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# EDITORIAL INTRODUCTION

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## ELECTRONIC MONITORING ON SOCIAL WELFARE DEPENDENCE

### Electronic Monitoring in Denmark and Beyond

**Randy Gainey**

*Old Dominion University*

In the following research article, Andersen and Andersen (2014, this issue) examine two changes in policy that expanded the use of electronic monitoring on offenders sentenced to prison in Denmark. Rather than focusing on recidivism, which has been so common in studies in the United States, Andersen and Andersen focus on how electronic monitoring affects welfare dependency (basically unemployment) among offenders in the country. In a carefully orchestrated quasi-experiment, Andersen and Andersen find that electronic monitoring reduces welfare dependency among young offenders but does not have the same positive effects (or deleterious effects) among older offenders. Employment, looking for employment, or job training, is a condition for being placed on electronic monitoring in Denmark, and therefore, one might think that the relationship is tautological; however, the research design by Andersen and Andersen includes similarly matched subjects in the control group providing a strong test of the causal effects and the utility of electronic monitoring in Denmark, with policy implications for if and how it might be effective elsewhere.

The research article by Andersen and Andersen (2014) is followed by two policy essays, by Brian K. Payne (2014, this issue) and Matthew DeMichele (2014, this issue), respectively. You will detect some common themes in these essays, which is not surprising as they have coauthored several articles together focused on electronic monitoring and attitudes toward electronic monitoring in the United States. For example, both Payne and DeMichele emphasize that electronic monitoring is just a tool and that like any other tool, it can be effectively used for the appropriate purposes and of course ineffectively used where it should never had been applied in the first place. This is no small point because electronic monitoring has been heralded as a panacea by some and labeled a failure by others. It is just

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a tool and can be used effectively or ineffectively or can actually do harm, such as when one slams a hammer into his or her finger or cuts him- or herself with scissors.

Both Payne (2014) and DeMichele (2014) also applaud the Denmark study by Andersen and Andersen (2014) and recognize the importance of focusing on welfare dependency as opposed to recidivism. In essence, although not complete, the Denmark study takes a look into the black box of the potential relationship between electronic monitoring and recidivism by focusing on one possible mediating variable—welfare dependence or unemployment. Many other mediating variables are possible. Gies et al. (2012), for example, found that electronic monitoring was associated with greater treatment compliance among high-risk sex offenders in California. Electronic monitoring may be effective in many other unexplored ways as well.

Understanding where and how electronic monitoring should be used requires extensive research, which has only recently begun. The article by Renzema and Mayo-Wilson (2005) referenced by both Payne (2014) and DeMichele (2014) points to the lack of solid research out there but that is now beginning thanks largely to efforts and funding by the National Institute of Justice (see, for example, Erez and Ibarra, 2007; Bales et al., 2010; Gies et al., 2012, 2013).

There are some notable differences as well. DeMichele (2014), for example, emphasizes the differences between the Scandinavian and the U.S. approaches to punishment with the former being far more rehabilitative and geared to reentry than the U.S. approach, which tends to be more demoralizing, dangerous, and punitive. The sentences in Scandinavia are also much shorter than those in the United States, which raises important questions related to policy and how the two social contexts (actually many social contexts, for example, across states in the United States) might experience electronic monitoring. DeMichele also argues that we should be moving toward cognitive transformation of offenders, and he discusses if (and how) electronic monitoring, as a tool, might be useful in this process.

Payne (2014) focuses a bit more on the history and expansion of electronic monitoring in the United States. Based on his earlier work as well as on that of many others, Payne contemplates the “experience of electronic monitoring” and how that relates to the experiences of offenders in prison (i.e., Gresham Sykes’s [1958] “pains of imprisonment”) as well as to the unique “pains” associated with being on house arrest with electronic monitoring. In contrast to the Denmark study by Andersen and Andersen (2014), who argue that electronic monitoring is less stigmatizing than prison, Payne argues that electronic monitoring can be stigmatizing at different times and in different ways. I was reminded of doing field research where we observed parole officers being trained to use electronic monitoring with GIS for high-risk sex offenders. We were given the opportunity to wear the device for 24 hours and then examine our movements via computer the next day. A female colleague accepted the offer and at lunch the next day was approached by a gentleman who asked her if she had a medical condition or if she was “in trouble with the law.” Although my colleague took this in good stride and explained the situation, it is obvious that this might be painful and



stigmatizing for someone actually on the program before he or she is even found guilty (as a pretrial condition), while serving time on electronic monitoring instead of residing in jail or prison, or as a condition of his or her release on probation or parole.

Payne (2014) also emphasizes the role of theory and even praises Andersen and Andersen (2014) for “weaving in an innovative theoretical approach to assess the deterrent value of electronic monitoring.” Payne adds to this discussion several theories including deterrence, rational choice, routine activity, and general systems theories. I think this is an important contribution, and more theoretical work should be focused on when, where, and how electronic monitoring might be used most effectively.

In conclusion, the research article by Andersen and Andersen (2014) provides one of the few tests of the effectiveness of electronic monitoring outside of the United States, and it finds that electronic monitoring reduces welfare dependency among young offenders and has no deleterious effects, at least in terms of welfare dependency, among older offenders. The two policy essays by Payne (2014) and DeMichele (2014) translate that information to suggest how we might think about the uses of electronic monitoring, as a tool (not as a silver bullet), in the United States and beyond.

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**Randy Gainey** is a professor of sociology and criminal justice at Old Dominion University where he has been teaching since he received his Ph.D. from the University of Washington in 1995. He has written extensively on electronic monitoring, most recently focusing on electronic monitoring with GPS of gang members and high-risk sex offenders on parole in California. He is also interested in neighborhood organizations and their relation to crime.

# Effect of Electronic Monitoring on Social Welfare Dependence

**Lars H. Andersen**

*Rockwool Foundation Research Unit and University of Copenhagen*

**Signe H. Andersen**

*Rockwool Foundation Research Unit*

## Research Summary

*We studied the effect on social welfare dependence of serving a sentence under electronic monitoring rather than in prison using Danish registry data and two policy shifts that extended the use of electronic monitoring in Denmark. We found that electronic monitoring is less harmful than imprisonment, at least for younger offenders, whereas it does not leave older offenders worse off than imprisonment.*

## Policy Implications

*As the United States moves toward noncustodial alternatives to imprisonment, policy makers might benefit from knowledge on experiences from other contexts. The experiences from Denmark are clear: Electronic monitoring is less harmful than imprisonment to the life-course outcomes of offenders. Because electronic monitoring could be less costly for the corrections administrations than imprisonment, efforts to extend the use of electronic monitoring in the United States could be accelerated.*

## Keywords

*causal inference, electronic monitoring, noncustodial alternatives to imprisonment, register data, unemployment, welfare dependence*

**A**lthough the incarceration rate in the United States still greatly exceeds that of any other country throughout the world (e.g., Walmsley, 2011), the most recent accounts of the number of imprisoned people in the United States tell us that

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mass imprisonment might have passed its peak and is now in decline (e.g., Carson and Golinelli, 2013). In many ways, this news is good, as imprisonment is costly in a double sense. Not only is it costly to keep people in prison (e.g., Kyckelhahn, 2013), but also these individuals contribute less to society, as they cannot take up employment and make a living for themselves and their families during imprisonment. Numerous studies have documented the devastating consequences of imprisonment for the future life chances of offenders (e.g., Western, 2006) and their families (e.g., Wakefield and Wildeman, 2013).

Even though the number of people under community supervision in the United States has greatly increased since the early 1980s, the recent decline in the number of imprisoned Americans has largely been fueled by policy changes, in particular changes that favor noncustodial alternatives to imprisonment (Phelps, 2013). One tool central to the policy changes has been the use of electronic monitoring. Here, an electronic device monitors the offender by means of either radio frequencies or global positioning system. This device has been used as a pretrial supervision alternative to local jail, as a requirement for offenders placed on community supervision, and as a parole requirement for serious offenders released on parole (Bales et al., 2010).

Rising prison populations and increasing prison expenditures also has led other countries to adopt noncustodial alternatives to imprisonment. For example, in Denmark, electronic monitoring was first introduced in 2005 and is now a noncustodial alternative to short prison sentences, just as there are current considerations to mandate electronic monitoring for prisoners released on early parole, as is the case in many other European countries (Kalmthout and Durnescu, 2008).

The anticipated benefits of electronic monitoring over imprisonment are many (e.g., Bales et al., 2010). First, electronic monitoring eases the challenges of prison overcrowding, as the offender does not enter prison. Second, electronic monitoring is usually cheaper than imprisonment, as a smaller prison staff is needed to perform the monitoring now automated by the electronic device. Third, electronic monitoring allows the offender to keep his or her job (or keep searching for a job) and take care of his or her family while serving the sentence or awaiting trial.

Yet although the policy changes that favor noncustodial alternatives to imprisonment sweep confidently across the developed world, empirical research evaluating them is falling behind, leaving a significant gap between the anticipated benefits of noncustodial alternatives to imprisonment, such as electronic monitoring, and the knowledge we have of these benefits (Subramanian and Shames, 2013).

To provide new knowledge, this study analyzes the effects of electronic monitoring on social welfare dependence in Denmark. For this purpose, we use Danish population data coupled with data from the Danish Prison and Probation Service, and we include individuals who experienced electronic monitoring and imprisonment before and after two policy reforms that expanded the use of electronic monitoring in Denmark in 2006 and 2008. As the reforms are exogenous to offender characteristics, our estimates of the effects

of electronic monitoring on welfare dependence are causal in nature. Although similarities in offender characteristics across the reforms prove this exogeneity, we take additional steps to enhance the credibility of our main results. Our results uniformly show that, relative to imprisonment, electronic monitoring lowers social welfare dependence among young offenders, but it does not leave older offenders worse off. These results represent important new knowledge of the effects of the recent changes toward noncustodial penal policies.

## Background

Electronic monitoring was first introduced in the United States in 1984 as a direct response to prison overcrowding and to meet a demand for sanction types that were more lenient than prison but harsher than standard probation (MacKenzie, 2006; Payne and Gainey, 2000). Policy makers expected this new noncustodial sentence to produce good outcomes (e.g., better rehabilitation) at a lower cost than imprisonment and celebrated electronic monitoring as the future of American corrections. During the late 1990s, however, electronic monitoring lost its appeal in the United States for several reasons. First, the money saved on imprisonment thanks to the use of electronic monitoring was now spent on testing and supervising the electronically monitored people (e.g., alcohol tests). Second, electronic monitoring and other noncustodial alternatives to imprisonment tended to widen the punitive system by putting more people under the purview of the criminal justice system. Third, the more intensive testing and supervision increased detection rates for recidivism and technical violations, which in turn sent even more people into custody. This led some policy makers to view electronic monitoring and other noncustodial alternatives to imprisonment as failed social experiments, and the popularity of these programs faded in the United States (Petersilia, 2011).

Despite the challenges experienced in the United States, other countries adopted the idea of intensive surveillance under home detention (see Newburn, 2002, on policy transfers). As is often the case, these other countries did not implement a copy of the American program—maybe because of the local inertia in adopting policies from abroad (e.g., Tonry, 2013) or because different countries value different aspects of the policies they adopt (Newburn, 2002). Instead, the implementation of electronic monitoring largely reflected the specific context in which it occurred (Renzema and Mayo-Wilson, 2005).

For this reason, electronic monitoring in Europe is not a uniform sanction type, but there are signs of convergence. Specifically, Kalmthout and Durnescu's (2008) review of European probation showed a general trend toward policies derived from the Anglo-Saxon model, which emphasizes correction and control more than rehabilitation and social welfare (for a discussion of penal organizational isomorphism across contexts, see DeMichele, 2014).

As we will explain in this article, this observation is true also for electronic monitoring in Denmark. But even though we have witnessed fluctuations in Danish imprisonment and probation rates since the 1980s, the imprisonment rate in Denmark and in other Scandinavian countries is remarkably stable and has been so for the last half century (Lappi-Seppälä

and Tonry, 2011). Hence, the increase in mass social control—mass imprisonment paralleled by mass probation, as observed in the United States (Phelps, 2013)—is unprecedented in a Danish context. Also, the parallel trends of increased use of community supervisions and increased social control from the early 1980s onward in the United States (DeMichele, 2014) are not found in Denmark, at least not to the same degree, despite the recent emergence of “tough-on-crime” policies (for a general introduction to the Scandinavian penal justice systems, see Lappi-Seppälä and Tonry, 2011).

Given this history of policy transfers between the United States and Europe, the criminal justice policy debate in the United States could gain important insight from non-U.S. experiences on electronic monitoring while taking the peculiarities of these other contexts into account. This observation is particularly useful in view of the recent willingness to reform the American penal system (see Subramanian and Shames, 2013). Our article contributes to such insight by analyzing the effect of electronic monitoring on social welfare dependency in Denmark.

### **Electronic Monitoring in Denmark**

In Denmark, electronic monitoring is not a sentence given by a judge; it is a way of serving a prison sentence. When a judge has passed a prison sentence, the Danish Prison and Probation Service informs select offenders of the opportunity to serve their sentence at home under electronic monitoring. In case the offender wishes to use this opportunity, he or she applies to the prison service, which then makes the final decision on whether to grant the particular offender the right to serve at home under electronic monitoring.

Denmark first introduced electronic monitoring in May 2005. From then on, traffic offenders could serve sentences shorter than 3 months in their own homes under intensive surveillance and control, which mainly involved electronic monitoring and a strict organization of everyday life—especially with regard to activities outside the home. To serve a sentence this way, offenders had to accept one to three unannounced weekly control visits that involved testing for alcohol and drug use (Danish Corrections Act, Law no. 367).

Furthermore, they had to have a permanent address (e.g., not be living in a shelter for the homeless) and a job, participate in some form of active labor market program for the unemployed, or be enrolled in education. Unemployed offenders could fulfill this employment criterion by working at institutions appointed by the prison service. If the offender had cohabiting family members, then these members had to accept the electronic monitoring of the offender formally in writing. In addition, only offenders who had not previously committed any serious crime—defined as a crime punishable by more than a fine—within 2 years prior to the conviction could serve this new type of sentence.

If the offender violated certain conditions during the sentence, then he or she would go to prison. These conditions included avoiding crime, alcohol, and drugs; sticking to the strict organization of everyday life; and agreeing to obey the rules and guidelines associated

with electronic monitoring. Finally, electronically monitored offenders were required to participate in a crime prevention program.<sup>1</sup>

In April 2006, the scheme was extended to include all offenders younger than 25 years, regardless of the offense type (yet still with a maximum sentence length of 3 months), and in June 2008, another change removed the age requirement completely (this last reform prohibited the use of electronic monitoring for offenders convicted on charges of possession of weapons or explosives who were sentenced to a prison term lasting less than 2 weeks). Our analyses exploit the implementation of the two last reforms.

From a non-Scandinavian viewpoint, the maximum sentence length requirement of 3 months might seem overly restrictive and might seem to target electronic monitoring at a small group of offenders. But sentences in Denmark are generally short, and almost two thirds of all prison sentences in Denmark are shorter than 3 months (Danish Prison and Probation Service, 2013). Thus, our results on electronic monitoring as a noncustodial alternative to imprisonment concern the majority of prison sentences in Denmark.

Of the convicted offenders who apply for and are granted permission to serve under electronic monitoring, some have second thoughts, and in other cases, the prison service revokes permission. In addition, some offenders begin the electronic monitoring but violate the conditions and are then transferred to prison. Thus, not all offenders eligible for electronic monitoring apply for it, and not all offenders who apply for it actually end up serving at home and complete their sentence under electronic monitoring. An assessment of the electronic monitoring scheme in Denmark shows that approximately 43% of offenders who are offered electronic monitoring complete their sentence under this type of scheme (Sorensen and Kyvsgaard, 2009).

### *Electronic Monitoring and Social Welfare Dependency in Denmark*

In this article, we investigate whether serving under electronic monitoring affects social welfare dependency. In a social democratic welfare state such as in Denmark, social welfare dependency is almost always synonymous with unemployment, as this type of welfare state provides universal coverage in case of unemployment (Esping-Andersen, 1990). The official statistics on unemployment rates in Denmark are generated using information on the number of people who depend on social welfare. In effect, this study investigates the effect of electronic monitoring on unemployment.

The link between a country's punishment regime and its spending on public assistance has been investigated thoroughly in the United States, and studies have found evidence that states actively prioritize between spending money on prisons versus spending money

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1. With the obligation to participate in a crime prevention program, it may seem as if serving under electronic monitoring represents multiple treatments. But imprisoned offenders also face obligations to engage in activities during their stay in prison, which enhances their chances to desist from crime once they are released.

on social welfare (Beckett and Western, 2001; Western and Beckett, 1999). This finding suggests that the two policy areas are endogenously related and that a change within one area is always paralleled by a change within the other. This is the so-called double regulation of the poor (Fording, Soss, and Schram, 2011; Wacquant, 2009), which limits the possibility of estimating meaningful causal relationships between phenomena such as the use of electronic monitoring and social welfare dependence.

Importantly, this confounding relationship seems much more predominant in the United States than in Europe. Evidence is shown by Western and Beckett (1999), who described how the far smaller prison populations in most European countries limit the scope for such interrelation of policies. Specifically for Denmark, if we count all inmates as unemployed (which is not realistic), then the unemployment rate would only increase by 0.15 percentage points (from 6.0% to 6.15%—November 2012 figures). This number compares with an estimated increase in the United States unemployment rate of 1.9 percentage points (Western and Beckett, 1999). The lenient Danish criminal justice system with short prison sentences and the overall low share of incarcerated Danes means that, in Denmark, imprisonment seizes only small parts of the labor force.<sup>2</sup> This makes it less obvious to use penal practices as instruments of social policy in Denmark. The suspicion that increased use of electronic monitoring is confounded by a change in social welfare dependence thus seems less likely in a Danish context compared with the United States.

### Causal Relationship?

The question then becomes why we should expect a causal relationship between serving a sentence under electronic monitoring and the subsequent unemployment of the offender. We can explain this by looking to the literature on the harmful effects of imprisonment.

Apart from incapacitating and deterring criminal offenders, a central aim of imprisonment is to prevent recidivism by rehabilitating and restraining the offender. It is the belief that imprisoned offenders cannot cause additional harm to the community and will use their time spent in jail to realize their wrongdoings, thus being transformed into law-abiding citizens at the time of release. This belief seems to miss the mark, however, as research has shown important and negative consequences of imprisonment for the life-course outcomes of offenders (e.g., Lopoo and Western, 2005; Massoglia, 2008; Massoglia, Firebaugh, and Warner, 2013; Schnittker and John, 2007; Turney, Wildeman, and Schnittker, 2012; Wald-fogel, 1994; Western, 2006; Western, Kling, and Weiman, 2001) and their families (e.g., Clear, 2007; Wakefield and Wildeman, 2013; Wildeman, Schnittker, and Turney, 2012).

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2. Sixty-two percent of all prison sentences in Denmark are shorter than 3 months, which should be compared with an average sentence length for all sentences in the United States of 63 months. Also in 2009, only 0.34% of the adult Danish population experienced incarceration (our own calculations based on data from Statistics Denmark) compared with 3.1% of the American adult population (Glaze, 2010).



The theoretical and empirical literature on these negative effects of imprisonment has identified several explanations of the harmful effects of imprisonment.

### *Stigmatization*

One explanation emphasizes the stigmas related to imprisonment. Many studies have used experiments to demonstrate significant discrimination against job applicants with criminal records: By varying only the criminal history of otherwise identical applicants, the studies have demonstrated employers' reluctance toward hiring ex-offenders, even for positions that do not require the holders to have a clean record (Cohen and Nisbett, 1997; Pager, 2003; Schwartz and Skolnick, 1962). The experiments suggested that the absence of full knowledge of an individual—in this case, the applicant—causes people (e.g., potential employers) to extrapolate from the knowledge obtained in the criminal record to unobserved individual characteristics (Goffman, 1963). The image formed might describe the individual incorrectly, but the studies have demonstrated a common perception of ex-offenders that prolongs their informal punishment indefinitely. Thus, the stigma of imprisonment could reduce the chances of ex-offenders gaining a strong foothold on the labor market after their imprisonment has ended.

### *Loss of Capital*

A second explanation emphasizes how incarceration affects human capital by giving inmates fewer years on the ordinary labor market. Thus, it is likely that their job skills are eroded and their opportunities for acquiring experience are restricted, which in turn increases their risk of unemployment (Waldfogel, 1994; Western et al., 2001). This mechanism might not only apply to labor market experience but also could affect an offender's opportunities for "investing" in other social relations, such as friendships and marriage, which could have promoted positive outcomes both on the labor market and in other domains (Hagan, 1993; Lopoo and Western, 2005). Thus, if ex-offenders have worse outcomes than others—for example, on the labor market—it might be a result of their unstable affiliation with normal society, which prevents them from making continuous social investments.

### *Offender Networks*

According to a third explanation, being imprisoned with other offenders affects the poor life-course outcomes associated with imprisonment through contagion. Theories of social networks (as well as many sociological theories in general) explain how peers strongly influence an individual's identity formation (e.g., Wasserman and Faust, 1994). Thus, spending time in prison—where the offender might become associated with other criminals and learn new tricks of the trade—will almost inevitably expand the offender's criminal network and thereby will most likely influence his or her behaviors and attitudes (e.g., Krohn, 1986). We can assume that these behaviors and attitudes are likely to be antisocial and could affect the offender's labor market orientation.

Thus, according to these three explanations, the negative effects of imprisonment originate from society's response to imprisonment, the offender's absence from society, and exposure to alternative norms and criminal networks experienced during imprisonment. All these explanations suggest that imprisonment reduces ex-offenders' chances in the labor market. The question then becomes what are the likely benefits of noncustodial alternatives to imprisonment, such as electronic monitoring?

### *Noncustodial Alternatives*

Even if empirical evidence is scarce, both scholars and practitioners seem to agree that the use of noncustodial alternatives to imprisonment, such as electronic monitoring, could help to alleviate some of the negative consequences of imprisonment (e.g., DiTella and Schargrodsky, 2013; Schwartz and Skolnick, 1962). Although both offenders who serve custodial and noncustodial sentences are likely to suffer from stigma—as they both hold criminal records—we can assume that people in their social surroundings make harsher judgment of the offenders who were incarcerated: The public is likely to perceive a prison term as a stronger marker of a bad personality than a noncustodial sentence. As a result, offenders who serve noncustodial sentences might experience fewer stigmas than offenders who are imprisoned when their sentences are completed (Western et al., 2001), which is likely to reduce the risk that offenders serving noncustodial sentences could face unemployment.

Furthermore, serving a noncustodial sentence will reduce the offender's absence from society, giving him or her better chances of keeping or at least searching for a job—as well as taking care of his or her family. Because the offender avoids imprisonment, he or she will acquire fewer new criminal peers compared with imprisoned offenders.

Thus, we can assume that noncustodial alternatives to imprisonment are less likely to cause human capital losses such as the ones suffered by imprisoned people, just as they reduce the exposure to negative peer influence. However, noncustodial alternatives might not completely remove the stigmas associated with being a convicted offender, but the stigmas are likely to be fewer than those associated with imprisonment. As a consequence, we can expect noncustodial alternatives to produce better outcomes for the offender and to reduce the risk of the offender facing unemployment and a need for social welfare after serving a sentence.

Qualitative evidence of offenders' experiences with electronic monitoring supports these claims. Informants from both the United States (Payne and Gainey, 2004) and Denmark (Jorgensen, 2011) confirm that the most painful consequence of imprisonment is the weakening of ties to family and the workplace, and they appreciate the ability to preserve these ties during electronic monitoring. Generally, although not exclusively, offenders prefer electronic monitoring over imprisonment, even though electronic monitoring is viewed as punitive, controlling, and painful (Gainey and Payne, 2000; Jorgensen, 2011; Payne and Gainey, 1998, 2004; Vanhaelemeesch, Vander Beken, and Vandevelde, 2013). Importantly,

these experiences do not correlate with the social and demographic backgrounds of offenders (Gainey and Payne, 2000).

### *Measuring the Effects of Noncustodial Alternatives*

Few existing studies have analyzed the causal effect of electronic monitoring on outcomes such as recidivism and unemployment, but those that have done so consistently have failed to show protective effects (MacKenzie, 2006; Renzema and Mayo-Wilson, 2005). Padgett, Bales, and Blomberg (2006), however, evaluated the effectiveness of electronic monitoring among 75,661 serious offenders on home confinement in Florida from 1998 to 2002. They found that technical violations, reoffending, and absconding were lower among offenders experiencing home confinement with electronic monitoring (similar results for different types of serious offenders) compared with those who were experiencing home confinement without electronic monitoring.

In Denmark, Jorgensen (2011) found no significant effect of electronic monitoring on recidivism among traffic offenders (targeted by the first reform in 2005) but discovered important protective effects of electronic monitoring on recidivism among young offenders targeted by the reform in 2006. Offenders convicted after the reform have lower recidivism rates, lower reimprisonment rates, and fewer reoffenses. These results allow us to expect rehabilitative effects of electronic monitoring in Denmark—at least among younger offenders targeted by the reform in 2006.

Similarly, in Sweden, Marklund and Holmberg (2009) used register data to compare the criminal recidivism rate of the first 260 offenders released early on parole with electronic monitoring to a matched historical control group of parolees. They found protective effects of electronic monitoring, especially among offenders with intermediate levels of previous crimes. Thus, compared with the historical control group, this sentence type lowers reoffense rates and reimprisonment.

Also a recent study from Argentina used variation between judges' preferences for sentencing offenders to electronic monitoring to claim causal inference. As these judges are randomly assigned to cases, their results are causal in nature and they found large, negative, and significant effects of electronic monitoring on rearrests (DiTella and Schargrodsky, 2013).

A few studies have used data from experiments that randomized imprisonment and noncustodial sentences, mainly community service (see, for example, Killias, Gilliéron, Villard, and Poglia, 2010, for a review of the literature; and see Killias and Villetaz, 2008; Renzema and Mayo-Wilson, 2005; Villetaz, Killias, and Zoder, 2006). In the case of perfect randomization, a simple comparison of group outcomes will express the mean effect of serving the one type of sentence compared with the other, as when serving with electronic monitoring rather than in prison. These experimental studies generally have found insignificant results, suggesting limited benefits of noncustodial alternatives to imprisonment.

Interestingly, these studies have focused only on the effect of noncustodial sentences on recidivism rates. Although this outcome is vital in a criminological context, other outcome measures are important indicators of how well offenders fare when their sentence has been served. Our study provides such additional knowledge by analyzing the causal effect of serving under electronic monitoring on offenders' subsequent labor market attachment. For this purpose, we exploit reforms implementing electronic monitoring in Denmark for specific types of offenders that would have been sent to prison prior to the reforms. Thus, we use the date of conviction as our instrument of randomization and rely on a historical control group in the same way as the Swedish study by Marklund and Holmberg (2009) and the Danish study by Jorgensen (2011).

## **Identification Strategy, Data, and Method**

### *Identification Strategy*

The offenders who serve their sentence under electronic monitoring represent a highly selected group, which means that we cannot assess the effect of this type of noncustodial sentence simply by comparing the outcomes of electronically monitored offenders with imprisoned offenders. These two groups will differ on a range of observed and unobserved characteristics that could cause severe bias in our estimates (Gable and Gable, 2005).

Instead, we exploit the implementation of the scheme where, from one day to the next, a group of offenders who previously would have faced imprisonment now have the opportunity to serve their sentence at home under electronic monitoring (as explained earlier). The reform represents a sharp discontinuity in offenders' chances of serving under electronic monitoring that is uncorrelated with their individual observed and unobserved characteristics, and its implementation therefore mimics an experiment in which the allocation of electronic monitoring and imprisonment is random.

For the purpose of our study, one way of assessing the effect of serving under electronic monitoring on social welfare dependence would be to estimate the average treatment effect on the treated (ATT), which is the effect of completing electronic monitoring compared with imprisonment. This would be done by comparing dependency rates for those who completed electronic monitoring after the scheme was introduced with dependency rates of those who were given a prison sentence before the scheme was introduced but whom we expect would have completed electronic monitoring had they been given the chance. But as we cannot tell who would have been granted permission for electronic monitoring and who would have completed it before the scheme was introduced, this strategy is not straightforward and would require the use of statistical models based on more or less defensible assumptions.

To avoid this, we settle for the more conservative intention-to-treat-parameter (ITT) (Angrist, 2005). We estimate this parameter by comparing dependency rates of those eligible for electronic monitoring in a given time period before and after the reform. This will show whether the reform—which provides the possibility of serving with electronic monitoring

and causes a significant share of offenders to serve under electronic monitoring—increases or decreases dependency of offenders. As indicated by its name, this parameter does not show the direct effect of electronic monitoring; rather, it shows how the availability of this way of serving a sentence affects average social welfare dependency in a group of offenders of which a significant share serve under electronic monitoring and the rest go to prison.

Our treatment is then being sentenced after the reform, where both imprisonment and electronic monitoring are available alternatives, whether the offender is imprisoned or at home under electronic monitoring. This strategy will provide us with a lowest bound estimate of the effect of electronic monitoring on those who actually serve under electronic monitoring because the estimate will reflect both the average treatment effect on the treated as well as the zero-influence from offenders who are sentenced to imprisonment after the reform. The control group consists of offenders who were sentenced before the reform, where imprisonment was the only available alternative. After presenting our main results, we clarify the relationship between the estimated policy effect (ITT) and the effect of electronic monitoring on those who experienced electronic monitoring (ATT), and we discuss the options for providing an estimate of the latter.

In sum, we compare social welfare dependency rates of offenders who were sentenced before and after the reforms and who fulfilled the formal criteria for being offered electronic monitoring. Under the assumption that the groups sentenced before and after each reform are comparable on observed and unobserved characteristics, this identification strategy provides us with an unbiased estimate of the effect of using electronic monitoring as an alternative to imprisonment.

Importantly, a change in judge behavior caused by the reform will violate our central assumption. Lenient judges might sentence offenders to 3 months of imprisonment instead of 4 to make them eligible for electronic monitoring, whereas harsher judges who are less in favor of electronic monitoring might increase sentence lengths to prevent offenders from being offered electronic monitoring. The same would apply if, for some other reason, sentencing practices or other macrolevel phenomena—as for instance rules regarding social benefit eligibility—change over time. We address these concerns by conducting a sensitivity analysis that tests such possible sources of bias.

