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CCTV surveillance for crime prevention. A 40-year systematic review with meta-analysis

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CCTV Surveillance for Crime Prevention: A 40-Year Systematic Review with Meta-Analysis

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Abstract

Research Summary:

This paper reports on the findings of an updated systematic review and meta-analysis of the effects of closed-circuit television (CCTV) surveillance cameras on crime. Findings show that CCTV is associated with a significant and modest decrease in crime. The largest and most consistent effects of CCTV were observed in car parks. The analysis also generated evidence of significant crime reductions within other settings, particularly residential areas. CCTV schemes incorporating active monitoring generated larger effect sizes than passive systems. Schemes deploying multiple interventions alongside CCTV generated larger effect sizes than schemes deploying single or no other interventions alongside CCTV.

Policy Implications:

Results of this systematic review—based on 40 years of evaluation research—lend support for the continued use of CCTV to prevent crime as well as provide a greater understanding of some of the key mechanisms of effective use. Of particular salience is the continued need for CCTV to be narrowly targeted on vehicle crimes and property crime and not be deployed as a "stand-alone" crime prevention measure. As CCTV surveillance continues to expand its reach in both public and private space and evolve with new technology, policy will benefit from high-quality evaluations of outcomes and implementation.

Keywords:

Closed-circuit television (CCTV); surveillance; crime prevention; systematic review; meta-analysis

Introduction

Recent decades have seen the emergence of closed-circuit television (CCTV) surveillance as a mainstream crime prevention measure used around the world. Its rise can be traced to Great Britain, where three-quarters of the Home Office budget was allocated to CCTV-related projects from 1996 to 1998 (Armitage, 2002). Such policy decisions increased dramatically the number of CCTV systems in Britain, from approximately 100 in 1990 (Armitage, 2002) to over four million less than two decades later (Farrington, Gill, Waples, and Argomaniz, 2007). In the past decade, cities throughout the United States have likewise made substantial investments in CCTV. According to the most recent estimates, 49% of local police departments in the United States report using CCTV, with usage increasing to 87% for agencies serving jurisdictions with populations of 250,000 or more (Reaves, 2015). The increased prevalence of surveillance cameras in public places has led scholars to consider CCTV as a "banal good" that has become part of everyday life, taken-for-granted by the public and subjected to little scrutiny by the media (Goold, Loader, and Thumala, 2013; Greenberg and Hier, 2009; Hier, 2010; Hier, Greenberg, Walby, and Lett, 2007).

During the early expansion of CCTV, many scholars attributed the marked and sustained growth of this technology to political motivation and public enthusiasm. Painter and Tilley (1999: 2) argued that CCTV's rise in Britain was due to the "surface plausibility" of the measure and the political benefits officials expected from "being seen to be doing something visible to widespread concerns over crime…." Pease (1999: 53) further lamented that policymakers seemingly did not readily consult the scientific evidence when considering the adoption of CCTV, stating: "one is tempted to ask where rigorous standards went into the headlong rush to CCTV deployment."

While research on CCTV was once sparse, the state of the literature can no longer be described as such. The number of CCTV evaluations has increased significantly over time.

Furthermore, while public surveillance research in general has been previously described as methodologically weak, with over 55% of studies using less than a comparable experimental-control area design (Welsh, Peel, Farrington, Elffers, and Braga, 2011), rigorous designs have been increasingly used in the study of CCTV. We now have several examples of randomized field trials testing the effect of video surveillance cameras as a stand-alone crime deterrent (Hayes and Downs, 2011; La Vigne and Lowry, 2011) or as part of proactive place-based patrol strategies (Piza, Caplan, Kennedy, and Gilchrist, 2015). Others have used sophisticated matching techniques in the absence of randomization to help ensure statistical equivalence between treatment and control conditions (Farrington, Gill, Waples, and Argomaniz, 2007; Piza, 2018a). Researchers have also taken advantage of opportunities afforded by naturally occurring social occurrences to reduce problems of endogeneity, when the allocation of CCTV is correlated with unobserved factors that determine crime (Alexandrie, 2017). This increased rigor of the CCTV literature has offered far more insight to help guide policy and practice.

The aim of this paper is to present the results of our updated systematic review and meta-analysis of the crime prevention effects of CCTV. In considering the newly identified evaluations, alongside those included in the last update by Welsh and Farrington (2008, 2009a), the present review includes 80 distinct evaluations of CCTV, representing an 82% increase in studies (from 44). In an attempt to increase understanding on why CCTV may be effective in some contexts but not others (Taylor and Gill, 2014), we follow the approach of the prior systematic reviews (Welsh and Farrington, 2002, 2008, 2009a) by examining CCTV effects across different settings, crime types, and countries, and build upon the prior reviews by incorporating additional moderator variables to measure how effects may vary with different camera monitoring types and the use of other interventions alongside CCTV.

CCTV and Crime Prevention

CCTV is a type of situational crime prevention (SCP) strategy that increases levels of formal surveillance within a target area (Cornish and Clarke, 2003; Welsh and Farrington, 2009: 717). SCP focuses on preventing crime by reducing criminal opportunities and increasing the perceived risk of offending through modification of the physical environment (Clarke, 1995). The situational prevention of crime is largely rooted in the rational choice perspective, which considers crime as "purposive behavior designed to meet the offender's commonplace needs" (Clarke, 1997: 9-10). As per the rational choice perspective, offenders consider a number of "choice structuring properties," which include the potential rewards and inherent risks involved in the commission of a particular crime. The primary aim of CCTV is considered to be the triggering of a perceptual mechanism that impacts an offender's choice structuring properties in a manner that persuades them to abstain from crime (Ratcliffe, 2006).

The research literature indicates that the primary anticipated benefit of CCTV is the prevention of crime, with the majority of evaluations investigating CCTV's effect by measuring crime level changes from "pre" to "post" camera installation periods. While such a research agenda seems to reflect an emphasis on deterrent effects (Piza, Caplan, and Kennedy, 2014a), CCTV can prevent crimes through other mechanisms (Welsh and Farrington, 2009b). Scholars have concluded that increased offender apprehension, increased natural surveillance, publicity, and improved citizen awareness are potential mechanisms of CCTV-generated crime reduction (Gill and Spriggs, 2005). Furthermore, CCTV has the potential to assist police after the commission of crimes, specifically by improving the response of personnel to emergencies (Ratcliffe, 2006), providing visual evidence for use in criminal investigations (Ashby, 2017), and securing early guilty pleas from offenders (Owen, Keats, and Gill, 2006). We must also acknowledge the possibility for CCTV to increase reported crime, as CCTV can detect crimes that would have otherwise gone unreported to police (Winge and Knutsson, 2003) or to make citizens more vulnerable by providing a false sense of security, causing them to relax their vigilance, or stop taking precautions in public settings (Armitage, Smyth, and Pease, 1999).

Systematic reviews and meta-analyses conducted by Welsh and Farrington (2002, 2008, 2009a) have synthesized the empirical knowledge on CCTV. The initial review (Welsh and Farrington, 2002) included 22 evaluations and found that CCTV had a small but significant effect on vehicle crimes and no effect on violent crimes. The updated review (Welsh and Farrington, 2008, 2009a) included 44 evaluations and examined the effect of CCTV across four main settings: city and town centers, public housing, public transport, and car parks. It was found that CCTV was associated with a 16% reduction in crime, a significant effect. This effect was driven by a 51% reduction in crime in the car park schemes, with CCTV in the other settings having small and non-significant effects on crime.

More recently, Alexandrie (2017) reviewed seven randomized and natural experiments of CCTV, finding crime reductions between 24% and 28% in public streets and urban subway stations, but no effect in parking facilities or suburban subway stations. The findings of Alexandrie (2017) diverged somewhat from those of Welsh and Farrington (2008, 2009a). Smaller effect sizes associated with quasi-experiments, varying study settings (i.e., countries), and differing integration with police practices as contextual factors may explain this difference. Recent research supports Alexandrie's (2017) argument that integration with police practices may determine the effects of CCTV (La Vigne, Lowry, Markman, and Dwyer, 2011; Piza, Caplan, and Kennedy, 2014b; Piza, Caplan, Kennedy, and Gilchrist, 2015). However, it is important to note the small number of studies used in Alexandrie (2017) represent a small proportion of the knowledge base on CCTV.

