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State versus Consumer Regulation
An Evaluation of Two Road Safety Interventions in Kenya

James Habyarimana and William Jack

8.1 Introduction

Traffic fatalities constitute a large share of both deaths and the burden of disease in the developing world, and continue to rise. Traffic accidents were ranked as the tenth leading cause of death in 2001, and were projected to be the third or fourth most important contributor to the global disease burden in 2030 (Lopez et. al. 2006). By that date, road accidents are expected to account for 3.7 percent of deaths worldwide—twice the number due to malaria (Mathers and Loncar 2006).[1]

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1. Country-level data are generally less reliable. Odero et al. (2003) suggest that fatality rates in Kenya are extremely high with seven deaths from thirty-five road crashes every day, and that the impact of prevailing interventions is dismal.

Absent market failures, traffic accidents can be viewed simply as a cost of driving, and one that road users rationally incur in their consumption of transport services.[2] On the other hand, dangerous driving imposes obvious external costs on other road users—both those traveling in other vehicles, and, especially in the developing world, pedestrians. Standard approaches to such market failures include either direct government regulations, the use of the price mechanism through the imposition of taxes and/or penalties levied on dangerous driving, or both.

Social marketing and advertising campaigns provide an alternative means by which behavior change might be induced, and have been used extensively in areas of public health in general, and road safety in particular. Which of the two approaches—that is, social pressure through marketing or government enforcement of explicit regulations—is more (cost-) effective in inducing prudent behavior is of general interest. But it is especially important in the context of developing countries with at best limited, and at worst dysfunctional, institutions that compromise the effective enforcement of well-intentioned rules.[3]

This chapter makes such an assessment by comparing two interventions in the Kenyan minibus, or *matatu*, sector that broadly fall into the two categories. In particular, the safety of matatu travel as measured by insurance claims is compared in two settings: first, in the wake of the so-called Michuki rules, regulatory requirements implemented in 2004 governing the operation of matatus; and second, in the context of a consumer empowerment campaign that circumvented all forms of public intervention and enforcement but instead appealed directly to passengers to monitor their drivers. This comparison should be interpreted as a case study, as the regulatory reforms reflected a broad-based policy intervention in 2004, and the consumer empowerment project was a relatively small-scale randomized control trial (RCT) implemented three years later. Nonetheless, the comparison provides useful information on the comparative effectiveness of alternative interventions in contexts with limited institutional capacity.

The Michuki rules, which required retrofitting of vehicles with certain safety devices and other reforms as outlined in the next section, were widely believed to have led to an immediate and sustained improvement in the safety of Kenya's roads. However despite this view, we find that most of the perceived effects were driven by the short-run compliance costs imposed on vehicle owners and drivers, as opposed to their behavior, and that a month after the rules were introduced there was no discernible effect on insurance claims. In contrast, the consumer empowerment campaign we examine,

2. Of course, the uncertainty surrounding road accidents rationalizes insurance against such events and the costs they impose, but this does not imply that the number of accidents is too high.

3. The two approaches could also exhibit important complementarities, although we cannot assess this possibility here.

which encouraged passengers to actively complain directly to their drivers when they felt unsafe, led to a remarkably large reduction in insurance claims of between one-half and two-thirds.

8.2 Two Approaches to Driver Behavior Change

Although official data are incomplete, fourteen-seater minibuses, or *matatus*, are believed to be involved in, and indeed to cause, a large share of the over 3,000 road deaths in Kenya each year. Traditionally overcrowded and undercapitalized, matatus were notorious for careening along Kenya's roads, from the highways joining the Indian Ocean coast with Lake Victoria deep in the interior, to the crowded streets of the capital Nairobi and the country's larger cities of Mombasa and Kisumu. In this section we review two approaches to improving the safety of matatu travel.

8.2.1 The Michuki Rules

In February 2004, new government regulations initiated by and subsequently named after then-Minister of Transport John Michuki became effective. The objectives of the regulations were to "reduce accidents caused by overspeeding; enhance safety of commuters; ensure responsibility, accountability and competence of drivers and conductors; eliminate illegal drivers, conductors and criminals that had infiltrated the industry; and facilitate identification of vehicles and restrict their operation to authorized routes" (Chitere and Kibua 2004, 7). Under the reform, all matatus were required to comply with a series of rules aimed at reducing reckless driving, including:

- the installation of *speed governors*, devices that would cause the engine to shut down automatically if the vehicle's speed surpassed the national speed limit of 80km/h;[4]
- the installation of *passenger safety belts*, which had until then been rare in public service vehicles (i.e., minibuses and buses), and infrequently used even when available;
- the painting of a yellow stripe on all matatus;
- the restriction of matatus to clearly specified and documented *routes*;
- the limitation on the *number of passengers* to thirteen, plus the driver;[5] and
- the licensing and vetting of drivers and conductors.[6]

4. Speed limits in the cities are 50km/hr.

5. Previously the official passenger limit had been eighteen (five standing), although vehicles with as many as thirty passengers could be observed plying the streets of Nairobi. The seat belt rule was as much a means of enforcing penalties for overcrowding (passengers without a seat belt were fined) as a direct safety intervention.

6. Upon implementation of the rules, drivers and conductors had to receive a "Certificate of Good Conduct" before being able to resume work. They had to dress in prescribed uniforms and post their pictures in the vehicle.

The resource costs of adopting the vehicle modifications were high, amounting to about \$750 for seat belts, speed governors, and inspections (Chitere and Kibua 2004).

Although we do not have data on enforcement of the Michuki rules, anecdotal accounts suggest that compliance with the new requirements, such as the installation of speed governors and seat belts, was initially relatively high. However, the impact of the regulations on actual driving behavior was not clear. For example, the development of second-generation devices known as "speed governor governors," which would allow a driver to manually engage or disable the speed governor while in motion, made it harder for police patrols to apprehend cheating operators. As noted by other authors (Chitere and Kibua 2004), corruption also likely limited the impact of the rules on actual driving practices, as matatus would be randomly stopped with high probability at ubiquitous police roadblocks, independent of their speed or safety, and their drivers shaken down for bribes. These shakedowns were made easier and more remunerative by the new regulations, as they provided the police with a variety of additional dimensions on which drivers and conductors might be found in noncompliance, and the high fixed costs associated with appearing in court to contest a citation generated sizable rents. Such arbitrary taxation would have reduced the return to driving safely in general, and to adhering to the speed limit in particular.

The initial impact of these rules was a sudden reduction in the number of matatus and larger buses on the roads, as they were removed from service to be fitted with the necessary control devices and seat belts. In the days and weeks following the adoption of the regulations, thousands of Kenyans walked miles to work and to school, and there was a popular belief that the roads were safer. Accordingly, bus fares increased dramatically in this period, as demand far outstripped supply.

Government statistics showed an immediate and dramatic reduction in road accidents following the adoption of the new rules. For example, Mutugi and Maingi (2011) report a fall in all road accidents from 2003 and 2004 of about 20 percent.[7] Similarly, Chitere and Kibua (2004) report "fatal, serious, and slight" accidents all falling by about 40 percent each in February–July 2004 compared with the same six-month period a year earlier. However, they also reported instances of tampering with speed governors, underuse of low-quality seat belts, continued overcrowding, and laxity of law enforcement.