### *Data*

For the analyses, we couple registry data from Statistics Denmark with information from the Danish Prison and Probation Service on who served their sentence under electronic monitoring. Both population data sources enable the selection of all offenders who fulfill the requirements of electronic monitoring and who are sentenced within reasonable time windows before and after each policy reform, and they enable the identification of offenders who actually serve under electronic monitoring rather than in prison. These administrative

data are rich in information and are subject to only small measurement errors. This enables control of a range of relevant and precisely measured covariates.<sup>3</sup>

From these data, we construct two samples, one for each reform. Each sample contains offenders that fit the selection criteria for electronic monitoring within a time window of  $\pm 1$  year of the reform dates (April 21, 2006 and July 1, 2008). They were thus sentenced to 3 months of imprisonment or less, and they were not sentenced to imprisonment within the preceding 2 years. They have not been remanded in pretrial custody in relation to the case in question, and they were not imprisoned for previous offenses when the conviction under observation was stated, just as they had no remaining prison terms that in combination with the current sentence would add up to more than 3 months. Also, only offenders who actually serve their sentence are in the samples.

Our control groups then are offenders convicted within 1 year before the reforms and our treatment groups are offenders convicted within 1 year after the reforms, and the 2006 raw sample has 1,811 offenders, whereas the 2008 raw sample has 4,763. As the 2006 reform targets offenders younger than 25 years of age, the 2006 reform sample only contains offenders younger than this age. With the 2008 reform, the age criterion was gone, but to secure a clean control group, we keep only offenders older than 25 in our 2008 reform sample (which drops 1,652 [35%] from this sample). We exclude offenders charged with possession of weapons or explosives who were sentenced to an imprisonment of less than 2 weeks in the 2008 sample, as these offenders were not eligible for electronic monitoring (which drops 31 [1%] from this sample).

As described, we do not analyze the 2005 reform, which concerns electronic monitoring of traffic offenders. However, to ensure that offenders treated by this reform do not pollute our analyses, we delete all traffic offenders from our samples (which drops 67 and 1,083 [4% and 35%, respectively] from the samples).

Because of the low number of women in our samples, we restrict our analyses to males (dropping 86 and 142 [5% and 7%, respectively]). Furthermore, we exclude offenders who serve other types of noncustodial sentences (e.g., community service) and offenders who disappear from the records during our observation period because of death, migration, missing information in the registry or duplicate entries, and so on (366 and 207 offenders, 24% and 11%, respectively).

The last sample selection step concerns the employment criterion for serving under electronic monitoring. Offenders could fulfill this criterion by working at firms appointed by the prison service. Yet we cannot identify individuals in the control group who would have been offered and would have accepted such employment, and we therefore exclude offenders in the treatment and control groups who appeared in the unemployment register in the week when their sentence started. This final step reduces our 2006 sample from 659

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3. For a general introduction to the merits of Scandinavian register data for criminological research, we refer the reader to Lyngstad and Skardhamar (2011).

TABLE 1

## Reform Samples

Reform Date	Description	Control Group	Treatment Group	Electronically Monitored
April 21, 2006	Males younger than 25, not traffic	499	513	243
July 1, 2008	Males older than 25, not traffic <sup>a</sup>	404	482	254

<sup>a</sup>Except violators of the weapon law and the law on explosives sentenced to prison terms of less than 14 days.

Source. Our own calculations based on data from Statistics Denmark and the Danish Prison and Probation Service.

controls and 633 treated offenders to 499 and 513 (a 22% reduction), and it reduces our 2008 sample from 709 controls and 939 treated offenders to 404 and 482 offenders (a 46% reduction). This careful, but restrictive, sample selection—where the analytic samples hold only 56% and 19% of the gross samples—ensures comparability between treatment and control groups and still leaves us with a substantial amount of data.

Table 1 shows the distribution of treated and controls in the two reform samples. Offenders in the control groups are sentenced to less than 3 months of imprisonment within 1 year preceding the reforms, and offenders in the treatment groups are sentenced to less than 3 months of imprisonment within 1 year after the reforms. The last column of the table shows the number of offenders in the treatment groups who actually completed their sentences under electronic monitoring. As shown, this applies to approximately half of the offenders in each of the two treatment groups (243 of 513, respectively, and 254 of 482).

### Variables

**Dependent variable.** Our dependent variable is social welfare dependence—or unemployment—measured as the offender's average rate of dependence on social welfare benefits during the first 52 weeks after the offender's release date. As is standard in Danish studies using administrative data, we use this variable as an indicator of unemployment, which is relevant in our Danish context, where social welfare dependence, as mentioned, almost always equals unemployment (and vice versa).

To construct this variable, we rely on a register from the Danish Ministry of Employment, which has information on the weekly benefit receipt of all Danes. Specifically, we define each offender's social welfare dependency level as the number of weeks each offender is dependent on unemployment-related public financial support within a period of 52 weeks.<sup>4</sup> The dependency rate then falls between 0 and 1, with 0 indicating no weeks of dependency and 1 indicating a full 52 weeks of dependency.

4. This measure excludes publicly supported vacation or leave from employment, publicly supported parental leave, publicly supported education, public pensions, and sick leave benefits.

Table 2 shows the distribution of our dependent variable in the two samples, by treatment status. The social welfare dependency level is 0.14 for those convicted after the first reform (treated) and 0.19 for those convicted before the reform (controls), which is a statistically significant difference. Dependency rates among those convicted before and after the reform in 2008 are close to identical at 0.26.

**Control variables.** Table 2 also shows the distribution of our choice of covariates. Prior social welfare dependency is identical to the dependent variable but is measured during the 52 weeks preceding the sentence. We find no differences between the treated and the controls on this variable, suggesting that the reforms are exogenous to prior unemployment.

We also control for social welfare dependency during the last week before the sentence, 26 weeks before the sentence, and 52 weeks before the sentence. These variables are dummies, and each indicates dependency during that particular week. We find no significant difference between our treatment and controls groups on these variables, which underlines the exogeneity of the reforms with regard to social welfare dependency. As we will explain, we only use these dummy variables in our more elaborate analyses (difference-in-differences matching).

The control variables in Table 2 fall into four categories: demographic variables (age, marital status, parenthood, and ethnicity), socioeconomic variables (on social pensions, enrolled in school, and elementary school as highest education attained), labor market affiliation (prior income and job type), and criminal career characteristics (violent offender, sentence length [both imprisonment and probation], age at criminal onset, total number of prior convictions, days since the individual was last convicted of a crime, total number of incarceration terms, and the cumulated number of days an individual was incarcerated prior to the prison term in question).

As expected, the offenders in our samples hold relatively marginalized social positions. Few are married and many belong to ethnic minorities. Many receive social pensions and the schooling level is low. Also, their mean income is low and approximately half of the offenders are unemployed. Last, two thirds are convicted of violent crimes; the younger offenders have on average two previous incarceration terms (including arrests), and the older offenders have four. The mean sentence length in both samples is approximately 45 days, which reflects the maximum sentence length requirement of 3 months for electronic monitoring. In contrast, the extent of probation assigned to the offenders differs across samples and within samples.

As a result of the exogeneity of the reforms, we expect the covariate distribution of our treatment and control groups within each sample to be similar. A comparison of the control variable distribution in the 2008 reform sample (last two columns of Table 2) reveals only two differences. These concern parenthood and the length of probation. With a total of 25 covariates, this then means that the treatment group and the control group differ on 8% of the covariates. According to the 95% test described in Agodini, Thornton, Kahn, and Peikes (2002), significant differences between the treatment and control group in up to 5%



TABLE 2

## Descriptive Statistics, by Sample and Tested for Significant Differences

Variable	Younger Offenders (2006)		Older Offenders (2008)	
	Before Mean (SD)	After Mean (SD)	Before Mean (SD)	After Mean (SD)
Dependency rate	0.189 (0.296)	0.140 (0.255)**	0.257 (0.370)	0.262 (0.354)
Prior dependency rate	0.141 (0.252)	0.130 (0.241)	0.165 (0.290)	0.158 (0.283)
Dependency rate 1 week before treatment	0.002 (0.045)	0.000 (0.000)	0.002 (0.050)	0.002 (0.046)
Dependency rate 26 weeks before treatment	0.134 (0.341)	0.144 (0.352)	0.171 (0.377)	0.168 (0.374)
Dependency rate 52 weeks before treatment	0.178 (0.383)	0.156 (0.363)	0.193 (0.395)	0.172 (0.378)
Age	20.822 (2.233)	20.947 (2.182)	35.475 (8.290)	36.004 (8.403)
Unmarried	0.988 (0.109)	0.992 (0.088)	0.790 (0.408)	0.801 (0.400)
Parent	0.447 (0.498)	0.427 (0.495)	0.297 (0.458)	0.156 (0.363)***
Ethnic minority	0.224 (0.418)	0.230 (0.421)	0.153 (0.361)	0.178 (0.383)
On social pension	0.397 (0.490)	0.331 (0.471)*	0.213 (0.410)	0.239 (0.427)
In school	0.152 (0.360)	0.090 (0.286)**	0.007 (0.086)	0.019 (0.136)
Schooling: elementary	0.924 (0.266)	0.908 (0.289)	0.557 (0.497)	0.579 (0.494)
Prior gross income <sup>a</sup>	109.781 (96.150)	116.173 (97.532)	253.548 (251.471)	254.556 (17.581)
Job type: other <sup>b</sup>	0.176 (0.382)	0.228 (0.420)*	0.245 (0.431)	0.237 (0.425)
Job type: crafts <sup>b</sup>	0.218 (0.414)	0.214 (0.411)	0.205 (0.405)	0.174 (0.380)
Job type: service <sup>b</sup>	0.086 (0.281)	0.123 (0.329)	0.079 (0.270)	0.079 (0.270)
Job type: knowledge <sup>b</sup>	0.034 (0.182)	0.004 (0.062)***	0.059 (0.237)	0.068 (0.253)
Job type: no job <sup>b</sup>	0.485 (0.500)	0.431 (0.496)	0.411 (0.493)	0.442 (0.497)
Convicted of violence	0.631 (0.483)	0.678 (0.468)	0.668 (0.471)	0.608 (0.489)
Sentence length	43.617 (25.350)	43.774 (23.296)	46.020 (24.494)	44.081 (22.193)
Suspended sentence length	24.629 (65.021)	9.682 (41.220)***	6.450 (25.091)	2.633 (14.983)**
Age at criminal onset	18.426 (2.319)	18.341 (2.285)	25.896 (1.067)	25.440 (1.441)
Total number of convictions	1.888 (2.404)	2.322 (2.529)**	4.592 (6.413)	4.635 (5.577)
Days since last crime	2992.367 (3257.347)	2329.856 (3039.083)***	4787.790 (5687.357)	4529.108 (5629.391)
Total number of incarcerations <sup>c</sup>	2.281 (2.705)	2.232 (2.902)	4.047 (6.013)	4.162 (5.148)
Total days incarcerated <sup>c</sup>	21.040 (89.654)	29.704 (121.383)	112.295 (346.908)	124.253 (349.449)
<i>n</i>	499	513	404	482

Source. Our own calculations based on data from Statistics Denmark and the Danish Prison and Probation Service.

<sup>a</sup>Gross income is reported in DKK 1,000 (2009 prices). DKK 1,000 = US\$185 in 2009.

<sup>b</sup>Job type refers to the position held in November the year before treatment.

<sup>c</sup>Including arrests.

\* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$ .

of the covariate distributions are not automatic indications of systematic and problematic differences between the two groups. Rather, we should actually expect this finding given the principles of statistical testing and the likely presence of statistical noise in our data. With 8% differences, our sample crosses the acceptable 5% limit, although not by much. However, we test whether these differences affect our results (as we will explain shortly).

We also find differences in 7 of 25 control variables for the 2006 sample, which exceeds what common statistical noise should produce. The seven unbalanced variables include the share on social pensions, the share in school, the share working in knowledge or “other” jobs, the number of previous convictions, the number of days that had passed since they were convicted the last time, and as mentioned, the length of probation.

These discrepancies between treated and controls are puzzling given the exogeneity of the reform, and to the extent that they reflect systematic differences between the two groups, they compromise the validity of our causal estimate if we do not deal with them in a proper manner. We shall return to this question shortly.

Last, it should be noted that, except for having lower prior dependency rates, those who actually serve their sentence under electronic monitoring are remarkably similar to the entire offender groups sentenced after the reforms, especially regarding young offenders sentenced during the first year after the 2006 reform. In relation to older offenders (sentenced during the first year after the 2008 reform), a few important differences emerge, however. Specifically, those who served their sentence under electronic monitoring were slightly older when they were convicted of their first criminal offense, they had committed fewer crimes previously, and they had been incarcerated fewer times and had a shorter cumulative length of previous incarcerations than their imprisoned counterparts who were also sentenced during the first year after the 2008 reform (results available from the authors upon request).

### *Method*

As described, we get exogenous variation in those who are offered to serve under electronic monitoring from the implementation of the two reforms. In principle, we could then assess the effect of the reforms by simply comparing the outcomes of our control and treatment groups. However, as indicated, there are some minor observed—and probably unobserved—differences between offenders in the two groups, just as they are likely to be exposed to different macrolevel environments (i.e., historical effects), because we observe them during different years (by definition, the members of the control group always serve their sentence on average 1 year before the treated ones serve their sentences). Thus, we need to apply a statistical method suitable for taking these differences into account to be sure that any differences in dependency rates between the treated and the controls reflect the possibility of serving under electronic monitoring rather than other differences. For this purpose, we calculate the effect of electronic monitoring on social welfare dependence using two different approaches: the difference-in-differences estimator and difference-in-differences matching.

Using the difference-in-differences models, we calculate the treatment effect as differences between the treatment group and the control group in the change in social welfare dependence from before they are sentenced until after they have served their sentence. The treatment effect then reflects the relative change in the dependence of the treatment group compared with the relative change in the dependence of the control group. With this strategy, the model produces an unbiased estimate, provided that any influential observed and unobserved differences between the treatment and the control group are time constant or are exposed to the same trends over time (Blundell and Dias, 2009). This is because the comparison of *changes* in social welfare dependency levels, rather than just of *levels* observed in the two groups, eliminates the influence from initial differences: The subtraction of the postconviction dependency level from the preconviction level removes time constant, individual-specific components that are otherwise unaccounted for—because they are time constant, they simply net out. Also, the subtraction of the observed change in the control group from the observed changes in the treatment group eliminates the influence from time trends; given the assumption that the time trends are similar in both groups, they also simply net out. We present results from a simple difference-in-differences model, as well as from an expanded model with controls.

In the presence of time-variant differences between the two groups, the difference-in-differences estimates might become biased. Such time-variant differences could result from macrolevel changes in social welfare dependency. We test this potential problem in a sensitivity analysis in which we normalize our outcome variable using the general time trend in dependency rates across the reform years. Another macrolevel change could result from judges changing their sentencing behavior in lieu of the reform, which would affect the composition of the before and after reform samples. We test this concern in another sensitivity analysis.

As we discussed, some of the unbalanced covariates in our samples could be indicators of unobserved time-variant differences that invalidate the results from the difference-in-differences model. We address this concern by also presenting results from difference-in-differences matching. This model combines propensity score matching with the difference-in-differences approach by balancing the treatment and control groups on control variables, as well as on pretreatment observations of the outcome variable—in our case, social welfare dependence prior to the sentence. The estimator more specifically compares the outcome of the treatment group with the outcome of the control group, weighted by their predicted probability—the propensity score—of being in the treatment group. We predict the propensity score by regressing treatment status on the control variables, including prior welfare dependence. We then simply calculate the effect of electronic monitoring as the difference-in-differences estimate weighted by the propensity score. Assuming that offenders with similar observed characteristics and who have identical pretreatment outcomes are exposed to the same time-variant heterogeneity, this strategy produces unbiased estimates of the treatment effect (Blundell and Dias, 2009; Heckman, Ichimura, and Todd, 1997).

**FIGURE 1**

### Average Weekly Dependency Rates Before and After Sentence, by Reform Sample



*Source.* Our own calculations based on data from Statistics Denmark and the Danish Prison and Probation Service.

The difference-in-differences matching estimator reduces to simple propensity score matching when the treatment and control groups are balanced on their prior dependency rates (Chabé-Ferret, 2010).

As a sensitivity analysis, we report results from three different ways of weighting the propensity score. We use 1:1 nearest-neighbor matching, where each treated offender is compared with the most identical offender in the control group; 1:10 nearest-neighbor matching, where each treated offender is compared with the 10 most identical offenders in the control group; and kernel matching, where each treated offender is compared with all of the control offenders weighted by how much they differ in their propensity score. We match with replacement (each control offender might be matched to more than one offender in the treatment group) and use only offenders within the area of common support (which means that extreme observations are excluded).

## Results

This section presents our results, both our main results from the difference-in-differences models and from difference-in-differences matching, and from our sensitivity analyses. But first, we provide an intuitive understanding of the reform effect.

Figure 1 depicts the average weekly social welfare dependence rate among the treatment and control groups, by reform sample. The figure disregards actual time served either in prison or under electronic monitoring. Zero on the time axis indicates the week the sentence was initiated, and the data points to the left of zero count the weeks prior to the sentence.

TABLE 3

**Estimated Effects of Electronic Monitoring on Social Welfare Dependency**

Estimator	Younger Offenders (2006) Estimate (SE)	Older Offenders (2008) Estimate (SE)
Simple difference-in-differences	−0.038 (0.017)*	0.013 (0.021)
Covariate adjusted difference-in-differences	−0.038 (0.018)*	0.014 (0.021)
1:1 Nearest-neighbor matching	−0.072 (0.031)*	−0.010 (0.037)
1:10 Nearest-neighbor matching	−0.054 (0.015)***	−0.005 (0.029)
Kernel matching	−0.063 (0.015)***	−0.005 (0.027)

Source: Our own calculations based on data from Statistics Denmark and the Danish Prison and Probation Service.

\* $p < .05$ ; \*\*\* $p < .001$ .

Zero on the time axis also indicates the release date, and data points to the right are weeks after release. By construction, no offenders receive social welfare benefits during the week their sentence started, as is observed from the drop in the dependency rates as one approaches zero from the left.

The left panel of Figure 1 (Figure 1a) concerns the 2006 reform and shows marked differences in dependence rates during the time after release, especially within the first weeks. However, there is no difference in presentence rates. This finding suggests that serving under electronic monitoring has an effect on offenders younger than 25 years of age.

The right panel (Figure 1b) concerns the 2008 reform, and it shows no presentence differences between the dependence rates of the treated and the controls; yet, neither does it reveal any differences after release. The dependence rates of offenders older than 25 years, who were targeted by the last reform, are thus not affected by how they serve their sentence.

Interestingly, the postrelease dependence rates differ between the two panels of Figure 1. For young offenders, they are rather stable at around 14% (after) and 19% (before) but increase from below 25% to approximately 30% within the first 52 weeks after release for older offenders. The negative effects of both imprisonment and electronic monitoring thus seem to increase during the first year after release for older offenders.

Table 3 shows our estimated effect of electronic monitoring on social welfare dependence using each of the model approaches described in the Methods section.<sup>5</sup> The first column shows results from the 2006 sample of younger offenders, and the second column shows results from the 2008 sample of older offenders.

5. When using difference-in-differences matching, the matched samples have identical covariate distributions, except for prior income (treatment group lower than control group at 5% level) and number of days since last conviction (treatment group fewer than control group, again at 5% level) when we only match to one nearest neighbor in the 2008 reform sample. Tables on the covariate distributions of the matched samples are available from the authors upon request.

For the younger offenders, we find consistent results across all five models. All estimates are negative and statistically significant, which suggests that young offenders who receive their sentence after the 2006 reform experience lower dependence rates than young offenders sentenced before the reform. The effect size varies between the difference-in-differences models and the difference-in-differences matching models, indicating that offenders sentenced at a time when electronic monitoring was possible experienced dependence rates that were between 3.8 and 7.2 percentage points lower during the first 52 weeks from their release. This reduction in dependency amounts to between 14 and 26 days of a year and could reduce public expenditures by as much as DKK 19,000 (~US\$ 3,500) per offender (depending on the year and type of unemployment).

For the older offenders, we find no effect of receiving a sentence after the 2008 reform and thus facing the possibility of serving under electronic monitoring. None of the estimates are significant just as their signs change across models—suggesting that the “effect” of electronic monitoring is in the range of  $-0.5$  to  $1.4$  percentage points. Thus, we cannot reject the null hypothesis that there is no effect of the reform on this group’s unemployment. This result was also hinted at in Figure 1, which showed identical welfare dependence rates for the treatment and control groups both prior to and after their sentence.

#### *What Is the Effect on Those Who Experienced Electronic Monitoring?*

The results presented in Table 3 show the average effect of electronic monitoring across the entire group of offenders sentenced after each reform, which corresponds to the effect of implementing the reforms. However, as discussed, only approximately half of the treatment group completes its sentence under electronic monitoring while the rest is imprisoned. Provided that the reform does not affect social welfare dependence among imprisoned offenders in the treatment group (which we assume that it does not), the reform effect consists of a zero influence from the imprisoned offenders and a nonzero influence from offenders serving under electronic monitoring. By definition, the reform effect is then a lower bound estimate of the effect of serving under electronic monitoring.

One might, however, also be interested in the effect of the treatment on those who are given the treatment, which is the effect of electronic monitoring on those offenders who actually serve under electronic monitoring. We can calculate this effect by dividing the estimated effects in Table 3 by the participation rate, which is defined as the share of offenders convicted after the reform who serve under electronic monitoring (this share is obtainable from Table 1 and from the matching results). This strategy scales the effect to reflect the share that is actually contributing to it. Importantly, this strategy relies on the assumption described previously, that the behavior of our never-takers—offenders who are sentenced to prison after the reform—does not change as a result of the reform. If this assumption is violated and the never-takers do change their behavior, then our strategy would wrongfully ascribe all of the treatment effect to those who are actually treated, and we would not obtain an accurate result of the treatment effect on the treated. As mentioned, the offenders who

TABLE 4

**Effect on Those Who Experienced Electronic Monitoring (ATT), Estimated from the Policy Effect (ITT) and the Participation Rate (P)**

Estimator	Younger Offenders (2006)		Older Offenders (2008)	
	ITT / (P)	ATT	ITT / (P)	ATT
Simple difference-in-differences	−0.038 / (243/513)	−0.081	0.013 / (254/482)	0.026
Covariate adjusted difference-in-differences	−0.038 / (243/513)	−0.081	0.014 / (254/482)	0.026
1:1 Nearest neighbor matching	−0.072 / (242/511)	−0.163	−0.010 / (248/475)	−0.018
1:10 Nearest neighbor matching	−0.054 / (242/511)	−0.115	−0.005 / (248/475)	−0.010
Kernel matching	−0.063 / (242/511)	−0.134	−0.005 / (248/474)	−0.010

Source. Our own calculations based on data from Statistics Denmark and the Danish Prison and Probation Service.

actually serve their sentence under electronic monitoring do not differ substantially from those who are imprisoned on most covariates in our study, especially among the young offenders targeted by the 2006 reform, but because this type of unobserved contamination of the never-takers is still a possibility, we advise that the following results are interpreted with caution.

The results are presented in Table 4, and they show that the effect on those who did serve under electronic monitoring is approximately twice the size of the estimates in Table 3 (participation rates are just below 50% in both samples). Hence, young offenders, who served their sentence under electronic monitoring after the reform in 2006, reduced their social welfare dependence by an extra 15 to 30 days within the first year after release. These young offenders clearly gained from serving this type of noncustodial sentence. As our estimates for the older offenders targeted by the 2008 reform are insignificant and change sign across models, we refrain from interpreting the effect on this group (and most likely the average treatment effect on this group is simply 0).

### *Sensitivity Analysis*

As discussed in the Methods section, our results rely on the assumption that the reforms are exogenous to offender characteristics and on the assumption that the treatment and control groups experience similar general time trends in their dependency rates, despite not being sentenced at the same point in time. However, these assumptions are violated if judges change their sentencing behavior either as a result of the reform or for other reasons, or if macro trends in unemployment differ from one year to the next. In this section, we therefore present results from two sets of sensitivity analyses, investigating each of these challenges.

*Judges' sentencing behavior.* Lenient judges might, as explained earlier, sentence offenders to 3 months of imprisonment instead of 4 to make them eligible for electronic monitoring. In contrast, harsher judges, who might be less in favor of electronic monitoring,

might increase sentence lengths if they expect the Prison and Probation Service to offer the offender to serve under electronic monitoring. In addition, judges' sentencing behavior might change across the reforms for other reasons, because of other alterations of the punitive system, or because of changing rhetoric about punishment in society. Both situations will either decrease or increase sentence lengths after the reforms, and we would potentially mismatch offenders from the treatment and control groups in our difference-in-differences matching and pick wrong control group members for the difference-in-differences models. As a consequence, the results presented in Table 3 would be biased.

We investigate this potential problem by first comparing the distribution of sentence lengths before and after each reform for the relevant offender groups. This is a check to determine whether more offenders are given short or long sentences across the reforms and, in particular, to determine whether there is a change in the share of offenders who are given sentences just over or under the threshold of 92 days (92 days is the threshold for being eligible for electronic monitoring). From Table 2, we already know that sentence lengths do not change on average before and after each reform. But these within-sample changes only reflect possible changes in the sentence lengths of offenders who are sentenced to less than 92 days (because of our sample selection criteria described earlier) and are therefore not indicative of changes across this threshold. Also, differences in means only reflect part of a potential change in the entire distribution and might disguise changes in specific areas of the distribution rather than for all offenders.

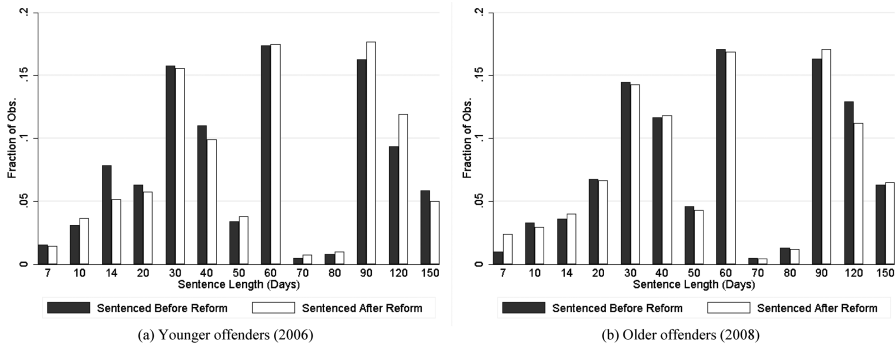
We therefore test for the presence of changes in sentence lengths caused by changes in judge behavior by focusing on prison sentences between 7 (which is the shortest possible prison term in Denmark) and 150 days, as most prison sentences in Denmark are within this span (approximately 80%). We do not expect judges to respond to the electronic monitoring reforms in criminal cases that are likely to result in long (>150 days) prison sentences simply because any reasonable change in sentence length for such cases is unlikely to cross the threshold of 92 days.

Figure 2 shows these distributions by sample. The lighter bars show the sentence length distribution of those sentenced after the reforms, whereas the darker bars show the sentence length distribution for those sentenced before the reforms. As observed, there are only minor differences between these groups. The figure for young offenders targeted by the 2006 reform (Figure 2a), however, hints that fewer offenders sentenced after the reform are given really short sentences and more offenders are given longer sentences. This finding might indicate a general "push to the right" for offenders affected by this reform and could reflect changed sentencing behavior among judges, either as a result of the reform or for other reasons. In the figure for older offenders targeted by the reform in 2008 (Figure 2b), it is difficult to identify differences between the groups, and it seems that sentence lengths are stable across this reform.

If we focus more narrowly on offenders who are given sentences just over or under 92 days (the electronic monitoring threshold), however, there are no indications that judges



FIGURE 2

**Sentence Length Distribution Before and After the Reforms**

*Source.* Our own calculations based on data from Statistics Denmark and the Danish Prison and Probation Service.

have changed their behavior across the reforms. We do not find any consistent indications of more offenders receiving just less than 92 days, paralleled by less offenders receiving more than 92 days, or vice versa. We therefore have no reason to suspect that there is a bias in our results arising from judges' explicit reactions to the implementation of electronic monitoring.

Yet to eradicate any suspicion that different sentence lengths across the reforms bias our results, we conduct sensitivity analyses that rerun our analyses only on those offenders who are sentenced to 80 days or are given an even shorter sentence. In this part of our samples, the sentence length distribution is close to identical across groups. Restricting the samples in this way reduces the sample sizes by 126 offenders (2006 sample) and 112 offenders (2008 sample). The upper half of Table 5 shows results from this analysis, and as shown, they are similar to our main results presented in Table 3. Note, however, that the estimates from the 2006 sample of young offenders are slightly smaller than in our main results and that the lower number of offenders used for these analyses reduces the statistical significance of the estimates. Both sets of results (from Table 3 and Table 5) point in the same direction and allow us to conclude that younger offenders are better off serving under electronic monitoring than in prison, whereas older offenders remain unaffected by the sentence type—at least in terms of social welfare dependency.

**Changing macro-trends.** Another concern is that changes in the business cycle drive or bias our results. The macroeconomic conditions affecting the labor market at the time when newly released offenders need to take up jobs are likely to impact their risk of becoming unemployed and hence depend on social welfare benefits. This is especially true in our case, as both reforms were introduced at or after the onset of the global financial crisis.

**T A B L E 5**

**Results from Sensitivity Analyses**

<b>Estimator</b>	<b>Younger Offenders (2006) Estimate (SE)</b>	<b>Older Offenders (2008) Estimate (SE)</b>
Maximum Sentence Length Set to 80 Days		
Simple difference-in-differences	−0.037 (0.018)*	0.006 (0.022)
Covariate adjusted difference-in-differences	−0.038 (0.018)*	0.003 (0.022)
1:1 Nearest neighbor matching	−0.051 (0.032)	0.013 (0.048)
1:10 Nearest neighbor matching	−0.048 (0.022)*	0.001 (0.032)
Kernel matching	−0.046 (0.020)*	−0.008 (0.033)
Normalized Dependency Rates		
Simple difference-in-differences	−0.039 (0.017)*	0.013 (0.021)
Covariate adjusted difference-in-differences	−0.039 (0.018)*	0.013 (0.021)
1:1 Nearest neighbor matching	−0.026 (0.028)	0.015 (0.035)
1:10 Nearest neighbor matching	−0.050 (0.021)*	−0.009 (0.028)
Kernel matching	−0.052 (0.018)**	−0.011 (0.028)

Source: Our own calculations based on data from Statistics Denmark and the Danish Prison and Probation Service.

