Traffic Congestion and Infant Health: Evidence from E-ZPass*

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Abstract: This paper provides evidence of the significant negative health externalities of traffic congestion. We exploit the introduction of electronic toll collection, or E-ZPass, which greatly reduced traffic congestion and emissions from motor vehicles in the vicinity of highway toll plazas. Specifically, we compare infants born to mothers living near toll plazas to infants born to mothers living near busy roadways but away from toll plazas with the idea that mothers living away from toll plazas did not experience significant reductions in local traffic congestion. We also examine differences in the health of infants born to the same mother, but who differ in terms of whether or not they were "exposed" to E-ZPass. We find that reductions in traffic congestion generated by E-ZPass reduced the incidence of prematurity and low birth weight among mothers within 2km of a toll plaza by 6.7-9.1% and 8.5-11.3% respectively, with larger effects for African-Americans, smokers, and those very close to toll plazas. There were no immediate changes in the characteristics of mothers or in housing prices in the vicinity of toll plazas that could explain these changes, and the results are robust to many changes in specification. The results suggest that traffic congestion is a significant contributor to poor health in affected infants. Estimates of the costs of traffic congestion should account for these important health externalities.

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Motor vehicles are a major source of air pollution. Nationally they are responsible for over 50% of carbon monoxide (CO), 34 percent of nitrogen oxide (NO_2) and over 29 percent of hydrocarbon emissions in addition to as much as 10 percent of fine particulate matter emissions (Ernst et al., 2003). In urban areas, vehicles are the dominant source of these emissions. Furthermore, between 1980 and 2003 total vehicle miles traveled (VMT) in urban areas in the United States increased by 111% against an increase in urban lane-miles of only 51% (Bureau of Transportation Statistics, 2004). As a result, traffic congestion has steadily increased across the United States, causing 3.7 billion hours of delay by 2003 and wasting 2.3 billion gallons of motor fuel (Schrank and Lomax, 2005). Traditional estimates of the cost of congestion typically include delay costs (Vickrey, 1969), but they rarely address other congestion externalities such as the health effects of congestion.

This paper seeks to provide estimates of the health effects of traffic congestion by examining the effect of a policy change that caused a sharp drop in congestion (and therefore in the level of local motor vehicle emissions) within a relatively short time frame at different sites across the northeastern United States. Engineering studies suggest that the introduction of electronic toll collection (ETC) technology, called E-ZPass in the Northeast, sharply reduced delays at toll plazas and pollution caused by idling, decelerating, and accelerating. We study the effect of E-ZPass, and thus the sharp reductions in local traffic congestion, on the health of infants born to mothers living near toll plazas.

This question is of interest for three reasons. First, there is increasing evidence of the long-term effects of poor health at birth on future outcomes. For example, low birth weight has been linked to future health problems and lower educational attainment (see Currie (2009) for a summary of this research). The debate over the costs and benefits of emission controls and traffic congestion policies could be significantly impacted by evidence that traffic congestion has a deleterious effect on fetal health. Second, the study of newborns overcomes several difficulties in making the connection between pollution and health because, unlike adult diseases that may reflect pollution exposure that occurred many years ago, the link between cause and effect is immediate. Third, E-ZPass is an interesting policy experiment because, while pollution control was an important consideration for policy makers, the main motive for consumers to sign up for E-ZPass is to reduce travel time. Hence, E-ZPass offers an example of achieving reductions in pollution by bundling emissions reductions with something consumers perhaps value more highly such as reduced travel time.

Our analysis improves upon much of the previous research linking air pollution to fetal health as well as on the somewhat smaller literature focusing specifically on the relationship between residential proximity to busy roadways and poor pregnancy outcomes. Since air pollution is not randomly assigned, studies that attempt to compare health outcomes for populations exposed to differing pollution levels may not be adequately controlling for confounding determinants of health. Since air quality is capitalized into housing prices (see Chay and Greenstone, 2003) families with higher incomes or preferences for cleaner air are likely to sort into locations with better air quality, and failure to account for this sorting will lead to overestimates of the effects of pollution. Alternatively, pollution levels are higher in urban areas where there are often more educated individuals with better access to health care, which can cause underestimates of the true effects of pollution on health.

In the absence of a randomized trial, we exploit a policy change that created large local and persistent reductions in traffic congestion and traffic related air emissions for certain segments along a highway. We compare the infant health outcomes of those living near an electronic toll plaza before and after implementation of E-ZPass to those living near a major highway but further away from a toll plaza. Specifically, we compare mothers within 2 kilometers of a toll plaza to mothers who are between 2 and 10 km from a toll plaza but still within 3 kilometers of a major highway before and after the adoption of E-ZPass in New Jersey and Pennsylvania.

New Jersey and Pennsylvania provide a compelling setting for our particular research design. First, both New Jersey and Pennsylvania are heavily populated, with New Jersey being the most densely populated state in the United States and Pennsylvania being the sixth most populous state in the country. As a result, these two states have some of the busiest interstate systems in the country, systems that also happen to be densely surrounded by residential housing. Furthermore, we know the exact addresses of mothers, in contrast to many observational studies which approximate the individual's location as the centroid of a geographic area or by computing average pollution levels within the geographic area. This information enables us to improve on the assignment of pollution exposure. Lastly, E-ZPass adoption and take up was extremely quick, and the reductions in congestion spillover to all automobiles, not just those registered with E-ZPass (New Jersey Transit Authority, 2001).

Our difference-in-differences research design relies on the assumption that the characteristics of mothers near a toll plaza change over time in a way that is comparable to those of other mothers who live further away from a plaza but still close to a major highway. We test this assumption by examining the way that observable characteristics of the two groups of mothers and housing prices change before and after E-ZPass adoption. We also estimate a range of alternative specifications in an effort to control for unobserved characteristics of mothers and neighborhoods that could confound our estimates.

We find significant effects on infant health. The difference-in-difference models suggest that prematurity fell by 6.7-9.16% among mothers within 2km of a toll plaza, while the incidence of low birth weight fell by 8.5-11.3%. We argue that these are large but not implausible effects given previous studies. In contrast, we find that there are no significant effects of E-ZPass adoption on the demographic characteristics of mothers in the vicinity of a toll plaza. We also find no immediate effect on housing prices, suggesting that the composition of women giving birth near toll plazas shows little change in the immediate aftermath of E-ZPass adoption (though of course it might change more over time).

The rest of the paper is laid out as follows: Section I provides necessary background. Section II describes our methods, while data are described in Section III. Section IV presents our results. Section VI discusses the magnitude of the effects we find, and Section V details our conclusions.

I. Background

Many studies suggest an association between air pollution and fetal health.[1] Mattison et al. (2003) and Glinianaia et al. (2004) summarize much of the literature. For more recent papers see for example Currie et al. (2009); Dugandzic et al. (2006); Huynh et al. (2006); Karr et al. (2009); Lee et al. (2008); Leem et al. (2006); Liu et al. (2007); Parker et al. (2005); Salam et al. (2005); Ritz et al. (2006); Wilhelm and Ritz (2005); Woodruff et al. (2008). Since traffic is a major contributor to air pollution, several studies have focused specifically on the effects of exposure to motor vehicle exhaust (see Wilhelm and Ritz (2003); Ponce et al. (2005); Brauer et

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[1] There is also a large literature linking air pollution and child health, some of it focusing on the effects of traffic on child health. See Schwartz (2004) and Glinianaia et al. (2004b) for reviews.

al. (2008); Slama et al. (2007); Beatty and Shimshack (2009); Knittel, Miller, and Sanders (2009)).

At the same time, researchers have documented many differences between people who are exposed to high volumes of traffic and others (Gunier et al, 2003). A correlational study cannot demonstrate that the effect of pollution is causal. Women living close to busy roadways are more likely to have other characteristics that are linked to poor pregnancy outcomes such as lower income, education, and probabilities of being married, and a higher probability of being a teen mother. This is partly because wealthier people are more likely to move away from pollution. Depro and Timmins (2008) show that gains in wealth from appreciating housing values during the 1990s allowed households in San Francisco to move to cleaner areas. Banzhaf and Walsh (2008) show that neighborhoods experiencing improvements in environmental quality tend to gain population while the converse is also true.

