

# 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.*

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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.<sup>1</sup> 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 discreet 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.<sup>2</sup> When the theater has few patrons, for example, shouting fire likely ensures the patrons escape the emergency unscathed. The policy choices - paging Inspector 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,

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<sup>1</sup>Many believe the code name is still used by public transit authorities in the United Kingdom. See <http://www.telegraph.co.uk/comment/personal-view/3599228/When-a-voice-calls-Inspector-Sands-terror-is-never-far-away.html> for reports of its use.

<sup>2</sup>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 (<http://www.foxnews.com/story/0,2933,431285,00.html>).

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.<sup>3</sup>

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.<sup>4</sup> As pedestrian countdown signals become more commonplace at intersections of major cities in the US 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 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.<sup>5</sup> 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.<sup>6</sup>

<sup>3</sup>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 to 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.

<sup>4</sup>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 U.S. 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>).

<sup>5</sup>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.

<sup>6</sup>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.

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 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)).<sup>7</sup> We instead focus on the impact of a policy

<sup>7</sup>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.

which increases the information that participants on all sides have about an event that is in their common interest.<sup>8</sup> 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.<sup>9</sup>

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.<sup>10</sup> While the debate focuses 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.<sup>11</sup> As a result, the findings can speak to the appropriateness of policies that disclose information asymmetrically.<sup>12</sup>

### 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

<sup>8</sup>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.

<sup>9</sup>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.

<sup>10</sup>An early summary of the broad debate can be found in (Stiglitz, 2002).

<sup>11</sup>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.

<sup>12</sup>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](http://eprints.jcu.edu.au/19780/1/19780_Goudie_2007.pdf) for more details).

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.<sup>1314</sup> Importantly, the time counted at each crosswalk was unchanged when the countdowns were introduced.

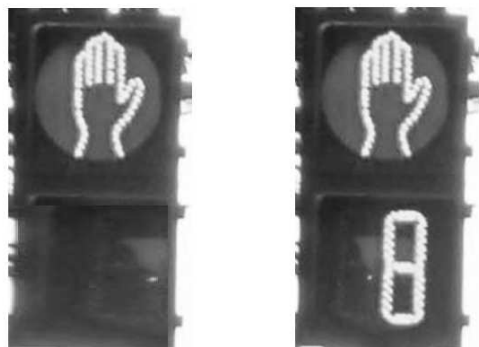


Figure 1.  
Flashing Don’t Walk signal, with and without countdown

### 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.<sup>1516</sup> 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.<sup>17</sup>

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

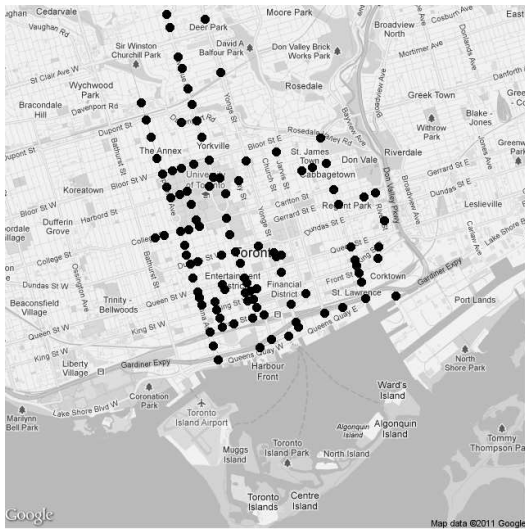
<sup>13</sup>In Toronto, the duration of vehicular signals (green and red lights) is based on the time of day. These durations are based on historical traffic volumes in each direction at different times of the day.

<sup>14</sup>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.

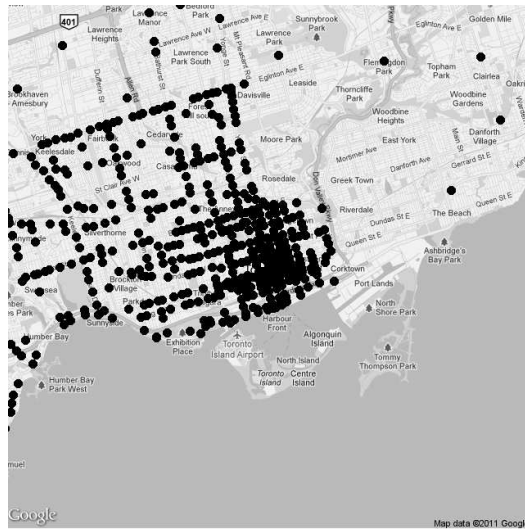
<sup>15</sup>The initiative was actually part of broader program to retrofit all city streetlights with more energy-efficient lamps.