Recent developments in research on and use of CCTV point to the need for an updated systematic review. The present review builds upon the insights provided by the last systematic review, while investigating new questions about the effectiveness of CCTV as a crime prevention modality. We begin with a description of our methodology.

Methodology

Criteria for inclusion of evaluation studies

In following the methodology of systematic reviews, we used a rigorous approach for locating, appraising, and synthesizing evidence from prior evaluation studies (see Welsh and Farrington, 2002, 2008, 2009a). Studies were selected for inclusion in the review according to the following four criteria:

1) CCTV was the main focus of the intervention. For evaluations involving one or more interventions alongside CCTV, only those evaluations in which CCTV was the main intervention were included. We determined the main intervention based upon the author's identification of such. When the authors did not explicitly identify the main intervention, we based this determination on the importance the report gave to CCTV relative to other interventions.

2) The evaluation used an outcome measure of crime.[1]

[1] It should be noted that certain studies include outcome measures of crime that were not derived from police records. Sivarajasingam, Shepherd, and Matthews (2003) included emergency room visits as well as police records to measure incidents of assault injury. We considered both measures in our calculation of effect size. Reid and Andresen (2014) used insurance data along with police recorded data to evaluate vehicle crime in a car park. However, the insurance data totaled less than 20 incidents during the pre-intervention period in the treatment area, so this measure was excluded from our analysis. Scott, Higgs, Caulkins, Aitken, Cogger, and Dietze (2016) measured the purchase and injection of heroin in public settings through a survey of intravenous drug users.

3) The research design involved, at minimum, before-and-after measures of crime in treatment and comparable control areas. This is widely accepted as the minimum interpretable design in evaluation research (Cook and Campbell, 1979).

4) Both the treatment and control areas experienced at least 20 crimes during the pre-intervention period. Any study with less than 20 crimes in the pre-intervention period would lack sufficient statistical power to detect changes in crime.

Search strategies

Systematic reviews incorporate rigorous methods for locating, appraising, and synthesizing evidence from prior evaluation studies, using a similar level of reporting detail that characterizes high-quality reports of original research (Welsh, van der Laan, and Hollis, 2013). In following this framework, we incorporated a rigorous approach to identify evaluation studies for inclusion in our review.

We searched for CCTV evaluations published from 2007 through 2017, to account for the time period since the last review.[2] Five comprehensive search strategies were used to locate studies meeting the inclusion criteria for this review.[3]

1) Searches of electronic bibliographic databases. In total, 11 bibliographic databases were searched using relevant key words:[4] Criminal Justice Abstracts, CrimeSolutions.gov, National Criminal Justice Reference Service (NCJRS) Abstracts, Sociological Abstracts, Educational

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[2] Piza (2018a) was originally published as an early view article in 2016, thus falling within our search period.

[3] Phyllis Schultze of the Gottfredson Library at the Rutgers University School of Criminal Justice assisted us in developing our search strategies. As we conducted the search, she provided further assistance by making available full-text versions of articles we were unable to collect and contacting CCTV evaluation authors and librarians at other universities to obtain titles not housed at the Rutgers library.

[4] The following search terms were used: CCTV, Closed-Circuit Television, Video Surveillance, Public Surveillance, Formal Surveillance, Video Technology, Surveillance Cameras, Camera Technology, and Social Control. Each of these terms was searched on their own and in conjunction with (i.e. "AND") the following: crime, public safety, evaluation.

Resources Information Clearinghouse (ERIC), Google Scholar, Government Publications Office Monthly Catalogue (GPO Monthly), Psychology Information (PsychInfo), Proquest Dissertation and Theses Global, Rutgers Gottfredson Library grey literature database, and the Campbell Collaboration virtual library (www.campbellcollaboration.org/library).

2) Manual searches of CCTV evaluation study bibliographies. As our search progressed, we conducted manual searches of the references section of each study identified for potential inclusion.

3) Manual searches of other CCTV study bibliographies. We conducted manual searches of the following theoretical articles, policy essays, qualitative studies, and literature reviews published in the last ten years: Adams and Ferryman (2015); Alexandrie (2017); Augustina and Clavell (2011); Gannoni, Willis, Taylor, and Lee, (2017); Hempel and Topfer (2009); Hier (2010); Hollis-Peel, Reynald, van Bavel, Elffers, and Welsh (2011); Keval and Sasse (2010); Lett, Hier, and Walby (2012); Lorenc et al. (2013); Piza (2018b); Taylor (2010); Welsh, Farrington, and Taheri (2015); and Woodhouse (2010).

4) Forward searches of CCTV evaluations. We used Google Scholar to conduct forward searches of all evaluation studies identified in the prior review (Welsh and Farrington, 2008, 2009a) as well as during our updated search. Through this process we obtained all articles that cited a study included in this updated review and manually reviewed the references sections.

5) Contacts with leading researchers.

These search strategies identified 68 new CCTV evaluations.[5] Twenty-nine studies did not meet the inclusion criteria and thus were excluded.[6] This process resulted in the collection of 36

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[5] We were unable to obtain an evaluation of CCTV in Cairns, Australia, conducted by Pointing, Hayes-Jonkers, and Clough (2010). We could not determine if this study met the criteria.

[6] Summaries of the excluded studies are provided in Appendix A, which is available as supplemental material.

new evaluations of CCTV that met the inclusion criteria.[7] In considering these new evaluations alongside those included in the last review, the present review includes a total of 80 evaluations, with 76 providing the requisite data to be included in the meta-analysis. Our approach allowed for the inclusion of both published and unpublished studies in the systematic review. Published reports accounted for 34 (44.7%) of the evaluations, with 42 (55.3%) reports coming from the grey literature.

Analytical approach

Meta-analytic techniques were used to assess the effectiveness of CCTV in preventing crime. A comparable measure of effect size and an estimation of its variance are needed in each evaluation (Lipsey and Wilson, 2001). In the case of CCTV evaluations, the measure of effect size had to be based on the number of crimes in the experimental and control areas before and after the intervention. This is because this was the only information that was regularly provided in these evaluations. Here, the odds ratio (OR) is used as the measure of effect size. The OR effect size is best suited for this type of data, and it has a straightforward and meaningful interpretation. It indicates the proportional change in crime in the control area compared with the experimental area. An OR greater than 1.0 indicates a desirable effect of the intervention, and an OR less than 1.0 indicates an undesirable effect. An OR of 1.25, for example, shows that crime increased 25% in the control area relative to the target area. The inverse of the OR communicates the crime difference within the treatment area, with a value of 1.25 indicating that crime

[7] The system in Newark, NJ, was the focus of three separate evaluations. Caplan, Kennedy, and Petrossian (2011) and Piza, Caplan, and Kennedy (2014b) presented a preliminary analysis of the first wave of cameras and a micro-level analysis of individual camera sites in Newark, NJ, respectively. Piza (2018a) evaluated the fully deployed system. We used the findings of Piza (2018a) in the meta-analysis. Waples, Gill, and Fisher (2009) used the findings reported in Gill and Spriggs' (2005) study to demonstrate GIS methods for testing spatial displacement. Given that Waples, Gill, and Fisher (2009) did not present any new evidence about the systems, the findings of Gill and Spriggs' (2005) study were used in our meta-analysis.

decreased by 20% (1 / 1.25 = 0.80) in the treatment area compared to the control area. The OR is calculated from the following formula:

$$OR = (a * d) / (b * c)$$

where *a* is the number of pre-intervention crimes in the treatment area, *b* is the number of post-intervention crimes in the treatment area, *c* is the number of pre-intervention crimes in the control area, and *d* is the number of post-intervention crimes in the control area.

