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Commuting time and absenteeism: Evidence from a natural experiment*

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Abstract

This paper investigates the effect of commuting time on absenteeism using a natural experiment. This relationship is notoriously difficult to assess without exogenous shocks to commuting and with the survey data typically exploited. The study uses detailed administrative data for Luxembourg to measure the impact on work absences of a temporary shock to commuting time caused by large-scale roadworks at the border between Belgium and Luxembourg. The roadworks affected the commuting time of cross-border workers from Belgium, leaving cross-border commuters from France as a natural control group in a difference-in-difference setup. The findings reveal a positive – but quantitatively relatively small – effect of commuting time on absenteeism, driven mainly by increased absences due to reported illness or family reasons. Male workers appear to respond more than female workers to the shock in commuting time.

Keywords: Absenteeism, Health, Commuting, Cross-border workers, Luxembourg
JEL codes: J62, J68, I12, R42, M51

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1 Introduction

The time dedicated to traveling to work and the frequency of commuting has increased in recent years in many Western countries (Gimenez-Nadal & Molina, 2016; Kirby & LeSage, 2009; McKenzie & Rapino, 2011). More than 20% of European workers spend more than an hour and a half daily on these trips (Giménez-Nadal et al., 2020).

Commuting can have both positive and negative effects on workers. On the positive side, commuting longer distances can expand the pool of potential job opportunities and increase the chances of finding a better match between workers and job offers (Goerke & Lorenz, 2017). Extending the search radius by commuting longer enables workers to access more housing options when choosing their place of residence (Goerke & Lorenz, 2017). Such matching could potentially positively affect the well-being of individuals and the productivity of firms (Bhat, 2014). However, commuting may also negatively affect individuals. Commuting is one of the least pleasurable activities (Choi et al., 2013; Kahneman et al., 2006; Kahneman et al., 2004). Long commutes reduce available time to engage in physical activities and are a source of daily stress (Gottholmseder et al., 2009; Lucas & Heady, 2002; Novaco et al., 1990; Stutzer & Frey, 2008). The environmental impact of long commutes and exposure to air pollution are also major concerns. Commuting may thus, directly and indirectly, affect individuals' physical health (Evans & Wener, 2006; Evans et al., 2002; Hämmig et al., 2009; Hansson et al., 2011; Künn-Nelen, 2016; Novaco et al., 1990; Roberts et al., 2011) and mental health (Choi et al., 2013; Dickerson et al., 2014; Friman et al., 2017; Gatersleben & Uzzell, 2007; Novaco & Gonzalez, 2009). Such adverse effects on mental and physical health may lead to absenteeism and reduce workers' productivity (Grinza & Rycx, 2020; Oswald et al., 2015).

The relationship between commuting time and employee absenteeism remains relatively understudied in the literature, with few studies moving beyond descriptive associations at the micro-level (Ma & Ye, 2019). Some (mostly descriptive) studies confirm a positive correlation between commuting and absenteeism (Kluger, 1998; Magee et al., 2011), while others find no robust correlation (Künn-Nelen, 2016). Gimenez-Nadal et al. (2022) indicate that a 1% increase in daily commute results in a 0.018% increase in male workers' absenteeism and a 0.027% increase in female workers' absenteeism per year in the US. Most causal studies rely on employer-induced changes in commuting distance due to company relocations and find mixed results. Van Ommeren and Gutiérrez-i-Puigarnau (2011) find that commuting distance increases absences for medical reasons in Germany. Goerke and Lorenz (2017) point towards similar results in Germany again, concluding that only employees with long commutes are 20% more absent than those with no commutes. Hassink and Fernandez (2018) find, in contrast, no effect on monthly absences in the US, except for workers reporting low morale. Ma and Ye (2019) exploit an instrumental variable technique (with commuting instrumented by population density at home or job locations) and find that commuting distance is positively linked to absenteeism in Australia. Finally, Lu et al. (2021) use a natural experiment based on the opening of a subway line affecting commuting in a Chinese city and find no significant change in absenteeism following the opening of the line.

Despite the seemingly evident disutility of the time spent commuting and its potential impact on productivity, empirical evidence of an impact of commuting on absenteeism is limited and mixed. These mixed results can have different explanations. Descriptive studies that do not attempt to control for the simultaneity of location and employment decisions are bound to underestimate the effect of commuting time – workers choosing a longer commute endogenize the disutility of the commute in their decisions. Studies with a design allowing for a plausibly causal interpretation may suffer from low power or measurement error when relying on self-reported survey data (e.g., Gimenez-Nadal et al., 2022; Ma and Ye, 2019). Finally, natural experiments such as the one exploited in Lu et al. (2021) are not ideal as major infrastructures – here, the construction of a subway line – is a permanent and foreseeable shock to commuting time: workers likely anticipate (and therefore endogenize) their future commuting time in their employment and residential location decisions.

Our study exploits another form of natural experiment and better data that provide an improved design for identifying a causal effect of commuting time on absenteeism. We exploit a shock to commuting time induced by major roadworks undertaken on the highway connecting Belgium to Luxembourg in 2018 and 2019. This particular event has at least two attractive characteristics. First, it was relatively large: it significantly affected the commuting time of a large number of workers commuting across the border between the two countries for about seven months. Second, its impact was limited in time. A disruption in commuting time over seven months is unlikely to affect residential location decisions, especially since the roadworks did not lead to any persistent change in commuting time relative to prior levels after completion.

The roadworks affected the commuting time of workers residing in Belgium and working in Luxembourg. This cross-border setting comes with useful features too. Cross-country commuting limits substitution strategies. First, international tax and social security regulations severely constrain remote work possibilities when the worker does not reside in the country of work. Second, the cross-national network of roads and public transport is much more limited than any of the national networks. This limits the possibilities of finding alternative routes to work. Third, the lack of infrastructure cooperation on either side of the border means that the roadworks undertaken on the Belgian side did not lead to any long-run reduction of commuting time (as we explain below).

The setup makes cross-border workers traveling from France toward Luxembourg a natural control group. The transport network structure is analogous – with one main highway connecting France to Luxembourg heading toward Luxembourg City and with limited (often saturated) public transportation alternatives – and the distance from the border to Luxembourg City (where most of the jobs are based) is similar.

Finally, we have access to fine-grained, accurately recorded administrative data on absences from work and data on residential location, individual, and employment characteristics. The analysis uses the recorded absences of all Luxembourg-based private sector workers living in Belgium or France between 2015 and 2019. Employers report absences. Because sickness payments are compensated at 80% from the first day of absence and are fully taken over by the *Caisse Nationale de Santé* from the seventy-seventh day of absence over an 18-month period, employers have an obligation and an incentive to report absences to the social security administration accurately. The absences reported to social security are encoded by type (such as illness, injury, or maternity leave), allowing for fine-grained analysis. Disentangling by cause of absence allows us to get a sense of whether increased absenteeism primarily reflects increased shirking behavior (to avoid the disutility of extended commuting time) or actual adverse health effects of the increased commuting.

In sum, this setup makes for a robust design for assessing any plausibly causal effect of commuting time on absenteeism. Our results show that disruptions to commuting time lead to a significant but quantitatively small increase in absenteeism. However, there seems to be a threshold effect with workers who commute more than 40 kilometers to work responding more strongly to the commuting time shock. Results also highlight significant differences in absenteeism related to gender. Unlike what could be conjectured from usual gender imbalances in family responsibilities, we observe that men are more affected by shocks in commuting time than women. While illness- and family-related absences respond to the commuting shock, we see no change in injury-related absences. This could suggest a predominance of a 'shirking' explanation – rather than a direct health impact – for the increase in absences.

The rest of the paper is structured as follows: Section 2 briefly discusses the mechanisms that may link commuting and absenteeism. Section 3 presents our methodology, including a description of the natural experiment, data sources, and empirical model. Section 4 presents results. Section 5 concludes.

2 Residential choice, commuting and absenteeism

A simple way to formalize mechanisms linking commuting and absenteeism is through a classic Alonso-Muth-Mills model (Alonso, 2013; Mills, 1967; Muth, 1969; Wheaton, 1974). A worker has preferences over consumption and commuting time represented by an individual utility function $U(C, T)$, where U increases with consumption C and decreases with commuting time T. Employment is located in a single central business district (CBD). Each worker resides around the CBD and travels to the CBD to get to work through a dense radial road network. An agent's residential location away from the CBD determines her commuting time.

In this model, agents choose their residential location l to optimize $U(C, T)$ subject to the constraint $C = W - H$, where W is earnings and H is housing costs. Since U decreases with commuting time T, agents choose, all other things being equal, to live as close as possible to the city center. However, the central business district has a limited housing capacity. The density of the housing market is higher closer to the CBD, which is associated with higher prices per square meter (Brueckner, 1987) to clear the market. So housing costs decrease monotonically with the distance to the CBD. Agents therefore make a trade-off between living in a desirable location close to the CBD, which comes with higher housing costs H (and hence lower consumption) but a shorter commute, or living in a less desirable location farther away, leading to longer commutes, which has lower housing density and costs H, but a longer commute T.[1] In equilibrium, the optimal location balances the disutility of commuting with the consumption obtained by lower housing costs (Alonso, 2013; Gutiérrez-i-Puigarnau & van Ommeren, 2010; Mills, 1967; Muth, 1969; Wheaton, 1974; Zenou, 2009).

Commuting time from any location is known and constant in the basic Alonso-Muth-Mills model. In real life, commuting time is, however, stochastic – with variations due to, for instance, incidents, strikes, and weather conditions. In the face of shocks to commuting time, agents are typically unable to re-optimize residential location choices, so the utility is directly affected by such shocks. While it is easy to think of a model in which workers would factor in uncertainty in commuting time when choosing an optimal residential location, stickiness in residential location choices still implies that short-term shocks to commuting affect utility through T. In practice, variations in T that cannot be compensated by adjustments to residential location may lead workers to absenteeism. This may arise through work avoidance behavior ('shirking') with workers calling in sick if significant traffic congestion is expected due to exceptional weather events, roadworks, or strikes (Ross & Zenou, 2008). This may also arise from genuine health shocks caused by a longer commute (Evans & Wener, 2006; Hansson et al., 2011; Künn-Nelen, 2016; Roberts et al., 2011).

