Does new information technology change commuting behavior?

Abstract

We estimate the long-run causal effect of information technology, i.e. Internet and powerful computers, on average commuting distance within professions in the Netherlands. We employ data for two years—1996 when information technology was hardly adopted and 2010 when information technology was widely used in a wide range of professions. Variation in information technology adoption over time and between professions allows us to infer the causal effect of interest using difference-in-differences techniques combined with propensity score matching. Our results show that the long-run causal effect of information technology on commuting distance is too small to be identified and likely to be absent. This suggests that, contrary to the assertions by public opinion makers, the advent of information technology did not have a profound impact on the spatial structure of the labor market.

Keywords: Information technology, commuting, difference-in-differences

JEL-classification: R1, R2

1. Introduction

John Maynard Keynes in 1930 (see Keynes, 2010) infamously envisioned the future in which, thanks to technological progress, a fifteen-hour working week would be the norm.

While for many individuals who currently endure a forty-hour working week this prediction might stand as wishful thinking, it is widely accepted that technological progress positively affects productivity and output growth (Jorgenson et al., 2008; Commander et al., 2011) although not necessarily hours worked (e.g., Pissarides, 2000). Ongoing advancement in information technology which is probably the most spectacular technological development over the past two decades strongly affects how people perform their job duty. This paper takes a closer look at one such aspect by examining the causal long-run effect of the growth in adoption of information technology on average commuting distances within professions.

It is nowadays quite common that a job duty is subdivided into separate job tasks which might be performed at locations other than the conventional workplace. Residential location is one of the places where employees might carry out job tasks by engaging in teleworking, an out-of-office work arrangement, for some days of the week or hours of the day. Until the nineties, teleworking was mainly associated with (low-paid) manual jobs (see IDS, 1996). However, it has become increasingly relevant for other types of jobs as well, largely due to the continuing technological progress with regard to telework enabling technology, such as e-mail, smartphones and the Internet. For example, EU average incident rates of teleworking among employees were around 4% and 7% in, respectively, 2000 and 2005, but with large variations between countries and industries (Paoli, 2001; Welz and Wolf, 2010). Potential benefits from increased teleworking adoption, which include, among others, reduced congestion, better work-life balance (James, 2014), improved job matching and higher productivity (while the latter might be debatable due to shirking although Bloom et al. (2015) provide quasi-experimental evidence of a positive effect of teleworking on routine tasks' performance), have led many governments to develop friendly policy measures towards adoption of information technology. For instance, the US Telework Enhancement Act of 2010 endorses teleworking among public employees, while the European Framework Agreement on Telework in 2002 promotes telework-friendly policies in the EU.[1]

Despite strong policy support for information technology adoption, not least on the ground of its mitigating effects on the negative externalities of transport, such as congestion and pollution (De Borger and Wuyts, 2011), the possible countervailing causal effect of improved information technology on commuting distance traveled has never been convincingly estimated. Previous research has been largely descriptive, mostly due to lack of data (see discussions on causality identification in Mokhtarian et al., 2004; and Moos and Skaburskis, 2008). It is neither straightforward to anticipate the sign of the technology effect based on socio-economic theory. If an employee works from home for some days during the week, then the number of trips to work is reduced.[2] However, teleworking might in the long-run also result in a relocation of residential and employment sites which changes the commuting distance per trip.

Thus, the possibility to telework may induce individuals to choose their residential locations further away from the workplaces or, alternatively, choose workplaces which are further from home, so the commuting distance (per trip) increases. Lund and Mokhtarian (1994), Safirova (2002), Rhee (2008) and Glaeser (2008, p. 41) provide urban economic models on the relocation of households due to information technology, which show that commuting distance might be longer for employees who telework (hereafter, teleworkers) than for other commuters. Technological progress not only has allowed workers to perform tasks at home, but it also has allowed many workers to perform tasks at other work locations than their own work place, which provides an even stronger incentive to workers to lengthen their commute. Thus, such long-run behavioral response of employees to information technology might, in principle, be detrimental for social welfare, as negative

[1]Several governments have official Internet pages that endorse teleworking, for example, `http://www.telework.gov.au` in Australia and `http://www.telework.gov` in the USA.

[2]In a similar fashion, an employee who teleworks part-time during the day, to avoid peak period congestion, experiences a lower generalized commuting cost as well.

transport externalities, for example, congestion and pollution, might aggravate.

In these models, however, it is ignored that the information technology might also affect non-teleworking employees (hereafter, non-teleworkers) within firms where relatively many people telework. This might happen for various reasons. First, workers who prefer to work from home in the near future (e.g., as they expect children) are more likely to move their residence further away from the job (e.g., to the suburbs) when this job allows for teleworking (Van Ommeren et al., 1999). Importantly, this possibility is consistent with several studies that point out that many employees iterate short periods of teleworking with prolonged periods of conventional working, which is often driven by the nature of a particular job task at hand (Bailey and Kurland, 2002). Secondly, due to new technology, firms might find it profitable to locate away from central urban places with high land rents and instead, to save on land costs, locate in cheaper but more accessible places that might be closer to residential locations. Finally, and perhaps most importantly, progress in information technology in general and teleworking in particular most likely changes agglomeration economies. Some research suggests that agglomeration economies might decrease because face-to-face interactions of workers employed by different firms are less frequent and thus might be less valuable (see, for example, literature surveys by Anas et al., 1998; and Audirac, 2005)). However, most theoretical and empirical research suggests exactly the opposite: information technology increases agglomeration economies (see, among others, Gaspar and Glaeser, 1998; and Storper and Venables, 2004). Therefore, we focus on the empirical question of the long-run effect of technology adoption on the average commuting distance of both teleworkers and non-teleworkers combined.