8.2.2 Consumer Empowerment

Institutional weakness and corruption may compromise the effectiveness of a variety of reform efforts, especially those that rely on third-party

7. Mutugi and Maingi report accidents falling from 133,378 in 2003 to 10,717 in 2004, but we suspect a typographical error in the first figure, which was more likely 13,378.

enforcement. In the case of public transportation, an alternative to top-down campaigns like the Michuki rules is to empower passengers to demand higher quality services directly, not by threatening to report a bad driver, but simply by openly complaining to him.[8] To motivate the potential impact of such a strategy, we argue that complaints to the driver represent contributions to a local public good, and that a collective action problem among passengers could arise accordingly. Multiple equilibria can exist in such environments, characterized by different aggregate levels of public good provision. This suggests that lowering the resource or psychic costs of complaints, for example, by making them appear more legitimate and thereby giving passengers a voice, could lead to discrete changes in the intensity of consumer monitoring and enforcement, and perhaps meaningful changes in safety and outcomes.

To test these ideas, we conducted a randomized control trial of an intervention aimed at empowering matatu passengers to exert pressure on drivers to drive more safely.[9] The intervention was simple and cheap: stickers with evocative messages intended to motivate passengers to take demonstrative action—to "heckle and chide" a dangerous driver—were placed in about half of roughly 2,300 recruited matatus. The stickers included graphic images of injuries, and text in English and Kiswahili encouraging passengers to "Don't just sit there! Stand up! Speak up!"

An initial small pilot in fall 2007 was compromised when we discovered that stickers were quickly being removed from treated vehicles. In response to this, we reissued stickers and scaled up to the full sample in early 2008, but in an attempt to ensure higher rates of compliance we ran a weekly lottery among drivers of participating treatment matatus. Each week three prizes of 5,000, 3,000, and 2,000 Kenyan shillings were awarded to drivers (about US$60—roughly a week's wages, US$35, and US$25) if their vehicle was found to have all stickers intact upon inspection by our field staff.

8.3 Data and Empirical Strategy

We were given access to vehicle-level insurance data by four large insurance companies in Kenya.[10] These data sources spanned different time periods, as illustrated in figure 8.1, and provided different levels of detail. All four companies provided data on a claim-by-claim basis including the date of the accident, the class of vehicle (private cars, commercial vehicles such

8. Neither author of this chapter has observed a female matatu driver in Kenya.

9. A full description of the intervention and results can be found in Habyarimana and Jack (2011).

10. The companies were Standard Assurance, Blue Shield Insurance, Africa Merchant Assurance Company Ltd. (Amaco), and Direct Line Assurance. Since mid-2009, Standard ceased to operate, while Blue Shield was placed under receivership in September 2011.

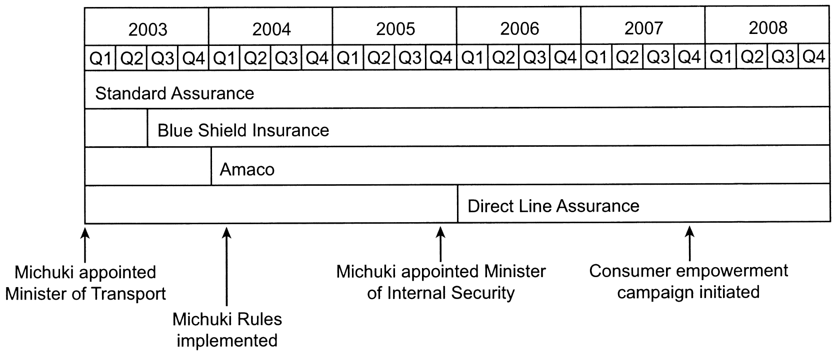

Fig. 8.1 Timeline of policy and experimental interventions and data availability from insurance companies

as trucks, buses, matatus, motorcycles), deaths, injuries, and claim amounts. In addition, Standard Assurance and Direct Line Assurance provided data on insurance policies, not just claims, on a month-by-month basis.

Given the timing of events, as illustrated in figure 8.1, and the comprehensive nature of the information provided, the data from Standard Assurance are best suited to evaluating the impact of the Michuki reforms. On the other hand, we use data from all four insurers to assess the impact of the consumer empowerment campaign.

8.3.1 Michuki Rules: Summary Statistics and Empirical Strategy

Table 8.1 shows a summary of the underlying policy-level data that is used to estimate the impact of the Michuki rules. While the duration of the typical policy sold varies from one month for matatus to a year for private vehicles, we expand all annual policies into twelve month-level policies. As the table shows, there is considerable variation from year to year, driven primarily by the presence of close substitutes offered by competing insurance companies.

Figures 8.2, 8.3, and 8.4 expand the policy and claims data shown in table 8.1 for matatus, private vehicles, and other buses, respectively. The number of matatu policies sold fell somewhat in the month before and of the reform, while policies for the other two classes of vehicle did not respond. Claims rates for private vehicles and other buses are noisy, but exhibit no obvious change around the time of the reforms, while matatu claims appear to drop in the month preceding the intervention.

Data (not shown here) from the other insurance company that was in business at the time of this reform confirms this dip in operational vehicles.

In evaluating the impact of the Michuki reforms we employ a difference-in-differences estimation strategy, using private vehicles as a control group. Private vehicles, while not directly affected by the reforms, do not constitute

Table 8.1 **Average monthly policies sold/claims filed between February 2002 and February 2006**

	Policies			Claims		
Month	Private vehicles (cars)	Buses	Matatus	Private vehicles (cars)	Buses	Matatus
January	4,477	2,163	2,833	38	14	76
	(300)	(283)	(825)	(11)	(5)	(26)
February	3,768	1,837	2,182	41	15	81
	(1,516)	(637)	(918)	(8)	(1)	(23)
March	3,664	1,845	2,134	38	19	76
	(1,482)	(605)	(807)	(11)	(5)	(20)
April	3,737	1,868	2,140	37	13	92
	(1,278)	(461)	(674)	(10)	(4)	(38)
May	3,812	1,910	2,169	37	21	79
	(1,092)	(322)	(553)	(10)	(2)	(26)
June	3,841	1,926	2,254	35	14	78
	(884)	(131)	(489)	(13)	(3)	(33)
July	4,004	2,045	2,489	44	14	88
	(697)	(112)	(623)	(13)	(3)	(8)
August	4,119	2,106	2,654	37	18	89
	(566)	(186)	(689)	(11)	(4)	(16)
September	4,195	2,111	2,684	41	19	95
	(470)	(252)	(705)	(11)	(3)	(21)
October	4,298	2,126	2,826	41	17	101
	(378)	(293)	(859)	(16)	(4)	(20)
November	4,390	2,164	2,954	41	9	84
	(273)	(322)	(1,005)	(17)	(1)	(19)
December	4,536	2,194	2,965	35	14	77
	(273)	(338)	(902)	(11)	(7)	(16)
Total	4,064	2,021	2,517	39	16	84
	(855)	(356)	(752)	(11)	(5)	(22)

Notes: Standard deviation in parentheses.

an ideal control group, however, because safer operation of matatus could result in fewer accidents involving other vehicles, and hence lower claims. To the extent that not all matatu claims events involve private vehicles, this estimation strategy has a chance of at least identifying the effect of the reform on nonprivate vehicle-related accidents. Unfortunately, the claims data used here does not identify the type of vehicle involved in the claims event.