Most previous studies include a minimal set of controls for potential confounders. Families with higher incomes or greater preferences for cleaner air may be more likely to sort into neighborhoods with better air quality. These families are also likely to provide other investments in their children, so that fetuses exposed to lower levels of pollution also receive more family inputs, such as better quality prenatal care or less maternal stress. If these factors are unaccounted for, then the estimated effects of pollution may be biased upwards. Alternatively, emission sources tend to be located in urban areas, and individuals in urban areas may be more educated and have better access to health care, factors that may improve health. Omitting these factors would lead to a downward bias in the estimated effects of pollution, suggesting that the overall direction of bias from confounding is unclear.

Several previous studies are especially relevant to our work because they address the problem of omitted confounders by focusing on "natural experiments." Chay and Greenstone [2003a,b] examine the implementation of the Clean Air Act of 1970 and the recession of the early 1980s. Both events induced sharper reductions in particulates in some counties than in others, and they use this exogenous variation in pollution at the county-year level to identify its effects. They estimate that a one unit decline in particulates caused by the implementation of the Clean Air Act (or by recession) led to between five and eight (four and seven) fewer infant deaths per 100,000 live births. They also find some evidence that declines in Total Suspended Particles (TSPs) led to reductions in the incidence of low birth weight. However, the levels of particulates studied by Chay and Greenstone are much higher than those prevalent today; for example, PM10 levels have fallen by nearly 50 percent from 1980 to 2000. Furthermore, only TSPs were measured during the time period they examine, which precludes the examination of other pollutants that are found in motor vehicle exhaust.

Other studies that are similar in spirit include a sequence of papers by Pope and his collaborators, who investigated the health effects of the temporary closing of a Utah steel mill (Pope, 1989; Ransom and Pope, 1992; Pope, Schwartz, and Ransom, 1992) and Friedman et al. (2001) who examine the effect of changes in traffic patterns in Atlanta due to the 1996 Olympic games. However, these studies did not look at fetal health. Parker et al. (2008) examine the effect of the Utah steel mill closure on preterm births and find that exposure to pollution from the mill increased the probability of preterm birth. This study however does not speak to the issue of effects of traffic congestion on infant health.

Currie, Neidell, and Schmeider (2008) examine the effects of several pollutants on fetal health in New Jersey using models that include maternal fixed effects to control for potential confounders. They find that CO is particularly implicated in negative birth outcomes. In pregnant women, exposure to CO reduces the availability of oxygen to be transported to the fetus. Carbon monoxide readily crosses the placenta and binds to fetal haemoglobin more readily than to maternal haemoglobin. It is cleared from fetal blood more slowly than from maternal blood, leading to concentrations that may be 10 to 15 percent higher in the fetus's blood than in the mother's. Indeed, much of the negative effect of smoking on infant health is believed to be due to the CO contained in cigarette smoke (World Health Organization, 2000). Hence, a significant effect of E-ZPass on CO alone would be expected to have a significant positive effect on fetal health.

E-ZPass is an electronic toll collection system that allows vehicles equipped with a special windshield-mounted tag to drive through designated toll lanes without stopping to manually pay a toll. The benefits include time saved, reduced fuel consumption, and reductions in harmful emissions caused by idling and acceleration at toll plazas. In addition, the air quality benefits are thought to be large enough that some counties have introduced ETC explicitly in order to meet pollution migitation requirements under the Clean Air Act (Saka et al. 2000).

Engineering estimates of the reduction in pollution with E-ZPass adoption vary. They are typically based on a combination of traffic count data, and measures of the extent to which reducing the idling, deceleration and acceleration around toll plazas would reduce emissions for a given vehicle mix. For example, Saka et al. (2000) compared data on traffic flows through manned toll lanes and electronic toll collection lanes at one toll plaza at a single point in time and estimated that reductions in queuing, decelerations and accelerations in the ETC lanes resulted in reductions of 11% for NO_2 and a decrease of more than 40% for hydrocarbons and CO relative to emissions in the manned lanes. A similar study of the George Washington Bridge toll plaza, one of those included in this study, by Venigalla and Krimmer (2007), estimated that VOC, CO, and NO_2 emissions from trucks were reduced in the E-ZPass lanes by 30.8%, 23.5%, and 5.8%.

Although these studies suggest that E-ZPass could lead to substantial reductions in ambient pollution, these studies may over-estimate or under-estimate the extent of that reduction. For example, if reducing toll plaza delays encourages more people to drive rather than take public transit, then this may offset the reduction in pollution per-vehicle to some extent. Conversely, to the extent that drivers in non E-ZPass lanes also benefit from reduced congestion, comparing delays at E-ZPass and manual lanes will understate the benefits of E-ZPass. We were unable to find a study that measured pollution in the radius of a toll plaza before and after the introduction of ETC.

However, the New Jersey Turnpike Authority commissioned a study of the extent to which E-ZPass reduced total delays at toll plazas (New Jersey Turnpike Authority, 2001). This study used before and after data on traffic counts at each toll plaza, and measured the delays at toll plazas using video cameras. Evidently, the total delay is given by (number of vehicles)*(delay per vehicle). This study concluded that total delay at toll plazas dropped by 85% after the implementation of E-ZPass, saving 1.8 million hours of delay for cars, and 231,000 hours of delay for trucks in the year after adoption. If pollution around the toll plaza is proportional to these delays, then it is reasonable to conclude that it was also reduced considerably. The report estimated that E-ZPass reduced emissions of NO_2 by .056 tons per day, or 20.4 tons per year. In 2002, mobile on-road sources emitted approximately 300 tons of NO_2 per year (New Jersey Department of Environmental Protection, undated). Hence, a crude estimate is that E-ZPass reduced NO_2 emissions from traffic by about 6.8%. Unfortunately, the EPA's air quality monitors are placed throughout the state such that there is only one monitor located near a toll plaza in our study area. Furthermore, this particular monitor only measures NO_2 and SO_2. Nevertheless we show evidence that suggests a sharp decline in NO_2 levels following E-ZPass adoption. This is in contrast to SO_2 levels at the same monitor, for which we see no noticeable decline. This is consistent with the fact that cars produce a large percentage of local NO_2 emissions, while they are responsible for a very small fraction of SO_2 emissions.

An important unresolved question is how far elevated pollution levels extend from highways or toll plazas? Most studies have focused on areas 100 to 500 meters from a roadway. However, Hu et al (2009) find evidence that pollution from the 405 Freeway in Los Angeles is found up to 2,600 meters from the roadway. Moreover, their study was conducted in the hours before sunrise, when traffic volumes are relatively light, but most people are in their homes. We investigate this issue below.

We focus on the implementation of E-ZPass on three major state tollways in New Jersey and Pennsylvania, the Pennsylvania Turnpike, the New Jersey Turnpike, and the Garden State Parkway. Portions of all three of these state highways rank nationally as some of the busiest in the country. In addition to these state tollways, we also use the major bridge and tunnel tolls connecting New Jersey to New York (George Washington Bridge, Lincoln Tunnel, and the Holland Tunnel). Each of these bridges and tunnels are extremely well traveled, transporting around 105 million, 42 million, and 35 million vehicles respectively. New Jersey has 38 toll plazas, 3 at bridge/tunnel entrances to New York City, 11 along the Garden State Parkway, 22 along the New Jersey Turnpike, and 2 along the Atlantic City Expressway. There are 60 toll plazas in Pennsylvania. Figure 1 shows the toll plazas and major highways that we use.

Our research design exploits the fact that E-ZPass was installed at different times and in different locations across the two states. The Port Authority of New York and New Jersey implemented E-ZPass at the bridge and tunnels entering New York City in 1997. Soon after, New Jersey installed its first E-ZPass toll plazas on the Atlantic City Expressway. Starting in December 1999, New Jersey began installing E-ZPass on the Garden State Parkway. Throughout the course of the following year, toll plazas were added at the rate of 1 per month (working from North to South on the GSP), with the final plaza installed in August of 2000. In September 2000, the NJ Turnpike installed E-ZPass at all their toll collection terminals throughout the system. Similarly, the PA Turnpike installed most of their toll-plazas with E-ZPass in December 2000, with a major addition occurring in December of 2001. E-ZPass adoption and take up was extremely rapid. By early 2001 (1 year after implementation of the Garden State Parkway and NJ Turnpike), 1.3 million cars had been registered with E-ZPass in New Jersey.

II. Data

Our main source of data for this study are Vital Statistics Natality records from Pennsylvania for 1997 to 2002 and for New Jersey for the years 1994 to 2003. Vital Statistics records are a very rich source of data that cover all births in the two states. They have both detailed information about health at birth and background information about the mother, including race, education, and marital status. We were able to make use of a confidential version of the data with the mother's address, and we were also able to match births to the same mother over time using information about the mother's name, race, and birth date. Like most previous studies of infant health, we focus on two birth outcomes, prematurity (defined as gestation less than 38 weeks) and low birth weight (defined as birth weight less than 2500 grams).[2]

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[2] Outcomes such as infant deaths and congenital anomalies are much rarer, and when we restrict the data set to those who are within 2km of a toll plaza, there are insufficient cases in our data for us to be able to expect to see an effect.

