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Derailing the Car? The Effect of Germany's Deutschlandticket on Road Traffic[*]

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Abstract

This paper examines the causal impact of Germany's *Deutschlandticket*, a flat rate nationwide public transport ticket introduced in May 2023, on extra-urban passenger traffic. Using a synthetic difference-in-differences approach, we construct a counterfactual for German traffic volumes utilizing automatic counting station data from Austria, Switzerland, Finland, and Great Britain. Contrary to the policy's objectives, we find that this drastic fare reduction failed to induce a lasting modal shift away from private cars. While a temporal analysis reveals a transient traffic reduction during the initial summer months of the policy (approximately 7.5%), this substitution effect vanishes rapidly and does not persist into the subsequent year. These findings are robust to the spatial composition of the donor pool, as well as to road-type and day-of-week heterogeneity. Our results are consistent with recent literature suggesting that standalone demand-side subsidies are insufficient to overcome non-monetary generalized costs, such as the reliability issues and capacity constraints currently affecting the German rail network. Consequently, the efficacy and efficiency of the *Deutschlandticket* as a climate policy instrument remains highly questionable.

Keywords: Flat-Rate Public Transport Policy, Modal Shift, Deutschlandticket, Synthetic Difference-in-Differences.

JEL Codes: R48, R41, L91.

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1 Introduction

In May 2023, the German government has introduced the *Deutschlandticket* as a permanent nationwide local public transport (PT) ticket.[1] This ticket is a subscription model and is valid on all local and regional buses and trains throughout Germany. The initial monthly price was €49, with a price increase to €58 at the beginning of 2025 and a further price increase to €63 in 2026. At the end of 2025, approximately 14.6 million people used the ticket[2]. The *Deutschlandticket* is the successor ticket of the *9-Euro-Ticket*, which provided travelers the unlimited usage of local PT (e.g., regional trains, subways, and buses) for €9 per month between June and August 2022.[3] This pricing strategy provides travelers unlimited access to local PT at a fraction of conventional fare costs. Because it includes the entire suburban and regional rail as well as bus network, the ticket heavily targets short- and medium-distance commuters traveling between suburban areas and urban centers. To contextualize the financial relief for travelers, under the standard 2024 tariff structure of the Rhine-Main Transport Association, a standard monthly ticket for a major urban center like Frankfurt (fare zone 3) cost €106.20, while a single daily ticket cost €8.10.[4] Consequently, even with increased prices up to €63 in 2026, the *Deutschlandticket* undercuts usual standard regional pricing substantially, aiming to incentivize a modal shift toward PT, reduce fuel consumption, and hence decrease greenhouse gas emissions.

The direct costs for the *Deutschlandticket* amount to 3 billion Euros per year, divided equally between the German government and the federal states.[5] Although its predecessor ticket was primarily intended to relieve the financial burden on citizens, both interventions simultaneously simplified the highly fragmented tariff structures of Germany's local transport associations. Additionally, the government aimed to incentivize a modal shift towards PT

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[1] See `https://dserver.bundestag.de/btd/20/055/2005548.pdf`.

[2] See `https://www.vdv.de/deutschlandticket.aspx`.

[3] See `https://www.bmdv.bund.de/SharedDocs/DE/Artikel/K/9-euro-ticket-beschlossen.html`.

[4] See `https://www.vgf-ffm.de/fileadmin/VGF/Tickets__Tarife__Plaene/Fahrpreise/Documents/RMV-Preisliste_2024.pdf`.

[5] See *Regionalisierungsgesetz* - RegG, § 9.

to reduce fuel consumption.[6] Economically, the *Deutschlandticket* represents a lasting nationwide introduction of a uniform price setting for local PT. This could eliminate regional price signals that previously captured the value of services, potentially obscuring demand information necessary for optimal capacity planning. As a consequence, this may contribute to capacity challenges and variations in service quality (Liebensteiner et al., 2024).

Governments frequently intervene in the market by means of subsidies to decrease fares. Such interventions – in the form of subsidies – might be reasonable and justified based on two economic grounds: first, to utilize positive externalities through higher PT frequency and lower opportunity costs for the passengers (Mohring, 1972; Button, 2010); second, to internalize the negative externality of motorized individual traffic by making PT traffic relatively more appealing (Basso and Silva, 2014; Parry and Small, 2009). However, the efficacy and efficiency of this second channel relies heavily on the cross-price elasticity between car and PT usage. Studies consistently demonstrate that car demand is highly inelastic to PT fares, with cross-elasticities often close to zero (e.g., Fearnley et al., 2017)). Consequently, empirical evaluations of drastic price reductions, such as fare-free public transport (FFPT) schemes, typically find that ridership gains stem from induced demand or shifts from active modes like walking, rather than a meaningful substitution of car trips (e.g., Cats et al., 2017).

In this paper, we evaluate the causal effect of the *Deutschlandticket* on extra-urban passenger traffic in Germany. Specifically, we analyze whether the drastic, nationwide fare reduction induced a significant decrease in traffic outcomes, serving as a proxy for a modal shift from private cars to PT. Such substantial fare reductions might impact individuals' decisions to lower their car usage, whether by substituting daily trips or even reducing vehicle ownership. To do so, we compile a comprehensive dataset of traffic volumes from automatic counting stations covering the German highway network and employ the synthetic difference-in-differences (SDiD) approach proposed by

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[6] See, e.g., for the *9-Euro-Ticket* `https://www.bundestag.de/dokumente/textarchiv/2022/kw20-de-neun-euro-ticket-894660`, and the *Deutschlandticket* `https://www.bundesregierung.de/breg-de/aktuelles/deutschlandticket-2134074`.

Arkhangelsky et al. (2021). We construct a valid counterfactual using traffic data derived from automatic counting stations in four European countries: Austria, Switzerland, Finland, and Great Britain.

Our results indicate that the introduction of the ticket did not lead to a lasting reduction in aggregate traffic volumes. While we identify a transient, statistically significant reduction of 7.5 % during the first summer of implementation (June to August 2023), this effect dissipates rapidly. The aggregate average treatment effect on the treated (ATT) remains economically negligible and statistically indistinguishable from zero, suggesting that the fare reduction failed to alter driving outcomes and induce a modal shift to PT.

The remainder of the paper is structured as follows: Section 2 reviews the related literature regarding subsidies in PT, modal shift and uniform fare strategies, often introduced through FFPT. Section 3 presents the data and descriptive statistics. Section 4 outlines the empirical strategy, followed by the main estimation results in Section 5. Section 6 provides robustness and sensitivity analyses, Section 7 discusses policy implications, and Section 8 concludes the paper.

2 Related Literature

The literature on pricing and subsidies in public transport (PT) demonstrates that while fare reductions reliably increase transit ridership, their capacity to induce a modal shift from private cars is structurally limited. This limitation is grounded in the underlying structure of demand. While PT is broadly own-price inelastic among existing users – as lower fares rarely induce them to travel more frequently – policies targeting negative externalities rely on attracting new users from other modes. Their success depends on mode substitution rather than generating new mobility, making the cross-price elasticity of private car use with respect to transit fares the critical parameter. Empirical evidence demonstrates that this cross-price elasticity is consistently inelastic and close to zero.[7] Consequently, even substantial fare reductions are expected to induce only marginal decreases in car usage (Paulley et al., 2006; Litman, 2004). Although there is some evidence that ticket price reductions can cause a demand increase in PT usage (Wallimann et al., 2023; Storchmann, 2003), empirical evidence for the presence of a modal shift away from private cars remains scarce.

Several structural factors drive this low cross-mode substitutability. First, demand elasticities vary by trip purpose and time, with peak-hour commuting exhibiting lower price sensitivity than off-peak leisure travel (e.g., Horn af Rantzien and Rude, 2014; Paulley et al., 2006). Second, intertemporal dynamics matter: short-run elasticities are minimal, whereas long-run elasticities increase as permanent price changes influence discrete household choices, such as vehicle ownership and residential location (Litman, 2004; Wardman et al., 2018). Third, the elasticity may also differ with respect to urban or rural areas, since PT availability is lower in rural areas, making individuals more reliant on private cars (Paulley et al., 2006). According to Cools et al. (2016), a zero fare policy (such as an FFPT) could have a much greater effect compared to a pure fare reduction. Finally, because motorized individual traffic often accounts for the vast majority of the passenger mode share, even a marginal percentage reduction in car usage requires a massive proportional surge in PT ridership (Fearnley et al., 2017; Paulley et al., 2006).

Furthermore, endogenous quality degradation serves as a critical barrier to cross-mode substitution. In the presence of capacity constraints, drastic fare reductions induce significant new transit demand, which subsequently exacerbates station crowding, reduces in-vehicle comfort, and increases delays (Lu et al., 2024; Liebensteiner et al., 2024). As crowding externalities strictly increase the non-monetary generalized cost of travel, car drivers –

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[7] A broad review of the empirical literature confirms that the own-price elasticity of PT demand is generally inelastic (e.g., Holmgren, 2014; Paulley et al., 2006; Litman, 2004; Wallimann et al., 2023; Fearnley et al., 2017; Horn af Rantzien and Rude, 2014; Holmgren, 2007; Kholodov et al., 2021). More importantly for modal shift dynamics, cross-price elasticity estimates of car demand with respect to transit fares consistently cluster near zero (e.g., Glaister, 2001; Litman, 2004; Wardman et al., 2018; Fearnley et al., 2017).

who typically exhibit high values of travel time savings and lower tolerances for congestion – are deterred from switching modes (Tirachini et al., 2013; Basso and Silva, 2014).

As summarized in Table 1, a growing body of empirical literature evaluates various fare subsidies and FFPT implementations. The evidence spans multiple geographic contexts and methodological approaches, consistently highlighting the friction between ridership gains and cross-mode substitution.