Previous studies, such as urban efficiency wage models, have primarily examined the relationship between commuting and absenteeism and productivity through shirking (e.g., Brueckner and Zenou, 2003; Ross and Zenou, 2008; Zenou, 2002, 2009; Zenou and Smith, 1995). These models generally posit that long commutes can have a negative impact on productivity and lead

[1] This simple model postulates that wages in the CBD are independent of workers location of residence (unlike in, e.g., Ross and Zenou (2008)).

to higher absenteeism as they can take a toll on work effort (Gutiérrez-i-Puigarnau & van Ommeren, 2010). Individuals may choose to shirk at work, depending on the costs associated with doing so (Goerke & Lorenz, 2017; Gutiérrez-i-Puigarnau & van Ommeren, 2010).[2] Some authors suggest that costs of shirking are independent of commuting time (Gutiérrez-i-Puigarnau & van Ommeren, 2010), as workers are not punished differently for shirking when they call in sick for a 20-minute commute versus a 30-minute commute. However, other authors suggest that workers may choose to commute longer in the first place for reasons such as higher wages, better housing, better working conditions (Goerke & Lorenz, 2017; Stutzer & Frey, 2008), or a stronger underlying desire to be involved in their work. As a result, long commuters may disproportionately suffer when punished for shirking, especially when facing a disruption in their commuting time.

3 Data and Empirical Strategy

Our analysis builds on the idea that, under the following conditions, roadworks can be seen as exogenous events that disrupt commuting time but do not lead to changes in equilibrium locations. First, roadworks must be of sufficient magnitude to impact commuting time. Second, roadworks must be of sufficiently short duration to avoid changes in individuals' structural decisions, such as relocating or changing jobs, between the start and end of the roadworks. Third, roadworks should not result in long-term changes in traffic flows and fluidity. Under these conditions, roadworks can be considered exogenous to employees' decisions and, therefore, a natural experiment through the stochastic variations in commuting time they generate.

3.1 Roadworks at the Luxembourg-Belgian border as a Natural Experiment

The theoretical framework outlined in Section 2 aligns well with the reality in Luxembourg. Luxembourg is a small and highly urbanized country with a large number of cross-border workers commuting from Belgium, Germany, and France.[3] Most economic activity is centered in Lux-

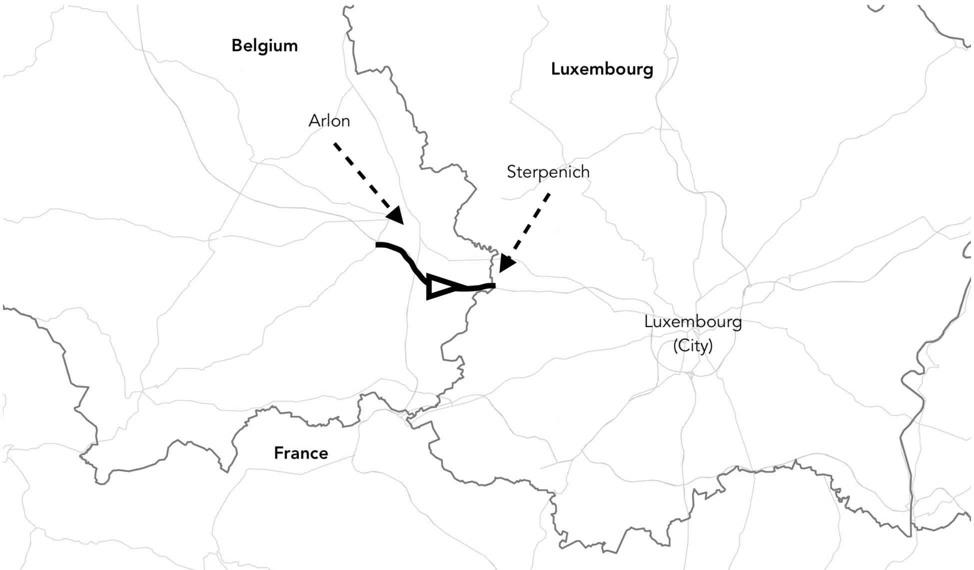
[2] It is worth noting that the efficiency wage theory posits that firms pay higher wages to promote effort and discourage shirking, seen as moral hazard (Shapiro & Stiglitz, 1984). However, monitoring abusive behaviors can be costly and challenging to implement in practice, so employers generally do not pay enough to eliminate workers' shirking entirely (Shapiro & Stiglitz, 1984).

[3] STATEC (2019) reports 192,000 workers living outside Luxembourg's borders in 2018.

embourg City, located at almost equal distances to France, Germany, and Belgium. One-third of Luxembourg's employers are based in Luxembourg City (STATEC, 2021). Luxembourg is characterized by high housing demand and a limited supply, leading to high prices that increase with proximity to Luxembourg City. This has led workers to spread out over large geographical areas, resulting in significant daily flows of commuters beyond the country's borders.

In this context, the natural experiment that we exploit is a road construction project consisting of widening over approximately 10 kilometers of the E411 highway between Arlon and Sterpenich (just before the border between Belgium and Luxembourg) in order to create a carpool lane, as illustrated in Figure 1. This project was undertaken as a pilot scheme by the Walloon Region in Belgium to address mobility-related issues. The roadworks took place over seven months, beginning on September 17, 2018, and ending on April 30, 2019. The additional lane was officially opened on May 7, 2019 (Wiessler, 2019).

Figure 1. Roadworks between Arlon (Belgium) and the Luxembourg border.



The E411 highway sees more than 40,000 vehicles cross the border daily, with more than 80% of motorists in Belgium commuting alone in their cars. However, the project has been criticized for its relatively restrictive rules for using the carpool lane, which only permit light vehicles with a minimum of three people and limit the speed to 50 kilometers per hour.[4] Furthermore, the project was carried out without proper consultation with Luxembourg authorities and ended at the border, reducing lanes at the border point and creating a bottleneck. [5]

The roadworks resulted in a considerable increase in commuting time. For instance, over 8km of traffic jams were recorded at 6 PM on September 17, 2018 (RTL, 2018). Based on the assumption that the average speed in traffic jams is 20 kilometers per hour, compared to 120 kilometers per hour without congestion, the roadworks added approximately 20 minutes to commuting time. Furthermore, resorting to minor roads as an alternative proved ineffective, as traffic in the villages was heavily saturated, resulting in additional traffic jams and no time-saving advantages. Although public transportation was not directly affected by the roadworks, it is improbable that a significant portion of commuters switched to public transit to avoid roadworks, considering the existing congestion in public transportation systems.[6]

3.2 Data

We use administrative microdata from the Luxembourg Microdata Platform on Labour and Social Protection. The platform brings together data extracted from the Common Center for Social Security (CCSS), the Employment Development Agency (ADEM), and the National Health Fund (*Caisse Nationale de Santé*, CNS).

3.2.1 Coverage and worker characteristics

We obtained pseudonymized information on all private-sector, cross-border employees affiliated with the Luxembourg social security system and residing in Belgium and France. The extraction covers the period from 2015 to 2019 and includes information on sociodemographic characteristics, distance from the municipality of residence to the border, job and contract characteristics, and the composition of the firms that employ these workers (see details below).

[4] See https://www.wort.lu/fr/granderegion/la-bande-de-covoiturage-s-avere-etre-un-fiasco-5de4e1a2da2cc1784e351173 (accessed 2023-05-04).

[5] Belgian Minister Philippe Henry even declared in 2021 that the roadworks “look like useless roadworks” (https://www.lessentiel.lu/fr/story/les-motards-utiliseront-la-bande-de-covoiturage-941091627096, accessed 2023-05-04). See also https://paperjam.lu/article/covoiturage-sur-e411-on-retrog (accessed 2023-05-04).

[6] See https://paperjam.lu/article/arlon-met-pression-son-pr-a-vi (accessed 2023-05-04).

The population covered and the variables are updated month-by-month from January 2015 to December 2019 to form a panel data structure with a total of 152,249 employees and 5,183,488 person-month observations.

3.2.2 Measures of work absences

The extraction contains data provided by the CNS on the number of recorded absence days for each employee in every month of the period covered. Absences are categorized into six distinct types: illness-related, pregnancy-related, injury-related, family-related, maternity-related, and palliative-related. Illness-related absences occur when an employee is unable to work due to sickness, while pregnancy-related absences apply to work exemptions in the framework of a protection scheme exclusively for pregnant or nursing women. Injury-related absences result from work incapacitation due to injuries. Family-related absences refer to leaves granted to a parent when their child is ill and no alternative childcare option is available. Maternity-related absences encompass parental leaves for the birth or adoption of a child, and palliative-related absences involve time off for end-of-life care.

The legal system allows for some degree of flexibility in reporting absences. First, employees incapacitated from work due to illness- or injury-related reasons may be absent for up to two consecutive days without providing a medical certificate but by still notifying the employer from the first day of absence. Nevertheless, since sickness payments are compensated at 80% from the first day of absence (whether medically justified or not), and the *Caisse Nationale de Santé* entirely takes over payments from the seventy-seventh day of absence within an 18-month time frame, employers are both obligated and motivated to accurately report absences to the social security administration. Second, family-related absences, which involve leaves granted to a parent when their child is ill and no alternative childcare is available, may also be prone to misuse. Employers face difficulties verifying the child's illness or the unavailability of alternative childcare options. This leniency in absence reporting could be exploited by employees seeking to take time off without legitimate grounds, particularly in the event of commuting time shocks.

Absences due to illness are the most common, with an average of 0.88 days of absence per employee-month; see Table 1. All types combined, the average number of days of absence per month in our data is 1.13 (or 5.18 percent of working days). On average, 16.53 percent of workers claim at least one day of absence per month.

