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Assessing the Impact of Ferry Transit on Urban Crime

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Introduction

In densely populated areas, the expansion of public transit can contribute to the quality of life of residents, by providing more options to commute to work and to reach recreational outlets, and can even become a driver of the economic growth of the area itself. Still, researchers, and perhaps more influentially, residents and policy makers, voice concerns about crime and the safety of passengers in the area. The theory typically suggests that the increased density of individuals in stations increases the likelihood of crime, therefore crime rates ought to increase. This concern has generated a substantive literature on transit stations and crime. However, the measured impacts on crime vary in both direction and magnitude from study to study (Ihlanfeldt 2003; Billings, Leland, and Swindell 2011; Weber 2019; Ariel and Partridge 2016; Ridgeway and MacDonald 2017).

This paper focuses on analyzing the association between the introduction of the NYC Ferry transit network and crime in the surrounding area. We argue that there is significant evidence of a general reduction in crime in the region around ferry stations, and no significant evidence of increase. We also argue there is no evidence that crime is displaced further from the station.

These results have been obtained by using the traditional difference-in-differences methodology at various radii around the stations, and supplementing it with two additional major considerations. The first important consideration is the different treatment propensities in the treated stations, examined using Causal Random Forests (CRF), a method which originates in machine learning and has been gaining popularity in both the geospatial and economic literature (Davis and Heller 2020; Deines, Wang, and Lobell 2019; Zhang et al. 2018; Hoffman and Mast 2019; Credit 2021; Ho et al. 2007). The second important consideration is the tendency of CRF to explore the space of interaction effects between the independent variables, which mirrors manual inclusions such as individual time fixed effects. These findings contradict the presupposition that additional transit stations necessitate crime increases, and accentuate the potential benefits of public transit that are already well known in the urban economics literature.

The urban economic impact of transit networks are lower transportation costs, which have numerous positive effects including altering property values as access to amenities increases (Mills 1967; Wheaton 1974; Ahlfeldt et al. 2015; Letdin and Shim 2019). The NYC Ferry allows passengers to move between the coastal areas of Brooklyn, Queens, and Manhattan neighborhoods, via the Hudson River. The NYC Ferry is part of an already heavily utilized and wide set of transit options residents and commuters in such areas are offered. The urban context for this study is rather peculiar. In fact, it features a highly densely populated area, the presence of pre-existing alternative transit options at discontinuous intervals, and a constrained geographical area (waterways). The NYC transit includes: subways, bus, taxi, bicycle sharing services, ride-sharing services, (rarely) helicopters, and now ferries (Schillinger 2017). Analyzing the effect of a single transit line in this context is not straightforward: multiple factors can be at play that can influence crime events, including the transit services in the area of investigation. Crucially for this study, the NYC Ferry is a brand new transit option, opened in three waves in May, July and September 2017, with numerous expansions planned along 2019-2021 (NYC Ferry 2019b). We focused on weekly crime trend analysis from two years before the start of the service operation, to two years after. This exhausts the currently available data in a symmetrical window around the station introductions, and contains the full set of NYC Ferry openings.

To analyze the crime trends around the stations of the new ferry line, we aggregated the schedule of the line with the crime reports from the city of New York. Data are assembled into panel data that includes all crime occurrences within several radii from the ferry stations. Specifically, the radii considered are: $0.25mi, 0.50mi, 1.00mi$, representing roughly a small, medium and large distance from the station. Similar distances have been used in (Jackson and Owens 2011) and (Wu and Ridgeway 2021) to determine crime around subway stations, and (Bertaud 2003) in terms of a walking distance for a commute to transit stations. We check larger radii since we are inherently coastal and the area will contain a fair deal of water. All data are publicly available and part of the open data initiative the city of New York is pursuing.

Overall, we find significant evidence of crime decreases around the treated stations. We consider displacement as a factor and so we explore this reduction at several radii around the station, and find that the crime reduction does not diminish as the radii increase in size. This suggests that displacement is not sufficient to counteract the effects of the station. Because of the peculiar urban context we introduce two important additional measures. Comparisons between random forests and the spatial models have very recently been explored in (Credit 2021), where the random forest models have been found to slightly outperform other spatial models. The CRF method makes efforts to account for the differing treatment propensities of stations based on their observable characteristics, as uniquely determined by their geospatial position. CRF accounts for nonlinear and discontinuous spatial differences between the regions of treatment. These methods suggest similar findings and have a similar pattern of results, suggesting that the finding is robust to the problems we considered, and reinforcing the CRF as a potential tool for future urban transportation challenges where location factors are relevant.

The paper is organized as follows: in Sec. Description and Related Research we describe the experiment and present the related research; Sec. Data Selection and Preparation describes the data set and the data transformation to aggregate the data sets and organize the data in a format that is suitable for the analysis; in Sec. Methodology & Results we describe the methodology to analyze the data set and define the crime trends and comment on the results of the analysis; and finally, in Sec. Conclusions, we draw our conclusions.

