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What is the optimal speed limit on freeways?☆

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ABSTRACT

When choosing his speed, a driver faces a trade-off between private benefits (time savings) and private costs (fuel cost and own damage and injury). Driving faster also has external costs (pollution, adverse health impacts and injury to other drivers). This paper uses large-scale speed limit increases in the western United States in 1987 and 1996 to address three related questions. First, do the social benefits of raising speed limits exceed the social costs? Second, do the private benefits of driving faster exceed the private costs? Third, what is the optimal speed limit? I find that a 10 mph speed limit increase on highways leads to a 3–4 mph increase in travel speed, 9–15% more accidents, 34–60% more fatal accidents, and elevated pollutant concentrations of 14–24% (carbon monoxide), 8–15% (nitrogen oxides), 1–11% (ozone) and 9% higher fetal death rates around the affected freeways. Using these estimates, I find that the social costs of speed limit increases are two to seven times larger than the social benefits. In contrast, many individual drivers would enjoy a net private benefit from driving faster. Privately, a value of a statistical life (VSL) of \$6.0 million or less justifies driving faster, but the social planner's VSL could be at most \$0.9–\$2.0 million to justify higher speed limits. I conclude that the optimal speed limit was lower, but not much lower, than 55 mph.

1. Introduction

Two interesting and actively debated policy questions that economists are well-positioned to consider are: should we increase speed limits on freeways, and what is the optimal speed limit? When choosing his speed, a driver faces a trade-off between private benefits (time savings) and private costs (increased fuel use, risk of personal injury, death or damage). It is thus an empirical question if driving faster than the current speed limit is rational. Besides private costs, there are external costs to driving faster that motivate the use of speed limits: increased pollution, adverse health impacts and damage or injury to other drivers. Speed limits have recently been under active debate. Early in 2011, Spain temporarily reduced the freeway speed limit from 120 to 110 kilometers per hour (kph) to achieve gasoline reductions, while the Netherlands raised it from 120 to 130 kph to reduce travel time.[1] In the United States, travel time reduction inspired Illinois (2014), Kentucky (2007), Utah (2009), Ohio (2011) and Texas (2012) to increase their posted maximum speed. Germany's "no speed limits" rule for rural *autobahns* is facing increased criticism from politicians and environmentalists.

This paper aims to answer three related questions. First, should we raise speed limits? A social planner would do so only if the social benefits of speed limit increases exceed the social (private plus external) costs. Second, do speed limits constrain drivers' speed choices? That is, would individuals enjoy private net benefits from driving faster if speed limits were raised? Third, what is the optimal speed limit? To answer these questions, I estimate the effect of speed limit increases on a wide range of outcome variables: travel time, accidents, air pollution and health. I use these estimates to calculate the private and external benefits and costs summarized in Fig. 1.

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[1] Spain's deputy prime minister Alfredo Pérez Rubalcaba expressed it as follows: "We are going to go a bit slower and in exchange for that we are going to consume less gasoline and therefore pay less money." (http://www.guardian.co.uk/environment/2011/feb/25/spain-speed-limit-oil-prices). Dutch transport minister Melanie Schultz van Haegen defended her decision by claiming that "a higher speed limit leads to a travel time reduction of up to eight percent." (http://www.rijksoverheid.nl/ministeries/ienm/nieuws/2011/02/28/130-km-u-van-start-op-afsluitdijk.html). Other governments proposed to decrease speed limits to reduce traffic accidents (United Kingdom, 2009) or pollution and associated adverse health effects (Texas, 1992; green parties in Europe).

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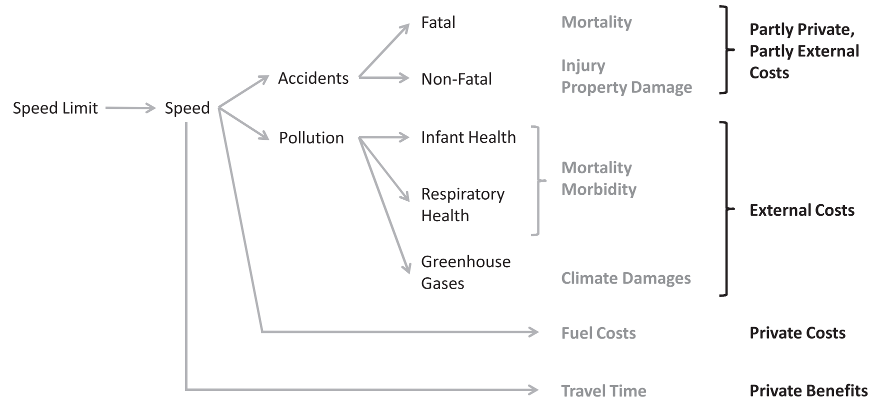

Fig. 1. An overview of the costs and benefits of speed limit changes. Higher speed limits may lead to a higher average travel speed. This higher speed has a direct benefit (reducing travel time), but also three direct costs: higher accident rates, increased pollution and increased fuel expenditures. The pollution channel has indirect negative effects on infant and respiratory health, and climate change. Time savings benefits are private, while some of the costs are externalities.

I use a unique setting and rich data to address these questions. The 1987 amendment and 1995 repeal of the National Maximum Speed Law in the United States provide quasi-experimental variation in speed limits. Between 1974 and 1987, this law prescribed a maximum speed limit of 55 mph across the entire United States. In 1987, states were allowed to raise the speed limits to 65 mph on rural interstates, but not on other similar urban or rural highways. In 1996, speed limit authority was returned to the states, which decided to raise speed limits on a variety of highways. This provides a rare opportunity to use difference-in-differences and ratio-in-ratios (count data) methods to identify the effect of speed limit changes on travel speed, accidents, pollution and health. I construct control highways or areas that are unaffected by the speed limit changes, but otherwise very similar to the affected highways or areas. Also, I exploit geographically precise micro data to make within-state difference-in-differences comparisons while holding constant weather, daylight, hour-of-day, traffic density, road construction, and much else.

My paper uses a detailed data set to evaluate the effects of speed limit changes. First, I use location descriptions of speed limit changes in California, Oregon and Washington. These states are selected because of superior data quality and availability. Second, I collect hourly measurements of actual traffic speed. Third, I use a data set of all highway accidents. Fourth, I use daily air pollution measurements at various monitoring stations. Fifth, I requested all birth records in California to estimate the effect on infant health. Finally, I use geographical mapping techniques to augment these data sets with meteorological and geographic information wherever applicable.

In terms of the specific outcome variables, I find that a 10 mph speed limit increase leads to a 3–4 mph increase in travel speed, 9–15% more accidents, 34–60% more fatal accidents, a shift towards more severe accidents, and elevated pollution concentrations of 14–24% (carbon monoxide), 8–15% (nitrogen oxides) and 1–11% (ozone) around the affected freeways. The increased pollution leads to a 0.07 percentage point (9%) increase in the probability of a third trimester fetal death, and a positive but small and statistically insignificant increase in the probability of infant death. I use these estimates to calculate the time saving benefits and the private and external costs from accidents and deteriorated infant health. Moreover, I combine the travel speed estimates with engineering data to compute the increase in fuel consumption at higher speed. Similarly, I combine the air pollution estimates with epidemiology research to compute adverse health effects for adults.

Using these estimates and a wide array of plausible values of a statistical life (VSL) and values of time routinely adopted by governments, I find that the social costs of raising the speed limit from 55 to 65 mph are two to seven times larger than the social benefits. My social cost estimates are two to four times larger than in previous studies, in large part due to the greater comprehensiveness of my approach: I not only consider travel time and fatal accidents, but also non-fatal accidents, climate damages, fuel costs and health. While net social benefits are negative, I find that many individual drivers would enjoy a net private benefit from driving faster as a result of the higher speed limit. Privately, a VSL of $6.0 million or less justifies driving faster, but the social planner's VSL could be at most $0.9 million to justify higher speed limits ($2.0 million if adult health impacts are conservatively left out due to their uncertain nature). While $6.0 million is within the conventional range of VSL estimates, $0.9–$2.0 million falls well below it. Although these results suggest a surprisingly large difference between the social and private optimal speed choices, the optimal speed limit was likely not much below 55 mph since driving slower does not yield substantial pollution reduction benefits and gasoline savings in that speed range.

A seminal paper in this literature is Ashenfelter and Greenstone (AG; 2004), who use the 1987 speed limit changes to estimate the value of a statistical life based only on the trade-off between travel time and fatal accident risk. This is the only well-known study that has produced modern empirical evidence on how speed limits affect speed and fatal accidents.[2] AG use annual data by state and road type in a difference-in-differences framework to estimate the impact of the 1987 speed limit changes on speed and fatal accidents. They can employ cross-state variation in the adoption of the 65 mph speed limit: seven states in the Northeast retained the 55 mph limit, whereas the other eligible states adopted the 65 mph limit. They find that the average speed increased by 2.5 mph and fatality rates by 35%. The paper calculates an upper bound on the value of a statistical life: $1.54 million (1997 USD) for the full sample, but higher estimates for California ($4.75 million) and Oregon ($5.41 million). Section 8 discusses how my results are different.

My paper's main contributions are threefold. First, I explicitly distinguish between private and external costs and benefits and show a stark contrast between them. Second, because I employ an unusually rich data set, I can estimate the effect of speed limit changes on additional important outcome variables such as severe but non-fatal injuries, property damage from accidents, air pollution and the health of infants and others who live near freeways. These estimates allow me to perform a more complete cost–benefit analysis. They are also interesting in their own right. Third, by exploiting within-state variation in speed limits and a wide range of control variables, my approach mitigates potential

[2] This is surprising, since other driving-related policies have attracted considerable attention from economists. Examples include the impact on accidents of seat belt laws (Cohen and Einav, 2003), highway police enforcement (DeAngelo and Hansen, 2014) and vehicle weight (Anderson and Auffhammer, 2014; Jacobsen, 2013).

omitted variable bias. Using within-state speed limit variation is a useful and necessary complementary approach to existing cross-state regressions for speed and fatal accidents. AG use the fact that not all states raised their rural interstate speed limits in 1987 and compare highways of the same type (e.g., rural interstates), but across states with potentially dissimilar driving conditions (e.g., New Jersey vs. Iowa). I compare similar but differently classified highways (e.g., rural interstates and "rural principal arterials"), but within one state so that driving conditions are more similar. I further mitigate concerns about omitted variables by including a wide set of control variables, such as weather, road and driver conditions and demographics.[3]

This research is relevant for today's policy makers, since past experiences can inform them about whether speed limits are desirable and if raising speed limits further would be in society's best interest. Using engineering and epidemiological evidence on the past and current relationships between speed, pollution and health, many results in this paper can be extrapolated to obtain pollution and health effects for current speed limit changes. The large difference between private and social net benefits is likely to persist, especially because governments currently consider speed limits in the 75–90 mph range where the strong upward sloping relationship between speed and emissions remains even for today's new vehicles. This paper also demonstrates that the common approach to evaluating speed limits, based on a single cost or benefit or a single trade-off between travel time and fatal accidents, may lead to incorrect conclusions.

2. Empirical strategy

In an ideal speed limit experiment, one would consider two freeways that are identical in terms of driving characteristics (speed limit, traffic counts, number of lanes, curvature, slope, driver experience, vehicle type, weather, etc.) and then randomly raise the speed limit on one freeway while keeping the speed limit fixed on the other. However, for good reasons, governments do not randomly assign speed limits. Nevertheless, there exists a natural experiment that allows me to identify the treatment effects with some additional assumptions.

2.1. A series of quasi-experiments: the national maximum speed law

In response to the First Oil Crisis, Congress adopted the National Maximum Speed Law in 1974, which prohibited speed limits in excess of 55 mph on any highway in the country. Its goal was to reduce oil imports from the Middle East. States had to comply in order to keep receiving federal highway funding. The law was amended in April 1987, when states were allowed to increase the speed limits to 65 mph *on rural interstates only* but not on similar freeways. Oregon and Washington responded by increasing the speed limit on (virtually) all of their rural interstates; California increased the speed limit on the majority of its qualified freeways. In December 1995, the National Maximum Speed Law was repealed, returning all speed limit authority to the states. California raised the speed limit to 65 mph on 2200 highway miles, and in addition raised the speed limit on 1272 highway miles to 70 mph (these were primarily the segments that had already been raised to 65 mph in 1987). Washington also further increased speed limits. Oregon adopted no further changes. Across all three states, few highways have experienced further changes in their speed limits since 1996.

2.2. Identification

In the absence of an ideal experiment, two potential concerns arise when using the amendment and abolishment of the National Maximum Speed Law to identify a causal impact of speed limit changes on travel speed, accidents, pollution and health. The first issue is local treatment effects and applies mostly to the 1987 speed limit changes on rural interstates. Although we cannot be certain that the estimated impact of these changes is representative for, e.g., urban highways, this local treatment effect is a particularly interesting one: rural highways are likely candidates to experience (further) speed limit increases today. In 1996, speed limits increased on a more diverse range of rural and urban highways.

The second issue is non-random variation in speed limits. However, the 1987 speed limit changes actually provide a source of almost random variation: rural interstates were allowed higher speed limits, whereas rural principal arterials and urban highways must be kept at 55 mph. The Federal Highway Administration (FHWA) classifies both interstates and principal arterials (mainly federal – "US" – highways) as highways that "serve corridor movements having trip length and travel density characteristics indicative of substantial statewide or interstate travel" (Federal Highway Administration, 1989). I now discuss why the distinction between these road types is somewhat arbitrary in many respects.

