exists yet.

The reason why remote work may potentially affect children's development and their educational outcomes is intuitive: parents who work from home may find it easier to invest in the human capital of their children. Evidence from time-use surveys shows that teleworkers do indeed spend more time with family (Pabilonia and Vernon, 2022; Aksoy et al., 2023). In turn, increased parental interactions may 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.[4]

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 timing in the changes to Collective Labor Agreements (*Collectieve Arbeidsovereenkomsten*, CLA) signed between workers' representative and firms (or employers' organizations) in the Netherlands, which is the country in Europe with the highest share of workers teleworking at least some days (52% in 2024).[5] Collective Labor Agreements (CLA) codify the rights of workers, including when and where work 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

[4]The indirect supervision effect that children may benefit from echoes findings from a study by Reynoso and Rossi (2019) showing that teenagers spending more time alone during the day are more likely to engage in risky behaviors.

[5]Source: Statistics Netherlands: https://www.cbs.nl/en-gb/news/2024/11/over-half-of-dutch-people-work-from-home-sometimes

pandemic.[6] To account for time variations, firms formally granting their workers the right to work from home are matched to firms in the same sector with similar characteristics that do not 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 working in these firms at least one year before the implementation of the agreement, who gain more flexibility in their work schedule; this is our treatment group. The control group consists of people working in *T-1* 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 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 or equivalent) which largely determines the track they will be allowed to attend in secondary education. Thus, it is a very salient high-stakes 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 CITO test, the allocation of treatment to the child can be considered 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; i.e. the identification relies on the exogeneity of the timing of the change

[6] 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.

in CLA with regards with the timing of the child CITO test. We compute the same difference in control firms to account for any age specific time effects, and implement a difference-in-differences among the children whose parents work at treated or matched firms. 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 formalize 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 labor market outcomes, to assess potential costs of working from home to the parents. This provides us with a secondary contribution to the literature as we are able to assess the long-run effects of working from home. Looking at these outcomes together with educational outcomes of the children helps us getting a broader picture of how family-friendly policies affect all members of a family.

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 17 percentage points more likely to report working from home at least some of the time. The change in CLA does not solely reflect de-facto arrangements but real behavioral changes regarding work arrangement, confirming the plausibility of our identification strategy. Parents working in firms that granted WfH rights experience no change in key labor 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.[7] There is a relatively large literature on the impact of parental leave policies on parental careers (see Kleven et al. (2024) for a review), and a growing but smaller literature on the impact of these policies on children's developmental outcomes (Ginja et al., 2020), 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.

The rest of the paper is structured as follows. Section 2 discusses the recent related literature, Section 3 presents the Dutch context and relevant institutions; the empirical analysis and main results are presented in Section 4; while Section 5 discusses mechanisms. We conclude in Section 6.

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 randomized 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 $2/3^{\text{rd}}$ of the increase driven by increased working time. Similarly, Angelici and Profeta (2024) and Choudhury et al. (2021) estimate increase in productivity of up to 4% for workers allocated to an hybrid work model in Italy and

[7] 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 well-being.

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 1/3$^{\text{rd}}$ of the effect driven by reduced learning. The differences in outcomes 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).[8] 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 positively 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) randomized IT workers to hybrid or office work for 6 months and Choudhury et al. (2024) uses a natural experiment which daily randomized 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.

Most similar to our identification strategy is Goux and Maurin (2025) 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-differences strategy, based on comparing low-level employees, who were unlikely to use WfH and

[8] Dutcher (2012), using a lab experiment, reports productivity gains of WfH but only for creative tasks, and 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 sector to make firms and their workers close substitute observationally, and compare workers within firms, not based on their seniority but on the age of their children when the policy is implemented. We are primarily interested in the effect on workers' children, whose outcomes we can observe only at a specific age. Consequently, our identification relies on differences in the age of these children when the reform affecting their parents is implemented. This timing is more likely to be exogenous. In additional robustness checks, we assess heterogeneity 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, all details of work arrangements are specified by collective labor agreements between firms or 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 labor 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 favorable 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 at the Ministry of Social Affairs and Employment. Since our identification strategy relies on matching treated and control firms within sector, we focus solely on company collective agreements.

The contents of the telework arrangements vary, but they are usually relatively generic—acknowledging the option to telework, a potential minimum or maximum number of days employees can telework, and possible subsidies for internet and equipment. We do not distinguish between these conditions and consider a treated firms one that has an explicit provision in their collective agreement related to work from home, regardless of the nature of the provision.

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 approximately, pupils take a standardized test, the most common is the CITO exam.[9] It consists of a multiple choice questionnaire testing the pupils' 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 secondary education 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 eligible 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-stakes exam.

[9] 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 the CITO test. Which test provider is used is determined by the school and affects all pupils in a cohort. In a robustness check - not reported here - we restrict the sample to schools that consistently use the CITO for the period of analysis, the estimates are not substantially different from those reported for the full sample, but are less precise.

The test is set and marked externally by a private company, not by the child's teacher. CITO scores are normalized and scaled between 501 and 550. A score below 536 leads to a 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 majority of students we have their scores separately in the Math and Dutch components of the test, which we normalize by subject and year.

Prior to students sitting the CITO, teachers recommend a secondary track. The recommendation can be revised following the test, after which teachers provide a final recommendation (Timmermans et al., 2023). Our data includes the initial teacher's track recommendation only. Informed by both the CITO score and the teachers' recommendation, parents have the final choice to select the secondary education track for their child, but secondary schools might refuse a place if they believe the education will not be at the appropriate level for the child.

We do no have data on other children's outcomes (such as measures of well-being or non-cognitive skills), beyond these educational outcomes.

4 Empirical Analysis

4.1 Identification Strategy

The central hypothesis we wish to test is that remote work improves children's educational outcomes. A direct comparison between children of parents in firms that have teleworking provisions in place and those in firms that do not is unlikely to estimate the causal effect 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 accounts for parental selection into firms that might provide WfH opportunities. Since WfH policies are not designed to directly improve CITO results, treatment assignment driven by birth year variations and firm-level policy changes, can be considered quasi-random, as the ITT estimates only rely in differences in the timing of the policies relative to the age of the children of the affected employees.[10] 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. One would then estimate a simple before and after using only treated firms and comparing the test scores of children sitting the CITO in year t: ($y_{i(t)}$), who were age less than 12 ($Young_i$ =1 if age $\in$[8,11] or $Young_i$ =0 if age $\in$[13,16]) when the policy was introduced in year T); i.e. rather than relying on differences over time this estimate relies on differences in the age of the child when the policy is introduced.

$$y_{i(t)} = \alpha + \lambda_t + \Gamma X_i + \eta \text{Young}_i + \epsilon_i \ , \forall f \ Treat_f = 1 \tag{1}$$

Since the policy changes are implemented in different years, any child-cohort specific effects can be captured by λ_t, a fix effects for the year in which the test is taken. However, one remaining concern is that this strategy identifies the effect out of children being younger when the policy is introduced. Their parents are also likely to be younger themselves, and might differ in other relevant characteristics; they 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

[10] 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.

children's outcomes through channels beyond teleworking.

To address this challenge, we need a set of control firms that do not have a WfH policy, so that the children of their employees can serve as a control group to identify age effects. 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-out of a single policy, but firm-specific policy changes, 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. This is very similar to the identification strategy implemented in Gathmann et al. (2020) when estimating spillover effects of plant closures.[11]

To identify the counterfactual firms we match each firm that implemented a WfH policy at time T with a set of firms operating in the same sector f based on their characteristics at period *T-1.* 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 representation of women and the extent 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 distance is

[11] 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.

selected. In further tests, we assess the robustness of the result to variations in the set of covariates used to match firms, or number of matched-firms kept as control.

Note that since we match on the year CLAs were signed, we obtain a fictional year of treatment for the control firms. Having identified a set of counterfactual firms for each treated firm, we then compare the outcomes for 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, to the outcomes for children of the same age whose parents work in control firms. This is akin to a difference in differences but rather than relying on pre/post implementation of a policy, we use differences in the age of children when the policy is introduced. This allows us to control for age-related effects.

Formally, we estimate the following equation (2), where $y_{i(f(k),t)}$ is the educational outcome for a child i who turn 12 in year t and whose parents work in firm f in sector k. We consider a series of outcomes: normalised CITO scores in Dutch and in math, and whether the overall 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.[12] In robustness checks, we also consider the track recommendation of the teacher.

$$y_{i(f(k),t)} = \alpha + \lambda_t + \Gamma X_i + \eta \text{Young}_i + \rho \text{Treated}_f + \beta \text{Young}_i {*} \text{Treated}_f + \epsilon_{i(f)} \tag{2}$$

Treated$_f$ is an indicator of treated firm, which allowed WfH at time T and *Young*$_i$ is an indicator of a child being young; i.e. age 8 to 11 when the WfH policy is introduced in year T. X_i is a vector of child-specific characteristics, including its age, gender, and the age and gender of the treated parent. To control for variations in the difficulty of the CITO test between years, or cohort specific effects λ_t is included to capture these CITO-specific

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

year effects. In some specifications, equation (2) also includes firm's sector fixed effects (θ_k) to capture unobservable characteristics of the parents linked to their occupational choice that might affect their propensity to work from home and to invest in the human capital of their children. Finally, our favorite specification replaces the sector fixed effect by a firm-specific match effect ($\theta_{f(k)}$), to further account for these unobservable workers' characteristics. In which case, the children's outcomes are compared within the specific matched firms of a treated firm.