The variance of the OR is calculated from the variance of LOR (the natural logarithm of OR). The typical calculation of variance is as follows:

$$V(LOR) = 1/a + 1/b + 1/c + 1/d$$

This estimation of variance is based on the assumption that the total numbers of crimes (*a, b, c, d*) follow a Poisson distribution. However, much research suggests that extraneous factors that influence crime totals may cause overdispersion. In other words, the variance of the number of crimes (VAR) may exceed the actual number of crimes (N). Where there is overdispersion, V(LOR) should be multiplied by D. By estimating VAR from monthly crime counts, Farrington Gill, Waples, and Argomaniz (2007) derived the following equation:

$$D = 0.008 * N + 1.2$$

In order to obtain a conservative estimate, V(LOR) calculated from the usual formula above was multiplied by D in all cases.

Following the calculation of these measures, we inputted the OR, LOR, and V(LOR) for each evaluation in BioStat's Comprehensive Meta-Analysis software (version 3.0). We conducted all analyses as random effects models under the assumption that effect sizes are heterogeneous across individual evaluations as well as sub-populations of evaluations (Lipsey and Wilson, 2001). In each case, observed *Q* statistics and associated *p* values supported this assumption, demonstrating significantly heterogeneous effect sizes across studies.

In this review, we pay particular attention to the potential influence of outcome measures on observed effect sizes. As discussed by Braga, Weisburd, and Turchan (2018: 12), social scientists commonly do not prioritize examined outcomes, considering the lack of prioritization good practice. However, this complicates the presentation of findings because the choice of reporting one outcome over others may present misleading results (Braga, Weisburd, and Turchan, 2018). This is an important issue in the present review, as the new evaluations include a much wider range of outcomes. In following the analytical approach of recent systematic reviews (Braga, Papachristos, and Hureau, 2014; Braga, Weisburd, and Turchan, 2018), we conduct our meta-analyses based on three approaches. First, all reported outcomes are summed in order to present an overall average effect size statistic. This is a conservative measure of the effect of CCTV. Second, the largest reported effect size for each study is used, which presents a "best-case" estimate. Third, we used the smallest reported effect size for each study to provide a highly conservative measure, representing the lower bound estimate of the effect of CCTV.

Also relevant to this review are the issues of displacement of crime, especially spatial, and the diffusion of crime prevention benefits. Displacement is commonly defined as the unintended increase in crime in other locations consequent from the introduction of a crime prevention program in a targeted location. While the literature has identified five distinct forms of displacement (Reppetto, 1976; see also Barr and Pease, 1990), spatial displacement poses a particular threat to place-based crime prevention efforts, such as CCTV (Guerette and Bowers, 2009). Diffusion of benefits has often been referred to as the complete opposite of displacement: a decrease in crimes not directly targeted by the intervention (Clarke and Weisburd, 1994). To investigate these topics, the minimum design should involve one experimental area, one adjacent comparable control area, and one non-adjacent comparable control area. If crime decreased in the experimental area, increased in the adjacent area, and stayed constant in the control area, this might be evidence of displacement. If crime decreased in the experimental and adjacent areas and stayed constant or increased in the control area, this might be evidence of diffusion of benefits.

Results

Pooled effects

Figure 1 displays the results of the meta-analysis of effect sizes across the 76 studies.[8] The follow-up periods in these evaluations averaged 17.47 months with a low of two months and high of 60 months. Overall, the OR for the CCTV studies was 1.141 ($p < 0.001$), which indicates a modest but significant crime prevention effect. Crime decreased by approximately 13% in CCTV areas compared to control areas. A desirable effect was also found in both the largest- (OR = 1.205, $p < 0.001$) and smallest-effect size (OR = 1.079, $p = 0.026$) analyses.

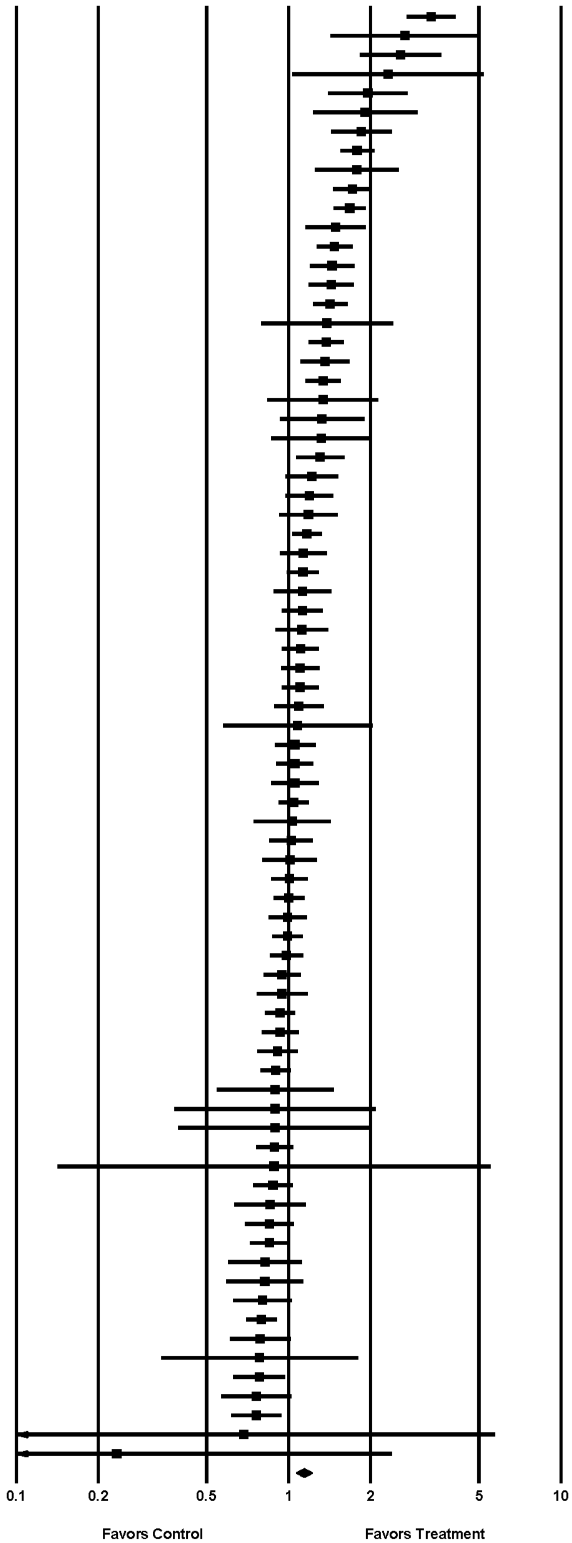
[8] Summaries of the included studies are provided in Appendix B, which is available as supplemental material.