Table 1. Absence Statistics by Type

	Average days of absence per month	Share of absent days (in % of working days)	Percentage of employees with at least one day of absence per month
All reasons	1.125	5.185	16.526
By reason			
Illness	0.878	4.048	14.356
Pregnancy	0.180	0.827	0.914
Injury	0.048	0.223	0.442
Family	0.018	0.084	1.152
Maternity	0.000	0.001	0.001
Palliative	0.000	0.001	0.008

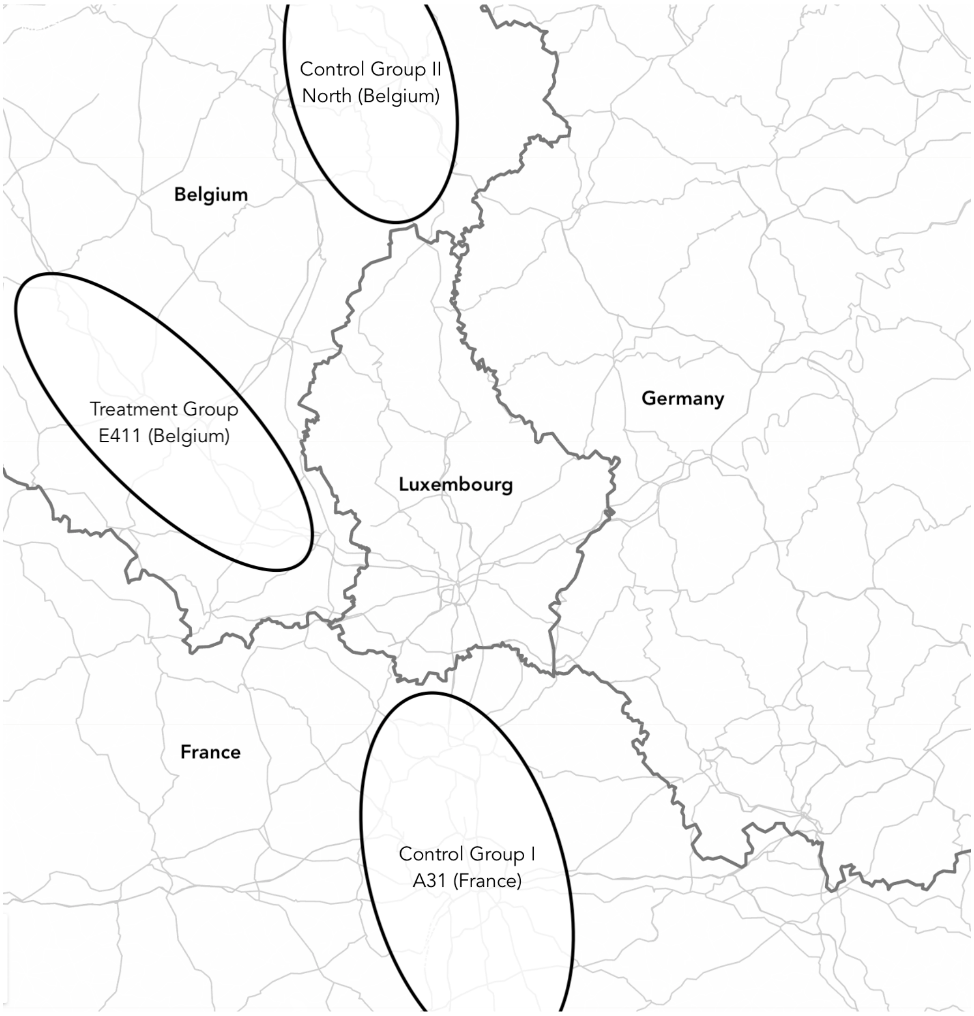
3.2.3 Definition of treatment and control groups

We define individuals residing in Belgian municipalities around the E411 highway and upstream from the location of the roadworks in September 2018 as our Treatment Group. We select municipalities ('communes') in close proximity to or intersected by the E411 highway, which was impacted by the roadworks, where few viable alternative routes exist for bypassing the highway and avoiding traffic jams.

We define two control groups that have not been affected by the roadworks and are otherwise similar to treated cases. The first control group is composed of employees residing in France along the A31 highway in September 2018 (Control Group I). The second control group is composed of employees residing in Belgium but in municipalities farther north and away from the E411 roadworks (Control Group II). Figure 2 illustrates this construction (details of the geographical areas assigned to the three groups are given in Appendix B).

The Treatment Group and Control Group I present similar commuting conditions as they both live along major highways (E411 and A31, respectively) and face an entry in point in Luxembourg at a comparable distance from Luxembourg City. Cross-border workers from France

Figure 2. Treatment and control groups



share similar characteristics such as language, professional activities, and culture (Pigeron-Piroth & Wille, 2019). Belgium and France also provide the majority of Luxembourg's cross-border labor force. The similarity of the group characteristics is confirmed in our data. Table 2 shows that the control and treatment groups are relatively homogeneous in terms of demographic composition.

Use of Control Group I as our preferred benchmark specification is guided by the similarity in the distance of this group to Luxembourg City and the absence of possible contamination. Control Group II comprises residents of municipalities located further north and, therefore, more distant from Luxembourg City. Also, unlike Control Group I, we cannot completely rule out that some of these residents would use the E411 highway as an entry point to Luxembourg (under normal traffic conditions) and would therefore be affected by the roadworks.

Figure 3 shows the share of workers absent at least one day in each month between January 2015 and December 2019, for both the Treatment Group and Control Group I. Three observations stand out. First is the strong cyclicality of absences (with peaks in February and March and lows in July). Second is the generally lower absenteeism in the treatment Group (Belgian cross-border workers) than in the control group (French cross-border workers). Third is that this pattern is reversed in the period of the roadworks between September 2018 and April 2019 – months during which absences are higher in the treatment group.

3.3 Empirical Model

To examine the effect of the roadworks rigorously, we implement a standard difference-in-differences model with monthly panel data. We primarily focus on absenteeism measured as a binary variable, where 1 indicates that an individual has been absent for at least one day in a given month and 0 otherwise – a measure of the extensive margin of monthly absenteeism. It is noted that individuals who are already frequently absent may be less likely to be affected by the roadworks, as they are likely to commute less.

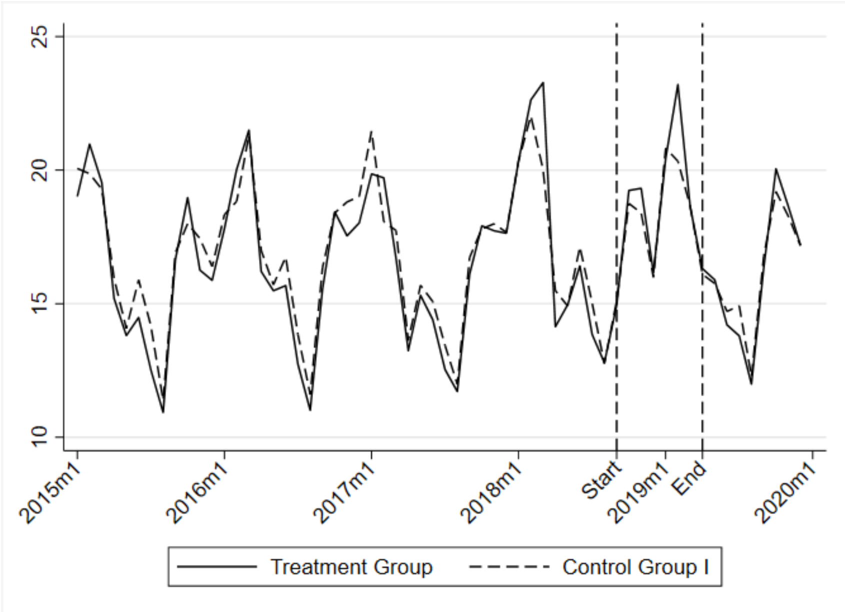
For tractability given the size of our dataset, we use a linear regression model as the main specification. The model incorporates fixed effects to control for unobserved heterogeneity at the

Table 2. Descriptive Statistics

	ALL	**Treatment Group**	**Control Group I**	**Control Group II**
Contract type				
Permanent contract	0.92	0.95	0.91	0.94
Fixed-term contract	0.04	0.03	0.05	0.04
Temporary contract	0.04	0.01	0.04	0.02
Apprenticeship job	0.00	0.00	0.00	0.00
Blue collar	0.33	0.22	0.34	0.37
Ability of working from home (Bin)	0.54	0.44	0.53	0.64
Monthly total wage ($1000s)	4.07	4.97	3.88	3.95
Hourly total wage	26.33	31.98	25.13	25.84
Number of worked hours	154.71	156.94	154.48	153.74
Enterprise Size				
Less than 5	0.09	0.08	0.08	0.11
6 to 20	0.16	0.17	0.15	0.19
21 to 50	0.14	0.14	0.13	0.17
51 to 200	0.23	0.23	0.22	0.24
More than 200	0.39	0.39	0.42	0.29
Distance to border (Continuous)	21.03	22.22	19.96	23.57
Distance (Bins)				
Less than 15km	0.52	0.50	0.51	0.54
15 to 40km	0.32	0.25	0.42	0.07
More than 40km	0.16	0.26	0.06	0.39
Female	0.38	0.37	0.40	0.34
Age				
Less than 20 years	0.00	0.00	0.00	0.01
20-24 years	0.06	0.05	0.06	0.07
25-29 years	0.14	0.14	0.14	0.14
30-34 years	0.15	0.14	0.16	0.14
35-39 years	0.16	0.16	0.16	0.15
40-44 years	0.16	0.17	0.16	0.15
45-49 years	0.15	0.15	0.15	0.14
50-54 years	0.11	0.12	0.11	0.12
55-59 years	0.06	0.05	0.05	0.06
60 years and more	0.02	0.01	0.01	0.02
Has not Luxembourg citizenship	0.96	0.93	0.98	0.93
Has a child under 19	0.53	0.56	0.52	0.52
N	5,183,231	798,818	3,351,255	1,033,158
Individuals	152,249	21,537	100,129	30,583

Note: Statistics are employee-month averages aggregated over the entire 2015–2019 period.

Figure 3. Share of Individuals with at least one absence in a month in Treatment and Control Group I



individual level. The baseline model is thus specified as

$$Y_{it} = \gamma(D_t \times \text{Treatment Zone}_i) + X_{it}\beta + (\text{Year}_t \times \text{Month}_t)\delta + u_i + e_{it} \tag{1}$$

where Year_t and Month_t are year and month dummies interacted, u_i is an employee fixed effect, and $D_t = 1$ if t is in the period covered by the road disruption and 0 otherwise. X_{it} incorporates individual (time varying) characteristics, such as age, parental responsibilities, whether an employee holds a permanent contract, and the employer's size. Treatment Zone_i is equal to 1 for Belgians living around E411 (i.e., the Treatment Group) and 0 for the control groups. Long-term trends and seasonal variations in absenteeism are captured by including monthly and yearly dummy variables (but not their interaction). For heterogeneity analysis, we further include interaction terms between the ($D_t \times$ Treatment Zone_i) term and some key covariates (gender, age, and the residence's straight distance d_{it} to the border or dummies for different distance categories – see below).

The difference-in-difference specification identifies the causal effect of the disruption in commuting time due to the roadworks under a parallel trends assumption, namely that the difference in absenteeism between the treatment and control groups would have remained constant over time in the absence of treatment. Such condition is plausible in the present context since the time window of the present study is relatively short, spanning over five years with a seven months disruption. It is unlikely that long-term structural changes in location and employment decisions affected the control and treated groups in different ways. Furthermore, no long-run trend over time in work absenteeism is detected, neither in the control groups over the five years studied, nor in the treatment group outside of the treatment period (levels of absenteeism appear to return to their pre-treatment values after the treatment).

