Zero fare, cleaner air? The causal effect of Luxembourg's free public transportation policy on transport emissions

Tobias Eibinger*[1] and Sachintha Fernando†[2]

[1]University of Graz ‡
[2]Martin Luther University Halle-Wittenberg§

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Abstract

In March 2020, Luxembourg became the first country to make public transport free. We use this unique setting to evaluate the policy's impact on carbon emissions. Synthetic difference-in-differences allows us to identify a suitable control group. We use spatial emissions data to construct a panel of NUTS 2 control regions in the EU from 2016–2022. We estimate an average reduction of 6.7% in road transport CO_2 and overall road transport GHG emissions, with larger effects for NO_x. We account for potential confounders, such as the COVID-19 pandemic, shifts in commuting behaviors and advancements in vehicle technologies. Robustness checks support the credibility of our results. Complementary event-study evidence from automatic traffic counters and air quality stations through 2023 corroborates our findings and suggests larger effects on weekends.

Keywords: Emissions, Public Transport, Synthetic DID

JEL Codes: C31, Q54, R48

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*Corresponding author, tobias.eibinger@uni-graz.at, eibinger.tobias@gmail.com
†sachintha.fernando@wiwi.uni-halle.de
‡Universitätsstraße 15, 8010 Graz, Austria.
§Große Steinstraße 73, 06108 Halle, Germany.

1 Introduction

In March 2020, Luxembourg became the first country in the world to abolish fares on all modes of public transit, including buses, trains, and trams, throughout the country to mitigate transport-related externalities (Research Luxembourg, 2021). The provision of affordable and efficient public transport is often discussed as an effective way to reduce emissions from the transport sector (Federal Transit Administration, 2010; International Transport Forum, 2020). Accessible, affordable and efficient public transport can encourage a shift from private motorized transport to more environmentally friendly modes. However, despite these benefits, fully free public transport policies are scarce.

We leverage this quasi-experimental setting in Luxembourg to causally identify and quantify the impact on carbon (CO_2) emissions, nitrogen oxides (NO_x), and greenhouse gas (GHG) emissions in the road transport sector. To evaluate the effect of this policy, we use the recently introduced synthetic difference-in-differences (SDID) method to construct a credible counterfactual for Luxembourg and compare the post-intervention outcomes against it (Arkhangelsky et al., 2021). This allows us to isolate the policy's effect from other confounding factors to achieve robust causal inference.

Luxembourg stands out from other European Union (EU) countries in many ways. It has the highest Gross Domestic Product (GDP) per capita, the highest motorization rate, and the highest per capita CO_2 emissions from transport. These unique characteristics pose challenges in finding comparable regions for constructing a counterfactual scenario. To overcome this, we conduct our analysis at the Nomenclature for Territorial Units for Statistics (NUTS) 2 level, as Luxembourg itself constitutes a NUTS 2 region.[1] While entire countries may not serve as suitable comparison units for Luxembourg, other NUTS 2 regions such as Brussels, Amsterdam, or Paris offer more appropriate benchmarks. This level of analysis ensures a more meaningful comparison of emission trajectories.

To enhance the robustness of our identification, we employ SDID, which combines elements of traditional difference-in-differences (DID) and synthetic control (SC) approaches while overcoming their limitations in our context. The uniqueness of Luxembourg's case makes it less plausible that the parallel trends assumption required for DID estimation will hold. SC methods require a donor pool of units similar in predictors of the outcome to the treated unit–a requirement that is unlikely to be met in our setting. In contrast, the SDID method, combines elements of both DID and SC and allows us to construct a counterfactual CO_2 emission trajectory for Luxembourg from a pool of donor regions without relying on matches in absolute levels at any stage of the procedure–which is essential to draw causal inferences about the policy's impact in our specific setting.

Moreover, we address potential confounding factors related to the COVID-19 outburst.

[1] NUTS is an EU classification system that divides countries into three levels. These classifications are used for collecting, developing, and harmonizing European regional statistics, conducting socio-economic analyses, and framing EU regional policies.

The pandemic likely caused variations in mobility patterns that are unrelated to the free public transportation policy. This complicates identification insofar as mobility behavior in Luxembourg changed differently compared to the control regions. To examine this, we draw on data on working from home and commuting inflow for Luxembourg. We find that Luxembourg's mobility patterns in response to the pandemic were largely consistent with those observed in other EU regions. Nevertheless, we explicitly account for these patterns in our models to enhance the accuracy of our identification strategy. We control for heterogeneous mobility restrictions with a stringency index measuring the degree of restrictions imposed at a country level, and to control for regional variation in pandemic response, we additionally control for daily regional COVID-19 cases in our estimations. Additionally, we account for interactive effects of relative fuel prices between regions to capture changes in fuel tourism, which can substantially impact emissions, even though kilometers driven remain constant.

The potential donor pool for constructing Luxembourg's counterfactual comprises all other predominantly urban European regions at the NUTS 2 level over the period 2016-2022.[2] From this pool, we exclude regions that have implemented any form of public transportation subsidy during the study period (this is elaborated in Section 4). After ensuring a balanced sample, our final donor pool includes 47 NUTS 2 regions and 611 region-time observations. Based on gridded sector-specific emissions data from the European Emission Database for Global Atmospheric Research (EDGAR) (Crippa et al., 2025), we estimate that the free public transport policy in Luxembourg led to an average treatment effect on the treated (ATT) of around -0.067 on road transport CO_2 emissions, i.e., a reduction of around 6.7%. Our results are significant at the 95% confidence level. We find a similar effect for overall GHG emissions from road transport and a somewhat larger effect for NO_2 emissions at -10%, which lies just outside the 95% significance level.

We conduct an event study analysis to verify that parallel trends hold in the pre-treatment period. We conduct various robustness and sensitivity tests, including a placebo test by backdating the policy to 2019, iteratively leaving out regions and countries from the donor pool, and analyzing the results in a more broadly defined sample of NUTS 2 regions. In addition, we carry out an SDID analysis on emissions from non-transport sectors as placebo outcomes. A particularly informative case is the energy use in the building sector because it was a sector that was also affected by COVID. If our main estimates were primarily picking up pandemic-related shocks, we would expect to see similar effects in this sector. Our findings remain consistent across these tests. Reassuringly, the magnitude of our estimates also aligns with survey-based assessments of Luxembourg's free transit policy.

[2] The EU classifies NUTS 3 regions as urban, rural, or suburban. Based on this classification, we define a NUTS 2 region as predominantly urban if it contains at least one NUTS 3 region classified as urban (Eurostat, 2025).

We corroborate our findings with analyses based on rich data from automatic traffic counters and air quality stations. These datasets are available for more recent years and higher frequencies, which allows us to exclude 2020 and 2021 (periods which were still affected by COVID), and to instead focus on a post-treatment and post-COVID window covering 2022-2023. This restriction reduces the scope for direct pandemic-related confounding. In addition, these outcomes are not directly impacted by emissions in the tank (fuel tourism), which eliminates another potential confounding source. For traffic counters, we estimate a reduction of around 4.7% in a conditional specification that controls for changes in fuel prices and precipitation. The effect is somewhat more pronounced on weekends, indicating a stronger modal shift for discretionary travel. The corresponding analysis based on NO_2 concentrations from air quality stations yields a larger effect that is comparable to the estimates obtained from the EDGAR-based NO_x emissions analysis. While these estimates are not statistically significant at conventional significance levels, the sustained declining trends in the post-treatment period are nevertheless informative and suggest a structural shift toward public transport.

The literature on the causal effects of free public transport on emissions remains limited but is growing, largely because such policies have historically been rare. One prominent example is Tallinn (Estonia), which introduced fare-free public transit in 2013 and subsequently expanded the policy. Using descriptive evidence, Cats et al. (2017) show that the reform increased public transport usage but did not significantly affect car use. We contribute to the literature by providing the first causal assessment of a nation wide free public transport policy on road transport related emissions. Methodologically, we employ novel approaches to address the unique challenges presented by Luxembourg's distinct characteristics and the concurrent COVID-19 pandemic. Additionally, this study offers a framework for addressing COVID-19 as a potential confounder in similar research contexts. To the best of our knowledge, there is only one other study that directly looks at Luxembourg's free public transportation policy. Bigi et al. (2023) use an agent-based modeling approach and indicate that the policy significantly contributed to a modal shift from private vehicles to public transport. Our findings contribute to this narrative by providing a causal ex-post evaluation of the policy's impact on CO_2 emissions.

Our paper links to a larger body of literature that ex-post evaluates transport policies designed to decrease reliance on motorized vehicles. Policies aimed at mitigating transport emissions can be categorized into three main types. The first examines policies intended to directly reduce or restrict the use of motor vehicles by making driving more costly or less convenient. These include initiatives such as driving restrictions (Davis, 2017; Gallego et al., 2013; Davis, 2008), low-emission zones (Wolff, 2014; Sarmiento et al., 2023), road pricing (Gibson and Carnovale, 2015), and tax-based instruments (Andersson, 2019; Pretis, 2022).

The second type of policy promotes a shift to public transport, mainly by subsidizing public transit systems or improving infrastructure. This body of literature is particularly relevant to our study, as we also investigate the effects of improved public transport, specifically through enhanced access. Recent research on subsidized transit demonstrates mixed evidence regarding environmental outcomes. For instance, Aydin and Kürschner Rauck (2023) and Gohl and Schrauth (2024) study Germany's 9-Euro ticket introduced in 2022 and find improvements in air quality, especially in regions with strong public transit networks and during weekdays, when commuting activity is highest. However, contrasting findings are presented by Liebensteiner et al. (2024), who observe that while the 9-Euro ticket led to a significant increase in train rides during leisure hours, it only marginally reduced car usage. Similarly, Albalate et al. (2024) find no significant effect on air quality from a four-month public transport subsidy in Spain.

The evidence from studies outside of Europe is also mixed. Two studies carried out in China point in the same direction with respect to prices: lower fares improve air quality, while higher fares worsen it. Liang and Wang (2025) document a 2.1% reduction in $PM_{2.5}$ concentrations in Fuzhou following a fare-free policy in the city center on non-working days between August and October 2020. Yang and Tang (2018) analyze a fare increase in Beijing in 2014 and find a 16% short-run increase in air pollution—measured over a 120-day window—though the effect dissipates in the longer term. By contrast, Colorado's one-month fare-free transit policy in August 2022, introduced to improve ground-level ozone, shows no measurable improvements in ozone concentrations (Webster, 2024). Vieira et al. (2025) show that fully subsidizing fares for older adults raises their transit ridership by about 9.4%, but this increase comes mainly from substitution away from walking rather than from car use. Bull et al. (2021) randomly assigned free public transport vouchers to workers in Santiago (Chile), which were primarily used during off-peak hours. This suggests that the vouchers were more often utilized for leisure activities rather than reducing car usage. Overall, the evidence suggests that while subsidized and fare-free public transport policies can increase ridership, and improve environmental outcomes, they are highly context-dependent and hinge on whether they meaningfully displace private car use.

We next turn to the literature on public transit infrastructure improvements and expansions, which offers additional insights. Li et al. (2019), Lalive et al. (2018), and Chen and Whalley (2012) show that expanding subway and rail services in China, Germany, and Taipei, respectively, improves air quality. Gendron-Carrier et al. (2022) find no average effect from subway openings across 58 cities, but reductions in pollution in more polluted cities. Overall, these studies suggest public transit investments yield mixed results and outcomes vary by local context. Some studies indirectly measure the effects of public transport in the absence of explicit policy interventions, using transit strikes to assess substitution between public and private transport. For instance, Anderson (2014), Adler and van Ommeren (2016), and Bauernschuster et al. (2017) find significant increases in congestion following transit strikes in Los Angeles, Rotterdam, and Germany's five largest cities, respectively.

Policies related to the third type aim to improve the energy and fuel efficiency of vehicles through regulations such as gasoline content standards (Auffhammer and Kellogg, 2011). While most studies focus on individual policies, some jointly examine multiple interventions (Koch et al., 2022; Winkler et al., 2023; Kuss and Nicholas, 2022).

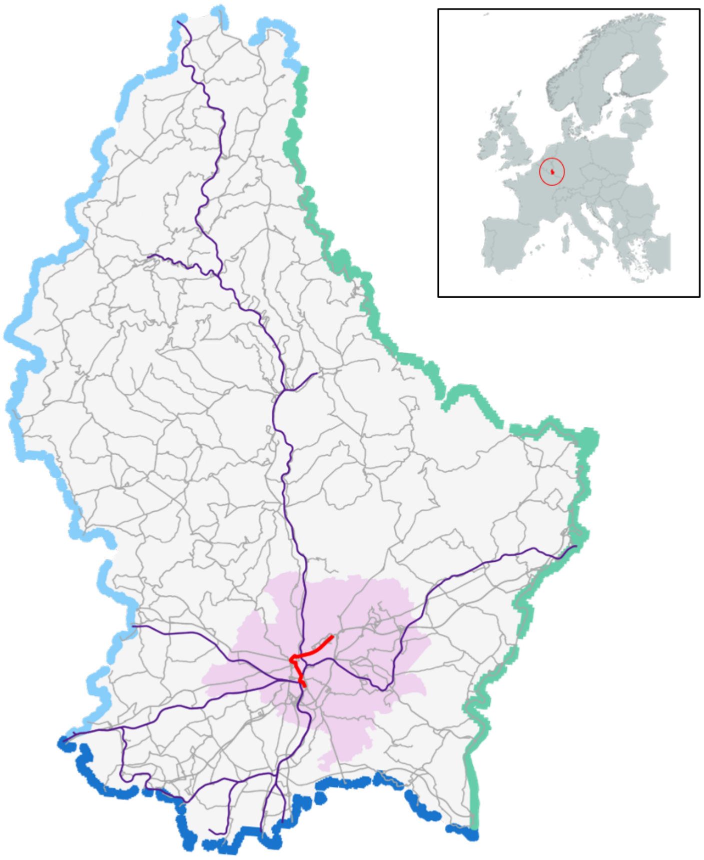
The rest of the paper is organized as follows. Section 2 briefly introduces Luxembourg's free public transport policy. The Data used is detailed in Section 3. The identification strategy is discussed in Section 4. The empirical strategy, including the SDID procedure, is detailed in Section 5. Section 6 provides our empirical results and Section 7 our robustness tests. The results and potential mechanisms are discussed in Section 8. Finally, Section 9 provides concluding remarks.

2 Background: Luxembourg and the policy

Luxembourg is a small country in Western Europe and spans an area of about 2,586 km^2, making it one of the smallest countries in the EU. In the NUTS statistical classification, Luxembourg is treated as a single region at all levels. The country hosts several EU institutions, with its economy primarily driven by banking and finance. Despite its small size and population, Luxembourg has the highest GDP per capita among EU countries, at approximately 140,000 USD. The economic hub is concentrated in Luxembourg City, the capital, located in the south. The country experiences a significant daily inflow of commuters from neighboring Belgium, Germany, and France, with around 200,000 people commuting daily, representing a substantial portion of its population of approximately 660,000. Luxembourg has the highest per capita CO_2 emissions from transport among EU member states, at around 8,200 kg. It also has the highest car density in the EU, with about 700 cars per 1,000 inhabitants. These characteristics set the country quite far apart from other EU countries.

On March 1, 2020, Luxembourg became the first country to eliminate fares for all domestic public transport (excluding first-class rail). This policy, part of the "Modu 2.0" mobility strategy, aimed to shift modal share away from private vehicles to alleviate chronic congestion (Ministère du Développement Durable et des Infrastructures, 2018). The existing public transportation infrastructure forms the backbone of the policy initiative and comprises buses, trams, and trains. The public transit network is sketched in Figure 1, where bus lines are shown in grey, train lines are in purple, and the tram line in red. Buses are the predominant mode of public transportation in Luxembourg and offer quite a comprehensive coverage across the entire country. They connect different localities as well as cross-border lines. Altogether about 400 bus lines are running through Luxembourg, connecting the entire country (Administration des Transports Publics (ATP), 2025a). Trains additionally cover the country in a star-like network, originating in Luxembourg City and connecting it to cross-border connections (Administration des Transports Publics (ATP), 2025b).

Figure 1: Luxembourg public transport network and traffic camera posts



Note: The map shows Luxembourg's borders with Belgium (light blue), Germany (green), and France (dark blue). Light grey lines represent regional bus routes, dark purple lines are national rail, and the red line marks the tram. Luxembourg City is shaded in light pink. Public transport networks shown as of 2018; data is from Luxembourg's open data portal.