Figure 1. Pooled Effects

Study name	Odds ratio	Lower limit	Upper limit	p-Value
Hawkeye	3.340	2.732	4.084	0.000
Bradford	2.671	1.431	4.986	0.002
Ung (S)	2.576	1.840	3.608	0.000
Malmo (Blixit)	2.316	1.036	5.175	0.041
Coventry	1.952	1.406	2.710	0.000
Birmingham	1.913	1.236	2.961	0.004
Westcap Estate	1.850	1.442	2.375	0.000
Airdire	1.787	1.557	2.051	0.000
Hartlepool	1.779	1.255	2.521	0.001
Montreal (R)	1.712	1.462	2.006	0.000
Seoul	1.675	1.471	1.907	0.000
Sutton	1.488	1.162	1.907	0.002
Gillingham	1.475	1.276	1.705	0.000
Baltimore (TD)	1.444	1.203	1.734	0.000
Glasgow	1.434	1.194	1.724	0.000
Doncaster	1.422	1.239	1.631	0.000
City Hospital	1.384	0.797	2.404	0.249
Burnley	1.375	1.194	1.583	0.000
Denver (D6)	1.359	1.110	1.665	0.003
City Outskirts	1.337	1.160	1.541	0.000
Northern Estate	1.337	0.841	2.124	0.219
Chuncheon	1.328	0.934	1.889	0.114
Ung (N)	1.320	0.865	2.014	0.197
Cincinnati (R)	1.305	1.069	1.594	0.009
Shire Town	1.216	0.978	1.512	0.078
Newark (C)	1.191	0.978	1.449	0.082
Footscray	1.183	0.929	1.506	0.174
Chicago (HP)	1.168	1.038	1.315	0.010
Malaga	1.133	0.933	1.374	0.207
Philadelphia	1.128	0.992	1.283	0.065
Borough Town (C)	1.124	0.888	1.423	0.331
D.C. (MVS)	1.124	0.951	1.328	0.171
Newark (R)	1.116	0.898	1.388	0.323
Mutiple	1.106	0.951	1.285	0.191
Baltimore (G)	1.104	0.944	1.292	0.217
Southwark (C)	1.104	0.950	1.282	0.197
Cincinnati (S)	1.090	0.889	1.338	0.407
Gwand Myeong	1.080	0.579	2.017	0.808
Las Vegas (FS)	1.056	0.893	1.249	0.522
Southwark (EC)	1.054	0.907	1.224	0.494
D.C. (Parking)	1.054	0.867	1.281	0.600
Las Vegas (FS-E)	1.042	0.922	1.178	0.507
Eastcap Estate	1.031	0.749	1.418	0.851
Montreal Metro	1.021	0.856	1.218	0.817
Toronto (D52)	1.011	0.807	1.265	0.927
D.C. (City-Wide)	1.007	0.869	1.166	0.930
Cincinnati (FM)	1.002	0.885	1.134	0.978
L.A. (HB)	0.993	0.849	1.160	0.925
South City	0.989	0.877	1.116	0.856
Cincinnati (N)	0.983	0.857	1.127	0.802
Southwark (E)	0.947	0.812	1.104	0.485
Montreal (C)	0.946	0.767	1.167	0.606
Chicago (WGP)	0.932	0.825	1.052	0.255
Stockholm (M)	0.931	0.801	1.082	0.352
Cincinnati (HP)	0.911	0.773	1.073	0.263
Newcastle	0.896	0.793	1.012	0.077
NYC (PCV)	0.893	0.548	1.457	0.651
NYC (Musheno)	0.891	0.383	2.072	0.789
Ung (C)	0.891	0.396	2.005	0.781
Stockholm (S)	0.888	0.765	1.031	0.119
Warsaw (M)	0.884	0.142	5.497	0.895
Baltimore (NA)	0.875	0.744	1.029	0.107
Toronto (D51)	0.856	0.636	1.151	0.303
Deploy Estate	0.851	0.696	1.040	0.115
Cambridge	0.848	0.725	0.991	0.038
L.A. (JD)	0.819	0.602	1.113	0.201
Surrey, BC	0.817	0.595	1.123	0.213
Borough Town (R)	0.802	0.628	1.023	0.075
Cincinnati (C)	0.794	0.701	0.899	0.000
Market Town	0.786	0.611	1.010	0.060
Malmo (Gerell)	0.781	0.342	1.785	0.558
Dual Estate	0.780	0.630	0.967	0.023
Southcap Estate	0.761	0.568	1.019	0.067
Oslo	0.760	0.618	0.935	0.010
Warsaw (CR)	0.684	0.082	5.694	0.726
Guildford	0.234	0.023	2.379	0.220
	1.141	1.072	1.215	0.000

Statistics for each study

Odds ratio and 95% CI



Note: Random effects model, Q=553.130 , *df*=75, *p*<0.001

Setting

Used as a moderator in the meta-analysis, six categories comprised the geographic setting variable: car park, city/town center, housing,[9] residential,[10] public transport, and other (see Table 1a). In the prior review, residential was included as part of the "other" category because only two CCTV evaluations were conducted in this setting. In the present review, residential was the second most common study setting (n = 16) behind city/town center (n = 33). Public transport and "other" settings were the most infrequent, with four and five evaluations, respectively. Similar to the prior review, observed effects were largest in car parks. However, whereas all other settings previously generated non-significant effects, significant crime reductions were observed outside of car parks, most consistently within residential areas.

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[9] Welsh and Farrington (2008, 2009a) referred to the housing category as "public housing" given that all of the complexes in the identified evaluations were publicly owned. The present review identified CCTV evaluations that were conducted in housing complexes that were privately owned and operated, rendering the "public housing" label inaccurate. Rather than treat the different types of housing complexes separately, we use the more generic label "housing" in reference to all evaluations of CCTV in housing complexes.

[10] Given the potential overlap between the setting categories, we feel that further explanation of the classification is necessary. Residential settings are distinguished from housing in terms of the areas that are under the view of CCTV. In housing schemes, CCTV cameras cover the grounds of the complex, such as the courtyard or areas in front of building entrances. Conversely, residential CCTV schemes cover all public areas, such as streets. Even if a housing complex is present within the view of residential CCTV cameras, such settings were considered residential if public areas, rather than housing-complex property, were the target of surveillance. City/town centers refer to areas primarily comprised of non-residential building types, such as commercial businesses. In most cases, studies explicitly identified the setting type. When the setting type was unclear, we contacted the authors to ask how the study area would be best classified. This ensured that the setting classification met the intent of the study authors.

Table 1. Effects by Setting, Crime Type, and Country

(a) Setting

Category	**N**	**Odds Ratio**	**95% Confidence Interval** Lower	Upper	***p***
Car park	8	1.588	1.054	2.394	0.027
City center	33	1.066	0.986	1.153	0.107
Housing	10	1.028	0.824	1.282	0.805
Residential	16	1.133	1.031	1.245	0.009
Public transport	4	1.370	0.822	2.284	0.227
Other	5	1.265	0.975	1.641	0.077

Q=85.947, df=5, p.<0.001

(b) Crime Type

Category	**N**	**Odds Ratio**	**95% Confidence Interval** Lower	Upper	***P***
Disorder	6	0.994	0.849	1.163	0.935
Drug crime	6	1.249	1.006	1.551	0.044
Property crime	22	1.161	1.023	1.317	0.021
Vehicle crime	23	1.164	1.015	1.335	0.030
Violent crime	29	1.050	0.954	1.155	0.320

Q=47.862, df=4, p.<0.001

(c) Country

Category	**N**	**Odds Ratio**	**95% Confidence Interval** Lower	Upper	***P***
Canada	6	1.041	0.812	1.333	0.753
South Korea	3	1.506	1.212	1.871	<0.001
Sweden	4	0.944	0.787	1.132	0.533
UK	34	1.259	1.122	1.414	<0.001
US	24	1.050	0.990	1.113	0.104
Other	5	0.996	0.779	1.273	0.973

Q=89.694, df=5, p.=<0.001

Car parks. Eight of the included evaluations were conducted in car parks. Follow-up periods in the car park schemes averaged 12.75 months, with a low of eight months and a high of 24 months. Five of the car park schemes demonstrated statistically significant reductions in crime. The combined OR of the car park schemes was 1.588 ($p = 0.027$), meaning that crime was reduced by approximately 37% in treatment areas compared to control areas. Crime reduction findings were replicated in both the largest- (OR = 1.618, $p < 0.018$) and smallest-effect (OR = 1.620, $p = 0.024$) analyses.[11] Four of the car park studies tested for spatial displacement. Two studies found no evidence of either displacement or diffusion, one found evidence of displacement, and one found evidence of diffusion of benefits.

City and town centers. Thirty-three evaluations meeting the criteria for inclusion were conducted in city and town centers. The follow-up periods in city and town centers averaged 16.43 months, with a low of two months and high of 60 months. Since the last review, the number of evaluations measuring the effect of CCTV in city and town centers increased by 45%. Seven studies found desirable effects, while three evaluations found evidence of undesirable effects (i.e., crime significantly increased in experimental areas compared to control areas). The remaining 23 evaluations generated non-significant effects. The pooled data from the city and town center evaluations indicates an OR of 1.066, which did not achieve statistical significance. The smallest-effect size meta-analysis similarly generated a non-significant effect on crime (OR = 1.005, $p = 0.896$). Conversely, the largest-effect size meta-analysis suggested a statistically significant effect on crime (OR = 1.21, $p = 0.012$). Twenty-three (71.88%) of the city and town center evaluations examined displacement or diffusion of benefits. More than half (13) found no evidence of either displacement or diffusion. Six studies found evidence of diffusion of benefits, three found some evidence of displacement, and one found evidence of both diffusion and displacement.