The number of highway miles that states can designate as interstates is limited, and de facto rationed. States can apply to add a principal arterial to the interstate system. To obtain approval, a road needs to be a "logical addition" to the interstate system from a national defense perspective, and meet all interstate standards (e.g., sufficient length, connections to other interstates on either end, and safety and environmental standards). If a principal arterial is deemed to duplicate an existing interstate, inclusion will be denied. The year of construction and national defense reasons, as opposed to safety standards, often determine why one highway is designated as an interstate while another similar highway is not. In fact, some highway miles that are ineligible to become interstates (because of rationing) are signed as "non-chargeable" interstates, to avoid driver confusion. Also, strict design standards can make it undesirable for states to turn principal arterials into interstates. If a highway meets all interstate standards but has a short segment without full access control (e.g., US-101 near Salinas, CA), inclusion will be denied. Since adding interstates hardly increases federal highway funding and limits the state to add new access points, states do not always try to add likely candidates as interstates (FHWA, personal communication, 26 August 2011). These examples illustrate that the distinction between interstates and principal arterials can be quite arbitrary.

Therefore, rural principal arterials share many important characteristics with rural interstates. Both road types had a 55 mph speed limit before 1987, are designed for speeds at or above 70 mph, and are typically divided highways with multiple lanes and full or at least partial access control. Rural principal arterials have fewer lanes than rural interstates on average, but wide shoulders make driving conditions similar. Urban highways are even more similar to rural interstates in terms of access control and number of lanes, but face a higher traffic density.

Unfortunately, limited data only allowed me to verify to what extent rural interstates differed from rural principal arterials and urban highways in Oregon in 1987. Although these roads obviously share many similarities, Table 1 reveals at least one observable difference: accident rates. Rural interstates and urban highways have lower accident rates than rural principal arterials. These differences are similar to those reported by Ashenfelter and Greenstone (2004) for all states that adopted the new 65 mph speed limit.

I take these observable differences in 1987 for granted and state the conditions under which a causal treatment effect is identified. Since one might be concerned about temporal trends that affect all highways, such as a gradual decline in fatal accidents because of improved vehicle engineering, seat belt use and air bags, I use a difference-in-differences estimator. For speed and accidents, the treatment group consists of rural interstates and the control group consists of (subsets of) rural principal arterials and urban highways. For pollution and health, I compare

[3] For example, weather shocks around the time of the speed limit changes are potential confounders if they affect treatment freeways differently from control freeways. Another confounder could be changes in drunk driving and seat belt laws across states.

Table 1
Accident rates by road type before the 1987 speed limit change (Oregon).

	Adopted 65 mph (rural interstates)	Retained 55 mph (rural principal arterials)	Retained 55 mph (urban highways)
Fatal accidents	24	130	26
Per 100 million VMT	0.82	3.76	0.46
Incapacitating accidents	111	411	191
Per 100 million VMT	3.83	11.91	3.37
Non-incapacitating accidents	240	813	520
Per 100 million VMT	8.30	23.57	9.16
Total accidents	1010	3412	3143
per 100 million VMT	34.94	98.86	55.34
AADT per lane	3554	1823	5091
VMT (million)	2892	3451	5567

Notes: Annual averages over 1985–1987. Oregon is chosen because of data availability. AADT = average annual daily traffic (supplied by ODOT). VMT = vehicle miles traveled (Federal Highway Statistics).

treatment areas close to highways where the speed limit changed with control areas further away. For speed, pollution and health, I make the standard identification assumption that treatment (T) and control (C) groups would have followed the same trend *in levels* in the absence of the speed limit changes:

$$E\left[speed_1^T|\Delta SL=0\right]-E\left[speed_0^T\right]=E\left[speed_1^C\right]-E\left[speed_0^C\right] \tag{1}$$

where ΔSL denotes the speed limit change, 0 corresponds to the pre-period ($t < t_{\Delta SL}$) and 1 to the post-period ($t \geq t_{\Delta SL}$). The expressions for pollution and health are analogous, but the treatment group is now defined as an area close to the treatment freeways.

For accident rates, I assume that absent the policy change, there would have been equal time trends *in ratios* for T and C highways:

$$E\left[\frac{accidents_1^T/VMT_1^T}{accidents_0^T/VMT_0^T}\middle|\Delta SL=0\right]=E\left[\frac{accidents_1^C/VMT_1^C}{accidents_0^C/VMT_0^C}\right] \tag{2}$$

To assert the reader of the plausibility of these assumptions, the results sections and Appendix B present evidence that there were no differential pre-existing trends in the outcome variables for treatment and control highways and motivates the choice of identification assumption (2). Moreover, the paper contains a series of robustness checks that add to the credibility of the identification, such as controlling for pre-existing trends in the speed regressions and only including control highways with comparable accident rates to the treated highways.

The speed limit changes in 1996 were not as randomly assigned as in 1987, as states faced no federal restrictions and based their decisions on accident histories. Therefore, the identification assumption of equal trends is less convincing (although there is no evidence of differential pre-existing trends in speed). Nevertheless, I investigate how changes in the outcome variables are associated with this speed limit change. This is only possible for speed data. I find that the estimated effect on speed is similar to the estimate using the 1987 speed limit changes.

3. Data

First, I collected a list of the freeway segments on which the speed limits changed in 1987 from the three Departments of Transportation (CA: Caltrans; OR: ODOT; WA: WSDOT). Following the April 1987 amendment to the National Maximum Speed Law, California (May 1st), Oregon (December 8th) and Washington (April 23rd) changed the speed limit for 1157, 604 and 526 rural interstate miles, respectively.[4] Fig. 2 (left panel) displays the interstates on which the speed limits increased in 1987, as well as highways with no change in the speed limit.[5] The speed limit changes in 1996 are more difficult to summarize and are discussed in Appendix A.

Second, I obtained detailed speed data for a reasonable range of monitoring stations in the period 1994–1998 (CA), 1983–1992 (OR) and 1994–2001 (WA). These data were extracted from archived databases and scanned paper records. Depending on the state, I observe counts by speed bin (e.g., 55–60 mph), by year–month–day–hour, by direction, by lane. California data are available for one month per quarter. Oregon data are available for one day per quarter. The Washington data contain speeds for almost all days. I have data for 61 speed stations in California, 48 of which had a speed limit increase (treatment stations). The Oregon data cover 51 stations (12 treatment stations). In Washington, I only have six stations, but well balanced (three treatment stations) and extensive daily coverage throughout the year. Fig. 2 (right panel) shows the speed stations. Taken together, these locations represent a wide range of road characteristics. The U.S. Department of Transportation required states to submit speed data for a representative sample of roads, which mitigates sample selection concerns.

Third, I requested the universe of accident records since 1985 (OR) and 1980 (WA). The inclusion of *all* accidents takes away concerns about selective monitoring in high-risk areas. Unfortunately, Caltrans has destroyed all accident data older than 10 years. Detailed information on each accident is reported, such as the date, time, type (fatality, incapacitating injury, non-incapacitating injury, property damage), location (highway number and milepost), city, county, type of highway, urban status and a range of road, weather, daylight and driver characteristics. Using the accident location and speed limit change information, I assign each accident a treatment or control status.

Fourth, the California Department of Public Health's birth cohort files (1984–1990) contain infant health information from all birth records, including birth weight, gestational age, infant deaths in the first year of life and fetal deaths in the second or third trimester of the pregnancy. The files also contain a large number of characteristics of the child, mother and father, as well as medical information about the pregnancy. I use zip code information to approximate the mother's residence during pregnancy, and use ArcGIS to calculate the distance between the zip code's population-weighted average centroid and the closest highway segment on which the speed limit changed.

Fifth, daily measurements for CO, NO_2, O_3 and PM_{10} were obtained from the California Air Resources Board, the Oregon Department of Environmental Quality, and the Washington Department of Ecology. Fig. 2 (right panel) plots the 431 stations that reported emissions for some part of the period 1984–1990. I calculate the distance between each air pollution monitoring station and the closest point on a highway segment where the speed limit changed. Stations are located at various distances from such highways, but only occasionally right next to them. 31 stations were located within three miles of these highways and reported both before and after the speed limit changes (treatment stations), 42 within five miles and 69 within 10 miles. Since few stations monitor all five pollutants, the number of treatment stations by pollutant is more limited.

Finally, I obtained weather data from the National Climatic Data Center's "Global Summary of the Day" (1980–2010). Each weather station in California, Oregon and Washington reports average, maximum and minimum temperature, precipitation, wind speed,

[4] This corresponds to 56.3%, 97.3% and 100% of all eligible rural miles per state.

[5] Throughout the entire study period, truck speed limits remained at 55 mph in California and Oregon. In Washington, truck speed limits were raised from 55 to 60 mph in 1996 on those segments where the car speed limits were raised to 70 mph. I ignore trucks, since they only constitute a small fraction of vehicles (see footnote 20).

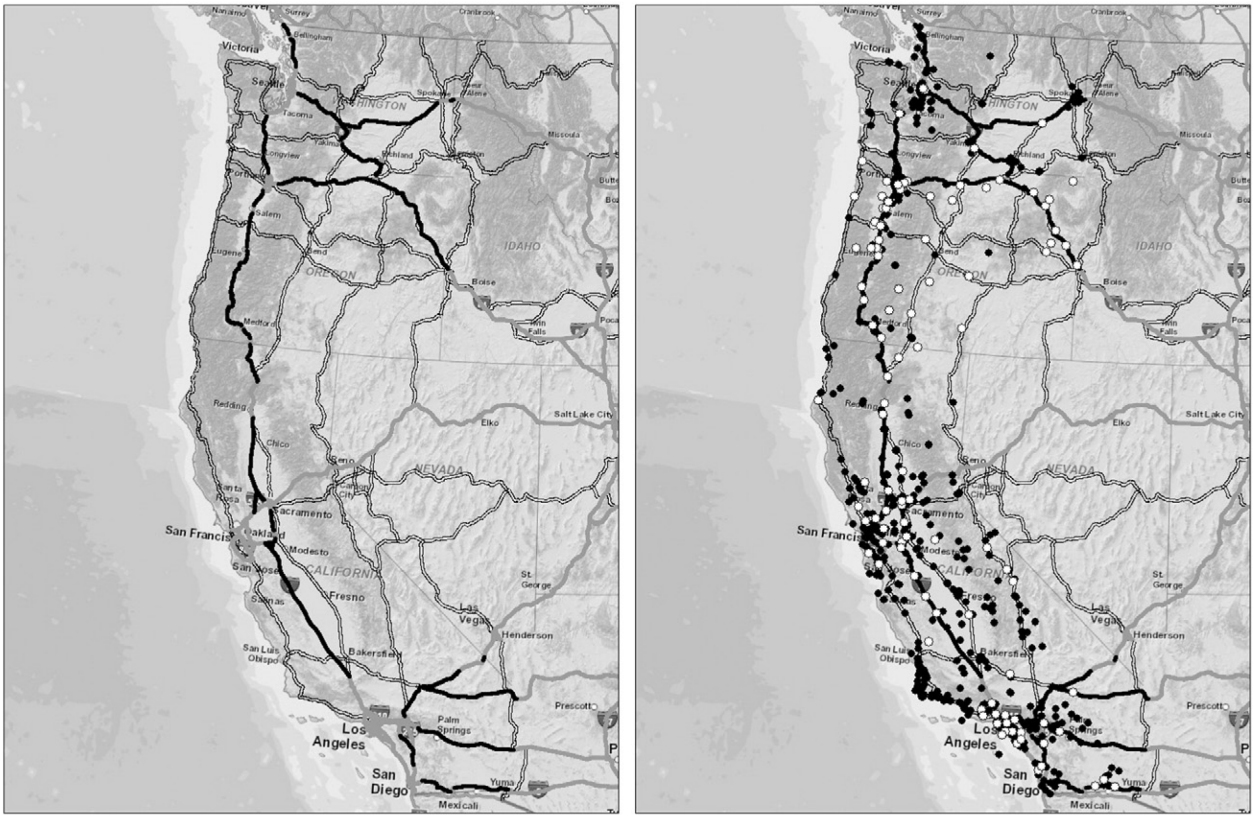

Fig. 2. Rural interstates with increased speed limits in 1987 (left) and the distribution of pollution and speed monitoring stations (right). (Left panel) Gray solid lines indicate interstates. Black solid lines indicate the interstate segments in CA, OR and WA on which the speed limit changed from 55 to 65 mph in 1987. Double black lines are a subsample of principal arterials, on which no speed limit change occurred. (Right panel) Black dots indicate pollution monitoring stations that were active in the period 1984–1990. White dots indicate speed monitoring stations.

plus indicator variables for rain, fog, snow, hail, thunder or a tornado. I follow Knittel et al. (2011) and assign each pollution/speed station a set of weather variables: the inverse distance-weighted average of observations from all weather stations within a 20-mile radius.

4. The effect of speed limit changes on travel speed

4.1. Econometric framework

I estimate the effect on travel speed using the following linear difference-in-differences specification:

$$speed_{ijth} = \beta_0 + \beta_1 1(i \in T) * 1(t \geq t_{\Delta SL}) * TI + \beta_2 X_{it} + \theta_{ij} + \theta_{lj} + \theta_h + \theta_d + \theta_m + \theta_y + \varepsilon_{ijth} \quad (3)$$

for speed station i, traffic direction j, lane l, date t, hour-of-day h, day-of-week d, month-of-year m and year y. Control variables include weather conditions (maximum, minimum and average temperature, wind speed, fog, rain, snow, hail, thunder, tornado) and a measure of traffic density (number of vehicles per lane per hour). Since California had speed limit changes from 55 to 65 mph and from 65 to 70 mph, the treatment intensity varies: $TI = 1$ if $\Delta SL = 10$ and $TI = 0.5$ if $\Delta SL = 5$. This assumes an equal treatment effect for every mph increase in the speed limit. I also estimate separate coefficients for the two speed limit changes. Moreover, I estimate separate treatment effects for different years, traffic conditions and hours of the day.

4.2. Travel speed results

The first panel in Table 2 shows the treatment effect of a 10 mph speed limit increase in California (1996), Oregon (1987) and Washington (1996).[6] A potential concern is that the speed limit increases caused substitution of traffic towards highways with higher speed limits. This would make treatment highways more congested, and reduce the effect on speed. In addition, traffic density would be an outcome of the treatment and should not be included as a control. In column (5) of the first panel, I exclude traffic density as a control variable. In the second panel, I test for traffic substitution more directly by estimating Eq. (3) with the log of the hourly number of vehicles as the left-hand side variable.