Description and Related Research

Economic theory suggests that crime counts change depending on the cost of committing a crime and the expected benefits of committing the crime (Becker 1968). This aligns directly to the routine activities perspective on crime - the cost & benefits of committing a crime are dramatically altered by the convergence in space and time of likely offenders, suitable targets, and a lack of capable guardians (Cohen and Felson 1979). Appropriately, a large body of research indicates that transportation networks are associated with changes in the crime rate (Ihlanfeldt 2003; Billings, Leland, and Swindell 2011; Weber 2019; Ariel and Partridge 2016; Ridgeway and MacDonald 2017), but the measured changes in the literature vary in both magnitude and direction- even for similar treatments. Estimates range from about $\pm 5\%$ for aggregated crime at the districts surrounding stations. Shifts in crime are often significant and measurable even when the scope of the transportation network change is modest (Weber 2014; Heywood and Weber 2019; Herrmann, Maroko, and Taniguchi 2021; Jackson and Owens 2011). This is not a guarantee of impact in all cases, however some networks have large changes and little regional effect can be found (Sedelmaier 2014). In our case, however, we do have such a citywide change, that is the introduction of multiple regular ferry lines into NYC. Below, we summarize the literature's arguments for either crime increases and crime decreases along the transit stations.

On one hand, one might expect crime to increase as the aggregation of individuals (and their belongings) waiting or exiting the ferries are condensed into a smaller space, leading to a denser body of potential targets for criminals (Christens and Speer 2005; Loukaitou-Sideris, Liggett, and Iseki 2002). Other work utilizing subway station closures also finds an association between subway station openings and robbery hotspots (Herrmann, Maroko, and Taniguchi 2021). However, it is not clear if this is going to increase the crime rate above and beyond the ambient rate. Previous work finds that open stadiums increase the rate of crime (over time) but not the rate of crime per capita (Kurland, Johnson, and Tilley 2014). Crime increases at open stations (such as in the DC area) can be substantive; (Irvin-Erickson and La Vigne 2015) measure that larceny occurs nearly twice as often during peak hours than it does during all non peak hours combined. One such recent study has identified total crime increases by about 5% at open stations, even after controlling for ridership, (Phillips and Sandler 2015), suggesting that there are more factors than the simple aggregation. In addition to aggregating potential targets, transit stations may attract new potential targets to the area as new commuters or tourists pass through the area (Altindag 2014). Unique to the ferries, one might be concerned that alcohol distribution on the ferries themselves may lead to increased crime as individuals may be more vulnerable than on other modes of transit (Markowitz 2005; Livingston 2008).

On the other hand, one might emphasize that these waterborne trips are encapsulated for long durations and dropped off at relatively few end destinations compared to bus or subway stops. One might also argue that these trips serve as a complement to an already overworked transit system, and reduce overall walking time for commuters who might otherwise be vulnerable while walking to further away bus or subway stations (Heywood and Weber 2019). Previous work highlights an association between subway station closures, drunk driving, and assaults on those stations (Jackson and Owens 2011). The stations themselves are also naturally secure - they have a single entrance and exit (besides the boats), and their built space is open, naturally lit by daylight, and unobstructed (Sohn 2016). Furthermore, unlike bus stations and many subway stations, several of the ferry stops are regularly staffed by ticket sellers. These officiating staffers may act as eyes and ears which have a crime suppressing effect. In addition to staffers, these stations might serve as natural areas for police patrol relative to an extensive bus or subway route (Newton, Johnson, and Bowers 2004). All of these may generate crime-mitigating effects, which can be particularly strong in neighborhoods that are already low-crime (Ihlanfeldt 2003; Ridgeway and MacDonald 2017).

The lines have been immensely popular, seeing 6,400 riders on their first day of service (Honan 2017), but their services remain a small portion of the NYC transit network. For example, over the data window, the ferry lines averaged about 10,000 daily riders while the subway system transported 5 million and the bus system transported 2 million (Gordon, Offenhartz, and Witford 2018)[1]. In our study, we observed the introduction of the NYC Ferry in 2017 over three phases. In 2017-5, the East River and Rockaway lines were introduced, in 2017-7, the Bay Ridge line was introduced, and in 2017-9 the Astoria line was introduced. We centered our data window around these introductions, extending from 2015-7 to 2019-7, roughly. More introductions occurred in 2020 and after, but the network already existing in 2017 carried a large number of stations.

Fig. 1 illustrates a preliminary examination of the weekly crime counts in $1mi$ around the treated stations. The figure depicts the weekly crime by station (to help visually unify the stations) and by month (to remove some of the potentially distracting seasonal elements). In the figure, the time axis is normalized by weeks to treatment, so that 0 (zero) represents the exact week of treatment for the various

[1] We do not have firsthand access to ridership data for these ferries, they are a private enterprise and we rely on secondary sources for these estimates.

stations, and negative and positive values represent periods before and after treatment, respectively. Untreated stations cannot be plotted on the same figure since they have no such "time after ferry opened." We observe a substantial drop in average weekly crime after the introduction of the ferry, which the results of our investigation confirm to be a trend when accounting for all controls and factors. There appears to be a slight upward time trend over the window that is modestly interrupted by the crime increase around the introduction of the program, and then the upward time trend appears to continue. Still, the net size and significance of any effects are not quantified in Fig. 1, and any control stations are also absent from this figure, so the relative change is of paramount importance. As such, there is a need for more detailed investigation.