To answer this question, one has to estimate the information technology effect on commuting distances of both teleworkers and non-teleworkers within the same profession (we define profession as a particular job within a given industry), while accounting for reverse causality and omitted variable bias.[3] The issue of reverse causality is fundamental in the estimation of the effect of technology adoption on commuting distance, as employees who commute long distances might have stronger incentives to telework. Thus, a naïve OLS approach of explaining employees' commuting distance by teleworking would produce biased estimates (likely to be overestimates). An experimental setup, in which the opportunity to use information technology would be provided to only one of two otherwise identical groups of employees is one of the ways to avoid this bias and estimate the average causal effect (see, e.g., Angrist and Pischke, 2008). The major disadvantage of this approach, provided that it is feasible, is the short-run nature of a typical experiment in comparison to the effect of teleworking on commuting distances which manifests itself over the long term through changes in workplaces and home locations. An instrumental variable approach is another alternative. However, an instrument for the use of information technology that does not correlate with commuting distance is hard to find, as commuting distance and teleworking are both related to behavior in labor and housing markets.[4]

In this study we introduce an innovative methodology that uses information from two years—one year when information technology was scarcely adopted for teleworking and a more recent year when teleworking is technologically possible and adopted in a wide range of professions. We employ cross-sectional Dutch labor force surveys for the years 1996 and 2010, which provide relevant data on teleworking activities of workers. The information technology of 2010, such as high-speed Internet and powerful computers, was generally not widely available in 1996.[5] In contrast, high-speed Internet and powerful computers were pervasive in 2010. We also note that technological progress affects production functions

[3]For example, workers with larger residences are more likely to live further away from the workplace and are more likely to prefer to work from home.

[4]Zhu (2012) and Zhu (2013) analyses the effect of teleworking adoption on teleworkers and employs "Internet use at home" as an instrument for teleworking. However, when unobserved professional abilities of employees are correlated with the use of information technology, such as the Internet, then the instrument is not valid. So, this strategy implicitly assumes the absence of such a relation. Our identification strategy avoids such a restrictive assumption.

[5]Note, that we will *not* assume that the teleworking incidents rate was zero in 1996. Our estimation approach allows for the possibility that employees were working from home in 1996.

of various professions in a different way, making telework a more feasible arrangement in some professions, but less so in others. Such variations in teleworking adoption over time and professions will allow us to infer the causal effect of interest *for professions* through difference-in-differences after applying propensity score matching.

In a nutshell, at first, we consider commuting distances of workers in 2010 who work in teleworking professions—we define these professions based on a minimum share of teleworkers in 2010, a year when the use of information technology is common.[6] Workers in these professions are considered treated. Then, we compare these commuting distances with the ones of *comparable* employees in the same professions in 1996 who, by assumption, did not have access to information technology. These employees are considered the non-treated sample. In our preferred specification, to find comparable employees across time in treated professions, we match employees from 2010 and 1996 within the same industries. The difference in commuting distance between these matched employees of 1996 and 2010 is due to information technology plus an industry-specific time trend, which we account for. We define this trend, which might vary across industries due to, for example, variations in demand shocks, by the difference in commuting distances of matched employees from 2010 with 1996 within a given industry in non-treated professions. We interpret the resulting difference-in-differences estimate as the average causal effect of information technology on commuting.[7]

While different workers might sort into different types of professions in 1996 and 2010, our estimation procedure aims to account for this, by comparing similar employees in terms of their socio-demographic and job related characteristics (which include, among others,

[6]Multiple factors affect technology adoption by an employee, including idiosyncratic distaste for teleworking, managerial practices (for example, Yahoo! forbade teleworking in 2013), or certain characteristics of the labor market (see Mokhtarian, 1998).

[7]As we explain later, our method differs from "difference-in-differences propensity score matching" methodology (see, e.g., Hijzen et al., 2013), which relies on panel data to observe the same individuals or firms over time. We observe the same *professions* over time but use propensity score matching to account for differences in labor force composition *within professions* over time.

age, gender, family size, education, industry, job, firm size, and total hours of work). Although we cannot fully exclude the possibility that some variables uncorrelated with such characteristics might affect the general trends differential in commuting distances between professions within the same industry, it is not obvious what these variables are and how empirically relevant they could be. However, if our main assumption does not hold and thus the within industry time trends are not similar for treated and non-treated profession, our results should be seen as overestimates if—as some might argue—within industry time trends are steeper for treated than for non-treated professions.

We find no evidence that the adoption of information technology causes commuting distances to increase. In treated and non-treated professions, the average commuting distance between 1996 and 2010 increased by about 2 km.

The paper is organized as follows. Section 2 presents the identification strategy and inference. Section 3 gives an overview of the data, provides definitions of treated and non-treated professions and presents results of the matching procedures and difference-in-differences estimation. Section 4 offers varying robustness checks related to our main assumptions. The last section concludes with the discussion of the results.

2. Methodology

2.1. Identification strategy

This paper aims to estimate the long-run change in average commuting distances caused by the availability of information technology by profession. An ideal experimental study to uncover the causal effect of interest would be to randomly supply this technology to some professions, so there is a group of treated professions with and a group of untreated professions without technology (Angrist and Pischke, 2008). Then, after a certain time period, a comparison of both groups' average commuting distances would identify the causal effect. This hypothetical experiment must take a considerable period of time as changes in home and work locations induced by workers occur infrequently (e.g., Zax, 1991). Also changes in distance due to workplace relocations induced by employers who have an incentive to change the workplace location of treated and untreated professions will often take quite a considerable time (Mulalic et al., 2014). Obviously, this ideal experiment is infeasible.

Therefore, we propose an alternative identification strategy based on observational data, which, arguably, comes close to this ideal setup. Our goal is to estimate the following expression:

$$\Delta = \mathbb{E}[Y_j|d_j = 1] - \mathbb{E}[Y_j|d_j = 0], \qquad \text{for } j = 1 \tag{1}$$

where Δ denotes the average treatment effect of information technology on commuting distance, Y_j refers to the average commuting distance of individuals who works in profession type j and $\mathbb{E}$ denotes the expectation operator. We distinguish between non-treated ($j = 0$) and treated professions ($j = 1$). The treatment dummy d_j equals 1 if the technology is used by a substantial share of employees in profession j and equals 0 if there is no teleworking in profession j. In our empirical application, we will use different thresholds that define such a "substantial share" and will focus on professions with relatively small shares of teleworkers in the sensitivity analysis.[8] So, treatment in this paper is an profession's exposure to and adoption of information technology, which we measure by observing the teleworkers share within a profession. Our definition of

[8]The main advantage of using a binary measure of teleworkers share is that it drastically reduces the effect of measurement error in our teleworking variable. It seems reasonable to expect the effect of teleworking on commuting to be stronger the larger is the share of adopters within the profession. We confirm this assertion by repeating the entire analysis using data on professions with relatively low adoption rates, as we find no effect of technology on commuting.

treatment captures the impact of adoption of teleworking technology on commuting distance of *all* employees in a profession (and not only of the share who are observed to telework during a certain period). As already argued above, including employees in treated professions who do not telework is essential, because firms and non-teleworkers might change their location of work and residence as well due to the availability of new technology.