Using this information, we first divided mothers into three groups: Those living within 2km of a toll plaza; those living within 3km of a major highway, but between 2km and 10km from a toll plaza; and those who lived 10km or more away from a toll plaza. Our treatment group in the difference-in-difference design is the mothers living within 2km of a toll plaza, while the control group is those who live close to a highway, but between 2km and 10km from a toll plaza. We drop mothers who live more than 10km away from a toll plaza. In total, we have 98 toll plazas that adopted electronic tolling in our sample. We also drop births that occurred more than 3 years before or after the E-ZPass conversion of the nearest plaza, in an effort to focus on births that occurred around the changes. All of the mothers in the sample are assigned to their nearest toll plaza, which effectively divides the states into 98 regions. We will include a fixed effect for each toll plaza/region in the model.

Figure 2 illustrates the way that we created the treatment and control groups. As one can see from the figure, there are many homes within the relevant radius of the toll plaza. Moreover, housing tends to follow the highway. The areas more than 2km away from either a toll plaza or the highway are somewhat less dense. We also repeat this procedure using mothers less than 1.5km from a toll plaza as the treatment group, comparing them to mothers who live within 3km of a highway but between 1.5 and 10km from a toll plaza.

In the analysis including mother fixed effects, we select the sample differently. Specifically, we keep only mothers with more than one birth in our data. We then restrict the sample to only mothers who have had at least one child born within 2km of a toll plaza, since only these mothers can help to identify the effects of E-ZPass. (The other mothers could in principal identify some of the other coefficients in the model, but as we show below, they have quite different average characteristics so we prefer to exclude them). We use all available years of sample data, in order to maximize the number of women we observe with two or more children.

We obtained data on housing prices in New Jersey from 1989 to 2009 by submitting an open access records request. In addition to the sales date and price, these data include information about address, square footage, age of structures, whether the unit is a condominium, assessed value of the land, and assessed value of the structures. We will use these data to see if housing prices changed in the neighborhood of toll plazas in response to amenity benefits generated from reduced traffic congestion and increased air quality surrounding E-ZPass implementation.

Means of the outcomes we examine (prematurity and low birth weight) and of the independent variables are shown in Table 1 for all of these groups. Panel A shows means for the treatment and control group used in the difference-in-differences analysis. For the control group, "before" and "after" are assigned on the basis of when the closest toll plaza converted to E-ZPass. The last column of Panel A shows means for mothers who live more than 10km from a toll plaza. They are less likely to have a premature birth, and their babies are less likely to be low birth weight. They are also less likely to be black or Hispanic. These mothers are omitted from our difference-in-difference analysis.

The treatment and control groups are similar to each other before the adoption of E-ZPass except in terms of racial composition: Mothers close to toll plazas are much more likely to be Hispanic and somewhat less likely to be African-American than other mothers. Mothers close to toll plazas are also less likely to have smoked during the pregnancy. These differences have potentially important implications for our analysis, since other things being equal, African

Americans and smokers tend to have worse birth outcomes than others. Hence, it is important to control for these differences, and we will also examine these subgroups separately.

In terms of before and after trends, both areas show increases in the fraction of births to Hispanic and African-American mothers, and decreases in the fraction of births to smokers and teen mothers over time. The fraction of births that were premature rose over time, especially in the control areas. The fraction of births that were low birth weight showed a slight decrease in the treatment area near toll plazas, but an increase in the control areas. These patterns reflect national time trends in the demographic characteristics of new mothers and in birth outcomes. We can use these means tables to do a crude difference in difference comparison. Such a comparison suggests that prematurity and low birth weight fell by about 7% in areas less than 2km from a toll plaza after E-ZPass. Appendix Table 1 shows changes in mean outcomes when the treatment group is restricted to those who were within 1.5km of a toll plaza.

Panel B of Table 1 shows means for the sample that we use in the mother fixed effects analysis. Panel B shows that in general, the mothers with more than one birth in the sample have somewhat better birth outcomes—their children are less likely to be premature or low birth weight than in the full sample of children (Panel A). The sample of women who have more than one birth and who ever had a child within 2 km of a toll plaza changes over time. Comparing columns 1 and 2 shows that over time this population has become more Hispanic, less educated, and somewhat more likely to be having a higher order birth. Columns 3 and 4 of Panel B show that the population of women who never had a birth within 2 km of a plaza are quite different—they are less likely to be Hispanic, the sample tends to gain education over time, and (not surprisingly) lives further from a highway.

Panel C shows means from the housing sales data. All prices were deflated by the CPI into 1993 dollars. Comparing columns 1 and 3 suggests that sales prices were similar in areas close to toll plazas and a little further away from toll plazas before E-ZPass, but that prices increased faster near toll plazas after adoption. The same comparison is shown for the area within 1.5km of a toll plaza and areas 1.5-10km away from toll plazas in Appendix Table 1. We show below that controlling for a fairly minimal set of covariates (month and year of sale, square footage, age of structure, municipality and whether it is a condominium) reduces this estimate to statistical insignificance. Still, the idea that prices may have increased, thereby changing the composition of mothers in the neighborhood provides a motivation for the models we estimate below including mother fixed effects.

Figures 3 to 6 provide more nuanced pictures of the relationship between E-ZPass adoption, birth weight, and prematurity. Figures 3 and 4 focus on mothers within 2km of a toll plaza and take the average values over .1km bins before and after E-ZPass. Figure 3 shows that there is a dramatic reduction in low birth weight after E-ZPass in the area closest to the toll plaza. The reduction tapers off and the lines cross at a little after 1km. Figure 4 shows a similar pattern for prematurity, although here the lines cross at about 1.5 km from the toll plaza.

Figures 5 and 6 compare low birth weight and prematurity in households more than 1.5km from a toll plaza and households less than 1.5km from a toll plaza in the days before and after E-ZPass. These figures indicate a higher incidence of low birth weight in the 500 days prior to E-ZPass adoption in the area near the toll plaza. Around the time of E-ZPass adoption, the incidence of low birth weight near toll plazas begins to decline dramatically, and falls below the control rate soon after adoption. Figure 6 shows increasing rates of prematurity in both mothers near toll plazas and mothers further away. Around the time of E-ZPass adoption, the rate of prematurity begins to fall for the near toll plaza group.

It is noticeable that in both figures, the incidence of poor outcomes begins to decline slightly before the official date of E-ZPass adoption. We believe that this slight discrepancy in the timing may be explained by E-ZPass construction. Prior to the official opening date, each plaza had to be adapted for E-ZPass. The New Jersey E-ZPass contract included the installation of fiber optic communications networks, patron fare displays, E-ZPass toll plaza signs and road stripping at a cost of $500 million (New Jersey Department of Transportation, 1998). In one recent example, the toll plaza for the I-78 Toll Bridge is being upgraded to E-ZPass. Construction is scheduled to take place between early January 2010 and Memorial Day, approximately 5 months.[3] In the meantime, commuters are being advised to use an alternative route so that traffic may be lighter than usual near this plaza (Warren Reporter, 2010).

III. Methods

To implement our difference-in-difference estimator, we begin by testing the assumptions for the estimator to be valid, namely that any trends in the observable characteristics of mothers are the same across both treatment and control groups. The models for these specification checks take the following form:

$$(1)\ Mom\_Char_{it} = a + b_1 E\text{-}ZPass_{it} + b_2 Close_{it} + b_3 Plaza_{it} + b_4\, E\text{-}ZPass{*}Close_{it} + b_5 Year + b_6\, Month + b_7\, Distance_{it} + e_{it},$$

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[3] The construction includes: partial demolition and removal of the canopy over a portion of the toll plaza; new overhead sign structures, construction of a canopy over the new open road tolling lanes to house the ETC array; the construction of a concrete barrier to separate the ETC lanes from the others; restriping; and the construction of electrical systems to support the ETC equipment (Delaware River Joint Toll Bridge Commission, 2009).

where Mom_Char_{it} are indicators for mother i's race or ethnicity, her education, teen motherhood, and whether she smoked during pregnancy t. *E-ZPass* is an indicator equal to one if the closest toll plaza has implemented E-ZPass, $Close_{it}$ is an indicator equal to one if the mother lived within 2km (or 1.5km) of a toll plaza, and $Plaza_{it}$ is a series of indicators for the closest toll plaza. This indicator is designed to capture any unobserved, time-invariant characteristics of each of the 98 toll plaza sample region. The coefficient of interest is that on the interaction between $E\text{-}ZPass_{it}$ and $Close_{it}$. We also include indicators for the year and month to allow for systematic trends, such as the increase in minority mothers. Finally, we control for linear distance from a busy roadway. Standard errors are clustered at the level of the toll plaza, to allow for correlations in the errors of mothers around each plaza. If we saw that maternal characteristics changed in some systematic way following the introduction of E-ZPass, then we would need to take account of this selection when assessing the effects of E-ZPass on health outcomes.

