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RESEARCH ARTICLE

Registering a proposed business reduces police stops of innocent people? Reconsidering the effects of strip clubs on sex crimes found in Ciacci & Sviatschi's study of New York City

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ABSTRACT

Ciacci & Sviatschi's 'The Effect of Adult Entertainment Establishments on Sex Crime: Evidence from New York City,' published in *The Economic Journal*, concluded that opening new adult entertainment businesses reduces sex crimes, with the most compelling finding that '[strip clubs, gentleman's clubs, and escort services] decrease sex crime by 13% per police precinct one week after the opening.' We contend that the study's conclusions speak beyond the data, which cannot support these findings because they do not measure the necessary variables. The study uses the date a business is registered with New York State as a proxy for its opening date, but the actual date of opening comes weeks or months later, after requirements such as inspections, licensure, and community board approval. The study then uses police Stop, Question and Frisk Reports as data about subsequent crimes. As reports created to memorialize forcible police stops based on less than probable cause, 94% of these reports document that the police had an unfounded belief in criminal activity, and the person stopped was innocent of any crime. In effect, what the study has done is measure changes in police encounters with innocent people in the week after an entity has filed the paperwork that will eventually allow it to open as a business. The study lacks construct validity, cannot reject the null hypothesis of its most important finding, and its methods fall short of the rigor necessary to permit replication.

Introduction

This paper takes up the problem of reliability and replication in research about crime and policing. It will consider a published, peer-reviewed study with results we will argue are neither reliable, nor replicable. In doing so, we hope to: 1) ensure people who consider this particular study a contribution to knowledge understand what we will argue are its acute and fatal shortcomings; 2) explore of the hazards of utilizing administrative data as an operationalized proxy for variables without taking great care to understand the provenance, meaning, and limits of the data; 3) emphasize the need for researchers to closely collaborate with practitioners in study design, execution, and the interpretation of results. Accomplishing these goals, even in part, will hopefully help improve the rigor of police and crime-related research at a time when social science is under increased scrutiny for the overall quality of its work, and there has been little success in producing durable results in police-related research (Monk & Koziarski, 2023).

In the summer of 2021, Ciacci & Sviatschi published 'The Effect of Adult Entertainment Establishments on Sex Crime: Evidence from New York City' in *The Economic Journal*. It found strip clubs, gentlemen's clubs, and advertised escort services reduce sex crimes in New York City each time one opens, with the most compelling finding that they 'decrease sex crime by 13% per police precinct[1] one week after the opening, and have no effect on other types of crime,' an assertion featured in the abstract and showcased as the study's principal result. The paper made international news (Ciacci & Sviatsch, 2021; Cohen, 2021; Oppenheim, 2021), contributing to debates about policing and effective public safety. The study was important because it tested a novel hypothesis about the relationship between sex crimes and lawful alternative outlets for the behaviors typically associated with them. It suggested that providing such outlets – in contrast to heavily regulating consensual sex work – markedly improves public safety. As the authors note, this has 'several policy implications,' (p. 4),[2] especially as the nation reevaluates the role of police in public safety, and reconsiders the stigma and criminalization of sex work.

We believe the study has fatal problems, however, and they preclude drawing any of its conclusions. We will argue the data types used by the study do not – and cannot – measure what the study's model requires them to, so the study therefore lacks construct validity, makes fatal category errors, and speaks beyond its data. This problem also precludes attempts at replication, which would only be possible if the data were able to causally power the model utilized in the study, which they cannot. Following recent analyses that draw attention to considerable errors in peer-reviewed scholarship about law enforcement (Carroll et al., 2023) and public health (Lambdin et al., 2023), the analysis here can be used as a case study that demonstrates the need for closer collaboration between researchers and the practitioners who create and maintain the data used in their studies.

Construct validity and the limits of replication

We are not suggesting fraud, malice, or deception. Further, we have every reason to believe the analytical methods were thorough and properly executed. To be clear, we believe there are problems fundamental to the study's data types, which should have been known to the study team had they investigated their provenance and meaning. Given that these data problems went unacknowledged in the study, a proper replication would exclude all data used for its independent variable, and approximately 95% of the data used for the dependent variable, leaving anyone unable to execute the study's model to an acceptable level of rigor. We therefore argue that the study cannot reject the null hypothesis underlying its most important finding: that one new adult business decreases sex crimes at the precinct level within a week of opening.