To estimate $\mathbb{E}[Y_1|d_1 = 1] - \mathbb{E}[Y_1|d_1 = 0]$, i.e., the average treatment effect on the treated, where the treatment is the profession's adoption of information technology, we start from the observation that information technology, such as e-mail and the Internet, were not widely available to, and hardly used by, workers in any professions in a certain year 0, but widely available to many workers in year 1.[9] We also observe that in year 1 the probability of teleworking differs strongly among professions due to differences in the production functions which require certain professions to be present at the workplace, whereas other professions are more footloose (conditional on available technology). One might think, for example, of a hospital doctor who has to be present at the workplace and a graphic designer who might work from home on certain days of the week in year 1, but not in year 0. It is then plausible that the designer chooses a longer commuting distance in year 1. At the same time, in year 1 organizations may relocate the workplace of designers closer to their residence locations (e.g., from the central business district to the suburbs), as the new technology may make it beneficial to locate further away from other business organizations. We will exploit variations in adoption of teleworking across professions and time to infer the causal effect of information technology on commuting distance.

Formally, we observe the average commuting distance in year 1 of a group of employees who work in treated professions (and who are comparable in observed characteristics to

[9] In our application we will use the years 1996 and 2010.

those in treated professions in year 0). We also observe the average commuting distance in year 1 of employees who work in non-treated professions (and who are comparable in observed characteristics to those in non-treated professions in year 0). We denote both average commuting distances for these groups as $\mathbb{E}[Y_j|d_j = j; t = 1]$ where t denotes the year of observation. The long-run causal effect of technology, Δ, is defined by:

$$\begin{aligned}\Delta =& \mathbb{E}[Y_1|d_1 = 1; t = 1] - \mathbb{E}[Y_1|d_1 = 0; t = 0] - \\ & \mathbb{E}[Y_0|d_0 = 0; t = 1] - \mathbb{E}[Y_0|d_0 = 0; t = 0],\end{aligned} \tag{2}$$

where the first row is the change over time in average commuting distances of employees in treated professions. This difference may be attributed to a general time trend in commuting distances (e.g., due to changes in the transport infrastructure supply, the spatial structure of the built environment or income) and the effect of the adoption of the technology. The second row captures the change over time in the average commuting distances for employees in non-treated professions. The latter difference reflects solely a general (or industry-specific) time trend (as neither in year 1 nor in year 0 the technology is adopted).

We subtract and add the term $\mathbb{E}[Y_1|d_1 = 0; t = 1]$, which refers to the average commuting distance of employees in treated professions in year 1 if they *would not have adopted* the technology. So, (2) can be written as:

$$\begin{aligned}\Delta =& \mathbb{E}[Y_1|d_1 = 1; t = 1] - \mathbb{E}[Y_1|d_1 = 0; t = 1] + \\ & \mathbb{E}[Y_1|d_1 = 0; t = 1] - \mathbb{E}[Y_1|d_1 = 0; t = 0] - \\ & \mathbb{E}[Y_0|d_0 = 0; t = 1] - \mathbb{E}[Y_0|d_0 = 0; t = 0].\end{aligned} \tag{3}$$

The first row of (3) is the average treatment effect on the treated in year 1. The second row is a time trend which shows how commuting distance would have changed for employees who work in treated professions if the technology was not adopted in these professions in both years. The third row is the same as the last row of (2). To identify the causal effect, in our preferred specification, we perform industry-specific estimations, and thus assume that the time trends in average commuting distance in a given industry for workers in non-treated professions and for workers in treated professions are identical if information technology would not have been available to them, i.e., the second and the third rows in (3) are assumed to be equal. In other words, on average across professions within a given industry time trends are assumed to be the same. For comparison reasons we also perform the analysis assuming the same trend across all industries. This (industry-specific) same-trend assumption seems reasonable given the common exposure to changes in the (national) transportation infrastructure, changes in housing markets and the general development of the economy.[10] Note, however, that it still may be the case that *within* industries the workforce composition within non-treated professions might differ from the workforce composition within treated professions. We will check for a possible violation of this crucial assumption in the empirical analysis section.

Given this assumption, we can write (3) as:

$$\Delta = \mathbb{E}[Y_1|d_1 = 1; t = 1] - \mathbb{E}[Y_1|d_1 = 0; t = 1]. \tag{4}$$

Expression (4) is identical to (1) for year $t = 1$. Figure 1 outlines the identification strategy's idea graphically.

As one step further, one could decompose the average treatment effect (on the treated professions), as defined by (4) into two (non-causal) parts. By construction, this effect is the sum of the change in distance for teleworkers in treated professions plus the change

[10]Treated and non-treated professions comprise of many different professions (both comprise professions which require low, medium or high education), so we allow for the possibility that some professions have a different trend, but not the average (industry-specific) trend.

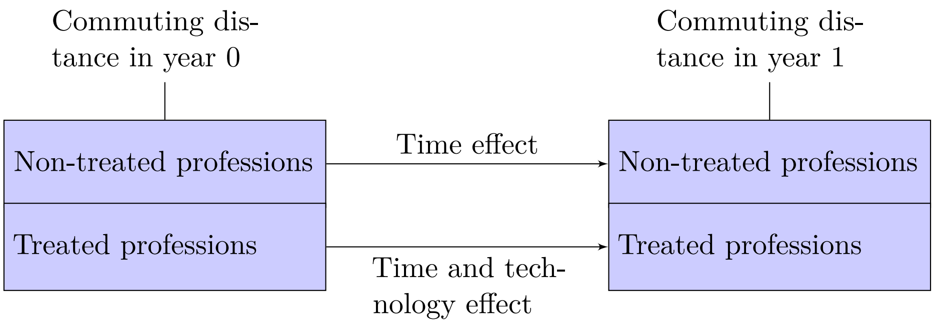

Figure 1 – Identification strategy

in distance for non-teleworkers in these professions. So, a straightforward accounting identity holds:

