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A radical change in traffic law: effects on fatalities in the Czech Republic

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Abstract

Josef Montag: **A radical change in traffic law: effects on fatalities in the Czech Republic**

This study examines short- and long-run effects of a new—stricter—road traffic law on traffic accident-related fatalities in the Czech Republic. The law introduced tougher punishments through the introduction of a demerit point system and a manifold increase in fines, together with augmented authority of traffic police. Identification is based on difference-in-differences methodology, with neighbouring countries serving as a control group. I find a sharp, 33.3%, decrease in accident-related fatalities during the first three post-reform months. This translates into 127 saved lives (95% confidence interval: 51, 204). The decline was, however, temporary; the estimates of the effects going beyond the first year are around zero. Unique data on traffic police activity reveal that police resources devoted to traffic law enforcement gradually declined. Tougher penalties have significant, but often short-lived effects. Weaker enforcement in the aftermath of such reforms may explain the absence of long-run effects.

Key words

Traffic law, traffic accidents, demerit point system, law enforcement.

JEL: J28, I12, I18.

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1 Introduction

Each year, road traffic accidents (RTAs) result in as many as 50 million injuries and more than 1.2 million deaths, making it the ninth leading cause of death worldwide—effecting especially young people. Enacting comprehensive laws with appropriate penalties and ensuring necessary resources for enforcement are acknowledged as critical instruments to improve road safety.[1] However, proper policy choice requires that we understand how alternative measures perform when put in place and how they interact with other key variables. Exploiting past policy experiments is a natural way to learn about these phenomena.

This study evaluates the effects of a new road traffic law in the Czech Republic that became effective on July 1, 2006. It was aimed at disciplining drivers and improving road traffic safety through tougher sanctions for traffic offences and strengthened authority of the police. The manifold increase in fines introduced by the law is illustrated in panel (a) of Figure 1. Yet, the greatest change was the introduction of a demerit point system (DPS): The law defines points that a driver receives for committing an offence; points range from zero to seven, depending on the offence. A driver who accumulates 12 points has her driver's licence immediately suspended for the following 12 months. The licence is then returned to the driver upon completion of a driving test and the driver continues to carry 12 points on her record (four points are deleted after each 12 months without an offence). The relative toughness of the Czech DPS can be illustrated by comparison with the Italian penalty point system: points are deducted from an initial 20-point endowment. After exhausting the endowment, Italian drivers must only attend a driving course. If they pass a driving test, their their point endowment is replenished.

2 Methods

2.1 Difference–in–differences estimator

This study evaluates the effects of the Czech traffic law reform using a difference-in-differences (DD) set-up, in which the neighbouring countries (Austria and Germany) serve as a control group. The well-known advantage of this approach is that any unobservable shocks to the outcome variable are controlled for, provided that they affect the treated and the control group.

Specifically, denote y the outcome of interest, then a DD regression can be written as

$$y_{rt} = \rho_r + \boldsymbol{\tau_t} + \boldsymbol{\beta'} C_c \boldsymbol{T_t} + \boldsymbol{\gamma'} \boldsymbol{x_{rt}} + \epsilon_{rt},$$

where c denotes a country; r denotes a region; t denotes a date; ρ_r is region r's fixed effect, capturing the initial differences in levels; $\boldsymbol{\tau_t}$ is a full set of time effects picking up common trends, shocks, and seasonal regularities; C_c is a dummy for the Czech Republic; $\boldsymbol{T_t}$ is a vector of time effects; $\boldsymbol{x_{rt}}$ is a vector of additional controls; and ϵ_{rt} is the residual. Specifically, $\boldsymbol{T_t}$ consists of dummies for four post-reform quarters and a dummy for the fifth to tenth quarter after the reform. The parameter of interest is the vector $\boldsymbol{\beta}$, which consists of five coefficients capturing the effects of the traffic law reform on fatalities over time. The DD model is estimated by OLS; reported standard errors are clustered on regions to allow for an arbitrary correlation of errors over time for a particular region.[2]

Statistical analysis was performed in R 2.15.3.[3] Data and code are available from the author upon request.

2.2 Data

I have collected monthly regional-level data on RTA-related casualties that occurred between January 2004 and December 2008 in the Czech Republic, Germany, and Austria, obtained upon specific requests from the Czech Traffic Police Headquarters and statistical offices of Germany and Austria. I then merged this data with yearly regional-level data on the population and number of cars from Eurostat and yearly country-level data on transport and economic statistics from the same source. Table I summarizes data on RTA-related casualties before and after the Czech traffic law reform.

Because the regions of the former East Germany exhibited a very different trend compared to the rest of the countries in the region—specifically, fatalities were falling much faster in Eastern Germany than elsewhere—East German regions were excluded from the data. In addition, there were discrepancies between the police data on fatalities in Prague and data data from the Institute of Health Information and Statistics. Prague exhibited very different behaviour from the rest of the country, constituting an outlier, and was dropped from the analysis.