The city of Luxembourg is additionally served by the only tram line in the country, which covers around 16 km through 24 stations (Luxtram , 2025). Before the implementation of the free public transportation policy, Luxembourg charged differentiated public transport fares based on the duration and length of travel. Special rates for children and the elderly were available, as outlined in the Ministerial Regulation of July 14, 2017–Règlement ministériel du 14 juillet 2017 fixant les tarifs des transports publics (Le Ministre du Développement durable et des Infrastructures, 2017). Short-term tickets, valid for a maximum of 2 hours from validation were priced at 2 euros. Long-term tickets, valid for 1, 2, and 3 days, ranged from 4 to 12 euros, while annual network subscriptions were priced at 440 euros.[3] Prior to the policy, annual ticket revenue was €41 million which covered only about 8% of total operating costs (RTL Today, 2023; Deutsche Welle, 2020).

It is important to note that the free public transit policy was complemented by enhancements in the transportation infrastructure, notably through extensions in the tram line coverage. In 2017, Luxembourg introduced a tram line traversing Luxembourg City, initially connecting 8 stations(3.5 km). The following year saw the line's expansion by 3 more stops (1.6 km) [4]. December 2020 marked another extension, enlarging the network by 2 kilometers and incorporating 4 additional stations. In September 2022, the tram network was further expanded by an additional 2 new stations. This was followed in July 2024 by the opening of five more stations. Finally, in March 2025, the line reached completion with the addition of the last two stations, establishing a direct link to the Luxembourg airport (Luxtram , 2025). Currently, the tram stretches over 16 kilometers, serves 24 stations, and includes 10 major interchanges (Luxtram , 2025). Four more tramlines are planned to be completed by the end of 2035. [5]

Further, significant improvements to the rail infrastructure are currently being implemented. Specifically, a new line between Luxembourg City and Bettembourg is under construction to streamline connectivity with the South. The gradual entry into service for this new corridor is anticipated by the end of 2026 [6]

Substantial enhancements to bus transport services are also forthcoming. Strategic planning includes the implementation of several new direct routes in the southern region, alongside a comprehensive restructuring of bus timetables scheduled for commencement in 2026.[7] Earlier, in March 2025, a dedicated bus lane was inaugurated between Aire de Berchem and Croix de Gasperich to alleviate congestion on this high-volume commuter corridor.[8] Furthermore, the beginning of 2026 will see the introduction of dynamic lane assignment via overhead sign gantries and manual speed control to optimize traffic flow.[9]

A core pillar of Luxembourg's mobility strategy involves the expansion of Park and Ride (P+R) infrastructure at strategic transportation hubs to facilitate multimodal transit. The expansion program has seen several major milestones. In 2018, 611 spaces were added to the existing 800-space facility in Howald. This was followed in April 2023 by the opening of the Rodange P+R, a multi-story facility providing 1,600 spaces near the Belgian and French borders.[10] In June 2023, the Mersch interchange was inaugurated,

[3] A detailed schedule of public transport fares is available at (Le Ministre du Développement durable et des Infrastructures, 2017).

[4] Schwandl (2020)

[5] LuxToday (2022)

[6] Ministry of Mobility and Public Works, Grand Duchy of Luxembourg (2024)

[7] Kollwelter (2025)

[8] Ministry of Mobility and Public Works (2025)

[9] Ministry of Mobility and Public Works (2025)

[10] Chronicle.lu (2023a)

adding 405 covered spaces.[11] More recent developments in 2025 include the April commissioning of the Héienhaff P+R, which provides an initial 481 spaces near Findel Airport with a planned ultimate capacity of 3,700.[12] Additionally, in July 2025, the Troisvierges and Colmar-Berg facilities were completed, contributing 388 and 237 spaces, respectively, to the national network.[13]

To align with the national fare-free public transport policy, negotiations with neighboring transport networks led to reduced cross-border fares and the introduction of specialized regional tickets for commuters, which account for the free leg on the Luxembourg side[14] Consequently, the scheme is designed to benefit both residents and the significant volume of international commuters.

A central challenge in evaluating the policy is the potential confounding effect of infrastructure expansions. Our primary study period spans 2016 to 2022, which captures the pre-policy baseline and the immediate post-policy shift. Notably, the most substantial network enhancements—including the 2024–2025 tram extensions and the significant bus lane and rail corridor completions, as well as P+R openings fall outside this 2016–2022 window. Additionally, we show in our event study estimates that the 2017, and 2018 expansion did not lead to significant reductions in emissions, suggesting that expansions in isolation are insufficient to trigger modal shifts.

3 Data

We combine the following data to estimate the causal effect of Luxembourg's free public transport policy on emissions from road transport. Data on our main outcome variable, per capita CO_2 emissions from the road transport sector, are constructed by combining spatial road transport emissions extracted from the European Emission Database for Global Atmospheric Research (EDGAR) v8.1 (Crippa et al., 2025) with population data from Eurostat's (2024) regional statistics. In addition to CO_2, we also use data on total GHG and NO_x emissions from road transport, both obtained from EDGAR.

We select emissions as our primary outcome variable not only because they directly relate to a core policy goal, but also because emissions data are consistently available as a panel dataset across regions. This availability is essential for conducting a robust causal evaluation in this context. In contrast, data on transit ridership or vehicle mileage remain scarce and are not always measured uniformly, making them less suitable for a systematic assessment of policy impacts. However, such indicators often offer higher temporal frequency and shorter publication lags. We provide additional evidence based

[11] Chronicle.lu (2023b)
[12] Gouvernement du Grand-Duché de Luxembourg (2025)
[13] Groupe CFL (2025)
[14] Luxembourg's Ministry of Mobility and Public Works (2024); Mobiliteit.lu (2025)

on such data in Section 8.

To control for other factors that may influence emissions from road transport, we include several covariates. Controls related to the pandemic include daily COVID-19 cases at the NUTS 2 level, sourced from Naqvi (2021), as well as the Our World in Data (OWID) COVID-19 Government policy stringency index from Hale et al. (2021). Data on working from home and commuting inflows are obtained from a special extraction from the EU Labor Force Survey (EU-LFS). Other controls encompass fuel prices, which we source from the European Commission's (2024) weekly oil bulletin. Energy intensity is taken from EEA (2024) and captures changes in efficiency of cars. Data on loaded goods is included to capture the effect of freight transport emissions and is obtained from Eurostat's (2024) regional statistics. Finally, we use data on real GDP per capita from the regional statistics to control for overall differences in economic development.

We drop regions with missing data and regions that experienced methodological breaks or data-quality disruptions in data generation to ensure a coherent balanced panel.[15] After additionally dropping bad controls and focusing on urban regions (see Section 4), we are left with 13 EU countries (including Luxembourg) and a total of 47 regions over the sample period 2016-2022, giving a total of 611 region-year observations. The following subsections discuss in more detail the outcome variable, CO_2 emissions from road transport, and the COVID-19 related controls used in our analysis for this remaining set of countries.

3.1 Road transport emissions

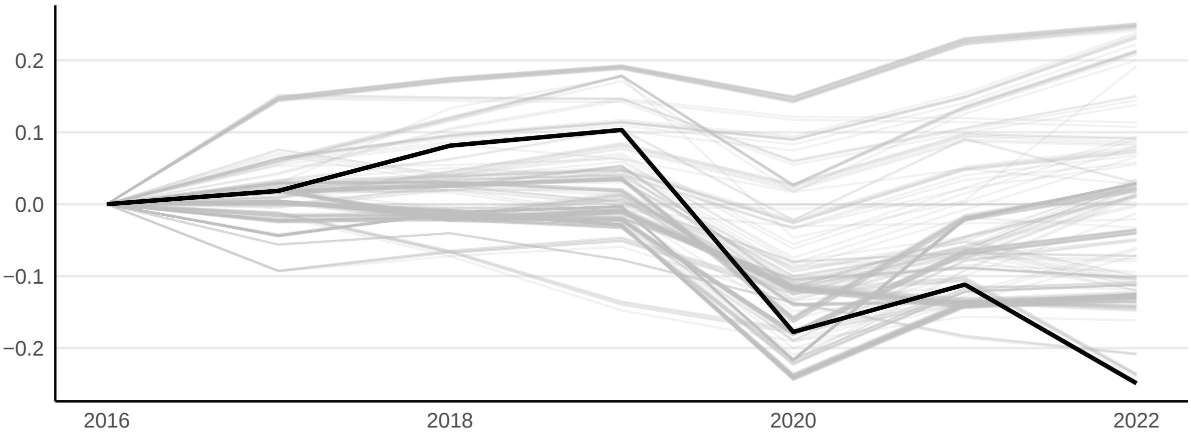
Here, we focus on our main outcome variable, CO_2 emissions for illustration purposes. Road transport emissions are categorized under the Intergovernmental Panel for Climate Change (IPCC) 1996 sector category 1.A.3.b. Emissions are calculated as the product of fuel consumption times the associated IPCC emission factors. The EDGAR database provides annual sector specific grid maps expressed in ton substance with a spatial resolution of 0.1 degrees $\times$ 0.1 degrees. We aggregate these grid cells to the corresponding NUTS 2 regions for the following 19 located in Europe: Austria, Belgium, Croatia, Czech Republic, Denmark, Finland, France, Greece, Hungary, Ireland, Italy, Latvia, Luxembourg, Netherlands, Poland, Portugal, Slovakia, Slovenia, and Spain. The NUTS 2 regional borders are extracted from the Eurostat database (European Commission, 2022). Quality-controlled data is available up to 2022. The panels for NO_x and total GHG are constructed similarly.

We present the evolution of CO_2 emissions from road transport for Luxembourg and

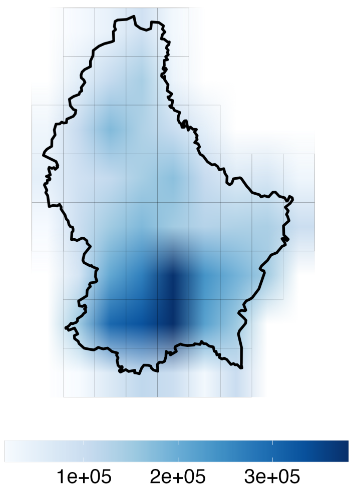
[15] We drop the United Kingdom, Norway, Romania, Sweden, Switzerland, Liechtenstein, and Lithuania due to missing data. Germany experienced methodological and quality breaks in EU-LFS data generation.

Figure 2: Evolution of CO_2 emissions in Luxembourg over time and space

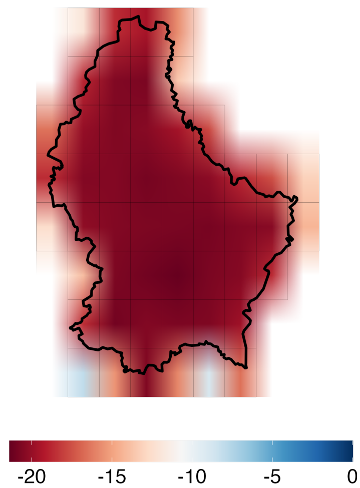
(a) Evolution of Log-CO_2 Emissions for Luxembourg and other NUTS 2 Regions



(b) Average Emissions 2016-2019



(c) %-Change 2020-2022 vs. 2016-2019



Note: Road transport CO_2 emissions (tons) are extracted from the EDGARv8.1 at 0.1x0.1 grid cells. (a) Shows the evolution of Log-CO_2 emissions, centered at zero in 2016. Luxembourg is indicated by the black line. (b) and (c) display spatial distributions of emissions for Luxembourg. (b) shows average emissions over the pre-treatment period, 2016-2019. (c) shows the %-change from average emissions over the post-treatment period (2020-2022) compared to the pre-treatment period.

other NUTS2 regions over time in Figure 2.[16] Panel (a) shows the evolution of the log of annual CO_2 emissions from road transport over the period 2016–2022. Luxembourg is indicated by the solid black line, while other NTUS 2 regions are shown in gray. The impact of COVID-19 can be seen in a drop in emissions from 2019 to 2020 across all regions. In 2021, an increase in emissions can be observed, while 2022 shows noticeable decline in Luxembourg. However, emissions series show substantial variation across regions.

Luxembourg seems to have experienced a relatively large drop in 2020 relative to

[16] Grid-cells that intersect with the NUTS 2 boundaries of Luxembourg are allocated according to their fraction that falls inside these boundaries.

other regions, and emissions in 2021 and 2022 stay consistently below pre-pandemic levels. Panel (b) shows the spatial distribution of average road transport emissions over the period 2016-2019, which constitutes our pre-treatment period. High emissions are indicated in dark blue and lower emissions in light blue. Emissions are concentrated around Luxembourg City and border regions with France. Panel (c) shows the percentage change of average post-treatment (2020-2022) emissions relative to average pre-treatment emissions. Emissions on average stayed below the pre-policy average in the entire country. The largest difference can be observed around Luxembourg City, while differences on the Eastern border of Luxembourg are less pronounced. The overall average emission reduction for the country for the post-treatment period relative to the pre-treatment period is around -18.5%. To extract the extent to which this reduction can be attributed to the free-public transport policy is the aim of our paper.

The reduction in CO_2 emissions shown in Figure 2 is directly related to a reduction in fuel consumption, indicating a shift in mobility patterns. This shift may be attributed to various factors. Our primary interest is the causal effect of the free public transport policy. To discern this causal effect, we need to account for potential variation caused by other confounding effects. These potential sources of variation in CO_2 emissions include COVID-19 related restrictions and reduced mobility, as well as an increase in the number people working from home and fewer commuting trips.

3.2 COVID-19 related variables

In this section, we discuss variables that capture COIVD-19-related effects. We describe these variables in some detail for the periods most directly affected by the pandemic, i.e., 2020 and 2021. By comparing changes in these variables immediately before and after the onset of COVID-19, we can assess whether Luxembourg experienced materially different pandemic dynamics than other European regions. Data quality and completeness of COVID cases deteriorated after 2021 as testing becomes less common and reporting practices changed in 2022.

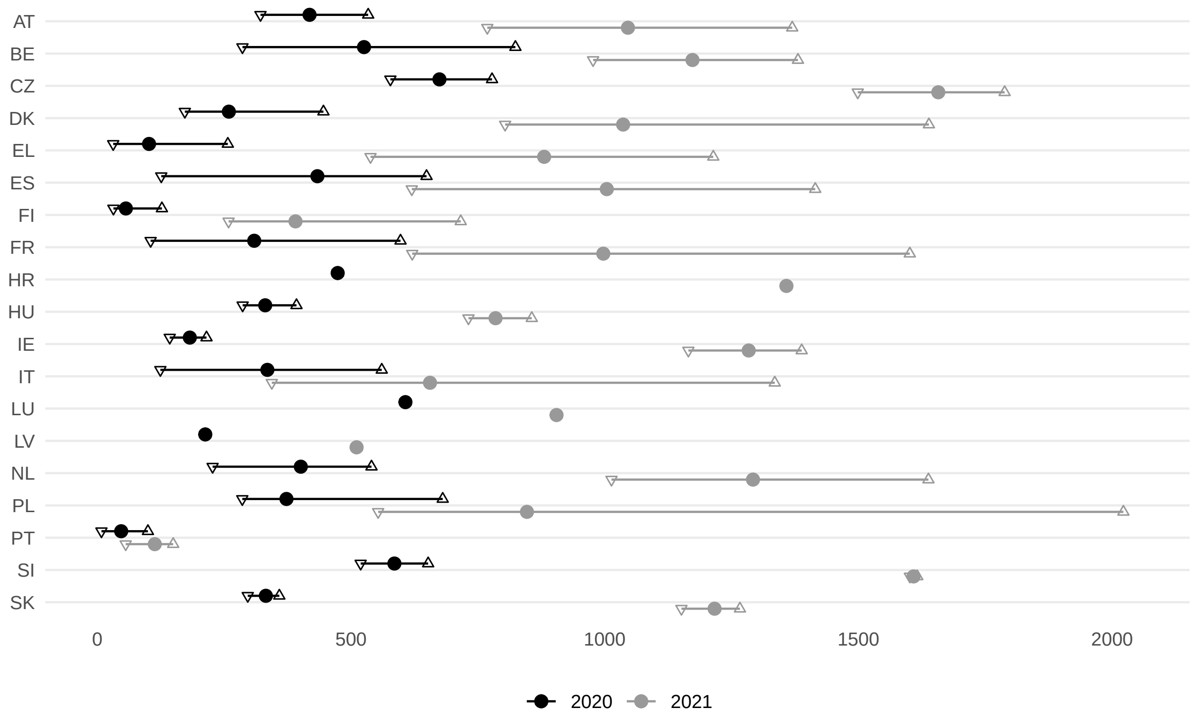
With the onset of the COVID-19 pandemic, many countries implemented lockdowns and travel restrictions to curtail the spread of the virus (Hale et al., 2021). Luxembourg was no exemption, with its government convening an extraordinary Government Council to respond to the pandemic on the 12th of March 2020. Subsequently, mobility restrictions aimed at containing the spread of the virus came into effect on the 13th of March, 2020 (Government of the Grand Duchy of Luxembourg, 2020).