The effects on travel speed (panel 1) is quite stable across states (a 3–4 mph increase in travel speed for a 10 mph speed limit increase), even though the speed limits were not all raised by the same amount, from the same original speed limit, and at the same point in time. This is reassuring and suggests that the estimated coefficient is representative of a range of speed limit changes between 55 and 70 mph. The results are robust to the inclusion of station by direction fixed effects (columns (2) and (4)) and of station by day-of-week fixed effects (columns (3) and (4)). This is to be expected if the experiment is clean and the panel is well-balanced. The estimates are considerably below 10 mph. This might not be surprising, since it is not always the

[6] Standard errors are clustered at the station by year level and at the station level. Clustering at the station level increases the standard errors, but does not affect the conclusions about statistical significance. A particular concern about correlation of errors within a station across time is road construction. Since road construction is typically a shorter-term (e.g., several weeks or months) phenomenon, clustering at the station by year level is likely to be conservative enough while mitigating concerns about a paucity of clusters.

Table 2
The effect of speed limit changes on travel speed and traffic substitution.

Panel 1: effect on travel speed					
	(1)	(2)	(3)	(4)	(5)
CA 1996					
Speed limit	3.10***	3.06***	3.12***	3.08***	2.98***
	(0.52)	(0.51)	(0.58)	(0.58)	(0.50)
	[0.80]	[0.80]	[0.80]	[0.80]	[0.78]
Observations	356,661	356,661	356,661	356,661	356,661
OR 1987					
Speed limit	4.09***		4.23***		4.10***
	(0.46)		(0.46)		(0.46)
	[0.76]		[0.71]		[0.77]
Observations	19,103		19,103		19,103
WA 1996					
Speed limit	3.69***	3.60***	3.69***	3.60***	3.54***
	(0.61)	(0.63)	(0.61)	(0.62)	(0.50)
	[0.67]	[0.67]	[0.65]	[0.66]	[0.54]
Observations	1,371,156	1,371,156	1,371,156	1,371,156	1,371,156
Fixed effects: station, year, month, day-of-week, hour-of-day, plus					
Direction	Y (CA, WA)	Y (CA, WA)	Y (CA, WA)	Y (CA, WA)	Y (CA, WA)
Station × direction	N	Y (CA, WA)	N	Y (CA, WA)	Y (CA, WA)
Lane × direction	Y (WA)	Y (WA)	Y (WA)	Y (WA)	Y (WA)
Station × day-of-week	N	N	Y	Y	N
Controls	All	All	All	All	Weather only

Panel 2: effect on traffic substitution towards treatment highways			
	(1)	(2)	(3)
	California (1996)	Oregon (1987)	Washington (1996)
Interaction (β_1)	0.0549	0.0002	0.0020
	[0.0342]	[0.1186]	[0.0870]
Observations	356,661	27,931	1,371,156
Fixed effects: station, direction, year, month, day-of-week, plus			
Station × direction	Y	N	Y
Lane × direction	N	N	Y
Hour-of-day	Y	Y	Y
Controls	All	All	All

Notes: (Panel 1) The dependent variable is hourly travel speed. The coefficient on the interaction term of 13 separate regressions is reported. Coefficients are normalized to reflect the effect of a 10 mph speed limit change. (Panel 2) The dependent variable is the log of the hourly number of vehicles. The coefficient on the interaction term of 3 separate regressions is reported. (Both panels) Standard errors in parentheses clustered at the station by year level in (), and clustered at the station level in [] (used for stars). The time window is 1994–1998 (CA), 1983–1992 (OR) and 1994–2001 (WA). ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

posted speed limit that constrains a driver's speed. During times of congestion and poor visibility, we should expect little effect. Moreover, some drivers might not respond to higher speed limits because their optimal speed choice is at or below the old speed limit.

Panel 2 shows that the traffic substitution coefficients are small and statistically insignificant at the 10% level, but some of the standard errors are large. Treatment and control roads do not appear to be strong substitutes. One reason is that they are often quite far apart, while more than 75% of vehicle miles are for commuting and other short-distance travel (National Personal Transportation Survey, 1995): not many drivers are at the margin between choosing one over the other. This is confirmed by column (5) in panel 1: the exclusion of traffic density as a control variable hardly changes β_1. These results suggest that traffic substitution towards roads with increased speed limits is not a major issue.

Table 3 reports the effect of the speed limit changes on the distribution of travel speed. The results reveal – consistently across the three states – that the average and 85th percentile speed increase by approximately the same amount. This suggests that most drivers drive "with the flow of traffic". This result is important, since not only an increase in average speed but also in speed variance at higher speeds could cause more accidents. The impact on the speed variance is negligible in California, and small and statistically insignificant in Washington. However, the variance is imprecisely measured since the lowest speed bin is truncated at 30 mph or below. Also, the data does not fully capture extreme differences in speed, such as rapid changes between high-speed driving and stop-and-go congestion.

Column (4) reports two robustness checks. First, when the coefficients on the two speed limit changes in California (55 to 65 mph and 65 to 70 mph) are not constrained to be equal, the resulting estimates are almost identical. Second, when I only include rural control highways in Oregon, the speed increase is slightly lower.[7]

The graphs in Fig. 3 plot the difference-in-differences coefficients, allowing for a separate treatment effect for each year before and after the speed limit change. All three states show an increasing treatment effect over time, which reflects adaptation towards the new driving conditions. There is no visual evidence of differences in pre-trends across treatment and control stations. To address this issue more formally, Appendix Table C.1 re-estimates the speed effects after estimating and extrapolating pre-existing trends. The results are very similar. Appendix C also shows that the treatment effect also varies across different driving conditions and hours of the day (Appendix Fig. C.1) and that the immediate effect of the speed limit changes was small (using a time discontinuity specification).

In summary, I find that a 1 mph speed limit increase leads to a 0.3–0.4 mph increase in travel speed. This result is similar across the three

[7] I also did a placebo experiment in which specification (3) is estimated as if the speed limit change occurred on 1 January 1992, …, 1996. I should find that the coefficients are insignificant and close to zero, since no speed limit change occurred in reality. This experiment is only possible for Oregon, since the speed data for California and Washington do not cover a long enough period. Using a symmetric eight year window around the five hypothetical treatment dates, I indeed find small and insignificant (at the 5% level) treatment coefficients: 0.86 (1992), 0.56 (1993), −0.06 (1994), −0.63 (1995) and 0.41 (1996) mph.

Table 3
Speed regression results: distribution and robustness.

LHS variable	(1)	(2)	(3)	(4)
	$speed_{ijth}$	$speed85p_{ijth}$	$var(speed)_{ijth}$	$speed_{ijth}$
CA 1996				
Speed limit	3.06***	3.12***	−0.81	
	(0.51)	(0.56)	(4.04)	
	[0.80]	[0.87]	[5.71]	
Speed limit (65 to 70 mph)				3.11***
				(0.82)
				[1.24]
Speed limit (55 to 65 mph)				3.08***
				(0.52)
				[0.80]
Observations	356,661	356,661	356,501	356,661
OR 1987				
Speed limit	4.09***	4.10***		
	(0.46)	(0.45)		
	[0.76]	[0.81]		
Speed limit (U.S. highways)				2.96***
				(0.68)
				[1.00]
Speed limit (rural arterials)				3.18***
				(0.49)
				[0.81]
Observations	19,103	19,103		6962; 11,008
WA 1996				
Speed limit	3.60***	3.72***	5.58	
	(0.63)	(0.69)	(3.54)	
	[0.67]	[0.86]	[3.77]	
Observations	1,371,156	1,370,345	1,350,409	

Notes: The coefficient on the interaction term of 11 separate regressions is reported. Coefficients normalized to reflect the effect of a 10 mph speed limit change. Standard errors in parentheses clustered at the station by year level in (), and clustered at the station level in [] (used for stars). The time window is 1994–1998 (CA), 1983–1992 (OR) and 1994–2001 (WA). All specifications contain weather controls, as well as station, year, month, day-of-week and hour-of-day fixed effects. The regressions for CA and WA also contain station-direction fixed effects, and the regressions for WA contain lane-direction fixed effects. No variance regressions are shown for OR, since the low and high speed bin definition changed discontinuously around 1988. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

states, and for speed limit changes of various magnitudes between 55 and 70 mph. I now proceed to estimating various costs related to this increase in speed.

5. The effect of speed limit changes on accidents

Vehicles are more difficult to control at higher speed. It is harder to get and retain traction if a sudden change in speed or direction is required. There is also less time to avoid hitting other cars or fixed objects (Wong, 2008). This section tests two hypotheses about the effects of increased speed limits on accidents. The first hypothesis is that traveling at higher speed leads to an increase in the *amount* of accidents. The second hypothesis is that a higher speed causes a shift towards *more severe* accidents.

5.1. Econometric framework

To test these hypotheses, I use count data models to estimate the impact of the 1987 speed limit changes in Oregon and Washington on the number of various types of accidents (fatality (*fatal*), incapacitating injury (*inc*), non-incapacitating injury (*non-inc*), property damage only (*pd*)). The second hypothesis can also be tested using ordered choice models. See Appendix C for details.

Accidents are discrete events and a natural application of count data models. Moreover, under identification assumption (2) and given the graphical evidence provided in Appendix B, it is reasonable to employ a "ratio-in-ratios" estimator. Count data models possess that feature and are preferred to a linear model in logs, which leads to biased estimates if there are zero count observations — days with no accidents on a particular road type.[8] The two dimensions for the ratio-in-ratios estimator are time (before and after the speed limit change), and the type of highway (treatment: rural interstates vs. control: (subsets of) rural principal arterials and urban highways).

I estimate a negative binomial model (using maximum likelihood) since it fits the data better than a Poisson model. It has the following expressions for the conditional mean and variance:

$$\begin{aligned} E(y|X) &= \lambda = exp(X'\beta) \\ Var(y|X) &= \lambda + \alpha^{-1}\lambda^2 = exp(X'\beta) + \alpha^{-1}exp^2(X'\beta) \end{aligned} \tag{4}$$

where $\alpha^{-1} = \sigma^2 > 0$. I separately estimate the following specification for each accident type $i \in \{\text{fatal, inc, non - inc, pd}\}$:

$$E\left(\frac{a_{ijt}}{vmt_{jy}}\middle|X\right) = \exp\Big(\beta_0 + \beta_1 1(j = \text{rural interstate}) * 1(t \geq t_{\Delta SL}) + \beta_2 X_{jt} + \theta_d + \theta_m + \theta_y + \theta_j\Big) \tag{5}$$

where a_{ijt} represents the number of accidents of type i on highway type $j \in$ {rural interstate, rural principal arterial, urban highway} on date t. X_{jt} includes control variables (weather, road, daylight and driver characteristics). Since the data are collapsed to the daily level, these control variables are averages (or relative frequencies) over all accidents that occurred on road type j. vmt_{jy} denotes vehicle miles traveled. θ indicates fixed effects for day-of-week d, month-of-year m, year y and highway type j.[9] In the definition of the dependent variable, the count variable accidents a_{ijt} is scaled by vmt_{jy} to reflect an accident rate. Count data models are equally suitable when the dependent count variable is scaled to non-integer values.

The average marginal effect is a function of both the covariates and the estimated parameters. However, the parameters have an easy interpretation in terms of proportional changes: β is the proportional change in $E(y|X)$ associated with a small change in X. If the dependent variable is binary, β is an approximation of the proportional change in $E(y|X)$ when the dummy variable changes from 0 to 1.

5.2. Accident results

Table 4 shows the effect of the speed limit changes on various types of accidents, for Oregon and Washington (1987) combined, using the negative binomial model in Eq. (5).[10] Columns (1)–(4) restrict the sample to fatal, incapacitating, non-incapacitating and property damage accidents, respectively. Column (5) reports the effects on total accidents.

The results from the upper panel indicate that the rates of various types of accidents on rural interstates went up sharply and significantly following the 1987 speed limit increases. The effect is strongest for fatal accidents, with an estimated increase of 44.1%.[11] This effect may appear large at first, but could be explained by the fact that the impact of a collision increases in the square of the difference in speed between two colliding objects. Thus, any given collision becomes substantially more likely to be fatal at higher speed. Moreover, I find that collisions

[8] This happens frequently for fatal (88.39%) and (non-)incapacitating injury (22.20%, 57.08%) accidents.

[9] Non-linear panel data models with fixed effects can suffer from incidental parameters bias. Since the numbers of groups in Eq. (5) is relatively small (28) and the total number of observations relatively large (6573), the fixed effects are estimated using a reasonable number of observations per group. Greene (2004) finds that the bias rapidly diminishes when the number of observations per group exceeds 10. Therefore, the incidental parameters concern is likely mitigated in this setting.

[10] Appendix Table C.3 presents similar regression results for Oregon and Washington separately.

[11] Standard errors are clustered at the highway type by year level. This assumes common unobserved shocks to accidents on a particular highway type within a given year. This is a conservative choice, since I control for road characteristics, road construction and weather. Moreover, accidents happen at different locations and times within a particular day and highway type, and are plausibly uncorrelated conditional on observables.