To make our argument, we will:

(1) Assess the suitability of the data used to measure the independent variable,
(2) Assess the suitability of the data used to measure the dependent variable,
(3) Discuss the implications of excluding these data for the study's implied null hypothesis,
(4) Recommend a re-review of the study and a retraction if necessary,
(5) Make a case for closer collaboration between researchers and the practitioners who create and manage administrative datasets of interest.

Analysis

Assessing data for the independent variable: the opening date of a strip club

In the study's abstract and conclusion, it asserts this principal finding: 'We find that these businesses decrease sex crime by 13% per police precinct one week after the opening, and have no effect on other types of crime.' In the study's methodology, the date of registration of the sex club as a business with the State of New York is used as a proxy for its opening date, with the study asserting that 'we can use the date of registration as a quasi-natural experiment to study the effect of these

businesses on sex crime' (p. 2). In doing so, the authors used Reference USA, a national data source for commercial establishments, to compile a list of 'all registered adult entertainment establishments from 2004–2012 in NYC' (p. 8). This reference contains data from New York State about when the business was registered with the Department of State. To cross-check sources and assign registration dates as opening dates, the authors used 'businesses' records such as the Yellow Pages, Superpages and the NY State Department of State records, we match almost every establishment with a registration date to proxy the effective opening date of each establishment … ' (p. 9). The study also used the Yellow Pages and Superpages to note when adult businesses appeared on these sites, with the logic that 'Advertising a business in these directories is free, and it takes at most five business days to get an establishment advertised after applying online' (p 8).

The suitability of superpages and the yellow pages as data sources

Before looking at the acute problem with using New York State business registration dates as opening dates, we will examine the use of Superpages and Yellow Pages as data sources, and discuss the problem this method of data collection broaches with regard to the effects of outcall escort services at the precinct level.

First, we can discard all opening dates surmised only by when a strip club or adult entertainment business first appeared in a Yellow Pages or Superpages, if the paper's conclusion purports an effect within a week of opening. At a time when these modalities are anachronistic throwbacks to the pre-internet era, and the main source of online information about businesses is Google or websites such as Yelp, there is no plausible reason to believe an adult business will contact these services for a listing around the time it opens, and the authors note there will in any case be a five day margin of error around the publication date if they decide to do so.

More importantly than this unreliability, these sites are poor sources of data. The authors cited from 76 to 280 adult entertainment businesses in their sample between 2004 and 2012 (p. 8). A search of the Superpages on 20 August 2023 produced a total of 15 listings for 'strip club,'[3] of which 11 are citywide outcall escort services that preclude an analysis at the precinct level (and several of which aren't located in New York City, making a precinct-level specification impossible). A search for 'escort' likewise yields six listings for citywide and metro area outcall services, and two listings for male strip clubs in Manhattan.[4] A search of 'gentlemen's club' in the New York City area yields five listings.[5] Four are strip clubs in New Jersey, to be excluded for that reason, and two are for the same male strip clubs. Searches for these terms in the Yellow Pages yields the nearly identical results: 15 strip clubs,[6] five gentlemen's clubs,[7] and eight escort services,[8] nearly all of them either operating citywide or from locations outside of New York City. The listings also overlapped between categories, with many of the same businesses appearing in two, or all three. If the records on these websites were similar in number to those in 2023, these websites were not meaningful sources of either the analytic sample, or the value of its opening dates.

Concurrent problems with the study's model

As alluded to above, the inclusion of citywide outcall escort services reveals another concern. If the authors used the physical location of an outcall escort service to study effects within its precinct, it indicates a fundamental misunderstanding the citywide nature of the sex work concerned. Outcall escort work does not confine itself to particular neighborhoods or precincts, which is problematic for the study's design. Workers for these services do not need to live in or near the physical location of their dispatcher, and customers do not seek online outcall services because the location of the business is near them at the precinct-level; instead, they search the internet and engage with services based on their appeal and affordability rather than precinct-level geographic convenience. The inherent citywide nature of these businesses, the ways in which their workers are often disconnected from their administrative business locations, and their reliance on universally accessible websites

suggests a deeper problem with construct validity: its causal model is untenable at the precinct level. Scores of individually advertised escorts, and escort services, come and go from several widely accessible internet websites that advertise their services citywide and beyond,[9] most of which are never registered with any entity because they are illegal both as sources of prostitution and as businesses that do not pay taxes or comply with labor laws. The idea that there is a significant marginal effect on sex crime at the precinct level the week one of the observable businesses opens, and its effect is detectable at the precinct level above the constant churn of workers and businesses in an illicit market of several hundred escorts advertising citywide services on the internet at any given time, outright strains credulity.

