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Examining the Collateral Consequences of Proactive Policing Strategies: A Case Study of the L.A.P.D.'s Operation L.A.S.E.R.

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Apolinar, Cristian

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Examining the Collateral Consequences of Proactive Policing Strategies:
A Case Study of the L.A.P.D.'s Operation L.A.S.E.R.

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Criminology, Law & Society

by

Cristian Apolinar

Dissertation Committee:
Professor Emily Owens, Chair
Professor Amanda Geller
Professor George Tita
Professor Ojmarrh Mitchell

2026

DEDICATION

To

This dissertation is dedicated to my parents
for their unconditional love, support, and encouragement.
-
Esta tesis doctoral está dedicada a mis padres
por su amor, apoyo y aliento incondicionales.

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Disclaimer:

The views expressed in this dissertation are my own and may not reflect those of the individuals or institutions acknowledged above.

VITA

Cristian Apolinar

2017	A.A. in Social and Behavioral Sciences, Ventura Community College
2017	A.S. in Administration of Justice, Ventura Community College
2017	A.S. in Criminal Justice, Ventura Community College
2018	A.A. in Sociology, Ventura Community College
2019	B.A. in Criminology & Justice Studies, California State University, Northridge
2023	M.A. in Social Ecology, University of California, Irvine
2026	Ph.D. in Criminology, Law & Society, University of California, Irvine

FIELD OF STUDY

Criminology, Law & Society

ABSTRACT OF THE DISSERTATION

Examining the Collateral Consequences of Proactive Policing Strategies:
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by

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Doctor of Philosophy in Criminology, Law & Society

University of California, Irvine, 2026

Professor Emily Owens, Chair

The present dissertation examines the collateral consequences of proactive policing strategies, using the Los Angeles Police Department's Operation L.A.S.E.R. as a case study. Rather than evaluating the program solely as a crime-control strategy, this dissertation asks how the intervention affected crime, educational outcomes, and neighborhood desirability in Los Angeles. The introductory chapter traces its development, expansion, criticism, and termination. Chapter 1 evaluates whether the 2015 expansion reduced crime without displacement into adjacent, non-treated census blocks. Division-, zone-, and block-level analyses find mixed evidence of crime reduction and little evidence of displacement. Chapter 2 examines whether exposure to Operation L.A.S.E.R. was associated with changes in assessment scores among Los Angeles Unified School District high school students. Active exposure was associated with lower assessment scores in nearly every all-student and student-subgroup analysis, though no association was statistically significant at conventional levels. Lastly, Chapter 3 examines neighborhood desirability through single-family home sale prices. Treatment was concentrated in Hispanic-majority, renter-heavy neighborhoods and associated with higher logged sale prices. Pooled officer-presence estimates were positive, though not statistically significant at conventional levels, and higher sale prices did not coincide with rapid demographic turnover.

INTRODUCTION

Research shows that proactive policing strategies can produce short-term, localized crime reductions, though their effectiveness varies. However, less is known about their long-term effects and community consequences (National Academies of Sciences, Engineering, and Medicine, 2018). The present chapter introduces readers to the Los Angeles Police Department (L.A.P.D.)'s Operation L.A.S.E.R., covering the program's policy history, its potential to suppress crime, and its risk of collateral harm to affected communities. The intervention began as an effort to reduce violent crime in selected hot spot corridors, formally known as L.A.S.E.R. Zones, through evidence-based, data-driven policing. Early presentations and evaluations described the program as a successful response to violent crime (Gamero & Swatt, 2013; Newton Division & Justice & Security Strategies, Inc. [J.S.S.], 2012; Uchida & Swatt, 2013; Uchida et al., 2012a, 2012b); however, affected residents, civil rights advocates, and journalists later associated the intervention with excessive surveillance, neighborhood change, racial disparities in policing, and the power of a police department to label people and places as high-risk (Bhuiyan, 2021; Lapowsky, 2018; Puente, 2019; Smith, 2019).

Thus, a full understanding of Operation L.A.S.E.R. requires attention to both parts of that history: the reasons officials supported the program and the reasons community members rejected it. The present chapter discusses the program's development, expansion, criticism, and termination to explain why Operation L.A.S.E.R. serves as a useful case for evaluating proactive, data-driven policing programs.

Origins of Operation L.A.S.E.R.

In 2009, the Bureau of Justice Assistance (B.J.A.) launched the Smart Policing Initiative (S.P.I.) to encourage collaboration between law enforcement and researchers (Braga et al., 2014;

Bureau of Justice Assistance [B.J.A.], 2013). As part of the grant-funded program, S.P.I. teams, consisting of local law enforcement agencies and their research collaborators, were selected throughout the United States to address crime using innovative, data-driven, evidence-based strategies (B.J.A., 2013; Braga et al., 2014). In Los Angeles, the S.P.I. initiative united the L.A.P.D. with its research partner, Justice & Security Strategies, Inc. (J.S.S.), forming the local S.P.I. team responsible for developing, implementing, and assessing an innovative police intervention (City Administrative Officer, 2010; Uchida et al., 2012a, 2012b). This intervention would soon become known as Operation L.A.S.E.R., or the Los Angeles Strategic Extraction and Restoration program.

During an S.P.I. webinar in 2017, L.A.P.D. Deputy Chief Dennis Kato and J.S.S. President and Founder Dr. Craig D. Uchida explained that the Los Angeles S.P.I. team developed the L.A.S.E.R. acronym “around 2010 or 2011.” According to their recollection, the captain in the Newton Division had mentioned during the planning stage that he did not want to saturate neighborhoods with large numbers of officers or to repeat the enforcement tactics used by the L.A.P.D. in the 1980s and 1990s. Instead, he wanted officers to enter neighborhoods with a more focused approach and 1) identify whom the department should target, 2) determine which types of crime required attention, and 3) identify where the department should direct its resources. Thus, the Los Angeles S.P.I. team selected the acronym, given the department’s intention to use “L.A.S.E.R.-like, non-invasive techniques to rid areas of crime and criminals” (Smart Policing Initiative, 2019, 34:00-50:30). By 2011, the Los Angeles S.P.I. team’s focus was to address gang- and gun-related[1] crime using a combination of state-of-the-art technology and the S.A.R.A. (Scanning, Analysis, Response, and Assessment) model.

The S.A.R.A. model provided Operation L.A.S.E.R. with a problem-solving framework to avoid using the outdated, reactive policing strategies that the captain in the Newton Division was concerned about. The model grew out of American criminologist Herman Goldstein's work on problem-oriented policing, which criticized older, reactive policing tactics for treating calls for service, arrests, and response times as measures of success rather than as clues identifying and addressing the recurring problems that drive crime and disorder in the first place (Goldstein, 1979, 1987). Eck and Spelman (1987) later adapted this approach into the S.A.R.A. model, which organized police problem-solving through scanning, analysis, response, and assessment.

The official L.A.P.D. website describes the department's take on the S.A.R.A. model as a useful approach for examining the characteristics of community problems and then developing the necessary strategies to reduce those community-identified problems that influence crime and disorder (Los Angeles Police Department [L.A.P.D.], n.d.). Operation L.A.S.E.R. adopted this framework by framing the program as a targeted, data-driven response to repeated violence in specific places and among specific people. A meta-analysis conducted by Weisburd et al. (2008) found that problem-oriented policing strategies based on the S.A.R.A. model can reduce crime and disorder; however, the effects are generally limited and depend on the nature of the problem and on how law enforcement responds.

In collaboration with J.S.S., the L.A.P.D. applied this problem-solving approach by focusing first on gun violence and gang activity in Los Angeles. During the planning phase of Operation L.A.S.E.R., the pair concluded that the problem they wanted to address was gang- and gun-related crime. Specifically, the Los Angeles S.P.I. team noted that although overall crime had declined, gun-related crimes and gang activity continued to impact the city (Uchida et al., 2012a, 2012b). Thus, the Los Angeles S.P.I. team analyzed crime data from January 2006 to

2011 and used Geographic Information System (G.I.S.) mapping software to identify areas within the L.A.P.D.'s jurisdiction where officials believed a new targeted police intervention was most warranted (Uchida et al., 2012a, 2012b).

A statistical analysis of violent crime data showed that the Newton Division consistently ranked among the top three divisions with the highest number of gun-related crimes across the 21 L.A.P.D. divisions (Uchida et al., 2012a, 2012b). As a result, the Los Angeles S.P.I. team selected the Newton Division as the pilot site for reducing gun and gang-related crimes. With the assistance of G.I.S. mapping software, the Los Angeles S.P.I. team identified five crime hot spots within the Newton Division and used those hot spots as visual markers to create five L.A.S.E.R. Zones for the pilot program. Four zones were unique to Operation L.A.S.E.R., while the fifth zone was carried over from the 1995 Community Law Enforcement and Recovery (C.L.E.A.R.) police intervention (Uchida et al., 2012a, 2012b).

The Newton Pilot's Approach to Crime Reduction

In September 2011, the Los Angeles S.P.I. team officially launched the Operation L.A.S.E.R. pilot program in the Newton Division. During this period, the L.A.P.D. increased patrols across the five designated L.A.S.E.R. Zones. To measure L.A.S.E.R. activity, the Los Angeles S.P.I. team tracked intervention dosage in two ways: 1) documenting additional minutes spent in L.A.S.E.R. Zones per month and 2) tracking crime counts by month and by four-week L.A.P.D. reporting intervals, formally known as Deployment Periods (D.P.) (Newton Division & Justice & Security Strategies, Inc. [J.S.S.], 2012).

Rather than creating a new L.A.S.E.R.-specific patrol unit, the Newton Division deployed existing patrol and specialized units across the five L.A.S.E.R. Zones. These units included vehicle patrols, Watches 2, 3, and 5, bike and foot patrols, and the Parole Compliance Unit

(Newton Division & J.S.S., 2012). Over one year, these units logged an average of 13,326 additional minutes, or 222 additional hours per D.P., across the five L.A.S.E.R. Zones. To support the program's intelligence-gathering and prepare for future data analysis, the Los Angeles S.P.I. team established a specialized unit called the Crime Intelligence Detail (C.I.D.), consisting of two police officers and one intelligence analyst (Newton Division & J.S.S., 2012). Unlike their counterparts who focused on patrol, surveillance, and data collection, the specialized team compiled information from patrolling officers to identify individuals the department believed were responsible for repeated violent crime and the locations associated with recurring criminal activity. C.I.D. records included field interview (F.I.) cards, arrest information, and criminal background information. Through this process, the C.I.D. developed Chronic Offender Bulletins with the assistance of Palantir[2] software (Newton Division & J.S.S., 2012).

The bulletins included, but were not limited to, information on suspects' gang affiliation, physical characteristics, criminal background, parole or probation status, vehicles driven or owned, frequented areas, and previous contact with law enforcement (Gamero & Swatt, 2013; Newton Division & J.S.S., 2012). After completing the bulletins, the C.I.D. would distribute the files to patrol officers, specialized units, detectives, and command staff to inform personnel of the most recently known individuals labeled as chronic offenders and the locations requiring additional patrol. During this same period, Palantir developed Automatic License Plate Reader (A.L.P.R.) software, which enabled officers to promptly identify nearby individuals classified as chronic offenders, thus making monitoring easier for law enforcement (Gamero & Swatt, 2013).

Early Findings from the Newton Pilot

Following the first year of implementation, the Los Angeles S.P.I. team presented its early findings through the B.J.A.'s national S.P.I. network, where participating sites shared updates on their approach to reducing crime and preliminary results. During this presentation, the Los Angeles S.P.I. team described the Newton pilot as evidence that Operation L.A.S.E.R. was associated with reductions in violent crime (Newton Division & J.S.S., 2012). The pair reported that the Newton Division identified and rank-ordered 124 chronic offenders between August 2011 and January 2012, and by June 2012, 87 of those individuals, or 70 percent, had been arrested at least once. Upon presenting these findings, the Los Angeles S.P.I. team stated that the department would continue using C.I.D. teams, maintain increased patrol and dosage monitoring in L.A.S.E.R. Zones, keep targeting chronic offenders within those zones, and extend L.A.S.E.R. in the Newton Division to property crimes, with particular attention to vehicle burglaries (Newton Division & J.S.S., 2012).

In May 2013, L.A.P.D. Officer Dave Gamero and Dr. Marc Swatt of J.S.S. presented a more detailed summary of the Newton pilot's preliminary results. They reported that Newton ended 2012 with 16 homicides, a 56 percent decrease from 2011 and a 59 percent decrease from 2010. The pair also reported that overall violent crime declined by 19 percent from 2011 to 2012 and that Newton ranked first among L.A.P.D. divisions in violent-crime reduction for 2012 (Gamero & Swatt, 2013). Similar to the 2012 presentation, the 2013 version cited the Newton results to support the program's expansion. After reporting that Part I violent crimes declined by an additional 5.393 crimes per month after implementation, homicides by an additional 22.6 percent per month, and robberies by an additional 0.218 crimes per month, the pair reported that the L.A.P.D. had agreed to continue L.A.S.E.R. in Newton for violent and property crime and to expand the program to four additional divisions. The proposed expansion was set to focus on violent crime in Hollenbeck and Southwest and on property crime in Foothill and Wilshire (Gamero & Swatt, 2013).

Later in 2013, Uchida and Swatt published a formal panel analysis of Operation L.A.S.E.R. in Police Quarterly. The J.S.S. article found that gun crime declined by an additional 5.2 percent per month across the Newton Division after implementation. When they separated reporting districts by treatment type, the decline was concentrated among those that received both the chronic-offender and chronic-location components. In those areas, gun crime declined by an additional 7.2 percent per month after the intervention. In essence, the published article reiterated the earlier findings from S.P.I. presentations and provided a more formal evaluation of the Newton findings (Uchida & Swatt, 2013).

Community Concerns

Following the initial pilot in the Newton Division, Operation L.A.S.E.R. grew from a small-scale, data-driven police intervention into a larger L.A.P.D. initiative spanning numerous divisions (Uchida et al., 2018). In 2015, the L.A.P.D. began its first expansion, covering Newton, 77th Street, Southeast, and Southwest. During this period, however, public attention shifted from whether L.A.S.E.R. reduced crime to how it affected communities already experiencing high levels of police contact. Community members, scholars, civil rights groups, defense attorneys, and city officials argued that L.A.S.E.R. disproportionately targeted Black and Latino[3] neighborhoods and granted the L.A.P.D. substantial discretion to label people and places as high risk (Bhuiyan, 2021; Chambers & Ball, 2018; Lapowsky, 2018; Police Commission, 2019; Puente, 2019; Spolin, 2018). See Appendix A for a map of Operation L.A.S.E.R.'s expansion timeline.

Within the dedicated L.A.S.E.R. Zones, increased patrols left many residents feeling surveilled, even when the L.A.P.D. did not formally classify these individuals as chronic offenders. Although the program targeted specific people and places to ensure L.A.S.E.R.-related activity was non-invasive, the increase in patrols translated to heightened surveillance levels of everyone within the zones, including people who lived, worked, or passed through those areas (Lapowsky, 2018; Smith, 2019). The program's use of Palantir software contributed to those fears. Because L.A.S.E.R. heavily focused on data collection and Palantir software made it easier for law enforcement to compile information obtained through field interview cards, police contacts, and chronic offender bulletins into single reports, many individuals who had little or no prior contact with law enforcement worried that these records could connect them to targeted individuals, vehicles, addresses, or locations (Lapowsky, 2018; Smith, 2019).

The L.A.P.D.'s use of chronic offender labels created a similar but distinct concern. Bhuiyan (2021) reports that Operation L.A.S.E.R. used gun-related crime data, arrests, calls for service, field interview cards, gang affiliation, probation status, and other police records to identify problem areas and assign risk scores to individuals. Similar accounts described L.A.S.E.R. as a Palantir-supported system that used prior police and criminal records to score individuals and place those with higher scores on Chronic Offender Bulletins (Chambers & Ball, 2018; Lapowsky, 2018; Smith, 2019). According to critics, the problem with labeling individuals as high risk in a database is that the process could create a feedback loop, as the information collected through police contact could be fed back into the risk-scoring process to justify continued surveillance of the same people (Bhuiyan, 2021; Brayne, 2021; Lapowsky, 2018).

Relatedly, some individuals whom the L.A.P.D. classified as chronic offenders argued that the Chronic Offender Bulletins led officers to surveil them from the moment they stepped out of their homes, hoping to witness a crime or infraction (Chambers & Ball, 2018; Lapowsky, 2018). From these individuals' perspective, the label made it difficult to move forward in life without further contact with the police.

Other individuals that the L.A.P.D. classified as chronic offenders felt threatened by the department's tactics, including receiving unsolicited calls, letters, and home visits. In some cases, the department sent letters by mail or had specialized units, including gang and narcotics units, visit people's homes to inform or remind these individuals that the agency was aware of their presence and activities and to request that they refrain from future offenses (L.A.P.D., 2017; Smith, 2019). Records released to the public in 2018 reveal that L.A.P.D. officials instructed officers to document each warning in a departmental database (L.A.P.D., 2017; Smith, 2019). Although internal documents released to the public indicate that chronic offenders were to be regarded as persons of interest rather than suspects (L.A.P.D., 2017; Smith, 2019), numerous individuals designated as such felt that, in practice, the opposite occurred (Chambers & Ball, 2018; Lapowsky, 2018).

Research on procedural justice helps explain why many members of the public perceived Operation L.A.S.E.R. as harmful, even though the Los Angeles S.P.I. team described the program as non-intrusive. According to procedural justice theory, public trust in law enforcement depends not only on whether police reduce crime but also on whether people experience police encounters as fair, respectful, and transparent (Hough et al., 2010; Peyton et al., 2019). At the same time, distrust does not necessarily mean that residents reject police protection. Campeau et al. (2021) found that recently arrested residents could remain deeply skeptical of police practices while still viewing effective police protection as necessary for safety and recognition. In Los Angeles, limited transparency, racial and ethnic disparities in police interactions, and the program's reliance on records from earlier police encounters ultimately influenced the public's perception of L.A.S.E.R. (Bhuiyan, 2021; Lapowsky, 2018; Police Commission, 2019; Smith, 2019; Spolin, 2018).

Termination of Operation L.A.S.E.R.

Public opposition to L.A.S.E.R. intensified as community groups sought more information about how the program selected chronic offenders, where the program operated, and how the L.A.P.D. evaluated its effects. The opposition eventually led to litigation, public pressure, an official audit, and the program's termination in April 2019. Organized opposition began as early as May 2017, when the Stop L.A.P.D. Spying Coalition filed a California Public Records Act (P.R.A.) request seeking information on the chronic offender selection process, participating divisions, implementation, funding, and evaluation of Operation L.A.S.E.R. (Stop L.A.P.D. Spying Coalition, 2019). After the L.A.P.D. failed to respond for nearly a year, the group filed a lawsuit against the department in February 2018 and later received information about grants, hot spots, and studies related to the development and implementation of L.A.S.E.R. (Stop L.A.P.D. Spying Coalition, 2019).

In July and August 2018, privacy rights activists gathered at Board of Police Commissioners' (B.O.P.C.) meetings to demand a thorough investigation into Operation L.A.S.E.R. (Macias, 2019; Stop L.A.P.D. Spying Coalition, 2019). These demands led the B.O.P.C. to order an audit of the L.A.P.D.'s data-driven policing programs, including Operation L.A.S.E.R. and PredPol[4]. The 48-page audit from the Inspector General of the Police Commission, sent to the Honorable Board of Police Commissioners on March 8th, 2019, identified several problems in the department's handling of Operation L.A.S.E.R. The audit found that many of the L.A.P.D.'s data-driven policing strategies lacked departmental oversight. As a result, officers across divisions used inconsistent criteria to label suspects as chronic offenders (Bhuiyan, 2021; Puente, 2019; Smith, 2019; Stop L.A.P.D. Spying Coalition, 2019).

On April 9th, 2019, L.A.P.D. officials met with the civilian oversight panel during a Police Commission meeting. During the meeting, panel members questioned the efficacy of data-driven police interventions that relied on computer programs and historical police data. Members of the public also questioned the program's transparency, racial bias, and effects on communities already subject to frequent police contact (see Appendix B for a detailed recount of community concerns). In response, L.A.P.D. Chief Michael Moore defended the program and argued that proactive policing strategies such as Operation L.A.S.E.R. "have led to the lowest crime rates in years" and that "the algorithms do not use race or gender as identifiers" (Puente, 2019).

Despite Chief Moore's defense of the program, the audit found that the L.A.P.D. lacked sufficient data to accurately measure the program's success. The audit also noted that updated records from Operation L.A.S.E.R.'s Chronic Offender Program lacked crucial information. The Office of the Inspector General (O.I.G.) examined 2,250 of the most recent entries concerning chronic offenders at the time. Of those entries, the O.I.G. found that 34 percent of the most recent arrest entries specifically mentioned chronic offenders; however, the records did not specify whether the arrests were officially L.A.S.E.R.-related or whether the L.A.P.D. used the term "chronic offender" as a generic descriptor through traditional policing methods.

In addition, the department's own records supported one of the public's primary concerns. Activity that the L.A.P.D. identified as Operation L.A.S.E.R.-related subjected racial and ethnic minorities to police contact at much higher rates than other racial or ethnic groups in Los Angeles. Specifically, the audit found that nearly 80 percent of individuals labeled as active chronic offenders were Black or Hispanic males (Macias, 2019; Smith, 2019). Following the O.I.G.'s audit and the public outcry from the April 9th meeting, Chief Moore announced the termination of Operation L.A.S.E.R. on April 10th, 2019 (Bhuiyan, 2021; Puente, 2019).

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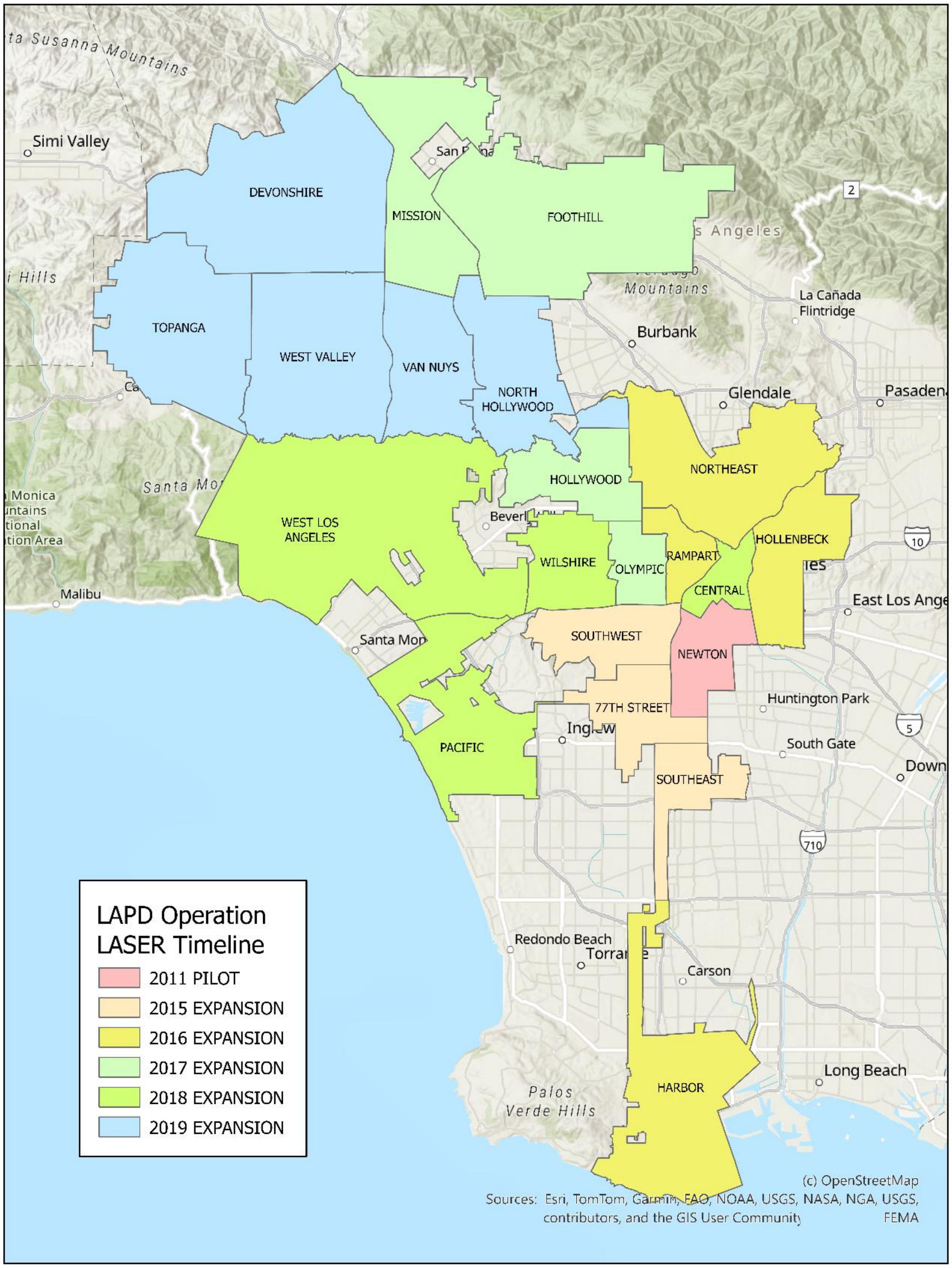
FOOTNOTES

1. The Los Angeles S.P.I. team defined gun-related crimes as misdemeanors with deadly weapons, aggravated assaults, robberies, drive-by shootings, and homicides (Newton Division & J.S.S., 2012).
2. Palantir is a data analytics company specializing in developing intelligence software for law enforcement agencies throughout the United States.
3. The terms "Hispanic" and "Latino" are used interchangeably in the present chapter to match the terminology used by references and data sources.
4. PredPol was predictive-policing software used by the L.A.P.D. and other agencies to identify crime hot spots and identify locations where future crimes were likely to occur.

APPENDIX A

Appendix Figure A1. L.A.P.D. Operation L.A.S.E.R. Expansion Timeline



APPENDIX B

Public Criticisms from the April 9th, 2019 Police Commission Meeting

During the April 9th, 2019, Los Angeles Police Commission meeting, members of the public criticized the L.A.P.D.'s data-driven policing programs, including Operation L.A.S.E.R. and PredPol. Some arguments were grounded in research, while others drew on personal experiences, observations, or political perspectives. During several Public Comment sessions, speakers connected these programs to concerns about transparency, neighborhood change, and racial bias. One critique was that the L.A.P.D. developed and implemented these programs without public input and did not comply with Public Records Act (P.R.A.) requests until the Stop L.A.P.D. Spying Coalition filed a lawsuit. Thus, speakers criticized what they viewed as a lack of transparency and total control over the programs' designs and evaluations.

During these sessions, several speakers argued that the department repeatedly presented itself as transparent while withholding information about L.A.S.E.R. and PredPol, including which people and places were targeted, what the selection criteria were, and who determined the success of these programs. The department later released much of this information after litigation concluded. One member of the public who expressed concerns about a lack of transparency took the podium and directly accused L.A.P.D. officials of withholding information not only from the public but also from the Police Commission. One related concern was a conflict of interest: the same people who helped create L.A.S.E.R. and PredPol were responsible for evaluating the success of these programs (Police Commission, 2019).

Additionally, many members of the public associated L.A.S.E.R. and PredPol with over-policing and described these programs as aggressive, suppressive, and harmful to families and minority communities. Several speakers argued that predictive policing strategies might appear neutral on the surface but often ignored the demographics of the people who lived, worked, and raised families in affected areas. One speaker argued that concentrating officers in “hot spots” or “micro hot spots” failed to address racial bias if the selected areas for intervention remained primarily within Black and Brown neighborhoods. Thus, such strategies would still lead to racial profiling, criminalization, and violence against minorities (Police Commission, 2019).

A related concern raised during the meeting was the fear of gentrification. Multiple speakers expressed concern that the L.A.P.D. was influencing neighborhood change through over-surveillance, criminalization, and the removal of longtime Black and Hispanic residents, as well as by making neighborhoods more attractive to wealthier, White residents and driving up renter costs. One speaker noted that the L.A.P.D. was essentially trying to turn Venice into Malibu and Downtown Los Angeles and Skid Row into San Francisco. Moreover, this individual claimed that these policing efforts were not aimed at reducing crime but at displacing the people already living in those neighborhoods (Police Commission, 2019).