A vast body of literature deals with the effect of FFPT policies on travel demand. Fare reductions are generally able to increase ridership (e.g., Cats et al., 2014; Mendez, 2025). However, substantial surges in FFPT ridership are predominantly driven by induced demand and mode shifts from active transport (walking and cycling), rather than from private cars (e.g., Cats et al., 2017). Evaluations of the 2013 FFPT introduction in Tallinn identify mobility improvements primarily for low-income residents, with minimal uptake among working-age commuters (Cats et al., 2014; Cats et al., 2017). Similar substitution patterns emerge in Hesse, Germany (Busch-Geertsema et al., 2021), and in randomized field experiments in Santiago, Chile, where free transit passes increased off-peak subway utilization by 23% without inducing substitution away from cars (Bull et al., 2021). Although a fare abolition in Templin, Germany, substantially increased transit ridership, a mode shift was found for pedestrians and bicycles only, but not for cars (Storchmann, 2003). The assessment of Austria's *Klimaticket*, a nationwide public transport pass offering users unlimited access to PT services throughout Austria for an annual fee of €1,095, did not identify any statistically significant increase in passenger volumes (Wallimann, 2024). While experimental evidence suggests targeted, temporary free tickets might induce marginal shifts (Fujii and Kitamura, 2003), the literature broadly concludes that standalone fare subsidies fail to reduce car usage efficiently. Effective internalization requires pairing subsidies with policies that directly increase the generalized cost of driving (Parry and Small, 2009; Cats et al., 2017; Fearnley et al., 2017).

Recent literature examining the German *9-Euro-Ticket* and the subsequent *Deutschlandticket* – both weak forms of a FFPT policy – predomi-

Reference	Setup	Method	Data	(Main) Outcome Variables	(Main) Result
Fujii and Kitamura (2003)	One-month experiment of free bus tickets in Kyoto (Japan)	Before-and-after comparison (immediately after and one month after experiment)	Survey of participants	Attitudes towards, habits of, and frequency of using private vehicles and bus	(+) attitudes towards bus, (+) frequency of bus use, (–) habits of using automobile
Storchmann (2003)	Introduction of FFPT in Templin (Germany)	Before-and-after comparison and evaluation of survey data	Ridership data provided by the city of Templin & survey of PT users	Transit ridership, modal shift	(+) use of PT, (+) shift from pedestrians and bicycles to PT, (+) minimal shift cars to PT
Cats et al. (2014)	Introduction of FFPT to Tallinn's residents in January 2013	Before-and-after comparison (3 months after the introduction)	Automated vehicle location and automatic passenger count data	Passenger demand (measured by number of boarding passengers)	(+) passenger demand, when accounting for supply variables (+1.2%)
Cats et al. (2017)	Introduction of FFPT to Tallinn's residents in January 2013	Before-and-after comparison (1 year after the introduction)	Interviews and travel diaries of 1,500 random households	Modal shift, travel attitudes and satisfaction, trip destination choices	(+) PT usage (+14%), (-) walking trips (-40%), (-) car trips and share (-10% and -5%), (+) car vehicle-km (+31%)
Bull et al. (2021)	Random assignment of free public transport tickets in Santiago (Chile)	Randomized Control Trial	Trip diary	Number and time of trips by different modes	(+) overall travel (12%)
Busch-Geertsema et al. (2021)	Introduction of FFPT for state employees in Hesse (Germany)	Before-and-after comparison	two-wave survey at Goethe University Frankfurt	Public transport use, car use, car availability	(+) use of public transport
Gohl and Schrauth (2022)	Introduction of *9-Euro-Ticket* in Germany	Difference-in-differences	Air pollution data	Air pollution index	(–) air pollution (-8%)
Wallimann et al. (2023)	Reduction of public transport ticket prices in Geneva (Switzerland)	Synthetic control method	Transport companies' annual reports	Public transport use (passenger trips per vehicle kilometer)	(+) public transport demand (+10.6%)
Andor et al. (2023)	Introduction of *9-Euro-Ticket* in Germany	Difference-in-differences	Surveys on mobility data	Sum of trips and distances by mode; cost-benefit analysis FFPT	(–) car use (-10%), (+) public transport use
Aydin and Kürschner Rauck (2023)	Introduction of *9-Euro-Ticket* in Germany	Difference-in-differences	Air pollution data	Particulate matter	(–) air pollution
Liebensteiner et al. (2024)	Introduction of *9-Euro-Ticket* in Germany	Difference-in-differences, Event study design	mobile network-based mobility, traffic volume, and rail traffic data	number, purpose and mode choice of trips, train delay	(+) train trips (+34%), (–) number of passenger cars (-1.4%), (+) train delays (+4%p)
Loder et al. (2024)	Introduction of *9-Euro-Ticket* in Germany	Before-during-after comparison	Mobilität.Leben study	public transport trips, private transport trips	(+) public transport use, (–) car use
Wallimann (2024)	Introduction of the KlimaTicket in Austria	Synthetic control method & synthetic difference-in-differences	Yearly data of PT users provided by PT companies	Rail travel demand	No short-run effect on passenger growth
Guajardo Ortega and Link (2025)	Introduction of *9-Euro-Ticket* in Germany	Mixed logit model	GPS-tracked trips	Mode inertia	(+) positive inertia for car
Schlett and Loder (2025)	Introduction of *Deutschlandticket* in Germany (focusing on the Munich metropolitan region)	Before-and-after comparison	Multimonth, semipassive, smartphone-based GPS tracking data (Mobilität.Leben study)	Cost of travel, travel distance, activity time, and travel direction	(+) PT travel distance, (-) car travel distance, (+) monetary savings for ticket holders
Waldorf et al. (2025)	Introduction of *9-Euro-Ticket* & *Deutschlandticket* in Germany	Propensity score matching	Mobilität.Leben study	activity participation, mode use, financial relief, and attitudes	(+) increase in PT demand, (-) car use

Table 1: Overview of empirical evaluations regarding FFPT schemes and extreme fare reductions.

nantly relies on survey and descriptive data (e.g., Rozynek, 2024), though recent structural and quasi-experimental approaches provide more robust causal estimates. Applying a dynamic difference-in-differences (DiD) framework, Liebensteiner et al. (2024) identify a strong increase in leisure rail travel coupled with notable adverse effects on rail infrastructure quality, but find limited substitution between transportation modes. Using GPS-tracked trip data, Guajardo Ortega and Link (2025) document significant inertia effects among car users, resulting in negligible and small modal shifts from car to public transport. Applying a propensity score matching method, Waldorf et al. (2025) similarly present an increase in PT trips that is, however, paired with only a small decrease in car trips, suggesting that fare reductions alone cannot stimulate a mode shift.

Survey-based and descriptive evaluations yield heterogeneous, albeit slightly more optimistic, estimates of mode substitution. Concerning the *9-Euro-Ticket*, on the one hand, aggregate mobility data indicates a 44% increase in PT demand alongside minimal reductions in motorized private transport.[8] On the other hand, survey analyses estimate that 10% to 20% of ticket holders substituted at least one car trip, corresponding to an approximate 10% reduction in aggregate vehicle kilometers traveled (Loder et al., 2024; Andor et al., 2023).[9]

Furthermore, early evidence explores the environmental externalities of the *9-Euro-Ticket*. Utilizing a DiD design, Gohl and Schrauth (2022) demonstrate that the intervention reduced a benchmark air pollution index by over 6%. Similarly, Aydin and Kürschner Rauck (2023) identify a significant short-term reduction in particulate matter emissions, attributing this decline to reductions in car usage.

Finally, expanding on spatial and temporal dimensions, Schlett and Loder (2025) analyze the *Deutschlandticket* using multi-month, smartphone-based GPS tracking. They reveal that the ticket's effects extend beyond pure monetary relief, varying significantly across fare zones and weekdays. Notably, the

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[8] See the Federal Statistical Office `https://www.destatis.de/DE/Presse/Pressemitteilungen/2022/09/PD22_377_12.html`.

[9] Industry reports echo these survey findings; see the Association of German Transport Companies (VDV) evaluation at `https://www.vdv.de/bilanz-9-euro-ticket.aspx`.

fare innovation induced a spatial substitution effect: ticket holders increased their public transit travel primarily within the city center while decreasing their car travel in suburban zones.

3 Data and Descriptive Statistics

3.1 Data

Our data set covers extra-urban, non-local motor vehicle traffic volumes for Germany and the control countries – Austria, Switzerland, Finland, and Great Britain – from January 2018 to December 2024.[10] For each country, our data set includes observations of automatic and permanent traffic counting stations in the respective road network. To harmonize reporting frequencies, station-level data are aggregated to monthly traffic counts for cars, vans and motorcycles (vehicles weighing < 3.5 tons), pooling all lanes and directions.[11] The panel is balanced by restricting the sample to stations continuously active from January 2018 through December 2024, dropping any unit with missing monthly observations. However, we retain stations where daily reporting gaps were bridged by the data providers' estimation procedures, provided a valid monthly aggregate is available.[12] Finally, we align heterogeneous national road definitions into two categories: motorways (such

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[10] Data sources: Germany: *Federal Highway Research Institute* (*BASt*), Austria: (*Motorway and Expressway Financing Joint-Stock Company* (*ASFiNAG*), Switzerland: *Swiss Federal Roads Authority* (*ASTRA*), Finland: *Fintraffic*, and Great Britain: Department for Transport.

[11] We harmonize the reporting frequencies by aggregating the raw data to a monthly resolution, a step that smooths daily and weekly fluctuations. For Germany, Finland, Switzerland, and Great Britain, this involves aggregating raw hourly data. However, in the case of Austria, the data is reported as monthly daily averages separated by day type (weekdays vs. weekends). To ensure comparability, we reconstruct the total monthly flow for Austria by projecting these average counts into a single aggregate figure.

[12] A balanced panel is a prerequisite for the SDiD estimator used in this analysis. Furthermore, restricting the sample prevents compositional bias arising from differential station entry or exit within countries.

as German Autobahn) and main roads (such as German Bundesstraße).[13]

While our counting station data primarily captures extra-urban highway traffic (motorways and main roads), these networks constitute critical commuting corridors connecting suburban and rural populations to major urban centers. In Germany, a substantial share of the workforce commutes across municipal boundaries, heavily relying on highway roads and the rail network. According to the Federal Institute for Research on Building, Urban Affairs and Spatial Research, in 2024 approximately 20.5 million employees in Germany (approximately 60% of the total) commuted to a different municipality for work. The average one-way commute distance was 17.2 kilometers, with 7.23 million employees commuting more than 30 kilometers.[14] Crucially, the *Deutschlandticket* encompasses all regional rail, suburban rail, and bus networks, which run parallel to these exact motorways and serve as the primary public transport substitutes for medium-distance car commuters. Since approximately 65% of commuters rely on private cars,[15] our dataset on highways and federal roads captures flows targeted by the policy.

