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The causal effect of wrong-hand drive vehicles on road safety*

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Abstract

Left-hand drive (LHD) vehicles share higher road accident risks under left-hand traffic because of blind spot areas. Due to low import prices, the number of wrong-hand drive vehicles skyrockets in emerging countries like Georgia, Kyrgyzstan and Russia. I identify the causal effect of wrong-hand drive vehicles on road safety employing a new "backward version" of the synthetic control method. Sweden switched from left-hand to right-hand traffic in 1967. Before 1967, however, almost all Swedish vehicles were LHD for reasons of international trade and Swedish customer demand. I match on accident figures in the period after 1967, when both Sweden and other European countries drove on the right and used LHD vehicles. Results show that right-hand traffic decreased road fatality, injury and accident risk in Sweden by approximately 30 percent. An earlier switch would have saved more than 4,000 lives between 1953 and 1966.

JEL classification: R41; K32; C53

Keywords: Road accidents; Sweden; natural experiment; synthetic control method

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1. Introduction

International mobility and vehicle imports have triggered a safety debate on the regulation of left-hand drive (LHD) vehicles in left-driving countries. If the vehicle configuration does not fit the traffic side, large blind spot areas arise that increase road accident risk, especially when overtaking or changing lanes (for an illustration see, Figure 1). For example, more than 10,000 LHD heavy goods vehicles use British roads each day. These LHD trucks are highly overrepresented in UK accident data (Danton et al. 2009).

[Figure 1 about here]

A pertinent question therefore is whether the control of wrong-hand drive vehicles can enhance road safety. Remarkably, regulation varies substantially across countries, ranging from total bans to virtually no limitations. Australia, Bangladesh, Kenya, New Zealand, and Singapore, for example, basically banned the import of non-vintage LHD vehicles. Similarly, right-hand drive (RHD) vehicles are not permitted in right-hand traffic countries such as Brazil, the Philippines, Taiwan, and some European countries (Croatia, Lithuania, Poland, Slovakia). Other European Union member states, however, have not imposed any regulations on RHD vehicles, including, Germany, France and Spain. Hungary and Romania allow RHD vehicles under certain conditions. In all these countries, however, wrong-hand drive vehicles are more or less uncommon.

In stark contrast, wrong-hand drive vehicles have become a major issue in emerging countries, especially in states that were once part of the Soviet Union. Cars imported from Japan are usually cheaper, come with less mileage than US or European cars, and are of higher quality than Russian made cars.[1] Lower prices leads to a skyrocketing number of wrong-hand drive vehicles in countries like Georgia, Kyrgyzstan and Russia. These countries have right-hand traffic but experienced a drastic influx of cheap Japanese made RHD imports in recent years. Georgia is the most prominent case in point. The number of imported and newly registered RHD surged astronomically and now even exceeded the number of LHD imports. By 2015, around one out of four registered cars in Georgia was RHD. Similar trends apply to Kyrgyzstan and Russia. By December 2013, 127,383 out of 991,888 registered cars in Kyrgyzstan were RHD. In Russia, around 1.5 million cars were estimated to be RHD (3.6% of total vehicles); in some Eastern Russian regions close to Japan, RHD vehicles have a market share of up to 60%.[2]

Why are wrong-hand drive vehicles cheaper than their better-fitting counterparts? One major reason might be that wrong-hand drive vehicles bear higher accident risks. Car prices do only partly internalize excessive risks because wrong-hand drive vehicles impose additional safety externalities on other road users. The number of traffic accidents in Kyrgyzstan with RHD cars involved (24 %) is reported to be as twice as high as share of registered RHD vehicles (13 %). In right-driving Canada, there is a similar safety discussion in many provinces over how to regulate imported RHD vehicles from the UK and Japan.[3] Canadian car dealers admit that RHD vehicles "[…] aren't designed for our roads, and thus pose a greater safety risk in this country than left-hand drive vehicles" (Cohen 2010). Descriptive statistics on road accidents in Canada underpin safety concerns. Cooper et al. (2009) show that RHD vehicles share higher accident rates of 30% compared to LHD vehicles. Tardif and Baril (2009) document an increase in accident rates of 32% for men at the age of 16 to 34 using RHD vehicles compared to LHD

[1] See, for example, the *Georgian Journal* online newspaper (https://www.georgianjournal.ge/society/31555-driving-in-georgia-doing-it-right.html).
[2] For the Russian case see *Sputnik* online news (https://sputniknews.com/analysis/20121003176382673/).
[3] Approximately 95,000 RHD vehicles are registered in Canada, see Kent (2011). RHD vehicles thus account for around 0.5% of all light road motor vehicles in Canada.

drivers of the same age. A causal relationship, however, has not been revealed yet. Regressing traffic accident figures on vehicle configuration is likely to lead to biased OLS estimates. First, driving a somehow more risky LHD vehicle in a left-hand traffic country might be endogenous to driver's preferences and further unobservable characteristics. Second, foreignness and vehicle configuration are often difficult to separate.

This study aims at identifying the causal effect of wrong-side drive vehicles on road safety using a natural experiment in Sweden. Swedes drove on the left side of the road for 233 years. For reasons of international trade and Swedish customer demand, however, "cars in Sweden typically had the steering wheel on the left, leading to many accidents, especially on narrow roads" (Hipple 2014).[4] On September 3, 1967 ("*Högertrafikomläggningen*"), Sweden switched from left-hand traffic to right-hand traffic following the convention of continental Europe, and Swedish vehicles remain LHD. The traffic side change constitutes a drastic and unique shift in standards (Konrad and Thum 1993). I apply a new "backward version" of the synthetic control method to this setting: I match on accident figures in the period *after* 1967, when both Sweden and other European countries drove on the right and used LHD vehicles. Results show that the switch to right-hand traffic decreased road fatality, injury, and accident risk by approximately 30%. An earlier switch would have saved more than 4,000 lives between 1953 and 1966. Imposing tough regulations on wrong-hand drive vehicles is therefore highly recommended.