Finally, community members argued that L.A.S.E.R. and PredPol could not eliminate racial bias because these programs relied on biased crime data from the start. Specifically, these individuals argued that the L.A.P.D. had historically tracked crime in marginalized communities while paying less attention to crime elsewhere in the city. In their view, this repeated patrolling led to more stops and reports in minority areas. Because these programs relied on historical crime data, areas that had already experienced heavy policing appeared more crime-prone in the records. The L.A.P.D. then fed those records into algorithms that labeled these areas as high-risk, prompting law enforcement to return and creating a cycle of surveillance and criminalization. One community member summarized this concern by stating that these programs did not predict crime patterns, but rather predicted policing patterns (Police Commission, 2019).

CHAPTER 1

An Independent Evaluation of the L.A.P.D.'s Operation L.A.S.E.R.:

Impacts on Crime and Displacement

ABSTRACT

Operation L.A.S.E.R. was one of the Los Angeles Police Department (L.A.P.D.)'s most prominent, data-driven policing programs during the 2010s. The present chapter reevaluates Justice & Security Strategies, Inc. (J.S.S.)'s 2016 evaluation of the 2015 expansion and asks two main research questions. First, was L.A.S.E.R. associated with reductions in crime within treated locations? Second, were any reductions in crime accompanied by displacement into adjacent, non-treated blocks? The present chapter answers these questions using three approaches. First, the present chapter revisits the 2016 evaluation's division-level analysis of the 77th Street, Newton, and Southwest Divisions. Second, the chapter reassesses the 2016 report's L.A.S.E.R. Zone-level analysis, which examines changes in crime within the hot spot corridors, officially known as L.A.S.E.R. Zones. Finally, this chapter adds a supplemental block-level analysis to compare treated census blocks with adjacent, non-treated blocks over equal pre- and post-implementation time periods. Overall, the present chapter finds mixed results. The most compelling evidence of crime reduction comes from the division-level trend estimates, particularly in the 77th Street and Newton Divisions. At the L.A.S.E.R. Zone level, crime decreased in 43 cases, increased in 40, and remained unchanged in two. Meanwhile, the block-level analyses reveal that treated blocks did not experience meaningful reductions in crime relative to adjacent, non-treated blocks, and that adjacent, non-treated blocks did not show statistically significant increases in crime that would indicate crime displacement had occurred.

Keywords: proactive policing; problem-oriented policing; data-driven policing; displacement

INTRODUCTION

Operation L.A.S.E.R., or the Los Angeles Strategic Extraction and Restoration program, was among the Los Angeles Police Department (L.A.P.D.)'s most prominent, data-driven violence-reduction programs throughout the 2010s. The previous chapter discussed the program's development from the Newton Division pilot to a larger, departmentwide intervention that later drew criticism over a lack of transparency, excessive surveillance, neighborhood change, racial disparities in policing, and the department's extensive discretion to classify people and places as "high risk." Chapter 1 transitions from policy history to policy evaluation by independently assessing Justice & Security Strategies, Inc. (J.S.S.)'s 2016 evaluation of Operation L.A.S.E.R.

Following the 2011 pilot program in the Newton Division, the L.A.P.D. and J.S.S. produced a series of presentations, reports, and evaluations advocating for the expansion of Operation L.A.S.E.R. beyond the Newton Division. In 2015, the L.A.P.D. officially expanded the intervention to include the 77th Street, Southeast, and Southwest Divisions. Soon after, Swatt and Uchida (2016) evaluated the 2015 expansion and reported that L.A.S.E.R. was associated with reductions in several crime categories across the expanded divisions. At the time, these findings were a significant contribution to research on data-driven policing, as the report suggested that a single intervention could reduce crime across multiple divisions in a major city.

However, one major public concern at the time was that the people evaluating the program had also helped develop it, which cast doubt in many people's minds about the program's true effects on communities and crime (Police Commission, 2019; Smith, 2019; Stop L.A.P.D. Spying Coalition, 2021). In addition, Brayne (2021) reports that concerns about opacity, civil rights, accountability, and uncertain effectiveness contributed to demands for an independent review of the L.A.P.D.'s data-driven policing programs. Therefore, the present chapter revisits and reassesses the 2016 evaluation and asks two main research questions. First, was L.A.S.E.R. associated with reductions in crime in treated places? Second, did any observed reductions coincide with increases in adjacent, non-treated blocks, consistent with crime displacement?

The present chapter answers the two research questions through three separate analyses. The first approach is to replicate the 2016 evaluation's division-level analysis to examine whether crime decreased across treated L.A.P.D. divisions after the department implemented the program. The second approach replicates the 2016 evaluation's zone-level analysis to examine whether crime patterns changed within hot spot corridors, formally known as L.A.S.E.R. Zones. Lastly, the present chapter adds a supplemental block-level analysis to compare treated census blocks with adjacent, non-treated blocks over equal pre- and post-implementation periods. Thus, the present chapter transitions from a division-level analysis to a zone-level analysis and finally to a block-level analysis.

Justice & Security Strategies, Inc.'s 2016 Evaluation

To determine which L.A.P.D. divisions to target for the 2015 expansion of Operation L.A.S.E.R., the L.A.P.D. and J.S.S. used Geographic Information System (G.I.S.) software to analyze historic violent crime data from 2006 to 2014, and to examine more recent crime trends from 2012 to 2014. In doing so, the L.A.P.D. and J.S.S. determined that 77th Street, Newton, Southeast, and Southwest would benefit most from an expansion of L.A.S.E.R., as they consistently had the highest number of violent crimes across multiple analyses. In addition to selecting the intervention divisions, the pair identified four to six crime hot spots per division where violent and gun-related crimes were concentrated (Swatt & Uchida, 2016; Uchida et al., 2018). In total, the selected zones covered 3.05 square miles, or about 0.65 percent of the city of Los Angeles. Patrol officers were deployed to the zones each week, while Crime Intelligence Details identified chronic violent offenders within each division (Swatt & Uchida, 2016).

As previously mentioned, J.S.S. relied on two approaches to measure the impact of L.A.S.E.R. on crime. At the division level, J.S.S. used interrupted time-series models to determine whether crime patterns changed after L.A.S.E.R. began. The outcomes of interest were Part 1 crimes, violent Part 1 crimes, gun crime, robbery, and aggravated assault. For the L.A.S.E.R. Zone-level analysis, J.S.S. compared average weekly crime counts before and after L.A.S.E.R. implementation for each L.A.S.E.R. Zone. Specifically, the analysis compared average crime levels across six four-week L.A.P.D. reporting periods, formally known as Deployment Periods or D.P.s, before implementation and nine after implementation began. To account for differences in zone size, the pre-post difference was divided by each zone's square-mile area (Swatt & Uchida, 2016).

Overall, J.S.S.'s findings were mixed. At the division level, J.S.S. found statistically significant post-L.A.S.E.R. trend reductions for several outcomes in every division except for Southwest. At the zone level, some L.A.S.E.R. Zones showed decreases in one or more crime categories, while others showed increases or very little improvement after the implementation of L.A.S.E.R. Ultimately, the report presented L.A.S.E.R. as promising, but it also acknowledged that gains were not constant across all L.A.S.E.R. Zones and that some zones remained areas of concern (Swatt & Uchida, 2016).

DATA

Data Sources and Analytic File Construction

To follow the 2016 evaluation's division- and zone-level designs and to provide a supplemental block-level analysis, the present chapter relies on publicly available L.A.P.D. incident-level crime data from the City of Los Angeles Open Data Portal. Because the crime records included a date-of-occurrence field but lacked weekly or D.P. indicators, the crime data were merged with a custom time-series dataset using the public crime dataset's incident date-of-occurrence field and the time-series dataset's daily date field.

By merging incident-level crime data with a custom time-series dataset, daily, weekly, monthly, yearly, and D.P. indicators were added to each incident record. The merged records were then organized into three working files. The first working file supports the division-level analysis, the second supports the L.A.S.E.R. Zone-level analysis, and the third supports the block-level analysis. For the division-level file, incidents were collapsed by division and D.P. For the L.A.S.E.R. Zone- and block-level files, incidents were organized by week and assigned to L.A.S.E.R. Zones, treated blocks, or adjacent, non-treated blocks using G.I.S. software as needed.

Analytic Sample

Although the 2016 evaluation of Operation L.A.S.E.R. examined four L.A.P.D. divisions, the present study only retains the 77th Street, Newton, and Southwest Divisions. The present chapter excludes the Southeast Division from all analyses because the publicly available crime dataset severely undercounts incidents in that division and in several other non-treated divisions, rendering any statistical analyses relying on these data unreliable. Appendix Figure C1 summarizes this undercounting problem in detail. After accounting for this problem, the division-level file includes 93 observations across 3 divisions and 31 D.P.s.

Crime Outcomes

As in the 2016 evaluation, the present chapter relies on five outcomes. These outcomes are Part 1 crimes, violent Part 1 crimes, gun crime, robbery, and aggravated assault. Part 1 crimes were identified using an existing flag in the crime dataset, while violent Part 1 crimes were defined using the same flag and filtered further to include only incidents involving homicide, rape, robbery, and aggravated assault. A constructed measure of gun crime was created based on a L.A.S.E.R. discovery file released through a Public Records Act (P.R.A.) request, which instructed data analysts to define gun crime as all Part I and Part II incidents involving "M.O. codes 0309, 0430, 1100", or "Weapon Used Codes 100-199". Similarly, a constructed measure of robbery was created that includes incidents in which the crime description contained the text "robbery," and a constructed measure of aggravated assault was created that includes incidents in which the crime description contained the text "aggravated."

METHODS

The present chapter employs three distinct analyses to evaluate changes in crime after the implementation of Operation L.A.S.E.R. Rather than relying on a single unit of observation, the chapter moves from the division level to the L.A.S.E.R. Zone level and finally to the census block level. The first analysis reconstructs the 2016 evaluation's division-level interrupted time-series models. The second analysis revisits the same report's L.A.S.E.R. Zone-level comparison of crime before and after implementation. Meanwhile, the third analysis adds a block-level comparison between treated census blocks and adjacent, non-treated blocks to examine whether crime reductions were accompanied by displacement into those blocks.

Division-Level Crime Reduction Analysis

At the division level, the analysis follows the interrupted time-series design used in J.S.S.'s 2016 evaluation of Operation L.A.S.E.R. In this approach, the unit of observation is the L.A.P.D. division by D.P. As in the original report, the models examine five outcomes: Part 1 crimes, violent Part 1 crimes, gun crimes, robbery, and aggravated assault. Because the original analytic files and code are unavailable to the public, the present chapter reconstructs the division-level analysis using publicly available L.A.P.D. crime data and the modeling information described by Swatt and Uchida (2016).

To determine whether crime changed after L.A.S.E.R. began, the interrupted time-series models divide each division's crime patterns into three parts. First, the models estimate the crime trend that was already present before implementation. Next, the models estimate whether crime levels immediately shifted in the first D.P. following L.A.S.E.R. implementation. Lastly, the models estimate whether the crime trend changed after implementation.

Given that the published report did not provide the original code, several coding choices had to be inferred based on described methods and reported estimates. In the present chapter, L.A.S.E.R. begins one D.P. before full implementation, with post-implementation time set to zero in the first L.A.S.E.R. period and increasing by one in each subsequent D.P. Meanwhile, time is measured as the elapsed number of D.P.s since the beginning of the time series. Regarding the analytic window, the division-level analysis uses a 31-D.P. window, beginning with 2013 D.P. 9 and ending with 2015 D.P. 13, which is consistent with the 2016 evaluation's starting point and its description of nine post-L.A.S.E.R. D.P.s.

Consistent with the 2016 evaluation, the present study's division-level models also account for two factors that could significantly influence regression estimates. First, Newey-West standard errors with one time lag are used because crime is measured repeatedly across D.P.s, and the model errors may be correlated from one D.P. to the next. Second, the models include seasonal controls for summer, fall, winter, and spring because each season can influence crime differently throughout the year for reasons unrelated to L.A.S.E.R. In addition to these controls, the reconstruction includes an aggravated-assault recode indicator beginning on June 1, 2014. The adjustment accounts for errors in which the L.A.P.D. classified certain aggravated assaults as simple assaults (Bustamante, 2015; Swatt & Uchida, 2016).

L.A.S.E.R. Zone-Level Crime Change Analysis

For the zone-level analyses, the unit of observation is the L.A.S.E.R. Zone by week. This approach examines changes in crime within L.A.S.E.R. Zones before and after L.A.S.E.R. began. To accomplish this, the pre-L.A.S.E.R. weekly mean is subtracted from the post-L.A.S.E.R. weekly mean for each zone. The difference is then divided by the zone's area in square miles, with negative values indicating crime declines after L.A.S.E.R. began and positive values indicating crime increases. The purpose of this approach is to account for size differences across L.A.S.E.R. Zones and to improve comparability of the results.

Regarding the analytic window, this approach uses a 24-week pre-L.A.S.E.R. window and a 36-week post-L.A.S.E.R. window, which correspond to six D.P.s in the pre-implementation window and nine D.P.s in the post-implementation window. It must be noted that the 2016 evaluation made a single reference to a 25-week pre-period in its appendices. While the single reference is believed to be a typographical error or a remnant of an earlier analysis, results using a 25-week pre-period are provided in Appendix Table D2 as a sensitivity check given its explicit mention. The zone-level change is calculated as follows:

$$\Delta Y_z = (\bar{Y}_{z(post)} - \bar{Y}_{z(pre)}) / A_z$$

In this model, $\bar{Y}_{z(post)}$ and $\bar{Y}_{z(pre)}$ represent the average weekly crime counts in L.A.S.E.R. Zone$_z$ during the 36-week post- and 24-week pre-L.A.S.E.R. periods, while A_z represents the zone's area in square miles. In addition, because crime is commonly standardized by population rather than land area, Appendix Table D3 reports the 24-week zone-level results standardized by area-weighted 2010 resident population rather than by L.A.S.E.R. Zone area.

Block-Level Crime Reduction and Displacement Analysis

The final analysis shifts from L.A.S.E.R. Zones to census blocks to evaluate whether crime changes in treated areas were accompanied by changes in adjacent, non-treated blocks. In place-based policing research, studies often compare crime trends in treated locations with those in adjacent, non-treated locations to determine whether reductions in treated areas were associated with displacement into neighboring areas (Braga et al., 2019; MacDonald et al., 2016). The present chapter adopts the same logic. Specifically, if L.A.S.E.R. displaced crime from treated areas to neighboring areas, then adjacent, non-treated census blocks should show relative increases in crime following implementation.

For this approach, the unit of observation is the census block by week. First, the sample is built by classifying census blocks into retained divisions, treated blocks, and adjacent, non-treated blocks. A block is retained if at least 51 percent of its area is within a retained L.A.P.D. division. Then, a retained block is classified as treated if at least 51 percent of its area overlaps a retained 2015 L.A.S.E.R. Zone. A block is deemed non-treated if it does not meet treatment criteria but touches at least one treated block.

This method creates a treatment group and an adjacent, non-treated comparison group to detect potential displacement. Appendix Figure A2 shows a map of divisions, treated blocks, and adjacent, non-treated blocks. Although a tract-level analysis was also considered, tracts were much larger than the 2015 expansion zones examined in the retained divisions, and the zones often accounted for only small portions of larger tracts; thus, many zones did not meet the inclusion criteria for a tract-level analysis. Therefore, for the restricted 2015 expansion and the chapter's displacement analysis, the block-level approach provides a more precise comparison between treated and adjacent, non-treated blocks.

The analytic window for this approach adopts equal pre- and post-implementation periods of 36 weeks each. Tables 4 and 5 compare mean weekly crime counts in treated and adjacent, non-treated blocks before and after L.A.S.E.R. began. Regression models then formalize this comparison with a difference-in-differences approach, incorporating block and week fixed effects. The estimate in this approach is the treated-by-post interaction, which indicates whether crime changed differently in treated blocks after L.A.S.E.R. began than in adjacent, non-treated blocks during the same period. The modeling approach for this difference-in-differences analysis is as follows:

$$Y_{bt} = \beta(Treated\ Block_b \times Post_t) + \alpha_b + \lambda_t + \varepsilon_{bt}$$

In this model, Y_{bt} is the weekly crime count for $block_b$ in $week_t$, Treated $Block_b$ indicates whether $block_b$ was located inside a L.A.S.E.R. Zone, $Post_t$ indicates whether $week_t$ occurred after L.A.S.E.R. began, α_b represents block fixed effects, λ_t represents week fixed effects, and β is the difference-in-differences estimate. In the model, block fixed effects control for time-invariant differences between blocks, and week fixed effects account for temporal trends affecting all blocks. Standard errors are clustered at the block level.

RESULTS

Table 1. Division-Level Immediate Shift in Crime Following L.A.S.E.R. Implementation

Division	Outcome	Coefficient	Std. Error	p Value
77th Street	Part 1 Crimes	11.416	35.801	0.753
77th Street	Violent Pt. 1	-14.520	12.930	0.274
77th Street	Gun Crime	-5.023	9.219	0.591
77th Street	Robbery	-5.295	5.325	0.330
77th Street	Agg. Assault	-9.134	9.261	0.335
Newton	Part 1 Crimes	27.499	23.995	0.264
Newton	Violent Pt. 1	-5.262	8.840	0.558
Newton	Gun Crime	2.274	8.544	0.793
Newton	Robbery	-1.622	4.788	0.738
Newton	Agg. Assault	-3.197	7.397	0.670
Southwest	Part 1 Crimes	-14.520	41.431	0.729
Southwest	Violent Pt. 1	6.253	10.802	0.569
Southwest	Gun Crime	-10.863	9.981	0.288
Southwest	Robbery	1.235	6.609	0.853
Southwest	Agg. Assault	0.916	8.223	0.912

Notes: Coefficients represent the immediate level shift in the 31-D.P. reconstruction; S.E.s are Newey-West with one lag; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 2. Division-Level Change in Crime Trend Following L.A.S.E.R. Implementation

Division	Outcome	Coefficient	Std. Error	p Value
77th Street	Part 1 Crimes	-5.136	5.240	0.338
77th Street	Violent Pt. 1	-4.953***	1.191	< 0.001
77th Street	Gun Crime	-4.502***	1.100	< 0.001
77th Street	Robbery	-2.227*	0.890	0.020
77th Street	Agg. Assault	-2.568*	1.127	0.033
Newton	Part 1 Crimes	-14.621***	3.068	< 0.001
Newton	Violent Pt. 1	-2.744	1.842	0.151
Newton	Gun Crime	-3.430	1.908	0.086
Newton	Robbery	-1.658*	0.746	0.036
Newton	Agg. Assault	-0.656	1.448	0.655
Southwest	Part 1 Crimes	5.326	9.339	0.574
Southwest	Violent Pt. 1	1.025	2.279	0.657
Southwest	Gun Crime	1.771	1.612	0.284
Southwest	Robbery	-0.684	1.461	0.644
Southwest	Agg. Assault	1.076	1.806	0.557

Notes: Coefficients represent the post-L.A.S.E.R. change in crime trend in the 31-D.P. reconstruction; S.E.s are Newey-West with one lag; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3. L.A.S.E.R. Zone-Level Crime Change Using a 24-Week Pre-L.A.S.E.R. Window

Division	Zone	Area (Mi²)	Part 1	Violent Pt. 1	Gun Crime	Robbery	Agg. Assault
77th Street	1	0.24	5.22	2.35	0.76	0.06	1.64
77th Street	2	0.20	4.19	-2.72	-0.14	-1.12	-1.54
77th Street	3	0.21	-2.14	0.33	-0.20	1.61	-1.07
77th Street	4	0.16	0.27	0.89	3.20	1.42	-0.53
77th Street	5	0.13	5.95	-0.63	-0.21	0.00	-0.21
77th Street	6	0.13	-3.24	-0.31	0.10	0.84	-0.84
Newton	1	0.13	-5.62	-4.79	-3.95	-2.60	-1.87
Newton	2A	0.08	3.89	5.59	-2.54	4.40	1.69
Newton	2B	0.07	-1.65	-0.62	-3.92	-0.83	1.45
Newton	3	0.12	0.45	0.00	-1.24	-1.13	1.13
Newton	4	0.12	5.85	2.41	0.34	-0.11	2.07
Southwest	1	0.50	-2.79	-0.81	-1.15	-0.70	-0.31
Southwest	2A	0.09	-9.74	0.59	0.59	1.48	-0.15
Southwest	2B	0.09	1.97	1.21	-0.46	-0.15	1.52
Southwest	3	0.14	-3.09	0.50	-0.10	-0.50	1.30
Southwest	4	0.10	15.75	7.08	3.47	6.01	1.47
Southwest	5	0.05	-3.04	-0.91	-1.22	-2.13	1.22

Notes: Values equal the post-L.A.S.E.R. weekly mean minus the pre-L.A.S.E.R. weekly mean per square mile; pre-window = 24 weeks; post-window = 36 weeks.

Table 4. Mean Weekly Crime Counts in Treated Blocks by Division

Division	Outcome	Pre-Period	Post-Period	Change
77th Street	Part 1	0.173	0.176	0.003
77th Street	Violent Pt. 1	0.064	0.071	0.007
77th Street	Gun Crime	0.027	0.026	-0.001
77th Street	Robbery	0.023	0.030	0.007
77th Street	Agg. Assault	0.038	0.038	0.001
Newton	Part 1	0.191	0.172	-0.019
Newton	Violent Pt. 1	0.072	0.071	-0.001
Newton	Gun Crime	0.035	0.020	-0.016
Newton	Robbery	0.036	0.032	-0.003
Newton	Agg. Assault	0.033	0.036	0.003
Southwest	Part 1	0.399	0.368	-0.031
Southwest	Violent Pt. 1	0.105	0.104	-0.001
Southwest	Gun Crime	0.037	0.029	-0.007
Southwest	Robbery	0.054	0.054	0.000
Southwest	Agg. Assault	0.046	0.046	0.000
Combined	Part 1	0.249	0.237	-0.012
Combined	Violent Pt. 1	0.079	0.082	0.003
Combined	Gun Crime	0.032	0.026	-0.006
Combined	Robbery	0.036	0.038	0.003
Combined	Agg. Assault	0.039	0.040	0.001

Notes: Values are mean weekly counts per treated block; pre- and post-implementation windows = 36 weeks; change = post mean minus pre mean.

Table 5. Mean Weekly Crime Counts in Adjacent, Non-Treated Blocks by Division

Division	Outcome	Pre-Period	Post-Period	Change
77th Street	Part 1	0.209	0.223	0.015
77th Street	Violent Pt. 1	0.075	0.069	-0.006
77th Street	Gun Crime	0.029	0.029	0.000
77th Street	Robbery	0.038	0.032	-0.006
77th Street	Agg. Assault	0.033	0.033	0.000
Newton	Part 1	0.151	0.142	-0.009
Newton	Violent Pt. 1	0.045	0.052	0.007
Newton	Gun Crime	0.022	0.024	0.002
Newton	Robbery	0.024	0.017	-0.007
Newton	Agg. Assault	0.020	0.032	0.013
Southwest	Part 1	0.180	0.183	0.003
Southwest	Violent Pt. 1	0.048	0.061	0.014
Southwest	Gun Crime	0.019	0.021	0.002
Southwest	Robbery	0.025	0.028	0.003
Southwest	Agg. Assault	0.020	0.031	0.011
Combined	Part 1	0.185	0.191	0.005
Combined	Violent Pt. 1	0.059	0.062	0.003
Combined	Gun Crime	0.024	0.025	0.001
Combined	Robbery	0.030	0.027	-0.004
Combined	Agg. Assault	0.026	0.032	0.007

Notes: Values are mean weekly counts per adjacent, non-treated block; pre- and post-implementation windows = 36 weeks; change = post mean minus pre mean.

Table 6. Difference-in-Differences Estimates for Treated vs. Adjacent, Non-Treated Blocks

Outcome	Coefficient	Std. Error	t Statistic	p Value
Part 1	-0.018	0.031	-0.558	0.577
Violent Pt. 1	-0.001	0.008	-0.076	0.939
Gun Crime	-0.008	0.004	-1.723	0.086
Robbery	0.006	0.005	1.147	0.252
Agg. Assault	-0.006	0.005	-1.199	0.231

Notes: Coefficient = treated-by-post interaction; models include block and week fixed effects; S.E.s are block-clustered; n = 40,176 block-week observations across 558 blocks.

DISCUSSION

Consistent with the original 2016 evaluation, the present chapter's division- and zone-level analyses find mixed evidence on whether Operation L.A.S.E.R. reduced crime in treated locations. The finding also aligns with the National Academies of Sciences, Engineering, and Medicine's (2018) conclusion that the crime-reduction effects of proactive policing vary substantially across strategies and are frequently localized and short-term. Meanwhile, the present chapter's block-level analysis offers a unique perspective not available in the original J.S.S. evaluation.

At the division level, Table 1 suggests that Operation L.A.S.E.R. had an inconsistent impact on crime across the three retained divisions in the first D.P. following implementation. In the 77th Street Division, Table 1 shows that crime decreased across all categories, with the exception of Part 1 crimes. In the Newton Division, Table 1 shows decreases for violent Part 1 crime, robbery, and aggravated assault, and increases for Part 1 crime and gun crime. Meanwhile, Southwest shows decreases in Part 1 crime and gun crime but increases in violent Part 1 crime, robbery, and aggravated assault. Across all of the division-outcome combinations, none of the estimates reached statistical significance at the conventional $p < 0.05$ level. Thus, the immediate-shift estimates provide inconclusive evidence that L.A.S.E.R. consistently reduced crime in the first D.P. following implementation. This finding is consistent with Swatt and Uchida's (2016) evaluation, which also noted that unusually high or low crime counts may drive the immediate shift estimate in the first post-L.A.S.E.R. D.P., hence their greater emphasis on crime trend patterns than on the immediate shift.

When focusing on crime trends rather than immediate shifts, Table 2 provides stronger evidence of reductions, particularly in the 77th Street and Newton Divisions. In both divisions, the post-L.A.S.E.R. trend estimates are negative across all five outcomes. In 77th Street, the estimates indicate post-L.A.S.E.R. trend reductions for Part 1 crime (-5.136), violent Part 1 crime (-4.953), gun crime (-4.502), robbery (-2.227), and aggravated assault (-2.568), with violent Part 1 crime and gun crime reaching statistical significance at the $p < 0.001$ level, and robbery and aggravated assault reaching statistical significance at the $p < 0.05$ level. Similarly, the Newton Division shows negative trend estimates for Part 1 crime (-14.621), violent Part 1 crime (-2.744), gun crime (-3.430), robbery (-1.658), and aggravated assault (-0.656), with Part 1 crime reaching statistical significance at the $p < 0.001$ level, and robbery at the $p < 0.05$ level. However, the Southwest Division shows positive crime trends across all outcomes, with the exception of robbery, though none of the five outcomes reach statistical significance.

At the zone level, Table 3 reveals that Operation L.A.S.E.R. had an inconsistent impact on crime across the 17 retained L.A.S.E.R. Zones. For example, the Newton Division's L.A.S.E.R. Zone 1 declined across all five outcomes, including Part 1 crime (-5.62), violent Part 1 crime (-4.79), gun crime (-3.95), robbery (-2.60), and aggravated assault (-1.87). Meanwhile, other zones experienced the opposite trend. For example, the Southwest Division's L.A.S.E.R. Zone 4 shows increases across all five outcomes, including Part 1 crime (15.75), violent Part 1 crime (7.08), gun crime (3.47), robbery (6.01), and aggravated assault (1.47).

At the census block level, Tables 4 and 5 show that mean weekly crime counts changed only slightly after implementation in both treated and adjacent, non-treated blocks. Across the 20 rows in Table 5, crime decreased in five comparisons, remained unchanged in two, and increased in 13. Table 6 reaches a similar conclusion after accounting for block and week fixed effects. Specifically, the difference-in-differences model yields near-zero estimates across all five outcomes, none of which are statistically significant at the conventional $p < 0.05$ level. Thus, the block-level models do not provide sufficient evidence that L.A.S.E.R. reduced crime in treated blocks relative to adjacent, non-treated blocks, nor do they show statistically significant increases in crime in adjacent, non-treated blocks, which would indicate displacement.