We also estimate models of the effects of E-ZPass on housing prices. These models are similar to (1) above except that they control for whether it is a condominium, age (in categories, including missing), square footage (in categories, including missing), fixed effects for the municipality, and year and month of sale. We have also estimated models that control for the ratio of assessed structure to land values, with similar results.

Our baseline models examining the effects of E-ZPass on the probabilities of low birth weight and prematurity are similar to equation (1). The estimated equation takes the following form:

$$(2)\ Outcome_{it} = a + b_1 E\text{-}ZPass_{it} + b_2 Close_{it} + b_3 Plaza_{it} + b_4\, E\text{-}ZPass_{it} * Close_{it} + b_5 Year + b_6 Month + b_7 X_{it} + b_8\, Distance_{it} + e_{it},$$

where *Outcome* is either prematurity or low birth weight, and the vector X_{it} of mother and child characteristics includes indicators for whether the mother is black or Hispanic; 4 mother education categories (<12, high school, some college, and college or more; missing is the left out category); mother age categories (19-24, 25-24, 35+); an indicator for smoking during pregnancy; indicators for birth order (2nd, 3rd, or 4th or higher order); an indicator for multiple birth; and an indicator for male child. Indicators for missing data on each of these variables were also included. Again, the main coefficient of interest is b_4 which can be interpreted as the difference-in-differences coefficient comparing births that are closer or further from a toll plaza, before and after adoption of E-ZPass.

We perform a series of robustness checks. First, we estimate models that restrict the sample to mothers within 5km of a toll plaza. Second, we include interactions of $Close_{it}$ and a linear time trend. It is possible that areas close to toll plazas are generally evolving in some way that is different from other areas (e.g. racial composition), but as we shall see, this does not seem to affect our estimates. Third, we estimated models of the propensity to live close to a toll plaza to see whether mothers were more or less likely to live near a toll plaza before or after E-ZPass adoption. The propensity models are estimated using all of the maternal and child characteristics listed above, the interactions of these variables, as well as zip code fixed effects.[4] We then excluded all observations with a propensity less than .1 or greater than .9 as suggested by Crump et al. (2009). We estimated separate models for African Americans and non-African Americans since these groups tend to have very different average birth outcomes. We also looked separately at estimates for non-smokers. As we show below, our difference in difference results

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[4] We obtained similar results using models that controlled for county fixed effects instead of zip code fixed effects.

are robust to these changes, though we do find larger effects for African-Americans and for smokers.

The estimates from (2) reflect an average effect of E-ZPass on people anywhere within the 2km (or 1.5km) window. We have also experimented with allowing the effect to vary with distance from the toll plaza. To do this requires that some assumption be made about the rate at which the effects decay with distance from the toll plaza. The engineering literature is not particularly helpful in this respect, since most studies focus on areas very close to roadways. As we show below, the estimates are somewhat sensitive to these assumptions, but are qualitatively consistent with the results from the simple difference-in-difference models.

One possible threat to identification is that new mothers with better predicted birth outcomes could select into areas around toll plazas after E-ZPass is adopted. Although we do not find evidence of changes in the average demographic characteristics of those living near toll plazas after E-ZPass, an arguably better way to control for possible changes in the composition of mothers is to estimate models with mother fixed effects. These models take the following form:

$$(3)\ Outcome_{it} = a_i + b_1 E\text{-}ZPass_{it} + b_2 Close_{it} + b_3 Plaza_{it} + b_4\, E\text{-}ZPass_{it}{*}Close_{it} + b_5 Year + b_7 Month + b_8 Z_{it} + b_9\, Distance_{it} + e_{it},$$

where a_i is a fixed effect for each mother i, and Z is a vector including child gender and birth order and potentially time varying maternal characteristics including mother's age, education, and an indicator for smoking. Although all the mothers are selected to have had at least one child while residing within 2km of a toll plaza, we alternatively define the indicator for *Close* either as less than 2km from a toll plaza, or as less than 1.5km from a toll plaza.[5]

IV. Results

Table 2 shows the results of estimating equation (1), the effects of E-ZPass on the characteristics of mothers who live near toll plazas and on housing prices. Each coefficient represents an estimate of b_4 from a separate regression. The only maternal characteristic to show any significant changes with E-ZPass adoption is smoking, where it is estimated that E-ZPass has a positive effect. Note that if more smokers move to areas after E-ZPass adoption (or if mothers smoke more) this will tend to work against finding any net benefit of E-ZPass on birth outcomes. The last column shows that there is no immediate significant effect on housing prices (although the coefficient is positive), suggesting that it takes time for any effects through the housing market to be felt. These results suggest that the estimated health effects of E-ZPass are not due to changes in the composition of mothers who live close to toll plazas.

Table 3 shows our estimates of (2). Again, each coefficient is an estimate of b_4 from a separate regression. The first and third columns show a model that controls only for month and year of birth, toll plaza fixed effects, and distance to highway. These estimates are somewhat

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[5] One difficulty with the interpretation of these models is that they are identified primarily from movers (there are few mothers with two or more births, both within 2km of a toll plaza). This would be a problem if we thought that women systematically moved closer to toll plazas when their circumstances improved, and that improved circumstances led to better birth outcomes. The birth certificates do not record income, but marital status is likely to be correlated with maternal wellbeing and does change over time. We have estimated placebo models similar to (3) using an indicator for married as the dependent variable, and find a negative coefficient on the interaction of close*E-ZPass which is not statistically significant suggesting. This suggests that if anything, women are less likely rather than more likely to be married when they live near toll plazas post E-ZPass so that any bias due to movers probably causes an underestimate of the effects of E-ZPass in the mother fixed effects models.

higher than the raw difference-in-difference estimates implied by Table 1, suggesting that it is important to control for time trends and regional differences. The second and fourth columns add maternal characteristics as in equation (2). Assuming our research design is valid, adding controls for the mother's characteristics should only reduce the sampling variance while leaving the coefficient estimates unchanged. The results in columns (2) and (4), are consistent with the validity of the research design, since adding maternal characteristics has little impact on the estimated coefficients. These estimates suggest that E-ZPass adoption reduced prematurity by 8.6 percentage points. This suggests that in the 29,677 births that we observe within 2km of a toll plaza after E-ZPass, 255 preterm births were averted. A similar calculation indicates that E-ZPass reduced the incidence of low birth weight by 9.3 percentage points, which means that in our sample 275 low birth weight births were averted (of course many of these births overlap since most preterm infants are low birth weight).

Panel 2 of Table 3 shows that the estimates are not generally significantly different when we define "close" as 1.5km from a toll plaza. The point estimates are somewhat higher for prematurity, and somewhat lower for low birth weight. In what follows we focus on models using the 2km cutoff and explore the robustness of our results.

The first panel of Table 4 shows the effect of restricting the sample to mothers within 5km of a toll plaza only. This cuts our sample size by about 40%. Still, the standard errors are quite similar to those shown in the comparable columns of Table 3 although the point estimates are somewhat reduced. In this specification, there is a 6.7% reduction in prematurity and an 8.5% reduction in low birth weight. Panel 2 shows the results of adding interactions between $Close_{it}$ and a linear time trend to the model. These interactions capture any differences in the evolution of areas near toll plazas and other areas (such as, perhaps, different trends in demographic characteristics or in housing markets). Adding these time trends again lowers the estimates somewhat from those in Table 3, to 7.4 percentage points for prematurity and 8.4 percentage points for low birth weight. Similarly, the propensity-score trimmed estimates shown in Panel 3 of Table 4, are a little smaller than those in Table 3 (7.9 and 8.6 percentage points for prematurity and low birth weight respectively).