[4] The president of Volvo, Assar Gabrielsson, described Swedish vehicle demand in 1936 as follows: "Through this, the Swedish people has become used to have the steering wheel on the left side, in spite of Sweden having left-hand traffic. In most other countries, the steering wheel is located at the right side when the traffic is left-hand, or at the left when traffic is right-hand. We at Volvo are fully convinced that taking the road standard into consideration, the left shoulder is of little or no importance. It is much more important to have a clear view of the road ahead when overtaking. Therefore, the most logic thing would be that Volvos were made with right-hand drive. In spite of this, we have kept left-hand drive because we do not feel that we have to be pioneers in this area. We believe that we would only meet resistance from our customers and create extra work for our dealers if we only delivered right-hand drive Volvos. We will therefore continue to sell left-hand drive cars." Cited after Volvo Owners' Club (2007).

2. Identification

I employ the synthetic control method developed by Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2015) in a rather new fashion; OLS and WLS difference-in-differences estimates as proposed by DeAngelo and Hansen (2014) validate the results as I will show later on. The basic idea of the synthetic control method is to construct a synthetic counterfactual for a treated unit by matching on non-treatment period trends of the outcome variable of interest. The counterfactual is a simple weighted average of untreated control units from a proper donor pool; weights sum to one. Further (cross-sectional) matching predictors can be used to improve the comparability of counterfactual and treated units.

In the basic version of the synthetic control method, the non-treatment period is followed by the intervention period. In the Swedish case, however, this would require a donor pool of countries with widespread wrong-hand drive vehicles both before and after 1967 in order to construct a post-1967 counterfactual to Sweden. However, there is no country worldwide where wrong-hand drive vehicles are conventionally used, pre-1967 Sweden being the sole exception. Therefore, I modified the strategy and match on *post*-intervention trends instead on *pre*-intervention trends. After 1967, both Sweden and other European countries drove on the right and used LHD vehicles. The treatment of the traffic side change therefore harmonized the institutional framework across Sweden and the rest of Europe. Conversely, pre-1967 times represent the treatment period of the idiosyncratic Swedish traffic law. I propose the identification assumption that pre-1967 accidents in Sweden would have evolved in the fashion of "Synthetic Sweden" if there had been right-hand instead of left-hand driving already before 1967. This modification of the synthetic control method is rather new and unusual. Being the sole exception, Bechtel et al. (2017) use a somewhat similar approach and match on non-treatment periods that are interrupted by treatment periods.

I collect data on road accidents from 1953 to 2012 in 14 European countries (including Sweden), where virtually all vehicles used throughout the period were LHD.[5] Data on road accidents, fatalities, injuries, road network length and the number of vehicles are obtained from annual publications of the United Nations. I collect information on GDP and population density from the Total Economy Database of The Conference Board. Data on seatbelt legislation and national general speed limits (dummy variables) are self-compiled.

Sweden is the only sample country that switched from left-hand to right-hand traffic. The donor pool weights (w_j) are derived in such a way that the post-1967 trend ($1968 \leq t \leq 2012$) in road fatalities of the treated unit Sweden (y_t) fits road fatalities of "Synthetic Sweden" ($\hat{y}_t = \sum_j \bar{y}_{jt} w_j$) best in terms of a minimized Root Mean Square Percentage Error ($RMSPE$); $\bar{y}_{jt}$ denotes road fatalities in donor pool country j in period t:

$$RMSPE = \sqrt{\sum_{t=1968}^{2012} \left[\frac{(y_t - \hat{y}_t)^2}{(2012-1968)}\right]} \quad (1)$$

In the absence of further policy changes, pre-1967 differences between Sweden and "Synthetic Sweden" give the causal effect of a wrong-hand drive vehicle configuration. I discuss the issue of simultaneous policy interventions in more detail later on. I use additional predictors such as GDP per capita, population density, vehicles per road kilometer (1968–1970 average), and dummies for mandatory seat belt usage and general speed limits (average for entire observation

[5] I started with a full sample of all 34 OECD countries. I dropped non-European countries (AUS, CAN, CHL, ISR, JAP, KOR, MEX, NZL, USA), left-hand traffic countries (GBR, IRL), and countries that do not exist for the whole period within their current external borders (CZE, DEU, EST, SVK, SVN). I further drop BEL, POL and GRC because of a serious lack of data. ISL changed from left-hand to right-hand traffic in 1968 and was also dropped. The remaining countries are: AUT, CHE, DNK, ESP, FIN, FRA, HUN, ITA, LUX, NLD, NOR, PRT and TUR.

period) to improve the fit of Sweden and “Synthetic Sweden”. The selection of these variables is based on recent studies on determinants of road accidents.[6]

[Table 1 about here]

3. Results

3.1 Baseline

A synthetic control unit consisting of 80.1% Norway, 16.2% Finland, and 3.7% Netherlands best fits the Swedish trend in road fatalities in the right-hand traffic period after 1967. On average, 92 out of one million inhabitants died in the course of traffic accidents between 1968 and 2012 in “Synthetic Sweden” which parallels to the Swedish figures of 90 road fatalities per million capita (see, Table 1). Figure 2 shows also a close fit of post-1967 Sweden and its counterfactual counterpart from a time series perspective (graphs at the right-hand side of the vertical line). Finally, “Synthetic Sweden” is also highly comparable to actual Sweden in terms of further post-intervention predictors (lower part of Table 1). Altogether, the fitting procedure yields a reasonable control unit to Sweden.