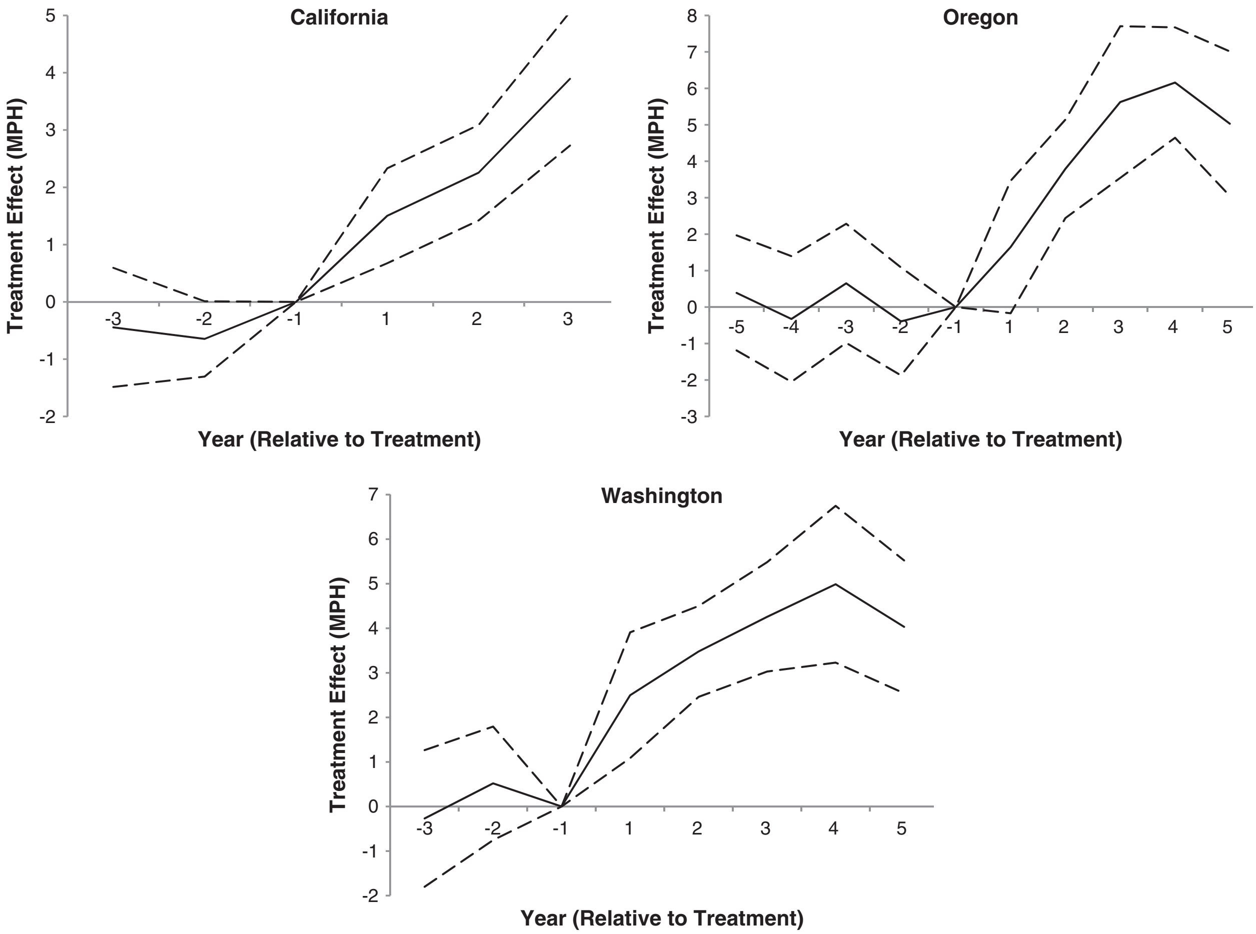

Fig. 3. Travel speed treatment effect coefficients by year. Graphs report the coefficients on the interaction between the treatment indicator and indicators for each year before and after the speed limit changes. The treatment effect is normalized to zero for the first year before the speed limit changes. Coefficients are normalized to reflect the effect of a 10 mph speed limit change. Dotted lines represent 95% confidence intervals, with standard errors clustered at the station by year level.

(of any type) happened more frequently after the speed limit changes. All increases in the rates of less severe types of accidents are substantial and statistically significant at the 1% level, and vary between 13.2% and 23.5%. Non-fatal accidents, not included in previous speed limit research, have the potential to be important contributors to total accident costs. Their coefficients are 2–3 times smaller than the coefficient on fatal accidents, but their incidence rates are about 4–50 times larger.

The estimates above show that both hypotheses hold: the speed limit changes not only led to a sizable increase in accidents, but also caused a shift towards more severe accidents. The ordered choice models presented in Appendix Table C.2 confirm this.

The lower three panels investigate if these results are robust to alternative econometric specifications. First, to mitigate concerns about traffic substitution, I exclude rural principal arterials and compare rural vs. urban portions of interstates. Table 2 shows that the effect of the speed limit changes on substitution of traffic towards treatment highways is estimated to be small, although some of the standard errors are large. Since traffic substitution is more likely between parallel rural freeways than between the urban and rural stretches of the same interstates, I include rural and urban interstate segments only in the second panel of Table 4. The accident effects are similar to the main specification. The effect on the most important accident category, fatal accidents, even increases in magnitude. This provides further evidence that the results are not driven by traffic substitution.

Second, I limit the control highways to a subsample of highways with (more) similar accident rates to the treatment highways, to alleviate the concern that rural interstates typically have lower accident rates per vehicle mile traveled than rural principal arterials (see Section 2). Using data on highway specific VMT in Oregon, I calculate total accident rates for all highway segments and remove the least safe rural principal arterials and urban highways until all three highway types have the same average accident rate. In Washington, highway specific VMT data have only been collected since 2004. I therefore cannot compare accident rates around 1987 directly.[12] Instead, I compute the total accident rate by highway using 2004 VMT data, and keep the control highways with accident rates in the bottom half. The third panel shows that the main results in the first panel are remarkably similar to those when the control highways are limited to a set of highways with similar accident rates to the rural interstates. In other words, the treatment effect is unlikely to be driven by the inclusion of the more accident prone control freeways (mostly unsafe rural principal arterials). This finding contributes to the credibility of identification assumption (2).

Third, I include more flexible, highway type specific, control coefficients. Accidents follow strong seasonal trends: the majority of accidents occur between November and February. It is conceivable that the effect of a speed limit change on accidents varies by season. A potential bias could arise if accidents on different highway types are affected by the weather in different ways, but this is not allowed in the econometric specification. If the winters following the speed limit changes were more severe than before, and if accidents on rural interstates responded more strongly to winter conditions, the treatment effect

[12] The negative binomial model is invariant to the *level* of VMT by highway type. Therefore, under the assumption that trends in VMT on individual roads were the same as the VMT trend for the corresponding highway type, the highway type specific VMT data can be used to estimate the treatment effect using subsets of highways of a particular type.

Table 4
Impact of the 1987 speed limit changes on accident rates: negative binomial models.

	(1)	(2)	(3)	(4)	(5)
	Fatal	Incapacitating	Non-incapacitating	Damage only	Total
Oregon & Washington combined					
Interaction (β_1)	0.365***	0.211***	0.140***	0.124***	0.131***
	(0.113)	(0.048)	(0.030)	(0.019)	(0.019)
Exact proportional change	0.441	0.235	0.150	0.132	0.140
Rural vs. urban portions of interstates					
Interaction (β_1)	0.495***	0.165***	0.160***	0.089***	0.101***
	(0.124)	(0.030)	(0.035)	(0.022)	(0.020)
Exact proportional change	0.615	0.179	0.173	0.094	0.106
Safe control highways					
Interaction (β_1)	0.510***	0.237***	0.154***	0.142***	0.151***
	(0.097)	(0.047)	(0.038)	(0.020)	(0.023)
Exact proportional change	0.666	0.267	0.166	0.153	0.163
Highway type specific control coefficients					
Interaction (β_1)	0.344***	0.216***	0.166***	0.165***	0.169***
	(0.082)	(0.040)	(0.030)	(0.027)	(0.024)
Exact proportional change	0.410	0.241	0.180	0.179	0.184
Share of total accidents	1.5%	6.7%	17.2%	74.6%	100.0%

Notes: The dependent variable is the number of accidents per VMT per day. The coefficient on the interaction term of 20 separate negative binomial regressions is reported. Highway type, year, month-of-year and day-of-week fixed effects are included. Controls are included. "Safe control highways" have the same average accident rates per vehicle mile traveled as rural interstates (OR), or include only the safest rural principal arterials per vehicle mile traveled (US-97, US-195, US-395, US-730). The exact proportional change is calculated as $exp(\beta_1) - 1$. Standard errors clustered at the highway type by year level in parentheses. Observations are taken within a six year symmetric time window around the dates of the speed limit changes. The number of observations is 6573 for all specifications except the "rural vs. urban interstates", which have 4382 observations. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

would absorb this weather-induced change in accidents. Specification (5) restricts β_2 to be equal for all highway types. The last panel of Table 4 relaxes this assumption and allows the control variable coefficients to vary by highway type.[13] It is reassuring to observe that the estimates are similar to the main specification.

Fig. 4 shows how the treatment effect for fatal and total accidents varies over time by allowing for a separate treatment effect for each pre- and post-treatment year.[14] There are no obvious visual differences in pre-trends, although the year-by-year estimates for fatal accidents are noisy due to their relative infrequency. After the speed limit changes, a strong effect on accidents appears directly in the first year, while the effect on speed builds up more gradually (Fig. 3). This suggests that speed limit changes have an immediate negative effect on road safety as drivers adjust to new traffic conditions, even if it takes longer for the effect on speed to reach the average post-treatment effect. One potential explanation is that congestion caused by increased accidents during an early adjustment period puts downward pressure on the average speed.

Appendix Table C.4 presents additional robustness checks. These demonstrate that the main results are not driven by alcohol or drug related accidents. The results of a time discontinuity specification are somewhat similar in magnitude to the main results, but the estimates are sensitive to the choice of the order of the time polynomial.[15] Finally, a comparison of proportional changes in accidents instead of accident rates per VMT yields somewhat higher point estimates. This underscores the importance of controlling for trends in VMT.[16]

Appendix Table C.5 shows the results of a difference-in-differences specification that investigates if the composition of vehicle types involved in accidents changed following the speed limit changes. I find that accidents involving only cars became more likely, while accidents involving only trucks, or both cars and trucks, became less frequent. This may suggest that light-duty trucks are relatively safer to drive at higher speeds given their better vision from the driver seat.

Notwithstanding the various robustness checks above, there remains a risk of confounding unobserved trends that affected accidents in the period around the speed limit changes. A first possibility is changes in seat belt laws (Cohen and Einav, 2003) and child seat laws (Insurance Institute for Highway Safety, 2001). However, changes in these laws affect occupants of vehicles on all highways. A confounding effect would only be present if there was a differential impact of the seat belt laws on rural interstates. Moreover, most of the changes happened outside the six-year window.[17] I also confirmed that there were no changes in DUI laws during the study period (Insurance Institute for Highway Safety, 2001).

A second challenge would be the existence of differential trends in enforcement of speed limits. It is not inconceivable that the higher speed limits were accompanied by a period of increased enforcement on rural interstates. This could have put a downward pressure on traffic speed and accidents. In that case, the estimates presented in this section should be interpreted as the combined effect of higher speed limits plus increased enforcement. Increased enforcement is likely to reduce the benefits of higher speed limits (time savings) as well as the costs (e.g., accidents), but the benefit–cost ratio is likely to remain similar. Moreover, the highway patrol in all three states stated that there had been no official orders to increase enforcement following the speed limit increases.[18]

[13] The treatment effect in winters is higher than in summers (Appendix Table C.4).

[14] See Appendix Table C.6 for a more detailed overview of the yearly post-treatment effects.

[15] I also performed a placebo test for the accident results. I ran regressions in which a hypothetical speed limit change was introduced on rural interstates 6, 7, 8, 9 and 10 years after the real speed limit change, using symmetric time windows of 6, 8 and 10 years (sufficiently long after the 1987 speed limit changes). This adds up to a total of 75 regressions: 15 for each category of accidents. I find that only 4 of these 75 regressions have t-statistics larger in absolute value than the corresponding t-statistics from the regressions with the 1987 speed limit changes. This corresponds to a "p-value" of 5.3%. These results suggest that the probability that the treatment effects found in 1987 are a result of pure chance is small. This test should be interpreted with caution, since the 75 regressions are not performed on independent samples. Details are available on request.

[16] Analyzing accidents per vehicle mile traveled also largely deals with potential substitution of traffic from control highways to rural interstates as a result of the speed limit changes, although the robustness checks in Tables 2 and 4 suggest that this is not a major concern.

[17] Oregon implemented a seat belt law on 7 December 1990. This falls almost outside the study period. Oregon and Washington introduced child seat laws on 1 January 1984, which is outside the six year windows for both states. Washington, however, required the use of seat belts from 11 June 1986.

[18] None of the states has collected data on enforcement by highway type, so I cannot control for enforcement in the regressions.

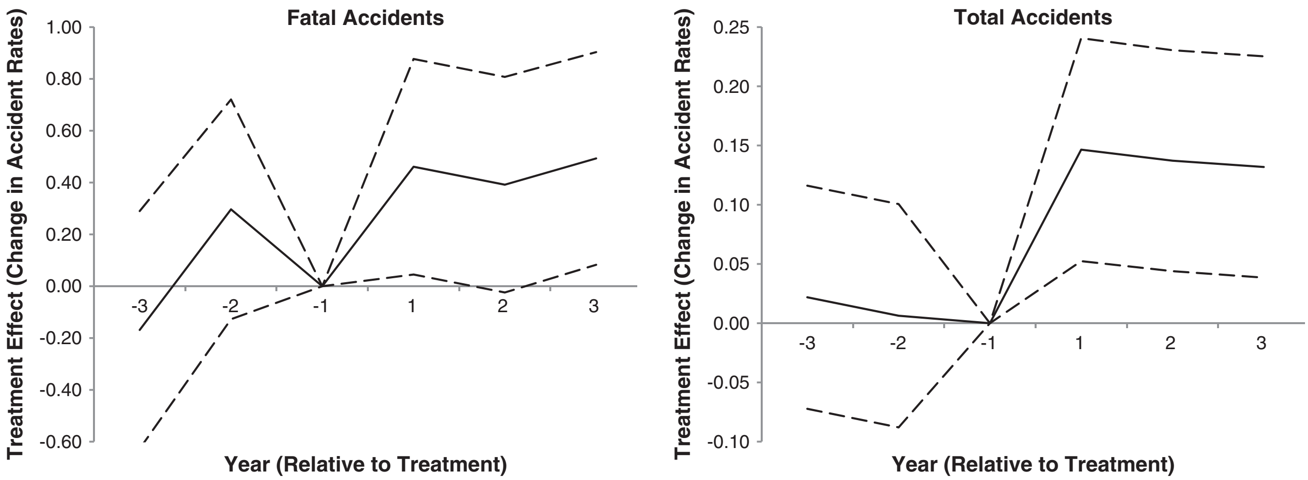

Fig. 4. Accident treatment effect coefficients by year. Graphs report the coefficients on the interaction between the treatment indicator and indicators for each year before and after the speed limit changes. The treatment effect is normalized to zero for the first year before the speed limit changes. Dotted lines represent 95% confidence intervals, with standard errors clustered at the highway type by year level.

