SURVEILLANCE CAMERAS AND CRIME: A REVIEW OF RANDOMIZED AND NATURAL EXPERIMENTS

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ABSTRACT: Research on the effectiveness of surveillance cameras in reducing crime suffers from potential threats to causal validity. This paper reviews seven studies that address some of these problems using the rigorous research designs of randomized and natural experiments. Included studies that reported changes in total crime found crime reductions ranging from 24-28% in public streets and urban subway stations, but no desirable effects in parking facilities or suburban subway stations. Moreover, surveillance cameras may help reduce unruly behaviour in football stadiums and theft in supermarkets/mass merchant stores. These findings indicate that video surveillance can reduce crime in several settings.

Keywords: Surveillance cameras; CCTV; crime prevention; randomized experiments; natural experiments; review

Introduction

Video surveillance or closed-circuit television (CCTV) has become widely used in many parts of the world. Although its potential applications include detecting, investigating, and reducing fear of crime, empirical research has mainly focused on its use in crime prevention (Welsh & Farrington, 2008). The most frequently cited mechanism through which video surveillance may deter crime is its potential effect on the perceived certainty of punishment (Piza, Caplan, Kennedy, & Gilchrist, 2015). However, there are methodological problems associated with demonstrating a causal relationship between surveillance cameras and crime (Stutzer & Zehnder, 2013). It requires, among other things, the use of a control condition to estimate the counterfactual situation in which video surveillance was not introduced (Farrington, 2003). Unfortunately, evaluations of public area surveillance interventions have often used weak research designs, sometimes lacking controls entirely (Welsh, Peel, Farrington, Elffers, & Braga, 2011).

There is, however, also a substantial literature that examines the effects of surveillance cameras using *quasi-experimental* research designs involving before-and-after measures of crime in both the experimental area(s) and at least one control area (Welsh and Farrington, 2008). A noteworthy example is Gill and Spriggs' (2005) seminal study of 13 video surveillance projects in the UK. Although the study found a 'relatively substantial reduction' of police reported crime in six of the projects, only two of these reductions were statistically significant and the authors argue that one of the two may be explained by the presence of confounding variables. Similarly, victimization surveys found no statistically significant changes in crime (Gill and Spriggs, 2005).

Welsh and Farrington (2008) present a comprehensive systematic review and meta-analysis of 41 quasi-experiments whose follow-up periods ranged between 3-60 months. Their findings indicate that surveillance cameras reduced crime by on average 16%. However, this result was mainly driven by a large crime decline in car parks. No significant desirable effects were found in other public settings. Some of the included studies also found spatial *displacement* (i.e. crime increases in areas adjacent to the experimental condition), but others found no such effects or even *diffusion of benefit* (i.e. crime decreases in adjacent areas).

The problem of endogeneity

Credible estimation of causal relationships requires that the causal variable is at least partly *exogenous* (Mitchell, 2015). In the context of video surveillance research, exogeneity implies that the allocation of surveillance cameras must be *uncorrelated* with unobserved factors that determine crime. Unfortunately, given that institutions often deploy surveillance cameras in response to upward crime trends (Priks, 2015b), unobserved causes of such trends are likely to be *correlated* with cameras. Video surveillance research may therefore suffer from the

problem of *endogenous* (i.e. non-exogenous) camera installations (Priks, 2014). As the quotation below illustrates, such endogeneity may introduce biases in either direction:

> *CCTV schemes are installed subsequent to intensified episodes of criminal activity. Depending on the evaluation framework, biases in either direction might emerge. Regression towards the mean (i.e. an extreme level of crime in an area in one period is followed by a moderate level of crime in the next period) might spuriously indicate a deterrence effect after the installation of CCTV. If the exogenous effect on crime, which also provoked the adoption of CCTV, remains, CCTV schemes are spuriously associated with higher crime rates. (Stutzer & Zehnder, 2013)*