The Our World in Data (OWID) COVID-19 Government policy stringency index, a composite index based on 9 response measures, illustrates that many countries, including Luxembourg, adopted similar measures during this period (Hale et al., 2021). These restrictions were often enforced at regional or local levels, triggered by the number of cases reported in specific areas. To capture these more regional effects of the pandemic in addition to nation restrictions captured by the index, we use data on confirmed COVID-19 cases as a proxy for various policy responses and reduced mobility.

This data is collected and reported by the COVID-19 European Regional Tracker at the NUTS 3 level (Naqvi, 2021). Information on the number of confirmed cases is taken from each country's official institutions responsible for providing COVID-19 related data. The regional data is then aggregated up to the country level and cross-checked against data from OWID, which provides confirmed COVID-19 cases at the country level (Mathieu et al., 2020). The data matches well for 2020 and 2021.

Data quality, however, deteriorates in 2022, because the number of countries regularly reporting cases decreases strongly in 2022. The COVID-19 European Regional Tracker reports cases for all regions that we consider in our study, except for Luxembourg. However, since the regional data is validated against the OWID data and matches well for our sample period, we resort to COVID-19 cases from OWID for Luxembourg. For our analysis, we aggregate the NUTS 3 level data in the COVID-19 European Regional Tracker to the NUTS 2 level.

Figure 3: Regional variation in COVID-19 cases for 2020 and 2021



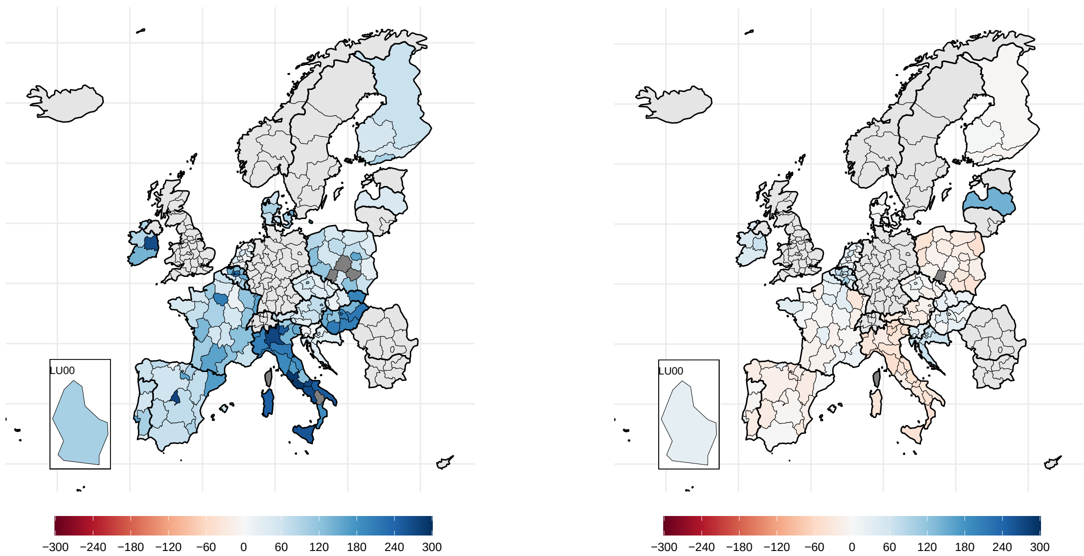
Note: The average daily confirmed COVID-19 cases and their spatial distribution across countries for 2020 and 2021. Data for Luxembourg is from Our Wold in Data (OWID), while data for NUTS 2 regions in other countries is taken from the COVID-19 European Regional Tracker (Naqvi, 2021).

Figure 3 shows the average regional variation in the number of confirmed daily COVID-19 cases per 10,000 persons for 2020 and 2021. Dots represent the mean of confirmed cases at the NUTS 0 level (i.e., country level), the downward-facing triangle represents the NUTS 2 region with the lowest and the upward-facing triangle the region with the highest number of confirmed cases per 10,000 persons within a country. The distance between these two points spans the spatial variation across NUTS 2 regions within a country. It is evident that this spatial variation is significant, which further motivates the choice to conduct our study at a regional level compared to the country level.

Overall, the number of cases per 10,000 persons as well as their spatial variation is smaller in 2020 compared to 2021. Countries with a larger population also tend to show a bigger variation in cases across their regions. Luxembourg does not show any regional variation because its NUTS 0 and NUTS 2 regional boundaries are identical. Average daily cases per 10,000 persons for Luxembourg in 2020 and 2021 are around 600 and 900, respectively. In 2020, this puts Luxembourg at the higher end of the spectrum of regional cases per 10,000 persons, while it puts it on the lower end in 2021. Compared to country averages, we find only few comparable units to Luxembourg. At the regional level, however, we find several regions with more cases in 2020 and fewer ones in 2021, further motivating our usage of regional data.

Figure 4: Change (%) in persons usually working from home for NUTS 2 regions

(a) 2019-2020 **(b)** 2020-2021



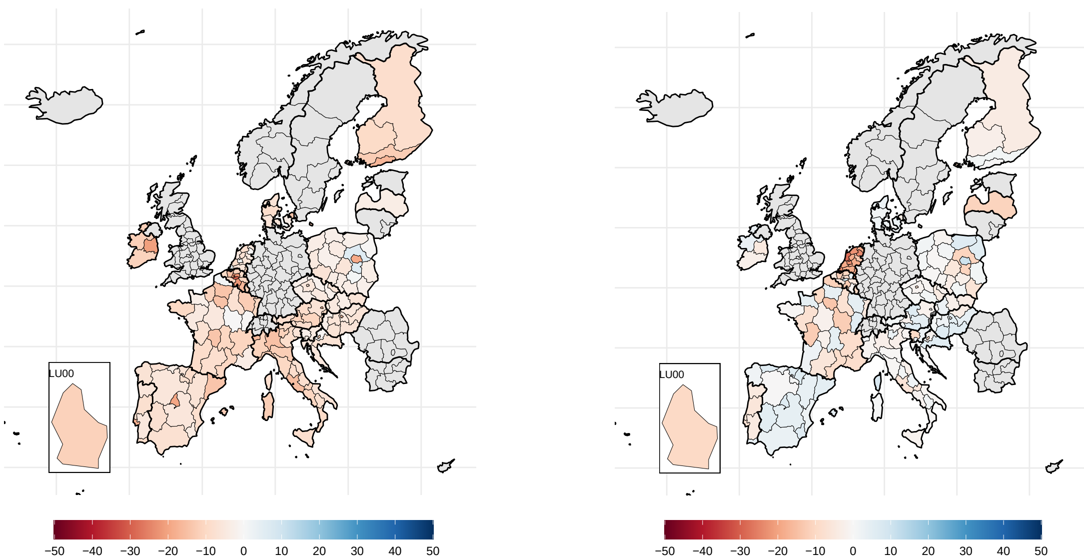
Note: Data is from a special extraction from the EU-LFS. Persons usually working from home with workplace at the NUTS 2 region shown in the figure and their location of residence in the associated country of the region.

We use data on working from home and commuting inflow to further address changes in mobility behavior as a response to the pandemic. A person is classified as usually working from home when they were working at home half of the days that they worked in a reference period of four weeks preceding the end of the reference week in the EU-LFS survey. We focus on persons usually working at home with their workplace location in the associated NUTS 2 region and their location of residence within the same country.[17]

However, this dataset does not capture commuting patterns across regions, which seems particularly important for Luxembourg, which traditionally experiences a large commuting inflow. To get a more complete picture of changes in mobility behavior with respect to work, we consider persons never working from home at a regional level. This category captures all persons commuting to work irrespective of their location of residence and thus incorporates commuting inflow from other regions and countries.

Figure 5: Change (%) of persons never working from home for NUTS 2 regions

(a) 2019-2020 **(b)** 2020-2021



Note: Data is from a special extraction from the EU-LFS. The figure shows yearly changes of persons never working at home for NUTS 2 regions which are the location of the workplace of these persons irrespective of their location of residence.

Figure 4 shows yearly changes of persons usually working from home for NUTS 2 regions. Figure 4a shows the change from 2019-2020, i.e., the immediate effect of the pandemic. Blue indicates an increase in working from home, whereas red indicates a decrease. As expected, almost all regions experienced an increase in people working from home. The figure zooms in on Luxembourg, which also experienced an increase, but notice that the change is not particularly strong relative to other regions, i.e., Luxembourg is not an outlier. In Luxembourg, the change of people usually working from home from 2019-2020 almost doubled at around +98%. Figure 4b shows the change from 2020-2021. The map now shows a more nuanced picture. Some regions experienced a decrease in working from home, while some experienced another increase. Luxembourg is among the latter group and experienced a change of around +28%.

Figure 5 shows yearly changes of persons never working at home for NUTS 2 regions. Figure 5a shows percentage changes from 2020 to 2021. Overall, the map shows a de-

[17]Ideally, we would want to focus on persons working and living in the same NUTS 2 region. However, this is not available in the EU-LFS data structure.

crease in persons never working from home, i.e. a decrease in commuters. This is to be expected since the pandemic caused an increase in working from home in most regions. Figure 5b shows percentage changes from 2020-2021 and shows a mixed picture. Some regions experienced a further decrease in persons never working from home, while others experienced an increase following the first year of the pandemic. Luxembourg experienced a decrease in 2019-2020 and 2020-2021 of -12% and -10%, respectively. Again, Luxembourg does not appear to have experienced a particularly strong change relative to other countries.

4 Identification strategy

The inability to directly observe the potential outcomes of a specific unit both in the presence and in the absence of a policy event (treatment) complicates establishing causal relationships. In the case of Luxembourg, this translates to 'what would the emissions from road transport have been if the free public transport policy had not been introduced?" To overcome this problem, it is necessary to design an appropriate identification strategy that constructs a credible comparison group to serve as a counterfactual for Luxembourg after the policy's introduction.

Given that Luxembourg differs significantly from other EU countries in observable characteristics such as emissions per capita, GDP per capita, and motorization rates (refer to Section 2), we conduct our analysis at the NUTS 2 level. This approach is feasible because Luxembourg itself constitutes a NUTS 2 region, and it is likely that we can find more comparable units to construct the counterfactual for Luxembourg at the NUTS 2 regional level than at the country level. However, even at this regional level, Luxembourg records the highest per capita CO_2 emissions from road transport. We therefore need an estimation strategy that can handle these complexities in our setting.

The canonical DID estimator calculates the difference in outcomes over time between treated and control units and relies on the parallel trends assumption. This assumption implies that, in the absence of treatment, the treated and control groups would have followed similar trends over time. By assuming parallel trends, the DID estimator controls for unobserved characteristics that remain constant over time, which might otherwise confound the results. Additionally, the DID method assumes that any time-varying shocks affecting the outcome are common to both treated and control groups, thereby isolating the treatment effect. However, the parallel trends assumption is often untestable, and in our specific setting, where Luxembourg already exhibits considerable differences in observable characteristics, we have reduced confidence that this assumption holds.

Some drawbacks of the DID method can be mitigated by the Synthetic Control (SC) method, which does not rely on the parallel trends assumption. Instead, the SC method creates a synthetic control unit as a weighted combination of units from the donor pool, ensuring that the pre-intervention outcomes of the synthetic unit closely match those of the treated unit. Importantly, not all units in the donor pool receive equal weights; higher weights are assigned to regions that are more similar to Luxembourg based on predictors of the outcome (Abadie, 2021).

The validity of the SC method depends on the trajectory of the outcome variable of the SC closely following that of the treated unit over a long pre-intervention period. This close alignment lends confidence that any deviations in outcome trends after the intervention can be attributed to the policy intervention. However, the substantial differences in predictors of emissions between Luxembourg and other units, coupled with Luxembourg's status as the country and even the NUTS 2 region with the highest per capita emissions, challenge the applicability of this method in our context.

Therefore, we employ the recently proposed estimation procedure, the SDID approach introduced by Arkhangelsky et al. (2021). SDID combines the strengths of both DID and SC methods and circumvents the common drawbacks associated with traditional DID and SC methods. Specifically, it overcomes the challenge of estimating causal relationships when parallel trends are unlikely to hold in aggregate data for DID and eliminates the necessity for the treated unit to be within the convex hull of control units for SC. SDID essentially constructs a synthetic parallel trend for Luxembourg. Section 5 discusses the SDID estimation procedure in detail.

Identification is further complicated by the COVID-19 pandemic, which coincides with the policy's introduction. Since the pandemic was a global shock affecting all regions, its effects should bias an unconditional analysis as far as Luxembourg was hit differently by the pandemic relative to other regions.

Regions adopted varying measures and policies to limit the spread of the virus, which could have differential impacts on mobility across regions. For instance, a higher number of COVID-19 cases may lead to shifts toward remote working, online education, and changes in consumer behavior. These policy responses, potentially influenced by the number of cases, could correlate with regional mobility restrictions.

To account for these factors, we control for COVID-related restriction based on the OWID COVID-19 Government policy stringency index at a national level. However, these restrictions were often enforced at regional or local levels, triggered by the number of cases reported in specific areas. To capture these regional-specific effect of the pandemic, we in addition use data on confirmed COVID-19 cases as a proxy for various policy responses and reduced mobility.

Even conditional on pandemic-related restrictions, mobility patterns may have also shifted. This is again only problematic insofar as regions experienced such shifts differently form one another. These changes include individuals who did not work from home prior to the pandemic but began and continued doing so after the COVID-19 outbreak. Consequently, mobility within countries (and within regions) and commuting patterns across borders might have changed. However, as discussed in detail in Section 3.2, Luxembourg did not experience particularly significant changes relative to other regions. This mitigates the associated threat to identification. We nevertheless control for these factors in our empirical strategy to capture any residual confounding effects. In particular, these include measures of working from home and commuting inflow. As Luxembourg traditionally experienced lower fuel prices compared to its neighboring regions, changes in commuting have a particularly strong effect on fuel-based emissions in Luxembourg. To account for this, we interact the relative fuel prices with cross-border commuting inflows (see Section 6.

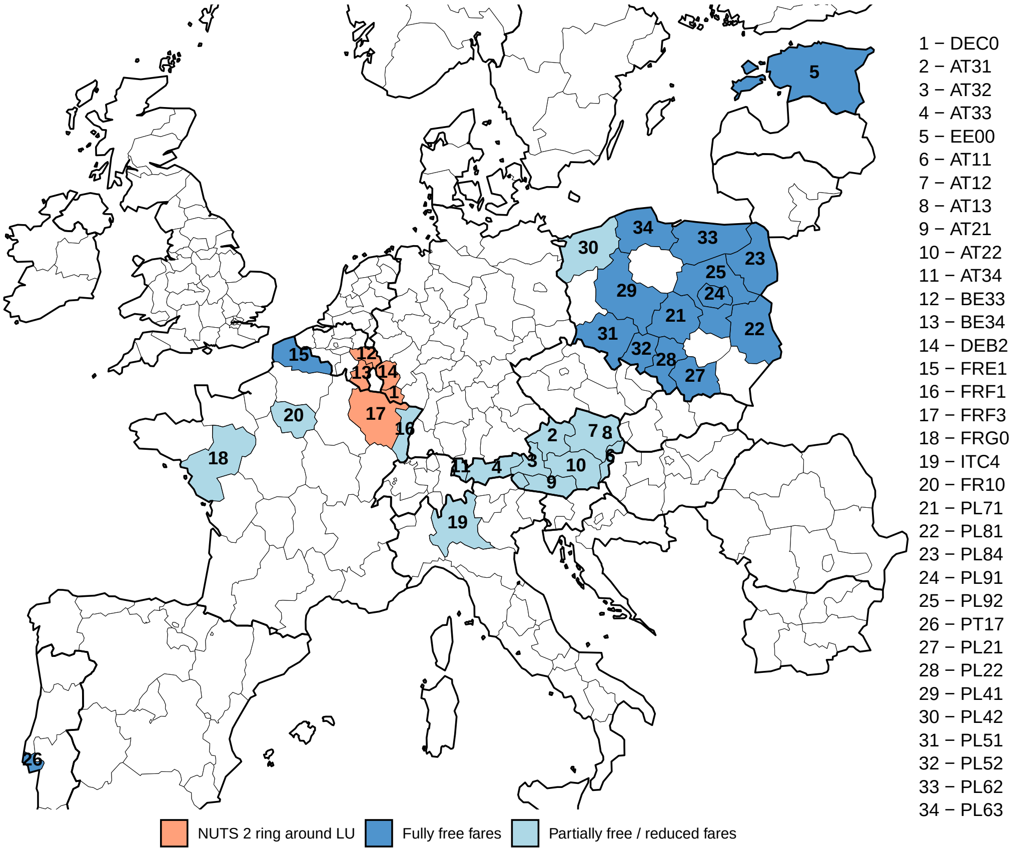
Finally, to avoid bad comparisons with already treated units, we excluded NUTS 2 regions that introduced free fares during our sample period. We drop the following regions before estimating our main results. Estonia (EE) introduced free public transport in Tallin in 2013 and further extended it in 2017. Given that Estonia is in itself a NUTS 2 region, we drop the whole country. Dunkirk and Calais in France introduced free public transport for all passengers in 2018 and 2020, respectively. Both are located within the same NUTS 2 region (FRE1) that we drop. We also drop Cascais in Portugal (PT17), which introduced free fares in 2020.