Housing. Ten evaluations were carried out in housing complexes. The follow-up periods in the housing schemes averaged 10.13 months, with a low of three months and high of 12 months.

[11] La Vigne and Lowry (2011) was the only car park evaluation to report multiple outcome measures. For all other evaluations, the average, largest, and smallest effects were identical. The high variance of the random effects model led to the counterintuitive finding of the smallest-effect meta-analysis having a larger OR than the largest-effect meta-analysis.

Only two studies reported statistically significant reductions in crime. The pooled effects of the housing schemes suggest a non-significant effect, with an OR of 1.028 ($p = 0.805$). Non-significant effects were also found for both the smallest-effect size (OR = 0.992, $p = 0.940$) and largest-effect size (OR = 1.056, $p = 0.663$) meta-analyses. Six of the housing evaluations tested for displacement or diffusion, with each reporting no evidence of either.

Residential areas. Sixteen evaluations were carried out in residential areas. The follow-up periods in the residential schemes averaged 19.15 months, with a low of five months and a high of 36 months. Five of the residential schemes reported statistically significant crime reductions. The meta-analysis found that the use of CCTV in residential areas is associated with a significant reduction in crime (OR = 1.133, $p = 0.009$), meaning that crime decreased about 12% in experimental areas compared to control areas. However, while the largest-effect size meta-analysis further suggests a significant crime reduction (OR = 1.239, $p < 0.001$), the smallest-effect size meta-analysis was non-significant (OR = 1.055, $p = 0.268$). Eleven studies (68.75%) tested for the presence of displacement or diffusion of benefits. Four found evidence of diffusion of benefits and one found evidence of both. The others did not find any evidence of displacement or diffusion of benefits.

Public transport. Four evaluations were carried out in public transport systems. The follow-up periods in the public transport schemes averaged 22.0 months with a low of 12 months and high of 32 months. These are the same four evaluations included in the prior CCTV review; no new public transport evaluations meeting the inclusion criteria have been reported. Results indicate a non-significant effect in each of the meta-analyses: average- (OR = 1.370, $p = 0.227$), largest- (OR = 1.368, $p = 0.219$) and smallest-effect size (OR = 1.310, $p = 0.368$). Two of the studies tested for displacement or diffusion effects, with one finding evidence of diffusion of benefits and the other finding evidence that some displacement occurred.

Other settings. Five evaluations were conducted in settings that did not fit any of the above classifications and thus comprise the "other" settings category.[12] The follow-up periods in other settings averaged 22.25 months, with a low of 12 months and high of 36 months. Only one "other" setting evaluation detected a significant reduction in crime, and the overall effect suggested a large but non-significant reduction in crime (OR = 1.265, $p = 0.077$). However, differing findings were suggested by the largest- (OR = 1.351, $p = 0.014$) and smallest-effect (OR = 1.151, $p = 0.447$) size meta-analyses. Four of the evaluations measured displacement and diffusion effects. Three evaluations found evidence of diffusion of benefits and one found no evidence of displacement or diffusion.

Crime type

In the 76 studies included in the meta-analysis, violent crime was the most commonly reported (n = 29), followed by vehicle crime (n = 23) and other property crime (n = 22). In comparison, disorder and drug crime were rarely reported, with each of these crime types included as outcomes in only six studies. Similar to the findings of the last review, CCTV was associated with significant reductions in vehicle crime (OR = 1.164, $p = 0.030$) and property crime (OR = 1.161, $p = 0.021$). The ORs translate to reductions of approximately 14% for both vehicle crime and property crime. CCTV had the largest effect on drug crime (OR = 1.249, $p = 0.044$), for a

[12] One evaluation was conducted at a city hospital (Gill and Spriggs, 2005), one was conducted in a school/university setting (Lim and Wilcox, 2017), two were conducted across entire cities but were unable to be disaggregated to smaller settings (Kim, 2008; La Vigne et al., 2011), and one reported that the target area was comprised of undisclosed mixed environments (Lim, Kim, Eck, and Kim, 2016).

reduction of approximately 20%. No significant effects were observed for violent crime or disorder (see Table 1b).

Country comparison

The 76 evaluations included in the meta-analysis were carried out in nine different countries. Most of the studies (n = 34; 44.7%) were conducted in the UK. The US contributed 24 (31.5%) of the studies in the meta-analysis. This has increased from four of 41 studies (or 9.7%) in the prior review. Studies were also carried out in Canada (n = 6), South Korea (n = 3), Sweden (n = 4), Norway (n = 1), Spain (n = 1), Poland (n = 2), and Australia (n = 1). For the purposes of the meta-analysis, the latter four countries are grouped as "other country."

CCTV was associated with a significant reduction in crime in the UK (OR = 1.259, $p < 0.001$) and South Korea (OR = 1.506, $p < 0.001$). The small number of studies in South Korea calls for caution in interpreting the magnitude of effects. In addition, while both the smallest- and largest-effect size meta-analyses supported crime reductions in the UK, the smallest-effect size analysis did not find a significant effect of CCTV in South Korea (OR = 1.354, $p = 0.112$) (see Table 1c).

Monitoring styles and use of other interventions

Sixty-five studies reported information on the type of monitoring used by CCTV (active or passive). CCTV schemes incorporating active monitoring (n = 54) were associated with a significant reduction in crime (OR = 1.172, $p < 0.001$) (see Table 2). This finding was supported by the smallest-effect (OR = 1.091, $p = 0.050$) and largest-effect size (OR = 1.241, $p < 0.001$) meta-analyses. This finding stands in sharp contrast to passively monitored systems, which showed non-significant effects across all three meta-analyses: average-effect size (OR = 1.015, $p = 0.633$), smallest-effect size (OR = 0.991, $p = 0.804$), and largest-effect size (OR = 1.036, $p = 0.383$).

Table 2. Effects by Monitoring Type

Category	N	Odds Ratio	95% Confidence Interval Lower	Upper	*P*
Active	54	1.172	1.080	1.272	<0.001
Passive	11	1.015	0.954	1.081	0.633

Q=12.623, df=1, p.<0.001

In recognition of recent research that finds CCTV may work best when deployed alongside other interventions (La Vigne, Lowry, Markman, and Dwyer, 2011; Piza, Caplan, and Kennedy, 2014b; Piza, Caplan, Kennedy, and Gilchrist, 2015), we coded each study to determine the use and types of complementary interventions in CCTV projects. Seven main intervention categories were evident: signage,[13] improved lighting, police operations (e.g., enhanced patrols), security guards, access control (e.g., swipe card access to apartment buildings or new fencing), community outreach (e.g., youth outreach programs), and communications systems (e.g., call boxes where citizens can alert security/police officers). Of these interventions, signage was the most frequently deployed, with 23 studies noting this intervention alongside CCTV. The next most commonly used interventions were improved lighting (n = 9), police operations (n = 8), community outreach (n = 7), access control (n = 5), communications systems (n = 4), and security guards (n = 2).

In addition to the frequency of interventions, we were interested in the different combinations in which interventions were deployed. We followed the conjunctive analysis of case configurations (CACC) approach developed by Miethe, Hart, and Regoeczi (2008). CACC is a

[13] Some studies reported the presence of flashing lights on top of CCTV cameras. Rather than consider this a separate category, we classified these studies as "signage" given that they related to a similar causal mechanism (i.e., visible confirmation of the CCTV camera presence).

useful tool to summarize categorical data, specifically by creating a data matrix that compiles all possible combinations of categorical attributes. Table 3 presents a CACC data matrix of the various other intervention types. Each cell in the matrix contains a binary measure denoting whether the intervention in question was used alongside CCTV. Each row in the table represents a unique configuration of interventions. The "Total Cases" column notes the number of times each configuration is present within the database. In total, 18 different configurations of interventions appear in our data. The most common configuration was each intervention marked as "no" (n = 36), meaning that CCTV was not deployed alongside any other interventions. The three other most common configurations deployed single interventions alongside CCTV: signage (n = 14), community outreach (n = 5), and police operations (n = 3). Improved lighting alone was deployed alongside CCTV in two schemes, while access control and communications systems were each deployed as the sole complementary intervention in one scheme. All of the other configurations involved the deployment of multiple interventions alongside CCTV.

