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Paging Inspector Sands: The Costs of Public Information†

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We exploit the introduction of pedestrian countdown signals—timers that indicate when traffic lights will change—to evaluate a policy that improves the information of all market participants. We find that although countdown signals reduce the number of pedestrians struck by automobiles, they increase the number of collisions between automobiles. They also cause more collisions overall, implying that welfare gains can be attained by hiding the information from drivers. Whereas most empirical studies on the role of information in markets suggest that asymmetric information reduces welfare, we conclude that asymmetric information can, in fact, improve it. (*JEL* D82, D83, R41)

Few know who Inspector Sands is, and no one has ever met him. This is for good reason. Theater companies in the United Kingdom are believed to use the code name "Inspector Sands" in order to alert ushers to pending emergencies, such as fires and bomb threats, without inciting panic among their patrons.[1] When theater staff learn of a fire, for example, they page Inspector Sands to the fire's location. When ushers arrive they can put out the fire or help to evacuate the premises in a discrete and orderly manner. By ensuring the threat remains hidden from the public eye, the code name allows ushers to complete the tasks without having to deal with panicked crowds. While paging Inspector Sands is the sensible course of action in a crowded theater, there are situations where shouting "fire" is the more sensible thing to do.[2] When the theater has few patrons, for example, shouting fire likely ensures the patrons escape the emergency unscathed. The policy choices—paging Inspector

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[1] Many believe the code name is still used by public transit authorities in the United Kingdom. An Internet search of the phrase "Inspector Sands" yields, for example, several reports of Inspector Sands being paged in the subways of London.

[2] Shouting "fire," even in the case of a false alarm, can have disastrous consequences for public safety. In several instances, false alarms alone have caused large-scale injury and death. In one recent example, 168 people died after visitors to a Hindu Temple in India became hysterical upon learning of a bomb threat in the area (See "Bomb Threat Rumors Blamed for India Stampede That Killed 168," Fox News, October 1, 2008, accessed December 7, 2013, http://www.foxnews.com/story/2008/10/01/bomb-threat-rumors-blamed-for-india-stampede-that-killed-168/).

Sands or shouting fire—represent the extremes of what policymakers can do when they have private information about the state of the world. The dilemma for the policymaker is in determining when one policy is more sensible than the other.

The resolution to the dilemma hinges on identifying the potential for negative externalities to outweigh individual gains from having better information. Shouting fire improves welfare because it allows individuals to make better decisions from the perspective of their own self interest. In a crowded theater, patrons can run for the exits. Shouting fire reduces welfare, on the other hand, because it may induce individuals to physically harm one another. When the individual gains are expected to outweigh the negative externalities, full disclosure is the smart policy. When negative externalities are expected to dominate, withholding information is a better idea.[3]

In this paper, we draw on a large-scale natural experiment to study the effects of shouting fire or, in other words, the effects of providing the public with information about the state of the world. Specifically, we evaluate the effects of pedestrian countdown signals—timers that indicate when traffic lights will change—on the behavior and safety of road users. Although the timers were originally intended for pedestrian use only, they are visible to all who transit an intersection. The timers therefore increase the information that each user of the intersection has about the time until a light change. In turn, we exploit the setting to quantify the overall effects of public information as well as to address questions about whether and how policymakers should reveal their private information to the public.

In addition to providing a context in which to study the role of public information, the introduction of pedestrian countdown signals is itself an issue of significant policy relevance. The growing prevalence of pedestrian countdown signals in major cities worldwide is consistent with a belief in their ability to improve public safety.[4] As pedestrian countdown signals become more commonplace at intersections of major cities in the United States and the rest of the world, it is important to have a clear understanding of their impact on public safety.

There is an analogy between the situation we consider here and the ones typically faced by policymakers. In a typical case, agents prefer the right of way to access a resource. They can pursue it aggressively, in order to bypass others and be the first to obtain the resource. Or they can pursue it passively, waiting and taking the risk of not getting the resource in time. The worst possible outcome has all individuals aggressively pursuing the right of way. This is especially true for vulnerable (or physically weaker) individuals, as they are unlikely to obtain the resource when everyone pursues it aggressively. In the case of a crowded theater on fire, the

[3] This dilemma also arises in settings that are commonly studied by economists. For instance, Stiglitz (2002) makes an analogy between announcing a fire in a crowded theater and bank runs. He identifies an IMF announcement about the closure of several banks, where the IMF did not announce which banks were closing, as a cause of the run on banks that led to the 1997–1998 Indonesian banking crisis. While the IMF announcement gave people a chance the withdraw their funds before the closures, it also increased the chance that everyone would try to do so at the same time. Because banks only keep some of their deposits on reserve, some people were left standing in line when the banks ran out of money.

[4] Major cities that have adopted pedestrian countdown signals include New York, London, Toronto, Chicago, Boston, Los Angeles, San Francisco, Bangkok, Singapore, Mexico City, Tokyo, Seoul, Mumbai, New Delhi, Paris, and Shanghai. Moreover, the US Department of Transportation estimates that, among the 33,000 fatalities caused by motor vehicle crashes in 2010, more than 20 percent happened at intersections (for more details, see http://www-nrd.nhtsa.dot.gov/Pubs/811552.pdf).

resource is safe passage through the exits. If individuals yield the right of way, they will not escape immediately (there is some risk of being in the theater too long) but they avoid a stampede to the exit. The vulnerable individuals in this case are the elderly, disabled, women, and children, while the less vulnerable are young adult males.[5] In our setting, the resource is safe passage through the intersection. If individuals yield the right of way, they will not transit the intersection immediately and may have to sit through a red light, but they avoid a collision. Drivers are less vulnerable than pedestrians and cyclists, as they are protected by the car they drive in.[6]

Our venue for assessing the impact of pedestrian countdown signals is the city of Toronto. The venue has several features that are particularly useful for the present study. The first is that decisions about where and when to install countdowns were based on cost considerations rather than the collision history of each intersection. As a result, the installations provide exogenous variation for identifying the effects on the behavior and safety of road users. The second is that the installations were gradual and eventually covered every eligible intersection in the entire city. This allows us to compare nearby intersections with and without a countdown at the same time. That countdown signals eventually covered the entire city lessens concerns that intersections with countdowns are, in some inadvertent and unseen way, different from ones without. The third is that the decision to adopt pedestrian countdown signals was unrelated to the collision history of the city as a whole. The decision to adopt the signals was incidental to a citywide initiative to refit existing streetlights with more energy-efficient lamps. The city decided that including countdown timers at this stage was less expensive than installing them at a later date. Because there was nothing specific about the collision history of Toronto that led to the adoption, our conclusions should apply to other settings where policymakers are deciding whether they should share information with the public.

Our empirical analysis reveals that countdown signals resulted in about a 5 percent increase in collisions per month at the average intersection. The effect corresponds to approximately 21.5 more collisions citywide per month. The data also reveals starkly different effects for collisions involving pedestrians and those involving automobiles only. Specifically, although they reduce the number of pedestrians struck by automobiles, countdowns increase the number of collisions between automobiles. That the total number of collisions increased while collisions involving pedestrians decreased suggests that pedestrian countdown signals had a very

[5] Another setting where this type of problem arises is one where a government agency is aware of an outbreak of infectious disease. In this case, the scarce resource individuals pursue is vaccination. If individuals yield the right of way, they wait in line for the vaccination and risk being too late (if the vaccine runs out, for example), but they avoid a fight for the vaccine. In addition, there are individuals who are more susceptible to contracting the disease, or are at higher risk of serious illness conditional on contracting it. These individuals may not receive the vaccination in time unless the less vulnerable yield right of way to them. In order to limit the harmful consequences for public health the agency can issue a public warning through all available media. While the warning prevents further spread of the disease because it allows people to take precautions that reduce their exposure, it increases the chances that individuals pursue the vaccination aggressively.

[6] While individuals have similar decision problems in the cases of busy intersections and crowded theaters, there are differences in the disclosure options policymakers can choose from. In a crowded theater full disclosure is not an option unless the authority can, at the same time, regulate interactions among patrons and, in particular, the order in which they exit. In these regards, our study speaks to optimal disclosure policies when there are few restrictions on options available to policymakers.

significant effect on driver behavior. In fact, we find that collisions rose largely because of an increase in tailgating among drivers, a finding that implies drivers who know exactly when traffic lights will change behave more aggressively.

To assess the welfare implications of countdown signals, we consider the effects on various types of injuries, various types of collisions, and on the number of pedestrians and cars who transit through intersections. We find that although countdowns reduced the number of minor injuries among pedestrians, they increased the number of rear ends among cars. We show that the number of pedestrians who transit intersections with countdowns is the same as or more than the number who transit ones without. We also show that the number of cars who transit intersections with countdowns is the same as or less than the number who transit ones without. Altogether, the findings imply that fewer pedestrians were injured or struck by automobiles *for every pedestrian on the road* and that there were more collisions and rear ends *for every car on the road*. We conclude that welfare gains can be attained by disseminating information to pedestrians and hiding it from drivers, perhaps by announcing the countdown through a speaker that pedestrians can hear but approaching drivers can not.