The endogeneity problem described in the quotation can be viewed as an instance of *selection bias*, which is a persistent threat to the causal validity of quasi-experiments (Mitchell, 2015). More generally, the problem of non-comparable experimental and control areas is widely recognized in video surveillance research. For instance, Gill and Spriggs (2005) acknowledge that their study was 'constrained by the difficulty of identifying a suitable control for each target area'. Similarly, Welsh and Farrington (2008) note about the studies included in their meta-analysis that 'in some cases investigators did not provide sufficient detail to allow for a determination that the experimental and control areas were comparable on the most important dimensions'. One might therefore question the extent to which the control areas used in this literature provide credible estimates of the counterfactual scenario in which video surveillance was never introduced in the experimental areas.

Randomized and natural experiments

It is widely believed that the *randomized experiment* is the research design that deals with selection bias most convincingly (e.g. Sherman et al., 1997). In these studies, researchers randomly assign multiple areas to either experimental or control conditions. This ensures that the causal variable is exogenous and that experimental and control areas are equal *in*

expectation on all variables before the introduction of the intervention (Mitchell, 2015). Well-conducted randomized experiments can therefore provide unbiased estimates of the average causal effect (Deaton & Cartwright, 2016).

The exogeneity condition can also be fulfilled by the means of *natural experiments*. Some authors use 'quasi-experiment' and 'natural experiment' interchangeably, but in the present paper, the latter term (and only the latter term) refers to research designs that exploit exogenous variation in the causal variable arising from natural or social factors (Mitchell, 2015). For instance, researchers could exploit situations in which bureaucratic factors render surveillance cameras exogenous by distributing them *as-if*-randomly between different areas. Natural experiments can therefore be seen as a way of imitating randomized experiments as far as possible (Priks, 2015b). In particular, ideal randomized and natural experiments share the virtue of alleviating selection bias.

The orthodox view within evidence-based policy is that randomized experiments are the 'gold standard' for determining 'what works' (Sherman et al., 1997; though see Deaton & Cartwright, 2016). Accordingly, randomized experiments have now been used to evaluate many criminological interventions, including place-based ones such as hot spots policing (Braga, 2005). In empirical microeconomics, the increased use of randomized experiments has been paralleled by a rise in the number of natural experiments – a development that is part of what is sometimes called the "credibility revolution" (Angrist & Pischke, 2010). There are now many natural experiments in the economics of crime literature (see Priks, 2015b, for a review) and the method is making inroads in criminology (Mitchell, 2015).

Theoretical arguments for adopting strong research designs such as randomized and natural experiments are complemented by empirical research. Comprehensive reviews have found an inverse relationship between study outcomes and the causal validity of research designs both in evaluations of crime prevention generally (Weisburd, Lum, & Petrosino, 2001) and public area surveillance specifically (Welsh et al., 2011). In contrast, a review by Braga (2005) found that 'the most powerful effects in favor of the hot spots policing treatment were associated with [randomized] experimental evaluations'. The important take away for the purpose of this paper is that the strength of research design may indeed matter for study outcomes.

Given the methodological advantages of randomized and natural experiments, there is a need for a review of video surveillance evaluations that use such research designs. Unfortunately, previous reviews were conducted prior to the emergence of this literature (Phillips, 1999; Welsh & Farrington, 2008). The aim of the present paper is therefore to provide a literature review of randomized and natural experiments that examine the effects of surveillance cameras on crime. This may serve as a complement to previous reviews that rely exclusively on quasi-experiments.