\* $p < .05$ ; \*\* $p < .01$ .

In Denmark, for example, unemployment rates increased annually from the onset of this crisis, a macro-trend that might matter to our difference-in-differences approach. Notice, however, that if anything, our treatment groups finish their sentences at worse times than the control groups because members of the treatment groups always finish 1 year later. This might cause a downward bias in our results and suggests that, in times when the economic situation is stable, our estimated treatment effect would be even bigger for young offenders.

Also, changes in the rules regarding the eligibility of social welfare that co-occur with our reforms might bias our results. For instance, on March 27, 2006, a new law restricted the possibilities of dual social welfare dependence of married couples in Denmark. This law coincides with our 2006 reform and represents a macrolevel change in eligibility for social welfare that could potentially influence our results (some married couples simply lost half of their social welfare because of the new law). Although very few offenders in the 2006 sample are married (approximately 1%), other less obvious macrolevel changes might bias our results.

We test these concerns by rerunning our analyses using a normalized outcome variable. For this purpose, we normalize the social welfare dependence rates in our samples by the corresponding dependence rates (for the relevant years) in a sample of nonconvicted men in comparable age groups. This normalization ensures that contextual changes affecting unemployment among our offenders as well as among other Danish men do not influence our results. The lower half of Table 5 shows these results, and as observed, the estimates do not differ greatly from those in our main results (Table 3). Normalizing the dependency

rates thus does not alter our results, and the general economic conditions do not seem to jeopardize our conclusions.

## Discussion and Conclusion

This study shows that serving a sentence under electronic monitoring rather than being imprisoned affects social welfare dependency of young offenders after release. In contrast, offenders older than 25 years of age follow the same dependence trajectory regardless of how they serve their sentence. Because of the close connection between unemployment and social welfare dependence in a Scandinavian welfare state such as in Denmark, our findings show that serving a noncustodial sentence affects the risk of unemployment among select groups of offenders.

As a result of a policy reform in 2006, offenders younger than 25 years of age could apply for electronic monitoring and avoid prison if they accepted certain conditions and fulfilled certain requirements. In 2008, another reform removed the age criterion and allowed offenders older than 25 years to apply for electronic monitoring. Both reforms targeted offenders sentenced to less than 3 months of imprisonment.

Both reforms resulted in more people serving their sentences under electronic monitoring, and the new sentence type made a difference, particularly for offenders younger than 25 years of age. All models clearly indicate that young offenders who received their sentence at a time when it was possible to serve a sentence under electronic monitoring had lower dependence rates after release compared with offenders who were sentenced at a time when electronic monitoring was not an option. This finding suggests that more efforts should be made to organize and implement the use of noncustodial sentences, at least for young offenders.

In contrast, we find no statistically significant results from the reform in 2008, which targeted offenders older than 25 years. Hereby social welfare dependence of offenders older than 25 years of age remains unaffected by how they serve their sentence.

Importantly, another Danish study, which has already been mentioned, showed that, in addition to reducing social welfare dependence, the 2006 reform also lowered recidivism (Jorgensen, 2011). Thus, there is no indication that the reduced unemployment experienced by the 2006 treatment group is paralleled by negative outcomes in other relevant spheres (for example, increased criminal activities because of a lack of an incapacitation effect).

## Policy Implications

The different results for younger and older offenders provide two key insights. First, serving a noncustodial sentence such as electronic monitoring only affects younger offenders. And second, regardless of sentence type, the dependence rates continue to increase for older offenders after their release, whereas the dependence rates of the younger offenders are far more stable. This finding suggests that younger offenders are more vulnerable to the negative influences of imprisonment—as serving a noncustodial alternative sentence reduces

their welfare dependence—whereas being convicted and sentenced might have far-reaching social consequences for older offenders, as they typically have more at stake in terms of social relations, human capital, and job market positions. The cost of imprisonment might in this sense be greater for younger offenders, whereas the stigma of conviction matters more for the older offenders—at least in Denmark. This null finding for older offenders is, however, still interesting from a societal point of view. It is far more costly to send offenders to prison than to monitor them electronically in their own home, and even in the absence of individual gains from serving under electronic monitoring for this group of offenders, society could save money when choosing electronic monitoring instead of prison.

Importantly, our identification strategy does not provide information on the mechanisms driving such larger costs of imprisonment for younger offenders. As explained in the Background section, the mechanisms include the social perception of imprisoned young people (stigma effect), fewer new affiliations with criminal peers (network effect), and the loss of basic skills (human capital effect). Future studies should investigate these mechanisms as a greater understanding of their explanatory powers would enhance our knowledge on why noncustodial sentences, which have recently become so popular among policy makers, can make a substantial difference to the lives of offenders.

Two central caveats need discussion. First, our dependent variable of social welfare dependence does have some limitations, even though data from the Danish registry are generally very precise, especially compared with other data sources. We measure social welfare dependence as the average weekly rate of receiving social welfare benefits. But although weeks without welfare benefits might be weeks of employment, they could also be weeks of other types of self-support. Such self-support could include being supported by the income of a spouse or waiting to become eligible for benefits. Thus, in some cases, the absence of social welfare dependence does not equal labor market participation. This observation suggests that we should also examine other outcome measures to assess the full effects of electronic monitoring.

The second caveat concerns our ability to generalize our results to other contexts, especially the American context, which differs so much from the Danish one. The two countries differ in geographical, social, as well as penal terms (Lappi-Seppälä and Tonry, 2011), and according to the World Prison Population List (Walmsley, 2011), incarceration rates in the United States and in Denmark differ more than almost in any other country. The obvious question then becomes one of what insights the results on noncustodial sentencing in a small Scandinavian country with a universal welfare state and a lenient penal regime have to offer to policy makers in the United States and other countries.

In our opinion, our findings are informative in several ways. First, as discussed, the literature on the effects of noncustodial sentences is limited, and most previous studies that exploit exogenous variation in sentence type either focus exclusively on recidivism as their outcome measure or are experimental studies that suffer from small sample sizes. Although it is likely that small sample sizes cause the insignificant results from these experimental studies,

we currently do not know whether the anticipated benefits of noncustodial sentences vis-à-vis imprisonment are only anticipated or whether they are causal effects in nature. Yet with the help of more than twice as many offenders in our sample and by means of using policy reforms to provide exogenous variation in sentence type, our study confirms the positive effects of noncustodial alternatives (in our case, electronic monitoring) on an important alternative outcome measure—and does so with statistical significance.

Second, as the United States moves toward noncustodial alternatives to imprisonment, it makes sense for policy makers to direct attention to experiences from other contexts, especially because the speed with which the reforms sweep across the country seems to exceed the speed of quantitative evaluations of such reforms. With, for example, the European-American Prison Project, state officials have in fact already begun their search for experiences and inspiration across the Atlantic Ocean, as the American penal system could potentially learn much from the corrections practices of other countries (Subramanian and Shames, 2013).

Third, compared with the American context, the requirement that a prison sentence in Denmark cannot exceed 3 months for the offender to be eligible for electronic monitoring might seem to aim electronic monitoring at a specific type of offender, and therefore, the effects of electronic monitoring might be valid only for this highly selected group. But as mentioned, the majority of sentences in Denmark are in fact that short. As we show even when we convert these (comparatively) short sentences into electronic monitoring, the outcomes of offenders are vastly improved, which suggests that relatively minor adjustments of the lenient Danish penal system might have important effects that could, in time, contribute positively to Danish society. One might only speculate as to what the consequences of expanding the use of electronic monitoring in the United States would be for the American public and for the millions of Americans who are affected by the negative effects of mass imprisonment—especially because the amount of time offenders spend in prison in the United States is so dramatically different from the time they spend in prison in Denmark and because the time spent in prison in the United States might cause even greater stigma, even more human capital loss, and even further-reaching negative network effects than it does in Denmark.

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### **Statute Cited**

Danish Corrections Act, Law no. 367: Fuldbyrkelse af straf på bopælen under intensiv overvågning og kontrol samt begrænset fællesskab for “negativt stærke” indsatte m.v [The execution of sentences as home confinement under intensive monitoring and control, and decreased community privileges for “negatively strong” prisoners, etc.]. Enacted on May 24, 2005.

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## It's a Small World, but I Wouldn't Want to Paint it

### Learning from Denmark's Experience with Electronic Monitoring

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Comedian Steven Wright once quipped, "It's a small world, but I wouldn't want to paint it." The first part of the sentence points to the global nature of the world in which we live, how easy it is to get from one country to the next, and how all cultures are connected in one way or another. The second part of the quote points to the vastness of our world. Implicit within this quote is an understanding that cultural meanings, experiences, and growth are influenced by a particular country or region's political, legal, social, technological, and geographical contexts. Within this context, Andersen and Andersen's (2014, this issue) research on electronic monitoring in Denmark can help to expand our understanding about electronic monitoring in the United States and other countries.

Andersen and Andersen (2014) should be applauded for their efforts to study the impact of electronic monitoring reform in Denmark. Their research shows that electronic monitoring reduces the social welfare dependency of younger offenders, and although it does not reduce the dependency of older offenders on welfare, it is equally effective (or equally problematic) as prison for the older age group. The strength of this study is that it provides additional insight into the utility of electronic monitoring, and this insight can be used to add context to the debate about the use of electronic monitoring. In particular, from my perspective, Andersen and Andersen's study points to the need to address the following areas: (a) the evolution of electronic monitoring, (b) how electronic monitoring is defined, (c) the experience of electronic monitoring, (d) the role of theory in electronic monitoring practices, (e) the future of electronic monitoring research, (f) entrepreneurial

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thinking and electronic monitoring, and perhaps most importantly, (g) what we can learn from the Denmark experience.

### **Evolution of Electronic Monitoring and Alternative Sanctions in General**

Electronic monitoring did not simply appear one day as a tool to monitor offenders. In the United States, six historical stages characterize the evolution of electronic monitoring in the criminal justice system. These stages include pretechnology, sporadic use, innovation, expansion, acceptance, and reliance. The pretechnology stage is traced back to 1977 when New Mexico Judge Jack Love first came up with the idea. Judge Love was enamored with a Spider Man comic story featuring the hero being electronically monitored by his arch villain. Love realized that a similar tool could be used to monitor low-risk offenders. He enlisted the help of Mike Goss, a former Honeywell salesman, who eventually developed the first electronic monitoring anklet (Beato, 2012).

The sporadic use stage began when the first offender was placed on electronic monitoring in 1983. Shortly after, a jurisdiction in Florida began to use electronic monitoring in conjunction with house arrest. By the end of the 1980s, the electronic monitoring tool was being used in many different jurisdictions across the United States. At the time, critics of the new alternative sanction cited a variety of concerns. One concern was that the sanction widened the net of criminal justice control by bringing additional offenders into the system who otherwise would not have been sanctioned. Another concern was that the sanction was a form of “Big Brotherism” that invaded the privacy of individuals’ homes. Some also questioned whether the sanction was too lenient, perhaps a mere slap on the wrist compared with incarceration. Concern about public safety also was present, with some questioning whether the sanction allowed dangerous offenders to roam the streets. This criticism was generally countered at the time with suggestions that the sanction was applied only to less serious offenders.

Despite these concerns, the use of electronic monitoring began to soar in the 1990s as part of what can be coined the “innovation” stage. In this stage, technology began to shift and the ability of criminal justice officials to monitor offenders even more closely increased. Original technologies relied on radio frequencies. Typically, offenders would have a device hooked up to their phones and would be expected to connect another device to the telephone when their probation officer called. Through innovation, the technology began to rely on more precise monitoring of offenders. By 1999, 75,230 offenders were being monitored electronically (Beato, 2012).

Expansion of electronic monitoring occurred in the 2000s. Global positioning system (GPS) monitoring became even more precise. Passive GPS monitoring allowed officers to track offenders’ whereabouts and review the data highlighting their movements on a daily basis, whereas active GPS monitoring technology allowed officials to track offenders’ whereabouts in real time. Technology was developed so that officials could create exclusion zones that would alert officers if the offender were somewhere he or she was not supposed

to be. By 2009, 200,241 offenders were being monitored electronically in the United States (Beato, 2012).

During this timeframe, the stage of acceptance occurred. Acceptance refers to the time period when public officials embraced electronic monitoring as a viable criminal justice tool. Indeed, by the end of the 2000s, electronic monitoring was being used for violent offenders, including sex offenders, gang members, and domestic violence offenders (Button, DeMichele, and Payne, 2008). Just two decades before, there was concern that the sanction was too lenient and a potential threat to community safety.

The current stage of electronic monitoring can be characterized as reliance. Across the United States, nearly 200,000 electronic monitoring units are in use each year (DeMichele and Payne, 2009). States facing fiscal crises and prison overcrowding have turned to GPS electronic monitoring as a tool to help address those fiscal concerns. In California, a law known as Jessica's Law or Proposition 86 was passed. This law required sex offenders paroled from prison to be placed on GPS monitoring for life. The law was justified by supporters on the grounds that the sanction reduced costs that would be incurred from a life sentence in prison. Indeed, states across the country have come to rely on electronic monitoring as a prominent tool in their alternative sanction toolkit.

### **Defining Electronic Monitoring**

Scholars and practitioners define electronic monitoring in various ways, including as (a) a sentence, (b) a part of the justice process, (c) a tool, and (d) an experience. The way that electronic monitoring is contextualized has important implications for the way that the sanction is used and evaluated. Andersen and Andersen (2014) tend to define electronic monitoring as a sentence. This definition seems to follow the way that the sanction is applied in Denmark. A question that comes up is whether electronic monitoring is a substitute for prison or is an enhancement to probation and parole. The answer to the question will determine the types of offenders assigned to the sentence.

Electronic monitoring also can be perceived as a part of the criminal justice process. Indeed, electronic monitoring is applied at various stages of the process, including as (a) part of pretrial release, (b) a condition of probation, (c) a part of a jail sanction, and (d) a condition of parole. Here again, the way that electronic monitoring is contextualized has important implications for the types of offenders it is used for, how widespread the use is, and how it is applied.

One of my colleagues and I (see DeMichele and Payne, 2010; Payne and DeMichele, 2011) prefer to define electronic monitoring as a tool rather than as a sentence. In the United States, it is rare that judges "sentence" an offender to electronic monitoring. Of course, electronic monitoring might be assigned as a condition of probation or parole, but the actual sentence is either probation or incarceration. This distinction is important for several reasons. First, identifying electronic monitoring as a tool draws attention to the fact that criminal justice officials use many different tools in their efforts to supervise offenders.

Drug tests, motivational interviews, and home visits are a few other tools. Note that one would not say that drug offenders have been sentenced to drug tests. Instead, one would say that they have been sentenced to probation and, as a condition of their probation, they must submit to drug tests.

Second, when evaluating the success of these tools, it might become particularly difficult to isolate the actual impact of the electronic monitoring tool. Andersen and Andersen (2014) allude to this in their first endnote. It is important that we not oversell the importance of this tool. Is the change among the younger offenders a result of the electronic monitoring technology or the actual practices that officials followed in implementing the technology? Did other community-based tools (such as treatment strategies) play a role in reducing dependency? It is difficult to isolate the actual source of the positive change, but the questions must at least be considered.

In addition to defining electronic monitoring as a tool, it is useful to conceive of electronic monitoring as an experience. This suggests that we should focus on how the electronic monitoring tool is used, when it should be used, and how it is experienced by offenders. It is important to recognize that by itself, the electronic monitoring tool is just a piece of equipment and technology. The tool will only be as useful as the officials are who put it to use. To be sure, the tool has implications for officers' workload (Blackwell, Payne, and Prevost, 2011; Payne and DeMichele, 2011), which means that officials will need to be trained on how to use the technology, when to use it, when to avoid it, and how to avoid any associated pitfalls. When used effectively, offenders should, at least theoretically, experience electronic monitoring in a way that prevents offending and assists with reintegration back into the community.

Imagine a tool on the shelf of a hardware store. That tool does not serve a purpose until someone uses the tool. The tool might be used appropriately and serve its purpose well. However, my personal experience has been that my new tools are frequently misused. I have spent more hours than I would like to admit trying to wind string into my weed whacker! It is not the tool that makes it effective. It's the user and the context of the use. In other words, electronic monitoring does not prevent future crime; probation and parole officers who use electronic monitoring to supervise offenders prevent future crimes. And, how the offenders actually experience the tool will help to determine whether the tool has been effective.

### **Experience of Electronic Monitoring**

Because the sentence is served in the community, it is sometimes wrongfully assumed that house arrest with electronic monitoring is not punitive. Research I conducted with Randy Gainey (see Gainey and Payne, 2000; Payne and Gainey, 1998, 2000, 2004) suggested that offenders experienced electronic monitoring in similar ways to how offenders might experience prison. We found that the "pains of imprisonment" discussed by Gresham M. Sykes (1958) were relevant to the monitoring experience. In particular, monitored offenders

reported that they were deprived of goods and services; lost certain rights as a result of the monitoring; lost their autonomy; and were, at least in some ways, deprived of their traditional heterosexual relationships.

In addition to the traditional pains of imprisonment, we found that offenders experienced a few “pains of electronic monitoring.” These pains included monetary effects, family effects, watching others effects, and shame. With regard to *monetary effects*, most offenders we interviewed complained about the costs of electronic monitoring. While incarcerated, offenders do not have to pay to be punished. Under electronic monitoring in the United States, in most states, offenders must pay the costs that come along with the implementation of the technology. These costs can be high, particularly for families who live near or below the poverty line.

As Andersen and Andersen (2014) note, “society could save money when choosing electronic monitoring instead of prison.” This statement is certainly correct. Instances have been reported, however, when electronic monitoring was used for older offenders as a way to get around having to cover the costs of their health care while they were incarcerated. Such a decision produces moral and ethical questions. If two offenders committed the same crime and one is given a different sentence because of their health status so the agency can avoid covering health care costs, is this prejudicial to one offender over the other? Another caveat related to cost is that cost savings are tied to how the tool is defined: If the tool is replacing a prison sentence, then cost savings occur, but if it is enhancing probation or parole sanctions, then the costs of punishing offenders increase. One study (Gies et al., 2012) found that GPS electronic monitoring of paroled sex offenders cost approximately \$8.50 each day more than traditional parole strategies, but these costs were justified given the success of the tool in preventing offending.

*Family effects* refer to problems that arise in relationships when offenders are electronically monitored. The electronic monitoring experience can place a great strain on family members. I recall one offender I interviewed who complained that his brother would not pay the phone or electric bills because the brother knew how important those services were to the offender (see Payne and Gainey, 1998). In other situations, spouses or cohabitating partners are essentially serving the sentence along with the offender. They never have the home or apartment to themselves, they have to deal with the offenders’ frustrations over their own lack of autonomy, and they have to share in the costs of the tool.

*Watching others effects* refer to our finding that offenders found it somewhat “painful” that they could only watch others participate in entertainment activities. One offender we interviewed likened it to going to the beach and not being able to touch the sand. In prison, all offenders are in the same situation. When on electronic monitoring, the offender has to watch as his or her family members, loved ones, and friends participate in activities without them.

*Shame* is another experience that has been found among monitored offenders. Although Andersen and Andersen (2014) suggest that stigma is less for inmates than for monitored

offenders, one must not discount the role that stigma will play for monitored offenders. For monitored offenders, the stigma occurs during the time they are experiencing their sanction and wearing the monitor. For prison inmates, their stigma occurs after their release from prison. In the United States, whether a prison sentence stigmatizes offenders in some communities is debatable as so many individuals from the offenders' neighborhood have been incarcerated themselves or know individuals who are incarcerated. For less serious offenders with little exposure to the criminal justice process, wearing a monitor on their body in public can produce shame (Martin, Hanrahan, and Bowers, 2009; Payne and Gainey, 1998). Even though devices might not be as clunky as they once were, they still can sometimes be viewed. Here is a description of how one type of GPS monitoring works: "Offenders wear an ankle bracelet that communicates with a larger device that they must carry. The device is about 5 inches wide, 2 inches thick and 5 inches tall and must be visible. *It is a distinctive piece of equipment that is noticed by others*" (U.S. Department of Justice, 2011, pp. 2-3, emphasis added).

### **Role of Theory in Electronic Monitoring Practices**

It is sometimes incorrectly assumed that practitioners and criminal justice officials are not interested in theory. In reviewing their support for various technologies, it is clear that some sanctions are supported by theories that the policy makers have about electronic monitoring. To understand fully how electronic monitoring is used, attention can be given to deterrence theory, rational choice theory, routine activities theory and situational crime prevention, and general systems theory.

Deterrence theory at its most basic level presumes that punishment will keep offenders from committing future offenses. Traced to Beccaria's (1819) suggestion that punishment must be certain, swift, and more severe than the pleasure one gets from committing a crime, deterrence ideals have been examined in a handful of electronic monitoring studies. These studies showed that, for the most part, electronic monitoring has a deterrent effect (Bales et al., 2010; Padgett, Bales, and Blomberg, 2006). The question that arises is why the sanction has a deterrent effect. After all, the sanction is not certain, applied swiftly, or necessarily more severe than the crime the offender committed.

Rational choice theory offers an answer to why the sanction deters monitored offenders. This theory suggests that offenders are rational individuals who will weigh the benefits and costs of offending (Clark and Cornish, 1985). Knowing that one's whereabouts are being monitored constantly might have a way of making would-be offenders act rationally.

Routine activities theory and situational crime prevention ideals also help us to understand how electronic monitoring as a tool "works." Routine activities theory suggests that crime occurs when three factors are present at the same time: a motivated offender, a vulnerable target, and the absence of a capable guardian (Cohen and Felson, 1979). The electronic monitoring tool provides a capable guardian, which should reduce the motivations for offending, and the restrictions to where offenders can go should reduce the



presence of vulnerable targets. Situational crime prevention ideals are based on the premise that crime can be prevented when prevention strategies are tailored to specific types of crime. The electronic monitoring tool fits these ideals for many types of offenses. When applied in domestic violence cases, for example, the technology can alert officers when the offender violates protective orders. A few years ago, an assistant district attorney approached me about using the technology for car burglars in response to a rash of car break-ins. The idea was that exclusion zones could be created around parking garages frequently targeted for burglaries and offenders believed to be responsible for the bulk of the offenses could be monitored.

Sometimes, the electronic monitoring tool is applied in ways that do not fit nicely within the ideals of situational crime prevention. Consider how the tool is used for sex offenders. Restriction zones are placed around schools, day cares, playgrounds, and other places regularly frequented by children. The issue that arises is that most sex offenses do not occur in these locations: They occur in the offender's home. In this case, the monitor would indicate only whether the offender is home; it would not necessarily keep the offender from offending. Having said that, research has shown that GPS monitoring does deter sex offenders from future sex offending (Gies et al., 2012).

General systems theory also is useful in understanding the application of the electronic monitoring tool. The principles of the theory that relate specifically to electronic monitoring are outlined as follows:

- General systems theory suggests that societal systems are interrelated. With regard to electronic monitoring, changes in our technological system produced the opportunity for a new type of tool to be used to control offenders. This new tool, in turn, impacted our criminal justice system. For offenders exposed to the tool, electronic monitoring impacts their social systems.
- General systems theory suggests that if certain subsystems fail, then the entire system is flawed. By itself, electronic monitoring cannot produce positive changes. The rest of the justice process must be operating effectively for the tool to work.
- General systems theory points out that systems are ascribed a purpose. When implementing electronic monitoring tools, officials must assign a purpose to the tool. Is the tool designed as a strategy to reduce prison overcrowding or as a way to enhance probation supervision?
- The principle of equifinality in general systems theory suggests that a task can be accomplished in more than one way. Electronic monitoring is not a panacea. It is just one tool that can be used.
- The principle of entropy in general systems theory refers to disorder and reminds us about the need to review systems or else they will be prone to fail. This principle relates to electronic monitoring on two levels. First, the electronic monitoring equipment must be reviewed frequently or else the equipment will fail. Second, the electronic

monitoring strategies, in general, must be empirically evaluated to identify patterns of success and areas of concern.

### **Future of Electronic Monitoring Research**

Andersen and Andersen (2014) note that “the speed to which reforms [alternatives to incarceration] sweep across the country seems to exceed the speed of quantitative evaluations of such reforms.” Certainly, a lack of systematic research on the various types of electronic monitoring is problematic. Several years ago, Renzema and Mayo-Wilson (2005) pled for experimental designs to test whether electronic monitoring truly deterred criminal behavior and improved an offender’s behavior. We have not seen such studies, but they are truly needed.

Future research on electronic monitoring also should be theory driven. Andersen and Andersen (2014) are to be applauded for weaving in an innovative theoretical approach to assess the deterrent value of electronic monitoring. Similar studies should be done with U.S. samples.

In addition, we still know little about how various groups experience electronic monitoring. How do women experience electronic monitoring? We know that incarceration negatively impacts disadvantaged communities (Rose and Clear, 1998), but does electronic monitoring have any impact on these same communities? What effect does electronic monitoring have on family members exposed to monitored offenders? Given that minorities have negative views toward electronic monitoring (Payne, DeMichele, and Okafo, 2009), what is the source of these perceptions? Can future researchers expand cost/benefit analyses to account for the reduced welfare dependency that is tied to the sanction for younger offenders? How can the electronic monitoring technology be improved? There is no shortage of research questions for electronic monitoring scholars and practitioners!

### **Entrepreneurial Thinking and Electronic Monitoring**

In my efforts to teach critical thinking to my students, I sometimes focus too much on how to be critical and not enough on how to find solutions to societal problems. With regard to electronic monitoring, it is obvious that entrepreneurial thinking has produced the technology and tools needed to make the strategy viable. As scholars, we might resist the temptation to be entrepreneurs, but it is imperative that someone engages in entrepreneurial thinking to develop alternative sanctions. A few decades ago, most of us would have never imagined that we would have the type of technology we now have at our fingertips. This technology has revolutionized criminal justice practices. Probation officers can drive by an offender’s home and use a wand to determine whether drugs or alcohol are on the premises. Drunk drivers have ignition locks placed on their ignition and must prove they are sober before their car will start. Some jurisdictions have kiosks where up to 1,000 supervised offenders report as part of their release conditions (DeMichele and Payne, 2009). While promoting entrepreneurial thinking and innovation in alternative-based sanctions, it is

important to focus on social entrepreneurship and strategies to ensure that the alternative sanctions are serving the good of the community.

What does the future hold for technology and alternative sanctions? How can technology be used to improve criminal justice outcomes? Judge Love and Mike Goss took the lead and were key figures in the early development of electronic monitoring. Perhaps we should be encouraging our students to start thinking as entrepreneurs when discussing the future of alternative sanctions. A few weeks ago, my children were delighted to have their pictures taken at an amusement park with their hands and head in a stock from the colonial days. Probably a few centuries from now, some criminologists' children are going to laugh at having their picture taken wearing an electronic monitor! Technology we consider "cool" today probably will not be so cool in the future.

### **Learning from Denmark's Experience**

Andersen and Andersen (2014) should be commended for this addition to the literature. Although we have much to learn from their study, three things in particular stand out:

- Size matters.
- One size does not fit all.
- Success can be measured many different ways.

With regard to "size matters," the short sentences used in Denmark demonstrate that we do not have to go overboard in the way we punish offenders. In the United States, we seem to be infatuated with "super sizing." In general, we eat too much, drink too much, smoke too much, have larger homes than individuals from most other countries, and sentence offenders to far longer sentences than might be necessary. The incarceration binge has had untold negative effect on inmates, their families, and our communities.

Andersen and Andersen (2014) find that electronic monitoring produced more positive outcomes in younger offenders than in older offenders. This finding demonstrates that "one size does not fit all" when considering sanctions for offenders. However, when considering the ideals for situational crime prevention, we are reminded that specific situations (or individuals) might need specific strategies to prevent future crime. Future effort in the United States should focus on how we can reduce the "McDonaldization" of punishment (see Bohm, 2006).

As criminologists, we tend to define the success of crime interventions based on whether the offender recidivated. This measure is not perfect. Offenders might commit new crimes but not get caught. They might get arrested but not convicted. As Andersen and Andersen (2014) note, the purpose of sanctions might go beyond simply preventing crime. The difficulties we have in measuring recidivism can be overcome if we identify other measures that demonstrate successful intervention.

The task at hand is to explore how alternative sanctions fare in terms of the relationship between the sanction and various consequences including recidivism, unemployment, welfare dependency, and so on. Cross-cultural research will, as shown here, be helpful in informing how various countries form their specific practices. Some practices might have widespread utility across countries, whereas others will not. Borrowing once again from comedian Steven Wright, “Cross country skiing is great if you live in a small country.” Some sanctions are probably more relevant and more appropriate given particular cultural contexts, whereas those sanctions would not be relevant elsewhere. After all, it’s a small world, but I wouldn’t want to paint it.

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## Electronic Monitoring

### It Is a Tool, Not a Silver Bullet

**Matthew DeMichele**

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High incarceration rates have become an engrained aspect of the American social landscape. The oft-cited figures show that the U.S. incarceration rate has grown from approximately 125 per 100,000 throughout much of the 20th century to more than 700 per 100,000. The effects of mass incarceration (Garland, 2001) have been felt unevenly as the poor, uneducated, and young experience the brunt of this growth (Western, 2006). The negative consequences of incarceration do not end after release from prison as a criminal conviction hinders many from participating in the central components of prosocial life, including employment (Pager, Western, and Sugie, 2009) and voting (Uggen and Manza, 2006). Community supervision with electronic monitoring is believed to be an alternative sanction to alleviate the high financial costs and social consequences associated with mass incarceration.