I. Related Literature

The present study contributes to the empirical literature on the role of information in markets. Most existing studies analyze the effect of policies that increase the information that participants on one side of a market have about participants on the other side (Ippolito and Mathios 1990; Dranove et al. 2003; and Jin and Leslie 2003).[7] We instead focus on the impact of a policy which increases the information that participants on all sides have about an event that is in their common interest.[8] In these regards, our finding that countdowns increase collisions between drivers complements those of Dranove et al. (2003), who also show that information can reduce welfare. Dranove et al. (2003) considers the effects of publicly disclosing the patient health outcomes—through, e.g., cardiac surgery report cards—of physicians and hospitals. They find that disclosure worsens outcomes for at-risk patients, because it induces physicians and hospitals to selectively choose the patients they treat. While their paper considers the adverse effects of disclosure on *who* agents interact with, we explore the adverse effects of disclosure on *how* agents interact with each other.[9]

Our finding that information benefited pedestrians at the expense of drivers speaks to questions about the role of transparency in public policy. Specifically, we provide an empirical contribution to the philosophical debate over whether governments with private information should share it with the public.[10] While the debate focuses

[7] For papers that study the effect of these policies on consumer choice, see Beaulieu (2002); Wedig and Tai-Seale (2002); Jin and Sorensen (2006); Dafny and Dranove (2008); Dranove and Sfekas (2008); Hastings and Weinstein (2008); Bundorf et al. (2009); and Dellavigna and Pollet (2009). For papers that study their effect on the behavior of organizations or of their representatives, see Jacob and Levitt (2003) and Jacob (2005). Dranove and Jin (2010) provides an extensive review of these and other papers.

[8] In this way, our paper also relates to a large finance literature on the effects of macroeconomic news on the behavior of investors. See Tetlock (2010) for a recent example.

[9] The idea that public information can worsen outcomes is known to theorists. Morris and Shin (2002), for example, shows that public information can have adverse welfare effects when agents also have private information.

[10] An early summary of the broad debate can be found in Stiglitz (2002).

on whether they should share or hide information, our findings point to the importance of considering *who* they share information with.

II. Data and Context

Busy intersections have several features which are essential for studying the effects of information disclosure. First, the prospect of an undesirable event—the light change—is commonplace for millions of road users each day. Bomb threats, fires, and outbreaks of infectious disease are rare and unpredictable, which hampers the accuracy of the empirical conclusions that one could draw in these contexts. Second, there is variation in what road users know about light changes. Before countdown signals were introduced they were left to guess when the light would change. After the introduction, they knew exactly when it would change. We observe behavior in both of these situations. Third, we can assess whether full disclosure has different implications for the vulnerable and less vulnerable, as well as whether there are more winners than losers. This is a challenge in other strategic settings because it is difficult either to identify the winners and losers or to quantify the separate effects that full disclosure has on different individuals. Since our data allows us to distinguish drivers from pedestrians we can assess the separate effects of full disclosure for different road users.[11] As a result, the findings can speak to the appropriateness of policies that disclose information asymmetrically.[12]

A. *How Countdown Signals Inform Road Users*

Figure 1 displays walk signals in the city of Toronto before and after pedestrian countdown signals were introduced. The flashing hand indicates to all road users that a yellow light for adjacent vehicular traffic is imminent. The timer begins when the orange hand starts to flash. It counts the time between the solid "Walk" signal, as represented by a walking stick figure, and the solid "Don't Walk" signal, as represented by a solid orange hand. The time counted is independent of the time of day, but it is longer at wider crosswalks.[13,14] Importantly, the time counted at each crosswalk was unchanged when the countdowns were introduced.

[11] Much like busy intersections, in crowded theaters the benefits from disclosure differ from individual to individual. In crowded theaters there are young children, elderly, and disabled. These vulnerable individuals may not escape safely unless the less vulnerable, for example young adult males, yield right of way to them. This reality is consistent with the social norm of "women and children first" in times of emergency.

[12] Early warning systems (for the onset of natural disasters) are, in contrast with crowded theaters, a context where asymmetric disclosure is an option for policymakers. In this context the resource is safe passage away from the disaster. As with crowded theaters, if individuals yield the right of way then they will not escape immediately, but they avoid the mass exodus of people trying to escape the disaster. Some individuals, such as those residing far from safe shelter, are more vulnerable than others. These individuals may not escape unless the less vulnerable—individuals who live closer to safe shelter—yield right of way to them. To limit the harmful effects on public safety the authority can inform and evacuate the more at-risk individuals ahead of others. This is a strategy often recommended by disaster planners (see page 49 of http://eprints.jcu.edu.au/19780/1/19780_Goudie_2007.pdf for more details).

[13] In Toronto, the duration of vehicular signals (green and red lights) is based on the time of day. These durations are based on historical traffic volumes in each direction at different times of the day.

[14] At intersections with side streets, vehicles and pedestrians can affect countdown signals along side streets. These intersections have sensors that detect the presence of vehicular traffic along side streets. Pedestrians along side streets can use push buttons to initiate the timers.

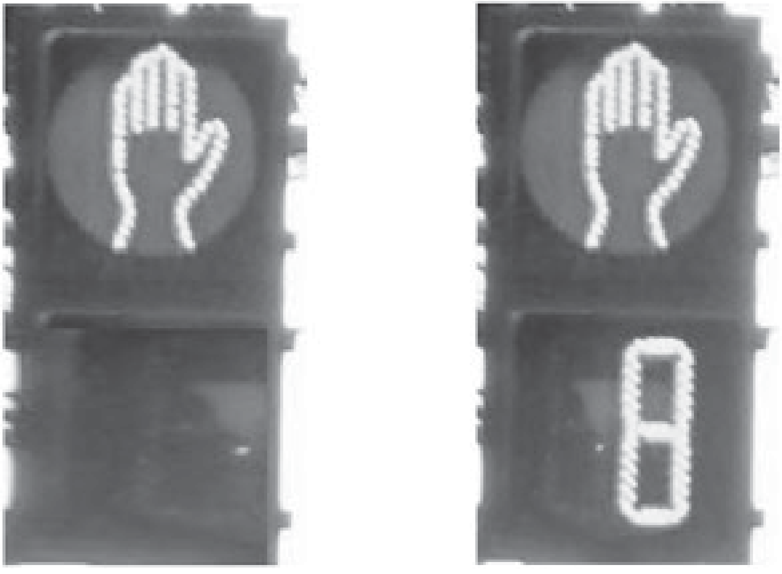

Figure 1. Flashing Don't Walk Signal, with and without Countdown

B. *The Natural Experiment*

The adoption of countdown signals was incidental to a citywide initiative that retrofits pedestrian and vehicular displays with more energy-efficient LED lamps.[15,16] The city's view was that installing countdowns alongside LED lamp installations was more cost effective than retrofitting the LED lamps with countdowns at a later date. As such, the original motivation for the adoption of countdowns was unrelated to the city's history of traffic collisions, fatalities, and injuries.[17]

Because adopting countdowns was secondary to the city's goal to reduce the energy costs of traffic signals as well as CO_2 emissions, the timing and locations of installations was unrelated to the collision history at each intersection. The installation dates and locations for the LED lamps were based on cost considerations and, moreover, were largely chosen before countdowns were included in the city's initiative. The first countdown was installed in November of 2006. In the period that we study the last countdown was installed in December of 2008.

Figure 2 graphically depicts the evolution of countdown installations over time. The figure supports the idea that installation dates and locations were motivated by cost considerations, as initial installations were geographically concentrated in a

[15] The initiative was actually part of broader program to retrofit all city streetlights with more energy-efficient lamps.

[16] Originally, the streetlights were fitted with incandescent lamps. The program retrofits streetlights with Light Emitting Diode (LED) lamps. LED lamps use fewer watts to produce the same luminescence as incandescent lamps.

[17] These claims are supported by official city documents. These documents can be found at the city's website: http://www.toronto.ca.

Figure 2. Countdown Installations in the City of Toronto

few central locations and diffused outwards thereafter. It supports the idea because geographically concentrating the installations is likely to reduce their costs.

C. *A Description of the Data*

We complement the rich variation generated by the city's natural experiment with detailed retrospective monthly collisions data collected over a five-year span. The data describes every collision that occurred in the city, including injuries and fatalities to the involved parties, the precise location of the collision, and which party was at fault and for what reason. We exploit the wealth of detail to identify specific mechanisms that drive the increase in collisions. We investigate whether countdowns provide road users with information that they use to act more aggressively and whether increased acts of aggression harm others on the road.