The next section discusses the origin and nature of the data sets, and the transformation needed in order to structure data in a format that enables crime trend analysis.

Data Selection and Preparation

In this section, we present the data sources and transformations used in this study. We first describe the nature of the data sources and provide a sense of the type of information they contain. Then, we describe the data transformation and augmentation we perform in order to integrate and format the data to enable the crime trends analysis.

Data Sources

We aggregated the following data sources: the list of crimes reported by the New York City Police Department (NYPD) for the NYC area, and the transit data for the (new) ferry lines.

The first data set is the New York City complaint (crime) data. This is a public data set published and maintained by the city of New York. This data set and its availability fall under the open data initiative the city is pursuing to provide free and transparent access to residents and beyond. The NYC crime data

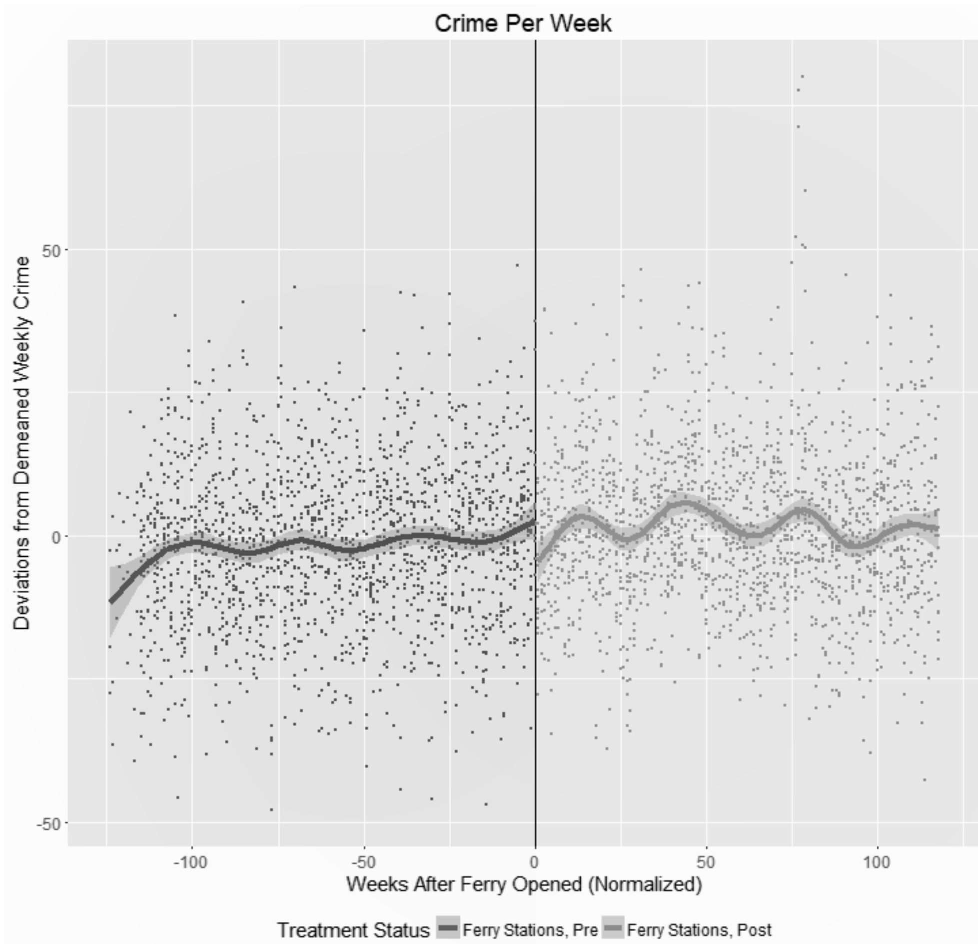

Figure 1. Weekly crime (demeaned by month and station) within a 1mi radius of all treated stations as normalized to their own individual time of treatment (vertical line). The pre-ferry (left) and post-ferry (right) crime patterns were each evaluated with a generalized additive model. This figure shows a large decline in crime after treatment in the treated stations. Untreated stations cannot be plotted on the same figure since they have no “time to treatment”.

set contains complaints reported to the NYPD, including felony, misdemeanor, violation crimes, etc. It extends well before (2015-07) and well after (2019-07) the introduction of the ferry lines, and contains geographic information to identify if the crime is within a given radius of a particular station.

The second data set describes the ferry service schedule (NYC Ferry 2019a). This data set is also provided by the city of New York under the open data initiative. Data is organized as per the General Transit Feed Specification (GTFS) format, where data is normalized (decomposed) in several structures. We observed that the hourly data is extremely sparse (almost entirely zeros), and the ferry schedule does not vary much on a week-to-week basis, so we cannot exploit the hourly variation. Instead, we simply aggregate it as weekly to use as the basis of our analysis.