Several municipalities in Poland introduced some form of free public transport schemes during our sample period. Štraub et al. (2023) chart the spatial distribution of these policies in Poland, which covers over 90 free-fare programs since 2007. Polish municipalities that introduced free fares for everybody during our sample period cover 12 NUTS 2 regions which we drop (PL21, PL22, PL41, PL51, PL52, PL62, PL63, PL71, PL81, PL84, PL91, PL92). We also exclude the NUTS 2 regions surrounding Luxembourg to control for possible spillover effects. These regions include the Province of Luxembourg (BE34) and the Province of Liege (BE33) in Belgium, Trier (DEB2), and Saarland (DCE0) in Germany, and Lorraine in France (FRF3).

Regions that introduced substantial fare reductions without fully abolishing fares are partially treated, which compromises the purity of the control group and, in turn, threatens identification. We therefore additionally drop regions that introduced free fares for specific groups (e.g., students, residents, elderly, etc.) or subsidized public transport during our sample period. These cases can distort the estimated effect if these policies significantly shifted the modal split in favor of public transport systems. Regions we drop in our robustness checks include the following. Attica in Greece (EL30), and Nantes (FRG0), Strasbourg (FRF1), and Paris (FR10) in France. These regions all introduced some form of free public transport for residents and/or students (Fre, 2024). Austria (AT) introduced a nationwide climate ticket for all public transport modes in 2021. This increased accessibility and significantly reduced prices for comparable tickets prior to the policy introduction.

The different regions that we drop are shown in Figure 6. The figure zooms in on

Figure 6: NUTS 2 regions - bad controls



Note: NUTS 2 regions that are potential bad control are highlighted. A NUTS 2 ring around Luxembourg in orange, regions that introduced free fares during our sample period in dark blue and regions that introduced reduced fares or partially free public transport in light blue.

NUTS 2 regions in Europe to highlight potentially bad controls. NUTS 2 regions that introduced free fares for all passengers during our sample period are shown in darker blue. Those that introduced free fares for specific groups only or introduced reduced fares are shown in lighter blue. The NUTS 2 ring around Luxembourg is shown in orange and is dropped in all specifications.

Finally, we exclude regions that are not predominantly urban, as these are structurally very different from Luxembourg in ways that are directly relevant for transport-related emissions (e.g., population density, trip lengths, public transport supply, commuting, . . .). Restricting the analysis to urban regions improves comparability between Luxembourg and the potential donor pool. The final donor pool used for the emissions-based analyses is shown in Figure B.1 in Appendix B, which displays the weights for each donor region for the CO_2 specification.

5 Synthetic difference-in-differences (SDID)

We use the SDID methodology to estimate the impact of Luxembourg's free public transport policy on CO_2 emissions from road transport. The analysis covers a sample period from 2016 to 2022. As the policy is implemented in 2020, the analysis includes four years before the policy is introduced and two years after, which allows for a comparative analysis of the pre- and post-policy effects. Schenk (2023) shows that the SDID estimator performs remarkably well in short T panels, is able to handle interactive fixed-effects that can influence the outcome, and provides conservative standard errors. Considering the few pre- and post-treatment periods in our sample, this reassures us that the applied methodology is consistent under our setting.

The SDID estimator aims to consistently estimate an ATT without relying on parallel pre-treatment trends between treated and not-treated units. In essence, SDID estimates the ATT, $\widehat{\tau}^{sdid}$, from a weighted two-way fixed-effects regression. Compared to SDID, DID approaches use an unweighted two-way fixed-effects regression, thus relying on parrallel pre-treatment trends in aggregate data. SC relaxes this requirement but uses only unit-specific weights and does not explicitly weigh time periods optimally. Contrary to SC method, SDID additionally allows for level differences between treatment and synthetic control units in estimating optimal weights. Following this rationale, Arkhangelsky et al. (2021) argue that SDID is more flexible compared to DID and SC methods.

The SDID-ATT is estimated by:

$$\left(\widehat{\tau}^{sdid}, \widehat{\mu}, \widehat{\alpha}, \widehat{\beta}\right) = \underset{\tau,\mu,\alpha,\beta}{\arg\min} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau)^2 \widehat{\omega}_i^{sdid} \widehat{\lambda}_t^{sdid} \right\}, \tag{1}$$

where the outcome of interest, Y_{it}, is observed for each unit i at each time t, with $i = 1, ..., N$ and $t = 1, ..., T$. W_{it}, indicates treatment, with $W_{it} = 1$ if unit i is treated at time t and $W_{it} = 0$ else. μ is an intercept, α_i and β_t are unit and time fixed-effects, respectively. $\widehat{\omega}_i^{sdid}$ and $\widehat{\lambda}_t^{sdid}$ are unit and time weights, respectively.

Unit weights are computed to align pre-treatments trends between treated and control units:

$$\left(\widehat{\omega}_0, \widehat{\omega}^{sdid}\right) = \underset{\omega_0 \in \mathbb{R}, \omega \in \Omega}{\arg\min} \sum_{t=1}^{T_{pre}} \left(\omega_0 + \sum_{i=1}^{N_{co}} \omega_i Y_{it} - \frac{1}{N_{tr}} \sum_{i=N_{co}+1}^{N} Y_{it} \right)^2 + \zeta^2 T_{pre} ||\omega||_2^2, \tag{2}$$

with $\Omega = \{\omega \in \mathbb{R}_+^N,\ with\ \sum_{i=1}^{N_{co}} \omega_i = 1\ and\ \omega_i = 1/N_{tr}\ \forall\ i = N_{co}+1, ..., N\}$, where $||\omega||_2$ is the Euclidean norm and $\mathbb{R}_+$ denotes the positive real line. N_{co} and N_{tr} are the number of untreated and treated units, respectively. Similarly, T_{pre} is the number of pre-treatment periods. ζ is a regularization parameter to increase dispersion and ensure unique weights, it is defined in Arkhangelsky et al. (2021). Contrary to traditional synthetic control unit weights, these SDID weights do not aim to find comparable regions in absolute terms conditional on covariates, but the procedure rather assigns weights to align pre-treatment trends in the (adjusted) outcome.

Time weights are computed to align pre- and post-treatment periods for untreated units:

$$\left(\widehat{\lambda}_0, \widehat{\lambda}^{sdid}\right) = \underset{\lambda_0 \in \mathbb{R}, \lambda \in \Lambda}{\arg\min} \sum_{i=1}^{N_{co}} \left(\lambda_0 + \sum_{t=1}^{T_{pre}} \lambda_t Y_{it} - \frac{1}{T_{post}} \sum_{t=T_{pre}+1}^{T} Y_{it} \right)^2 + \zeta^2 N_{co} ||\lambda||^2, \tag{3}$$

with $\Lambda = \{\lambda \in \mathbb{R}_+^T, \ with \ \sum_{t=1}^{T_{pre}} \lambda_t = 1 \ and \ \lambda_t = 1/T_{post} \ \forall \ t = T_{pre}+1, ..., T\}$, where the regularization term ensures unique weights and is very small.

5.1 Handling covariates

We follow the procedure for handling covariates outlined in Arkhangelsky et al. (2021) and refined in Clarke et al. (2023).

Handling covariates in this setting is treated as a pre-modeling approach, in which the outcome variable is adjusted by covariates before estimation. The procedure does not put any stationarity requirements on the covariates, i.e., they can be time-varying. This adjustment procedure contains two steps. In the first step, we estimate the coefficients of the covariates. To obtain estimates that are unconfounded by the treatment itself, we follow Kranz (2022) and exclude the treated unit in the estimation. This allows us to run the following model on the entire sample period (2016–2022):

$$Y_{it}^{co} = \alpha_i + \gamma_t + X_{it}^{co}\beta + u_{it}, \tag{4}$$

where the super-script *co* indicates control units, Y_{it}^{co} measures emissions from road transport, X_{it}^{co} collects covariates and may include daily COVID cases, the COVID stringency index, and the number of persons working from home, commuting inflows, fuel prices as well as relative fuel prices and their interaction with cross-border commuting inflows, freight transportation, and GDP per capita. To capture differences between regions and time, we can include region-specific effects, α_i, and time-specific effects, γ_t. In a second step, we adjust the outcome variable for the aforementioned effects for all units:

$$\widehat{Y}_{it}^{adj} = Y_{it} - X_{it}\hat{\beta}. \tag{5}$$

Finally, the SDID procedure is then applied to the adjusted outcome variable.

5.2 Placebo inference and event-study analysis

Arkhangelsky et al. (2021) show that the estimated ATT, $\widehat{\tau}^{sdid}$, is asymptotically normal. This means that conventional confidence intervals can be used to conduct asymptotically valid inference if the asymptotic variance, $\widehat{V}_\tau$, can be consistently estimated: $\tau \in \widehat{\tau}^{sdid} \pm z_{\alpha/2}\sqrt{\widehat{V}_\tau}$. Arkhangelsky et al. (2021) propose several estimators for the asymptotic variance (bootstrap, jackknife, placebo). But in cases where there is only one treated unit (i.e., $N_{tr} = 1$), only placebo estimates are well defined. The idea of this procedure is to replace the exposed unit with unexposed units, then randomly assign those units to a placebo treatment and compute a placebo ATT. This is repeated many times to obtain a vector of placebo ATTs. The variance of this vector can then be used to obtain an estimate for the asymptotic variance.

To evaluate the robustness of the results, we perform an event-study analysis, which enables us to study the dynamics of the policy effect and allow us to evaluate the credibility of pre-treatment parallel trends. We follow the discussion in Clarke et al. (2023) on how to compute these estimates manually. In principle, we want to estimate the differences in the outcome variable between treated and the non-treated synthetic control region for each time period t. This allows us to evaluate parallel pre-treatment trends by studying whether these differences changed over time prior to the policy adoption. Additionally, we can study the evolution of the treatment over each post-treatment period.

The difference at each time period t is denoted as d_t and given by:

$$d_t = (\bar{Y}_t^1 - \bar{Y}_t^0) - (\bar{Y}_{base}^1 - \bar{Y}_{base}^0), \tag{6}$$

where 1 indicates a treated unit and 0 the non-treated synthetic control unit. The first term in brackets calculates the difference in mean CO_2 emissions at time period t for treated and control units. The second term in brackets captures the difference between the pre-treatment baseline means of these units. The baseline outcomes are weighted aggregates over pre-treatment periods rather than arbitrarily chosen time periods (as is usually done in DID applications). They are given by:

$$\bar{Y}_{base}^1 = \sum_{t=1}^{T_{pre}} \hat{\lambda}_t^{sdid} \bar{Y}_t^1 \quad and \quad \bar{Y}_{base}^0 = \sum_{t=1}^{T_{pre}} \hat{\lambda}_t^{sdid} \bar{Y}_t^0,$$

where the time weights, $\hat{\lambda}_t^{sdid}$, come from equation (3).

Confidence bands around the estimated d_t's are generated with a placebo-based approach in the following sequence: (i) Exclude the treated unit (in our case Luxembourg) from the sample; (ii) Randomly assign treatment to a unit (from the remaining units, which are all controls units); (iii) Calculate the outcome adjusted for covariates following equations (4) and (5); (iv) Compute equation (6) and store the result; (v) Repeat 2-4 many times (e.g., 1,000 times); and (vi) Obtain the 5% quantile from the sample distribution of the stored results for each time period t.

6 Results

This section reports our main results. Our model specifications are outlined in Section 6. We provide results for three different emission types: CO_2, overall GHG, and NOx. All three adjust the outcome variable for COVID-19-related covariates as described in Section 5.1. The outcome variables are measured as log emissions per capita, i.e., $y_{it} \in \log(\text{CO2/cap})_{it}, , \log(\text{NO}x/\text{cap})_{it}, , \log(\text{GHG/cap})_{it}$.

We use the inverse hyperbolic sine transformation on covariates that include zero-values because the natural logarithm of zero is undefined and the transformation approaches the natural log. This allows us to interpret the estimated coefficients as elasticities under certain assumptions.[18] The model is given by:

$$log(y)_{it} = \alpha_i + \gamma_t + \mathbf{x}'_{it}\boldsymbol{\beta} + u_{it}, \tag{7}$$

where $log(y)_{it}$ is the outcome, which is either log of CO_2, log of NOx or over log of GHG emissions in per capita terms for NUTS 2 region i in year-month t. α_i and γ_t denote region and year-month time fixed effects, respectively. The covariate vector $\mathbf{x}_{it}$ includes the inverse hyperbolic sine of COVID cases, the COVID stringency index and measures of working from home based on the work-place location in the associated NUTS 2 region. We additionally consider the log of real GDP per capita, energy intensity, real diesel and petrol prices in logs (adjusted with the harmonized index of consumer prices) to capture cross-unit variations in fuel prices, and the log of freight transport, measured as tons of goods loaded in the region, to control for changes in freight transport.

Finally, we allow fuel price responses to vary with cross-border commuting to capture fuel tourism effects. We proxy spatial price differences by relative fuel prices, calculated as the fuel price of a NUTS 2 region relative to the mean of its neighbours that are not part of the same country. We focus on diesel prices, as these are most relevant for commuting trips. Taking the log relative diesel price, 0 indicates price parity, negative values indicate relatively cheaper fuel. As such differentials are particularly relevant for commuters who can refuel en route to or from Luxembourg, where fuel prices are typically lower. We therefore control for cross-border commuter inflows via two indicators for regions below versus above the 75th percentile of the inflow distribution (excluding Luxembourg). We then interact these indicators with the relative price measure. Estimation results for the auxiliary regressions based on Specification (7) are shown in Table A.1 in Appendix A.

Figure C.4 in Appendix C compares both absolute and relative fuel prices between Luxembourg and its neighboring regions. Up to 2021, Luxembourg's absolute fuel prices are consistently lower than those of its neighbours, resulting in relative prices below one. The estimated ATT is based on a comparison between weighted averages of the pre-and post-treatment periods. As shown in Table C.1 in Appendix C, there is some difference between these weighted averages for diesel and petrol prices in Luxembourg relative to its neighbors.

We provide estimates of the ATTs for the post-treatment periods but excluding the immediate COVID year 2020, which was a period of little to no mobility, and thus would not contribute much to our identification. Our post period therefore encompasses 2021-2022. Estimates for the ATTs are shown in Figure 7 and the event-study estimates are

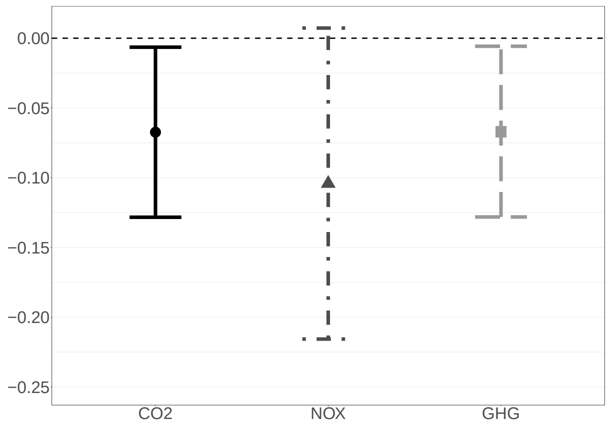
[18] As suggested by Bellemare and Wichman (2020), we multiply these covariates by a constant to generate average values greater than 10, which provides stable elasticities.

shown in Figure 8. Time weights for the CO2 specification are assigned to 2016 with 0.458, 2017 with 0.78 and to 2018 with 0.465. Figure 8 shows no statistically significant violation of pre-treatment trends.

The estimated ATTs for CO_2 indicate an effect at around -0.067, i.e., a 6.7% reduction in transport CO_2 emissions as a response to the free-public transport policy implemented in March 2020. We find similar results for overall GHG emissions with an ATT of again around -0.067. The estimates are statistically significant at the 5% significance level. For NO_x we find a larger ATT of around -0.10. But this effect is only marginally statistically significant at the 10% level.