The simplest approach is to collapse all the vehicle-month-level data into two periods representing the time before and after February 2004, and to use the following specification:

$$y_{ijt} = \theta_0 + \phi M_j + \delta M_j \text{Post} + \gamma \text{Post} + \varepsilon_{ijt}, \tag{1}$$

where y_{ijt} is an indicator variable equal to 1 if vehicle i in category j has had an accident, M_j is a dummy variable equal to 1 if category j is a matatu,

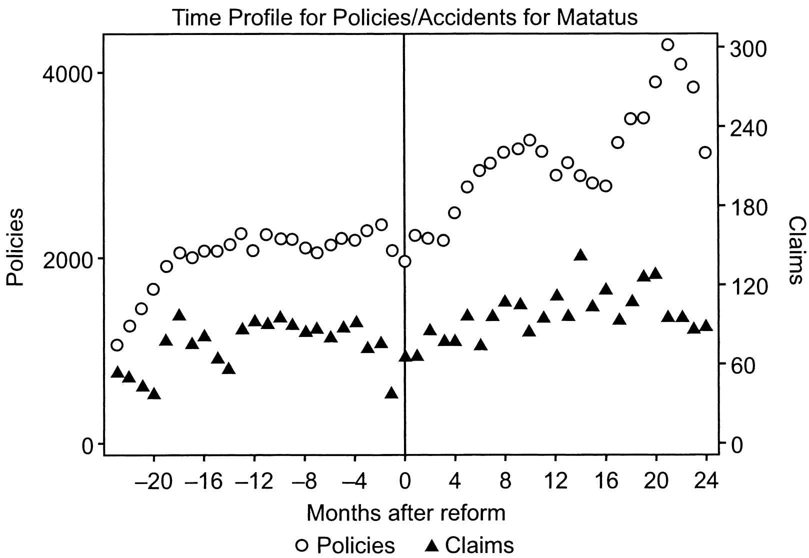

Fig. 8.2 Policies and claims, matatus

Note: The figure plots the number of monthly policies sold for minibuses/matatus (left axis) and claims-related incidents (right axis) against time in months since the Michuki reforms of February 2004.

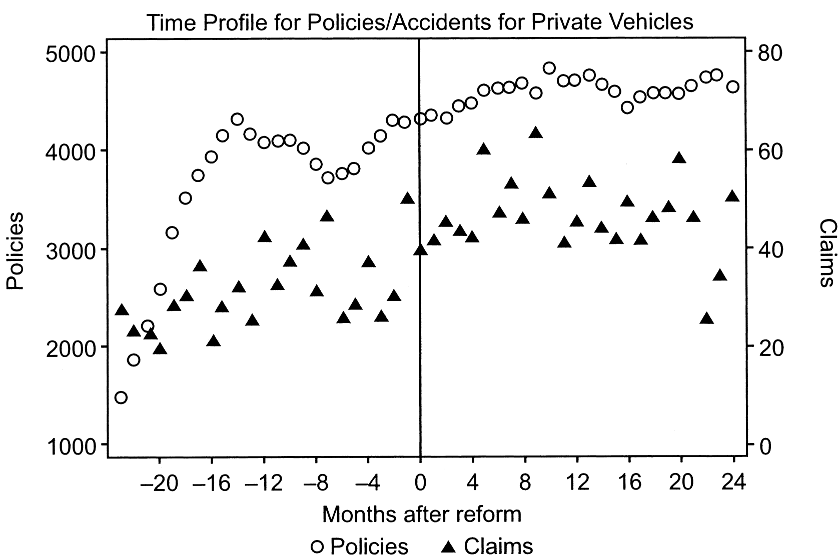

Fig. 8.3 Policies and claims, private vehicles

Note: The figure plots the number of monthly policies sold for private, noncommercial vehicles (left axis) and claims-related incidents (right axis) against time in months since the Michuki reforms of February 2004.

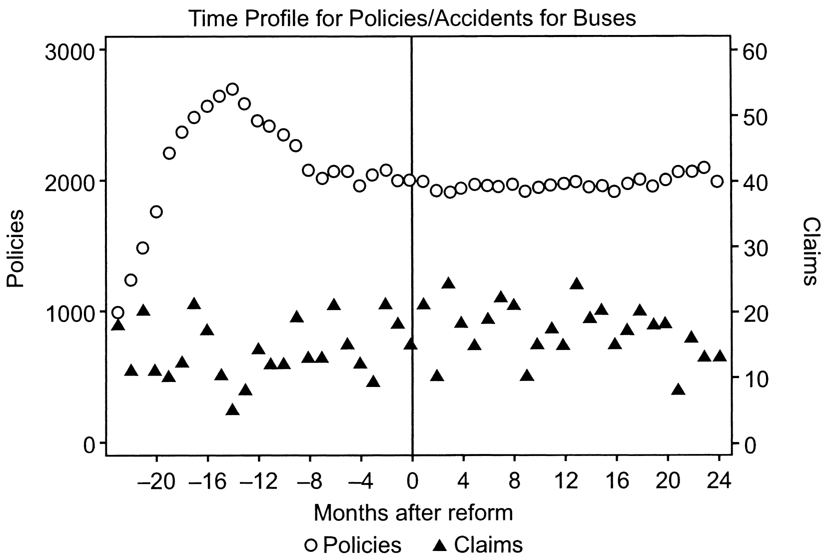

Fig. 8.4 Policies and claims, other buses

Note: The figure plots the number of monthly policies sold for large buses (left axis) and claims-related incidents (right axis) against time in months since the Michuki reforms of February 2004.

$\text{Post}_t = 1$ if $t > 0$ (i.e., after the reform date of February 2004), and zero otherwise. This simple specification assumes that the trend in matatu accident rates would have been the same as the evolution of private vehicle accidents but for the adoption of the Michuki reforms. A negative and significant value of δ would indicate a reduction in claims associated with the new rules.

Specification (1) above can be extended to a multiperiod estimation simply by specifying Post as a set of indicators corresponding to each month of the four-year window around the reforms. Estimating specification (2) below allows one to examine temporal effects of reforms as well as adjust for differential trends.

$$(2) \qquad y_{ijt} = \theta_0 + M_j + \sum_{\tau} \delta_{\tau} M_j D_{t=\tau} + \sum_{\tau} \lambda_{\tau} D_{t=\tau} + \varepsilon_{ijt},$$

where we represent each month as an indicator variable D_t equal to 1 in that particular month t, and 0 otherwise. The set of coefficients δ_t now define the time profile of the impact of the reforms for all time periods after the reform date. This specification allows us to examine the evolution of the reform's impact over time, including any persistence or waning of its effects. As with specification (1), the implicit counterfactual is that accident rates of matatus have the same trend as the private vehicles (defined by the family of parameters λ_{τ}) in the absence of the reform. With multiple periods, we can relax this assumption by including vehicle category specific trends as in specification (3) below.

$$(3) \qquad y_{ijt} = \theta_0 + M_j + \sum_{\tau}\delta_\tau M_j D_{t=\tau} + \phi T + \kappa M_j T + \varepsilon_{ijt},$$

where ϕ and $\phi + \kappa$ define the vehicle category specific linear time trends.[11]

8.3.2 Heckle and Chide: Summary Statistics and Empirical Strategy

We recruited a total of 2,276 long-distance matatus in Nairobi and a number of regional centers, adopting field-based randomization to treatment and control status using the last digit of the vehicle's license plate.[12] Claims data were provided by insurance companies that at the time together covered about 90 percent of matatus in the country. Measurement error could arise due to selective reporting of accidents, although we do not believe this would have been associated with treatment status. (See table 8.2.)