[Figure 2 about here]

Contrasting the times of joint right-hand traffic after 1968, pre-1967 figures differ substantially (Figure 2, left-hand side of the vertical line). In this period, Sweden droves on the left side but “Synthetic Sweden” droves on the right side of the road – both however used LHD. Before the switch to right-hand traffic, Sweden exhibited substantially higher fatality rates than its

[6] See, e.g., Grimm and Treibich (2012) and Lindo et al. (2016). Weights for predictors are derived according to their predictive power on the outcome as proposed by Abadie et al. (2015). I do not match over pre-1967 predictors, because figures might be endogenous to treatment-induced road safety outcomes.

synthetic counterpart. On average, Swedish fatality rates outweighed synthetic fatality rates by 41 fatalities per million capita before 1967 (see, Table 1). Thus, driving a wrong-hand side vehicle leads to an increase in road fatality risk of roughly 30% corroborating prior descriptive results by Cooper et al. (2009) and Tardif and Baril (2009). Given Sweden's population of 7.5 million inhabitants in the 1960s, an earlier switch would have saved more than 4,000 lives between 1953 and 1966.

3.2 Robustness

I conduct several robustness tests that underpin baseline results. First, I substitute road fatalities with road accidents and road injuries in order to account for the number and severity of accidents. I have to restrict the dataset to the years 1955 to 2002 because of missing data in several countries. On average, the number of road accidents was lower by 35% in "Synthetic Sweden" than in actual Sweden before 1967 (Figure 3, left-hand side, and Table 1). Road injuries would have been lower by 32% under right-hand traffic. These results strongly corroborate the findings for road fatalities (29%).

[Figure 3 about here]

Second, I test pseudo treatments. The synthetic control group method should not yield any effect in the absence of a treatment. First, I test Sweden's neighbor Denmark where there was always right-hand traffic. Both Denmark and Sweden simultaneously experimented with speed limits in the 1960s and Denmark implemented a general speed limit only few years after Sweden (Evans 1985). Both Scandinavian countries were similar in culture and policies are therefore somewhat comparable in terms of mid-1960s traffic regulation – the unique change of the traffic side in Sweden being the main exception. Since Denmark did not experience a shock in fashion of the Swedish case, one would not expect any treatment effect. To test this empirically, I apply a (placebo) treatment to Denmark. The left-hand graph of Figure 4 shows the results. As expected, “Synthetic Denmark” neither differs from actual Denmark before 1967 nor after 1967. Denmark is not a cherry picked case. I apply the pseudo treatment procedure to all countries in the dataset and compute RMSPE ratios. RMSPE ratios measure to which extent the non-treatment period fit differs from the treatment period fit of the synthetic unit and the treated observation (see, e.g., Abadie et al. 2015). Large ratios indicate substantial differences in fits. The right-hand right graph of Figure 4 shows that the ratio for Sweden is an exceptional outlier among all other sample countries including the Nordic countries of Denmark, Norway and Finland. Thus, Sweden is the sole European sample country that experienced an idiosyncratic shock at around 1967.

Third, I test whether results are robust to alternative estimation techniques. I perform two different difference-in-differences estimations with year and country fixed effects to validate synthetic control results and to avoid any concerns regarding the comparably large synthetic control weight of Norway. In a first set of difference-in-differences estimations, I rely on OLS and the full sample of 14 countries. Sweden is the treated unit, the 13 other countries are equally weighted. By definition, this procedure reduces the weight of Norway drastically below its synthetic control method weight of around 80 % and allows to assess the sensitivity of the counterfactual composition. As a downside, however, this procedure, may result in a violated common trend assumption. Therefore, I conduct a second set of estimations combining difference-in-differences with the synthetic control group as proposed by DeAngelo and Hansen (2014). The basic idea is to use donor pool weights derived by the synthetic control group as observation weights in a WLS difference-in-differences regression. This allows to derive difference-in-differences inferences for a counterfactual with an even closer fit in post-

1967 outcomes than in unweighted OLS regressions. OLS and WLS difference-in-differences estimations therefore differ in the usage of regression weights; both sets of estimations are specified as follows:

$$y_{it} = \alpha_i + \beta(Sweden_i \times Pre1967_t) + X_{it}{}'\gamma + \delta_t + \varepsilon_{it} \tag{2}$$

$$\text{with } i = 1, \dots, 14;\ t = 1, \dots, 59$$

with y_{it} denoting the outcome variable road fatalities, road accidents, or road injuries per one million capita. β is the coefficient of interest measuring the pre-1967 effect of Swedish left-hand traffic; $Sweden_i$ is a dummy that takes the value of 1 for Sweden and zero otherwise, $Pre1967_t$ equals one for the period before 1967 and zero otherwise. The vector X_{it} refers to control variables (seatbelt usage, speed limit legislation). α_i and δ_t represent country and year fixed effects, ε_{it} is the error term. I estimate the models with standard errors robust to heteroskedasticity (Huber-White sandwich standard errors, see Huber 1967 and White 1980).[7]

Table 2 compares the results of the synthetic control method (columns (1) to (3)) to OLS and WLS difference-in-differences estimations. Column (4) shows that the OLS difference-in-differences coefficient for pre-treatment Sweden amounts to 56.7 road fatalities per million capita which is larger than the synthetic control method findings (40.6 road fatalities per million capita, column (1)). By contrast, if using synthetic control weights in a WLS estimation, post-1967 effects (31.5 fatalities per million capita) are smaller compared to synthetic control group results. Both OLS and WLS estimates are at least significant at the 5% level. By contrast, neither seatbelt nor speed limit legislation predict road fatalities. Difference-in-differences