Comparative research has the potential to identify alternative strategies to address the negative consequences associated with high incarceration rates. Andersen and Andersen (2014, this issue) provide an intriguing analysis pointing to the potential for electronic monitoring as a less harmful punishment that reduced the dependency on social welfare among younger offenders in Denmark. Electronic monitoring is used in several countries including the United Kingdom (Nellis, 2005), Belgium (Maes and Mine, 2013), Sweden (Marklund and Holmberg, 2009), and Argentina (DiTella and Schargrodsky, 2013). Electronic monitoring was adopted in the United States in the 1980s and has spread to every state in the country (Button, DeMichele, and Payne, 2009), but there has been little rigorous research evaluating the impacts of electronic monitoring (Renzema and Mayo-Wilson, 2005). The lack of U.S. research leaves questions about the efficacy of community supervision with electronic monitoring. Looking to comparative scholarship can help U.S. policy

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makers and researchers better understand the potential for electronic monitoring and other correctional options.

In this essay, I hope to contribute to the public policy debate about how electronic monitoring can be integrated within a punishment approach that is effective and humane. First, I highlight some important contextual differences between the Scandinavian and U.S. systems. Second, I discuss how Andersen and Andersen's (2014) findings contribute to the electronic monitoring debate. Third, I shift the discussion to emphasize that electronic monitoring is a technology, not a solution. Electronic monitoring is a tool, and its effects cannot be separated from the officers and agencies monitoring these devices. To understand how to use electronic monitoring most effectively, there needs to be a rigorous research framework that incorporates front- and back-end sanctions, including randomized experiments and high-quality quasi-experiments using natural experiments. Such a research agenda would not only vary electronic monitoring as the treatment but also investigate how these technologies interact with different supervision practices.

### **Comparative Research for Policy Transfer**

Comparative research can provide valuable insights about the differences in *how* justice and punishment are carried out among countries. All industrialized countries have professional services that arrest, prosecute, incarcerate, and supervise individuals in the community. But, there are important differences in how these functions are carried out that have profound impacts on the amount (DeMichele, 2013; Lacey, 2008; Nelkin, 2009) and type of punishment (Pratt and Eriksson, 2011; Whitman, 2003). In the Danish system, some people sentenced to prison are allowed to apply to serve their sentence in the community under electronic monitoring supervision. The general criteria for electronic monitoring are a permanent address and being employed, actively looking or training for employment, going to school, and serving a maximum sentence of 3 months.

For a U.S. audience, a 3-month maximum sentence is reserved for the least serious of offenders who are typically locked up in county jails and would have minimal impact on incarcerated populations or budgets. Andersen and Andersen (2014) point out that nearly two thirds of all sentences in Denmark are for less than 3 months. This statistic is in contrast to individuals sentenced to state prisons in the United States, where the average sentence lengths are more than 5 years (65 months; Bonczar, 2011). The United States is well known for giving offenders exceptionally long prison sentences, which speak to the differences in severity in punishment between countries. But, there are several differences in how punishment is carried out that are suggestive of an alternative conceptualization about criminal punishment between the U.S. and Scandinavian countries that have implications for incorporating electronic monitoring within community supervision (e.g., private vs. public vendors).

First, Scandinavian prisons operate under the philosophy of normalization in which the punishment is the removal of liberty; that is, incapacitation is the punishment (Pratt, 2008).



The incarceration experience should resemble normal life as closely as possible to prepare the individual for release. In the United States, being incarcerated is only one aspect of the punishment; the rough living conditions and treatment of the inmate are another. One legal historian, James Q. Whitman (2003), described the emergence of American correctional exceptionalism as our tolerance for increasingly degrading and inhumane treatment compared with similar societies. Tough U.S. prison conditions likely have effects on postrelease outcomes that typically go unobserved in electronic monitoring studies of parolees (e.g., releasing inmates from administrative segregation or solitary confinement).

Second, Scandinavian prisons have inmate-to-officer ratios that are close to 1:1 (Pratt, 2008), whereas comparable U.S. figures range between 5 and 10 for state and federal facilities, respectively (Stephan, 2008). Third, Pratt (2008) wrote about Scandinavian exceptionalism as a result of the reserved use of severe punishment and shows that between 20% and 30% of all inmates serve their time in open prisons. These institutions allow inmates to work or attend school/training, purchase groceries, cook meals, own a car, and participate in other aspects of normal life. Numerous differences exist between U.S. and Scandinavian criminal justice systems: Recruitment, training, and health care are provided in the community (not in the prisons); inmates have input in prison policies; there is limited violence; and inmates are given individual cells (Christie, 2000; Pratt, 2008). Essentially, then, many Scandinavian inmates are working toward reentry after their admission to prison, whereas in the United States, inmate reentry is just beginning to gain serious traction.

Comparative researchers can identify evidence-based practices as long as contextual factors are considered during any policy transfers (DeMichele, forthcoming). Identifying policy implications from Andersen and Andersen's (2014) study requires considering them within an agency's overall supervision framework to fit with agency missions, goals, resources, and staffing capacities.

### **Electronic Monitoring: Current State of Research**

Electronic supervision was initially intended as a low-cost alternative to incarceration for relatively minor offenders to assist with rehabilitation and social reintegration (Gable, 1986). The spread of these systems through the 1990s brought criticisms of net widening, and research demonstrating their effectiveness was lacking. There is still surprisingly little research investigating the potential for electronic monitoring, with early research painting an equivocal picture and recent research being more optimistic. Some researchers found no difference between the use of jail and electronic monitoring for drunk drivers (Courtwright, Berg, and Mutchnick, 1997), others found mixed support for lower risk individuals (Gainey, Payne, and O'Toole, 2000), and yet others found significant differences between high-risk parolees in 1-year recidivism rates that disappeared by 3 years (Finn and Muirhead-Steves, 2002).

More recently, two notable National Institute of Justice studies showed positive results for electronic supervision. In Florida, Padgett, Bales, and Blomberg (2006) found that

offenders monitored with either radio-frequency or global positioning systems (GPS) had significantly lower rates of revocations for technical violations or new crimes as well as lower absconding rates. Bales et al. (2010) conducted a follow-up study in which they found that electronic supervision offenders had a 31% lower failure rate than comparable offenders not on electronic supervision. And, those monitored with GPS had a 6% lower failure rate than those on radio-frequency monitoring. Gies et al. (2012, 2013) found significant differences in arrests, reconvictions, and returns to prison among sex offenders in California, and similarly positive findings were found with a sample of released gang members. Both Bales et al. (2010) and Gies et al. (2012, 2013) took a step in the right direction by including matched comparison groups using propensity matching methods.

Each study focused on a measure of recidivism as the outcome. Andersen and Andersen (2014) do not test the efficacy of electronic supervision on criminal justice outcomes; instead, they find improvements in removal from unemployment benefits. This finding is, essentially, a proxy measure for unemployment, and it seems that younger people had lower welfare dependency rates when placed on electronic supervision, with no differences found among older people. Unemployment is tightly linked with crime outcomes, and electronic monitoring might reduce offending indirectly by improving alternative outcomes. This outcome is in line with research using Canadian samples in which electronic supervision did not have direct effects on recidivism, but effects were found on treatment completion and those that completed treatment had improved outcomes (Bonta, Wallace-Capretta, and Rooney, 2000).

### **Electronic Monitoring: How Can It Enhance Cognitive Transformation?**

Something that needs to be stated clearly is that GPS, radio-frequency devices, and other forms of electronic monitoring are only tools that officers can use. So, asking questions such as “does electronic monitoring work?” are illogical. This would be similar to asking whether computers, cars, or other tools that officers use work. These tools are all dependent on humans and only work as well as the infrastructures supporting them and the people operating them. This, of course, is not to say that electronic monitoring cannot improve supervision, just that researchers and policy makers need to step away from treating these tools as programs or strategies. They are an additional tactic that can be helpful to provide officers with a sense of where offenders were at certain times (GPS) and whether they were at home when they were supposed to be (radio frequency). These devices do not make officers’ jobs easier. Instead, they increase the workload and costs associated with supervision (DeMichele and Payne, 2009; Geis et al., 2013).

When thinking of electronic monitoring as a tool, we can understand that these tools have the potential for both positive and negative effects. They have the potential to break, fail to report correctly, and increase officer stress and workload. Electronic monitoring tools do not have intrinsic supervisory powers; they provide some indication of a person’s location, but they tell us nothing about what people are doing. A prime example involves

a California case in which Phillip Garrido and his wife kidnapped and held a young girl captive for nearly 18 years. During part of this time, Mr. Garrido was on parole supervision with GPS tracking, but it went undetected that he had a kidnapped girl (and the two young children he fathered with her) in tents in the backyard. His GPS revealed that he was exactly where he was supposed to be—at his home and in his backyard. Parole officers failed to conduct regular in-depth searches of the home or even walk through to the backyard. This example shows how reifying these tools can allow officers to place too much faith in them as though they are a “silver bullet.”

Currently, there is a push for community supervision to alleviate the negative consequences associated with mass incarceration (i.e., overcrowding) by releasing inmates early and/or sentencing more people to probation instead of incarceration. This push, at first glance, is a good idea. But the problem is that probation and parole have grown from 500 to 1,500 and from 100 to 260 per 100,000, respectively, since 1980 (DeMichele, 2014). This growth has stretched officer workloads to unrealistic levels for them to engage effectively in evidence-based practices (e.g., cognitive behavioral interventions and motivational interviewing). Instead, officers spend 5–15 minutes each month with most individuals on supervised release, and electronic monitoring takes more officer time; it does not free up time to allow officers to interact directly with probationers or parolees. The Tennessee Board of Probation and Parole (2007) process evaluation showed that officers have to sift through millions of data points per offender annually to identify noncompliance. These data points can include numerous alerts that are difficult for officers to respond to. A recent tragic case in Colorado points to problems related to response protocols in which an open strap warning went ignored for 5 days because officers were inundated with alerts, and some went unnoticed. By time the alert was reacted to, Evan Ebel had shot and killed two men, including the Director of the Colorado Department of Corrections, and eventually he was killed in a shootout with Texas police.

The point here is not to draw on sensational cases to suggest that electronic supervision is ineffective. Rather, I suggest that treating electronic supervision as a program has the potential to result in various unanticipated negative consequences that will set up many agencies for failure. Electronic supervision is expensive and requires a lot of officer time, and jurisdictions that cannot dedicate ample resources in time and money should avoid incorporating these technologies. They are not a silver bullet or panacea. Instead, if electronic monitoring is going to be used, then policies and research should identify how this component is embedded within larger supervision goals and missions.

Policy-relevant research should be focused toward understanding the potential for supervision with electronic monitoring to improve long-term outcomes. A lot of rhetoric suggests that community supervision should foster prosocial behavioral change in the form of cognitive transformation. However, research has not investigated this potential. The research, to date, has suggested that offenders supervised with electronic monitoring have lower recidivism rates (and may have higher treatment completion and employment) while

they are on supervision. Such findings are important to understand the intermittency effects during a spell of electronic monitoring, but future research should consider the potential to promote cognitive transformations with community supervision. I am unaware of any research that measures changes in cognitive transformations for adults related to supervision with electronic monitoring.<sup>1</sup> Instead, recidivism is used as a proxy for cognitive transformation, but recent desistance literature has demonstrated that recidivism and cognitive transformation are not the same (Maruna, 2001; Paternoster and Bushway, 2009).

Future research should study electronic monitoring as one element of a community supervision plan to determine how it contributes to cognitive transformations. This research agenda is difficult. Experimental and quasi-experimental techniques could be used to vary the electronic monitoring component (and other supervision elements) to conduct regular cognitive testing of individuals. Simply, cognitive transformations take time and might move in a more zigzag, nonlinear fashion in which we need to make baseline and periodic measures of attitudes and self-perception to understand how the supervision process and electronic monitoring is contributing to transformation.<sup>2</sup> Currently, what little we know about the lived experience of supervision is negative; individuals dodge supervision officers, are harassed by law enforcement, and have little hope for their future (Goffman, 2009). The intentions here are not to sound the “nothing works” bell; I am suggesting that we cannot put all of our hope into one tool to determine whether it works. Future research should focus on how electronic monitoring contributes to overall cognitive transformations to shape prosocial life trajectories.

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1. For something related, see the Pathways to Desistance study ([pathwaysstudy.pitt.edu/](http://pathwaysstudy.pitt.edu/)).

2. I thank Kelle Barrick and Pamela Lattimore for contributing to these ideas on cognitive transformations during private conversations.

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# EDITORIAL INTRODUCTION

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## PAYING RESTITUTION

### Realizing the Promise of Restitution

**Maureen C. Outlaw**

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It has been more than 30 years since victim restitution re-emerged as a formal sanction in the criminal justice system. Responding to the call from the victim's rights movement of the 1970s, The Victim Witness Protection Act of 1982 authorized restitution "to any victim of the offense . . . in addition to or in lieu of any other penalty authorized by law." As was the intent of the act, states began to follow suit with their own restitution laws and policies in an attempt to give victims a voice and a place in the criminal justice process.

Research regarding the use and effectiveness of victim restitution has indicated that its utilization is both a blessing and a curse. First, restitution is not ordered in every case, even in jurisdictions with so-called "mandatory restitution" laws (Ruback, Ruth, and Shaffer, 2005). Rather, it is more likely to be ordered when the offender is female, White, and educated (Ruback, 2004) and when the damage from the crime is easily quantifiable (Outlaw and Ruback, 1999). Second, restitution often goes unpaid (Davis, Smith, and Hillenbrand, 1991), drastically reducing any benefit it could provide to the victim or the offender, and efforts to increase payment and enforcement often prove too costly to be of use (Weisburd, Einat, and Kowalski, 2008). Finally, because restitution, when ordered, is rarely the only financial sanction the offender must bear, offenders are often confused about how much they owe, how to pay, or what the money is for (Ruback, Hoskins, Cares, and Feldmeyer, 2006). When offenders do pay restitution, there is an obvious benefit for victims. Additionally, a few studies have indicated some rehabilitative effect and lower recidivism (Outlaw and Ruback, 1999; Ruback, 2004) among those who pay restitution, although the effect might be limited to specific populations.

Ruback, Gladfelter, and Lantz's (2014, this issue) study presents an experimental test of a strategy to increase payment of restitution and, therefore, to allow the sanction to provide the benefits to offenders and victims that justify its use. Taking its lead from previous attempts to affect restitution payment (Davis and Bannister, 1995; Lurigio and Davis, 1990; Weisburd et al., 2008) and research regarding attitude change (e.g., Glasman

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and Albarracin, 2006), Ruback et al. conducted an experiment with reminder letters that (a) provided information about how much they had paid, how much they owed, and how the payments get applied; (b) provided a rationale for why making a restitution payment was important in terms of accepting responsibility and reducing harm in an effort of rehabilitation; or (c) provided both information and the rationale. The fourth group in the experiment received no letters. Ruback et al.'s design, which allowed for a separate assessment of the provision of information from that of the rationale behind restitution, represents an important distinction that has not been explored in previous studies.

Ruback et al.'s (2014) results indicate that those provided with information regarding how much was owed, thanking them for payments made, and so on, were significantly more likely to make payments and to continue to make payments over time, even after the letters providing the information ceased. In contrast, letters providing the rationale behind restitution, with or without information, were more likely to be met with resistance and ultimately reduced both the amount and the number of payments. Ruback et al. conclude that although the initial spike in payments most likely reflected compliance with a request from the court, the continued payments by those in the information condition might represent internalization of the notion that they needed to "do the right thing." Finally, Ruback et al. conclude that providing the rationale for payment elicited a negative reactance and, ultimately, lack of payment.

Ruback et al.'s (2014) study provides a pragmatic approach to the (more) successful collection of restitution, to be sure. Beyond that, however, policy analysts point to the larger messages the findings suggest. The accompanying policy essays to this article by both Sarnoff (2014) and Mears (2014) call for an expansion in the way we think about criminal sanctions in general and restitution in particular.

Sarnoff (2014) reminds us that often restitution is not used as effectively as it could be, regardless of the enforcement of payment. Furthermore, she bemoans the lack of information sharing with victims, who often do not know they are eligible to receive restitution, just as the offenders do not truly understand what they owe and why. She argues that the point of restitution may be lost if it is ordered only when convenient or obvious rather than in all but the most indigent of cases. Ultimately, Sarnoff calls for nearly universal restitution in criminal sentences, indicating that the ability to pay can be taken into consideration with a sliding scale and that offenders' economic circumstances could change over time, allowing them to pay. She effectively makes the argument that the payment of restitution has far more benefit for the victim, the offender, and the system than the payment of other monies (such as fines and costs) and that restitution orders and payment should be given priority over other economically based sanctions.

Mears (2014), in contrast, highlights the shift in thinking about offenders that information provision implies. Pointing to numerous other examples (e.g., recycling and energy use) in which providing information and making the desired action "easy" has been successful, Mears calls on criminal justice scholars and practitioners to consider applying such



an approach to other sanctions in the criminal justice system. On a broad scale, Mears argues the benefits of moving away from fear-, threat-, and judgment-based sanctions and toward a model that would be aimed at helping probationers accomplish what they need to. Such an approach “humanizes” the probationers. Rather than viewing them as “others,” this approach recognizes them as human beings who make mistakes and need reminding. Ultimately, Mears suggests that changing probationers’ behavior, with regard to payment and perhaps beyond, is not dependent on individual characteristics of the offenders that need to change but on the system that processes them. The motivation to “do the right thing” exists, according to Mears, but the system has to make it clearer and easier for probationers to achieve what they need to. Ruback et al.’s (2014) findings are consistent with such a notion, finding increased payment with information but not with the “rationale” condition.

In sum, the contribution contained in this discussion based on Ruback et al.’s (2014) study is both specific and general. Ruback et al.’s findings from the experimental manipulation provide clear policy implications regarding the enforcement and collection of restitution orders. The key to obtaining payment from probationers could be as simple and as low cost as reminding them of their responsibility. Perhaps more importantly, however, it should inspire criminal justice scholars and practitioners to think about both the larger potential for restitution and the large-scale non-threat-based enforcement model for criminal sanctions. That is, if we take the specific lessons gained from this experiment (i.e., reminding probationers increases payment) and then expand the sanction use (order more restitution, provide more victims with information, etc.) as well as shift away from threat-based, antagonistic sanction enforcement, we could then move toward a more effective (reducing recidivism) and more efficient (actually collecting more restitution monies and helping victims with the restoration process) system of restorative sanctions. Only then can we claim to be realizing the full promise of restitution.

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# Paying Restitution

## Experimental Analysis of the Effects of Information and Rationale

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### Research Summary

*Most crime victims do not receive the restitution they are owed. This study is an experiment that addresses two reasons offenders give for why they do not pay their court-ordered restitution: (a) lack of understanding of how much they owe and where their payments are directed and (b) a belief that the sanctions are unfair. A total of 771 offenders were randomly assigned to one of four conditions in a  $2 \times 2$  between-subjects design in which, over a 6-month period, three quarters of the offenders received monthly letters that contained (a) information or no information about the economic sanctions they had paid and what they still owed (Information manipulation) and (b) a statement or no statement about reasons for paying restitution (Rationale manipulation). The remaining offenders did not receive a letter. The results 1 year after the experiment began (6 months after the last letter had been sent) indicated that offenders who had received letters containing information (Information condition) paid significantly more money and made significantly more monthly payments than did offenders in the other three experimental conditions. A cost-effectiveness analysis indicated that for every dollar spent on the experimental manipulation, approximately \$6.44 in restitution was received.*

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## Policy Implications

*Research on deterrence has suggested that threatened punishment can lead to compliance only if the threat is real. The problem with such enforcement measures, however, is that this type of deterrence is too expensive to be broadly imposed. By contrast, inducing offenders to act based on internal motivation rather than in response to external contingencies of reward and punishment can lead to long-term behavior change. This study suggests that informing offenders on a regular basis about how much restitution they have paid and how much they still owe can lead them to pay more restitution and to make more monthly payments. We believe these effects primarily are the result of a change in internal motivation because if it were fear of being monitored that led to the increased payments, then the absence of monitoring (i.e., no letters for 6 months) should have reduced that fear, meaning that payments should have been reduced. They were not. Although the question of the generalizability of the procedure remains open, the results of this experiment suggest that, at relatively little cost (because information about payments was gathered from individuals' court dockets publicly available on the Web), governments can increase restitution payments, benefiting victims, society, and perhaps the offenders themselves.*

## Keywords

*restitution, economic sanctions, experiment, behavior change, probation*

Economic sanctions on criminal offenders can serve three different purposes: punishment, deterrence, and the restoration of justice. Fines (monetary penalties for crime) as well as costs and fees (monies used to reimburse the government for the expenses of administering the criminal justice system) are aimed primarily at the relationship between the offender and society, especially by correcting the offender's behavior through punishment and deterrence (Ruback and Bergstrom, 2006). In contrast, restitution (court-ordered payments from an offender to a crime victim for the harm caused by the crime) is primarily aimed at restoring justice to the victim. Even those critical of fines, fees, and costs believe that restitution is a justifiable economic sanction (Beckett and Harris, 2011).

Restitution is imposed in only approximately two thirds of cases where it could be imposed (Ruback, Ruth, and Shaffer, 2005), a pattern that is a result of a combination of factors about the offender, the crime, and the county context (Ruback and Clark, 2011). Even when restitution is imposed, it is fully paid in less than half of those cases (Davis, Smith, and Hillenbrand, 1991). In general, little can be done to offenders who do not pay their court-ordered restitution because, under the U.S. Constitution, individuals cannot be imprisoned for debt unless there is a determination that their failure to pay is

willful (*Bearden v. Georgia*, 1983). Proving willful nonpayment often is difficult in that the information needed about assets and income is usually unavailable.

Because paying restitution helps restore justice to the victim, helps the offender recognize the harms caused by the crime and the need to repair the harm, and helps reintegrate the offender into society, increasing the payment of restitution to victims would accomplish several goals of the criminal justice system. This study is an experimental test of whether noncoercive messages can increase the amount of restitution paid and the number of restitution payments made. In the experiment, probationers who were delinquent in paying court-ordered restitution were randomly assigned to one of four experimental conditions, and their payment history was followed for 1 year after the beginning of the study.

### **Paying Restitution**

Studies in Pennsylvania have suggested that (a) the imposition of economic sanctions varies across counties, offenses, and offenders (Ruback, 2004; Ruback, Cares, and Hoskins, 2006; Ruback and Clark, 2009, 2011; Ruback, Shaffer, and Logue, 2004); (b) the imposition of restitution increased after a statute making it mandatory primarily because judges agreed with the aim of the statute (Hoskins, Ruback, and Cusick, 2010; Ruback, Ruth, and Shaffer, 2005; Ruback and Shaffer, 2005); and (c) only approximately half of victims receive their full restitution (Ruback, Cares, and Hoskins, 2008). Surveys of offenders indicated that they do not understand the system, do not know how much money they owe, and do not know what their payments are for (Ruback, Hoskins, Cares, and Feldmeyer, 2006).

It could be that offenders are more motivated to pay sanctions when they understand where their payments are sent. Moreover, understanding that their payments go to victims has implications for rehabilitation because restitution is likely to be an effective tool for rehabilitation only if offenders understand that they are taking responsibility and making reparations for their wrongdoing (Outlaw and Ruback, 1999). This rehabilitative effect seems to be stronger among married individuals, and it is somewhat stronger among employed and older individuals, which is consistent with Braithwaite's (1989) notion that marriage, employment, and age are indicators of integration.

Aside from not paying restitution because they do not know and understand the restitution they owe, offenders might not pay because they believe restitution is unfair; that is, the amounts are too high or the procedures used to determine the amounts are not legitimate. With regard to unfairness, research in social psychology has suggested that individuals care about both the outcomes they receive (distributive fairness) and the process by which those outcomes were reached (procedural fairness). Research in criminal justice has supported this notion. For example, probation officers typically have some flexibility in terms of payment schedules for fines and restitution (Alexander et al., 1998). Thus, offenders might be treated inequitably and could feel that their sanctions are unfair. Consistent with this argument, in their survey of individuals sentenced in two Pennsylvania counties in

2003, Ruback et al. (2006) found that offenders believed economic sanctions are unfair, although restitution was viewed as less unfair than fines, costs, and fees.

Studies concerned with understanding and improving the payment of restitution have focused on monitoring offenders. In a survey of 75 restitution offices across the country, Davis et al. (1991) found that jurisdictions in which restitution programs monitored offenders more closely had higher rates of restitution payment (amount of restitution paid within two years of award), although the extent of compliance estimated by the directors of the restitution programs was probably higher than the actual level of compliance.

Based on this notion that monitoring is important, three experimental studies investigated the enforcement of economic sanctions. In one, Lurigio and Davis (1990) sent letters to probationers chosen at random who were delinquent in paying restitution. These letters reminded them of how much they owed, informed them about how to make payments, and threatened them with serious sanctions if they did not comply. The researchers found that the notification procedure brought in money that was owed. Lurigio and Davis's (1990) study indicated that threats work when they can be regularly enforced through incarceration and other punitive measures; this finding suggests that the effect was a result of the threat rather than of the information provided.

In a second experimental study, conducted in Kings County (Brooklyn), New York, with 449 misdemeanor probationers who owed between \$12 and \$1,990 in restitution (median = \$263), Davis and Bannister (1995) manipulated three levels of enforcement:

- (1) Maximum enforcement, which included initial invoices, payment schedules, reminder letters sent out monthly, phone contact for those who did not pay, and after 3 months of nonpayment, a warning that they would be returned to court if they did not pay
- (2) Moderate enforcement, which included all of the actions of maximum enforcement except for the phone call
- (3) Minimum enforcement, which included only the initial invoice and, if payments were not made, a second demand letter containing a warning that they would be returned to court if they did not pay

The results after 9 months indicated that offenders had paid more of their restitution in the maximum enforcement condition (53%) and moderate enforcement condition (48%) than in the minimum enforcement condition (33%). However, by 1 year, all three groups had paid between 50% and 60% of the restitution owed, and no differences between groups were found. It is important to note that Davis and Bannister's (1995) experiment involved misdemeanants because it is likely to be much more difficult to elicit restitution payments from felons who likely owe higher amounts of money.

In the third study, Weisburd, Einat, and Kowalski (2008) randomly assigned low-risk probationers who were delinquent in payments to one of three conditions:

- (1) An intensive program involving four steps (being served with a violation, performing community service for every missed payment, intensive supervision coupled with job training and placement, and termination from the program and a recommendation for incarceration in jail)
- (2) A violation of probation condition in which offenders' probation was revoked for failure to pay their financial obligations, and
- (3) Regular probation.

More money was paid in the first two conditions than in the regular probation condition, an effect that Weisburd et al. (2008: 31) called "the miracle of the cells." The problem with the procedure, however, is its cost. On average, the amount of additional money that could be expected to be paid was \$100, less than the cost of processing such additional amounts.

In summary, the results of these studies suggest that threat works only as long as the threat is enforced. As Tyler (2006) argued, the downside of coercive methods, like those used in these studies, is that they are effective only as long as the probability of punishment is high. Moreover, punishing nonpayment through jail time is expensive.

### **Social Influence to Change Behavior**

Changing behavior through punishment and rehabilitation is one primary goal of the criminal justice system. Despite its importance, however, most of the research on effective ways of producing behavioral change in applied settings has been conducted in other disciplines, particularly health, education, and environmental studies.

Research in these other disciplines has suggested that written communications can be effective in changing attitudes, that messages giving only one side of an issue are more effective than messages giving both sides of the issue, and that messages should praise the sought-after behavior. With regard to written communications, several researchers have found print interventions to be effective (e.g., Marcus et al., 2007, 2008; Wenzel, 2006). Although interventions using a telephone can be more effective than print interventions (e.g., Napolitano et al., 2008), telephone interventions are not feasible with large populations. With regard to the nature of the communication, compared with two-sided messages, one-sided messages tend to lead to more confidence and stability and, therefore, to more attitude change (Glasman and Albarracin, 2006). With regard to praise, positive feedback reminds individuals both that they are doing well and that they have a behavioral commitment to maintain (DeLeon and Fuqua, 1995). Positive feedback is linked to positive changes in health behavior, diet, energy conservation, proenvironmental behaviors, and safe driving (Aitken, McMahon, Wearing, and Finlayson, 1994; Avis, Smith, and McKinlay, 1989; Becker, 1978; Campbell et al., 1993; Kreuter and Strecher, 1996; Schultz, 1999; Schultz, Nolan, Cialdini, Goldstein, and Griskevicius, 2007; Van Houten and Nau, 1983). In general, the inclusion of an injunctive norm (some form of feedback regarding what

respondents *should* be doing) makes it more likely that the resulting behavior will be more commensurate with socially acceptable norms (Mahler, Kulik, Butler, Gerrard, and Gibbons, 2008). Given these findings in other research areas, we decided to use written communications, one-sided messages, and positive feedback in our experiment.

We expected these attempts at attitude change to be effective for two reasons. First, we expected that they would increase perceived procedural justice. Procedural justice includes being treated with politeness, dignity, and respect (Tyler and Lind, 1992), as well as what Colquitt (2001) called “informational justice.” Informational justice refers to receiving complete and truthful explanations for decisions and procedures (Shapiro, Buttner, and Barry, 1994), and it incorporates explanations that are timely and that are tailored to the level of understanding of the specific person. A system that imposes economic sanctions that offenders do not understand is unlikely to be viewed as legitimate, and prior surveys of offenders found that they do not understand economic sanctions and think most of them are unfair (Ruback et al., 2006).

Second, we expected the attempts at attitude change to be effective because they would lead to change produced through intrinsic rather than extrinsic motivation (Pelletier and Sharp, 2008; Ryan and Deci, 2000; Sansone and Harackiewicz, 2000; Viets, Walker, and Miller, 2002). Intrinsic motivation is being driven to do something for internal reasons, such as inherent interest or enjoyment. Extrinsic motivation arises from external conditions such as social pressure and punishment, and generally any effect is short lived. One of the most effective ways of inducing intrinsic motivation is through the use of positive feedback (Deci, 1971; Harackiewicz, 1979; Ryan and Deci, 2000; Sansone and Harackiewicz, 2000).

### *Outcomes of Social Influence Attempts*

Research in other disciplines, particularly social psychology, has suggested that influence attempts to change behavior can produce one of four outcomes: compliance, internalization, no change, or reactance. First, people can comply because they seek to receive rewards and avoid punishments. This change process, termed *compliance* (Kelman, 1958), is effective only as long as the target is aware of being observed and aware that rewards or punishments will actually follow, although cues to being watched might be sufficient in some circumstances (Bateson, Nettle, and Roberts, 2006). In criminology, several studies have made it clear that the mere threat of punishment is not an effective method for changing behavior (Hollin, 2002; Meier and Johnson, 1977; Ray and Kilburn, 1970; Tyler, 2001, 2011), particularly if a violation of the threat is not followed by the punishment.