$$\Delta = \mathbb{E}[Y_1|d_1 = 1; t = 1] - \mathbb{E}[Y_1|d_1 = 0; t = 1] = s\alpha_1 + (1 - s)\alpha_0, \tag{5}$$

where s is the share of non-teleworking employees within treated professions, α_1 is the effect of technology on commuting distance of teleworkers within treated professions, α_0 is the effect for non-teleworkers within treated professions. It is difficult to identify the causal effect of technology adoption on commuting distances of teleworkers and non-teleworkers in treated professions, as we do not observe the relevant counterfactuals. Apart from the potential nontrivial measurement error of distinguishing teleworkers and non-teleworkers within treated professions, it is very likely that individuals self-select themselves into longer commutes based on certain unobserved characteristics, for example, they own a larger house, where they can telework more productively as they have a special office room. We do not know how their commuting distance would have changed over time if they would not have been exposed to the technology, although the knowledge of the magnitude of the technology effect on individuals is relevant for policy-making. In this empirical exercise we think this is hardly a problem for the estimation of the average effect of technology on commuting for the entire profession, as the unobserved characteristics do not correlate across a large set of professions which we aggregate into two types—treated and non-treated professions.

2.2. Inference

We emphasize that we match observations from years 0 and 1. So, we aim to find comparable employees within both types of professions in years 0 and 1. In our preferred specification we match within a given industry, as it would later require less restrictive assumption on the same time trend in commuting distances across treated and non-treated professions. However, the mechanics of inference is the same if one matches across all industries. For the ease of exposition we perform matching across all industries and later report results of within industry matching.

Let us first focus on employees in treated professions. Our method must create a counterfactual of average distance of these employees in year 1, that is, the average distance if none of these employees had adopted technology in year 1. This is achieved by estimating the employee's probability of being exposed to the technology. By assumption, for employees in treated professions this probability is identical to the probability of being observed in year 1 based on sociodemographic and job-related employee characteristics. So, we compute the probability of an individual to be exposed to information technology, the so-called propensity score. Conditional on this score and in the absence of selection on unobserved characteristics, whether individuals have been exposed to information technology is assumed to be random.[11] This assumption allows for an inference of the causal effect of interest in a similar fashion to a randomized experiment.[12] We use kernel matching and match a treated employee (in year 1) with control employees (in year

[11]We formally test the balancing property of the matching to insure that observations in two matched groups are similar in observed variables.

[12]For an overview of the method and its limitations we refer to Dehejia and Wahba (1999), Angrist and Pischke (2008), and Caliendo and Kopeinig (2008).

0) who are within the kernel bandwidth, by weighting proportionally to the difference between propensity scores, i.e., if a control observation has propensity score which is closer to that of a treated observation than the weight of such a control is larger Caliendo and Kopeinig (2008).[13] Effectively, we match a single person within treated professions in year 1 to a statistical composite of individuals in year 0 who work in treated professions. The same procedure applies for employees in non-treated professions. Given the two matching estimations (for treated and non-treated professions), we calculate the average commuting distances of the matched groups in years 0 and 1 and then estimate expression (2) to obtain the causal effect of information technology on commuting distance.

Our methodology is distinctively different from, but related to, a similar methodology, sometimes referred to as "difference-in-differences in combination with matching" that has been applied in the labor and international trade literature (Girma and Görg, 2007; Arnold and Javorcik, 2009; Stiebale and Trax, 2011; Hijzen et al., 2013). This methodology follows individuals (or firms) over time, whereas we follow professions over time. However, we deviate from this literature by matching individuals *within professions* before and after treatment, where this literature matches individuals in the treatment with the control group. The latter approach would provide the sum of the effect of information technology and an age cohort effect and since the latter might be non-negligible, as we focus on a long time gap between both years, this approach is not preferable in our context.[14] The sensitivity analysis shows that the results are robust if, among other, we repeat the analysis on workers in year 1 who are matched with the appropriately younger (14 years in our study) workers in year 0.

[13] We will perform robustness checks with respect to the matching procedure.

[14] In case the cohort effect is not important or that it does not differ across treated and non-treated professions, panel data would be preferable.

3. Empirical analysis

3.1. Data

Our main data source provided by Statistics Netherlands is the cross-sectional Dutch Labor Force Survey for the years 1996 and 2010 which refer to years 0 and 1 in the discussion above. The dataset contains information on socio-demographic and job-related characteristics of 74,235 individuals (38,179 and 37,122 for 1996 and 2010, respectively).[15] We have information on a person's age, gender, marital status, education level, household size and composition, country of origin and municipality of the residential and job location. We also know employment status, total number of hours worked per week, whether a person has a fixed job contract, number of working hours, number of overtime hours, whether one manages subordinates, number of workers of the establishments, and job and industry type (as classified by Statistics Netherlands). We use the share of teleworkers as a measure of the take-up of information technology. The composition of teleworkers in 2010 is substantially different from the composition of non-teleworkers in that year. We refer to Table A.1 in Appendix A for descriptive statistics. However, the characteristics of non-teleworkers in treated professions are quite similar to the characteristics of non-teleworkers in non-treated professions.

Commuting distance is measured based on the centroids of the residential and workplace municipalities. There are around 400 municipalities in the Netherlands. The commuting distance is assumed to be zero for persons who work and live in the same municipality (about 43% of employees in 1996 and 2010).[16] One-way average commuting distances

[15] We consider employed individuals, between 18 and 64 years, working more than 12 hours per week, with a one-way commute distance of less than 100 km.

[16] When changes of distance within municipalities are in the same direction as changes across municipalities, which is plausible, our estimates of changes of commuting distances over time will be an underestimate. Exclusion of those who work and live within the same municipality does not change our main result

were 10.1 km and 12.4 km in, respectively, 1996 and 2010.[17]

Respondents in 2010 answered a question about their usual workplace location.[18] We define a teleworker as an employee who answers that he or she performs some job tasks "at home" or "at home and at a location other than home". In turn, a non-teleworker is a respondent who answers that he or she works "at locations other than home". Our data contains 1,065 teleworkers (around 3% of all employees in the data), of whom 85 work exclusively from home. Other employees will be labeled as non-teleworkers. We do not have information on the number of days that they telework. So, we measure the extensive margin of teleworking.[19] No information on teleworking is available for the year 1996. Given the low penetration level of the Internet in the Netherlands in this year, it seems reasonable to assume that information technology was not available to workers in any profession. This is not essential: if some employees have adopted technology in 1996, then our results show the effect of a quite substantial change in technology between 1996 and 2010.