2.3 The outcome of interest and measurement issues

I focus solely on fatalities as the outcome of interest. Police data on accidents and injuries are not directly comparable across countries and—more importantly—police data on RTAs as well as RTA-related injuries are very likely to suffer from reporting biases that are correlated with the new traffic law: (i) The amount of damage below which police do not have to be notified of an accident, unless an injury occurred, was raised from 20 000 CZK to 50 000 CZK (approximately 800 and 2 000 Euro). (ii) Severity of injuries is a continuous variable and judgement is required for its classification. Tougher punishments create incentives to avoid involving the police; simultaneously, minor injuries may be more often overlooked and injuries on the margin may be more often classified as light, rather than severe. I have found some evidence supporting these concerns. Such issues are absent in the case of fatalities. It is highly unlikely that an RTA-related fatality would go unnoticed by the police and there are no marginal fatalities that could be labelled differently.

As reported in Table I, the number of cars grew much more rapidly in the Czech Republic relative to its two neighbours. This suggests that the intensity of road traffic was changing and grew faster in the Czech Republic relative to the two neighbours. Under such circumstances, it is not feasible to take the number of accidents or fatalities in a country as a measure of traffic safety, as risk exposure changed. I therefore use the number of fatalities per one million cars as a measure of road safety and the main variable of interest. Of course, the number of kilometres per car as well as the average number of passengers in a car may change: according to the statistics from Eurostat, passenger-kilometres per car declined by almost 5% in the Czech Republic (kilometres driven grew by 20%, but the decrease in the number of passengers per car offset it) and did not change in Austria between 2004 and 2008 (data for Germany is not available). This indicates that using the number of cars as a measure of exposure to the risk of dying in a traffic accident is biased upwards in the case of the Czech Republic after 2006; i.e. it produces a bias towards finding a—long run—negative effect.

2.4 Validity of assumptions

The identifying assumptions of the DD estimator are: (i) a common trend between the treated and the control group; (ii) the absence of any unobserved shock specific either to the control countries or to the treated country. In other words, the DD estimator requires that after controlling for relevant differences between the control and treated groups, the only systematic difference between the two is the presence of the treatment.

There are good reasons to expect that factors generating shocks to RTA-related fatalities are shared among the Czech Republic, Austria, and Germany, so the neighbouring countries offer themselves as a natural control group. Specifically, common borders and the relatively small size of the Czech Republic make it likely that weather conditions will affect these three countries similarly. Exports represented over 60% of Czech GDP in 2006, and Germany is its main trading partner with a $1/3$ share of Czech exports. Thus, economic shocks and the corresponding changes in traffic intensity and freight transport are correlated across these three countries. Finally, there was no substantial policy change in either control country. Looking at the development of fatalities per million cars, panel (b) of Figure 1 provides visual evidence of similar (log-linear) trends before the traffic law reform as well as similar seasonal regularities across the Czech Republic, Austria, and the former West Germany. Additional formal tests of the validity of identification are reported in the Supplementary material.

3 Results

The estimates of the effects of the new Czech road traffic law on fatalities using the DD regression are reported in Table II. Base results in specification (1) indicate that the immediate effects of the law were substantial, but short-lived. The point estimate for the first post-reform quarter suggests that fatalities in the Czech Republic declined by one third ($[e^{-0.406} - 1] \times 100 = -33.37$). This point estimate translates into 127 saved lives over the initial 3-month period with the 95% confidence interval ranging from 51 to 204 lives. (Compare with 326 fatalities during the corresponding quarter in the previous year.) While the estimates for the second, third, and fourth post-reform quarters are about -10%, they are not statistically significant (individually or jointly; testing whether these three interactions are jointly equal to

zero yields $F(3, 1473) = 0.45$). Furthermore, the estimate of the last coefficient suggests that the long-run effects, that is, beyond the first year after the traffic law reform, were essentially zero.

The rest of Table II reports alternative specifications to check the robustness of these results. In specification (2) I drop the distant regions of Austria and Germany; neighbouring regions may be more alike in their behaviour over time. This results in somewhat smaller as well as shorter estimated effects, yet the big picture is not altered. In specification (3), I replace the outcome variable by the log of fatalities per passenger-kilometre, which is an alternative measure of traffic intensity (data is available only for Austria and the Czech Republic). The main findings are again corroborated, although the initial effects seem to decay even faster—everything beyond the initial 6 months has positive point estimates.

To check for potential pre-reform effects, I added dummies for two pre-reform quarters in specification (4). Both pre-reform coefficients are close to zero and the estimated effects are therefore the same as in specification (1). An additional analysis of daily country-level data on fatalities in Austria and the Czech Republic (provided by the same sources) did not reveal any signs of pre-reform effects. The law realised its first effects in the very first week it was enforceable. Details are available in the Supplementary material.

Specifications (5) and (6) in Table II re-estimate models (1) and (2) controlling for GDP per capita, the number of kilometres travelled by trucks and lorries, the number of cars per capita, and the average age of cars. The Czech economy grew faster as compared to Austria and Germany, which may have led to higher traffic intensity. The increase in freight transport that occurred after the Czech Republic joined the EU (May 1, 2004) was often criticized by media and politicians as adversely affecting the safety of Czech roads. Also, the number of cars per capita grew faster in the Czech Republic than in Austria and Germany; this may negatively influence the number of fatalities per car (see Table I), because the number of passengers in a car may decrease and the new cars may be safer. Lastly, the average age of cars may capture changes in the composition of the quality of cars.