[7] Inferences do not change when standard errors are clustered at the country level. I stick to robust standard errors because the maximum number of clusters is however fairly low.

estimates for road accidents and road injuries per million capita do also corroborate synthetic control findings (compare columns (2), (5) and (8) in Table 2 for road accidents, and columns (3), (6) and (9) for injuries). Difference-in-differences estimates fairly reproduce the synthetic control group findings for road accidents whereas difference-in-differences for road injuries are somewhat smaller than the related synthetic control findings. All OLS and WLS difference-in-differences coefficients, however, turn out to be statistically different from zero. In conclusion, I can exclude that the composition of the counterfactual, i.e., the comparably large synthetic control weight of Norway, drives the results.

4. Excluding other channels

The main assumption of the synthetic control method is the absence of simultaneous policy changes other than the treatment of interest. In the following section, I show that neither speed limit legislation, public transport, nor accidents caused by tourists drive the results.

4.1 Speed limit legislation

Around 1967/1968, Sweden did not only change to right-hand traffic but also extended its highway summer speed limit to an all-season general speed limit (Evans 1985). Speed limit legislation thus overlap with the traffic side change to some extent which may seriously challenge the results. I use speed limit legislation in the synthetic control method as predictor which should reduce biases to some extent. However, I present four further reasons why decreases in road accidents are likely to be caused by the traffic side switch and not by speed limits.

First, if all-season speed limit legislation would have been effective, we would expect that the relative share of winter accidents decrease and the share of summer accidents increase –

compared to summer speed limit times. To test this consideration, I collect data on the monthly distribution of road fatalities in Sweden just before and after the traffic side switch. Figure 5 shows that the monthly distribution of road fatalities hardly changed after the speed limits were rolled over to the entire year. The share of road fatalities in June and July somewhat increase under the all-season speed limits but the share for August and September decreased. This finding does not suggest that speed limit legislation drive accidents.

[Figure 5 about here]

Second, speed limits before 1967 mainly applied to highways and therefore non-built-up locations. If speed limits were effective, the distribution of accidents across built-up and non-built-up areas should have changed after general speed limits replaced the location specific speed limits in 1967. Figure 6 however show that there was no shift in the location of road accidents in Sweden after 1967.

[Figure 6 about here]

Third, the change of the traffic side mainly affected drivers of cars whose field of view was considerably bad before 1967. Motorcycles, by contrast, have a single and fixed position of the driver and were therefore arguably less affected by left-hand traffic (only indirectly by riskily overtaking cars). Both types of vehicles however are subject to speed limits. If speed limits have caused the decrease in accidents, one should observe comparable effects for both types of vehicles. I collect data on road causalities (injuries and fatalities) by the type of the vehicle involved (car, motorcycle) for Sweden and for its main synthetic counterpart country, Norway. I compute the difference between both types of vehicles in the annual growth rates of road causalities per million capita. Figure 7 show the results of this procedure for the period before

1967 and after 1967 comparing Sweden to Norway. Bars larger than zero show that car related road causalities per capita increase faster than road causalities by motorcycles for all years. After 1967, this differential was at 0.8 to 0.9 percentage points in Sweden and in Norway. In Norway, we observe a comparable differential also before 1967 (0.9 %). In pre-1967 Sweden, by contrast, road causalities of car passengers grew around 10 % faster than road causalities of motorcycles. Thus, Swedish cars were exposed to a drastically higher increase in causality risk before 1967 compared to motorcycles. This finding of an asymmetrical effect of the pre-1967 period on cars and motorcycles gives further support to the hypothesis that results are driven by the change of the traffic side rather than by speed limits.

[Figure 7 about here]

Fourth, and finally, difference-in-differences estimates indicate that speed limit legislation does not predict fatality figures in the dataset of 14 European countries. Table 2 shows that dummies for speed limit legislation are not significantly correlated with road fatalities and road accidents per capita. I find a significantly negative effect of speed limits on road injuries which however do not change the effect of the traffic side switch (compare columns (6) and (9) to column (3) in Table 2. This finding is in line with the traffic safety literature which generally acknowledges at least some accident severity reducing effects of speed limits (e.g., Elvik 2005, Vadeby and Forsman 2017). Evidence for Scandinavian countries in the mid-1960s however is mixed because effects mainly depend on driver’s compliance with speed limits; speed limits do not necessarily translate in a reduction of actual speed (see Færdselssikkerhedskommissionen 1966, Haight 1977, Nilsson 1982; see also Heydari et al. 2014). Moreover, recent evidence from Denmark shows that raising speed limits can even lower accident rates because risky overtaking decreases (Road Safety GB 2014). Against this background, effects of the Swedish general speed limit legislation are far from clear and might be less important than the traffic side switch.

Altogether, I do not find evidence that the change from temporal and spatial speed limits to a general speed limit changed the distribution of road accidents across time and space. Moreover, 1967 legislation seem to have asymmetrical effects on road causalities involving cars and motorcycles. I do also find evidence that speed limit legislation predict fatality figures in the dataset of 14 European countries. Altogether, I conclude that speed limit legislation should not drive the results.[8]

4.2 Other safety regulations

Further safety regulations were introduced in Sweden and in the main donor pool country, Norway, at about the same time. For example, in both countries, seat belt usage became mandatory in 1975 (see Lindgren and Stuart 1980, Assum 2003). Other important measures, such as alcohol legislation and vehicle inspections, do not predict road accidents in Sweden and Norway (Lindgren and Stuart 1980, Fosser 1992).[9] Finally, safety requirements, e.g., for tires, sun visors, or windshields, were not introduced in Sweden until 1971, and they mainly affected vehicles of model year 1969 and later only.[10] In conclusion, widespread wrong-hand drive vehicles remain the main difference between pre-1967 Sweden and "Synthetic Sweden".