6. The effect of speed limit changes on pollution

This section estimates the changes in air pollutant concentrations as a result of the 1987 speed limit changes. In Section 8, I use epidemiological "concentration–response functions" to translate these estimates to adult health impacts not covered in my data set.

6.1. Speed and air pollution

Vehicle emissions are important sources of local air pollution and global greenhouse gas emissions. Carbon monoxide (CO), nitrogen oxides (NO_2/NO_x), volatile organic compounds (VOCs) and carbon dioxide (CO_2) are all direct byproducts of the internal combustion process. Motor vehicles are responsible for the majority of CO emissions (67–97%), and a large share of NO_2 emissions (33–50%) (Chatterjee et al., 1997; Environmental Protection Agency, 2011a). Vehicles are also indirectly responsible for the formation of ground level ozone (O_3), through a reaction between NO_x and VOCs in the atmosphere in the presence of sunlight. Direct emissions of particulate matter smaller than 10 μm in diameter (PM_{10}) from gasoline car engines are minimal, but NO_x can react with other atmospheric pollutants to form PM_{10} indirectly (Environmental Protection Agency, 2011a). PM_{10} emissions from diesel-fueled commercial trucks are substantial, but truck speed limits did not change in most of the cases considered in this paper.[19]

Extensive engineering research shows that the relationship between vehicle speed, per-mile tailpipe emissions (CO, NO_x) and the rate of fuel consumption is U-shaped (e.g., Transportation Research Board, 1995; Litman and Doherty, 2009). Fig. 5 shows these relationships for the vehicle fleet in 1990.[20]

At low speeds, emissions per mile and the rate of fuel consumption are high. At moderate speeds (30–50 mph), fuel consumption and emissions per mile reach a minimum. At higher speeds, CO and NO_x emissions per mile increase rapidly and disproportionately to fuel consumption.[21] CO emissions triple when the vehicle speed increases from 55 to 65 mph. NO_x emissions increase by about 50%. The average fuel economy decreases by 18%.

In summary, the engineering literature suggests that we may expect that the 1987 and 1996 increases in speed limits above 55 mph led to substantial increases in most pollutant concentrations. However, we should expect little effect on PM_{10}, since most speed limits for trucks did not change.

6.2. The spreading of air pollution

There is a large literature in engineering, epidemiology and atmospheric modeling on the spreading of air pollution. One strand of papers focuses on *pollution gradients* at short distances from freeways.[22] At the same time, the EPA reports that many pollutants can spread very far: pollution from the U.S. Midwest can reach the East Coast (Environmental Protection Agency, 2011a). In fact, there are two relevant air pollution effects: changes in pollution gradients and changes in pollution background concentrations. These two phenomena can coexist: at any point in time, a pollution gradient can exist as long as atmospheric conditions (e.g., wind speed and direction) do not change much. Over the course of one or several days, wind directions change and the increased pollution disperses within a larger area ("buffer zone") around the freeway. There is no clear evidence on how wide such

[19] Several empirical studies have confirmed the strong relationship between traffic and local air pollution. Hu et al. (2009) document elevated air pollutant concentrations in a wide area surrounding freeways. Davis (2008) analyzes the effect of Mexico City's *Hoy No Circula* policy on local air pollution and finds no discernible impact. Knittel et al. (2011) find a strong relationship between unusually high local traffic density and pollution levels in California.

[20] Both gasoline and diesel engine emissions exhibit this U-shaped relationship, although the relationship between speed and CO is less steep for diesel (Transportation Research Board, 1995; Barlow and Boulter, 2009). I focus on gasoline vehicles, since diesel vehicles represented only 0.0–4.3% of new retail car sales and 1.7–8.5% of light-duty truck sales in the United States between 1975 and 2009 (Davis et al., 2010). Diesel-fueled commercial trucks represent only 3.2–3.6% of the vehicle fleet (and 6.8–7.6% of vehicle miles traveled) in the period 1980–2009 (Ward's, 2010).

[21] The laws of physics and vehicle engineering explain the positive relationship between speed and emissions at higher speeds. In terms of physics, the energy required per unit of distance to overcome air resistance is proportional to the square of speed. Thus, keeping technology fixed, fuel economy will eventually decline rapidly with speed. In terms of engineering, manufacturers can tune their engines to be most efficient at a higher speed following a speed limit increase within a limited range. In fact, today's cars operate more efficiently between 55 and 65 mph than the 1990 fleet did. However, this tuning makes the vehicle more polluting and less fuel efficient at lower speeds, and thus more expensive to drive. Moreover, it takes several years to bring new, re-optimized, vehicles to the market. Then, even if all new models were adjusted (an unlikely scenario), penetration would be slow due to limited vehicle fleet turnover: the average age of the U.S. light vehicle fleet between 1995 and 2009 was 8.4–10.2 years (Ward's, 2010). Today's engines still exhibit a U-shaped emissions pattern, albeit shifted to the right by about 10 mph.

[22] The typical experiment measures pollutant concentrations both upwind and downwind from a highway, and establishes how far the downwind station has to be moved from the highway to observe pollution levels within (e.g.) 5% of the upwind ("background") station (see Zhou and Levy (2007) for an overview). Most papers find that such gradients exist over relatively short distances, up to 1000 ft. However, several papers found gradients to stretch out over much longer distances up to two miles (Hu et al., 2009), or even that there is hardly any spatial decay in downwind concentrations during stable atmospheric conditions (Roorda-Knape et al., 1998).

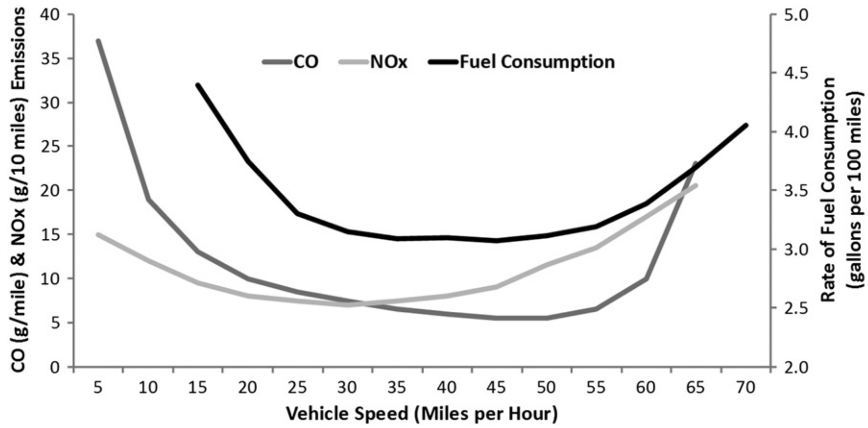

Fig. 5. The relationship between vehicle speed, emissions and fuel consumption. 1990 fleet-wide average for gasoline vehicles.
Source: Litman and Doherty (2009) based on the EPA's MOBILE5a model (http://www.epa.gov/oms/m5.htm).

buffer zones are in practice. It is reasonable to assume effects for distances up to 10 miles (EPA, personal communication, 2 June 2011). The relevant width of the pollution buffer is to some extent an empirical question.

Pollution gradients are relevant to people living very close to freeways by exposing them to high temporary emission levels. Pollution buffers are relevant to people within a wider area around the freeway by exposing them to generally elevated pollutant concentrations. Section 3 discussed the limited availability of pollution stations very close to freeways. For that reason, I can only test for the "pollution buffer effect" of speed limit changes with varying buffer distances. To keep a reasonable sample size, distances smaller than three miles are not used in this analysis.

6.3. Econometric framework

To estimate the effect of the 1987 speed limit change from 55 to 65 mph on pollution, I use a difference-in-differences estimator. I group stations based on their proximity to the relevant freeway segments. This estimator is preferred over a single difference design, since air pollution concentrations are not only determined by local emissions, but also by imported pollution from other regions and by state and federal environmental policies. I define treatment stations as being located at most x miles away from the 10 mph change. Control stations are located at least y miles away from the 10 mph change, where $y \geq x$ (Fig. 6). The central case is $(x, y) = (3, 3)$, but I report the robustness to the buffer distance.

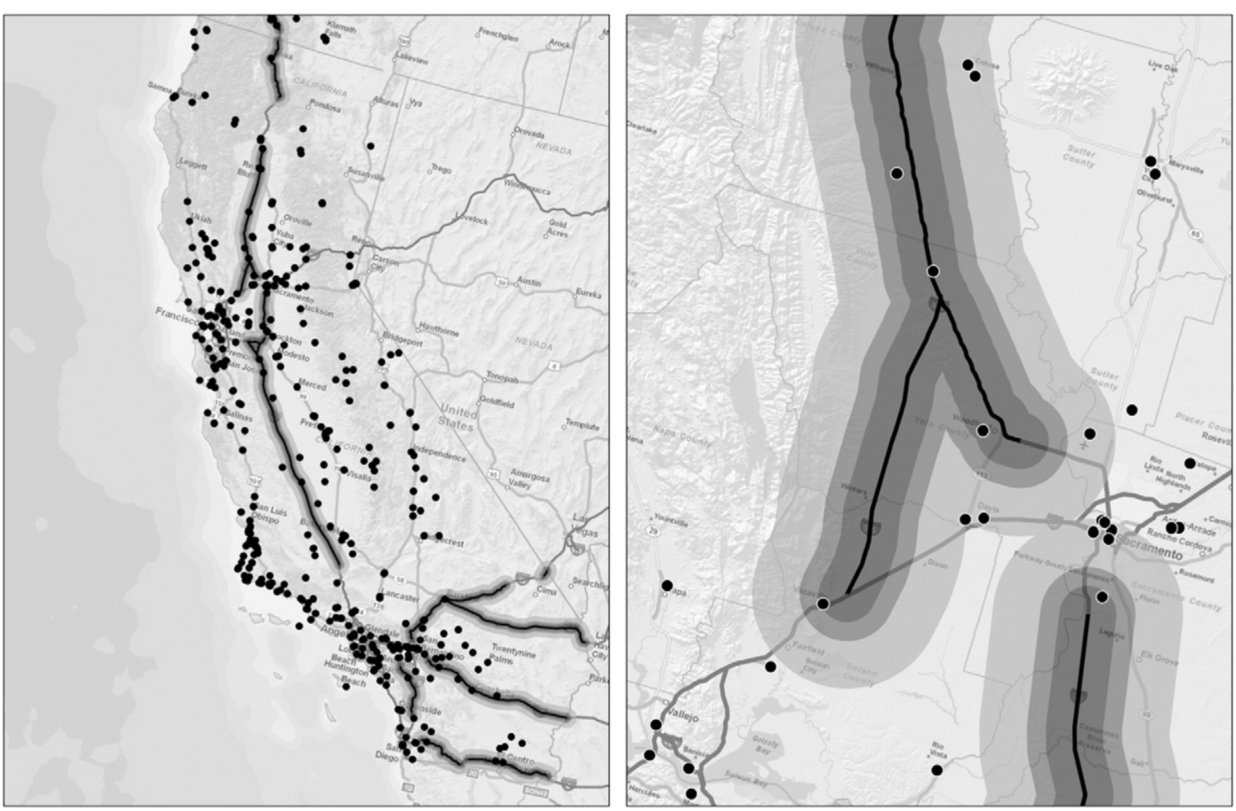

Fig. 6. Treatment and control pollution stations using various pollution buffer definitions. (Left panel) 3, 5 and 10-mile buffers around treatment freeways in California, 1987. Black dots: air pollution monitoring stations. (Right panel) 3, 5 and 10-mile buffers near Sacramento, CA. In this example, a (3,3) buffer corresponds to using 5 treatment stations. Using a (10,10) buffer, there are 15 treatment stations. Using a (3,10) buffer, there are 5 treatment stations and the additional 10 stations within the 10 mile buffer would be excluded.

Pollution concentrations are highly seasonal at both the monthly and daily levels, have a declining annual trend, and depend on meteorological conditions. For example, PM_{10} concentrations tend to be lower on rainy days. I therefore employ the following specification:

$$ln(p_{it}) = \beta_0 + \beta_1 1(dist \le x) * 1(t \ge t_{\Delta SL}) + \beta_2 X_{it} + \theta_i + \theta_d + \theta_m + \theta_y + \varepsilon_{it}, \quad dist \notin (x, y) \tag{6}$$

where $p_{it} \in \{CO, NO_2, O_3, PM_{10}\}$ indicates the pollution concentration for pollution station i at date t. X_{it} includes weather variables (average, maximum and minimum temperatures, wind speed and indicator variables for rain, fog, snow, hail, thunder or tornado) and county-level industry controls. θ represents fixed effects for pollution station i, day-of-week d, month-of-year m and year y. I include industry controls since unobserved shocks to industrial activity around the time of the speed limit changes could bias the estimates. This concern applies mostly to NO_2, since CO emissions from industrial sources are minimal. I therefore collected data on the number of industrial establishments and the associated employment levels (by industry SIC code) from the census County Business Patterns. I then selected the SIC codes for which CO, NO_2 and PM_{10} emissions exceeded one percent of total emissions during the period 1984–1990, and created county by year counts of "dirty sector" employment and large establishments (>1000 employees).

