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Three Essays in Applied Urban Economics

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Three Essays in Applied Urban Economics

Alexander Cardazzi

**Dissertation submitted to the
John Chambers College of Business and Economics
at West Virginia University
in partial fulfillment of the requirements for the degree of
Doctor of Philosophy
in
Economics**

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Abstract

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The first chapter examines the effect of policing strategies on housing prices. Crime has been shown to negatively affect housing values. However, there is not much empirical evidence documenting the amenity value of how people are policed. Stop & Frisk was a particularly salient policing tactic employed in New York City until a judge ruled it to be unconstitutional. This chapter uses exogenous variation in Stop & Frisk behavior resulting from landmark events in a federal class action lawsuit as well as exposure to Operation Impact, a high intensity policing strategy, to explain variation in property sales. Results indicate a 6% increase in sale price for properties most exposed to Stop & Frisk relative to nearby properties after the practice is ruled unconstitutional. However, this increase in value does not appear to be received by property owners who are predicted to be non-white or having lived in the property during an active Impact Zone. The decrease in perception of one's home value is comparable to that resulting from an economic recession. This result adds to a growing literature that shows the strong negative cognitive consequences of Stop & Frisk such as decreased mental health and lower educational outcomes.

The second chapter is joint work with Dr. Zachary Rodriguez and Joshua C. Martin. This chapter presents evidence of Earvin "Magic" Johnson's announcement that he contracted HIV served as a public-health catalyst for rapidly increasing the public's understanding of *who* was at risk for infection. Using a novel identification strategy, we estimate statistically significant differences in the rates of AIDS diagnoses between heterosexual and homosexual men in the United States immediately following Johnson's announcement. Results indicate that this effect was concentrated in areas with greater exposure to Johnson prior to his announcement. The results suggest that Johnson's effect on diagnoses was driven by an intertemporal substitution in testing. We verify Johnson's announcement as an important moment in the history of the AIDS pandemic as it significantly impacted beliefs about vulnerability to HIV/AIDS across the United States.

The third chapter examines the effect of Dynamic Message Signs (DMS) on traffic outcomes. Traffic safety and congestion represent important nationwide transportation issues. For example, car crash fatalities accounted for 1.3% of all deaths in the United States in 2017. Therefore, maintaining safe roads is a top priority for all transportation departments. Transportation departments use DMS to convey traffic conditions and other information to drivers in order to increase attentiveness, reduce harmful behavior, and caution drivers of upcoming incidents. Previous research ([Hall & Madsen, 2021](#)) has found certain uses of DMS can in fact increase the number of crashes on a highway by distracting drivers. This study uses variation in the message text displayed by Virginia's Department of Transportation's (VDOT) DMS to explain detailed speed and crash data along Virginia's Federal-Aid Highway System. Results indicate that messages that transition between multiple pages increase the risk of a crash and reduce speed around DMS. Policy implica-

tions of these results are DOTs should make efforts to reduce messages that transition as these are especially distracting to drivers.

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Contents

Abstract	ii
Acknowledgments	iv
List of Figures	viii
List of Tables	x
1 The Amenity Value of Aggressive Policing: Evidence from Stop & Frisk in New York City	1
1.1 Introduction	1
1.2 Literature Review	3
1.2.1 Crime and Housing	3
1.2.2 Stop & Frisk	5
1.3 Empirical Strategy	7
1.3.1 Model Specification	9
1.4 Data	11
1.5 Results	17
1.6 Discussion	22
2 Information Avoidance and Celebrity Exposure: The Effect of “Magic” Johnson on AIDS Diagnoses in the U.S.	24

2.1	Introduction	24
2.2	Literature Review	26
2.2.1	Health Consequences of Information Avoidance	26
2.2.2	Celebrity Exposure and Health Behavior	28
2.3	Data and Setting	29
2.3.1	Magic Johnson and The National Basketball Association	30
2.3.2	AIDS Public Information Data Set	31
2.3.3	NCHS Multiple Cause of Death Data	34
2.3.4	National Health Interview Survey	35
2.3.5	Homosexual Population Index	36
2.3.6	Ryan White CARE Act	36
2.4	Identification Strategy	37
2.4.1	Changing Attitudes towards AIDS	38
2.4.2	AIDS Diagnoses	40
2.5	Empirical Framework	42
2.6	Results	44
2.7	Conclusion	48
3	Nudging and Driving: An Empirical Analysis of Dynamic Message Signs in Virginia	51
3.1	Introduction	51
3.2	Literature Review	53
3.3	Data and Setting	57
3.3.1	DMS Use	59
3.3.2	Message Text	63
3.3.3	Traffic Outcomes	63
3.3.4	Summary Statistics	64
3.4	Empirical Strategy	66
3.5	Results	70

3.6 Conclusion	74
References	77
Appendix	86
G Additional Tables and Figures for Chapter 1	87
H Additional Tables and Figures for Chapter 2	89

List of Figures

1.1	Relative Frequency of Unique Properties and Number of Times in an Impact Zone .	12
1.2	Impact Zones from Jan. 2004 to Jan 2013	14
1.3	Distribution of non-Hispanic White % in Surnames	16
1.4	Average Log Sale Price by Quarter	17
1.5	Difference in Unconditional Mean Log Price in Buffer Control and Impact Zone Properties	18
1.6	Varying Cutoffs in Defining “Non-White”	21
2.1	AIDS Diagnoses & Deaths in the CDC’s selected MSAs	35
2.2	Percent of Respondants Expecting to be Tested for AIDS in the Next Year	40
2.3	Time Series of Group Definitions	47
2.4	Testing Different DID Cutoffs	48
2.5	Testing Different Triple Difference Cutoffs	49
3.1	Map of DMS Locations in Virginia	60
3.2	Safety Campaign Schedule	62
3.3	Number of Characters and Average Pages	64
3.4	Distribution of Speed	65
3.5	Message Timing	67
3.6	Coefficient Estimates By Distance	75
G.1	Daily Counts of Stop & Frisk in NYC from 2006 through 2015	88

G.2	Daily Counts of Crime in NYC from 2006 through 2015	89
G.3	Impact Zone Buffers	90
G.4	Identification Strategy - NTA	91
G.5	Identification Strategy - Buffer	92
H.1	Dec. 7, 1991 NYT Front Page Article	93
H.2	Daily Newspaper Mentions	94
H.3	Past Month Sources of AIDS Information in 1991	94

List of Tables

1.1	Number of Properties in Control Groups.	8
1.2	Ability to Match Census Surname Race to NYC Sales	15
1.3	Summary Statistics for Analysis Sample	16
1.4	Effect of Stop & Frisk on Housing Prices	19
1.5	Heterogeneous Effects	20
1.6	Percent of Analysis Sample by Borough	22
1.7	Heterogeneous Effects, Brooklyn and Queens	23
2.1	Tabulation of Sexuality and Relevant Transmission Categories, Full Sample	31
2.2	Sample Summary Statistics	34
2.3	CARE Funding and NBA Team breakdown	37
2.4	Questions Relevant to AIDS Knowledge	39
2.5	Effect of Magic Johnson on AIDS Diagnoses	46
3.1	DMS Type Frequency Matrix	59
3.2	Summary Statistics	65
3.3	DMS Use	71
3.4	DMS Use and Safety Messaging	72
3.5	DMS Use and Safety Messaging, Without Holidays	73
3.6	DMS Use and Multiple Page Messages	74
G.1	Table of Building Classes	87

G.2 Most Frequent Surnames	88
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Chapter 1

The Amenity Value of Aggressive Policing: Evidence from Stop & Frisk in New York City

1.1 Introduction

An often cited finding in real estate and urban literature is a negative relationship between crime and housing values. [Thaler \(1978\)](#) was the first to document this empirically, and there has been ample research on the topic since. However, something that has not gotten much attention in this same literature is the valuation of the steps taken towards a reduction in crime. Given the current attitudes towards police across the United States, there are lessons to be learned about how people value the way they are policed.

Police departments face a menu of options when deciding on tactics to employ within a community. Each tactic, or combination of tactics, has different costs and benefits. In an ideal world where police departments are only concerned with maintaining public safety and enforcing the law, police would choose the bundle of tactics that generates the lowest crime rate with respect to constraints. In reality, police respond to incentives other than those that minimize crime ([Garrett](#)

& Wagner, 2009; Makowsky & Stratmann, 2009) and their tactics and actions can produce externalities for the local community. While it is important to keep crime low, it could be the case that the externalities generated by policing tactics outweigh the benefits of a low crime rate.

A particularly invasive and aggressive form of policing is Stop, Question and Frisk (“Stop & Frisk”), also known as Terry stops, in which police can detain an individual when there is reasonable suspicion of someone’s involvement in criminal behavior. This policing tactic was famously used throughout New York City in the 1990s through the early 2010s. Then mayor Rudolph Giuliani and then police chief William Bratton employed Stop & Frisk in an endorsement of Broken Window Policing. This theory of policing hypothesizes that low level crime fosters an environment for more serious crime to thrive in. Therefore, cracking down on small crime in turn could be instrumental in curbing worse crime. During this time period, New York City saw crime decrease as the use of Stop & Frisk increased.

Stop & Frisk in New York City presents an interesting setting to study how people value the way they are policed for a few reasons. First, New York City’s five boroughs represent a remarkably heterogeneous urban area with excellent data availability. Second, during the time period of 2003 through 2012, New York City’s Police Department (NYPD) implemented a crime reduction strategy called Operation Impact, where high crime areas were flooded with police. A report from 2008 states that 1,800 new police graduates per year were assigned solely to Operation Impact. In the same year, the NYPD reported having about 36,000 uniformed officers, making this *additional* 1,800 officers about 5% of the entire force. Operation Impact represents exogenous shocks to the utilization of Stop & Frisk over space, and has been previously used in the literature (J. MacDonald, Fagan, & Geller, 2016; J. M. MacDonald & Fagan, 2019). Lastly, an influential court case was filed at the start of 2008. Floyd, *et al.* v. City of New York, *et al* claimed Stop & Frisk to be unconstitutional on the basis of racial profiling and suspicionless searches. This case was decided in favor of the plaintiff, and the NYPD effectively ceased employing Stop & Frisk. Landmark events in this case represent exogenous shocks to Stop & Frisk over time. This study uses these two features to estimate the effect Stop & Frisk had on property values in New York City.

The findings assert that citizens in New York City saw Stop & Frisk as a disamenity. Once the policy was ruled to be unconstitutional, those properties that were in Impact Zones saw an increase in property values of about 6% relative to nearby properties. However, this effect is concentrated in a group of property owners unlikely to be most experienced with (subjected to) Stop & Frisk. In other words, the positive effect is realized by homeowners who did not live in the property during an active Impact Zone, or homeowners who are identified as being white. This suggests a lasting impact of Stop & Frisk policies on homeowners. Other research has documented decreases in mental health, school performance, and trust in police resulting from Stop & Frisk, but mixed results in its ability to achieve its intended goal of preventing criminal behavior.

1.2 Literature Review

1.2.1 Crime and Housing

A lot of research has been devoted to estimating the impact of crime on property values. [Thaler \(1978\)](#) was the first empirical study to document this relationship. The study reported a negative correlation, suggesting people are willing to pay for lower levels of criminal activity. [Hellman and Naroff \(1979\)](#) point out that crime generates more costs than just direct bodily harm, property damage and increased police spending. Crime can also cost the local government in the way of property taxes. Since people supposedly pay less for properties in high crime neighborhoods *ceterus parabus*, these neighborhoods might generate less property tax revenues. Therefore, it is in the interest of local government to keep crime low as long as the increased revenue outweighs the costs of doing so. [Tita, Petras, and Greenbaum \(2006\)](#) show violent crimes impact housing prices the most, relative to other types of crime, though this effect is different across levels of wealth. The authors present evidence that low income neighborhoods are more responsive to changes in crime. Similarly, [Ihlanfeldt and Mayock \(2010\)](#) show housing values are negatively associated with aggravated assault, a type of violent crime. In addition to decreased valuations, [Dugan \(1999\)](#) uncovered a positive relationship between criminal victimization and the probability of moving.

While actual crime rates can impact housing values, so can the risk of crime. There are many studies examining the effect of sex offender registries on housing values. While it is not a crime for a sex offender to move into a neighborhood, it may change the perception of said neighborhood. There are many studies showing a decrease in housing values in the immediate vicinity of a sex offender (Caudill, Affuso, & Yang, 2015; Kim & Lee, 2018; Linden & Rockoff, 2008; Pope, 2008). These studies offer robust evidence that the effect is spatially located within a relatively small radius. The effect also appears to dissipate with distance as well as once the sex offender has moved away. Mastrorocco and Minale (2018) use a natural experiment in Italy, the staggered rollout of digital television, to estimate the effect this had on the perception of crime. Before a digital signal was available, standard television channels relatively over-reported on crime. The change to digital allowed for more channels, and thus less exposure to crime reporting. The authors observe decreased concerns about crime from individuals after digital television becomes available in their area. Buonanno, Montolio, and Raya-Vílchez (2013) use similar evidence from a survey of crime perception and apply this to housing values. Respondents are asked about their level of security in the city and in their district (a subset of the city). They find that feelings of security are positively associated with housing prices, but prices are discounted in districts that are perceived as less safe than the rest of the city. Signals and feelings about the severity of crime can influence perceptions, which in turn can influence individuals' property valuations.

While variation in crime is often used to explain variation in housing prices, there are studies that use shocks to housing to explain crime. There is quite a bit of research using a randomized housing voucher experiment to show decreased propensity of committing crime (Kling, Ludwig, & Katz, 2005; Ludwig, Duncan, & Hirschfield, 2001), increased safety, and increased health (Katz, Kling, & Liebman, 2001) among recipients. On a more aggregate level, Freedman and Owens (2011) demonstrate reductions in violent crime not through policing but rather development in low income housing. This result is also supported by Aliprantis and Hartley (2015) who show the demolition of high-rise public housing in Chicago appeared to have decreased crime. Moreover, these decreases outweigh any increases in crime where residents were displaced to.

1.2.2 Stop & Frisk

The literature has documented causal evidence of crime reductions due to the presence of police (DeAngelo & Hansen, 2014; Di Tella & Schargrodsky, 2004; Klick & Tabarrok, 2005), but not necessarily the effectiveness of different tactics. Stop & Frisk is one of the few policing tactics to be extensively studied due to data availability and questions of legality. Tiratelli, Quinton, and Bradford (2018) evaluate the policy in London and find its effectiveness to be marginal in terms of deterrence. Similar studies that use data from New York City find similar results (Weisburd, Wooditch, Weisburd, & Yang, 2016; Wooditch & Weisburd, 2016). J. MacDonald et al. (2016) leverages the same policy as this study, Operation Impact, to examine the effect of Stop & Frisk on crime. The authors suggest that assigning extra police to areas reduced crime, but Stop & Frisk in particular did not play a major part in this reduction. This result is similar to other studies (Di Tella & Schargrodsky, 2004; Klick & Tabarrok, 2005) in that reallocation of police is effective while adding new knowledge that Stop & Frisk was not a major driver. In a recent working paper, Bacher-Hicks and de la Campa (2021a) uses quasi-random movement of police commanders in NYC to test if commanders' preferences for Stop & Frisk alter implementation of the practice and, through this channel, crime. The authors find a significant decrease in misdemeanor crimes but no change in more serious crime. This appears to invalidate the point of Broken Window Policing - reductions in small crimes will generate reductions in worse crime.

Although there is consistent evidence of the ability of Stop & Frisk to deter low level crimes, there is also substantial evidence of the costs of this policy being large. Tyler, Fagan, and Geller (2014) use survey evidence to show the number of times a person was stopped is associated with decreased perceptions of police legitimacy. Using data from 1975 to 1996 in New York City, Kane (2005) showed that low perception of police legitimacy were associated violent crime. Moreover, besides simply views of police legitimacy, Geller, Fagan, Tyler, and Link (2014) shows that for youth (18-26 year old men) in New York City, more police contact (via Stop & Frisk) is associated with trauma and anxiety symptoms. This is also correlated with the number of stops and how intrusive those stops were. In accordance with *Floyd, et al. v. City of New York, et al.*, Gelman,

Fagan, and Kiss (2007) show that people of African or Hispanic descent are stopped at a higher frequency even after controlling for race-specific arrest rates within each precinct. Similarly, leverage timing in Operation Impact in a difference-in-differences framework, J. M. MacDonald and Fagan (2019) demonstrate that, in Impact Zones, people of African and Hispanic descent were again more likely to be arrested and/or frisked during live Impact Zones compared to white citizens. Bacher-Hicks and de la Campa (2021b) use the same police commander reassignment identification as Bacher-Hicks and de la Campa (2021a) and finds decreased high school graduation rates, college enrollment and college persistence where Stop & Frisk was utilized most. Reinforcing and tying together Bacher-Hicks and de la Campa (2021b) and J. M. MacDonald and Fagan (2019), Legewie and Fagan (2019) finds decreased test scores in areas where Operation Impact was applied.