The event-study analyses show no violation of parallel pre-treatment trends for all specifications. This also indicates that the tram extension in 2017 did not significantly alter Luxembourg's emissions trajectory compared to our synthetic control. Post-treatment effects show statistical significance in 2020 for all three specifications. In 2021, the confidence intervals based the specifications that adjusts the outcome variable for all covariates slightly cross the dashed zero-line at the 5% significance level.

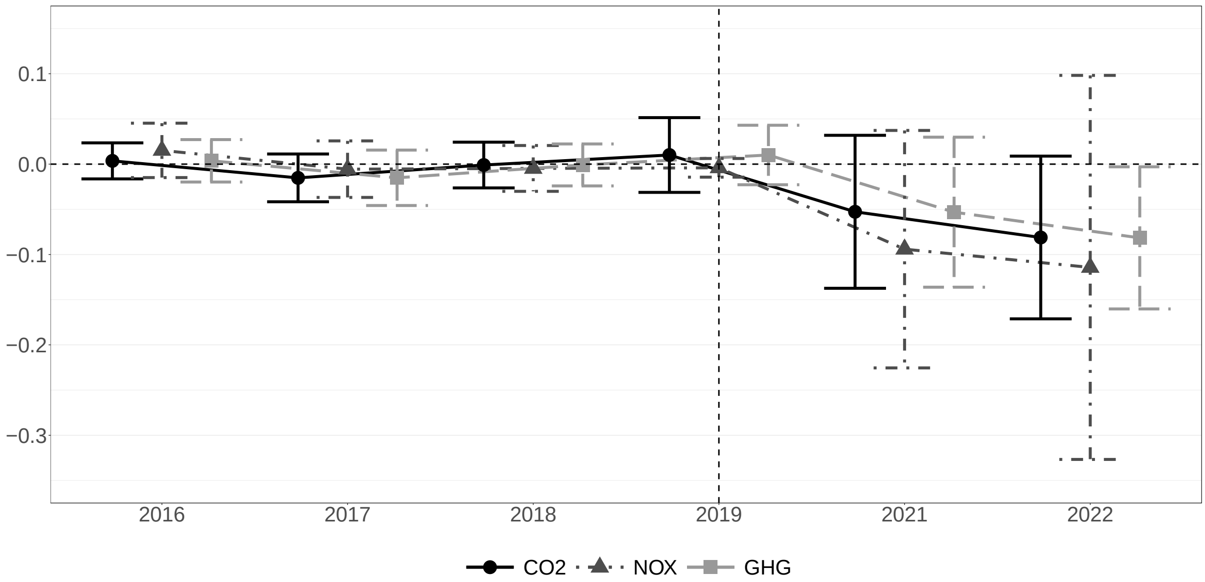
Figure 7: ATT estimates



Note: This figure presents the ATT for the post-treatment period (2021–2022). Estimates reflect the impact of free public transport in Luxembourg on per capita emissions of CO_2, NO_x, and total GHGs. Markers indicate point estimates; vertical bars represent 95% confidence intervals constructed via a placebo-based permutations. The estimated ATTs are: -0.669 ($p = 0.03$) for CO_2, -0.104 ($p = 0.07$) for NO_x, and -0.0674 ($p = 0.03$) for GHGs.

The control units that contribute to the synthetic control together with their respective weights for the third specification are graphically shown in Figure B.1 in Appendix B. The regions with the largest weights come from Belgium, Finland, Italy, and the Netherlands. In addition, Denmark, Spain, and Slovakia receive larger weights. Belgium, Denmark, Finland, and the Netherlands are among the EU countries with the highest GDP per capita and thus most comparable to Luxembourg in this respect. While Italy is among EU countries with the highest motorization rate after Luxembourg. It is therefore quite reasonable that the regions contributing to the synthetic control are taken from

Figure 8: Event study estimates



Note: This figure plots event-study coefficients for the impact of free public transport in Luxembourg on three outcomes: per capita emissions of CO_2, NO_x, and total GHGs. Markers indicate point estimates; vertical bars represent 95% confidence intervals constructed using a placebo-based permutations. The year 2020 is excluded from the sample to mitigate confounding effects from the COVID-19 pandemic.

these countries.

Figure B.2 in Appendix B shows how well the SDID estimation aligns pre-treatment trends for Luxembourg and its synthetic control. Luxembourg is shown as a solid line and the weighted average across control regions according to the SDID unit weights as a dashed line. The figure also shows the average pre-treatment trend in the adjusted outcome variable over all regions and the unweighted average over regions that received a positive weight. Figure (a) shows the absolute level of trends, while Figure (b) standardizes the trends so that they are visually more easily comparable.[19] We can see from the standardized trends in part b of the figure that pre-treatment trends for Luxembourg and the average across all regions shows the biggest visual difference in trends. The unweighted average across regions that received a positive weight is a much better fit. The best fit seems to be between Luxembourg and the weighted average according to the SDID unit weights. This reinforces our argument that the SDID procedure is preferable over standard DID and SC methods.

[19] Standardization is performed by subtracting the mean and dividing by the standard deviation within each group.

7 Robustness

The credibility of the SDID estimator depends on its ability to reproduce a counterfactual outcome for Luxembourg in the absence of the free public transport policy. In this section, we conduct standard robustness tests commonly used for synthetic controls, an in-time placebo test, where the policy is backdated to a fictitious date, as well as leave-one-out placebo tests to assess the sensitivity of the synthetic control to the composition of the donor pool (Abadie, 2021). Finally, we apply the SDID method to CO_2 from energy use in the building sector to assess whether there was an effect attributable to COVID-19.

In-time placebo: We conduct an in-time placebo test (also referred to as a back-dating test), following Abadie (2021). Specifically, we assign the free public transport policy to 2019, one year prior to its actual implementation. Because the treatment is artificially backdated, we should not observe any significant post-placebo effects. Figure C.1 in Appendix C illustrates these results. Consistent with this expectation, the 95% confidence intervals for the 2019 "treatment" period include zero throughout. These null results further provide evidence that the public transportation expansions, notably the extension of the tram line, did not have generate a detectable effect in the absence of the free public transport policy.

Leave-one-out placebo: We use a donor pool of 46 NUTS 2 regions in our analysis. To assess the sensitivity of our results, we conduct a leave-one-out robustness check by iteratively excluding one region at a time, re-estimating the SDID model, and obtaining a distribution of ATT estimates. The resulting distribution is presented in Figure C.2a in Appendix C. The estimated ATTs from this exercise range from approximately -0.077 to -0.057, with our main estimate of -0.0669 positioned near the center of the distribution. These estimates are not statistically different from our main result, indicating that our findings are robust to the exclusion of individual regions from the donor pool.

Next, we extend this robustness check by iteratively excluding one country at a time. Since the 46 NUTS 2 regions in the donor pool in our sample span 13 countries, this approach removes multiple regions at once. The resulting ATT estimates are plotted in Figure C.2b in Appendix C. The most pronounced deviations occur when we exclude Italy (-0.05) and the Netherlands (-0.08). Dropping Italy removes 12 NUTS 2 regions, while excluding the Netherlands removes 6 NUTS 2 regions. Notably, both Italian and Dutch regions receive high weights in our main specification (see Appendix B.1). While this approach introduces greater sensitivity than the leave-one-region-out analysis due to the simultaneous removal of NUTS 2 regions, our baseline estimate of -0.0669 remains centrally located within the distribution. Notably, the distribution of estimates peaks to the left of our primary result, suggesting that, if anything, our primary findings are conservative.

Alternate donor pool: To test the sensitivity of our results to the donor pool composition, we re-estimate the model using a significantly larger sample of 100 NUTS 2 regions. Unlike our primary specification, which uses a narrowly tailored donor pool, this expanded sample only excludes regions comprised entirely of "predominantly rural" NUTS 3 areas. As shown in Figure C.3, the resulting estimates for per capita CO_2 (-0.126), NO_2 (-0.166), and GHGs (-0.126) are all statistically significant ($p < 0.01$) and larger in magnitude than our baseline findings. We view these results as highly reassuring for two reasons. First, the fact that the coefficients obtained from the broader donor pool exceed those from our primary specification indicates that our baseline estimates represent a conservative lower bound of the policy effect. Second, by including regions with a higher share of suburban NUTS 3 areas, the expanded sample incorporates units that typically exhibit greater car dependence and higher per capita transport emissions due to limited transit alternatives. In contrast, our primary specification restricts the donor pool to more urbanized regions, thereby providing a more stringent and credible pool to construct Luxembourg's counterfactual. The smaller effects observed in the main specification (-0.0669), relative to the broader sample (-0.126), suggest that our preferred estimation strategy avoids overstating the policy impact by benchmarking Luxembourg against comparable urban regions.

Other sectors: The SDID estimator inherently accounts for global trends by constructing a counterfactual from a donor pool of NUTS 2 regions that were also subject to pandemic-related disruptions. If the observed emission reductions were driven solely by the pandemic, synthetic Luxembourg would mirror the treated unit's decline. The differential effect we identify, therefore, represents the marginal impact of the fare-free policy beyond the shared "COVID-only" trajectory. To ensure that Luxembourg did not experience an idiosyncratic pandemic recovery compared to its peers (e.g., a more aggressive shift to remote work), we conduct two diagnostic tests. We apply the SDID estimator to log transformed per capita CO_2 emissions from the building sector—an area highly sensitive to pandemic-induced shifts in energy use but untreated by the transport policy.[20] We find a null effect (Appendix C, Figure C.5), suggesting that the pandemic's broader economic footprint is not driving our results. If COVID-19 were driving a broad reduction or increase in emissions, we would expect to see an effect in this sector as well. However, we find a null effect, suggesting that the reduction in road CO_2 is not merely a byproduct of the pandemic.

Next, we re-estimate the model using national all sector CO_2 emissions both including and excluding road transport. As shown in Table C.5, the treatment effect vanishes entirely when transport is removed. As shown in our event study estimates, we find statistically significant reductions in per capita all sector CO_2 emissions following the policy introduction. However, when road transport CO_2 emissions are omitted, the treatment

[20] Building emissions are defined following IPCC (1996) sector categories 1A4 and 1A5 (energy use in buildings) and are sourced from Crippa et al. (2025).

effect vanishes entirely; the coefficients for the remaining sectors are statistically indistinguishable from zero. This contrast indicates that unaffected sectors in Luxembourg do not experience any significant deviation from the synthetic counterfactual. Consequently, we can rule out broad-based macroeconomic shocks and localize the causal impact of the 2020 policy strictly within the transport sector.

8 Mechanisms

The previous section indicated that Luxembourg's free public transport policy reduced per capita CO_2, NO_x, and total GHG emissions. We now examine whether these results can be related to car usage. We move beyond aggregate per capita estimates and leverage rich administrative data from traffic monitoring stations. Specifically, we utilize monthly traffic counts from automatic road counters and station-level nitrogen dioxide (NO_2) concentrations. The higher temporal frequency of this data allows us to study behavioral responses and localized air quality shifts that can be obscured in annual aggregate statistics.

8.1 Administrative Traffic Counts

Data on traffic volumes comes from automatic road traffic counters on motorways and other major roads that record traffic flows by type of vehicle. We collect total daily counts for passenger-car-like vehicles from Luxembourg, Denmark, Finland, and the Netherlands. Data for Luxembourg is retrieved from Luxembourg's open data portal, which is maintained by the Administration des Ponts et Chaussées.[21] Counting data for Denmark is obtained via Dataudveksleren, Denmark's National Access Point (NAP) for mobility-related datasets.[22] The Dutch National Road Traffic Data Portal (NDW) provides count data for the Netherlands[23], and data for Finland originates from Fintraffic's Digitraffic Traffic Measurement System (TMS).[24]

These countries offer a dense counter network and provide good temporal coverage. We restrict the sample to stations that report full daily hours, cover at least 90% of days per year, and show no more than 14 consecutive missing days. Stations must satisfy these criteria throughout our sample period 2019–2023 (2019 being the earliest reported year for Denmark). The associated metadata allows us to aggregate stations to the NUTS 2 level and compute monthly means of total daily traffic volumes for carl-like vehicles. Figure 9 plots the remaining counter stations after our quality restrictions by country.

[21] `https://data.public.lu/en/datasets/pch-comptage-trafic/`
[22] `https://du-portal-ui.dataudveksler.app.vd.dk`
[23] `https://docs.ndw.nu/producten/snelhedenenintensiteiten/`
[24] `https://tie.digitraffic.fi/ui/tms/history/index_en.html`

NUTS 2 regions are highlighted by color. All regions show a relatively dense network of stations within all four countries.

Administrative traffic counter data is available for more recent years than the emissions series, which allows us to extend the pos-treatment period to 2023. A further advantage is that these counts are less directly affected by fuel tourism than emissions-based measures. We leverage this feature to estimate the synthetic control using pre-treatment outcomes only. To minimize COVID-related confounding, we exclude 2020 from March onward and additionally drop 2021 to remove potential remaining pandemic effects. The monthly aggregation mitigates short-term fluctuations such as holiday effects. We then estimate the policy effect by comparing Luxembourg to a synthetic version of itself using the SDID procedure.

Figure 12 shows the estimated overall ATTs using the log of monthly mean traffic counts as the outcome. Results are differentiated by day-of-week groupings. The overall effect is through Monday to Sunday and settles at around -7% but is statistically insignificant at the 5% level. The effect through Monday to Friday is similar in magnitude, whereas the weekend effect is more pronounced.

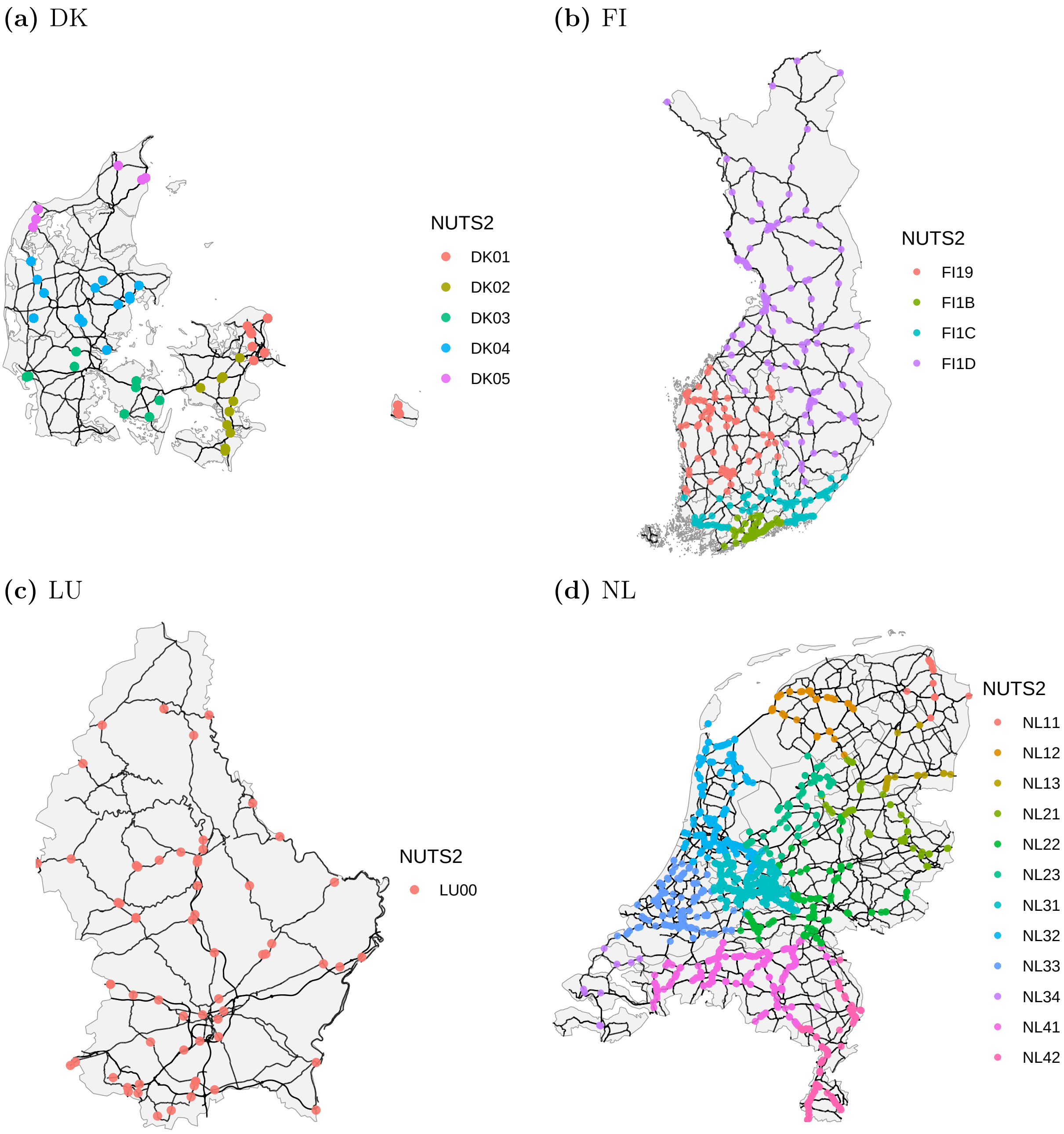
The corresponding event study is presented in Figure 11. We see an overall negative effect after February 2020. Although the effect is attenuated in the first three months of 2022, which we attribute to residual COVID-related effects that are not explicitly modeled in this specification. We find no evidence of significant pre-treatment trends. While the dynamic post-treatment effects are often borderline statistically significant, the individual point estimates remain largely below the weighted pre-treatment average. The series for Mo-Su and Mo-Fr are virtually indistinguishable, while the weekend series lies visibly lower, indicating a stronger effect on weekends.