3.2. Treated and non-treated professions

To define treated and non-treated professions, we start with an identification of treated professions in 2010, i.e., professions in which teleworking is (relatively) widespread, and non-treated professions, where it is completely absent. We have detailed information about the employee's job and industry types. We define a profession as a particular job

qualitatively.

[17] the self-reported average commuting distance in the Netherlands is 17 km over the period 2000–2008 (Groot et al., 2012). The difference with our data stems largely from within-municipality commutes that are assumed to be zero in our approach.

[18] The original question in the Dutch language is "Waar werkt u in deze werkkring doorgaans?" which one might translate as "Where do you usually work on this job?". This question in the survey is clearly distinguished from another question about instances of overtime work at home.

[19] For an overview on the measurement of teleworking we refer to Sullivan (2003).

within an industry at an aggregated classification level of 9 job types and 45 industry types, so we distinguish between 405 professions.[20] The distribution of shares of teleworkers within professions is given in Figure A.1 in Appendix A.

We define a profession to be treated if more than 5% of employees in this profession telework in 2010.[21] A profession is non-treated if none of its employees telework in 2010. Professions where the share of teleworkers is positive but below 5% is left out of the analysis, as the effect of the technology on commuting distance, which we try to identify, is the most pronounced in the professions with a high adoption rate of teleworking. Inclusion of the latter professions would bias our diff-in-diff result to zero, as professions are likely to have a substantial measurement error, since our definition of treated and non-treated professions are based on finite (and sometimes small) samples. To avoid small sample bias, we exclude professions for which we have less than 10 observations (less than 1% of employees in treated and non-treated professions). We are then left with 105 professions of which 35 are defined as treated. Our analysis is then based on 5,699 (6,756) and 5,662 (5,505) employees in, respectively, non-treated and treated professions in 2010 (1996). In the treated professions the share of teleworkers is 8%.

Our identification strategy is based on changes over time in commuting distance. Figure 2 shows distributions of average commuting distances for treated and non-treated professions in 1996 and 2010 for workers that commute between different municipalities.[22] These distributions clearly reflect differences in levels of commuting distances across profession types and years. Moreover, Figure 2 suggests that, although starting points are different, changes in commuting distances over the 14 years across professions were roughly in

[20] In choosing the scale of aggregation one has to trade off homogeneity of the resulting groups with the availability of the observations, to avoid classifying each employee as a single representative of a profession.

[21] As a robustness check, we also provide results that are derived for a cut-off value of 10%.

[22] Commuting within a municipality occurs in 49% (54%) and 31% (33%) in 2010 (1996) of, respectively, non-treated and treated professions.

the same direction and magnitude. For example, the share of employees who commute long distances (i.e., 20 km and more) is higher in treated than in non-treated professions in both years, but increased between 1996 and 2010 for both types of professions in a similar way.

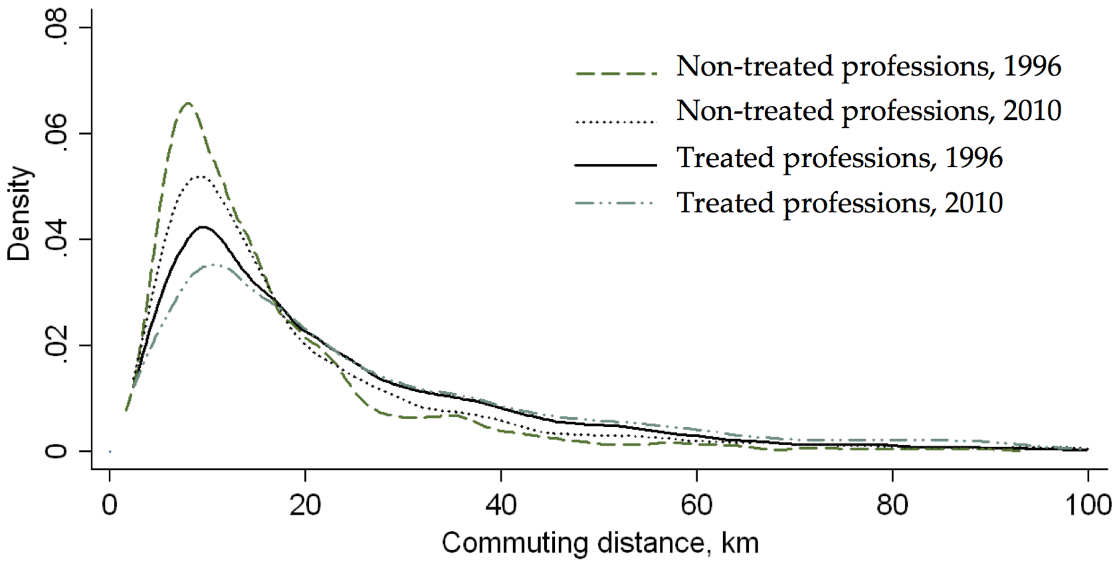

Figure 2 – Commuting distances in 1996 and 2010 (note: distances are shown only for employees who commute between different municipalities)

Employees in treated and non-treated professions account for 15.4% and 15.5% of all employees in 2010, not very different from 14.5% and 17.8% in 1996. The average commuting distance for treated professions has grown by 2.8 km from 15.1 (in 1996) to 17.9 km (in 2010). For non-treated professions, the average commuting distances has grown by 2.6 km from 7.4 (in 1996) to 10 km (in 2010).

The long-run causal effect, as defined by (2), can be estimated using the changes over time in average commuting distance for both profession types. When we ignore changes over time in socio-demographic and job-related characteristics (which we will account for later by using a matching procedure), this effect is equal to $2.8 - 2.6$, so 0.2 (with a standard error of 0.4). This suggests that there is no effect of technology adoption on average commuting distances between these two types of professions. The next subsection proceeds with propensity score matching to account for changes in worker and job characteristics over time.

3.3. Results

For the ease of exposition we first show results of the matching procedure in which workers are matched across all industries. Later we report results of our preferred specification in which we match within a given industry. The latter procedure allows for a less restrictive assumption on the differential time trend between professions. Both matching procedures lead to qualitatively similar outcomes.