The suitability of state registration as a data source

This brings us back to using the date a business is registered with New York State as a proxy for its opening date, considering it provides a discrete date memorialized by a formal process between the government and a business. However, the date of registration is not an opening date, and has no predictable relationship to it, regardless of the type of business, or whether it requires the extra reviews necessary for a liquor license. New York City's guidance to aspiring business owners reinforces the point that registration occurs well before opening. The city's website that lays out the steps for starting a bar,[10] or any general business,[11] and provides the same steps for both. It lists 'Register your Business' [with New York State] as the second step, after creating a business plan. The steps after registration with the state include 'Finance Your Business,' 'Find and Plan Your Space,' 'Prepare to Open,' (which includes completing pre-operational government inspections and licensing requirements), then 'Hire a Team,' and 'Open and Operate.' New brick and mortar businesses do not open to customers the day they register with New York State, but several weeks or months later, with strip clubs typically taking months, as we discuss below. A business cannot even obtain an insurance policy in contemplation of opening until after it is registered with New York State. The study therefore cannot defeat conclusive *prima facie* evidence that any short-term association between crime and the date of business registration is spurious, and findings that purport an effect within a week of registration are fatally unreliable. This is a critical methodological issue, the importance of which must be carefully understood and considered.

Most importantly for the study, perhaps, the date of registration is the first opportunity for the business to apply for a license from the New York State Liquor Authority (SLA), the principal regulatory authority of these businesses (Maslin-Nir, 2014; New York State Liquor Authority, 2019; Tracy, 2017). As the primary source of revenue for strip club operators, 'the presence of alcohol is far more important than the absence of pasties,' and the clubs 'can't make any money if they don't have a liquor license' (Maslin-Nir, 2014). This licensing process, which includes an evaluation by the community board concerned, took a minimum of 24 to 26 weeks during the study period. Registering the business with New York State was a prerequisite for it. The source of this information, apart from the professional experience of the authors here (three of whom participated in these processes), is a 3 August 2021 telephone call by the first author to an SLA employee (Lucas, employee ID# 62,950) whose role at the SLA is to inform applicants about the licensing process and guide them through its steps.

To confirm this information, an attorney for the SLA, Stefan M. Armstrong, advised the first author in a telephone call on 22 June 2022 that, as a matter of practice, the authority requires full board approval (rather than a faster temporary permit) for licensing new adult entertainment establishments in New York City, this approval takes approximately 6 months, and registering the entity seeking approval as a business with New York State is a prerequisite for initiating the process. This prerequisite is listed on the SLA's website.[12] Regardless of whether a registered business may open sooner under a temporary liquor license, New York State requires all businesses to provide 30 days' advance notice to the local community board before applying for either kind of liquor license.[13] Based on these problems, the independent time variable is a proxy for a latent time

variable that consistently occurs on an undetermined date several months after it. If we were to replicate this study to suitable standards of rigor, we would exclude the entire dataset for the independent variable.

Assessing data for the dependent variable: sex crimes in New York City[14]

A. Exclusion of reports of encounters with innocent people (94% of the analytic sample)

The study uses New York City Police Department stop, question and frisk (SQF) report data to measure what it asserts are police-observed sex crimes, and uses changes in the frequency of the reports to assert the effect of opening an adult entertainment establishment on these sex crimes. These reports document forcible police stops of people based on less than probable cause, not crimes. Affirmatively referring to the SQF incidents included in the study as 'sex crimes,' which the paper does throughout (see p. 2 and p. 6, for example), is a category error. Over 94% of the analytic sample used in the study records a finding that there was insufficient cause to believe the person stopped had committed a crime (see table for four sample years), and so no arrest was made (arrest (Y/N) and top charge upon arrest are two fields in the SQF database used in the study). In other words, 94% of the reports are records of people who were legally innocent of the crime the police stopped them to investigate. These encounters with innocent people were initiated by police for a variety of reasons, most of them based on the ultimately unfounded suspicion of a crime, but a federal court decision opined that many were made based on inadequate suspicion or no suspicion at all, and targeted racial minorities to the exclusion of white people, which for the purposes of the study would introduce a nonrandom sampling error based on precinct racial demographics. In sum, only approximately 6% of the SQF sample used in the study would qualify for analysis if records of police encounters with innocent people were excluded because they are not, in any way, records of crimes. See Table 1 for an example from four years of the study period.