LIMITATIONS

Despite a faithful replication attempt and the introduction of a unique, supplemental approach not present in the original evaluation, the present study is not without limitations. First, all analyses rely on publicly available L.A.P.D. incident-level crime data. Although these data enable an independent evaluation of Operation L.A.S.E.R., the publicly available dataset is not an exact, one-to-one copy of the internal file that the L.A.P.D. and J.S.S. used for their evaluation. In addition, the public dataset contains quality issues that limit how closely one can replicate the 2016 evaluation. As described in Appendix C, the public dataset severely undercounts crime in the Southeast Division and contains duplicate records in calendar year 2016. While the duplicate-record issue does not affect analyses, the undercounting problem prevents accounting for all four divisions present in the 2016 evaluation.

Second, the analysis required the reconstruction of L.A.S.E.R. Zone boundaries, timing, and coding decisions. The vast majority of the required information, including boundary information, implementation dates, and variable construction details, can be traced to public and administrative records obtained under the Public Records Act (P.R.A.) requests. However, some details are less clear and had to be inferred based on the available information, such as the timing of the aggravated-assault recode. For that reason, the reconstructed timing and coding choices may not perfectly align with the internal records used by the L.A.P.D. and J.S.S.

Third, the block-level approach relies on a 51 percent area-overlap rule to classify treated blocks. While this design helps narrow down the blocks with the highest exposure to L.A.S.E.R. based on their area overlap, some L.A.S.E.R. Zones were narrow and centered on street segments rather than actual blocks or buildings. Given that blocks commonly split along street segments, the occasional mismatch in geometry meant that a L.A.S.E.R. Zone's area could be split across two or more blocks, making it harder for any one block to meet the 51 percent treatment threshold. As a result, areas that received treatment may occasionally be missed using this approach. In the present chapter, the boundary issue only impacted the 77th Street Division's L.A.S.E.R. Zone 5, referred to as "SST_L.A.S.E.R.5" in official records. See Appendix Figure A1 for a map showing SST_L.A.S.E.R.5, and Appendix Figure A2 for a map showing the allocation of treated and adjacent, non-treated blocks.

CONCLUSION

In summary, the present chapter reevaluates Operation L.A.S.E.R. by asking whether the intervention reduced crime and whether any reductions were accompanied by displacement into adjacent, non-treated census blocks. While the original 2016 evaluation presented the intervention as promising and advocated for its expansion, the present chapter calls for a more cautious interpretation and conclusion.

Across division- and L.A.S.E.R. Zone-level analyses, there is limited evidence to suggest that L.A.S.E.R. directly reduced crime. For instance, division-level estimates show that two of the three treated divisions experienced downward trends in crime following L.A.S.E.R.'s implementation, which is consistent with findings reported by Swatt and Uchida (2016); however, zone-level estimates yield inconsistent results. Across the five outcomes for each of the

17 L.A.S.E.R. Zones (85 total observations), crime decreased in 43 cases, increased in 40, and did not change in two. Thus, there is limited evidence that the division-level crime reductions were attributable to targeted enforcement within L.A.S.E.R. Zones.

At the census block level, there is little evidence that L.A.S.E.R. displaced crime into adjacent, non-treated blocks, with Tables 4 and 5 showing only small changes in mean weekly crime counts for both treated and adjacent, non-treated blocks following implementation. However, Table 6, which accounts for block and week fixed effects, finds that the treated-by-post estimates are close to zero for all five outcomes, with none reaching statistical significance at conventional levels. Overall, these block-level findings suggest that although crime was not displaced to neighboring areas, there is no strong evidence that L.A.S.E.R. reduced crime differently in treated blocks than in adjacent, non-treated blocks.

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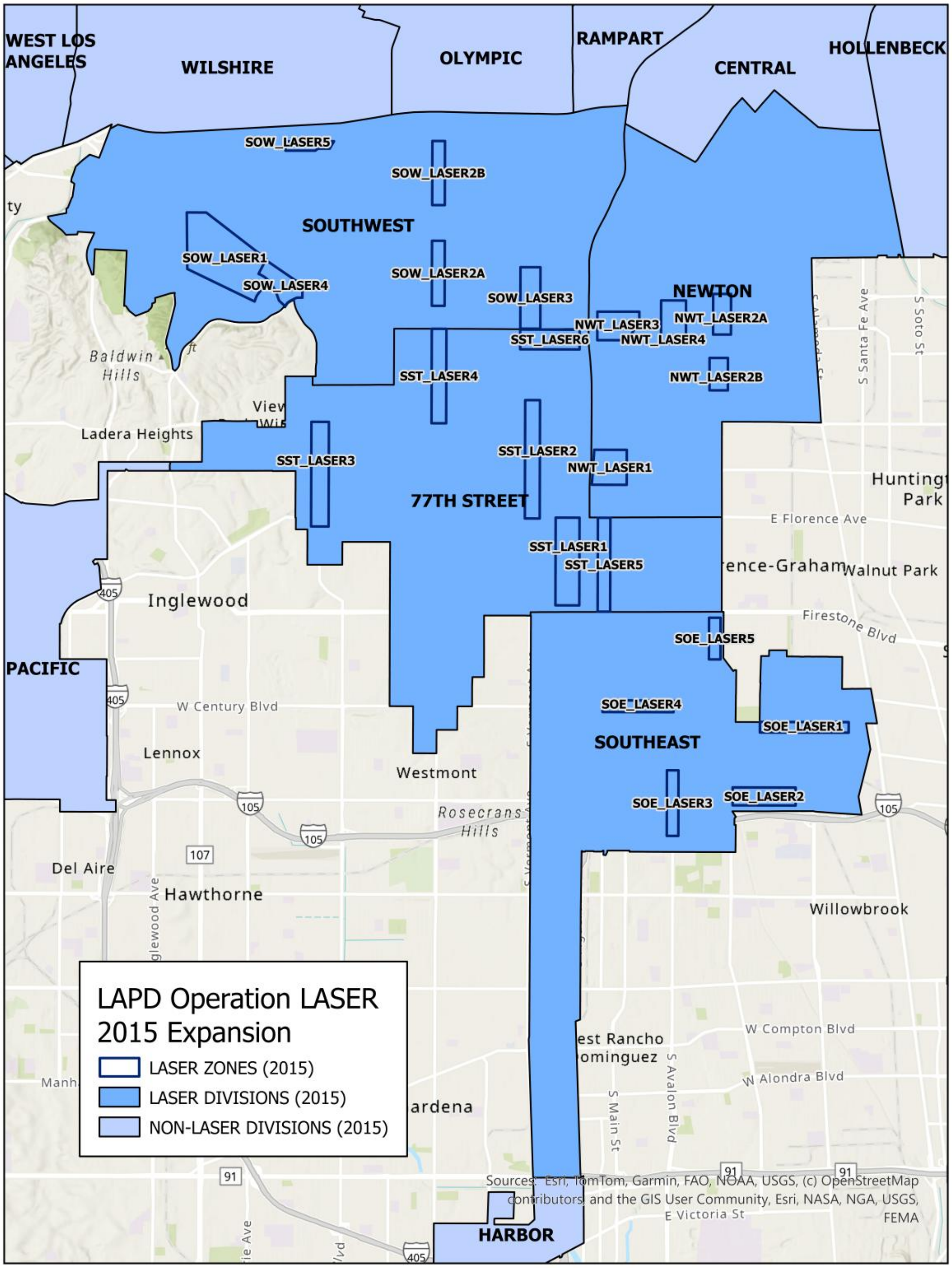
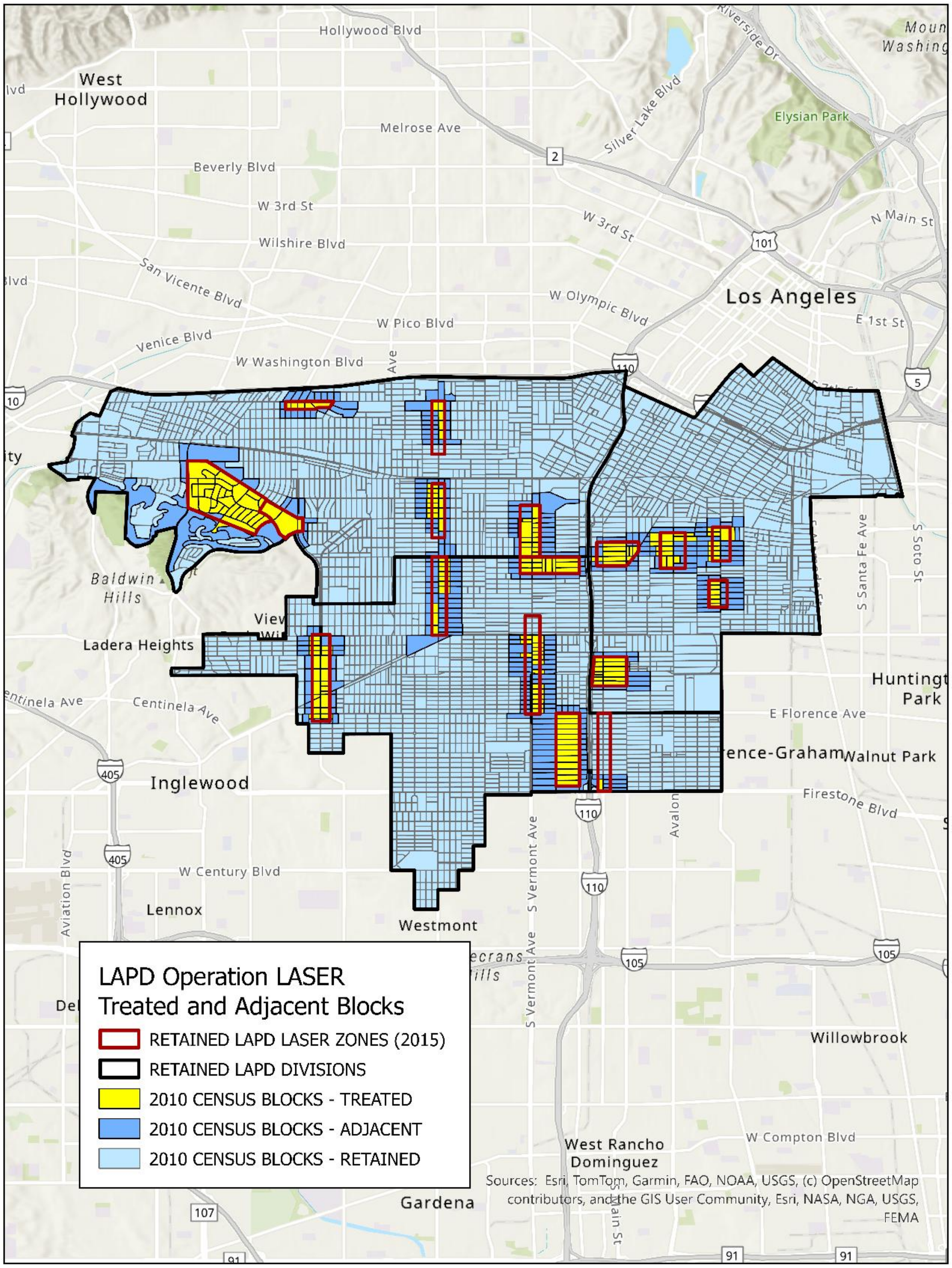
APPENDIX A

Appendix Figure A1. L.A.P.D. Operation L.A.S.E.R. 2015 Expansion



Appendix Figure A2. L.A.S.E.R. Treated and Adjacent Blocks



APPENDIX B

2016 Evaluation Division-Level Benchmark Results

Appendix Table B1. Immediate Shift in Crime Following L.A.S.E.R. - Benchmark Results

Division	Outcome	2016 Eval.	Reconstruction	Difference
77th Street	Part 1 Crimes	8.82	11.42	2.60
77th Street	Violent Pt. 1	-17.52	-14.52	3.00
77th Street	Gun Crime	-7.40	-5.02	2.38
77th Street	Robbery	-5.20	-5.29	-0.09
77th Street	Agg. Assault	-12.56	-9.13	3.43
Newton	Part 1 Crimes	25.30	27.50	2.20
Newton	Violent Pt. 1	-8.73	-5.26	3.47
Newton	Gun Crime	2.53	2.27	-0.26
Newton	Robbery	-0.98	-1.62	-0.64
Newton	Agg. Assault	-7.91	-3.20	4.71
Southwest	Part 1 Crimes	-15.40	-14.52	0.88
Southwest	Violent Pt. 1	-1.28	6.25	7.53
Southwest	Gun Crime	-7.97	-10.86	-2.89
Southwest	Robbery	1.56	1.23	-0.33
Southwest	Agg. Assault	-2.55	0.92	3.47

Notes: Values compare Swatt and Uchida's (2016) Table 2 estimates with the present study's 31-D.P. reconstruction; Southeast omitted.

Appendix Table B2. Change in Crime Trend Following L.A.S.E.R. - Benchmark Results

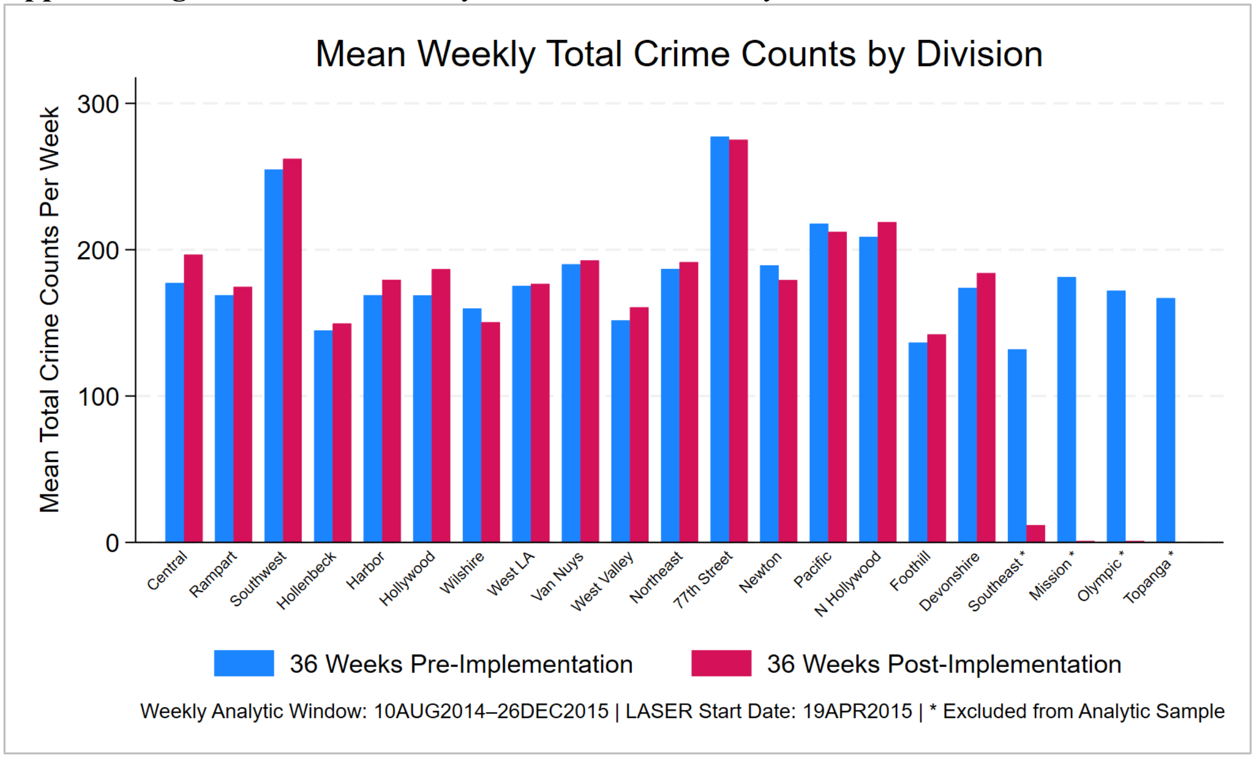
Division	Outcome	2016 Eval.	Reconstruction	Difference
77th Street	Part 1 Crimes	-6.38	-5.14	1.24
77th Street	Violent Pt. 1	-3.96	-4.95	-0.99
77th Street	Gun Crime	-3.83	-4.50	-0.67
77th Street	Robbery	-2.44	-2.23	0.21
77th Street	Agg. Assault	-1.31	-2.57	-1.26
Newton	Part 1 Crimes	-14.81	-14.62	0.19
Newton	Violent Pt. 1	-2.21	-2.74	-0.53
Newton	Gun Crime	-3.64	-3.43	0.21
Newton	Robbery	-1.93	-1.66	0.27
Newton	Agg. Assault	0.06	-0.66	-0.72
Southwest	Part 1 Crimes	5.07	5.33	0.26
Southwest	Violent Pt. 1	2.17	1.03	-1.14
Southwest	Gun Crime	1.94	1.77	-0.17
Southwest	Robbery	-0.79	-0.68	0.11
Southwest	Agg. Assault	2.56	1.08	-1.48

Notes: Values compare Swatt and Uchida's (2016) Table 2 estimates with the present study's 31-D.P. reconstruction; Southeast omitted.

APPENDIX C

Duplicate Records and Undercounting in Public Crime Data

Appendix Figure C1. Mean Weekly Total Crime Counts by Division



Appendix Table C1. Duplicate Records in Public Crime Data

Copy Count	Raw Rows	% of Raw Rows	Rows Removed	% Removed
1	2,017,519	94.58	0	0
2	115,618	5.42	57,809	2.71

Notes: All duplicate observations occurred from January 1 through December 31, 2016.

Appendix Table C2. Duplicate Observation Distribution by Division

Division	Duplicate Obs.	Percent
Southeast	23,046	19.93
Mission	22,168	19.17
Topanga	22,064	19.08
Olympic	21,198	18.33
Devonshire	14,246	12.32
North Hollywood	1,072	0.93
Pacific	1,010	0.87
77th Street	960	0.83
Southwest	958	0.83
Van Nuys	948	0.82
West Valley	918	0.79
West Los Angeles	808	0.7
Foothill	814	0.7
Wilshire	780	0.67
Hollywood	770	0.67
Northeast	790	0.68
Hollenbeck	642	0.56
Harbor	632	0.55
Rampart	626	0.54
Newton	600	0.52
Central	568	0.49
Total	115,618	100

Notes: Percentages are shares of all duplicate observations; all duplicates occurred from January 1 through December 31, 2016.

APPENDIX D

Zone-Level Benchmark and Sensitivity Checks

Appendix Table D1. 2016 Evaluation Zone-Level Findings for Retained Divisions

Division	Zone	Area (Mi^2)	Part 1	Violent Pt. 1	Gun Crime	Robbery	Agg. Assault
77th Street	1	0.24	4.03	0.25	-0.22	-0.80	0.56
77th Street	2	0.20	6.40	-0.36	1.22	-1.11	0.53
77th Street	3	0.21	0.86	2.09	1.34	1.82	0.31
77th Street	4	0.16	1.93	0.13	2.04	0.95	-0.67
77th Street	5	0.13	2.71	-0.46	1.89	1.26	-0.63
77th Street	6	0.13	-4.47	0.88	1.46	0.68	0.80
Newton	1	0.13	-1.32	-3.27	-2.29	-1.12	-2.14
Newton	2A	0.08	11.63	8.38	-0.21	4.08	4.12
Newton	2B	0.07	-2.87	-2.77	-3.79	-1.76	0.09
Newton	3	0.12	-0.33	2.64	-1.01	0.41	2.23
Newton	4	0.12	1.27	2.88	1.89	-0.65	2.91
Southwest	1	0.50	-3.00	-0.53	-1.30	-0.64	-0.23
Southwest	2A	0.09	-3.98	0.71	1.61	2.22	-0.08
Southwest	2B	0.09	3.53	0.30	0.86	0.42	0.15
Southwest	3	0.14	6.14	1.42	0.12	0.52	0.88
Southwest	4	0.10	12.57	8.47	4.44	7.10	1.76
Southwest	5	0.05	-4.89	-1.04	-0.58	-2.74	0.48

Notes: Results are from Swatt and Uchida (2016), Table 7; Southeast is omitted to match the retained sample.

Appendix Table D2. Zone-Level Replication Using a 25-Week Pre-L.A.S.E.R. Window

Division	Zone	Area (Mi^2)	Part 1	Violent Pt. 1	Gun Crime	Robbery	Agg. Assault
77th Street	1	0.24	4.85	2.40	0.57	0.22	1.52
77th Street	2	0.20	3.35	-2.83	-0.22	-1.40	-1.37
77th Street	3	0.21	-2.45	-0.17	-0.19	1.01	-1.01
77th Street	4	0.16	0.41	1.17	3.05	1.42	-0.28
77th Street	5	0.13	5.73	-0.88	-0.43	-0.48	-0.01
77th Street	6	0.13	-2.78	-0.15	0.17	0.86	-0.71
Newton	1	0.13	-5.35	-4.96	-4.33	-2.66	-2.00
Newton	2A	0.08	3.14	5.77	-2.36	4.44	1.81
Newton	2B	0.07	-2.35	-0.99	-3.75	-1.32	1.52
Newton	3	0.12	0.48	0.27	-1.12	-0.94	1.21
Newton	4	0.12	4.66	1.60	0.11	-0.65	1.79
Southwest	1	0.50	-3.29	-1.08	-1.12	-0.72	-0.57
Southwest	2A	0.09	-9.63	0.24	0.31	1.72	-0.38
Southwest	2B	0.09	1.85	1.07	-0.36	-0.46	1.66
Southwest	3	0.14	-3.20	0.18	-0.59	-0.61	1.36
Southwest	4	0.10	15.59	6.96	2.00	5.77	1.58
Southwest	5	0.05	-2.24	-0.58	-1.00	-1.95	1.36

Notes: Swatt and Uchida (2016) reference a 25-week period formula; results are provided as a sensitivity check; Southeast is omitted to match the retained sample.

Appendix Table D3. Zone-Level Mean Effects Standardized by 2010 Resident Population

Division	Zone	2010 Pop.	Part 1	Violent Pt. 1	Gun Crime	Robbery	Agg. Assault
77th Street	1	7,568	0.163	0.073	0.024	0.002	0.051
77th Street	2	3,351	0.249	-0.162	-0.008	-0.066	-0.091
77th Street	3	3,434	-0.129	0.020	-0.012	0.097	-0.065
77th Street	4	2,343	0.018	0.059	0.213	0.095	-0.036
77th Street	5	3,104	0.255	-0.027	-0.009	0.000	-0.009
77th Street	6	3,204	-0.134	-0.013	0.004	0.035	-0.035
Newton	1	3,174	-0.236	-0.201	-0.166	-0.109	-0.079
Newton	2A	2,137	0.149	0.214	-0.097	0.169	0.065
Newton	2B	1,394	-0.080	-0.030	-0.189	-0.040	0.070
Newton	3	3,370	0.016	0.000	-0.045	-0.041	0.041
Newton	4	3,178	0.223	0.092	0.013	-0.004	0.079
Southwest	1	15,187	-0.091	-0.027	-0.037	-0.023	-0.010
Southwest	2A	995	-0.921	0.056	0.056	0.140	-0.014
Southwest	2B	1,683	0.107	0.066	-0.025	-0.008	0.083
Southwest	3	3,060	-0.141	0.023	-0.005	-0.023	0.059
Southwest	4	67	24.318	10.923	5.358	9.274	2.267
Southwest	5	1,313	-0.106	-0.032	-0.042	-0.074	0.042

Notes: Values equal the post-L.A.S.E.R. weekly mean minus the 24-week pre-L.A.S.E.R. mean per 1,000 area-weighted residents; resident-based rates may understate exposure in commercial areas.

APPENDIX E

Data Curation

Crime Records and Outcome Construction

Chapter 1 relies on the Los Angeles Police Department (L.A.P.D.)'s publicly available incident-level crime data spanning 2010 through 2019. The source file used during data cleaning contains 2,133,137 records and includes each incident's occurrence date, L.A.P.D. division, Part 1 or Part 2 classification, crime description, modus operandi codes, weapon code, and geographic coordinates.

While the original data contained much of the information needed for the analyses, additional crime outcomes and time measures were constructed for the present study. For instance, to create consistent time measures across the three analyses, incident dates were linked to a custom time-series dataset containing the corresponding calendar day, week, year, and L.A.P.D. Deployment Period (D.P.). The exact D.P. start and end dates used to construct this file were obtained from public records available online. Sequential time measures and indicators identifying the pre- and post-L.A.S.E.R. periods were then added as needed for each analysis.

During data cleaning, 57,809 exact row duplicates were also identified, all occurring in 2016. Because duplicate incidents would artificially increase crime counts after aggregation, these records were removed before the division-, zone-, and block-level files were created. As a result, the duplicate records do not impact any of the analyses.

Constructed Crime Outcomes

As in the 2016 evaluation, Chapter 1 examines five crime outcomes: Part 1 crime, violent Part 1 crime, gun crime, robbery, and aggravated assault. Part 1 crime relies on the classification provided in the public incident file, while violent Part 1 applies a filter to that flag to identify incidents involving homicide, rape, robbery, or aggravated assault. Once identified, a separate flag is created to capture only those specific crimes.

To construct gun crime in a manner consistent with the original evaluation, information from L.A.S.E.R. records obtained through a Public Records Act (P.R.A.) request by the Stop L.A.P.D. Spying Coalition was used to identify the relevant codes. Specifically, an incident was classified as a gun crime if a Part 1 or Part 2 record contained modus operandi code 0309, 0430, or 1100, or a weapon code between 100 and 199. Meanwhile, robbery was identified when the crime description contained "robbery," and aggravated assault was identified when the description contained "aggravated."

Each outcome was first represented by an incident-level indicator and was later aggregated to the geographic and time unit required for the corresponding analysis. In addition to these crime indicators, the division-level files include season indicators and a measure accounting for the 2014 change in aggravated-assault reporting.

Lastly, among the four divisions examined in the 2016 evaluation, the Southeast Division was excluded because the publicly available crime data severely undercount incidents in that division, making comparisons over time unreliable. Thus, the analyses reported in Chapter 1 focus on the 77th Street, Newton, and Southwest Divisions.

Division- and L.A.S.E.R. Zone-Level Panels

For the division-level reconstruction, incidents were aggregated by L.A.P.D. division and D.P. To maintain comparability with the 2016 evaluation, the primary analytic window begins in 2013 D.P. 9 and ends in 2015 D.P. 13, preserving the starting point and nine post-L.A.S.E.R. periods described in the original report. The resulting window contains 22 pre-implementation periods and nine post-implementation periods, for 31 D.P.s in total. With three retained divisions, the resulting panel contains 93 division-by-D.P. observations. Each observation includes the five crime counts, treatment timing, elapsed time, season, and the aggravated-assault reporting-change measure.

Meanwhile, the L.A.S.E.R. Zone-level analysis uses the 2015 treatment polygons constructed for the present dissertation from public and administrative maps and records. These materials were used to identify each zone's name, division, geographic boundary, and implementation dates. Incident coordinates were then spatially assigned to the L.A.S.E.R. Zone polygons so that the five crime outcomes could be aggregated by zone and week. To preserve a complete weekly panel, weeks with no recorded incidents were retained and assigned crime counts equal to zero rather than being dropped from the dataset.

After excluding Southeast, the main sample contains 17 L.A.S.E.R. Zones. The primary analytic window includes 24 pre-L.A.S.E.R. weeks and 36 post-L.A.S.E.R. weeks, producing 1,020 zone-week observations. Because the 2016 evaluation also contains a reference to a 25-week pre-period, an alternative 25-week window was constructed as a sensitivity check, producing 1,037 observations. The zone-level files additionally contain measures of zone area, weekly crime, treatment timing, changes in mean weekly crime, and changes standardized by zone area. Given that crime is also commonly standardized by population, an area-weighted population measure was retained for the supplemental population-standardized analysis.

Block-Level Displacement Panel

Lastly, the displacement analysis combines 2010 Census block polygons with the reviewed L.A.P.D. division and 2015 L.A.S.E.R. Zone boundaries. The Census data provide each block's identifier and geographic boundary. Using these polygons, the analysis calculated each block's area, the percentage of that area falling within each retained police division, and the percentage overlapping a retained L.A.S.E.R. Zone.

To ensure that blocks were primarily located within one of the divisions included in the analysis, a census block entered the sample when at least 51 percent of its area fell within the 77th Street, Newton, or Southwest Division. The same majority-area approach was then used to identify treatment: among the retained blocks, a block was classified as treated when at least 51 percent of its area overlapped a retained L.A.S.E.R. Zone. To construct the comparison group used to examine displacement, an untreated block was classified as adjacent, non-treated when its boundary touched at least one treated block.