In addition to the unconditional estimates, we include a specification with covariates to account for potential confounding beyond of COVID-related disruptions. In particular, these include fuel prices. Luxembourg temporarily reduced fuel prices by around 7.5 cent per liter through April to August 2022.[25] To account for such national fuel policy changes, we include the log of diesel and super prices in real terms. We also account for mean precipitation levels.[26] Conditioning on these covariates attenuates the estimated effects, although the qualitative pattern remains similar, with stronger effects on weekends, as shown in Figure 10. The ATT for Mo–Su changes to around -0.047, with a similar estimand for Mo–Fr. The ATT for weekends is around -0.07. Figure 13 presents the event-study results. The event-study results are similarly consistent with the uncondidtional specification, but feature significantly wider confidence intervals.

[25] `https://legilux.public.lu/eli/etat/leg/loi/2022/10/26/a534/jo`

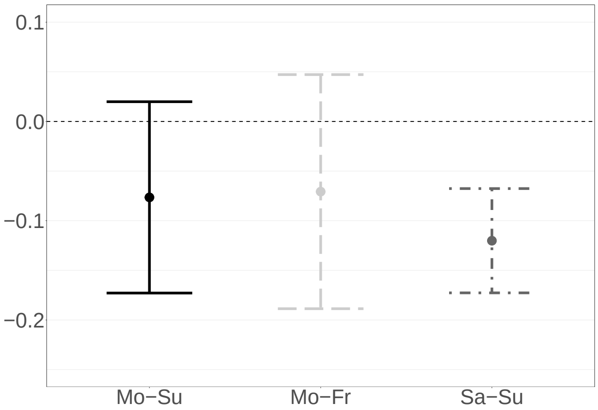
[26] Data for precipitation is obtained from ERAS 5 monthly data (Copernicus Climate Change Service, 2023)

Figure 9: Traffic count stations by country and NUTS 2 region



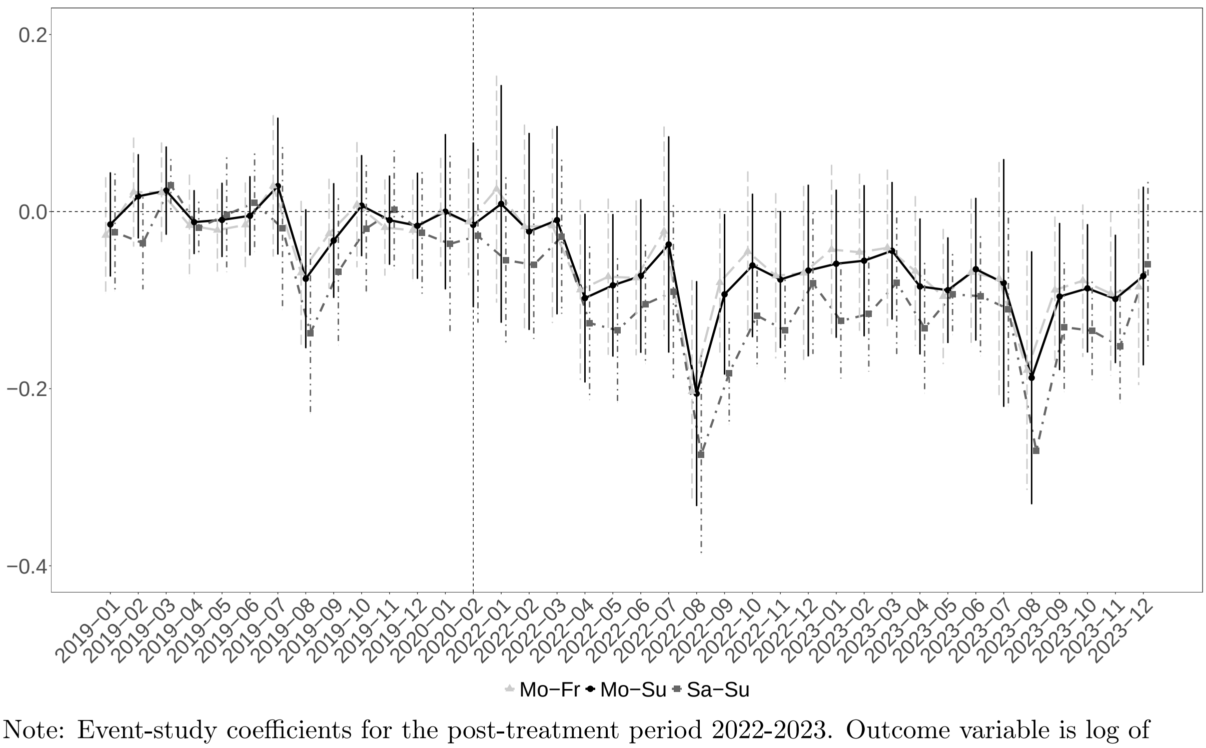
Note: Traffic count stations are for Denmark, Finland, Luxembourg, and the Netherlands. Stations are those on motorwayss and other major roads that report continuously from Jan 2019–Dec 2023 with a coverage of at least 90% of all days in a year and no more than 14 consecutive missing days. The stations are colored according to their NUTS 2 region.

Figure 10: ATT estimates



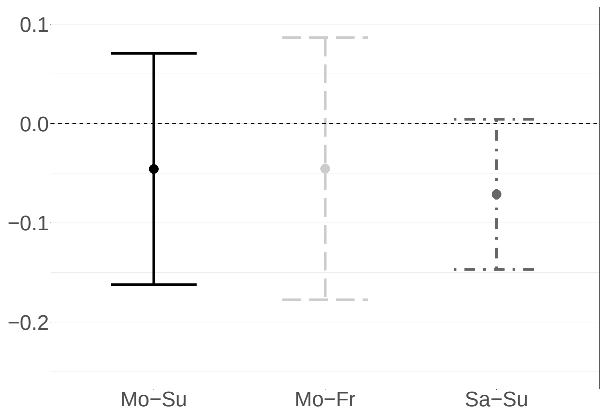
Note: ATTs for the post-treatment period 2022–2023. NUTS 2 regions come from DK, FI, LU, NL. Estimates reflect the impact of free public transport in Luxembourg on log traffic volume. Markers indicate point estimates; vertical bars represent 95% confidence intervals constructed via a placebo-based permutations.

Figure 11: Event study estimates for traffic counts



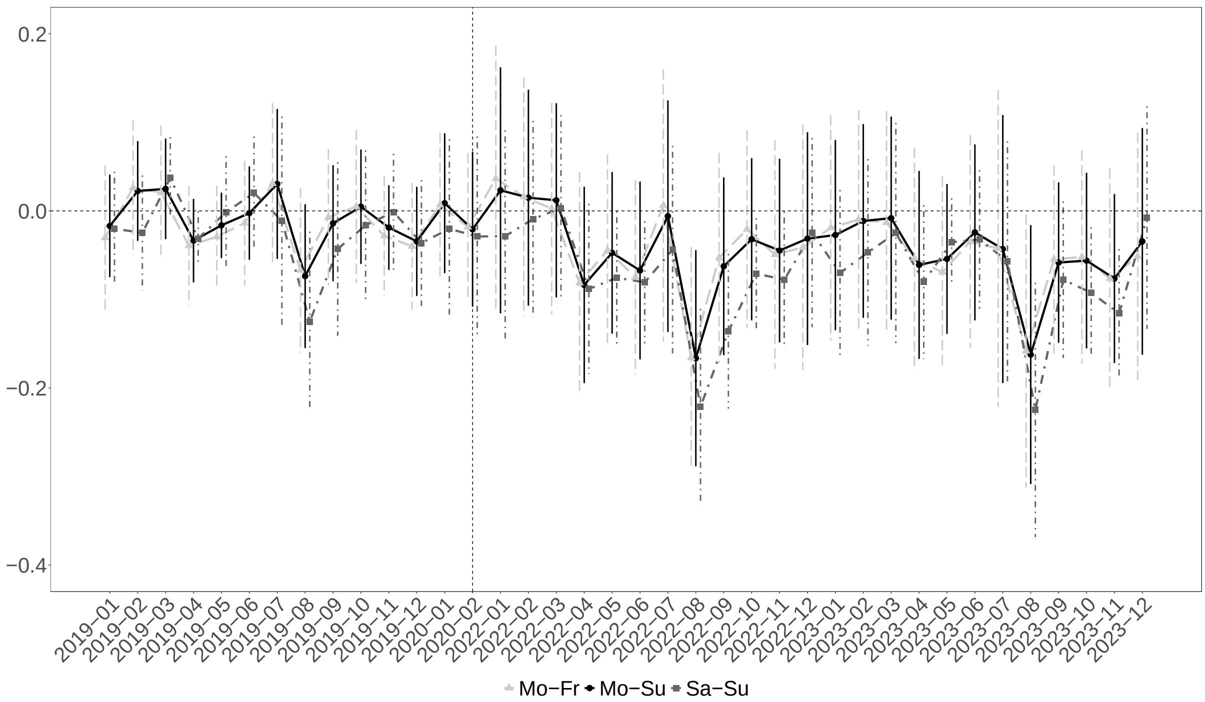
Note: Event-study coefficients for the post-treatment period 2022-2023. Outcome variable is log of monthly mean traffic counts for NUTS 2 regions. NUTS 2 regions come from DK, FI, LU, NL. Markers indicate point estimates; vertical bars represent 95% confidence intervals constructed using a placebo-based permutations.

Figure 12: ATT estimates - with covariates



Note: ATTs for the post-treatment period 2022–2023. NUTS 2 regions come from DK, FI, LU, NL. Estimates reflect the impact of free public transport in Luxembourg on log traffic volume. Markers indicate point estimates; vertical bars represent 95% confidence intervals constructed via a placebo-based permutations. Covariates include log of diesel and petrol prices in real terms and mean precipitation levels.

Figure 13: Event study estimates for traffic counts - with covariates



Note: Event-study coefficients for the post-treatment period 2022-2023. Outcome variable is log of monthly mean traffic counts for NUTS 2 regions. NUTS 2 regions come from DK, FI, LU, NL. Covariates include log of diesel and petrol prices in real terms and mean precipitation levels. Markers indicate point estimates; vertical bars represent 95% confidence intervals constructed using a placebo-based permutations.

8.2 Air quality data

To provide further corroboration of our estimated reductions in transport activity, we analyze ambient air pollution levels. We construct a panel of station-level (NO_2) concentrations (measured in micrograms per cubic meter) for the period 2018-–2023, utilizing hourly observations from the European Environment Agency (EEA) (European Environment Agency, 2024).[27] Our analysis focuses exclusively on monitors classified as traffic sites. These stations are situated in close proximity to major roadways and are uniquely positioned to capture the primary emissions most directly associated with road transport activity.

To mitigate potential bias from unbalanced panels or idiosyncratic sensor failures, we exclude stations with gaps in observations exceeding 14 consecutive days to ensure temporal continuity. We furhter restrict the sample to stations reporting valid data for at least 80 percent of the days in each year. From the remaining stations, we aggregate hourly measurements into daily means, retaining only those observations supported by at least 18 hours (75%) of valid data. To address remaining missing values, we employ a within-station imputation procedure: missing daily averages are replaced by the mean concentration of all available observations for the same weekday within that specific station-month-year cell. This procedure preserves the station-specific temporal profile while maximizing the available data for identification. Finally, we aggregate these daily values into monthly means, requiring a minimum 50% monthly coverage post-imputation

We aggregate these cleaned station-level observations to the NUTS 2 level for our primary analysis. For Luxembourg, the resulting regional average is constructed from three high-quality traffic monitors, all of which are situated in urban environments, with one located in Luxembourg City. We then perform SDID estimations on two distinct samples, both comprising 76 NUTS 2 regions in the donor pool [28]. The first sample utilizes all traffic stations satisfying our data-quality criteria to calculate regional averages. The second sample restricts the calculation of these regional averages exclusively to stations classified by the EEA as urban. This approach ensures that the averages for all 76 regions are derived from monitoring sites that share the same environmental classification as those in Luxembourg.

Finally, to prevent pandemic-related mobility restrictions from confounding our estimation, we omit the period from March 2020 through December 2021. This results in a final estimation sample consisting of 24 pretreatment months and 24 post-treatment months (comprising the full years of 2022 and 2023) [29]

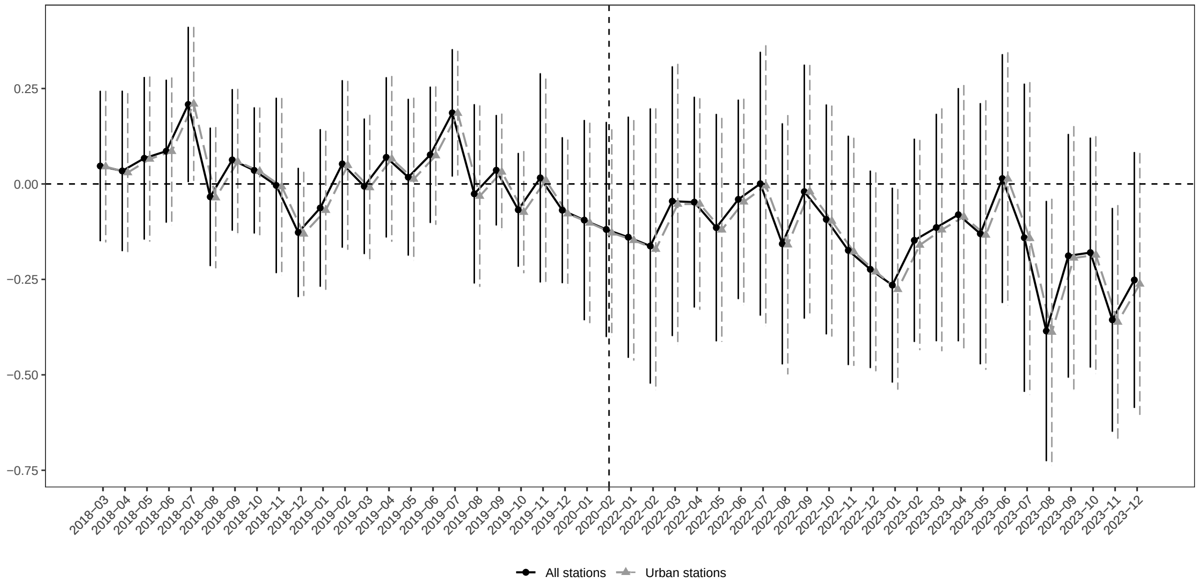
[27] We use the E1a-dataset which is the primary validated data provided by the EEA

[28] The donor NUTS 2 regions come from the following countries: Belgium, Czech republic, Denmark, Spain, Finland, France, Hungary, Italy, Lithuania, Netherlands, Sweden, Poland, and Slovakia. NUTS 2 regions that have introduced public transportation reforms have been excluded

[29] The pre-treatment period runs from March 2018 to February 2020. It begins in March 2018 to ensure that at least three stations are included when computing Luxembourg's monthly averages. Starting in

Figure 14 presents the event study results using the SDID estimator. These baseline specifications exclude additional covariates, allowing the estimator to rely solely on the optimal weighting of control units to isolate the treatment effect. The post-treatment dynamics indicate a consistent, albeit statistically noisy, improvement in air quality. In the sample with all stations, we find an ATT of -0.141 ($p = 0.154$), and a similar effect for the sample with only urban stations with an ATT of -0.146 ($p = 0.102$).