Methodology

Literature reviews can be divided into *systematic reviews*, which use extensive literature searches and pre-established inclusion criteria to identify studies whose findings are then assessed and synthesized following strict guidelines, and *narrative reviews*, which lack some or all of these features. The present review is best classified as a narrative review, but incorporates some features of systematic reviews. In line with previous narrative reviews of video surveillance research (Phillips, 1999), the initial plan was not to have any inclusion criteria beyond the statement of purpose: to review randomized and natural experiment that

examine the effects of surveillance cameras on crime. However, this statement turned out to be too vague to assist in deciding whether, or not, to include a randomized experiment by Piza et al. (2015). Although that study seems to be of high quality and, in one sense, assesses the effects of video surveillance on crime, it seemed inappropriate to include it since the causal variable was not video surveillance but rather police patrols that were randomly assigned to locations where cameras were already in place. To achieve transparency about such inclusion/exclusion-decisions, inclusion criteria were developed. The most important difference between the criteria of the present paper and ones used by Welsh and Farrington (2008) (on which my criteria are partly based) is that I excluded all research designs except randomized and natural experiments. Studies were included only if:

(1) *Video surveillance was the main intervention*. The causal variable had to be surveillance cameras and if other interventions were deployed simultaneously, video surveillance had to be the main intervention.
(2) *There was an outcome measure of crime*. Both police reports and unofficial measures such as victimization surveys were acceptable insofar that crime was measured.
(3) *The research design involved before-and-after measures of crime in multiple areas assigned to experimental or control conditions on the basis of either (i) randomization or (ii) exogenous variation in the causal variable arising from external factors*. This criterion excludes all studies except randomized and natural experiments.
(4) *The results of the evaluation were reported in English.*

The search strategy used search terms such as 'CCTV', 'surveillance cameras', 'video surveillance' in conjunction with terms such as 'randomized experiment', 'randomized control trial', 'RCT', 'natural experiment', 'exogenous', 'exogeneity'. There were no restrictions regarding when or even if the studies were published. Indeed Google and Google

Scholar were used as search engines, which allowed identification of research in the grey literature. In addition, two researchers who previously have reviewed the literature were asked whether they knew any randomized or natural experiments that investigated the effects of surveillance cameras on crime.[1][1]

Results

Characteristics of included studies

The search strategy resulted in the identification of seven evaluations[2] – two randomized and five natural experiments – whose characteristics and findings are presented in the Table 1. The included studies were carried out in commuter parking facilities, supermarkets/mass merchant stores, football (soccer) stadiums, subway stations, and public street settings in the United States, Colombia, Uruguay and Sweden. Most studies reported both total crime and a number of selected offences, but two focused exclusively on a single crime category.

The effects of surveillance cameras on crime

Included studies that reported changes in total crime found overall crime reductions ranging from 24-28% in public street settings and urban subway stations (Gómez et al., 2017; Munyo & Rossi, 2016; Priks, 2015a), but no desirable effects in commuter parking facilities or suburban subway stations (La Vigne & Lowry, 2011; Priks, 2015a). One study conducted in public streets did not report changes in total crime rates, but found that police reported property crimes declined by approximately 30% (King et al., 2008). Furthermore, the two included studies that only reported changes in one crime category found a 73% decline of shaving blade theft reported by store employees (Hayes & Downs, 2011) and a 65% reduction of referee-reported incidents in which spectators threw objects onto fields during football games (Priks, 2014). It should be noted, however, that the extent to which these results are statistically significant differ across model specifications (see Table 1).

Most of the crime reductions found by the included studies concerned property crimes such as theft or pickpocketing (Gómez et al., 2017; Hayes & Downs, 2011; King et al., 2008; Munyo & Rossi, 2016; Priks, 2015a). Some included studies also indicate that surveillance cameras can reduce certain types of violent crime, in particular unruly spectator behaviour (Priks, 2014) and robbery (Munyo & Rossi, 2016; Priks, 2015a; though King et al., 2008, found no such effect). However, no included study provided evidence of declines in aggregate violent crime, homicide, assault or sexual offenses (Gómez et al., 2017; King et al., 2008; Munyo & Rossi, 2016; Priks, 2015a).