Evidence tying together Stop & Frisk and housing is scant, however. Fagan, Davies, and Carlis (2012) show racial disparities in Stop & Frisk behavior in NYC housing units. Hispanic and Black populations are much more likely to be stopped, which is in alignment with most other research examining differences in stop probabilities stemming from race. In a study most similar to this one, Friedman (2015) uses data from 2006-2012 to demonstrate Stop & Frisk incidents nearby properties in NYC are correlated with lower housing values. This present study works to extend Friedman (2015) in a few key ways. First, this study is able to control for crime incidents as well as Stop & Frisk, which should be highly correlated by nature. Second, this study shifts the time period of interest in order to better identify effects on housing. By studying a slightly later time period, this allows for leveraging the plausibly exogenous shocks from Floyd, *et al.* v. City of New York, *et al.* as well as avoiding the 2008 recession. Finally, instead of using the entire city, this study targets those properties most effected by Stop & Frisk and nearby comparison groups. While Friedman (2015) shows about a 3% price decrease for each standard deviation increase in Stop & Frisk incidents, this study reports a similar, albeit larger, magnitude of 6% as an extensive margin.

1.3 Empirical Strategy

Estimating the effect Stop & Frisk tactics had on property values in NYC is the primary interest of this study. A hedonic difference-in-differences (“DiD”) approach, using landmark events in *Floyd, et al. v. City of New York, et al.* as shocks, will provide an estimate of an extensive margin. The treatment group in this setting are all properties that were ever inside of an Impact Zone, and thus subject to an inordinate amount of Stop & Frisk behavior relative to the local crime rate. These properties should realize the largest change in the aggressiveness of policing throughout the evolution of the case, and thus the largest change in property value *ceterus parabus*.

While the treatment group is rather easily defined, a comparison group is not. An ideal group would be other properties plagued with similar crime rates but not in Impact Zones during the same time period, but there are a couple of issues that prevent this type of identification. First, though the NYPD’s system is described as a nomination-based process, they do not disclose which areas were not chosen for Impact Zones. Second, Impact Zones do not necessarily follow a clean spatial unit and often vary in their borders, even for the same Impact Zone over time. Without such a spatial unit, constructing a comparison group becomes difficult. In addition, temporal variation in Impact Zones is also problematic. The final Impact Zone map provided by the NYPD ends in January 2013. Since the hearings in *Floyd, et al. v. City of New York, et al* begin and end after this date, there is no variation in which properties are in Impact Zones after January 2013. This issue is compounded by the fact that properties can be in an Impact Zone in time t and $t + 2$, but not in $t + 1$.

Due to these identification issues, a comparison group is defined in one of two ways. The first group is made up of all properties in the same neighborhood as an Impact Zone but not actually within the Impact Zone. This is intended to keep local amenities relatively similar across the treatment and comparison group, while allowing for variation in police tactics. Neighborhoods are defined by Neighborhood Tabulation Areas (“NTA”), which were developed by New York City’s Department of Planning. These NTAs are made up of groups of census tracts whose populations sum to at least 15,000 people. A limitation to using NTAs is that they occasionally have bound-

aries that would not necessarily be intuitive when considering historical neighborhoods¹. A second limitation might be the discontinuous spatial cutoff of an NTA. For example, if an Impact Zone is located on the edge of an NTA, the comparison group would consist of properties on the other side of the NTA but not properties across the street. To address these concerns, a second group is identified using a 3,000 foot (about half of a mile) buffer around each impact zone. Therefore, properties inside an Impact Zone are compared to properties just outside an Impact Zone. A limitation to using a buffer zone is this uses arbitrary cutoffs with might capture properties in different jurisdictions and thus subject to different policies. The study progresses with both comparison groups to ensure robustness to this decision. Table 1.1 describes the number of properties in each comparison group as well as the overlap between them. Of all the properties that are part of either comparison group, about 60% of properties are within both groups and about one fifth in each comparison group are different.

Table 1.1: Number of Properties in Control Groups.

	Outside Buffer	Inside Buffer
Outside NTA	0	5,570 (20.5%)
Inside NTA	4,944 (18.2%)	16,659 (61.3%)

Given the treatment and comparison groups, there are four important dates in the development of Floyd, *et al.* v. City of New York, *et al.* that could act as exogenous shocks to the intensity of Stop & Frisk. On January 31, 2008, the Center for Constitutional Rights filed a federal class action complaint against the City of New York. This complaint asserted violations of the Fourth and Fourteenth Amendments of the United States Constitution. Specifically, that the NYPD's Stop & Frisk policies resulted in state sanctioned racial profiling and unlawful searches. Filing such a complaint in federal court introduces a wave of motion practice that culminates to the class certification process. This class certification process alone is painstaking and arduous. The plaintiffs must persuade the judge that class action is the most efficient way to judge the case's claims. Consequently, on May 16, 2012, a judge granted the Center for Constitutional Right's motion for class certification. The next milestone in the life of a civil lawsuit is the trial itself.

¹<https://www1.nyc.gov/site/planning/data-maps/open-data/dwn-nynta.page>

After all subsequent motion practice, the trial began on March 18, 2013. After a trial spanning eight weeks, the court found the City liable for constitutional violations and ordered broad police reforms. This yields four landmark events, or five time periods, for a difference-in-differences approach. However, the first event (and thus first time period) is omitted from the analysis. January 31, 2008 is in the middle of the 2008 financial crisis which particularly affected housing prices. Secondly, according to Figure G.1, Stop & Frisk activity does not appear to have reacted to this event like it did the others. This is actually an important feature of the data, as the first major recoil in Stop & Frisk occurs at the point of class certification. For these reasons, the time period analyzed spans 2010 through 2015. This is the first study to utilize this court case to examine the effect of Stop & Frisk in any sense. Most other studies end the sample well before Floyd, *et al.* v. City of New York, *et al.*, or at least do not consider leveraging the sudden effect this had on the use of this policy.

1.3.1 Model Specification

To capture the effect of changes in Stop & Frisk tactics on property values, a simple model for the log sale price of property i in NTA n on day t is:

$$p_{int} = X_{int}\beta + \delta_1\text{Floyd}_t + \delta_2\text{Impact}_i + \delta_3(\text{Floyd}_t * \text{Impact}_i) + \epsilon_{int} \quad (1.1)$$

where p_{int} is the log price of the property, Floyd_t is a vector of landmark events, Impact_i is an indicator variable equal to one if a property was ever subject to an Impact Zone and X_{int} is a vector of controls such as square footage (gross & land), number of floors, number of units, population of the census tract, median income of the census tract, percentage of the census tract population that is white, the number of crimes, the number of Stop & Frisk incidents, NTA fixed effects and time fixed effects. δ_3 is the parameter of interest and represents the difference in the change in housing prices between the treatment and comparison groups. In other words, if $\delta_3 > 0$, properties

subjected to an Impact Zone sold for a higher price relative to the overall change in prices following the particular landmark decision.

A major assumption of this model is that buyers and sellers had some belief π_0 that a property would be inside of an Impact Zone during any given period. However, for the properties that *did* fall within an Impact Zone (the treatment group), buyers and sellers had the belief $\pi_1 > \pi_0$. Therefore, even though a property might have been purchased before it was inside of an Impact Zone, buyers and sellers still thought it to have a higher probability of being in an Impact Zone at some point.

Since the identification of treatment is based on a property ever being in an Impact Zone, there might be residents who did not actually live through an Impact Zone. It could be the case that the realization of an Impact Zone might have had a different effect than simply an expectation of one. For example, suppose owner j purchases a property in t_0 and sells in t_2 while the property falls inside of an Impact Zone in t_1 . Then, owner j would have been exposed to an Impact Zone during their tenure owning this property. This might have had a different impact on resale value compared to resident k who purchased a property in t_1 and sold in t_2 while the property fell inside of an Impact Zone in t_0 . Therefore, knowing the seller's tenure in the property is important in understand exposure to Operation Impact. Equation (1.1) can be rewritten as a triple-difference model

$$p_{int} = X_{int}\beta + \delta_1\text{Floyd}_t + \delta_2\text{Impact}_i + \delta_3(\text{Floyd}_t * \text{Impact}_i) + \gamma_1(\text{Impact}_i * \text{Exposure}_{it}) + \gamma_2(\text{Floyd}_t * \text{Impact}_i * \text{Exposure}_{it}) + \epsilon_{int} \quad (1.2)$$

where Exposure_{it} is an indicator variable equal to one if the seller owned property i while it was inside of an Impact Zone. Since Exposure_{it} is a subset of Impact_i , it is redundant to include both $\text{Exposure}_{it} \times \text{Impact}_i$ and Exposure_{it} in the model together. This logic is then similar for $\text{Exposure}_{it} \times \text{Floyd}_t$ and $\text{Exposure}_{it} \times \text{Impact}_i \times \text{Floyd}_t$. In this equation, γ_2 is the parameter of interest, as this represents the differential impact of actually living in a property while the Impact Zone was active.

If $\gamma_2 > 0$, then those seller's who lived in a property in an Impact Zone, while the Impact Zone was active, sold for more money after a landmark event relative to other sellers in Impact Zones that were not active.

Finally, since Stop & Frisk was claimed (and ruled) to be a racially discriminatory policy, another heterogeneity to examine is the effect for non-white and white property owners. Since race of buyers and sellers does not often come along with property transaction data, it is useful to at least know the names of buyers and sellers. This has been done in the literature before using tokenization (Humphreys, Nowak, & Zhou, 2019; Nowak & Sayago-Gomez, 2018). The process to identify the race of buyers and sellers in this study is slightly different and is described in the Data section. Equation (1.1) can be written once again as

$$\begin{aligned}
p_{int} = & X_{int}\beta + \delta_1\text{Floyd}_t + \delta_2\text{Impact}_i + \delta_3(\text{Floyd}_t * \text{Impact}_i) + \lambda_1\text{NonWhite}_{it} \\
& + \lambda_2(\text{Impact}_i * \text{NonWhite}_{it}) + \lambda_3(\text{Floyd}_t * \text{NonWhite}_{it}) \\
& + \lambda_4(\text{Floyd}_t * \text{Impact}_i * \text{NonWhite}_{it}) + \epsilon_{int}
\end{aligned} \tag{1.3}$$

where NonWhite_{it} is an indicator variable equal to one if the buyer/seller is not white. In this case, λ_4 is the parameter of interest. If λ_4 is positive, this would indicate that non white buyers (sellers) tend to buy (sell) for higher prices than white buyers (sellers) for properties exposed to Impact Zones after a landmark event.

1.4 Data

Data for this study come from a few different public sources. Information about property transactions is collected from New York City's Department of Finance ("DOF"). The DOF maintains Rolling Sales data which are made up of each real property transaction in NYC from 2003 until present. Each record includes a date, price, unique identifier for the property based on its Borough-Block-Lot ("BBL") combination, property class at the time of the sale, square footage, number of

units, etc. Each property transaction is then supplemented with information from MapPLUTO and the Automated City Register Information System (“ACRIS”). MapPLUTO data contain a geolocation for each property and is instrumental in matching properties into their respective census tract, impact zone, and NTA. ACRIS contains the name(s) of buyers and sellers which helps to identify the ethnicity of those involved in each transaction.

While the time period of interest spans 2010 through 2015, transaction data are collected as far back as possible in order to observe the previous owner’s tenure in the property. This date of purchase (or previous sale) is crucial to identify whether or not an owner lived in the property during an active impact zone. Although properties are identified as being inside an impact zone if they were *ever* in one, there could still be variation in whether the seller had lived through the impact zone. Figure 1.1 displays the relative frequency that a property was inside of an impact zone, provided they were inside at least one. For example, 25% of properties that were ever inside of an impact zone only experienced the effects of the impact zone one time. The modal number of impact zones for a property is six.

Figure 1.1: Relative Frequency of Unique Properties and Number of Times in an Impact Zone

These data indeed contain the residential property sales of interest, but they also contain sales of commercial properties, vacant land, etc. as well as in kind transfers between family members and friends. Therefore, the transactions data are limited to only properties defined as dwellings or homes for one to three families that were built before 2010. Properties with major alterations after

2010 or with a year built less than 1776 are also removed. Properties that are either more than 3 units, more than 3 floors, more than 8,000 square feet, or less than 400 square feet are also removed from the analysis sample. Properties sold before 2010 or twice within 45 days are also removed. Similarly, properties that sell 30 days or less after a landmark event in the development of the court case are dropped as well. This is done to avoid what might be ambiguous expectations of future Stop & Frisk behavior held by buyers and sellers. It is also likely these parties buying or selling a property soon after a land mark event were already in negotiations beforehand, thus muddying the identification. Finally, properties that sold for less than \$50,000 or more than \$2,000,000 are dropped. This last step kicks out about 40% of observations, though 95% of these are sales of \$10 or less, likely indicating transfers among friends or family. This sample is comprised of 104,194 transactions between January 1st, 2010 to December 31st, 2015.

Using census tract level data from the American Community Survey, each transaction is augmented with median income and racial composition (% of population that is white) in the year of sale. Since census tracts in NYC are often times single city blocks, these data are relatively granular with respect to the NTAs. On average, there are about 10 census tracts for each NTA.

The NYPD publicly provide individual records for both crimes and Stop & Frisk incidents with each record containing a geolocation and timestamp. The daily number of crime and Stop & Frisk incidents is displayed in Figures [G.1](#) and [G.2](#) in the Appendix. Both of these are counted for each NTA in rolling windows of 30 days. For each property sale, information on the number of crimes and Stop & Frisk incidents in the property's NTA in the last 30 days is known. Through a Freedom of Information Law (FOIL) request, the NYPD also provided maps of their impact zones for January 2004 to January 2013.² These impact zone maps were digitized by hand and can be seen in Figure [1.2](#).

The Census Bureau maintains an API that returns empirical racial breakdowns of surnames in the 2000 and 2010 decennial census. The data exported for this study include each surname that appears over 100 times in the census and the percent of people with that name that are non-

²The NYPD claimed to not have any more data before or after this period even though Operation Impact technically existed outside this time frame. [J. MacDonald et al. \(2016\)](#) use similar maps from the exact same period as this study.

Figure 1.2: Impact Zones from Jan. 2004 to Jan 2013

Hispanic White. According to the Censes Bureau, these data cover 92.8% of Americans.³ Each buyer and seller name in ACRIS is matched to a surname from the Census’s API. Often times, there are multiple buyers and/or sellers associated with each sale, so both the average and minimum percentage of non-Hispanic White are taken per buyer/seller group per transaction. This allows for a proxy of race and acts as a margin in which some homeowners may have been more victimized by Stop & Frisk policies. Table 1.2 shows the success rate for matching transaction names to Census data. While 25% of observations cannot be matched for neither buyer nor seller, it could be the case that one or both entities are not individuals but rather corporations or estates. Of all the names in ACRIS, 25% include at least one of the strings “LLC”, “INC”, “DECEASED”, “TRUSTEE”, “ASSOCIATION”, etc. Figure 1.3 shows the distribution of the average Non-Hispanic White percentage for both buyers and sellers surnames. To simplify the analyses, those with percentages above 50% are considered to be white buyers/sellers. Table G.2 in the appendix shows the most frequent names with a Non-Hispanic White percentage from 90% to 100% and 0% to 10%.

Table 1.2: Ability to Match Census Surname Race to NYC Sales

	Seller: Non-White	Seller: White	Seller: Missing
Buyer: Non-White	12.29	11.84	10.4
Buyer: White	3.12	11.86	7.99
Buyer: Missing	5.97	11.76	24.77

Summary statistics for the analysis sample are displayed in Table 1.3. The average (deflated) sale price is just over \$470,000. 32% of properties sold in the sample are inside of an Impact Zone, and about two-thirds of these sales had a seller who lived through an active Impact Zone. Of the parties that can be identified, 58% of buyers are non-white compared to 45% of sellers. The average number of crimes and Stop & Frisk incidents within an NTA 30 days before a sale is 333 and 260, respectively. While the average number of Stop & Frisk incidents appears to be lower than that of crimes, the maximum value is almost 2.5 times as large. Finally, 35% of the sample represents the pre-period, while the other 65% has some level of treatment (*e.g.* class certification, trial begin, or trial end).