Figure 14: Event study estimates for (NO_2) - no covariates



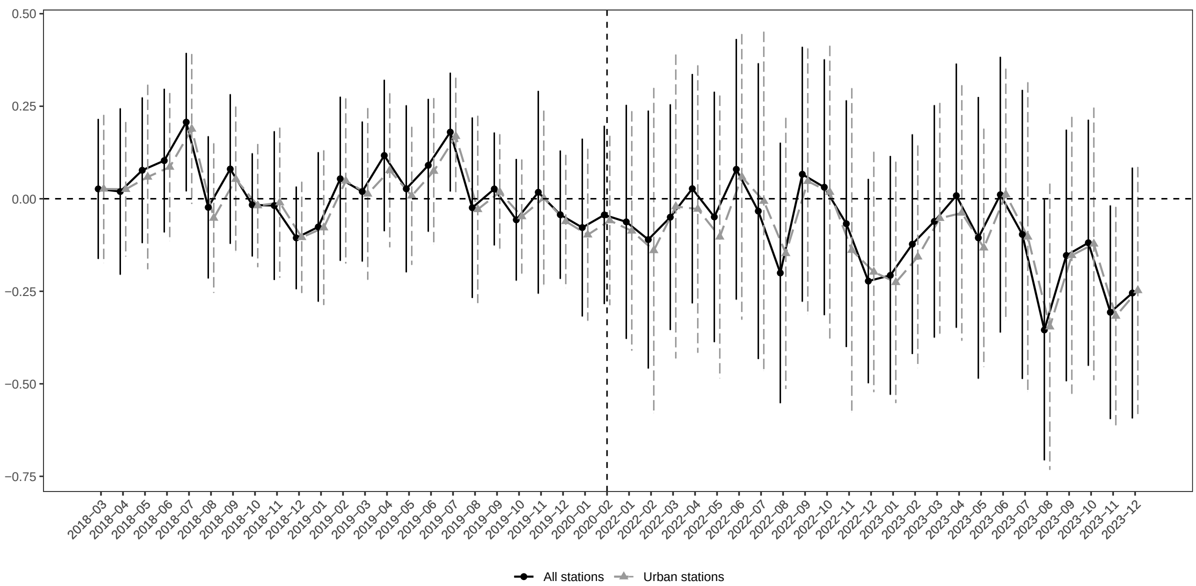
Note: Event-study coefficients for the post-treatment period 2022-2023. Outcome variable is log of monthly mean NO_2 for NUTS 2 regions. Markers indicate point estimates; vertical bars represent 95% confidence intervals constructed using a placebo-based permutations.

Analogous to the traffic count specification, we estimate a model with the same set of controls, the log of real diesel and petrol prices and mean precipitation. The results are shown in Figure 15. Over the post-treatment period, the estimated ATT for the all-stations sample is -0.095 ($p = 0.373$), while the corresponding estimate for urban stations is -0.107 ($p = 0.381$). The smaller estimated effect after adding covariates reflects the role of controlling for additional confounders. In particular, accounting for fuel prices and precipitation removes variation driven by rainfall and price-induced changes in vehicle travel that are unrelated to the policy.

Although NO_2 measurements are inherently noisy due to micro-climatic variation and sensor sensitivity—and noting that some NUTS 2 averages rely on a limited number of high-quality stations—the primary finding is the sustained downward trend revealed in the event study. This persistence suggests that the policy has induced a structural reduction in NO_2 levels, providing a consistent causal signal that remains visible despite significant atmospheric volatility.

January 2018, one station is dropped, which is why earlier months are excluded.

Figure 15: Event study estimates for (NO_2) -with covariates



Event-study coefficients for the post-treatment period 2022-2023. Outcome variable is log of monthly mean NO_2 concentration for NUTS 2 regions. Covariates include log of diesel and petrol prices in real terms and mean precipitation levels. Markers indicate point estimates; vertical bars represent 95% confidence intervals constructed using a placebo-based permutations.

9 Discussion and concluding remarks

Our estimated ATT for per capita CO_2 emissions from road transport is -6.7%. For overall GHG emissions from road transport we find a similar similar estimate, while we obtain a larger ATT for per capita road transport NO_x emissions at -10%. The larger ATT for NO_x relative to CO_2 is consistent with the technology-based emission factor approach utilized in the EDGAR road transport inventory (Lekaki et al., 2024). While CO_2 is primarily a function of total fuel consumption , NO_x emissions are highly sensitive to the specific vehicle technology mix and emission standards, such as the Euro 1–6 classifications. This discrepancy occurs because the underlying methodology captures how NO_x intensity varies significantly based on engine technology and real-world driving conditions. Since NO_x emissions are disproportionately higher for diesel vehicles and in congested traffic, reducing local commuting leads to a more pronounced percentage drop in air pollutants than in fuel-linked greenhouse gases (Anttila et al., 2011).

We complement these findings with analyses based on automatic road traffic counters. The estimated ATT for the unconditional specification is -7.6%, while the effect conditioned on covariates (fuel prices and precipitation) is around -4.6%. We find similar effects for traffic volumes through Monday–Friday, which indicates that the policy did have an effect on commuting behavior. However, both specifications show larger effects on weekends. While these estimates differ quantitatively, they provide a coherent qualitative picture of a negative policy effect, with somewhat more pronounced estimates in the emissions data relative to the traffic data. The stronger weekend effect suggests that the policy induced a larger modal shift for non-commuting trips. This interpretation is consistent with relatively persistent commuting behavior in Luxembourg, where commuters may be less responsive to public transport extension. Although the weaker effect on weekdays suggests that they are not entirely insensitive.

Results based on automatic air quality stations replicate the larger effect implied by the emissions estimates, with an ATT at around -13% for the unconditional and -10% for a specification conditioning on coavariates. These results are obtained based on urban station placements in Luxembourg (in Luxembourg City and motorways). Consequently, these findings represent near-road ambient conditions in high-traffic zones rather than national aggregate emissions. Furthermore, as discussed above, locally monitored NO_x concentrations are highly sensitive to driving speeds, congestion, and stop-and-go traffic, as well as climatic factors. This environmental sensitivity accounts for why the point estimates are both larger and exhibit greater statistical noise.

Overall, our findings suggest that we can attribute the estimated ATT of -6.7% to a modal shift from private motorized transport to public transport and ask whether this estimated effect size is reasonable. Consider the following back-of-the-envelop calculation.

Following figures from the European Commission and Directorate-General for Mobility and Transport (2021), we assume a modal split between private vehicles and public transport of 82 and 18%, respectively. We further assume that the observed reduction in CO_2 emissions results from a modal shift from private vehicles to public transport. A 6.7% reduction in CO_2 emissions from road transport then implies a corresponding decrease in private vehicle usage by approximately 5.5%. This decrease is derived from the fact that private vehicles represent 82% of the modal split and thus contribute the majority of emissions reductions (calculated as 82% of the 6.7% reduction). To maintain the overall transport capacity, public transport usage must increase by approximately 30% (5.5/18).

To assess the credibility of this effect size, we utilize data on the average daily number of people using trams on weekdays from the OECD (2023). In February 2020, this average tram usage was at around 31,000 persons. This increased to around 36,000 in February 2021 and to around 53,000 in February 2022. This amounts to an increase of around 16% and 47% from 2020-2021 and 2021-2022, respectively. These numbers align with our estimates, suggesting that our effect size is reasonable.

Additionally, we can relate these results to the LUXmobile survey, conducted by the Luxembourg City Council (Luxmobile, 2020). This survey reports that the free public transport policy has led to an average increase in public transport usage of around 34% and a 38% increase among residents in 2022, further adding credibility to our estimate. While the descriptive analysis does not direclty validate the causal estimates, the observed figures are consistent with our estimated effect size, lending further credibility to our findings.

Further, we calculate the associated marginal abatement cost of carbon for the policy as the government expenditure per ton of CO_2 abated. A simple calculation takes the foregone revenue from ticket sales of around 41 Mio. Euros and compares it to the tons of CO_2 emissions abated according to our estimates. The latter are calculated as the counterfactual post-treatment emissions for Luxembourg: $\frac{1}{T_{post}} \sum_{t=T_{pre}+1}^{T} CO2_t^{tr} / (1-\hat{\tau})$, where tr indicates the treated unit. With this back-of-the-envelop calculation, we estimate a marginal abatement cost of EUR 117 per ton of carbon. This is, of course, a crude estimate and does not capture the full costs nor the additional non-CO_2-benefits of the policy. As Hahn et al. (2024) argue, such calculations overlook the benefits to inframarginal individuals—those who do not alter their behavior in response to the policy—thereby potentially underestimating the policy's overall effectiveness. They suggest a more comprehensive approach, the Marginal Value of Public Funds (MVPF) framework, which captures these benefits and provides a more accurate assessment of the policy's impact. We leave such detailed calculations to future research.

To situate our findings within the broader literature, it is useful to distinguish between transitory incentives and permanent structural shifts in transport policy. The mixed evidence reported to date—ranging from measurable air quality improvements in Fuzhou (Liang and Wang, 2025) to negligible impacts in Colorado (Webster, 2024)—is not necessarily contradictory. Rather, it underscores the importance of policy duration and the populations such policies are able to reach.

Our results suggest that the effectiveness of public transport incentives critically depends on whether they are perceived as temporary or enduring. Evidence from Germany illustrates this contrast clearly. The temporary 9-Euro ticket introduced in summer 2022 did not lead to a substantial displacement of car usage, plausibly because its short duration limited households' willingness to reorganize established mobility routines (Liebensteiner et al., 2024). By contrast, recent work by researchers at the Mercator Institute of Global Commons and Climate examines the 49-Euro ticket (Deutschlandticket), introduced in May 2023 and still in force. Their findings document a significant and persistent shift in travel from road to rail, consistent with a more durable change in travel behavior (Koch et al., 2024).

Experimental evidence from Sweden supports the view that extended access is necessary for durable modal shifts. Gravert and Collentine (2021) show that lengthening a free-travel incentive from two to four weeks increased initial uptake by 16 percent and led to higher public transport use even eight months after the intervention ended. This suggests that longer periods of free access help overcome the fixed effort and monetary costs of switching modes and allow new transit habits to form. Moreover, ridership during the free-access period followed a clear Monday–Friday commuting pattern. Given that car travel accounted for 58% of city-bound commuting in the study region, the authors conclude that these new transit habits primarily reflect a shift away from private car use.

For a nation like Luxembourg, which has implemented indefinite free public transportation, these findings provide a robust behavioral justification: permanent, cost-free access provides the necessary duration for new transit habits to take root and remain resilient. Together, these results emphasize that long-term measures are essential for systematically and meaningfully changing individual behavior and reducing emissions.

Our findings help reconcile the mixed evidence in the literature regarding the environmental returns of public transit. While studies of subway expansions range from null average effects (Gendron-Carrier et al., 2022) to significant air quality improvements (Li et al., 2019; Lalive et al., 2018), our results suggest that infrastructure is a necessary but insufficient condition for emissions reductions. By demonstrating the efficacy of fare-free transit, we provide evidence that environmental benefits are maximized only when transit is "frictionless". This suggests that the policy's impact may derive not only from the zero-fare price point but also from a reduction in the non-monetary transaction costs that can otherwise discourage transit use.

We conclude that our estimate of a 6.7% reduction in road transport CO_2 emissions following Luxembourg's free public transport policy remains robust across various models that consider the effects of COVID-19, fuel prices, and commuting patterns, and is further validated through a battery of robustness checks. Our findings hold high policy relevance, particularly for policymakers in urbanized, affluent areas with robust public transport networks like Luxembourg. Demonstrating the policy's effectiveness in reducing CO_2 and other transport related emissions, our study highlights the potential of integrating free public transport into comprehensive sustainable transport and urban planning initiatives to meet climate targets and foster a sustainable future.

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Appendix A

Table A.1: TWFE regression for specification projected with all covariates and only adjusted for COVID-related controls

	(1) CO_2		(2) NO_x		(3) GHG	
	Coef.	SE	Coef.	SE	Coef.	SE
log(gdp)	0.3979^{***}	(0.1291)	0.3556^{*}	(0.2126)	0.3964^{***}	(0.1292)
asinh(cases)	-0.0180^{**}	(0.0080)	-0.0226^{***}	(0.0067)	-0.0181^{**}	(0.0080)
stringIndex	-0.0008^{***}	(0.0002)	-0.0005^{**}	(0.0002)	-0.0008^{***}	(0.0002)
asinh(some_wfh)	-0.1858^{**}	(0.0876)	-0.2309	(0.1470)	-0.1857^{**}	(0.0874)
asinh(usual_wfh)	-0.0348^{***}	(0.0074)	-0.0218^{*}	(0.0116)	-0.0347^{***}	(0.0074)
ei	0.0050^{***}	(0.0006)	0.0048^{**}	(0.0021)	0.0050^{***}	(0.0006)
log(diesel)	-0.9204^{***}	(0.1304)	-1.1230^{***}	(0.1013)	-0.9199^{***}	(0.1302)
log(petrol)	0.4970^{***}	(0.1160)	1.0105^{***}	(0.1754)	0.4972^{***}	(0.1157)
log(freight)	0.0175	(0.0117)	0.0140	(0.0342)	0.0175	(0.0117)
log(rel_diesel)	0.3269^{*}	(0.1960)	0.0823	(0.1590)	0.3277^{*}	(0.1957)
cb_inflow75	0.0027	(0.0087)	-0.0035	(0.0139)	0.0028	(0.0087)
cb_inflow75:log(rel_diesel)	-0.7618^{**}	(0.3007)	-0.4844	(0.9447)	-0.7601^{**}	(0.3004)
Obs	322		322		322	
N	46		46		46	

Note: Dependent variables are log emissions per capita (CO_2, NO_x, and GHG). $cb\_inflow75$ is cross-border commuting inflow for 75th percentile and higher among regions experiencing a positive inflow. Standard errors are in parentheses and clustered at the regional level. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.10$.