In assessing the impact of the passenger empowerment intervention, we adopt a similar statistical methodology to that employed in evaluating the Michuki rules, this time comparing matatus assigned to treatment and control groups before and after the assignment of stickers. As discussed in Habyarimana and Jack (2011), compliance with the random assignment was high, but not perfect, so we report intent-to-treat estimates and instrumental variable estimates using assignment status as an exogenous instrument. We also allow for different trends in accident rates for treatment and control vehicles before the intervention. Our first specification is the analog of equation (1) above:

$$(4) \qquad y_{it} = \alpha + \beta T_i + \gamma \mathrm{Post}_t + \delta T_i \times \mathrm{Post}_t + \phi X_i + \eta_i + \varepsilon_{it},$$

where T_i is a dummy equal to 1 if vehicle i was assigned to the treatment group, and $\mathrm{Post}_t = 1$ for observations that occur after the intervention.[13] Because we collected survey data on individual matatus and their drivers, we include additional controls, X_i, and a matatu credit cooperative fixed effect η_j.

To allow for differences in trends between treatment and control groups and potential seasonality, we augment equation (4) to the following form:

$$(5) \qquad y_{it} = \alpha + \beta T_i + \gamma \mathrm{Post}_t + \delta T_i \times \mathrm{Post}_t + \phi X_i + \sum_{\tau}(\psi_\tau^0 + \psi_\tau^1 T_i) \times Q_{t=\tau} + \sigma_t + \eta_i + \varepsilon_{it},$$

where Q_t is an indicator for the quarter corresponding to time t.

11. Nonlinear time trends can be estimated by including higher-order terms of the time variable T.

12. Those with odd last digits were assigned to receive the stickers, and those with even last digits were assigned to the control group. Informed consent was received from all participating vehicles.

13. Thus in this specification we collapse all the data into just two periods.

Table 8.2 Selected vehicle and driver characteristics by random assignment

	Control	Treatment	Difference *P*-value
A. Vehicle characteristics			
Odometer reading	356,506	361,386	.612
	(7,236)	(6,350)	
	[327,266]	[343,603]	[.288]
Seating capacity	14.52	14.52	.995
	(0.05)	(0.05)	
Proportion use tout	0.45	0.48	.087
	(0.02)	(0.01)	
Number of weekly trips	20.19	19.60	.211
	(0.36)	(0.30)	
Average daily distance, kilometers	420.48	414.10	.433
	(6.14)	(5.33)	
	[400]	[400]	
Proportion with an installed speed governor	1.00	1.00	.373
	(0.001)	(0.001)	
Share owned by large cooperative (> 300 vehicles)	0.49	0.51	.419
	(0.02)	(0.01)	
Involved in accident in last 12 months, self-reported	0.004	0.015	.008
	(0.002)	(0.004)	
Insurance claim filed in last 12 months before recruitment, (from administrative data)	0.061	0.071	.355
	(.008)	(.007)	
F-stat and *p*-value of joint test of significance of all vehicle characteristics	1.02		.415
Number of observations	*1,006*	*1,155*	
B. Driver characteristics			
Has access to phone[a]	0.96	0.98	.052
	(0.01)	(0.00)	
Owns a phone[a]	0.89	0.91	.135
	(0.01)	(0.01)	
Percent less than 30 years old	18.5	16.2	.612
	(3.4)	(3.0)	
Percent 30–40 years old	54.8	56.1	.831
	(4.3)	(4.1)	
Percent primary schooling	22.8	26.2	.494
	(3.5)	(3.5)	
Percent secondary schooling	13.9	14.7	.842
	(2.8)	(2.8)	
Percent married	74.8	77.0	.665
	(3.7)	(3.5)	
Number of children	2.0	2.0	.918
	(0.1)	(0.1)	
Proportion drivers assigned to one car only	0.72	0.70	.649
	(0.04)	(0.04)	
Proportion drivers started after recruitment	0.37	0.41	.515
	(0.04)	(0.04)	
Median driver tenure, days	296	305.5	.89
F-stat and *p*-value of joint test of significance of all driver characteristics	0.39		0.95
Number of observations	*139*	*145*	

Notes: Standard errors are in parentheses; medians are in brackets. The table presents mean/median of vehicle characteristics by *treatment assignment*. The sample is restricted to matatus for which information on random assignment is available. The 115 matatus that could not be matched to the initial assignment list are dropped.

[a]Statistics reported in these rows are based on the sample of all recruited matatus. The statistics reported in panel B of the table are based on a random sample of 284 matatu drivers who were surveyed six months after recruitment.

8.4 Results

8.4.1 Michuki Rules

The simple difference-in-differences estimate of the impact of the Michuki rules is shown in table 8.3, where the data are aggregated over a period of twenty-four months before the reforms and twenty-four months after. These results suggest that there was no sustained reduction in accidents involving matatus relative to private vehicles over the long term.

This analysis shows there was no discernible impact of the reforms over the long term. While the point estimate (–.003) is negative and economically large, it is statistically indistinguishable from the reform having had no effect on accident rates.

To assess the temporal impacts of the reforms, we estimate equation (3) and illustrate the results in figure 8.5. The horizontal axis measures months before or after the introduction of the reforms, with $t = 0$ representing February 2004. The solid line represents the values of the month-level coefficients δ_t, which can be interpreted as measuring the differential likelihood of an insurance claim by a matatu compared with that of private vehicles, assuming a common trend for $t < 0$. The dashed lines show the 95 percent confidence interval around the estimated effects. There is a significant negative impact of about 2 percentage points on the likelihood of a matatu claim at $t = 0$. For $t > 0$ the estimates are mostly negative, but they are statistically insignificant.

Figure 8.6 illustrates the same information, but allows for differential trends in matatu and private vehicle claims rates before the reform. If anything, these results indicate more emphatically that there was no discernible effect of the reforms. Apart from the impact at $t < 0$, the only significant coefficient occurs more than a year after their implementation, and is positive.

We interpret the sharp fall in claims likelihood during the first month of the reforms as deriving from matatus being pulled off the road in order to be fitted with the required equipment. Since we do not have data on miles traveled or days of active operation, we cannot say for sure if this is the case,

Table 8.3 **The 2 × 2 difference-in-differences estimate of Michuki reforms**

	Before reform	After reform	After-before
Private	0.001	0.001	0.000
	(0.001)	(0.001)	(0.001)
Matatus	0.016	0.013	–0.003
	(0.004)	(0.001)	(0.004)
Total	0.015	0.012	–0.003
	(0.004)	(0.001)	(0.004)

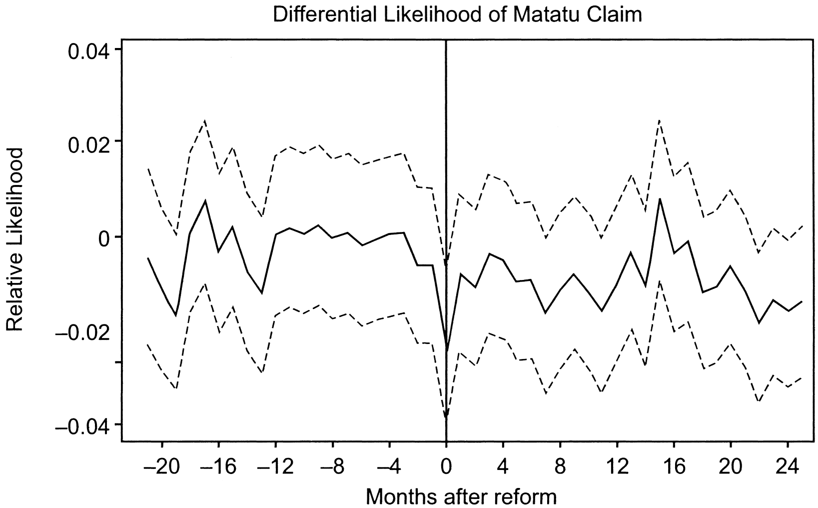

Fig. 8.5 Differential likelihood of claim for matatus vis-á-vis other private vehicles, common time trends

Note: The figure plots the monthXminibus_indicator coefficients from a regression of claims rates on a set of month dummies, a vehicle class indicator (the baseline category is private vehicles), and their interaction. The dashed lines show the upper and lower limits of the 95 percent confidence interval.