The signs of coefficient estimates in specification (5) are as expected and the magnitudes are sensible, except freight transport does not seem to increase fatalities. Estimates in specifica-

tion (6) are qualitatively similar, but none of the control variables is statistically significant (the number of observations is smaller). Not surprisingly, introducing additional control variables results in less precision in the estimates of the effects of the traffic law reform, notably in the case of the estimate of long run effects. However, the pattern of results is stable, notably for specifications (1) and (5). This suggests that the controls do not add much information and the neighbouring countries are a valid control group.

4 Why were the effects shortlived?

The short life of the initial effects is a puzzle, and the empirical literature so far has not provided much insight into what causes such effects to be short-lived. I suggest that a combination of two factors, namely expectations and enforcement, may explain the pattern of results. A radical change in traffic law is *ex ante* ambiguous with respect to the magnitude of its effects on the expected punishment (that is the punishment multiplied by the probability of being caught). Facing such ambiguity, people may behave according to a pessimistic scenario.[4] (Indeed, it is quite smart to be pessimistic in the face of ambiguity, because—ex post—one will either have been right, or she will be positively surprised.) The politicians, the police, and the media may all have a tendency to exaggerate the magnitude of the expected effects. Finally, people may expect that tougher punishments generate more incentives for the police to enforce the traffic law.

However, economic theory suggests the opposite—enforcement levels may have gone down in the aftermath of the reform.[5] Resources allocated to traffic police have alternative uses, within the police as well as within the public sector in general. It is also plausible that when similar large-scale changes in the law are adopted, police resources may be already overstretched. An improvement in drivers' behaviour may thus result in reallocation of resources away from traffic law enforcement—part of which can be optimal from the efficiency point of view.[6] In addition, politicians and bureaucrats may have less interest in traffic safety, if things improve initially.

To gain an insight into this, I have collected a unique dataset from monthly regional-level reports on traffic police activity covering January 2006 to December 2007. The reports were

provided by the Czech Traffic Police Headquarters who receive them each month from the regional police directorates. The thrust of the of the findings is captured in Figure 2, which plots the changes of manpower, man-hours in enforcement, and the number of hours of speed gun use by the traffic police. While the number of policemen assigned to enforcement exhibits a general upward trend, the number of man-hours in traffic law enforcement were declining. The number of hours the police spent behind speed guns was falling even more rapidly. More formal regression analysis confirmed that the declines in enforcement man-hours as well as speed gun hours are substantively and statistically significant. Further details are available in the Supplementary material.

5 Discussion

5.1 Main finding of this study

Taken at face value, the point estimates account for 193 saved lives during the first 12 post-reform months—a 17.7% decline relative to the 12 months preceding the reform—however, almost two thirds of the effects were concentrated in the short period immediately following the reform.

5.2 What is already known on this topic

Evidence of the efficacy of similar traffic law reforms is mixed.[7] All studies from countries that have recently experienced traffic law reforms and introduced a DPS (Brazil in 1998, Ireland in 2002, Italy in 2003, Spain in 2006) have found substantial short-run improvements in RTA-related injuries and fatalities.[8–24] However, the effects going beyond the initial six months are ambiguous, as many of these studies are based on short-term data and there are contradictions among those that do look at long-run effects: One of three Brazilian studies finds sustained effects while the other two do not have appropriate data.[8–10] For Italy, one research group finds lasting effects while two others do not, and one study is inconclusive.[11–15] Similarly, in the case of Ireland, of five studies, one finds long-run effects, two do not, and two lack data.[16–20] On the other hand, four studies available for Spain agree on effects lasting at least 18 months.[21–24]

5.3 What this study adds

The inconsistencies in previous findings may be related to research design, which is based on before-after comparison within a country. The fragility of this method increases with the length of the period under study as other variables (trends, seasonal regularities, weather shocks, fuel prices, business cycle, etc.) are likely to interfere. A more robust methodology is utilised in this paper, yielding cleaner identification of both short and sustained effects; the identifying assumptions are discussed in detail and are shown to be reasonable. Another key contribution of this study is an analysis of the police enforcement activity around the reform.

5.4 Limitations of this study

The primary limitation of this study is the imprecision in the estimation of long-run effects—while the point estimates are consistently in the neighbourhood of zero, moderate negative or positive effects cannot be ruled out. This limitation is a property of the data at hand and may be addressed by future research by collecting and analyzing data from more countries that have undergone traffic law reforms. In addition, while this study attempts to investigate the mechanisms behind the observed patterns, data limitations allow only tentative conclusions; more work along these lines is needed.

6 Conclusion and policy implications

Despite the absence of identifiable long-run effects, this study does not imply that the law was not socially beneficial. Taking the most recent estimate of the value of a statistical life in the Czech Republic, the law generated social benefits of approximately 463 million Euro during the initial 12 months only (193 saved lives $\times$ 2.4 million Euro).[25] Note that this figure does not include the value of prevented injuries and material loss. No official estimate of the costs associated with the traffic law reform is available. Yet, it is unlikely that they would exceed these benefits. In addition, the reform created a legal environment that is closer to the standards found in the rest of Europe. By introducing the demerit point system, sanctions became more independent of income (at least formally), thus providing added deterrent for drivers who may not care about fines; as well as incapacitation of drivers with a high frequency

of traffic law violations—there were as many as 34 000 drivers whose driver's licences were revoked through DPS as of December 2011. Nevertheless, effective sanctions are also a function of enforcement. The expectation that a nominal increase in penalties will solve the problem creates a risk that enforcement will become undervalued as a vital component in policy.