The resulting sample contains 558 census blocks, including 221 treated blocks and 337 adjacent, non-treated blocks. To compare crime over equivalent periods before and after implementation, these blocks were crossed with equal 36-week pre- and post-L.A.S.E.R. windows, producing 40,176 block-week observations. Each observation contains the treatment group, time period, L.A.P.D. division, and the five crime outcomes. Similar to the zone-level panel, weeks without a recorded incident remain in the dataset with crime counts equal to zero, ensuring that periods without crime are represented rather than omitted.

CHAPTER 2

The Academic Effects of Proactive Policing Exposure:

Evidence from L.A.U.S.D. Assessment Scores

ABSTRACT

Research shows that proactive policing strategies can influence academic performance in various ways. On the one hand, proactive policing may reduce crime rates and, as a result, increase student performance by creating safer environments that improve overall community well-being. On the other hand, if students perceive police activity as aggressive or unjust, they may experience increased stress and distrust, which in turn can negatively impact academic performance. The present chapter examines whether exposure to the Los Angeles Police Department (L.A.P.D.)'s Operation L.A.S.E.R. (Los Angeles Strategic Extraction and Restoration), a data-driven policing program aimed at targeting repeat offenders and crime-prone locations, was associated with school-level English Language Arts (E.L.A.) and Mathematics performance. In doing so, the present chapter follows a panel of 55 Los Angeles Unified School District (L.A.U.S.D.) high schools with reported California High School Exit Examination (C.A.H.S.E.E.) scores from 2010 to 2015, and Smarter Balanced Assessment Consortium (S.B.A.C.) scores from 2015 to 2019, creating a unique panel spanning 2010 to 2019. Results show that seven of the 55 schools had campus boundaries that intersected areas marked by the L.A.P.D. as active for increased patrols. A comparison of ever-treated and never-treated schools found that exposure to Operation L.A.S.E.R. was associated with lower assessment scores during years of active treatment, though the association was not statistically significant at conventional levels. In addition, findings revealed that both ever-treated and never-treated schools served predominantly Hispanic students, with Black students comprising the second-largest racial or ethnic group.

Keywords: proactive policing; educational outcomes; neighborhood effects

INTRODUCTION

Throughout the United States, law enforcement agencies implement proactive policing strategies to reduce or prevent crime. In some communities, these approaches are minimally invasive and aim to address crime by strengthening social bonds between a department and the public. Minimally invasive measures typically include events such as “coffee with a cop” and community block parties, as well as increased foot patrols that encourage officers to speak with the public, build trust, and learn about the issues their communities face. In denser, more crime-prone locations, however, proactive policing approaches often focus less on community relations and public perceptions and instead seek to address crime more directly, often through more assertive measures such as hot spot policing, broken windows policing, or stop-and-frisk (S.Q.F.). Through the latter, agencies often attempt to address more serious crimes by concentrating efforts on specific people or places believed to be associated with criminal activity, such as homicide, rape, robbery, aggravated assault, or other violent crimes.

While some studies have found that reducing crime can improve community well-being, others have found that focusing solely on crime statistics while neglecting community trust, public perceptions, and the social outcomes of policing tactics risks harming the very community law enforcement aims to protect. For instance, in recent years, research has found that teenagers and young adults who live, travel, or attend school in heavily policed areas are at higher risk of experiencing direct and vicarious police contact. These experiences with the police have a range of social impacts, including shaping perceptions of public institutions, altering travel routes to and from school or work, and affecting mental health, which in turn can impact students’ academic performance. Thus, it is essential to investigate not only how policing strategies can impact crime but also how these strategies shape social outcomes following exposure.

The present chapter focuses on the Los Angeles Police Department (L.A.P.D.)'s Operation L.A.S.E.R., also known as the Los Angeles Strategic Extraction and Restoration program, and examines whether the department's proactive policing approach to reducing crime was associated with assessment score outcomes across a panel of traditional Los Angeles Unified School District (L.A.U.S.D.) high schools. The central research question asks, "Did exposure to an active L.A.S.E.R. Zone relate to school-level English Language Arts (E.L.A.) and Mathematics performance at traditional L.A.U.S.D. high schools from 2010 to 2019?"

LITERATURE REVIEW

Operation L.A.S.E.R.

Operation L.A.S.E.R. was a data-driven policing program that sought to reduce crime in the City of Los Angeles by targeting repeat offenders and crime-prone locations. The Los Angeles Police Department (L.A.P.D.), in collaboration with its research partner, Justice & Security Strategies, Inc. (J.S.S.), began designing the program in 2009 as part of the Bureau of Justice Assistance (B.J.A.)'s Smart Policing Initiative (S.P.I.), a federal program that sought to reduce crime nationwide by awarding competitive grants to agencies and research collaborators who utilized new and innovative policing strategies to address crime.

In Los Angeles, the initiative led to the creation of the Los Angeles S.P.I. team, which brought together L.A.P.D. officials and J.S.S. researchers. Together, the team sought to develop a program that reduced serious violent crime throughout the City of Los Angeles without relying on the same techniques the department had used in previous years, such as concentrating a large number of officers in troubled areas without focusing on specific problems, people, or places.

While increasing the number of officers in troubled areas is, in a way, a form of proactive policing, the Los Angeles S.P.I. team sought to develop a more targeted approach.

By adopting the Scanning, Analysis, Response, and Assessment (S.A.R.A.) framework, the Los Angeles S.P.I. team settled on addressing gun- and gang-related crime, targeting repeat offenders and deploying officers to locations where such crimes frequently occur (Uchida et al., 2012). This innovative, targeted approach later became the Operation L.A.S.E.R. pilot program, named after the department's desire to create a minimally invasive policing program focused on extracting repeat offenders from troubled communities with L.A.S.E.R.-like precision. Upon analyzing recent and historical crime data, the Los Angeles S.P.I. team selected the Newton Division as the intervention division because it consistently ranked among the top divisions for having the most gun- and gang-related crime incidents across multiple years. By September 2011, the Operation L.A.S.E.R. pilot program was in full effect.

As part of the person-based approach, the Los Angeles S.P.I. established the Crime Intelligence Detail (C.I.D.) team. This team was responsible for collecting data on repeat offenders, compiling the information into concise reports, formally called Chronic Offender Bulletins, and distributing those reports both in print and electronically to supervisors and patrol officers, a process simplified by the assistance of Palantir software that helped compile the information into a unified database. These bulletins included details on suspects' frequented areas, associates, residences, driving patterns, and much more.

By contrast, the place-based approach relied on crime analysts to examine crime data to identify micro-locations in need of intervention. To accomplish this task, analysts examined historical and recent crime data, identified areas consistently affected by crime, established formal boundaries around those locations, and recommended increased patrols in those areas.

The Los Angeles S.P.I. team formally referred to these designated micro-locations as L.A.S.E.R. Zones.

After declaring the pilot program successful, the Los Angeles S.P.I. team expanded the intervention to additional divisions. In 2015, the Los Angeles S.P.I. team launched a new wave of Operation L.A.S.E.R. that included the Newton, 77th Street, Southeast, and Southwest Divisions, and continued to expand the intervention to additional divisions each year until L.A.P.D. Chief Michael Moore ultimately terminated the program in 2019.

The Public's Concerns

As the program expanded to additional divisions, and more people became aware of the intervention, public attention shifted from whether Operation L.A.S.E.R. reduced crime to how it affected communities already experiencing high levels of police contact. During the latter half of the 2010s, community members, scholars, civil rights groups, defense attorneys, and city officials argued that the intervention disproportionately targeted Black and Latino neighborhoods and granted the L.A.P.D. substantial discretion to label people and places as high risk (Bhuiyan, 2021; Chambers & Ball, 2018; Lapowsky, 2018; Police Commission, 2019; Puente, 2019; Spolin, 2018).

As a result of increased patrols, individuals living within dedicated L.A.S.E.R. Zones described feeling uneasy about the increased surveillance. The program's use of Palantir software contributed to those feelings, as the software made it easier for the L.A.P.D. to compile information from field interview cards, police contacts, and chronic offender bulletins. Thus, many people worried that these records could connect individuals to people labeled as chronic offenders or particular vehicles, addresses, or locations (Lapowsky, 2018; Smith, 2019).

The L.A.P.D.'s use of chronic offender labels created a similar but distinct concern. Given that Operation L.A.S.E.R. relied on gun-related crime data, arrests, calls for service, field interview cards, gang affiliation, probation status, and other police records to identify problem areas and assign risk scores to individuals (Bhuiyan, 2021), critics argued that this process could create a feedback loop, as information collected through police contact could be fed back into the risk-scoring process to justify continued surveillance of the same people (Bhuiyan, 2021; Brayne, 2021; Lapowsky, 2018).

Relatedly, some individuals whom the Los Angeles S.P.I. team classified as chronic offenders argued that officers surveilled them from the moment they left their homes, while others felt threatened by the L.A.P.D.'s unsolicited calls, letters, and home visits (Chambers & Ball, 2018; Lapowsky, 2018; L.A.P.D., 2017; Smith, 2019). Furthermore, when community residents and local activists requested detailed information regarding the chronic offender selection process, where the program operated, and how the L.A.P.D. evaluated its effects, the L.A.P.D. declined to provide those details.

The lack of departmental transparency eventually led to litigation, public pressure, and an official audit by the Office of the Inspector General (O.I.G.), which ultimately resulted in the program's termination. The official O.I.G. audit identified crucial problems in the department's handling of Operation L.A.S.E.R., including limited departmental oversight, inconsistent criteria for labeling suspects as chronic offenders, and insufficient data to accurately measure the program's success (Bhuiyan, 2021; Puente, 2019; Smith, 2019; Stop L.A.P.D. Spying Coalition, 2019). In addition, the audit also found that nearly 80 percent of individuals labeled as active chronic offenders were Black or Hispanic males (Macias, 2019; Smith, 2019). Following the

O.I.G.'s audit and public outcry, L.A.P.D. Chief Michael Moore announced the termination of Operation L.A.S.E.R. on April 10th, 2019 (Bhuiyan, 2021; Puente, 2019).

Overall, the combination of limited departmental transparency, racial and ethnic disparities in police stops, and the program's reliance on records from earlier police encounters negatively influenced the public's perception of the program (Bhuiyan, 2021; Lapowsky, 2018; Police Commission, 2019; Smith, 2019; Spolin, 2018).

Implications for Educational Outcomes

For students, exposure to proactive policing programs such as Operation L.A.S.E.R. could affect educational outcomes in various ways. For instance, if interventions reduce serious violent crimes around schools, students could experience fewer classroom disruptions, less stress, safer travel, better attendance, and improved concentration. In one study, Burdick-Will (2013) found that increases in violent crime at Chicago public schools reduced standardized test performance, with the results appearing to reflect cognitive stress and classroom disruption, which ultimately shows an association between violent crime and lower academic performance. In a separate study, Burdick-Will et al. (2019) found that Baltimore high-school students whose walking routes exposed them to more violent crime had higher rates of absenteeism. Given these findings, a minimally invasive policing program aimed at reducing violent crime could, in theory, increase academic performance by fostering safer neighborhood conditions.

However, the potential benefits of reduced crime must also be weighed against the possible educational consequences of increased police contact. For instance, Legewie and Fagan (2019) found that greater exposure to aggressive neighborhood policing reduced test performance among African American boys in New York City. Similarly, Gottlieb and Wilson (2019) found that arrests, police stops that did not result in arrest, and indirect exposure to police contact were each associated with lower educational achievement among adolescents. Thus, even if proactive policing strategies manage to reduce violent crime, increased contact with the police may introduce other forms of stress or disruption that could influence academic performance.

One potential way that increased exposure to police contact may influence academic performance is through school attendance. For example, Geller and Mark (2022) found that adolescents who reported experiencing police encounters missed more school than those who did not. Relatedly, Mark et al. (2022) found that recently arrested students experienced additional absences, which were largely attributable to suspensions and court appearances. Together, the findings imply that contact with the criminal justice system can reduce the time students spend in the classroom through both direct encounters and the institutional responses that follow police contact.

Meanwhile, other studies have found that the educational consequences of proactive policing strategies may accumulate over time and may not be distributed equally across students. Legewie and Cricco (2022), for instance, found that long-term exposure to neighborhood policing reduced high school graduation rates and contributed to racial and ethnic disparities in graduation. In Los Angeles, Ang (2021) found that lower grade-point averages, greater emotional disturbance, and lower rates of high-school completion and college enrollment among public high-school students followed exposure to nearby police killings. In that study, the effects were concentrated among Black and Hispanic students following police killings of other racial and ethnic minority individuals. Although police violence represents a more severe form of exposure than routine police presence within a L.A.S.E.R. Zone, the findings suggest that policing events can impact students even when they are not directly involved.

In essence, contemporary research presents mixed findings on the relationship between proactive policing and students' academic performance, consistent with a national review that finds less is known about how proactive policing affects communities than about its short-term effects on crime (National Academies of Sciences, Engineering, and Medicine, 2018). On the one hand, reductions in violence could improve the conditions under which students attend school and learn. On the other hand, increased police contact could increase stress, absenteeism, and institutional consequences that interfere with academic performance. Given these possibilities, the present study assesses whether active exposure to Operation L.A.S.E.R. was associated with school-level E.L.A. and Mathematics performance.

DATA

School Sample

The present chapter relies on a variety of annual, school-level datasets from the California Department of Education (C.D.E.) website. First, annual assessment records from the California High School Exit Examination (C.A.H.S.E.E.) were obtained and appended to create a single data file spanning 2010 to 2015. The data were then restricted to L.A.U.S.D. school-level records and restructured into a single school-year panel dataset. The process was then repeated with Smarter Balanced Assessment Consortium (S.B.A.C.) data files spanning 2015 to 2019. Once complete, the two data files were merged by unique school identifier and year to create a combined school-year-level dataset spanning 2010 to 2019. The combined dataset initially contained 228 schools appearing in one or both assessment files; however, after retaining only schools with assessment records every year from 2010 to 2019, a balanced panel of 131 schools was created.

The school-year assessment panel was then supplemented with demographic and institutional information. First, yearly enrollment records containing student demographics were obtained from the C.D.E. website. As before, the yearly files from 2010 to 2019 were appended, restructured to the school-year level, and merged with the assessment panel. Then, a separate 2024 C.D.E. dataset containing institutional information, such as school type, instructional level, mailing information, opening and closing dates, and geographic coordinates, was obtained and merged. After this process, the sample remained at 131 schools, or 1,310 school-year observations.

Once the school-year-level dataset spanning 2010 to 2019 was created and included assessment scores, demographic data, and institutional information, the next step was to refine the sample. Because the dataset was already limited to L.A.U.S.D. schools, the supplemental institutional information was used to restrict the sample by school type and instructional level. First, the sample was restricted to schools classified in the institutional file as “traditional.” The process eliminated other school types from sample selection, such as alternative, continuation, and special education schools, because their purposes and student populations differ from those of traditional schools, which could ultimately affect school-to-school comparisons. After removing these specialized variants, the sample included 66 schools. Lastly, six Elementary-High Combination schools and five charter schools were excluded to improve institutional comparability, resulting in a final sample of 55 traditional, non-charter L.A.U.S.D. high schools observed annually from 2010 to 2019, for a total of 550 school-year observations.

Spatial Data

In addition to the school-level datasets from the C.D.E. website, the present study incorporates a variety of spatial data files obtained from the City of Los Angeles GeoHub (City of Los Angeles, n.d.). First, data curation revealed that multiple school coordinates in the 2024 C.D.E. institutional dataset were inaccurate, placing schools several blocks from their actual locations. As a result, the present study incorporates an L.A.U.S.D. school point layer with corrected coordinates from the City of Los Angeles GeoHub website. Second, because a school's campus boundaries may overlap with a L.A.S.E.R. Zone even if the school's point location lies outside the zone, relying solely on the point layer could mistakenly classify a school as never-treated, despite part of its campus boundaries intersecting a zone. Thus, the present study also uses a school campus boundary shapefile. The final file obtained from the City of Los Angeles GeoHub was an L.A.U.S.D. high school attendance boundary shapefile, used to identify ever-treated and never-treated schools within the same catchment area.

Lastly, to identify treatment and comparison groups, the present study uses a unique L.A.S.E.R. Zone shapefile that includes zone start and end dates and geographic boundaries. L.A.S.E.R. Zone names, geography, and treatment dates were obtained through public records requests (L.A.P.D., 2018-2021).

METHODS

Assessment Outcomes Rationale

There are notable differences between C.A.H.S.E.E. and S.B.A.C. assessments. For instance, C.A.H.S.E.E. examinations were administered from 2001 to 2015 and were geared toward grade 10 students, whereas S.B.A.C. assessments were first administered in 2015 and include variations to assess students in grades 3 through 8 and grade 11. In addition, C.A.H.S.E.E. examinations were administered on paper, whereas S.B.A.C. exams rely on adaptive, computer-based models that adjust difficulty based on student performance (California

Department of Education, 2016). Most importantly, C.A.H.S.E.E. examinations carried substantially different consequences for students because passing the examination was generally required for graduation, and prior research found that this requirement disproportionately reduced graduation rates among low-achieving female and racial and ethnic minority students (Reardon & Kurlaender, 2009).

Given the differences between C.A.H.S.E.E. and S.B.A.C. assessments, online reports surfaced during the transition period claiming that the C.D.E. sought to discourage comparisons between S.B.A.C. results and scores from earlier state assessments because of differences in tested grade levels, academic standards, formats, and score scales (Fensterwald, 2015), though C.A.H.S.E.E. examinations were not explicitly mentioned. At present, the C.D.E. issues similar warnings on its official website, explaining that S.B.A.C. assessments are fundamentally different from earlier examinations (California Department of Education, n.d.).

While the assessments are not directly comparable in their raw states, Kurlaender and Martorell (2016) found that C.A.H.S.E.E. and S.B.A.C. showed similar patterns in student performance, despite differences in their designs and scoring scales. After converting scores from both assessments to standardized z-scores, the authors found substantial overlap in the distributions of E.L.A. and Mathematics. Additionally, the authors found that students who failed C.A.H.S.E.E. generally fell within the lower S.B.A.C. achievement levels, while those who passed C.A.H.S.E.E. tended to perform better on S.B.A.C. assessments.

Given that schools administered both the C.A.H.S.E.E. and S.B.A.C. examinations in 2015, the present study examines the correlation between the two examinations in E.L.A. and Mathematics at the school level, using only 2015 observations. The results show that schools that performed well on the C.A.H.S.E.E. generally performed well on the S.B.A.C., while those that underperformed on the C.A.H.S.E.E. also tended to underperform on the S.B.A.C. Thus, while direct comparisons of the two assessments are not appropriate, the 2015 correlation supports using predicted S.B.A.C. scale scores based on observed C.A.H.S.E.E. performance. For a scatterplot showing the correlation results for the full 55-school analytic sample, see Appendix Figure A1.

Assessment Outcomes Construction

To place scores from the two assessment periods on a common scale, the present chapter relies on 2015 assessment score data, as this was the only year when California schools administered both the C.A.H.S.E.E. and the S.B.A.C. First, observed 2015 S.B.A.C. mean scores for E.L.A. and Mathematics are regressed on corresponding C.A.H.S.E.E. mean scores. The estimated relationships are then used to predict S.B.A.C.-scale scores from observed C.A.H.S.E.E. scores for 2010 through 2014, while observed S.B.A.C. scores are retained for 2015 through 2019.

As a sensitivity check, the present study also converts each school's predicted and observed assessment scores into standardized z-scores relative to the 55-school sample in the corresponding year. This approach is helpful because standardized scores indicate each school's relative position within the corresponding subject-year distribution. When the fixed-effects models are repeated with standardized z-scores rather than predicted and observed scores, the resulting all-student estimates remain negative and not statistically significant in both subjects, consistent with the conclusion drawn from the two outcome scales. Because the results are consistent regardless of method, the present study adopts the predicted and observed S.B.A.C.-scale scores approach rather than standardized z-scores, as the former may be more intuitive for a general audience than changes expressed in standard deviation units.

In addition, the present study examines outcomes for an all-student group, as well as for subgroups, including Asian, Black, Hispanic, White, female, and male students. However, because California suppresses scores when too few students of a particular group are tested, analyses for certain subgroups may include fewer observations. Appendix Table A1 details the available prediction samples and school-year observations for each group.

Measuring School Exposure

To identify ever-treated and never-treated high schools, the present study uses school identifiers from the C.D.E. data and reviewed L.A.U.S.D. school-point and campus-boundary layers to locate schools. Once locations were verified, school campus boundaries were used, rather than a single map point, to determine exposure to Operation L.A.S.E.R., classifying schools as ever-treated when any part of their campus boundaries overlaps a dated L.A.S.E.R. Zone. The study also records the largest share of the campus that overlaps with a zone, the first and last zone dates, and the exposure duration.

For the annual analysis, a school is considered exposed in an academic year if its campus boundaries overlap with an L.A.S.E.R. Zone active at any point from July 1 of the preceding calendar year through June 30 of the assessment year, which aligns exposure with the academic-year structure of the outcome data. In addition, high-school attendance boundaries, also known as catchment areas, are used to identify rare cases in which a single catchment area contains both an ever-treated and a never-treated school. See Appendix Figure C4 for a map of the 55 L.A.U.S.D. schools and catchment area boundaries.

Student Demographics

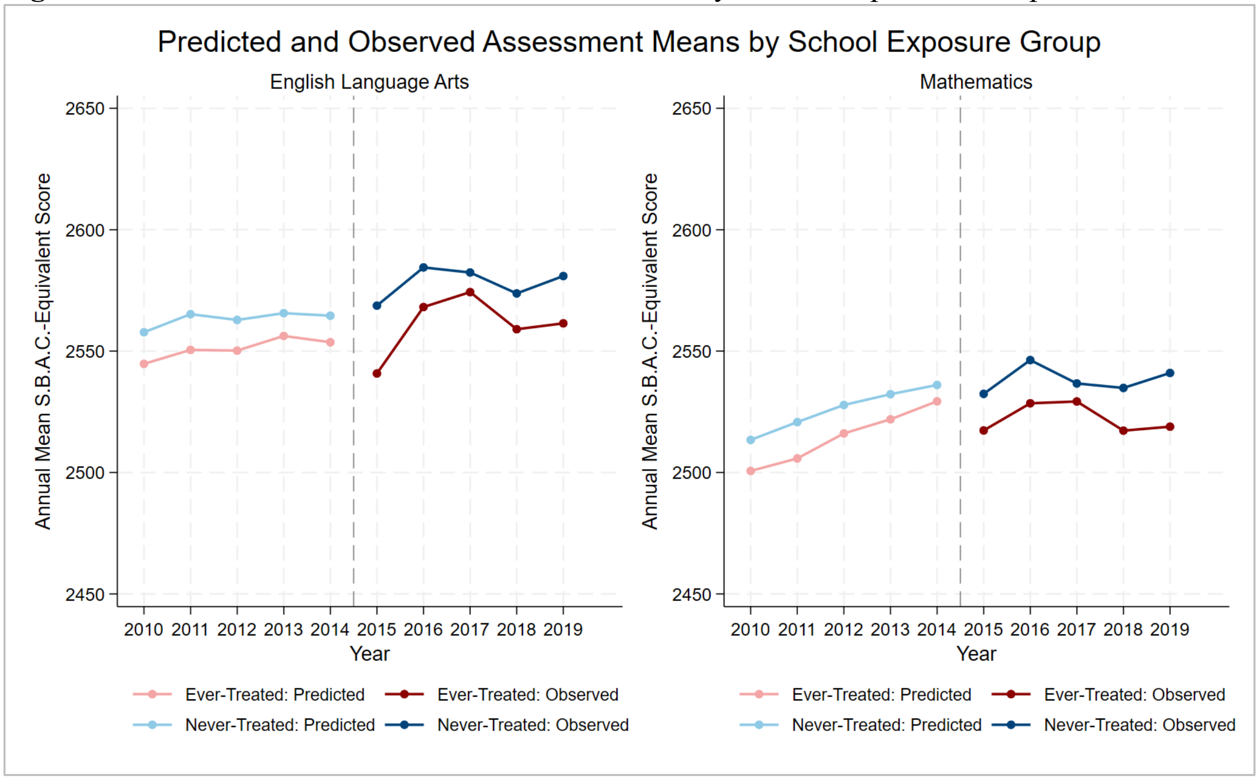
To determine racial and ethnic composition, Grade 10 and Grade 11 enrollment counts were combined within each school and academic year. For each racial or ethnic group, subgroup enrollment was divided by the school's combined Grade 10 and Grade 11 enrollment and multiplied by 100, allowing both grades to contribute in proportion to their enrollment. These school-level percentages were then averaged separately for ever-treated and never-treated schools, with each school contributing equally regardless of enrollment size. Thus, the resulting percentages reflect the composition of the 55 selected schools, the longitudinal and institutional sample restrictions, and mean school-level percentages, rather than a districtwide student-level percentage.

Analytic Approach

The present study compares changes in ever-treated and never-treated schools during years when Operation L.A.S.E.R. was active. To do so, school fixed effects are used to capture stable differences across schools, such as location, institutional history, and student demographics. Meanwhile, year fixed effects are used to capture changes shared across schools in a given year, such as statewide assessment conditions. In addition, the present study clusters standard errors at the school level to account for repeated observations within schools. Given that only seven schools were exposed to Operation L.A.S.E.R., statistical significance is assessed using wild-cluster bootstrap tests with 9,999 replications. According to Roodman et al. (2019), this approach is well suited for clustered data when the number of treated groups is small. However, the method cannot eliminate the uncertainty arising from the limited number of schools exposed to treatment.

RESULTS

Figure 1. Predicted and Observed Assessment Means by School Exposure Group



Notes: Predicted S.B.A.C.-equivalent scores cover 2010-2014; observed scores cover 2015-2019; Ever-Treated schools (n = 7); Never-Treated schools (n = 48).

Table 1. Traditional L.A.U.S.D. High Schools That Intersected L.A.S.E.R. Zones

Ever-Treated Schools	First Treated	Last Treated	Max. Overlap
Thomas Jefferson Senior High	2011	2012	85.38%
Manual Arts Senior High	2015	2019	87.35%
Jordan High	2016	2019	43.36%
Hollywood Senior High	2017	2019	25.62%
Fairfax Senior High	2018	2019	100.00%
Abraham Lincoln Senior High	2018	2019	0.35%
Theodore Roosevelt Senior High	2018	2019	100.00%

Notes: Ever-Treated schools (n = 7); First Treated and Last Treated identify calendar years and do not imply uninterrupted exposure; Maximum Overlap is the largest campus-area percentage intersecting a L.A.S.E.R. Zone.

Table 2. Ever-Treated vs. Never-Treated School Racial and Ethnic Composition

Academic Year	Race & Ethnicity	Ever-Treated	Never-Treated
2009-10	White	2.39%	5.31%
	Black	10.94%	7.92%
	A.I./A.N.	0.34%	0.35%
	Asian	6.33%	2.88%
	Filipino	0.98%	3.34%
	Pacific Islander	0.13%	0.41%
	Two or More Races	0.00%	0.01%
	Not Reported	0.54%	0.40%
	Hispanic	78.35%	79.38%
		n = 7	**n = 48**
2018-19	White	2.95%	5.69%
	Black	10.23%	7.19%
	A.I./A.N.	0.12%	0.20%
	Asian	6.08%	2.48%
	Filipino	1.07%	2.89%
	Pacific Islander	0.05%	0.31%
	Two or More Races	0.40%	0.67%
	Not Reported	0.14%	0.05%
	Hispanic	78.96%	80.52%
		n = 7	**n = 48**

Notes: Values are mean school-level percentages for grades 10 and 11; grade counts are pooled within schools; schools contribute equally to group means; A.I./A.N. = American Indian or Alaska Native.

Table 3. All-Student and Student-Subgroup Fixed-Effects Estimates of Active-Year Exposure

Subject	Group	Observations	Coefficient	S.E.	p Value
E.L.A.	All Students	550	-7.307	10.033	0.632
	White	207	-4.516	6.025	0.473
	Black	274	-10.589	13.588	0.520
	Asian	188	-9.648	9.996	0.378
	Hispanic	550	-4.769	9.623	0.770
	Male	550	-13.680	9.565	0.255
	Female	550	-1.652	10.225	0.892
Mathematics	All Students	550	-6.860	6.030	0.402
	White	203	-4.403	5.568	0.463
	Black	275	-13.550	10.018	0.316
	Asian	185	7.094	13.703	0.636
	Hispanic	550	-5.548	6.141	0.533
	Male	550	-14.550	6.624	0.069
	Female	550	-0.006	5.694	0.999

Notes: n = school-year observations; each row is a separate model with school and year fixed effects using predicted and observed scores; S.E.s are school-clustered; p values use 9,999 wild-bootstrap reps; Filipino and Pacific Islander categories are omitted due to insufficient data.