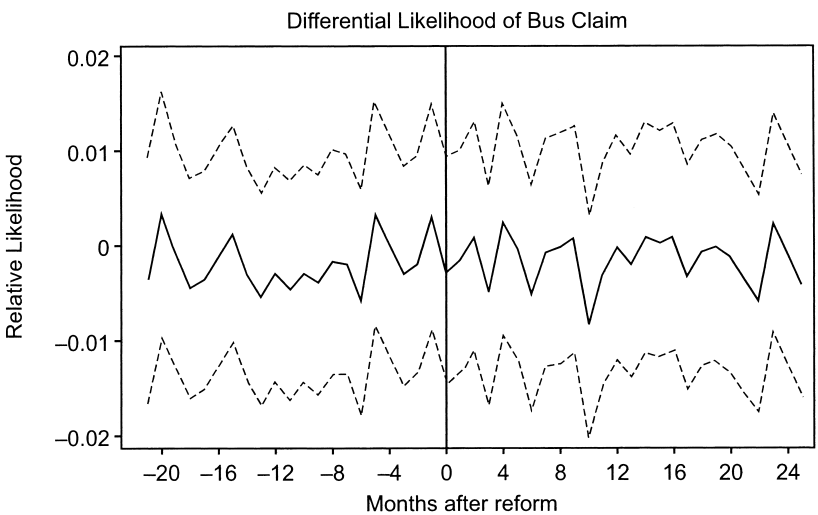

Fig. 8.6 Differential likelihood of claim for matatus vis-á-vis other private vehicles, class-specific time trends

Note: The figure plots the monthXminibus_indicator coefficients from a regression of claims rates on a set of month dummies, a vehicle class indicator (the baseline is private vehicles), and their interaction. The dashed lines show the upper and lower limits of the 95 percent confidence interval.

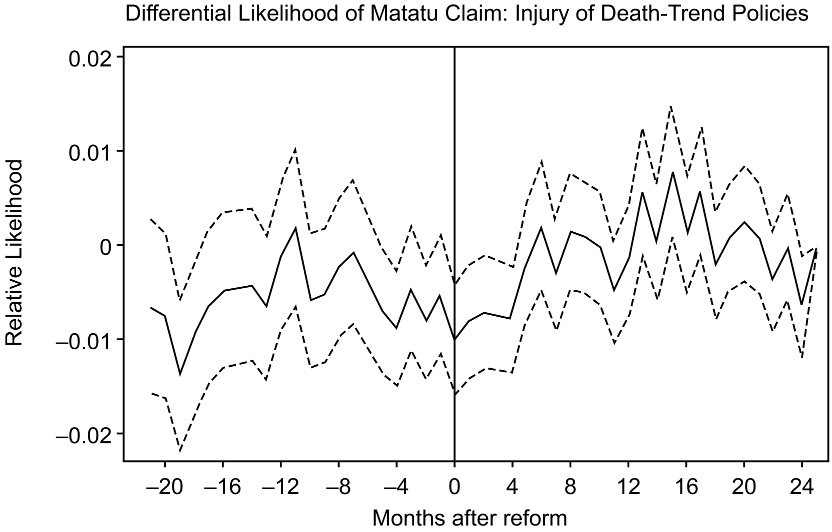

Fig. 8.7 Differential likelihood of claim involving an injury or death for matatus vis-á-vis other private vehicles, class-specific time trends

Note: The figure plots the monthXminibus_indicator coefficients from a regression of injury or death-related claims rates on a set of month dummies, a vehicle class indicator (the baseline is private vehicles), and their interaction. The dashed lines show the upper and lower limits of the 95 percent confidence interval.

but the anecdotal evidence reported above is consistent with a large reduction in the volume of matatu traffic in that period.

We also report the differential likelihood of matatus, compared with private vehicles, making claims that involve an injury or death, as shown in figure 8.7 (again, allowing for differential trends). Although there is a negative coefficient at $t = 0$ once again, and while it is sustained until four months after the reform, there appears to have been an (imprecise) negative differential for some months leading up to February 2004 as well. This suggests that other factors could have been at work to reduce serious matatu accidents leading up to the reforms, although we have no specific evidence of such. From $t = 5$ onward, the differential likelihood of a claim is either zero or positive.

Our results suggest that the Michuki rules had little if any effect on the safety of matatu travel in Kenya. Compliance with the new regulations disrupted transportation services in the early days of implementation considerably, but appears not to have reduced the likelihood that any operational vehicle would have an accident leading to the submission of an insurance claim.

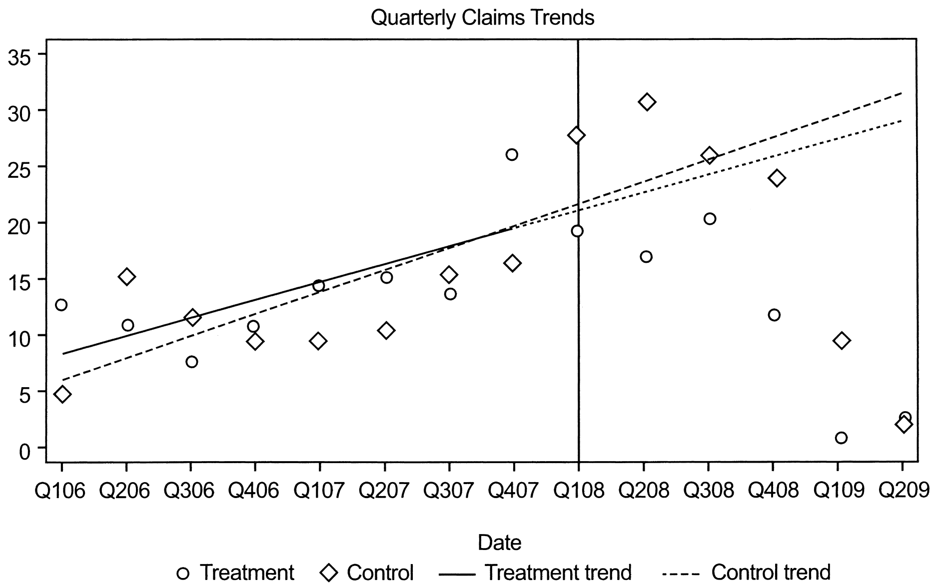

Fig. 8.8 Quarterly claims data, treatment and control matatus

Note: The figure presents the number of insurance claims by quarter between January 1, 2006, and May 25, 2009. *All* insurance claims are used to construct this figure. Solid and dashed lines represent fitted linear trends for the treatment and control group. We fit a linear trend to all claims for the pretreatment period for the treatment group (all claims from 2006–2007). The dotted line traces out counterfactual claims for the treatment group. For the control group, we fit a linear trend to all claims from 2006–2008, excluding claims from quarters 1 and 2 of 2009 due to incompleteness. We make the simplifying assumptions that matatus continue to operate after a claim event and were operating throughout this period.