Equation (2) is estimated 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 firm; i.e. we only keep a window of 4 years before and after the policy change to observe test scores. To eliminate other biases we limit the sample to workers 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. Finally, we exclude pupils who took the test in year T, as it is ambiguous whether they were affected by the WfH policy in time for it to affect their score at the CITO. In all specifications, standard errors are clustered at the level of treatment; i.e. firm level.[13]

4.2 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 matched children who took the CITO exam to their parents and integrated this information with the employer-employee database to construct our main dataset.[14]

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

[14]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.

4.2.1 Collective Labour Agreements

The data on collective lavor agreements was obtained from a private company, XpertHR, which has all records of 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 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.[15]

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. This process results in a final sample of 90 firms that altered their firm-specific CLA between 2006 and 2019, of which 39 can be identified by CBS. This sample, although small relative to the total number of firms

[15]Sectors are identified using the nomenclature provided by the Tax Office (variable sect in the dataset polisbus/spolisbus). While this classification may differ from the level or nomenclature at which collective labor agreements (CLAs) are signed, it offers a highly detailed categorization with 70 distinct entries—substantially more granular than the NACE one-digit classification. This detailed classification is advantageous for two main reasons: it allows for precise control of regulations specific to certain groups of firms and enhances the comparability between observations.

in the Netherlands, is substantially larger than the samples used in RCTs evaluating the impact of Wfh policies on productivity, which are typically conducted within a single firm (such as in Bloom et al. (2015), Bloom et al. (2024), Choudhury et al. (2021), Choudhury et al. (2024), Angelici and Profeta (2024))

To ensure reliable tracking of employees within firms, we include only firms that had a teleworking CLA 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 narrowed 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. For some of the control firms, we observe no children taking the CITO test in the relevant years, dropping these firms reduces the sample to 86 control firms, and 114 firms in total.[16]

To conduct robustness checks we also identify CLAs that provide other type of work flexibility unrelated to WfH such as rights to informal care and short-term leave to care for relatives. These other policies are unlikely to affect the ability of parents to invest in the human capital of their children and will be considered as placebo policies. There are also possible provisions related to parental leave, such as provisions allowing for splitting parental leave, which because of the time-frame cannot affect the children we consider in

[16]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.

our sample and can also serve as a placebo.

4.2.2 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 students. The scores are normalized to the performance of the population of all test sitters in that year. From this register we also retrieve initial teachers' track recommendation. There is no earlier age test scores available in the dataset, and it is not possible to account for past scores or compute some measures of value added.

We also use labor market information available in the matched employer-employee data, namely log of annual labor earnings, log of number of hours worked, labor market participation, 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 (LFS). The LFS is a nationally representative rotating panel survey. Respondents are interviewed thoroughly once, by an interviewer from Statistics Netherlands, and are subsequently interviewed by phone quarterly, four consecutive times. In the second 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 the analysis based on the matched employer-employee data and assess the long-run impact of the WfH policy on hours worked.

4.3 External validity and balancing

One legitimate question is how representative our sample is of the overall population of firms and employees. To validate our empirical strategy, we conduct two balancing tests by regressing indicator of being part of our sample either as treated or control firms, compared to all other firms in the NL. To assess the internal validity of our sample, we repeat the tests by regressing an indicator of the firm being treated for our sample of treated and matched firms only. These tests are conducted at the firms, parents, and children level. Note that while we matched firms on the observable characteristics at *T-1*, we did not include information on parents and children. Nothing ex-ante insures that their characteristics are balanced across treatment and control.

Table 1 Panel A reports the characteristics of the firms in our sample against all other firms in the database. It highlights that the sampled firms are substantially larger—784 employees vs 26, have more educated workers, who are less likely to work part-time or be females, and where employees earn substantially higher wages than at the average firm.

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 more (+19.6% and +41.3% for fathers and mothers, respectively), but also work longer hours (22.3% and 33.1% for fathers and mothers, respectively).

Panel C in Table 1 reports the differences in educational attainment between the children in our sample 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— above a baseline average of 50% in the non-experimental population.

These results indicate that the sample of firms and employees are positively selected. This confirms that a naive comparison of children whose parents work at treated and untreated firms is likely to be affected by selection bias. The positive selection also mean that we cannot assume our results apply to the entire population of firms and employees in the Netherlands, a point on which we will come back later.

We now assess the internal validity of the sample. Table 2 Panel A reports the estimates of separate regressions of the firm-level matching characteristics on an indicator of treatment for the subsample of treated and matched firms. The estimates are never statis-

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 A.2 for details on sample selection. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

tically 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 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, resulting in a sample of individuals who are balanced in term of their observable characteristics.

4.4 Event-study

Although having a balanced sample is reassuring, our strategy is not based solely on comparing children in treated and control firms. The strategy also involves comparing younger and older children in both types of firms. Therefore, 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.

The analysis is based on an event study model specified as follows:

$$y_{i(f(k),t)} = \alpha + \lambda_t + \theta_k + \Gamma X_i + \xi \text{ Treated}_f + \sum_{j=-4}^{3} \psi_j + \sum_{\substack{j=-4 \\ j\neq-1}}^{3} \beta_j \text{ Treated}_f \times \psi_j + \epsilon_{i(f)} \quad (3)$$

where, λ_t denotes year fixed effects, θ_k represents match-specific fixed effects, and ψ_j is the event time indicator, where $j = 0$ corresponds to the year of the CLA change. β_j measure the policy effect pre- and post-treatment, and allow to test for parallel trends.

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 A.2 for details on sample selection. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The effect on the year immediately preceding the policy change (*T-1*) is normalized to zero.

4.4.1 Pre-trends for parents

First, we analyze whether parents working in firms that changed their policies regarding working from home had different labor market trends before the policy was implemented and in its aftermath. To do so, we look at the labor market outcomes for parents in a window of four years before the implementation of the policy change to three years after—keeping as many pre- as post-treatment periods as suggested by De Chaisemartin and d'Haultfoeuille (2023). At this stage, our goal is simply to assess whether the descriptive evidence supports the absence of pre-trends between parents. To keep the analysis as simple and transparent as possible, we run separate regressions for each year, where the outcome is regressed on a treatment dummy and basic individual controls (age, age squared, and gender). This approach provides a straightforward check for any pre-existing differences between treatment and control groups. The results are displayed in Figure 1.[17] A more formal analysis will follow in Section 4.5, where we estimate Equation 3.

Across the four outcomes (log of yearly earnings, log of the number of hours worked, labor market participation, and the likelihood of working in the same firm in which observed at time *T-1* and *T*), 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, the differences in earnings are small and not statistically significant. Post-implementation, the differences remain small and not statistically significant. Section 5.2 discusses post-treatment effects on parents more in-depth.

[17] Figure 1 includes parents of both younger and older children. The pattern remains unchanged when restricting the sample to either group separately.

Initially, the probability of being employed at the same firm was larger in treated firms three to four years before the WfH policy change. This is primarily driven by control group firms that were newly established during this period and for which mechanically employees could not have been employed throughout the period. Hence, when considering the outcome "Work in the same firm"—and for that outcome only—we restrict the sample to firms (and their employees) which are observable since 2006—the first year the matched employer-employee dataset is available. This applies to all subsequent tables and figures relating to the "Work in the same firm" outcome, for all other outcomes we keep the full set of matched firms. We show in Table B.4 that implementing this restriction does not change the main results on test scores as compared to Table 3.

Figure 1: Pre-trend in Labor 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.

4.4.2 Pre-trends for children

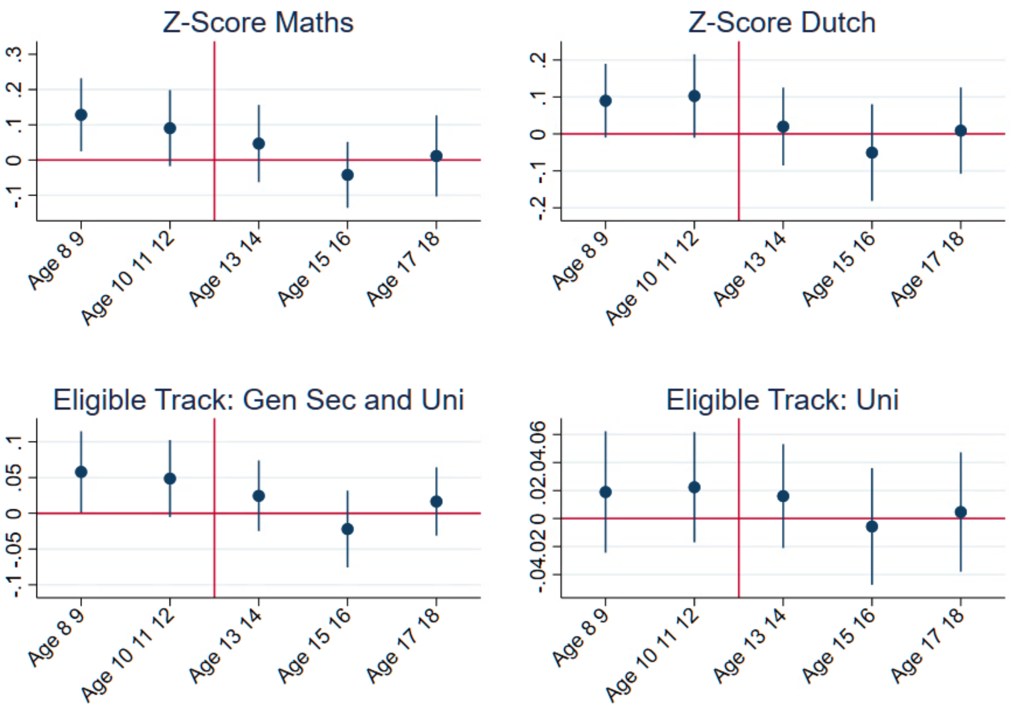
Testing for the absence of pre-trends differs for children since their educational outcome is observed only once. Instead of time passing, variation comes from the age children were when their parent's firm implemented the WfH policy. Figure 2 shows the difference in CITO test performance between children of treated and control firm employees, separately by age groups (8–9, 10–12, 13–14, 15–16 and 17-18). Note that in contrast to a standard event study, the 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. 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 5).