Table 1—Descriptive Statistics—Counts by Year

	Year				
	2004	2005	2006	2007	2008
Collisions	5,058	5,166	4,704	4,500	4,194
Driver-pedestrian	266	322	301	296	295
Driver-cyclist	124	127	136	128	129
Driver-driver	4,250	4,185	3,897	3,740	3,407
Fatalities	8	10	10	13	10
Major injuries	67	95	76	82	63
Minor injuries	267	244	232	243	212

Our sample is an extract from the internal collisions database maintained by the City's Transportation Services Division. The database contains information on all collisions that occurred between January, 2004 and December, 2008.[18] We restrict the sample to collisions that occurred at an intersection with traffic signals. The collisions data includes information on the parties involved, for example whether they were a cyclist, driver, or pedestrian, and whether they incurred an injury or fatality,[19] which party was at fault and why, as well as the precise time and location of the collision. Our analysis rests on monthly level observations.[20] Overall, we observe 1,794 intersections during a five-year period for a total of 107,640 observations.[21]

Table 1 provides summary counts for the main variables used in our empirical analysis, which illustrate clear downward trends in several variables of interest. The total number of collisions decreased from 5,058 in 2004 to 4,194 in 2008, seemingly driven by a sharp decline in driver-driver collisions. While fatalities and major injuries are relatively stable, minor injuries decline from 267 in 2004 to 212 in 2008. Later we provide evidence which suggests the trends in collisions and injuries simply reflect a downward trend in traffic volumes.

III. Empirical Specification and Identification

The baseline specification that we consider is given by

$$y_{it} = \alpha_i + \beta I(t \geq \tau_i) + \mathbf{X}_{it}\mathbf{\Gamma} + \gamma_t + \epsilon_{it}. \tag{1}$$

[18] Collision information is retrospectively based on police reports.

[19] The data classifies fatalities as those persons who die within 366 days of a collision.

[20] We focused on monthly data because the process of obtaining estimates with daily data is computationally extremely burdensome, even in the linear panel data framework. Moreover, the monthly data still allows us to credibly answer a causal question of interest.

[21] We excluded intersections without traffic signals at the start of our sample period because the decision to install signals is endogenous to collisions. We also excluded ones that never receive a countdown. These intersections are typically located near emergency response operations, such as firehalls, where traffic signals are fitted with preemptive systems that facilitate quicker response times. The intersections did not receive countdowns because preemptive systems confuse the countdown's timing.

y_{it} is the number of collisions at intersection i at time t.[22] The index t counts months, starting in January 2004 and ending in December 2008. α_i controls for time-invariant differences in the propensity for collisions across intersections, such as those that are generated by the number of lanes or the posted speed limits. τ_i is the installation date for intersection i. $I(t \geq \tau_i)$ is a binary variable that indicates whether the current date equals or exceeds the installation date, so that intersections with $I(t \geq \tau_i) = 1$ are in the treatment group. γ_t is a time-specific intercept. It allows for intersection-invariant differences across time in the propensity for collision, such as those that are generated by bad weather. ϵ_{it} is a random variable that measures idiosyncratic changes in collisions.

The random variables α_i and γ_t control for possible selection effects. For example, the city may have (inadvertently) installed the first countdowns at locations with collision propensities that fail to represent the typical intersection. In this case, intersection specific factors explain both observed installation decisions as well as observed collisions—excluding α_i would result in a biased estimate of the treatment effect. On the other hand, γ_t controls for time-based selection effects, in addition to trends in collisions. Specifically, it controls for the probability that an intersection receives a countdown, a probability that is increasing with time. Excluding γ_t would likely result in a (downward) bias in the estimated treatment effect.

Finally, although the pattern of installation indicates otherwise, $\mathbf{X}_{it}$ includes controls that allow for the possibility that intersections with a recent history of collisions are treated earlier than others. In particular, $\mathbf{X}_{it}$ includes lagged collisions. We show in the next section the evidence supports the city's claim that installations were unrelated to collision histories at intersections.

IV. Results

A. *Unintended Consequences*

We study the unintended consequences of pedestrian countdown signals. Table 2 presents estimates of the effect of countdown signals on collisions. The main finding is that countdown signals result in more collisions, once intersection- and time-specific factors are accounted for. The estimate in column 3 shows that there

[22] We use OLS fixed effects (FE) to estimate our specifications. We do so because it is the only available estimation method that credibly delivers consistent estimates of the treatment effect. In the OLS FE framework, we can flexibly account for permanent unobserved differences across intersections. In our context the count data framework is not applicable. This is because the standard Poisson FE estimator is not well-suited for handling counts with excess zeros. With excess zeros, the Poisson estimation procedure drops the individuals that never experience the event. In our context, the procedure drops almost half of the intersections in our sample and, ultimately, results in a substantial selection problem as well as a substantial loss of statistical power. Not including fixed effects is not a viable option either, as a key part of our identification strategy is being able to control for permanent, unobserved differences across intersections. To alleviate concerns about the appropriateness of our standard errors, in the online Appendix we consider a couple of alternative strategies. First, we consider the most common alternative method for approximating standard errors, bootstrapping (see Wooldridge 2010, chapter 12 for details). The validity of bootstrapped standard errors and the resulting test statistics does not rely on the assumption of normality of the regression model error (an assumption which may be violated with count data). Second, we consider a simple transformation of our outcome variable to a continuous measure. We redefine the outcome of interest to be the ratio of number of collisions to total traffic flow through an intersection. Both strategies strongly reinforce the robustness of our results.

Table 2—Collisions and Pedestrian Countdown Signals

	(1)	(2)	(3)	(4)
Pedestrian countdown	−0.055***	−0.022***	0.012**	0.011*
Signal activated	(0.006)	(0.004)	(0.006)	(0.006)
Controls				
Intersection	No	Yes	Yes	Yes
Month-year	No	No	Yes	Yes
Lagged collisions	No	No	No	Yes
R^2	0.001	0.003	0.004	0.003
Intersections	1,794	1,794	1,794	1,794
Observations	107,640	107,640	107,640	105,846

Notes: The dependent variable is number of collisions. Robust standard errors clustered at the intersection level.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

were 0.012 more collisions per month at the average intersection, where the estimate is statistically significant at the 5 percent level against a two-sided alternative. The increase in collisions represents a more than 5 percent increase over the mean number of collisions, which was 0.229 before countdown signals were introduced. The sign change when we include time-specific controls (columns 2 and 3) are consistent with a pre-existing downward trend in collisions[23] as well as with an upward trend in the probability that an intersection is assigned a countdown. Table 2 also shows that lagged collisions matter little for the estimated effect of countdowns.[24, 25]

To further understand why there were more collisions at intersections with a countdown, we consider the countdown's effect on three classes of collisions: ones involving only drivers; ones involving drivers and pedestrians; ones involving drivers and cyclists. The estimates can be found in Table 3. The evidence in column 1 suggests countdowns resulted in more collisions between drivers. We estimate 0.012 $(p < 0.05)$ more driver-driver collisions per month at the average intersection after countdowns were introduced.[26]

Table 3 also illustrates that countdowns resulted in fewer collisions between drivers and pedestrians. The estimate in column 2 suggests that there were 0.0032 $(p < 0.1)$ fewer driver-pedestrian collisions per month at the average intersection

[23] This result illustrates the benefits of a relatively long history of data from before the first installation. These data allow us to more accurately capture time trends that existed before countdowns were introduced.

[24] In the online Appendix, we show that the estimates are robust to many more lags of the dependent variable.

[25] To be certain of our identifying assumption, we explicitly tested that the probability of countdown assignment is unrelated to the collision history at the intersection. Specifically, using only intersection-month observations where $T_{it-1} = 0$, we use a probit to estimate the following:

$$T_{it} = I(\beta_0 + \beta_1 \text{history}_i + \delta_t + \varepsilon_{it} \geq 0),$$

where δ_t is unobserved time specific heterogeneity. history_i is the accident history of an intersection, as measured by the cumulative number of collisions at the intersection in the years preceding the city wide rollout of countdowns (2004 and 2005). The approach yields an estimated effect for β_1 of -0.0008 with an estimated standard error of 0.0010. The estimate supports the exogeneity of countdown assignment to historical accident patterns.

[26] A low R^2 arises in our context because collisions are highly idiosyncratic. We note that, under the assumption that installations (where and when) are exogenously assigned to intersections, the low R^2 does not bear on our ability to interpret the results as causal.

Table 3—Collision Involvements and Conditions

	Collisions involving:		
	Driver-driver	Driver-pedestrian	Driver-cyclist
Pedestrian countdown	0.0117**	−0.0032**	0.0014
Signal activated	(0.0052)	(0.0015)	(0.0011)
R^2	0.004	0.002	0.003
Intersections	1,794	1,794	1,794
Observations	107,640	107,640	107,640

Notes: Robust standard errors clustered at the intersection level. All regressions include fixed effects for the intersection and month-year.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

after countdowns were introduced. On the other hand, the estimate in column 3 suggests that countdowns had a positive but statistically negligible (at the 10 percent level) impact on collisions between drivers and cyclists.

Three explanations might justify the increase in collisions between drivers. The first, is that being informed about the precise time until a light change allows drivers to become *selectively aggressive* in their approach to an intersection. Specifically, in the effort to avoid stop lights, drivers might accelerate when they know just enough time remains than when they don't.[27] The second explanation is that countdowns distract drivers. They divert the driver's attention away from the road and, in turn, increase the chances that a collision ensues. The third is that countdowns do not directly cause collisions, rather they indirectly cause them through third-party responses to the countdown. The countdowns alter the behavior of cyclists and pedestrians, and in an effort to avoid these third parties, drivers collide with each other.