Appendix B

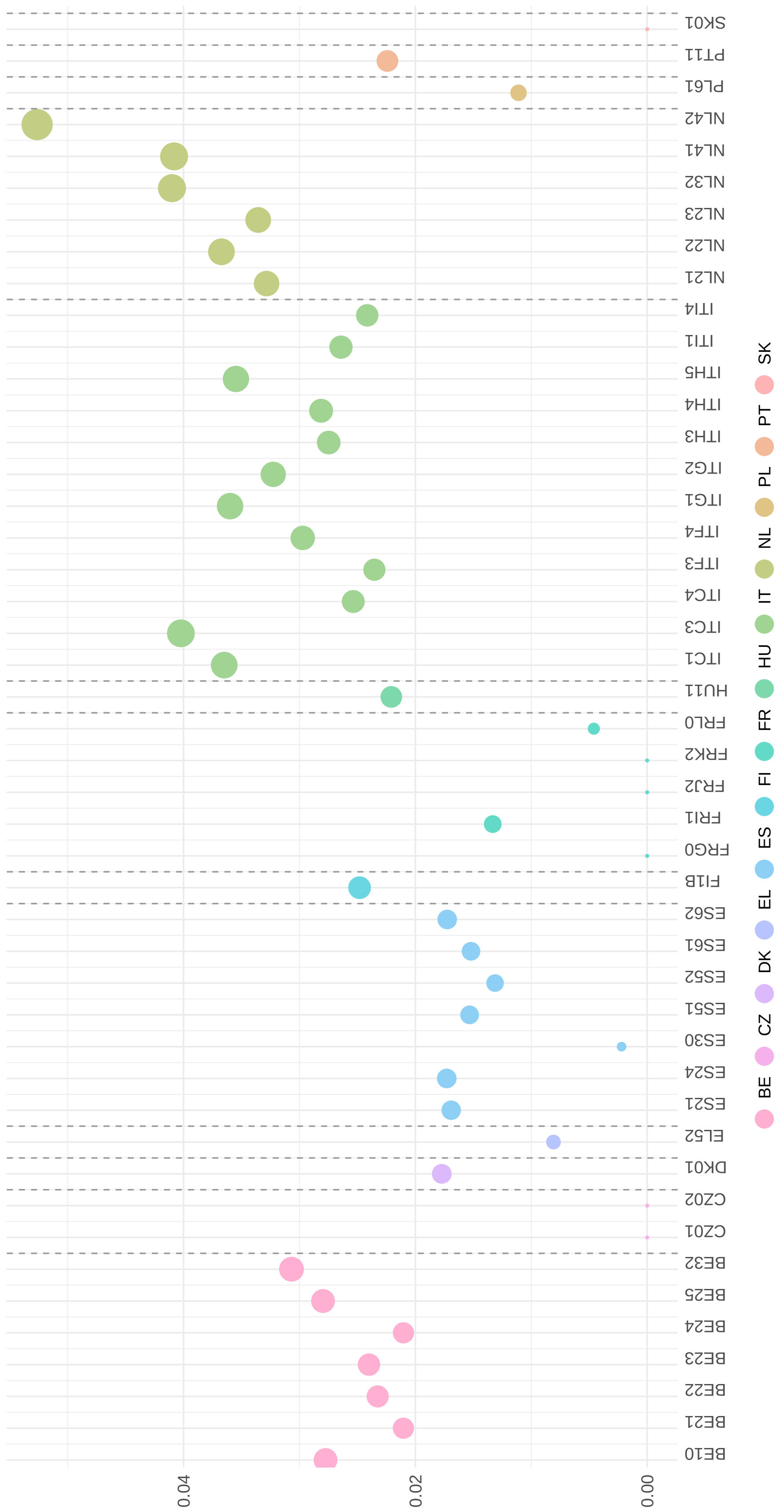
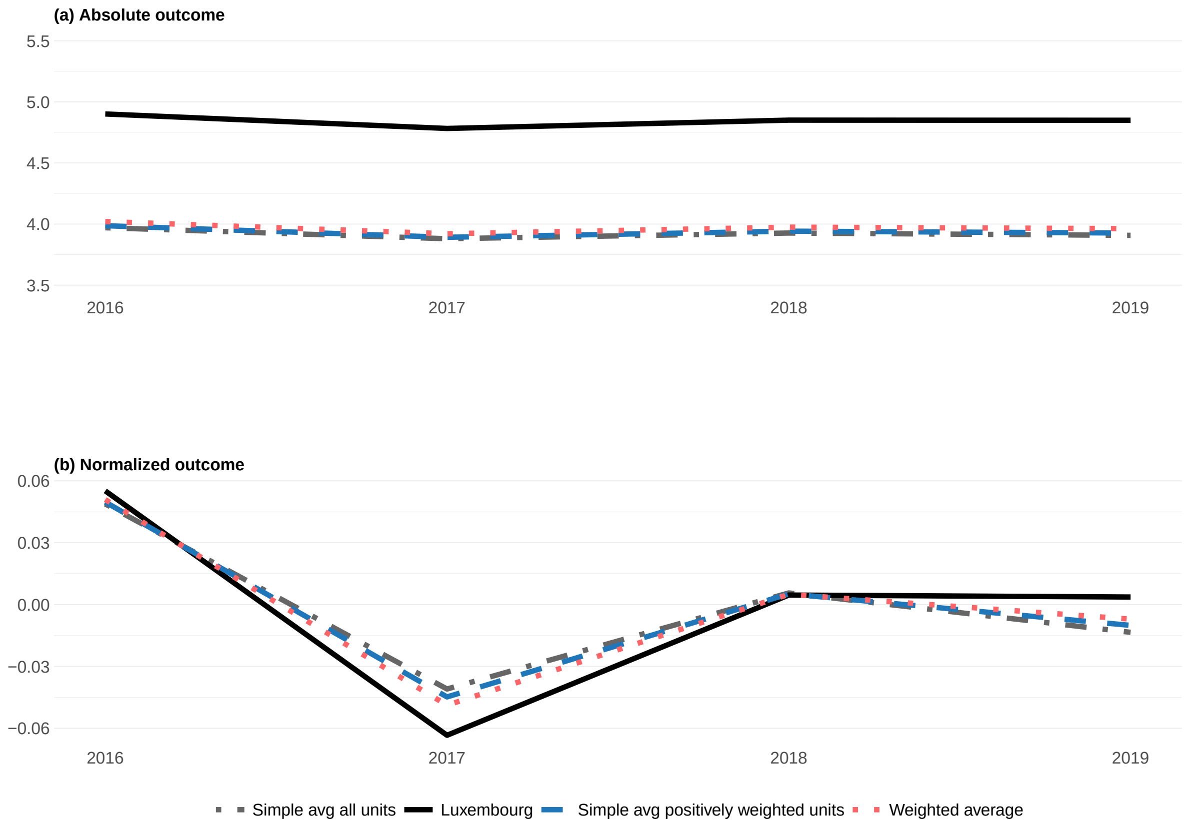


Figure B.1: Unit weights - all covariates

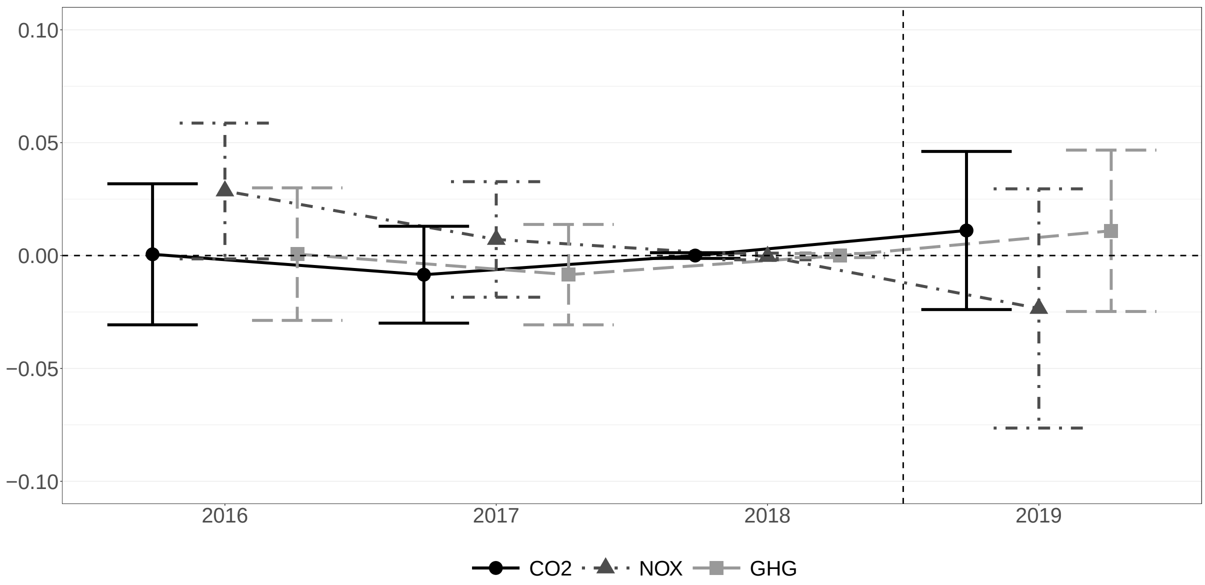
Figure B.2: Pre-treatment trends of the adjusted log CO_2 per capita emissions



Note: This figure displays pre-treatment adjusted trends in log CO_2 emissions. Panel A presents the trends in levels, while Panel B displays normalized values to facilitate a direct comparison of pre-treatment trajectories. *Luxembourg* denotes the observed trend for the treated unit. *Simple avg. all units* represents the unweighted mean of all NUTS 2 regions in the donor pool. *Simple avg. positively weighted units* represents the unweighted mean of donor units assigned non-zero weights in the synthetic control construction. *Weighted average* represents the Synthetic Luxembourg (the counterfactual), constructed as a weighted average of donor units that received positive weights.

Appendix C

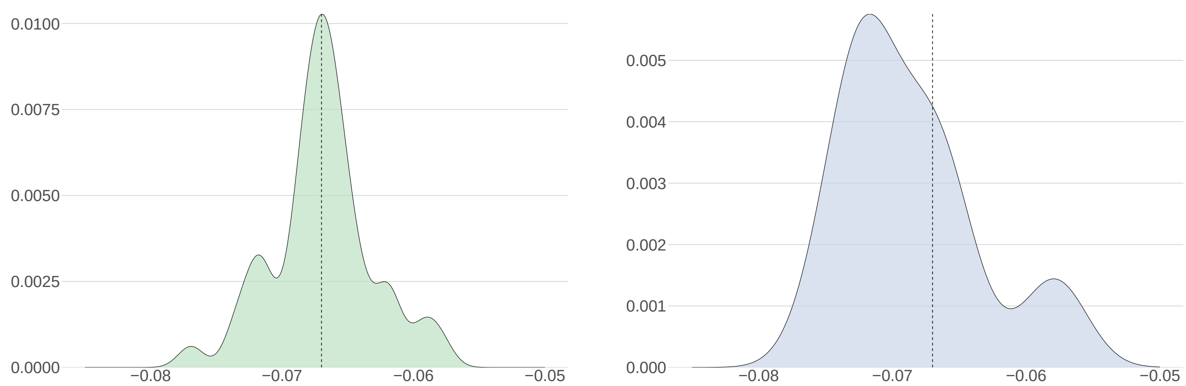
Figure C.1: In-time placebo test



Note: The figure reports results from a placebo exercise in which the policy start date is backdated to 2019. The post-treatment period is 2019, and the pre-treatment period covers 2016–2018. Markers denote point estimates, and vertical bars indicate 95% confidence intervals constructed using placebo-based permutations. The estimated average treatment effects on the treated (ATTs) are 0.0014 ($p = 0.947$) for CO_2, -0.026 ($p = 0.289$) for NO_x, and 0.0012 ($p = 0.947$) for GHGs.

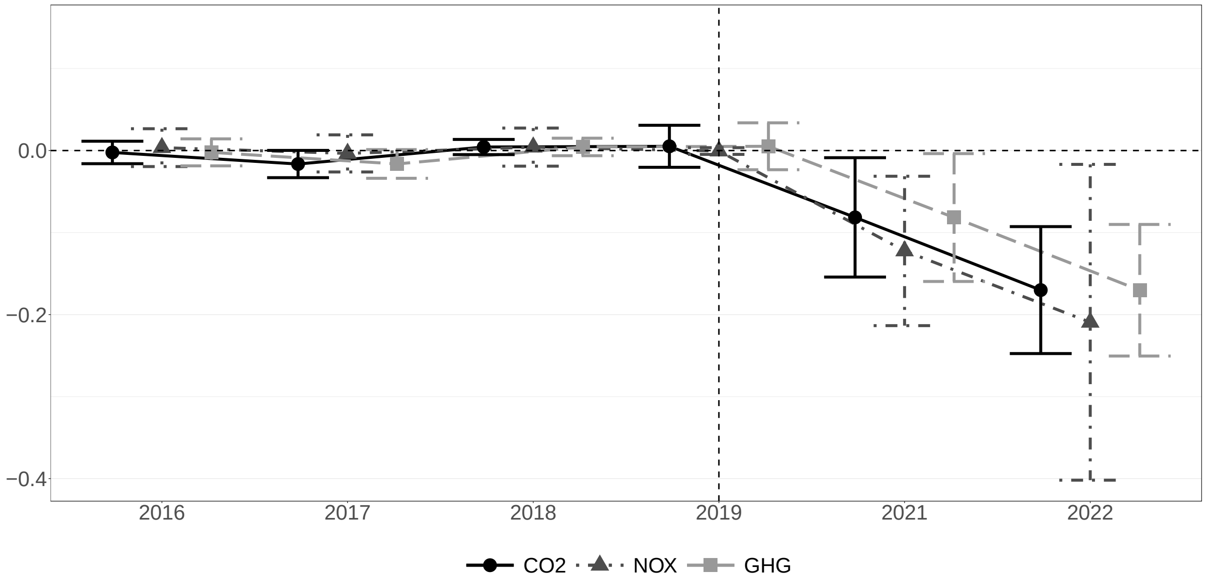
Figure C.2: Distribution of ATT: leave one out analysis

(a) Dropping a regions from the donor pool **(b)** Dropping a country from the donor pool



Note: Panel (a) presents the distribution of ATT estimates obtained by iteratively excluding one region at a time and re-estimating the SDID model. Panel (b) displays the distribution of ATT estimates obtained by iteratively excluding one country at a time and re-estimating the SDID model.

Figure C.3: Event study estimates for sample excluding only rural NUTS 2



Note: The figure reports event-study estimates for emission outcomes using an alternative donor pool that excludes only fully rural NUTS 2 regions. The resulting donor pool comprises 100 NUTS 2 regions (compared with 46 NUTS 2 regions in main sample. The ATTs across the post-treatment periods are −0.126 for CO_2, −0.166 for NO_x, and −0.126 for GHGs. All estimates are statistically significant ($p < 0.001$). Markers denote point estimates, and vertical bars indicate 95% confidence intervals constructed using placebo-based permutation.

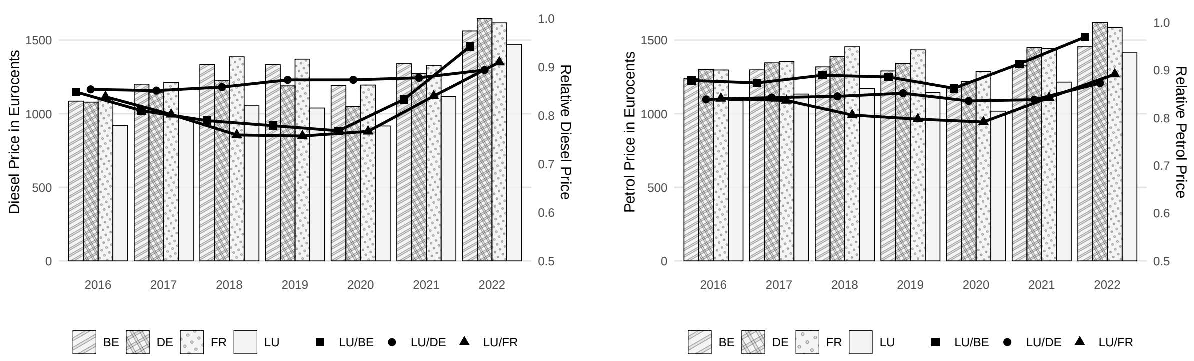
Table C.1: Pre- and post-treatment averages of relative fuel prices for Luxembourg

	Diesel		Petrol	
	Pre-Avg	Post-Avg	Pre-Avg	Post-Avg
BE	0.8045	0.8560	0.8816	0.9186
DE	0.8596	0.8834	0.8444	0.8503
FR	0.7865	0.8464	0.8191	0.8451

Note: Relative fuel prices of LU with respect to its neighboring countries. Pre-Avg are relative fuel prices based on time-weighted pre-treatment fuel prices, where time weights are taken from the SDiD main specification. Post-Avg are relative fuel prices based on post-treatment fuel prices.

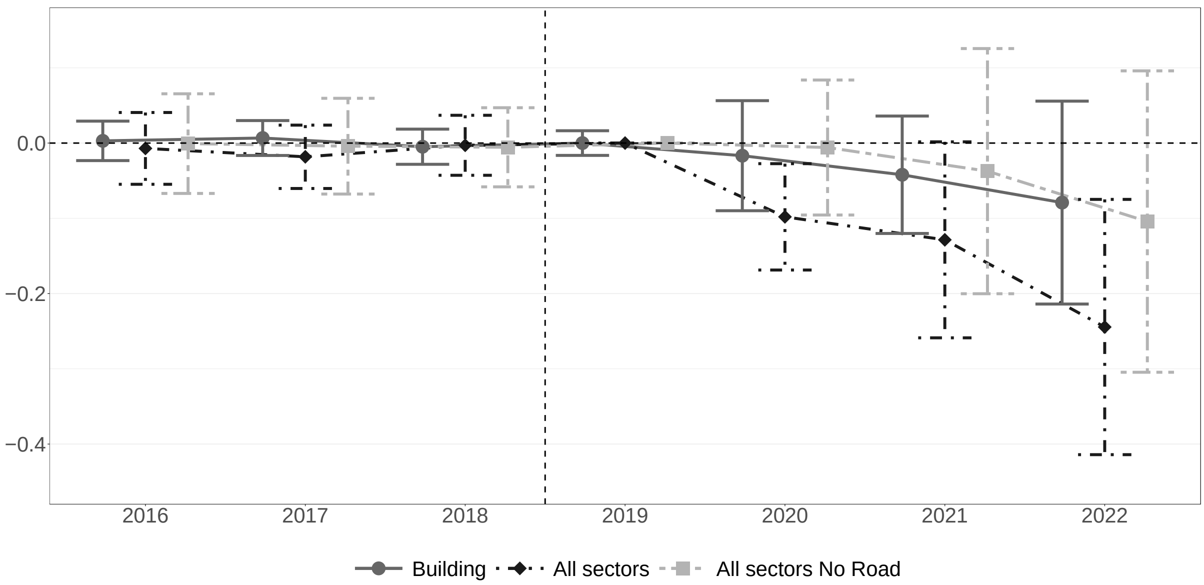
Figure C.4: Absolute and relative fuel prices for LU and neighbouring countries

(a) Diesel **(b)** Petrol



Note: The bars show fuel prices in Eurocents per 1,000 litres adjusted for inflation (HICP). Lines indicate fuel prices of Luxembourg relative to its neighbouring countries over time.

Figure C.5: Other sectors



Note: The figure reports event-study estimates for per capita CO_2 emissions from the building sector, per capita CO_2 emissions from all sectors, and per capita CO_2 emissions from all sectors excluding the road transport sector. All outcomes are measured in per capita logarithms. The average treatment effects on the treated (ATTs) over the post-treatment periods are -0.0459 ($p = 0.324$), -0.157 ($p = 0.004$), and -0.049 ($p = 0.480$), respectively. Markers denote point estimates, and vertical bars indicate 95% confidence intervals constructed using placebo-based permutations.