The control group is constructed to ensure socio-economic comparability with independence from local spillovers. The DACH-region (Germany, Austria, Switzerland), which encompasses more than 100 million individuals who largely exhibit similar socio-economic conditions, despite regional variations within and across each country[16], shares high economic integration and contiguous road networks, ensuring the control group is exposed to similar

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[13] We define motorways and main roads for each country, categorizing country-specific road types into these two classes. Broadly, we adhere to the European Commission report on motorways and the Vienna Convention on Road Signs and Signals (`https://road-safety.transport.ec.europa.eu/system/files/2021-07/ersosynthesis2018-motorways.pdf`). Here, a motorway/highway is a high-capacity road designed exclusively for motor traffic, featuring separate carriageways, no at-grade crossings, and controlled access. In contrast, a main road (such as a Bundesstraße in Germany) is a major road connecting cities and regions, allowing mixed traffic, including slower vehicles, and often featuring intersections. While motorways ensure uninterrupted high-speed travel, main roads can accommodate both regional and local traffic with varying access controls.

[14] See `https://www.bbsr.bund.de/BBSR/DE/startseite/topmeldungen/pendeln-in-deutschland-2024.html`.

[15] See `https://www.destatis.de/DE/Presse/Pressemitteilungen/2025/05/PD25_N027_13.html`.

[16] See `https://www.statista.com/topics/4623/dach-countries/`.

regional demand shocks. Conversely, Finland and Great Britain are included to introduce geographically distinct observations. While these markets may differ in specific weather, institutional or socio-economic factors, their lack of physical road connectivity to Germany ensures that traffic volumes are independent of local cross-border diversion effects (i.e., thereby mitigating spatial spillover effects).

Furthermore, with the exception of Austria, none of the control countries introduced a comparable nationwide PT fare policy during the observation period. The Austrian government introduced a comparable PT ticket, the *Klimaticket* in the end of October 2021. Similar to the *Deutschlandticket*, it gives ticket holders unlimited and nationwide access to PT for an annual price of €1,095[17]. This development may affect the suitability of using Austrian traffic volume data in our empirical analysis. However, empirical evidence of the *Klimaticket* indicates that there is no statistically significant increase in PT passengers (Wallimann, 2024, p. 206-207), therefore we argue that Austrian car traffic is not affected by the policy. To further underline the robustness of our result, we will evaluate a leave-one-out analysis excluding Austrian counting points from our dataset (see Table 5), which does not alter our results.

Table 2 summarizes the different types of roads that we include in our data set and our categorization, as well as the number of available counting stations in our sample. Since the Finnish road type *Valtatie* (which translates to highway) is very heterogeneous compared to a German *Autobahn*, we use the number of lanes for each counting station in order to make road types more comparable. Each *Valtatie* with four or more lanes is categorized in the motorway category, otherwise in the main road category.

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[17] See `https://www.bmimi.gv.at/themen/mobilitaet/1-2-3-ticket/fakten.html`, last accessed on February 19, 2026. By 2026, the price has increased to €1,400 for the standard ticket, see `https://www.klimaticket.at/#kosten`, last accessed on February 19, 2026.

Country	Motorway	Main road	Counting stations	Data source
Germany	Autobahn	Bundesstraße	777	*BASt*
Austria	Autobahn, Schnellstraße	-	104	*ASFiNAG*
Switzerland	Autobahn	Hauptstraße	50	*ASTRA*
Finland	Valtatie*	Kantatie, Valtatie*	309	Fintraffic
Great Britain	Motorway, road with motorway regulations	primary and secondary roads	132	Department for Transport
Total			1,372	

Table 2: Road types in the different countries and categorization in road types motorway or main road. Also displayed is the number of counting stations in our data set as well as data sources. The full data for Austria, Switzerland and Great Britain was available upon request. In order to make Finland's road types more comparable to Germany's classification, we split the road type Valtatie by the number of lanes. A Valtatie road with four or more lanes is categorized as motorway, with less than four lanes as main road.

Figure 1 illustrates the spatial coverage of counting stations across Great Britain, Finland, and the DACH-region. The counting stations are differentiated by road type, with dark blue markers representing locations on main roads and light blue markers indicating points on motorways, all overlaid against the road network in grey, where white lines delineating national and regional administrative boundaries. The distribution patterns reveal distinct regional characteristics: Great Britain and Germany exhibit high-density coverage corresponding to their complex road networks and population centers. In contrast, Finland displays a clear north-south divide, where monitoring is concentrated in the populous southern regions (in the vicinity of major cities such as Helsinki) and becomes sparser in the north. Similarly, the network in Austria and Switzerland reflects the constraints of alpine topography, with stations aligned primarily along major valleys and transit corridors. The Austrian sample, however, is restricted exclusively to motorways (Table 2). Overall, the visualization demonstrates that the dataset provides robust coverage of strategic transport arteries, capturing both high-speed motorways and essential main roads across diverse geographic landscapes.

The sample period (January 2018–December 2024) encompasses two dis-

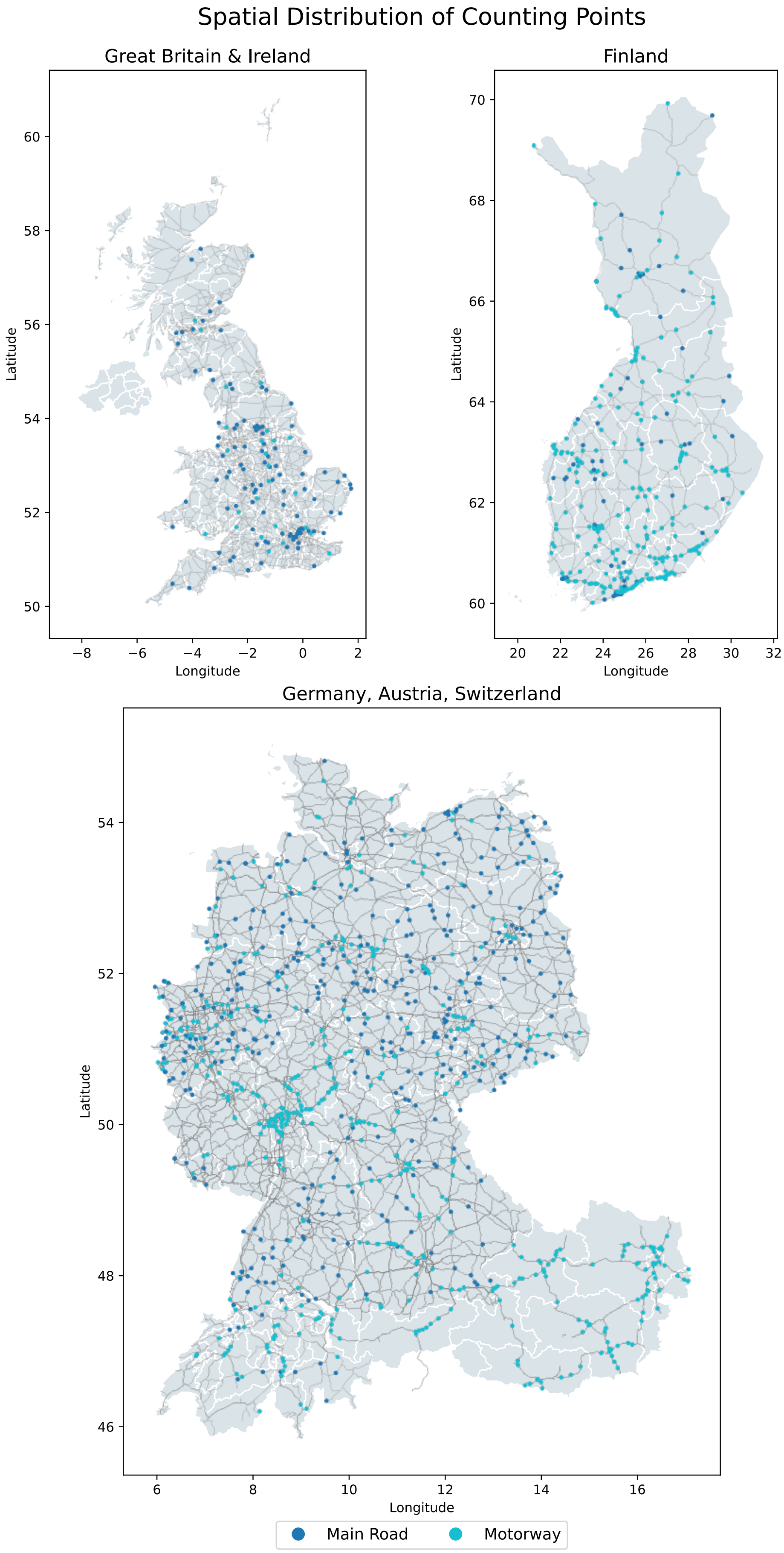

Figure 1: Spatial distribution of traffic counting stations on main roads and motorways in Great Britain, Finland, and the DACH-region.

tinct structural shocks to traffic outcomes. First, the COVID-19 pandemic profoundly impacted mobility. Arguably, not all of the countries in our sample are affected to the same extent. With Germany, Austria and Switzerland being transit countries for inner-European travel, traffic volumes might have decreased more heavily compared to traffic in Finland or Great Britain. Second, in response to the energy crisis following the Russian invasion of Ukraine, the German government introduced a temporarily available monthly PT ticket from June to August 2022 (*9-Euro-Ticket*). The ticket's conditions were very similar to those of the *Deutschlandticket* with one major exception: the price. As the name suggests, the *9-Euro-Ticket* was available for €9 per month, while the price of its successor, the *Deutschlandticket*, increased to €49 a month in the relevant time period. During that same period of the *9-Euro-Ticket*, the German government also decreased the fuel tax to the European minimum (Drolsbach et al., 2023, p. 3). Both of these policies could possibly affect travel patterns in Germany, acting as potential confounders for identifying an (unbiased) effect of the *Deutschlandticket* on traffic volumes.

To mitigate bias from these structural breaks, the estimation excludes the period from January 2020 through August 2022. This removes periods affected by the pandemic that might have influenced the traffic volumes heterogeneously across countries (e.g. home office policies), the *9-Euro-Ticket*, or the fuel tax reduction. While one might consider restricting the sample solely to the periods after the expiration of the *9-Euro-Ticket*, such a shortened panel lacks a pre-treatment summer baseline, raising concerns regarding seasonal bias, since travel patterns exhibit strong seasonality.[18]

To account for exogenous variation in travel conditions, we augment the panel with station-level weather data sourced from the *European Climate Assessment & Dataset* (ECA&D) project (Klein Tank et al., 2002). For every traffic counting station, we assign the nearest (available) weather station and utilize monthly average temperature, average wind speed, and the to-

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[18] See Figure 2.

tal number of ice days.[19] Including these covariates is necessary to control for spatially idiosyncratic meteorological shocks – such as freezing events – which can affect traffic volume, speed, and accident risk in different locations heterogeneously (Cools et al., 2010).