Table 1 shows logit estimates for a model that estimates the probability that an employee is observed to work in 2010. We estimate the model separately for employees in treated and non-treated professions. In both profession types, employees in 2010 are, on average, older, more likely to be females, belonging to a larger household, working less hours but working more overtime than in 1996.

The implied probabilities of Table 1 are taken as propensity scores to match employees from 1996 with those from 2010.[23] We apply non-parametric kernel matching with replacement such that the means of the standardized biases across covariates for the two matched groups are minimized (see Caliendo and Kopeinig, 2008).[24] We have investigated the sensitivity of the results in various ways with respect to specification and matching

[23]For the propensity score matching estimates (and subsequent testing of the results), we use the "psmatch2" and "pstest" commands in Stata, developed by Leuven and Sianesi (2015).

[24]The standardized bias is defined as the percentage difference of sample means in the treated and matched control subsamples as a percentage of the square root of the average of sample variances in both groups (Caliendo and Kopeinig, 2008; Leuven and Sianesi, 2015).

Table 1 – Logit estimates whether an employee works in 2010

	Non-treated professions		Treated professions	
	coefficient	s.e.	coefficient	s.e.
Age	0.0909 * **	(0.0031)	0.0662 * **	(0.0032)
Male	−0.1563 * *	(0.0607)	−0.4706 * **	(0.0613)
Foreign-born	0.3194 * **	(0.0777)	0.2682 * **	(0.0917)
Household size	0.3043 * *	(0.1400)	0.5352 * *	(0.2611)
Hours of work	−0.0204 * **	(0.0030)	−0.016 * **	(0.0042)
Hours of overwork	0.0528 * **	(0.0063)	0.2362 * **	(0.0092)
Fixed contract	−0.4980 * **	(0.0698)	−0.0090	(0.088)
Fixed hours	−1.0581 * **	(0.1263)	−0.9731 * *	(0.3761)
No other co-workers	−3.2128 * **	(0.2185)	−3.6987 * **	(0.3056)
Managerial position	0.4687 * **	(0.0645)	−0.3274 * **	(0.0528)
Number of observations	12,455		11,167	
McFadden's R^2	0.3386		0.2944	

Notes: $**p < 0.05$, $***p < 0.01$. These estimates also control for industry type (44), job type (8), education level (5), household type (5), marriage status (4), number of employees in establishment (6), number of children (5), and the country's region (4).

procedure.[25]

There are very few, less than 1.5%, off-support observations from the total number of observations in year 2010 (80 and 84 in, respectively, treated and non-treated professions). Off-support observations are those that have propensity scores that are not found among control group observations. Off-support observations in 2010 are not matched with control observations in 1996 and are discarded from the further analysis.

Table 2 shows balancing tests by comparing the mean values of covariates across unmatched and matched groups. For example, for the variable "age", the first row shows the mean values of age in the unmatched sample both in 2010 and 1996 and the difference

[25] For example, we have included interaction and higher order terms, excluded some variables, applied a more restrictive definition of treated professions. We have also applied other matching procedures such as one-to-one, three-to-one and five-to-one neighbors, and caliper, have varied the kernel bandwidth and performed estimations without replacements.

between the two, indicated by a standardized bias which is often used in the literature. The second row shows the same information for the matched samples and the reduction in the bias, which is due to the matching procedure. We examine whether different groups have the same covariate means (see Rosenbaum and Rubin, 1985).

Table 2 – Balancing tests

		Non-treated professions				Treated professions			
		Mean Value			Bias	Mean Value			Bias
Variance	[a]	2010	1996	Bias[b]	reduction	2010	1996	Bias[b]	reduction
Age	U	38.6	34.4	34.8		40.2	37.1	30.7	
	M	38.5	37.7	6.7	80.7	40.1	38.9	12.0	60.9
Male	U	0.554	0.588	6.8		0.66	0.71	−11.2	
	M	0.554	0.553	0.2	96.5	0.66	0.64	4.4	61.3
Foreign-born	U	0.13	0.1	9.4		0.08	0.06	7.7	
	M	0.13	0.17	13.0	−38.2	0.08	0.09	−2.9	61.9
Household	U	3.08	2.98	7.2		2.99	2.87	10.1	
size	M	3.08	3.09	0.8	88.3	2.99	3.04	−4.0	60.0
Hours of	U	31.1	33.7	27.7		36.56	37.6	−15.7	
work	M	31.2	31.8	6.5	76.4	35.84	36.5	10.1	35.7
Hours of	U	1.68	1.00	16.6		3.46	0.56	59.6	
overwork	M	1.66	1.90	−6.0	64.0	3.09	4.47	−28.0	53.1
Fixed contract	U	0.81	0.87	−16.6		0.91	0.91	0.4	
	M	0.816	0.78	10.7	35.6	0.911	0.89	4.8	−1179.6
Fixed hours	U	0.91	0.98	−31.3		0.99	1.00	−10.9	
	M	0.92	0.93	−6.1	80.4	0.99	0.99	−0.9	91.5
No other co-	U	0.01	0.02	−4.1		0.01	0.01	1.8	
workers	M	0.01	0.01	0.7	81.9	0.01	0.01	−0.2	90.8
Managerial	U	0.22	0.14	19.9		0.42	0.42	−0.5	
position	M	0.22	0.20	4.2	78.9	0.41	0.35	12.6	−2314.1
Mean standar-	U			8.8				10.5	
dized bias	M			2.8				5.1	

[a]: U–unmatched, M–matched

[b]: Mean standardized bias without and with matching, (see Rosenbaum and Rubin, 1985).

For non-treated professions, the mean standardized bias across the covariates is reduced from 8.8 to 2.8 due to matching, whereas for treated professions, it is reduced from 10.5 to 5.1. So, for both professions the reductions are substantial indicating a much improved similarity of the samples before and after treatment (Caliendo and Kopeinig, 2008).

Table 3 presents our key result for the matched employees. The average commuting distance in non-treated professions has grown by 2.30 km (with a standard error of 0.51) from 7.70 in 1996 to 10.00 km in 2010. The average commuting distance in treated professions has grown by 1.98 km, with a standard error of 0.76, from 15.82 km in 1996 to 17.80 km in 2010. The difference-in-differences estimate is then equal to −0.32 with a standard error of 0.92 (the diff-in-diff estimate given matching is close to this estimate for the unmatched samples which is 0.20). This implies that we cannot reject the hypothesis that changes in commuting distance over time for both treated and non-treated professions were statistically identical.