4 Results

4.1 The effect of a commuting time shock on absenteeism

Our baseline results are presented in Column (1) of Table 3. The interaction coefficient between the treatment group indicator and the roadworks period dummy shows that the disruption of the E411 highway led to a significant increase of 0.51 percentage points in absenteeism among Belgian commuters relative to their French counterparts. This translates in an approximate increase of 3.1 percent when compared to the benchmark of 16.43 percent on average in the absence of disruptions. Commuting time appears to have a direct, causal impact on work absences. Column (2) reproduces the model by omitting the employee fixed effects, time-varying covariates, and Month $\times$ Year dummies, and concludes similarly. The underlying mechanism, however, is uncertain at this point: it could be a result of health hazards associated with increased commuting (Hansson et al., 2011; Künn-Nelen, 2016; Roberts et al., 2011), or it could be due to workers' behavioral responses on the margin of shirking – we return to this in Section 4.3 below.

4.2 Heterogeneity analysis

The impact of commuting time on absenteeism is further investigated in Columns (3)–(7) of Table 3, where we examine the influence of distance and gender on the relationship. The examination of additional factors, including quality of work and professional grade, is presented in Table 4.

Table 3. Baseline estimates of the effect of roadworks on work absences and heterogeneity by commuting distance and gender

Model:	Baseline		Distance		Gender		
Sample:	Full					Males	Females
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment Group × Roadworks Period	0.51*** (0.16)	0.37** (0.17)	0.25 (0.23)	-0.07 (0.29)	0.75*** (0.19)	0.73*** (0.19)	0.19 (0.31)
Treatment Group × Roadworks Period × Between 15 and 40km			0.27 (0.40)				
Treatment Group × Roadworks Period × More than 40km			0.99** (0.45)				
Treatment Group × Roadworks Period × Distance to border (continuous)				0.03** (0.01)			
Treatment Group × Roadworks Period × Female					-0.59* (0.36)		
Time-Varying Covariates	Yes	-	Yes	Yes	Yes	Yes	Yes
Month × Year Dummies	Yes	-	Yes	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	-	Yes	Yes	Yes	Yes	Yes
F-test			4.57	7.73	8.10		
N	4,121,159	4,135,915	4,121,159	4,121,159	4,121,159	2,508,472	1,612,687
Individuals	121,338	121,666	121,338	121,338	121,338	74,230	47,108

Note: Robust standard errors are clustered at the individual level. Only coefficients of interest are reported for clarity. The individual, time-varying covariates include: age (bins); the presence of a child under the age of 19 in the household; the individual's employment status in terms of holding a permanent contract; the size of the employer; and the individual's ability to work from home. Significance levels: $^{*}=10\%$, $^{**}=5\%$, *and* $^{***}=1\%$. *F-tests are joint tests of equality for the interaction terms between all factors and the base category.*

By commuting distance We first investigate how commuting distance impacts the relationship between commuting time and absenteeism. To do so, we divided the sample into three groups based on commuting distance to the border point: workers who travel less than 15 km, those who travel between 15 and 40 km, and those who travel more than 40km. The regression is conducted on the entire sample, incorporating a double interaction *Treatment Zone × Roadworks Period × Distance (Bins)*, to explore a potential differential impact on absenteeism based on commuting distance.

The commuting time shock mainly affected workers with long commutes. The results in Column (3) show that individuals who commute more than 40 km were 0.99 percentage points more likely to be absent compared to those who commute less than 15 km, with no significant difference between those traveling between 15 and 40 km and those less than 15 km. The analysis is repeated in Column (4) using commuting distance in continuous form, and the double interaction remains significant and positive, indicating a positive relationship between commuting distance and absenteeism when facing a commuting shock.

By gender We then examine the relationship between roadworks disruption and gender and its impact on absenteeism in Column (5) of Table 3. Prior research indicates higher absenteeism among women compared to men (Casini et al., 2013; VandenHeuvel & Wooden, 1995; Vistnes, 1997). While we also observe this level difference on average (*coefficients omitted for clarity*), notably among women with children and in childbearing age, we unveil contrasting repercussions of the shock in commuting time. Specifically, our results show that men are more affected by the shock to their commuting time (i.e., the double interaction between being a woman and the disruption is significant at a 10% threshold, with a negative coefficient of –0.59). This divergence becomes even more apparent when analyzing Columns (6) and (7), which provide estimates for gender-specific models. Men significantly increased absenteeism by 0.75 percentage points, while no significant effect of roadworks is observed among women.

Several factors may contribute to this finding, including gender differences in commuting distances, flexibility in work arrangements, and other situational factors. As depicted in Figure C.1, women often have shorter commutes than men, which could lessen the impact of disruptions on their daily travel by mitigating the exposure to disruptions. A higher prevalence of flexible work arrangements among women could also potentially help respond to disruptions in commuting time (Lachance-Grzela & Bouchard, 2010; Plantenga, 2010). The higher baseline prevalence of work absences among women might also limit the scope for responding to the commuting time shock.

Other factors We show in Table 4 the influence of a number of other factors measured in our data in the response to commuting time disruptions. In Column (1), we consider the interaction with three age groups: under 34, 35 to 54, and 55 and over. The response to the shock is mostly observed among younger and older workers – no statistically significant increase in absenteeism is observed among the 35 to 54 category.

De Cuyper and De Witte (2006) underline the role of the relational psychological contract in explaining asymmetries in organization commitment between permanent employees and others. Quite surprisingly, having a permanent contract reduces the effect of disruption on absenteeism (Column (3)). However, the non-permanent contract category encompasses a range of employment arrangements, including apprenticeships, temporary contracts, and fixed-term contracts, which can vary widely in terms of job security and employment prospects. In the specific context of Luxembourg, where a majority of workers (95%) are employed under permanent contracts in the Treatment Zone, this finding highlights the potential importance of job security in mitigating the effects of disruptions on absenteeism.

Conversely, the double interaction with hourly wage is positive (Column (5)). This is in line with the findings of Ma and Ye (2019), Columns (7), (8) and (9) do not provide conclusive evidence on the effect of occupation type (blue vs. white collar jobs), firm size or industry on absenteeism in response to commuting shocks. The results on blue-collar may come as a surprise since blue-collar workers are expected to have less flexible work arrangements and may face greater occupational hazards associated to increased fatigue due to longer commutes (e.g., Joyce et al., 2010).

To sum up, we find a small but statistically significant increase in the share of workers that report absence from work when exposed to the commuting time shock. The effect appears driven by male, young or old workers, workers with long commutes (greater than 40kms), and workers with higher wages.

4.3 Health impacts or shirking?

As mentioned above, the increased absences may be due to genuine health hazards but also to increased shirking temptation by calling in sick. The possibility to take two days of absence without examination by a health care professional may facilitate some workers declaring themselves absent for non-legitimate reasons.

Table 5 analyzes three distinct types of absences. Column (1), using the benchmark model, is compared with the three other columns with variations in the dependent variable. While the primary dependent variable is a binary variable equal to 1 if the worker is absent for any reason at least once during the month, the subsequent columns consider different specific reasons for absenteeism. Columns (2), (3), and (4) consider absences due to illness, injury, and family reasons, respectively. Absences for illness (0.36 percentage points) and family reasons (0.15

Table 4. Heterogeneous effects of roadworks on work absences by age and employment characteristics

	Additional Specifications								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment Group × Roadworks Period	0.89***	0.51**	1.59**	0.51***	0.27	1.29*	0.64	0.52***	2.19
	(0.31)	(0.23)	(0.69)	(0.20)	(0.18)	(0.74)	(0.51)	(0.19)	(2.92)
Treatment Group × Roadworks Period × 35 to 54 years	-0.62*								
	(0.37)								
Treatment Group × Roadworks Period × 55 years and more	0.38								
	(0.71)								
Treatment Group × Roadworks Period × Has a child		0.01							
		(0.32)							
Treatment Group × Roadworks Period × Permanent Contract			-1.17*						
			(0.71)						
Treatment Group × Roadworks Period × Monthly total wage ($1000s)				0.01					
				(0.02)					
Treatment Group × Roadworks Period × Hourly total wage					0.01***				
					(0.00)				
Treatment Group × Roadworks Period × Number of worked hours						-0.00			
						(0.00)			
Treatment Group × Roadworks Period × Enterprise Size: 6 to 20							0.33		
							(0.65)		
Treatment Group × Roadworks Period × Enterprise Size: 21 to 50							0.45		
							(0.68)		
Treatment Group × Roadworks Period × Enterprise Size: 51 to 200							-0.26		
							(0.62)		
Treatment Group × Roadworks Period × Enterprise Size: More than 200							-0.51		
							(0.57)		
Treatment Group × Roadworks Period × Blue collar								0.01	
								(0.40)	
Treatment Group × Roadworks Period × Secondary sector									-1.70
									(2.94)
Treatment Group × Roadworks Period × Tertiary sector									-1.69
									(2.92)
Time-Varying Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test	4.72	5.00	5.70	6.57	12.24	6.48	2.81	5.06	3.20
N	4,121,159	4,121,159	4,121,159	4,121,153	4,118,824	4,121,153	4,121,159	4,121,159	4,112,607
Individuals	121,338	121,338	121,338	121,338	121,313	121,338	121,338	121,338	121,153

Note: Robust standard errors are clustered at the individual level. Only coefficients of interest are reported for clarity. The individual, time-varying covariates include: age (bins); the presence of a child under the age of 19 in the household; the individual's employment status in terms of holding a permanent contract; the size of the employer; and the individual's ability to work from home. Significance levels: $^{*} = 10\%$, $^{**} = 5\%$, *and* $^{***} = 1\%$. *F-tests are joint tests of equality for the interaction terms between all factors and the base category.*

percentage points) largely explain the estimated effect of roadworks on absences. Roadworks disruptions do not significantly impact absences due to injury. One plausible interpretation of this observation is that workers may partly falsely report illness or family absences to avoid the discomfort of a longer commute, as these specific absence motives are difficult to monitor if limited to two days. Absences for injury reasons – which are more challenging to claim unduly – do not respond to the commuting time shock, despite the potential occupational risks associated with commuting time fatigue.