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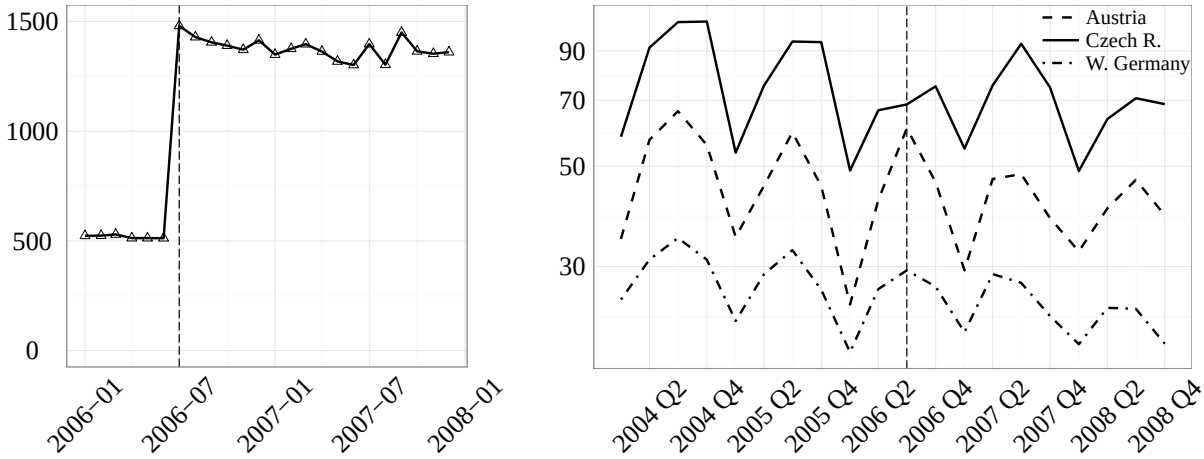
Figures



(a) Average fine for speeding

(b) Fatalities per 10^6 cars

Fig. 1 Panel (a): empirical fines for speeding in the Czech Republic (2006–2007). Panel (b): fatalities per million cars (2004–2008) in Austria, the Czech Republic, and Germany. The vertical line indicates the traffic law reform in the Czech Republic.

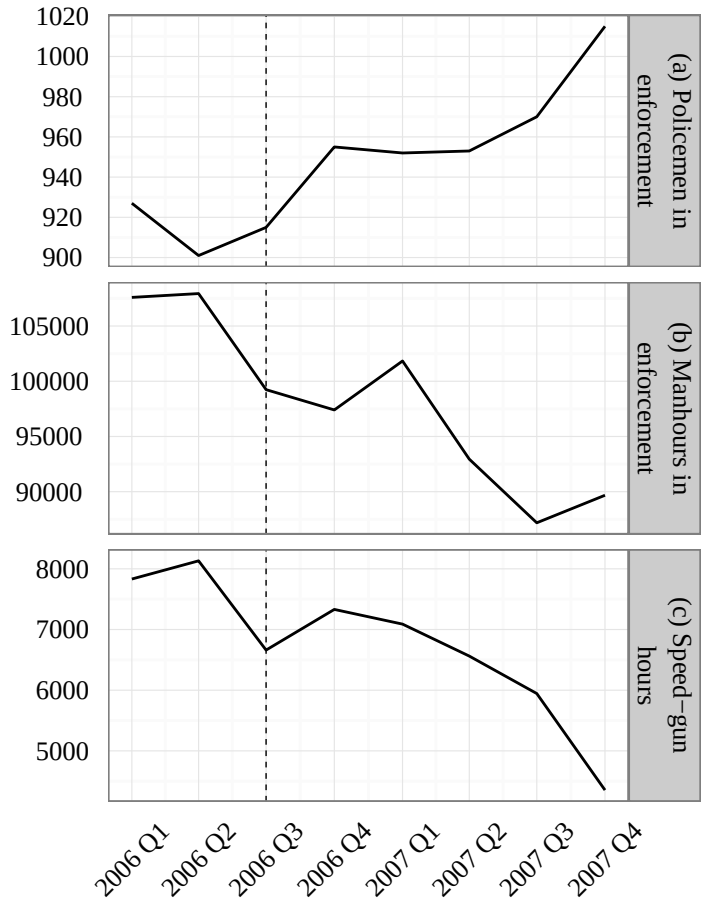


Fig. 2 Traffic police enforcement resources and activity in the Czech Republic (2006–2007)

Tables

Table I Summary statistics of RTA-related casualties in Austria, the Czech Republic, and Germany (July 2004–June 2008)