Table 3 – Commuting distance

	Employees in					
	1996	2010	Diff	s.e.	Diff-in-diff	s.e.
Treated professions	15.82	17.80	1.98	(0.76)	**−0.32**	**(0.92)**
Non-treated professions	7.70	10.00	2.30	(0.51)		

The main critical assumption of our estimation strategy is that we assume that average commuting distances for workers in non-treated professions and for workers in treated professions are identical if information technology would not have been available to them. Here, we relax this assumption by assuming that this assumption must hold for a given industry, so we allow for different time trends per industry. That is, we repeat the entire matching procedure described above for 7 separate broad industries for which we have enough observations (we exclude “agriculture” and “cultural and recreational services”).

Table 4 shows the diff-in-diff estimates per industry and the average effect which is small and equal to -0.79, but also far from statistically significant. As in Table 3, the results indicate that information technology does not affect workers' commuting distances.

Table 4 – Commuting distance per industry

Industry	Diff-in-diff	s.e.	Industry share
Raw manufacturing	−3.86	(4.86)	0.02
Electronic and auto manufacturing	4.98	(2.99)	0.03
Construction	−1.07	(3.81)	0.05
Wholesale and retail	−3.17	(1.71)	0.22
Transport and communications	−0.41	(2.26)	0.21
Services	0.83	(2.68)	0.35
Education, health	−2.83	(3.00)	0.12
Average effect	**−0.79**	**(1.20)**	

The above results suggest that the observation that teleworkers tend to have long commuting distances is mainly due to workers with long commuting distances choosing to telework rather than the other way around. To investigate this further let us now focus on employees in treated professions only, and distinguish between teleworkers and non-teleworkers. We then repeat the entire analysis for teleworkers and non-teleworkers within these professions separately. Such an analysis provides the change in commuting distances over time. We find that teleworkers increased their commuting distance by 12.07 km between 1996 and 2010, while non-teleworkers in treated professions have increased their commuting distance by only 1.32 km during this period, which is less than the increase of 2.3 of non-teleworkers in non-treated professions (see Table 3).

In light of the result from Table 3, which shows that the diff-in-diff estimator is not statistically significant, the large difference between commuting distances for teleworkers and non-teleworkers suggests that the spatial commuting pattern of employees in treated and non-treated professions is, at least to a large part, governed by sorting, in which employees who already live further away from work choose to adopt teleworking. These results indicate that the advent of information technology in the last two decades did not have a profound impact on the spatial structure of the labor market.

The absence of a long-run causal effect of information technology on average commuting distance could be partly explained by three other mechanisms as well: *(i)* firms with many treated professions relocate as well, *(ii)* the generic impact of information technology on the commuting distance of both treated and non-treated professions is negative, or *(iii)* our crucial assumption of a common time trend between treated and non-treated professions is violated.

First, to check whether this diverging effect of adoption of information technology on average commuting distance might be driven by the relocation of firms away from central urban locations towards cheaper locations to save on land rents, we studied changes over time in urbanization patterns for the two profession types. We have repeated the entire exercise and used as a dependent variable not the commuting distance, but the measure of urbanization (ratio of residents over employees in municipality of employment). The results (not reported here) show no difference in changes over time across profession types. This does not go in line with the hypothesis of teleworking firms relocation due to technology adoption.

Secondly, as argued in the introduction the technology that enables teleworking might as well increase agglomeration economies. This argument coincides with the large literature that followed after the controversial claim that the world is increasingly becoming flatter (see for the orginal claim Friedman, 2006; and for a rebuttal, e.g., Florida, 2005; McCann, 2008). Thus, the same technology that enables workers to work from home might increase the need for closeness and face-to-face contacts as well. However, note that this would only imply for treated professions and that the effect of the increasing agglomeration economies and of the possibility to telework would then cancel each other out.

Finally, we can not entirely rule out the possibility that, within industries, workers in treated professions faced a different time trend than workers in non-treated professions for reasons unrelated to the advent of information technology. If we assume that the possibility to telework would have a positive impact on commuting distance, then the time trend for treated professions would have been *less* steep than for non-treated professions. Perhaps, workers in treated professions have experienced a more positive change in preferences to reside in cities compared to workers in non-treated professions. However, this effect is likely to be small given that we look at the differences that occurred between 1996 and 2010 and that workers in treated professions and non-treated professions are not that different from each other; again we could as well be comparing the possibly teleworking graphical designer with the non-teleworking hospital doctor.

The next section will check most other assumptions we have made. All yield qualitatively the same results as in Table 3.

4. Sensitivity analyses

Table 5 shows that our main result is robust with respect to various matching procedures and it also holds if we restrict the samples of observations. We perform neighbor one-to-one, with and without replacement matching, which allow to check whether our result is driven by the subset of 1996 observations that are disproportionally often matched to 2010 observations. The main result remains the same. We also restrict the sample of employees in 2010 to those older than 40 years because older employees are less likely to have chosen their profession given the possibility of teleworking, which would upward bias our estimate due to sorting. If we assume that the within-municipality commute is 3 km, instead of being 0 km, we obtain similar results. To check whether our results are driven