Table 5. Regressions by absence type

Dependent:	Absence			
	All	Illness	Injury	Family
	(1)	(2)	(3)	(4)
Treatment Group × Roadworks Period	0.51***	0.36**	-0.02	0.15***
	(0.16)	(0.15)	(0.03)	(0.05)
Month × Year Dummies	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	Yes	Yes	Yes
N	4,121,159	4,121,159	4,121,159	4,121,159
Individuals	121,338	121,338	121,338	121,338

Note: Robust standard errors are clustered at the individual level. Only coefficients of interest are reported for clarity. The individual, time-varying covariates include: age (bins); the presence of a child under the age of 19 in the household; the individual's employment status in terms of holding a permanent contract; the size of the employer; and the individual's ability to work from home. Significance levels: $^{*} = 10\%$, $^{**} = 5\%$, *and* $^{***} = 1\%$.

Table 6. Regressions by absence duration

Specification:	Binary	
	1 or 2 days	3 or more days
	(1)	(2)
Treatment Group × Roadworks Period	0.0051***	0.0016
	(0.0010)	(0.0010)
Time-Varying Covariates	Yes	Yes
Month × Year Dummies	Yes	Yes
Employee Fixed Effects	Yes	Yes
N	4,121,159	4,121,159
Individuals	121,338	121,338

Note: Robust standard errors are clustered at the individual level. Only coefficients of interest are reported for clarity. The individual, time-varying covariates include: age (bins); the presence of a child under the age of 19 in the household; the individual's employment status in terms of holding a permanent contract; the size of the employer; and the individual's ability to work from home. Significance levels: $^{*} = 10\%$, $^{**} = 5\%$, *and* $^{***} = 1\%$.

Results presented in Table 6 further support the idea that the effect of roadworks on absenteeism is concentrated in the first two days of authorized absence. In the models presented in

Table 6, we consider as dependent variable an indicator variable equal to 1 if the person is absent at least once for one to two days or three days or more over a month, respectively. Roadworks increased the probability of having at least one period of absence of one to two days over the month, while the effect of roadworks is not statistically different from zero for periods of three days or more. (We return to variations in the definition of the dependent variable in Section 4.4.)

These results suggest that workers responded to the increase in commuting time by increasing the likelihood of reporting short absences that are not closely monitored.

4.4 Sensitivity

Variations in treatment definition and analysis period Our main result of a relatively small but significant increase in absenteeism during the disruption to commuting time is robust to variations in the definition of the treatment and analysis period.

Since the roadworks started in mid-September 2018 and finished early in May 2019, our month-by-month analysis does not perfectly match the dates of the start and end of the roadworks. In Column (1) of Table 7, we first show estimates in a model where the binary roadworks monthly indicator is replaced with a continuous variable – the number of roadworks days within the month. This change hardly modifies the effect that we observe.

In Column (2) of Table 7, the regression model is estimated only with observations spanning January 2015 through April 2019 – the last month fully impacted by the roadworks. Such a specification addresses the possibility of persistence – such as accrued fatigue or modified commuting routines due to the additional lane – lingering beyond the time frame of the roadworks. Column (3) shows estimates where, on the other hand, data from the year 2015 are omitted – this variation discards pre-treatment periods most distant from the treatment period. Column (4) is based on data with the months of February and March excluded. As shown in Figure 3, absenteeism has a consistent annual pattern, with a prominent peak occurring during February and March across all observed years. We expect the spikes to be due, at least in part, to difficult driving conditions due to weather and increased traffic due to the holidays during these months.[7] These might introduce a competing disruption to commuting time and therefore call for assessing the robustness of our results to their exclusion from the data. Reassuringly, the estimates of the overall impact of roadworks on absenteeism remain statistically significant and of similar magnitude.

Allowing for the February/March exceptions As an alternative to dropping February and March from the data to handle the February and March exceptions, we have also adapted the model specification by including interaction terms between the treatment group indicator and month dummies. Column (5) reports estimates in a model where we allow, for all months, different calendar month effects in the treatment and control groups. For columns (6) and (7), we only allow such interactions for the months of February and March (jointly or separately). What such an extended specification implies is that the effect of the roadworks is estimated only by *how much bigger than usual* is the Treatment-Control difference in absenteeism gap in each month during the roadworks period – this, therefore, allows for a possible systematic difference across the groups in absenteeism in some months.

Such a specification is motivated by Figure 4, which shows the regression coefficients of the Treatment Group indicator interacted with each year-month of the analyzed period – without including the actual treatment period indicator. February and March stand out as two months in which absenteeism often appeared higher in the Treatment Group than in the Control Group, even outside the treatment period. (This is also visible in raw indicators of absenteeism shown in Figure 3.) Reasons for this February/March exceptions are unclear, but as mentioned above, they can plausibly be attributed to differences in driving conditions in bad weather. The deviation observed in February 2018 coincides with a cold wave. Belgium registered its lowest temperature on record on February 28, 2018, with the mercury dipping to -18 degrees Celsius in certain localities. This cold wave, commencing on February 18 and finishing on March 4 (Mievies, 2018) possibly led to differential impacts on the treatment and control groups because of topological differences – the Treatment Group covers an area that generally has a higher altitudinal position

[7] Please refer to: https://weatherspark.com/y/53907/Average-Weather-in-Luxembourg-Year-Round for further details on weather conditions.

Table 7. Sensitivity to variations in treatment period, outcome variable and control group

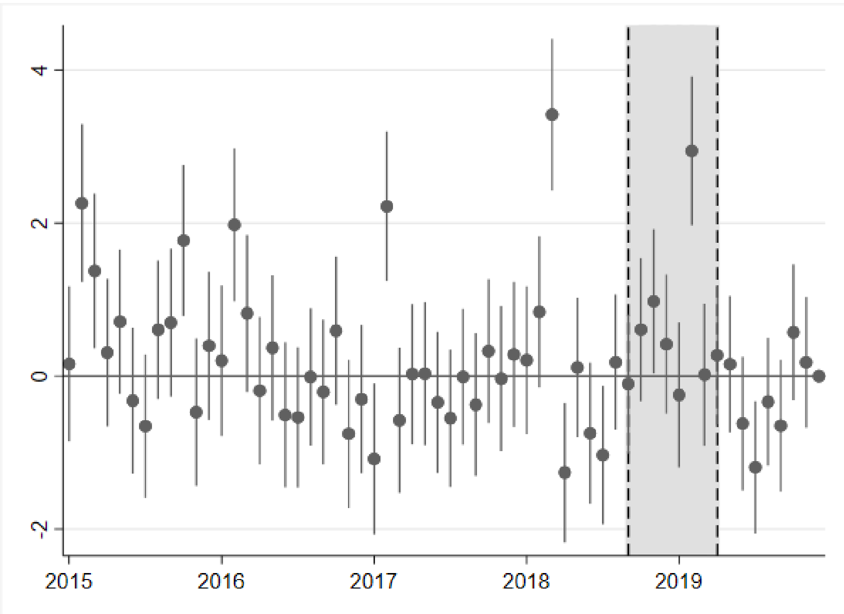
Model:	OLS								Poisson	OLS	Poisson	OLS	Poisson	OLS
Individual characteristics:	Baseline										Less controls		More controls	
Control Group	Control Group I							Control Group II			Control Group I			
Period:	Full	Jan 2015 – Apr 2019	Jan 2016 – Dec 2019	Excluding Feb – Mar					Full					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Roadworks Days	-0.33**													
	(0.15)													
Treatment Group × Roadworks Days	0.36**													
	(0.15)													
Roadworks Period		-0.33	0.31	-1.15***	-1.00***	-1.02***	-1.02***	-0.26	0.33***	0.35***	0.36***	0.40***	0.10***	0.09***
		(0.22)	(0.21)	(0.22)	(0.22)	(0.22)	(0.22)	(0.42)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Treatment Group × Roadworks Period		0.31*	0.56***	0.44**	0.24	0.34**	0.34**	0.39	0.02	0.01	0.01	-0.00	0.02	0.01
		(0.18)	(0.16)	(0.17)	(0.17)	(0.17)	(0.17)	(0.25)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Age														
20-24 years	4.08***	3.80***	4.07***	4.16***	4.08***	4.08***	4.08***	3.30***	0.55***	0.30***	0.65***	0.43***	0.68***	0.44***
	(0.66)	(0.76)	(0.73)	(0.69)	(0.66)	(0.66)	(0.66)	(1.05)	(0.09)	(0.07)	(0.09)	(0.07)	(0.09)	(0.07)
25-29 years	6.72***	6.31***	6.79***	6.87***	6.72***	6.72***	6.72***	6.30***	0.80***	0.48***	0.97***	0.72***	1.09***	0.79***
	(0.70)	(0.80)	(0.78)	(0.73)	(0.70)	(0.70)	(0.70)	(1.13)	(0.10)	(0.08)	(0.09)	(0.08)	(0.10)	(0.08)
30-34 years	7.57***	7.10***	7.61***	7.74***	7.56***	7.56***	7.56***	6.73***	0.80***	0.54***	0.99***	0.79***	1.26***	1.03***
	(0.74)	(0.84)	(0.81)	(0.77)	(0.74)	(0.74)	(0.74)	(1.20)	(0.10)	(0.09)	(0.10)	(0.09)	(0.10)	(0.09)
35-39 years	6.44***	6.14***	6.65***	6.75***	6.43***	6.43***	6.43***	5.89***	0.61***	0.32***	0.79***	0.57***	1.25***	1.01***
	(0.77)	(0.87)	(0.85)	(0.80)	(0.77)	(0.77)	(0.77)	(1.26)	(0.10)	(0.10)	(0.10)	(0.09)	(0.10)	(0.09)
40-44 years	5.02***	4.72***	5.35***	5.40***	5.01***	5.01***	5.01***	5.07***	0.46***	0.15	0.64***	0.38***	1.28***	1.03***
	(0.80)	(0.90)	(0.88)	(0.83)	(0.80)	(0.80)	(0.80)	(1.32)	(0.11)	(0.10)	(0.11)	(0.10)	(0.10)	(0.09)
45-49 years	3.92***	3.74***	4.18***	4.31***	3.92***	3.92***	3.92***	4.27***	0.44***	0.09	0.60***	0.31***	1.45***	1.17***
	(0.83)	(0.93)	(0.92)	(0.86)	(0.83)	(0.83)	(0.83)	(1.38)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.10)
50-54 years	3.78***	3.57***	3.77***	4.19***	3.78***	3.78***	3.78***	4.44***	0.48***	0.14	0.63***	0.33***	1.68***	1.41***
	(0.87)	(0.97)	(0.96)	(0.90)	(0.87)	(0.87)	(0.87)	(1.44)	(0.12)	(0.11)	(0.12)	(0.11)	(0.11)	(0.10)
55-59 years	5.47***	5.19***	5.28***	5.80***	5.46***	5.47***	5.47***	5.75***	0.68***	0.43***	0.82***	0.61***	2.07***	1.91***
	(0.92)	(1.02)	(1.01)	(0.96)	(0.92)	(0.92)	(0.92)	(1.54)	(0.13)	(0.12)	(0.12)	(0.12)	(0.11)	(0.11)
60 years and more	8.42***	8.21***	8.13***	8.41***	8.41***	8.42***	8.42***	7.59***	0.96***	0.87***	1.09***	1.01***	2.51***	2.52***
	(1.05)	(1.17)	(1.16)	(1.09)	(1.05)	(1.05)	(1.05)	(1.74)	(0.14)	(0.15)	(0.14)	(0.15)	(0.12)	(0.13)
Has a child under 19	1.59***	1.52***	1.35***	1.48***	1.59***	1.59***	1.59***	2.46***	0.11***	0.32***	0.11***	0.32***	0.16***	0.33***
	(0.17)	(0.19)	(0.20)	(0.18)	(0.17)	(0.17)	(0.17)	(0.29)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
Permanent contract	8.16***	7.78***	7.97***	8.08***	8.16***	8.16***	8.16***	7.35***	1.06***	0.92***			0.92***	0.87***
	(0.17)	(0.19)	(0.19)	(0.18)	(0.17)	(0.17)	(0.17)	(0.38)	(0.02)	(0.02)			(0.03)	(0.03)
Ability of working from home (Bin)	1.27***	1.35***	1.43***	1.16***	1.27***	1.27***	1.27***	1.20***	0.23***	0.19***	0.55***	0.55***	0.47	0.04
	(0.21)	(0.23)	(0.23)	(0.21)	(0.21)	(0.21)	(0.21)	(0.42)	(0.03)	(0.03)	(0.03)	(0.03)	(0.54)	(0.48)
Enterprise Size														
6 to 20	2.41***	2.31***	2.42***	2.43***	2.41***	2.41***	2.41***	2.30***	0.22***	0.18***			0.19***	0.17***
	(0.19)	(0.21)	(0.21)	(0.20)	(0.19)	(0.19)	(0.19)	(0.33)	(0.03)	(0.03)			(0.03)	(0.03)
21 to 50	4.11***	4.08***	4.05***	4.05***	4.11***	4.11***	4.11***	4.24***	0.32***	0.28***			0.29***	0.26***
	(0.22)	(0.25)	(0.25)	(0.23)	(0.22)	(0.22)	(0.22)	(0.41)	(0.03)	(0.03)			(0.03)	(0.03)
51 to 200	5.11***	4.96***	5.03***	5.13***	5.11***	5.11***	5.11***	5.12***	0.38***	0.33***			0.33***	0.31***
	(0.23)	(0.26)	(0.26)	(0.24)	(0.23)	(0.23)	(0.23)	(0.43)	(0.03)	(0.03)			(0.04)	(0.03)
More than 200	6.34***	5.96***	6.13***	6.27***	6.34***	6.34***	6.34***	7.40***	0.44***	0.38***			0.38***	0.37***
	(0.26)	(0.28)	(0.29)	(0.27)	(0.26)	(0.26)	(0.26)	(0.49)	(0.04)	(0.04)			(0.04)	(0.04)
Monthly total wage ($1000s)													0.01***	0.01***
													(0.00)	(0.00)
Has not Luxembourgish citizenship													-0.02	0.00
													(0.06)	(0.06)
Distance (Bins)														
Between 15 and 40km													-0.02	-0.01
													(0.03)	(0.04)
More than 40km													0.02	0.04
													(0.06)	(0.07)
Month × Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month × Treatment Group	-	-	-	-	Yes	-	-	-	-	-	-	-	-	-
February/March × Treatment Group	-	-	-	-	-	Yes	-	-	-	-	-	-	-	-
February × Treatment Group	-	-	-	-	-	-	Yes	-	-	-	-	-	-	-
March × Treatment Group	-	-	-	-	-	-	Yes	-	-	-	-	-	-	-
Employee Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	4121159	3513176	3375409	3445372	4121159	4121159	4121159	1182428	3559608	4121159	3570792	4135915	3537707	4093658
Individuals	121338	113498	115469	120614	121338	121338	121338	34503	86601	121338	86837	121666	86084	120433