Post implementation of the WfH policy, i.e. on the left side of the graphs, we observe a jump in the test 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.

4.5 Main results

In this section we report estimates of the parameters from equation (2) on the educational outcomes of interest.

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 separate regressions of the outcome on a treatment dummy. Time periods correspond 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.

4.5.1 Baseline results

Table 3 reports the estimates of β from equation (2). 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 controls, Panel C includes firm's sector fixed effects and Panel D includes match specific fixed effects.

The estimates are 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, which does not change significantly. The estimates are reduced when including sector fixed effects or match-specific fixed effects, highlighting the importance of comparing most similar firms to capture parents' selection. Across all outcomes, the

Table 3: Regression Results

	(1)	(2)	(3)	(4)
	Z-Score		Eligible Track	
	Maths	Dutch	Uni	General Sec. & Uni
Panel A: No Controls				
β	0.109***	0.124***	0.017	0.053***
	(0.035)	(0.035)	(0.018)	(0.018)
Panel B: With Controls				
β	0.102***	0.113***	0.015	0.049***
	(0.033)	(0.030)	(0.017)	(0.017)
Panel C: Sector FE				
β	0.098***	0.105***	0.013	0.047***
	(0.035)	(0.030)	(0.017)	(0.017)
Panel D: Matching FE				
β	0.086**	0.089***	0.008	0.041**
	(0.033)	(0.033)	(0.017)	(0.018)
No. of Obs	14,331	14,331	14,331	14,331
Mean - Control group	0.050	0.100	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, 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

estimates drop by 20% to 50% as the specifications become more restrictive, but remain statistically significant.

In our preferred specification—controlling for match-specific effects—children, whose parents became eligible to work from home, improve their test scores in both math and Dutch 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 children eligible for the general secondary track or higher. However, these gains do not raise scores beyond 545 – the cut-off for recommendation to the most academic track. 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. WfH policies have a strong positive effect on marginal students who were at risk of being placed into the lowest academic track

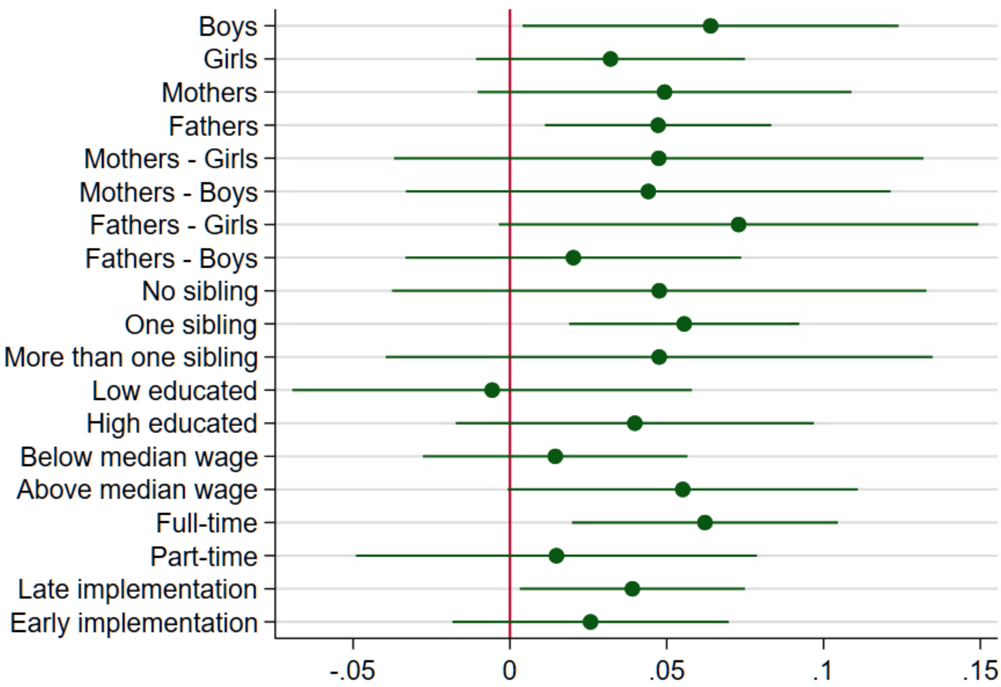
Overall, we find strong evidence that allowing parents to work for home improves the test scores of their children at a high-stakes exam at the end of primary school. To put this in perspective, the effect of WfH policy is similar to the median primary school interventions in the US which has an effect 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 section 5.2, we examine the impact on parents as well.

4.5.2 Heterogeneity

The average effect may mask important heterogeneities across children. We examine here whether the effect varies across different dimensions, including the gender of the treated parent, the gender of the child, the education level of the parent, the wage level of the parent, the number of sibling below the age of 16, whether the parent was working part-time and the period at which the right was granted. We report these estimates graphically—for the outcome achieving a score qualifying for general secondary education or the university track—in Figure 3. For the other outcomes, see Figures B.1, B.2 and B.3 and, regression coefficients in Table B.2. 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 gender gaps in achievement. The effect appears slightly larger for boys—6.4 pp vs 3.2 pp for girls—although the difference is not statistically significant.

Figure 3: Heterogeneity in the Effect of WfH – Eligibility to General Secondary or University tracks



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 outcome is a binary indicator 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, the number of siblings, whether the parent was working full-time 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. For this reason, a WfH policy might have different impact depending on the gender of the parent who is affected by the policy, although again it is unclear in what direction. 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. However, it is unclear whether the gender of the parent benefiting from WfH should matter since parents can reallocate tasks within the household so that whomever 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—4.7 pp for fathers and 4.9 pp for mothers—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, although subsamples are relatively small in particular when it comes to mothers having been treated, resulting in large confidence intervals.

Family characteristics might affect the decision to work from home and its effectiveness in improving children's test score. In the next set of heteogeneity, we split the sample by the number of siblings below the age of 16 in T=0. A larger number of children might increase the probability of working from home, as alternative post-school activities are relatively more expensive, but reduce the time that parents can invest in the education of their children. Indeed, we find little variation in the estimate by family size.

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 parents or those with a higher income might already be investing in support activities for their children, and WfH might not result in additional parental engagement but only allow parent to substitute externally provided support to parental support. Alternatively, more educated parents might have a greater preference for investing in the education of their children and have greater returns to their investments, in which case, granting WfH would result in larger educational gains for children of more educated parents (Guryan et al., 2008). The estimate is larger for higher-educated parents—approximately 4 percentage points—compared to a near-zero effect for lower-educated parents, defined as vocational qualification only, but not statistically different.[18] A similar pattern is observed when comparing parents earning above and below the sample median wage.

Changes in parents' schedule are likely to be less important for employees working less hours since they already enjoy more flexibility. We thus split the sample by part-time or full-time status of the affected parent. Note that this decision might be endogenous, driven by the firm's WfH policies or reflect parent's preferences for investing in their children. We find that the effect is very small and statistically insignificant on the subsample of part-time workers, and much larger and statistically significant for full-time employees—6.22 pp vs 1.48 pp.

Finally, we split the sample between early and late adopting firms. 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, or occupations that are particularly suitable to teleworking. 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 small and 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.

Altogether, the estimates reported in Figure 3 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 more educated, high-earning households, parents who were working full-time and late implementers. The last two findings provide supportive evidence that the effect is driven by increased workplace flexibility, while the

[18] It is important to note that education data is missing for about one-third of parents.

first two suggest that it is influenced by parents' characteristics, which are also associated with children's success. Note that we will come back on the impact of WfH policies on the inequality of educational attainment in the final discussion.

4.6 Robustness checks

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

4.6.1 Alternative Identification

As pointed out in Section 4.1, an alternative approach is to leverage the fixed age at which children take the CITO, and estimate equation (1). Rather than relying on matched difference-in-differences it compares older and younger children within treated firms. 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 difference-in-differences 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, substantially larger than the comparable estimates (0.10 to 0.11) in the difference-in-differences specification. Including sector fixed effects— to capture some unobserved characteristics of parents— drops the estimates closer to the ones estimated in the difference-in-differences specification. This confirms the importance of controlling for unobservable parental characteristics associated with their choice of workplace.