The remaining panels of Table 4 focus on some important subgroups. Panels 4 and 5 estimate separate models for African-Americans and all others. These estimates suggest that effects are much larger for African-Americans. Since these mothers are twice as likely to have small and/or premature babies, it is possible that similar reductions in gestation and birth weight are more likely to push African-American babies below the thresholds for concern. Alternatively, it is possible that African-American mothers are at a different point on the production possibility frontier, so that a similar exposure to pollution has a larger effect. In results not reported in the table, we compared the estimated effects on a continuous measure of birth weight for African-Americans and others and again found much larger effects for the former.

Panel 6 examines the effects for non-smokers. These are slightly smaller than the effects estimated in Table 3 (7.5 compared to 8.6 percentage point reduction in prematurity 7.9 compared to 9.3 percentage point reduction in low birth weight) suggesting that pollution from motor vehicles is more damaging for children of smokers. This result is consistent with Currie, Neidell, and Schmeider (2009).

Table 5 shows estimates in which we allow the effect of distance to vary within a 2km radius of the toll plaza. As discussed above, these specifications require assumptions about the form of the decay in the effects of E-ZPass. Table 6 compares two models. The first, shown in columns one and three, assumes that the decay in effects is linear and dies out completely after 2km. When we use this specification, the estimated effects of E-ZPass are negative, but relatively small and not precisely estimated. However, if the form of the decay is not in fact linear, then we can expect the imposition of linearity to bias the estimated coefficient towards zero. An alternative specification that conforms more closely to the pattern shown in Figures 3 and 4 assumes that the effects decay exponentially with distance from the toll plaza. Columns 3 and 4 show that imposing this assumption (specifically, interacting "after E-ZPass" with 1/(e**distance)) results in much larger point coefficients, although the coefficient on prematurity is significant only at the 90% level of confidence. This coefficient (of -.0153) implies, for example, that prematurity falls by 15.3 percentage points at 0km, 9.3 at .5km , 5.6 at 1km and 3.4 at 1.5km.

Table 6 shows estimates of (3) that include mother fixed effects. Panel A defines *Close* as less than 2km from a toll plaza while Panel B defines *Close* as less than 1.5km from a toll plaza. These estimates are significantly negative, suggesting that the effects we find in the difference-in-difference specification are not driven primarily by changes in unobservable fixed characteristics of mothers in the neighborhood of toll plazas after E-ZPass.

V. Discussion

Our results suggest that the adoption of E-ZPass was associated with significant improvements of infant health. While these results are robust to a number of different specifications, in the absence of a "first stage" it is difficult to interpret the magnitude of these effects. Unfortunately, there is only one air quality monitor located within 2km of a toll plaza, but it happens to be located just .15km from a toll plaza in our study. In this section we use data from this monitor as well as other air quality monitors maintained by the EPA as various control groups, allowing us to estimate the effect of E-ZPass.[6] We combine our results with information from the engineering studies discussed above to try to interpret our reduced form coefficients.

Columns 1 and 2 of Table 7 shows difference in difference estimates of the effects of E-ZPass on daily mean NO_2 and SO_2 levels at the one monitor that we observe within 2km of a toll plaza. These models compare pollution at this “close” monitor to pollution at all monitors further than 2km from a toll plaza, before and after E-ZPass. The model includes year, month, and day of week effects, as well as monitor specific time trends. Furthermore, since pollution is correlated with weather, we control for daily weather variation using quadratic polynomials in minimum temperature, maximum temperature, and precipitation at the site of the air quality monitor.[7] It is interesting to compare the effects on NO_2 and SO_2 , because cars are a major source of the former but not of the later. The estimates indicate that NO_2 fell by 10.8%, post E-ZPass, while SO_2, showed no change. The remaining columns of Table 7 show five similar models each estimated using a randomly selected monitor from the sample of all NO_2 monitors over 2km from a toll plaza as a control. Four of the five show a significant decline in NO_2 at the toll plaza monitor relative to the others, and these declines range from 6.5% to 20.8%.

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[6] The pollution data come from the Air Quality Standards (AQS) database of the Environmental Protection Agency (EPA). This database combines pollution readings for all pollution monitors administered by the EPA, including information on the exact location of the monitor. Data includes both daily and hourly pollution readings. We use the following algorithm when we aggregate the hourly data to mean daily pollution readings. The mean is the duration-weighted average of all hourly pollution readings. We define the duration as the number of hours until the next reading. We prefer this approach to simply taking the arithmetic average of all hourly readings on a day since hourly pollution data exhibit great temporal dependence. A missing hourly observation is better approximated by the previous nonmissing value than the daily average.

[7] The daily weather data comes from Schlenker and Roberts (2009). This daily data is gridded (2.5km by 2.5km) for the entire United States. We matched the pollution monitors in our sample with their corresponding grid in the Schlenker and Roberts dataset.

It is unfortunate that this monitor does not also measure CO, since CO has been specifically linked to poorer infant health outcomes in these data. However, the Saka et al. and Venigalla and Krimmer studies discussed above suggest that a 10% reduction in NO_2 due to E-ZPass would likely be accompanied by at least a 40% reduction in CO. Currie, Neidell, and Schmeider (2009) estimate that a one part per million (ppm) change in ambient CO levels among women within 10km of an air monitor in New Jersey reduced the incidence of low birth weight by 10.6%. While the mean levels of CO among all mothers within 10km of an air monitor was 1.64ppm, the standard deviation was .8, suggesting that more highly polluted areas of the state had ambient levels over 3 ppm. Hence, the finding that E-Zpass led to reductions in the incidence of low birth weight of 8.5-11.3% within 2km of a toll plaza seems reasonable.

VI. Conclusions

We provide the first estimates of the effect of improvements in traffic congestion on infant health. We show that E-ZPass reduced the incidence of prematurity and low birth weight in the vicinity of toll plazas by 6.7-9.1% and 8.5-11.3% respectively. These are large but not implausible effects given the correlations between proximity to traffic and birth outcomes found in previous studies. For example, Slama et al. (2007) measure levels of PM2.5 (particulates less than 2.5 microns in diameter) associated with traffic and find that mothers in the highest quartile of exposure had a risk of birth weight less than 3000 grams that was 1.7 times higher than mothers in the lowest quartile of exposure. Ritz and Williams (2003) find that the risk of preterm birth was 8% higher in mothers in the highest quartile of a distance weighted traffic exposure measure, an estimate that is remarkably similar to our own. The strength of our approach is that our estimates are based on a credible natural experiment rather than correlations between proximity and outcomes. Our results are robust across a variety of specifications, providing reassuring evidence on the credibility of the research design.

Our results suggest that policies intended to curb traffic congestion can have significant health benefits for local populations in addition to the more often cited benefits in terms of reducing travel costs. Traffic congestion is an increasingly salient issue, with annual congestion delays experienced by the average peak-period driver increasing 250% over the last 25 years. In 2007, a study of 439 U.S. urban areas found that congestion cost about $87.2 billion in terms of wasted time and fuel (Schrank and Lomax, 2009). Our results suggest that these numbers are lower bounds on the true costs, since the health externalities of traffic congestion contribute significantly to social costs.

The recent Institute of Medicine report on the costs of prematurity estimated that the societal cost was $51,600 per infant (in 2005 dollars, Behrman and Butler, 2007). Hence, the 6.7-9.1% reduction in the risk of prematurity (from a baseline of around 10%) in the 29,677 infants born within 2km of a toll plaza in the 3 years after the implementation of E-ZPass can be valued at approximately $9.8-$13.2 million. While it is difficult to know precisely how many of the roughly 4 million infants born each year in the U.S. are affected by traffic congestion, estimates from the American Housing Survey (2003) suggest that 26% of occupied units suffer from street noise or other disamenities due to traffic; hence, nationwide roughly 1 million infants per year are potentially affected. This figure suggests that nationwide reductions in prenatal exposure to traffic congestion could reduce preterm births by as many as 8,600 annually, a reduction that can be valued at $444 million per year. Since we have focused on only one of the possible health effects of traffic congestion, albeit an important one, the total health benefits of reducing pollution due to traffic congestion are likely to be much greater.