Displacement/diffusion of benefit

Most included studies that assessed displacement/diffusion provided no evidence of such effects (Gómez et al., 2017; King et al., 2008; La Vigne & Lowry, 2011; Priks, 2014). However, one study found local displacement (Priks, 2015a) and another found local diffusion of benefit (Munyo and Rossi, 2016). The study by Munyo and Rossi (2016) was the only one that, in addition to assessing local displacement/diffusion, also examined the effects of surveillance cameras on aggregate crime in the city as a whole. Their analysis indicates that the crime reduction in monitored areas was fully compensated by a crime increase in unmonitored parts of the city.

Discussion

The aim of the present paper was to provide a literature review of randomized and natural experiments that examine the effects of surveillance cameras on crime. The results reported in the previous section indicate that surveillance cameras can reduce crime, especially property offences, in several settings. Evidence on the prevalence of displacement and diffusion of benefit was mixed.

Contextual factors

A noteworthy finding of the present review is that that surveillance cameras may cause significant crime reductions in public streets and subway stations in the city centre (King et al., 2008; Priks, 2015a; Munyo & Rossi, 2016; Gómez et al., 2017). In contrast, the previously mentioned meta-analysis found no significant desirable average effects of video surveillance in city centre and public transport settings (Welsh & Farrington, 2008). These facts suggest a divergence in the findings of the present review and the meta-analysis.[3]

One possible explanation for this divergence is that that the quasi-experimental designs of previous research might underestimate the effects of surveillance cameras in these settings (see the quotation from Stutzer & Zehnder, 2013, in the introduction). However, research design is not the only factor that differs between the present review and the meta-analysis. First, studies included in the present review were carried out in the Americas and Sweden whereas the meta-analysis mostly relied on studies conducted in the UK (Welsh & Farrington, 2008), where, it has been argued, video surveillance takes place in a unique political context due to Britain's large-scale public investments in the technology during the 1990s (Weaver & Lahtinen, 2016). Second, and more importantly, the studies included in the present review generally examined more recent video surveillance systems whose effectiveness may have benefited from technological improvements. In particular, Welsh and Farrington's (2008) meta-analysis mainly includes studies that were conducted prior to the widespread transition from analogue CCTV systems to digital IP cameras that allow for higher resolution (Weaver & Lahtinen, 2016).

There are also human factors that impact the effectiveness of surveillance cameras (Taylor & Gill, 2014). Some recent studies suggest that video surveillance may reduce crime more

effectively when cameras are actively monitored and used in real-time to inform police patrols (La Vigne, Lowry, Markman, & Dwyer, 2011; Piza et al., 2015; though see Gerell, 2016). At least two different mechanisms might account for this. First, actively monitored cameras may assist police arrests and therefore reinforce the main deterrence mechanism of surveillance cameras, which is to increase the perceived certainty of punishment (cf. Piza et al, 2015). Second, active monitoring may enable the police to intervene before serious crimes occur (Gerell, 2016).

King et al.'s (2008) study indicates a reduction of property crime despite the absence of active monitoring. However, the fact that video surveillance was integrated with existing police patrols may well have contributed to its effectiveness in the other three studies that were conducted in public streets or subway stations (Gómez et al., 2017; Munyo & Rossi, 2016; Priks, 2015a). For instance, Priks (2015a) suggests that the possibility of rapid response by nearby security guards and police officers may explain why surveillance cameras caused crime reductions in subway stations in the city, but not in suburban stations.

The lack of active monitoring informing police patrols might also explain why the randomized experiment by La Vigne and Lowry (2011) found no desirable effects of video surveillance in parking facilities despite the fact that previous research (mostly evaluations of actively monitored systems) indicates that surveillance cameras are more effective in car parks than in other settings (Welsh & Farrington, 2008). However, it is also possible that prior evaluations overestimate the effects of video surveillance in car parks due to problems with confounding (cf. Zehnder, 2009) or the difficulty of ruling out regression towards the mean using quasi-experiments (cf. Farrington, 2003).