B. Exclusion of arrests for other than sex crimes and for unknown charges from the remaining sample

Another concern is that when an arrest is made after police stop a person for a suspected sex crime, there is insufficient reason to think it was for a sex crime per se. A person could have been initially stopped for a suspected sex crime, then arrested upon investigation for possession of controlled substances, weapons, an outstanding warrant, resisting governmental administration of the stop, a criminally suspended driver's license, etc. These charges are reflected in a column in the dataset, and initial inspection indicates significant variation between the reason for a stop and the resulting charge upon arrest. In approximately 20% of the records where an arrest was made in the sample years we analyzed, no charge is listed, and an analyst would not be able to determine why a person was arrested at all. If the sample is to contain only records where a stop resulted in an arrest for a sex crime, and not just any arrest for any charge, the study would have to exclude more than 20% of the remaining sample. While we understand the concept and use of proxy variables, in this case, given that over 95% of the time these records of police stops document insufficient cause to believe a crime has been committed, the use of this proxy variable as a measure of the study's dependent variable (i.e., sex crimes) is not appropriate.

Table 1. NYPD stop question and frisk outcomes by arrest and whether a charge was listed for a sample of four years used in Ciacci and Sviatschi (2021).

Year	Total SQF	Arrest made	%	Charge listed	%	% of total SQF
2004	313,524	16,355	5.2%	14,079	86.1%	4.5%
2008	540,303	32,207	6.0%	25,911	80.5%	4.8%
2011	695,725	40,884	5.9%	32,148	78.6%	4.6%
2012	532,913	32,316	6.1%	25,164	77.9%	4.7%

C. SQF reports are intentionally duplicative in ways the study cannot not account for

As records of forcible police stops of people based on less than probable cause, SQF reports are used to document multiple fruitless searches for a single criminal suspect. For example, a pattern sex offender on the loose in a precinct may generate several SQF reports, as officers stop and question different innocent people who match the suspect's description. Or, in the aftermath of a rape, police may stop several people in the immediate vicinity to see if the victim can identify any of them as the perpetrator. Each of these stops generates a separate SQF report (again, of an encounter with an innocent person, plus one of a legitimate suspect if police efforts were ultimately successful), because their purpose is to document *encounters*, not *crimes*. Yet, the study design being critiqued here would count each of these stops *as a separate and independent sex crime*.

D. The study should have excluded 95-96% of its SQF analytic sample

The most important point of these concerns about the dependent variable is that you cannot plausibly use records of innocent people stopped by the police as records of sex crimes, and any results emanating from the use of this data should be withdrawn, or recalculated using on only accurate records of sex crimes. Amending this error requires excluding approximately 94% of the study's analytic sample from the outset. Of the remaining records, approximately 20% do not offer insight into what charge resulted in arrest, and many sex crime stops would have resulted in arrests for an offense other than a sex crime, indicating the person may have been charged with an offense but was legally innocent of a sex crime. The suitable sample is therefore likely 95% smaller than the sample used in the study. This will have a profound effect on the power and results of the study, apart from the concerns above that the principal conclusion, even if supported, will be a spurious association because the study cannot use any of its data for the independent variable.

Other concerns: the baseline data contain substantial omissions

The initial canvass of clubs at the T = 0 shows no clubs in parts of Queens and Brooklyn (Figure A.7) where the authors know there to have been many. For example, Wild Wild West, Corrado's and Sweet Cherry were all clustered in an industrial neighborhood in the 72nd Precinct, and had been in business at the time the study began (Brick, 2006), and Mermaids was in operation in the 114th Precinct. It is very likely that there were other errors. Reporting by the *The New York Times*, for example, discusses a group of clubs that were set to open in 1999 in Queens Plaza, which marks the border between the 108th and 114th Precinct, in addition to several already in operation (Weir, 1999). It was an area that had enough strip clubs a few years prior to generate worries about being called a 'new Times Square.' This is an area in Western Queens that was reported also to have 0 clubs at T = 0. There may have been a half dozen or more, depending on how many of the ones mentioned in reporting remained in business five years later. There were at least two, Scandals and City Scape, listed by name in a 2004 article in the *New York Post* (Calabrese, 2004). We believe, based on recollection as longtime New York City residents, there were several more.