3.2 Descriptive Statistics

Our main data set contains traffic data for Germany (GER), Austria (AUT), Switzerland (CHE), Finland (FIN), and Great Britain (GBR). Table 3 presents descriptive statistics for the final panel, stratified by road type, and control and treatment group. The final sample comprises 1,372 counting stations observed over 52 periods ($N = 71,344$). Three structural features of the data are noteworthy. First, there are crucial level differences in traffic intensity across regions. German motorways record the highest average monthly volume (1.45 million vehicles), exceeding the AUT/CHE (1.26 million) and FIN/GBR (0.78 million) control groups. Second, the table quantifies the data availability constraints discussed in Section 3.1. While the motorway sample is geographically well-represented across all regions, the main road control group for Austria and Switzerland is limited (10 stations, only provided by Switzerland). Consequently, the donor pool, and thus the identification of treatment effects for main roads, is heavily based on the denser network of stations provided by Finland and Great Britain (300 stations). Third, meteorological covariates exhibit expected geographic heterogeneity. The FIN/GB control group is characterized by lower average temperatures and a higher frequency of ice days compared to the DACH region, validating the inclusion of weather controls to adjust for spatially asymmetric shocks.

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[19] Traffic stations are matched to the nearest weather station; missing daily observations are imputed using the next-nearest active station. We utilize ECA&D data series TG (daily mean temperature), FG (daily mean wind speed), and TX (daily maximum temperature), see: `https://www.ecad.eu/dailydata/predefinedseries.php`. The latter is used to compute the monthly count of *Ice Days*, defined as days where the maximum temperature remains below freezing ($T_{max} < 0°C$).

	Country	AUT		CHE		FIN		GBR		GER	
	Period	Pre	Post	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Road Type	Variable										
Motorway	Traffic Volume in Mio. Counts	1.24	1.30	1.32	1.36	0.63	0.63	1.49	1.36	1.45	1.44
		(0.77)	(0.77)	(0.86)	(0.87)	(0.39)	(0.39)	(0.61)	(0.51)	(0.80)	(0.79)
	Avg. Daily Tmp. in °C	10.22	12.92	9.45	11.86	4.61	7.27	9.76	11.68	9.96	12.35
		(7.42)	(7.32)	(7.16)	(7.03)	(8.22)	(9.00)	(4.74)	(4.34)	(6.46)	(6.03)
	WindSpeed in km/h	2.32	2.36	2.05	2.03	4.46	4.45	4.51	4.59	3.46	3.41
		(1.27)	(1.29)	(0.96)	(0.90)	(1.75)	(1.75)	(0.79)	(0.85)	(1.23)	(1.22)
	Number of Icedays	1.23	0.63	1.68	1.30	6.39	4.81	0.11	0.00	0.90	0.59
		(2.98)	(2.11)	(4.65)	(3.86)	(8.35)	(7.69)	(0.47)	(0.04)	(2.32)	(1.75)
	Unique Counting Points	104		40		116		25		382	
	Observations	3,328	2,080	1,280	800	3,712	2,320	800	500	12,224	7,640
Main Road	Traffic Volume in Mio. Counts			0.22	0.23	0.19	0.19	0.43	0.41	0.32	0.31
				(0.19)	(0.20)	(0.25)	(0.26)	(0.30)	(0.27)	(0.28)	(0.27)
	Avg. Daily Tmp. in °C			9.04	11.33	3.25	5.94	9.82	11.71	9.69	12.10
				(7.45)	(7.23)	(8.57)	(9.49)	(4.74)	(4.37)	(6.45)	(5.96)
	WindSpeed in km/h			2.69	2.75	3.45	3.51	4.52	4.61	3.58	3.49
				(2.30)	(2.24)	(1.31)	(1.34)	(0.82)	(0.92)	(1.29)	(1.27)
	Number of Icedays			2.12	1.46	8.15	6.39	0.08	0.00	1.04	0.62
				(5.50)	(4.28)	(9.78)	(9.00)	(0.40)	(0.06)	(2.63)	(1.87)
	Unique Counting Points			10		193		107		395	
	Observations			320	200	6,176	3,860	3,424	2,140	12,640	7,900
Total Counting Points		104		105		309		132		777	
Total Observations		3,328	2,080	1,600	1,000	9,888	6,180	4,224	2,640	24,864	15,540

Table 3: Summary statistics from January 2018 to December 2019, as well as September 2022 to December 2024. Note: Austria (AUT), Switzerland (CHE), Finland (FIN), Great Britain (GBR) and Germany (GER); standard deviations in parentheses.

Figure 2 shows the average monthly traffic volumes per counting station recorded on motorways and main roads for all five countries in our sample. Notably, traffic volumes in all chosen countries fluctuate seasonally with higher traffic counts during summer months. The level differences that can be seen in Figure 2 can be explained by differing characteristics of the specific road network (e.g., number of lanes), as well as differences in the location of the counting station (e.g., commuter belts vs. rural transit). As explained (and shown in Tables 2 and 3), observations for Austria only include motorway equivalents which measure a greater number of vehicles compared to main roads. Therefore, the average number of passenger vehicles depicted in Figure 2 is highest for Austria. Conversely, Finland's and Great Britain's road networks cover fewer relative motorway counting stations than main roads. Nevertheless, the development of passenger vehicle traffic over time is very similar between the five countries in our observation period. The grey shaded area marks the time period that will be excluded in the following estimations. As can be seen, the COVID-19 pandemic temporarily decreased traffic counts in spring 2020 for all five countries, but to different extents. The traffic outcomes have returned gradually to pre-pandemic levels by the end of 2021, however we further exclude observations until August 2022 because traffic volumes might be affected by other policies such as the *9-Euro-Ticket* and the fuel tax cut. The red shaded area highlights the period since the *Deutschlandticket* has been in place.

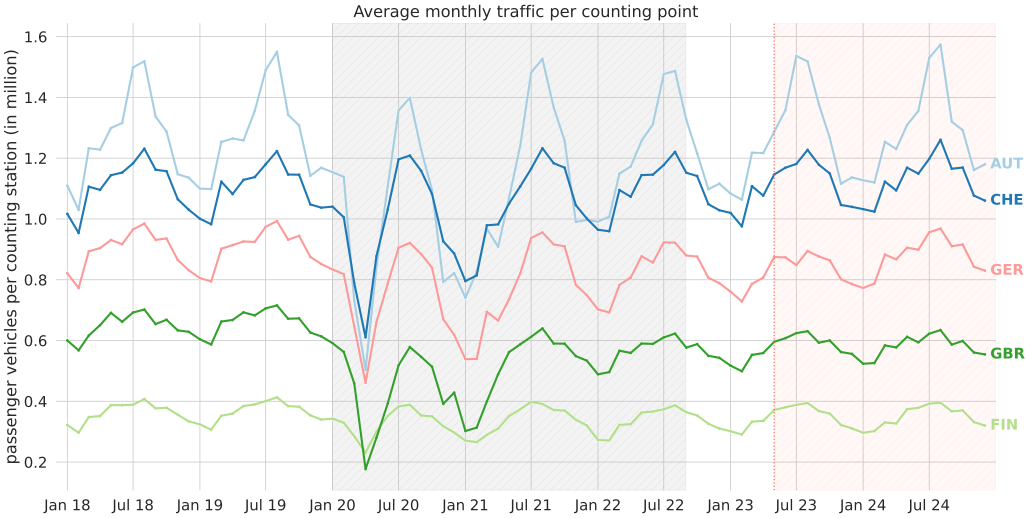

Figure 2: Monthly mean traffic of passenger vehicles per counting point in Austria (AUT), Switzerland (CHE), Germany (GER), Great Britain (GBR), and Finland (FIN) from January 2018 to December 2024. The grey shaded area represents the excluded period from January 2020 to August 2022. The red shaded area marks the treatment period of the *Deutschlandticket* with its introduction in May 2023.

4 Methodology

To assess whether the introduction of the *Deutschlandticket* exerted a statistically significant impact on passenger traffic in Germany, we employ the synthetic difference-in-differences (SDiD) estimation framework introduced by Arkhangelsky et al. (2021). We compare changes in extra-urban traffic in Germany to a weighted counterfactual drawn from our four European control countries. The SDiD method combines features of synthetic control (SC) methods and difference-in-differences (DiD). It reweights and matches on pre-exposure trends to weaken the reliance on parallel trends like SC while simultaneously being invariant to additive unit-level shifts and allowing for valid large-panel inference like DiD.

The average causal effect of the *Deutschlandticket* is estimated as follows:

$$(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \arg\min_{\tau,\mu,\alpha,\beta} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \alpha_i - \beta_t - Ticket_{it}\tau)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\}. \quad (1)$$

Including counting points from Germany, Austria, Switzerland, Finland, and Great Britain we obtain a balanced panel with 1,372 counting points and $T = 52$ months (total $N \times T = 71,344$ observations).[20] The variable Y_{it} in Equation (1) measures log traffic volume for counting point i (unique to each country) and month t, while the variables α_i and β_t denote counting point and time fixed effects, respectively, and μ is an intercept. The binary treatment indicator $Ticket_{it} \in \{0, 1\}$ indicates the introduction of the ticket in May 2023. The ATT is then given by τ. In addition, the weights $\hat{\omega}_i^{sdid}$ and $\hat{\lambda}_t^{sdid}$ denote unit and time weights, respectively. The unit weights $\hat{\omega}_i^{sdid}$ are chosen in a way that the trend for traffic volumes of control units aligns with that of treated units in the pre-treament period. Similarly, the time weights $\hat{\lambda}_t^{sdid}$ are calculated to emphasize pre-treatment periods that are similar to treatment periods for units that are never treated.[21]

Finally, as we aim to control for time-varying exogenous covariates, we follow the adjustment described in Arkhangelsky et al. (2021, Footnote 4) by using the residuals $Y_{it}^{res} = Y_{it} - X_{it}'\hat{\gamma}$ from the regression of Y_{it} on the vector X_{it}, where X_{it} contains the weather covariates – logarithmic daily mean temperature, wind speed, and the count of ice days (see Section 3) – to control for exogenous meteorological conditions affecting modal choice and traffic patterns.[22]

Since our data set includes more control unit observations than pre-

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[20] This time period includes January 2018 to December 2019 as well as September 2022 to December 2024. For an explanation see Section 3.1.