Methodology & Results

In order to have a sound analysis of the impact of the new ferry lines on crime counts, we need to compare crime activity trends in areas nearby the ferry stops against the respective surrounding areas. In practical terms, we need to identify a number of (other) areas reasonably close to the ferry stations that will function as a baseline for our analysis. We call these areas *placebo stops*, or *placebos* for short. Before presenting the results of our analysis, let us describe how placebo stops have been identified.

Identifying Placebo Stops

Ferry stations are limited to the coastal areas between the neighborhoods of Manhattan, Queens, and Brooklyn. Placebo stops are areas surrounding the ferry stops that are far enough to be deemed as not impacted by the traffic generated by the ferry lines. Placebos have been chosen according to the following characteristics: are in the mentioned neighborhoods, are on the borough boundaries (which are typically bounded by water), are at least one mile away from any ferry station, and have some crime in the data window (the ferry stations typically have some nearby crime).

To identify the stops we set up a semi-automatic process, composed of three steps: (i) generate a geo-fenced area that includes the three boroughs, (ii) generate a large number of suitable placebos within such area, (iii) remove the stops that lack crime entirely (i.e. placed on uninhabited islands). In step (i) we select the region within one-mile of the Brooklyn, Manhattan, or Queens borough boundaries. In step (ii), within this boundary region, we generate 150 placebos at least one-mile apart using QGIS. Step (iii) removes all stations which have essentially zero crime (less than 0.0001 crimes per day). These randomly generated placebo stations were almost entirely located in the Jamaica Bay Wildlife Refuge in the southeastern portion of NYC, or other large parks in this less populated region. In the end, the number of placebos is reduced from 150 to 125, and all the naturally created ferry stations had some amount of crime. Fig. 2 shows the placement of both actual and placebo stops. We note that the distance between stations is critical, and several of the treated stations have overlapping areas at the largest radius. We also note that the treated stations tend to be clustered in a particular longitude and latitude of the city (the central region of the Hudson River), a propensity which the causal forest approach takes into account.

Analysis

As we summarize each of the station types (treated ferry stops and placebo ferry stops) we find that the treated stations are broadly similar in overall crime before and after treatment, so there is no visible overt transformation of the region without the inclusion of controls. Table 1 shows the summary statistics for the ferry and placebo stops, pre and post treatment. The placebo stops do typically have higher mean weekly crime, but the placebo stations can be placed (at random) further inland than ferry stations. Since crime rarely occurs over the water, this results in placebo stations having slightly higher mean crime, an effect which is mitigated at the larger radii. Overall, the standard deviation of these placebo stations is quite large and envelops the mean crime of the pre- and post-treatment stations. At the $1mi$ radius, the crime counts are extremely close, so we present these results as primary. Importantly, the treatment stations are tightly clustered in a particular region of the city, see Fig. 2, but this is a natural feature of the geography of the region. The propensity of this region to treatment, and

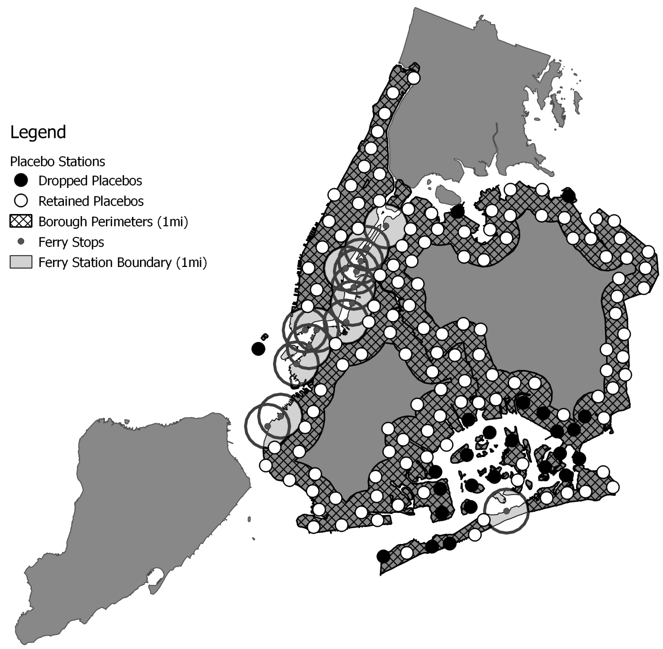

Figure 2. The ferry stations are indicated and the one-mile buffer around them is drawn. Placebo stations are marked in white and selected from the crosshatched area around the borough boundaries. All placebo stations are all at least 1.00 miles apart, while the natural ferry stations happen to be placed somewhat closer, only 0.25 miles apart.

the potentially differing crimes in various regions of the city are the two main advantages CRF approach.

We see smaller amounts of crime captured in smaller radii (about 2 per week in the smallest radius), but in general the crime is non-zero. Large areas represent a larger base of crime upon which reductions can be found, and we want to make sure we do not omit some possible impact of the stations at longer walking distances. Therefore, if the effect happens at a distance from the station (ex. commuters tend to walk in safe groups to the station), then crime will decline more in larger radii.