Note: Robust standard errors are clustered at the individual level. Only coefficients of interest are reported for clarity. Significance levels: $^{*} = 10\%$, $^{**} = 5\%$, *and* $^{***} = 1\%$.

relative to Control Group I.

The effect size of the treatment is reduced in these more flexible specifications, but it remains positive and significant if only February and March deviations are adjusted for. In the full specification with allowance for possible Treatment-Control difference in all months – as in Figure 4 with the addition of a treatment period interaction term – the coefficient remains positive but loses statistical significance. One could, however, argue that such a model may overfit the data – leaving little scope for identifying the effect of the roadworks.

Figure 4. Monthly difference in absenteeism between Treatment Group and Control Group I – regression-adjusted estimates without treatment period indicator



Note: The plotted point estimates are derived from interaction terms between the treatment group and year-month indicators. The model is specified as follows:
$Y_{it} = \gamma(Year_t \times Month_t \times Treatment\ Zonei) + Xit\beta + (Year_t \times Month_t)\delta + u_i + e_{it}$ *Standard errors are clustered at the individual level. The timeline is represented on the x-axis, while the y-axis illustrates the magnitude of the coefficients.*

Variation in control group The main result is also robust to changing the control group. Column (8) of Table 7 shows model estimates when Control Group II – Belgian cross-border workers residing close to the north of Luxembourg – instead of Control Group I – French cross-border workers. However, while the point estimate of the impact of the roadworks remains similar (0.39 against 0.51), its standard error is larger, and the coefficient loses statistical significance.

However, for reasons mentioned above regarding proximity to Luxembourg City and potential contamination, we trust Control Group I provides a more appropriate control group.

Variation in the outcome variable Finally, as mentioned above, the choice of outcome variable reveals important. Columns (9)–(14) of Table 7 shows estimates of the impact of roadworks on a continuous dependent variable – the number of days of absence in the month – instead of the binary variable denoting at least one day of absence. The model, estimated through both OLS and a Poisson approach, accommodates various specifications with different numbers of control variables.[8]

In all such specifications, our coefficient of interest ceases to be significantly different from zero. As mentioned above, commuting time disruptions predominantly influence absenteeism at the extensive margin: they amplify the probability of an incidence of absence, yet without a statistically noticeable impact on the average number of days absent in the month. This reinforces the observations from Table 6 showing that the effect of roadworks on absenteeism appears concentrated within the initial two days of authorized absence.

4.5 Placebo analyses

To ascertain the validity of the "roadworks effect", we conducted two types of placebo tests to ensure that no unanticipated effects appear in situations or periods where they logically should not occur. Firstly, the Treatment Group was replaced with Control Group II, which, per our expectations, was either unaffected or only slightly indirectly impacted by the roadworks. Secondly, we estimated the "roadworks effect" during months when no roadworks were causing disruptions.

The estimates derived from these placebo regressions analyses are detailed in Table 8. Reassuringly, substituting the Treatment Group with Control Group II does not yield a significant variation in absenteeism during roadworks (Column (1)).

In our next step, adopting the methodology suggested by Roth et al. (2023), we carried out several regression estimations for the second placebo test (Columns (2)-(10)). These models

[8]See Appendix A for details on the Poisson model specification.

integrated placebo treatment period indicators into the model specification – one for September 2017 to April 2018 and one for September 2016 to April 2017. These periods were tested both separately and concurrently. This technique allowed us to assess the outcomes' concurrent movement pre-disturbance.

Due to reasons previously discussed in Section 4.4, we needed to adjust for the increased absenteeism observed in February/March across most years. To this end, we incorporated a single dummy variable for the interaction of either February or March and the Treatment Group indicators (Columns (2)-(4)). Furthermore, we introduced separate dummy variables for both months, each interacting with the Treatment Group indicators (Columns (5)-(7)). In the final set of models (Columns (8)-(10)), we entirely excluded observations from February and March.

Accordingly, the Placebo Period (Sep 2017-Apr 2018) does not yield a significant result, hence successfully passing the placebo test. The Placebo Period (Sep 2016-Apr 2017) indicates a significant negative result in some columns, suggesting a lower bound for our coefficient of interest. However, this significance disappears upon excluding February and March observations (Columns (8)-(10)), thus satisfactorily passing the placebo test in this case.

Throughout all models, our primary coefficient of interest consistently exhibits positive significance. Even though the initial analysis raised some questions regarding the significance of one placebo period, subsequent models reinforced the confidence in the overall experimental design. Collectively, these placebo analyses enhance the robustness of our principal findings and underline the "roadworks effect" as a credible driver of absenteeism.

5 Conclusion

Assessing the causal effect of commuting time on absenteeism requires considering exogenous, short-lived variations in commuting. This is because residential and employment choices are frequently made in tandem by forward-looking, rational workers. A mere cross-sectional exploration of the distance-to-work and absenteeism relationship offers a narrow perspective. Surprisingly, despite the substantial implications of absenteeism – both a health risk and a productivity concern – particularly in urban areas plagued by traffic and congestion, few studies provide plausibly

Table 8. Placebo tests

Treatment:	Control Group II	Treatment Group								
Control:		Control Group I								
Period:		Full						Excluding Feb - Mar		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment × Roadworks Period	0.11	0.37**	0.28*	0.31*	0.37**	0.28*	0.31*	0.44**	0.42**	0.41**
	(0.22)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.18)	(0.17)	(0.18)
Treatment × Placebo Period (Sep 2017–Apr 2018)		0.18		0.11	0.18		0.11	-0.00		-0.04
		(0.17)		(0.17)	(0.17)		(0.17)	(0.18)		(0.18)
Treatment × Placebo Period (Sep 2016–Apr 2017)			-0.37**	-0.35**		-0.37**	-0.35**		-0.22	-0.23
			(0.16)	(0.17)		(0.16)	(0.17)		(0.18)	(0.18)
Time-Varying Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
February/March × Treatment Group	-	Yes	Yes	Yes	-	-	-	-	-	-
February × Treatment Group	-	-	-	-	Yes	Yes	Yes	-	-	-
March × Treatment Group	-	-	-	-	Yes	Yes	Yes	-	-	-
Month × Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employee Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3 722 813	4 121 159	4 121 159	4 121 159	4 121 159	4 121 159	4 121 159	3 445 372	3 445 372	3 445 372
Individuals	112 195	121 338	121 338	121 338	121 338	121 338	121 338	120 614	120 614	120 614

Note: Robust standard errors are clustered at the individual level. Only coefficients of interest are reported for clarity. Significance levels: $^{*} = 10\%$, $^{**} = 5\%$, *and* $^{***} = 1\%$.

causal estimates on this relationship.