Table 5 – Robustness checks of matching results for commuting distance

	Employees in					
	1996	2010	Diff	s.e.	Diff-in-diff	s.e.
N(1) matching with replacement						
Treated professions	16.40	17.80	1.40	(1.12)		
Non-treated professions	7.69	10.00	2.32	(0.73)	−0.92	(1.34)
N(1) matching without replacement						
Treated professions	15.06	17.80	2.65	(0.37)		
Non-treated professions	7.62	10.00	2.39	(0.27)	0.26	(0.46)
Only employees above 40 years in 2010						
Treated professions	16.13	18.21	2.08	(1.25)		
Non-treated professions	9.11	10.00	0.73	(0.81)	1.35	(1.49)
Intra-municipality commute of 3 km						
Treated professions	16.72	18.71	1.99	(0.73)		
Non-treated professions	9.34	10.00	2.14	(0.48)	−0.15	(0.87)
No large treated professions						
Treated professions	17.72	18.86	1.15	(0.93)		
Non-treated professions	7.70	10.00	2.30	(0.51)	−1.15	(1.06)
10% cut-off threshold for treated professions						
Treated professions	22.37	23.28	0.91	(2.74)		
Non-treated professions	7.70	10.00	2.30	(0.51)	1.39	(2.78)
Limited treated professions (0%—5%)						
Treated professions	9.38	11.69	2.31	(0.14)		
Non-treated professions	7.70	10.00	2.30	(0.51)	0.01	(0.58)

by a few treated professions which have a lot of employees, we include only professions that have less than 600 employees in our dataset. We also matched workers from 2010 with the workers in 1996 who are 14 years younger. For this matching procedure we have excluded life-cycle dependent variables, such as children, marital status, and job related characteristics. The outcomes (s.e.) are 2.50 km (2.80) and 1.72 km (5.89) for non-treated and treated professions respectively. The corresponding diff-in-diff estimate is statistically insignificant.

Finally, we repeat the entire analysis for the limited treated professions group to confirm that the average effect of adoption of the technology, for the workers in this profession group, is not larger than for the treated professions. In all these cases the diff-in-diff estimator remains statistically insignificant. Importantly, in a few specifications the alternative estimator has a lower standard error, but even then the coefficient is not significant.

5. Conclusions

This paper estimates a long-run causal effect of the adoption of information technology on commuting distances *for professions*. To estimate this effect, we apply a difference-in-differences method which exploits variation in information technology adoption between 1996 and 2010 and across professions. We distinguish between treated professions, where at least 5% of workers are involved in teleworking, and non-treated professions where no one of the workers teleworks. The latter group provides control estimates of how commuting distances change over time when there is no adoption of information technology. The former group should reveal the causal effect of information technology. To account for time changes in observable covariates, we apply a propensity score matching procedure to match workers observed in 1996 with workers observed in 2010. Our key identification assumption is that the difference over time between changes in the average commuting distances of these professions within a given industry is solely due to the information technologies.

Our results show that a long-run causal effect of information technology on commuting distance is too small to be identified and likely to be absent. Furthermore, we even find that non-teleworkers in treated professions have a lower growth in commuting distances between 1996 and 2010 compared to workers in non-treated professions. The latter fact appears not easy to explain, as the urbanization patterns of the profession types have changed over time in the same manner. This implies that the difference in relocation of teleworking firms as compared to relocation of non-teleworking ones is not the driving force behind the result of irresponsiveness of commuting distances. A strong sorting effect seems to matter much more. Workers in treated professions facing long commutes are more likely to adopt teleworking than workers with short commutes. The total impact of information technology on the spatial structure of the labor market, however, remains marginal at best.

Our results do not imply that teleworking might be a suitable tool to tackle the negative externalities of transportation. Information technology itself indeed does not seem to have affected aggregate commuting distances for professions, but the evidence we find suggests that teleworkers themselves indeed have or even adopt longer commuting distances. At the moment, teleworkers still comprise a minor share of the total labor force. Increasing this share, by, e.g., strongly subsidizing teleworking, could very well increase the aggregate commuting distance in the long-run.

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A. Appendix

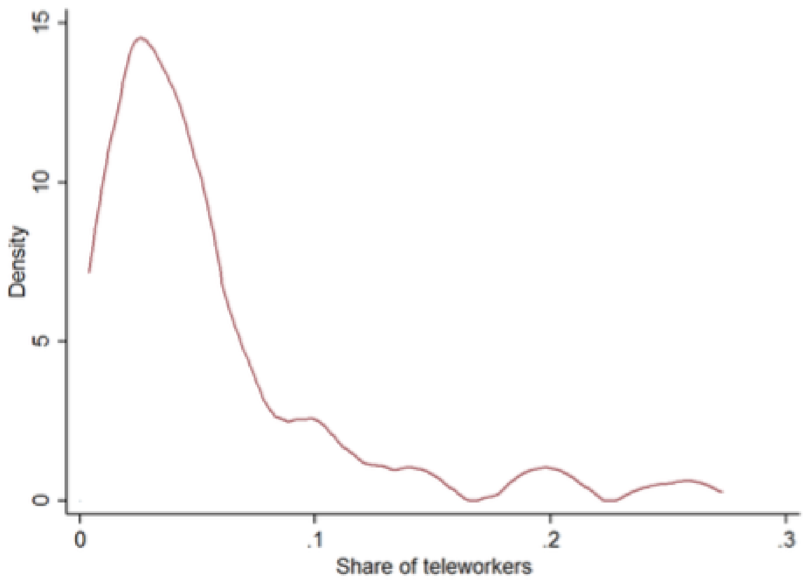

Figure A.1 – Share of teleworkers in 2010. (Note: The share of professions with no teleworkers is 0.53 and not shown here)

Table A.1 – Descriptive statistics

	1996	2010	
Variables	non-teleworkers	non-teleworkers	teleworkers
One-way commuting distance (km)	12.0	13.8	22.1
Age	36.9	40.6	43.2
Males (percent)	59.5	51.8	62.9
Born outside the Netherlands (percent)	7.3	9.6	6.1
Household size	3.0	3.0	3.1
Married (percent)	61.4	58.9	65.5
Children (age range)			
0–5	27.5	24.4	27.2
6–11	23.6	25.1	29.7
7–12	25.3	32.5	37.6
18+	32.0	27.3	36.8
Education level (percent)			
School education	22.4	14.8	5.5
Middle-level applied education	40.3	41.3	29.6
Higher professional education	5.0	3.3	2.4
Higher education	26.5	39.4	62.5
NA	5.8	1.2	0.0
Fixed contract (percent)	89.6	87.2	93.2
Fixed hours (percent)	99.2	96.0	97.4
Total working hours per week	34.9	32.6	35.4
Hours of overwork per week	0.8	2.2	5.5
Managerial position (percent)	25.4	27.0	42.6
Number of coworkers (percent)			
2–9	9.1	5.6	5.0
10–19	6.9	6.4	5.9
20–49	10.4	11.1	13.7
50–99	9.4	9.7	11.3
100+	61.7	37.0	43.7
NA	2.5	30.2	20.4
Number of observations	38,179	36,056	1,066