When looking at these averages (which do not normalize by month or station), we see almost no change in crime before or after treatment. However, we wish to compare the differences in the treated stations to the differences in the placebo

Table 1. Summary Statistics of Weekly Crime

	Pre-Treatment Ferry Stops			Post-Treatment Ferry Stops			Placebo Ferry Stops		
Variables	Mean	SD	Total	Mean	SD	Total	Mean	SD	Total
Crime, *0.25mi*	2.183	2.314	3,858	2.225	2.505	3,879	7.426	10.507	217,222
Crime, *0.50mi*	14.492	8.902	25,608	15.061	11.240	26,251	26.574	31.735	777,296
Crime, *1.00mi*	102.945	51.393	181,903	106.452	55.708	185,546	93.721	96.292	2,741,346
N. of Stops		15			15			125	
Avg. N. of Weeks		117.8			11.2			234	
N. of Obs.		1,767			1,743			29,250	

Note: We dropped all stations with less than 0.0001 crimes/day overall since they show nearly zero variation in crime and cause multicollinearity problems in the spatial estimation. This procedure only dropped placebo stations. These placebo stations typically were defined inside parks, such as the very large Jamaica Bay Wildlife Refuge. We note that treatment begins at different times for different stations, so we provide the average treatment - the panel is overall balanced.

stations. The methodologies we employ below will control for these matching trends, since we want to know if the crime rate *relative to placebo stations* has changed in the treated stations after treatment. Despite centering the data window in the middle of three treatment waves (as indicated in Fig. 1), the treated stations have been treated on average 59% of the time, slightly less than a perfectly even split.

To examine the association between the new ferry transit system and crime, we employ the traditional difference-in-differences (DiD) approaches employed for program introductions in the past, (**mammen2019**; Di Tella and Schargrodsky 2004).We will extend this baseline analysis by non-parametrically including the differing propensities for treatment for each geographic region in the subsection Causal Random Forests (CRF). We first use the DiD specification described in Equation 1. We use brackets, $[x]$, to indicate that there is one indicator for each and every category of x, excluding a base level.

$$\begin{aligned} crime_{it} = \beta_0 + \beta_1 TreatedStation_{it} + \beta_2 Trend_{it} * a_i[Station_i] \\ + e_{it} + a_i[Station_i] + b_t[Week_i] \end{aligned} \tag{1}$$

In Equation 1, a_i is a vector of individual-specific fixed effect terms (for each station), and b_t is a vector of time-specific fixed effect terms (for each week), following the panel data difference in differences structure in (Angrist and Pischke 2008). We absorb these coefficients since the variable of interest is β_1. The coefficient β_1 is the DiD coefficient, the interaction of treated groups in treated periods. Note that we do not include the treated group or treated period directly in the estimation since they are superseded by the fixed effects for both time and individual (a_i, b_t)(Wing, Simon, and Bello-Gomez 2018). We include individual time trends in accounted for by $\beta_2 Trend_{it} * a_i[Station_i]$. Finally, the error term is e_{it}.

Sensitivity to Regressors

In Table 2 we show how the addition of controls has an effect on the association between crime and treatment. We use the largest radius (1mi) to match Fig. 2. The

Table 2. Sensitivity to Regressors

VARIABLES	(1) LINEAR 1	(2) LINEAR 2	(3) LINEAR 3	(4) LINEAR 4
Treated Station	12.21***	3.597**	4.768***	-0.262
	(1.438)	(1.703)	(1.800)	(0.918)
Fixed Effects	No	Yes	Yes	Yes
Weekly Controls	No	No	Yes	Yes
Individual Time Trends	No	No	No	Yes
Observations	32,760	32,760	32,760	32,760
R-squared	0.001	0.973	0.977	0.979
Number of stop_id	140	140	140	140

Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

initial estimate with no controls for region or time, Table 2 column 1, shows a significant estimated in crime at treated stations relative to untreated stations of nearly 12 crimes per week. This is in an understandable contrast to the controlled Fig. 1, which controls for both month of year and station fixed effects. The addition of fixed effects for the station, columns 2, decreases the magnitude of the significant coefficient to only about 4 crimes per week. When included as indicators, the fixed effects capture a large portion of the variation in crime between stations. Further addition of weekly fixed effects leaves the coefficient significant and of a similar order of magnitude at about 5 crimes per week. Lastly, column 4, we include individual time trends for each station - since some stations may have upward trends in crime over the data window and others may have declining crime trends - the neighborhoods are many and varied (Friedberg 1998). Including these important trends drops the significance of the station opening to zero and inverts the estimated effect of station opening. This dramatic change highlights the importance of interaction effects. One creates the individual time trends as an interaction between the individual fixed effect indicators and the time trend. As we highlight later, CRF performs a deep search the space of interactions as part of a random forest and arrives on similar point estimates rapidly.