By exploiting roadworks that induced significant yet short-lived disruptions in commuting time for a large population of workers and using accurate population-wide data on work absences, our findings reveal that the disruption on the workers' route increased absenteeism by approximately 0.51 percentage points (or 3.1 percent) – a statistically significant effect, albeit modest in magnitude. While extended commuting times might bear health implications, our data suggests workers might adapt to such disturbances through work avoidance. Interestingly, no medical certificate is required until the third consecutive day of absence. The study highlights that workers commuting more than 40 km are the most affected. Additionally, there are notable gender and age disparities: men and individuals at both ends of the working age spectrum appear more susceptible to commuting shocks.

The findings are generally robust to various robustness checks and placebo analyses. However, the choice of outcome variable reveals pivotal: we find an effect at the intensive margin on the probability of being absent from work, but this does not translate into significant increases at the extensive margin (that is, on the average number of days of absence).

Altogether, our results highlight the importance of considering the toll commuting takes on worker productivity and health, especially in the face of urban congestion and traffic. These results should feed the contemporary debate surrounding the benefits of teleworking possibilities to help workers respond to unforeseen, temporary shocks to commuting time, a topic thrust into the limelight by the COVID-19 pandemic.

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Appendix A Poisson Model

Roadworks imply a relatively small shock, and it is expected that individuals will continue to work for long-term financial sustainability. Also absences longer than two days need to be supported by medical examination. Consequently, the marginal effect of taking a day of absence may differ between those who are not typically absent and those who are absent ten days a month.

To explore this, we use the number of days absent in a month as the dependent variable in some robustness checks, employing both a linear regression model (OLS with Fixed Effects) and a Poisson panel model with conditional Fixed Effects. The latter is computationally intensive but well-suited to the dependent variable form.

The outcome, a non-negative integer, represents the number of days of absence (the event) per month worked (the time unit) for each cross-border worker. Such data, far from being normally distributed, could suffer from two potential issues: an abundance of zeros and non-random selection. The Poisson distribution is commonly used for this type of data, known as count data (Hausman et al., 1984).

The Poisson model makes several assumptions, including event independence, a constant lambda arrival rate, and no limit on the number of occurrences. Heterogeneity of observations over time could violate the constant arrival rate assumption, as individuals at the beginning of their contract might be less likely to be absent than those with more seniority. The arrival of events must be independent so that the occurrence of one event does not influence the probability of another. Furthermore, the limited number of days of absence per month, equal to the number of possible work days, could violate the assumption of no limit on occurrences. Finally, the days of absence may vary from month to month depending on the number of days worked.

These factors could create a phenomenon of apparent overdispersion, where the conditional variance of the outcome variable is greater than its conditional expected value. In this case, the standard errors and estimated p-values may be too small. To address these concerns, we use the conditional Fixed Effects Poisson model estimated by maximum likelihood estimation techniques, as proposed by Wooldridge (1999), with robust clustered standard errors. Conditional Fixed Effects eliminate unobserved heterogeneity over time. Moreover, Wooldridge (1999) showed that the Poisson Fixed Effects estimator is robust to all failures of the Poisson model assumptions, except for the failure of the conditional correct mean assumption.[9].

Assuming the response variable Y follows a Poisson distribution, the model can be expressed as

$$\log\left(\mathbf{E}(Y|\mathbf{x})\right) = \alpha + \beta'\mathbf{x} + \log\left(exposure\right) \tag{2}$$

where $\alpha \in \mathbb{R}$, $\beta \in \mathbb{R}^n$, and $\mathbf{x}$ is an n-dimensional vector consisting of n independent variables. The model can be rewritten in a more compact form

$$\log\left(\mathbf{E}(Y|\mathbf{x})\right) = \theta'\mathbf{x} + \log\left(exposure\right) \tag{3}$$

which implies

$$\log\left(\mathbf{E}(Y|\mathbf{x})\right) - \log\left(exposure\right) = \log\left(\frac{\mathbf{E}(Y|\mathbf{x})}{exposure}\right) = \theta'\mathbf{x} \tag{4}$$

If Y_{it} are independent observations with corresponding x_{it} values of the predictor variables, then the coefficients can be estimated by maximum likelihood. To further refine the model, we introduce an exposure variable that accounts for variations in the length of the months studied in terms of working days. In this case, the exposure variable is the number of month working days, which standardizes the dependent variable on the same time scale. As a result, the dependent variable is expressed as a count of absences divided by the number of working days each month ($exposure$).

[9] Although many authors suggest using the Fixed Effects negative binomial approach, we have chosen, following the advice of Wooldridge (1999), not to use it. Indeed, Wooldridge (1999) has shown that the negative binomial approach suffers from several flaws, unlike the Fixed Effects Poisson estimator, which is robust to many hypothesis failures. For a complete discussion of this topic, please refer to his paper.

Appendix B Groups Composition

Treatment Group

Country	Arrondissement	Commune
Belgium	Arrondissement d'Arlon	Arlon
Belgium	Arrondissement de Neufchâteau	Bertrix
Belgium	Arrondissement de Neufchâteau	Herbeumont
Belgium	Arrondissement de Virton	Chiny
Belgium	Arrondissement de Virton	Etalle
Belgium	Arrondissement de Virton	Habay
Belgium	Arrondissement de Neufchâteau	Libramont-Chevigny
Belgium	Arrondissement de Neufchâteau	Léglise
Belgium	Arrondissement de Neufchâteau	Neufchâteau
Belgium	Arrondissement de Neufchâteau	Saint-Hubert
Belgium	Arrondissement de Virton	Tintigny
Belgium	Arrondissement de Neufchâteau	Daverdisse
Belgium	Arrondissement de Neufchâteau	Libin
Belgium	Arrondissement de Neufchâteau	Wellin
Belgium	Arrondissement de Neufchâteau	Tellin

Control Group I

Country	Arrondissement	Commune
France	Thionville	Rédange
France	Thionville	Algrange
France	Thionville	Knutange
France	Thionville	Aumetz
France	Thionville	Rochonvillers
France	Thionville	Nilvange
France	Thionville	Tressange
France	Thionville	Angevillers
France	Thionville	Lommerange
France	Thionville	Fontoy
France	Thionville	Boulange
France	Thionville	Russange
France	Thionville	Havange
France	Thionville	Ottange
France	Thionville	Neufchef
France	Thionville	Audun-le-Tiche
France	Thionville	Basse-Rentgen
France	Thionville	Zoufftgen
France	Thionville	Rodemack
France	Thionville	Évrange
France	Thionville	Thionville
France	Thionville	Gavisse
France	Thionville	Boust
France	Thionville	Kanfen
France	Thionville	Cattenom
France	Thionville	Breistroff-la-Grande

France	Thionville	Manom
France	Thionville	Mondorff
France	Thionville	Entrange
France	Thionville	Roussy-le-Village
France	Thionville	Koenigsmacker
France	Thionville	Illange
France	Thionville	Hettange-Grande
France	Thionville	Hagen
France	Thionville	Berg-sur-Moselle
France	Thionville	Puttelange-lès-Thionville
France	Thionville	Yutz
France	Thionville	Volmerange-les-Mines
France	Thionville	Fixem
France	Thionville	Escherange
France	Thionville	Beyren-lès-Sierck
France	Thionville	Terville
France	Thionville	Richemont
France	Thionville	Florange
France	Thionville	Fameck
France	Thionville	Mondelange
France	Thionville	Uckange
France	Thionville	Vitry-sur-Orne
France	Thionville	Rosselange
France	Thionville	Hayange
France	Thionville	Moyeuvre-Petite
France	Thionville	Serémange-Erzange
France	Thionville	Moyeuvre-Grande
France	Thionville	Clouange

France	Thionville	Gandrange
France	Thionville	Ranguevaux
France	Thionville	Kemplich
France	Thionville	Bettelainville
France	Thionville	Bertrange
France	Thionville	Stuckange
France	Thionville	Basse-Ham
France	Thionville	Oudrenne
France	Thionville	Valmestroff
France	Thionville	Volstroff
France	Thionville	Budling
France	Thionville	Bousse
France	Thionville	Kédange-sur-Canner
France	Thionville	Guénange
France	Thionville	Distroff
France	Thionville	Hombourg-Budange
France	Thionville	Veckring
France	Thionville	Metzeresche
France	Thionville	Rurange-lès-Thionville
France	Thionville	Luttange
France	Thionville	Elzange
France	Thionville	Buding
France	Thionville	Klang
France	Thionville	Metzervisse
France	Thionville	Kuntzig
France	Thionville	Monneren
France	Thionville	Inglange
France	Thionville	Aboncourt

France	Metz	Metz
France	Metz	Sainte-Ruffine
France	Metz	Coin-sur-Seille
France	Metz	Augny
France	Metz	Vernéville
France	Metz	Lorry-lès-Metz
France	Metz	Lorry-Mardigny
France	Metz	Jussy
France	Metz	Féy
France	Metz	Arry
France	Metz	Châtel-Saint-Germain
France	Metz	Moulins-lès-Metz
France	Metz	Ars-sur-Moselle
France	Metz	Pournoy-la-Chétive
France	Metz	Rozérieulles
France	Metz	Coin-lès-Cuvry
France	Metz	Vionville
France	Metz	Gorze
France	Metz	Gravelotte
France	Metz	Corny-sur-Moselle
France	Metz	Marieulles
France	Metz	Jouy-aux-Arches
France	Metz	Novéant-sur-Moselle
France	Metz	Ancy-sur-Moselle
France	Metz	Lessy
France	Metz	Pouilly
France	Metz	Vaux
France	Metz	Rezonville

France	Metz	Cuvry
France	Metz	Antilly
France	Metz	Hayes
France	Metz	Marsilly
France	Metz	Chesny
France	Metz	Mey
France	Metz	Mécleuves
France	Metz	Servigny-lès-Raville
France	Metz	Chieulles
France	Metz	Sanry-sur-Nied
France	Metz	Raville
France	Metz	Saint-Hubert
France	Metz	Sainte-Barbe
France	Metz	Courcelles-sur-Nied
France	Metz	Saint-Julien-lès-Metz
France	Metz	Vantoux
France	Metz	Courcelles-Chaussy
France	Metz	Montoy-Flanville
France	Metz	Glatigny
France	Metz	Sorbey
France	Metz	Maizery
France	Metz	Ars-Laquenexy
France	Metz	Charleville-sous-Bois
France	Metz	Argancy
France	Metz	Pange
France	Metz	Ogy
France	Metz	Ay-sur-Moselle
France	Metz	Flévy