6.4. Pollution results

Table 5 shows the estimation results for varying treatment and control group cutoff distances in columns (1), (3) and (5). Columns (2), (4) and (6) restrict the control group to stations within 10 miles of the affected freeways. The sample includes observations from 1984 to 1990, approximately a symmetric time window around the speed limit changes. Data availability for treatment stations before 1984 is very limited.

The estimates show a large and statistically significant increase in concentrations of CO (+23%), NO_2 (+15%) and O_3 (+11%). The effect on PM_{10} is small and not significantly different from zero.[23] Restricting the control station distance to 10 miles does not change the overall conclusions, though the point estimates for CO and O_3 are mostly higher and those for NO_2 are somewhat lower.

These estimates suggest that the speed limit changes led to elevated pollutant concentrations in at least a three-mile buffer zone. The relative magnitude of the coefficients on CO and NO_2 makes sense in light of the discussion of Fig. 5, which shows that the effect on CO is expected to be larger than the effect on NO_2. As argued above, I should find no effect on PM_{10}. The results in Table 5 confirm this. This is a useful specification check which adds to the credibility of the results. I also confirmed that traffic substitution towards treatment highways does not drive the results.[24] Appendix Table C.7 indicates that as the size of the buffer zone increases to 10 miles, the effects become smaller in magnitude. This is consistent with a gradual decay of the impact of pollution.

I interpret the pollution results as broadly consistent with the literature on speed and pollution, air pollution dispersion and the effect of speed limit changes on speed. However, given the limited number of treatment stations, the results cannot be estimated at a great degree of precision, and future research (e.g., using some of the recent speed limit changes) is warranted.

7. The effect of speed limit changes on health

7.1. Health and air pollution

A final input for the cost–benefit analysis is the effect of the higher speed limits, through increased pollution, on health. Air pollution has been shown to negatively affect several health outcomes, among adults, children and infants.[25] Some highly credible studies of related questions suggest that speed limit increases might have adverse infant health effects. Currie and Walker (2011) find that the reduction in congestion related air pollution from the introduction of electronic toll payments in New Jersey and Pennsylvania decreased prematurity and low birth weight by 10.8% and 11.8%, respectively. Chay and Greenstone (2003) estimate that a 1% reduction in total suspended particulates leads to a 0.35% decrease in the infant mortality rate. Significant effects of CO and PM_{10} on infant mortality have been found in California (Currie and Neidell, 2005) and New Jersey (Currie et al., 2009). Using periods of unusually heavy traffic as an instrument, Knittel et al. (2011) find that the impact of pollution on infant mortality is even higher.

I follow the EPA's approach and draw on the epidemiology and health economics literature to quantify the effect of air pollution on adults' and children's health in Section 8. Health economics studies typically seek to exploit quasi-random variation in pollution exposure. Most epidemiological studies use a case–control design, in which subjects are not randomly assigned to pollutant concentrations. Rather, they assign treatment and control groups based on cross-sectional variation in observed exposure levels and apply a matching estimator. In addition to the infant health results discussed above, key findings from these studies are that air pollution leads to premature deaths, mostly due to respiratory causes but also from cardiovascular disease (Bell et al., 2004). Wilhelm et al. (2008) find that children living in high O_3 and PM_{10} areas experienced more frequent respiratory problems. CO (Neidell, 2004; Schlenker and Walker, 2011) and O_3 (Lleras-Muney, 2010) have been shown to have significant effects on child hospitalizations for asthma. The EPA uses some of these, and various other, studies to quantify health effects associated with changes in pollution.

I focus my empirical analysis on fetal health and infant health at birth for three reasons. First, since a fetus can only be exposed to air pollution in a relatively short nine-month window, we can be sure that only recent (prenatal) exposure to pollution can affect health outcomes at birth. Second, the economic costs of (infant and fetal) deaths tend to

23 Standard errors are clustered at the station by month and county by month level. Clustering by station is reasonable since it is plausible that certain unobserved shocks, such as changes in imported pollution from other regions, lead to correlation between the measurements from a station over time. Clustering at the county level is more conservative, by not treating stations in the same county as independent observations. To determine the relevant time dimension of the clustering, I investigated the autocorrelation functions (ACFs) of the measurements of all stations and for all five pollutants. I then recorded the first time lag for which the ACF was insignificant, and calculated the average over all stations for each pollutant. The result is 34.5, 33.3, 30.7 and 16.5 days for CO, NO_2, O_3 and PM_{10}, respectively. The median is slightly lower. Therefore, I conclude that the relevant time dimension for clustering is the monthly level.

24 Sections 4 and 5 present estimates that suggest that such substitution is limited. Even if I combine the highest, but insignificant, point estimate (5.49%, for California) with the speed-emissions curves in Fig. 5, 93% of the increase in CO and 75% of the increase in NO_2 tailpipe emissions can be attributed to higher speed. The remainder would be due to substituted traffic. In the extreme case where I take the upper end of the confidence interval for the substitution coefficient, I can easily rule out that the pollution increase attributable to speed is zero: 87% and 59% of the CO and NO_x increases are still due to higher speed, respectively.

25 A large number of medical and epidemiological studies have documented a strong association between adverse health outcomes for infants and fetuses, and for patients with respiratory diseases (Wilhelm and Ritz, 2003). High CO concentrations suppress the body's ability to deliver oxygen to organs and tissues. NO_2 has been associated with respiratory problems. PM_{10} can cause heart and lung damage, possibly through inflammations that weaken the immune system. O_3 exposure is thought to lead to breathing difficulties, inflammation, aggravation of asthma and increased susceptibility to pneumonia and bronchitis, as well as permanent lung damage (Seaton et al., 1995; Environmental Protection Agency, 2011a). Further, O_3 and PM_{10} have been shown to be risk factors for respiratory related post-neonatal mortality and sudden infant death syndrome (Woodruff et al., 2008).

Table 5
Regression results for the effect of the 1987 speed limit changes on pollution, for various buffer distances (California, Oregon & Washington combined).

	$(x, y) = (3, 3)$		$(x, y) = (3, 5)$		$(x, y) = (5, 5)$	
	All control stations	Control stations ≤ 10 miles	All control stations	Control stations ≤ 10 miles	All control stations	Control stations ≤ 10 miles
	(1)	(2)	(3)	(4)	(5)	(6)
CO	0.2313***	0.1790***	0.2425***	0.3332***	0.2437***	0.3679***
	(0.0576)	(0.0720)	(0.0576)	(0.0765)	(0.0405)	(0.0752)
	[0.0572]	[0.0699]	[0.0579]	[0.0802]	[0.0449]	[0.0800]
NO_2	0.1477***	0.0993**	0.1486***	0.1214**	0.0888***	0.0759*
	(0.0350)	(0.0370)	(0.0351)	(0.0477)	(0.0233)	(0.0453)
	[0.0356]	[0.0420]	[0.0358]	[0.0502]	[0.0256]	[0.0455]
O_3	0.1069***	0.1641***	0.1043***	0.1625***	0.0559***	0.1194***
	(0.0199)	(0.0238)	(0.0200)	(0.0268)	(0.0166)	(0.0245)
	[0.0218]	[0.0247]	[0.0220]	[0.0284]	[0.0196]	[0.0262]
PM_{10}	0.0236	0.0331	0.0072	0.0078	−0.0290	−0.0327
	(0.0358)	(0.0402)	(0.0366)	(0.0465)	(0.0287)	(0.0399)
	[0.0376]	[0.0414]	[0.0394]	[0.0516]	[0.0325]	[0.0446]
N_{avg}	179,308	31,306	172,538	24,325	179,308	31,306

Notes: The dependent variable is the log of the pollutant concentration. The coefficient on the interaction term of 30 separate regressions is reported. Standard errors clustered at the station by month level in ( ), and clustered at the county by month level in [] (used for stars). The time window is 1984–1990. N_{avg} denotes the average number of observations for the specifications in a column. All specifications contain weather and industry controls, and pollution station, day-of-week, month-of-year and year fixed effects. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

overshadow non-fatal health costs in most existing cost–benefit analyses (Environmental Protection Agency, 2002, 2011b). Third, extensive infant health data are available from birth records.

Fetal health is typically measured by low birth weight or gestational age at birth. Another direct measure of fetal health is the occurrence of fetal death. Data on fetal deaths are often hard to obtain and incomplete (Sanders and Stoecker, 2011). Fetal deaths are by far the most likely during the first trimester of the pregnancy, and such early fetal losses are rarely officially recorded. Some states or countries require the reporting of late-term fetal deaths. Pereira et al. (1998) find a positive association between late-term intrauterine mortality (28 weeks of gestation or more) and prenatal exposure to CO and NO_2 in São Paulo, Brazil. California requires fetal deaths of 20 weeks gestation or more to be registered. Since many of these more developed fetuses would have been viable if born alive, the welfare costs of such late fetal losses should be of particular interest to policy makers.

Currie (2011) describes a selection mechanism that operates when using live birth data to estimate the impact of pollution on fetal health and birth outcomes. An increase in pollution leads to more fetal deaths, but also to the survival of fewer marginal, less healthy, fetuses. This decreases the number of less healthy infants whose birth weight and gestational age gets recorded. It is an empirical question whether this "harvesting effect" dominates the pollution-induced reduction in average birth weight and gestational age among non-marginal fetuses.

Using the universe of birth records in California during the period 1984–1990, I estimate the impact of the 1987 speed limit changes on four infant health outcomes: fetal death, infant death, low birth weight (less than 2500 g) and (extreme) prematurity (gestational age 28 weeks or less). In the cost–benefit analysis (Section 8), I combine these results with estimates from the health economics and epidemiology literature to also take into account the effect of the speed limit changes on important adult health outcomes such as premature mortality due to pollution.

7.2. Econometric framework

To estimate the effect of speed limit changes on infant health, I use a difference-in-differences estimator similar to specification (6) for air pollution. The zip code of the mother's residence during pregnancy is the finest available geographic entity in the data. I classify zip codes by the distance between their population-weighted average centroid and the closest highway on which the speed limit changed (see Appendix A for details). Treatment zip codes have centroids at most x miles away from the change; control zip codes' centroids are at least y miles away from the change ($y \geq x$). I report results for various buffer definitions. Appendix B contains evidence that treatment and control zip codes are similar on observables, and that there are no differential pre-existing trends. I employ the following specification:

$$health_{it} = \beta_0 + \beta_1 1(dist \leq x) * 1(t \geq t_{\Delta SL}) * TI + \beta_2 X_{it} + \theta_z + \theta_m + \theta_y + \varepsilon_{it}, \quad dist \notin (x, y) \tag{7}$$

where $health_{it} \in$ {fetal death, infant death, low birth weight, premature birth} are binary variables indicating the health outcome for baby/fetus i at date t. X_{it} includes controls such as the race of the baby/fetus i and its parents, the mother's age at birth, the month in which prenatal care began, and medical complications during pregnancy and delivery (see Appendix A for details). θ represents fixed effects for zip code z, month-of-year m and year y. TI is the treatment intensity that indicates how long the fetus was exposed to increased pollution. $TI = \frac{month}{9}$ where $month = 1$ if the date of birth falls within the first month after the speed limit change, 2 for the second month, etc. For all births in or after the ninth month following the speed limit change, $TI = 1$.

7.3. Infant health results

Table 6 shows the regression results for the four infant health outcomes discussed above.[26] In short, the table presents evidence that the 10 mph speed limit changes in 1987 resulted in more fetal deaths. Combining the estimates with the population of pregnant women in the buffer zones (computed in GIS using zip code information from the birth records), I find that the higher speed limits caused 17–45 additional fetal deaths per year in California, depending on the buffer specification. Of these, 4–36 are third trimester fetal deaths. Taking the average of the treatment coefficients in Table 6, the increased pollution leads to a 0.07 percentage point increase (+9.4%) in the probability of a third trimester fetal death. This large effect is consistent with other

[26] Standard errors are clustered at the zip code level, to account for unobserved shocks at a regional scale. An example could be the opening or closing of a hospital, which could change the quality of prenatal care for many inhabitants of a zip code.

Table 6
Regression results for the effect of the 1987 speed limit changes on infant health, for various buffer distances (California).

	$(x, y) = (3, 3)$	$(x, y) = (3, 3)$	$(x, y) = (3, 5)$	$(x, y) = (3, 5)$	$(x, y) = (3, 10)$	$(x, y) = (5, 5)$	$(x, y) = (5, 5)$	$(x, y) = (5, 10)$
	$dist \leq 10$	$dist \leq 20$	$dist \leq 10$	$dist \leq 20$	$dist \leq 20$	$dist \leq 10$	$dist \leq 20s$	$dist \leq 20$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fetal death	0.00061	0.00072	0.00101**	0.00083*	0.00083*	0.00163***	0.00131***	0.00132***
Gestational age ≥ 98	(0.00050)	(0.00046)	(0.00052)	(0.00046)	(0.00047)	(0.00045)	(0.00038)	(0.00040)
Observations	418,099	1,027,017	333,980	942,898	718,105	418,099	1,027,017	802,224
Fetal death	0.00016	0.00028	0.00060	0.00041	0.00038	0.00131***	0.00104***	0.00102***
Gestational age ≥ 196	(0.00040)	(0.00035)	(0.00042)	(0.00035)	(0.00036)	(0.00039)	(0.00032)	(0.00033)
Observations	414,687	1,018,569	331,243	935,125	712,185	414,687	1,018,569	795,629
Infant death	0.00003	−0.00001	0.00006	−0.00001	−0.00002	−0.00007	−0.00006	−0.00006
Gestational age ≥ 196	(0.00067)	(0.00059)	(0.00071)	(0.00060)	(0.00060)	(0.00060)	(0.00046)	(0.00046)
Observations	412,777	1,014,103	329,714	931,040	709,132	412,777	1,014,103	792,195
Infant death	0.00010	0.00008	0.00028	0.00013	0.00009	0.00045	0.00033	0.00030
Gestational age ≥ 259	(0.00059)	(0.00054)	(0.00063)	(0.00054)	(0.00055)	(0.00051)	(0.00039)	(0.00040)
Observations	376,106	924,135	300,428	848,457	646,562	376,106	924,135	722,240
Low birth weight	−0.0024	−0.0023	−0.0021	−0.0023	−0.0022	−0.0006	−0.0007	−0.0006
Gestational age ≥ 196	(0.0021)	(0.0019)	(0.0023)	(0.0020)	(0.0020)	(0.0020)	(0.0016)	(0.0017)
Observations	412,777	1,014,103	329,714	931,040	709,132	412,777	1,014,103	792,195
Low birth weight	0.0022	0.0010	0.0026*	0.0010	0.0004	0.0020	0.0005	−0.0001
Gestational age ≥ 259	(0.0014)	(0.0013)	(0.0015)	(0.0013)	(0.0014)	(0.0012)	(0.0010)	(0.0011)
Observations	376,106	924,135	300,428	848,457	646,562	376,106	924,135	722,240
Premature birth	−0.00034	−0.00038	−0.00044	−0.00041	−0.00041	−0.00036	−0.00037	−0.00037
Gestational age < 196	(0.00062)	(0.00054)	(0.00063)	(0.00054)	(0.00055)	(0.00057)	(0.00047)	(0.00047)
Observations	414,444	1,018,101	331,051	934,708	711,911	414,444	1,018,101	795,304

Notes: The coefficient on the interaction term of 56 separate regressions is reported. Standard errors clustered at the zip code level in parentheses. Zip code, month-of-year and year fixed effects are included. Controls are included. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively.

findings that fetuses are very sensitive to pollution in utero (Sanders and Stoecker, 2011).