B. *More Information Means More Aggression*

We show that information about light changes induced drivers to act more aggressively. Table 4 provides estimates of the effect on collisions where at least one driver was exceeding the speed limit or tailgating.[28,29] While the estimate of column 1 suggests a small and statistically insignificant impact on speeding, the estimate of column 2 suggests countdowns resulted in 0.0074 ($p < 0.05$) more collisions where at least one driver was tailgating another. As a result, the evidence supports a story where drivers act more aggressively when they are informed about the time until light changes.

[27] In the online Appendix, we show that in the context of a very simple textbook example of interactions between drivers (see "Approaching Cars" on page 130 of Osborne 2004 that providing drivers with information about the time until a light change causes drivers to approach traffic lights more aggressively on average).

[28] A driver is tailgating if they were reported as following another driver too closely.

[29] Tailgating is widely considered the model of aggressive behavior, and much effort, both by way of government policy and nongovernment initiatives, has gone into reducing tailgating among drivers. Examples of these efforts can be found at http://www.stopandgo.org/research/aggressive/taxca.pdf and http://www.dot.state.mn.us/trafficeng/tailgating/Tailgating-finalreport.pdf.

Table 4—Driver Actions and Conditions

	Collisions where a driver:	
	Speeds	Tailgates
Pedestrian countdown	0.0001	0.0074**
Signal activated	(0.0002)	(0.0020)
R^2	0.0005	0.0038
Intersections	1,794	1,794
Observations	107,640	107,640

Notes: Robust standard errors clustered at the intersection level. All regressions include fixed effects for the intersection and month-year.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

It's More than Just Inattention.—We consider the possibility that countdown signals distracted drivers. Specifically, we consider whether collisions increase because countdown signals divert driver attention away from the road. To do so, we compare and contrast the lasting effects of collisions with the more immediate ones. If countdown signals distracted drivers, then their positive effect on collisions should be more pronounced in the periods immediately after their installation. Initially, because drivers are unsure as to how to best use the countdown signals, it further distracts their attention from the road, and collision becomes more likely. As time passes, countdowns impose less of a burden on driver attention because drivers adjust to the new environment they face. Consequently, the chance of a collision should decrease.

To evaluate these alternative models, we use the following specification to estimate short-run and long-run treatment effects:

$$(2)\quad y_{it} \,=\, \alpha_i + \sum_{k=0}^{K} \beta_k I(t \,=\, \tau_i + k) + \beta_{K+1} I(t \,>\, \tau_i + K) + \mathbf{X}_{it}\boldsymbol{\Gamma} + \gamma_t + \epsilon_{it}.$$

The coefficients $\{\beta_k\}_{k=0}^{K+1}$ describe the collision trajectory that follows a countdown installation. The first K terms describe the transition—they capture the average effect of countdowns in a month following installation relative to the effect before the first installation. The last term captures the "permanent" effects. This specification is less restrictive than the base specification, as $I(t \geq \tau_i) = \sum_{k=0}^{K} I(t = \tau_i + k) + I(t > \tau_i + K)$. We also include leads of $I(t = \tau_i)$ in $\mathbf{X}_{it}$ to evaluate the role of collision histories in treatment effect estimates[30]—the leads describe the collision trajectory before a countdown installation.[31]

[30] Formally, the leads are $I(t = \tau_i - 1), I(t = \tau_i - 2), \ldots, I(t = \tau_i - s)$ for some $s \geq 1$.

[31] While this approach ostensibly resembles an event study, conceptually the two approaches differ. An event study effectively evaluates the effects of a one time event that is temporary, but that may have lasting effects. Examples of such events include worker displacement (Jacobson, LaLonde, and Sullivan 1993), which may adversely affect future earnings, or EPA plant inspections (Hanna and Oliva 2010), which may have lasting effects on plant emissions. We evaluate the effects of a one time event that is permanent, where these effects may vary from period to period. Specification 2 is appropriate for both cases.

Table 5—Collisions and Pedestrian Countdown Signals—Dynamic Treatment Effects

Months after installation	(1)	(2)	(3)	(4)	(5)
0 months	0.015	0.017	0.019	0.018	0.023*
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
1 month	0.005	0.007	0.009	0.009	0.013
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
2 months	−0.012	−0.010	−0.008	−0.008	−0.004
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
3 months	0.016	0.018	0.020*	0.020	0.024*
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
4 months	0.012	0.014	0.017	0.016	0.020
	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)
5 months	0.002	0.005	0.007	0.007	0.011
	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)
6 months		0.019	0.021	0.021	0.025*
		(0.014)	(0.014)	(0.014)	(0.015)
7 months			0.034**	0.033**	0.038**
			(0.015)	(0.015)	(0.016)
8 months				0.038**	0.043**
				(0.007)	(0.017)
9 months					0.029*
					(0.017)
After last month in specification	0.029***	0.034***	0.038***	0.037***	0.045***
	(0.008)	(0.009)	(0.010)	(0.010)	(0.011)
Intersections	1,794	1,794	1,794	1,794	1,794
Observations	107,640	107,640	107,640	107,640	107,640
p-value for *F*-test that leads don't matter	0.15	0.19	0.22	0.30	0.21

Notes: The dependent variable is number of collisions. Robust standard errors clustered at the intersection level. Regressions control for intersection and month-year fixed effects. They also include leads for first installation date.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

In Table 5, we present estimates of equation (2) for different values of K. Two things are apparent from these estimates. The first is that, as we lengthen the short run, the countdown's estimated long run effect grows in magnitude. The estimated long run effect ranges from 0.029 more collisions on average in column 1 to 0.045 more in column 5. Each of these are statistically significant at the 1 percent level. The second is that the estimated short run effects of countdowns, while varying in magnitude and statistical significance, appear somewhat smaller than those estimated for the long run. This is particularly true for the periods immediately following the initial installations.

We plot the estimates from column 5 of Table 5 in Figure 3. The solid line plots the estimates for leads to the left of the red line and the estimates for lags to the right. The dashed lines plot the 90 percent confidence interval. Figure 3 illustrates that in all but one case we fail to reject the hypothesis that collisions followed their usual patterns in the months leading up to a countdown installation (because zero enters the confidence interval only once). In contrast, it supports the hypothesis that collisions departed from their usual pattern when road users were informed about the time until light changes.

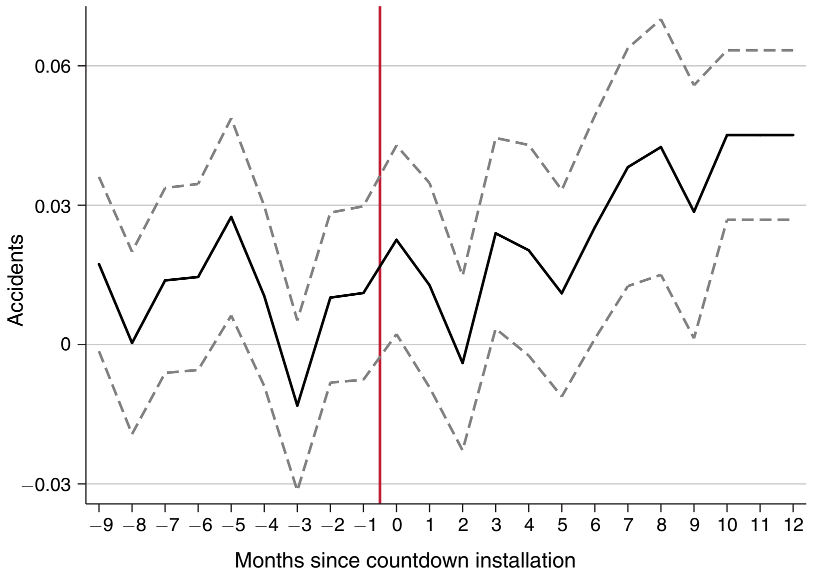

Figure 3. Pedestrian Countdown Signals and Their Effects on Collisions

The evidence fails to support the hypothesis that countdown signals distracted drivers. The results are unsurprising, mostly because a situation where countdowns cause inattention seems highly unlikely. This is because countdown signals and traffic lights are in the *same* line of sight for approaching drivers and because, consequently, drivers can use the information countdowns provide without having to look away from the traffic light. On the other hand, the evidence is consistent with the hypothesis that collisions increased because drivers became more aggressive when they were informed about the time until a light change. Specifically, if the information enables road users to better respond to their circumstances, and road users learn over time how timers can best be used to avoid getting caught waiting at intersections, then we would expect a more pronounced permanent effect of countdown signals and a less pronounced temporary one.[32]

It's Not Just Third-Party Effects.—We consider the possibility that changes in third-party behaviors explain the increase in driver-driver collisions. In particular, using more detailed collision information, we explore whether collisions among drivers increased because countdowns induced third parties to enter intersections at inopportune times. We argue that the estimates from column 2 of Table 3 and

[32] To elaborate, we interpret the findings in Tables 5–7 as evidence of drivers becoming more aggressive as they familiarize themselves with timers. Formally, we interpret the findings as evidence of a transition from one equilibrium to another following the installation of a countdown. Before the countdown is installed, the less-aggressive behavior of drivers is consistent with one type of equilibrium, implying a lower equilibrium probability of accident. Immediately after the countdown is installed, there is a period where individuals learn about how the countdown can work to their advantage. Over time, the behavior of drivers transitions to a new equilibrium. One where drivers are more aggressive and there is a higher equilibrium probability of accident. Our feeling is the new equilibrium, on average, appears three months after treatment and stabilizes six months after.