On Evidence of Potential Displacement

In Table 3, we show the consequences of increasing the radii from $0.25mi$ (no overlap) to $0.5mi$ (some overlap in nearby treated stations) to $1.00mi$ (further overlap in many of the nearby treated stations but none in placebo stations). One might consider such larger (or smaller) radii because it is unclear what distance crime would spread from the station. One would anticipate that if crime was displaced outward from the center, then the estimated decline in crime would be diminished as the radius increases. No such evidence is found. On the contrary, an attraction of crime towards the station, coupled with an overriding reduction in crime would match the coefficient pattern we see in Table 3. The coefficient on

Table 3. Difference in Differences Estimation At Different Radii

VARIABLES	(1) 0.25 MI	(2) 0.50 MI	(3) 1.00 MI
Treated Station	-0.266**	-0.448	-0.262
	(0.135)	(0.327)	(0.918)
Weekly Controls	Yes	Yes	Yes
Station Fixed Effects	Yes	Yes	Yes
Individual Time Trends	Yes	Yes	Yes
Observations	32,760	32,760	32,760
R-squared	0.903	0.957	0.979
Number of unique stops	140	140	140

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

treated stations for the 0.25 mile radius, Table 3 column 1, represents an significant decrease of about 12% in crime counts relative to pre-treatment stations[2]. As we increase the radius to 0.50 miles, the coefficient remains negative and increases in magnitude to about 0.5 crimes per day, just over 3% of weekly crime relative

[2] We obtain $12\% \approx 0.27/2.18$ using the average daily crime in the treated $0.25mi$ radius in Table 1.

to pre-treatment stations[3]. The largest radius, $1.00mi$, the coefficient on treated stations increases to a decline of about 0.25 crimes per week, about 0.25$\%$ of the daily crime in the pre-treatment stations[4]. Broadly, we interpret these results to mean that there is an associated decline in crime around the stations after treatment, but as the station radii increase the effect diminishes. This cannot rule out some sort of crime displacement - it is possible that the crime is pushed to further distances around the station which dampens the crime reduction. However, we note we do not find the same the pattern of declining coefficients in Tables and 4. In the same breath, as the station areas increase to a larger and larger region, one might also begin to worry about the spatial relationship between the stations (placebo or otherwise), which we consider in the next section. We will continue to show the relationship at each of the three radii, as the pattern may change in some specifications.

Causal Random Forests (CRF)

The Causal Random Forests has several advantages over the other methods that makes it appealing (Athey and Wager 2019). However, it has an approach that is fundamentally distinct from traditional linear methods, and therefore demands some discussion.

The CRF approach splits the data set repeatedly along input variables in order to generate a richly textured map, called "tree". Each "leaf" of the tree contains a rectangular cluster of stations, for which the the crime can be evaluated. Fig. 3 shows a illustration of such a tree, which was constructed using longitude and latitude to predict the anticipated average treatment effect of a new station across the surface of the city, holding all other variables constant at the mean. We note that since each station has a unique and fixed longitude and latitude, the leaves of these trees can become detailed enough to encompass a single station without needing to include unique fixed effects for each station. This figure is meant to

[3] We obtain $3.2\% \approx -0.45/14.49$ using the average daily crime in the treated $0.50mi$ radius in Table 1.
[4] We obtain $0.25\% \approx -0.27/102.95$ using the average daily crime in the treated $1.00mi$ radius in Table 1.

explicitly contrast the inherent structure of a linear model which would require that the maximums and minimums of crime prediction (if any slope was identified whatsoever) would be present in the corners.[5] We further note that the output of CRFs are average partial effects, not anticipated counts in a region as one would gather from a traditional linear model, so no extraction of DiD coefficients is needed. We note that the average treatment effect only varies in our example by two dimensions in Fig. 3, which one might anticipate implies the CRF estimate is essentially unvarying with respect to factors other than longitude and latitude. However, this represents a flattened cross section of the CRF, taken at the mean of all other factors (such as time). Restoring the other dimensions greatly expands the number of sharply divided regions and the range of estimated treatment effects, which in Fig. 3 have been averaged out.

A random forest creates multiple trees (5000 in our case), and then averages the predicted result. The leaves are clearly visible as different colored shaded regions. However, the leaves are not regulated to be flat surfaces along the map. The leaves can be assembled to be similar regions with dimensionality matching the number of model inputs.

Unlike other RF approaches, the CRF variant outputs an estimated treatment effect for a given treatment variable (Athey and Wager 2019). To calculate the average treatment effect, the CRF weights each change in outcome by the propensity of treatment, where the the likelihood of treatment is estimated by a random forest. The details about how these trees are made are describe in (Wager and Athey 2018), but the root estimation process of random forests have been applied with great success in the geospatial literature (Prasad, Iverson, and Liaw 2006; Stevens et al. 2015) and health literature (Lu et al. 2018). Recently, random forest has been compared to econometric models and found to be slightly superior at population density estimation (Credit 2021) than competing spatial models. These advantages, however, do not mean that this approach is a panacea for these

[5] We do not intend a model of crime as a linear function of longitude and latitude to function as a "straw man", but rather to illustrate the mechanical differences between the estimation techniques.