[21] See Equations (4) and (6) in Arkhangelsky et al. (2021).

[22] To capture potential non-linearities, temperature is entered in logarithmic form. Since the natural logarithm is undefined for negative values (in Celsius readings), we transform all temperatures to the Kelvin scale ($K = {}^\circ\mathrm{C} + 273.15$). This affine transformation ensures strict positivity while preserving the ordinal rank of observations.

treatment periods, the weighting algorithm does not necessarily lead to unique values for unit weights $\hat{\omega}_i^{sdid}$ (Doudchenko and Imbens, 2016). Arkhangelsky et al. (2021) address this by including a penalty term to increase dispersion. We depart from this dispersion-based approach by configuring the weighting algorithm to use the default specifications of the standard synthetic control method (Abadie et al., 2010), relying instead on (i) bootstrapped and (ii) placebo variance estimation to ensure stability and robust inference. Specifically, by omitting the intercept and setting the penalty term to zero (i.e., close to zero), we force the algorithm to build a counterfactual in levels rather than changes, effectively reducing the unit-weighting step closer to the SC method (Arkhangelsky et al., 2021, p. 4092).[23] While the standard SDiD ridge penalty successfully distributes weights across donor pools in aggregate, macro-level panels, applying it to our highly granular, heterogeneous station-level dataset risks severe interpolation bias (e.g., Abadie, 2021).[24] Standard SDiD tends to disperse weights across hundreds of dissimilar rural and urban counting stations simply to match an average trend (see Section 5). As established in the broader synthetic control literature, dispersing weights across a large number of heterogeneous control units can severely compromise the validity of the counterfactual when pre-treatment fit is noisy (Doudchenko and Imbens, 2016; Ferman and Pinto, 2021). Our approach enforces sparsity instead, shifting the focus to a selective set of control units that are structurally similar to treated units in absolute levels. As demon-

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[23] Formally, the unit weights $\hat{\omega}^{sdid}$ in Arkhangelsky et al. (2021) are obtained by solving the regularized minimization problem: $(\hat{\omega}_0, \hat{\omega}) = \underset{\omega_0,\omega}{\arg\min} \left\{ \sum_{t=1}^{T_{pre}} \left(\omega_0 + \sum_{i=1}^{N_{co}} \omega_i Y_{it} - \frac{1}{N_{tr}} \sum_{i=N_{co}+1}^{N} Y_{it} \right)^2 + \zeta^2 T_{pre} ||\omega||_2^2 \right\}$ where ω_0 is an intercept term allowing for level differences and ζ is a regularization parameter that penalizes weight concentration. To combine the behavior of the SDiD with the default values of the SC, we restrict this minimization by fixing $\omega_0 = 0$ and setting the penalty $\zeta \approx 0$ (effectively removing the ridge penalty). This reduces the objective function to the constrained least squares problem of Abadie et al. (2010), forcing the algorithm to match outcome levels rather than parallel trends and prioritizing the selection of a sparse set of control units (best fit) over the variance-reduction benefits of dispersed weights.

[24] Interpolation bias occurs when an algorithm assigns weights to control units with fundamentally different structural characteristics than the treated unit, simply because their weighted average coincidentally matches the treated unit's pre-treatment trends. In highly heterogeneous datasets, this creates a synthetic counterfactual that overfits to idiosyncratic noise rather than identifying truly comparable peers (Abadie, 2021).

strated by Ferman (2021), enforcing these strict SC-style constraints acts as a necessary regularization mechanism that prevents spurious fits when the number of control units is large relative to the pre-treatment periods. By adopting this hybrid SDiD specification, we ensure a rigorous pre-treatment fit via SC-style unit weights, while retaining the advantages of SDiD regarding time-weighting and robust large-panel inference. Results using this specification will be labeled *SDID-SCM*.[25]

5 Main Results

The estimation results from Equation (1) are outlined in Table 4. Columns (1) and (2) show our estimation results for the SDiD estimation with a regularized SC weighting algorithm (*SDID-SCM*), with and without the inclusion of covariates. We further subsample the estimation by weekend (Friday-Sunday), during the week (Tuesday-Thursday) and road types. The dependent variable in all four columns is the natural logarithm of monthly traffic volume; thus, coefficients are interpreted as semi-elasticities (e.g., a value of -0.01 implies a reduction of approximately 1 %). The ATT $\hat{\tau}^{sdid}$ is shown together with the standard error in parentheses below each ATT.

We find no evidence of a systematic long-lasting reduction in traffic volumes. The point estimate including all countries, road types and covariates (Column (2)) is economically negligible ($\hat{\tau} = -0.005$, s.e. 0.011) and statistically indistinguishable from zero. This null result holds regardless of the inclusion of meteorological covariates, suggesting that the policy intervention cannot induce a measurable modal shift at the aggregate level. Stratifying the sample by road type and time of week confirms the absence of overall average treatment effects across all subsamples.

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[25] All estimations have been conducted based on R *synthdid* package from Arkhangelsky et al. (2021): see `https://synth-inference.github.io/synthdid/index.html`.

Countries	Road Type/Covariates	(1) No	(2) Yes
All	Both	-0.015 (0.016)	-0.005 (0.014)
All (Friday - Sunday)		0.018 (0.020)	0.011 (0.018)
All (Tuesday - Thursday)		-0.016 (0.010)	-0.010 (0.011)
All	Main Roads	-0.009 (0.013)	-0.013 (0.014)
	Motorways	-0.009 (0.013)	-0.006 (0.012)

Table 4: Estimation results from equation (1) for the ATT measured by $\hat{\tau}^{sdid}$. Covariates include mean temperature, mean wind speed and the number of ice days. Columns (1)-(2) shows estimation results for *SDID-SCM* without and with covariates included. Note: Standard errors (in parentheses) are computed using a block bootstrap stratified by treatment status with 200 replications

To evaluate the temporal dynamics of the policy, we extract the time-varying treatment effect derived from the SDiD estimation procedure. While Equation (1) calculates a single average treatment effect ($\hat{\tau}^{sdid}$) across the entire post-intervention period, the underlying algorithm constructs a counterfactual that can be decomposed over time. Specifically, the estimation procedure first determines the optimal unit weights ($\hat{\omega}_i^{sdid}$) and time weights ($\hat{\lambda}_t^{sdid}$). To isolate the effect for any specific month t, we apply these fixed unit weights to the control pool to compute the synthetic spatial match for each month, and adjust it by time weights. The difference between the actual treated outcome and this adjusted counterfactual yields the month-specific ATT in the post-treatment phase, as well as a pseudo-ATT for each pre-treatment month (serving as a visual diagnostic for pre-trend fit). Figure 3 presents these time-varying estimates for the specification in Table 4, column (2), including covariates for all countries and all road types. The upper panel displays the curve for each point in time together with the respective 95%

bootstrapped confidence interval.[26] The black dotted line marks the introduction of the *Deutschlandticket*, while the red dashed line shows the ATT, averaged over all post periods. The lower panel displays time weights $\hat{\lambda}_t^{sdid}$ for every month in the pre-treatment period. Although the ATT of -0.005 (as can be seen in Table 4 and displayed by the red dashed line) is not statistically different from zero, the effect over time is heterogeneous. We observe a significant, transient reduction in traffic volumes in June, July, and August 2023, peaking with a magnitude of approximately 0.075 in July. However, this effect dissipates rapidly; after September 2023, estimates are statistically indistinguishable from zero. Even the same summer months (June, July and August) in 2024 show no evidence of a deviation in travel patterns.

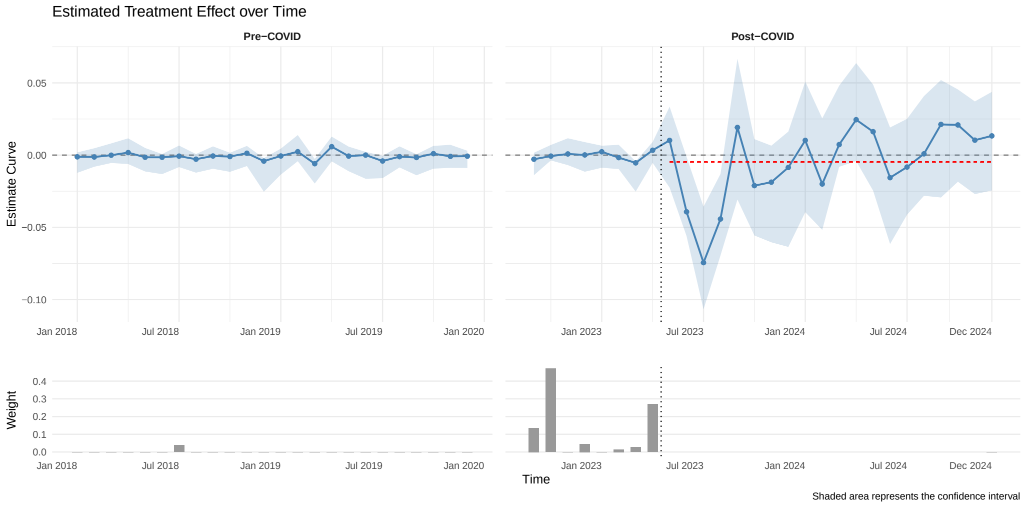

Figure 3: Estimation of the monthly treatment effect (blue marks) using the *SDID-SCM* approach with corresponding bootstrapped 95% confidence interval (blue shaded area) is displayed in the upper panel. The black dotted line marks the introduction of the *Deutschlandticket* in May 2023. The red dashed line represents the average treatment effect after treatment was introduced. The lower panel shows the weighting of time periods ($\hat{\lambda}_t^{sdid}$). Results correspond to Table A.1, SDID-SCM with covariates using all countries and road types. Note: Confidence intervals are computed using a block bootstrap stratified by treatment status with 200 replications

To validate our findings, we also estimate the model using the standard *SDID* weighting algorithm proposed by Arkhangelsky et al. (2021). Although