Table 6—Third-Party Effects

	Collisions where driver:		
	Turns left	Turns right	Not turning
Pedestrian countdown	0.0023	0.0024**	0.0075*
Signal activated	(0.0027)	(0.0012)	(0.0041)
R^2	0.0019	0.0010	0.0028
Intersections	1,794	1,794	1,794
Observations	107,640	107,640	107,640

Notes: Robust standard errors clustered at the intersection level. All regressions include fixed effects for the intersection and month-year.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 6 suggest the increase in collisions is unrelated to the behavior of third-party pedestrians.

If third-party pedestrians are the source of more driver-driver collisions, it should be the case that pedestrians are placing themselves in more risky situations. The estimates from column 2 of Table 3 and from Table 6 suggest otherwise. The estimates in Table 3, which show that countdowns resulted in fewer driver-pedestrian collisions, suggest that pedestrians might act more cautiously after the countdown installation.[33] The estimates from Table 6 provide further support for this idea, as they show that in interactions where drivers are more likely to meet pedestrians (turns) the rise in collisions is smaller than in ones where they're not. Columns 1 and 2 suggest there were 0.0022 more collisions among drivers when they make right or left turns, though only the coefficient for right turns is statistically significant. Column 3 suggests there were 0.0075 more collisions $(p < 0.1)$ among drivers where at least one driver was traveling straight through the intersection, a driving maneuver that is unlikely to involve third parties.

V. Implications for Social Welfare

We approach the welfare effects of countdowns from three directions. First, we consider the effect of countdowns on various types of collisions, such as rear ends and sideswipes. Second, we consider the impact on fatalities and injuries. Third, we study the effect on traffic and pedestrian volumes at intersections. Our major findings are that countdowns resulted in more rear ends, fewer minor injuries, and had a negligible effect on traffic and pedestrian volumes. The findings suggest that the welfare impacts hinge on a comparison of the additional costs of rear ends with the benefits of fewer minor injuries.

[33] The particular piece of evidence is also consistent with an alternative hypothesis. Under the alternative, drivers act more aggressively with each other, but less aggressively towards pedestrians, when informed about the time until a light change.

Table 7—Collision Types and Injuries

	Impact type					Injury type		
	Entering	Angle	Rear end	Sideswipe	Turning movement	Fatalities	Major	Minor
Countdown activated	0.0003	0.0004	0.0108***	−0.0009	0.0021	−0.0003	0.0006	−0.0027*
	(0.0007)	(0.0022)	(0.0032)	(0.0015)	(0.0030)	(0.0003)	(0.0009)	(0.0016)
R^2	0.0010	0.0007	0.0023	0.0011	0.0019	0.0005	0.0005	0.0007
Intersections	1,794	1,794	1,794	1,794	1,794	1,794	1,794	1,794
Observations	107,640	107,640	107,640	107,640	107,640	107,640	107,640	107,640

Notes: Robust standard errors clustered at the intersection level. All regressions include fixed effects for the intersection and month-year.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

A. *Injuries and Rear Ends*

Columns 1–5 of Table 7 suggest the costs of pedestrian countdown signals are comprised primarily by the costs of more rear ends. These columns provide estimates of the countdown's effect on various types of collisions, those where at least one driver: enters the intersection; collides with another at an angle; rear ends another driver; sideswipes another driver; or was turning when a collision occurred. The estimates show that countdowns resulted in 0.0108 more collisions per month $(p < 0.05)$ where one driver rear ends another at the average intersection.

Columns 6–8 of Table 7 suggests the benefits of pedestrian countdown signals are comprised primarily by the benefits of fewer minor injuries. Column 8 shows countdowns resulted in 0.0027 fewer minor injuries per month at the average intersection. This finding is consistent with our finding in column 2 of Table 3 of a reduction in collisions between pedestrians and drivers, because most collisions involving pedestrians and drivers result in minor injuries.

B. *Traffic and Pedestrian Flows*

In order to more properly assess the welfare implications of reducing road-user uncertainty about light changes, we consider the effects of countdown signals on vehicular and foot traffic at the intersections in our sample.[34] The specific goal is to determine whether countdown signals resulted in fewer pedestrian-driver collisions for every pedestrian on the road and whether they resulted in more collisions between cars for every car on the road. The finding that countdowns reduced driver-pedestrian collisions has positive welfare implications when the same or more pedestrians use intersections after countdowns were introduced. The implications are ambiguous when fewer pedestrians use intersections with countdown signals.

[34] As with the collisions data, the source for the vehicular and foot traffic data is the Transportation Services Division of the City of Toronto. The most recent counts for vehicular and foot traffic are found at http://toronto.ca/open.

Table 8—Countdowns and Pedestrian Flow

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Countdown activated	1,030.50***	1,017.06***	992.32***	1,812.81***	1,281.43***	137.48	−228.44
	(129.96)	(130.02)	(127.89)	(293.55)	(274.54)	(260.47)	(669.40)
Controls							
Day of week	No	Yes	Yes	Yes	Yes	Yes	Yes
Month	No	No	Yes	Yes	Yes	Yes	Yes
Year	No	No	No	Yes	Yes	Yes	Yes
N-S/E-W	No	No	No	No	Yes	Yes	Yes
Main street	No	No	No	No	No	Yes	Yes
Side street	No	No	No	No	No	No	Yes
R^2	0.02	0.02	0.08	0.11	0.17	0.52	0.81
Intersections (Observations)	1,912	1,912	1,912	1,912	1,912	1,912	1,912

Notes: The dependent variable is volume of pedestrians using the intersection over an 8-hour period. Robust standard errors.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Similarly, the finding that countdowns resulted in more driver-driver collisions has negative welfare implications when the same or fewer cars use intersections after countdowns were introduced. The implications are ambiguous when more cars use intersections with countdown signals.

To quantify the rise in collisions, and reduction in minor injuries, relative to the flow of road users, we draw on counts of pedestrian and automobile traffic at intersections throughout the city.[35] In both cases, we estimate specifications of the form

$$V_{it} = \delta_0 + \delta_1 T_{it} + \mathbf{X}_{it}\boldsymbol{\pi} + \upsilon_{it}, \tag{3}$$

where t is the time of the count, V_{it} represents volume (pedestrian or automobile) that passes through intersection i at time t, T_{it} indicates whether a countdown is installed, and $\mathbf{X}_{it}$ controls for time and geographic factors that might affect variation in V_{it} and T_{it}. We note that counts are done at different (and irregular) points in time. In the case of pedestrians, counts are done only once, while automobile counts are done repeatedly for most intersections.[36]

Table 8 provides estimates of the effect of countdown signals on the number of pedestrians transiting intersections. The data reveals three things. The first is that columns 3 and 4 show a downward trend in pedestrian traffic across years. This conclusion follows because intersections are more likely to have a countdown installed in the later years of our sample. The second is that columns 5 and 6 demonstrate that excluding geographic factors results in overestimates of the countdown's effect on

[35] These counts were done for most of the intersections in our study.

[36] In fact, for many intersections we have multiple observations from the same time period. This is because at separate counts are done for traffic flowing in various directions. At a minimum, this provides another useful source of variation for identifying an effect of countdowns on traffic volume.

Table 9—Countdowns and Automobile Flow

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Countdown activated	−1,079.74***	−1,097.60***	−996.12***	1,479.03***	1,456.83***	−655.97*	−346.34
	(389.78)	(363.41)	(362.41)	(459.51)	(471.90)	(340.79)	(224.89)
Controls							
Day of week	No	Yes	Yes	Yes	Yes	Yes	Yes
Month	No	No	Yes	Yes	Yes	Yes	Yes
Year	No	No	No	Yes	Yes	Yes	Yes
N-S/E-W	No	No	No	No	Yes	Yes	Yes
Street 1	No	No	No	No	No	Yes	Yes
Street 2	No	No	No	No	No	No	Yes
R^2	0.002	0.008	0.05	0.08	0.08	0.65	0.83
Observations	28,996	28,996	28,996	28,996	28,996	28,996	28,996
Intersections	1,637	1,637	1,637	1,637	1,637	1,637	1,637

Notes: The dependent variable is volume of pedestrians using the intersection over a 24-hour period. Robust standard errors.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

pedestrian traffic. The third is that pedestrian traffic was unaffected by the presence of countdown signals once all of the time and geographic factors are controlled for.

The results for pedestrian traffic as well as the reduction in pedestrian-driver collisions (Table 3) suggest that pedestrians benefited from the introduction of countdown signals. The estimate in column 7 shows that they benefited because fewer pedestrians were struck by automobiles for every pedestrian on the road. A potential welfare improvement for pedestrians is unsurprising because a major motivation for introducing countdown signals is that they "have been proven to improve pedestrian signal understanding, and have particular benefit for vulnerable road users such as seniors, children, and mobility-challenged pedestrians."[37] Pedestrians who were initially reluctant to use intersections may now feel safer doing so, and in fact are safer doing so.