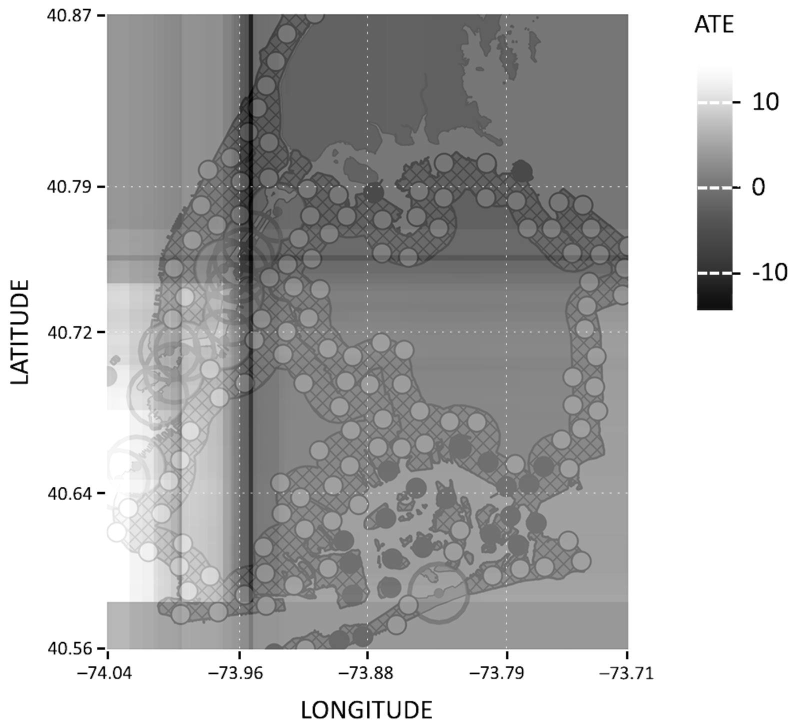

Figure 3. A composite of the map of the ferry stations from Fig. 2 overlaid with a causal random forest of the estimated average treatment effect of a new station at various latitudes and longitudes. The different shades each indicate a different, discrete leaf of this particular estimation. This stands in contrast to the smooth and monotonic gradient one would find if simply including longitude and latitude in a linear regression. Critically, this holds all other factors constant at the mean, as other factors vary, the longitude and latitude will interact and have heterogeneous effects - the leaves exist in more than just these two dimensions.

complications, which is why we have presented this evidence in tandem with traditional methods, highlighting some components where the CRF seems to have improved on the overall estimates.

Random forests first calculate an expected outcome $(\hat{\mu})$ for each training sample x, calculated as:

$$\alpha_i(x) = \frac{1}{B}\sum_{b=1}^{B}\frac{Y_i\mathbf{1}(\{X_i \in L_b(x), i \in S_b\})}{|i : X_i \in L_b(x), i \in S_b|} \tag{2}$$
$$\hat{\mu}(x) = \sum_{i=1}^{n}\alpha_i(x)Y_i$$

Where x is a particular training sample, B is the number of trees, S_b is the particular subsample used to create a particular tree. $L_b(x)$ represents the leaf of the bth tree which contains the training sample x. However, in CRF, these predictions are not our final output. (Wager and Athey 2018) highlight that $\alpha_i(x)$ serves as a data-adaptive kernel to measure how often the ith training example falls in the same leaf as the test point x. These estimates exist to serve as weights to estimate the average treatment effect $\hat{\tau}$:

$$\hat{\tau} = \frac{\sum_{i=1}^{n}\alpha_i(x)(Y_i - \hat{m}^{-i}(X_i))(W_i - \hat{e}^{-i}(X_i))}{\sum_{i=1}^{n}\alpha_i(x)((W_i - \hat{e}^{-i}(X_i))^2} \tag{3}$$

Where $\hat{e}(x)$ serves as an estimation of the propensity score and $\hat{m}(x)$ serves an estimate of the expected outcome at the given level of treatment, both calculated with separate regression forests. The superscript $(-i)$ denotes that the element i was omitted in creating an estimate, sometimes called an “out of bag” or “out of fold” estimate. See (Athey, Tibshirani, and Wager 2019) for more details and calculation of standard errors which are outside of the scope of this article.[6]

The first advantage for observational studies like ours is that CRF makes an estimate of the propensity of treatment for a station based on the observable, and weights the estimated treatment effect accordingly. This is advantageous in our application because, as it is visually clear from Fig. 2, the locations along the Hudson Bay are chosen for treatment and the placebo stations have inherent properties (identifiable by their specific position) that render them less viable.

[6]Numerous tuning parameters can be selected (such as the size and depth of trees), for which we use the self-tuning option as implemented in the R **grf** package.

Distant placebos should be less weighted and the nearby placebos should be more heavily weighted.