Table 4. All-Student Predicted and Observed Mean Scores for Same-Catchment School Pairs

Subject	School	Group	Predicted Mean	Observed Mean
E.L.A.	Theodore Roosevelt Senior High	Ever-Treated	2536.539	2550.360
	Felicitas and Gonzalo Mendez High	Never-Treated	2549.413	2584.020
	Thomas Jefferson Senior High	Ever-Treated	2526.481	2528.520
	Santee Education Complex	Never-Treated	2535.734	2583.420
	Abraham Lincoln Senior High	Ever-Treated	2559.471	2577.900
	Woodrow Wilson Senior High	Never-Treated	2549.413	2573.460
Mathematics	Theodore Roosevelt Senior High	Ever-Treated	2502.582	2495.060
	Felicitas and Gonzalo Mendez High	Never-Treated	2507.997	2550.300
	Thomas Jefferson Senior High	Ever-Treated	2487.689	2491.080
	Santee Education Complex	Never-Treated	2496.715	2524.220
	Abraham Lincoln Senior High	Ever-Treated	2528.306	2561.320
	Woodrow Wilson Senior High	Never-Treated	2515.669	2527.760

Notes: Ever-Treated schools (n = 3); Never-Treated schools (n = 3); each treated school precedes its same-catchment counterpart; predictions cover 2009-10 to 2013-14; observed scores cover 2014-15 to 2018-19.

DISCUSSION

Figure 1 presents annual mean S.B.A.C.-equivalent scores for ever-treated and never-treated high schools, distinguishing predicted scores for 2010 through 2014 from observed scores for 2015 through 2019. The figure suggests that, across the decade, ever-treated schools had lower mean scores in both E.L.A. and Mathematics than never-treated schools. However, because lower mean scores were already present in 2010, before any study school was treated, these findings do not provide evidence that exposure to the intervention caused the differences between the two groups.

Meanwhile, Table 1 identifies the seven ever-treated schools and explains why campus boundaries provide a more informative exposure measure than a single school point. For example, Hollywood Senior High's school point falls outside a L.A.S.E.R. Zone, even though 25.62 percent of the campus overlapped one. A similar situation occurs with Abraham Lincoln Senior High's school point; however, in this case, only 0.35 percent of the campus overlapped an active L.A.S.E.R. Zone. Despite the less than one percent overlap in the latter case, it remains plausible that proximity to an active L.A.S.E.R. Zone may have resulted in exposure to increased levels of policing. Because any amount of overlap was observed in the latter case, the school is classified as ever-treated.

Table 2 provides additional context on differences between ever-treated and never-treated schools by comparing their student racial and ethnic composition. Hispanic students represented the largest share of enrollment in both groups, followed by Black students. In 2009-10, the mean school-level Hispanic enrollment share was 78.35 percent at ever-treated schools and 79.38 percent at never-treated schools, while Black students accounted for 10.94 and 7.92 percent, respectively. By 2018-19, Hispanic enrollment was 78.96 percent at ever-treated schools and 80.52 percent at never-treated schools, while the corresponding Black enrollment shares were 10.23 and 7.19 percent. Importantly, these values are based on 55 traditional, non-charter high schools with complete longitudinal assessment records; thus, they may differ from the greater L.A.U.S.D. student population.

After accounting for school and year fixed effects, Table 3 estimates that E.L.A. scores were 7.31 points lower and Mathematics scores were 6.86 points lower in active-exposure school years than in non-active school years. The corresponding wild-bootstrap p values were .632 and .402, and neither estimate reached statistical significance at conventional levels. Appendix

Figure B1 presents standardized trends for ever-treated and never-treated schools as a descriptive sensitivity check. In the subgroup models, Table 3 shows that most student subgroup coefficients were negative, though none reached statistical significance at the .05 level using wild-bootstrap tests. In that regard, the Mathematics estimate for male students was closest (-14.55 points; $p = .069$). The sole exception to the negative coefficients was Asian students in Mathematics, for whom the estimated coefficient was positive (7.09 points; $p = .636$).

For same-catchment comparisons, three of the seven treated schools had a never-treated counterpart within the same catchment area: Thomas Jefferson Senior High, Abraham Lincoln Senior High, and Theodore Roosevelt Senior High. Their never-treated counterparts are Santee Education Complex, Woodrow Wilson Senior High, and Felicitas and Gonzalo Mendez High, respectively.

As shown in Table 4, the patterns vary across the three pairs. Jefferson and Roosevelt have lower predicted and observed averages than their counterparts in both subjects, whereas Lincoln has higher averages than Wilson in both periods and subjects. However, with only three pairs, the comparisons are too limited to support a separate analysis of program effects. Appendix C provides supplemental figures for within-catchment comparisons. Appendix Figure C1 displays predicted and observed mean-score trends for the two groups. Appendix Figure C2 presents the same comparisons using standardized scores, and Appendix Figure C3 provides a visual example of an ever-treated school and a never-treated school within the same catchment area.

LIMITATIONS

The most notable limitation of the present study is that the state of California changed assessment systems during the middle of the study period. The 2015 relationship between C.A.H.S.E.E. and S.B.A.C. scores provides a transparent basis for predicting scores in earlier years, but it does not make the two assessments equivalent or establish that the same school-level relationship applied in previous years. Relatedly, the exams differ in grade level, content, format, and academic standards. In addition, because the earlier scores are predicted, the final models do not account for all of the uncertainty introduced through the prediction process. Thus, the combined outcome should be interpreted as an approximation of changes over time.

Second, the study's panel and institutional restrictions limit the generalizability of the findings. Specifically, schools were required to appear in the assessment data every year from 2010 through 2019, reducing the initial merged assessment sample from 228 to 131 schools. This requirement excluded schools without complete assessment coverage, including those whose opening, closure, or reporting histories prevented them from appearing in every year.

The sample was further restricted to schools classified as traditional high schools using the January 2024 C.D.E. institution file. This restriction excluded specialized school types, six Elementary-High Combination schools, and five schools with "Charter" in their institutional names. While the January 2024 C.D.E. institution file helps identify the population of interest and improve institutional comparability across schools, limiting the sample to 55 schools means the findings may apply only to a subset of traditional, non-charter L.A.U.S.D. high schools rather than to the entire population of L.A.U.S.D. schools or students.

Third, exposure to Operation L.A.S.E.R. was not randomly assigned, as the Los Angeles S.P.I. team selected L.A.S.E.R. Zones in response to recorded violence, calls for service, gang activity, and departmental priorities. Although school and year fixed effects capture stable differences across schools and changes shared across schools in a given year, they cannot account for all local conditions that changed over time and may have been related to both Operation L.A.S.E.R. exposure and academic performance. This limitation is especially important given that only seven schools were ever treated, and four of those schools did not have a never-treated counterpart within the same catchment area. Wild-cluster bootstrap tests were therefore used to assess statistical significance given the small number of treated schools, although they cannot fully address the limits created by the small sample.

Lastly, the state suppresses student-subgroup assessment scores when too few students are tested, meaning that the available outcomes for Asian, Filipino, Black, and White students represent different sets of schools and years. Data availability was especially limited for Pacific Islander students, as each 2015 subject-specific prediction model included only two schools, and no ever-treated school had a non-missing Pacific Islander outcome. Consequently, no treatment estimate was provided.

CONCLUSION

As part of Operation L.A.S.E.R., the L.A.P.D. concentrated efforts on targeting specific people and places it identified as associated with crime. However, research has shown that the consequences of proactive policing strategies can extend beyond crime, affecting the conditions under which students attend school and learn. Given that prior studies have identified both positive and negative relationships between proactive policing and academic performance, the present study examined whether exposure to Operation L.A.S.E.R. was associated with school-level E.L.A. and Mathematics performance.

After accounting for school and year fixed effects, the findings indicate that E.L.A. scores were estimated to be 7.31 points lower and Mathematics scores were estimated to be 6.86 points lower in active-exposure school years than in non-active school years. In both subjects, neither estimate reached conventional levels of statistical significance. In essence, the findings do not support the conclusion that Operation L.A.S.E.R. negatively impacted academic performance, nor do they support the conclusion that the intervention had no educational consequences or provide evidence of an academic benefit.

In addition, these findings should also be understood in relation to the schools included in the analysis. The final sample consists of 55 traditional, non-charter L.A.U.S.D. high schools with complete longitudinal assessment records and may not adequately represent all L.A.U.S.D. schools or students. Thus, rather than providing a districtwide estimate of the educational consequences of Operation L.A.S.E.R., the findings provide evidence of its relationship with academic performance among the traditional high schools that could be observed consistently throughout the study period.

To conclude, the present study emphasizes the importance of considering outcomes beyond crime when evaluating proactive policing strategies. Even when the estimated educational effects are uncertain, examining whether policing strategies alter the environments around schools can provide a more complete understanding of how these interventions may affect community well-being, if at all.

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APPENDIX A

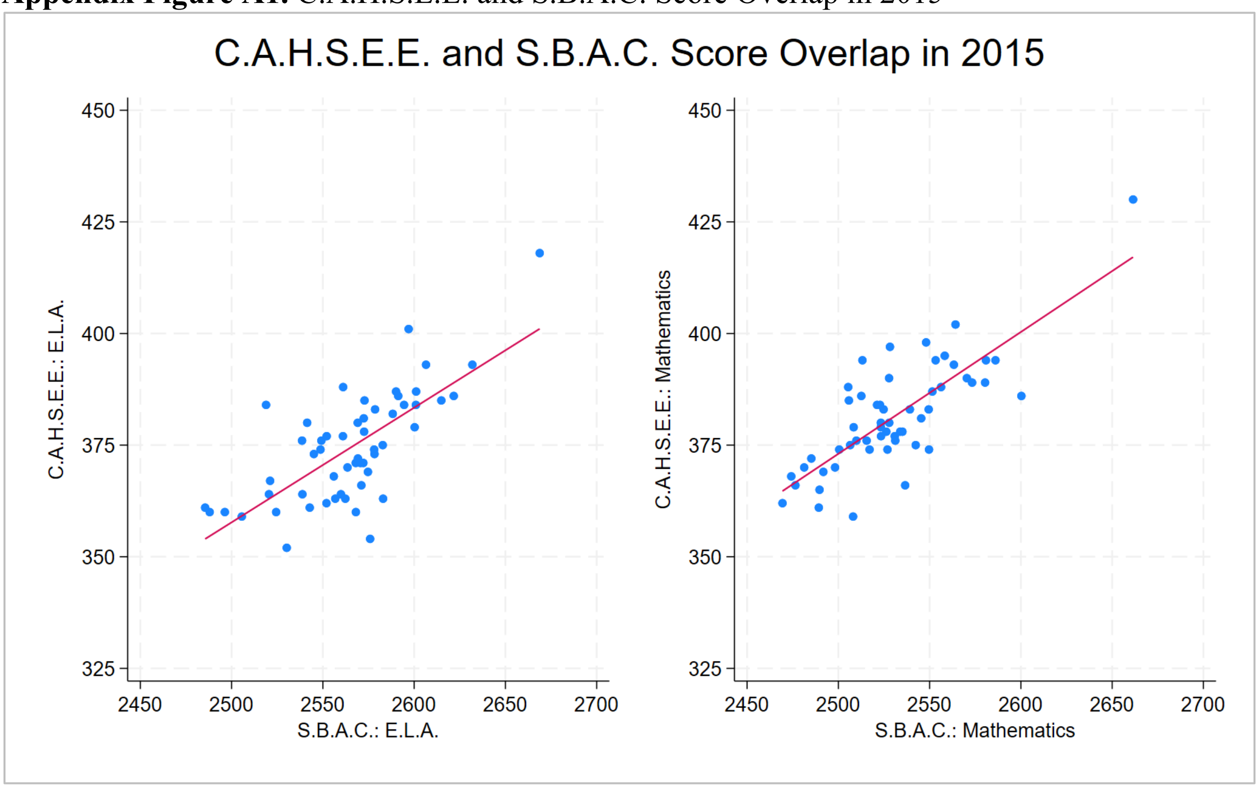
Assessment Score Construction and Data Availability

Appendix Table A1. Score-Prediction Samples and Ever-Treated School Outcome Availability

Student Group	2015 E.L.A. Prediction Schools	2015 Math Prediction Schools	Ever-Treated E.L.A. School-Years	Ever-Treated Math School-Years
All Students	55	55	70	70
White	17	15	21	21
Black	23	25	47	47
Asian	16	15	26	26
Filipino	14	14	15	15
Pacific Islander	2	2	-	-
Hispanic	55	55	70	70
Male	55	55	70	70
Female	55	55	70	70

Notes: Ever-Treated schools (n = 7) provide at most 70 school-years per subject (7 schools × 10 years); smaller counts reflect unavailable scores; no Pacific Islander outcome was constructed because each 2015 prediction sample included only two schools.

Appendix Figure A1. C.A.H.S.E.E. and S.B.A.C. Score Overlap in 2015

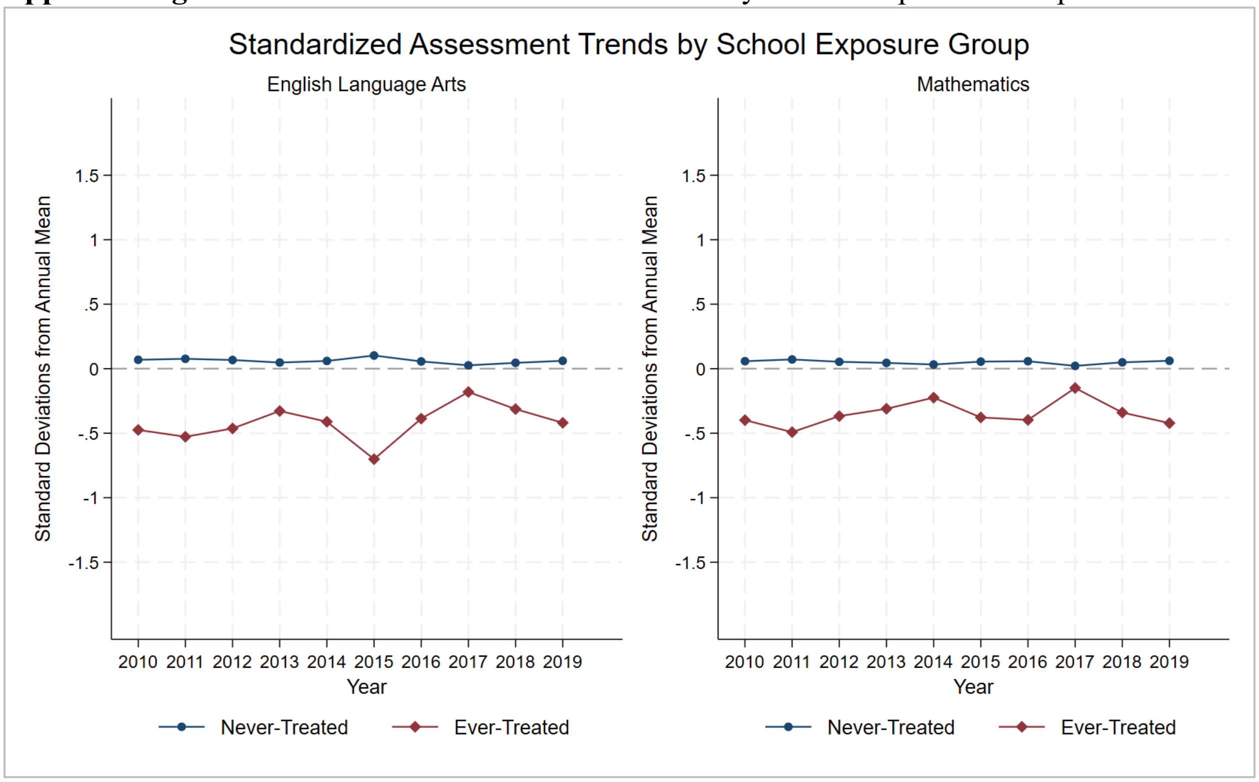


Notes: Full sample (n = 55) with non-missing 2015 scores for both assessments and subjects; E.L.A.: r = .719, p < .001; Mathematics: r = .785, p < .001.

APPENDIX B

Supplemental Analyses

Appendix Figure B1. Standardized Assessment Trends by School Exposure Group

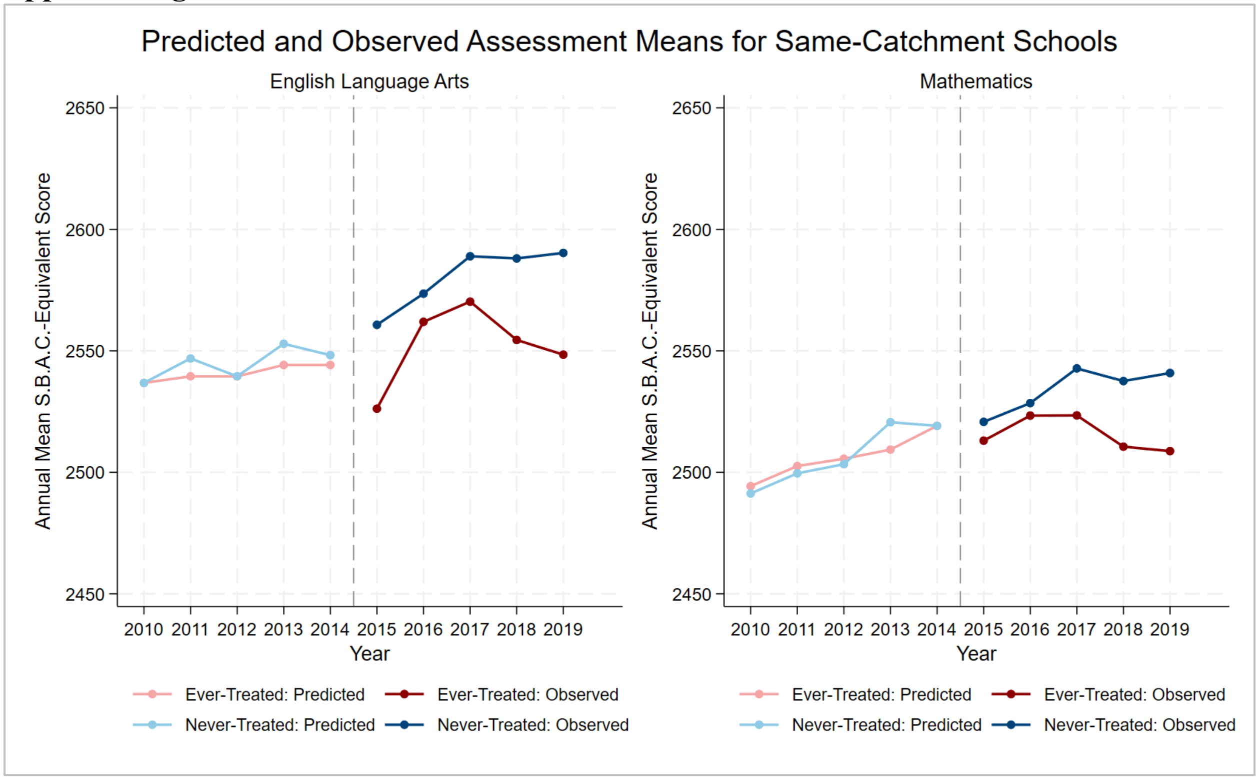


Notes: Ever-Treated schools (n = 7); Never-Treated schools (n = 48); scores are standardized within each subject-year using the full-sample distribution.

APPENDIX C

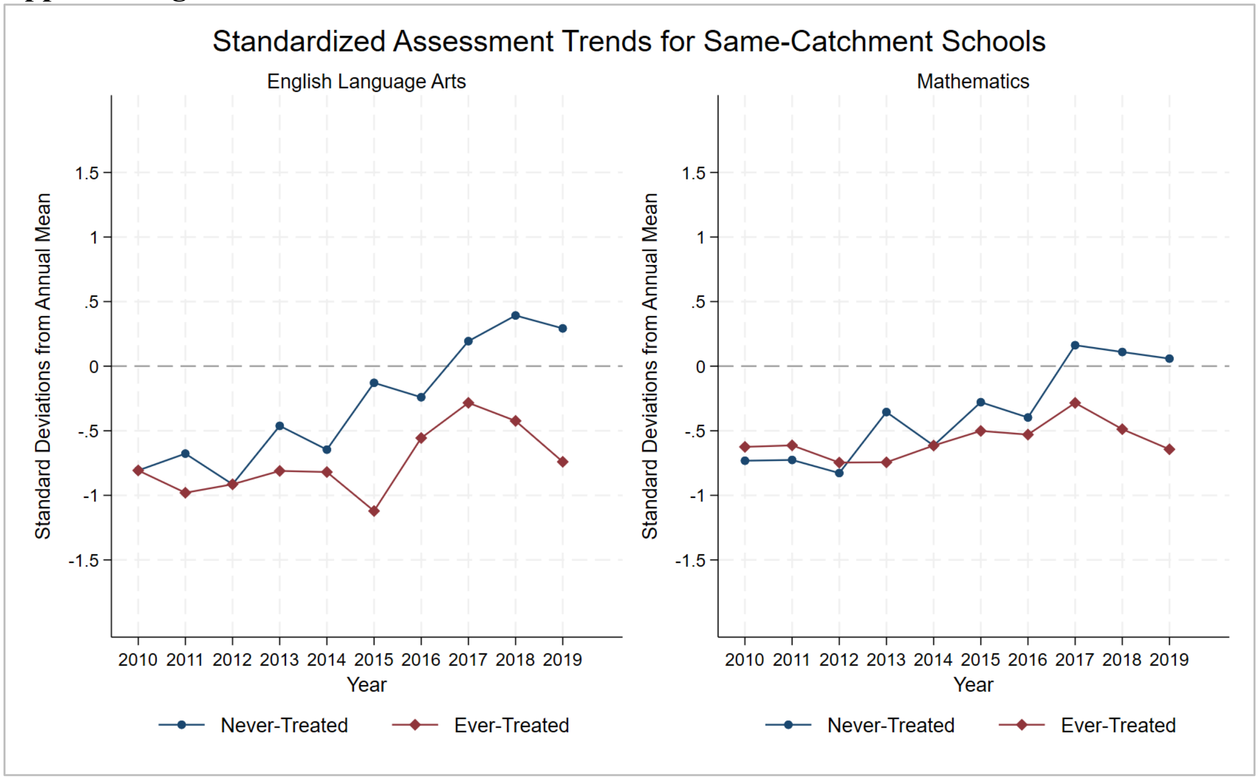
Same-Catchment Descriptive Comparisons

Appendix Figure C1. Predicted and Observed Means for Same-Catchment Schools



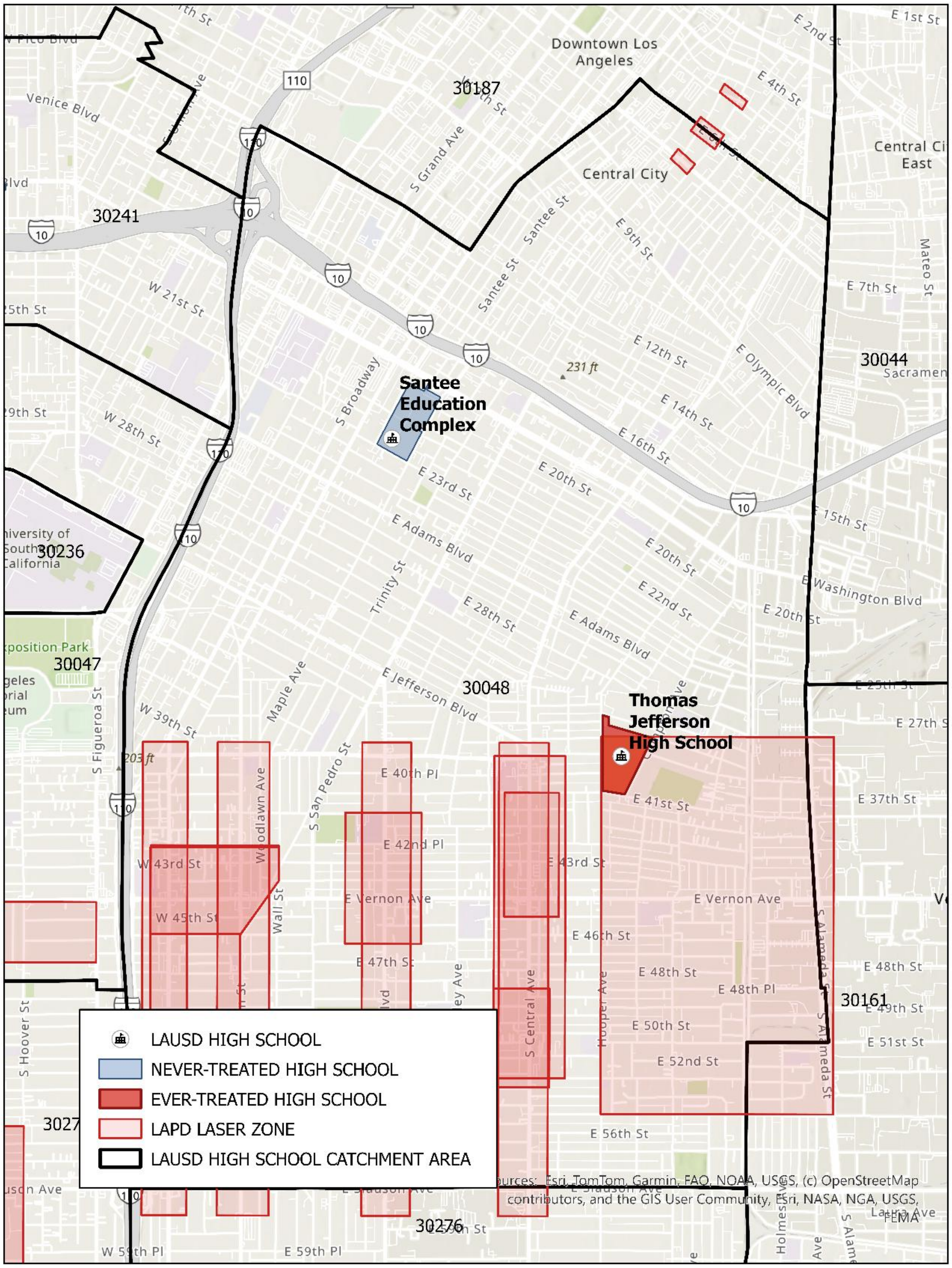
Notes: Ever-Treated schools (n = 3); Never-Treated schools (n = 3); predicted scores cover 2010-2014; observed scores cover 2015-2019.