The fairly wide range reflects that the results are sensitive to the exact buffer definition. The air pollution results in Section 6 suggest that increases in background concentrations occur in three and five-mile buffer zones, beyond which the effect diminishes. It is difficult to argue for a preferred buffer choice,[27] but the estimates for the first two specifications in which $(x, y) = (3, 3)$ might be biased downward if much of the additional pollution spreads further than three miles.[28] Ignoring these specifications, the additional third trimester fetal death range narrows to 11–36. Fig. 7 shows the time pattern of the treatment effects for fetal mortality. The figure does not suggest a presence of differential pre-trends.

I find no significant impact on the other three health outcomes (infant death, low birth weight and premature birth). The effect on infant deaths (conditional on being born in the third trimester; premature birth is estimated separately) is about zero, while infant deaths among children born within the normal window (gestational age at least 37 weeks) increase slightly. The (statistically insignificant) estimates translate to 2–11 additional infant deaths per year. Extrapolating Currie and Neidell's (2005) finding that infant deaths increase by 0.18 per 1000 births for each ppm increase in the CO concentration, one should expect about two additional infant deaths per year following the speed limit changes. The lack of statistical significance is not surprising given the coarse geographic information at the zip code level. Moreover, since I do not know the mother's exact address, the distance variable is measured with error. This may lead to attenuation bias.

The effects on low birth weight, premature birth and (by inference) infant deaths before 37 weeks are insignificant but often negative. A potential reason is the "harvesting effect" discussed above: more fetuses do not survive, instead of being born prematurely and/or with low birth weight. Conditional on surviving and on being born at full term, the risk of low birth weight increases.

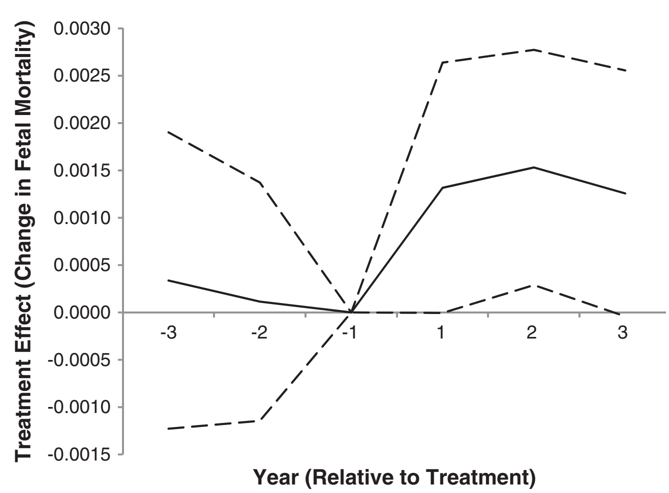

Fig. 7. Fetal mortality treatment effect coefficients by year. Graph reports the coefficients on the interaction between the treatment indicator and indicators for each year before and after the speed limit changes. The treatment effect is normalized to zero for the first year before the speed limit changes. Treatment zip codes have a population-weighted average centroid less than 5 miles from a highway with a speed limit change. Zip codes with centroids more than 20 miles away are excluded. Dotted lines represent 95% confidence intervals, with standard errors clustered at the zip code level.

[27] I removed mothers living in zip codes more than 10 or 20 miles away from the treatment freeways, since unobservable differences between geographically distant areas render the treatment–control comparison less convincing.

[28] The effects on fetal deaths are higher in the 3–5 mile range than in the 0–3 mile range. A possible explanation is that CO has been documented as the most relevant air pollutant for infant mortality (Currie and Neidell, 2005; Currie et al., 2009). Columns (2), (4) and (6) in Table 5 suggest that CO concentrations rose faster between 3 and 5 miles from the freeway than between 0 and 3 miles.

8. Cost–benefit analysis

8.1. Methodology

The cost–benefit analysis requires four key ingredients. First, the various costs and benefits need to be quantified. Many are estimated directly in Sections 4–7 (time savings, accidents and infant health). To make the cost–benefit analysis more complete, I use my estimates of the travel speed and pollution increases to infer increases in fuel consumption, greenhouse gases and several effects on adult health. Second, I classify the various costs into private versus external. Third, I value the costs and benefits using generally accepted literature estimates of, e.g., the value of time and the value of a statistical life. Since the range of such estimates is wide, I perform extensive sensitivity analysis. In Appendix D, I use the estimated standard errors to introduce uncertainty about several parameter estimates in a Monte Carlo simulation.

Of course, no cost–benefit analysis is complete. For example, the analysis abstracts from the marginal excess tax burden from changes in speeding ticket and gas tax revenues, changes in enforcement costs and increased driving pleasure at higher speeds.[29] Further, given that truck speed limits hardly changed (see footnote 5), I assume no beneficial effects on freight transport time and associated economic benefits such as inventory reduction (Shirley and Winston, 2004).[30]

8.1.1. Quantifying effects on fuel consumption and adult health

First, I quantify the increase in fuel consumption and associated greenhouse gases. Using engineering estimates for the fuel economy of vehicles of different vintages as a function of speed (Davis et al., 2010), I translate the estimated increase in travel speed into additional gallons of gasoline and tons of carbon emissions using the distribution of estimated speed increases (see Fig. C.1).

Second, I quantify the effect of increased pollution on several adult health outcomes. For that purpose, I rely on evidence from the health economics and epidemiology literature that the EPA uses to construct "concentration–response functions" to quantify health effects from changes in CO, NO_2 and O_3 concentrations (Environmental Protection Agency, 2011b). Specifically, I focus on premature adult mortality, which has accounted for a large majority of total health costs/benefits in previous cost–benefit analyses of air pollution regulations (Environmental Protection Agency, 2002, 2011b). In addition, I quantify the effect of increased pollution on respiratory related hospitalizations, emergency room visits for asthma, and productivity of outdoor workers. I then multiply these effects by the relevant affected population, computed using geocoded population data at the census block level. Appendix D describes the construction and sources of the concentration–response functions. Since several of the adult health estimates are highly uncertain and subject to methodological debate, I also present results without including them.

8.1.2. Private versus social cost–benefit classification

Time savings fully accrue to the vehicle occupants as private benefits. Increased fuel use is a private cost, while increased pollution and associated adverse health effects are external costs not taken into account by the driver. A driver need not be driving faster himself to experience higher accident costs of being on a road with a higher speed limit. A rational driver will consider his own increased risk of accidents and property damage, but ignore the risk imposed on others. I therefore split the effect on various types of accidents into private and external components. Fatalities from single-vehicle accidents (representing 52% of fatal accidents on highways) are treated as purely private if the driver or his passengers die in the crash. If a pedestrian (typically, someone standing in the shoulder of the road) dies, this effect is treated as purely external. For n-vehicle accidents not involving pedestrians, each driver bears a $1/n$ share of the total accident costs on average.[31]

8.1.3. Valuing costs and benefits

The two most important valuation parameters are the value of a statistical human life (VSL), and the value of travel time (VOT). I use broadly accepted values that are used in actual policy evaluation. For the VSL, I use the EPA's currently prescribed value of $6.3 million (2000 USD) as the central case estimate, but also perform sensitivity analysis. This value is based on a large literature in economics. An authoritative source is the meta-analysis by Viscusi and Aldy (2003), who report a mean VSL of about $7 million and a standard deviation of $5.6 million based on 49 studies. More recent papers find similar mean estimates for the VSL (Aldy and Viscusi, 2008; Kniesner et al., 2012).

The second key input is the value of travel time. I use the after-tax average wage as the central case value, supported by the economics literature (Small et al., 2005; Deacon and Sonstelie, 1985), and compute the value of time savings while adjusting for average vehicle occupancy. Small et al. (2005), who use combined revealed and stated preference data on people's choices whether or not to pay a toll for travel in a congestion-free express lane: note that their central estimate of 93% of the average wage may be an upper bound of the average VOT since their study area is affluent. In earlier work, Small (1992) reported a VOT of 20–100% of the average wage. In a recent study, Wolff (2014) estimates the effect of gasoline prices on unconstrained driving speeds, and finds that the implied VOT is 54% of the average wage rate. A further complication is the notion that the VOT should vary by trip purpose and by age, but convincing estimates are hard to find. I therefore also show results using the U.S. Department of Transportation's current VOT guidelines: valuing business travel at 100% of the average wage and personal travel at 60%. Finally, I value time by trip purpose using a recent proposal by the Victoria Transport Policy Institute (Victoria Transport Policy Institute, 2011), and apply age discount factors for children and the elderly.[32] The report proposes to value business travel at 150% of the average wage, commuting at 50%, personal travel at 25%, and leisure/vacation travel at 0% (Victoria Transport Policy Institute, 2011).

As described above, I use a single (statewide) value of the average wage as well as a single (nationwide) value of the VSL. This follows current government guidelines. An ideal cost–benefit analysis would use individualized VOT and VSL values, but such data is unavailable. This is especially important if driver characteristics on rural interstates are substantially different from the average. However, the average wage and family income in treatment areas is similar to control areas, suggesting that the aggregate values are a reasonable approximation.[33]

Table 7 lists the key valuation parameters. In addition to the parameters discussed above, I use broadly accepted estimates for non-fatal health costs. The EPA's numbers for hospitalization and emergency room visit costs only include medical expenditures and opportunity

29 Private benefits such as the latter make raising speed limits more attractive, but do not affect the main conclusion that there is a large gap between the private and social costs of driving faster.

30 One cannot rule out the possibility that the average *truck* speed increased somewhat as a result of faster-driving *s*. Unfortunately, the data does not allow me to test this.

31 While there exists considerable heterogeneity in the external accident costs depending on a vehicle's weight (Anderson and Auffhammer, 2014), this paper focuses on the average external cost for policy evaluation.

32 I use the adjusted 1990 data from the 1995 Nationwide Personal Transportation Survey to compute the fraction of person miles by trip purpose around 1990. This yields 19.6% (commuting), 8.2% (business travel), 5.0% (school/church), 30.8% (family/personal), 22.4% (leisure/vacation) and 13.4% (visiting friends/relatives). Under the U.S. DOT guidelines (http://www.dot.gov/sites/dot.dev/files/docs/vot_guidance_092811c_0.pdf), I value business travel time at the average wage, and all other purposes at 60% of the average wage (the average of the 50% and 70% prescribed for local and intercity traffic). In the VTPI scenario, I define personal travel as trips to school, church, family, relatives or friends. In addition, I multiply the VOT for people aged 65 and above or between 16 and 19 by 50%, and for younger children by 25%. Note that if highway travel is more likely to be business-related, the NPTS will understate time savings.

33 The average hourly after-tax wage in OR & WA counties with affected interstates was only 5.7% below the statewide average in 2001 (source: Quarterly Census of Employment and Wages). The median family income in the 5-mile buffer zone around the treated highways was 6.4% lower (source: 1990 U.S. Census).

Table 7
Cost–benefit valuation parameters (Oregon & Washington combined — 1987).

Parameter	Value	Source
Travel time valuation		
Average hourly after-tax wage	$15.37	Current Population Survey (CPS); NBER
Average vehicle occupancy rate	1.695	National Personal Transportation Survey, 1995
Fatal accidents and health impact valuation		
Value of a statistical human life	$7,375,305	Environmental Protection Agency (EPA, 2011b)
Cost of a respiratory hospitalization	$27,496	Environmental Protection Agency (EPA, 2011b)
Cost of asthma emergency room visit	$438	Environmental Protection Agency (EPA, 2011b)
Non-fatal accidents valuation		
Cost of an incapacitating injury	$319,272	National Safety Council (NSC)
Cost of a non-incapacitating injury	$77,866	National Safety Council (NSC)
Cost of a property damage accident	$7705	National Safety Council (NSC)
Gasoline cost valuation		
Gasoline price incl. taxes	$1.70/gal	Energy Information Administration (EIA)
Gasoline price excl. taxes	$1.32/gal	Energy Information Administration (EIA)
Social cost of CO_2	$21/t	Greenstone et al. (2013)

Notes: All values are expressed in 2006 USD. CPS: wage data obtained from http://cps.ipums.org/cps/. The hourly wage is inferred from annual income and hours worked per year. Observations with an hourly wage below $2.50 or above $100.00 per hour are dropped. NBER: average federal and state tax rate data obtained from http://www.nber.org/taxsim/allyup/. NSC: valuations obtained from http://www.nsc.org/news_resources/injury_and_death_statistics/Pages/EstimatingtheCostsofUnintentionalInjuries.aspx. The non-economic (QALY) part of the costs for incapacitating and non-incapacitating injuries is scaled by the ratio between the EPA's VSL and the NSC's VSL ($4,300,000). EIA: gasoline price data available at http://www.eia.gov/petroleum/data.cfm#prices.

costs (lost wages), but not lost quality of life. The National Safety Council's non-fatal accident costs do include the cost of reduced quality of life. The social cost of carbon is taken from Greenstone et al. (2013). I perform sensitivity analysis on the non-fatal health valuation parameters by specifying three health impact scenarios that reflect uncertainty in the parameters of the corresponding concentration–response functions (Appendix D). To be conservative, the central health impact scenario uses estimated treatment effects for fetal deaths using a 3-mile buffer distance only (Table 6), and values only third trimester fetal deaths (at the VSL).