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Figure 1: Locations of Roadways and Toll Plazas

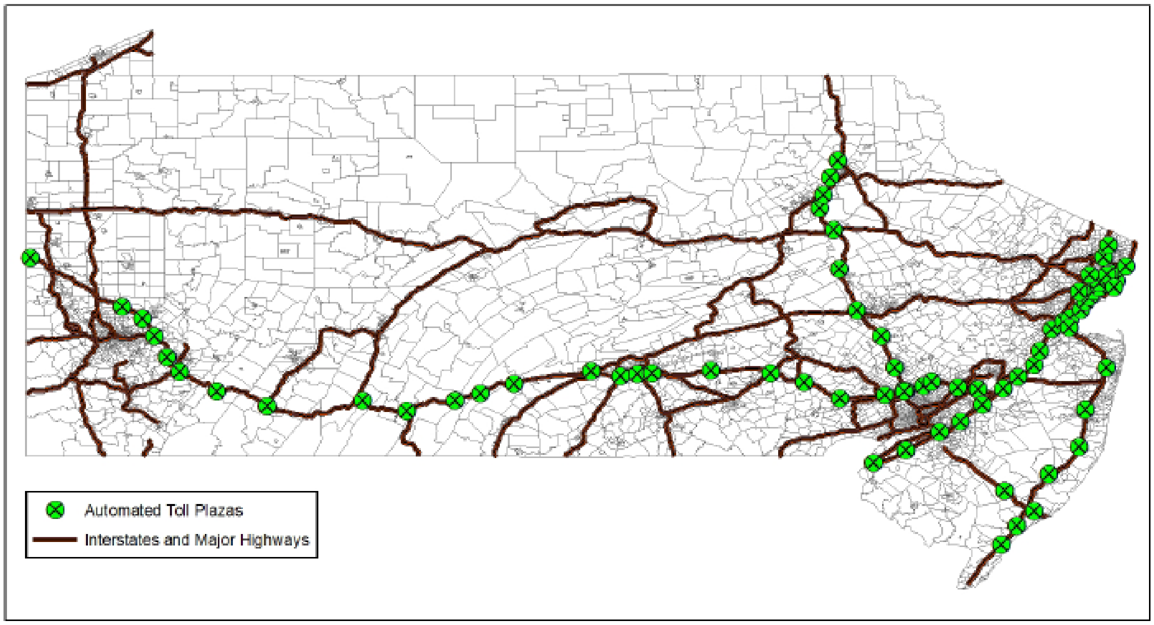
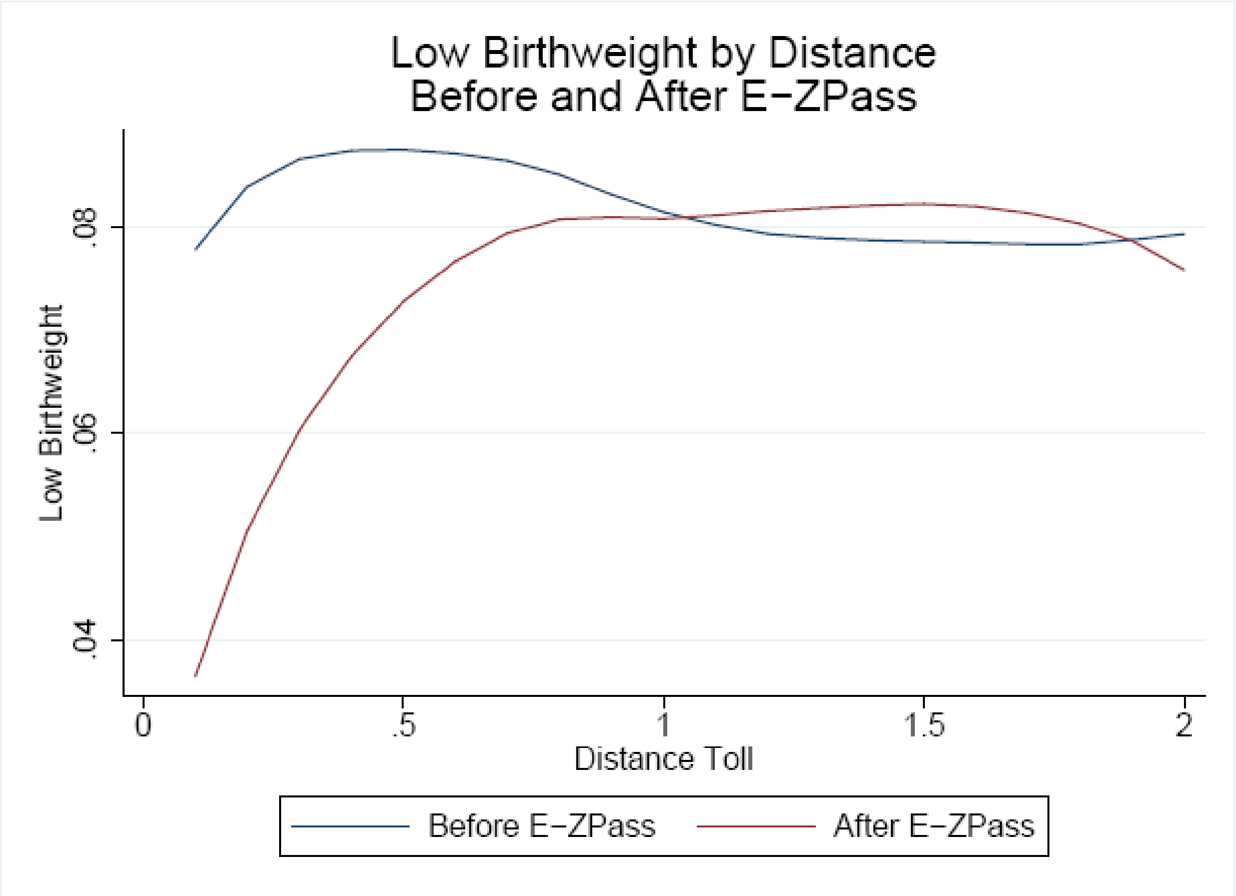


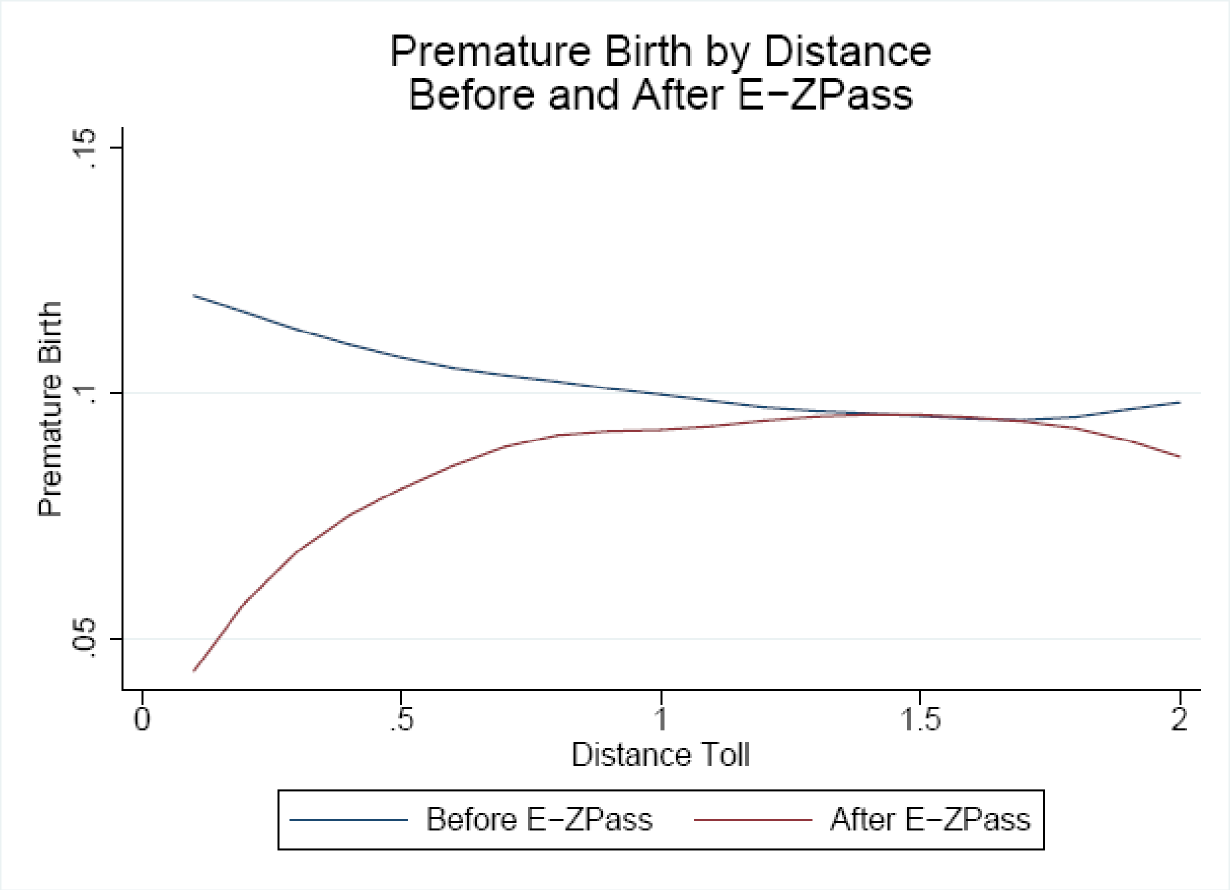
Figure 2: Research Design Showing 1.5km and 2km Treatment Radii and 3km from Highway Control Group