Table 3. CACC Data Matrix for Other Intervention Types

Signage	Improved Lighting	Police Operations	Security Guards	Access Control	Community Outreach	Communications Systems	TOTAL CASES
no	no	no	no	no	no	no	**36**
yes	no	no	no	no	no	no	**14**
no	no	no	no	no	yes	no	**5**
no	no	yes	no	no	no	no	**3**
no	yes	no	no	no	no	no	**2**
yes	no	no	no	no	no	yes	**2**
yes	no	yes	no	no	no	no	**2**
yes	yes	no	no	no	no	no	**2**
no	no	no	no	no	no	yes	**1**
no	no	no	no	yes	no	no	**1**
no	no	yes	no	no	yes	no	**1**
no	yes	no	no	no	yes	yes	**1**
no	yes	no	no	yes	no	no	**1**
no	yes	no	yes	yes	no	no	**1**
no	yes	yes	no	no	no	no	**1**
yes	no	no	no	yes	no	no	**1**
yes	no	no	yes	yes	no	no	**1**
yes	yes	yes	no	no	no	no	**1**

For the meta-analysis, we classified schemes into one of three categories: CCTV alone (n = 36), CCTV with one other intervention (n = 26), and CCTV with multiple interventions (n = 14) (see Table 4). Schemes incorporating multiple complementary interventions had the largest effect size, with an OR of 1.513 ($p < 0.001$), suggesting an approximately 34% reduction in crime in treatment areas compared to control areas. Significant crime reductions were also found in the largest-effect size (OR = 1.523, $p < 0.001$) and smallest-effect size (OR = 1.484, $p = 0.001$) analyses. The ORs for both schemes deploying no additional interventions (OR = 1.083) and schemes deploying one other intervention (OR = 1.076, $p = 0.103$) did not achieve statistical significance. For both categories, the smallest-effect size analysis generated non-significant findings ("none" OR = 1.017, $p = 0.684$; "single" OR = 1.004, $p = 0.926$), while the largest-effect analysis evidenced significant crime reductions ("none" OR = 1.138, $p = 0.007$; "single" OR = 1.160, $p = 0.001$).

Table 4. Effects by Use of Other Interventions

Category	N	Odds Ratio	95% Confidence Interval Lower	Upper	*p*
None	36	1.083	0.998	1.176	0.057
Single	26	1.076	0.985	1.175	0.103
Multiple	14	1.513	1.220	1.877	<0.001

Q=46.370, df=2, p.<0.001

Publication Bias

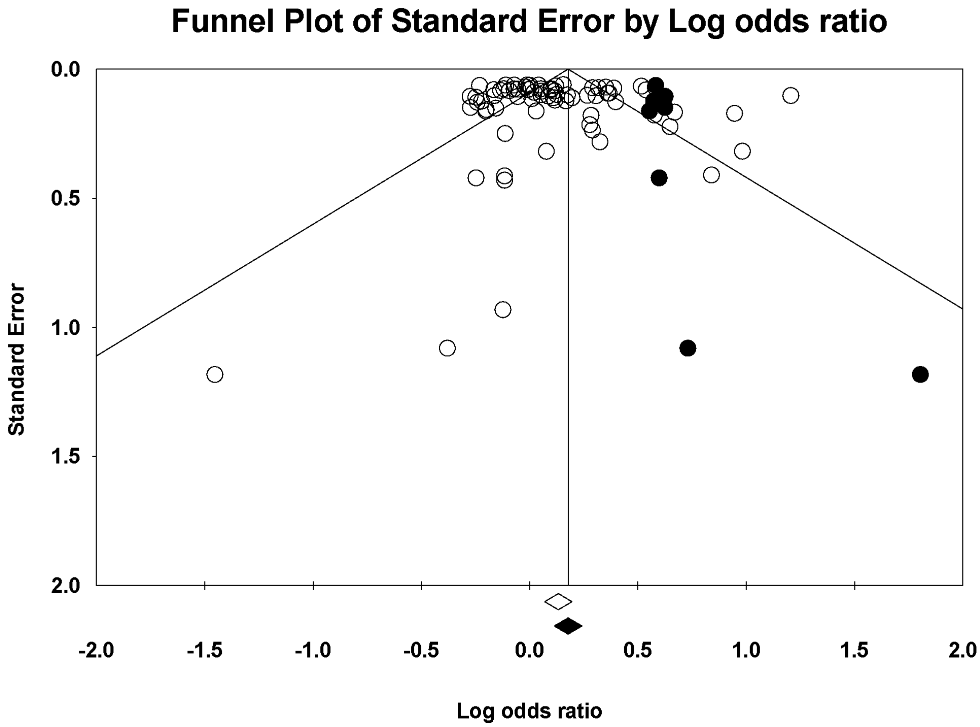
We conclude our analysis with a test of publication bias in our results. Similar to how a biased sample can generate invalid results in an individual study, a biased collection of studies can potentially lead to invalid conclusions in a systematic review (Braga, Weisburd, and Turchan, 2018). To determine the presence of potential publication bias, we used BioStat's trim-and-fill procedure to estimate how reported effects would change if bias was discovered and addressed

(Duval, 2005). The diagnostic funnel plot used to test publication bias assumes that effect sizes should show symmetry around the mean when a representative collection of studies has been obtained. When there is asymmetry, the trim-and-fill procedure inputs the hypothesized missing studies and re-computes a mean effect size.

In Figure 2, the funnel plot for the current study suggests asymmetry, with more studies to the left of the mean than to the right.[14] BioStat's trim-and-fill procedure determined that ten studies should be added to this portion of the funnel plot to create symmetry. When the effect size is re-computed to include these additional studies, the mean effect size increased from 1.141 to 1.194 However, the 95% confidence intervals of the observed and adjusted ORs overlap, suggesting that the effect sizes are not statistically significantly different. The smallest- and largest-effect version of the trim-and-fill procedure similarly produced estimates with overlapping confidence intervals. In light of these findings, we conclude that publication bias did not affect our results.

[14] The frequency of lower-effect studies in our meta-analyses is a bit counter-intuitive. Publication bias typically refers to the tendency for researchers to more readily publish evaluation results that demonstrate large effect sizes (Rothstein, Sutton, and Bornstein, 2005). Our results suggest the opposite: that small-effect studies are over represented in the CCTV literature. This observation can be explained by the nature of the program evaluations included in our review. As previously mentioned, 55.3% of included studies were research reports from the grey literature. Given that unpublished studies typically exhibit smaller effect sizes, the large proportion of grey literature studies resulted in a disproportionate number of observed effect sizes falling to the left of the mean.

Figure 2. Publication bias test



Note: Empty circles indicate the original studies. Filled-in circles indicate imputed studies from the trim-and-fill analysis.

Observed values: Random effects = 1.141 (95% C.I. 1.072, 1.215).

Adjusted values (10 studies trimmed): Random effects = 1.194 (95% C.I. 1.121, 1.273).