8.2. Results

8.2.1. Private versus social cost–benefit results

I now show the cost–benefit calculations evaluated at the central case parameter values from Table 7. Annual net social benefits are estimated at −$189 million excluding adult health impacts, with a standard deviation of $94 million. The social costs ($345 million) exceed the benefits ($156 million) by a factor of 2.2. Using the adult health impacts from the central health impact scenario, which are admittedly uncertain, the net benefits decrease to −$390 million, with a standard deviation of $102 million (see Appendix D for details). The social costs exceed the benefits 3.5 times.

A useful related metric is the VSL that equates the expected costs and benefits of the speed limit changes. This number could be interpreted as the upper bound of the social planner's VSL, if the realized social costs and benefits were in line with ex-ante expectations. To justify higher speed limits, the social planner's VSL has to be below this upper bound. Likewise, private VSLs below the private VSL upper bound justify driving faster as a result of the higher limit. Table 8 reports these upper bound VSLs.

Table 8
The difference between the social and private trade-off of faster driving.

	Social trade-off					Private trade-off
Value of a statistical life below which	Higher speed limits are justified					Driving faster is justified
	(1)	(2)	(3)	(4)	(5)	(6)
	5.08	3.70	2.78	1.96	0.87	6.02
Using the following costs						
Fatal accidents (private)	✓	✓	✓	✓	✓	✓
Fatal accidents (external)	✓	✓	✓	✓	✓	✓
Non-fatal accidents (private)		✓	✓	✓	✓	✓
Non-fatal accidents (external)		✓	✓	✓	✓	
Fuel costs (private)			✓	✓	✓	
Climate damages (external)			✓	✓	✓	
Infant and fetal health (external)				✓	✓	
Adult health (external)					✓	

Notes: The upper bound VSL (the VSL that equates expected costs and benefits) is expressed in million 2006 USD. Fuel costs are valued at the tax-exclusive gasoline price in the social trade-off, but at the tax-inclusive gasoline price in the private trade-off.

Columns (1)–(5) in Table 8 show the upper bound VSL for a government that takes into account various subsets of social costs. If, in line with previous literature, governments take into account (private and external) fatal accident costs only, the upper bound VSL is $5.08 million. This is close to the middle of the range of estimated VSLs. However, when the set of costs taken into consideration gets more complete, the upper bound VSL decreases substantially to $1.96 million. When adult health effects are included, the VSL goes down even more dramatically to $0.87 million. I therefore conclude that raising speed limits was justified only when VSLs are in the $0.87–$1.96 million range, well below any VSLs considered in real-world policy making. Raising the 1987 speed limits was therefore not a good decision ex-post from a societal perspective. Moreover, columns (1)–(5) demonstrate that estimating the effects on outcome variables beyond fatal accidents has the potential to significantly change the conclusions drawn from a cost–benefit analysis.

The trade-off is quite different from a private perspective. For individual drivers, VSLs of $6.02 million or less (column (6)) justify driving faster as a result of the speed limit increase. This value is close to the EPA's central value. Given that drivers are heterogeneous in their assessment of the VSL, accident risks and fuel costs, many drivers may value the VSL below $6.02 million. I therefore conclude that, for the states considered in this study, there is a large difference between the social and private optimal speed choices: driving faster appears a rational choice for many drivers but a poor outcome for society as a whole.[34]

Fig. 8 shows this stark contrast between private and social costs as well as the relative importance of the various costs. Fatal accidents account for a substantial share of total costs, but other cost components omitted in previous work play an important role as well. Adult health costs evaluated at the central case estimates are as large a cost component (35%) as fatal accidents (35%), although there is considerable uncertainty about concentration–response function parameters. Non-fatal accidents (8%), gasoline and climate costs (6%) and infant/fetal health (15%) also cannot be ignored in a cost–benefit analysis. Fig. 8 underscores that only including fatal accidents oversimplifies the cost–benefit analysis, potentially leading to incorrect conclusions. As an

[34] The reported spread between private and social net benefits is conservative. I count all single-vehicle non-pedestrian crashes as fully internalized. In reality, other "phantom" vehicles were involved in many of these crashes, but the police had no way to verify this and correctly report the accident as a multi-vehicle crash (National Institute for Safety Research, personal communication, 30 September 2011). For example, if this applied to 50% of the single-vehicle accidents, the private upper bound VSL increases to $7.04 million.

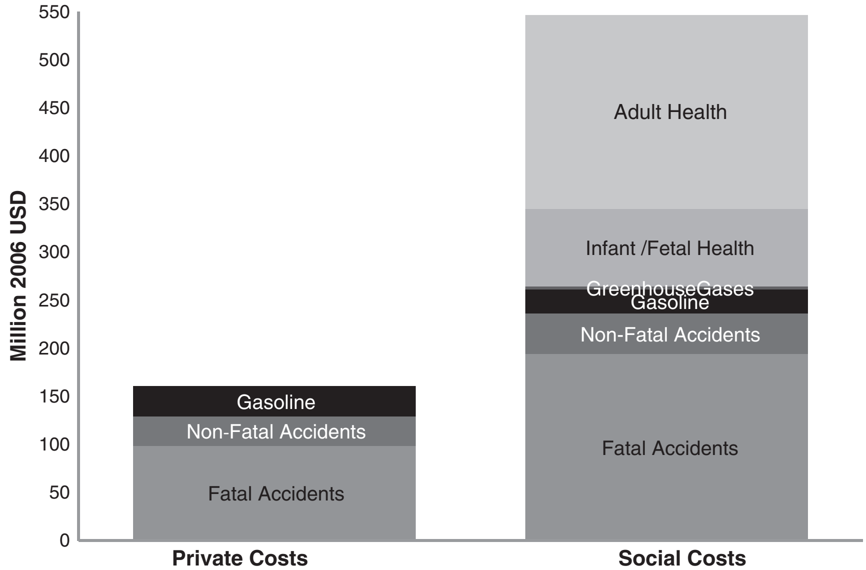

Fig. 8. Private versus social costs of the speed limit changes in 1987. Health impacts are evaluated at the central case scenario. Gasoline costs are valued at the tax-inclusive gasoline price in the private cost calculation, but at the tax-exclusive gasoline price in the social cost calculation.

illustration of this, Appendix Table D.2 compares my findings with the results from Ashenfelter and Greenstone (2004). While AG's full sample upper bound VSL estimate ($3.28 million; adjusted for inflation and vehicle occupancy) is on the lower end of conventional VSL estimates, their estimates for California and Oregon would lead to the conclusion that the speed limit increases in 1987 were beneficial from a societal perspective. This paper's upper bound VSL range of $0.87–$1.96 million rejects that conclusion. However, when I only include fatal accident costs, the social trade-off is similar to the results obtained by AG.

8.2.2. Sensitivity analysis of net benefits

The implied VSL upper bound of $0.87 million indicates that the conclusion would not change when the EPA's low and high VSL values are used ($3.7 and $8.9 million, respectively). Different health impact assumptions also do not affect the main findings. Even under the low impact scenario, social costs are 2.5 times as high as private costs and the upper bound VSL equals just $1.31 million. Under the high impact scenario, social costs exceed private costs 5.2 times, and the upper bound VSL equals $0.55 million. Different valuations of travel time are especially interesting to consider. Valuing time using the U.S. DOT or VTPI guidelines would reduce annual net benefits by $57–109 million. The costs, excluding adult health impacts, now exceed the benefits by a factor of 3.5 and 7.3, respectively. Using the U.S. DOT guidelines rather than the after-tax average wage, the upper bound VSL approaches zero ($0.08 million). In that case, the non-fatal social costs alone (non-fatal accidents, fuel costs, climate damages and non-fatal health costs) almost outweigh the benefits. The private upper bound VSL decreases to $2.36 million.

9. Discussion

This paper finds that 55 mph was a better speed limit than 65 mph and that there was a large difference in private and social net benefits. What, then, was the optimal speed limit? While I cannot directly calculate the optimal speed limit because I estimate a “slope” of the net benefits curve at 55 mph but no “curvature”, it was probably not much below 55 mph. The relationship between speed, pollution and fuel consumption becomes quite flat below 55 mph. Therefore, driving slower will yield few pollution related health benefits or save gasoline costs. Without such benefits, the cost/benefit ratio at 55 mph falls to 1.5 — fairly close to parity. The ratio will further approach 1 for speeds below 55 mph as the effect on accidents is likely to be non-linear in speed as well.

I reach my conclusions using speed limit changes between 55 and 70 mph for mostly rural freeways in the western United States in 1987 and 1996. Whenever comparing across time, state and urban/rural roads was possible, the estimates appeared stable. However, it is reasonable to ask how these findings can be relevant for speed limit changes today, in different countries, and for a different speed range. Various countries and states are currently debating speed limit changes anywhere in the 55–90 mph range. I discuss two questions. First, how would the various effects of speed limit changes be different today? Second, how would the difference between private and social net benefits change?

Regarding the first question, it is possible to extrapolate the pollution estimates using past and current speed-emission profiles. Such information is available. Cars have become less polluting over the past two decades. The EPA MOVES model's most recent estimates for the current vehicle fleet show that the U-shaped relationship in Fig. 5 is still present but less steep. It starts curving upwards sharply around 65 mph instead of 55 mph for the 1990 vehicle fleet (EPA, personal communication, 4 May 2011). The relationship between speed and fuel consumption is still increasing but shifted to the right by approximately 10 mph. This information, combined with up-to-date epidemiological and health economics studies, makes extrapolation of pollution and health effects possible. A 55 to 65 mph speed limit increase would lead to small pollution and adverse health effects today. These costs would rise again for the currently proposed 65–75 mph and 75–85 mph increases, for which the upward relationship between speed and pollution remains even for today's vehicles.

Extrapolating the effect on accidents is more difficult. The relationship between speed limits and accidents is likely non-linear. Also, base accident rates at 55 mph are lower than in the 1980s due to enhanced vehicle safety. This improved safety record has been used to argue for higher speed limits up to 90 mph. At these speeds, accidents and pollution are likely to be substantial costs even for today's vehicle fleet. The trade-off between travel time and the costs of accidents, pollution and health will therefore remain relevant for future generations. Given the recent speed limit changes and detailed data availability, it may eventually be possible to re-estimate the relationship between speed limits, speed and accidents at today's higher speeds.

Because the net benefits from raising speed limits vary across or even within states, my results do not imply that the optimal speed limit is the same everywhere. Net benefits depend on observable factors such as population density around freeways, pregnancy rates and traffic flows. Such extrapolation across locations is a straightforward extension of the cost–benefit analysis, although of course the estimated treatment effects remain local to the quasi-experiment in this paper.

The discussion about extrapolation implies that today's gap between private and social net benefits will be smaller for a 55 to 65 mph speed limit increase. For higher speed limits, the gap is likely to remain substantial because of the steeper speed-emission profile in that range and the external cost component of accidents. Several factors influence the ratio of private and social costs. Fuel costs are higher than in 1987. This increases private costs relative to external costs. Changing speed limits on urban rather than rural highways achieves the opposite, because the density of the surrounding population that gets exposed to higher pollution levels is higher.

10. Conclusion

In this paper I estimate the private and external costs and benefits of driving faster on freeways. I find that private and social net benefits differ substantially: many individual drivers rationally chose to drive faster when they could, but – at least ex-post – society should not have opted for the higher speed limits. The implied social upper bound VSL ($0.9–$2.0 million) is well below the implied private upper bound VSL ($6.0 million). I conclude that the optimal speed limit was lower than 55 mph, but not much lower because of the highly non-linear relationship between speed, pollution damages and gasoline consumption.

This raises the question why governments decided to raise speed limits. One potential explanation is that they did not behave as benevolent social planners, and responded to the private desires of their constituents, who may have wanted to drive faster. This is especially likely if the costs and benefits of driving faster are not evenly distributed across regions: urban drivers benefit from driving faster on rural freeways, but do not suffer from local pollution. Another possibility is that a lack of information explains the decision to raise freeway speed limits. Non-fatal accidents, pollution and health impacts were hardly or never mentioned in any official documents. The scientific evidence on the effect of pollution on health was largely unavailable in 1987 (see Appendix Table D.1). This leads to a potentially serious underestimation of the total costs.

This paper's conclusions do not rule out that other instruments could deal with speed externalities more effectively than speed limits. An ideal Pigovian tax on speed would consist of a combination of a gasoline tax for climate damages, emissions taxes for local air pollutants in exhaust gas (which vary with speed), plus a speed-dependent tax to internalize accident risk imposed on others (which is also a function of traffic conditions). A gasoline tax exists, real-time emissions taxes could conceivably be implemented using sophisticated on-board computers, but a speed tax on accidents would face enormous informational requirements and technical challenges. Therefore, speed limits are likely to remain the dominant policy instrument for the foreseeable future.

Flexible speed limits that vary by time-of-day and road conditions could move us closer to optimal speed taxation. Several freeways near places such as Amsterdam, Atlanta, London and Munich have implemented or are planning to use variable speed limits. This provides an interesting avenue for future research, especially because data from a network of real-time speed, congestion and accident monitoring on thousands of highway locations are becoming more readily available.

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2015.02.001.

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