Country	Period	Per 10^6 inhabitants			Per 10^6 cars			Cars per inhabitant
		Fatalities	Serious injuries	Slight injuries	Fatalities	Serious injuries	Slight injuries	
A: Totals by period before and after July 1, 2006								
Czech R.	Before	221.4	888.8	5451.2	572.5	2298.3	14095.7	0.39
	After	202.8	760.0	4824.4	489.6	1834.7	11646.9	0.41
Austria	Before	188.9	1708.2	9793.6	372.9	3371.4	19329.7	0.51
	After	175.0	1718.5	9670.4	342.0	3359.5	18904.3	0.51
W. Germany	Before	125.4	1805.0	8759.7	222.6	3202.4	15541.1	0.56
	After	117.0	1777.1	8821.1	202.3	3072.8	15252.8	0.58
E. Germany	Before	153.3	2116.4	7739.2	305.3	4215.1	15414.0	0.50
	After	135.6	2036.2	7855.2	263.1	3949.3	15235.4	0.52
B: Proportional change between before and after periods								
Czech R.		−0.08	−0.15	−0.12	−0.15	−0.20	−0.17	0.071
Austria		−0.07	0.01	−0.01	−0.08	0.00	−0.02	0.010
W. Germany		−0.07	−0.02	0.01	−0.09	−0.04	−0.02	0.026
E. Germany		−0.12	−0.04	0.02	−0.14	−0.06	−0.01	0.027

The first and last 6 months were dropped to make the within-country comparison free of seasonal effects. The number of fatalities for the Czech Republic refer to people who died within 24 hours of an accident, for Austria and Germany to people who died within 30 days of the accident. The estimate of the number of cars in Germany was revised downward in 2007; since this variable was developing virtually linearly, I replaced the revised numbers with the linear extrapolation. Data sources: Headquarters of the Police of the Czech Republic, Statistics Austria, Federal Statistical Office Germany, and Eurostat.

Table II Effects of the new traffic law on fatalities

	(1)	(2)	(3)	(4)	(5)	(6)
1st quarter post-reform	−.406*	−.310*	−.424*	−.406*	−.416*	−.365*
	(.108)	(.099)	(.153)	(.106)	(.127)	(.132)
2nd quarter post-reform	−.102	−.005	−.145	−.101	−.113	−.049
	(.129)	(.134)	(.153)	(.126)	(.155)	(.166)
3rd quarter post-reform	−.143	−.132	.103	−.142	−.102	−.138
	(.147)	(.160)	(.235)	(.143)	(.158)	(.190)
4th quarter post-reform	−.073	.057	.054	−.072	−.060	.032
	(.104)	(.148)	(.150)	(.106)	(.145)	(.154)
5th–10th quarters post-reform	−.004	.031	.007	−.004	−.008	−.027
	(.058)	(.055)	(.081)	(.055)	(.137)	(.139)
1st quarter pre-reform				−.015		
				(.130)		
2nd quarter pre-reform				.023		
				(.123)		
Log GDP per capita					.848*	.966
					(.377)	(.572)
Log of freight transport					−.029	−.103
					(.457)	(.591)
Log cars per capita					−2.645*	−1.316
					(1.271)	(1.003)
Average age of cars					.328	−.035
					(.711)	(.499)
adj. R^2	.600	.549	.337	.600	.601	.548
N	1560	840	960	1560	1560	840

The outcome variable in specifications (1), (2), (4), and (5) is the log of monthly fatalities per 10^6 cars in Austrian, Czech, and German regions between 2004 and 2008. Specification (2) is run on a sample without the distant regions of Austria and Germany. The outcome variable in specification (3) and (6) is log fatalities per 10^{11} passenger-kilometres—data available only for Austria and the Czech Republic. All specifications include region dummies and an unrestricted set of month×year effects. Huber-White standard errors clustered on regions are in parentheses: * $p < 0.01$. Data sources: Headquarters of the Police of the Czech Republic, Statistics Austria, Federal Statistical Office Germany, and Eurostat.

Supplementary material

Statistical tests of identifying assumptions

Since the data used in this study span 30 months before the traffic law reform in the Czech Republic, we can use this information to probe the identifying assumptions of the DD regression empirically. Pearson's correlation coefficients of log monthly fatalities per million cars between the Czech Republic and either control country from January 2004 until June 2006 are above 0.7 and are statistically significant at any conceivable level.

To assess formally the pre-reform similarity between the Czech Republic and Austria and Germany, I construct a Chow test of systematic deviations of fatalities in the Czech Republic from the control group. Specifically, I take a pre-July 2006 subset of the data and run a regression of log-fatalities per car on regional fixed effects, a full set of time effects (i.e. year-month dummies), and a set of interactions between time effects and a dummy for the Czech Republic. Time effects in this regression pick up trends and shocks common to all three countries, while the interactions capture deviations specific to the Czech Republic. The test of the hypothesis that all coefficients on the interaction terms are equal to zero produces $F(29,754) = 0.71$. In other words, there is no evidence that fatalities before July 2006 behaved differently in the Czech Republic compared to Austria and Germany.

To double-check this contention, I run the same test using the Eurostat yearly regional-level data on fatalities between 1999 and 2005 for all neighbouring countries, replacing year-month dummies with year dummies. This data included Austria, the Czech Republic, former West Germany, Poland, and Slovakia. Testing the hypothesis that year effects for the Czech Republic are equal to zero results in $F(6,228) = 0.889$. Running the same test on data that include only the Czech Republic, former West Germany, and Austria produces $F(6,156) = 0.7947$.