Conclusions and Directions for Policy and Research

This systematic review identified 80 studies that met the inclusion criteria, with 76 providing the requisite data to be included in the meta-analysis. We think that this increase in the number of evaluations has resulted in an improved knowledge base of the effects of CCTV on crime. The amount of new research conducted on CCTV in residential areas illustrates this point. While the prior review could only include two evaluations of CCTV in residential areas, the present review identified an additional 14 studies that met the inclusion criteria. This makes residential areas the second most common setting for CCTV evaluations (n = 16), behind city and town centers (n = 33). In addition, while evaluations carried out in the UK comprised the majority (82.9%) of studies in the last review, UK evaluations accounted for less than half (44.7%) of the studies included in this review. The field now has much more evidence on the effect of CCTV in other countries, particularly in the US. Welsh and Farrington (2008, 2009a) could only include four sufficiently rigorous CCTV evaluations that took place in the US. The paucity of rigorous CCTV evaluations in the US was not lost on the research community, with a number of US-based evaluations specifically noting the lack of relevant research evidence (Caplan, Kennedy, and Petrossian, 2011; Piza, Caplan, and Kennedy, 2014b). Disappointingly, as with the prior review, it was not possible to investigate the potential influence of evaluation design on study outcomes (see Weisburd, Lum, and Petrosino, 2001; Welsh et al., 2011). This is because there continues to be little variability in the evaluation designs used by the included studies. With the exception of the one randomized controlled experiment (La Vigne and Lowry, 2011),[15] all of the other studies can be classified as traditional quasi-experimental designs: measures of crime before and after the program in experimental and comparable control areas.

While the increase of evaluations in residential areas and in other countries is promising, we note that research in certain settings has stagnated since the last CCTV review. No new public transport evaluations were added, hindering the knowledge base on CCTV in this setting. While failing to achieve statistical significance, the effect sizes for public transport studies were among the largest in our meta-analysis. The lack of statistical significance may be more indicative of a small sample size than the ineffectiveness of CCTV, suggesting the need for more rigorous evaluations in public transport settings.

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[15] Two additional randomized controlled trials identified during our literature search did not meet the inclusion criteria. Piza, Caplan, Kennedy, and Gilchrist (2015) randomized the allocation of a directed patrol function to existing CCTV sites; thus, directed patrol, rather than CCTV, was considered the main intervention given that both experimental and control areas were covered by CCTV. Hayes and Downs (2011) randomized the use of CCTV across 47 retail stores, a setting that was outside the scope of this review.

The pooled effects meta-analysis shows that CCTV is associated with a modest and significant reduction in crime. The crime reductions were not negatively impacted by displacement, with only six of the 50 studies incorporating an adjacent control area (i.e., displacement buffer area) finding evidence of displacement. In three additional studies, researchers found some evidence of both displacement and diffusion of benefits. Fifteen of the studies found evidence of diffusion of benefits, suggesting that CCTV may more often lead to unanticipated crime control benefits.

Similar to the prior review, we also found the largest and most consistent effects of CCTV within car parks. The reduction of crime in car parks was further reflected in both the largest-effect size and smallest-effect size meta-analyses. The number of evaluations conducted in car parks increased slightly since the last review (from 6 to 8). Although difficult to disentangle the independent effects, a number of key factors played a role in car parks being the most effective setting for cameras to prevent crime. For one, seven of the eight car park studies included other interventions, such as security guards, signage, and improved lighting. Also, a similar number of the car park studies were targeted on vehicle crimes and six were actively monitored. There is also the matter of camera coverage. In the two studies that reported on it, camera coverage was near 100%. In the national UK evaluation of the effectiveness of CCTV, Farrington and colleagues (2007) found that effectiveness was significantly correlated with the degree of coverage of the CCTV cameras, which was greatest in car parks.

Whereas the prior review found that car parks was the only setting where CCTV was associated with significant effects, our new review found evidence of significant crime reductions within other settings. CCTV schemes in residential areas were associated with significant crime reductions in both the average- and largest-effect size meta-analyses. While not as stable as the observed reduction in car parks (which was supported by all three meta-analyses), these findings suggest that CCTV may be effective in residential areas. This stands in contrast to the CCTV schemes in city/town centers and "other" settings. In both of these settings, significant CCTV effects were only evident in the largest-effect size meta-analysis. Public safety agencies should be mindful that CCTV might only work in city/town centers and "other" settings when the maximum potential effect is achievable.

Welsh and Farrington (2008, 2009a) suggested that strategic aspects of CCTV schemes might be as important as the geographic setting. The findings of the current review provide further support for this observation. Schemes that incorporated multiple interventions alongside CCTV were associated with larger effect sizes than schemes deploying single or no interventions alongside CCTV. Actively-monitored CCTV schemes evidenced significant reductions in crime, while passively-monitored schemes were not associated with reductions in crime. This argues against the use of CCTV as a stand-alone tactic. Rather than relying on conspicuous camera presence, public safety agencies should employ active camera monitoring to proactively identify and address incidents of concern.

The findings of the present review echo those of the previous review in terms of CCTV use in the UK, with the 34 UK schemes demonstrating a significant crime reduction of approximately 10% in treatment areas compared to control areas. Another intriguing finding relates to the absence of significant effects in the US. Welsh and Farrington (2008, 2009a) also found no significant effects in the US. However, given that the present review included 20 more evaluations conducted in the US, the absence of significant effects in the US is particularly noteworthy.

In an attempt to better understand the differences between the UK and US, we compared the countries' CCTV schemes across contextual factors that have been found to influence the effect of CCTV: setting, monitoring type, and use of other interventions (see Table 5). Nearly 18% of UK evaluations (n = 6) were conducted in car parks, compared to only a single evaluation in the US. Given that the effect of CCTV is strongest in car parks, the general lack of car park schemes in the US may help explain the lower effect in this country. However, the US had a much larger proportion of CCTV schemes in residential areas (45.8%) than the UK (5.9%). Given that residential settings exhibited the second strongest effect, it is difficult to identify substantial patterns in the influence of settings across countries. Patterns of effect are much more evident in the manner by which public safety agencies use CCTV. In the UK, 88.2% of CCTV schemes incorporated active monitoring, as opposed to 58.3% in the US. Furthermore, 12 (35.3%) of the UK schemes used multiple interventions alongside CCTV compared to only one (4.2%) scheme in the US. Given the overall positive findings associated with active monitoring and the use of multiple interventions, these factors may help explain the difference in CCTV effects between the UK and US.

Table 5. Comparison of CCTV schemes in the UK and US

	United Kingdom		United States	
	N	%	N	%
Setting				
Car park	6	17.6	1	4.2
City center	15	44.1	7	29.2
Housing	7	20.6	3	12.5
Residential	2	5.9	11	45.8
Public transport	3	8.8	0	0.00
Other	1	2.9	2	8.3
Monitoring Type				
Active	30	88.2	14	58.3
Passive	0	0.0	7	29.2
Not specified	4	11.8	3	12.5
Use of Other Interventions				
None	12	35.3	11	45.8
Single	10	29.4	12	50.0
Multiple	12	35.3	1	4.2

Our review also found that the effect of CCTV is heterogeneous across crime types. The largest OR effect size (1.249) was observed for drug crimes. This finding is intriguing in light of prior research reporting that drug sellers claim that the fast-paced nature of drug markets enables participants to easily evade the gaze of CCTV (Gill and Loveday, 2003: 22). Our findings suggest that despite such proclamations from drug sellers, CCTV cameras may help combat the illicit drug trade. Research has found that drug sellers adopt situational prevention techniques to avoid apprehension by police (Jacques and Reynald, 2012), which can include activities such as the involvement of multiple sellers in single transactions, stash-spots to store drugs, and mediation schemes meant to obscure transactions (Piza and Sytsma, 2016). These processes can be quite complex and difficult for police officers to observe on the street. In this sense, CCTV may help disrupt drug selling through the elevated position and telescopic capacity of cameras, which affords the operators greater range of vision than street-level police officers (Norris and Armstrong, 1999: 159). Piza, Caplan, and Kennedy (2014a: 1036-1037) once observed such benefits within a CCTV control room, with a police Lieutenant monitoring a camera and relaying the following information to undercover officers in the field via two-way radio: *"The guys I saw selling on [street name] yesterday are now on [street name #2]. They just served [sold drugs to] a guy in a white Lexus. The kid who made the actual transaction is wearing a turquoise t-shirt. The other 2 dealers are on [street name #3]: [one is wearing a] red shirt, hat and a beard; the other one has a white t-shirt and thinner beard … they keep walking to the back of the building; I think that's where the stash [of drugs] is."*