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[8] This corroborates ambiguous findings of prior studies on the effect of speed limits on road fatalities. See, for example, McCarthy (1993). See also recent results from an experiment in Denmark: Road Safety GB (2014).

[9] Findings for the US and the UK are contradictory as well. See Cotti and Walker (2010), Lovenheim and Slemrod (2010), Green et al. (2014).

[10] On the effects of post-1971 road safety measures in Sweden, see Elvik et al. (2009).

4.3 Public transport

I can also exclude that public transport challenges the results. If bus doors open on the "wrong side", passengers will be exposed to substantially higher accident risks. In contrast to the common LHD passenger cars, however, busses in Sweden were usually RHD before 1967. After 1967, Sweden quickly replaced approximately 8,000 RHD busses with LHD busses. Public transport thus always fit the traffic side, so it should not drive accident figures.

4.4 Tourism

Finally, tourists and commuters from right-driving Europe might be an issue. However, accidents involving foreigners only accounted for 2% of total accidents 1966.[11] Thus, the results are not biased by tourists.

5. Conclusion

I show that adjusting the traffic side to the common LHD vehicle configuration decreased road fatality, injury and accident risk in Sweden by approximately 30% which is substantial. Translating this finding to UK figures, around 370 accidents involving 520 casualties would have been avoided in 2015 if LHD were banned from British roads; accidents with heavy goods vehicles could be reduced by 2.3% from approximately 6,040 to 5,900.[12] In Kyrgyzstan, around 70 out of the 920 annual road fatalities and 675 out of 9,370 road injuries in 2014 could be

[11] *Arkansas Highway Magazine* (September 1967) reports that 2,000 foreigners were involved in road accidents in Sweden in 1966. Given the average number of 1.4 persons per (fatal or injury) accident, foreigners account for 2.2% of the 63,451 total accidents. Figures: *Annual Yearbook of Sweden* (1968).

[12] According to the UK Government Department for Transport, LHD vehicles account for 1,240 of 140,056 accidents and 1,745 of 186,189 causalities in 2015. I assume that accidents caused by LHD decrease by 30%.

avoided by banning RHD vehicles.[13] Against the background of a comparable share of RHD vehicles and traffic injury figures, similar figures apply to Georgia.

From an isolated road safety perspective, the results clearly imply that tough regulations on the import of wrong-hand drive vehicles should be legislated. However, it is unclear whether the costs of bans outweigh the benefits. This might even more true for emerging former Soviet countries where wrong-hand drive vehicles have become increasingly popular in recent years. Newspapers reported that thousands of RHD drivers and car dealers protested in Tbilisi against the Georgian government's plan to ban RHD imports in late 2015; similar protests occurred in Kyrgyzstan.[14] Russia introduced tariffs of 100% to 200% on RHD vehicles from Japan in 2008 which leads to massive riots in Siberia. "Protests swept Vladivostok, with people on the streets swiftly moving from economic demands to anti-governmental slogans. Thousands of motorists blocked highways and even tried (unsuccessfully) to storm a local airport."[15] Political leaders reacted to protest and intervene harshly (Russia) or postponed planned import bans of wrong-hand drive vehicles (Georgia and Kyrgyzstan). For countries such as Georgia or Kyrgyzstan, changing the traffic side as in the case of Sweden may become a reasonable solution in future years if the rapid trends toward RHD vehicles continue. As an alternative, politicians should consider to impose mandatory *Fresnel* window lenses which may help improve driver's field of vision, but are less incisive than car bans.

[13] Figures on accidents taken from *KabarNews* online newspaper (see http://old.kabar.kg/eng/society/full/13263 and http://old.kabar.kg/eng/society/full/11738). Again, I assume that accidents caused by wrong-hand drive vehicles decrease by 30%.
[14] See *Georgia Today*, 8.–10.12.2015, p. 2 and footnote 2.
[15] *Sputnik* online news (https://sputniknews.com/analysis/20121003176382673/).