³<https://www.census.gov/data/developers/data-sets/surnames.2010.html>

Figure 1.3: Distribution of non-Hispanic White % in Surnames

Table 1.3: Summary Statistics for Analysis Sample

Statistic	Mean	St. Dev.	Min	Max
Price (in 2015 dollars)	470,245.50	278,570.50	50,169.24	2,112,409.00
Class Cert.	0.12	0.32	0	1
Trial Begin	0.06	0.24	0	1
Trial End	0.46	0.50	0	1
Impact	0.32	0.47	0	1
Exposure	0.22	0.42	0	1
Non-White Buyer	0.58	0.49	0	1
Non-White Seller	0.45	0.50	0	1
Res. Units	1.85	0.69	1	3
Total Units	1.89	0.71	1	3
SQFT (land)	2,383.76	942.79	359	7,850
SQFT (gross)	2,164.11	858.29	420	7,350
Num. Floors	2.23	0.47	1	3
Median Income	47,345.62	15,768.02	10,674	152,353
Population	3,895.11	1,837.19	423	16,399
White Percent	22.14	22.22	0	99.62
Crime	332.89	196.25	39	1,032
Stop and Frisk	260.39	354.35	0	2,475

Note: Summary statistics for the analysis sample and control group defined by NTA. The sample size for these data is 30,579. However, due to missing data, Non-White Buyer and Non-White Seller only have 16,619 and 17,356 observations respectively.

Figure 1.4 displays a time series of average log sale price for properties in Impact Zones, properties within the same NTA as an Impact Zone and properties 3,000 feet from an Impact Zone. Figure 1.5 shows the difference between the buffer control group and the treatment group. Albeit noisy, the treatment group appears to be a relatively constant distance away the control group until after the trial ends. This would signal the data satisfying the parallel trends assumption.

Figure 1.4: Average Log Sale Price by Quarter

1.5 Results

Table 1.4 reports the estimates of the parameters in Equation (1.1). A key finding from this table is the positive and significant estimate of the coefficient for Trial End x Impact. This result suggests that properties that were within an Impact Zone at least once experienced an increase in value of about 6% once Stop & Frisk was ruled to be unconstitutional as a result of *Floyd, et al. v. City of New York, et al.* This is an important finding as it highlights the disutility generated by

Figure 1.5: Difference in Unconditional Mean Log Price in Buffer Control and Impact Zone Properties

Stop & Frisk for residents in New York City. Another interesting result is the lack of significant coefficient estimates for both Class Cert. $\times$ Impact and Trial Begin $\times$ Impact. This would suggest that property prices did not in fact react to large changes in the quantity of Stop & Frisk, but rather only changed once it became illegal. Moreover, the coefficient for Stop & Frisk incidents is also insignificant. This is further evidence that these housing prices were depressed due to the expectation and existence of Stop & Frisk and not necessarily the intensity of use. In addition, there is very little difference in results between the two ways of constructing a control group.

An important point to this research is to identify who is obtaining this aforementioned 6% increase in property values. In particular, how does this increase vary with trauma from Stop & Frisk? One way to proxy for this would be to figure out which seller had lived through an active Impact Zone compared to those who purchased and sold a property in an Impact Zone without it having been active throughout their tenure. Columns 1 and 4 in Table 1.5 represent the estimated coefficients in Equation (1.2), which show how being exposed to an active impact zone affects housing price. The coefficient estimate for Trial End $\times$ Impact represents a 10% increase (.17 standard deviations) in property values compared to the previous 6%. However, what

Table 1.4: Effect of Stop & Frisk on Housing Prices

	log(Price)			
Stop & Frisk	-0.007 (0.009)	-0.007 (0.009)	-0.006 (0.007)	-0.006 (0.007)
Crime	-0.118** (0.058)	-0.132** (0.061)	-0.114** (0.048)	-0.130** (0.050)
Class Cert. * Impact	-0.001 (0.025)	-0.001 (0.024)	0.001 (0.024)	0.001 (0.024)
Trial Begin * Impact	0.011 (0.027)	0.010 (0.027)	0.001 (0.028)	0.001 (0.028)
Trial End * Impact	0.064** (0.028)	0.063** (0.028)	0.061** (0.025)	0.061** (0.025)
Control Group	NTA	NTA	Buffer	Buffer
Year & Month FE	Yes	No	Yes	No
Year x Month FE	No	Yes	No	Yes
Observations	30579	30579	31179	31179
R2	0.409	0.411	0.420	0.422

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Each regression includes controls for property characteristics, census tract characteristics, and NTA fixed effects. Both crime and Stop & Frisk variables use an inverse hyperbolic sine transformation. Standard errors are clustered at the NTA level.

Table 1.5: Heterogeneous Effects

	log(Price)					
Trial End * Impact	0.103*** (0.036)	0.065 (0.050)	0.082* (0.042)	0.100*** (0.033)	0.075* (0.043)	0.089** (0.038)
Trial End * Impact * Exposure	-0.094** (0.036)			-0.093** (0.036)		
Trial End * Impact * Non-White		-0.033 (0.043)	-0.068* (0.035)		-0.034 (0.039)	-0.061* (0.032)
Control Group	NTA	NTA	NTA	Buffer	Buffer	Buffer
Non-White Type	N/A	Buyer	Seller	N/A	Buyer	Seller
Observations	30579	16619	17355	31179	17577	18381
R2	0.418	0.469	0.442	0.429	0.469	0.438

* p < 0.1, ** p < 0.05, *** p < 0.01

Note: Each regression includes controls for property characteristics, census tract characteristics, NTA and year-by-month fixed effects, and crime and Stop & Frisk controls using an inverse hyperbolic sine transformation. Full sets of interactions are included but not reported to save space. Standard errors are clustered at the NTA level.

is more interesting is the negative 9% for those exposed to active impact zones. This provides some evidence that those most impacted by Stop & Frisk are not the ones realizing increases in property value.

Another way to tease out where this effect was strongest is to break down sales by race. Since Stop & Frisk mostly affected minorities, it is reasonable to think there might be a different impact for white and non-white individuals. Identifying race as discussed previously, Table 1.5 compares white and non-white buyers and sellers. Columns 2 and 5 show results for non-white buyers but do not coefficients that are able to be distinguished from zero. Columns 3 and 6 show results for non-white sellers and do find significant coefficients. The parameter estimate for Trial End x Impact is positive and therefore implies an overall 8% increase in property values after the case closes. However, the negative coefficient for Trial End x Impact x Non-White suggests this increase is only experienced by white sellers. This is further evidence of a lasting trauma from Stop & Frisk.

Since this model specification could be sensitive to the definition of non-white, the model is re-estimated for every cutoff from 30% to 55%. Each coefficient estimate of Trial End x Impact x Non-White with its corresponding 90%, 95% and 99% confidence interval are plotted in Figure 1.6. As the cutoff value decreases, or moves towards zero, the definition of non-white becomes more

(a) Non-White Buyer

(b) Non-White Seller

Figure 1.6: Varying Cutoffs in Defining “Non-White”

strict. Therefore, 30% represents the most strict definition while 55% is a more lenient definition. Since the identification is more strict towards 30%, the effect should get stronger although its precision is ambiguous.

Panel 1.6a shows how the coefficient changes for non-white buyers. The result is an always an estimate that is indistinguishable from zero and relatively constant over the range of cutoff values. On the other hand, panel 1.6b shows the coefficient estimate becoming more negative and more significant as the cutoff decreases. If anything, it appears that the strength of the results in columns 2, 3, 5, and 6 of Table 1.5 are likely understated given the relatively conservative cutoff used of 50%.

While Stop & Frisk was wide spread throughout the city, Operation Impact was more concentrated in specific boroughs. In particular, Figure 1.2 shows that most of the Impact Zones occurred in Brooklyn and Queens. Consequently, 80% of the observations from the previous analyses come from these two boroughs. The breakdown by borough of properties as a percent of the entire sample is displayed in Table 1.6. Since Operation Impact was relatively sparse in residential areas of Manhattan, the Bronx and Staten Island, the sample is limited to only Brooklyn and Queens. The

regressions in Table 1.5 are re-estimated after dropping these three boroughs and the results are displayed in Table 1.7. The results from these regressions are qualitatively similar to Table 1.5 albeit with stronger significance in Columns (3) and (6).

Table 1.6: Percent of Analysis Sample by Borough

	Manhattan	Bronx	Brooklyn	Queens	Staten Island
NTA	0.96%	10.09%	42.80%	40.43%	5.73%
Buffer	0.96%	13.41%	46.30%	35.18%	4.16%

1.6 Discussion

This research uncovers empirical evidence of the amenity value of a particular policing tactic, namely Stop & Frisk. While the literature does not support the efficacy of Stop & Frisk to reduce major criminal activity (via Broken Window policing), there is evidence that Stop & Frisk reduced lower level crime (Bacher-Hicks & de la Campa, 2021a; J. MacDonald et al., 2016; Weisburd et al., 2016; Wooditch & Weisburd, 2016). Since the literature appears to be in consensus about whether crime affects housing prices, and it appears that violent crime is the major driver (Ihlanfeldt & Mayock, 2010; Tita et al., 2006), then it should not necessarily be a surprise that Stop & Frisk would not be considered an amenity value. However, this study estimates a negative effect of Stop & Frisk on housing values, suggesting the policing tactic is in fact a disamenity.

This study uses plausibly exogenous spatial and temporal variation in the intensity of Stop & Frisk across New York City to isolate this effect. Properties located in Impact Zones were subject to an increased number of Stop & Frisk incidents relative to properties just outside (J. MacDonald et al., 2016; J. M. MacDonald & Fagan, 2019). This is due to the NYPD flooding these areas with extra officers in order to control crime. This study also uses variation in Stop & Frisk via landmark events in a federal class action lawsuit, Floyd, *et al.* v. City of New York, *et al.* Each of these moments in the court case generated distinct changes in the utilization of Stop & Frisk. Therefore, properties that were subject to Operation Impact are compared to nearby properties that were not, before and after these events. Properties in Impact Zones saw increases in value that were larger

Table 1.7: Heterogeneous Effects, Brooklyn and Queens

	log(Price)					
Trial End * Impact	0.112** (0.049)	0.073 (0.059)	0.099*** (0.023)	0.106*** (0.023)	0.072*** (0.021)	0.099*** (0.023)
Trial End * Impact * Non-White		-0.015 (0.045)	-0.080** (0.035)		-0.015 (0.028)	-0.078** (0.035)
Trial End * Impact * Exposure	-0.096** (0.042)			-0.096*** (0.025)		
Control Group	NTA	NTA	NTA	Buffer	Buffer	Buffer
Non-White Type	N/A	Buyer	Seller	N/A	Buyer	Seller
Observations	25442	14726	15425	25405	15028	15806
R2	0.405	0.460	0.435	0.407	0.457	0.428

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Each regression includes controls for property characteristics, census tract characteristics, NTA and year-by-month fixed effects, and crime and Stop & Frisk controls using an inverse hyperbolic sine transformation. Full sets of interactions are included but not reported to save space. Only observations from Brooklyn and Queens are used, since the majority of Operation Impact took place in those two Boroughs. Standard errors are clustered at the NTA level.

than the increase in the comparison groups. This effect is somewhat of a “catch-up”. In other words, the properties inside Impact Zones were selling for less than the comparison properties until after Stop & Frisk was ruled unconstitutional, at which point the properties were relatively equal in value.

Similar to other studies on Stop & Frisk, this work uncovers a lasting, “traumatic” effect on citizens most exposed to the practice. While there was an increase in the value of properties in Impact Zones, there is substantial heterogeneity in those realizing this increase. In particular, the effect size for those who owned the property during an active impact zone, or those most likely to be of African or Hispanic descent, obtain less of a premium, if at all. The literature has documented detrimental impacts of Stop & Frisk on mental health, educational outcomes, and now property values.

Chapter 2

Information Avoidance and Celebrity

Exposure: The Effect of “Magic” Johnson on AIDS Diagnoses in the U.S.

2.1 Introduction

The AIDS pandemic is one of the largest public health crises in the 20th century. While there is typically a large degree of informational asymmetries between health experts and at-risk populations which exacerbate similar contagions, the AIDS pandemic is in many ways defined by it. As the virus became increasingly prevalent throughout the world, scientific understandings of its origins, causes, and consequences rapidly increased, while the general public’s understanding lagged behind. Though a substantial majority of Americans during the AIDS pandemic understood how HIV could be contracted, there existed large gaps in knowledge regarding its transmission risks between heterosexual and homosexual sex ([Herek, Capitanio, & Widaman, 2002](#)). We contend that this behavior is representative of what the Economics literature refers to as information avoidance, wherein biased beliefs directly enter agents’ utility functions and create an incentive to avoid infor-

mation even in situations where it is both low-cost and useful ([Golman, Hagmann, & Loewenstein, 2017](#)).

There are few, if any, examples of shocks to this belief during the AIDS pandemic that are greater than when Earvin “Magic” Johnson publicly announced that he had contracted HIV on November 7, 1991. In this paper we present evidence that Magic Johnson served as a catalyst in the United States for rapidly changing the perception of who was at risk for contracting HIV. In doing so, we identify a powerful public health mechanism by linking the observations found in the literature of both information avoidance and celebrity exposure.

We document the effects of Johnson’s announcement in both public knowledge of, and testing for, AIDS through the use of data from the the National Health Interview Survey (NHIS). Our analysis reveals marked changes in the perceptions of risk amongst those who had never been tested for HIV prior to Johnson’s announcement. We compliment this finding by analyzing the the AIDS Public Information Data Set (APIDS) from the Centers for Disease Control and Prevention (CDC) which provides individual-level data for every AIDS diagnosis at a monthly interval across all major metropolitan statistical areas (MSA) in the United States. Crucially, the data also contains information regarding the age, gender, race, and transmission category of those diagnosed – the latter of which allows us to estimate the effect of Johnson’s announcement on groups of individuals with potentially very different prior beliefs regarding their probability of having developed AIDS.

Given the prevalence and extreme stigmatization of AIDS in the gay community at the time, we posit that homosexual people were much more likely to be aware of their chances of contracting HIV relative heterosexual people. We therefore expect the effect of Johnson’s announcement to be predominantly concentrated amongst heterosexuals. To this end, we employ difference-in-differences empirical framework wherein, of a population of people with undiagnosed AIDS infections, homosexual men serve as the control group and heterosexual men are the treated group. Using Johnson’s announcement as an exogenous shock to popular knowledge and perceptions of HIV, we demonstrate that there are large and statistically significant increases in the number of AIDS diagnoses amongst heterosexual men immediately following the announcement.

Given the high stature of Johnson as a professional basketball player at the time, we rely on a variable indicating whether there is a National Basketball Association (NBA) franchise located within the MSA of one's diagnosis in order to proxy for pre-announcement exposure to Johnson as an athlete. This addition reveals that exposure to Johnson before his announcement acted as an important explanatory variable in describing differences in diagnoses between groups.

The organization of the remainder of the study is as follows. Section 2 reports the findings within the academic literature relating to the concepts and events discussed. Section 3 provides background information of the setting during which the studied events take place, and includes descriptions of all of the data employed in the analysis. Section 4 focuses on the empirical identification strategy while section 5 describes our methodologies. Section 6 describes the primary results, and the study concludes with section 7.

2.2 Literature Review

We primarily rely upon the findings from two specific subsections of academic literature in order to help reinforce our hypothesis. While broadly related to Economics, these research papers are largely interdisciplinary and help to build the bridge from related fields in academics. The two sections that we draw the most influence from are Health research which focus on the consequences of information avoidance, and how exposure to celebrities can influence health-related attitudes, behavior and outcomes.

2.2.1 Health Consequences of Information Avoidance

Information avoidance can have serious health consequences not only for individuals but also for entire communities. Often the sooner that one seeks treatment for an illness, the quicker that treatment and the potential for healing can begin. Yet the fear of receiving negative news pertaining to one's health can adversely affect people's willingness to seek out information regarding their vulnerability to, or probability of transmitting, any illness. [Golman et al. \(2017\)](#) formalized the term

“information avoidance” in the Economics literature by analyzing this type of behavior through the lens of rational-agent decision-making.

Economists typically assume that information is valuable as a tool for reducing uncertainty in decision-making processes. As a result, forward-looking, utility-maximizing agents will purchase and consume information up until the point where its marginal benefit is zero. The novel addition of economic theorists was to build models of decision-making wherein it is optimal to avoid seeking out information when biased beliefs enter the agent’s utility function – even if that information is useful, free, and independent of strategic considerations.