4.6.2 Alternative matching

To further test the robustness of the results to the initial matching strategy, we assess the sensitiveness of the estimates to alternative matching procedure. 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

Table 4: Alternative Identification : Only Treated Firms

	(1)	(2)	(3)	(4)
	Z-Score		Eligible Track	
	Maths	Dutch	Uni	General Sec. & Uni
Panel A: No Controls				
β	0.178***	0.195***	0.0321	0.0883***
	(0.0418)	(0.0572)	(0.0206)	(0.0205)
Panel B: With Controls				
β	0.167***	0.238***	0.0440**	0.0972***
	(0.0302)	(0.0369)	(0.0173)	(0.0168)
Panel C: Sector Fixed Effects				
β	0.0763*	0.155***	0.00946	0.0534**
	(0.0413)	(0.0420)	(0.0134)	(0.0256)
No. of Obs	3,962	3,962	3,962	3,962
Mean	0.10	0.14	0.21	0.55

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, 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. Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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

Finally, the last panel in Table B.3 reports estimates of the WfH when we impose that parents work at the firm two years prior to the implementation of the policy 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 at most limited if not non-existent.

4.6.3 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, especially 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 2 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 the selection of parents.

Finally, we assume that the change in labor agreement introducing WfH took place in the same firms—as Table 3—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).[19] 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 (2) 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.

[19]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)
	Z-Score		Eligible Track	
	Maths	Dutch	Uni	General Sec. & Uni
Panel A: With Controls - Informal Care				
β	-0.001	-0.027	-0.010	0.006
	(0.052)	(0.049)	(0.020)	(0.027)
No. of Obs	19,818	19,818	19,818	19,818
Mean - Control group	0.17	0.20	0.23	0.58
Panel B: With Controls - Shortcare Leave				
β	0.010	-0.037	-0.004	-0.015
	(0.050)	(0.054)	(0.023)	(0.025)
No. of Obs	6,892	6,892	6,892	6,892
Mean - Control group	0.10	0.14	0.23	0.56
Panel C: With Controls - Split Leave				
β	-0.003	0.032	0.017	0.005
	(0.035)	(0.034)	(0.013)	(0.016)
No. of Obs	16,579	16,579	16,579	16,579
Mean - Control group	0.21	0.28	0.26	0.61
Panel D: Robustness - Placebos				
β	-0.009	-0.023	0.000	-0.014
	(0.045)	(0.046)	(0.020)	(0.019)
No. of Obs	9,946	9,946	9,946	9,946
Mean - Control group	0.11	0.15	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, 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

4.6.4 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 (2) omitting one treated firm, and its associated control firms, at the time. Figure B.4 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 scoring above 545 points, the main estimates were not statistically significant and the 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.

4.6.5 Additional outcomes

Table B.5 presents the results of estimating equation (2) on teachers' recommendations, specifically focusing on the likelihood of recommending either general secondary education, the university track, or exclusively the latter. Although this information is not available for the entire sample, it provides confirmation that students' progress was recognized by their teachers, extending beyond mere improvements in test scores. The estimates are remarkably similar to the main findings from Table 3; the recommendation to attend General or University track increases by 4 percentage points for students whose parents gain the right to work from home, identical to the 4 percentage points increase in the probability of reaching the test threshold for these tracks.

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.

5 Mechanisms and effects on parents

This section aims to achieve two objectives. First, it examines how the introduction of teleworking provisions has affected parents' work patterns, specifically focusing on whether it has affected the time spent at home. Second, it assesses the impact of these changes on labor market outcomes and career progression, which helps evaluating the policy in a broader sense and establish the trade-offs between the benefits accrued to the children and the possible negative impact on the career of their parents.

5.1 Impact on teleworking practices and reported hours worked

To study changes in work patterns, 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.

5.1.1 CLA and teleworking

The information on working from home is available in a single wave of the LFS; i.e. only once for each respondent. We recode all respondents who reported working from home at least one day a week as teleworking. Additionally, 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.

We then estimate equation (2) 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. This means that we change the identification strategy to a standard difference-in-differences where the identification comes from differences in behavior over-time between treated and control workers. This estimate again should be interpreted as 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.

Results are reported in Table 6. Working in a firm that grants the right to work from home leads to a 17 percentage points rise in the fraction of employees reporting remote working. This is a doubling of the prevalence of teleworking compared to the mean of the control group. It demonstrates that the opportunity to engage in teleworking, as outlined in the CLA, contributes to an observed increase in reported remote work. This rejects the alternative suggestion that the introduction of WfH mainly formalized previous working patterns. Instead, the provisions did have an impact on work patterns of the employees, making it a plausible mechanism for our main findings.

Table 6: LFS - Double Difference

	(1) Teleworking	(2) Hours Worked
Treated Firms	-0.04	-0.38
	(0.05)	(0.73)
Post-CLA	-0.01	-1.02
	(0.04)	(1.14)
Treated × Post-CLA	0.17***	1.96
	(0.06)	(1.21)
R-Squared	0.04	0.26
No. of Obs	3,794	9,951
Mean - Control group	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). Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

5.1.2 CLA, teleworking - heterogeneity

Table 7 reports which type of workers is more likely to work from home after a change in CLA. To simplify the interpretation of these results, we keep only individuals working in treated firms and regress a dummy for teleworking on the "Post-CLA" dummy, year fixed fixed effects and controls for gender and age, as specified in equation (1). We then separately assess heterogeneity by education level (graduate), gender (female) and presence of children aged less than 16.

Table 7: LFS - Mechanisms - Working from Home

	(1) Baseline	(2) Educated	(3) Female	(4) Children	(5) Control firms
Post-CLA	0.164***	0.133***	0.170***	0.112***	-0.0262
	(0.037)	(0.034)	(0.042)	(0.034)	(0.050)
Post-CLA × Educated		0.0407			
		(0.0313)			
Post-CLA × Female			-0.0170		
			(0.051)		
Post-CLA × Children				0.117***	
				(0.030)	
Intercept - Heterogeneity		0.161***	-0.009	-0.015	
		(0.017)	(0.029)	(0.033)	
R-Squared	0.02	0.07	0.02	0.03	0.02
No. of Obs	1,762	1,721	1,762	1,762	2,032
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 fixed effects and individual controls. The sample consists of individuals working in treated and control firms who were interviewed in the Dutch Labor Force Survey (LFS). Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

At baseline, gaining the right to work from home increases the proportion of workers reporting working from home in the affected firms by 16 percentage points. In columns 2 to 4 we assess the heterogeneity of this effect for specific groups of workers. While, highly educated workers are 16 percentage points more likely to report teleworking, the gap in teleworking remains stable after the WfH policy is introduced. We also find no difference in teleworking by gender either at baseline or after the WfH policy is introduced. This contrasts with the heterogeneity in behavior observed for parents. Following the introduction of WfH, parents are twice as likely to report working from home as other employees.

As a robustness check, the last column of Table 7 reports estimates for the sample of workers at control firms only. It is worth noting that, at baseline these workers are less likely to report working from home; 16% do compared to 28% in firms that implement a WfH policy. As expected, 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 behavior is observed for parents who, after the policy change, become twice as likely than other workers to report working from home.

Both the estimates on the full LFS and on the sub-sample of employees in firms implementing a WfH policy are similar and indicate a shift in reported teleworking of 16 to 17 percentage points. These estimates should not be used to compute a 2SLS estimate of the impact of parental WfH on child's educational attainment since the sample and the identification strategy differ between the "first stage" and the "reduced form". The LFS estimates are based on all employees while the test-score estimates are based on children aged 8 to 16 whose parents worked in a treated or control firms. Focusing on parents, the increase in teleworking post-policy is close to 23 percentage points. Moreover, the LFS estimate compares employees in treated and control firms before and after the reform, as in a standard difference-in-differences estimate but the test scores estimate is akin to a triple difference and adds a comparison between children aged less than 12 or over 12 at the time of the reform. While the LFS estimates cannot be used to compute a 2SLS it allows us to confirm that the change in CLA employees, and especially parents, become more likely to work from home, making teleworking a plausible mechanism behind the increase in test score.

5.1.3 Teleworkable occupations and Children's educational outcomes

Having demonstrated that changes in CLA affected the working patterns of some parents, we assess whether this results in heterogeneity in the impact of WfH policies on children educational attainments. In particular, we assess whether the impact of parents gaining the right to work from home differs for children whose parents work in the most teleworkable occupations, defined as occupations for which more than 20% of all workers report working from home.

First, we 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 an interaction term to the previous specification and estimate a triple difference-in-differences. The estimates are reported in table B.6 available in Appendix B. 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 more likely to work from home as those working in less teleworkable occupations. Although the overall effect is not statistically significant, the sum of the baseline effect and the interaction is highly significant.