Appendix Figure C2. Standardized Assessment Trends for Same-Catchment Schools

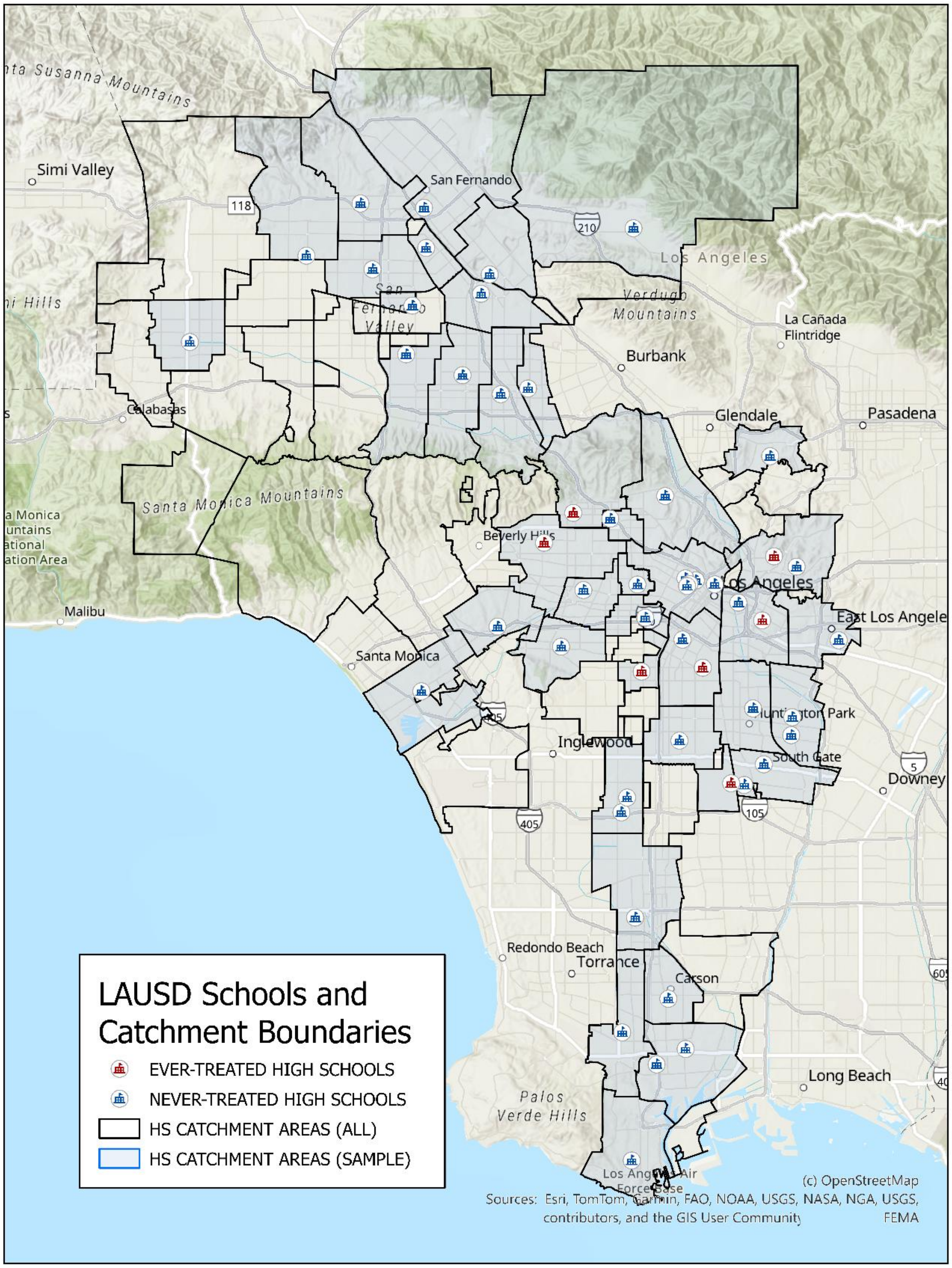


Notes: Ever-Treated schools (n = 3); Never-Treated schools (n = 3); scores are standardized within each subject-year using the full-sample distribution.

Appendix Figure C3. Catchment Area with Ever-Treated and Never-Treated High Schools



Appendix Figure C4. L.A.U.S.D. Schools and Catchment Boundaries



APPENDIX D

Data Curation

Assessment and Enrollment Records

Chapter 2 combines annual school-level assessment and enrollment data from the California Department of Education (C.D.E.). California High School Exit Examination (C.A.H.S.E.E.) records cover 2010 through 2015 and were restricted to Grade 10, combined-administration, school-level records for the Los Angeles Unified School District (L.A.U.S.D.). The files provide school identifiers, year, administration, grade, student subgroup, number tested, and mean scale scores in English Language Arts (E.L.A.) and Mathematics. Meanwhile, Smarter Balanced Assessment Consortium (S.B.A.C.) records cover 2015 through 2019 and were restricted to Grade 11 school-level E.L.A. and Mathematics results.

Because both assessment systems also report scores for student subgroups, the final files retain outcomes for all students and for Asian, Black, Hispanic, White, female, and male students. However, Filipino and Pacific Islander treatment estimates were not reported because too few usable scores were available for these groups.

In addition to the assessment records, annual C.D.E. enrollment files from 2010 through 2019 provide the 14-digit county-district-school identifier and Grade 10 and Grade 11 enrollment by racial or ethnic category. Counts and shares were constructed for American Indian or Alaska Native, Asian, Pacific Islander, Filipino, Hispanic, Black, White, multiracial, and not-reported students. Because C.A.H.S.E.E. measures Grade 10 performance while S.B.A.C. measures Grade 11 performance, the Grade 10 and Grade 11 enrollment counts were combined within each school-year to describe the racial and ethnic composition of the grades represented by the two assessment systems.

Lastly, a January 2024 California public-school directory provides contemporary information on school name, school type, instructional level, and geographic coordinates. These measures were used to identify the schools that met the study criteria and to connect the school records with the geographic data. All assessment, enrollment, and institutional records were linked using the 14-digit county-district-school identifier.

Sample Construction

Combining the C.A.H.S.E.E. and S.B.A.C. files produced 228 schools appearing in one or both assessment systems. Because Chapter 2 follows changes within the same schools across the full study period, each school was required to have assessment records in every year from 2010 through 2019. Applying this requirement produced a balanced panel of 131 schools. After the enrollment and institutional records were merged, the sample remained at 131 schools and 1,310 school-year observations.

Additional restrictions were then applied to identify the traditional L.A.U.S.D. high schools examined in Chapter 2. Restricting the sample to schools classified as traditional left 66 schools. Six elementary-high combination institutions and five remaining charter schools were then excluded so that the final sample represented traditional, non-charter high schools with comparable instructional settings. The resulting sample contains 55 L.A.U.S.D. high schools. Because each school is observed for 10 years, the final panel contains 550 school-year observations.

Assessment Outcome Construction

Because the C.A.H.S.E.E. and S.B.A.C. differ in grade level, content, format, academic standards, and scoring scale, their raw mean scores could not be combined directly into a single outcome series. Instead, the shared 2015 administration was used to estimate the school-level relationship between the two assessments separately for each subject and available subgroup. These relationships were then used to predict S.B.A.C.-scale E.L.A. and Mathematics scores from observed C.A.H.S.E.E. performance for 2010 through 2014. From 2015 through 2019, the analysis retains the observed S.B.A.C. mean scores. In doing so, the earlier C.A.H.S.E.E. years and later S.B.A.C. years are placed on a common scale while preserving the observed S.B.A.C. results once they became available. The resulting subject-specific series serve as the main academic outcomes.

As a sensitivity check, the predicted and observed all-student scores were also standardized within each subject-year using the 55-school sample. Doing so places school performance in standard-deviation units relative to the other schools observed in the same subject and year.

School Geography and L.A.S.E.R. Exposure

To determine school exposure, the geographic workflow combines C.D.E. school identifiers and coordinates with reviewed L.A.U.S.D. school points, campus boundaries, high-school attendance boundaries, and dated L.A.S.E.R. Zone polygons. The school points, campus boundaries, and attendance boundaries were obtained through City of Los Angeles GeoHub sources. Meanwhile, L.A.S.E.R. Zone boundaries and implementation dates were reconstructed from records obtained through public records requests. When the reviewed L.A.U.S.D. location more accurately represented a school than the C.D.E. directory coordinates, the L.A.U.S.D. location was retained.

Rather than classify exposure using a single school point, the main treatment measure uses the reviewed campus boundary so that exposure reflects the physical area occupied by the school. A school was classified as ever treated when any positive portion of its campus overlapped a dated L.A.S.E.R. Zone. Boundary contact without positive area overlap did not count as exposure. Under this definition, seven of the 55 schools were classified as ever treated. In addition, the maximum share of each campus overlapping a L.A.S.E.R. Zone was retained to describe how much of the school property fell within a treated location.

Annual treatment status was then constructed using each L.A.S.E.R. Zone's separate start and end dates. To align treatment with the school calendar, a school-year was classified as exposed when an overlapping zone was active at any point from July 1 of the preceding calendar year through June 30 of the assessment year. Because L.A.S.E.R. Zones could stop and later restart, separate treatment intervals were preserved rather than treating the period between the first and last treatment dates as continuous exposure. The resulting measures identify active school-year exposure, ever-treated schools, maximum campus overlap, and the first and last treatment dates.

Lastly, high-school attendance boundaries were used to identify cases in which an ever-treated school and a never-treated school were located within the same attendance boundary. The purpose of these matches was to provide descriptive comparisons between schools serving geographically similar areas without changing the campus-based treatment definition. Three of the seven ever-treated schools had a never-treated counterpart within the same boundary, providing the three same-catchment comparisons reported in Chapter 2. Thus, attendance-boundary location provides geographic context but does not determine whether a school is classified as exposed.

CHAPTER 3

The Housing-Market Effects of Proactive Policing Exposure:

Evidence from Los Angeles Home Sales Prices

ABSTRACT

The present study examines the collateral consequences of proactive policing programs on neighborhood desirability, using the Los Angeles Police Department (L.A.P.D.)'s Operation L.A.S.E.R. as a case study. In 2011, the L.A.P.D., in collaboration with Justice & Security Strategies, Inc. (J.S.S.), implemented the Los Angeles Strategic Extraction and Restoration Program, otherwise known as Operation L.A.S.E.R., in Los Angeles, California. Using a combination of geospatial analyses and a tract-week panel of 2010 census tracts, the present chapter investigates who was most exposed to the intervention and whether that exposure was associated with notable changes in officer presence and single-family residential home sale prices. Findings reveal that Operation L.A.S.E.R. was implemented in predominantly renter-occupied, Hispanic-majority neighborhoods with mean home sale prices below the citywide average, that pooled officer-presence estimates were positive, though not statistically significant at conventional levels, and that weeks under active treatment were associated with higher logged mean home sale prices after controlling for housing characteristics and the aggregate predicted buyer- and seller-composition measures. Meanwhile, dynamic treatment-timing models suggest that the intervention was not associated with immediate or short-term changes in logged mean home sale prices, but was instead associated with small gains over time. The overall findings suggest that, even after adjusting for inflation, Operation L.A.S.E.R. exposure was associated with gradual increases in single-family residential home sale prices in marginalized communities. However, because renters dominated the treated neighborhoods, these gains risked driving up costs for residents subjected to targeted proactive-policing surveillance while failing to increase access to wealth.

Keywords: proactive policing; neighborhood desirability; neighborhood effects

INTRODUCTION

Since Wilson and Kelling's (1982) publication of "Broken Windows: The Police and Neighborhood Safety" in *The Atlantic*, scholars have extensively debated the collateral consequences of proactive policing strategies, including hot spot policing, broken windows policing, and stop-question-frisk (S.Q.F.). While some studies suggest that increasing police presence, whether by expanding the total number of officers on the streets or concentrating existing resources in high-crime areas, can help mitigate crime (Uchida et al., 2012a, 2012b) and improve perceptions of neighborhood safety (Wilson & Kelling, 1982), scholars also warn that these approaches often result in unsolicited police encounters for individuals who might not otherwise come into contact with the police. These encounters risk criminalizing marginalized communities by leading to more detentions, citations, arrests, or the creation of criminal records for individuals without any previous criminal history.

In addition to the increased risk of exposure to the criminal legal system, research suggests that how officers and their departments operate can impact housing markets and neighborhood desirability, as perceptions of neighborhood safety, over-surveillance, and the designation of an area as a crime "hot spot" can directly or indirectly impact residential mobility and property values (Autor et al., 2017; Bell, 2020a; Buonanno et al., 2013; Ceccato & Wilhelmsson, 2020; Owens et al., 2021). Given the potential direct and indirect impacts of proactive policing strategies, the present study asks, "Was Operation L.A.S.E.R. exposure associated with notable changes in neighborhood desirability, as measured by logged mean home sale prices in Los Angeles, California?"

Policy Background

Operation L.A.S.E.R. began as part of the Bureau of Justice Assistance (B.J.A.)'s Smart Policing Initiative (S.P.I.), a federal program that sought to reduce crime nationwide by funding selected partnerships between law enforcement agencies and research collaborators using innovative, data-driven policing strategies, which the B.J.A. often referred to as "smart policing." In Los Angeles, California, the initiative funded a collaboration between the Los Angeles Police Department (L.A.P.D.) and its research partner, Justice & Security Strategies, Inc. (J.S.S.). Together, they formed the Los Angeles S.P.I. team and developed Operation L.A.S.E.R. to address persistent violent crime in the city.

During the planning stages, the Los Angeles S.P.I. team organized its work around Eck and Spelman's (1987) S.A.R.A. model, a problem-solving policing framework that addresses crime through four elements: scanning, analysis, response, and assessment. Focusing on the scanning and analysis stages, the Los Angeles S.P.I. team reviewed historical crime data and identified gun- and gang-related violence as a major public safety problem in Los Angeles, California. The Los Angeles S.P.I. team then selected the Newton Division as the pilot site because it consistently ranked among the divisions with the highest levels of shootings and gun-related crime (Uchida et al., 2012a, 2012b).

Operation L.A.S.E.R. distinguished itself from other interventions that simply concentrated patrols within crime hot spots by combining place-based and person-based strategies. Specifically, the team used geographic information systems (G.I.S.) to identify crime hot spot corridors, which became known as L.A.S.E.R. Zones. At the same time, the program used field interview (F.I.) cards, arrest records, criminal history information, and Palantir software to create chronic offender bulletins identifying people the department viewed as high-risk or repeat offenders. Patrol and specialized units were then directed toward the identified L.A.S.E.R. Zones and individuals (Uchida et al., 2012a, 2012b). After early evaluations deemed the pilot successful, the L.A.P.D. expanded Operation L.A.S.E.R. to additional divisions. By early 2019, the program had achieved citywide coverage, with at least one active L.A.S.E.R. Zone in each of the L.A.P.D.'s 21 Divisions (Los Angeles Police Department, 2018-2021).

Despite early reports of crime reductions within treated locations (Uchida et al., 2012a, 2012b), Operation L.A.S.E.R. drew widespread criticism from civil rights organizations and community members who argued that the program resulted in over-surveillance of Black and Latino neighborhoods (Stop L.A.P.D. Spying Coalition, 2018, 2019a, 2019b). Thus, concerns about racial profiling, a lack of departmental transparency, and data-driven targeting led to an audit by the Office of the Inspector General (O.I.G.), which identified significant flaws in program oversight and chronic offender classification (Puente, 2019; Smith, 2019). Due to years of public pressure, the inconsistencies in operations reported by the O.I.G. audit, and feedback from meetings with the L.A.P.D.'s civilian oversight panel, the L.A.P.D. officially discontinued Operation L.A.S.E.R. in April 2019 (Puente, 2019).

LITERATURE REVIEW

Perceptions of Policing and Neighborhood Desirability

Across the United States, law enforcement agencies and other government entities or officials frequently advocate for proactive policing measures to reduce crime and enhance public safety. Arguments in favor of proactive policing strategies are that these tactics can lead to safer neighborhood conditions, attract investment, and increase property values over time. In recent years, studies have found evidence to support these claims. For instance, Bell (2020a) notes that a visible police presence can instill a sense of security for some residents, particularly among those who view the absence of guardianship as a barrier to neighborhood stability. However, the study, along with many others, also warns of discrepancies in public perceptions of law enforcement, often due to variation in how officers interact with residents from different racial and socioeconomic backgrounds.

Specifically, it is well-documented that perceptions of proactive policing vary across racial and socioeconomic lines. Bell (2020a), for instance, shows that while some White homebuyers interpret a visible police presence as reassuring, many of their Black counterparts perceive it as threatening and stigmatizing, especially in neighborhoods subject to over-surveillance. In an earlier study, Bell (2016) found that residents of disadvantaged neighborhoods commonly hold mixed views of law enforcement. On the one hand, individuals from poverty-stricken neighborhoods with a high police presence may depend on the police for protection; on the other hand, frequent and aggressive encounters create deep-seated distrust.

In terms of neighborhood desirability, strained police-community relations can erode social bonds and create feelings of marginalization among residents, influencing their perceptions of and interactions with law enforcement (Tyler, 2006). These personal experiences and perceptions often determine whether residents and potential homebuyers feel safe or policed, ultimately shaping their decisions to stay, move, or invest in a given neighborhood.

Impact of Crime Perceptions and Enforcement on Property Values

In recent years, criminological literature on neighborhoods and crime has found that law enforcement operations and perceptions of crime can influence property values in various ways. For instance, one study found that declining crime rates were associated with rising property values in New York City, New York (Autor et al., 2017), while another study found that even a slight increase in perceived crime was sufficient to negatively affect property values in Barcelona, Spain (Buonanno et al., 2013). Meanwhile, a study of Sweden's Stockholm metropolitan region found that observed crime rates had a marginal impact on single-family home sale prices; however, when examining the influence of distance from a single-family home to a designated crime hot spot, the study found that greater distance from the labeled area was associated with higher home values (Ceccato & Wilhelmsson, 2020), suggesting that the stigma associated with the label can impact property values more than recorded crime itself. Similarly, Owens et al. (2021) examined the impact of civil gang injunctions in Southern California, finding that although crime decreased following the intervention, the aggressive enforcement ultimately harmed property values. Overall, the growing body of literature on neighborhoods and crime suggests that as neighborhoods become safer, prospective homebuyers and renters may be more willing to invest in them; however, when neighborhoods are stigmatized, perceived as unsafe, or over-surveilled, the consequences are often reflected in the housing market.

Long-Term Poverty and Residential Mobility

In addition, scholars note that social institutions, such as the criminal legal system, often intersect with patterns of concentrated poverty and residential mobility. For instance, Wilson (2012) argues that neighborhood decline often leads to outmigration. In predominantly Black communities, the outmigration of middle-class Black families often leaves behind concentrated poverty, and the individuals who remain bear the greatest impacts of neighborhood decline, including stigmatization, de-urbanization, limited job opportunities, decreased property values, increased crime rates, and higher poverty levels. These conditions, in turn, increase policing efforts and reinforce the cycle in those communities.

Relatedly, Sharkey (2013) shows that such disadvantages often create lasting hardships that span multiple generations, leaving individuals susceptible to conditions similar to those their ancestors experienced. Therefore, while proactive policing strategies can reduce crime rates, they can also contribute to a cycle of social and economic isolation, in which the consequences of outmigration ultimately hinder long-term community well-being. Meanwhile, other studies that examine how concentrated policing efforts impact structural disadvantage more directly find that these policing efforts can create barriers to upward mobility, perpetuating racialized housing inequalities in the process (Rosen, 2020; Sharkey & Faber, 2014).

Gentrification and Demographic Turnover

Alternatively, outmigration can also lead to gentrification and demographic turnover, especially in larger, highly desirable locations such as Los Angeles, California, or New York City, New York. As neighborhoods become safer and develop a more desirable reputation, they may attract outside investors and higher-income individuals, ultimately increasing renter costs, displacing longtime residents who can no longer afford to stay, and altering neighborhood composition over time.

In Los Angeles, California, community residents explicitly raised this concern during a Police Commission Meeting on April 9, 2019, arguing that programs such as PredPol and Operation L.A.S.E.R. were influencing neighborhood change by criminalizing predominantly Black and Hispanic communities, displacing longtime residents through increased police contact and heightened fear of such contact, and driving up renter costs by making these neighborhoods more appealing to wealthier, White individuals. During this meeting, one community resident argued that these programs were essentially trying to turn "Venice into Malibu, and Downtown Los Angeles and Skid Row into San Francisco" (Police Commission, 2019). A summary of community concerns expressed during this meeting is provided in Appendix B of the introductory chapter.

In recent years, research has supported these arguments. Specifically, Autor et al. (2017) caution that safety gains from increased enforcement can attract profit-driven investment, potentially displacing long-term residents. Similarly, Bell (2020b) finds that policing strategies intended to promote integration can instead accelerate demographic turnover, as shifts in perceived safety and community identity influence which groups feel welcome or displaced. The demographic turnover that follows increased enforcement can erode social cohesion and disrupt the everyday lives of long-standing residents, particularly in racially and economically diverse neighborhoods.

Overall, scholars agree that perceptions of safety alone can occasionally influence neighborhood desirability. When neighborhoods are stigmatized and criminalized, there is a risk of outmigration that can harm the residents who remain. Alternatively, when neighborhoods are perceived as safe by outsiders due to heightened police presence, gentrification and demographic turnover may occur, displacing longtime residents by driving up renter costs. More importantly, scholars note that while policing strategies can improve public safety and increase housing demand under specific conditions, their effects are uneven across populations and are heavily influenced by factors such as race, socioeconomic class, and historical context. The present study contributes to that conversation by examining how Operation L.A.S.E.R. coincided with changes in housing prices, police presence, and demographic stability in Los Angeles, California.

DATA

To evaluate the impact of Operation L.A.S.E.R. on neighborhood desirability, the present chapter follows 2010 TIGER/Line census tracts weekly from 2010 through 2019. Unlike Chapter 1, which examines the 2015 expansion within three retained divisions and uses census blocks to assess displacement near relatively small zones, the present chapter uses a citywide tract panel and all available L.A.S.E.R. Zones across the full 2010 to 2019 period. In addition, census tracts permit direct integration with the demographic and housing measures used in the present chapter, which are consistently available at the tract level.

The resulting panel consists of 996 census tracts over 522 weeks, yielding 519,912 tract-week observations. To support a variety of analyses, each week is also assigned to a corresponding month, quarter, and year. In addition to these time variables, the final dataset includes L.A.S.E.R.-specific treatment variables, aggregate housing measures, G.P.S.-based measures of officer activity, and decennial census data, all harmonized to 2010 tract boundaries to provide a comprehensive perspective on policing, housing market dynamics, and neighborhood demographics in both ever-treated and never-treated areas of Los Angeles.

Operation L.A.S.E.R. Treatment Data

The L.A.S.E.R.-specific treatment variables are constructed from information obtained through Public Records Act (P.R.A.) requests submitted via NextRequest, the City of Los Angeles' records request portal. These files contain the necessary information on L.A.S.E.R. Zone geography and activation dates (Los Angeles Police Department, 2018-2021). Once obtained, the zones were mapped and assigned to 2010 census tracts using an area-weighted overlay approach, in which a tract-week is coded as treated if at least 50 percent of the tract's geographic area overlapped with an active L.A.S.E.R. Zone during that week. The treatment data were then merged directly with the weekly tract-level panel for analysis. In addition to the treated tract-week indicator, a separate ever-treated flag was created to identify tracts that had ever received L.A.S.E.R. exposure.

Housing Market Data

The housing measures are drawn from a custom aggregate tract-week dataset created from licensed Zillow Transaction and Assessment Dataset (ZTRAX) extracts for Los Angeles County. The retained dataset covers calendar years 2010 through 2019 and contains aggregate measures of single-family residential home sale prices, lot size, building area, year built, bedrooms, bathrooms, and predicted buyer- and seller-composition shares. It contains no names or individual- or transaction-level records, and all Chapter 3 analyses use only aggregate tract-week measures. In total, the aggregate housing file contains 85,734 unique tract-week observations across 904 census tracts.

The retained dataset provides aggregate home sale price measures in nominal and inflation-adjusted dollars, in logged and unlogged form, and with and without extreme values. To minimize the influence of skewed data from unusual transfers (e.g., family sales or luxury properties), the present chapter uses home sale prices trimmed at the 1st and 99th percentiles, converted to 2019 dollars using the Consumer Price Index (C.P.I.), and log-transformed. As a result, the primary indicator of housing desirability is the logged and inflation-adjusted mean home sale price. However, non-logged versions of Figures 1 through 3 are provided in Appendix A as supplemental information to show changes in dollar amounts. Similar to their logged counterparts, these alternative figures also exclude outliers by trimming at the 1st and 99th percentiles and adjust for inflation.

Officer Deployment Data

Aside from the L.A.S.E.R.-specific treatment indicators, police presence is measured using cellphone-based G.P.S. location data, commonly referred to as cellphone "ping" data. These data were provided as a courtesy by Chen et al. (2025), who used anonymized smartphone location data to measure police presence across large U.S. cities. The version of the data provided was recorded at the census block and quarter-year level. For the present study, however, the block-level data were aggregated to the census tract level using each block's tract-level identifier, producing a tract-by-quarter-year measure of officer presence.

After collapsing the data, the tract-quarter-year ping file was merged with the master tract-week panel at the census tract-quarter-year level. Because the ping data are quarterly, each tract-quarter value was assigned to the weeks within that quarter. In total, 104,475 tract-week observations were matched to officer-presence measures. Due to limited data availability, G.P.S. ping records are available only for 2017 and 2019. To address zeros and skewness in the data, ping hours were transformed using the inverse hyperbolic sine (I.H.S.) function, which behaves similarly to a logarithm but is defined at zero.

United States Census Demographic Data

Demographic and housing composition measures were obtained from the Decennial Census via National Historical Geographic Information System extracts (Manson et al., 2024). These include total population, housing tenure, racial and ethnic composition, and counts of occupied households. The analysis uses 2010 and 2020 Census data harmonized to 2010 Census tract boundaries, enabling consistent geographic comparisons over time and eliminating errors that could arise from boundary changes across census years.

Although the full analytic panel contains 996 census tracts, the descriptive census tables use a balanced analytic sample of 994 tracts. Tables 1, 2, and Appendix Table A1 exclude tracts with zero census table denominators in either 2010 or 2020 to ensure that the 2010 and 2020 descriptive comparisons rely on the same tract set. This restrictive process applies only to the descriptive census tables and does not change the full tract-week panel used for the main analyses. In total, the balancing process removed only one tract in the Foothill Division with a zero-occupied-household denominator in 2010 and one tract in the Devonshire Division with a zero-occupied-household denominator in 2020, resulting in the 994-census-tract analytic sample.

METHODS

Analytic Strategy

Given that Operation L.A.S.E.R. Zones could be introduced, suspended, and later reinstated within the same tract, methods that assume treatment begins once and then continues permanently are not appropriate for the present study (Sant'Anna, 2023). Recent research shows that when treatment can start, stop, and restart, outcomes may be related to both current and earlier exposure (de Chaisemartin & D'Haultfœuille, 2026). Accordingly, the treatment-timing models examine contemporaneous exposure (treatment during the same week), lagged exposure (treatment one or two weeks earlier), and cumulative exposure (the total number of treated weeks through the current week) to determine whether home-price differences appeared immediately, emerged after a short delay, or accumulated gradually through repeated exposure.

Contemporaneous treatment captures whether logged home sale prices differed in the same week a tract was exposed to L.A.S.E.R. Meanwhile, the lagged treatment measures account for the possibility that housing-market responses may not appear immediately but instead emerge one or two weeks after exposure. Lastly, the cumulative treatment measure captures the total number of L.A.S.E.R. treatment weeks experienced by a tract through week t. One benefit of examining cumulative exposure is that the collateral consequences of proactive policing strategies may gradually build over time rather than appear as a single, immediate shift; thus, the models in Table 6 preserve the week-by-week structure of the treatment data while also accounting for the staggered and intermittent rollout of Operation L.A.S.E.R.

Sample Construction

The present study relies on three main analytic samples. The first is the officer-activity sample, which pools the 2017 and 2019 G.P.S. ping data used in the Table 4 models. Although the ping data yield 104,475 matched tract-week observations before model-specific restrictions, all Table 4 models use the same analytic sample. Within this sample, officer presence is measured quarterly, so each tract-quarter value is repeated across the corresponding weekly observations. To account for stable differences across tracts and changes common to all tracts within a given quarter, the models include census tract and quarter-year fixed effects. Given the repeated observations within tracts, standard errors are also clustered by census tract. Lastly, because G.P.S. ping data are available only for 2017 and 2019, pooling both years uses all available officer-activity data to assess whether Operation L.A.S.E.R. exposure was associated with notable changes in officer presence.

However, the two snapshots capture different moments in the program's history. One benefit of examining the 2017 snapshot alone is that it captures a midpoint in time between the initial 2015 expansion and the program's eventual termination in 2019. However, the snapshot does not account for divisions that adopted the intervention in later years. In contrast, examining the 2019 snapshot alone accounts for more locations, but because L.A.S.E.R. ended in April

2019, treated tracts were no longer expected to show higher patrol levels. Thus, Table 4 covers both years, while the year-specific models are presented in the appendix. See Appendix Table A2 for the 2017-only models, and Appendix Table A3 for the 2019-only models.

The second analytic sample is the home-price sample, which spans the full 2010 through 2019 period of home sales and includes tract-weeks with non-missing logged sale prices and the covariates required for the main home-price models. The final analytic sample is the treatment-timing sample, which includes tract-weeks with non-missing logged sale prices and valid contemporaneous, one-week-lagged, two-week-lagged, and cumulative treatment measures. Because these timing models are designed to isolate timing patterns, they exclude additional covariates.