The second advantage is that by providing the continuous variables longitude, latitude, and week, the CRF can construct station effects, temporal effects, and interactions between these effects and other variables. In traditional regression, one would have to bound regions of interest (in both time and space) either manually or by providing fixed effects for each station and/or time period. Fig. 3 shows one such rich set of leafs found within a cross-section of the data, the interactions become more complex when other dimensions are considered (ex. time, neighboring crime). Furthermore, manual interactions with these indicators are typically limited, but CRF searches for such interactions between variables as an incident of defining a new leaf. In practice it is recognized that the random forest family of estimators typically performs better with a single column of detailed information rather than a sparse set of binary indicators (Friedman, Hastie, and Tibshirani 2001), because it is able to search the reduced feature space more completely. As such, we provide longitude, latitude, and week instead of the fixed effects for each indicator. This means instead of supplying hundreds of dummy variables for both time and station, we can reliably construct sufficiently defined leaves with fewer inputs.

The final inputs upon which CRF generates the trees are listed in Equation 4.

$$crime_{it} = CRF(Longitude_i, Latitude_i, Week_t, TreatedStation_{it}) \tag{4}$$

We estimate the average treatment effect (and their statistical significance) for each of the three radii in Table 4.

Similarly to the results highlighted in previous two tables, we find general evidence of a decline in weekly crime in the treated stations. Such decline is significant at all three radii, though the smallest radii is not significant. The estimated decline in crime counts increases as we consider a larger region around the station, which is not in keeping with the thesis of displacement. The totals

Table 4. Causal Forest Estimation Results, Average Treatment Effects (ATE)

VARIABLES	(1) 0.25 MI	(2) 0.50 MI	(3) 1.00 MI
$\hat{\tau}_{overlap}$	-0.706	-4.89***	-11.475***
	(0.735)	(1.793)	(4.608)
Week Controls	Yes	Yes	Yes
Long. and Lat. Controls	Yes	Yes	Yes
Observations	32,760	32,760	32,760
Number of unique stops	140	140	140
Number of Trees	5000	5000	5000

Standard errors in parentheses
$\hat{\tau}_{overlap}$ the ATE was calculated with the recommended overlap weighting method.
*** p<0.01, ** p<0.05, * p<0.1

suggest that at the smallest radius, a decline of about 1 crime per week represents a 33% decline in crime in the immediate vicinity of the station.[7] We point out that although this effect is large as a proportion, it is a numerically small decline in a very small $0.25mi$ (1320 ft.) radius, much of which is not suitable for crime because it is water. A quick back-of-the-envelope calculation suggests this is a decline of about 7.23 crimes per square mile of treated area.[8] The built space in that range is an open, unobstructed pier that allows for unblocked vision, and only one natural entry or exit point other than the boats themselves. This finding seems in keeping with the notion that eyes and ears can mitigate crime in a small area that is immediately under supervision, at least under these types of built conditions. As the radius increases to a half-mile, the treated area triples, although roughly half of it remains water. The associated decline in crime increases numerically to approximately 5 crimes per week, but decreases as a proportion of the pre-ferry crime to 32%. [9] Finally, at the largest radii, the measured decline in crime of 11 per week is roughly 11% of the pre-ferry crime in these stations, which is in keeping with the idea that the transit stations may actually reduce potential

[7] We obtain 33% $\approx 0.71/2.18$ using the average daily crime in the treated $0.25mi$ radius in Table 1.
[8] $0.701/(0.25^2 * \pi) * 0.5$ for approximately half being water.
[9] A decline in the crime count of 32% $\approx 4.89/15.06$ and about 12.54 crimes per square mile $4.89/(0.5^2 * \pi) * 0.5$.

criminal behavior. The decline per square mile is about the same as the original estimate, about 7.3 crimes removed per square mile treated.[10]

Conclusions

In this paper we examined the introduction of 15 transit stations in 2017 which transport nearly 10,000 passengers per day. We identified a substantial decline in weekly crime in treated transit stations relative to placebo stations. This change in crime is consistently negative across linear estimations, the standard DiD methodologies, and significant for the smallest radii. When using the CRF approach, the coefficients are negative for all radii, and the significance increases as the radii increase in size. While the DiD uses fixed effects to account for treatment regions and manually must include time trends and interactions, the CRF methodology allows for automatically searching these using position of each station through latitude and longitude, and similarly includes temporal interaction terms. CRF also accounts for the propensity for treatment based on their observable characteristics- we note it has constructed an approximation of the landmass of Manhattan and treats it differently than Brooklyn and Queens despite not being provided water information. These methods, when used by CRF or manually done with a traditional approach, suggest a reduction in crime after the introduction of the ferry stations. These estimates suggest that estimated decline in crime is between approximately 11 crimes/week (11%) in a one mile radius, and at the smallest radius the estimated crime change is approximately 1 crime/week (33%) in the near exact vicinity of the station. None of these methods find any evidence of a statistically significant increase in overall crime along the station stops at any radius. Therefore, we conclude that the addition of these transit stations appears to be safe, at least to the extent that they are not associated with any measurable increase in crime. This is of particular interest in a time period where there is great concern about subway crime (Skelding 2022).

[10] A decline in the crime count of $11\% \approx 11.475/106.45$ and about 7.3 crimes per square mile $11.475/(1^2 * \pi) * 0.5$.

For future work, it would be of interest to explore the monitoring of nearby populations, and alterations of nearby real estate, as in this study we did not have suitable proxies collected at reasonable intervals.

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