We use Table 9 to study the countdown's effect on the number of cars transiting intersections. The estimates suggest at best that countdown signals had a statistically insignificant effect on the number of automobiles per 24-hour period at the average intersection.[38] As with pedestrian flows, Table 9 suggests that geographic and time factors matter for estimates of the countdown's effect on automobile flows. Specifically, a comparison of columns 3 and 4 reveals a downward trend in automobile traffic across years. Similarly, a comparison of columns 5 and 6 demonstrates that excluding geographic factors results in overestimates of the countdown's effect on automobile traffic.[39]

[37] See http://www.transportation.alberta.ca/.

[38] One caveat with this result is that with this data intersections are only observed with countdowns 6 percent of the time. However, it's likely that the number of observations more than compensates for the loss in statistical power this asymmetry generates.

[39] In contrast with the pedestrian count data, these street indicators fail to distinguish between main and side streets. Instead they indicate the street along which the measured flow is traveling (street 1) as well as the intersecting street (street 2).

The results for automobile traffic as well as the increase in driver-driver collisions (Table 3) suggest that drivers suffered from the introduction of countdown signals. The estimate in column 7 shows that they suffered because of more collisions between drivers for every driver on the road. As a result, the data reveals that countdowns may have had negative implications for the welfare of drivers who visit an intersection.

VI. Applicability to Other Cities

We assess the broader applicability of our main finding that countdowns cause more collisions. Our specific strategy compares the effects of countdowns on collisions at intersections that are historically safe with the effect at intersections that are historically dangerous. The comparison allows us to draw inferences about the effects of countdowns at intersections in cities with a mix of safe and unsafe intersections, to cities with many safe intersections, or to cities with many unsafe intersections. We can draw such inferences because the location and timing of installations in Toronto were unrelated to the collision histories of intersections, and because the decision to adopt countdowns was unrelated to the collision history of Toronto as a whole.

We estimate the specification

$$y_{it} = \alpha_i + \beta_1 I(t \geq \tau_i) + \beta_2 I(t \geq \tau_i) Z_i + \mathbf{X}_{it}\boldsymbol{\Gamma} + \gamma_t + \epsilon_{it}, \tag{4}$$

where

$$Z_i = \frac{hist_i}{vol_i}.$$

$hist_i$ is the number of collisions in the (pre-treatment) years 2004–2005, and vol_i is the number of cars transiting through intersection i.[40] Z_i is then the number of collisions per 1,000 cars that travel through the intersection. Estimates of the specification are found in Table 10.

The estimates reveal that countdowns make life at historically safe intersections more dangerous. At the median value for Z_i (0 collisions per thousand cars),[41] the estimate in column 3 shows there are 0.036 more collisions following the introduction of a countdown signal, three times more than the effect reported in Table 2. This implies that for intersections less dangerous than the median, the countdown causes a significant increase in collisions. On the other hand, the estimate for the median intersection implies that countdowns reduced the propensity for collision at historically very dangerous intersections.[42]

[40] For some intersections we observe volume more than once. In these cases, we use the average over the number of observations.

[41] Not surprisingly (given that collisions are relatively infrequent) the distribution of Z_i is very skewed to the right.

[42] The effect of countdowns on collisions becomes neutral at the 70th percentile of Z_i.

Table 10—Countdown Timers at Safe and Dangerous Intersections

	(1)	(2)	(3)	(4)
Countdowns activated	−0.207***	0.008***	0.036***	0.032***
	(0.012)	(0.003)	(0.006)	(0.006)
Interaction with accident history	0.353***	−0.069***	−0.070***	−0.065***
	(0.017)	(0.011)	(0.011)	(0.011)
Controls				
Intersection	No	Yes	Yes	Yes
Month-year	No	No	Yes	Yes
Lagged collisions	No	No	No	Yes
R^2	0.002	0.002	0.006	0.006
Intersections	1,692	1,692	1,692	1,692
Observations	101,520	101,520	101,520	99,828

Notes: The dependent variable is number of collisions. Robust standard errors clustered at the intersection level.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

We can infer two conclusions from these findings. The first is cities might benefit from installing countdowns at historically highly dangerous intersections and from not installing them at historically safe intersections. The second conclusion is that while countdowns can improve safety in historically dangerous cities, they may be detrimental to safety in historically safe ones. This conclusion applies to cities where the responses to countdowns by individual road users resembles the responses of individual road users in Toronto.

VII. Conclusion

Most existing studies analyze the effect of policies that increase the information that participants on one side of a market have about participants on the other side. We focus on the impact of a policy which increases the information that participants on all sides have about an event that is in their common interest. We draw on a natural experiment conducted in the city of Toronto to evaluate the impact that pedestrian countdown signals have on the behavior and safety of road users. We find that the installation of countdown signals resulted in approximately 21.5 more collisions citywide per month, a more than 5 percent increase over the average without countdown signals. The data reveals starkly different effects for collisions involving pedestrians and those involving automobiles only. Although they reduce the number of pedestrians struck by automobiles, countdowns increased the number of collisions between automobiles. We show that countdowns cause fewer minor injuries among pedestrians for every pedestrian on the road and more rear ends among cars for every car on the road.

The findings imply authorities can improve welfare by sharing the information with pedestrians and hiding it from drivers. For example, rather than making countdowns visible, the traffic authority might announce the time until a light change through a speaker that only pedestrians can hear. Although this policy makes it more difficult for drivers to use the information for their personal gain, it continues to provide pedestrians with information that can make their lives safer. More generally, rather than simply releasing or withholding information, policymakers can achieve welfare gains by creating asymmetries in the information that market participants possess.

The data also reveals that, though countdown timers make the typical intersection more dangerous, they have disparate effects on intersections with different propensity for collisions. In particular, countdown timers actually make historically very dangerous intersections safer. This finding provides policymakers with additional guidance concerning the adoption of pedestrian countdown signals. More specifically, two prescriptions follow from the finding. First, cities might benefit from installing countdowns at dangerous intersections and not at safe ones. Second, under the assumption that the response to countdowns by road users in other cities will resemble the response by road users in Toronto, cities that are historically dangerous for road users should consider adopting countdowns, while cities that are historically safe should not.

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PAGING INSPECTOR SANDS: THE COSTS OF PUBLIC INFORMATION
Online Appendix

Sacha Kapoor and Arvind Magesan*

This appendix has two sections. In the first section we provide a simple theoretical example to rationalize the key empirical result from the paper, that the installation of countdown timers increased the propensity for collisions among drivers. In the second we provide discussion and evidence of the robustness of the key empirical findings to alternative estimation methods.

1 A Textbook Example of Driver Interaction

We modify a simple textbook model of driver interaction,[1] where drivers can choose to act aggressively or cautiously, to rationalize the finding that informing drivers about light changes causes more driver-driver collisions. The modification is that we allow for uncertainty in what drivers know about the time until a light change at an intersection. We show that under rather innocuous assumptions, equilibrium collision probabilities are larger when drivers know the time that remains. The result follows from two simple features of equilibrium behavior in our model. First, aggressive behavior on the part of informed drivers is more likely when less time remains. Second, drivers are most responsive to information about light changes when they learn the time that remains is less than they expected. Taken together, the features of equilibrium behavior imply that informing drivers increases the chances of a collision.

1.1 Model

Suppose that two randomly matched drivers drawn from a single population approach an intersection from different directions. As they approach, each driver can choose either to proceed with caution (C) or to act aggressively (A). A driver who acts cautiously either slows down or stops, yielding the right-of-way to the other driver. A driver who acts aggressively either continues at the same speed or speeds up without conceding the right-of-way. We

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[1] See *Approaching Cars* on page 130 of Osborne [2004].

assume drivers are impatient and prefer the right-of-way to waiting for the other to pass. Drivers that are aggressive risk receiving a fine if they are caught, and risk collision with the other vehicle if its driver also chooses to be aggressive. This general framework encompasses several common interactions that occur at intersections. One such interaction occurs when a driver who is traveling straight through an intersection meets another who is turning left from the opposite direction. Another occurs when a driver is again traveling straight through but meets a driver who is turning right from the adjacent street.

Let $v > 0$ be the payoff from obtaining the right-of-way, $c > 0$ the cost of collision, and $b > 0$ be the cost of a fine to a driver caught crossing the intersection when the light is red. We assume that $c > b$. Let p be the probability that a collision occurs when both drivers act aggressively. Let $P_T(\omega)$ be the probability that a driver is caught and fined when he acts aggressively. ω is a random variable that represents the number of seconds until a light change (from green to yellow). Its probability distribution is given by $F(\omega)$. We assume that $P_T(\omega)$ decreases as ω increases. That is, the probability of a fine when acting aggressively diminishes as the amount of time left before the light changes from green to yellow increases. For example, driving through a red light is more likely when the driver acts aggressively just before a light change than when he acts aggressively with plenty of time remaining.

ω is known to drivers when a countdown is present at the intersection, and unknown to drivers when there is no countdown. When ω is known, drivers have better information about the consequences of conceding the right-of-way to another road user. In particular, drivers know whether conceding the right-of-way will cause them to have to sit through a red light.