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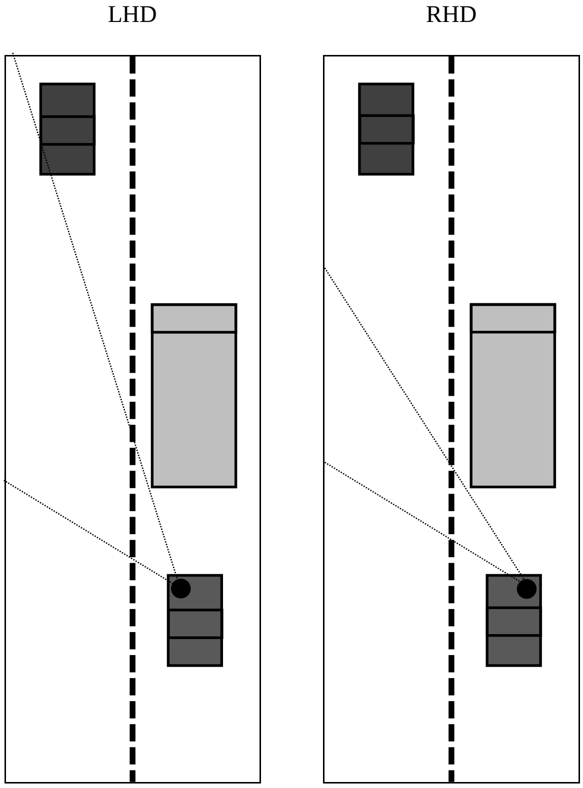
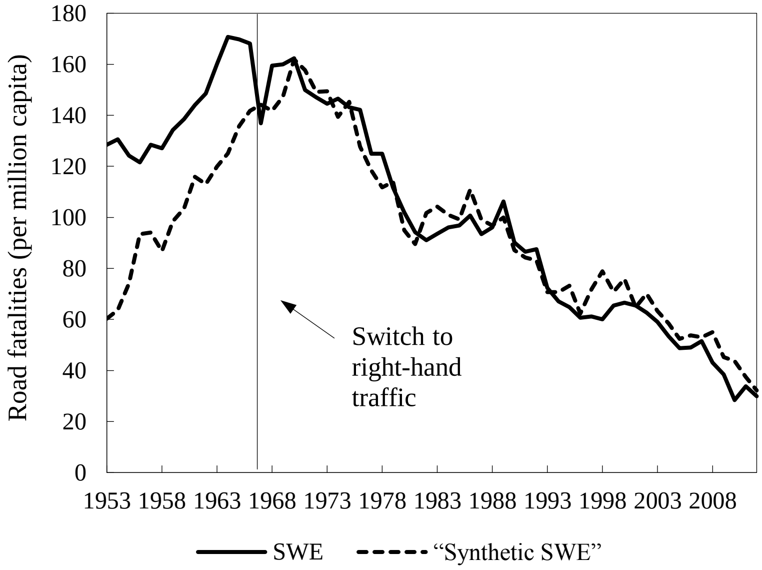
FIGURE 1. FIELD OF VIEW UNDER DIFFERENT VEHICLE CONFIGURATIONS



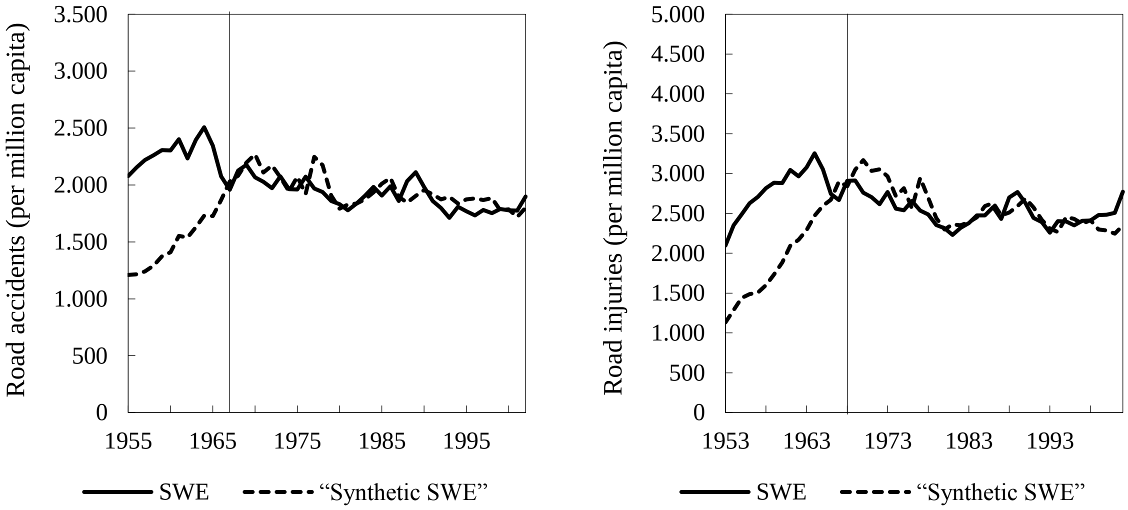
Notes: The figure shows the field of view for the driver of an LHD and RHD vehicle under right-hand traffic. The mirrored configuration applies to left-side traffic.

FIGURE 2. ROAD FATALITIES IN SWEDEN



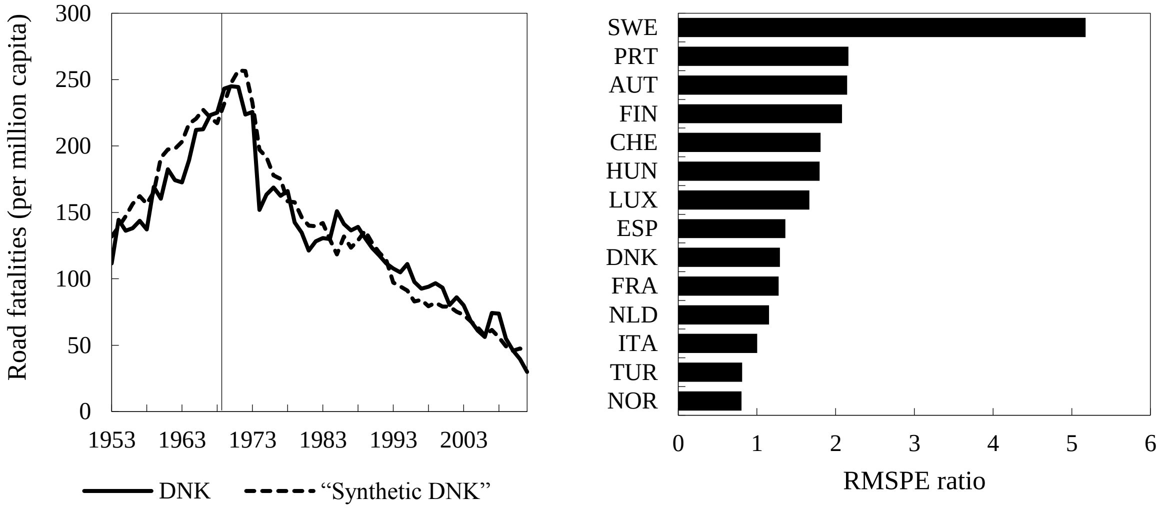
Notes: The figure depicts road fatalities per million capita in Sweden and its synthetic counterpart. "Synthetic Sweden" consists of 80.1% Norway, 16.2% Finland, and 3.7% Netherlands. The vertical solid line represents the year 1967, when Sweden switched to right-hand traffic.