Second, the attempt can produce a behavior change because the target of the attempt truly believes the reasons for the behavior change are correct. This change process, termed *internalization* (Kelman, 1958), is the most long lasting because it is independent of the source of the communication. Aside from being convinced of the correctness of arguments given in favor of a behavior change, internalization also can occur through a *change in*



*self-image*, as people generate their own reasons to justify their changed behavior (Cialdini, 2001).

Third, attempts to produce a behavior change can sometimes backfire; people behave in a way opposite to the influence attempt. This process, termed *reactance* (Brehm, 1966), refers to an individual's negative emotional state when his or her freedom of choice is reduced. A common result of reactance is a reassertion of freedom by doing the opposite of what was demanded.

Finally, attempts at behavior change can result in no actual change in behavior. Changing behavior is difficult, and across disciplines, this result is the most common research finding. The typical conclusion from work on social influence attempts is that intervention alone is not enough to change behavior (e.g., Burger et al., 2010).

## Gender

We included gender as a factor in the study for three reasons. First, compared with women, men commit more crime and are more likely to commit serious personal crimes (Steffensmeier and Allan, 1996). There are fewer gender differences for less serious crimes, like property offenses, and for crimes, like fraud and embezzlement, for which women are in positions that give them criminal opportunities. According to Steffensmeier and Allan (1996), women constitute approximately 90% of lower level bookkeepers and bank tellers. Second, men and women tend to have different roles in the commission of crime. Women tend to be more likely than men to commit crimes by themselves or in small, nonpermanent groups (Steffensmeier and Allan, 1996). When women do commit crimes with men, they tend to be accomplices helping the men (Decker, Wright, Redfern, and Smith, 2003). The exception is for crimes of access, like embezzlement. Third, because of gender differences in moral development (Gilligan, 1982), females might be more amenable to treatment and more likely to believe the criminal justice system is fair. Thus, we expected that women would be more likely to pay their restitution and more likely to be responsive to the manipulations, particularly the manipulation of rationale for paying restitution.

## Hypotheses

For the experiment, we had three hypotheses. First, we expected that probationers who get information about the restitution imposed on them (how much they have paid and how much they owe) would be more likely to pay the restitution they owe than would probationers who do not receive this information. Second, we expected that probationers who received a rationale for why they should pay restitution would be more likely to pay the restitution they owe than would probationers who do not receive a rationale. We expected that the two main effects, Information and Rationale, would be additive. Third, we expected that women would be more responsive than men to the letters and would be particularly responsive to the rationale manipulation.

Before beginning the experiment, we posted the experimental design with the *Criminological Protocol for Operating Randomized Trials* at Cambridge University in England. This document included information about how the experiment would be conducted, as well as the specific hypotheses that were being tested and the types of analyses that would be conducted.

## Method

The study was a true experiment in which participants were randomly assigned to a condition, including a Control condition that reflects the status quo. The experiment, which had been approved by the Penn State Institutional Review Board, was conducted in Centre County, Pennsylvania, with the cooperation of the Director of the Probation and Parole Department of Centre County. Data were collected from offenders' case files and from court information available on the Web via the Unified Judicial System of Pennsylvania Web Portal ([ujportal.pacourts.us/docketsheets.aspx](http://ujportal.pacourts.us/docketsheets.aspx)). Six months after the experiment ended, participants were sent a survey, part of which contained checks on the manipulated conditions in the experiment. Those manipulation checks are reviewed in the Discussion section. Approximately 5 months after all survey responses had been returned, as required by the Institutional Review Board, we sent a letter to participants describing the experiment and the survey and stating that they could withdraw their data if they wanted. At this time, four individuals withdrew consent to participate; their data are excluded from all analyses.

## Sampling

The sample was collected from a list, provided by the Centre County (Pennsylvania) Probation Department, containing the entire population of offenders who had been placed on the delinquent case list at the probation department as of January 2012. The necessary requirements for placement on the delinquent list were (a) the offender owed outstanding economic sanctions on at least one docket and (b) the offender had not paid any economic sanctions for a period longer than 3 months. From the cases on this list, we selected only those delinquent cases on which restitution was owed, a sample representing the complete population of offenders owing money for restitution in Centre County. The initial list of eligible cases consisted of 932 individuals.

The sample contained both juvenile and adult docket numbers. Because of human subject protection restrictions, juvenile dockets on which the offender was *still* a juvenile at the time of data collection were deemed ineligible and were not included in our sample. Juvenile dockets in which the offenders were no longer minors but still owed money for restitution on these juvenile dockets were included in the sample. All adult dockets owing restitution were included.

Each individual within the sample had at least one court docket number indicating a criminal conviction for which he or she owed restitution. Several individuals, however, owed restitution on more than one docket. We collected data on a maximum of nine dockets.

Only two offenders had more than nine delinquent dockets. Both individuals, however, were clear outliers, with more than 20 dockets per person. These two offenders received letters pertaining to their most recent nine docket numbers.

When initial data collection began, several cases were identified as ineligible for one of the following reasons: (a) addresses were missing from the probation database and could not be successfully tracked down; (b) offenders were currently incarcerated for another offense and were being housed in prison, jail, or other institutions from which they would be unable to pay; (c) offenders had paid all restitution owed between when we received the initial list of delinquent payments and when data collection began; or (d) individuals were no longer in the probation database (i.e., the docket had been expunged). After excluding cases for these reasons ( $n = 157$ ), we had a total of 775 cases for random assignment to experimental and control groups. Additionally, as mentioned, four individuals from three of the four experimental conditions withdrew consent to use their data at the conclusion of the study, reducing the analytical sample size to 771 cases.<sup>1</sup> Most of the cases (74%) dated from 2007 or later, and almost all (95%) were from 2003 or later. The year of sentence was marginally related to total restitution owed ( $r(694) = -.07, p = .052$ ), meaning that older cases were somewhat more likely to involve greater amounts of restitution. However, because this correlation was small and because the year of sentence was never significant when used as a covariate in analyses of variance of restitution payment by month and of restitution paid overall, we do not discuss the year of sentence further.

### *Experimental Design*

The experiment used a  $2 \times 2 \times 2$  (Information  $\times$  Rationale  $\times$  Gender) between-subjects factorial design. Factorial designs permit the examination of different treatments and the testing of interactions between factors (Shadish, Cook, and Campbell, 2002). Moreover, factorial designs are an efficient use of research participants. The participants were first blocked on gender and then randomized into four different treatment conditions, including a control group. The experimental treatment consisted of two between-subjects manipulations: Information, which refers to information about economic sanctions, and Rationale, which refers to the type of explanation research participants were given for paying restitution.

**Information manipulation.** With regard to Information, half of the research participants were given information monthly for 6 months about what they owed originally (in total and by type of economic sanction), how much money they still owed (in total and by type of sanction), how much their monthly payments had been, and the category or categories to which these payments had been applied. The other half of the participants did not receive any information about what they owed and what they had paid. Information is important because at the time of sentencing, and subsequently, offenders report generally that they

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1. Analyses with and without these four cases were nearly identical.

do not know how much money they owe. At sentencing, judges typically order that all applicable economic sanctions be imposed rather than listing all of the individual sanctions that were imposed.

The manipulation of information represents an aspect of procedural justice. Information alone might not be sufficient to cause people to change their behavior (e.g., knowing the number of calories in food at restaurant chains does not reduce the number of calories that patrons eat; Elbel, Kersh, Brescoll, and Dixon, 2009). Nevertheless, information should lead to a better understanding of the penalties, which is an important aspect of perceived legitimacy. The Information manipulation was delivered through letters, one per month for 6 months. An example of the Information treatment is included in the Appendix.

**Rationale manipulation.** The second between-subjects variable, Rationale, refers to the type of explanation research participants were given. Half of the research participants received no rationale for why they should make their payments. The other half of the participants received a letter monthly for 6 months describing why the economic sanctions are legitimate and why they should pay their restitution. The rationale emphasized the acknowledgment of harm, the taking of responsibility, and the increased likelihood of success when payments are made. The wording of the rationale was written in consultation with the Centre County probation staff to assure it was written at an understandable level. An example of the Rationale treatment is included in the Appendix.

The third experimental condition received a letter combining both (a) the information about amounts owed, paid, and remaining and (b) the rationale for payment of restitution. Taken together, the manipulation of Information and the manipulation of Rationale meant that research participants were in one of four experimental conditions: Information Only, Rationale Only, Information and Rationale, and Control (no letter).

**Blocking.** Blocking on gender allowed us to create groups that had equal proportions of women to men within each of the four treatment conditions. We blocked by gender because we wanted to test for differences by gender in response to the treatment. Blocking ensures that there will be as much statistical power as possible to test main effects and interactions.

**Assignment.** Participants were randomly assigned to one of the four conditions. Random assignment was completed by randomly generating a number (1–4) for each participant. After compiling the initial list of eligible cases, batch assignment was used to produce equal numbers in the four treatment conditions. This initial random assignment created the initial Wave 1 conditions.

Because the list of initial cases created a sample size that was slightly lower than expected, we collected additional cases as they were added to the delinquent list for failing to make restitution payments. Each of these cases was then randomly assigned to treatment groups. For logistical reasons and to avoid placing too many demands on the Probation Department, we decided against using a trickle-flow randomization process, and instead we waited until the end of each month to collect new, smaller batches. The monthly batches were then blocked and randomly assigned.

**Measurement.** Each month, coders looked up offender docket information on the Pennsylvania Unified Judicial System website, and any payments made since the last treatment were recorded according to which category of sanction (e.g. restitution, fees, and fines) the money was applied. A dummy variable was then created to indicate whether any payment was made (0 for none, 1 for any payment), and this information was used to determine whether the feedback in the next letter would be positive or neutral.

The first treatment letters were mailed on April 19, 2012. The letters acknowledged that it was possible that payments were received between the time of coding and when the letters were sent. Specifically, the letter told recipients that if they had made a payment in the previous two weeks, it might not have shown up in the database and thus the letter amounts might have been slightly inaccurate.

Each treatment group received five subsequent letters after the initial treatment. In total, each offender assigned to a treatment group received six letters. Payments were coded on each docket on the 12th to 15th of each month. Letters were printed and prepared for mailing on the 16th through 18th and mailed on the 19th of each month, or the Monday immediately following. The experiment ended in December 2012, although we continued to follow offenders' payments into 2013.

### ***Data Collection***

The data used in this experiment were collected from multiple sources. Much of the data were collected in person at the Centre County Probation Department using two separate case management systems: a state-level system (the Common Pleas Case Management System) and a county-level case management database used by the probation department. The initial case information, including offender demographics (i.e., gender, race/ethnicity, and age), addresses, and other pertinent but nonpublic information was collected using these sources.

Additional information was collected from the Pennsylvania Unified Judicial System website (<http://ujportal.pacourts.us>) using the Common Pleas Courts Docket sheet lookup tool. This database is free and open to the public, allowing users to search individual offenders using docket numbers. The docket sheets have information on date of payments and amount paid toward each of the economic sanctions ordered for each offender.

First, the initial amount of each type of economic sanction assessed to offenders was coded. Then, adjustments made by the court to the amount of the sanction assessed were coded. These adjustments could be either positive (representing a penalty) or negative (representing removal of sanctions). Finally, between the dates of March 31 and April 13, 2012, the baseline amounts for each offender docket were coded. The baselines were coded immediately before the sending of the letters and allowed us to capture payments made before the experiment started. Baseline amounts were calculated as follows: Baseline Sanctions Amount = Total Assessed – (Total Adjustment) – Total Amount Paid. Two

outcome measures were the primary dependent measures in the study: the total amount of restitution paid and the number of monthly payments made.

### *Attrition*

Attrition occurred in each treatment group. Because we had no actual contact with offenders and they did not know they were in an experiment, we defined attrition as those cases for which we received letters back as undeliverable. After initially receiving returned letters, we attempted to update our address information using a forwarding address noted by the postal service, if available, or driver's license information made available to us by the probation department. Of the initial batch of treatment letters sent, 40 returned letters could be updated with new addresses. These cases were then switched to the next wave of the experiment (receiving treatments May–November). Because of the relative mobility of the population being studied, it was expected that some offenders might move during the experiment. By the final treatment period after 6 months, a total of 89 cases were lost across groups and could not be updated with new addresses.

The nature of the attrition in our sample produces attrition in each treatment group, while no attrition is possible in the control group as they were not receiving letters. To deal with differential attrition, the treatment groups were analyzed using an intent-to-treat approach. It is likely that there could be substantial differences between those dropping out of the sample and those who remain at the same address. Those who did not complete all treatments might represent a more itinerant population that is less likely to have social ties, or formal ties, as they did not update their change of address with the probation office.

## **Results**

This section begins with descriptive information about the sample, including information about the total amount of restitution owed. For each of the 771 individuals in the experiment, we have payment information by month for each of the first 6 months of the experiment, at 9 months after the experiment began, and at 12 months after the experiment began. Then, the analyses of the several dependent measures (the amount of restitution paid overall and by month, and the number of monthly payments) are presented.

### *Description of the Sample*

Table 1 presents information about the sample. Three fourths of the sample were male, more than 80% were White, and the average age at conviction was 34 years. Also shown in the table is the crime that was listed first in the probation files (although not necessarily the most serious conviction offense). At least 56% of the crimes are property offenses, and at least 11% were person crimes (simple assault, aggravated assault, robbery). Listed crimes that do not have obvious victims (i.e., DUI and drug offenses) could mean that there were other conviction offenses that did have victims to whom restitution was owed. In some

**T A B L E 1**
**Sample Description**

Variable	<i>n</i>	Overall Mean or Proportion
Gender		
Male	577	0.75
Female	194	0.25
Race		
White	628	0.81
Black	114	0.15
Other	24	0.03
Missing	5	
Age at Conviction		34.0
Number of Dockets for Which Restitution Was Owed		1.5
Crime Listed First in the Offender's Probation File		
Access device fraud	15	0.02
Aggravated assault	10	0.01
Bad checks	39	0.05
Burglary	34	0.04
Criminal mischief	32	0.04
DUI	44	0.06
Forgery	27	0.04
Harassment	17	0.02
Drugs	67	0.09
Receiving stolen property	49	0.06
Retail theft	62	0.08
Robbery	13	0.02
Simple assault	66	0.09
Theft by deception	50	0.06
Theft/unauthorized use of motor vehicle	18	0.02
Theft/unlawful taking	102	0.13
Other theft	15	0.02
Other crimes	111	0.14

Notes.  $N = 771$ . The percentages might not add to 100 because of rounding.

cases, DUI convictions and drug convictions prompted court-ordered restitution to the state, county, or insurance agencies.

**Brief description of outliers and sensitivity.** Outlier tests revealed that the top three amounts of restitution owed (\$527,084, \$639,032, and \$947,639) were outliers based on three of four tests. An analysis of studentized residuals, dfbeta measures, and Cook's *D* values indicated that these cases would be overly influential. Additionally, one case was an outlier on restitution paid (\$39,603) based on three of the four outlier tests. Analyses were conducted with all cases included, with the three outliers on restitution owed excluded, with the outlier on restitution paid excluded, and with all four outliers excluded. Analyses also were conducted using square root, logarithmic, and reciprocal transformations of the

data (because of the highly skewed nature of the data). In general, the results presented in this article seem to be fairly robust. Because tests for gender differences generally indicated no significant differences (either as main effects or in interaction with other variables), we dropped the gender variable from the subsequent analyses reported in this study. Next, we present the results excluding the four outliers on restitution owed and paid.

***Amount of restitution owed.*** The purpose of randomly assigning cases to experimental condition is to ensure extraneous factors that might be related to the dependent measures are not more likely to be in one condition than in another (Aronson, Ellsworth, Carlsmith, and Gonzales, 1990). If that goal is met, then it is more reasonable to conclude that the independent variable caused the observed differences between conditions. In this study, the primary factor that would affect the payment of restitution is how much restitution is owed. The mean amount owed was \$7,260; the median amount owed was \$891. The distribution was not normal (skewness = 15.28; kurtosis = 262.22). In Table 2, we present the mean amounts owed by experimental condition. Figures are displayed for analyses including and excluding the four outliers, although our discussion focuses on the exclusion of outliers. The third column in Table 2 presents the means for the four experimental conditions in total restitution owed before the experiment began excluding the three outliers on total restitution imposed and the one outlier on restitution paid. As would be expected, when the four outliers were excluded, the means became more similar.

***Amount of restitution paid.*** Regarding the payment of restitution, the amounts paid ranged from \$0 to \$39,603 (mean = \$388; median = \$25). The distribution was not normal (skewness = 14.22; kurtosis = 246.37).

A  $2 \times 2$  (Rationale  $\times$  Information) analysis of variance of the overall amount of restitution paid at 12 months for the sample excluding outliers (Table 2, column 5) revealed a significant Rationale effect ( $F[1, 763] = 4.44, p < .04$ ). Offenders paid significantly more restitution when they did not receive a rationale ( $M = \$449.75$ ) than when they did receive a rationale ( $M = \$236.91$ ). There was also a marginally significant Information effect ( $F[1, 763] = 3.11, p < .08$ ). More restitution was paid when information was given ( $M = \$432.34$ ) than when no information was given ( $M = \$254.32$ ). Finally, there was a significant Rationale  $\times$  Information interaction ( $F[1, 763] = 5.02, p < .03$ ). The amount of restitution paid was higher in the Information Only condition ( $M = \$651.84$ ) than in the Control condition ( $M = \$247.65$ ), the Rationale Only condition ( $M = \$261.00$ ), and the Information and Rationale condition ( $M = \$212.83$ ), which did not differ from each other.<sup>2</sup>

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2. When the entire sample was examined, including outliers, the results were slightly different. Only the Rationale  $\times$  Information effect was significant ( $F[1, 767] = 5.19, p < .03$ ) such that individuals who received Information Only and Rationale Only paid more than those who received the combined treatment or the controls.



T A B L E 2

Mean Amounts of Restitution Owed and Paid at 12 Months and Mean Number of Payments Made at 12 Months by Condition

Condition	Mean Restitution Amount Owed <sup>1</sup>	Mean Restitution Amount Owed <sup>2</sup>	Mean Restitution Amount Paid <sup>1</sup>	Mean Restitution Amount Paid <sup>2</sup>	Mean Number of Months Paid <sup>1</sup>	Mean Number of Months Paid <sup>2</sup>
Control (no letter)	\$4,110	\$4,110	\$248	\$248 <sup>a</sup>	1.41 <sup>a</sup>	1.41 <sup>a</sup>
Rationale Only	\$4,470	\$4,278	\$475	\$261 <sup>a</sup>	1.81 <sup>ab</sup>	1.81 <sup>ab</sup>
Information Only	\$14,764	\$6,645	\$647	\$652 <sup>b</sup>	2.25 <sup>b</sup>	2.25 <sup>b</sup>
Information and Rationale	\$6,474	\$3,092	\$220	\$213 <sup>a</sup>	1.73 <sup>ab</sup>	1.71 <sup>ab</sup>
One-way <i>F</i>	2.07	1.91	1.95	4.00 <sup>3</sup>	4.70 <sup>3</sup>	4.71 <sup>3</sup>

Note: Within a column with a significant *F*, means not sharing a significant superscript (<sup>a</sup> or <sup>b</sup>) are significantly different according to a post hoc Newman–Keuls test ( $p < .05$ ).

<sup>1</sup>Entire sample ( $N = 771$ ).

<sup>2</sup>Sample excluding four outliers ( $n = 767$ ), three because the restitution imposed was greater than \$500,000 and one because the offender made a single payment of more than \$39,000.

<sup>3</sup> $p < .01$

**TABLE 3**

**Mean Payments and Number of Payers Over Time for Sample Excluding Outliers**

Time	Mean Payment	Number of Payers
Time 1	\$54 <sup>b,c</sup>	202
Time 2	\$22 <sup>a</sup>	153
Time 3	\$25 <sup>a</sup>	124
Time 4	\$20 <sup>a</sup>	136
Time 5	\$24 <sup>a</sup>	156
Time 6	\$41 <sup>a,b</sup>	133
Time 9	\$90 <sup>d</sup>	231
Time 12	\$67 <sup>c</sup>	224

$F(7, 5341) = 5.14; p < .001$ .

Notes.  $N = 767$ . Means not sharing a common superscript(<sup>a</sup>, <sup>b</sup>, <sup>c</sup>, or <sup>d</sup>) are significantly different according to a post hoc Newman–Keuls test ( $p < .05$ ).

***Repeated Measures Analysis of Variance***

We collected payment information at the end of 1, 2, 3, 4, 5, 6, 9, and 12 months after the beginning of the experiment (i.e., after the first letter was sent out). We used these payment amounts in a repeated measures analysis of variance, with Rationale and Information as between-subject factors (see Table 3). This analysis revealed a significant effect for Rationale ( $F[1, 763] = 4.44, p < .04$ ), a marginally significant effect for Information ( $F[1, 763] = 3.11, p < .08$ ), and a significant Rationale  $\times$  Information interaction ( $F[1, 763] = 5.02, p < .03$ ). Significantly more restitution was paid each month when no rationale was given ( $M = \$56.22$ ) than when a rationale was given ( $M = \$29.61$ ), and marginally more restitution was paid when information was given ( $M = \$54.04$ ) than when no information was given ( $M = \$31.79$ ). For the interaction, more restitution was paid in the Information Only condition ( $M = \$81.48$ ) than in the Rationale Only condition ( $M = \$32.63$ ), the Control condition ( $M = \$30.96$ ), and the Information and Rationale condition ( $M = \$26.60$ ), which did not differ significantly from each other.

In terms of the within-subject analyses, there was a significant Payments (the repeated measure) effect ( $F[7, 5341] = 5.14, p < .001$ ), a significant Payments  $\times$  Information effect ( $F[7, 5341] = 2.75, p < .01$ ), and a significant Payments  $\times$  Rationale  $\times$  Information interaction ( $F[7, 5341] = 2.38, p < .03$ ).

For the Payments effect, there was a significant linear increase over time, following a drop after the first month (see Table 3).

For the significant two-way Payments  $\times$  Information interaction, payments over the 12 months tended to go down in the No Information condition but tended to go up in the Information condition (see Table 4). Similarly, for the significant three-way Payments  $\times$  Rationale  $\times$  Information effect, payments over the 12 months were basically constant and

**TABLE 4**
**Means for Information Manipulation by Payment Period Interaction**

Time	Experimental Condition	
	No Information	Information
Time 1	\$77 <sup>cd</sup>	\$31 <sup>abc</sup>
Time 2	\$22 <sup>ab</sup>	\$22 <sup>ab</sup>
Time 3	\$9 <sup>a</sup>	\$41 <sup>abcd</sup>
Time 4	\$19 <sup>ab</sup>	\$21 <sup>ab</sup>
Time 5	\$17 <sup>a</sup>	\$31 <sup>abc</sup>
Time 6	\$9 <sup>a</sup>	\$73 <sup>bcd</sup>
Time 9	\$57 <sup>abcd</sup>	\$123 <sup>e</sup>
Time 12	\$44 <sup>abcd</sup>	\$90 <sup>de</sup>

Notes.  $N = 767$ . Means not sharing a common superscript (<sup>a</sup>, <sup>b</sup>, <sup>c</sup>, <sup>d</sup>, or <sup>e</sup>) are significantly different according to a post hoc Newman–Keuls test ( $p < .05$ ).

not significantly different in the Rationale Only condition, the Control condition, and the Information and Rationale condition. In contrast, in the Information Only condition, payments increased over time (means not presented).

In sum, the results from the analyses suggest that the manipulation of information had a positive effect on the payment of the restitution owed. Probationers who received information monthly for 6 months about how much they still owed tended to pay more money over the 12 months than did probationers who did not receive such information. Providing a rationale for paying restitution seemed to be counterproductive, in that individuals who were given the rationale monthly paid less money than did probationers who did not receive a rationale for paying restitution.

### *Number of Months of Restitution Payments*

We also conducted analyses using the number of months in which probationers made any payment toward restitution. The results are similar to those for the amount of money paid. A one-way analysis of variance of the number of months in which restitution was paid yielded a significant effect (see Table 2). Probationers in the Information Only condition made significantly more payments across months than did probationers in the Control condition. Probationers in the Rationale Only condition and in the Information and Rationale condition did not differ significantly from each other or from the other two conditions.

A  $2 \times 2$  (Information  $\times$  Rationale) analysis of variance yielded significant effects for Information ( $F[1,763] = 4.74$ ,  $p < .03$ ) and for the interaction of Rationale  $\times$  Information ( $F[1,763] = 8.20$ ,  $p < .01$ ). With regard to the Information main effect, probationers who received information paid more months ( $M = 1.98$ ) than did probationers who did not

receive information ( $M = 1.62$ ). The interaction means are the same as for the one-way analysis of variance presented in Table 2.

### *Cost-Effectiveness*

At the follow-up data collection 6 months after the final letter was sent, we collected information about how much restitution and other sanctions had been paid during the course of the experiment for the three treatment (called Rationale, Information, and Information and Rationale) and the Control conditions. Amounts that members in a treatment condition paid above what was paid in the Control condition are assumed to be a result of the experimental treatment because participants had been assigned at random to the four conditions in the study. That is, without the application of the treatment (i.e., receipt of a notification letter), this excess amount would not have been paid. Because the Information Only condition had the strongest effect on amounts paid, the following discussion compares the Information condition with the Control condition.

Regarding the amount of sanctions paid, the Information condition seems to have been successful. Members of the Control condition paid a total sum of \$54,235 in restitution and \$70,616 for all sanctions by the 6-month follow-up (12 months after the initial letter was sent). By comparison, probationers receiving the Information Only treatment paid \$116,028 in restitution and \$150,357 for all sanctions over the same period. Thus, it seems that the information treatment induced the payment of roughly twice as much money as that paid in the Control condition.<sup>3</sup>

Comparatively little money was spent on the delivery of the experiment (see Table 5).<sup>4</sup> It took approximately 40 hours to set up the data file and to format the letters for easy importation of information from the database. Additionally, the total time spent collecting data and preparing the letters for mailing was approximately 240 hours over the course of 12 months. Furthermore, we spent a total of 50 hours tracking cases for which letters were marked undeliverable. In seeking missing addresses, we did no more or less than the probation office ordinarily does, either through recording new forwarding addresses provided by the post office or by searching probation records for an updated driver's license address. Based on an estimated labor rate of \$20 per hour, the total labor costs therefore were estimated at \$6,600 over 330 hours. Supplies were cheaper than labor. The greatest expense for supplies was postage. We used physical postage stamps because some research

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3. These figures represent the intent-to-treat effects of random assignment to treatment after excluding the outlier cases from analysis. Thus, the differences reported are conservative and represent only the expected effects of treatment as assigned, not the treatment-on-treated effects of actually receiving a letter.

4. It is important to note that the costs of treatment development and administration will inevitably vary as a function of wage rates, technologies available to assist in the process (e.g., automatic letter folders and labelers), worker speed, caseload size, and ease of accessing and importing court docket information into a database.

**T A B L E 5**
**Estimated Costs of Administration Versus Net Gains**

Item	Quantity	Restitution Only	All Sanctions
<b>Expenses</b>			
Labor			
\$20/hour, 30 records/hour	240 hours	\$4,800	\$4,800
Data file setup	40 hours	\$800	\$800
Track missing addresses	50 hours	\$1,000	\$1,000
Paper	6,000 sheets	\$60	\$60
Envelopes	6,000 pieces	\$240	\$240
Stamps	6,000 stamps	+ \$2,700	+ \$2,700
<b>Subtotal</b>	—	<b>\$9,600</b>	<b>\$9,600</b>
<b>Gains</b>			
Paid by information group, less	—	\$116,028	\$150,357
Paid by control group	—	– \$54,235	– \$79,741
<b>Subtotal</b>	—	<b>\$61,793</b>	<b>\$70,616</b>
<b>Net Gain of Information Treatment</b>			
Gains, less	—	\$61,793	\$70,616
Expenses	—	– \$9,600	– \$9,600
<b>Grand total</b>	—	<b>\$52,193</b>	<b>\$61,016</b>

*Notes.* Estimates based on 7,200 records (800 cases  $\times$  9 coding periods). Labor includes coding information from public docket sheets, folding letters, stuffing envelopes, and stamping envelopes, plus setup of the data file and tracking missing cases. Any automated parts of this process would decrease the cost of administration.

has suggested individuals are more responsive to messages in envelopes with stamps than with postage meters (Yammarino, Skinner, and Childers, 1991). Labor costs would have been substantially reduced if postage meters had been used. The overall costs of supplies and labor were estimated at \$9,600, including the costs of resending undeliverable supplies to updated addresses.

To calculate the cost effectiveness of the experimental manipulation, we computed the net gain associated with treatment, or the difference between the gains produced by the Information condition over the Control condition less the costs of delivering the experimental treatments. The net gain in restitution paid by probationers in the Information condition as a result of the experiment that would not have been paid in the absence of treatment was approximately \$52,000 over that of the Control group. The net gain in overall economic sanctions paid by the Information group was approximately \$61,000. Stated another way, probationers in the Information condition paid approximately \$6.44 in restitution for every dollar spent administering the experiment ( $\$61,793 / \$9,600$ ) and approximately \$7.36 in total sanctions for every dollar spent ( $\$70,616 / \$9,600$ ) that would not have been paid in the absence of treatment.

The \$6.44 figure is the most conservative estimate that we calculated. If the costs of labor were reduced, for example by having an unpaid undergraduate student or an intern at

the probation office complete coding and letter preparation, the net gain in this study would have been much higher. At a wage rate of \$8.50 per hour, for instance, the overall costs of supplies and labor would drop to \$5,805. Probationers in the Information condition would have paid approximately \$10.64 for every dollar spent administering the experiment in this scenario (\$61,793 / \$5,805).<sup>5</sup>

## Discussion

This study was a  $2 \times 2$  between-subjects (Information  $\times$  Rationale) experiment in which probationers who were delinquent in paying their restitution (a) received six letters containing information about what they owed and what they had paid or did not receive letters containing this information and (b) received six letters containing rationales for why they should pay restitution or did not receive letters containing this rationale. Measures of payment of restitution were taken before the experiment began, monthly for the first 6 months of the study, and then at 9 and 12 months after the study began. This experiment is part of a growing effort among governments, in both the United States (Thaler and Sunstein, 2008) and Britain (Benhold, 2013), to test whether small-scale interventions can prove to be cost-effective means to change behaviors in ways that benefit society (e.g., paying taxes, quitting smoking, and donating to charity).