Next, we assess the heterogeneity in the impact of WfH policies on the education of children by their parents occupation type. 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. Instead, we identify LFS respondents in the matched employer-employee dataset; this leaves us with only 791 observations of children aged 8 to 16 at the time of a change in CLA. Table 8 reports estimates of a model similar to equation (2) including an interaction term between working in a firm granting rights to work from home and having a teleworkable occupation. Due to the small sample size the estimates are imprecisely estimated but indicate some clear patterns nonetheless. Children whose parents work in a teleworkable occupation score 28% of a standard deviation higher at baseline, which results in them being 12 to 14 percentage points more likely to be eligible for a University or general secondary and University tracks, respectively.

The introduction of WfH policies increase this attainment gap further. The estimates are imprecise but suggest that the effect on test scores of having parents working in firms granting them the right to work is 2 to 4 times greater if the parents work in a teleworkable occupation. Since these children had higher test scores at baseline, the implementation of WfH policies increases the attainment gap.

Table 8: LFS - Baseline Effect - Heterogeneity Teleworkable

	(1)	(2)	(3)	(4)
	Z-Score		Eligible Track	
	Maths	Dutch	Uni	General Sec. & Uni
β	0.0370	0.0959	-0.0731	0.0918
	(0.199)	(0.177)	(0.0988)	(0.0978)
Teleworkable	0.284***	0.279***	0.118***	0.141**
	(0.0857)	(0.0867)	(0.0433)	(0.0594)
β × Teleworkable	0.154	0.176	0.171	0.0750
	(0.303)	(0.263)	(0.157)	(0.149)
R-Squared	0.15	0.11	0.06	0.10
No. of Obs	791	791	791	791
Mean - Control group	0.21	0.24	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, 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

5.2 Parental Labor Market Outcomes

5.2.1 Using the Labor Force Survey

By teleworking, parents can improve the academic performance of their children. Without time use data it is not possible to precisely pin down the mechanisms but potential avenues are direct involvement with homework, which might 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 and thus wages and promotion prospects in the longer run. The employer-employee register used in section 4.4 already highlighted that the introduction of WfH policy did not lead to any change in labor force participation, tenure with the firm, or earnings. However, since only contracted hours were reported in the register, we further investigate this issue by analyzing the Labour Force Survey. The LFS allows us to measure reported working time, a good complement to contractual working hours reported in matched employer-employee register.

Since the question about working time is asked at each quarterly wave, the data is an unbalanced panel with up to five observations per participant, yielding 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.[20] Treating the data as pooled cross-sections, we estimate model 2. The estimates for the parameters of interest are reported in the second column of Table 6.

Consistent with the findings of Bloom et al. (2015), homeworking leads to a statistically insignificant 5.9% 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 8.3%; this change is statistically significant as shown in Appendix B Table B.6, 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 only the sample of workers whose firms changed their WfH policies, and using a simple before-after strategy. These estimates are reported in Table 9.

The findings suggest a relatively strong effect among educated individuals, who following the introduction of WfH policies, increased their hours worked by 3.7 compared

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

Table 9: LFS - Mechanisms - Hours worked

	(1) Baseline	(2) Educated	(3) Female	(4) Children	(5) Control firms
Post-CLA	0.913	-1.415	0.509	0.993	-1.110
	(0.650)	(1.037)	(0.711)	(0.616)	(1.178)
Post-CLA × Educated		3.705***			
		(0.981)			
Post-CLA × Female			1.075		
			(1.289)		
Post-CLA × Children				-0.149	
				(1.244)	
Intercept - Heterogeneity		-1.229	-6.625***	-1.698	
		(1.013)	(0.806)	(1.242)	
R-Squared	0.21	0.23	0.21	0.22	0.29
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 fixed effects and individual controls. The sample consists of individuals working in treated and control firms who were interviewed in the Dutch Labor Force Survey (LFS). Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

to less educated employees. We find no significant effect of WfH on hours worked among parents, the primary population of interest in this study. This lack of significance is not driven by sample size, but reflects that the point estimate is close to 0. Parents are more likely to work from home following the introduction of WfH policy, but this does not affect their total hours worked. As in Table 7, we report estimates for the placebo regression using a sample of workers in the control firms. There is also no evidence of changes in working hours in control firms.

5.2.2 Using matched employer-employee data

To further investigate the impact of the WfH policy on parents' labor market outcomes and provide a more comprehensive analysis, we re-analyse the matched employer-employee register but compared to the initial event study presented in Figure 1, we use only the sample of parents of the children whose results are presented in Table 3. For these employees, we construct a panel dataset covering four years before and three years after the policy change.

As in the initial event study, we examine four key outcomes: the logarithm of yearly earnings, the logarithm of contracted hours worked, the probability of participating in the labor market, and the probability of remaining employed at the same firm where the collective labor agreement (CLA) change occurred. We present two F-tests alongside the coefficient estimates: one testing the joint hypothesis that all pre-policy coefficients are zero, and another testing that all post-policy coefficients are zero. The results are summarized in Table 10.[21]

Despite the more restrictive focus on parents, the results align with the findings in Figure 1. First, there is no evidence of differential pre-trends between treated and control parents: none of the pre-policy coefficients are statistically significant, and the associated

[21] De Chaisemartin and d'Haultfoeuille (2023) provides a detailed discussion of two-way fixed effects models. As shown in Appendix B.7, a standard TWFE model is appropriate in our context, see Table B.7.

Table 10: Effects on parents

t	(1) (Ln) Earnings	(2) (Ln) Hours Worked	(3) Participate in Labor Market	(4) Work in Same Firm
-4	-0.030	0.049	0.004	-0.027
	(0.030)	(0.046)	(0.005)	(0.076)
-3	-0.023	0.032	0.002	-0.033
	(0.030)	(0.032)	(0.002)	(0.037)
-2	0.006	0.006	0.000	-0.022
	(0.006)	(0.018)	(0.002)	(0.024)
0	0.012	-0.006	0.000	-0.006
	(0.015)	(0.016)	(0.001)	(0.017)
1	0.018	0.006	0.002	0.018
	(0.016)	(0.045)	(0.005)	(0.036)
2	0.023	-0.024	-0.001	0.020
	(0.026)	(0.070)	(0.008)	(0.036)
3	-0.017	-0.024	-0.003	0.027
	(0.026)	(0.084)	(0.009)	(0.041)
R-Squared	0.04	0.07	0.02	0.08
No. of Obs	81,144	82,413	82,413	37,690
F-test pre	0.76	0.44	0.46	0.49
p-value pre	0.52	0.72	0.71	0.69
F-test post	0.44	0.44	0.44	0.32
p-value post	0.78	0.77	0.86	0.84

Notes: This table presents the estimated effects of the policy change on parents' labor market outcomes using an event study model. The coefficients correspond to the estimated impacts of the policy on the logarithm of yearly earnings, logarithm of hours worked, the probability of participating in the labor market, and the probability of remaining employed at the same firm. Standard errors are clustered at the firm level and reported in parentheses. F-tests assess the joint significance of coefficients in the pre- and post-policy periods. As explained in subsection 4.4.1, the sample size in (4) is smaller.

p-value of the pre-trend F-test always exceed 0.5. Similarly, we observe no significant effects in the post-policy period, with all coefficients remaining statistically insignificant and p-value of the associated F-tests exceeding 0.75. While the coefficient on log hours worked is negative in t=2 and t=3, when it was positive in Table 9, the effect is not statistically significant in either case. Appendix Tables B.8 and B.9 provide a breakdown of these results by gender, separately reporting outcomes for fathers and mothers. There is no evidence of an effect on either group.

While these results do not allow us to pin down the mechanisms by which parents benefiting from WfH policies improve their children's educational attainments, they highlight that these do not come at a cost to parents or employers. Parents do not reduce their hours worked and suffer from lower wage growth in order to increase their investment on the education of their children.

6 Conclusion

Using plausibly exogenous variations in the availability of home working arrangement and administrative records on the results of a national high-stakes exam, we find that children whose parents become eligible for teleworking improve their score at that exam by 9% of a standard deviation. The improvement leads to marginal students being recommended to attend higher tracks in secondary education. There is little heterogeneity in this effect based on the characteristics of the child or the parents. Eligible parents increase their use of teleworking by 17 percentage points, almost doubling the number of workers reporting teleworking, and even more if they work in an occupation with higher probability of homeworking. Working from home does not come at a cost to the firm (no drop in hours worked) or workers (no drop in wages) but has potentially large return to the child. Our preferred estimate is that having parents working from home increases the probability of attending the middle or upper secondary track by 4 percentage points. Borghans et al. (2019) estimate that switching from the lowest to the middle track in the Netherlands is associated with an increase in years of education of up to 1.5 years and a wage return of 6% to 11%. The returns to the marginal child affected by the policy appear large.

Overall we find that allowing WfH leads to an increase in the fraction of employees teleworking by 17 percentage points, which is close to the double the baseline rate. Since our estimates are Intention-to-treat estimates, the impact on children of parents who switched working mode is likely much larger. Even so, the effects appear large without correction. A recent study by Haelermans et al. (2022) based on Dutch data and CITO scores for reading, spelling and mathematics after one and-a-half years of the COVID-19 pandemic had an impact of similar magnitude (0.07 SD for reading and spelling, and 0.11

SD lower for mathematics). In a different context, Bettinger et al. (2014) evaluate the effects of a stay-at-home program in Norway targeting parents of children below the age of 3 and find that the program led to improvements in the GPA of the siblings (the older children of the parents affected by the reform) that were much smaller in magnitude: the older siblings in those families increased their grade-point averages in 10th grade by .02 points on Norway's grading scale of 1 to 6 points, where the standard deviation is around .8. However, only 5% of the parents changed their labor force participation in response to the reform, and the IV estimates focusing on the affect population were much larger (1.5 a standard deviation). As previously explained, due to differences in the sample and estimation strategy we cannot report an IV estimate but our reduced form estimate appears to be in line with these previous studies.