Short–Run Development

The availability of daily country-level data on RTA-related fatalities in Austria and the Czech Republic makes it possible to study the response to the new law in more detail. To inspect the reaction of fatalities to the traffic law reform, I first index daily fatalities by the change in the

number of cars and standardize their values by demeaning and dividing by respective standard deviations of each country.[1] Then I run a regression of normalized fatalities on a constant, a dummy for the Czech Republic, a full set of week effects, and a full set of interactions between the dummy for the Czech Republic and week effects using daily data ranging from 2005 to 2008.[2]

Panel A of Figure S1 plots the demeaned coefficients on the interactions capturing the average weekly change in Czech fatalities net of common shocks.[3] To improve the readability of the figure, it shows only coefficients for an 18-month window around the reform. The first day of the reform, July 1, 2006, is marked by a solid vertical line, and dashed lines mark one week before and three months after the date. There is no apparent positive or negative trend during the pre-reform period, which is reassuring. Neither are there signs of pre-reform effects. On the contrary, the plot suggests that the law saw its first effects in the very first week it was enforceable. Importantly, the Figure S1 corroborates our main finding that the effects of the law were concentrated in the first three months, as most of the estimates for that period lie below the zero line. Specifically, panel A suggests that the bulk of the decline in fatalities was concentrated in July and October, the first and third post-reform month. Fatalities seem to revert to their pre-reform levels soon after this initial shock.

As an alternative specification, I run a regression of normalized daily fatalities on a constant, a dummy for the Czech Republic, a full set of week-of-the-year effects, a third-degree-polynomial trend, and interactions between the dummy for the Czech Republic and week effects spanning from six months before to 12 months after the reform.[4] The estimated coefficients on the interaction terms are plotted in panel B of Figure S1. Estimates are less erratic compared to panel A, as the restrictive model is likely to filter out some noise. The main difference between the two plots is that the rebound seems to be more gradual in panel B. Otherwise, the interpretation of both figures is remarkably similar. Daily data thus corroborate our main finding of

[1]Note that daily fatalities are small numbers, often taking the value of zero, and there is higher variability of fatalities in the Czech Republic relative to Austria. The results do not depend on the standardization, however.

[2]I tested the equality of pre-July 2006 development in daily fatalities between Austria and the Czech Republic with results far from any rejection criteria. Pearson's correlation coefficient of pre-July 2006 weekly fatalities in the two countries is 0.43 and is highly statistically significant.

[3]More precisely, these are the sums of the coefficients on interactions and the coefficient on the dummy for the Czech Republic.

[4]Results are the same for linear or quadratic trends.

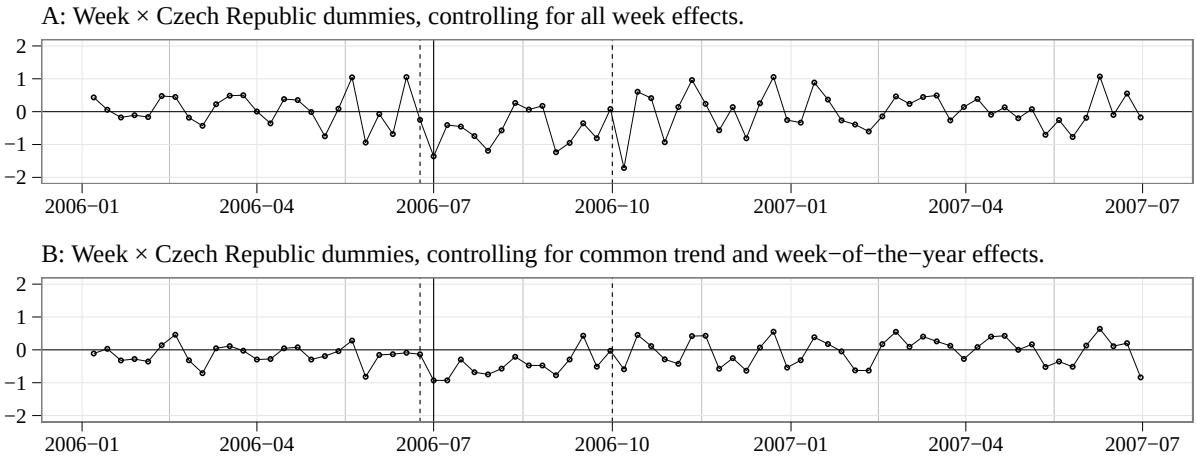


Fig. S1 Weekly effects of the new traffic law on standardized fatalities (6 months before–12 months after)

substantial immediate effects of the traffic law reform in the Czech Republic and a subsequent rebound toward pre-reform levels.

The Data on Police Activity

This subsection presents an analysis of a unique dataset parsed from internal monthly regional-level information on traffic police activity in 2006 and 2007, which I have obtained from the Czech Traffic Police Headquarters. The original source of the information are the local police directorates, who report to the Headquarters.