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[26] The original *synthdid* R package from Arkhangelsky et al. (2021) implementation does not provide functionality for bootstrapping the time-varying treatment effect curve. We therefore extended the code to compute pointwise 95% confidence intervals using the block bootstrap procedure.

this specification yields a statistically significant but small reduction in traffic ($\hat{\tau} = -0.015$), diagnostics reveal that it fails to construct a valid counterfactual in our setting, i.e., the pre-trend deviations are substantial and significantly different from zero, violating the parallel trends assumption (see Appendix Table A.1 and Figure A.4). The discrepancy stems from the standard algorithm's reliance on trend-matching via unit fixed effects and a strong emphasize on the ridge penalty (see Section 4), which in our high-dimensional sample leads to a spurious fit by averaging hundreds of heterogeneous, low-quality control units (see Appendix Figure A.1) to minimize variance at the cost of bias. Because traffic volumes vary drastically between major transit corridors and minor regional roads, constructing a synthetic German highway out of hundreds of fractionally weighted, low-volume foreign roads empirically produces an invalid counterfactual. In contrast, our preferred *SDID-SCM* specification restricts the weighting algorithm to match on traffic levels and enforces sparsity (see Figures A.2 and A.3 which highlight that 31 counting points make 90% of the synthetic control weights with a strong reliance on Great Britain) by reducing the regularization penalty (Arkhangelsky et al., 2021, p. 4092). As evidenced by the superior pre-treatment fit in Figure 3 (compared to Figure A.4), this selectivity minimizes bias effectively by filtering out irrelevant variation from dissimilar roads and irrelevant control stations. Consequently, we interpret the significant estimates from the standard algorithm as artifacts of poor pre-treatment fit rather than evidence of a structural modal shift.[27]

6 Robustness and Sensitivity Analyses

This section assesses the robustness of our main findings along two dimensions. First, we examine the sensitivity of the estimates to the composition

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[27] Furthermore, we conducted a placebo randomization where we bootstrapped a 50/50 split of the control units 200 times (see Figures A.5 and A.6). In each iteration, half of the donor pool was randomly assigned as a placebo treated unit while the remaining half served as the control group, allowing us to construct an empirical distribution of treatment effects under the null hypothesis of no effect. The results confirm the standard bootstrap analysis.

of the donor pool by systematically excluding individual control countries (a "leave-one-out" analysis). Second, we calculate the Minimum Detectable Effect (MDE) to determine whether our statistically insignificant results reflect a true absence of treatment effects or merely a lack of statistical power.

Table 5 presents the results of the leave-one-out analysis, designed to verify that the baseline estimates are not driven by idiosyncratic trends in any single control country. We re-estimate the model by systematically excluding individual countries from the donor pool (rows 1–4) and restricting the donor pool to specific sub-regions (rows 5–6). This leave-one-out approach verifies whether the inclusion of a specific country (e.g., Great Britain or Switzerland) is disproportionately influencing the construction of the synthetic control.

The results demonstrate high stability in non-significance even when we systematically exclude entire countries from the donor pool. Across all permutations – whether excluding individual countries or restricting the sample to sub-region – the point estimates remain statistically indistinguishable from zero, fluctuating between -0.002 and -0.021. Even the model estimation that yields the largest coefficient (-0.021) -— achieved by excluding Finland -— remains statistically insignificant. Similarly, restricting the sample to the DACH region or to Finland and Great Britain, yields estimates close to zero (-0.007 and -0.009, respectively). This consistency confirms that the null result is not driven by idiosyncratic trends in the Nordic or British road networks, but reflects a robust absence of modal shift in the treated German units.

	(1)	(2)
Countries/Covariates	No	Yes
Without Austria	-0.011 (0.016)	-0.002 (0.012)
Without Switzerland	-0.017 (0.018)	-0.016 (0.014)
Without Finland	-0.016 (0.011)	-0.021 (0.012)
Without Great Britain	-0.010 (0.015)	-0.010 (0.015)
Only DACH	-0.019 (0.014)	-0.007 (0.012)
Only Finland and Great Britain	-0.012 (0.015)	-0.009 (0.013)

Table 5: Sensitivity analysis results from Equation (1). Columns (1) and (2) display estimates for the *SDID-SCM* algorithm (with and without covariates). Rows represent samples with specific control countries excluded (leave-one-out) or regional subsamples (DACH, Finland/GB). Note: Standard errors (in parentheses) are computed using a block bootstrap stratified by treatment status with 200 replications.

To formally distinguish whether our statistically insignificant estimates reflect a true absence of effect or merely a lack of statistical power, we calculate the Minimum Detectable Effect (MDE). Following the standard framework (see e.g., Duflo et al., 2007), the MDE is defined as the smallest true effect size that our specific research design has a specified probability (power) of detecting at a given significance level.

Technically, the MDE is calculated $(t_{1-\alpha/2} + t_{1-\beta}) \times SE(\hat{\tau}^{sdid})$, where $SE(\hat{\tau}^{sdid})$ is the standard error of the point estimate, $t_{1-\alpha/2}$ is the critical value for the significance level α and $t_{1-\beta}$ is the critical value for the desired statistical power $(1-\beta)$. For a standard two-sided test at $\alpha = 0.05$, $t_{1-\alpha/2}$ is 1.96, testing against Type I errors (false positives) and for the standard power benchmark of 80 %, $t_{1-\beta}$ is 0.84, testing against Type II errors (false negatives). The sum of these thresholds (roughly 2.8 for 80 % power) effectively determines how many standard errors away from zero the true effect must be for us to reliably detect it.

	Target Statistical Power for MDE		
Covariates	**50% Power**	**80% Power**	**90% Power**
No	0.0311	0.0445	0.0515
Yes	0.0271	0.0387	0.0448

Note: Minimum Detectable Effect (MDE) calculated as $(t_{\alpha/2} + t_{1-\beta}) \times SE$. Significance level α set to 0.05.

Table 6: Results for the MDE.

Table 6 presents the MDE sensitivity analysis. First, the inclusion of meteorological covariates significantly improves precision; adding weather controls reduces the MDE at 50 % power from 3.11 % to 2.71 % (at 80 % power from 4.45 % to 3.87 %.) Consequently, we can confidently rule out the existence of treatment effects larger than approximately 3-4 % – magnitudes typically required for an effect to be considered *economically* meaningful (as discussed in Section 7). Crucially, this degree of statistical power is driven by our rich dataset, which features a large number of cross-sectional units ($1,372$ counting stations) and an effective 50/50 split between treated and control stations. These results confirm that the failure to find a significant average treatment effect is not driven by a lack of statistical power, but rather provides robust evidence that the policy did not generate a lasting modal shift of relevant magnitude, but only a short-run effect (see Figure 3).

7 Discussion

Our primary empirical finding – a statistically insignificant effect of the *Deutschlandticket* on highway traffic volumes – warrants careful economic interpretation. Although point estimates are negative, suggesting a reduction in traffic volume, they are economically negligible and statistically indistinguishable from zero. A statistically significant temporary reduction of up to 7.5 % is detected only in July 2023, indicating a causal yet strictly temporary effect of the *Deutschlandticket*.

To put the effect of 7.5 % in July 2023 into perspective, Figure 4 translates the estimated relative treatment effect over time into *absolute* vehicle counts per counting station (utilizing the percentage effects of Figure 3). The top panel demonstrates that the synthetic counterfactual accurately tracks the seasonal fluctuations of average German traffic volumes during the pre-treatment period. Following the introduction of the *Deutschlandticket* in May 2023, the distinct divergence emerges. As explicitly detailed in the bottom panel, the peak relative reduction in July 2023 corresponds to an absolute decrease of approximately 68,000 passenger vehicles per counting point (approximately 2267 vehicles per day), relative to a synthetic baseline of roughly 916,000 vehicles. The visualization reinforces the strictly transient nature of this shift. The absolute difference rapidly reverts toward zero by late autumn of 2023, exhibits no significant structural or lasting divergence (see Figure 3) in the subsequent summer of 2024.

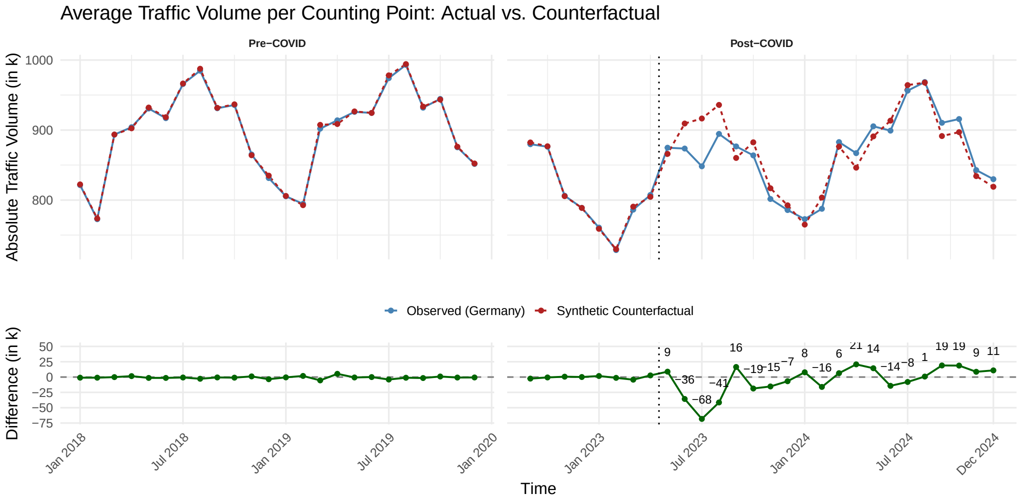

Figure 4: Average monthly traffic volume per counting station (in thousands) comparing the observed average German traffic volume to the synthetic counterfactual. The top panel displays the absolute volume levels, demonstrating the pre-treatment fit. The bottom panel illustrates the absolute difference (Observed minus Counterfactual), with numeric labels indicating the exact deviation in the post-treatment period. The vertical dotted line marks the introduction of the *Deutschlandticket* in May 2023.