8.4.2 Heckle and Chide

The effects of the consumer empowerment intervention are both more reliably estimated, due to the randomized assignment of treatment, and demonstrably larger than the impact of the regulatory reform. Figure 8.8 (from Habyarimana and Jack 2011) reports quarterly data by random assignment from the first quarter of 2006 to the second quarter of 2009 (the intervention was implemented in Q1, 2008). Each point in the graph represents the number of claims per 1,000 insured matatus, and the two lines are trend lines (estimated using preintervention data for the treatment group, and data through 2008 for the control). The data were collected in 2009, but the figures reported for that year are incomplete, given the considerable lag between a claims-related event and the digital recording of the associated claim.

Prior to the intervention, claims rates exhibited an upward trend, and showed no discernible differences between vehicles assigned to the control

Table 8.4 Regression results

	All claims		Driver-at-fault claims		Injury/death claims	
	(1)	(2)	(3)	(4)	(5)	(6)
		A Intent-to-treat				
Post: γ	0.029	0.030	0.025	0.026	0.018	0.018
	(0.013)*	(0.012)*	(0.011)*	(0.011)*	(0.009)+	(0.009)*
Assigned to treatment: β	0.010	0.009	0.011	0.011	0.011	0.011
	(0.010)	(0.011)	(0.010)	(0.010)	(0.008)	(0.008)
PostXAssigned to treatment	–0.050	–0.051	–0.046	–0.047	–0.040	–0.041
	(0.016)**	(0.016)**	(0.014)**	(0.014)**	(0.012)**	(0.012)**
Constant: α	0.061	0.042	0.052	0.039	0.038	0.036
	(0.008)**	(0.013)**	(0.007)**	(0.012)**	(0.006)**	(0.010)**
Percentage effect:	–50	–63	–52	–62	–60	–63
		B. IV estimates				
Effect of treatment on the treated	–0.073	–0.075	–0.068	–0.069	–0.059	–0.060
	(0.023)**	(0.023)**	(0.021)**	(0.021)**	(0.017)**	(0.017)**
Percentage effect:	–73	–93	–77	–91	–88	–92
Controls for SACCO		X		X		X
Observations	4,322	4,318	4,322	4,318	4,322	4,318
R-squared	0.003	0.02	0.002	0.01	0.002	0.01
Mean post recruitment claims rate for vehicles assigned to control group	0.09		0.077		0.055	
First stage: F-stat	2,421.33	2,364.44				

Notes: Table reports the estimates of ordinary least squares regression in specifications (1–4) and instrumental variables estimates in specifications (5–6). The dependent variable is the annualized rate of a claim-generating accident for each matatu in the sample. We make a simplifying assumption that matatus continue to operate after a claim event and were operating throughout the pre- and postrecruitment period. First-stage F-stat reports the F-stat of the test of the null that random assignment to treatment does not predict actual treatment status at recruitment. The sample excludes 3 percent of recruited vehicles for which treatment assignment information could not be reliably established. Robust standard errors in parentheses.

**Significant at the 1 percent level.

*Significant at the 5 percent level.

and treatment groups.[14] However after the intervention, claims rates were consistently lower for vehicles assigned to the treatment group.

Panel A of table 8.4 reports intent-to-treat (ITT) estimates for equation (3), collapsing all the data into two periods—before and after the intervention.[15] In the first two columns, the probability of any claim falls by between 50 and 63 percent (from projected annual rates of 10 and 8.1 percent, respectively). Similar reductions are observed for claims in which the matatu driver was at fault (columns [3] and [4]), and for claims involving an injury or death (columns [5] and [6]).

14. A similar upward trend was underway for larger thirty to forty-one-seater buses (see Habyarimana and Jack 2011), suggesting a secular trend.

15. Columns (2), (4), and (6) include SACCO fixed effects. Most long-distance matatus are organized in SACCOs, Savings and Credit Cooperatives, of which there were twenty-one in our study.

Panel B reports instrumental variable results, where we use assignment as an instrument for treatment status. The estimates of the impact of the treatment on the treated, or local average treatment effect, are effectively scaled by the inverse of the compliance rate, and are correspondingly larger than the ITT estimates. According to these estimates, among those vehicles that were induced to accept stickers by being assigned to the treatment group, claims were close to eliminated, falling by between 73 and 93 percent.

Similar results are obtained using the specification with more temporal structure in equation (5), and can be found in Habyarimana and Jack (2011).

8.5 Evidence on Mechanisms of Change Underlying Heckle and Chide

Although the consumer empowerment intervention appears to have sizable impacts on accidents and insurance claims, exactly what mechanisms underlie these large effects is unclear. One concern is that drivers of treated matatus misunderstood the lottery as being a prize for safe driving, and that they responded to the perceived financial incentive. Since the design did not include a placebo intervention, it is impossible to know whether this is the case. And even if we are confident that the lottery did not drive our results, we do not know if the stickers worked by inducing passenger complaints, or if they affected the driver directly (he was aware of them, even if they were not in view as he drove).

In this section we present some suggestive evidence for potential mechanisms that underlie the reduction in accident rates estimated above. Although we do not have the data to definitively discriminate among all plausible mechanisms that could underlie our results, we nonetheless present two pieces of evidence in support of passenger-action mechanisms, and discuss the plausibility of a number of other mechanisms including direct effects on drivers, ex post sorting of drivers, and the effects of the lottery.

8.5.1 Survey Evidence

The obvious mechanism by which the intervention leads to improved safety is that the stickers empower passengers to voice their concerns over bad driving and that the resulting social pressure changes the behavior of the driver. To investigate this we analyze data from a survey fielded in November 2008 of drivers, plus up to three passengers per vehicle. A total of 284 vehicles were sampled for this survey.[16]

We face two difficulties in detecting evidence for this mechanism. First, even if the stickers are effective in empowering passengers, we might observe little or no difference in heckling if drivers of treatment vehicles quickly learn to adapt their behavior to minimize passenger complaints. On the

16. We interviewed 306 drivers, but twenty-two of them were operating vehicles that had not been recruited earlier.

Table 8.5 **Sticker retention**

Number of stickers in vehicle	Distribution at recruitment (%) (1)	Distribution in November 2008 (%) (2)
0	46.5	63.0
1	2.1	4.9
2	2.8	4.2
3	4.2	7.4
4	0.3	2.5
5	44.0	18.0
Total	100.0	100.0

Note: Table reports the distribution of stickers for the random sample of matatus surveyed eight months after recruitment. Column (1) reports the distribution at recruitment while column (2) reports the distribution eight months after recruitment.

other hand, whether heckling is observed in equilibrium or not, we might expect passengers to report their trips as being safer in treatment matatus. Second, given the rarity of traffic accidents, events that generate heckling will also be rare. Compounding this power problem is the fact that, despite the weekly lottery, after eight months a considerable number of the treatment vehicles had lost some or all of their stickers. Table 8.5 shows that among our sample of 284 matatus the share with all five stickers had fallen from 44 percent at recruitment to 18 percent eight months later, and the share with at least one sticker had fallen from 53 percent to 37 percent.

Table 8.6 presents evidence of heckling from the survey of drivers (panel A) and passengers (panel B) and passenger-reported safety ratings (panel C). We present intent-to-treat estimates for all outcome measures. Note that this considerably limits our ability to find any evidence for this mechanism as a result of low sticker retention.