The thrust of the of the findings is captured in Figure S2, which plots the development of manpower and man-hours in enforcement as well as the number of hours of speed gun use by the traffic police across Czech regions. The number of policemen assigned to enforcement a exhibits general upward pattern over 2006 and 2007. Despite that, the man-hours worked by traffic policemen were declining in all regions but one. The number of hours the police spent behind speed guns was falling even more rapidly.[5]

Tables S1 and S2 study traffic police activity in more detail. I simply regress the logs of police activity indicators on year×quarter dummies, where the second quarter of 2006 is the omitted category. One may worry that some variability may be driven by seasonal regularities, an issue

[5]Certainly the enforcement infrastructure was improving over recent years. Many static cameras were put in place, so that drivers' speed may be measured with higher frequency. However, speeding captured by static radars gets recorded, and only a subset proceeds through the administrative procedure and possibly results in punishment of the driver, with a delay.

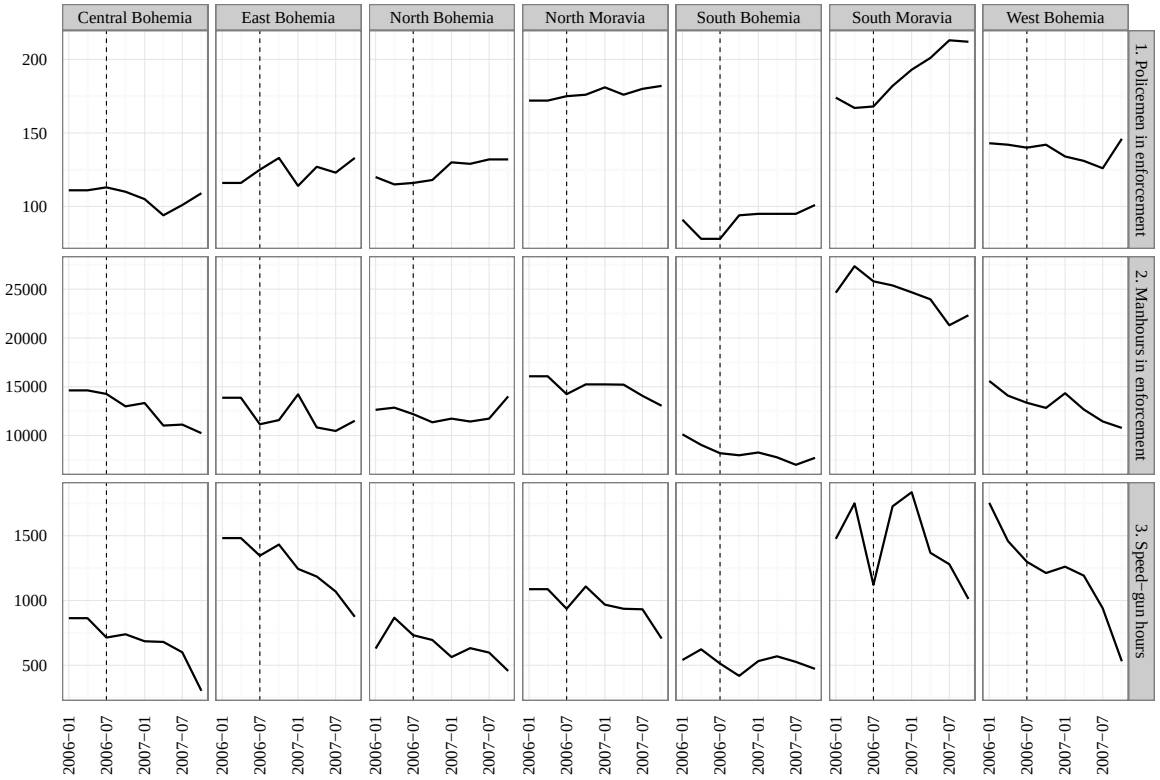


Fig. S2 Traffic police manpower and man-hours in enforcement by Czech regions in 2006 and 2007

hard to deal with properly with two years of data. I use half-yearly country-level data on man-hours in enforcement ranging from 2005 to 2008 and estimate a coefficient for a second half-year, which I then use to deseason the monthly data on police activity.

Column (1) of Table S1 suggests that the total number of policemen remained constant throughout 2006 and 2007, while the number of policemen in enforcement was slowly increasing as seen in column (2) and Figure S2. In other words, there is no sign that fewer policemen were available for traffic law enforcement after June 2006. Nonetheless, the total amount of traffic police man-hours dedicated to enforcement was gradually declining from the third quarter of 2006 onwards. The declines are statistically significant at the 5 percent level and the pace was accelerating. Since man-hours in enforcement are usually smaller in the second half of the year (although fatalities are higher), the results for deseasoned data places the beginning of the decline to the first quarter of 2007. Nonetheless, there were almost 9% fewer traffic policemen on the streets and roads in the first quarter following the introduction of the new traffic law compared to the preceding quarter, and the decline was nearly 24% by the end of