The normal form for the simple game we consider is presented in Figure I, where one driver chooses a row and the other a column. Payoffs are symmetric. The matrix lists the payoffs for the row player. $\pi(\omega)$ is the payoff to acting aggressively when the other driver

	Cautious	Aggressive
Cautious	0	0
Aggressive	$\pi(\omega)$	$p[-c] + (1-p)\pi(\omega)$

Figure I: The game when countdowns are active.

acts cautiously, where

$$\pi(\omega) = P_T(\omega)[-b] + (1 - P_T(\omega))v. \tag{1}$$

We assume that the chances of getting caught and fined for aggressive behavior is small enough so that $\pi(\omega) \geq 0$. If $\pi(\omega) < 0$, being cautions is the dominant strategy for both players, and in equilibrium collisions are never observed.

1.2 Public Information Increases Collision Probabilities

Under these assumptions, the game has three Nash equilibria: two asymmetric pure strategy Nash equilibria where one driver is cautious and the other aggressive and a symmetric mixed strategy Nash equilibrium (MSNE) where each player acts cautiously with probability

$$q^*(\omega) = \frac{pc - (1-p)\pi(\omega)}{p(c+\pi(\omega))}. \tag{2}$$

We focus our analysis on the MSNE for two important reasons. The first is that pure strategy equilibria are at odds with what we observe in the data, as they suggest that collisions never happen. On the other hand, when drivers use mixed strategies, the equilibrium probability that an collision occurs is given by:

$$P_i^*(a|\omega) = (1-q^*(\omega))^2 p \tag{3}$$

$$= \frac{1}{p}\Big[\frac{\pi(\omega)}{c+\pi(\omega)}\Big]^2 > 0. \tag{4}$$

The second reason we focus on MSNE is that the MSNE is the only symmetric Nash equilibrium of the game.[2] The pure strategy equilibria each require one driver to defer to the other by social convention. However, we are unaware of any social convention that would lead one of these equilibria to be the norm.[3]

Figure II describes the game where drivers are unable to observe countdown signals and are therefore uninformed about the time until a light change. $E\pi(\omega)$ is the expected payoff

	Cautious	Aggressive
Cautious	0	0
Aggressive	$E\pi(\omega)$	$p[-c] + (1-p)E\pi(\omega)$

Figure II: The game when countdowns are inactive.

to acting aggressively, where the expectation is taken with respect to ω.[4] Similar to the case where drivers are informed about ω, the unique MSNE probability of being cautious is given by:

$$q^*(F) = \frac{pc - (1-p)E\pi(\omega)}{p(c+E\pi(\omega))}. \tag{5}$$

We can use the unique MSNE to solve for the collision probability when there are no countdown timers - drivers are *uninformed*:

$$P^*(a|F) = (1-q^*(F))^2 p \tag{6}$$

$$= \frac{1}{p}\Big[\frac{E\pi(\omega)}{c+E\pi(\omega)}\Big]^2. \tag{7}$$

[2]In the language of evolutionary game theory, the MSNE is the unique evolutionarily stable strategy of our game.

[3]As we imagine that drivers are drawn and matched randomly from the same population, the MSNE can also be interpreted as the 'steady state' of interactions at intersections. Under this interpretation some fraction of the population of drivers act cautiously while the other fraction acts aggressively (See pp.37-39 of Osborne and Rubinstein [1994]).

[4]We assume that driver beliefs about the time until a light change are consistent with the true distribution $F(\omega)$.

Proposition 1 *The probability of an accident occurring at an intersection is larger when countdowns are present than when they are not:*

$$E_\omega[P^*(a|\omega)] > P^*(a|F) \tag{8}$$

Proof 1 *Simple algebra reveals that :*

$$E_\omega[P_i^*(a|\omega)] > P^*(a|F) \Leftrightarrow E\Big[\frac{1}{1-\frac{-c}{\pi(\omega)}}\Big]^2 > \Big[\frac{1}{1-\frac{-c}{E\pi(\omega)}}\Big]^2. \tag{9}$$

Our assumption that $c > b$ combined with Jensen's inequality ensure that $E\Big[\frac{1}{1-\frac{-c}{\pi(\omega)}}\Big]^2 > \Big[\frac{1}{1-\frac{-c}{E\pi(\omega)}}\Big]^2$. To see this Let $h(r) = \frac{\pi(\omega)}{c+\pi(\omega)}$ where $r = P_T(\omega)$. We define $f(t) \equiv \frac{1}{(1-t)}$ and $g(r) \equiv \frac{c}{r(b+v)-v}$ so that $h(r) = f(g(r))$. The functions f and g have the following properties: a) $f(t)$ is increasing and convex if $t < 1$; b) g is decreasing and convex in r. These properties imply that h is convex in r for $t < 1$. Or, equivalently, $h(r)$ is convex in r if and only if $-c < r(-b) + (1-r)v$. The inequality $-c < r(-b) + (1-r)v$ clearly holds when $c > b$. It follows that the square $h(r)^2$ is also convex in r. Since r is a monotone function of ω, applying Jensen's Inequality to $h(r)^2$ yields the result.

Mathematically speaking, the result is driven by the fact that the MSNE probability a driver is aggressive is convex and increasing in ω.

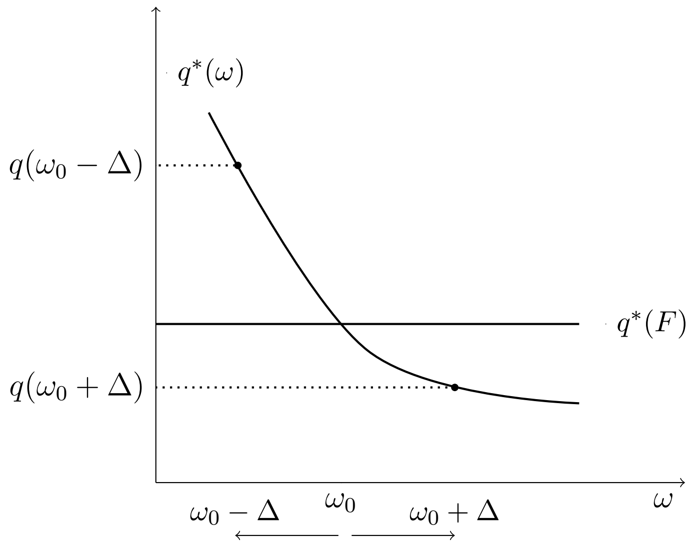
Consider figure III, which illustrates the key properties of the equilibrium with and without knowledge of ω. In the figure $q(\omega)$ traces the informed driver's equilibrium probability of behaving aggressively as a function of the state of the world ω. $q(F)$ represents the uninformed driver's equilibrium probability of behaving aggressively. It is independent of the true state of the world. Δ represents a unit change in the time remaining until a light change. ω_0 satisfies $q(\omega_0) = q(F)$. Two important facts are evident in the figure:

1. As ω gets smaller informed drivers are more likely to be aggressive - $q(\omega)$ is decreasing.

2. The sensitivity of a driver's behavior to knowledge of ω, as measured by $q(\omega) - q(F)$, is largest when the information is that he has less time than he expected.

Taken together, the facts imply that the presence of countdown timers raises the propensity for collision. Driver behavior is most responsive to information about the state of the world, ω, precisely at the states of the world where they are most aggressive. As such, on average, collisions are more likely when a countdown timer is present at the intersection.

Figure III: Equilibrium Probability of Aggression with and without Knowledge of ω



2 Robustness

The outcome variable in our study is the number of collisions, which is of course a non-negative integer, or more specifically, a count variable. As such, one may argue that count data methods are more appropriate than Ordinary Least Squares with Fixed Effects (OLS FE) for estimating the effect of interest in our study. Unlike the case of OLS, the estimating equation in the count data context is non-linear in nature. Thus, while the fixed effects are simply differenced away in the OLS FE case, they can not simply be differenced away in these other non linear frameworks. Estimating the fixed effects may introduce an incidental parameters problem and lead to unreliable estimates of the effect of interest. Not including fixed effects is not a viable option either, as a key part of our identification strategy is being able to control for permanent, unobserved differences across intersections.

Hausman et al. [1984] propose a method for estimating count models while allowing for permanent unobserved heterogeneity. They show that under certain assumptions about the structure of the econometric error, conditioning on the sum (over time) of the outcome for each intersection delivers a likelihood function that does not depend on intersection fixed effects. An implication of the Hausman et al. [1984] approach, however, is that it excludes intersections where there were no collisions. Because the number of collisions is a non-negative integer, if the sum over time is zero then the count in any particular month must also be zero. It follows that the collision probability in any particular month is zero and, consequently, that observations from these intersections cannot contribute to the likelihood function. This is a serious issue in our context for two reasons. First, the intersections that are dropped are *not* randomly dropped. Put differently, intersections where countdowns were installed and where there were no collisions are informative for accurately estimating average treatment effects. Restricting the analysis to the selected sample where at least one collision occurs yields an overestimate of the average effect. The second reason is that there is a substantial loss in statistical power when we limit the analysis to the selected sample. The loss of statistical power is particularly important in the present context because we are studying the effects of countdown timers on a rare event (collisions). With rare events, one requires an abundance of data in order to detect seemingly small yet statistically significant effects. As the [Hausman et al., 1984] method excludes 819 intersections, or approximately 46 percent of the intersections in the sample, the loss of power in our context is particularly severe.