<sup>16</sup>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.

<sup>17</sup>These claims are supported by official city documents. These documents can be found at the city’s website: <http://www.toronto.ca>.



(a) 2006



(b) 2007



(c) 2008

Figure 2.  
Countdown Installations in the City of Toronto

### 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 5-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.

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.<sup>18</sup> 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,<sup>19</sup> 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.<sup>20</sup> Overall, we observe 1794 intersections during a five-year period for a total of 107,640 observations.<sup>21</sup>

Table 1—: Descriptive Statistics - Counts by Year

	Year				
	2004	2005	2006	2007	2008
Collisions	5058	5166	4704	4500	4194
Driver-Pedestrian	266	322	301	296	295
Driver-Cyclist	124	127	136	128	129
Driver-Driver	4250	4185	3897	3740	3407
Fatalities	8	10	10	13	10
Major Injuries	67	95	76	82	63
Minor Injuries	267	244	232	243	212

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 5058 in 2004 to 4194 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:

<sup>18</sup>Collision information is retrospectively based on police reports.

<sup>19</sup>The data classifies fatalities as those persons who die within 366 days of a collision.

<sup>20</sup>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.

<sup>21</sup>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.

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

$y_{it}$  is the number of collisions at intersection  $i$  at time  $t$ .<sup>22</sup> The index  $t$  counts months, starting in January 2004 and ending in December 2008.  $\alpha_i$  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.  $\tau_i$  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.  $\gamma_t$  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.  $\epsilon_{it}$  is a random variable that measures idiosyncratic changes in collisions.

The random variables  $\alpha_i$  and  $\gamma_t$  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  $\alpha_i$  would result in a biased estimate of the treatment effect. On the other hand,  $\gamma_t$  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  $\gamma_t$  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 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

<sup>22</sup>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 Chapter 12 of (Wooldridge, 2010) 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.



trend in collisions<sup>23</sup> 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.<sup>24,25</sup>

Table 2—: Collisions and Pedestrian Countdown Signals

	(1)	(2)	(3)	(4)
Pedestrian Countdown Signal Activated	-0.055*** (0.006)	-0.022*** (0.004)	0.012** (0.006)	0.011* (0.006)
Controls				
Intersection	N	Y	Y	Y
Month-Year	N	N	Y	Y
Lagged Collisions	N	N	N	Y
$R^2$	0.001	0.003	0.004	0.003
Intersections	1794	1794	1794	1794
Observations	107640	107640	107640	105846

1. The dependent variable is number of collisions.
2. 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.

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.<sup>26</sup>

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 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.<sup>27</sup> The second

<sup>23</sup>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.

<sup>24</sup>In the online appendix, we show that the estimates are robust to many more lags of the dependent variable.

<sup>25</sup>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  $\delta_t$  is unobserved time specific heterogeneity.  $\text{history}_i$  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  $\beta_1$  of  $-0.0008$  with an estimated standard error of 0.0010. The estimate supports the exogeneity of countdown assignment to historical accident patterns.

<sup>26</sup>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.

<sup>27</sup>In the online appendix, we show that in the context of a very simple textbook example of interactions between drivers

Table 3—: Collision Involvements and Conditions

	Collisions Involving		
	Driver-Driver	Driver-Pedestrian	Driver-Cyclist
Pedestrian Countdown Signal Activated	0.0117** (0.0052)	-0.0032** (0.0015)	0.0014 (0.0011)
$R^2$	0.004	0.002	0.003
Intersections	1794	1794	1794
Observations	107640	107640	107640

1. Robust Standard Errors clustered at the intersection level.
  2. 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.

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.<sup>28 29</sup> 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.

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- and long-run treatment effects:

(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.

<sup>28</sup>A driver is tailgating if they were reported as following another driver too closely.

<sup>29</sup>Tailgating is widely considered the model of aggressive behavior, and much effort, both by way of government policy and non-government initiatives, has gone into reducing tailgating among drivers. Examples of these efforts can be found at <http://www.stopandgo.org/research/aggressive/tasca.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 Signal Activated	0.0001 (0.0002)	0.0074** (0.0020)
$R^2$	0.0005	0.0038
Intersections	1794	1794
Observations	107640	107640

1. Robust Standard Errors clustered at the intersection level.

2. 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.

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

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<sup>30</sup> - the leads describe the collision trajectory before a countdown installation.<sup>31</sup>

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.