One caveat of our study and analysis is that we are not able to identify how teleworking arrangements affect entry into the labor force. It is possible that these arrangements may increase labor force participation and thereby reduce parental involvement for a fraction of the population. However, since teleworking arrangements are most relevant for the higher-educated part of the population, which has a relatively high participation rate (around 80%), the effects on such extensive margin may plausibly be low.

Allowing parents to work from home appears to be a Pareto efficient policy, improving test scores, and thus long-run outcomes of children at not costs to parents or firm. However, such a policy is not innocuous. While our sample of treated and control firms is balanced in terms of firms and workers characteristics, it is unrepresentative of the general population of firms and employees. Employees 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, especially if the number of places per track is fixed. 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 increases, 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 the social gap in educational attainments.

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A Data Annex

A.1 CBS Data

Table A.1: Definition of the main variables

Original data - CBS		Use
Dataset	Variable	
HOOGGSTEOPLTAB	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

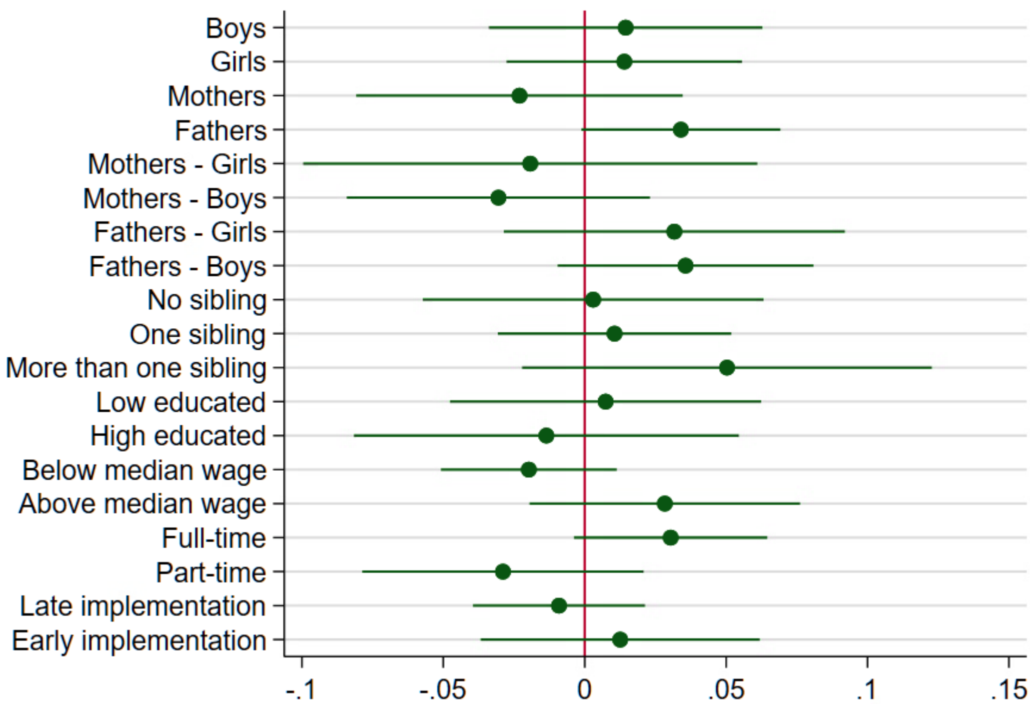
	(1)	(2)	(3)	(4)
	Z-Score		Eligible Track	
	Maths	Dutch	Uni	General Sec. & Uni
Panel A: Boys				
β	0.088*	0.084	0.014	0.032
	(0.048)	(0.057)	(0.021)	(0.022)
No. of Obs	7,186	7,186	7,186	7,186
Panel B: Girls				
β	0.112*	0.139***	0.015	0.064**
	(0.058)	(0.047)	(0.024)	(0.030)
No. of Obs	7,145	7,145	7,145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

	(0.066)	(0.088)	(0.040)	(0.043)
No. of Obs	2,679	2,679	2,679	2,679
Panel I: Low Educated				
β	0.034	0.072	0.007	-0.006
	(0.062)	(0.075)	(0.028)	(0.032)
No. of Obs	3,294	3,294	3,294	3,294
Panel J: High Educated				
β	0.085	0.047	-0.014	0.040
	(0.062)	(0.049)	(0.034)	(0.029)
No. of Obs	5,332	5,332	5,332	5,332
Panel K: Early Implementation				
β	0.059	0.039	0.013	0.026
	(0.043)	(0.042)	(0.025)	(0.022)
No. of Obs	7,228	7,228	7,228	7,228
Panel L: Late Implementation				
β	0.057*	0.112***	-0.009	0.039**
	(0.029)	(0.030)	(0.015)	(0.018)
No. of Obs	7,103	7,103	7,103	7,103
Panel M: Below Median Wage				
β	0.025	0.071	-0.020	0.015
	(0.043)	(0.044)	(0.016)	(0.021)
No. of Obs	7,165	7,165	7,165	7,165
Panel N: Above Median Wage				
β	0.121*	0.090**	0.028	0.055*
	(0.061)	(0.043)	(0.024)	(0.028)
No. of Obs	7,166	7,166	7,166	7,166
Panel O: Full-time				
β	0.112**	0.119***	0.0304*	0.0622***
	(0.0429)	(0.0334)	(0.0172)	(0.0214)
No. of Obs	10,404	10,404	10,404	10,404
Panel P: Part-time				
β	0.0659	0.0895*	-0.0289	0.0148
	(0.0490)	(0.0527)	(0.0251)	(0.0322)
No. of Obs	3,927	3,927	3,927	3,927
Panel Q: No sibling				
β	0.126*	0.126*	0.0297	0.0476
	(0.0688)	(0.0704)	(0.0304)	(0.0430)
No. of Obs	3,646	3,646	3,646	3,646

Panel R: One sibling				
β	0.115***	0.119***	0.0105	0.0556***
	(0.0358)	(0.0449)	(0.0208)	(0.0185)
No. of Obs	7,358	7,358	7,358	7,358
Panel S: More than one sibling				
β	0.0743	0.0963	0.0523	0.0476
	(0.0803)	(0.0667)	(0.0365)	(0.0439)
No. of Obs	3,327	3,327	3,327	3,327

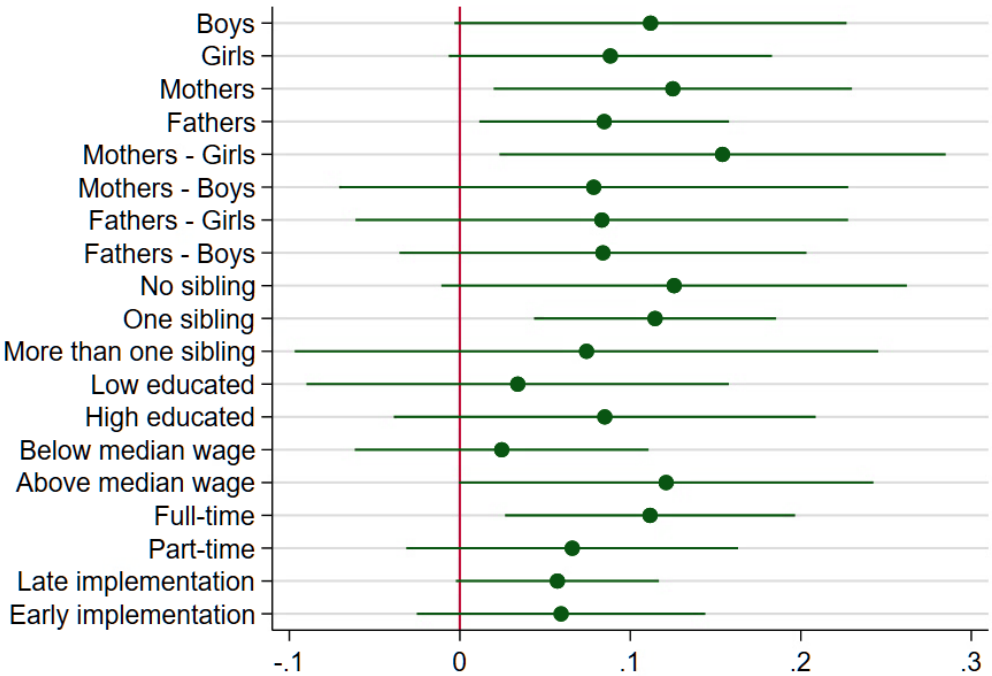
Notes: This table presents estimates of the effect of parental teleworking on children's educational outcomes, examining heterogeneity by the gender of the parent, the gender of the child, the education level of the parent, the wage level of the parent, the number of siblings, whether the parent was working full-time 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, 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure B.1: Heterogeneity in the Effect of Wfh – Eligibility Uni track