Table I illustrates the problems that come from restricting the sample to a non-random sample of intersections experiencing at least one collision. Column 4 of Table I presents the marginal effect from a Poisson regression (employing the Hausman et al. [1984] method) of collisions on the introduction of a countdown. The marginal effect implies the increase in collisions for the average intersection in the restricted sample was 17 percent. Unsurprisingly, this effect is much larger than the 5 percent estimate implied by OLS fixed estimates that use a representative sample (Column 1). It is also unsurprising that the estimate is statistically insignificant - there is substantial loss in statistical power that comes from basing the estimate on the restricted sample of 975 intersections rather than on the representative sample of 1794 intersections.

Column 2 of Table I presents OLS FE estimates of the effect of countdowns for the restricted sample the Hausman et al. [1984] method uses. The estimate further illustrates the effects of selection and a loss in statistical power - it shows that this effect is not specific to the Hausman et al. [1984] method. At the same time the coefficient estimate is more conservative than the Poisson fixed effects estimate of Column 4. The estimate implies the introduction of a countdown resulted in 0.019 more accidents at the average intersection, where the estimate is statistically significant at the 10 percent level. As with the Poisson estimate, the estimate of column 2 is much larger than the estimate of Column 1. It is also more imprecisely estimated, as its standard error is more than double the standard error of Column 1. The estimate is more conservative than the Poisson estimate, as it translates into an 8.3 percent increase (rather than a 17 percent increase) in the number of collisions after a countdown was introduced.

Ideally, to deal with the fact that collisions are rare, one would like to apply "zero-inflated" count methods to the full sample [Cameron and Trivedi, 2005]. Zero-inflated methods account for rare events by explicitly modeling a separate data generating process for the zeros in the sample. Unfortunately, these methods not yet developed to the point where they can account for fixed effects. This is a problem because, as the OLS FE estimates for the full sample reveal, accounting for the fixed effects is crucial for properly evaluating the effects of countdown timers. It shows, specifically, the importance of using the substantial within-intersection variation in identifying a treatment effect.

Since there is no well-developed statistical framework for accounting for both fixed effects and counts with excess zeros, we provide a back-of-the-envelope Poisson estimate that allows us to keep all of intersections in the data while, at the same time, accounting for fixed effects.

Table I: Poisson with Fixed Effects, Selection, and a Loss in Statistical Power

	OLS FE on Representative Sample	OLS FE on Selected Sample	Back-of-the Envelope Poisson FE on Representative Sample	Poisson FE on Selected Sample
Pedestrian Countdown Signal Activated	0.012** (0.006)	0.019* (0.012)	0.012** (0.006)	0.040 (0.040)
Implied Percentage Change in Collisions	5.2	8.3	5.2	17.5
Intersections	1794	975	1794	975
Observations	107640	58500	107640	58500

1. The dependent variable is number of collisions.
2. Robust Standard Errors clustered at the intersection level, *** for $p < .01$, ** for $.01 < p < .05$, * for $p < .1$.
3. Regressions include intersection and month-year fixed effects.

The approach proceeds in two-steps. In the first step we use OLS to "partial out" the intersection and time fixed effects from our treatment variable, by regressing the treatment variable on time and intersection fixed effects and retaining the residual. In the second step we simply run a standard Poisson regression of accident count on this residual.[5] We do not need to consider fixed effects in this second stage because the treatment residual is by definition orthogonal to the fixed effects. It is the part of the treatment not explained by time and intersection fixed effects. This back-of-the-envelope estimate is found in Column 3 of Table I. Note the striking similarity to the result in Column 1, where the OLS FE estimator (for the full sample) is presented. The average partial effect is 0.012, an estimate which implies a 5.2 percent increase in collisions at a representative intersection after the countdown timer is introduced. The average partial effect, its statistical significance, and implied percentage change are all similar to the one we obtain with OLS FE on the full sample.

While the lack of a feasible count data method is disappointing in our context, OLS estimation of econometric models with a non-negative integer outcome is not necessarily problematic in terms of obtaining consistent estimates of the effect of interest. OLS regression applied to a model with a binary outcome (the Linear Probability Model) "produces consistent and even unbiased estimators" of the regression coefficients [Wooldridge, 2010, pg. 562] and in the case of models where the outcome is a count, OLS may "provide good estimates of average partial effects (APE's) on the conditional mean." [Wooldridge, 2010, pg. 723] The real drawback of applying OLS to econometric models with a non-negative integer

[5]Note that if we were to consider simple OLS regression of accident count on the residual in the second stage, by the Frisch-Waugh Theorem we would simply obtain the same estimate as in Column 1 (the main result of the paper) [Angrist and Pischke, 2009, pg. 35].

Table II: Collisions and Countdowns with Bootstrapped Standard Errors

	(1)	(2)	(3)	(4)
Pedestrian Countdown Signal Activated	-0.055*** (0.005)	-0.022*** (0.004)	0.012*** (0.004)	0.011** (0.004)
Controls				
Intersection	N	Y	Y	Y
Month-Year	N	N	Y	Y
Lagged Collisions	N	N	N	Y
Intersections	1794	1794	1794	1794
Observations	107640	107640	107640	105846

1. The dependent variable is number of collisions.
2. Bootstrap Standard Errors (based on 10 replications) in parentheses,
*** for $p < .01$, ** for $.01 < p < .05$, * for $p < .1$.

outcome is that the usual OLS test statistics do not apply. Essentially, the count nature of the outcome implies that the statistical error is not normally distributed. However, normality of the error term in a regression is not a prerequisite for consistency or unbiasedness of the OLS estimator. Normality of the error term is typically assumed in when using OLS in order to ensure that the test statistic has a t-distribution. In other words, if the errors of the regression model are not normally distributed, it is not necessarily the case that the usual critical values apply. That is, the count nature of the outcome is a problem for inference, not identification.

We tackle the inference issue in a couple ways. First, we consider the most common alternative method for approximating standard errors, bootstrapping. The validity of bootstrapped standard errors and the resulting test statistics does not rely on the assumption of normality of the regression model error.

Table II presents the coefficient estimates from our main table (Table 2 in the paper) with bootstrap standard errors in place of the usual standard errors. The results show that bootstrapping the standard errors leads to more precise estimates of the effect of countdowns on accidents. Specifically, Column 3 of Table II shows the p-value falls from a value less than 0.05 to a value less than 0.01. Thus the main result of the paper is robust to relaxing the assumption of a normal error.

Second, we consider a simple transformation of our outcome variable to a continuous measure. We redefine the outcome to be the ratio of number of accidents to total traffic flow through an intersection. We thus consider the effect of countdowns on collisions *per-capita*, or the rate of collisions, a smooth variable. In principle, the collision rate can equal any positive real number. As such, it is more likely that the error in a regression model follows a normal distribution than when the dependent variable is a (discrete) count. Fixed effect estimates with the accident rate as the dependent variable are found in Table III. The estimates show our main results are robust to our continuous transformation of the dependent variable.

Table III: The Effect of Countdowns on Collision Rates

	(1)	(2)	(3)	(4)
Pedestrian Countdown Signal Activated	-0.004** (0.001)	-0.002** (0.0004)	0.00093* (0.00054)	0.00095* (0.00056)
Controls				
Intersection	N	Y	Y	Y
Month-Year	N	N	Y	Y
Lagged Collisions	N	N	N	Y
Intersections	1794	1794	1794	1794
Observations	106393	106393	106393	105552

1. The dependent variable is number of collisions.
2. Robust Standard Errors clustered at the intersection level, *** for $p < .01$, ** for $.01 < p < .05$, * for $p < .1$.

Finally, we end the robustness section by illustrating that our main result is robust to the inclusion of several lags of the dependent variable. Estimates with two to six lags are found is Table IV. The table shows there were 0.010 more collisions at the average intersection once a countdown was introduced, where the estimates are generally statistically significant at the 10 percent level. As Table IV illustrates, the point estimates and statistical significance are strikingly similar across all five specifications.[6]

[6]Two factors can explain the decline in statistical significance in Column 6. The first is that the statistical power of our estimator falls as we exchange more lags for fewer observations. The decline in statistical power makes it more difficult to detect small but statistically significant effects. The second is that the incidental parameters problem, which arises because autoregressive parameters mechanically depend on intersection fixed effects, is of greater concern as the cross-sectional dimension grows relative to the time-series dimension. This dimensionality problem reduces the chances of obtaining consistent estimates of the countdown's effect.

Table IV: Collisions and Pedestrian Countdown Signals

	(1)	(2)	(3)	(4)	(5)
Pedestrian Countdown Signal Activated	0.010* (0.006)	0.010* (0.006)	0.010* (0.006)	0.010* (0.006)	0.010 (0.006)
Controls					
Collision Lags	Two	Three	Four	Five	Six
Intersections	1794	1794	1794	1794	1794
Observations	104052	102258	100464	98670	96876

1. The dependent variable is number of collisions.
2. Robust Standard Errors clustered at the intersection level, *** for $p < .01$, ** for $.01 < p < .05$, * for $p < .1$.
3. Regressions include intersection and time fixed effects as well as a control for the months since the first installation.

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