RESULTS

Figure 1. Logged Mean Home Sales Price by Treatment Exposure Status

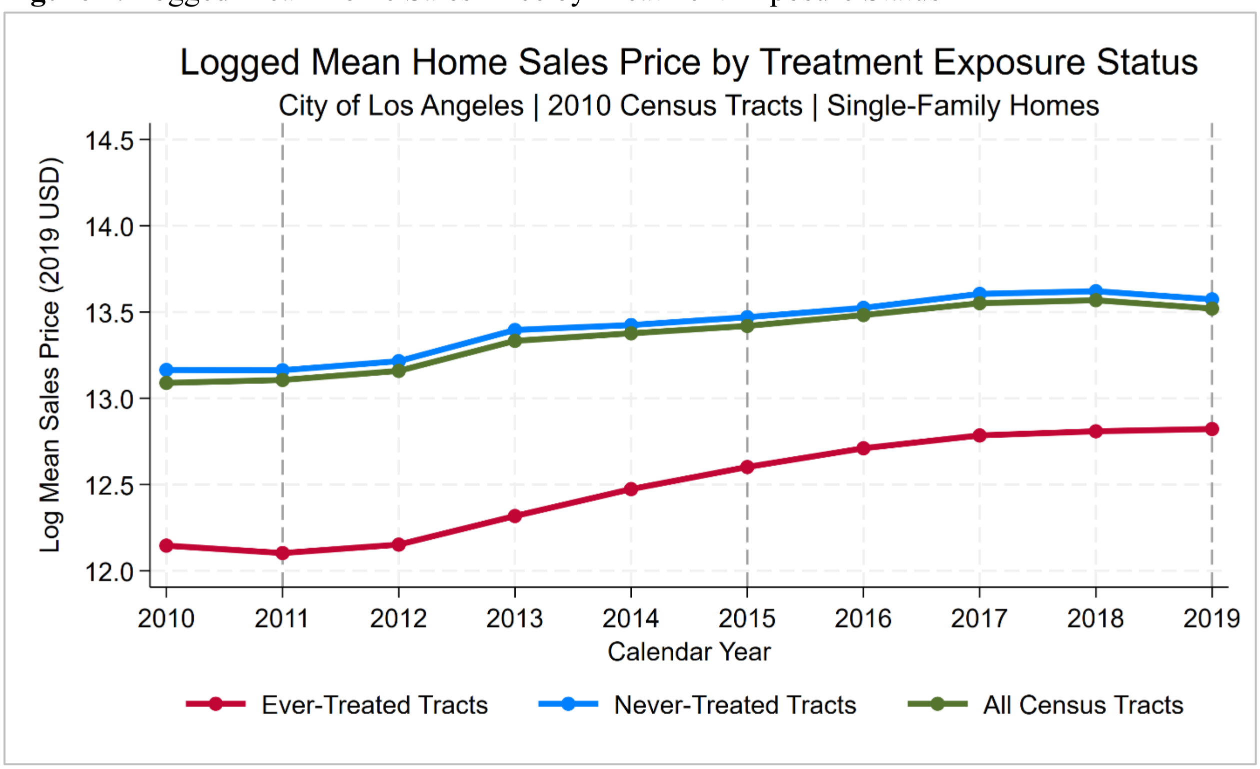


Figure 2. Logged Mean Home Sales Price by L.A.S.E.R. Cohort and Year

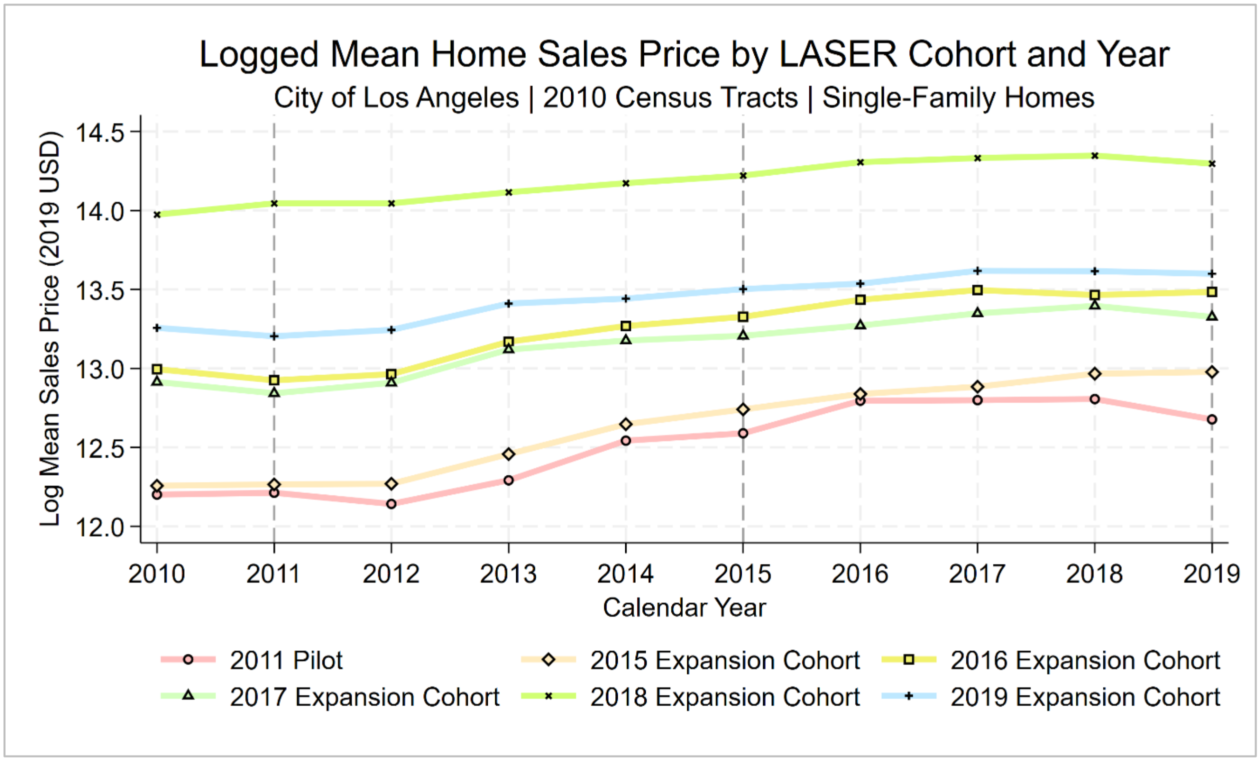


Figure 3. Logged Mean Home Sales Price by L.A.S.E.R. Cohort and Time

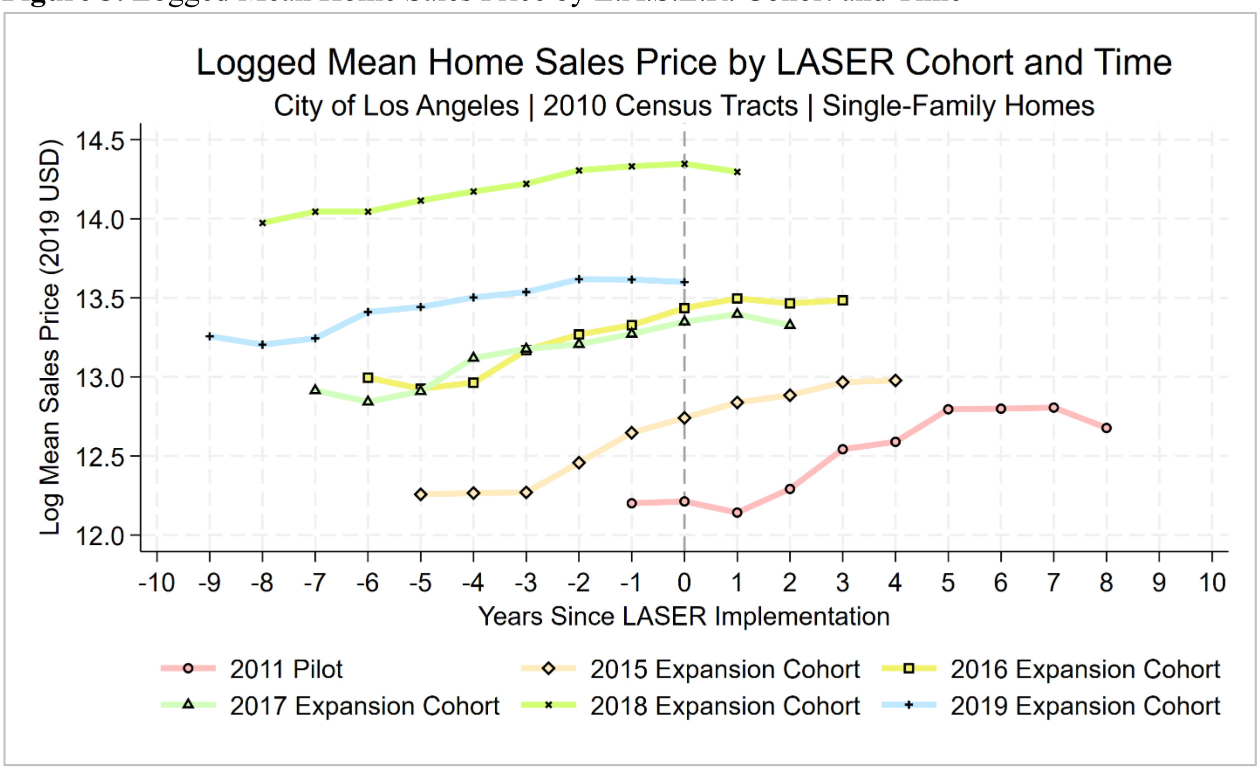


Table 1. Percent of Occupied Households by Tenure, Ethnicity, and Treatment Status

Data Source	Tenure by Ethnicity	Ever-Treated	Never-Treated	All Tracts
2010 Census	Owner: Not Hispanic	8.37%	27.66%	26.34%
	Owner: Hispanic	14.12%	12.07%	12.21%
	Renter: Not Hispanic	32.12%	33.21%	33.13%
	Renter: Hispanic	45.39%	27.06%	28.32%
		n = 68	**n = 926**	**n = 994**
2020 Census*	Owner: Not Hispanic	6.90%	26.10%	24.78%
	Owner: Hispanic	15.14%	12.05%	12.27%
	Renter: Not Hispanic	31.74%	34.60%	34.40%
	Renter: Hispanic	46.22%	27.25%	28.55%
		n = 68	**n = 926**	**n = 994**

Notes: 2020 census data are interpolated to 2010 tract boundaries using N.H.G.I.S. standardized geographies; tracts with zero denominators in either year are excluded from both years; n values are tract counts.

Table 2. Percent of Occupied Households by Householder Race, Ethnicity, and Treatment Status

Data Source	Race & Ethnicity	Ever-Treated	Never-Treated	All Tracts
2010 Census	N.H. White alone	6.90%	36.43%	34.41%
	N.H. Black or A.A. alone	27.44%	9.80%	11.00%
	N.H. A.I./A.N. alone	0.21%	0.21%	0.21%
	N.H. Asian alone	4.54%	12.18%	11.66%
	N.H. N.H.P.I. alone	0.08%	0.11%	0.11%
	N.H. Some Other Race	0.26%	0.26%	0.26%
	N.H. Two or More Races	1.05%	1.89%	1.83%
	Hispanic (any race)	59.51%	39.13%	40.53%
		n = 68	**n = 926**	**n = 994**
2020 Census*	N.H. White alone	8.32%	35.53%	33.67%
	N.H. Black or A.A. alone	22.42%	8.55%	9.50%
	N.H. A.I./A.N. alone	0.19%	0.20%	0.20%
	N.H. Asian alone	4.86%	12.49%	11.97%
	N.H. N.H.P.I. alone	0.09%	0.11%	0.10%
	N.H. Some Other Race	0.46%	0.60%	0.59%
	N.H. Two or More Races	2.30%	3.22%	3.16%
	Hispanic (any race)	61.36%	39.31%	40.81%
		n = 68	**n = 926**	**n = 994**

Notes: 2020 census data are interpolated to 2010 tract boundaries using N.H.G.I.S. standardized geographies; tracts with zero denominators in either year are excluded from both years; n values are tract counts.

Table 3. Aggregate Predicted Buyer- and Seller-Composition Shares by Treatment Status

Race & Ethnicity	Ever-Treated	Never-Treated	All Tracts
Buyer-Side Composition			
Most likely White	5.78%	61.60%	59.14%
Most likely Black	0.98%	0.33%	0.35%
Most likely A.I./A.N.	0.00%	0.00%	0.00%
Most likely Asian	0.37%	0.74%	0.73%
Most likely Other	0.13%	1.07%	1.03%
Most likely Hispanic	92.74%	36.26%	38.75%
	n = 56	**n = 848**	**n = 904**
Seller-Side Composition			
Most likely White	6.21%	62.38%	59.90%
Most likely Black	1.11%	0.33%	0.36%
Most likely A.I./A.N.	0.00%	0.00%	0.00%
Most likely Asian	0.31%	0.66%	0.64%
Most likely Other	0.12%	0.89%	0.86%
Most likely Hispanic	92.26%	35.74%	38.23%
	n = 56	**n = 848**	**n = 904**

Notes: Percentages reflect predicted buyer and seller race and ethnicity among qualifying home sales summarized in the aggregate tract-week data; name-based predictions may misclassify multiracial people, married or anglicized surnames, and sparsely covered groups such as A.I./A.N.

Table 4. Impact of Operation L.A.S.E.R. on Officer Activity (Measured as Ping Hours)

VARIABLES	(1) **Raw Ping Hours**	(2) **(I.H.S.) Ping Hours**	(3) **(I.H.S.) Ping Hours**	(4) **(I.H.S.) Ping Hours**	(5) **(I.H.S.) Ping Hours**
L.A.S.E.R. in Effect	5.480	0.084	0.086	0.083	0.083
	(5.051)	(0.111)	(0.112)	(0.112)	(0.112)
Lot Size (sq. ft.)			0.000	0.000	0.000
			(0.000)	(0.000)	(0.000)
Building Area (sq. ft.)			0.000	0.000	0.000
			(0.000)	(0.000)	(0.000)
Year Built			-0.000	-0.000	-0.000
			(0.000)	(0.000)	(0.000)
Total Bedrooms			-0.007	-0.007	-0.007
			(0.007)	(0.007)	(0.007)
Total Bathrooms			0.014	0.014	0.014
			(0.009)	(0.009)	(0.009)
Home Seller Race: White				0.016	
				(0.009)	
Home Seller Race: Black				-0.085	
				(0.061)	
Home Seller Race: Asian				-0.019	
				(0.040)	
Home Seller Race: His.				0.010	
				(0.010)	
Home Seller Race: Other				-0.056	
				(0.039)	
Homebuyer Race: White					0.014
					(0.009)
Homebuyer Race: Black					-0.066
					(0.059)
Homebuyer Race: Asian					-0.020
					(0.043)
Homebuyer Race: His.					0.012
					(0.010)
Homebuyer Race: Other					-0.054
					(0.035)
Constant	10.021***	2.199***	2.721***	2.682***	2.685***
	(0.633)	(0.026)	(0.713)	(0.712)	(0.712)
Obs. (Tract-Week)	15,550	15,550	15,550	15,550	15,550
R-squared	0.034	0.139	0.139	0.140	0.140
Total Tracts	**850**	**850**	**850**	**850**	**850**

Notes: Robust S.E.s in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; sample years = 2017 and 2019; all models include census tract and quarter-year fixed effects; standard errors are clustered by census tract.

Table 5. Impact of Operation L.A.S.E.R. on Logged Mean Home Sales Price

VARIABLES	(1) **$\bar{x}$ Home Sales Price - Logged**	(2) **$\bar{x}$ Home Sales Price - Logged**	(3) **$\bar{x}$ Home Sales Price - Logged**	(4) **$\bar{x}$ Home Sales Price - Logged**
L.A.S.E.R. in Effect	0.085*	0.106*	0.107**	0.105*
	(0.042)	(0.042)	(0.041)	(0.042)
Lot Size (sq. ft.)		-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)
Building Area (sq. ft.)		0.000***	0.000***	0.000***
		(0.000)	(0.000)	(0.000)
Year Built		-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)
Total Bedrooms		-0.004	-0.002	-0.002
		(0.005)	(0.005)	(0.005)
Total Bathrooms		0.005	0.006	0.006
		(0.009)	(0.008)	(0.009)
Home Seller Race: White			0.011**	
			(0.004)	
Home Seller Race: Black			-0.018	
			(0.029)	
Home Seller Race: Asian			0.020	
			(0.020)	
Home Seller Race: His.			-0.041***	
			(0.005)	
Home Seller Race: Other			-0.048	
			(0.027)	
Homebuyer Race: White				0.010**
				(0.004)
Homebuyer Race: Black				0.036
				(0.033)
Homebuyer Race: Asian				-0.020
				(0.016)
Homebuyer Race: His.				-0.037***
				(0.005)
Homebuyer Race: Other				-0.052**
				(0.017)
Constant	13.356***	13.366***	13.417***	13.415***
	(0.000)	(0.499)	(0.495)	(0.495)
Obs. (Tract-Week)	16,985	16,985	16,985	16,985
R-squared	0.812	0.842	0.844	0.843
Total Tracts	**790**	**790**	**790**	**790**

Notes: Robust S.E.s in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; sample years = 2010-2019; all models include census tract and week fixed effects; standard errors are clustered by census tract.

Table 6. Dynamic Treatment-Timing Models for Logged Home Sale Prices

VARIABLES	(1) **Contemporaneous Effects**	(2) **Lagged Effects**	(3) **Cumulative Exposure Effects**
L.A.S.E.R. in Effect: *Week t*	0.079	0.005	
	(0.042)	(0.046)	
L.A.S.E.R. in Effect: *t-1 Week*		0.064	
		(0.057)	
L.A.S.E.R. in Effect: *t-2 Weeks*		0.033	
		(0.046)	
L.A.S.E.R. in Effect: *Cumulative Exposure*			0.003***
			(0.001)
Constant	13.358***	13.358***	13.356***
	(0.000)	(0.000)	(0.000)
Obs. (Tract-Week)	16,985	16,985	16,985
R-squared	0.812	0.812	0.813
Total Tracts	**790**	**790**	**790**

Notes: Robust S.E.s in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$;
all models include census tract and week fixed effects; standard errors are clustered by census tract.

DISCUSSION

Housing Market Trends

Figures 1 and 2 display vertical lines for the years 2011, 2015, and 2019. The first line marks the year the Los Angeles S.P.I. team launched the Operation L.A.S.E.R. pilot program within the Newton Division; the second line marks the year the team expanded the intervention to additional divisions; and the final line marks the year the department terminated the program.

When examining the two figures individually, Figure 1 shows that never-treated tracts maintained consistently higher home sale prices than their ever-treated counterparts and that home sale prices increased gradually over time without notable shocks. By contrast, Figure 1 shows that ever-treated tracts had home sale prices well below the citywide average, with the largest discrepancy occurring one year prior to the program's 2011 implementation in the Newton Division. In addition, Figure 1 shows that over the study period, ever-treated tracts experienced faster growth than never-treated tracts and the citywide average. However, although the gap between ever-treated and never-treated tracts narrowed, a notable discrepancy remains.

Meanwhile, Figures 2 and 3 provide additional context by shifting the focus from treatment status to cohort groups based on the year that divisions adopted L.A.S.E.R. Unlike Figures 1 and 2, which mark three points in time, Figure 3 displays a single vertical line at year zero to represent the year of adoption for each cohort. See the introductory chapter's Appendix A for a map of the six groups that displays the divisions and their cohorts using the same color scheme. Consistent with Figure 1, Figures 2 and 3 suggest that treatment is associated with increased home sale prices over time; however, Figures 2 and 3 also show that areas that received early treatment, such as the 2011 pilot and the 2015 expansion cohort, experienced sharper growth, while groups that adopted L.A.S.E.R. at a later point in time, such as the 2017 and 2018 cohorts, exhibited more subtle growth, which could indicate lower crime rates to begin with, fewer years under treatment, or both.

Overall, Figures 1 through 3 suggest that Operation L.A.S.E.R. was concentrated in neighborhoods with long-standing racial and economic disadvantage, and that price appreciation in treated tracts emerged gradually rather than immediately. Although appreciation was faster in treated tracts, values never fully reached the levels observed in never-treated tracts or the citywide average.

Racial and Tenure Composition

Tables 1 and 2 draw on 2010 and 2020 decennial census data, standardized to 2010 census tract boundaries for comparability. Table 1 focuses on tenure and Hispanic origin, showing that Hispanic renters accounted for the largest group in ever-treated tracts in both 2010 and 2020. In 2010, 45.39 percent of occupied households in ever-treated tracts were Hispanic renters, and by 2020, that share had risen slightly to 46.22 percent. Non-Hispanic renters were also common in ever-treated tracts, accounting for 32.12 percent of occupied households in 2010 and 31.74 percent in 2020. In contrast, non-Hispanic owners made up a much smaller share of occupied households in ever-treated tracts, declining from 8.37 percent in 2010 to 6.90 percent in 2020. Meanwhile, never-treated tracts were less dominated by Hispanic renters and had larger shares of non-Hispanic owners and non-Hispanic renters.

Table 2 shifts the focus from ownership status to householder race and ethnicity. Once again, Hispanics emerge as the majority group in ever-treated tracts, accounting for 59.51 percent of householders in 2010 and 61.36 percent in 2020. Never-treated tracts have a different composition. Although Hispanics of any race represented the largest share in both years, at roughly 39 percent, non-Hispanic Whites accounted for approximately 36 percent of householders, compared with less than 10 percent in ever-treated tracts. Black householders also represented substantially larger shares in ever-treated tracts, 27.44 percent in 2010 and 22.42 percent in 2020, than in never-treated tracts, where the corresponding shares were below 10 percent.

Shifting to the aggregate buyer- and seller-composition measures, Table 3 shows that ever-treated tracts had predicted Hispanic shares of 92.74 percent for buyer-side measures and 92.26 percent for seller-side measures. The corresponding shares in never-treated tracts were approximately 36 percent. Conversely, the predicted White shares in never-treated tracts were approximately 62 percent for both buyer- and seller-side measures, compared with roughly 6 percent in ever-treated tracts. All other predicted racial and ethnic composition shares were comparatively small across both tract groups.

Across the three tables, the findings show that ever-treated tracts were majority Hispanic and renter-heavy. Never-treated tracts were less renter-heavy and had more balanced shares of Hispanic and non-Hispanic White householders. Overall, the census data show larger concentrations of Hispanic and Black householders in treated areas. The aggregate housing measures similarly show substantially larger predicted Hispanic buyer- and seller-side shares in ever-treated tracts than in never-treated tracts.

Officer Presence and Market Outcomes

Tables 4 and 5 assess whether Operation L.A.S.E.R. exposure is associated with officer presence and home sales prices. In Table 4, the raw ping-hours treatment estimate is positive but not statistically significant at conventional levels. The inverse-hyperbolic-sine treatment estimates are also positive, ranging from 0.083 to 0.086, but none reaches conventional levels of statistical significance. Thus, the pooled models do not provide evidence that Operation L.A.S.E.R. exposure was associated with a statistically detectable change in officer activity. Appendix Tables A2 and A3 report the corresponding 2017-only and 2019-only models; neither year-specific set produces a statistically significant treatment estimate.

Table 5, however, shows a positive association between Operation L.A.S.E.R. exposure and logged home sales prices. The pattern appears in the base model, after controlling for housing characteristics, and after adding aggregate predicted seller- or buyer-composition measures. Across the final models, L.A.S.E.R. exposure is associated with higher logged home prices, with estimates ranging from 0.085 to 0.107 log points. These results suggest that treated tract-weeks had higher sale prices than untreated tract-weeks after accounting for tract- and week-fixed effects.

Lastly, Table 6 examines whether the timing of treatment exposure makes a difference. The contemporaneous treatment estimate is positive; however, this finding is not statistically significant at conventional levels. The same is true for the one-week- and two-week-lagged treatment indicators. In contrast, cumulative exposure is positive and statistically significant. Specifically, each additional week of L.A.S.E.R. exposure corresponds to a 0.003 log-point increase in logged home sales price. Although this weekly estimate is small, the cumulative model suggests that price differences accumulated gradually over repeated exposure rather than appearing as an immediate one-week shift after treatment began.

CONCLUSION

In summary, the present chapter examined the collateral consequences of Operation L.A.S.E.R. for neighborhood desirability, measured by home sale prices across Los Angeles census tracts. The findings show that the intervention was concentrated in Hispanic-majority, renter-heavy neighborhoods. In addition, the pooled officer-presence models produced positive estimates for both raw and transformed ping hours, although neither was statistically significant at conventional levels. The intervention was also associated with higher logged home sale prices in treated tract-weeks. However, these price increases did not coincide with rapid demographic turnover, which was a concern expressed by many Los Angeles residents while the program was in place. Instead, findings suggest that appreciation may have reflected housing-market changes within treated neighborhoods rather than immediate gentrification. Nonetheless, the increase in values was accompanied by notable tradeoffs.

Given that treated neighborhoods were renter-heavy and disproportionately occupied by Hispanic and Black residents, many racial and ethnic minorities may have faced higher housing costs and lacked the opportunity to build generational wealth through property ownership, a concern consistent with Sharkey's (2013) argument that neighborhood disadvantage can reproduce hardship across multiple generations. Meanwhile, targeted proactive policing may also have heightened exposure to the criminal legal system, ultimately reinforcing cycles of inequality. Overall, these findings suggest that proactive policing strategies can influence neighborhood desirability beyond crime control; thus, future policing strategies and policy evaluations should focus not only on crime outcomes but also on the potential collateral consequences for marginalized groups exposed to proactive policing.

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APPENDIX A

Supplemental Tables & Figures

Appendix Table A1. Percent of Total Population by Race, Ethnicity, and Treatment Status

Data Source	Race & Ethnicity	Ever-Treated	Never-Treated	All Tracts
2010 Census	N.H. White alone	5.56%	31.18%	29.43%
	N.H. Black or A.A. alone	19.26%	7.98%	8.75%
	N.H. A.I./A.N. alone	0.16%	0.17%	0.17%
	N.H. Asian alone	3.97%	11.90%	11.35%
	N.H. N.H.P.I. alone	0.08%	0.12%	0.11%
	N.H. Some Other Race	0.30%	0.31%	0.31%
	N.H. Two or More Races	0.92%	2.14%	2.05%
	Hispanic (any race)	69.75%	46.20%	47.81%
		n = 68	**n = 926**	**n = 994**
2020 Census*	N.H. White alone	6.78%	30.81%	29.16%
	N.H. Black or A.A. alone	16.52%	7.22%	7.86%
	N.H. A.I./A.N. alone	0.16%	0.17%	0.17%
	N.H. Asian alone	4.24%	12.15%	11.61%
	N.H. N.H.P.I. alone	0.10%	0.12%	0.11%
	N.H. Some Other Race	0.58%	0.67%	0.67%
	N.H. Two or More Races	1.78%	3.59%	3.46%
	Hispanic (any race)	69.83%	45.27%	46.95%
		n = 68	**n = 926**	**n = 994**

Notes: 2020 census data are interpolated to 2010 tract boundaries using N.H.G.I.S. standardized geographies; tracts with zero denominators in either year are excluded from both years; n values are tract counts.