CCTV was associated with significant reductions in both vehicle crime and property crime in general, with no significant effects observed for violent crime. Public safety agencies combatting violent crime problems may need to consider whether resources would be better allocated toward other crime prevention measures. For jurisdictions with existing CCTV systems, public safety agencies may need to make changes to their existing strategies to effectively combat violence. Actively-monitored CCTV, which can detect incidents of concern in real time, may be able to deploy police officers on-scene before a situation escalates into serious violence. This potential benefit of CCTV was observed by Piza, Caplan, and Kennedy (2017) in their systematic social observation of violent crime events recorded in their entirety (i.e., the moments immediately prior to, during, and following the event) on CCTV. Most violent crime incidents were preceded by an "intervention opportunity," such as a fight, disorderly behavior, or drug transaction, providing probable cause for a police response. Piza, Caplan, and Kennedy (2017: 259) argued that while a police response would not have guaranteed the prevention of the subsequent violent crime, police officers being on-scene would have made the incident less likely to occur than the absence of police presence. Indeed, Piza, Caplan, Kennedy, and Gilchrist (2015) hypothesized that early intervention by police may help increase the certainty of punishment in CCTV target areas, ultimately generating crime reductions. Piza and colleagues' (2015) randomized controlled trial pairing active CCTV monitoring with directed police patrol supported this causal mechanism, finding that violent crime as well as social disorder significantly decreased.

It should be noted, however, that actively-monitored CCTV systems require a greater commitment of resources than passive systems. This is especially the case if agencies wish to maintain current levels of active monitoring as CCTV systems expand—because high camera-to-operator ratios can negatively affect active monitoring practices (Piza, Caplan, and Kennedy, 2014a). Towards this end, police have increasingly integrated crime control technologies such as gunshot detection technology (GDT) in an attempt to maximize efficiency (La Vigne, Lowry, Markman, and Dwyer, 2011). Given that operators cannot monitor all cameras in a system simultaneously, such technology is expected to better focus operator attention by identifying precisely when an operator should monitor a specific camera (Piza, Caplan, and Kennedy, 2014a: 1038-1039). However, there is no guarantee that such technology will increase CCTV effectiveness. Piza, Caplan, and Kennedy (2014a) found that the introduction of GDT in Newark, New Jersey, did not improve active monitoring practices of CCTV. Given the high cost associated with technology, introducing additional camera operators and/or patrol officers into CCTV operations may be a more cost-effective measure than complementary crime control technologies. For example, the costs of the additional camera operators, police officers, and patrol vehicles deployed in Newark's CCTV Directed Patrol Project were approximately $76,000 (Piza, Gilchrist, Caplan, Kennedy, and O'Hara, 2016). In contrast, ShotSpotter, the industry leader in GDT technology, reports that subscriptions for their service cost between $65,000 and $90,000 per square mile per year.[16] In the case of Newark, which has ShotSpotter's GDT installed in a seven-square mile area of the city (Government Technology, 2008), this translates to a yearly cost of between $455,000 and $630,000. At an average cost of about $6,897 per week ($75,873.07 / 11-week intervention period), conducting the CCTV Directed Patrol Project each week of the year (totaling $358,644) would cost between $96,356 and $271,356 less per year than GDT.

However, we must note that technology besides GDT can be used in an attempt to improve CCTV monitoring functions and may provide a more cost-effective solution. Recently, Idrees, Shah, and Surette (2018) explored the potential benefits that computer vison technology (CVT; also known as machine learning) can provide to CCTV interventions. CVT applies mathematical algorithms to each frame of CCTV footage for the purpose of automating the detection of crime-related events. Upon detection of an image of concern such as a weapon, fugitive vehicle, or physical behavior indicative of crime (e.g., a person repeatedly striking a vehicle window as if trying to break in), CVT alerts the CCTV operator (who may have been monitoring a different camera at the time). Within a CVT-assisted CCTV scheme, the primary role of the human operator is shifted from the traditional role of manually mining video footage in search of criminal behavior to a supervisory role emphasizing assessment of detected images and response decision-making (i.e., whether to report detected events to the police) (Idrees, Shah, and Surette, 2018). This may bolster the efficiency of active CCTV monitoring, as research has shown the bulk of camera operator time is spent on activities other than camera monitoring (e.g., see Norris and McCahill, 2006). To date, little use of CVT has been made by law enforcement (Idrees, Shah, and Surette, 2018). None of the evaluations we identified for potential inclusion in this review mentioned the

[16] http://www.shotspotter.com/system/content-uploads/SST_FAQ_January_2018.pdf

use of CVT. As the use of CVT expands, researchers should conduct case-controlled evaluations to measure whether CVT improves the effectiveness and efficiency of CCTV.

Even with further policy insights from an increase in evaluations of CCTV, there continue to be opportunities for further improvement in evaluation research. For one, randomized controlled experiments are a rarity in the study of CCTV. La Vigne and Lowry (2011) and Piza, Caplan, Kennedy, and Gilchrist (2015) carried out the only randomized experiments of CCTV in public settings. Piza (2018a: 16) noted that, because CCTV cameras are hard wired to physical structures and configured to wireless communications networks, moving locations after experimentation would require additional expenditures. Other crime prevention strategies, such as hot spots policing or body-worn cameras, do not present such difficulties and are more amenable to randomization.

Nonetheless, random assignment of CCTV cameras may be possible in certain cases. As argued by Piza (2018a: 26-27), agencies could identify priority locations at the outset of a program and randomly select a subset of locations to receive cameras during the first phase of installation. In a waiting-list design, other priority sites could receive cameras in later installation phases, after completion of the experiment. Under this strategy, officials could simultaneously generate the most rigorous evidence of the effects of CCTV while still ensuring that all priority locations received CCTV (presuming that experimental results support the installation of more cameras). In this sense, there may also be a role for redeployable CCTV cameras, meaning that experimental areas can be moved around.[17]

[17] The Toronto Police Service's re-deployable fiber infrastructure allowed the agency to post CCTV cameras at various places within the entertainment district as necessary. Verga and Douglas (2008) reported that this configuration led to a significant cost savings as compared to the installation of permanent, hard-wired cameras in other parts of Toronto.

Future research should continue to ensure the policy relevance of CCTV research. It is important to note that knowing whether a technology "works" is not enough for policymakers; the contextual and procedural aspects necessary to maximize effects are equally important when considering the adoption of a crime prevention technology (Salvemini, Piza, Carter, Grommon, and Merritt, 2015). In recognition of this fact, the College of Policing developed the *What Works Toolkit* to summarize the research evidence on a variety of crime prevention strategies in a format that is easily interpreted by practitioners.[18] The toolkit identifies five dimensions of programs that are of interest to policy makers: 1) intervention effect, 2) causal mechanisms, 3) moderating factors, 4) implementation issues, and 5) economic costs (Johnson, Tilley, and Bowers, 2015). The College of Policing noted that CCTV meta-analyses (Farrington, Gill, Waples, and Argomaniz, 2007; Welsh and Farrington, 2009a) have provided a great deal of evidence on the intervention's effect, causal mechanisms, and moderating factors, but have generated much less evidence on implementation issues and economic costs. In a sense, this is unsurpising given that the Toolkit focused on meta-anlyses that exclsuvely included studies incorporating crime as an outcome measure. In order to generate sufficient knowledge on implementation issues and economic costs associated with CCTV, researchers may need to conduct systematic reviews that prioritize research directly focusing on these factors, irrespective of whether crime was directly tested in the evaluation.

Lastly, researchers should expand the focus of CCTV evaluations to include more outcome measures than crime prevention. While crime prevention is obviously an important consideration, police departments also largely invest in CCTV for its ability to detect and identify offenders for investigatory purposes (Ratcliffe, 2006). Despite this potential benefit of the technology, a body

[18] http://whatworks.college.police.uk/toolkit/Pages/Toolkit.aspx

of research on the investigatory benefits of CCTV has yet to develop. To our knowledge, Piza, Caplan, and Kennedy (2014a) and Ashby (2017) represent the only case-controlled tests of CCTV's effect on on-scene offender apprehension and retroactive criminal investigations, respectively. The field would benefit from an increased evidence-base on the effect of CCTV on such outcomes.

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