The results are suggestive of passenger heckling as one of several potential contributors to the reduction in accident rates. In rows 1 and 2 of panel A, we estimate the effect of assignment on the likelihood that the driver reports passenger heckling in the past week and most recent trip. The point estimate in row 1 has the right sign, but is imprecisely estimated. The sign of the coefficient in row 2 is wrong, but again imprecise. However, in OLS results not reported here, we find substantial and marginally significant effects of having a sticker eight months into the study. In particular, drivers of vehicles with stickers at the time of the survey were about three times more likely to report passenger heckling.[17]

17. In a simple OLS estimation of the effect of stickers on heckling, nonrandom removal or depreciation of stickers could bias our results. On the one hand, dangerous drivers might have removed them, either in advance or in response to unwelcome heckling as they learned about their effectiveness over time. This would work against finding evidence of passenger action in treated vehicles. On the other hand, if the stickers provided drivers who otherwise lacked self-

Table 8.6 Evidence on passenger action mechanisms

Dependent variable	Assigned to treatment	Unsafe trip	Assigned to treatments * unsafe Trip	Number of observations
A. Driver reports of heckling				
(1) Driver reports heckling (past week)	0.027	—	—	259
	(0.034)			
(2) Driver reports heckling (last trip)	–0.027	—	—	259
	(0.027)			
B. Passenger reports (most recent trip)				
(1) Any passenger expressed concern	–0.005	0.172	–0.022	788
	(0.088)	(0.070)*	(0.097)	
(2) Respondent expressed concern	0.014	0.084	–0.043	788
	(0.071)	(0.064)	(0.078)	
(3) At least two respondents expressed concern	0.092	0.300	–0.092	260
	(0.130)	(0.101)**	(0.145)	
(4) All three respondents expressed concern	–0.058	0.081	0.031	260
	(0.079)	(0.077)	(0.093)	
C. Passenger perceptions of safety (most recent trip)				
(1) Safety rating	–0.007	—	—	788
	(0.078)			

Notes: Panel A reports the results of a linear probability model on the likelihood of drivers reporting heckling in the past week and on the most recent trip. Panel B reports the results of an OLS regression of the likelihood of passengers reporting expressions of concern to driver/conductor on *treatment assignment status*, safety rating, and the interaction of the two variables. A sample of up to three passengers exiting each matatu surveyed eight months after recruitment is used to construct these estimates. Passengers from twenty-two matatus that could not be matched to the assignment lists are dropped, leaving a total of 788 passengers (see below on the coding of unsafe). Panel C reports the results of an ordered probit model on passenger perceptions of safety. Passengers were asked to rate the safety of the just-completed trip on a scale from 1 to 10, where 1 implies no danger, 10 implies high likelihood of serious injury/death, and 55 corresponds to "cannot say." A trip is considered safe or unsafe if at least one respondent reports a safety rating of 6 or higher. We recode this variable as follows: 1 = Safe (a rating 1–5), 2 = Cannot Say (55), and 3 = Dangerous (a rating 6–10). Robust standard errors are in parentheses.

**Significant at the 1 percent level.

*Significant at the 5 percent level.

We next turn to self-reported evidence of passenger action in panel B of table 8.6. Sampled passengers were asked to report if they or any other passengers had said something to the driver/conductor about reckless driving behavior on the just-concluded trip. In order to avoid conflating potentially frivolous actions with legitimate heckling, we control for the reported safety of the trip. In particular, passengers were asked to rank the safety of the

control with an effective enforcement technology, removal could be concentrated in the pool of relatively safe drivers, who simply find them distasteful and perhaps bad for business. This would bias our results in favor of finding an effect. Although we cannot distinguish econometrically between these two directions of bias, we believe the former is more plausible and highly likely to dominate the latter.

trip on a scale of 1 to 10, with 1 denoting no danger and 10 denoting life-threatening danger. While nearly 45 percent of the respondents reported that they "could not say," we define a trip to be reported as safe if the passenger reports a rating equal to or less than 5. For our current purposes we create an indicator for whether at least one passenger had rated the safety of the trip as dangerous (a rating of 6 or higher). Evidence for the passenger heckling mechanism is then captured by the extent to which there is a greater likelihood of heckling on trips deemed dangerous by at least one passenger. We present ITT estimates for four different outcomes that correspond to the rows in panel B of table 8.6: likelihood of heckling by (a) the respondent, (b) any passenger, (c) at least two respondents, and (d) all respondents. The latter two outcomes represent a crude measure of the extent to which the intervention facilitates collective passenger action and the unit of observation is the vehicle.[18] The coefficient of interest is the interaction of the indicator for stickers and whether at least one passenger rated the trip as unsafe.

Our estimates for this parameter are of the wrong sign in rows 1–3, but in all cases are very imprecise. In row 4, that estimates the likelihood that all correspondents heckle the driver, we obtain the right sign but once again the coefficient is statistically insignificant.

One way in which this mechanism could operate is by making passenger heckling a credible threat to reckless driving. In the absence of more objective measures of driving behavior, we rely on passenger ratings of safety of the just-concluded trip. Our results in panel C report the results of an ordered probit estimation across three safety ratings categories (safe trip, cannot say, unsafe). About two-thirds of all passengers in the control matatus rated the most recent trip as safe according to this definition. The ordered probit estimate in panel C has the right sign, but is very imprecisely estimated.

While the evidence above suggests that passenger action may well lie at the heart of the observed effects, we cannot definitively rule out a number of other potential mechanisms. For instance, while passenger ratings of safety do not confirm this (see panel C of table 8.3), it is possible that a driver's beliefs regarding the preferences of the vehicle's owner, over either passenger safety or the life of the vehicle, could be affected by this intervention. More direct observations of driver behavior might shed more light on the plausibility of this mechanism.

8.5.2 Driver Sorting

Alternatively, although the ex ante assignment of stickers to drivers was random, the ex post assignment may have exhibited sorting. That is, it is possible that rather than stickers having altered the behind-the-wheel behavior of drivers, either directly or via passenger action, they induced sorting of

18. For two or more respondent reports of heckling we are unable to condition on the same dangerous event.

drivers across treatment and control matatus. For example, suppose reckless drivers in treated vehicles tended to switch to control vehicles, or to exit this labor market entirely, while safe drivers in control vehicles on average moved to treated matatus. Such sorting behavior could have led to the observed changes in claims rates, but would not have been associated with any change in driving practices per se. We present three pieces of evidence suggesting that this kind of ex post sorting does not constitute a likely explanation of the results.

First, the share of treatment vehicles within each matatu cooperative (SACCO) is about half, so sorting within SACCOs is definitely feasible. However, the authority to hire and fire drivers rests not with the SACCO, but with the owners of the vehicles. But since matatu ownership is very diffuse, sorting within an individual owner's fleet (which can be as small as one or two vehicles) is unlikely to generate our measured effects. And given the costs of sorting out-of-treatment vehicles, it would be much easier for the drivers to remove the stickers than to find an eligible and willing partner with whom to switch.

Second, it is possible that this sorting operates more on the participation margin, if reckless drivers tend to quit the treatment group. Data on driver tenure suggests that the median tenure is about ten months and that while overall turnover since recruitment has been high (an average of 39 percent), there is no statistically significant difference in turnover rates across treatment and control vehicles (41 vs. 37 percent). This holds true among the drivers assigned to a single vehicle.

And third, selective sorting could take place within just those SACCOs that have a policy of regularly rotating drivers across vehicles, as long as such rotation was nonrandom. However, our results could be driven by selective sorting among the relatively small group of drivers in such SACCOs only if there was a high concentration of claims among "reckless" drivers. The insurance claims data from the period before our intervention do not support this pattern. Although the identity of the driver is not recorded in the data, we do know that before our intervention, fewer than 8 percent of all claims were associated with multiple-claim vehicles (and possibly drivers). Overall, these three pieces of evidence suggest that while we cannot rule out driver sorting as a response to the intervention, the scale at which such sorting could be occurring cannot explain the results obtained above.