The results of the study indicated that the Information manipulation had reliable effects on both dependent measures; probationers who had received six monthly letters about the amount of restitution they had paid and still owed were significantly more likely to pay more of their restitution and to make more monthly payments. In contrast, not only was the Rationale manipulation not effective, but there was evidence that probationers who received letters giving reasons for paying restitution were significantly less likely to pay their restitution.

From research in other disciplines, we had reason to expect that our experiment could produce one of four outcomes:

- (1) Compliance (changing behavior to receive rewards and avoid punishments)
- (2) Internalization (changing behavior because of a belief that the new behavior is correct)
- (3) Reactance (changing behavior in the opposite direction of the influence attempt)
- (4) No effect

In fact, we found some evidence for all four outcomes. The evidence for compliance appeared as the initial spike in payments after the first letter. After the first letter, 208 of the full sample of 771 made a restitution payment, ranging in size from \$.30 to \$39,603. Across all eight observation points, the payments made after receiving the first letter were

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5. Additionally, administration costs here are estimated for all cases in all groups. Restricting the cost-effectiveness analysis to only those cases in the Information condition would further reduce the costs of the study.

the highest ( $M = \$106$ ) primarily because of the single large payment of \$39,603, which paid off the amount this person owed. The fact that the first letter prompted this single large payment is consistent with the notion of compliance after the person became aware that he was being monitored. When this outlying payment was excluded from the sample, the pattern of a larger payment after the first letter was still present, although the effect was not as strong and the first payment was no longer the largest amount of money paid (see Table 3).

Evidence for internalization came from the probationers in the Information condition; these participants paid more restitution and made more monthly payments than did participants in the other three experimental conditions. That this effect was the result of internalization, rather than of compliance, is evident in the fact that participants in the Information condition paid significantly more at 6 and 9 months, and marginally more at 12 months, than did participants in the No Information condition. If the effect were merely compliance, then the effect should have occurred only during the first 6 months of the experiment, not after the letters had stopped. Or, if participants were operating under a continuing fear of being monitored, then there would have been no differences at 6, 9, and 12 months. Consistent with the internalization explanation, it could be that once offenders started making payments, their self-image changed to one where they acknowledged guilt and the need to help the victim (Cialdini, 2001).

Aside from internalization, it could be that the Information condition increased restitution payments because the information was useful, as a prior survey had indicated that offenders often do not know how much they owe and where the money they do pay goes (Ruback et al., 2006). Also, it could be that the external reward (the congratulations in the letter from the Director of Probation for making a restitution payment in the prior month) might have been sufficient, although this congratulations also was present in the Rationale Only and the Information and Rationale condition for probationers who made payments and did not seem to have any effect in those conditions.

Reactance is the likely explanation for the fact that participants in the Rationale condition paid significantly less than participants in the No Rationale condition. It could be that participants resented the forced rehabilitation—that they should acknowledge the harm they had caused and that they should take responsibility for correcting that harm. Ariel (2012) found a similar backfire effect as a result of moral persuasion efforts at eliciting tax compliance from corporations in Israel. It is likely the case that it is better for participants to come to the realization of acknowledging harm and taking responsibility themselves than to have those rationales forced on them.

Prior studies of attempting to increase the payment of economic sanctions have used threats to induce compliance and have generally found that threats have limited effectiveness. As Tyler (2006) argued, the downside of the type of coercive methods used in these studies is that they are effective only as long as the probability of punishment is high. Thus, threat might lead to payment but might not lead to a change in attitude because it does not lead to

internalization. Moreover, because punishing nonpayment through jail time is expensive, it is not a realistic option for the criminal justice system.

Although the ineffective nature of threats—with little likelihood of actual punishment—by the criminal justice system could explain why probationers in two treatment conditions did not pay significantly more than probationers who received no letters at all, it also is possible that guilt coerced some individuals who received the treatment into a state of reactance. The Rationale manipulation could be viewed as an attempt to induce guilt in offenders for the harms they caused. Offenders who felt that the Rationale manipulation was an attempt to manipulate them into paying restitution out of a guilty conscience could have reacted against the treatment and paid nothing. Whether a result of reactance against guilt or an improbable punishment resulting from threats, our findings suggest that coercive methods of inducing payment are ineffective.

The absence of any effect is a common finding in most studies of behavioral interventions. In this study, the Information and Rationale condition was not significantly different from the Control condition. The fact that there were main effects for the two conditions separately suggests that the absence of difference was not the result of weak manipulations. Rather, it is likely the result of the power of the Information condition to elicit payment balancing out the power of the Rationale condition to prevent payment.

Before the study began, we expected gender differences in effects, particularly for the Rationale manipulation. We hypothesized that women would be more amenable, or at least sympathetic, to appeals for victim restitution that seek to undo the harms caused by offenders' actions. However, analyses of the treatment by gender effects showed no significant gender differences. If reactance drove the (lack of) rationale effects, then the negative reaction to persuasion apparently applied to both men and women equally.

That we found no gender differences in the payment of restitution as a function of our manipulations of Information and Rationale, contrary to our hypotheses, fits within the larger research literature on the absence of gender differences in influenceability (Eagly, 1978). Rather than thinking that women would be more influenced by the Rationale and the Information condition, we should have expected no gender difference. In fact, some research has suggested that men are more influenced by prosocial appeals (Meier, 2007), particularly by descriptive norms (Croson, Handy, and Shang, 2010).

### *Effects of Notification*

To help determine why the manipulation of information affected payment, we sent a survey to participants in the experiment. In late June 2013, the individuals who had been part of the experiment received a sealed letter from the Director of Probation of the Centre County Probation Department telling them that they would be receiving a questionnaire as part of a study being conducted at Penn State University. In July 2013, we mailed packets to 736 probationers (39 prenotification letters were returned as undeliverable) containing a cover letter describing the study, a survey, and a \$1 incentive payment. Participants were



**T A B L E 6**

**Percentage Correct Responses for the Five Manipulation Check Questions by  
What the True Answer Was**

Questions	True Answer Is "Yes"	True Answer Is "No"	$\chi^2$	p Level
Received letters from the Probation Department regarding payment of restitution. <sup>a</sup>	68%	24%	.40	n.s.
Letter gave me information about how much money I owed.	67%	50%	3.82	.051
Letter gave me information about how much I had paid.	34%	87%	8.08	.004
Letter congratulated me for making payments.	16%	97%	6.48	.011
Letter encouraged me to pay restitution because it is the right thing to do.	57%	81%	21.44	.001

Note. The  $\chi^2$  value uses the correction for continuity.

<sup>a</sup>"Don't know" and missing were counted as incorrect.

promised that their responses would remain completely confidential and that neither their participation nor their responses would be shared with their probation officers or any other criminal justice personnel. Of the 736 locatable probationers, 149 (20%) responded to the survey. After we received a completed survey, we sent the respondent a \$20 Wal-Mart gift card for participating in the study.

The survey, which took about 30 minutes to complete, consisted of six sections. The first section contained questions asking respondents whether they had received any letters (the manipulation in the experiment) and about the content of those letters. Those responses constituted a manipulation check for the experiment. Manipulation checks are measures given to determine whether participants were aware of the treatments (Aronson et al., 1990). At least among the sample of individuals who returned the survey, the experimental manipulation of Information and Rationale had no effect on attitudes toward treatment, beliefs about the probation officer, or knowledge and attitudes about restitution.

**Manipulation checks for the experiment.** In general, there was only limited evidence that respondents could recall that they had received letters and what the letters contained. As shown in Table 6, respondents were not very accurate at remembering whether they had received letters. Respondents who received information about how much money they owed responded at above chance levels, whereas respondents who had not received that

information were right at chance. With regard to information about how much they had paid, congratulations for payment, and being encouraged to pay restitution, respondents who had received that content were at levels below chance, whereas respondents who did not receive that content were generally accurate that they had not received it.

In summation, some evidence showed that the participants in the Information condition remembered that they had received information about how much money they owed. But the other manipulations did not seem to have much impact on participants, suggesting that the manipulations might not have been strong enough; this issue could be investigated in future research. Of course, respondents were answering questions more than 6 months after they had received their last letter, and the lapse in time might explain the low levels of accuracy. Interestingly, however, the response rate to the follow-up survey was significantly affected by experimental condition ( $\chi^2(3) = 8.05, p < .05$ ), suggesting that the letters did affect probationers' behavior, even if they did not remember the content of the letters 6 months later. The response rate was higher in the Information Only condition (23%) and the Rationale Only condition (23%) than in the Information and Rationale condition (16%) and the Control condition (15%). The remainder of the survey results are discussed elsewhere (Ruback, Lantz, and Gladfelter, 2014).

### *Strengths and Limitations of the Study*

This study was a true field experiment; participants were randomly assigned to a condition in a real-world context. Moreover, there were enough participants in the study to ensure adequate power for most of the investigated main effects and interactions.

However, besides these strengths, the study has three limitations worth noting. First, we are limited in what we know about the probationers. That is, our manipulation (letters) was restricted to those probationers for whom we had a correct address. The location of highly mobile probationers is a real problem, both for studies like ours and for probation officers trying to monitor probationers' behavior. Moreover, because the information was not available in the files, we do not know how long individuals had been on the delinquent list. However, because year of sentence was not a significant predictor of payment, we do not believe this is a serious shortcoming.

Second, it is possible that our significant findings are the result of the Hawthorne effect, the tendency of individuals to change behavior in response to the mere fact of an environmental change rather than to the nature of the change (Roethlisberger and Dickson, 1939). In our study, the Hawthorne effect could have induced individuals to pay restitution because they were being observed (i.e., they received letters from the probation department) rather than because of the content of the letters. However, that only individuals in the Information condition paid more restitution is evidence against the Hawthorne effect because any individual receiving a letter should have paid more if being observed were the primary reason for the observed effect.

It is perhaps more likely that the Information effect reflected a fear of punishment. That is, some individuals, who were no longer under active probation supervision and likely had not had contact with the probation office for some length of time but who were still legally required to pay restitution, could have been intimidated by the sudden, repeated contact from the probation office and subsequently paid restitution that they owed. This contact could have invoked the perception that the government was once again monitoring their behavior with the threat of punishment for noncompliance. This uncertainty about the actual mediating mechanism involved in the Information condition is a third limitation of the study, a limitation that calls for both replication and for ways to discover the processes involved.

One possible criticism of the study is that probationers had the freedom, to some extent, to choose whether payments are applied to restitution or other sanctions, although it is an unlikely explanation for the results. The concern is that individuals in different conditions might have reacted differentially to treatment and paid less restitution than other sanctions, an effect that would not have been captured in the primary analyses reported in this study. For instance, individuals in the Rationale condition might have paid as much total money as individuals in the Information condition, but with less of the money to restitution and more to other sanctions because of reactance against the rationale. However, this possibility is highly unlikely. Pennsylvania law requires that the first \$60 be applied to costs/fees for victim compensation and victim witness funds and that at least half of all monies collected go toward restitution [42 PA CSA §9728(g)]. Moreover, in practice, in Centre County, all monies are typically applied toward restitution until it is paid in full. Additionally, there were no significant differences between the Information and other conditions in the amount of costs/fees and fines paid.

### **Summary and Implications**

This experiment examined whether (a) giving probationers a rationale for paying restitution, (b) giving them information about how much they have paid and how much they still owe, or (c) giving them both a rationale for payment and information about what they have paid affects how much money they pay. All three treatment conditions were compared with each other and with a control condition in which probationers did not receive either a rationale for payment or information about how much they had paid.

Our research suggests that the increased payments in the Information condition occurred because of a change in internal motivation. However, regardless of whether the mechanism is internal motivation or fear of punishment because of increased monitoring, the current research and prior studies (e.g., Davis et al., 1991) have suggested that sending letters to offenders can be effective in increasing restitution payments. Moreover, the intervention of sending a monthly letter containing the amount of restitution paid in the last month and the amount still owed is relatively inexpensive to implement and, with the right

software, could be computer generated, as Davis et al. (1991) suggested more than 20 years ago.

In our study, all of the probationers had been delinquent in paying restitution, in some cases for years. Davis et al. (1991) suggested that it would be better to intervene before they become delinquent to establish a routine of regular payments. This suggestion should be tested in future studies, as this early intervention is likely to be especially cost-effective in the long run.

Importantly, only those cases where restitution was imposed were analyzed in this study. Pennsylvania passed a statute in 1995 making the imposition of restitution mandatory. Despite the law, restitution is imposed in only approximately 63% of cases (Ruback et al., 2005). Imposition is most likely for White offenders, female offenders, offenders with no prior record, and offenders who pleaded guilty, and for property offenses and more serious interpersonal offenses. Furthermore, judges are more likely to impose restitution in cases where the victim suffered direct, tangible (i.e., quantifiable) losses (Outlaw and Ruback, 1999; Ruback and Shaffer, 2005). Also, it is possible that judges consider the ability of offenders to pay economic sanctions, although considerations of the ability to pay are prohibited by the law (Ruback and Shaffer, 2005).

Research over the past 25 years has found that courts have been imposing higher amounts of economic sanctions on offenders, that offenders pay less than half of these amounts on average, and that offenders respond to threatening letters only if the threats are perceived as real. What these studies have not investigated is the relationship between payment and recidivism. Although victims and local and state governments are interested in receiving the money due them, an issue at least as important is whether the offender is likely to commit a new crime. Future work needs to examine whether the payment of restitution is negatively related to recidivism.

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APPENDIX

*Manipulation Letters*

*Information Condition*

[Address]

[Date]

Attention [Name Here]:

This letter concerns the economic sanctions that you were ordered to pay for the following docket numbers: xxxx-xxxx xxxx-xxxx xxxx-xxxx xxxx-xxxx

To date our records show that you have paid a total of \$695 toward the total amount of \$3,785 that you were ordered to pay. You still owe \$3,090 in total. The total amounts for all docket numbers combined are as follows:

	Amounts Originally Owed	Amounts Paid	Amounts Still Owed
Fines	\$100.00	\$0.00	\$100.00
Fees/Costs	\$2,550.00	\$60.00	\$2,490.00
Restitution	\$1,135.00	\$635.00	\$500.00
Total	\$3,785.00	\$695.00	\$3,090.00

[Our records show that a payment was posted to your account within the past 6 weeks. We thank you and encourage you to continue making these payments in a timely manner.

OR

Our records show that a payment was not posted to your account within the past 6 weeks. We strongly encourage you to make a payment as soon as possible.]

Please note that if you have made a payment within the past 6 weeks, the above amounts may not be up to date. If you have any questions about your economic sanctions, you should contact your probation officer.

Sincerely,

xxxxxxxxxx

Chief Probation Officer

*Rationale Condition*

[Address]

[Date]

Attention [Name Here]:

This letter concerns the economic sanctions that you were ordered to pay for the following docket numbers: xxxx-xxxx xxxx-xxxxxxxx-xxxx xxxx-xxxx

We encourage you to pay your restitution and other sanctions for several reasons. First, it acknowledges the harm you have caused to your victims, both in terms of the monetary losses they have suffered and in terms of the disruptions to their lives. Second, it sends a message to your victims and to the probation office that you are taking responsibility for

repairing the harm you have caused. Third, paying restitution on a regular basis is related to better performance on probation and is predictive of success. That is, paying restitution means you are less likely to commit a crime while on probation.

[Our records show that a payment was posted to your account within the past 6 weeks. We thank you and encourage you to continue making these payments in a timely manner.

OR

Our records show that a payment was not posted to your account within the past 6 weeks. We strongly encourage you to make a payment as soon as possible.]

If you have any questions about your economic sanctions, you should contact your probation officer.

Sincerely,

xxxxxxx

Chief Probation Officer

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**R. Barry Ruback** is a professor of criminology and sociology at Penn State University. He conducts research examining the predictors and effects of sentencing decisions, particularly economic sanctions, and he is the faculty consultant to the Pennsylvania Commission on Sentencing. He has published in *Law and Human Behavior*, *Psychology*, *Public Policy and Law*, and the *Journal of Interpersonal Violence*.

**Andrew S. Gladfelter** is a graduate student in the Department of Sociology and Criminology at Penn State. His interests generally focus on victimization and spatial disparities in crime. He was recently involved in a state-funded grant exploring urban and rural disparities in hate crime in Pennsylvania. His dissertation focuses on the mismatch between people and places, with particular emphasis on how people adapt to a variety of spatial and ecological contexts. Gladfelter holds a BS in Criminal Justice and MS in Administration of Justice from Shippensburg University of Pennsylvania.

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### Restitution

#### Exploring Disparities and Potentials

**Susan Sarnoff**

*MSW, DSW, retired*

Ruback, Gladfelter, and Lantz's (2014, this issue) study clearly demonstrates that many more of the offenders ordered to pay restitution can and will pay more consistently if they can be both encouraged and educated about doing so. However, the study's own background notes many troubling factors:

1. Victims are often left unaware of the availability of restitution.
2. The possibility of imposing of fees and fines against offenders creates perverse disincentives to provide restitution to victims.
3. Judges might be using restitution in arbitrary and even biased ways and, conversely, might be assessing indigence similarly.

These factors suggest that restitution could be used far more effectively, extensively, and fairly not only by enforcing payment compliance but also by altering policies and practices affecting restitution.

Restitution regained popularity during the last century primarily as an alternative sentence in lieu of prison time (McGillis, 1986). It continues to be used this way to some extent but only to privileged female, White, educated, lawyered offenders, regardless of the crimes they commit (Ruback, 2002). Of course, most offenders are never caught or linked to the majority of the crimes they commit (McGillis, 1986). Most of those offenders who are caught are never required to pay restitution because of vagaries in state laws, victims' lack of awareness of its availability, and determinations of indigence on the part of offenders (Lurigio and Davis, 1990).

Criminal justice agencies also have perverse incentives to limit the use of restitution (Beckett and Harris, 2011). Over the past several "law-and-order" decades, the government has increasingly assessed the costs of prosecuting cases and overseeing incarceration,

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probation, and parole on offenders (Sarnoff, 1996). As Beckett and Harris (2011) noted, “financial penalties are now imposed on most people convicted of felonies in the United States . . . two thirds of felons sentenced to prison, and more than 80% of other felons and misdemeanants, were assessed fees and fines by the courts in 2004” (p. 515). This finding suggests that the failure to inform victims about restitution might often be intentional.

Ruback et al. (2014) also note that assumptions of indigence on the part of offenders are arbitrary at best. Judicial discretion permits judges to determine indigence and to set restitution amounts, but no studies have found consistency in judges’ determinations of the factors that would suggest who should pay and how much (Ruback, 2002). Furthermore, most offenders, and a majority of indigent offenders, are young (McGillis, 1986)—and there have never been attempts to determine the degree to which such young offenders later in life accumulate adequate means to pay restitution had they been ordered to do so. In fact, juvenile offenders are often exempted from restitution requirements entirely (McGillis, 1986).

Several studies have noted that factors other than indigence affect whether an offender will receive a restitution order. Conservative and rural judges tend to lean toward increased incarceration and away from restitution, especially if the offender is non-White, male, or lacking an attorney (Huang, Finn, Ruback, and Friedmann, 1996)—suggesting that restitution is often considered a lighter punishment and that its use or lack could reflect racism, sexism, and classism. Conversely, some judges use restitution as an additional punishment. Such variations result in wide disparities among the applications and uses of restitution, incarceration, and fees assessed against offenders (Ruback, 2002). This discrepancy creates competition for funds and other resources (such as numbers of offenders and probationers) between government agencies and crime victims; it creates incentives to focus on those crimes that could offer the highest possibility of fines (Sarnoff, 1996). It also makes it easy to disguise abuse (such as allegations that a large portion of the Ferguson, Missouri, budget derives from criminal fines assessed against non-Whites).

Judges often use an offender’s education or employment to determine his or her ability to pay (Lurigio and Davis, 1990), but many people, even ex-offenders, continue their education or gain employment and employment training after committing a crime. It is assumed that a criminal history will make it difficult for offenders to earn in the future; although it is certainly a risk factor, a criminal history does not preclude offenders from working for family or friends, developing a lucrative skill, inheriting funds, or winning a legal settlement. Ohio Attorney General Mike DeWine recouped more than \$2 million in unpaid child support over the past 4 years from state lottery winnings alone, suggesting that such efforts can be effective even if they identify that only a small number of individuals owe money (Sheldon, 2014). For another example, at least two current reality television stars began businesses after serving prison time. Although they could be anomalies, ex-offenders seem particularly well suited for such jobs as bounty hunting and automobile repossession.

Victims often have choices in regard to receiving reimbursement, including whether they request restitution and how extensively they assess their damages. Victims who have private insurance coverage for their losses might not wish to bother pursuing additional reimbursement means. Victims eligible for victim compensation often prefer the latter, as it is paid in a lump sum, which is much faster than restitution is generally provided (Sarnoff, 1996). Ironically, victim compensation programs are funded by offender fees and fines, so some offenders might be paying for the same costs twice or more. The last is the easiest problem to solve—crime victim compensation agencies can be enabled to require that restitution be considered and to set up automatic means to recoup restitution once victims have been compensated. (Many of these suggestions would require legislative changes.) Furthermore, “Son of Sam” laws were created to prevent criminals from earning windfall profits on depictions of their crimes, but these laws have been broadened to allow for victims to sue for any future income of criminals, regardless of the source (Sarnoff, 1996).

Paying restitution also lowers recidivism, which is not true of the payment of fees and fines (Ruback, 2002). Conversely, fines and fees enable the “prison-industrial complex” to feed itself endlessly, skewing focus toward those crimes and criminals that result in the highest financial returns. For all these reasons, restitution should be prioritized, altered, and expanded by the following methods:

- Making victim notification mandatory
- Ensuring that restitution always takes precedence over fees or fines (and eliminating many fees or fines)
- Ensuring that all offenders pay some amount of restitution or perform service restitution

Rather than arbitrary determination of whether and how much restitution an offender should pay, there should be an assumption that all offenders pay some restitution in some form on a sliding scale according to ability to pay, as is done in most countries (Harris, Evans, and Beckett, 2010), as well as according to age, recognizing that younger offenders have a longer time to pay even if they are currently poor. Offenders, victims, and communities benefit from restitution. If used in lieu of fines and fees and to reduce incarceration, then restitution could lower criminal justice costs, downsize the criminal industrial complex, and refocus the criminal justice system on the most serious, violent crimes.

To be effective, however, such a program must offer meaningful community work and reasonable pay (work that develops the skills of the offender and benefits the community) to unemployed offenders. As community restitution is currently popular, it could be an additional way for offenders to “pay” for their crimes, especially if their crimes affect communities as well as individuals, as they usually do.

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# The Role of Information in Changing Offender Behavior, Criminal Justice System Actions, and Policy Maker Decisions

**Daniel P. Mears**

*Florida State University*

**I**n Ruback, Gladfelter, and Lantz's (2014, this issue) study, we learn something simple and profound—that providing probationers with basic information about their restitution payment obligations, such as how much they owe, increases the likelihood that they will make payments. The benefits (i.e., increased payments) seem to exceed greatly the labor time and postage costs of providing restitution information to probationers.

There is, of course, the question of why. Perhaps the letters reminded probationers of their obligation and led them to view restitution as the morally right thing to do. Perhaps, more simply, the letters reminded them of what they were supposed to do and provided pragmatic information on which they could act (e.g., the amount due and where to send payments). The latter explanation seems the most plausible. For example, as part of the experimental design, the researchers provided one group with the information along with a rationale for why the probationers should pay. Including the rationale did not seem to lead to more payments and, indeed, could have reduced them. (Probationers might have resented being told what to do or the idea that they were being judged.)

Many interesting implications flow from this study. For example, what if providing information can change probationer behavior more so than can threats? What if sanctions were structured in ways that did not rest solely on the notion of fear (i.e., deterrence), moral change, or rehabilitation, but rather the sanctions included a focus on providing information about obligations associated with probation? Even a small improvement might provide benefits that have ripple effects throughout the correctional system. For example, a considerable number of individuals in prison today are there because they previously had a “shot” at probation and failed in some way (e.g., through new crimes or violations of the

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conditions of probation). The courts take a dim view of this type of person, and the result can be that he or she gets sent to prison. If providing probationers more information about their obligations could avert some violations or new crimes, then the pressure to rely on incarceration as a “last resort” might decline.

Ruback et al. (2014) present a compelling case for restitution payment reminders as a potentially cost-effective approach for increasing restitution payments. I want to draw attention to a broader potential implication that turns our focus away from probationers and shifts it more generally toward offenders, the criminal justice system, and policy makers, respectively. A detour to discuss “information effects” first will ground the discussion of how information-based strategies might decrease crime and improve justice.

### **Information Effects**

Providing information to people and, at the same time, making it easy for them to act on the information can produce socially desirable behavior changes. Examples from far afield of criminal justice illustrate this possibility.

#### *Recycling*

People are more likely to recycle when they are told how to do so and when recycling bins are situated in easy-to-access places. I work in an “ivory tower” setting. It turns out that people who work in these settings are as susceptible as others to changed behavior in response to simple cues and opportunities. University faculty and staff recycle more when bins are available and easy to access and when a sign provides specific instructions along with an appeal to recycle (Austin, Hatfield, Grindle, and Bailey, 1993). In universities, people tend to be highly motivated and educated; yet neither characteristic seems to be especially relevant to this phenomenon. Rather, the recycling request and the ability to comply or participate with little effort seem to be the critical factors that increase recycling.

#### *Household Energy Usage*

An analogous phenomenon occurs with household energy usage. Meadows (2008: 109) described a study of what happened when electric meters were installed in Dutch homes in an Amsterdam suburb. In this suburb, some houses by chance had been constructed with the electric meter in the basement, whereas others had been constructed with the meter in the front hallway. The meters included an easily visible dial that indicated total kilowatt-hour consumption during a given pay period. Researchers discovered that houses with the meter in the front hallway—highly visible and, indeed, difficult to miss—used one third less electricity than did the houses with the meter in the basement.



### *Vehicle Energy Usage*

Providing fuel usage meters in places other than homes can affect consumer behavior. For example, real-time information about estimated fuel usage while driving has the potential to promote energy-efficient driving habits (Stillwater, 2011).

### *Food Calories*

When restaurants provide food calorie information in easy-to-see locations, consumers purchase fewer total calories (see, e.g., Bassett et al., 2008; Gerend, 2009).

### *Illegal Parking*

Illegal parking in disabled parking spots declines when vertical signs, not just ground signs, are placed at these spots (Cope, Allred, and Morsell, 1991). Drivers tend to see vertical signs more frequently and easily than they do signs painted on the ground.

### *Handwashing in Public Restrooms (and the Doctor's Office!)*

In public restrooms, visual behavioral prompts to wash hands can increase the likelihood of handwashing (Johnson, Sholcosky, Gabello, Ragni, and Ogonosky, 2003). Similarly, training and introduction of information, along with visual cues to wash hands, can increase handwashing compliance by health care workers (Bischoff, Reynolds, Sessler, Edmond, and Wenzel, 2000).

### *Voter Turnout*

Voter turnout can be increased by providing information about upcoming elections (Lassen, 2005).

These examples barely scratch the surface, but they highlight a theme. In each instance, the common denominator is that information—when provided in prominent, difficult-to-miss places and when placed in settings where it is easy to act on the information—can lead to socially desirable outcomes.

Several important observations flow from the examples. Consider the electric meter illustration from the Amsterdam study. First, the change did not derive from focusing on individual characteristics. No attempt was made, for example, to encourage residents at one set of houses to be more virtuous or mindful about their environment.

Second, no threat or penalty was involved. Rather, the “intervention” simply consisted of providing information to households in easy-to-see places where they could immediately act on the information. (This approach clearly is not a “cure all.” I remember looking in awe at my older brother one December morning in New Hampshire when I was a freshman in high school. Freezing temperatures had turned the house into a refrigerator. He went over to the thermostat and turned up the heat. It of course says something about my childhood or perhaps my intelligence that I did not realize that this option existed; I knew what a thermostat was, but it never crossed my mind that I could adjust it myself. No parental

firestorm ensued. But the thermostat quickly returned to the “below freezing” mark after my father came downstairs.)

Third, the change was simple—all that was needed was to make it easy for people to see how much energy they were using. They then changed their behavior. The motivation to use less energy, or to pay less in electric bills, existed all along. It was the provision of kilowatt usage next to the thermostat that mattered.

Fourth, this common-sense set of conditions extends to many aspects of life. As identified previously, when it is *easy*, almost effortless, to recycle, reduce household energy use or car fuel usage, consume fewer food calories, avoid parking in illegal parking spots, wash hands, and vote, people tend to do so.

Fifth, the effectiveness of various information approaches likely varies across a range of conditions. Decay effects may set in—after a while, people might ignore a message. Conversely, repeated displays of information might reinforce the occurrence of socially desirable behaviors. Some individuals might be impervious to various cues and information, whereas others might be more susceptible to them. Playing to moral sensibilities could influence some people in some contexts and for some types of decisions. Many other such complexities might exist. Yet, they do nothing to undermine the basic insight that the mere provision of information in strategic ways, along with the creation of conditions or opportunities to act easily on the information, can improve decisions and lead to socially desirable outcomes.

### **Information and Its Potential Effects on Offenders, the Criminal Justice System, and Policy Makers**

We have crime laws, criminal justice sanctions, and correctional system policies that tend to emphasize sanctions or rehabilitation as a way to reduce crime or recidivism. Penalties and rehabilitation clearly can “work” sometimes. Under the right conditions for certain individuals, these policies could lead to crime reductions. However, the record of success is less than inspiring. During an era of mass incarceration, for example, little credible evidence remains that prisons, much less lengthy stays in prison, reduce recidivism (Cochran, Mears, and Bales, 2014; Mears and Cochran, 2014; Mears, Cochran, and Cullen, 2014; Nagin, Cullen, and Jonson, 2009).