Appendix Table A2. Impact of Operation L.A.S.E.R. on Officer Activity (Restricted to 2017)

VARIABLES	(1) **Raw Ping Hours**	(2) **(I.H.S.) Ping Hours**	(3) **(I.H.S.) Ping Hours**	(4) **(I.H.S.) Ping Hours**	(5) **(I.H.S.) Ping Hours**
L.A.S.E.R. in Effect	6.654	-0.086	-0.085	-0.091	-0.093
	(6.656)	(0.118)	(0.118)	(0.121)	(0.121)
Lot Size (sq. ft.)			-0.000	-0.000	-0.000
			(0.000)	(0.000)	(0.000)
Building Area (sq. ft.)			0.000	0.000	0.000
			(0.000)	(0.000)	(0.000)
Year Built			-0.000	-0.000	-0.000
			(0.000)	(0.000)	(0.000)
Total Bedrooms			0.002	0.002	0.002
			(0.008)	(0.008)	(0.008)
Total Bathrooms			-0.001	-0.001	-0.001
			(0.009)	(0.009)	(0.009)
Home Seller Race: White				0.005	
				(0.008)	
Home Seller Race: Black				-0.118	
				(0.084)	
Home Seller Race: Asian				-0.028	
				(0.046)	
Home Seller Race: His.				-0.000	
				(0.011)	
Home Seller Race: Other				-0.011	
				(0.039)	
Homebuyer Race: White					0.006
					(0.008)
Homebuyer Race: Black					-0.124
					(0.077)
Homebuyer Race: Asian					-0.026
					(0.048)
Homebuyer Race: His.					-0.000
					(0.011)
Homebuyer Race: Other					-0.024
					(0.036)
Constant	9.746***	2.200***	2.573***	2.564***	2.565***
	(0.730)	(0.022)	(0.710)	(0.712)	(0.710)
Obs. (Tract-Week)	9,292	9,292	9,292	9,292	9,292
R-squared	0.037	0.143	0.144	0.144	0.144
Total Tracts	**831**	**831**	**831**	**831**	**831**

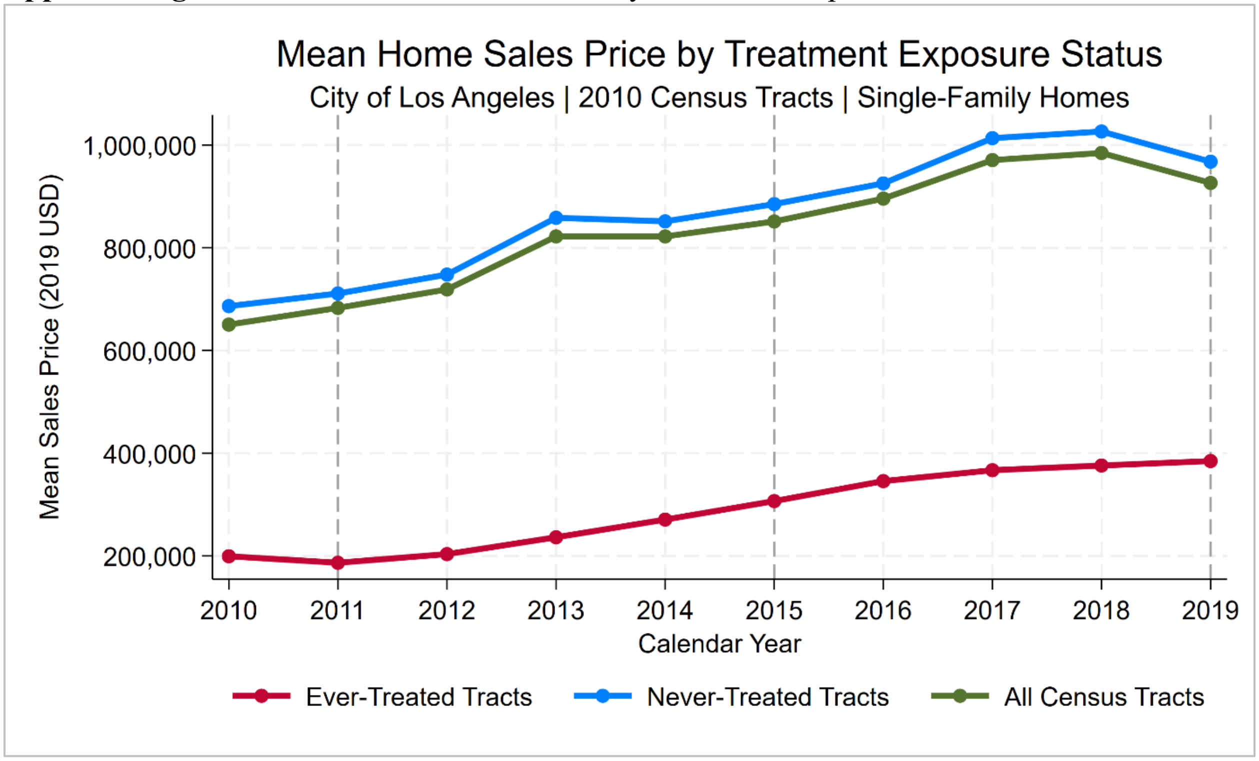
Notes: Robust S.E.s in parentheses; * p < 0.05, ** p < 0.01, *** p < 0.001; sample years = 2017; all models include census tract and quarter-year fixed effects; standard errors are clustered by census tract.

Appendix Table A3. Impact of Operation L.A.S.E.R. on Officer Activity (Restricted to 2019)

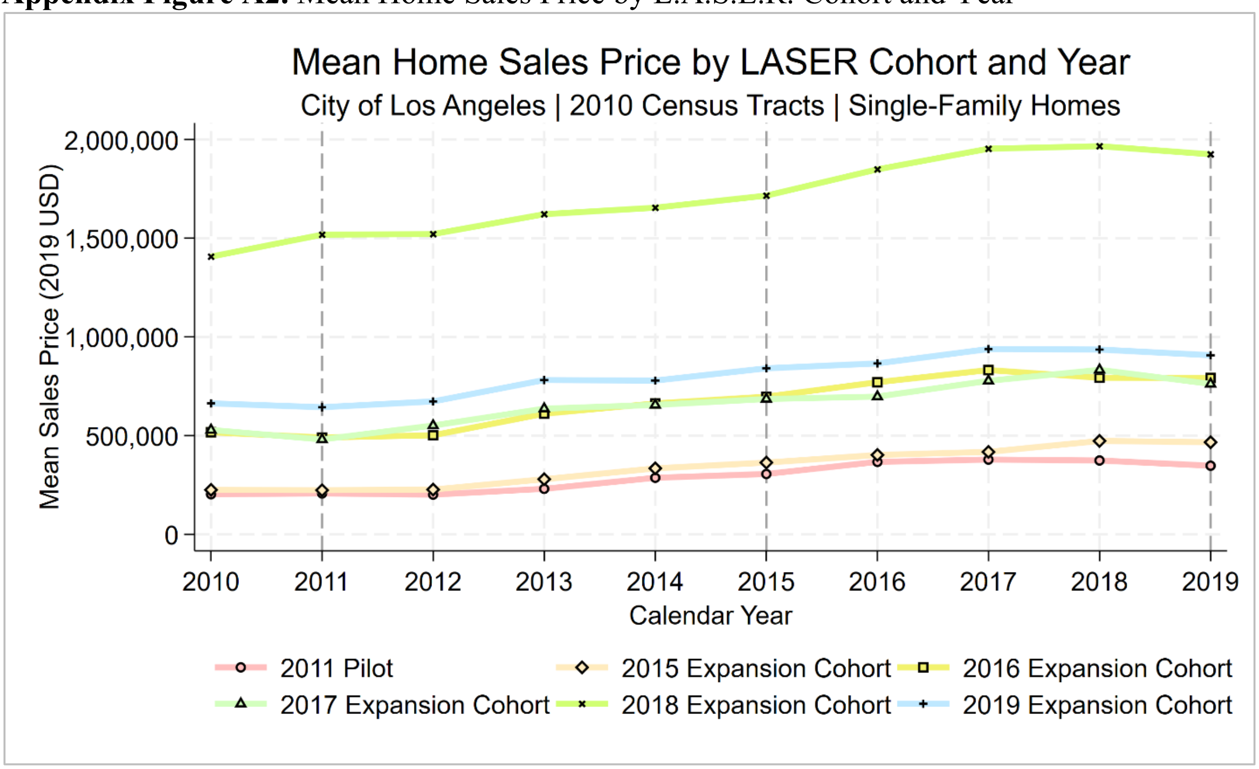
VARIABLES	(1) **Raw Ping Hours**	(2) **(I.H.S.) Ping Hours**	(3) **(I.H.S.) Ping Hours**	(4) **(I.H.S.) Ping Hours**	(5) **(I.H.S.) Ping Hours**
L.A.S.E.R. in Effect	-1.136	-0.073	-0.070	-0.066	-0.065
	(1.733)	(0.186)	(0.189)	(0.190)	(0.190)
Lot Size (sq. ft.)			-0.000	-0.000	-0.000
			(0.000)	(0.000)	(0.000)
Building Area (sq. ft.)			0.000	0.000	0.000
			(0.000)	(0.000)	(0.000)
Year Built			-0.001	-0.001	-0.001
			(0.001)	(0.001)	(0.001)
Total Bedrooms			-0.008	-0.008	-0.008
			(0.010)	(0.010)	(0.010)
Total Bathrooms			0.018	0.018	0.018
			(0.013)	(0.013)	(0.013)
Home Seller Race: White				0.014	
				(0.014)	
Home Seller Race: Black				-0.074	
				(0.081)	
Home Seller Race: Asian				-0.037	
				(0.056)	
Home Seller Race: His.				0.021	
				(0.015)	
Home Seller Race: Other				-0.232***	
				(0.057)	
Homebuyer Race: White					0.011
					(0.014)
Homebuyer Race: Black					-0.040
					(0.094)
Homebuyer Race: Asian					-0.056
					(0.062)
Homebuyer Race: His.					0.025
					(0.016)
Homebuyer Race: Other					-0.182**
					(0.059)
Constant	10.631***	2.141***	3.473***	3.334***	3.357***
	(0.528)	(0.028)	(1.014)	(1.007)	(1.011)
Obs. (Tract-Week)	6,258	6,258	6,258	6,258	6,258
R-squared	0.002	0.005	0.006	0.009	0.009
Total Tracts	**796**	**796**	**796**	**796**	**796**

Notes: Robust S.E.s in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; sample years = 2019;
all models include census tract and quarter-year fixed effects; standard errors are clustered by census tract.

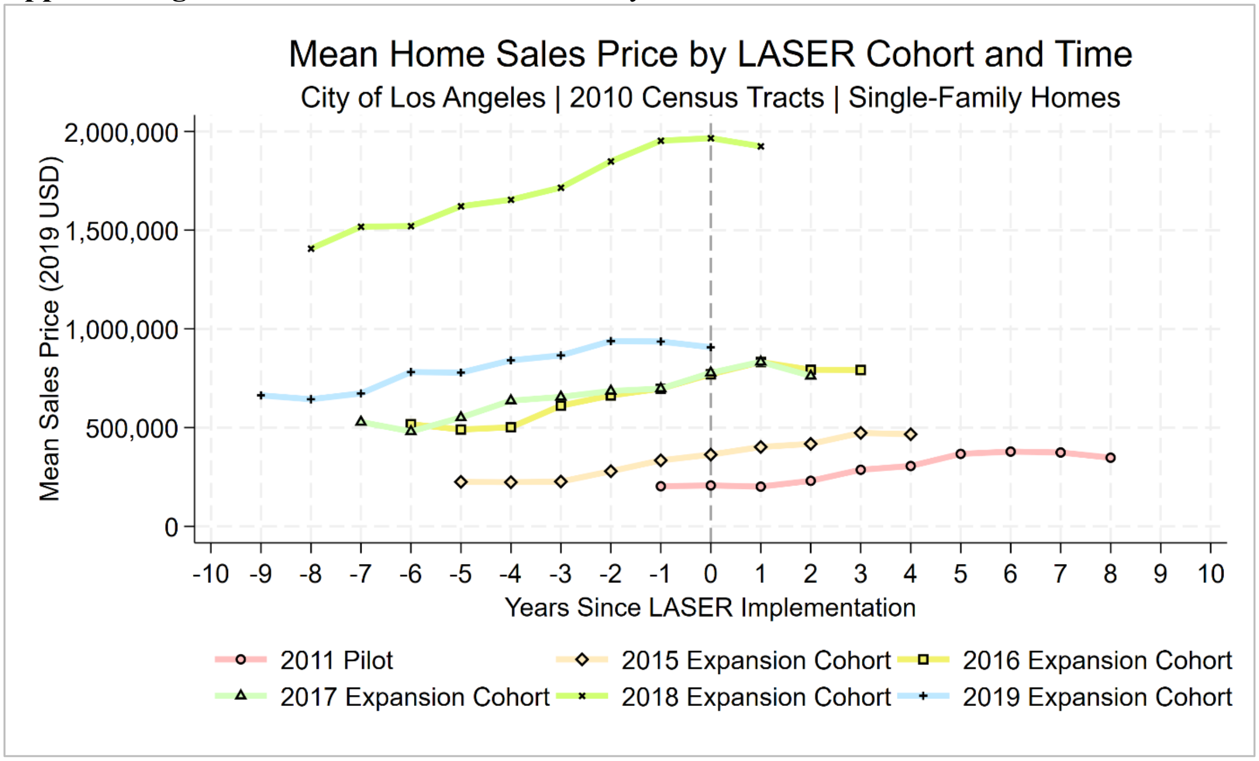
Appendix Figure A1. Mean Home Sales Price by Treatment Exposure Status



Appendix Figure A2. Mean Home Sales Price by L.A.S.E.R. Cohort and Year



Appendix Figure A3. Mean Home Sales Price by L.A.S.E.R. Cohort and Time



APPENDIX B

Data Curation

Tract Geography and Demographic Measures

Chapter 3 begins with 2010 Census TIGER/Line tract polygons for Los Angeles County. Additional geographic files provide the City of Los Angeles boundary and L.A.P.D. division boundaries. To define a consistent citywide sample, the analysis calculated each tract's area and the percentage of that area falling within the City of Los Angeles. A tract was retained when at least 50 percent of its area fell within the city boundary, producing a fixed sample of 996 census tracts. Each retained tract was also assigned to the L.A.P.D. division containing the largest share of its area.

In addition to establishing a consistent geography, demographic measures were obtained from the 2010 and 2020 decennial Census through the National Historical Geographic Information System (N.H.G.I.S.). Because census tract boundaries can change over time, the 2020 measures were harmonized to 2010 Census tract boundaries so that neighborhood composition could be compared using the same geography in both years. The source tables provide total population, occupied households, housing tenure, Hispanic origin, and the race and ethnicity of residents and householders. These measures support the demographic comparisons reported in Tables 1, 2, and Appendix Table A1.

From these records, the analysis created four mutually exclusive tenure-by-ethnicity groups: non-Hispanic homeowners, Hispanic homeowners, non-Hispanic renters, and Hispanic renters. Additional measures describe the racial and ethnic composition of householders and the total population. Thus, the renter characteristics examined in Chapter 3 come from Census and N.H.G.I.S. data rather than the housing transaction data.

Lastly, although the complete weekly panel contains 996 tracts, the balanced descriptive comparisons use 994 tracts with positive denominators in both 2010 and 2020. This restriction ensures that the demographic comparisons across the two Census years rely on the same tract sample. Importantly, the restriction applies only to the descriptive Census tables and does not alter the 996-tract weekly panel used for the remaining analyses.

L.A.S.E.R. Treatment Panel

The treatment data contain 300 dated L.A.S.E.R. Zone records constructed for the present dissertation from public and administrative maps and records. Each record identifies the zone name, police division, division rollout cohort, geographic boundary, and start and end dates.

To assign L.A.S.E.R. exposure to the tract-level panel, each zone was intersected with the 996 retained census tracts. A tract-zone relationship was retained when at least 50 percent of the tract's area overlapped the zone. Because treatment could begin or end partway through a week, qualifying treatment intervals were first expanded to individual calendar days, linked to the common time series, and then summarized by tract and week. When multiple qualifying zones were active within the same tract on the same day, that date counted only once as a treated tract-day so that overlapping zones did not duplicate exposure.

Using these daily records, the main treatment measure identifies whether any L.A.S.E.R. treatment occurred within a tract during a given week. An ever-treated indicator was also created to distinguish tracts that experienced treatment at least once from those that never did. In addition, the treatment-timing analyses use one- and two-week treatment lags and the cumulative number of treated weeks through the current week. Because L.A.S.E.R. Zones could stop and later restart, untreated gaps remain untreated rather than being treated as continuous exposure. As a result, the final measures preserve the intervention's actual timing while allowing current, recent, and cumulative exposure to be examined separately.

The resulting panel contains 996 census tracts observed over 522 weeks from 2010 through 2019, producing 519,912 tract-week observations. Of the 996 tracts, 68 experienced treatment at least once, while 4,541 tract-weeks contain at least one active treatment day. Thus, the final unit of observation for the combined Chapter 3 dataset is the 2010 Census tract-week.

Officer Presence

Chapter 3 measures officer presence using cellphone-based G.P.S. data provided by Chen et al. (2025). The source records are available for 2017 and 2019 and were originally measured at the census block-quarter level. Among the available measures, the present chapter uses total officer hours to represent officer presence.

To align these data with the Chapter 3 geography, census blocks were linked to their parent 2010 Census tracts, and officer hours were aggregated to the tract-quarter level. Because the source data are reported quarterly rather than weekly, each tract-quarter value was then assigned to the corresponding tract-weeks within that quarter. This process produced 104,475 tract-week observations with an officer-presence measure before model-specific restrictions. To use the available officer-presence data together in the main analysis, the 2017 and 2019 records were pooled, while Appendix Tables A2 and A3 retain the two years separately.

Officer hours were retained in their original units and were also transformed using the inverse hyperbolic sine. The transformation reduces skewness while preserving observations in which officer hours equal zero. Meanwhile, a tract-week without a matched value in the source data remains missing rather than being recoded as zero, since the absence of a matched record does not necessarily indicate zero officer presence. Both the raw and transformed officer-hour measures were retained for analysis.

ZTRAX-Derived Aggregate Housing Measures

Chapter 3 uses a custom aggregate tract-week dataset created from licensed ZTRAX extracts for Los Angeles County. The retained file contains only aggregate tract-week measures and does not contain names or individual- or transaction-level records. Its measures include mean inflation-adjusted and logged home sale prices, mean housing characteristics, and aggregate predicted buyer- and seller-composition shares. The predicted composition measures are probabilistic aggregates and should not be interpreted as observed or self-identified racial or ethnic identities.

The aggregate housing file contains 85,734 tract-weeks with a reported home-sale measure across 904 retained census tracts. After merging this file with the complete treatment panel, tract-weeks without an aggregate sale-price measure were left missing rather than assigned a value of zero.

Predicted Buyer- and Seller-Composition Measures

The retained dataset contains aggregate tract-week shares for six predicted buyer- and seller-composition categories: White, Black or African American, Asian, Hispanic, American Indian or Alaska Native, and other race. These measures are probabilistic aggregates as opposed to observed or self-identified racial or ethnic identities, and they should not be interpreted as classifications of particular people. Because the measures are prediction-based, they may include classification error, particularly for multiracial and sparsely represented groups.

Table 3 summarizes the aggregate predicted buyer- and seller-composition shares separately for ever-treated tracts, never-treated tracts, and all tracts represented in the qualifying-sales data. The regression models use the corresponding tract-week composition shares as aggregate covariates. The retained data used in these analyses contain no names or individual- or transaction-level records.

Aggregate Sale-Price and Housing Measures

The retained dataset contains tract-week means of inflation-adjusted home sale prices, logged inflation-adjusted home sale prices, lot size, building area, year built, bedrooms, and bathrooms. Sale-price measures are expressed in 2019 dollars, and the retained measures reflect the first- and ninety-ninth-percentile trimming applied when the aggregate file was created. The primary housing-market outcome is mean logged home sale price at the tract-week level. Appendix A reports the corresponding non-logged, inflation-adjusted tract-week means.

CONCLUSION

In summary, the Los Angeles Police Department (L.A.P.D.) developed Operation L.A.S.E.R. (the Los Angeles Strategic Extraction and Restoration program) in collaboration with Justice & Security Strategies, Inc. (J.S.S.) to mitigate serious violent crime in Los Angeles, California. Throughout the program's lifespan, the pair conducted a series of evaluations to assess whether the program reduced crime in treated areas by focusing on repeat offenders and crime-prone locations. However, evaluating the "success" of a proactive policing intervention requires more than assessing whether crime increased or decreased in treated locations, as research shows that increased police activity may also shape educational outcomes, influence neighborhood housing markets, and produce consequences not captured by crime statistics alone. Thus, the present dissertation examined the association between Operation L.A.S.E.R. and three outcomes: crime and displacement, academic performance, and neighborhood desirability, as measured by home sale prices.

Overall, the findings do not provide a simple basis for either fully endorsing or fully condemning Operation L.A.S.E.R. For instance, Chapter 1 finds that after the L.A.P.D. and J.S.S. implemented Operation L.A.S.E.R., crime declined in some treated areas but not others, with little evidence of crime displacement into adjacent, non-treated census blocks. Meanwhile, Chapter 2 finds that school-level exposure to Operation L.A.S.E.R. was not associated with statistically significant changes in assessment scores at conventional levels. Lastly, Chapter 3 finds that Operation L.A.S.E.R. was associated with increased home sale prices in treated neighborhoods, with potential benefits for homeowners and potential costs for renters. The pooled officer-presence models also produced positive estimates for raw and transformed ping hours, although neither was statistically significant at conventional levels. As a result, judging the intervention as simply “successful” or “unsuccessful” overlooks the differences observed across the three chapters.

Crime Reductions and Displacement

Across the three empirical chapters, Chapter 1 provides the strongest support for Operation L.A.S.E.R. Specifically, the division-level analysis identified downward crime trends following implementation in the 77th Street and Newton Divisions, with several estimates reaching conventional levels of statistical significance. However, the same pattern was not observed in the Southwest Division, and the immediate-shift estimates were less consistent across the three divisions.

The L.A.S.E.R. Zone analysis also produced mixed results. Across the 85 zone-by-outcome comparisons, crime decreased in 43 cases, increased in 40, and remained unchanged in two. At the census block level, treated blocks did not show meaningful crime reductions relative to adjacent, non-treated blocks. At the same time, the models did not identify statistically significant increases in adjacent, non-treated blocks that would indicate crime displacement.

Thus, Chapter 1 provides some evidence supporting the intervention, particularly because crime declined in certain divisions with little evidence of displacement into adjacent, non-treated blocks. However, the mixed pattern also warrants caution. Swatt and Uchida (2016) reported similarly mixed results, noting that gains were not consistent across L.A.S.E.R. Zones and that additional observations were needed, but ultimately concluded that Operation L.A.S.E.R. appeared promising and should be continued.

Consequently, the L.A.P.D. expanded the intervention to additional divisions. Given the policy history and controversy surrounding Operation L.A.S.E.R., as discussed in the introductory chapter, the present findings suggest that the mixed evidence on crime reduction is better viewed as a reason for continued evaluation than as sufficient support for further expansion.

Academic Performance

Meanwhile, Chapter 2 finds little evidence that exposure to Operation L.A.S.E.R. was associated with notable changes in assessment scores among high school students at traditional Los Angeles Unified School District (L.A.U.S.D.) schools. During years of active exposure, English Language Arts (E.L.A.) scores were estimated to be 7.31 points lower, and Mathematics scores were estimated to be 6.86 points lower. However, neither estimate reached conventional levels of statistical significance, and the differences of less than 10 points were small relative to school-level assessment scores, which were generally measured in the mid-2000s.

Still, these findings should not be taken as evidence that students experienced no consequences from Operation L.A.S.E.R., as only seven schools in the final sample were exposed to treatment, and school-level averages cannot identify which students lived in L.A.S.E.R. Zones, encountered police, or experienced changes in neighborhood safety. In addition, assessment performance represents only one educational outcome. Attendance, graduation, school discipline, feelings of safety, stress, and direct encounters with police may have followed different patterns.

Neighborhood Desirability

Chapter 3 presents a more complex set of findings. Specifically, the results show that Operation L.A.S.E.R. was concentrated in Hispanic-majority, renter-heavy neighborhoods, with home sale prices consistently below the citywide average. The pooled officer-presence models produced positive estimates for both raw and transformed ping hours, although neither was statistically significant at conventional levels. Meanwhile, treatment was positively associated with higher logged home sale prices in treated tract-weeks. Additionally, the timing analyses suggest that differences in home sale prices accumulated gradually with continued exposure rather than appearing immediately after treatment began. Yet the increase in home values did not coincide with rapid demographic turnover during the period examined.

In essence, the findings suggest that higher home values may have different consequences depending on whether residents own or rent their homes. For homeowners, appreciation can increase household wealth by raising the value of an existing asset. Renters, however, do not receive the same financial benefit and may instead face higher housing costs if rising property values are accompanied by rent increases. Additionally, because renters represented most households in neighborhoods exposed to Operation L.A.S.E.R., they also represented the largest tenure group exposed to the intervention and its surveillance. Given that Hispanic renters comprised the largest tenure group in these neighborhoods, the distribution of these potential benefits and costs is especially important when interpreting the findings.

Implications

The findings have several implications for evaluating future proactive policing programs. First, because these interventions are designed primarily to reduce crime, evaluations will understandably place considerable emphasis on crime outcomes. However, crime should not be the sole outcome considered. Police interventions operate within communities where residents attend school, work, rent homes, own property, and interact with public institutions. As a result, evaluations focused only on arrests, reported crime, or officer activity may overlook other changes occurring alongside the intervention.

Second, evaluations should also account for how an intervention's benefits and costs are distributed among residents. For example, higher home values may represent a financial benefit for homeowners while providing no comparable wealth-building benefit for renters in the same neighborhoods. Given that renters represented most households in neighborhoods exposed to Operation L.A.S.E.R., the benefits associated with higher home values were not equally available to all residents. Therefore, policymakers should consider not only whether neighborhood conditions improve, but also which residents are positioned to benefit from those improvements and which may face additional costs.

Lastly, the present dissertation shows why public institutions should maintain accurate, accessible, and usable records for programs that rely on data and geographic targeting. Several parts of the present analysis required reconstructing L.A.S.E.R. Zone boundaries, implementation dates, and other program details from maps and administrative records obtained through public records requests and related litigation. Although these records ultimately made it possible to identify the locations and timing of L.A.S.E.R. Zones, they did not include a consolidated dataset or G.I.S. shapefile that could be used for analysis. As a result, the present study had to reconstruct the boundaries and create the spatial files needed to evaluate the program.

A separate challenge involved the publicly available L.A.P.D. crime data obtained through the City of Los Angeles. Although these records made an independent analysis possible, they did not fully align with the data used in the original evaluation. In particular, data quality problems limited how closely portions of the 2016 analysis could be reproduced and prevented the present study from retaining every division examined in the original report. Thus, making data publicly available is an important first step; however, the records must also be sufficiently complete and consistent to support independent evaluation.

Given these limitations, police departments implementing similar programs should maintain consistent records of treatment locations, dates, officer activity, stops, arrests, and changes in program design. Those records should also be made publicly available in formats that researchers can readily use, as greater access to complete and usable records would improve transparency, make independent evaluations easier to conduct, and allow researchers to determine whether reported findings can be reproduced. In doing so, agencies can provide researchers and the public with a clearer basis for assessing how these programs operate and whether the evidence used to support them can be independently examined.

Future Directions

Given that Chapter 1 focuses on the 2015 expansion, future research could examine Operation L.A.S.E.R. as the program later spread to other L.A.P.D. bureaus and divisions. The divisions included in Chapter 1 were located relatively close to one another and largely focused on violent, gun-, and gang-related crime. As Operation L.A.S.E.R. expanded to other parts of Los Angeles, however, divisions faced different crime patterns and placed greater attention on different crime types. Comparing these later implementations could help determine whether the crime patterns observed in Chapter 1 were limited to the 2015 expansion or were also present as Operation L.A.S.E.R. spread across the city.

The educational analysis could also extend beyond school-level assessment scores. Student-level records could examine attendance, graduation, disciplinary outcomes, school transfers, and academic performance among students who lived in or regularly traveled through L.A.S.E.R. Zones. Linking those outcomes to violent crime near schools and measures of police activity would also help determine whether changes in neighborhood safety and police contact moved in different directions.

For housing outcomes, additional research could examine changes that home sale prices alone cannot capture. Information on rents, evictions, and residential moves could help determine whether rising home values were accompanied by greater financial pressure on existing residents. In addition, following treated neighborhoods beyond the end of Operation L.A.S.E.R. in 2019 could help determine whether the observed appreciation persisted or whether the relationship weakened over time.

Overall, the findings suggest that Operation L.A.S.E.R. was associated with some potentially beneficial outcomes, but the benefits and costs were not necessarily equal across communities. As a result, agencies considering implementing similar programs should evaluate more than whether crime increased or decreased after implementation. Although crime reductions can improve community well-being, evaluations should also consider which residents are most exposed to the intervention, who is positioned to benefit from any resulting changes, and who may bear additional costs.

In addition, policymakers should anticipate and mitigate potential harms, particularly when burdens are likely to fall disproportionately on communities already facing social and economic disadvantage, as even seemingly beneficial outcomes, such as higher home values, may carry different consequences for homeowners and renters. For that reason, transparency about where proactive policing programs operate, how they are implemented, and how their benefits and costs are assessed should remain an important part of program design and evaluation.