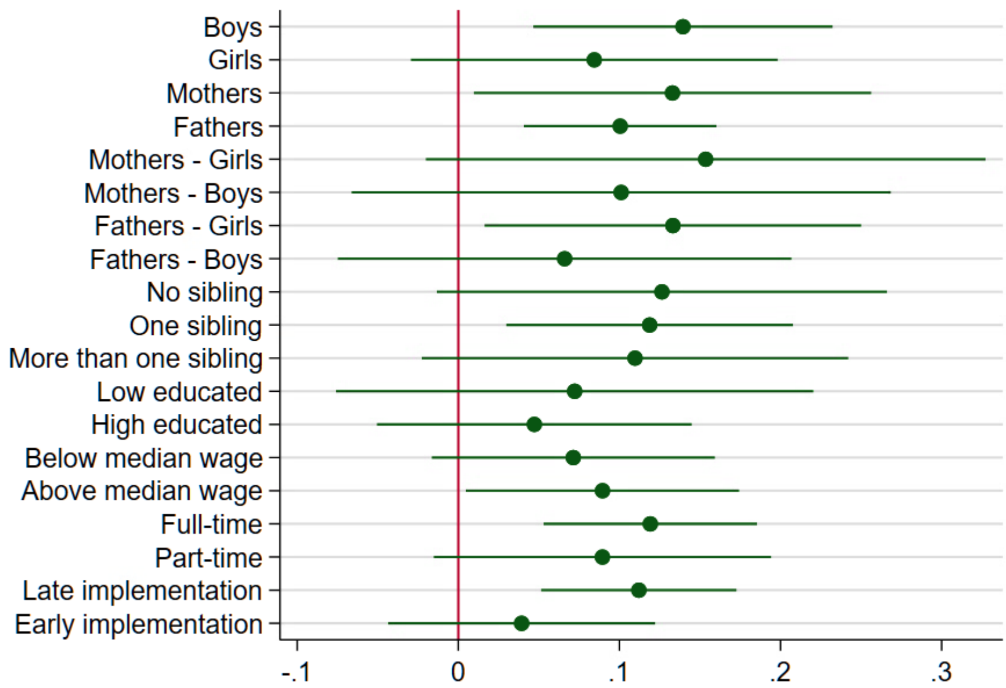
Notes: This figure presents heterogeneity in the estimated effect of teleworking, based on the specification in Table 3, Panel B. Each estimate comes from a separate regression. The outcome is a dummy for achieving a score qualifying for the university track. The rows represent different parental characteristics, namely the gender of the parent, the gender of the child, the education level of the parent, the wage level of the parent, the number of siblings, whether the parent was working full-time and the period when the right to WfH was granted. Standard errors are clustered at the firm level.

Figure B.2: Heterogeneity in the Effect of Wfh – Z-Score Maths



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 outcome is the Z-Score of the Maths component of the CITO. 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, the number of siblings, whether the parent was working full-time and the period when the right to WfH was granted. Standard errors are clustered at the firm level.

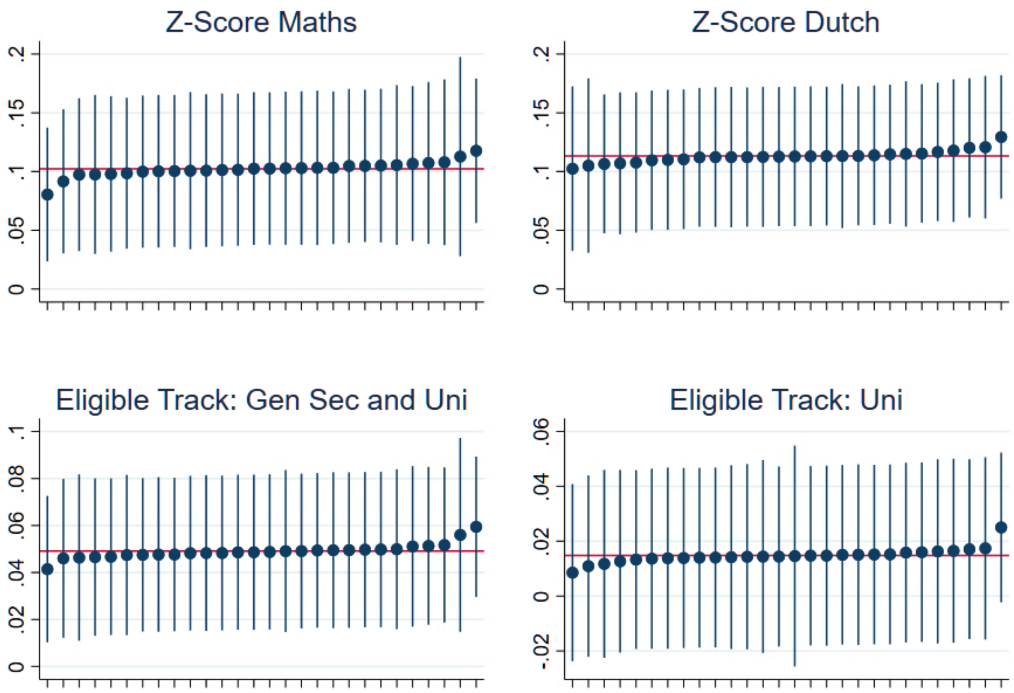
Figure B.3: Heterogeneity in the Effect of Wfh – Z-Score Dutch



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 outcome is the Z-Score of the Dutch component of the CITO. 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, the number of siblings, whether the parent was working full-time and the period when the right to WfH was granted. Standard errors are clustered at the firm level.

B.2 Jackknife estimates

Figure B.4: **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, 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

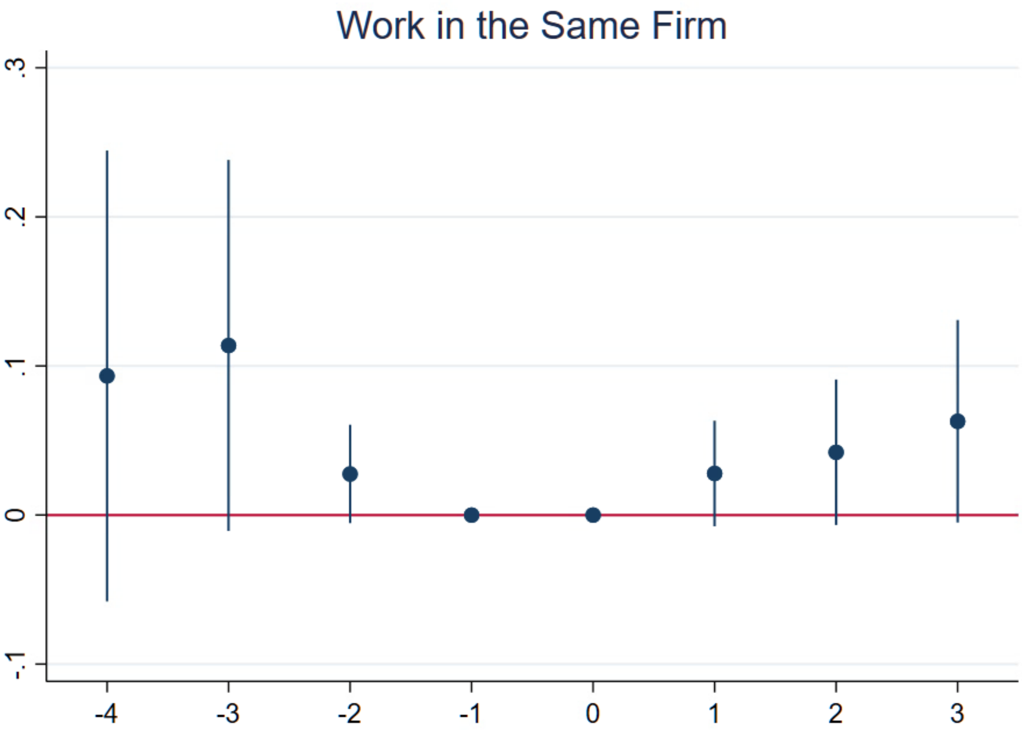
Table B.3: Robustness Checks: Alternative Sample and Matching Algorithms

	(1)	(2)	(3)	(4)
	Z-Score		Eligible Track	
	Maths	Dutch	Uni	General Sec. & Uni
Panel A: With Controls - 3 Matches				
β	0.09***	0.093***	0.003	0.045***
	(0.034)	(0.031)	(0.017)	(0.017)
No. of Obs	11,815	11,815	11,815	11,815
Mean - Control group	0.05	0.10	0.20	0.53
Panel B: With Controls - Alt1				
β	0.086***	0.092***	0.037**	0.038**
	(0.032)	(0.034)	(0.016)	(0.018)
No. of Obs	13,431	13,431	13,431	13,431
Mean - Control group	0.12	0.17	0.20	0.56
Panel C: With Controls - Alt2				
β	0.099***	0.105***	0.022	0.041**
	(0.033)	(0.032)	(0.016)	(0.017)
No. of Obs	15,934	15,934	15,934	15,934
Mean - Control group	0.03	0.07	0.20	0.51
Panel D: Robustness - 2 Years Prior				
β	0.11***	0.112***	0.019	0.046**
	(0.036)	(0.032)	(0.017)	(0.019)
No. of Obs	13,345	13,345	13,345	13,345
Mean - Control group	0.05	0.09	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, 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.4 Alternative samples

Figure B.5: **Event study all firms**



Notes: This figure corresponds to the one reported in Figure 2 when the sample restriction detailed in subsection 4.4.1 is not applied.