This suggests the data used in the study were based on inaccurate inclusion criteria. That a casual investigation reveals at least eight omissions in three precincts that were reported to have 0 businesses at the beginning of the study suggests the omissions at T = 0 were substantial. Although it is possible that these omissions resulted in an attenuation that strengthened the results of the study by biasing them toward zero, they more importantly cast doubt on the study's ability to properly identify the adult establishments it needed to include throughout its period of analysis. This concern is ancillary, however, to the more important one that nearly all the data used to conduct the study should be excluded based on the category errors their inclusion represents.

Reassessing the null hypothesis

We can reframe the study's principal finding as a null hypothesis: opening a strip club in New York City has no significant effect on sex crimes in the week after it opens. To evaluate it, we would need data on when a strip club opens, and what sex crimes are recorded in the following week. As we have shown, registering a business with New York State has no determinate relationship with the actual opening date of a business, and if it does, it is consistently at some unknown point long after registration. This leads us to reject the entire dataset of business registration dates as an acceptable proxy for the date on which strip clubs open. Yet, the principal finding of the study asserts strong effects within a week of registration and tests for robustness at a month which, conclusions which cannot be drawn given the causal model's reliance on an accurate opening date: 'our results suggest that these effects are driven by potential customers who substitute sex crimes with services provided by adult entertainment businesses' (p. 25). Second, our analysis of the stop question and frisk reports used in the study shows that they document a police encounter with an innocent person 94% of the time, and for an arrest on an unknown charge 1% of the time. Excluding these two categories of data means excluding 95% of the data for the dependent variable.

These exclusions are fatal to the original study, and any attempts at replication. In either case, we are unable to proceed with an analysis using the data provided: we have no data for the independent variable, only 4% of the original data for the dependent variable, and we cannot reject the null hypothesis in support of the study's original findings. More importantly, this strongly suggests the principal conclusion of the original study is unsupported by the data as well. *What the study has done is measure changes in police encounters with innocent people in the week after an entity has filed the paperwork necessary to apply for business licenses that will eventually allow it to open a strip club.* This has no bearing on the study's null hypothesis and cannot be used to test it.

Discussion

If our assessment is true, the data used in the original study cannot support its results. To be clear, we do not take a position that the analytical methods used in the study were inappropriate or incorrect, and we have no cause to believe they were anything but rigorously executed. After extensive conversations with the authors, who were forthcoming and courteous, we are certain there was never an intention to use the data in their study in a way that intentionally drew unsupportable conclusions, which we believe resulted from inadvertent but not unavoidable category errors about what the data represented. Perhaps, the study's underlying assumptions went unexamined because the authors did not benefit from consultation with expert practitioners familiar with the data both during the study and the review process. While it may not be easy for economists to enlist the collaboration of people with expertise in NYPD crime data, or the workings of the New York State Liquor Authority and Department of State, it is far from impossible, and the quality of the study would have greatly benefitted from it. Their intimate knowledge of their subject matter will make for more robust, reproducible results. There are resources that researchers can use in this regard. For example, the National Institute of Justice administers the Law Enforcement Advancing Data and Science program, which has designated nearly 100 police practitioners nationwide as scholars committed to partnering with researchers to advance the accurate and reliable production of evidence in policing. Some would have been presumably amenable to assisting in this case and as well as others. Our goal is therefore not to impugn one particular study, but to make a case for increased collaboration between researchers and the stakeholders who create and maintain custody over the administrative data used in social science studies.

Limitations

This analysis has limitations. We do not know the distribution of the cases for the independent variable among the data sources consulted, so we cannot parse them in greater detail, but we do assert that all of the sources used preclude inclusion in the study if their registration date is used as a proxy for their opening date. We also do not know what the precise results would have been if the authors used nothing but complaint report data for the dependent variable, only that it was a secondary dataset used for a robustness check of their primary one, and they found the datasets were correlated and both showed a reduction, though it was slower and of smaller magnitude for the complaint reports. Regardless, the results cannot hold if the entire dataset for the independent variable is excluded, and the same underlying events drive both stop and frisk reports and a substantial number of complaints closed by police to an arrest. Finally, as mentioned above, out of a need for economy our analyses of stop reports was conducted in the aggregate, and for four years of the study, rather than for only sex crimes over the study's eight-year period of analysis. There is no cause to believe, however, that changing either parameter would affect the finding that almost none of the data is a record of an alleged crime.