The evidence fails to support the hypothesis that countdown signals distracted drivers. The

<sup>30</sup>Formally, the leads are  $I(t = \tau_i - 1), I(t = \tau_i - 2), \dots, I(t = \tau_i - s)$  for some  $s \geq 1$ .

<sup>31</sup>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

	(1)	(2)	(3)	(4)	(5)
Months after installation					
0 months	0.015 (0.012)	0.017 (0.012)	0.019 (0.012)	0.018 (0.012)	0.023* (0.012)
1 months	0.005 (0.013)	0.007 (0.013)	0.009 (0.013)	0.009 (0.013)	0.013 (0.013)
2 months	-0.012 (0.011)	-0.010 (0.011)	-0.008 (0.011)	-0.008 (0.011)	-0.004 (0.011)
3 months	0.016 (0.012)	0.018 (0.012)	0.020* (0.012)	0.020 (0.012)	0.024* (0.012)
4 months	0.012 (0.013)	0.014 (0.013)	0.017 (0.013)	0.016 (0.014)	0.020 (0.014)
5 months	0.002 (0.013)	0.005 (0.013)	0.007 (0.013)	0.007 (0.013)	0.011 (0.014)
6 months		0.019 (0.014)	0.021 (0.014)	0.021 (0.014)	0.025* (0.015)
7 months			0.034** (0.015)	0.033** (0.015)	0.038** (0.016)
8 months				0.038** (0.007)	0.043** (0.017)
9 months					0.029* (0.017)
After last month in specification	0.029*** (0.008)	0.034*** (0.009)	0.038*** (0.010)	0.037*** (0.010)	0.045*** (0.011)
Intersections	1794	1794	1794	1794	1794
Observations	107640	107640	107640	107640	107640
<i>p</i> -value for <i>F</i> -test that leads don't matter	0.15	0.19	0.22	0.30	0.21

1. The dependent variable is number of collisions.
  2. Robust Standard Errors clustered at the intersection level.
  3. 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.

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.<sup>32</sup>

<sup>32</sup>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.

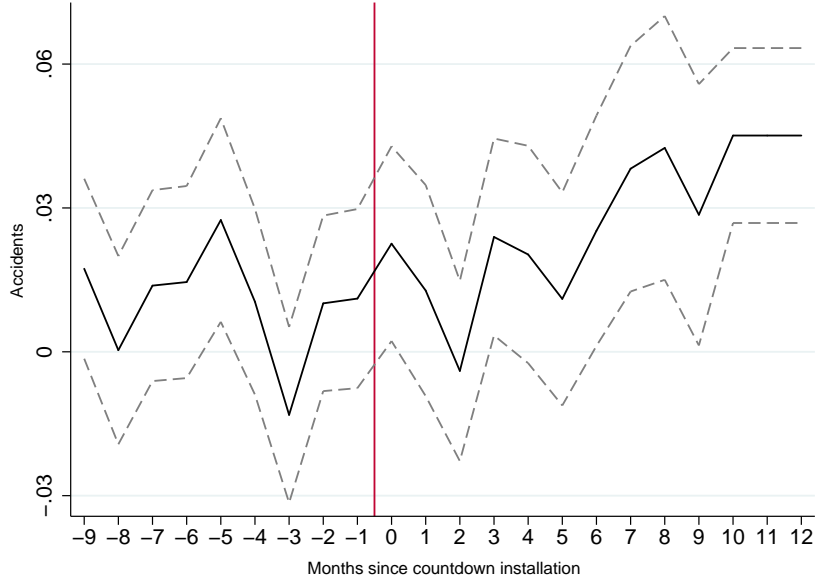


Figure 3.  
Pedestrian countdown signals and their effects on collisions

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 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.<sup>33</sup> 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 manoeuver 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

<sup>33</sup>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 6—: Third Party Effects

	Collisions where driver		
	Turns Left	Turns Right	Not Turning
Pedestrian Countdown Signal Activated	0.0023 (0.0027)	0.0024** (0.0012)	0.0075* (0.0041)
$R^2$	0.0019	0.0010	0.0028
Intersections	1794	1794	1794
Observations	107640	107640	107640

1. Robust Standard Errors clustered at the intersection level.
2. 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.

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.

#### A. Injuries and Rear Ends

Columns (1)-(5) of Table 7 suggests 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 an 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.