Table B.4: Regression Results – Robust Sample

	(1)	(2)	(3)	(4)
	Z-Score		Eligible Track	
	Maths	Dutch	Uni	General Sec. & Uni
Panel A: No Controls				
β	0.119***	0.139***	0.0236	0.0520***
	(0.0379)	(0.0380)	(0.0194)	(0.0191)
Panel B: With Controls				
β	0.109***	0.111***	0.0199	0.0453***
	(0.0360)	(0.0336)	(0.0181)	(0.0182)
Panel C: Sector FE				
β	0.106***	0.105***	0.0185	0.0437**
	(0.0377)	(0.0338)	(0.0184)	(0.0187)
Panel D: Matching FE				
β	0.0984***	0.0936**	0.0150	0.0399**
	(0.0354)	(0.0368)	(0.0183)	(0.0187)
No. of Obs	11,828	11,828	11,828	11,828
Mean - Control group	0.06	0.11	0.21	0.53

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, 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. The sample corresponds to children whose parents were working—at the time of change in CLA—in firms which appear in the administrative registry in 2006—as explained in subsection 4.4.1. Standard errors are clustered at the firm level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.5 Additional outcome variables

Table B.5: Regression Results – Teachers' recommendations

	(1)	(2)
	Teacher Recommendation	
	Uni	General Sec. & Uni
Panel A: No Controls		
β	0.0295	0.0556**
	(0.0179)	(0.0242)
Panel B: With Controls		
β	0.0281*	0.0526**
	(0.0158)	(0.0227)
Panel C: Sector FE		
β	0.0229	0.0476**
	(0.0142)	(0.0222)
Panel D: Matching FE		
β	0.0167	0.0409*
	(0.0130)	(0.0222)
No. of Obs	10,520	10,520
Mean - Control group	0.17	0.48

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 teachers' recommendation 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.

B.6 Working from Home by Teleworkable Occupation

Table B.6: LFS - Triple Difference - Teleworkable Occupations

	(1) Teleworking	(2) Hours Worked
Treated Firms	-0.038	1.620
	(0.0484)	(1.314)
Post-CLA	-0.00225	-0.728
	(0.0491)	(1.413)
Teleworkable	0.140***	3.355***
	(0.039)	(0.721)
Treated × Post-CLA - θ	0.0952	-0.418
	(0.0627)	(1.737)
Teleworkable × Treated	-0.0129	-3.687**
	(0.0409)	(1.525)
Teleworkable × Post-CLA	0.0826	0.669
	(0.0444)	(1.250)
Treated × Post-CLA × Teleworkable - β	0.0566	2.769
	(0.0605)	(1.807)
R-squared	0.08	0.29
N Obs	3,794	9,951
Mean - Control group	0.17	33.43
F-test - $\mathbb{H}_0 : \theta + \beta = 0$	7.91	15.01
p-value	0.01	< 0.01

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 × Post-CLA × Teleworkable Occupation) captures the differential effect for individuals in teleworkable occupations. The specification includes year fixed effects and individual controls. The sample consists of individuals working in treated and control firms who were interviewed in the Dutch Labor Force Survey (LFS). The F-test corresponds to the null hypothesis that the effect on people with a teleworkable ocupation is equal to zero. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.7 Results on the parents

As highlighted by De Chaisemartin and d'Haultfoeuille (2023), two-way fixed effects (TWFE) provide unbiased estimates when policy changes occur in the same year—the design is not staggered—and control groups are never treated—there is no "forbidden comparisons". To ensure unbiased estimation, we run the following regression separately for each treatment year a, defined as the year when $j = 0$:

$$y^a_{i,t} = \alpha + \lambda_t + \theta_k + \Gamma_a X_i + \beta_a \text{ Treated}_f + \sum_{\substack{j=-4 \\ j \neq -1}}^{3} \beta^a_j \text{ Treated}_f \times \psi_j + \epsilon_{i(f)} \tag{B.1}$$

This specification produces an unbiased estimate β^a_j for each treatment year. To obtain a combined estimate across all treatment years, we use the following weighted formula:

$$\beta_j = \frac{N^a}{N} \text{ATT}^a_j = \frac{N^a}{N} \beta^a_j \tag{B.2}$$

where N^a is the number of observations for treatment year a, and N is the total number of observations across all years. The results are reported in Table B.7.

While the combined estimates are not numerically identical to those in Table 10, they are similar in magnitude—if anything, they tend to be slightly smaller in absolute value. This consistency further supports the validity of the two-way fixed effects model used in our analysis.

Table B.7: Combination of ATTs

t	(Ln) Earnings	(Ln) Hours Worked	Participate in Labor Market	Work in Same Firm
-4	0.0041	0.0157	0.0023	0.0894
-3	0.0093	0.0014	0.0010	-0.0178
-2	0.0037	0.0040	-0.0001	-0.0126
0	0.0052	-0.0119	0.0002	0.0003
1	0.0095	-0.0059	0.0014	0.0374
2	0.0141	-0.0417	-0.0020	0.0427
3	0.0208	-0.0520	-0.0042	0.0532

Notes: This table presents the combined estimated effects of the policy change on parents' labor market outcomes, calculated using a weighted two-way fixed effects (TWFE) model. The four outcome variables are the same as in Table 10: logarithm of yearly earnings, logarithm of hours worked, probability of participating in the labor market, and probability of remaining employed at the same firm. The coefficients represent the weighted average of treatment effects across different treatment years.

Table B.8: Effects on fathers

t	(1) (Ln) Earnings	(2) (Ln) Hours Worked	(3) Participate in Labor Market	(4) Work in Same Firm
-4	-0.039	0.015	0.001	-0.034
	(0.037)	(0.042)	(0.001)	(0.084)
-3	-0.038	-0.018	-0.001	-0.031
	(0.037)	(0.028)	(0.002)	(0.039)
-2	-0.003	-0.019	-0.001	-0.019
	(0.010)	(0.020)	(0.002)	(0.026)
0	0.004	-0.022	0.001	0.004
	(0.014)	(0.014)	(0.001)	(0.022)
1	0.015	-0.042	-0.002	0.056
	(0.017)	(0.057)	(0.006)	(0.045)
2	0.023	-0.081	-0.006	0.061
	(0.023)	(0.099)	(0.009)	(0.046)
3	0.038	-0.078	-0.009	0.070
	(0.024)	(0.107)	(0.011)	(0.052)
R-squared	0.04	0.03	0.04	0.10
No. of Obs	51,434	52,189	52,189	23,939
F-test pre	0.41	0.50	0.25	0.26
p-value pre	0.66	0.48	0.86	0.85
F-test post	1.32	0.81	0.30	0.60
p-value post	0.25	0.52	0.88	0.67

Notes: This table presents the estimated effects of the policy change on fathers' labor market outcomes using an event study model. The coefficients correspond to the estimated impacts of the policy on the logarithm of yearly earnings, logarithm of hours worked, the probability of participating in the labor market, and the probability of remaining employed at the same firm. Standard errors are clustered at the firm level and reported in parentheses. F-tests assess the joint significance of coefficients in the pre- and post-policy periods. As explained in subsection 4.4.1, the sample size in (4) is smaller.

Table B.9: Effects on Mothers

t	(1) (Ln) Earnings	(2) (Ln) Hours Worked	(3) Participate in Labor Market	(4) Work in Same Firm
-4	-0.022	0.093	0.008	0.007
	(0.037)	(0.071)	(0.008)	(0.074)
-3	0.000	0.104**	0.005	-0.036
	(0.034)	(0.049)	(0.005)	(0.036)
-2	0.012	0.040*	0.002	-0.021
	(0.009)	(0.020)	(0.002)	(0.023)
0	0.019	0.017	0.000	-0.015
	(0.024)	(0.029)	(0.002)	(0.012)
1	0.013	0.077	0.009	-0.036
	(0.022)	(0.053)	(0.006)	(0.026)
2	0.012	0.056	0.006	-0.040
	(0.027)	(0.066)	(0.007)	(0.028)
3	0.014	0.033	0.008	-0.047
	(0.031)	(0.070)	(0.008)	(0.037)
R-squared	0.18	0.030	0.02	0.07
No. of Obs	29,710	30,224	30,224	13,697
F-test pre	2.25	2.51	0.65	2.48
p-value pre	0.09	0.06	0.59	0.07
F-test post	0.80	0.58	2.04	0.54
p-value post	0.94	0.50	0.08	0.71

Notes: This table presents the estimated effects of the policy change on mothers' labor market outcomes using an event study model. The coefficients correspond to the estimated impacts of the policy on the logarithm of yearly earnings, logarithm of hours worked, the probability of participating in the labor market, and the probability of remaining employed at the same firm. Standard errors are clustered at the firm level and reported in parentheses. F-tests assess the joint significance of coefficients in the pre- and post-policy periods. As explained in subsection 4.4.1, the sample size in (4) is smaller.