FIGURE 3. ROAD ACCIDENTS AND INJURIES



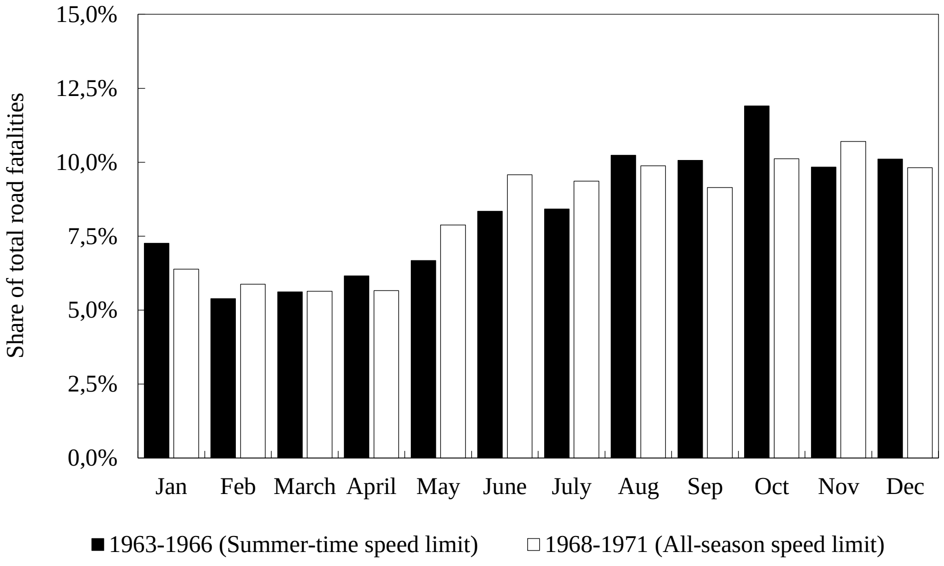
Notes: The figure depicts road accidents and injuries per million capita in Sweden and its synthetic counterpart. The vertical solid lines represent the year 1967, when Sweden switched to right-hand traffic. Left-hand side (accidents): "Synthetic Sweden" consists of 85.2% Norway, 4.7% Denmark, and 10.0% Turkey. Right-hand side (injuries): "Synthetic Sweden" consists of 68.5% Norway, 28.6% Finland, 1.7% Netherlands, and 1.2% Turkey.

FIGURE 4. PSEUDO TREATMENTS



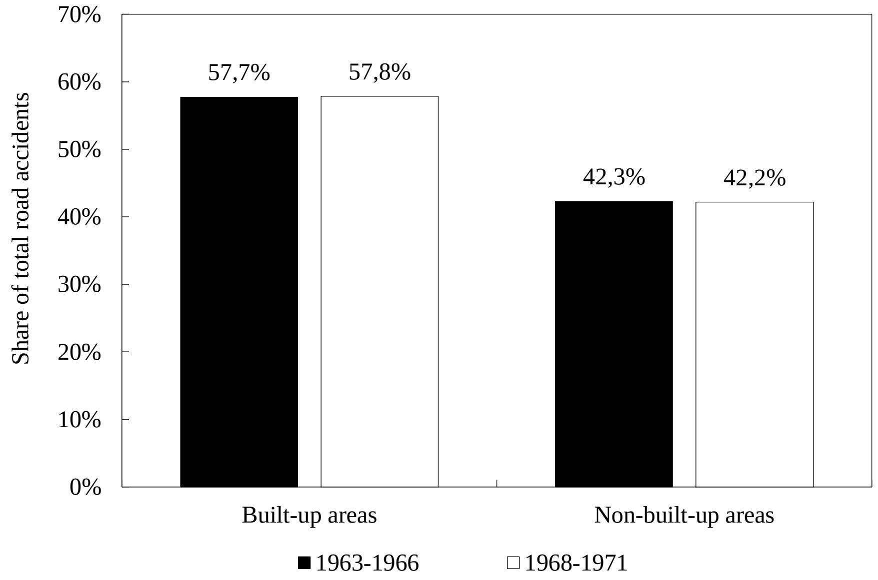
Notes: Left-hand side: The figure depicts road fatalities per million capita in Denmark and its synthetic counterpart. The vertical solid line represent the year 1967, when Sweden switched to right-hand traffic. "Synthetic Denmark" consists of 50.1% Finland, 28.4% Switzerland, 19.8% Netherlands, 1.6% Luxemburg, and 0.1% Norway. Right-hand side: Ratio of non-treatment period and treatment period RMSPE.