An underlying assumption of punitive policies and rehabilitative policies, respectively, is that the individual has failings that, if addressed, should contribute to prosocial behavior. Severe penalties, it is hoped, will create a needed dose of deterrence; offenders somehow do not sufficiently fear the costs associated with offending. Rehabilitation, it is hoped, will correct criminogenic beliefs and attitudes and improve employment, education, and life skills; in turn, offenders will become law-abiding citizens.

A focus on individual failings unites deterrence-based and rehabilitation-based juvenile and criminal justice policies. So, too, does the implicit belief that “offenders” are just that—“offenders” who somehow must be changed.

Ruback et al. (2014) show, in part, that offenders seem in many respects to be like the rest of us—they sometimes make better decisions when provided with relevant information in an effective way and in settings or contexts where they can easily act on the information. That does not mean that penalties or rehabilitation are not needed. Rather, they simply are not the only or even the best, most cost-efficient way to improve decision making by individuals who might commit crime (whether previously convicted of a crime or not).

Consider the probationers. Penalties were assigned, but few payments were made. Now consider convicted felons in general. Some get placed on intensive probation. Many will fail to comply with the various conditions or requirements placed on them. Other felons will go to jail or prison. There we hope that they will comply with various rules, participate in programming, and then continue to follow rules and seek assistance after release. Typically, they will not. If they act out in prison, then they receive further punishment or removal of privileges. We cannot be confident that the punishments improve behavior. We can, however, be confident that they create substantial financial costs. And when they backfire and result in more crime, human costs in the form of additional and avoidable pain and suffering arise.

Changing individuals—their motivation, attitudes, beliefs, and the like—seems to be difficult. It is potentially easier and cheaper to channel behavior in ways that require no particular individual changes, at least not to the person. Restitution, again, is illustrative. Instead of investing in expensive supervision or counseling that seeks to change probationers, we might instead invest in various types of information provision at select times and places that would encourage prosocial behavior. Ruback et al.'s (2014) study focuses solely on restitution payments. However, the more general emphasis could be on providing useful, actionable information along a range of dimensions that might be helpful to probationer decision making. For example, correctional authorities might mail postcards to probationers describing a job fair on a given day in a nearby neighborhood along with public transportation details for getting there, or they might mail flyers with information about a parenting workshop or with information about specific effective parenting practices. Clearly, many probationers might ignore the postcards and flyers. But the better the design, the more focused the message or appeal, and the more proximate in time or space the opportunities to act on the information, the more likely that at least some probationers might take action.

Similar possibilities exist for all aspects of the criminal justice or correctional system. Law enforcement officers could be provided with information about language to use with certain populations or in certain contexts, such as a call to respond to domestic violence. Language matters, but it can be difficult in stressful situations to respond with appropriate language that de-escalates rather than escalates problem behavior. Visual cues located in strategic locations can help in such situations. Similarly, prisons could provide informational cues to inmates about what it takes to earn certain privileges. Daily updates about progress

made toward certain privileges per inmate might be posted in inmate cells. Taking such steps is, of course, easier said than done. However, the challenge is tractable. For example, research can be undertaken to identify where certain types of problem behaviors occur most frequently, to test different ways of presenting requests or information, and to determine which place-message combinations best increase compliance with particular rules. Similarly, progress toward behavioral goals can be tracked in various ways. Some may work best for certain inmates and not as well for others. Would such steps work? Perhaps not. But the risk of trying is minimal and so, too, is the cost.

In some cases, information could result in decisions that could reap substantial financial returns. Consider supermax prisons. Several years ago, the Vera Institute began an effort to work with states to help them identify who was in their supermax facilities and why. Many states do not monitor supermaxes in such a way as to be able to answer a simple but important question, “How many of the inmates in supermax housing consist of ‘nuisance’ inmates?” The question is important because, by design, supermaxes typically have been designed to house serious and violent inmates. Vera worked with state correctional system researchers and helped them to learn that in some instances—contrary to what officials claimed and believed—large numbers of nuisance inmates were being housed in supermaxes. Hesitant to trust the results, the officials consulted with their own researchers and realized that indeed that was the case. In turn, they revisited their use of supermax housing (Browne, Agha, and Austin, 2012; see also Browne, Cambier, and Agha, 2011; Mears, 2013). The “magic” here consisted of providing relevant information—in this case, information that corrections staff could act on. The implications of this simple step are considerable. For example, what if correction system directors were presented monthly with information about levels of inmate violence and reports, by inmates, of prison conditions? Officials could use such information to identify when inmate or officer reports indicated the potential for problems and, in turn, when to take some type of action.

The potential for information to improve decisions goes beyond a focus on offenders or the correctional system. Nationally, we pass laws that frequently rest on incorrect assumptions. Policy makers, for example, might claim that crime is on the increase when it is not or that the public demands “tougher” sanctioning when in fact the public may demand both tougher punishment *and* more rehabilitation (Mears, 2010). What if public opinion polling data were regularly collected, statistical briefs were produced that provided information in easy-to-understand ways across a range of dimensions (not just “support for tougher punishment” or “support for rehabilitation”), and the information and briefs were posted on congressional Web sites or distributed or posted widely? It might do little. In contrast, it might help to temper extreme legislation. Avoiding excessive swings in policy alone could save taxpayer money and avoid creating more harm than good.

Similarly, if court officials were regularly provided easy-to-interpret court processing statistics and information about the prevalence and effectiveness of the sanctions and

interventions that they employed, then better decisions might result. The goal in part consists of avoiding making unnecessary decision-making errors (Forst, 2004; Mears and Bacon, 2009). Perhaps some judges excessively encourage plea bargains to terms of incarceration for minorities but not for Whites. Without empirical monitoring of different decision-making points throughout the processing of arrests, it is impossible to know for sure. If the monitoring occurs and court officials see no problems, fine. However, if they can easily observe significant minority disproportionality, then they at least know to investigate what might be causing it.

## Conclusion

It would be Pollyannaish to believe that providing information alone will greatly reduce crime, improve criminal justice and correctional system decisions, or lead policy makers to enact better laws. Yet, it also would be irrational to allow unchecked errors in decision making to occur (Mears and Bacon, 2009). Ideally, rational decisions involve clear logic and empirical claims backed by research. Ideally, they also involve recognition of the assumptions on which policies rest. When we recognize that many offenders make decisions the same way that many nonoffenders do, as shown by Ruback et al. (2014), we are led to the insight that we might want to craft interventions for offenders in ways that would improve our own decision making and behavior. At the same time, since we know that criminal justice and corrections are filled with innumerable decisions and that many decision-making errors occur (Forst, 2004), better information about decision making and these errors is one way to improve crime and justice policy. Provision of this information to those in a position to act on it is another. Sometimes knowing that a problem exists can be enough to prod us to action. That is likely true of offenders as much as it is for policy makers, practitioners, and, well, everyone.

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# EDITORIAL INTRODUCTION

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## CORRELATES OF TERRORIST ATTACKS IN THE UNITED STATES

### Preventing Terrorism

#### Complex Social, Scientific, and Political Tasks

**L. Sergio Garduno**

**William D. Bales**

*Florida State University*

This segment of *Criminology & Public Policy* analyzes correlates of terrorist attacks in the United States by county. LaFree and Bersani (2014, this issue) analyze county-level characteristics of where terrorist attacks have occurred in the United States. Consistent with social disorganization theory, LaFree and Bersani find that terrorist attacks are more common in counties characterized by higher levels of urbanization, with a greater proportion of foreign-born residents, high language diversity, and high residential instability.

Consistent with social disorganization theory, Sherman, Gartin, and Buerger (1989) found that criminal activity concentrates in specific areas, and those criminal hot spots remain stable across long periods of time (Weisburd, Bushway, Lum, and Yang, 2004). Although terrorism and guerrilla warfare committed by government dissidents are not considered ordinary criminal behaviors, research has found that terrorist and paramilitary groups also concentrate their violent actions on specific geographic locations (Behlendorf, LaFree, and Legault, 2012; Johnson and Braithwaite, 2009; LaFree, Dugan, Xie, and Singh, 2012; Townsley, Johnson, and Ratcliffe, 2008).

LaFree and Bersani (2014) go beyond the existing level of knowledge concerning the study of terrorism on geographic locations. They not only study the incidence of terrorism attacks on specific regions, but also they analyze social, economic, and urbanization characteristics of geographic areas where terrorist attacks have occurred in the United States. Specifically, these authors analyze data from 1990 to 2011 from the

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Our thanks to Nancy Morris for serving as senior editor on the article "County-Level Correlates of Terrorist Attacks in the United States" (2014, this issue). Direct correspondence to L. Sergio Garduno, College of Criminology and Criminal Justice, Florida State University, 112 South Copeland Street, Eppes Hall, Tallahassee, FL 32306-1273 (e-mail: lsg12b@my.fsu.edu).

Global Terrorism Database to test whether the incidence of terrorist attacks is influenced by counties' variation in their levels of urbanization, residential instability, population heterogeneity, and concentrated disadvantage.

From a policy perspective, both Wormeli (2014, this issue) and Pelfrey (2014, this issue) believe that LaFree and Bersani's (2014) findings represent an important step forward in preventing terrorism. Wormeli and Pelfrey argue that the ability to predict, using science-based methods, possible targets for terrorist groups should help policy makers and homeland security personnel develop preventive counterterrorism policies and strategies, particularly on counties with the highest odds of suffering a terrorist attack. Both of these authors highlight the importance of law enforcement agencies in preventing terrorist attacks on the United States. Efforts to detect and deter terrorist attacks will be more fruitful if local, state, and federal agencies work together in information sharing and response coordination.

According to Wormeli (2014), counties with higher possibilities of experiencing a terrorist attack might need more surveillance and active policing. However, he notes that such practices should be closely evaluated to prevent any violation against civil rights and liberties. He claims that preventing terrorist activities within the United States is a task that involves all law enforcement and intelligence agencies. Wormeli (2014) argues that although LaFree and Bersani (2014) find that terrorism attacks are more prevalent in higher urbanized counties than in lower urbanized counties, many of the worst terrorist attacks have been planned and developed by terrorists in small suburban or rural communities. Wormeli's statement makes it clear that efforts to prevent further terrorist attacks on American soil will have to involve constant information sharing among the three levels of government.

Pelfrey (2014) also states that local police agencies have to close the communication and trust gap with their communities. He argues that law enforcement agencies can play the critical roles of addressing cultural diversity and subsequent intergroup tensions in their local communities. Addressing cultural diversity is of key importance in the United States as tensions resulted from our multiculturalism can deviate in crime and other harmful effects to our communities. Both Wormeli (2014) and Pelfrey (2014) state that local law enforcement agencies play a critical role in gathering information of suspicious, and potentially dangerous activities. However, police agencies across the nation will first have to reinforce the trust from their local communities, especially among minority groups. The tension raised from America's multicultural societies can limit law enforcement agencies' capacity to gather critical information. To overcome this limitation, police departments will need to work with community leaders to ease the tensions among diverse groups and, perhaps more important, ease their tensions with members of minority groups. Trustful communities of their police departments will be more likely to report suspicious behaviors to the police and to let the police evaluate and share potentially critical information to their counterparts at state and federal levels to prevent terrorist acts in the United States.

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# County-Level Correlates of Terrorist Attacks in the United States

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### Research Summary

*We develop a set of hypotheses informed by a social disorganization framework and test them using newly available data on nearly 600 terrorist attacks in U.S. counties from 1990 to 2011. Our results show that terrorist attacks were more common in counties characterized by greater language diversity, a larger proportion of foreign-born residents, greater residential instability, and a higher percentage of urban residents. Contrary to the social disorganization perspective but in keeping with most prior research, terrorist attacks were less common in counties marked by high levels of concentrated disadvantage. More generally, we found steady declines in the number of terrorist attacks on U.S. soil from 1990 to 2011. We discuss the implications of the results for theory, future research, and policy.*

### Policy Implications

*Terrorism, like ordinary crime, is highly concentrated. Of the 3,144 counties in the United States, only 250 (7.95%) experienced a terrorist attack from 1990 to 2011; 5*

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*counties (0.002% of total U.S. counties) accounted for 16% of all attacks. Moreover, counties at greatest risk of terrorist attack have identifying characteristics. Just as random preventive patrol policing has generally been replaced by more targeted strategies, efforts to counter terrorism might benefit from strategies that target certain counties: those with high population heterogeneity and great residential instability that are highly urban. And just as targeting particular neighborhoods raises equity concerns in policing, policies aimed at counties with particular characteristics pose a challenge for countering terrorist attacks. However, unlike the situation in policing ordinary crime, high-terrorism-risk counties are generally not characterized by economic disadvantage or a large proportion of racial and ethnic minorities.*

The importance of understanding terrorism in the United States assumed heightened prominence in the wake of the coordinated attacks of September 11, 2001. Yet with few exceptions (Freilich, Chermak, Belli, Gruenewald, and Parkin, 2014; Smith and Damphousse, 2009), surprisingly little is known about general patterns of terrorist attacks in the United States, including where attacks are most likely to occur and whether specific characteristics of places increase the risk of attacks. Moreover, most research aimed at understanding the distribution of terrorist attacks has developed without a firm theoretical foundation. In this article, we develop a set of hypotheses about terrorist attacks drawn generally from the social disorganization tradition in criminology. Using data from the Global Terrorism Database (GTD), we test these hypotheses by examining county-level counts of terrorist attacks in the United States from 1990 through 2011. Our results are largely consistent with the hypotheses derived. Across the two decades spanned by our data, terrorist attacks were more common in counties characterized by greater language diversity, a larger proportion of foreign-born residents, greater residential instability, and a greater proportion of urban residents. In contrast to social disorganization theory but in line with prior research on terrorism, attacks were less common in counties with high levels of concentrated disadvantage. Despite the general applicability of the social disorganization perspective for understanding aggregate levels of terrorist attacks in the United States, much work needs to be done to explain the microlevel processes behind these patterns. We conclude with a discussion of the implications of the findings for future research and policy.

### **Geographic Concentration of Crime and Terrorism**

Over the past several decades, a growing body of research has found that certain areas are “hot spots” of criminal activity (Sherman, Gartin, and Buerger, 1989) and that crime is not randomly dispersed across areas, but instead it is systematically concentrated. In fact, research has shown that the clustering of ordinary crime in geographic areas is stronger than the clustering of ordinary crime among individuals. As a result, the prediction of where crime occurs may be easier than the prediction of who commits it. Specifically, Sherman (1995:

36–37) demonstrated that ordinary crime is “six times more predictable by the address of the occurrence than by the identity of the offender.” Moreover, although geographic areas do change (e.g., changes in population composition and land use), research has demonstrated that ordinary crime hot spots are relatively stable over extended periods of time (Weisburd, Bushway, Lum, and Yang, 2004).

Similar to the spatial analysis of ordinary crime, there are compelling reasons to expect that patterns of terrorist attacks will be geographically clustered. For example, in a study of insurgent attacks in Iraq after the U.S.-led invasion in 2003, Townsley, Johnson, and Ratcliffe (2008) found that improvised explosive device (IED) attacks in Iraq were heavily concentrated geographically and that distances between attacks were set to maximize efficiency without increasing the risk of capture. Similarly, Johnson and Braithwaite (2009) examined the presence of chains of IED and non-IED attacks in Iraq down to a spatial resolution level of 100 m, and they found evidence that both were highly concentrated in geographical space. A study by LaFree, Dugan, Xie, and Singh (2012) on attack patterns in Spain by the terrorist organization Euskadi Ta Askatasuna (known as ETA) found that attacks were heavily concentrated in the four main Spanish provinces claimed as a homeland by the organization, and 40% of all attacks in a given province were immediately followed by another attack in the same province. This heavy concentration of terrorist attacks by ETA is confirmed in research by Behlendorf, LaFree, and Legault (2012), who found high geographic concentrations for attacks by province not only by ETA in Spain but also by the Farabundo Martí National Liberation Front (known as FMLN) terrorist organization in El Salvador.

In this article, we draw on a major criminological perspective to provide a framework for examining factors related to the geographic concentration of terrorist attacks. This strategy is predicated on the assumption that behavior officially labeled “criminal” is extremely broad, and although terrorism clearly differs in some respects from more common forms of crime (see LaFree and Dugan, 2009, for a review), terrorism is nonetheless “a form of crime in all essential respects” (Clarke and Newman, 2006: vii). More specifically, LaFree and Dugan (2004: 67) pointed out that criminal events resemble terrorist events in that “both . . . can be counted and display non-random temporal and spatial patterns that are likely associated with endogenous and exogenous characteristics of offenders, targets, and situations.” Furthermore, because much criminological research has emphasized the understanding of crime patterns across spatial and temporal dimensions, theoretical perspectives and research methods commonly used in criminology should be relevant in the study of terrorism. In the next section, we review the social disorganization perspective briefly and develop a set of hypotheses about the spatial concentration of terrorist attacks in U.S. counties.

### **Social Disorganization and Terrorist Attacks**

In his classic formulation, Durkheim (1951 [1930]) argued that a well-organized society integrates members into the whole, provides them with a sense of community, and offers

them realistic goals and aspirations. But when there is a breakdown in social organization, both institutional (law and the legal system) and informal (family, work, school, and voluntary organizations) sources of social control lose their ability to channel individuals into conventional behavior. These developments free social actors to engage in a wide variety of antisocial behavior, including politically motivated violence (Davies, 1962; Smelser, 1962). Social disorganization has been defined most famously as the inability of communities to self-regulate or to realize shared values and solve commonly experienced problems (Kornhauser, 1978: 63; Shaw and McKay, 1942). Social disorganization theory leads us to predict higher levels of deviance, including protest and crime from individuals residing in disorganized communities that lack shared norms and face less threat of institutional and informal punishment.

In this research, we rely on the social disorganization perspective to develop predictions about the macrolevel characteristics of areas where terrorist attacks have occurred in the contemporary United States. Social disorganization theory highlights the importance of place in understanding the distribution and concentration of political violence and crime. Heterogeneous communities experience high levels of crime because population heterogeneity increases the deviant behavior of community members and decreases the effectiveness of informal and formal social control. Many studies have demonstrated support for social disorganization theory in understanding the geographic concentration of crime across neighborhoods (Sampson, Morenoff, and Raudenbush, 2005; Sampson, Raudenbush, and Earls, 1997), cities (Krivo and Peterson, 1996; Ramey, 2013), and counties (Arthur, 1991; Osgood and Chambers, 2000).

Although few researchers have applied this perspective to terrorist behavior, prior research has suggested that both extremist violence (Baudains, Braithwaite, and Johnson, 2013; Gruenewald, Chermak, and Freilich, 2013) and terrorist attacks (Behlendorf et al., 2012; Johnson and Braithwaite, 2009; LaFree et al., 2012) are highly concentrated in time and space. Moreover, the importance of place is implicit in much work examining terrorist attacks. For example, ethnic diasporas concentrate individuals with similar cultures and languages potentially fostering the recruitment of new members of ideological belief systems who share these characteristics. Thus, recent research has linked support for diverse terrorist organizations directly to diaspora communities, including the Irish in the case of the Irish Republican Army (known as the IRA; Clutterbuck, 2008), Armenians in the case of Armenian Secret Army for the Liberation of Armenia (known as ASALA; Dugan, Huang, LaFree, and McCauley, 2008), and Tamils in the case of the Liberation Tigers of Tamil Eelam (known as the LTTE; Aryasinha, 2001; Nadarajah and Sriskandarajah, 2005; Sheffer, 2006).

In the next section, we review the literature relevant to the main social disorganization concepts to be tested and offer our hypotheses. We then describe our data and methods before presenting the results and discussing their implications.



### *Population Heterogeneity*

The expectation that the level of population heterogeneity in a community is related to disorder and crime has historical roots in the dramatically changing urban landscape of the United States in the early 20th century. With massive numbers of immigrants of mostly European origin flocking to cities, urban communities were rapidly transformed into centers of diversity, the result of which was not always positive. An inherent by-product of immigration is that migrants bring with them sets of rules and norms unique to their homeland: norms that to some degree may be different from and sometimes in opposition to the dominant values in the host society as well as the values of other immigrants (Sellin, 1938). Moreover, prior research also has shown that both informal (Markowitz, Bellair, Liska, and Liu, 2001; Sampson and Groves, 1989) and formal (Greene and Herzog, 2009; Weisburd and Braga, 2006) controls are weak in heterogeneous communities. As a result, communities in which large concentrations of immigrants initially settle may be characterized by instability as groups of individuals—each acting in accordance with their own sets of norms—come in contact with one another.

Researchers have typically operationalized population heterogeneity as the percentage of the population that is foreign born residing in specified geographic areas; however, more recently empirical work has argued for the use of alternative measures of population heterogeneity that tap into differences in the extent to which areas are characterized by ethnic or language diversity (Graif and Sampson, 2010). Putnam (2007) argued that at least in the short term, neighborhood population heterogeneity reduces community solidarity and social capital thereby reducing trust and increasing feelings of isolation. He found that in the United States, high levels of ethnic diversity in neighborhoods are related to low levels of trust. Shihadeh and Barranco (2010) reported negative consequences of heterogeneity and particularly linguistic isolation, concluding that counties characterized by a greater proportion of linguistically isolated households (i.e., not fluent in English) experience larger rates of homicide than counties that consist of mostly English-speaking households.

Following prior research, we expect that population heterogeneity may increase the risk of terrorist attacks by increasing feelings of marginalization from the host community. In support, researchers have linked feelings of alienation in diaspora communities to a perceived schism between the West and traditional values from the migrant's home culture (LaFree and Ackerman, 2009; McCauley and Moskalenko, 2011; Thachuk, Bowman, and Richardson, 2008). Importantly, these feelings of alienation increase the chances that individuals in affected communities will participate in terrorist plots, but more generally, such feelings undermine the effectiveness of social control and the ability of communities to self-regulate. Thus, population heterogeneity is likely both to increase levels of deviance and to decrease the effectiveness of informal and formal social control. Based on these arguments, we expect to find that population heterogeneity will be positively associated with terrorist attacks.

*Hypothesis 1:* As percent foreign born increases, terrorist attacks will increase.

*Hypothesis 2:* As language diversity increases, terrorist attacks will increase.

### *Residential Instability*

Starting with Shaw and McKay (1942), a substantial body of research (Bogges and Hipp, 2010; Osgood and Chambers, 2000; Sampson et al., 1997; Xie and McDowall, 2008) has demonstrated a strong link between residential instability and high crime rates. Social disorganization researchers have argued that a heightened level of mobility in a neighborhood destabilizes the community by weakening social ties, impeding communication, and undermining the ability of community residents to establish and uphold norms in their neighborhoods (Bellair, 1997; Sampson and Groves, 1989). As a result, crime increases in highly transient neighborhoods. Although most prior research on this issue has examined crime in cities or communities, Osgood and Chambers (2000) reported a strong link between residential instability and juvenile violence measured across 264 nonmetropolitan counties. Again, residential instability increases the likelihood that individuals will engage in deviance and reduces the effectiveness of informal and formal sources of social control. Although we know of no prior research that has specifically examined connections between residential instability and terrorism, to the extent that weak social ties with neighbors, limited communication, and feelings of isolation or alienation are higher in communities with greater residential instability, we expect that terrorist attacks will be more common in counties with high rates of residential instability.

*Hypothesis 3:* As residential instability increases, terrorist attacks will increase.

### *Concentrated Disadvantage*

Early formulations of social disorganization theory (e.g., Shaw and McKay, 1942) linked crime to poverty in general; however, more recent applications have refined this proposition by focusing on the role of concentrated disadvantage in explaining variation in crime levels across communities (Krivo and Peterson, 1996; Sampson and Wilson, 1995). The concentration of disadvantage (e.g., poverty and joblessness) results in areas, and the residents in these areas, being socially isolated from mainstream society and generally lacking an ability to mobilize resources to prevent crime. The relationship between concentrated disadvantage and crime has received fairly consistent empirical support (Krivo and Peterson, 1996; Kubrin and Weitzer, 2003; Morenoff, Sampson, and Raudenbush, 2001). Interestingly, Osgood and Chambers (2010), who conducted one of the few tests of social disorganization perspectives at the county level, did not find a significant link between a measure of concentrated disadvantage (poverty) and crime (juvenile violence), although their analysis was limited to nonmetropolitan counties in four states.

Although the social disorganization perspective has long posited a positive (albeit indirect) connection between concentrated disadvantage and crime, and while similar arguments have been made with regard to terrorism (Arnold, 1988: 135–136), empirical tests of these expectations have received little support when applied to either the communities where terrorism occurs or the individuals who commit terrorist acts. Thus, much prior research

(Krueger, 2007; Krueger and Maleckova, 2003; Piazza, 2006; Silke, 2008; for a review, see LaFree and Ackerman, 2009) has shown that in general, terrorists are not drawn from poor communities, and those who participate in terrorist actions are, if anything, somewhat better educated and more prosperous than the populations from which they are drawn. For example, Russell and Miller (1977: 110) compiled profiles of more than 350 individual terrorist cadres and leaders across 18 different terrorist groups active during the 1966 to 1976 time span. The prototype derived from their composite described young men with middle-upper-class backgrounds and some university education (and included being an urban resident and holding an extremist political philosophy). Although it is persuasive, existing research examining the link between disadvantage and terrorism has been limited almost entirely to individual rather than to aggregate-level relationships. Although prior research has not been conclusive for the aggregate analysis proposed in this article, on balance the weight of the evidence suggests a break with the social disorganization perspective in terms of the expected impact of concentrated disadvantage on rates of terrorism.

*Hypothesis 4:* As concentrated disadvantage increases, terrorist attacks will decline.

#### ***Percent Population in Urban Areas***

The proportion of the population living in urban areas has been a common predictor of crime in social disorganization applications with the expectation being that a high percentage of urban residents will be associated with high crime rates (Markowitz et al., 2001; Sampson and Groves, 1989). There is thus far very little empirical data on this topic in the study of terrorist attacks. However, research (LaFree and Dugan, 2009) has shown that terrorists often target major metropolitan areas. Similarly, Smith, Fitzpatrick, Roberts, and Damphousse (2012) found that terrorist attacks in the United States prosecuted by the U.S. Federal Bureau of Investigation occurred disproportionately in urban areas. To the extent that this targeting pattern is consistent and sustained, it leads us to predict that highly urban areas will face elevated risks of terrorist attacks.

*Hypothesis 5:* Compared with low urbanized areas, more highly urbanized areas will experience increases in terrorist attacks.

## **Data and Methods**

### ***Measuring Terrorist Attacks***

To test the arguments developed in the preceding section requires a comprehensive database including all terrorist attacks that occurred in the United States over time. We use the Global Terrorism Database (GTD), which provides systematized descriptive information about terrorist attacks from unclassified print and electronic media sources where the attack is the unit of analysis (LaFree, 2010; LaFree and Dugan, 2007) and is now the most comprehensive of the unclassified databases on terrorist attacks (Dugan and Chenoweth,

2012; Enders, Sandler, and Gaibullov, 2011).<sup>1</sup> The definition of a terrorist attack used by the GTD is sufficiently broad to fit our purposes: *the threatened or actual use of illegal force by nonstate actors, in order to attain a political, economic, religious or social goal, through fear, coercion, or intimidation.*<sup>2</sup> This definition includes threats of violence as well as the actual use of violence. For example, instances where individuals who seize an aircraft and threaten to blow it up unless their demands are met are defined in this study as terrorist attacks. It includes attempted as well as completed attacks. Note also that by specifying the threatened or actual use of force, the definition of terrorism used by the GTD excludes hoaxes. The requirement that events be limited to the actions of “nonstate actors” means that terrorist attacks that are directly attributable to states or their militaries are excluded. Also, the requirement that the act have a direct political goal means that we make every effort to exclude ordinary criminal violence. Thus, the GTD excludes state terrorism and many types of crime and genocide; these topics are important and complex enough to warrant their own separate analysis.

Our dependent variable is the frequency of terrorist attacks for all 3,144 U.S. counties for each year from 1990 through 2011.<sup>3,4</sup> Most counties (92%) experienced no terrorist attacks from 1990 to 2011 and were coded “zero.” Although more than half of the remaining counties (58%) experienced just one attack, the incidence of attacks in a county over this 21-year period ranged from 1 to 30 attacks. The attacks varied greatly in terms of lives lost. In 541 attacks (91%), there were no fatalities. In some cases, terrorist organizations target facilities or buildings. Attacks by the Animal Liberation Front (ALF) and the Earth Liberation Front (ELF) are prominent examples. In other cases, attackers plan but fail to produce fatalities. This was the case with Umar Farouk Abdulmutallab’s aborted attempt to

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1. The complete worldwide GTD can be found at [start.umd.edu/gtd](http://start.umd.edu/gtd). The U.S. data for this analysis were downloaded on October 1, 2013.
  2. This was the original definition of terrorism applied to the data (LaFree, 2010). However, starting with the 1998 data, the GTD data collection team required that two of the following three criteria be met for inclusion in the database: The violent act (a) was aimed at attaining a political, economic, religious, or social goal; (b) included evidence of an intention to coerce, intimidate, or convey some other message to a larger audience (or audiences) other than the immediate victims; and (c) was outside the precepts of international humanitarian law. These inclusion criteria are evaluated for each case to determine whether it should be added to the GTD; however, there is often definitional overlap between terrorism and other forms of crime and political violence, such as insurgency, hate crime, and organized crime. Likewise, for some cases, the information provided in source documents is insufficient or conflicting, and coders cannot make a clear determination regarding whether the inclusion criteria are met. Note that the requirement that a violent act must be aimed at attaining a political, economic, religious, or social goal is not satisfied in cases where perpetrators acted out of a pure profit motive or from an idiosyncratic personal motive unconnected with advocating broader societal change. We include data in the present analysis that meet any two of the three criteria outlined by GTD.
  3. The term “county” is also used to reference county-equivalent geographic subdivisions including parishes and boroughs.
  4. Data from 1993 were lost by the original data collectors in an office move, and researchers have never been able to restore them successfully (LaFree and Dugan, 2007). We, therefore, treat 1993 as missing.