Personal health is one area where the literature identifies information avoidance behavior. [Kőszegi \(2003\)](#) provides some of the first theoretical support that is related to this concept by presenting a model where patient’s anxious beliefs about the state of their health in the next period enter their utility function. The model allows patients to endogenously chose how the information they gather will affect their beliefs and, thus, utility. If the patient is risk averse, then they will exchange the anxiety of the bad news for the decision-making value of knowing the state of their health.¹ As a result, the model predicts that patients can refuse useful, lost-cost information should their anxiety be sufficiently high.

Prior to 1991, many Americans exclusively associated HIV/AIDS with homosexual men despite research documenting the risks for HIV transmission among heterosexuals ([Padian, Shiboski, and Jewell 1990](#); [Peterman, Stoneburner, Allen, Jaffe, and Curran 1988](#)). We posit that this false belief led many groups, which were largely unaffiliated with the gay community, to systematically underestimate their own probability of contracting and transmitting the disease. Beliefs and attitudes toward HIV within the United States also reflected a strong bias toward people living with AIDS (PWA) which further exacerbated one’s unwillingness to get tested. For example, [Herek et al. \(2002\)](#) report that in 1991 one-in-three Americans said that they feared people living with HIV/AIDS, and that one-in-four admitted to feelings of anger or disgust towards them.

¹The only assumption for risk adverse agent in this context is one who derives more disutility from bad news than they would gain from an equal amount of good news.

Unfortunately, these misconceptions affected more than either prejudice towards PWAs or overly cautious behavior in those without the virus. Of those in already at-risk communities, [Stokes and Peterson \(1998\)](#) finds that psychological distress as a result of community stigmatization is a likely indicator for even less safe behavior. For those in historically safer communities, [Valdiserri \(2002\)](#) argues that the high costs of HIV diagnoses, be they tangible costs such as diagnostic or healthcare service costs, or non-tangible costs such as time, lifestyle changes, stigma or expectations of a loss of job, serve as a strong disincentive for seeking HIV testing. While traditional economic theory states that this type of information-avoidance behavior is optimal, theoretical work and empirical studies have repeatedly identified that information shocks can cause individuals to radically change their behavior upon simply updating their beliefs.

2.2.2 Celebrity Exposure and Health Behavior

Incentivizing individuals to seek out their current health status can be very difficult. Celebrities from film, television, and sports often play a role in marketing products as their endorsement encourages consumers to seek out new experiences. Studies have found that celebrities can affect discrimination ([Alrababah, Marble, Mousa, & Siegel, 2019](#)), election outcomes ([C. Garthwaite & Moore, 2013](#)), book sales ([C. L. Garthwaite, 2014](#)), and tax delinquency ([Garz & Pagels, 2018](#)). As these studies show how celebrities can affect public consumption, beliefs, and attitudes, a growing literature in the field of health economics identifies how celebrities make individuals aware of unknown health risks.

Closely related to our analysis of Magic Johnson, [Nattinger, Hoffmann, Howell-Pelz, and Goodwin](#) and [Cram et al.](#) find that celebrity endorsements can inform personal health decisions. [Nattinger et al. \(1998\)](#) estimates the effect of Nancy Reagan's mastectomy on the choice of surgery for breast cancer in American women. They find that women who resemble Reagan demographically are less likely to choose breast-conserving surgery, which is an alternative to a mastectomy. Also, [Cram et al. \(2003\)](#) measures the "Katie Couric Effect", which refers to how the newscaster used her celebrity to campaign for colon cancer screenings. Results show an increase in screenings

among women after Couric’s campaign. Each study provides strong evidence that people feel more comfortable seeking out information once a demographically representative public figure is also seen as vulnerable to illness.

Several studies have qualitatively analyzed the effect of Magic Johnson’s public HIV announcement on beliefs and attitudes toward both the illness and PWAs. [Pollock III \(1994\)](#) uses survey evidence collected in Florida directly before and after Johnson’s announcement to show a shift in opinion from heterosexuals through increased support for the amount of money dedicated towards AIDS treatment and research. Importantly, this shift in opinion did not exist for homosexuals. [Casey et al. \(2003\)](#) review several small studies further investigating Johnson’s effect on health-related attitudes, beliefs, and behavior. They find changes in the number of people being tested after the announcement, beliefs regarding their vulnerability of contracting HIV, and willingness to obtain more information about HIV and AIDS.

2.3 Data and Setting

Deaths as a result of AIDS, or acquired immunodeficiency syndrome, were first noticed in the United States within clusters of intravenous drug users and homosexual men in 1981. Often these patients developed a previously very rare skin cancer known as Kaposi’s sarcoma which received national media attention – sparking the beginning of the idea that the new contagion was a “gay cancer”. Due to the population of patients experiencing these symptoms, coupled with widespread prejudice towards them at the time, the general media coined the term “GRID” or gay-related immune deficiency. Even scientific agencies such as the CDC initially labeled AIDS as a “4H” disease referring to four of the most at-risk patient types – homosexuals, heroin users, hemophiliacs and Haitians ([U.S. Health & Human Services, 2019](#)).

While institutions such as the CDC and the general media eventually chose more objective and public-health conducive descriptors for AIDS, the lasting-impact of their initial mistakes remained. This perception emboldened some who interpreted the pandemic as confirmation of long-held

religious or moral beliefs which regarded homosexuality as immoral, but the naive beliefs of the general American public regarding the causes and consequences of the AIDS pandemic was much more widespread.² For example, [Herek et al. \(2002\)](#) shows that at the height of the pandemic 50% of Americans believed that one could contract human immunodeficiency virus (HIV) either by being coughed/sneezed on by, or through sharing a drink out of the same glass as someone with AIDS. Indeed, many Americans during the AIDS pandemic were unaware that HIV could be contracted via heterosexual sex.

In the history of the AIDS pandemic, there are few examples of shocks to public opinion or knowledge greater than that of Earvin “Magic” Johnson. Widely considered to be one of the greatest basketball players of all time, Johnson was not only the face of the fastest growing sport in the United States, the NBA, but of all American sports. Perhaps most importantly for our identification strategy in this study, Johnson represents a widely adored, non-injection-drug using, heterosexual male.

On November 7, 1991, Johnson suddenly and unexpectedly announced that he had tested positive for the virus which is responsible for the onset of AIDS, the human immunodeficiency virus (HIV), as a result of promiscuous, unprotected, heterosexual sex. He immediately retired from the NBA. This occurred not only at the height of his professional career and fame, but also in the lead up to the peak of the AIDS pandemic within the United States.

2.3.1 Magic Johnson and The National Basketball Association

At the start of the 1980s, the rapidly growing NBA was dominated by the Los Angeles Lakers and the Boston Celtics. Magic Johnson and Larry Bird were the stars of each team respectively – seeing both quickly became some of the most famous athletes in the country regardless of sport.³ Prior to the 1979-80 season, Johnson was selected with the first overall pick in the NBA draft. Over the

²In 1993 then world-famous televangelist Billy Graham rhetorically asked an audience, “Is AIDS a judgment of God?... I could not say for sure, but I think so” ([Petro, 2015](#)).

³His fame and recognition persists to present day. Johnson was recently ranked as the 5th best basketball player of all time according to a 2020 list on the subject done by ESPN.com.

course of his career from 1979 - 1991, Johnson was a 5x NBA champion, a 3x NBA finals MVP, a 3x NBA MVP, a 12x All-Star and 9x All-NBA First Team member. Johnson, with the help of Bird and later Michael Jordan, helped to considerably improve the image and direction of the league. However, even with the increasing popularity of the NBA, many games were still either played on a regional television station or were not broadcast at all. Given the nature of regional television contracts from the 1980s and 1990s, in addition to the fact that many NCAA or NBA basketball games could occur at the same time, television providers would almost always broadcast the games which occurred geographically closest to the consumer.

2.3.2 AIDS Public Information Data Set

The CDC began tracking individual AIDS diagnoses across metropolitan areas with populations of more than 500,000 persons beginning in 1981.⁴ The data include information such as an individual's age, gender, race, vital status as of 2000, main transmission category, gender and sexual orientation/behavior. In Table 2.1, we break down the number of non-adolescent, non-pediatric cases (20+) from September 1987 through December 1992 in MSAs by reported sexuality and transmission category.

Table 2.1: Tabulation of Sexuality and Relevant Transmission Categories, Full Sample

Transmission Category	Sexual Identity and Gender			
	Homosexual	Bisexual	Heterosexual	Female (all)
Homosexual Sex	104,197 ⁺	22,333	111	0
IV Drug Use	1	0	43,459 ⁺	16,517
MSM & IV Drug Use	11,166 ⁺	5,848	391	0
Hemophilia	0	0	1,361	72
Heterosexual Sex	0	0	4,783 ⁺	10,338
Blood Transfusion	0	0	1,745	1,305
Unidentified / Not Reported	0	0	6,958	2,196

Note: Individuals included in the samples used for the primary analyses are marked with ⁺.

Sexuality and gender are of ultimate importance to our identification strategy. A unique feature of these data are that they are self reported behaviors. Further, the CDC follows up and investigates

⁴It appears that the CDC uses the 1999 PMSA definitions and the 1990 NECMA definitions. While MSAs change over time, we make sure to capture the same counties over our sample period instead of MSAs for this reason.

each case in APIDS. In fact, the CDC updated and changed information in the data to reflect the results of their investigations. This allows us to trust the given data as is, though the CDC occasionally handles missing data in unintuitive ways. For example, if a sexuality is missing, they lump it in with MSM rather than identify the absence. Similarly, missing ages are set in the 35-39 group.

We create our sample at the “community”, or MSA-by-sexuality, level by observing counts of diagnoses of the groups from Table 2.1 over time. The first group, which would act as our control group within a traditional difference-in-differences framework, consists of homosexual men who contracted AIDS via sex with another man or through intravenous drug use. Conversely, our “treated” group consists of heterosexual men who contracted HIV through sex with a woman or through intravenous drug use. We omit other groups provided in the data (women and bisexual men) from our primary analysis due to the fact that our identification strategy relies upon clear distinction of sexual identity. In this context, sexual identity is likely to be highly correlated with (a proxy for) beliefs regarding one’s probability of contracting HIV. Following this, we also omit certain situations in the data that would not pertain to individuals knowing their risk or being willingly tested. For example, we omit individuals with AIDS diagnoses due to Hemophilia or blood transfusions. We also drop observations of individuals without an identified transmission category.

We begin our sample in September 1987 to account for two important milestones in the HIV and AIDS timeline. First, in March 1987, the FDA approval and release of the first medication for AIDS: azidothymidine (AZT). Beginning the sample in September allows us to avoid this large change in medical treatment during the early parts of the sample. Second, there is a definition change for AIDS that takes place in August 1987. Beginning the sample in September, we avoid any oddities that come with changing definitions. We then end our sample in December 1992, at the peak of AIDS diagnoses in the United States. There are three reasons we choose to end the data at this point in time. First, doing so adds simplicity in estimating an econometric model.⁵This

⁵Allowing the data to continue beyond this point would require more complicated estimation techniques in order to account for the natural downturn in the data.

allows for 13 periods of observation in order to accurately estimate any effect resulting from the announcement. Estimation beyond this period of time increases the likelihood of detecting spurious effects of later developments in the rapidly changing landscape of HIV/AIDS.

Second, in order to maintain a homogeneous time-to-death period over the data, we need to again avoid any large medical advancements which substantially change life expectancies of living with AIDS. At the end of 1995, the FDA approved the first protease inhibitor which led to a new era of highly active antiretroviral therapy (HAART). This drug dramatically improved the treatment and lifespan expectations for PWAs. As discussed in greater detail in the following section, we utilize AIDS related deaths two years into the future in order to proxy for the underlying population with HIV by summing the number of diagnoses by month, MSA, and transmission category starting from August 1987 to December 1992. This five- to six-year period represents a time period when options for treatment and testing remain constant over time.

Third, the definition of what constitutes an AIDS diagnosis again changed in January 1993 as the medical community learned more about the disease. According to the CDC, there were three major definition changes: 1985, 1987 and 1993. Since our sample starts in 1987 and ends in 1993, we are able to avoid any major definition changes regarding HIV/AIDS diagnosis.

Having established our sample definitions and time frame, we also omit certain MSAs due to their status as statistical outliers in the data. We begin with 104 MSAs, and order them by the number of diagnoses they had. We drop the top three MSAs which are New York (28,097 cases), Los Angeles (13,077 cases) and San Francisco (11,270 cases). We then drop the bottom 40 cities since NBA MSAs never appear so low in the ordering. Next, we remove San Juan to only keep MSAs in the contiguous United States.⁶ We are left with 59 MSAs in our main analyses, but we vary the sample in the robustness section to ensure the internal validity of our results.

We display summary statistics for our data in Table 2.2. Of note, the sample size in our analysis is double what is reported here due to the fact that we create different “communities” based on sexuality for the analysis. In other words, we observe 3,776 MSA-level observations, each of

⁶We would have also removed Honolulu, though this is dropped with the bottom 40.

which contain a homosexual and heterosexual “community”. We report N by observed MSA since this is the level at which we observe the variables reported. *Magic* is an indicator variable for the observations after October 1991. The *NBA* and *CARE* variables are binary indicators for whether there is an NBA team in a given MSA, and whether an MSA received funding as part of the Ryan White CARE Act. Lastly, we include data on population size, AIDS related deaths, which we discuss in the coming sections, and overdose deaths.

Table 2.2: Sample Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Max
Homosexual Diagnoses	3,776	17.087	17.099	0	107
Heterosexual Diagnoses	3,776	5.865	7.871	0	52
Homosexual Index	3,648	117.407	101.553	37.4	773
AIDS Deaths _{$t+24$}	3,776	23.109	20.524	0	128
OD Deaths _{$t+24$}	3,776	1.919	3.024	0	30
Magic	3,776	0.219	0.413	0	1
NBA	3,776	0.407	0.491	0	1
CARE	3,776	0.339	0.473	0	1

2.3.3 NCHS Multiple Cause of Death Data

The National Center for Health Statistics (NCHS) maintains records of every death occurring in the United States. Using these records, researchers can identify when and where a person died as well as the causes of that person’s death. We extract all records of HIV/AIDS related deaths from 1987 through 1995 and then aggregate a sum for each month and MSA combination in order to perform our analysis.

A combination of information from these two data sources allows for the creation of Figure 2.1 which shows the aggregated number of AIDS diagnoses and deaths per month within our data sample. The upper trend line represents the number of diagnoses while its lower counterpart shows number of deaths as a result of AIDS. The leftmost vertical line in Figure 2.1 illustrates the first date used in defining the sample for our analysis. This line represents January 1987 which coincides with the beginning of the CDC’s efforts to formally record deaths as a result of AIDS in

the United States. The second line from the left represents November 1991, the date of Johnson's announcement while the third vertical line is the bound for where our sample ends. This date is December 1992 and corresponds with the highest number of AIDS diagnoses observed in a given month within in the United States. The rightmost line represents the peak number of deaths as a result of AIDS, occurring in December 1994.

Visually, it seems that a 24 month lag of AIDS deaths is highly predictive of its diagnoses. Further, a lag of 24 months gives us the highest R^2 (as opposed to any other number of lags) when trying to predict diagnoses using deaths. While imperfect, this visual evidence coupled with the R^2 is our justification for using AIDS related deaths in $t + 24$ as a control for underlying AIDS prevalence in month t .⁷

Figure 2.1: AIDS Diagnoses & Deaths in the CDC's selected MSAs

2.3.4 National Health Interview Survey

The National Health Interview Survey (NHIS), beginning in 1957, has monitored and collected information regarding the health of those living in the United States. We employ data from the 1991

⁷Lemp, Payne, Neal, Temelso, and Rutherford (1990) report that the median survival for patients diagnosed with AIDS was 12.5 months, with a 5-year survival rate of 3.4%. While insightful, these statistics were gathered between July 1981 and December 1987 – before the introduction of AZT treatment which was shown to prolong the lifespan of patients with AIDS (Fischl et al., 1987).

survey in order to evaluate public conceptions about HIV/AIDS.⁸ This survey gives us the ability to identify any sudden changes in popular conception resulting from Johnson’s announcement. Specifically, we look at variables related to media exposure, knowledge, risk assessment, and testing. We discuss the NHIS survey questions in more detail with regards to our identification strategy in Section 4.

2.3.5 Homosexual Population Index

Walther and Poston Jr (2004) created indices for gay and lesbian partnering in large metropolitan areas in the United States by using data from the 1990 census. We are able to utilize this measure in order to roughly estimate the population of homosexual men and women within an MSA since the data overlaps with our sample period and the indices cover a large majority of the MSAs in our sample.⁹ In particular, we use their index which estimates the number of homosexual males per 100,000 males aged 18 and older. We couple this with yearly population data from the US Census Bureau’s county estimates and adjust for MSA definition changes.