FIGURE 5. MONTHLY SHARES OF ROAD FATALITIES IN SWEDEN



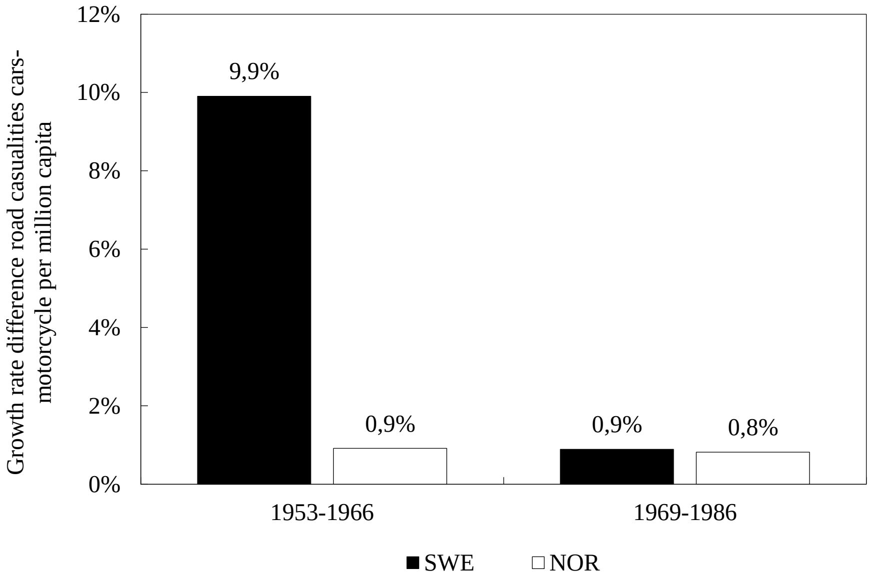
Notes: The figure depicts the distribution of Swedish road fatalities among months. Black bars represent the average monthly share of total Swedish road fatalities between 1963 and 1966 (summer-time speed limit), and white bars represent the average between 1968 and 1971 (all-season speed limit).

FIGURE 6. LOCATION OF ROAD ACCIDENTS IN SWEDEN



Notes: The figure depicts the distribution of Swedish road fatalities among locations. Black bars represent the share of total Swedish road accidents by location between 1963 and 1966 (no formal speed limits), and white bars represent the average between 1968 and 1971 (fade-in of speed limits, especially in non-built-up areas).

FIGURE 7. GROWTH RATE DIFFERENTIAL IN ROAD CASUALITIES CARS–MOTORCYCLES



Notes: The figure depicts the difference in growth rates of road causalities (injuries, fatalities) per million capita of car passengers vs motorcycle passengers 1967 (left-hand side) and afterwards (right-hand side). Black bars represent Sweden, white bars represent Norway. For example, in pre-1967 Sweden, road causalities by car accidents increase 9.9% faster than causalities caused by motorcycles.

TABLE 1. SUMMARY STATISTICS

	Sweden	*"Synthetic Sweden"*	*Full sample (without Sweden)*	*Ratio "Synthetic Sweden"–Sweden*	*Period*
Before 1967					
Road fatalities	142	101	147	71%	1953–1966
Road accidents	2,274	1,482	3,503	65%	1953–1966
Road injuries	2,785	1,884	2,904	68%	1953–1966
After 1967					
Road fatalities	90	92	144	102%	1968–2012
Road accidents	1,911	1,949	3,811	102%	1968–2002
Road injuries	2,526	2,574	3,173	102%	1968–2002
Predictors					
GDP (Euro per capita)	22,909	22,751	19,282	99%	1968–1970
Population per km²	17.7	23.1	107.2	130%	1968–1970
Vehicles per km road	15.6	17.6	28.1	112%	1968–1970
Seatbelt legislation	0.6	0.6	0.6	100%	1953–2012
Speed limit	0.9	0.9	0.7	105%	1953–2012

Notes: The table shows summary statistics of outcome variables (upper panel) and predictors (lower panel). Road fatalities, accidents and injuries per million capita; 1967 excluded.

TABLE 2. DIFFERENCE-IN-DIFFERENCES

	Synthetic control group			*Difference-in-differences*			*Difference-in-differences using synthetic control group weights*		
				OLS			*WLS*		
	Road fatalities	*Road accidents*	*Road injuries*	*Road fatalities*	*Road accidents*	*Road injuries*	*Road fatalities*	*Road accidents*	*Road injuries*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(Sweden × Pre-1967)	40.60	791.74	901.45	56.71***	836.48**	797.59**	31.51**	640.03*	748.45***
	–	–	–	(14.18)	(319.81)	(324.70)	(7.59)	(255.66)	(128.04)
Seatbelt legislation				16.10	875.44	472.31*	-3.40	158.87	89.50
				(12.22)	(562.97)	(247.22)	(2.32)	(156.95)	(402.25)
Speed limit				-19.51	-479.96	-847.60*	-34.11	-612.53	-863.15**
				(19.94)	(553.50)	(417.23)	(15.12)	(464.36)	(209.00)
Year fixed effects	–	–	–	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	–	–	–	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	826	686	517	826	686	517	236	188	245
Countries	14	14	11	14	14	11	4	4	5
Within R^2	–	–	–	0.59	0.25	0.23	0.95	0.67	0.75

Notes: The table reports the effects of the switch to right-hand traffic in Sweden in 1967 on road fatalities, road accidents, and road injuries per million capita as dependent variable. The interaction term (Sweden × Pre-1967) equals one for Sweden in the period of left-hand traffic before 1967. Columns (1) to (3) report synthetic control group results (difference between actual and synthetic observation in pre-1967 period), columns (4) to (6) report (unweighted) OLS differences-in-differences estimates, and columns (7) to (9) report differences-in-differences estimates using the synthetic control weights (for weights see notes of Figure 2 and Figure 3) in WLS estimations. Significance levels (Robust standard errors in brackets): *** 0.01, ** 0.05, * 0.10.