Possible next steps

The study in question should be retracted, and media outlets that presented its findings as reliable knowledge should be notified. At the very minimum, the journal publishing the study should put a notice of concern on the abstract page and in a revised PDF that there is serious doubt as to the findings, which are unreliable. An example of a suitable response is the retraction of a meta-analysis of the effects of supervised drug injection sites by the *International Journal of Drug Policy* for the incorrect pooling of heterogeneous results that were otherwise correctly calculated (May et al., 2018). The conclusions could not be presented as reliable knowledge, so the editors felt it was important to withdraw the paper from the scholarly record.

Conclusion

The effect of strip clubs on sex crimes is an important one that deserves close study. The idea that they offer an outlet for behaviors that would otherwise be exercised criminally is interesting, and understanding the merits of the argument is important from a public safety perspective, especially given the vulnerability of women, already a marginalized group in many ways, to sex crimes. Still, if the authors found an association between registering a business with New York State and a reduction in crime, it cannot be used to conclude that the business caused the reduction within a week of its debut because a registration date is an invalid proxy of when a business opens. Likewise, highly subjective police encounters with innocent people, indeed ones that were judicially ruled as deriving from an unconstitutional law enforcement practice because of the overwhelming innocence of the people stopped, cannot be used to measure a city's crime rate. However, the category errors that arose from the original study were avoidable. Practitioners well-versed in the topic under study could have provided insight into what the data can and cannot speak to, what it indicates, and what it can be used to analyze. We encourage others with the relevant expertise and an interest in this topic to reexamine both the original study and our concerns here, so they can continue research in this vein at the proper levels of rigor.

Notes

1. The Patrol Services Bureau of the New York City Police Department divides the city into 77 precincts (see p. 2 of Ciacci and Sviatschi (2021)), with a mean population of 110,000 residents (i.e., New York City's 2021 population of 8.46 million divided by 77). The study asserts that the opening of one adult entertainment establishment has a marginal effect of significantly reducing sex crimes among this mean population within a week.
2. In the references to Ciacci and Sviatschi (2021) here, we provide page numbers for the PDF of the paper published in its final form in *The Economic Journal*.

3. https://www.superpages.com/search?search_terms=strip+club&geo_location_terms=New+York%2C+NY
4. https://www.superpages.com/search?search_terms=escort&geo_location_terms= New+York+City%2C+NY
5. https://www.superpages.com/search?search_terms=Gentlemen%27s+Club&geo_location_terms=New+York%2C+NY
6. https://www.yellowpages.com/search?search_terms=strip+club&geo_location_terms=New+York+City%2C+NY
7. https://www.yellowpages.com/search?search_terms=Gentlemen%27s+Club&geo_location_terms=New+York%2C+NY
8. https://www.yellowpages.com/search?search_terms=Escort+Service&geo_location_terms=New+York+City%2C+NY
9. See https://www.eros.com/new_york/new_york/eros.htm for one of many examples of services that operate citywide, outside the realm of this study, but bear heavily on its proposed causal model. **Warning**: sexually explicit content.
10. https://www1.nyc.gov/nycbusiness/startabusiness/bar
11. https://www1.nyc.gov/nycbusiness/startabusiness
12. https://www.businessexpress.ny.gov/app/answers/cms/a_id/3737/kw/on%20premises
13. https://sla.ny.gov/municipalcommunity-board-30-day-advance-notices-premises-applicants
14. To present this problem in an economical way, we have performed this analysis using the entire body of stop question and frisk reports, but we presume the percentages and trends discussed closely match the subcategory of sex crimes defined by the study; to our knowledge, no evidence exists that they do not. The stop, question and frisk data used in our analysis can be found here: https://www1.nyc.gov/site/nypd/stats/reports-analysis/stopfrisk.page. Mr. Donohue, Mr. Hall, and Dr. del Pozo were custodians and analysts of the NYPD's SQF data during the period of the study.

Disclosure statement

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