Table 7—: Collision Types and Injuries

	Impact Type					Injury Type		
	Entering	Angle	Rear End	Sideswipe	Turning Movement	Fatalities	Major	Minor
Countdown Activated	0.0003 (0.0007)	0.0004 (0.0022)	0.0108*** (0.0032)	-0.0009 (0.0015)	0.0021 (0.0030)	-0.0003 (0.0003)	0.0006 (0.0009)	-0.0027* (0.0016)
$R^2$	0.0010	0.0007	0.0023	0.0011	0.0019	0.0005	0.0005	0.0007
Intersections	1794	1794	1794	1794	1794	1794	1794	1794
Observations	107640	107640	107640	107640	107640	107640	107640	107640

1. Robust Standard Errors clustered at the intersection level.

2. 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.

## 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.<sup>34</sup> 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. 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.<sup>35</sup> In both cases, we estimate specifications of the form:

$$(3) \quad V_{it} = \delta_0 + \delta_1 T_{it} + \mathbf{X}_{it} \boldsymbol{\pi} + v_{it}$$

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.<sup>36</sup>

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 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.”<sup>37</sup> 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

<sup>34</sup>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 [toronto.ca/open](http://toronto.ca/open).

<sup>35</sup>These counts were done for most of the intersections in our study.

<sup>36</sup>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.

<sup>37</sup><http://www.transportation.alberta.ca/>

Table 8—: Countdowns and Pedestrian Flow

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Countdown Activated	1030.50 *** (129.96)	1017.06*** (130.02)	992.32*** (127.89)	1812.81*** (293.55)	1281.43*** (274.54)	137.48 (260.47)	-228.44 (669.40)
Controls							
Day of Week	N	Y	Y	Y	Y	Y	Y
Month	N	N	Y	Y	Y	Y	Y
Year	N	N	N	Y	Y	Y	Y
N-S/E-W	N	N	N	N	Y	Y	Y
Main Street	N	N	N	N	N	Y	Y
Side Street	N	N	N	N	N	N	Y
$R^2$	0.02	0.02	0.08	0.11	0.17	0.52	0.81
Intersections (Observations)	1912	1912	1912	1912	1912	1912	1912

1. The dependent variable is volume of pedestrians using the intersection over an 8-hour period.
2. Robust Standard Errors.
- \*\*\* Significant at the 1 percent level.
- \*\* Significant at the 5 percent level.
- \* Significant at the 10 percent level.

number of automobiles per 24-hour period at the average intersection.<sup>38</sup> 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.<sup>39</sup>

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:

<sup>38</sup>One caveat with this result is that with this data intersections are only observed with countdowns 6% of the time. However, it’s likely that the number of observations more than compensates for the loss in statistical power this asymmetry generates.

<sup>39</sup>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).



Table 9—: Countdowns and Automobile Flow

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Countdown Activated	-1079.74 *** (389.78)	-1097.60*** (363.41)	-996.12*** (362.41)	1479.03*** (459.51)	1456.83*** (471.90)	-655.97* (340.79)	-346.34 (224.89)
Controls							
Day of Week	N	Y	Y	Y	Y	Y	Y
Month	N	N	Y	Y	Y	Y	Y
Year	N	N	N	Y	Y	Y	Y
N-S/E-W	N	N	N	N	Y	Y	Y
Street1	N	N	N	N	N	Y	Y
Street2	N	N	N	N	N	N	Y
$R^2$	0.002	0.008	0.05	0.08	0.08	0.65	0.83
Observations	28996	28996	28996	28996	28996	28996	28996
Intersections	1637	1637	1637	1637	1637	1637	1637

1. The dependent variable is volume of automobiles using the intersection over a 24-hour period.

2. Robust Standard Errors.

\*\*\* Significant at the 1 percent level.

\*\* Significant at the 5 percent level.

\* Significant at the 10 percent level.

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

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  $i$ .<sup>40</sup>  $Z_i$  is then the number of collisions per 1000 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),<sup>41</sup> the estimate in Column 3 shows there are 0.036 more collisions following the introduction of a countdown signal, 3 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.<sup>42</sup>

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

<sup>40</sup>For some intersections we observe volume more than once. In these cases, we use the average over the number of observations.

<sup>41</sup>Not surprisingly (given that collisions are relatively infrequent) the distribution of  $Z_i$  is very skewed to the right.

<sup>42</sup>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*** (0.012)	0.008*** (0.003)	0.036*** (0.006)	0.032*** (0.006)
Interaction with Accident History	0.353*** (0.017)	-0.069*** (0.011)	-0.070*** (0.011)	-0.065*** (0.011)
<b>Controls</b>				
Intersection	N	Y	Y	Y
Month-Year	N	N	Y	Y
Lagged Collisions	N	N	N	Y
$R^2$	0.002	0.002	0.006	0.006
Intersections	1692	1692	1692	1692
Observations	101520	101520	101520	99828

1. The dependent variable is number of collisions.
2. 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.

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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