2.3.6 Ryan White CARE Act

Two major events in the history of the AIDS pandemic in the United States were the diagnosis and death of American teenager Ryan White. A hemophiliac, White contracted HIV as a result of a tainted blood transfusion in December 1984. He died of AIDS-related health complications in April 1990. An otherwise normal kid from Indiana, the perception of White’s “innocence” (relative to the “guilty” homosexuals and drug-users) propelled him to become an symbol for the harsh realities facing PWAs.

⁸NHIS is a cross-sectional household interview survey, which contains approximately 35,000 households and 87,500 individuals. This dataset is used by several government agencies “to track the progress of toward achieving national health objectives”.

⁹We do not have index estimates for Ann Arbor MI, Colorado Springs CO, Columbia SC, Daytona Beach FL, Fort Wayne IN, Las Vegas NV, McAllen TX, Mobile AL, Orange County CA, San Juan PR, Sarasota FL, Stockton CA, Vallejo CA, Ventura CA, Wichita KS, Wilmington DE, or Youngstown OH. This accounts for 19% of the full sample.

Moved by stories such as his, Congress passed the Ryan White Comprehensive AIDS Resources Emergency (CARE) Act in August 1990 shortly after White’s death. This policy was meant to provide funds to cities that were most impacted by the virus. Over the course of our sample, the funding from the Act was dispersed to 25 MSAs with the highest prevalence of HIV, and was the largest federally funded program in the United States for PWAs.

Table 2.3: CARE Funding and NBA Team breakdown

	NBA Team	No NBA Team
CARE Funding	13	11
No CARE Funding	13	65

2.4 Identification Strategy

Our identification strategy leverages documented differences in the knowledge of and attitudes toward contracting HIV between heterosexual and homosexual men in the United States prior to November 1991. Theoretically, information avoidance posits that rational, utility-maximizing agents can optimally avoid seeking out information when biased beliefs enter the agent’s utility function.¹⁰ Thus, our prediction is that sufficiently strong information shocks will serve to adjust individual’s underlying biased beliefs which, in turn, will result in changes to health-related behavior. The primary method through which we expect that individuals would respond in this context would be through increased testing for HIV.

As the largest newspaper in the United States at the time, The New York Times provides evidence which both supports the mechanisms elaborated in our hypothesis. On December 7, 1991, The New York Times reported that HIV tests were up 60% in New York City following Magic Johnson’s HIV diagnosis.¹¹ This increase could be accredited to the fact that New York City was the epicenter of the AIDS crisis in the US. However, given that we do not see a comparable increase in AIDS diagnoses over the same time period, we posit that this shock is causing large,

¹⁰Biased beliefs create an environment which does not perfectly mirror reality. The degree to which one’s reality is distorted is a function of the degree to which the endogenous belief is biased.

¹¹See Table H.1 in the Appendix.

positive changes in health-related behavior. We add to the findings from The New York Times by providing an analysis of data from the National Health Interview Survey and the National HIV Serosurveillance Summary.

2.4.1 Changing Attitudes towards AIDS

The news that “Magic” Johnson had contracted HIV was instant national news and became a front-page headline all across the United States. Figure [H.2](#) in the Appendix calculates the number of mentions of Johnson’s name by day – peaking at nearly four times the number of average mentions as the basketball player had typically received.¹² We compliment this finding with data from the 1991 National Health Interview Survey. There are three questions in particular which ask about the primary media source through which the respondent obtained information regarding HIV or AIDS. We track the percentage of respondents that report hearing about HIV or AIDS over television, radio or print by week of the year. Figure [H.3](#) in the Appendix displays the time series of these percentages. We find large and significant increases in information regarding at the exact week of Magic Johnson’s announcement through each medium.

However, this only presents evidence that people were likely aware of Johnson’s announcement. In Table [2.4](#), we analyze several questions that test respondents’ knowledge of HIV/AIDS. The first row displays results of a survey question that asks subjects if a person can have AIDS but look and feel healthy. The row shows the results of a question about whether a person can have the AIDS virus (HIV) without having developed AIDS. The third survey question asked if respondents had heard of the drug AZT, the drug which was prescribed for extending the life of patients with HIV. Lastly, the fourth question measures the percent of respondents who had not received a test for HIV since they did not believe that they were at risk. The first four rows in Table [2.4](#) indicate increased knowledge and understanding of AIDS immediately following Johnson’s announcement.

Using the same methodology, we analyze questions about respondent’s beliefs of the chances that the respondent had AIDS at the time of the survey and/or the chances of them developing AIDS

¹²These data comes from newspapers.com’s API, a subsidiary of Ancestry.com

Table 2.4: Questions Relevant to AIDS Knowledge

Survey Question	Pre-Magic	Post-Magic	p-value
Have AIDS, Feel Healthy	0.909	0.932	9.11e-11
Have Virus, Not AIDS	0.922	0.952	1.76e-21
Heard of AZT	0.519	0.577	2.44e-17
No Test b/c No Risk	0.913	0.897	9.56e-04
Chance of Having AIDS (w/ Test)	0.263	0.295	0.043
Chance of Getting AIDS (w/ Test)	0.357	0.384	0.106
Chance of Having AIDS (w/o Test)	0.178	0.209	1.34e-06
Chance of Getting AIDS (w/o Test)	0.250	0.270	2.79e-03

in the future in the last four rows of Table 2.4. First, respondents were asked if they had ever had an HIV/AIDS test in the past. We use this variable to differentiate between groups of respondents – those who are more familiar with their risk (those who had had a test in the past) and those who are less familiar with their risk (those who had never had a test in the past).¹³ Next, respondents were asked about the chances of having AIDS at the moment and the chance of developing AIDS in the future. Respondents were allowed a categorical answer, either high, medium, low, or none. Accordingly, we coded these responses as binary variables – 1 if the response fell into one of these categories and 0 if they reported “none”. As a result, these decimals can be interpreted as deviations from the “none” category. We observe increases in all four groups, but find results are more precisely defined for respondents who had never been tested before.

Of particular interest for our analysis, there is a survey question regarding the expectations of being tested in the future. Respondents were asked if they expected to have an AIDS test in the next twelve months. In Figure 2.2, we show the differences in stated expectations given previous testing history. We find a modest increase in the expectation of being tested in the future for those who have previously been tested following Johnson’s announcement. However, for those who had never been tested, we see a much larger increase. This evidence, coupled with our earlier results in Table 2.4, suggests that those familiar with their risk level did not update their future expectations as much as those who were less familiar. Moreover, while there is a relatively larger increase in

¹³15% (78.5%) of all survey respondents reported having (not) had an HIV/AIDS test in the past. 93.4% (N = 42,725) of respondents answered this question.

the those who have never been tested, it is still the case that those who had been tested before were more likely to be tested in the future.

To conclude, through media outlets and the NHIS, we find suggestive evidence that people not only updated their beliefs about the possibility of HIV infection through hearing about Johnson’s announcement, but also that these people reported an increased willingness to be tested in the future. These results taken together provide strong evidence that Johnson’s announcement acted as an information shock to the people’s beliefs with regards to their vulnerability to HIV.

Figure 2.2: Percent of Respondants Expecting to be Tested for AIDS in the Next Year

2.4.2 AIDS Diagnoses

An additional part of our identification strategy is the use of AIDS rather than HIV diagnoses. While this choice was brought about due to data constraints, as the CDC only reports AIDS diagnoses, there are many reasons we believe this does not substantively limit the interpretation of our results. In fact, employing AIDS rather than HIV diagnoses, or even tests, implies that our results will be biased downward due to the fact that it is much more likely that one will be diagnosed with HIV than its more progressed form in AIDS.

An AIDS diagnosis in the early 1990s was an almost certain death sentence for many, particularly the poor ([Lemp et al., 1990](#); [Shayne & Kaplan, 1991](#)). However, it was not an *instant* death sentence due to the nature of HIV/AIDS which, among many other things, dramatically weakens the immune system leaving one vulnerable to minor external health shocks ([Hogg et al., 1997](#)). The definition, which differentiates patients who are living with HIV from those who have developed AIDS, is typically much more clear in the eyes of physicians than it is for patients due to the fact that PWAs typically do not notice any substantially change in well-being when moving beyond the threshold for having developed AIDS.¹⁴ Though patient's lives are at great risk following any health shock or sickness, PWAs can be largely asymptomatic with very low CD4 levels as a result of how the virus typically progresses. For example, patients experience the majority of their HIV-related symptoms during the first 2-4 weeks after infection.^{15,16}

The fact that potentially significant portions of the population with T-cell counts low enough to have developed AIDS can live largely asymptotically bolsters our identification. Further, the [Panel on Antiretroviral Guidelines for Adults and Adolescents \(2019\)](#) notes that clinicians may fail to recognize HIV infection in patients who are symptomatic due to the fact that early symptoms are often very similar to those of many other viral infections such as influenza and infectious mononucleosis. Even today many remain unaware of their HIV+/AIDS status. The CDC estimated in 2016 that 1.1 million people over the age of 13 lived with an HIV infection, of whom

¹⁴One requires 200 or less CD4+ T cells (compared to the 1000 avg) and/or for one's CD4+ T cells to consist of less than 14% of all lymphocytic cells to be diagnosed with AIDS([Garcia & Guzman, 2018](#)).

¹⁵The CDC describes the three stages of disease that one typically progresses through when people get HIV (without modern treatment). The first stage is defined as an "acute HIV" which occurs within 2-4 weeks after infection. One typically has a large amount of the virus in one's blood during this stage. Further, one is very contagious during this period and will likely experience a flu-like illness which can persist for a few weeks as a result. The transition to the second stage is formally referred to as clinical latency, where HIV is still active but the rate at which the virus reproduces slows dramatically. This amount of time largely varies from patient to patient and is a function of the amount of one's CD4 cells pre-infection. As a result, one may not have any symptoms during this time. It is for this reason that this stage is occasionally referred to as asymptomatic HIV. The third stage is when one's HIV infection has eroded away sufficiently enough CD4 cells in order to progress into AIDS. From here, the degree to which one presents symptoms is a function of how quickly their viral load and CD4 count change inversely. For example, if two patients progress into stage 3 at the same time, the patient who has spent less time in stages 1 and 2 of infection is more likely to have clear symptoms.

¹⁶The subject of patients living with, but failing to develop HIV/AIDS-related symptoms has a large medical literature dedicated to it ([Choudhary et al., 2007](#); [Group, 2015](#); [Hatano et al., 2013](#); [Sax, Bartlett, & Bloom, 2019](#)).

14% were completely unaware. The following section describes our empirical framework for how we measure the effect of Johnson’s announcement on AIDS diagnoses in the United States.

2.5 Empirical Framework

In the 1980s, the AIDS epidemic infected certain populations of people at wildly disproportionate rates. The populations that were most affected by the virus were the homosexual and intravenous drug-using men. Due to this, the media framed the pandemic as a disease targeting these groups, when in reality HIV, like all other viruses, is non-discriminatory in its ability to infect. This allowed people who considered themselves as separate from these specific groups to mostly ignore the risks of HIV often until it was too late. We hypothesize that Magic Johnson’s announcement of a positive HIV diagnosis in early November 1991 updated the beliefs for those who considered themselves less prone to infection. It is evident in the 1991 NHIS data that Johnson’s announcement increased the public’s knowledge and perceived risk of contracting HIV. To estimate the causal effect of Johnson’s announcement on AIDS diagnosis, we use APIDS to evaluate the number of positive AIDS diagnoses in a given MSA by sexuality.

Each MSA-by-sexuality (or “community”) combination in our sample is observed monthly. We transform the raw number of diagnoses in order to estimate changes in the percentage of diagnoses. While this is traditionally done in the literature by using logarithms, we instead make use of the inverse hyperbolic sine. This method allows for the same interpretation as logarithms while empirically accommodating for zeros in the independent variable.¹⁷ We include population and prevalence estimates as control variables rather than part of the dependent variable due to the fact that our population estimates are drastically different in magnitude while diagnoses are much

¹⁷This is a deviation from epidemiology literature which often uses rates per 100,000.

closer in scale. In Equation (2.1), we specify a reduced form model for the inverse hyperbolic sine of AIDS diagnoses D in community c in month m as follows:

$$D_{cm} = \alpha_c + \beta_c \text{Trend} + \gamma_1(\text{Magic}_m) + \gamma_2(\text{Heterosexual}_c * \text{Magic}_m) + \phi X_{cm} + \epsilon_{cm} \quad (2.1)$$

The time trend is measured in number of months since Magic's announcement. For interpretability reasons, November 1991 is set as 0. The variable Magic represents a vector of zeros for observations before November 1991 and ones including and after November 1991. The variable Heterosexual equals one if the observation is for a heterosexual community and equal to zero otherwise. We also allow for each community combination to have its own pre-trend β_c and intercept α_c . X_{cm} is a vector of control variables such as population, AIDS related deaths two years in the future, and month fixed effects. γ_1 is an estimate of a change in AIDS diagnoses for all groups while γ_2 estimates the additional change unique to heterosexual men relative to homosexual men in the same MSA. While both coefficients are meaningful, γ_2 in particular is our parameter of interest.

To further test the hypothesis that the effect observed is coming from Magic's announcement, we leverage the existence of NBA teams in certain MSAs. We hypothesize that people in MSAs with NBA teams are likely to have felt a closer association to, or at least more familiarity with, Johnson after watching him play or hearing about him in the media. Since we do not know the number of people who followed the NBA within each MSA, our effect is an intent to treat estimate. We rewrite the equation 2.1 with an added interaction as follows:

$$\begin{aligned} D_{cm} = & \alpha_c + \beta_c \text{Trend} + \gamma_1(\text{Magic}_m) + \gamma_2(\text{Heterosexual}_c * \text{Magic}_m) \\ & + \delta_1(\text{NBA}_c * \text{Magic}_m) + \rho_1(\text{CARE}_c * \text{Magic}_m) \\ & + \delta_2(\text{NBA}_c * \text{Heterosexual}_c * \text{Magic}_m) + \rho_2(\text{CARE}_c * \text{Heterosexual}_c * \text{Magic}_m) + \phi X_{cm} + \epsilon_{cm} \end{aligned} \quad (2.2)$$

In Equation (2.2), NBA is equal to one if there is an NBA team in the MSA and is equal to zero otherwise. This parameter estimate represents the exposure effect by allowing for a different change for NBA cities with δ_1 . A positive estimate for this parameter would mean an increase in diagnoses for all men in NBA cities relative to non-NBA cities, suggesting these men were more affected by Magic Johnson overall. δ_2 is an estimate of the differential effect for heterosexual men in NBA cities relative to homosexual men in the same city. In this model, our parameter of interest is δ_2 .

In this equation, we also include an indicator variable for MSAs that received funding from the Ryan White Comprehensive AIDS Resources Emergency (CARE) Act in 1991, 1992 or 1993. Since this funding occurred right around Johnson's announcement, we interact this indicator, which would be captured in the fixed effects, with the announcement. In equation 2.2, we additionally estimate ρ_1 and ρ_2 , which can be interpreted similarly to δ_1 and δ_2 .

2.6 Results

We begin our main analysis by estimating Equation (2.1) and (2.2) and presenting the results in Table 2.5. The first column does not use community specific trends or intercepts whereas the second column does. Further, in the second column, we cannot estimate the parameters for Heterosexual, Trend or Trend * Heterosexual since they become specific to each community. However, there is still value in interpreting the signs of the coefficients in the first column.

The estimate for Trend represents a positive trend in AIDS diagnoses, which is visible from Figure 2.1. The estimate of .012 implies an additional 1.2% increase in AIDS diagnoses with each passing month. The parameter estimate for Sex represents the overall difference, in percentages, of AIDS diagnoses for heterosexual male communities relative to homosexual male communities. The point estimate of -1.240 represents 124% decrease in AIDS diagnoses for heterosexuals when compared to homosexuals. The estimate for Trend * Heterosexual represents the difference in the trends for heterosexual male communities relative to homosexual male communities before

Johnson's announcement. The parameter estimate for Magic, or γ_1 in Equation (2.1), suggests a decrease in the number of AIDS diagnoses for both groups after Magic Johnson announced he was HIV positive. However, we are unable to detect if this effect is different from a null effect. Finally, the estimate for γ_2 in Equation (2.1), is positive and significant. Therefore, after Magic Johnson's diagnosis, there was an additional 10% increase in AIDS diagnoses for heterosexual men relative to homosexual men. This suggests that Magic's announcement indeed did not effect the two groups equally, and that diagnoses for heterosexual men increased relative to homosexual men.