As demonstrated by the MDE analysis, the null result is not an artifact of insufficient statistical power; rather, aggregate *average* traffic reductions larger than 3% can be confidently ruled out. In line with findings from the literature (see Section 2) this suggests that the (short-run) cross-price elasticity between private driving and PT is extremely low, even in the face of a drastic price reduction for the substitute good. As the literature on cross-mode substitutability demonstrates, policies exclusively targeting public transit fares are structurally ill-suited to reduce car usage. Cats et al. (2017) establish that travelers are substantially more responsive to policies that increase the generalized cost of driving than to those that decrease the monetary cost of PT. This asymmetry aligns with recent evidence evaluating the *9-Euro-Ticket*, where strong mode choice inertia prevented significant substitution away from private vehicles (Guajardo Ortega and Link, 2025). Ultimately, the anticipated modal shift is bounded by non-monetary costs – specifically travel time, reliability, and convenience – which a pure fare reduction intrinsically fails to mitigate.

Although the aggregate effect is not evident, the short-run reduction in traffic volumes in the first summer of implementation (June–August 2023) likely reflects a novelty effect or follow-the-crowd behavior during the vacation season (similar to the *9-Euro-Ticket*), where leisure travel is more flexible than commuting. However, this effect dissipates rapidly, and does not occur in 2024. The lack of persistence suggests that while the ticket may induce experimentation or occasional leisure usage, it has failed to alter the habitual commuting patterns that drive baseline traffic volumes from private vehicles. This contrasts with the hypothesis that a permanent ticket would allow households to make long-term adjustments (e.g., reducing vehicle ownership), suggesting that reliability concerns may outweigh the financial savings for daily commuters.

We interpret this lack of substitution in the context of binding supply-side constraints, a mechanism well-documented in recent evaluations of the German rail network. As demonstrated by Liebensteiner et al. (2024) and Lu et al. (2024), the massive demand shock induced by fare reductions significantly exacerbated station crowding and train delays. If perceived transit quality degrades due to these external capacity constraints, the cross-price elasticity of demand effectively drops to zero, particularly for car commuters with a high value of travel time savings. Consequently, injecting a substantial demand-side subsidy without simultaneous supply-side capacity expansion –

in a network already facing severe investment gaps (Krebs and Steitz, 2021) – results in a structural mismatch. Hence, in light of the findings of this paper, it may be economically more meaningful to allocate funds toward supply-side investments in the public transport sector, rather than subsidizing a uniform ticket price, particularly if the goal is to induce a modal shift.

Our analysis is subject to specific data limitations. First, our dataset covers extra-urban highways and main roads. While this captures the bulk of suburban-to-urban and regional commuting we cannot observe changes in purely inner-city traffic volumes. It is plausible that the *Deutschlandticket* induced stronger spatial substitution effects within short-distance urban transit networks – a dynamic documented by Schlett and Loder (2025) – which remains unobservable by highway counters. Second, we lack observations of rail journeys. Because we only observe that private car volumes remained statistically unchanged, we cannot determine if rail journeys increased due to new, induced demand (trips that would not have been undertaken otherwise). If the ticket primarily subsidized existing rail passengers and induced new mobility from other modes (e.g. by foot or bike) rather than reducing car trips, the net environmental benefit of the policy remains highly ambiguous.

The absence of a significant reduction in traffic volume has profound implications for the cost-effectiveness of the *Deutschlandticket* as a climate policy instrument. While we cannot rule out modal shifts within purely inner-urban transit networks, our findings indicate that the policy failed to decarbonize the energy-intensive regional commuting sector. Given that medium-to-long distance commuting accounts for a disproportionate large share of transport emissions compared to short inner-city trips (see, e.g., Wadud et al., 2024; Leroutier and Quirion, 2022), the lack of response on these roads suggests that the aggregate environmental impact of the ticket is likely lower than anticipated by policy makers. Although the policy succeeds as a social transfer by relieving the financial burden on existing passengers, its efficacy and efficiency as a tool to reduce high-mileage private transport appear limited. With annual direct costs of approximately 3 billion Euros – shared equally between the federal government and the states – the implicit cost per ton of CO_2 abated is likely prohibitively high given the negligible decrease in vehicle kilometers traveled. While the policy succeeds as a social transfer by relieving the financial burden on existing passengers, its efficacy as an environmental policy is limited.

Furthermore, the fiscal sustainability of such demand-side subsidies is questionable. The subsidy of 3 billion Euros annually absorbs substantial public funds. Simultaneously, the cap on ticket prices restricts the ability of transport operators to generate the revenue necessary for maintaining service quality, and distorts the price signal required for efficient capacity allocation. Given that Krebs and Steitz (2021) estimate an investment gap of 50 billion Euros for the digitization and expansion of the German rail network by 2030 and considering that German rail infrastructure is almost entirely dependent on public grants to fund network development (Götz and Schäfer, 2020), our findings suggest that reallocating these resources toward supply-side infrastructure improvements – thereby reducing non-monetary costs such as delays and overcrowding – may yield higher long-term economic returns than subsidizing fares. This is corroborated by empirical evidence from (Schäfer, 2026), who demonstrates that railway funding structures achieve higher efficiency when public funds are prioritized for upstream infrastructure management rather than downstream operational subsidies.

Finally, the temporal horizon and ongoing policy uncertainty play a crucial role in shaping the observed demand responses. Although the *Deutschlandticket* was introduced as a permanent intervention, continuous political debates regarding future price increases and funding availability generate substantial uncertainty for consumers. As established in the literature, PT demand is significantly more elastic in the long run than in the short run; long-run elasticities are typically two to three times larger because permanent price signals are required to induce discrete, structural household adjustments, such as modifying vehicle ownership or residential location (Holmgren, 2007; Litman, 2004; Wardman et al., 2018). Consequently, if consumers cannot reliably anticipate the long-term persistence and price stability of the ticket, cross-mode substitution away from private cars will remain bounded by the rigid, short-run elasticity estimates.

8 Conclusion

In this paper, we evaluate the impact of the *Deutschlandticket* on passenger vehicle traffic using a SDiD approach by Arkhangelsky et al. (2021). Our central empirical finding is that this nationwide fare reduction failed to induce a lasting modal shift away from private cars. While we identify a short-term reduction in traffic volumes during the initial three months (June to August 2023) of the policy, this substitution effect rapidly dissipates and does not reappear in the subsequent year, effectively ruling out purely seasonal fluctuations.

Ultimately, these results highlight the limits of price instruments in transport markets. Our findings are consistent with the hypothesis that in capacity-constrained networks, non-monetary costs – such as reliability, punctuality, and comfort – act as binding barriers to substitution. When public transit quality is degraded by infrastructure deficits, even drastic price reductions fail to outweigh the generalized costs of switching from a private car.

From a policy perspective, this suggests that the current strategy of subsidizing demand is insufficient to decarbonize the regional commuting sector. Rather than indefinitely funding fare subsidies that function primarily as social transfers, our analysis suggests that long-term environmental goals would be better served by redirecting public funds toward supply-side infrastructure investments. Future research should leverage granular mobility data (e.g., Schlett and Loder, 2025) to explore potential heterogeneity between rural and urban areas and the general long-term effects of the *Deutschlandticket*, but the aggregate evidence indicates that without providing sufficient funds for rail infrastructure, cheaper tickets will fail to lower traffic volume on roads, let alone induce to a modal shift necessary to *derail* the car.

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Declarations of competing interests

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Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the authors used OpenAI's ChatGPT, Google's Gemini and Writefull in order to improve language. After using these services, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

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A Appendix: Additional Figures and Tables

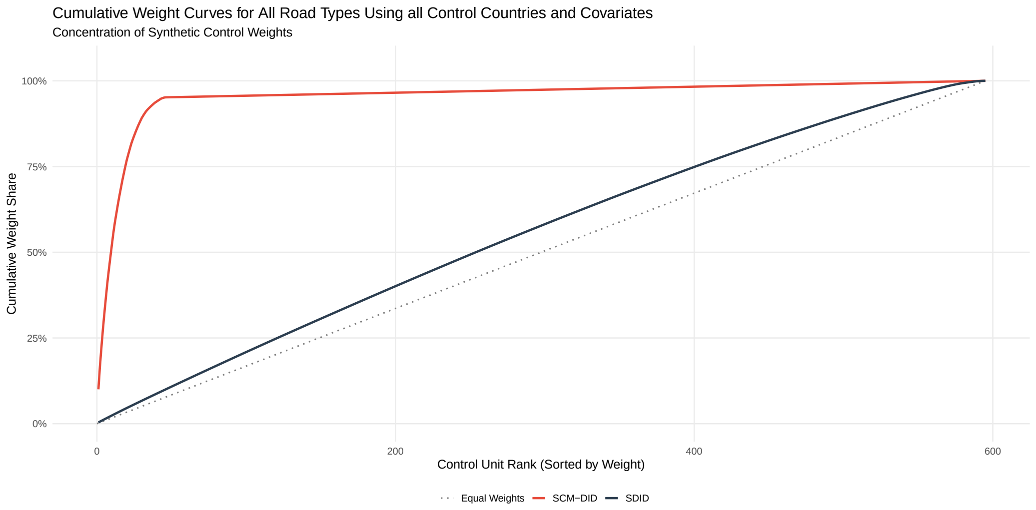


Figure A.1: Cumulative share of weights assigned to control units, sorted by weight magnitude. The red curve refers to the *SDID-SCM* model with covariates (Table 4, Column (2)). The black curve refers to the standard *SDID* with covariates (Table A.1, Column (2)). The dotted line represents the case of equal weights per unit.

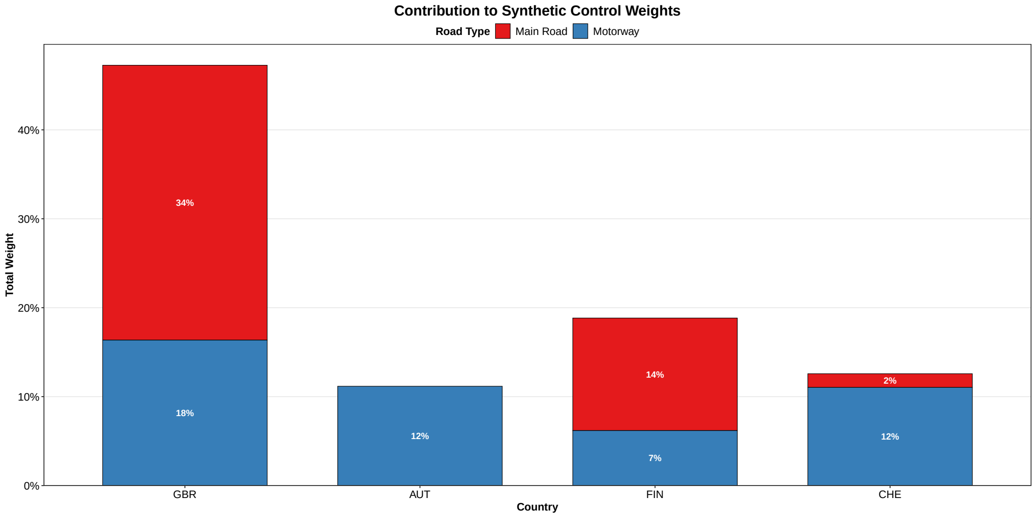


Figure A.2: Distribution of the top 90 % of synthetic control weights by country and road type from the *SDID-SCM* model (Figure A.1).