Figure 3



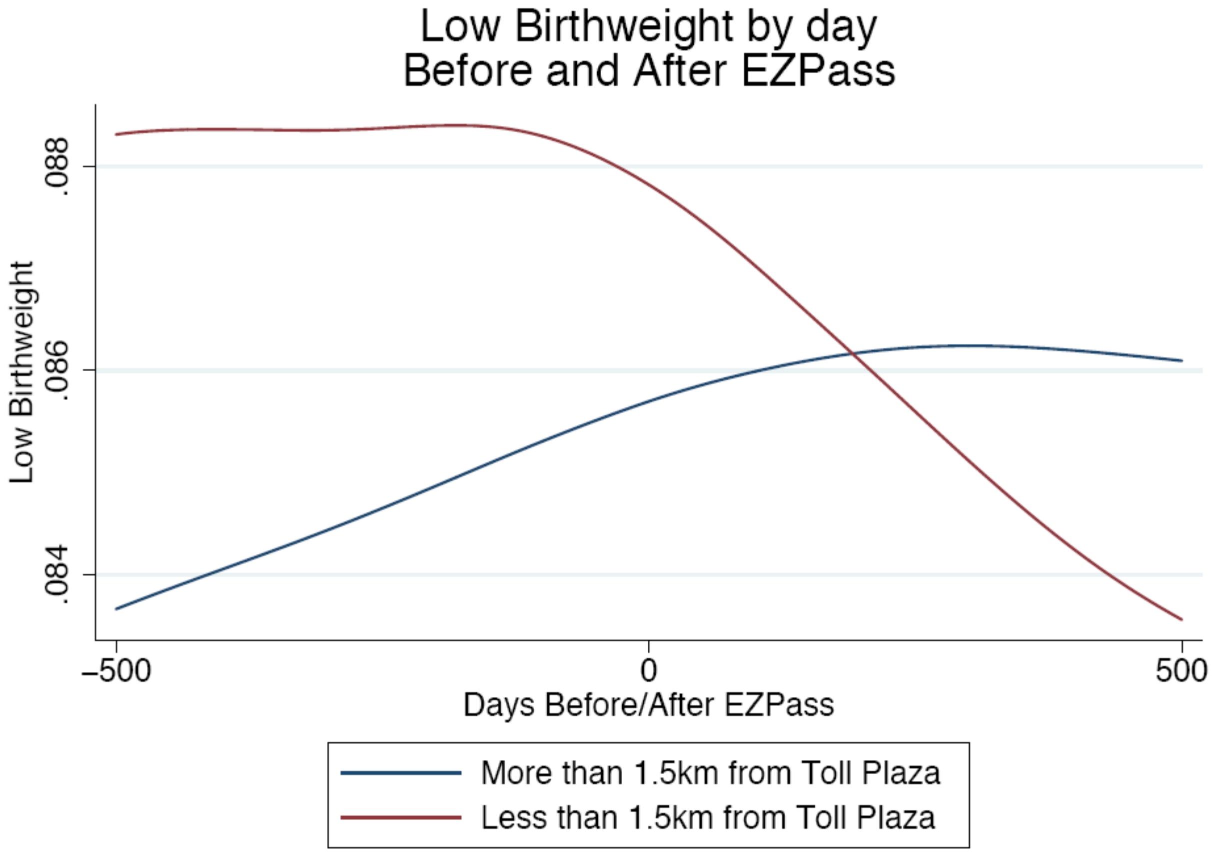
Note: Smoothed plots of treatment and control groups using locally weighted regression. To facilitate computation, observations are first grouped into 0.1-mile bins by treatment and control and averaged. The weights are applied using a tricube weighting function (Cleveland 1979) with a bandwidth of 1.

Figure 4



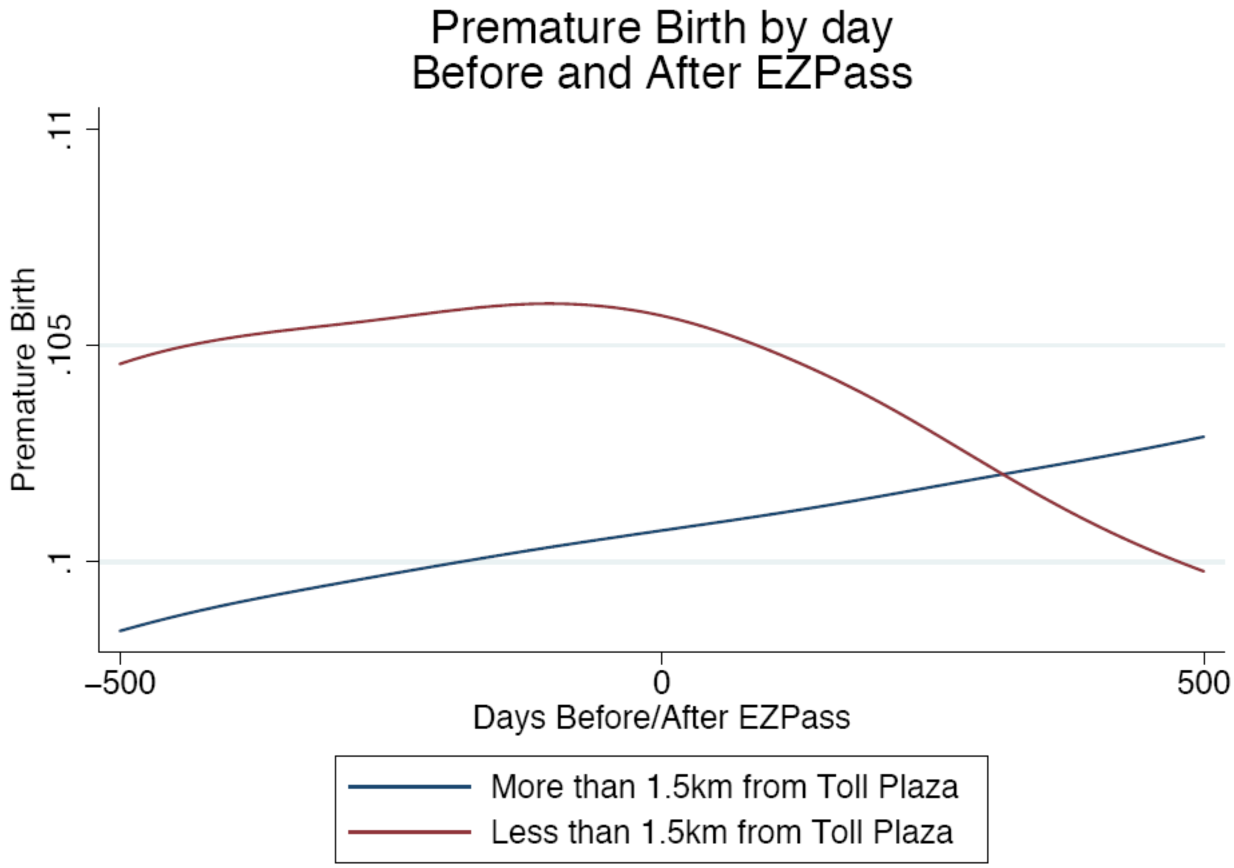
Note: Smoothed plots of treatment and control groups using locally weighted regression. To facilitate computation, observations are first grouped into 0.1-mile bins by treatment and control and averaged. The weights are applied using a tricube weighting function (Cleveland 1979) with a bandwidth of 1.

Figure 5



Note: Smoothed plots of treatment and control groups using locally weighted regression. The weights are applied using a tricube weighting function (Cleveland 1979) with a bandwidth of 1.

Figure 6



Note: Smoothed plots of treatment and control groups using locally weighted regression. The weights are applied using a tricube weighting function (Cleveland 1979) with a bandwidth of 1.