8.5.3 Direct Effects of the Lottery

Finally, we discuss the possibility that the presence of the lottery, designed to improve sticker retention, could itself lead to our empirical results. Recall that drivers who accepted all five stickers at recruitment were divided into five groups of roughly 200 vehicles, and that each week on a five-week rotating basis, members of one of the groups were eligible to win one of three prizes if, when randomly drawn, upon inspection they were found to have retained all five stickers. The total prize money each week of 10,000 shillings (about two weeks' wages) was awarded in three amounts (5,000, 3,000, and 2,000) to three different winners.

The lottery itself could have changed the beliefs of drivers of treatment vehicles about the likelihood and consequences of an accident. Alternatively, while the rules of the lottery were very explicit, and drivers were told that eligibility was based on sticker retention and not an accident-free record, it is still conceivable that drivers with stickers might have misconstrued the lottery as a reward for safe driving. The policy implications of such findings would, of course, be radically different to those that would otherwise be drawn.

On the first point, knowledge of the lottery and its association with the road safety project were not confined to treatment vehicles alone or lottery nominees. Inspection of stickers was done at parking lots where control and treatment drivers interacted quite frequently, and where awareness of the role of the sticker inspector was clear to both. As a result, we believe that any small differences in road safety salience attributable to the lottery across the two groups is unlikely to explain the large effect measured above.

On the second point, which is potentially of greater concern, the payment is likely to have been too small to alter driving behavior. Expected winnings were very low (equal to wages equivalent to about twenty minutes work), and even if drivers had unreasonable priors of winning, the first prize was considerably less than what a driver could make by squeezing in one extra trip (unreported to owner) per month.

Nonetheless, to address this second issue more quantitatively, we investigate the beliefs that drivers would have had to maintain in order that the observed reduction in claims rates could be rationalized in terms of a response to the misguided belief that safe driving would increase the chance of winning the lottery. This kind of exercise is, of course, laden with assumptions and can only inform the analysis if the results suggest wildly counterfactual driver beliefs. In fact, we find that such extreme beliefs, plus an impossibly high response of accidents to speed reductions, are indeed necessary to support the claim that the lottery was the driving force behind the impact we observe.

The key parameter in this exercise is the elasticity of accidents with respect to speed, estimates of which are not available in Kenya or other developing countries to our knowledge. Ashenfelter and Greenstone (2004) report data for the United States suggesting an elasticity of fatalities of about four, which provides a benchmark against which to compare our data.[19]

As we illustrate below,[20] even if a driver (a) thought he would win the

19. The approximately equal estimated proportional impacts of the intervention on all claims, claims in which the driver was at fault, and claims involving an injury or death suggest that this fatality elasticity is a good proxy for the elasticity of all accidents.

20. Each week three prizes totaling 10,000 shillings were awarded. We assume driver risk neutrality and denote the size of the average weekly prize by $x = 10{,}000/3$. The probability the driver assesses to winning a prize, conditional on not having had an accident, is denoted p, and

lottery with certainty (instead of with average weekly probability 0.003), (b) was sure of reducing his chance of an accident to zero, and (c) thought that there was a single prize of 10,000 shillings every week, the elasticity of accidents with respect to speed would still need to be more than thirty times larger that the US estimate for the expected financial benefit of slowing down to outweigh the expected costs. In light of the evidence, recently reviewed by Delavande, Gine, and McKenzie (2009), that people in developing countries generally understand the concept of probability, we believe this calculation, while clearly subject to wide margins o]f error, nonetheless strongly suggests the lottery itself did not affect driver behavior enough to account for any meaningful share of the estimated effects of the intervention.

In ongoing work we attempt to explicitly address the concerns about mechanisms voiced above. In particular, in a new study of more than 10,000 matatus we include a placebo arm in which vehicles are assigned stickers that say simply "Travel well," while remaining eligible for the lottery. In addition, we send enumerators on up to 7,000 trips, during which they monitor driving behavior and passenger responses directly.

8.5 Conclusions

We present evidence that tough government regulations were unsuccessful in inducing sustained changes in accident rates of minibuses in Kenya,

the expected winning each week are px. Let w be the driver's weekly wage, and denote $z = x/w$ as the ratio of the average prize to the wage. In order to reduce the chance of being involved in an accident, thereby increasing his chance of winning, the (misinformed) driver slows down. We want to compare the expected increase in winnings to the cost this would impose on him.

Let π_0 be the weekly probability of having an accident under the assumption of no behavior change. (The projected counterfactual annual accident rate among treated matatus during the year following the intervention was approximately 10 percent, so $\pi_0 = 0.1/52$.) Drivers in the treatment group experienced a claims rate about half the projected rate. Assuming for the sake or simplicity a constant proportional reduction over the year, their actual weekly probability of having an accident was $\pi_1 = \pi_0/2$, which is also the change in the probability, $\Delta\pi$. Engaging in this behavior change increases expected weekly winnings by $B = \Delta\pi px = \Delta\pi pzw$.

The expected cost per week of slowing down is the wage times the extra time taken, $w\Delta t$, which is approximately equal to $C = w(\Delta s/s)$, where s is the average speed of the vehicle. Define the elasticity of accidents, a, with respect to speed, s, by $\varepsilon = [(\Delta a/a)]/[(\Delta s/s)]$. Although the relationship between speed and accident rates in Kenya is not known, Ashenfelter and Greenstone (2004) present fatality and speed data from the United States that suggest an elasticity of fatalities with respect to speed of about 4. (In their data, a 4.55 percent reduction in speed is associated with a 15.46 percent reduction in fatalities.) Thus the cost incurred by the driver in reducing accidents by this much is approximately $C = w(\alpha\varepsilon)$ where $\alpha = \Delta a/a \approx 1/2$.. This cost is less than the expected benefit $1 < \alpha\varepsilon\Delta\pi pz$.

Using our data, the right-hand side of this expression is approximately $2 \times 4 \times (((0.05)/(52))) \times ((3/(1{,}000))) \times ((2/3)) = 1/65{,}000$. That is, for a driver to respond only to the incentive of a lottery whose eligibility criteria he misinterpreted, and not to the stickers or the response they evoked on the part of passengers, he would need to overestimate the right-hand side of the inequality condition above by a factor of 65,000. Even if he thought he would win the lottery with certainty ($p = 1$), was sure of reducing his chance of an accident to zero ($\Delta p = 0.1/52$), and thought that there was a single prize of 10,000 shillings ($z = 2$), the elasticity of accidents with respect to speed would still need to be 32.5 times larger that the US estimate for the condition above to be satisfied.

despite strong political leadership and dedicated resources. On the other hand, an intervention that relied on no third-party enforcement, and whose implementation was in fact unknown to and unsupported by the government or the police, appears to have been remarkably successful in bringing down accidents rates, by at least one-half.

We do not have enough information to explain why the Michuki reforms had little effect. Although there was an initial dip in the number of insurance claims involving vehicles subject to the regulations, we argue that most of this was due to the need to comply with new hardware requirements, which took a large number of minibuses off the road. Once the necessary vehicle modifications had been made, it appears that claims rates returned quickly to their prereform levels.

It would be unhelpful to claim that all regulation is doomed to failure. Instead, our analysis suggests that in institutionally weak environments, innovative consumer-driven solutions might provide an alternative solution to low-quality service provision.

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