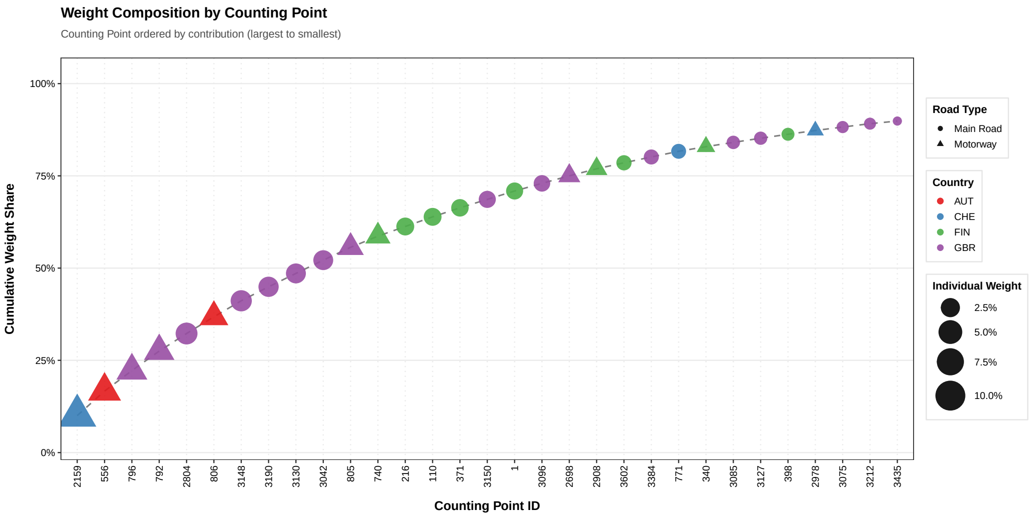
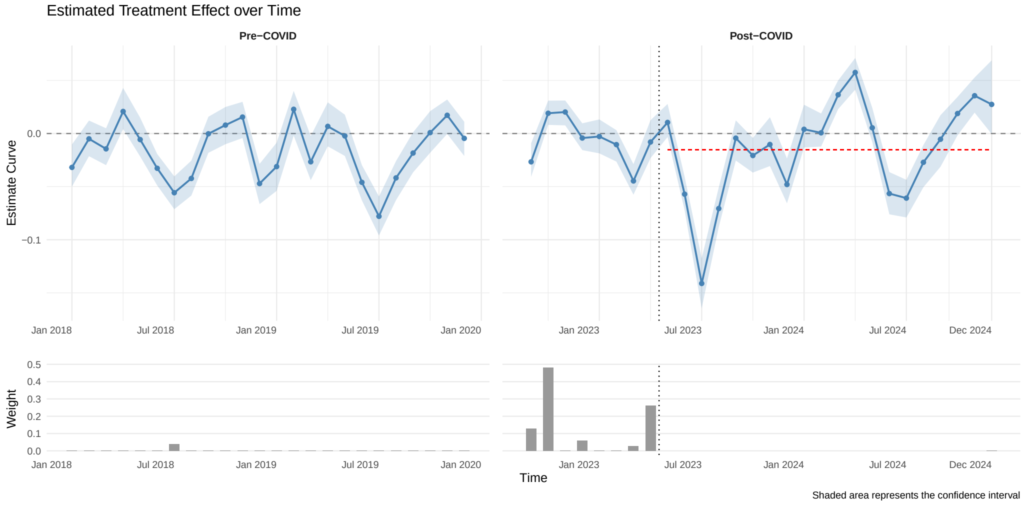


Figure A.3: Concentration of the top 90 % of weights among individual counting points (31 points), ordered by weight, from the *SDID-SCM* model (Figure A.1).

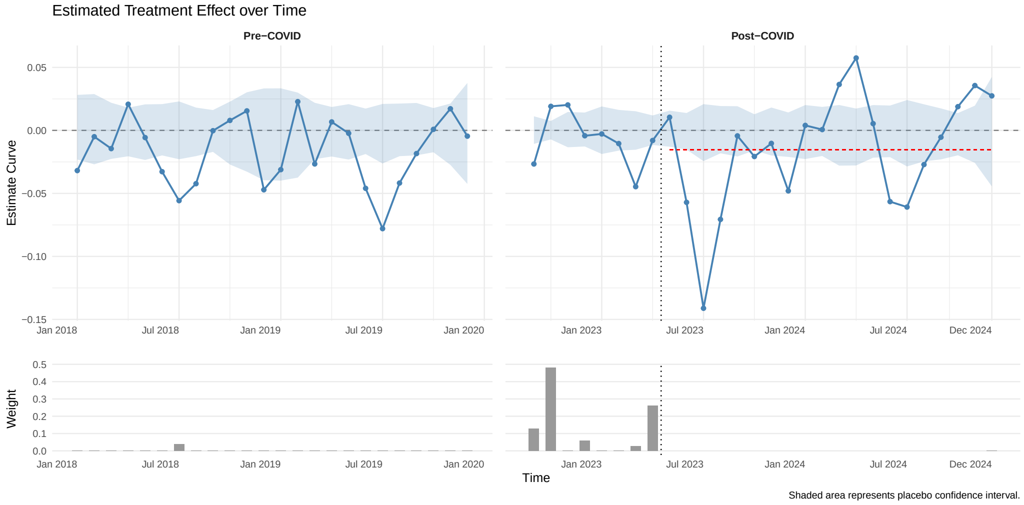
		(1)	(2)
Countries	Road Type/Covariates	No	Yes
All	Both	-0.007	-0.015
		(0.005)	(0.006)
All (Friday - Sunday)		-0.002	-0.010
		(0.006)	(0.008)
All (Tuesday - Thursday)		-0.009	-0.022
		(0.005)	(0.006)
All	Main Roads	-0.009	-0.020
		(0.010)	(0.011)
	Motorways	0.007	0.007
		(0.010)	(0.009)

Table A.1: Estimation results from equation (1) for the ATT measured by $\hat{\tau}^{sdid}$. Covariates include mean temperature, mean wind speed and the number of ice days. Columns (1)-(2) shows estimation results for SDiD standard values. Note: Standard errors (in parentheses) are computed using a block bootstrap stratified by treatment status with 200 replications



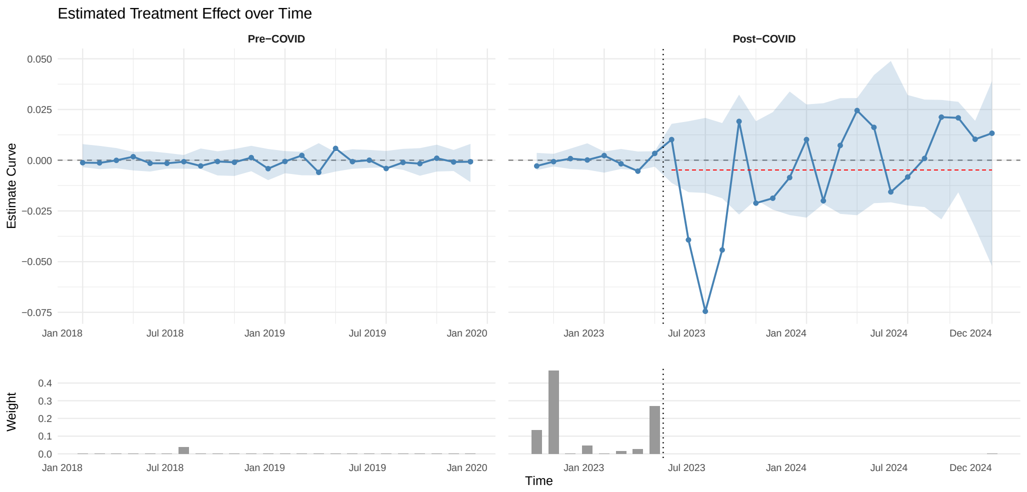
Shaded area represents the confidence interval

Figure A.4: Estimation of the monthly treatment effect (blue marks) using the SDiD approach with corresponding bootstrapped 95% confidence interval (blue shaded area) is displayed in the upper panel. The black dotted line marks the introduction of the *Deutschlandticket* in May 2023. The red dashed line represents the average treatment effect after treatment was introduced. The lower panel shows the weighting of time periods ($\hat{\lambda}_t^{sdid}$). Results correspond to Table A.1, Column (2), *SDID* with covariates using all countries and road types. Note: Confidence intervals are computed using a **block bootstrap** stratified by treatment status with 200 replications



Shaded area represents placebo confidence interval.

Figure A.5: Estimation of the monthly treatment effect (blue marks) using the SDiD approach with corresponding 95% placebo-based confidence interval (blue shaded area) is displayed in the upper panel. The black dotted line marks the introduction of the *Deutschlandticket* in May 2023. The red dashed line represents the average treatment effect after treatment was introduced. The lower panel shows the weighting of time periods ($\hat{\lambda}_t^{sdid}$). Results correspond to Table A.1, Column (2), *SDID* with covariates using all countries and road types. Note: **Placebo-based intervals** are computed using a 50/50 split of control units with 200 replications.



Shaded area represents placebo confidence interval.

Figure A.6: Estimation of the monthly treatment effect (blue marks) using the *SDID-SCM* approach with corresponding 95% placebo-based confidence interval (blue shaded area) is displayed in the upper panel. The black dotted line marks the introduction of the *Deutschlandticket* in May 2023. The red dashed line represents the average treatment effect after treatment was introduced. The lower panel shows the weighting of time periods ($\hat{\lambda}_t^{sdid}$). Results correspond to Table 4, Column (2), *SDID-SCM* with covariates using all countries and road types. Note: **Placebo-based intervals** are computed using a 50/50 split of control units with 200 replications.