Next, we present estimates of the parameters in Equations (2.2) in columns (3) and (4) of Table 2.5. The original difference-in-differences coefficient (Magic * Heterosexual) in Table 2.5 and γ_2) loses its significance in these two columns. We no longer observe an increase in AIDS diagnoses for heterosexual men in all MSAs. Rather, since the coefficient estimate for NBA * Magic * Heterosexual is positive and significant, we see the previous effect for heterosexual men is actually concentrated in NBA MSAs. Following Magic Johnson's announcement, we observe about a 17% increase in AIDS diagnoses among heterosexual men in NBA MSAs. The average number of AIDS diagnoses in a month for heterosexual men in NBA MSAs is about 6.8, and a 17% increase equates to about 1 extra diagnosis. An secondary result from these two regressions is the insignificant coefficient estimate for NBA * Magic. This suggests that having an NBA team in an MSA only matters for the heterosexual male group and not the homosexual group. This is in line with our hypothesis that homosexual men were more aware of their risks to start. In both of these regressions, we also condition on whether an MSA obtained CARE funding.

A visualization of these results can be seen in Figure 2.3.

While the results from the NHIS survey questions gives us confidence that a change occurred in November 1991, we want to ensure that diagnoses were not trending upwards before or after our identification of Johnson's announcement. In order to do this, we rerun the model identifying other months in the sample as a placebo for Johnson's announcement. We collect the parameter estimates and standard errors in each regression and plot them in Figure 2.4. Ideally, we would observe only a single statistically significant point above the dashed zero line corresponding to Johnson's

Table 2.5: Effect of Magic Johnson on AIDS Diagnoses

	<i>Dependent variable:</i>			
	AIDS Diagnoses			
	(1)	(2)	(3)	(4)
Heterosexual	-1.240*** (0.184)			
Trend	0.012*** (0.001)			
Magic	-0.043 (0.030)	-0.043 (0.030)	-0.059 (0.045)	-0.051 (0.048)
Trend * Heterosexual	0.004** (0.002)			
Magic * Heterosexual	0.103** (0.045)	0.103** (0.046)	0.083 (0.062)	0.090 (0.065)
NBA * Magic			0.019 (0.059)	0.007 (0.061)
NBA * Magic * Heterosexual			0.168** (0.082)	0.173** (0.085)
N	7552	7552	7552	7296
MSA-Sex Trend	No	Yes	Yes	Yes
CARE Ctrl	No	No	Yes	Yes
Population Ctrl	No	No	No	Yes
AIDS Death Ctrl	No	No	No	Yes
Adj. R Sqr	0.293	0.851	0.851	0.852

Note:

*p<0.1; **p<0.05; ***p<0.01

The sample used in these regressions are the MSAs with the 4th to 64th most AIDS diagnoses over the sample period. We omit New York, Los Angeles, San Francisco, Honolulu and San Juan. This provides a sample where 40% of observations are NBA MSAs.

Figure 2.3: Time Series of Group Definitions

announcement. However, we observe a set of three points, and the dates of which are September, October and November 1991. Given these dates are bunched around Johnson’s announcement, we consider this evidence of a clean break. We also demarcate two other potentially important dates. The dashed vertical lines, from left to right, represent Ryan White’s death and the date that the CARE act was passed.

Figure 2.4: Testing Different DID Cutoffs

Note: We estimate column (1) in Table 2.5 and imagine the Magic Johnson’s announcement came in different months. We collect the parameter estimates and standard errors and plot them here. The solid vertical line represents the month of Johnson’s announcement and the dashed vertical line marks White’s death.

Finally, in Figure 2.5 we again test our temporal identification by redoing the placebo test in Figure 2.4, but this time for δ_2 , or the parameter estimate for $\text{NBA} : \text{Magic} * \text{Heterosexual}$. This plot closely resembles Figure 2.5 in that there is only one spot that appears to be most significant.

2.7 Conclusion

Informational asymmetries between health officials and at-risk populations are fundamental problems in any public health crisis. This issue can be most clearly observed during the AIDS pandemic in part due to the unique prejudices towards those who were believed to be solely susceptible to

Figure 2.5: Testing Different Triple Difference Cutoffs

Note: We estimate column (3) in Table 2.5 and imagine the Magic Johnson’s announcement came in different months. We collect the parameter estimates and standard errors and plot them here. The solid vertical line represents the month of Johnson’s announcement and the dashed vertical line marks White’s death.

the virus along with with the high social and medical costs associated with being identified and labeled as “infected”. Using surveys and AIDS diagnosis data, we both demonstrate and estimate the causal impact of Earvin “Magic” Johnson’s announcement of his HIV status towards fixing this asymmetry through its effects which simultaneously increased the public’s understanding of the risk of, as well as the willingness to be tested for, HIV.

If we interpret our model as causal, we can easily get predictions of AIDS diagnoses in a counterfactual world - the one where Magic Johnson is never diagnosed with AIDS. We subtract the predicted number of AIDS diagnoses from the actual diagnoses and find that Magic’s announcement increased diagnoses by about 3,354 men. Over 50% of these additional diagnoses belong to men who self identify as heterosexual. While this may seem small when compared with the overall number of people infected with HIV in the United States (and perhaps even futile considering the median life expectancy for PWAs at the time), this event potentially allowed people to come to terms with their health, prepare their finances and funerals, and say goodbye to friends

and loved ones. The long-run effects stemming from the public's updated knowledge and beliefs allowed people who previously engaged in risky sexual or drug-using behavior to potentially stop the spread of the infection. The additional diagnoses identified likely had a dramatic effect not only for PWAs, but also for those who would have contracted HIV otherwise ([Godlonton & Thornton, 2013](#)).

Chapter 3

Nudging and Driving: An Empirical Analysis of Dynamic Message Signs in Virginia

3.1 Introduction

Traffic safety and congestion represent large scale, nationwide transportation issues. According to the United States Department of Transportation's (USDOT) Fiscal Year Budget Estimates, government agencies allocated nearly \$1 billion to traffic safety measures nationwide in 2021. Over 37,000 Americans died as a result of traffic crashes in 2017 , which accounted for about 1.3% of all deaths in the United States . The USDOT estimated the statistical value of a life at \$9.6 million in 2016, which yields an approximate total cost of fatal crashes of \$380 billion. Traffic congestion contributes to a host of negative externalities including increased travel time and pollution. For example, [Currie and Walker \(2011\)](#) demonstrated that reductions in congestion, and thus pollution, by simply replacing a standard toll booths with electronic toll collection led to increased infant birth weight of about 11%.

Reducing these outcomes is of great interest to society. Unfortunately, many solutions to traffic externalities can be prohibitively expensive such as improving infrastructure (Bock, Cardazzi, & Humphreys, 2021) or investing in law enforcement (DeAngelo & Hansen, 2014). In addition, transportation departments (DOTs), the ones most familiar with these issues, do not typically have the ability to elect for these types of solutions on their own accord. Therefore, low cost and easy to implement interventions are especially attractive in this setting. One course of action that many DOTs have taken is to use Dynamic Message Signs¹ (DMS) to update the information sets of drivers on the highway.

DMS are large LED signs with the ability to quickly and costlessly change the messages they display based on current traffic conditions. DOTs use these signs to notify drivers of travel times estimates, congestion levels, toll prices, and lane impacting items such as crashes or debris. In addition, many DOTs use DMS to display safety slogans in hopes of encouraging drivers to engage in safer behaviors. DMS are popular along highways all over the world as their flexibility is an important tool for DOTs.

Although DMS are widespread and generally thought to be effective, their impact is not yet well established empirically. Providing information during travel is an obvious benefit for drivers, but there might be unintended costs of DMS use. Understanding these costs is important for DOTs in order to make better decisions when using DMS to communicate with drivers. Previous research (Hall & Madsen, 2021) has identified a very particular use case of DMS which increases the risk of a crash. This study builds on that finding by taking a more comprehensive approach to identify other factors that could generate this result. In addition, this study has the advantage of being able to estimate the impacts of DMS on speed in addition to crashes, which has so far only been done in driver simulation studies.

Data for this study come from Virginia's Department of Transportation (VDOT) and INRIX. Like most DOTs in the United States, VDOT uses DMS to communicate with drivers in real time. VDOT maintains detailed minute-by-minute log files from DMS that contain information such

¹These are also called Variable Message Signs, Changeable Message Signs or Matrix Signs.

as location, time and message. Variation in crash data from VDOT and speed data from INRIX is explained using variation in the messages displayed by DMS in the log files in a strict fixed effects framework. Since DMS are automated, messages are scheduled and then only change if a lane impacting incident occurs. Therefore, the message displayed at the beginning of the hour is intended to persist throughout. If an incident occurs and changes the message displayed, this incident can be attributed to the original message at the beginning of the hour.

The first result stemming from the analysis is a significant increase in crashes immediately downstream from DMS when signs are turned on compared to when they are turned off, or compared to locations without signs. The risk increases by about 11% of the already low probability of a crash. There is a negative but insignificant effect on speed for DMS that are turned off. The second result provides evidence that safety slogan messaging, on average, is effective at generating positive outcomes compared to typical use. However, this effect is not due to safety campaigns, where DMS are expected to show safety slogans, but where the safety slogans are indeed being displayed. This is important information for DOTs as one of their main tools at improving traffic outcomes appears to be effective compared to typical DMS use. The final result is a positive and significant increase in crashes, and reduction in speed, for multi-page messages. The effect is relatively large and dissipates as a function of distance from the DMS.

The rest of the study proceeds as follows. Section 2 outlines the related literature about traffic outcomes and messaging. Section 3 and 4 describe the data obtained and how they are used to answer the research questions. Section 5 discusses the empirical results while Section 6 concludes.

3.2 Literature Review

Standard economic theory suggests the need to overhaul incentives in order to get agents to make different decisions. However, a growing literature on nudges challenges this idea. A nudge, or a subtle and effectively costless intervention that does not alter incentives have been used to obtain socially-optimal outcomes. If, when and how well nudges work are important questions for policy

makers to be informed of, since there might exist inexpensive solutions to their dilemmas. Nudging has been shown to be effective in many contexts such as court date appearances ([Fishbane, Ouss, & Shah, 2020](#)), promotion of healthier food options ([Bucher et al., 2016](#)), and energy conservation ([Costa & Kahn, 2013](#)).

One issue disproportionately plaguing the United States in particular is traffic fatalities. According to the World Health Organization, the United States has 2 to 4 times as many per-capita (or per-vehicle) traffic fatalities relative to comparable countries such as Canada (2.1x), England (4.3x), Germany (3.4x), and Japan (3x). Reducing traffic fatalities, or crashes in general, is a main concern of transportation departments around the country. This is especially true since most traffic crashes are preventable and caused by human error.

Governments often attempt to encourage safer driving not through nudges but by explicitly altering incentive structures. Usually, this is done through enacting legislation. Laws mandating seat belt ([Cohen & Einav, 2003](#)) and motorcycle helmet ([Dee, 2009](#)) use are shown to be effective at reducing traffic fatalities. [Van Benthem \(2015\)](#) demonstrates an increase in the speed limit by 10 miles per hour can increase crashes by 9-15%, however, alarmingly, this 10 MPH increase can generate a disproportionate increase in fatalities of 34-60%.

Law enforcement also plays a big part in reducing harmful traffic outcomes. [DeAngelo and Hansen \(2014\)](#) find a significant increase in injuries and fatalities as a result of a large scale state police lay-off. This decreased police presence generated less citations, which in turn decreased the costs of reckless driving in a Beckerian framework by diminishing the probability of a sanction. [DeAngelo and Hansen \(2014\)](#) estimate a one unit reduction in fatalities can be achieved by an additional \$309,000 in police spending.

Instead of police, some cities have implemented electronic monitoring systems at intersections to alter incentives for drivers running red lights. In theory, these monitoring systems should decrease the probability of a crash due to driving through a red light as the probability of getting caught increases. However, these programs might unintentionally increase the probability of a rear-end crash resulting from drivers stopping short to avoid being caught for running the red light.

[Gallagher and Fisher \(2020\)](#) examine a setting in which red-light cameras were removed by voter referendum. The study shows an insignificant change in the amount of crashes but a significant change in the composition of crashes. Specifically, when red-light cameras were removed, there was a shift from read-end accidents to angle-crashes, or crashes that occur within an intersection. The results in [Gallagher and Fisher \(2020\)](#) are not aligned with the expectations of most policy makers.

The results from the above literature all analyze specific pieces of legislation and their enforcement via surveillance. However, transportation departments cannot usually implement nor enforce policies to reduce harmful behavior in the same way governments can. In other words, they do not necessarily have the ability to alter the economic decisions of drivers. Rather, DOTs main option is to nudge by the way of signage.

Before DOTs begin to place signage along the roads, it is important to understand how drivers will react to them being there. Multiple early studies have investigated the effectiveness of specific weather warnings (e.g., fog, ice, mist, etc.) displayed by DMS in various countries and found decreases in average speed ranging from 1 to 6 miles per-hour ([Al-Ghamdi, 2007](#); [Cooper & Sawyer, 1993](#); [Hogema, van der Horst, Van Nifterick, & Stoop, 1996](#); [Rämä & Kulmala, 2000](#)). [Reinolsmann et al. \(2018\)](#) show this effect for congestion warnings, although only when the warning was for relatively close congestion incidents. This latter result is expected, since it would not make sense for drivers to slow too far ahead of the congestion they are warned of. Taken at face value, this suggests that drivers are indeed internalizing and reacting to information. On the other hand, of course drivers slow for warnings about nearby congestion, since if they do not slow, they will risk crashing into the rear end of another vehicle.

Warning drivers of too far congestion could be thought of as information that is “irrelevant” to driving conditions. Similarly, billboard advertisements act this same way. In a review of the literature, [Oviedo-Trespalacios, Truelove, Watson, and Hinton \(2019\)](#) report inconclusive evidence of the relationship between roadside advertising and traffic safety. One suggestion made by the authors that is relevant to this study is for research to focus on transitioning, multi-panel billboards

as in [Belyusar, Reimer, Mehler, and Coughlin \(2016\)](#) and [Mollu, Cornu, Brijs, Pirdavani, and Brijs \(2018\)](#). These studies show that a message transition results in more eye glances and less focus on the road. In addition, [Harasimczuk, Maliszewski, Olejniczak-Serowiec, and Tarnowski \(2018\)](#) find that longer messages slow driver response time whereas graphical messages are least harmful to a driver's attention ([Marciano, 2020](#); [Marciano & Setter, 2017](#)).

Research on how advertisements on billboards can influence driving outcomes can inform DOTs using DMS for safety campaigns even though the mediums differ slightly. For example, billboards have the ability to be much more colorful and decorative compared to the block text on DMS. Billboards and DMS also contain very different content in addition to how the information is displayed. However, there are some studies that have investigated the effectiveness of safety slogans separate from billboard advertisements. Hardly any of the studies in this literature, however, are empirical investigations but rather simulation studies.

In a driving simulation study, drivers did not change their driving behavior when exposed to general safety slogans, but were then able to better react to more targeted warnings later. However, this effect dissipated when drivers were overexposed to the safety slogans, suggests drivers can become desensitized ([Jamson & Merat, 2007](#)). [Meuleners, Fraser, and Roberts \(2020\)](#) offer some insight into how safety campaigns might be most helpful. The study uses a driving simulation to analyze the effect of alternate speed warnings. Drivers were presented with one of four options: a sign that was either painted with the speed limit (80 km/h or 50 mph), painted "slow down", would flash the speed limit, or flash "slow down". While the flashing speed limit sign reduced speed more than the painted one, the opposite was true for the textual, "slow down" signs. In another study, [Chaurand, Bossart, and Delhomme \(2015\)](#) examine framing effects of messages on speeding. Relative to no message, showing drivers a message reduced their speed. This effect was larger for messages framed positively ("respected speed limit = less crashes") compared to negatively ("exceeded speed limit = more crashes") framed. However, there was no difference in effect based on message content (crash vs fuel consumption).

[Hall and Madsen \(2021\)](#) is the study closest to this one. The authors examine a particular safety campaign and its corresponding “slogan” in Texas that discloses to drivers the number of traffic fatalities in that year to date. In fact, this specific safety campaign is implemented in about half of the states in the United States, and might be one that [Shealy, Kryschal, Franczek, and Katz \(2020\)](#) would endorse. The results suggest an increase in crashes immediately following DMS (spatially) in safety campaign weeks compared to the same location in the same month. In the study’s many analyses, the effect is found to persist over time and increase with both death count and complexity of the highway segment. Therefore, the mechanism proposed by the authors to be driving the effect is cognitive overload.