Limitations

The most obvious limitation of the present review is that it is a narrative review and not a systematic review. For instance, I used only two search engines and focused exclusively on papers published in English. These methodological choices were made due to lack of resources, but may potentially have resulted in the exclusion of eligible studies. Although this possibility cannot be ruled out entirely, it is somewhat reassuring that the search strategy also involved the consultation of two experts that have previously reviewed the literature.

A more important deviation from systematic reviews is the fact that the inclusion criteria were not established before the literature search was initiated. It is therefore possible that the results of the literature search biased the inclusion criteria that eventually were developed. However, since it was decided prior to the literature search that the aim of the study was to review randomized and natural experiments that examine the effects of surveillance cameras on crime, the leeway for such potential biases to substantially affect the inclusion or exclusion of studies was small.

Another limitation is that only seven studies matching the inclusion criteria were found, which can be compared to the 41 studies included in the meta-analysis by Welsh and Farrington (2008). Although reviews with seven or even fewer included studies are common in criminology (e.g. Braga 2005), the overall conclusions of such reviews are more likely to be distorted by potential errors in individual studies. However, the very point of this paper was to selectively focus on randomized and natural experiments. Given that the present review is meant to complement rather than replace previous reviews, the smaller number of included studies is an acceptable (and unavoidable) consequence of concentrating on these research designs.

Interpreting the results of the present review also requires understanding of the fact that although randomization renders experimental and control areas equivalent *in expectation*, it does *not* entail equivalence *in any given trial* since factors that influence the effectiveness of the intervention can be unbalanced between experimental and control areas by chance (Deaton & Cartwright, 2016). In general, such imbalance is more likely when the number of experimental and control areas is small, which is the case in some of the included studies (e.g. Hayes & Downs, 2011). These studies should therefore be interpreted with caution.

A further limitation is the possibility of measurement error in the source of crime data. Most included studies used police recorded crime statistics (Gómez et al., 2017; King et al., 2008; La Vigne & Lowry, 2011; Munyo & Rossi, 2016; Priks, 2015a), but evidence from, for example, research on police interventions indicates that measurement error in official crime statistics can result in estimation biases (Vollard & Hamed, 2012). In the context of video surveillance, such biases may emerge if, for instance, camera operators tend to uncover crimes that otherwise would not have been reported (Taylor & Gill, 2014). Moreover, the two included studies that relied on other sources of crime data may also potentially suffer from measurement errors. One of these used data from football referee reports (Priks, 2014) that may be highly subjective (Zehnder, 2009). The other study relied on data from in-store counts of theft, but suffered from counting and recording errors, which may account for the very large effect size that was found in the study (Hayes and Downs, 2011).

Lastly, it should be acknowledged that randomized and natural experiments are not immune to potential endogeneity problems.[4] In randomized experiments, the random assignment that generates exogeneity can be undone by *differential attrition* (cf. Mitchell, 2015), which might have been a problem in the study by Hayes and Downs (2011). In natural experiments, the

lack of random assignment forces investigators to provide a convincing institutional and/or data-driven argument for exogeneity (cf. Angrist & Pischke, 2010). However, although the natural experiments included in the present review are generally supported by such arguments, one can of course not be completely certain in the exogeneity of camera installations. Despite these potential problems, the studies included in the present review seem far more likely to fulfil the exogeneity condition than most other studies found in the literature.

Implications for future research

Many aspects of video surveillance are not addressed by the present review. These include its use in detecting, investigating, and reducing fear of crime as well as broader matters concerning its effects on peoples' expectation of privacy. Some of these issues have been somewhat neglected and constitute promising opportunities for future research. In this section, however, I want to highlight on two other relatively unexplored research questions that are both important for policy and more closely linked to the present review.