Table S1 Czech Traffic Police Activity in 2006 and 2007

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Manpower	Manpower in Enforcement	Manhours in Enforcement	Speed Gun Hours	Targeted Actions	Media Activity	Manhours in Enforcement	Speed Gun Hours	Targeted Actions
2nd quarter pre-reform	.029†	.031	−.019	−.102†	−.034	−.059	−.019	−.102†	−.034
	(.015)	(.021)	(.037)	(.053)	(.076)	(.058)	(.037)	(.053)	(.076)
1st quarter post-reform	.021	−.001	−.092†	−.196†	−.021	.026	.044†	−.060	.115†
	(.019)	(.005)	(.025)	(.045)	(.049)	(.054)	(.025)	(.045)	(.049)
2nd quarter post-reform	.031†	.048†	−.130†	−.195†	−.013	−.045	.006	−.059	.123
	(.012)	(.017)	(.019)	(.040)	(.135)	(.076)	(.019)	(.040)	(.135)
3rd quarter post-reform	.030†	.030	−.057	−.137†	−.213	−.281†	−.057	−.137†	−.213
	(.018)	(.039)	(.037)	(.058)	(.163)	(.100)	(.037)	(.058)	(.163)
4th quarter post-reform	.028	.038	−.139†	−.179†	−.136	−.366†	−.139†	−.179†	−.136
	(.024)	(.047)	(.033)	(.045)	(.149)	(.152)	(.033)	(.045)	(.149)
5th quarter post-reform	.030	.055	−.187†	−.262†	.228	−.311†	−.051	−.126†	.364†
	(.024)	(.046)	(.043)	(.050)	(.151)	(.127)	(.043)	(.050)	(.151)
6th quarter post-reform	.034	.100†	−.272†	−.583†	.573†	−.377†	−.136†	−.447†	.709†
	(.028)	(.038)	(.060)	(.084)	(.181)	(.166)	(.060)	(.084)	(.181)
Deseazoned	-	-	-	-	-	-	yes	yes	yes
adj. R^2	.965	.956	.808	.819	.611	.944	.808	.819	.611
N	192	192	192	192	192	192	192	192	192

NOTES: The outcome variables are in logs. The 1st quarter post-reform is the omitted category. All specifications include region dummies. Deseasoning is done using a coefficient for the second half-year estimated on country-level half-yearly data on man-hours in enforcement from 2005 to 2008. Huber-White standard errors clustered on regions are in parentheses: † < 0.1.
SOURCES: Headquarters of the Police of the Czech Republic.

Table S2 Czech Traffic Police Alternative Activities in 2006 and 2007

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Persons at Large Found	Stolen Cars Found	Persons Checked	Suspitious Items Checked	Persons at Large Found	Stolen Cars Found	Persons Checked	Suspitious Items Checked
2nd quarter pre-reform	.124	.297†	.074	−.176	.124	.297†	.074	−.176
	(.199)	(.162)	(.089)	(.165)	(.199)	(.162)	(.089)	(.165)
1st quarter post-reform	.000	.320	.247	−.375	.136	.456†	.383†	−.239
	(.160)	(.234)	(.208)	(.314)	(.160)	(.234)	(.208)	(.314)
2nd quarter post-reform	.357†	.603†	.240	−.371	.493†	.739†	.376†	−.235
	(.175)	(.231)	(.164)	(.312)	(.175)	(.231)	(.164)	(.312)
3rd quarter post-reform	.337†	.630†	.493†	−.005	.337†	.630†	.493†	−.005
	(.136)	(.271)	(.256)	(.355)	(.136)	(.271)	(.256)	(.355)
4th quarter post-reform	.341†	.256	.400†	.349	.341†	.256	.400†	.349
	(.205)	(.209)	(.201)	(.232)	(.205)	(.209)	(.201)	(.232)
5th quarter post-reform	.285	.437†	.366†	.246	.421†	.573†	.502†	.382
	(.177)	(.230)	(.198)	(.320)	(.177)	(.230)	(.198)	(.320)
6th quarter post-reform	.331†	.521	.333†	−.174	.467†	.657†	.469†	−.038
	(.124)	(.346)	(.155)	(.431)	(.124)	(.346)	(.155)	(.431)
Deseazoned	-	-	-	-	yes	yes	yes	yes
adj. R^2	.277	.514	.900	.687	.277	.514	.900	.687
N	181	130	192	192	181	130	192	192

NOTES: The outcome variables are in logs. The 1st quarter post-reform is the omitted category. All specifications include region dummies. Deseasoning is done using a coefficient for the second half-year estimated on country-level half-yearly data on man-hours in enforcement from 2005 to 2008. Huber-White standard errors clustered on regions are in parentheses: † < 0.1.
SOURCES: Headquarters of the Police of the Czech Republic.

2007. The decay is even more dramatic in the case of speed gun hours by the traffic police. The number of targeted actions—that is the temporarily increased presence of traffic police in a specific area with the purpose to increase the number of checks and the salience of police presence—remained steady through 2006, then declined in the first half of 2007 and later increased substantially. The presence of the police in the media decreased in 2007; it was probably unusually high in 2006.

Table S2 looks at activities of the traffic police other than direct traffic law enforcement. Specifically, I looked at the numbers of persons and cars at large found, and the numbers of suspicious items and persons checked. Although most coefficients are not statistically significant, there seems to be a general increase in these measures, despite fewer working hours in enforcement. This suggests that the traffic police may have given higher priority to general law enforcement activities relative to enforcement of traffic rules.

I note that these findings are only indicative that weakening enforcement may have caused the short-livedness of the effects and do not present direct evidence. At the same time, they are in line with the predictions of standard theory.