[Hall and Madsen \(2021\)](#) rigorously show some behavioral interventions can be “too salient” and instead backfire to generate the opposite of what was intended. This work sets up the question of, how should policy makers use DMS to get drivers to use safer driving habits? Do safety slogans work and is there some way to structure messages to reduce the unintended consequences?

3.3 Data and Setting

To address the research questions, a few different data sources are used to create an hourly panel. Data collected for this study include Virginia’s linear referencing system (LRS) for locations, detailed minute-by-minute log files for each DMS, crash counts, and speed readings from June 2017 until March 2020.

VDOT’s LRS is a GIS tool that allows spatial data to be easily referenced to a linearized route via mileposts, which denote the distance from said route’s origin. Although latitude and longitude measurements are helpful standards to summarize locations in two dimensional space, highways can easily be thought of in one dimension. Frequently traveled routes are often accompanied by physical mileposts which allow first responders, construction workers, drivers, and others to effectively communicate their positions to one another without GPS coordinates. VDOT’s LRS can be thought of as a digital, continuous milepost. Anything with latitude and longitude measurements

can be spatially matched to an LRS segment and converted to a linear position. Each of the datasets used in this study are matched to a specific segment in the LRS.

VDOT maintains over 1,200 individual DMS, although there is substantial heterogeneity in their capabilities and purposes. Many DMS have the primary function of displaying variable speed limits, travel times, or lane closures and only have the capability to display a handful of characters or symbols at a time. These DMS are typically small and often times built into standard, static highway signs. Standalone DMS are larger and come in one of two forms: stationary or portable. Stationary DMS are typically affixed to an overhead structure spanning the highway (a gantry) or to a pole beside the road (a cantilever). Portable DMS are attached to frames with wheels so they can be moved to different locations. These portable DMS are usually placed where VDOT plans to put a stationary DMS in the future, where a stationary DMS is currently malfunctioning, or where there is active roadwork. Both types of these standalone DMS are able to display information in full phrases instead of a single number or symbol.² These are the DMS VDOT use in order to communicate with, and occasionally nudge, drivers on the road.

Of all the DMS operated by VDOT, 450 are standalone and were located along a mainline Interstate or US Route at one point in time. 285 (63%) of these signs are stationary and 165 (37%) are portable. DMS that only serve on/off ramps, arterial or collector roads are not counted in these numbers and were removed from the sample due to less comparable traffic patterns and road features.

These 450 DMS are located at 532 unique locations. The latter is larger than the former due to some portable DMS being in multiple locations throughout the sample. However, there are also times where two or more DMS reside next to one another. This occurs due to two stationary DMS having been affixed to the same gantry, a portable DMS having been placed next to a malfunctioning stationary DMS, or simply two DMS placed within an immaterial distance from one another. In addition, it could also be the case that one DMS is swapped out for another DMS. As a final note about the number of unique locations, a portion of them (34) never obtain a DMS throughout

²There are a few DMS who have the capability of displaying full phrases but are strictly used to display the status of HOV lanes.

Table 3.1: DMS Type Frequency Matrix

	No Mobile	One Mobile	Two Mobile
No Stationary	36.73%	11.05%	0.12%
One Stationary	50.57%	0.15%	-
Two Stationary	1.38%	-	-

Note: This table contains the relative frequencies for which DMS appear at the locations in the analysis sample (N = 10,679,817), which is made up of 485 locations and 414 DMS units. Of the full 532 locations, 19 locations are removed for having less than 50% complete data and 28 locations are removed for not being able to match speed data.

the sample period, but do obtain a DMS after March 2020. These locations are used as control units since they have similar characteristics that make them candidates for DMS.

The 532 unique locations represent the spatial unit of analysis for this study. If all DMS were stationary, alone, and existed for the entire sample period, this setup would be identical to that in previous empirical research ([Hall & Madsen, 2021](#)). However, portable DMS and the placement of new stationary DMS allow for another dimension traffic could be impacted. In other words, it is possible to measure the effect of the DMS unit's presence in addition to its use given the structure of the data. The breakdown for the analysis sample of DMS count and type by location is detailed in Table 3.1. Figure 3.1 displays the locations of the segments included in the sample. The distribution of the driving distance in kilometers between locations has a mean, standard deviation and median that is comparable to the distribution in [Hall and Madsen \(2021\)](#) (14.17/36.32/4.2 in this study vs 13.96/29.17/6 in [Hall and Madsen \(2021\)](#)).

3.3.1 DMS Use

The main, default function of DMS in Virginia is to display travel time estimates, distances, and toll prices, especially during peak travel hours. This information allows drivers to make more informed decisions when traveling along the highway. According to VDOT policy, default messages must follow a fairly standard, uniform format. In addition, all travel times, distances and toll prices must update automatically in order to avoid costly labor and potential errors. VDOT also considers

Figure 3.1: Map of DMS Locations in Virginia

Note: This figure shows the locations of DMS within the sample, interstate highways, and other major roadways.

blank messages to be valid as they are used to communicate to drivers that there is no unusual traffic information to be aware of. DMS can also be turned off due to unreliable power sources (e.g. solar) or malfunctioning equipment.

A major role of DMS is alerting drivers to upcoming incidents or blockages. For instance, in the event of a crash, roadwork, debris or another lane impacting item, DMS switch their message to one that warns drivers of the upcoming delay or obstruction. Lane impacting items get reported to Virginia's 511 system and are then confirmed by Virginia's Traffic Operations Center. Since DMS are connected to VDOT's Automated Traffic Monitoring System (ATMS), message updates are triggered almost immediately. Moreover, most messages are auto-generated from a template and only require basic information (location, lane, etc.) that is fed in from the ATMS. Lane impacting items generate messages that take priority over the default message until the lane impacting item is taken care of.

DMS also display messages aside from immediate, pertinent information about upcoming traffic conditions. A fundamental example of this is when DMS display safety slogans during VDOT's scheduled safety campaigns. These campaigns often relate to seat belt usage, distracted driving, speeding, or other dangerous driving habits or activities. During safety campaigns, DMS across Virginia display safety slogans related to the campaign topic instead of their default messages. Although the topic of safety slogans can vary, the themes of the messages are usually generic, holiday, or popular culture.³ While safety slogan usage is much more likely to occur during safety campaigns, they are also displayed other times as well. About 10% of observations where a sign exists begin the hour with a safety slogan compared to only about 1% for non-campaign hours.

Figure 3.2 depicts VDOT's safety campaign schedule during the sample period. These campaigns are not planned in reaction to recent or current traffic conditions, but rather planned several months in advance. This is an important point as safety campaigns can be treated as exogenous

³There are 24 days of pop culture campaigns (8 total campaigns), 53 days of holiday campaigns (16 total campaigns) and 93 days of generic campaigns (22 total campaigns).

Figure 3.2: Safety Campaign Schedule

Note: This figure displays a calendar view of VDOT's safety campaigns. Circles represent weekdays (Monday through Thursday) and the squares represent weekends (Friday, Saturday, Sunday). The darker colored dots represent campaign days. The black dots are regular campaigns, red are holiday themed and blue are popular culture themed.

to trends in traffic conditions. There are 46 campaigns in total which take up 170 days over the sample period. These campaigns span between one and seven days with a mode of three days.⁴

3.3.2 Message Text

Data detailing the actual messages for each DMS is taken from SmarterRoads, one of VDOT's open data portals. The sample of DMS log files begins 11:09 am on June 7th 2017 and continues through present day, though the sample used ends 12:00 am on March 1st 2020. The temporal frequency of these log files are by minute and include a timestamp, geolocation, unique identifier and the message's text in MULTI (Mark-Up Language for Transportation Information) format.

The MULTI formatting contains information such as the location of line breaks, font, colors, etc. Most importantly, the format contains information about the number of "pages" each message contains. Like some billboards, DMS have the ability to alternate, or transition, between messages in order to display more information at once. In addition to the number of pages, the length of the message measured by the number of alphanumeric characters is also recorded. Figure 3.3 depicts the relative frequency of the number of characters of the messages displayed at the beginning of each hour across locations and the average number of pages for each number of characters.

3.3.3 Traffic Outcomes

Information about crashes come from a second VDOT data portal called Virginia Roads. These records contain information on individual crash incidents with both location and time elements. For each hour, the number of crashes that occur within d kilometers of a DMS are recorded, where $d \in \{3, 10\}$ km. If drivers travel at 100 km/h, they will cover these distances in 1.8 and 6 minutes, respectively.

⁴The number of days between campaigns has a minimum of 2 days, a maximum of 85 days, and an average of 17. The weekday with the most campaign days is Friday (31) and the least is Sunday (17). The months with the most campaign days over the entire sample are December (25) and August (24) and the months with the least are January, February and May (7).

Figure 3.3: Number of Characters and Average Pages

Speed data are obtained from INRIX, a private company specializing in traffic data. These speed data are the result of probing GPS devices in smartphones and vehicles instead of by way of fixed roadway sensors. This allows for much more detailed, granular measurements of relevant highway performance metrics for DOTs around the world. While VDOT maintains similar data using traditional roadway sensors, these are less comprehensive relative to what INRIX can obtain by probing devices and vehicles. A measurement from INRIX that is of interest to this study is vehicle speed. Hourly speed aggregates are extracted from INRIX's tools and matched to each unique road segment. In addition, speed two kilometers before each DMS is also recorded. The distribution of speed in kilometers per hour is displayed in Figure 3.4.

3.3.4 Summary Statistics

Summary statistics for the analysis sample are displayed in Table 3.2.

Figure 3.4: Distribution of Speed

Note: This plot displays the relative frequency of speed across the sample.

Table 3.2: Summary Statistics

	N	Mean	St. Dev.	Min	Max
Any DMS	9486430	0.618	0.509	0	2
Stationary DMS	9486430	0.495	0.516	0	2
Portable DMS	9486430	0.123	0.333	0	2
Safety Campaign Day	9486430	0.168	0.373	0	1
Crashes within 3 km	9486430	0.003	0.056	0	5
Crashes within 10 km	9486430	0.009	0.097	0	8
Speed at Location	9486430	97.651	19.334	2	131
Speed 2 km Before Location	9486430	97.784	19.36	2	129
Number of DMS On	5787337	0.425	0.511	0	2
Safety Message Displayed	2432219	0.063	0.244	0	2
Crash Message Displayed	2432219	0.072	0.26	0	2
Pages	2432219	1.528	0.529	1	6
Characters	2432219	40.592	15.819	1	156

Note: This table contains summary statistics for the analysis sample (N = 9,486,430), which is made up of 434 locations and 365 DMS units. Of the full 532 locations, 19 locations are removed for having less than 50% complete data and 28 locations are removed for not being able to match speed data. The sample size decreases to account for observations without a DMS and without a DMS that is turned on. The observations removed from the rows are all equal to 0. Speed is measured in km/h.

3.4 Empirical Strategy

An ideal identification strategy to estimate how DMS and their message parameters (e.g., length, content, etc.) influence traffic outcomes would be random assignment. An experiment that places DMS around Virginia and randomly assigns messages with varying construction and content would be best for identifying causality. Unfortunately (for researchers), complete randomization is prohibitively expensive and thus not possible. Instead, it is important to identify comparable instances where DMS location and use is plausibly exogenous.

To estimate the effects of DMS use and message attributes on traffic outcomes, a strict fixed effects model is employed. Segment-month-weekday-hour fixed effects are used to control for idiosyncratic factors and general trends in traffic across different segments. These fixed effects partial out differences in crash rates and average speeds due to peak travel times, weekends, and typical fluctuations due to seasonality for each segment. Federal holiday fixed effects are also included to account for differences in commuting during those days. Speed two kilometers before the DMS is also included as an independent variable to control for the upstream flow of traffic. This specification compares all Mondays in June at 8:00 am for a specific segment to one another. Given the length of time of this panel, each fixed effect contains less than twelve observations.

As pointed out by [Hall and Madsen \(2021\)](#), naïve analyses of DMS will result in biased estimates. If a crash occurs in the middle of an hour, DMS operators will change the message to warn drivers of the upcoming obstruction. Similarly, DMS operators are instructed to blank (turn off) DMS if there is a noticeable increase in congestion resulting from a message. Therefore, averages of message characteristics across the hour will suffer from reverse causality. To avoid this, the message being displayed at the beginning of the hour is used for the entire hour. The assumption behind this is that DMS operators only change messages within an hour if some incident occurs within the hour. Figure 3.5 shows the probability of a DMS displaying a safety message, incident message and being turned on for the 0th, 1st and 2nd minute of each hour of the day. There is a lag in the probabilities of displaying a safety message and being turned on from, say, 10:00 to 10:02. Therefore, what the DMS says two minutes into the hour is used as the definition of “beginning”.

Figure 3.5: Message Timing

Using these definitions of DMS characteristics and other controls, outcome Y for segment s in time period t is modeled as:

$$Y_{s,t} = \delta_1 \text{DMS Off}_{s,t} + \delta_2 \text{DMS On}_{s,t} + X_{s,t}\beta + \mu_{s,m(t),d(t),h(t)} + \tau_{y(t)} + \eta_{\text{holiday}} + \epsilon_{s,t} \quad (3.1)$$

where δ_1 and δ_2 are treatment effect of DMS being turned off or on, μ is a fixed effect for each segment-month-weekday-hour combination, τ is a fixed effect for each year, and η is a fixed effect for each federal holiday. This regression compares segments without DMS to segments with DMS when they are turned off and when they are turned on. Y represents three possible variables: crashes from 0 to 3 km, 3 to 10 km and speed on the segment of the DMS.

To test the effect of safety campaigns and slogans on traffic outcomes, a second regression to be estimated is:

$$\begin{aligned} Y_{s,t} = & \delta_1 \text{DMS Off}_{s,t} + \delta_2 \text{DMS On}_{s,t} + \alpha_1 \text{Campaign}_t \\ & + \gamma_1 (\text{Campaign}_t \times \text{DMS Off}_{s,t}) \\ & + \gamma_2 (\text{Campaign}_t \times \text{DMS On}_{s,t}) \\ & + X_{s,t}\beta + \mu_{s,m(t),d(t),h(t)} + \tau_{y(t)} + \eta_{\text{holiday}} + \epsilon_{s,t} \end{aligned} \quad (3.2)$$

where α_1 represents the average difference in outcomes due to scheduled safety campaigns and γ_1 and γ_2 represent the differential impacts of a DMS being on or off during a safety campaign. This regression most resembles that in [Hall and Madsen \(2021\)](#). Since VDOT encourages DMS operators to display safety slogans during safety campaigns, this reduced form equation should be able to identify intent-to-treat estimates of displaying safety slogans on traffic outcomes. The differences in this equation and that in [Hall and Madsen \(2021\)](#) are two fold. First, instead of examining just one specific safety slogan, this study leverages a menu of them. A more comprehensive analysis of safety slogans in general will provide DOTs with more information regarding

the impact of these campaigns. Second, information on which DMS are turned on during the hour is used in this regression. If safety campaigns are truly random, the γ_1 should be equal to zero. This is due to safety slogans obviously not being displayed when the DMS is off, and therefore there should be no differential effect.

An important feature about the safety slogan studied in [Hall and Madsen \(2021\)](#) is that the message contains multiple pages. In other words, the DMS shows two (or more) messages at a time and transitions between them every couple of seconds. These messages can be related or unrelated depending on what the DOT is trying to communicate. While the aforementioned research presents evidence of the safety slogan increasing crashes, the authors are not able to differentiate the effect of multiple page messaging compared to single page.

Recent research ([Belyusar et al., 2016](#); [Mollu et al., 2018](#)) has shown how drivers are more distracted in the presence of billboard advertisements with transitioning pages. In addition, [Oviedo-Trespalacios et al. \(2019\)](#) call for more to be done on the topic. To estimate the impact of using multi-page messages instead of single page, the following equation is used:

$$Y_{s,t} = \delta_1 \text{DMS Off}_{s,t} + \zeta_1 \text{Single Page}_{s,t} + \zeta_2 \text{Multi Page}_{s,t} + \text{Char}_{s,t} + X_{s,t}\beta + \mu_{s,m(t),d(t),h(t)} + \tau_{y(t)} + \eta_{\text{holiday}} + \epsilon_{s,t} \quad (3.3)$$

where δ_2 from the previous equations is broken down into ζ_1 and ζ_2 depending on the number of pages of the message displayed at the beginning of the hour. If evidence from billboards can be directly applied to DMS, then ζ_2 should be larger than ζ_1 .