The first issue concerns the prevalence of displacement effects. Although the present review provides little evidence of displacement *to adjacent areas*, Munyo and Rossi's (2016) findings from Montevideo indicate that the entire crime reduction in experimental areas was compensated by a crime increase in other parts of the city. One should of course be cautious of generalising from a single study, but general equilibrium effects of this kind are worrying and more knowledge about their frequency would be important for policy makers. In addition to assessing local displacement, future evaluations of large-scale video surveillance interventions could therefore provide additional policy relevance by examining the prevalence of such effects.

The second issue that I would like to highlight in the context of future research is the divergent effects of surveillance cameras across different types of offences. In accordance with previous research (Welsh & Farrington, 2008), the present review found more robust effects on property crimes than on violent crimes. Some empirical evidence suggests that one explanation for the corresponding divergence between property and violent crimes found in police research is that intensified police presence increases the share of violent crime that finds its way into police-recorded crime statistics (Vollard & Hamed, 2012). However, it is not clear that an analogous measurement error could explain the divergence found in video surveillance research. A more widespread and, in my opinion, at least equally plausible explanation is that property crimes are planned whereas violent offences are crimes of passion whose perpetrators are less sensitive to detection and punishment (e.g. Priks, 2014). Given that these two explanations yield very different implications for the use of surveillance cameras in reduction of violent crime, future randomized and natural experiments could provide particularly valuable policy insights by complementing police statistics with other sources of crime data.

Concluding remarks

A general conclusion of the present review is that findings of randomized and natural experiments indicate that surveillance cameras can reduce crime, in particular property offences, in several settings. However, there are potential concerns with respect to displacement effects. As discussed above, the included studies are also mostly in line with recent evaluations highlighting the importance of integrating video surveillance with police patrols (La Vigne et al., 2011; Piza et al., 2015). It is hard to provide detailed policy recommendations, though, and policy-makers should be aware of the difficulties associated with transporting findings of one study to other settings (see e.g. Deaton & Cartwright, 2016, for a general discussion). As a final remark, I want to stress that although the methodological

advantages of randomized and natural experiments render such studies highly relevant, policy makers should not rely entirely on this body of research, but also be informed by quasi-experimental studies, especially those that address selection bias using matching techniques (e.g. La Vigne et al., 2011; Piza, 2016).

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[1] Consulted experts were: Mikael Priks, Department of Economics at Stockholm University, and Brandon Welsh, School of Criminology and Criminal Justice at Northeastern University. I am grateful for their suggestions.

[2] Although one of the consulted experts suggested that 'it may be possible' that Webb and Laycock (1992) and Sivarajasingam, Shepherd and Matthews (2003) could be classified as natural experiments, they were excluded since the authors did not provide any reason for believing that camera installations were exogenous.

[3] This divergence should not be exaggerated since there are several exceptions. For instance, the study by Priks (2015a) in the present review found no significant crime changes in the subway system as a whole, whereas the study by Webb and Laycock (1992) in Welsh and Farrington's (2008) meta-analysis found a reduction of robbery.

[4] Indeed measurement errors, including some of the ones discussed in the previous paragraph, may also induce endogeneity.

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Table 1: The effects of surveillance cameras on crime perpetration