France	Metz	Jury
France	Metz	Les Étangs
France	Metz	Charly-Oradour
France	Metz	Trémery
France	Verdun	Sorbey
France	Metz	Vry
France	Metz	Silly-sur-Nied
France	Metz	Laquenexy
France	Metz	Servigny-lès-Sainte-Barbe
France	Metz	Sanry-lès-Vigy
France	Metz	Noisseville
France	Metz	Failly
France	Metz	Colligny
France	Metz	Ennery
France	Metz	Nouilly
France	Metz	Coincy
France	Metz	Burtoncourt
France	Metz	Malroy
France	Metz	Vigy
France	Metz	Bazoncourt
France	Metz	Vany
France	Metz	Chailly-lès-Ennery
France	Metz	Maizeroy
France	Metz	Peltre
France	Metz	Retonfey
France	Metz	Saulny
France	Metz	Roncourt
France	Metz	Bronvaux

France	Metz	Amanvillers
France	Metz	Plesnois
France	Metz	Sainte-Marie-aux-Chênes
France	Metz	Norroy-le-Veneur
France	Metz	Marange-Silvange
France	Metz	Fèves
France	Metz	Saint-Privat-la-Montagne
France	Metz	Montois-la-Montagne
France	Metz	Amnéville
France	Metz	Rombas
France	Metz	Pierrevillers
France	Metz	Woippy
France	Metz	Semécourt
France	Metz	Hagondange
France	Metz	Talange
France	Metz	Maizières-lès-Metz
France	Metz	Hauconcourt
France	Metz	La Maxe
France	Metz	Plappeville
France	Metz	Le Ban-Saint-Martin
France	Metz	Longeville-lès-Metz
France	Metz	Scy-Chazelles
France	Metz	Marly
France	Metz	Montigny-lès-Metz

Control Group II

Country	Arrondissement	Commune
Belgium	Arrondissement de Liège	Fléron
Belgium	Arrondissement de Liège	Flémalle
Belgium	Arrondissement de Liège	Visé
Belgium	Arrondissement de Liège	Soumagne
Belgium	Arrondissement de Liège	Oupeye
Belgium	Arrondissement de Liège	Liège
Belgium	Arrondissement de Liège	Awans
Belgium	Arrondissement de Liège	Sprimont
Belgium	Arrondissement de Liège	Grâce-Hollogne
Belgium	Arrondissement de Liège	Beyne-Heusay
Belgium	Arrondissement de Liège	Ans
Belgium	Arrondissement de Liège	Dalhem
Belgium	Arrondissement de Liège	Chaudfontaine
Belgium	Arrondissement de Liège	Seraing
Belgium	Arrondissement de Liège	Juprelle
Belgium	Arrondissement de Liège	Trooz
Belgium	Arrondissement de Liège	Comblain-au-Pont
Belgium	Arrondissement de Liège	Bassenge
Belgium	Arrondissement de Liège	Aywaille
Belgium	Arrondissement de Liège	Esneux
Belgium	Arrondissement de Liège	Blégny
Belgium	Arrondissement de Liège	Neupré
Belgium	Arrondissement de Liège	Herstal
Belgium	Arrondissement de Verviers	Dison
Belgium	Arrondissement de Verviers	Aubel
Belgium	Arrondissement de Verviers	Limbourg

Belgium	Arrondissement de Verviers	Welkenraedt
Belgium	Arrondissement de Verviers	Baelen
Belgium	Arrondissement de Verviers	Thimister-Clermont
Belgium	Arrondissement de Verviers	Jalhay
Belgium	Arrondissement de Verviers	Plombières
Belgium	Arrondissement de Verviers	Herve
Belgium	Arrondissement de Verviers	Lontzen
Belgium	Arrondissement de Verviers	Raeren
Belgium	Arrondissement de Verviers	La Calamine
Belgium	Arrondissement de Verviers	Eupen
Belgium	Arrondissement de Verviers	Spa
Belgium	Arrondissement de Verviers	Olne
Belgium	Arrondissement de Verviers	Trois-Ponts
Belgium	Arrondissement de Verviers	Theux
Belgium	Arrondissement de Verviers	Stavelot
Belgium	Arrondissement de Verviers	Malmedy
Belgium	Arrondissement de Verviers	Stoumont
Belgium	Arrondissement de Verviers	Verviers
Belgium	Arrondissement de Verviers	Waimes
Belgium	Arrondissement de Verviers	Pepinster
Belgium	Arrondissement de Verviers	Lierneux

Appendix C Figures

Figure C.1. Distribution of Distance to Border, by Gender

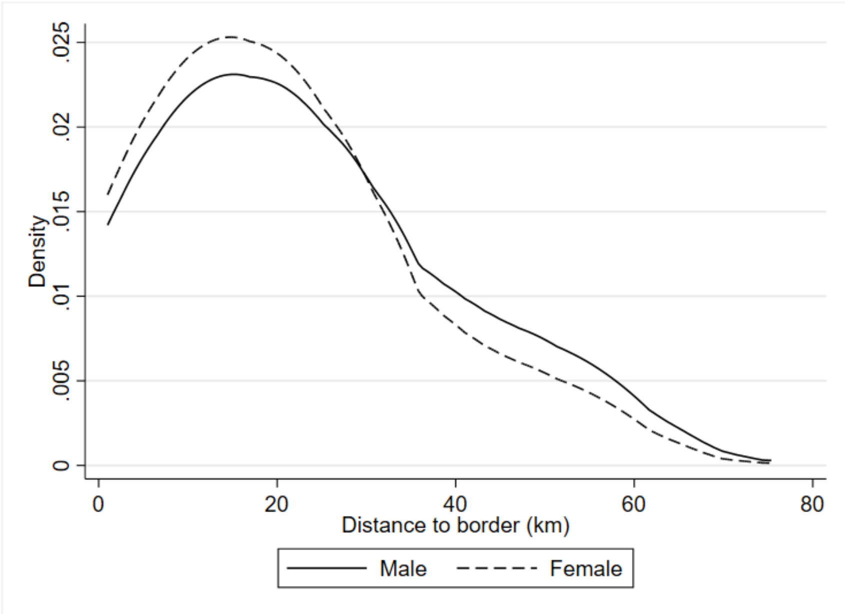


Figure C.2. Absenteeism by Roadworks Status, over Distances

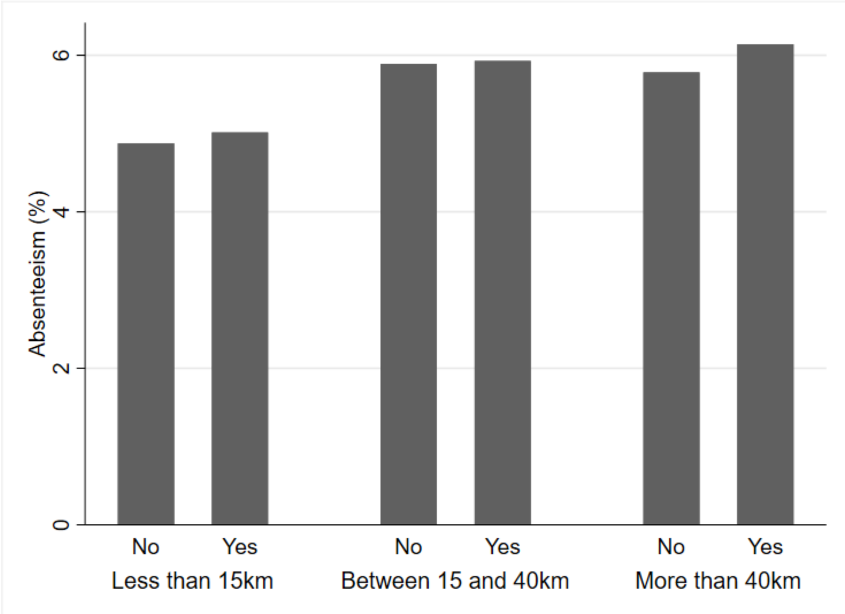
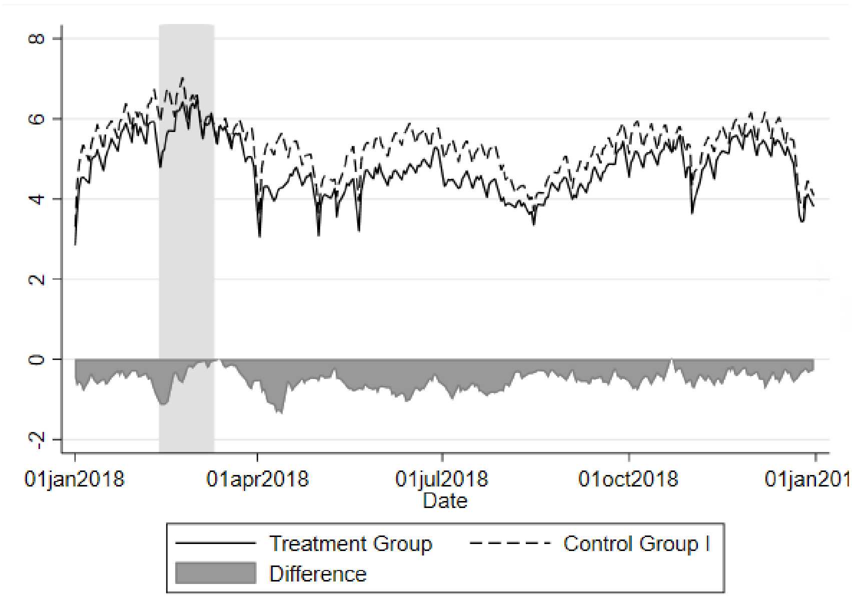


Figure C.3. Comparison of Daily Absenteeism Rates in 2018 Between Treatment and Control Group I



Note: This figure illustrates the 2018 absenteeism rate trajectories for both the Treatment Group and Control Group I. The 'Difference' line is derived by subtracting the absenteeism rate of Control Group I from that of the Treatment Group at each point in time, thereby illustrating the rate disparity between these two groups. A shaded region from February 18th to March 4th, 2018, is emphasized to indicate a period hypothesized to be significantly impacted by weather conditions. The y-axis denotes the absenteeism rate as a proportion, while the x-axis represents the progression of time throughout 2018.

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