Table 1: Summary Statistics

Panel A: Difference-in-Difference Sample

Outcomes	**<2km E-Zpass Before**	**<2km E-Zpass After**	**>2km & <10km E-Zpass Before**	**>2km & <10km E-Zpass After**	**>10km Toll Plaza**
Premature	0.095	0.095	0.102	0.109	0.085
Low Birth Weight	0.082	0.078	0.089	0.092	0.078
Controls					
Mother Hispanic	0.291	0.332	0.165	0.229	0.054
Mother Black	0.16	0.173	0.233	0.264	0.047
Mother Education	13.12	13.2	13.276	13.24	12.92
Mother HS Dropout	0.169	0.164	0.154	0.163	0.173
Mother Smoked	0.089	0.075	0.109	0.086	0.152
Teen Mother	0.073	0.061	0.082	0.069	0.079
Birth Order	1.3	1.37	1.39	1.46	1.68
Multiple Birth	0.028	0.033	0.032	0.037	0.033
Child Male	0.51	0.512	0.514	0.512	0.512
Distance to Roadway	1.099	1.074	1.507	1.482	21
Number of Obs.	33,758	29,677	190,904	161,145	185,795
New Jersey Obs.	26,415	26,563	128,547	133,560	70,484
Penn. Obs	7,343	3,114	62,357	27,585	115,311

Panel B: Mothers with More than One Birth in Sample

Outcomes	**Ever Birth<2km E-Zpass Plaza Before**	**Ever Birth<2km E-Zpass Plaza After**	**Never Birth<2km E-Zpass Plaza Before**	**Never Birth<2km E-Zpass Plaza After**
Premature	0.088	0.099	0.092	0.103
Low Birth Weight	0.081	0.077	0.086	0.086
Controls				
Mother Hispanic	0.167	0.29	0.088	0.161
Mother Black	0.145	0.157	0.169	0.171
Mother Education	12.78	12.6	12.75	13.13
Mother HS Dropout	0.168	0.201	0.178	0.162
Mother Smoked	0.113	0.076	0.135	0.095
Teen Mother	0.041	0.044	0.072	0.047
Birth Order	1.575	1.708	1.598	1.735
Multiple Birth	0.03	0.037	0.033	0.046
Child Male	0.513	0.512	0.512	0.512
Distance to Highway	3.702	2.561	5.598	5.3
Total # Obs.	179,537	58,180	1,640,118	485,351
NJ Obs.	85,565	47,012	678,025	352,751
PA Obs.	93,972	11,168	962,093	132,600

Panel C: Summary Statistics for Housing Sales Data (New Jersey Only)

	<2km E-Zpass Before	**<2km E-Zpass After**	**>2km & <10km E-Zpass Before**	**>2km & <10km E-Zpass After**
Sales Price	94,883	126,006	95,518	116,691
Assessed Land Value	42,146	43,219	46,551	46,126
Assessed Building Value	78,234	81,437	70,093	69,752
Total Assessed Value	119,166	123,640	115,129	114,403
Year Built	1952	1954	1951	1950
Square Footage	1,573	1,569	1,646	1,675
# Obs.	22,350	22,604	105,341	102,048

Notes: All observations in Panels A and C are selected to be within 3km of a busy roadway. Housing price data is only for New Jersey and pertains to housing units, not mothers, as described in the text. The housing price data has been deflated by the CPI (base year=1993).

Table 2: Testing the Validity of the Research Design: Regressions of Maternal Characteristics on E-Zpass Adoption Difference in Difference Specification

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel 1	**Black**	**Hispanic**	**Mother Yrs. Ed**	**Dropout**	**Teen Mother**	**Mother Smoked**	**Housing Sale Price**
<2km toll*after E-Zpass	-0.011	-0.01	0.037	-0.007	-0.001	.005*	0.149
	[0.011]	[0.010]	[0.040]	[0.005]	[0.005]	[0.003]	[0.103]
# observations	397,201	406,641	406,198	397,201	412,884	402,590	252,343
Panel 2							
<1.5km toll*after E-Zpass	-0.014	-0.01	0.013	-0.003	0.001	.007**	0.031
	[0.055]	]0.011]	[0.010]	[0.006]	[0.003]	[0.003]	[0.106]
# observations	397,201	406,641	406,198	397,201	412,884	402,590	252,343

Notes: Each coefficient is from a separate regression. Each coefficient in columns 1-6 is from a regression that also included controls for being within 2km (or 1.5km) of a toll plaza, year of birth, month of birth, indicators for each toll plaza, an indicator for post E-Zpass at nearest toll plaza, and distance to highway. Housing sale price regressions in column 7 include year and month of sale, indicators for nearest toll plaza, an indicator for condo units, distance to highway, municipality fixed effects, square footage (in categories including dummies for missing), and age (in categories, including dummies for missing). Standard errors in brackets. A ** indicates that the estimate is statistically significant at the 95% level of confidence. A * indicates significance at the 90% level of confidence.

Table 3: Regressions of Birth Outcomes on E-Zpass Adoption
Difference in Difference Specification

Panel 1	**[1] Prematurity**	**[2] Prematurity**	**[3] LBW**	**[4] LBW**
<2km toll*after E-Zpass	-0.0085	-0.0086	-0.0094	-0.0093
	[0.0039]**	[0.0034]**	[0.0032]**	[0.0028]**
R-squared	0.0044	0.0034	0.0032	0.0028
Panel 2				
<1.5km toll*after E-Zpass	-0.0088	-0.0098	-0.0077	-0.0084
	[0.0051]*	[0.0048]**	[0.0035]**	[0.0032]**
R-squared	0.0042	0.0048	0.0035	0.0032
Maternal Characteristics	no	yes	no	yes
# Obs.	405,802	405,802	409,673	409,673

Notes: Each coefficient is from a different regression. All regressions also included controls for being within 2km (or 1.5km) of a toll plaza, year of birth, month of birth, toll plaza indicators, an indicator for post E-Zpass, and distance to highway.
Maternal characteristics include: mother black, mother hispanic, mother education (<hs, hs, some college, college +), mother age (19-24, 25-34, 35+), smoking, multiple birth, gender, and birth order, and indicators for missing values.
Standard errors in brackets. A ** indicates that the estimate is statistically significant at the 95% level of confidence. A * indicates significance at the 90% level of confidence.

Table 4: Robustness Checks, Birth Outcomes on E-Zpass Adoption
Difference in Difference Specification

Panel 1: All obs. within 5km toll plaza	[1] Prematurity	[2] LBW
<2km toll*after E-Zpass	-0.0064	-0.007
	[0.0035]*	[0.0028]**
R-squared	0.104	0.1224
# Obs.	255,711	258,226
Panel 2: Add time trend for areas near toll plazas		
<2km toll*after E-Zpass	-0.0074	-0.0084
	[0.0035]**	[0.0029]**
R-squared	0.1053	0.1222
# Obs.	405,802	409,673
Panel 3: Propensity Trimmed, .1<=P(near toll)<=.9		
<2km toll*after E-Zpass	-0.0079	-0.0086
	[0.0037]**	[0.0036]**
R-squared	0.1011	0.1222
# Obs.	123,467	124,672
Panel 4: Non-African Americans Only		
<2km toll*after E-Zpass	-0.0052	-0.0059
	[0.0035]	[0.0029]**
R-squared	0.1078	0.1267
# Obs.	311,038	314,269
Panel 5: African-Americans Only		
<2km toll*after E-Zpass	-0.0213	-0.0242
	[0.0067]**	[0.0064]**
R-squared	0.0882	0.0989
# Obs.	94,764	95,404
Panel 6: Non-Smokers Only		
<2km toll*after E-Zpass	-0.0075	-0.0079
	[0.0032]**	[0.0028]**
R-squared	0.1074	0.1232
# Obs.	367,465	371,089

Notes: See Table 3.

Table 5: Using Linear and Exponential Functions of Distance from Toll Plaza

	[1] Prematurity	[2] Prematurity	[3] LBW	[4] LBW
Argmax(2-Distance,0)*	-0.0019		-0.0043	
after E-Zpass	[0.0035]		[0.0027]	
1/(e**distance)*		-0.0153		-0.0225
after E-Zpass		[0.0093]*		[0.0080]**
R-squared	0.1051	0.1051	0.122	0.122
# observations	405,802	405,802	409,673	409,673

Notes: All regressions control for after E-Zpass, a dummy for being less than 2km from a toll plaza, distance to highway, and fixed effects for toll plaza, year, and month of birth, as well as the full set of maternal characteristics listed for Table 3.

Standard errors in brackets. A ** indicates that the estimate is statistically significant at the 95% level of confidence. A * indicates significance at the 90% level of confidence.