Reference	Setting	Research design	Outcome measure (data source)	Findings	
				Crime reduction in experimental areas relative to controls	**Displacement/diffusion of benefit**
Randomized experiments					
Hayes and Downs (2011)	25 mass merchant/ supermarket stores in the United States	Stores were randomly assigned either to the control condition or to in-aisle CCTV dome cameras (other experimental conditions are irrelevant in the present context).	Shaving blade theft (in-store counts by employees)	Shaving blade theft[1]: –73%[a]	Not reported
La Vigne and Lowry (2011)	100 commuter parking facilities in Washington DC, United States	Parking facilities were blocked and randomly assigned either to the control condition or to a combination of live and dummy cameras.	Total, property, violent and selected crimes (police reports)	No significant changes in total crime, property crime, larceny theft, violent crime, car crime, auto theft, attempted auto theft or theft from auto	No evidence of displacement or diffusion of benefits
Natural experiments					
King, Mulligan, and Raphael (2008)	19 public street sites in San Francisco, CA, United States	The evaluation strategy exploited differences in the timing of camera installations.[2]	Property, violent, and selected crimes (police reports)	Property crime: −30%[c] Property crimes *in public places*: ↓[a-b] Larceny theft: ↓[b] Larceny theft in public places: ↓[a] No significant changes in violent crime, burglary, motor vehicle theft, assault, robbery, sex offences, drug incidents or prostitution/vandalism/suspicious occurrences.[3]	No evidence of displacement or diffusion of benefits
Priks (2014)	13 football (soccer) stadiums in Sweden	The evaluation strategy exploited exogenous variation in the timing of camera installations occurring due differential processing times for camera permits and delays in the	Spectators throwing objects onto football fields (referee	Unruly behaviour (incidents where spectators threw objects such as coins, bottles, and lighters onto the field): –64%[b]	No evidence of displacement or diffusion of benefits

		supply of equipment.	reports)		
Priks (2015a)	100 subway stations in Stockholm, Sweden	The evaluation strategy exploited exogenous variation in the timing of camera installations occurring due to differential application times for camera permits.	Total and selected crimes (police reports)	*All stations:* Total crime: no significant change *Stations in the city centre:* Total crime: –25%[b-c] Pickpocketing: –23%[a-d] Robbery: –60%[b-d] No significant changes in assaults or drug-related crimes	*Local displacement:* 15% of the deterred crimes were displaced to areas surrounding the stations where cameras were not used.
Munyo and Rossi (2016),	277 public street sites in Montevideo, Uruguay	The evaluation strategy exploited exogenous variation in the timing of camera installations occurring due to the fact that installation sites were not prioritized or driven by crime incidence.	Total and selected crimes (police reports)	Total crime: –28%[a] Outdoor crime: ↓[a] Theft: ↓[a] Robbery: ↓[a] No significant reduction of indoor crime (assaults and domestic violence) was found	*Local diffusion:* Crime declined in street segments that were contiguous to monitored streets segments. *General displacement:* Crime reduction in the target areas was fully compensated by a crime increase in unmonitored parts of the city.
Gómez, Mejía, and Tabón (2017)	587 public street sites in Medellin, Colombia	The evaluation strategy exploited exogenous variation in the timing of camera installations occurring due to bureaucratic permit/logistic procedures and the fact that installation sites were not prioritized.	Total, property and violent crime (police reports)	Total crime: –24%[b] Property crime (all kinds of theft): –27%[b] Violent crime (homicide and assault): no significant change	No evidence of displacement or diffusion of benefits

Notes. ↓: Crime reduction that was at least marginally significant in one model specification and whose magnitude was not reported in the original paper, *a:* Significance at the 1% level, *b:* Significance at the 5% level, *c:* Significance at the 10% level, *d:* p-value greater than 10%. Multiple significance signs (e.g. a-d) for one outcome indicate that the original study presented several model specifications that yielded different p-values. In these cases, the reported effect size is the one highlighted by the authors of the original study.

[1] Hayes and Downs (2011) report an odds ratio of 3.66 (95% CI 2.50-5.37) for the CCTV dome condition. However, an error seems to have occurred when the odds ratio was converted to per cent changes in their paper. The conversion should yield the per cent change that I report in this table.

[2] Although King et al. (2008) do not explicitly argue that camera installations were exogenous, the study seemed qualified for the present review given its inclusion in Priks' (2015b) review of 'empirical literature that uses exogenous variation'. The main findings of the present paper are not sensitive to this decision.

[3] King et al. (2008) also analysed the effects of cameras on homicide, but the findings were somewhat complex. Although they found a significant decline in overall homicides, disaggregation of data into homicides occurring in public as opposed to private areas yielded little evidence of a decline.