While estimating regressions with speed as the outcome is fairly straightforward, using crashes instead presents more complication. First, an important assumption made about the crashes is that the measurement error in the report time is randomly distributed. However, if DMS consistently update before the crash is reported by the police, then there still might be reverse causality for a portion of observations. To mitigate this issue, message characteristics in t can be used to explain outcomes in $t + 1$. This may shift some crashes too far in one direction whereas the other specifi-

cation might push them too far in the other direction. If the hour of the report time is correct more often than not, coefficient estimates for crashes in t will likely be larger than those for crashes in $t + 1$. Second, the chosen functional form might be important for a variable as rare as crashes within an hour over a 3 kilometer span of highway. Regressions with crashes as outcomes are also estimated using a logistic regression.

3.5 Results

The first analysis is designed to test if the use of DMS effects traffic outcomes. Table 3.3 displays estimates for parameters in Equation (3.1). The crash outcomes are multiplied by 1000 and divided by the length of road. The coefficient estimate for DMS Off (δ_1) are always statistically insignificant. Intuitively, this suggests no difference between a DMS that is turned off and a segment without a DMS. The coefficient estimates for DMS On in the first two columns are positive and significant.⁵ The coefficient estimates in these first two columns result from regressions assuming police report times are accurate, or at least randomly distributed around the actual time of the crash. However, if report times are consistently late, this could introduce reverse causality. To avoid this, crashes in $t + 1$ are also used as an outcome. The results are similar though smaller. Finally, there is a negative but insignificant effect on speed when a DMS is turned on.

The second analysis estimates the parameters in Equation (3.2). Since VDOT schedules some of their safety campaigns on holidays, and this is an important part of the identification, two samples are used when estimating the model. Table 3.4 presents the results with holidays included, but controlled for with fixed effects, while Table 3.5 removes them all together.

The results in Table 3.4 show that speed is higher and crashes are lower during campaigns, which is likely due to the differences in traffic patterns on some of those days. However, the previous result of DMS being turned on influencing traffic outcomes remains. Columns 3 through 6 do not use safety campaigns but rather the hours when safety messages are actually used. The

⁵Results from a logistic regression are qualitatively similar

Table 3.3: DMS Use

	Crashes _t	Crashes _t	Crashes _{t+1}	Crashes _{t+1}	Speed _t
DMS Off	-0.026 (0.050)	-0.023 (0.053)	0.010 (0.036)	0.001 (0.020)	-0.044 (0.182)
DMS On	0.115** (0.054)	0.110* (0.063)	0.043 (0.041)	0.063*** (0.023)	-0.285 (0.178)
Distance From DMS	[0,3)	[3,10)	[0,3)	[3,10)	0
Num.Obs.	9486430	9486430	9483845	9483845	9486430
R2	0.105	0.111	0.104	0.110	0.907

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: These regressions evaluate the effect of the existence and general use of stationary and portable DMS at a location. The sample includes 485 locations and 414 DMS units. Location-by-hour-by-weekday, month-by-year and specific holiday fixed effects are used in each model. Controls for speed 2 km before the sign are also included. Standard errors are clustered at the location level.

coefficient for safety messages is insignificant, which implies the slogans produce traffic outcomes similar to DMS being turned off.

Table 3.5 shows the estimates when federal holidays and the surrounding travel days are removed from the sample. Here, the results become stronger albeit similar. DMS, when turned on, increase crashes and reduce speed. While there is not difference for DMS being turned on during a campaign day, DMS that are indeed displaying safety messages reduce the risk of crashes and do not reduce speed as much. The difference in these results is likely due to the relatively low take up of safety messaging during campaigns. For locations where a DMS exists, 1.3% of hours not during safety campaigns display a safety slogan. On the other hand, 9% of hours display a safety slogan during campaigns. For locations where DMS exist and are turned on, these percentages change to 3.3% and 19.8%. DMS are more likely to be turned on during safety campaigns (40% vs 45%).

Finally, Table 3.6 contains the estimates of Equation 3.3. These results compare the differences between single page and multi-page messages. Columns 1 through 4 reveal a pattern where multi-page messages increase crashes closer to the DMS more than single page. Similarly, speed is reduced around multi-page messages as well. Figure 3.6 presents a more granular view of the effect size as a function of distance from DMS. The largest effects for both crashes in t and $t + 1$

Table 3.4: DMS Use and Safety Messaging

	Crashes _t	Crashes _t	Speed _t	Crashes _t	Crashes _t	Speed _t
DMS Off	-0.027 (0.048)	-0.015 (0.052)	-0.053 (0.182)	-0.027 (0.050)	-0.025 (0.053)	-0.041 (0.183)
DMS On	0.123** (0.053)	0.122* (0.063)	-0.287 (0.178)			
Campaign Day	-0.043* (0.026)	-0.008 (0.015)	0.099*** (0.028)			
DMS Off x Campaign	0.009 (0.041)	-0.053** (0.025)	0.059 (0.043)			
DMS On x Campaign	-0.041 (0.056)	-0.063** (0.029)	-0.001 (0.046)			
DMS On (non-Safety Message)				0.123** (0.055)	0.123* (0.064)	-0.304* (0.178)
DMS On (Safety Message)				0.023 (0.066)	-0.030 (0.061)	-0.070 (0.189)
Distance From DMS	[0,3)	[3,10)	0	[0,3)	[3,10)	0
Num.Obs.	9486430	9486430	9486430	9486430	9486430	9486430
R2	0.105	0.111	0.907	0.105	0.111	0.907

* p < 0.1, ** p < 0.05, *** p < 0.01

Note: These regressions evaluate traffic outcomes during safety campaign days in addition to DMS use at a location. The sample includes 485 locations and 414 DMS units. Location-by-hour-by-weekday, month-by-year and specific holiday fixed effects are used in each model. Controls for speed 2 km before the sign are also included. Standard errors are clustered at the location level.

Table 3.5: DMS Use and Safety Messaging, Without Holidays

	Crashes _t	Crashes _t	Speed _t	Crashes _t	Crashes _t	Speed _t
DMS Off	-0.004 (0.038)	-0.016 (0.021)	-0.049*** (0.014)	-0.010 (0.038)	-0.025 (0.021)	-0.040*** (0.014)
DMS On	0.127*** (0.043)	0.106*** (0.025)	-0.279*** (0.015)			
Campaign Day	-0.009 (0.030)	0.001 (0.016)	0.062*** (0.010)			
DMS Off x Campaign	-0.036 (0.042)	-0.055** (0.024)	0.050*** (0.015)			
DMS On x Campaign	-0.010 (0.054)	-0.023 (0.032)	-0.022 (0.016)			
DMS On (non-Safety Message)				0.133*** (0.043)	0.114*** (0.025)	-0.297*** (0.015)
DMS On (Safety Message)				0.025 (0.064)	-0.032 (0.037)	-0.093*** (0.022)
Distance From DMS	[0,3)	[3,10)	0	[0,3)	[3,10)	0
Num.Obs.	8616286	8616286	8616286	8616286	8616286	8616286
R2	0.114	0.121	0.909	0.114	0.121	0.909

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: These regressions evaluate traffic outcomes during safety campaign days in addition to DMS use at a location. The sample includes 485 locations and 414 DMS units. Location-by-hour-by-weekday, month-by-year and specific holiday fixed effects are used in each model. Controls for speed 2 km before the sign are also included. Standard errors are clustered at the location level.

Table 3.6: DMS Use and Multiple Page Messages

	Crashes _t	Crashes _t	Crashes _{t+1}	Crashes _{t+1}	Speed _t
DMS Off	-0.046 (0.051)	-0.019 (0.053)	-0.006 (0.037)	-0.004 (0.020)	-0.025 (0.183)
DMS On (Single Page)	0.093 (0.077)	0.052 (0.084)	0.060 (0.060)	0.069* (0.036)	-0.212 (0.192)
DMS On (Multi Page)	0.310*** (0.100)	0.072 (0.082)	0.198*** (0.075)	0.105** (0.044)	-0.466** (0.213)
Number of Characters	-0.003* (0.002)	0.001 (0.001)	-0.003** (0.001)	-0.001 (0.001)	0.002 (0.003)
Distance From DMS	[0,3)	[3,10)	[0,3)	[3,10)	0
Num.Obs.	9486430	9486430	9483845	9483845	9486430
R2	0.105	0.111	0.104	0.110	0.907

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: These regressions evaluate traffic outcomes given DMS existence, usage and message structure at a location. The sample includes 485 locations and 414 DMS units. Location-by-hour-by-weekday, month-by-year and specific holiday fixed effects are used in each model. Controls for speed 2 km before the sign are also included. Standard errors are clustered at the location level.

occur in the distance between 0 and 1 kilometer, and the smallest effect occurs between 5 and 10 kilometers. This pattern confirms that the effect is largest nearest to the DMS.

3.6 Conclusion

Transportation issues such as safety and congestion represent large problems for transportation departments. DOTs use Dynamic Message Signs to notify drivers of upcoming conditions, hazardous or otherwise, and display safety slogans as reminders in an attempt to reduce negative outcomes. However, research estimating the impacts of DMS is far from complete, even though their use is widespread and thought to be helpful. This study uses detailed, publicly available data from Virginia to add to the understanding of DMS. Results indicate that crashes (speeds) are relatively higher (lower) when DMS are turned on, and this effect is mostly concentrated during times when DMS display multi-page messages. There is some evidence that displaying safety slogans has less harmful effects compared to other messages, which is contrary to previous research in the literature.

Figure 3.6: Coefficient Estimates By Distance

Note: Coefficient Estimates by Distance from DMS

DOTs can use the results in this study to better inform their use of DMS. When DMS are turned on, crashes increase and speed decreases, compared to when they are turned off. Instead of using travel time estimates as a default, especially when the estimate is just signaling free flow traffic, it might be worthwhile for DOTs to simply turn off the DMS instead. Second, multi-page messaging generates significantly worse outcomes compared to single page messages. Of course, it is not always possible to condense all relevant information onto a single page due to the sheer volume of information. However, there are instances where VDOT elected for a two page message when a single page message was available. For example, on the same DMS, VDOT displayed both “US 1 / EXIT 126 // 26 MILES / 23 MINUTES” and “EXIT 126 / 26 MILES / 23 MINUTES”. The latter message is able to transmit the same information in a much more condensed way, which should yield less distractions for drivers on the margin.

Future research should focus on teasing apart the effects of each element of DMS. [Shealy et al. \(2020\)](#) claim that nonstandard messages have the greatest effect on drivers’ biological indicators. However, it is unclear how this might translate to the road. [Hall and Madsen \(2021\)](#) suggest that nonstandard messaging, or least an example of such, can lead to increased crashes. While displaying the message in that study was exogenous to road conditions, there is still the confounding factor of the message being one with more than one page. This study demonstrates the dangers of messages with more than multiple pages. Therefore, the effect derived in that study could be a result an increased probability in multi-page messaging.

A second point is that crash report times contain quite a bit of measurement error. Studies that use such fine temporal segments such as an hour should be aware of this and look for more accurate measurements. An example of how this might be possible would be to use 511 reports instead of police reports. Since DMS and 511 reports are often linked to the same automated traffic monitoring system, this would yield much more accurate measurements.

Finally, the parameter estimates for speed in each table of results are small and intuitive at first glance. Since speed is a major determinant of crashes ([Van Benthem, 2015](#)), DMS reducing speed might seem at odds with an increase in crashes. However, the magnitude of the coefficient

estimates suggest that people slow down slightly in order to read the messages. If only a handful of individuals do this, then there might be an increase in the variance of speed. Increases in speed variance have been linked to increases in crash risk ([Wang, Zhou, Quddus, Fan, & Fang, 2018](#)). Therefore, a joint multivariate analysis might yield more insight into this relationship.

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G Additional Tables and Figures for Chapter 1

Table G.1: Table of Building Classes

Code	Building Class	NTA	Buffer
A0	CAPE COD	48	29
A1	TWO STORIES - DETACHED SM OR MID	4,586	4,103
A2	ONE STORY - PERMANENT LIVING QUARTER	775	744
A3	LARGE SUBURBAN RESIDENCE	10	15
A4	CITY RESIDENCE ONE FAMILY	331	337
A5	ONE FAMILY ATTACHED OR SEMI-DETACHED	3,301	2,968
A7	MANSION TYPE OR TOWN HOUSE	3	3
A9	MISCELLANEOUS ONE FAMILY	1,083	1,248
B1	TWO FAMILY BRICK	4,619	5,164
B2	TWO FAMILY FRAME	4,370	4,766
B3	TWO FAMILY CONVERTED FROM ONE FAMILY	4,566	4,410
B9	MISCELLANEOUS TWO FAMILY	1,305	1,335
C0	THREE FAMILIES	5,588	5,986
S0	PRIMARILY 1 FAMILY WITH 2 STORES OR OFFICES	8	10
S1	PRIMARILY 1 FAMILY WITH 1 STORE OR OFFICE	328	351
S2	PRIMARILY 2 FAMILY WITH 1 STORE OR OFFICE	987	1,065
Total		31908	32534

Note: The tabulated data is for all property sales in Impact Zones, corresponding NTAs or Buffers between the dates January 1st, 2010, and December 31st, 2015.

Table G.2: Most Frequent Surnames

Surname	Count	% Non-Hispanic White	% Hispanic	% Black
OLSON	164035	94.76	2.03	0.4
SNYDER	160262	94.07	1.92	1.46
WAGNER	155795	92.47	2.41	2.6
MEYER	150895	94.84	2.28	0.52
SCHMIDT	147034	95.15	2.34	0.37
RYAN	143452	91.65	2.55	3.02
HANSEN	137977	94.22	2.54	0.53
HOFFMAN	131401	94.08	2.12	1.26
JOHNSTON	131373	91.62	2.62	2.41
LARSON	122587	94.79	1.95	0.49
GARCIA	1166120	5.38	92.03	0.45
RODRIGUEZ	1094924	4.75	93.77	0.54
MARTINEZ	1060159	5.28	92.91	0.49
HERNANDEZ	1043281	3.79	94.89	0.36
LOPEZ	874523	4.86	92.92	0.57
GONZALEZ	841025	4.03	94.97	0.35
PEREZ	681645	4.96	92.95	0.45
SANCHEZ	612752	4.98	92.99	0.45
RAMIREZ	557423	3.89	94.48	0.31
TORRES	437813	5.35	92.16	0.57

Figure G.1: Daily Counts of Stop & Frisk in NYC from 2006 through 2015

Note: This plot shows the daily number of Stop & Frisk incidents in New York City for ten years. The solid vertical lines represent landmark events in *Floyd, et al. v. City of New York, et al.* while the dashed lines represent the days included in the study sample.

Figure G.2: Daily Counts of Crime in NYC from 2006 through 2015

Note: This plot shows the daily number of crimes in New York City for ten years. The solid vertical lines represent landmark events in *Floyd, et al. v. City of New York, et al.* while the dashed lines represent the days included in the study sample.

H Additional Tables and Figures for Chapter 2

Figure G.3: Impact Zone Buffers

Figure G.4: Identification Strategy - NTA

Note: This map shows a random sample of properties from the sample and what group they belong to. Groups are defined by the property being within an Impact Zone, in the same NTA as an Impact Zone, or neither.

Figure G.5: Identification Strategy - Buffer

Note: This map shows a random sample of properties from the sample and what group they belong to. Groups are defined by the property being within an Impact Zone, in a 3000 foot radius of an Impact Zone, or neither.

H.I.V. Tests Up 60% Since the Disclosure From Magic Johnson

By CALVIN SIMS

The number of people being tested for H.I.V. has increased almost 60 percent in New York City and has risen sharply throughout the nation in the month since Magic Johnson announced that he was infected with the virus that causes AIDS.

In New York, the increase has strained the resources of the City Health Department, which said it can now take up to seven weeks to get an appointment for a test at the city's counseling and testing centers. Before Mr. Johnson's announcement on Nov. 7, most centers provided tests without an appointment or had a maximum wait of two weeks.

Officials of the Federal Centers for Disease Control in Atlanta said yesterday that while no statistics had been compiled, a random survey of state health departments nationwide showed an equally sharp increase in the number of people seeking tests for H.I.V.

"It's all anecdotal at this point, but in some cities we are hearing of increases as large as 10 times the number of tests previously requested," said Fred Kroger, director of AIDS education and information programs for the centers. Mr. Kroger attributed the rise in H.I.V. testing to people who are not now engaged in risky sexual behavior, but are

Figure H.1: Dec. 7, 1991 NYT Front Page Article

Figure H.2: Daily Newspaper Mentions

Note: Number of Times "Magic Johnson" appears in newspapers across the country by day. The spike occurred on November 7th, 1991.

Figure H.3: Past Month Sources of AIDS Information in 1991