TABLE 1

**Descriptive Statistics of Study Variables, U.S. Census**

Variable	1990		2000	
	Mean	SD	Mean	SD
Language diversity	0.12	0.11	0.14	0.12
Percent foreign born	2.25	3.63	3.47	4.87
Percent urban	36.61	30.04	40.10	31.00
Black	8.52	14.33	8.71	14.46
Hispanic	4.47	11.14	6.19	12.00
Percent male ages 15 to 24 years	21.87	3.16	14.26	3.49
Total population	80468.31	2.67 <sup>e5</sup>	89586.02	2.93 <sup>e5</sup>

Notes. SD = standard deviation. Residential instability and concentrated disadvantage are both regression factor scores with mean = 0 and standard deviation = 1.

detonate explosives sewn into his underwear in a plane approaching Detroit in December 2009 and Faisal Shahzad's failed plan to explode a car bomb in Times Square, New York City, in May 2010.

Only eight cases in the analysis (1.34%) resulted in more than 10 fatalities. By far, the deadliest of the events included in the analysis were the four coordinated attacks of September 11, which according to the GTD resulted in 2,996 fatalities. Terrorism researchers (e.g., Enders et al., 2011; LaFree, Yang, and Crenshaw, 2009) commonly distinguish domestic and international terrorist attacks. Only 14 cases examined in this study were linked convincingly to non-U.S. citizens.<sup>5</sup> We will return to these issues later in the analysis when we conduct robustness checks on the results.

### *Explanatory Variables*

All county-level indicators of social disorganization (e.g., population heterogeneity and residential instability) and control variables are taken from the 1990 U.S. Census; descriptive statistics for both 1990 and 2000 are reported in Table 1 to allow comparisons. In general, language diversity, percent foreign born, percent urban, percent Black, and percent Hispanic all increased between the two decades, whereas percent males ages 15 to 24 years declined. The average population size of the 3,144 counties also increased across the two decades. Although counties in these data are, on average, somewhat large (more than 80,000 people), the size of the counties varies widely and is significantly skewed toward smaller populations (1990 range 52 to 8,948,425; 2000 range 67 to 9,519,315).<sup>6</sup> We choose a county-level

5. For a more detailed discussion of the complexities of distinguishing domestic and international terrorist attacks, see LaFree, Dugan, and Miller (2015: ch. 8).

6. Because the total population variable is highly skewed, we use the natural log of total population in the robustness analyses.

analysis because we are interested in the effects of community measures of social disorganization on terrorist attacks, and the county is the lowest level of disaggregation allowing statistical analysis. It is a strategic choice because much of the quantitative analysis of terrorism to date has focused on examining either differences across countries (Enders et al., 2011; LaFree, Morris, and Dugan, 2009) or differences across perpetrator groups (Cronin, 2011; LaFree et al., 2009).

**Population heterogeneity.** We include two measures of population heterogeneity: percent foreign born and language diversity. Although both capture the heterogeneous character of a geographic area and are significantly correlated, they are not synonymous measures (see the Appendix). Consistent with many criminological applications of social disorganization theory, we include the percentage of the population that is foreign born in our models. Counties range from a high of 45% of the population reporting being born outside the United States to a low of 0%.

Following Graif and Sampson (2010), we also include in the analysis a measure of language diversity. Our language diversity measure refers to the language spoken at home and captures 29 different languages: English, Spanish or Spanish Creole, French (including Patois and Cajun), French Creole, Italian, Portuguese or Portuguese Creole, German, Yiddish, other West Germanic languages, Scandinavian, Greek, Russian, Polish, other Slavic languages, Indo-European languages, Chinese, Japanese, Korean, Mon-Khmer and Cambodian, Vietnamese, Tagalog, Hungarian, and Arabic. We calculate language diversity using the Herfindahl Index<sup>7</sup>:

$$L_t = 1 - \left( \sum \pi_r^2 \right)_t$$

where  $t$  is the county,  $r$  is a particular language group in that county, and  $\pi_r$  is the proportion of the population speaking that language in the county. Language diversity ranges from 0 to 1, where 0 indicates that the same language is spoken at home by all residents in the county. As the language diversity value approaches 1, it indicates a greater proportion of the county's population that speaks different languages (more language heterogeneity), whereas values closer to 0 indicate a smaller proportion of the population speaking different languages (less language heterogeneity). In these data, language heterogeneity ranges from a high of .65 to a low of zero.

**Residential instability.** We use two variables to measure the level of residential instability in a county: percentage of residents 5 years old or older who resided in the same household for 5 or more years prior to the survey and the percentage of owner-occupied housing units in the county. These two items are strongly correlated ( $r = 0.517$ ;  $p \leq .001$ ), and a

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7. Referred to also as the Herfindahl-Hirschmann Index, which is a commonly used econometric tool to measure the concentration of firms in a market. Graif and Sampson (2010) adapted the index to measure the concentration of languages spoken in a geographic area.

factor analysis indicated that the two items load well together (factor loading value = 0.77). Regression scores were saved and used in the subsequent analyses.

**Concentrated disadvantage.** To construct our concentrated disadvantage measure, we conducted a factor analysis of the percentage of families below the poverty line, unemployed individuals in the civilian labor force, female-headed households with children younger than 18 years of age, individuals in low-wage employment positions, and individuals receiving public assistance. Factor analysis indicated that all items loaded well on one component (factor loading values between 0.78 and 0.93) with strong reliability ( $\alpha = 0.80$ ). Regression scores were saved and used in the subsequent analyses.

**Percent urban.** Our measure of urbanization captures the percentage of individuals living in areas (territory, population, and housing units) with a population of 2,500 or more inhabitants.<sup>8</sup> The average proportion of the population in each county living in an urban area so defined is slightly more than one third.

**Control variables.** Predictions about differential deviance rates by race and ethnicity were not a part of Shaw and McKay's (1942) original formulation, and indeed a key finding of their research was that local communities retain their relative delinquency rates despite changing racial and ethnic composition. Nevertheless, a tremendous amount of research examining social disorganization perspectives has reported racial and ethnic differences in the commission of crime (Hawkins, Laub, and Lauritsen, 1998; Sampson and Wilson, 1995; for a review, see Peterson, Krivo, and Hagan, 2006), and several recent studies have examined variables that account for contemporary race differences in violence (Kaufman, 2005; LaFree, Baumer, and O'Brien, 2010; McNulty and Bellair, 2003; Parker and McCall, 1999; Sampson et al., 2005; Vélez, Krivo, and Peterson, 2003) or compare Black and White crime rates over time (LaFree, O'Brien, and Baumer, 2006; Parker, 2008). We therefore include race and ethnicity measures as control variables. The racial and ethnic composition of each county was measured by the percentage of the population that is non-Hispanic Black and Hispanic. The average percentage of the non-Hispanic Black population was 8.52, and the average percentage that was Hispanic was 4.47. We also include the percentage of the population that is male, ages 15 to 24 years (mean = 21.87) as a control variable. Because a large portion of terrorist attacks occurs in the borough of Manhattan, New York (5% of all attacks from 1990 to 2011), we include a dummy variable identifying this

8. We experimented with including an indicator of population size in our models; however, the percent urban is strongly correlated with total population ( $r = 0.711$ ; see the Appendix for details), and the inclusion of both measures introduces considerable multicollinearity. Our decision to emphasize percent urban was driven by its connection to the original articulation of social disorganization theory. However, we conducted a series of analyses (available on request) parallel to those presented in the main text substituting total population for percent urban. The results are substantively similar with the exception that adding total population to our models reduces the percent foreign-born measure to nonsignificance. However, the language diversity measure of population heterogeneity remains a strong and significant predictor of terrorist activity when total population is included in the analysis. We return to these issues later in our tests for the robustness of our results.

location. Finally, we include an annual measure of time capturing the 21 years of data examined in the models.<sup>9</sup>

### *Analytic Strategy*

Following past practice (e.g., Dugan and Chenoweth, 2012; Enders et al., 2011; LaFree, Morris, et al., 2009), our dependent variable is a count of the number of terrorist attacks in each county by year. Our county-level data are based on annual observations over time, and because counties are nested within the 21 years of data (e.g., counties have repeated observations over time), using standard multivariate modeling approaches (e.g., OLS and negative binomial) could produce incorrect estimates of the standard errors and thereby increase the likelihood that we falsely conclude that statistically significant relationships exist. We therefore employed a generalized hierarchical linear modeling strategy (Raudenbush and Bryk, 2002) where the dependent variable is a count of the number of terrorist attacks in each county in each year. As noted, a large number of counties experienced no terrorist attacks during the years spanned by these data, resulting in a skewed distribution with a high frequency of zeroes. To account for the non-normal distribution of our dependent variable, we use a Poisson-based regression analytic strategy (Allison, 1999) with an extension of HLM to allow for the inclusion of an overdispersion parameter (HLM equivalent to a negative binomial model). Compared with standard models, models with the addition of an overdispersion parameter have been shown to result in more accurate significance tests (Osgood, 2000).

We specify a two-level hierarchical model whereby county-year trajectories of terrorist attacks are modeled at Level 1 and county-level characteristics are modeled at Level 2. More specifically, the Level 1 model characterizes within-county change in terrorist attacks over time, controlling for annual deviations in the frequency of terrorist attacks:

$$\eta_{it} = \log(\lambda_{it})$$

$$\eta_{it} = \pi_{0i} + \pi_{1i}(\text{year}_{it}) + e_{it}$$

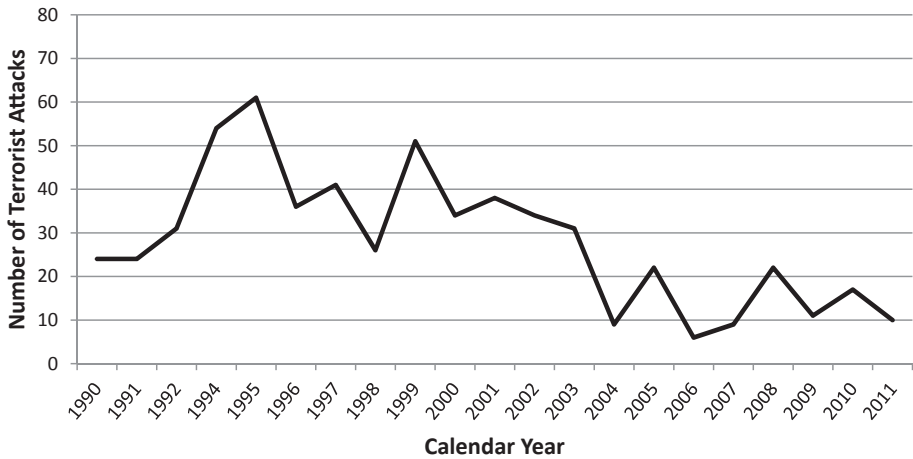
where  $\eta_{it}$  is the log of the terrorist attack rate for county  $i$  at time  $t$ . To capture the decline in terrorist attacks over the two decades observed in this study, we specified the equation to include a measure of time (i.e., year) centered on the first wave, which represents the average rate of terrorism in 1990. The subscript  $i$  attached to the variables at Level 1 indicates that these variables can take on different values for each county.

We add the independent variables and controls to the equation at Level 2 and assess between-county differences in terrorist attacks based on our variables of interest. Coefficient

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9. The total number of years reported here (21) reflects the 22-year period from 1990 to 2011 minus the missing data for the year 1993.



**FIGURE 1****Terrorist Attacks in the United States, 1990 to 2011**

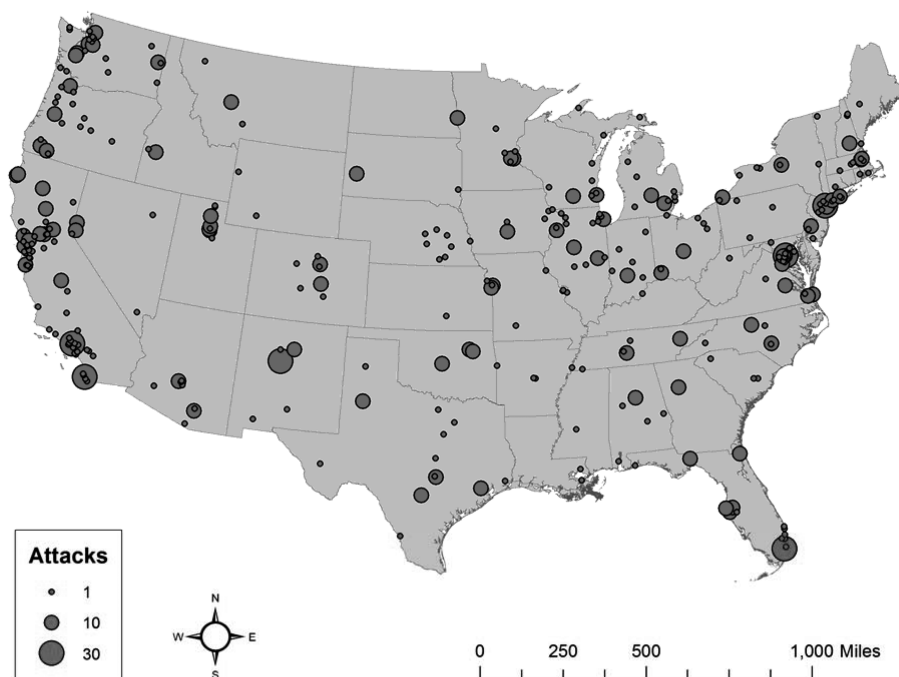
effects at this level indicate how much variation in the intercept (i.e., initial terrorist attack level) is explained by between-county characteristics. The Level 2 equation is:

$$\begin{aligned}\pi_{0i} = & \beta_{00} + \beta_{01}(\text{language diversity})_i + \beta_{02}(\text{foreign born})_i + \beta_{03}(\text{residential instability})_i \\ & + \beta_{04}(\text{concentrated disadvantage})_i + \beta_{05}(\text{urbanicity})_i + \beta_{06...k}(\text{controls})_i + r_{0i} \\ \pi_{1i} = & \beta_{10} + r_{1i}\end{aligned}$$

Variation in the log-odds of a terrorist attack is explained by the social disorganization measures and the county-level controls. All continuous independent variables are grand-mean centered. We allow for variation between counties in the rate of terrorist attacks and year as indicated by the error terms. In sum, the generalized hierarchical linear modeling approach allows us to accommodate the clustered nature of the data, and the Poisson-based regression analysis provides conservative estimates of terrorist attacks at the county level over 21 years.

## Results

Before proceeding to the findings for the hypotheses, we first present descriptive information on the number and location of terrorist attacks in the United States. In Figure 1, we show the annual trend in terrorist attacks from 1990 to 2011. In total, 597 attacks occurred from 1990 to 2011. Even though the high point for terrorist attacks in the United States was 1995 (62 attacks), the total numbers gradually decline over the 21 years captured in

**FIGURE 2****Geographic Concentration of Terrorist Attacks in the United States, 1990 to 2011**

these data. The decline is especially striking for the years just after 2001, when total attacks dropped from 39 (2001) to 34 (2002) to 9 (2004).<sup>10</sup>

In Figure 2, we provide a symbol map to display visually the concentration of terrorist attacks across U.S. counties in the 48 contiguous states.<sup>11</sup> Two patterns are clear from Figure 2: (a) A small number of counties account for a large portion of U.S. terrorist attacks, but (b) the impact of terrorist attacks is felt broadly across the country as at least one terrorist attack has occurred in 47 of the 48 contiguous states (the exception is Delaware). Of the 597 attacks that occurred between 1990 and 2011, 16% took place in five counties: New York County, NY (Manhattan) ( $n = 30$ ); Los Angeles County, CA

10. These relatively low levels have continued in the 2 years since we conducted the original analysis, with 13 recorded attacks against the U.S. homeland in 2012 and 15 attacks in 2013 (<http://www.start.umd.edu/gtd>).

11. Because there were no terrorist attacks in Alaska or Hawaii during the time period studied, we exclude them from the map but not from the quantitative analysis that follows.

**TABLE 2**

**Bivariate Correlations Between Explanatory Variables and Terrorist Attacks**

Variable	Frequency of Terrorism 1990–2011
Language diversity	0.210***
Percent foreign born	0.359***
Residential instability	0.266***
Concentrated disadvantage	−0.025
Percent urban	0.271***
Black	0.038*
Hispanic	0.112***
Percent males ages 15 to 24 years	−0.003

\* $p < .05$ . \*\*\* $p < .001$ .

( $n = 19$ ); Washington, D.C. ( $n = 17$ ); San Diego County, CA ( $n = 16$ ); and Miami-Dade County, FL ( $n = 14$ ).

We present the bivariate correlations between our independent variables and total county-level terrorist attacks in Table 2. In general, the bivariate results support our hypotheses. Greater language diversity, a higher percent foreign born, and more residential instability are all associated with an increased risk of terrorist attacks. By contrast, we find a negative relationship between our measure of concentrated disadvantage and terrorist attacks (although it is insignificant). As expected, percent urban is positively associated with the frequency of terrorist attacks.

We next model annual county-year trends in the frequency of terrorist attacks over the 21 years using the hierarchical modeling strategy described previously. To examine trends across the two decades, we use census measures from 1990 and predict terrorist activity annually from 1990 to 2011. We present the results of the longitudinal multivariate analyses in Table 3.

In Table 3, Model 1, controlling for a wide range of variables, we find that counties with greater population heterogeneity (i.e., language diversity and percent foreign born) and greater residential instability had significantly higher numbers of terrorist attacks. In line with our hypothesis but contrary to social disorganization perspectives, we find that counties with higher concentrated disadvantage had lower numbers of terrorist attacks. Finally, when controlling for other variables, counties with a higher percentage of urban residents experienced significantly more terrorist attacks.

Among the control variables included in the analysis, percent Hispanic was associated with significantly fewer terrorist attacks. The percentage of African Americans, the percentage of young men, and whether the attack occurred in New York City had no significant effect on the number of terrorist attacks. In support of the trends shown, the negative effect

T A B L E 3						
Multivariate HGLM Predicting the Frequency of Terrorist Attacks by U.S. County, 1990 to 2011						
Variable	Model 1		Model 2		Model 3	
	Coefficient	SE	Coefficient	SE	Coefficient	SE
Intercept	−6.088***	0.054	−5.575***	0.060	−5.582***	0.050
Language diversity	1.846*	0.727	2.537***	0.480	—	—
Percent foreign born	0.101***	0.020	—	—	0.102***	0.020
Residential instability	0.574***	0.064	0.634***	0.060	0.589***	0.060
Concentrated disadvantage	−0.298***	0.068	−0.383***	0.070	−0.368***	0.070
Percent urban	0.031***	0.002	0.034***	0.000	0.031***	0.000
Black	0.007	0.005	0.010*	0.010	0.009*	0.010
Hispanic	−0.023**	0.007	—	—	—	—
Percent males ages 15 to 24 years	−0.024	0.016	−0.021	0.020	−0.020	0.020
New York City	1.758	2.415	3.016	2.420	2.030	2.420
Year (1990 to 2010)	−0.051***	0.001	−0.051***	0.000	−0.051***	0.000

Note. SE = standard error.  
\* $p < .05$ . \*\* $p < .01$ . \*\*\* $p < .001$ .

of the year variable indicates that the risk of terrorist attacks significantly diminished over time.

To test the robustness of these findings, we ran a series of sensitivity analyses. First, although statistically our measures of language diversity, percent foreign born, and percent Hispanic did not seem to be influenced by multicollinearity (variance inflation factor values below 3), nonetheless given the theoretical significance of population heterogeneity to our arguments, we modeled terrorist attacks excluding first percent foreign born and percent Hispanic (Table 3, Model 2) and then excluding language diversity and percent Hispanic (Table 3, Model 3). In both instances, the original relationship with terrorist attacks is maintained (in the case of percent foreign born) or strengthened (in the case of language diversity).

Second, given that our analysis aggregates attacks that claim many lives (most notably the four coordinated attacks of September 11, 2001) with attacks that claim no lives (91%), we replicated the analysis excluding mass casualty attacks, operationally defined in this study as those that claimed the lives of 10 or more victims. When we dropped the eight events that took the lives of at least 10 persons, terrorist attacks were still most common in counties characterized by high language diversity, a high percentage of foreign-born residents, greater residential instability, a greater proportion living in urban areas, and low levels of concentrated disadvantage.

Third, given the extraordinary importance of the four coordinated attacks of September 11, 2001, we reanalyzed the data including a post-2001 measure to control for differences in the years after the attacks. When we added a dummy variable to our model that distinguished attacks after September 11, it was highly significant and, as expected, showed that fewer terrorist attacks occurred in the post-2001 era. But the only substantive change in the findings was that when we included a post-September 11 measure in the model, our annual measure of time was no longer significant.

Fourth, to examine the possibility that differences between international and domestic terrorist attacks influenced the results, we reanalyzed the data excluding the 14 attacks in which the perpetrators were known to be non-U.S. citizens. Excluding these attacks produced no substantive changes in the effects of the social disorganization measures on our findings.

Finally, because the characteristics of counties may have changed over the period included in our study, we conducted a parallel analysis substituting 2000 for 1990 census data in our model. In all five of these analyses, the results (available on request) were substantively similar to those presented in this study. In short, the results reported in this study are fairly robust.

## Discussion

Although there has been much concern about terrorism in the United States in recent decades and especially since September 11, 2001, and even though these fears have greatly altered

the structure of government, law enforcement, and attitudes toward individual privacy, little empirical research to date has examined patterns and predictors of terrorist attacks within the United States. In this article, we draw mostly on social disorganization theory to develop a set of hypotheses about the expected effects of several structural measures commonly applied in criminology on the frequency of county-level terrorist attacks. In general, our research suggests that county-level indicators of social disorganization have considerable utility in explaining attacks.

We should acknowledge limitations of our data and design that could be the basis for future research. Like much of the prior literature on social disorganization, our analysis is based on aggregate data. This means that we cannot speak to the potential influence of terrorism motivations or perpetrator characteristics. It also means that we cannot unambiguously determine whether, for example, the importance of population heterogeneity is a result of changes in policing, changes in the quality of informal social controls, terrorist targeting or behavior, or some combination of all these factors. Although the relatively small numbers of terrorist attacks put limits on statistical options, future research should explore attack patterns in smaller aggregate units such as cities and communities as well as focus on modeling radicalization and extremist behavior among individuals. Moreover, our conclusions are limited to a single country and a limited period of time.

Given the county-level nature of the analysis, we cannot distinguish between group-level and individual-level explanations. For this reason, we were careful to point out earlier that increased population heterogeneity could increase the risk of terrorist attacks both by increasing the chances that members of more heterogeneous populations plan and commit attacks and by reducing the effectiveness of informal and formal social control in more heterogeneous communities to prevent attacks. Thus, it could be that more heterogeneous counties are targeted more frequently by terrorists, that terrorist attacks are more often committed by those living in more heterogeneous counties (either by the more heterogeneous members of those communities or by others), that counties characterized by high levels of population heterogeneity raise unique challenges for law enforcement resulting in less effective prevention, or that a combination of these factors causes an increase in terrorist attacks. In fact, evidence supports several of these possibilities. For instance, Disha, Cavendish, and King (2011) showed that county-level hate crimes directed against Arabs and Muslims significantly increased after the September 11 attacks. Relatedly, Clarke and Newman (2006) argued that because terrorist operations are resource dependent, efforts to maximize efficiency without increasing the risk of capture suggests that groups will seek to minimize the distance traveled between events, and to the extent that foreigners are perpetrating attacks, they are more likely to do so within areas with heavy concentrations of other foreigners (see also Townsley et al., 2008). Geographic proximity might also make recruitment of new members easier and facilitate the dissemination of ideology.

Finally, much research (Chan, 1997; Greene and Herzog, 2009; Leonard, 2003; Nguyen, 2005; Tyler, Schulhofer, and Huq, 2010) has shown that policing success depends

on winning the trust and support of the local community and that population heterogeneity is likely to provide a hindrance to gaining such trust (Hill, Hubal, and Gowen, 2010). Communication difficulties with officials are often cited to explain why natural disasters have significantly greater impact on immigrant than on nonimmigrant communities (Khasu, Busch, and Latif, 2005; Marsella and Christopher, 2004; Shiu-Thornton, Balabis, Senturia, Tamayo, and Oberle, 2007). Research (Davis and Miller, 2002; James, Hawkins, and Rowell, 2007; Marsella, Johnson, Watson, and Gryczynski, 2008) also has shown that compared with native-born residents, immigrants are less likely to cooperate with authorities, including police (although see Kirk, Papachristos, Fagan, and Tyler, 2012). Many immigrants to the United States come from communities abroad in which there are few incentives for cooperating with police. Menjivar and Bejarano (2004) and others (Brandl, Frank, Worden, and Bynum, 1994; Culver, 2004; Davis and Miller, 2002; Rosenbaum, Schuck, Costello, Hawkins, and Ring, 2005) showed that previous negative experiences with police strongly influence current attitudes. Future research examining connections among population heterogeneity, participation in terrorist attacks, and law enforcement, both in the United States and in other countries, would be useful for untangling these complex relationships.

Also in line with both social disorganization perspectives and a good deal of research on ordinary crime, we find that our measure of residential instability is consistently associated with a high frequency of terrorist attacks. The common argument (Bellair, 1997; Sampson and Groves, 1989) that a high level of mobility in neighborhoods destabilizes communities by weakening social ties, impeding communication, and undermining the ability of residents to establish and uphold norms seems to work as well in explaining community-level instances of terrorist attacks as it does in explaining more common types of crime. Moreover, the arguments made in this article about the connection between population heterogeneity and law enforcement also apply to residential instability and law enforcement. There is much reason to believe (Greene and Herzog, 2009; Weisburd and Braga, 2006) that compared with their effectiveness in highly stable communities, police are less effective in communities undergoing rapid population change.

Also in support of our hypotheses and much criminology research on ordinary crime (Markowitz et al., 2001; Sampson and Groves, 1989), we found that the proportion of the population living in urban areas was consistently related to a high frequency of terrorist attacks. Interestingly, even though connections between urbanization and terrorist attacks are widely assumed, we could find few systematic tests of the relationship in the empirical literature. It would be useful to know more about the specific characteristics of urban areas that are responsible for this elevated risk. In particular, are urban areas attacked more frequently because those planning and executing attacks are more likely to live in urban areas, because informal and formal social control are less effective in urban areas, because they provide a wider range of symbolic targets, or because of some combination of these possibilities?

Although our results support the prediction that communities with greater population heterogeneity and more residential instability will be at higher risk for terrorist attacks, our findings were contrary to social disorganization theory predictions about the impact of concentrated disadvantage. In contrast not only to social disorganization theory but also to most prior research on ordinary crime, we found that concentrated disadvantage was associated with lower rather than with higher frequencies of attacks. As noted, this finding is in keeping with the empirical literature on terrorism (Krueger and Maleckova, 2003; LaFree and Ackerman, 2009), which generally has shown that terrorists are not drawn from poor communities and that those who participate in terrorist actions are, if anything, somewhat better educated and more prosperous than the population as a whole. More research on how economic and social well-being are related to willingness to participate in terrorism would be useful. More generally, these differences between social disorganization measures and terrorist attacks compared with the findings in prior research examining social disorganization measures and their connections to ordinary crime suggest the value of research (Deloughery, King, and Asal, 2012) exploring in greater detail the similarities and differences between terrorism and other forms of crime. For example, it could be that the prime targets of terrorists and ordinary criminals are fundamentally different: Although ordinary criminals may operate in their own neighborhoods (which might often be characterized as high in concentrated disadvantage), perhaps those engaged in terrorist attacks are more often drawn to wealthy counties that contain targets whose destruction will cause great impact.

## Conclusion

Despite growing research and policy interest in terrorism in recent years, few empirical studies to date have examined predictors of terrorist attacks in the United States and even fewer have tested major theoretical explanations for these attacks. Our results show that county-level terrorist attacks in the United States over the last two decades can be predicted in part on the basis of variables commonly included in criminological tests of social disorganization theory. As Shaw and McKay (1942) argued nearly a century ago, crime and deviance are more common in areas characterized by population heterogeneity, residential instability, and high urbanization. Our results suggest that researchers and policy makers should focus on the underlying connections of measures of social disorganization, participation in terrorist attacks, and law enforcement.

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### APPENDIX: Bivariate Correlations Between Explanatory Variables

Variable	1	2	3	4	5	6	7	8	9
1 Language diversity	—	0.673**	0.076**	0.295**	0.290**	−0.134**	0.715**	−0.066**	0.148**
2 Percent foreign born		—	−0.020	0.420**	0.406**	−0.078**	0.613**	−0.003	0.349**
3 Concentrated disadvantage			—	−0.042*	−0.081**	0.485**	0.173**	0.162**	−0.037*
4 Residential instability				—	0.590**	0.029	0.163**	0.200**	0.412**
5 Percent urban					—	0.072**	0.211**	0.124**	0.712**
6 Black						—	−0.116**	0.151**	0.126**
7 Hispanic							—	−0.030	0.031
8 Percent males ages 15 to 24 years								—	0.096**
9 Total population									—

\* $p < .05$ ; \*\* $p < .01$ .

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# POLICY ESSAY

## CORRELATES OF TERRORIST ATTACKS IN THE UNITED STATES

### Policing in an Omnicultural Environment

#### Population Heterogeneity and Terrorism Prevention

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LaFree and Bersani (2014, this issue) advance an important dialogue concerning the prediction of future terrorist events. Enemies of the United States abound, and the scope and boldness of terrorist organizations is trending upward. Major successes have eluded terrorists in the United States since 2001; however, there have been many thwarted attempts and some minor events. Domestic terrorism and self-radicalized perpetrators (such as the Tsarnaev brothers) represent continuing and perhaps growing threats. Because terrorism is manifested as a local act, understanding local predictors has important deterrence and prevention implications. This policy analysis extends the implications of the extant research and considers the relevance for law enforcement agencies and decision makers.

LaFree and Bersani (2014) make a compelling case for the importance of understanding local variables as they predict terrorist events. Terrorism, much like crime, is often predicated on locale (Behlendorf, LaFree, and Legault, 2012; Johnson and Braithwaite, 2009; LaFree, Dugan, Xie, and Singh, 2012). Utilizing social disorganization theory (Shaw and McKay, 1942), which is a locus-specific theory, to predict locus-specific terrorism events, is both novel and important. An unusual aspect of this research is that it does not try to predict “who” will become a terrorist or “why” they threaten, but it focuses on “where” events are likely to occur by assessing the attributes of jurisdictions (counties). This policy essay seeks to build on that thesis and extends the policy implications by focusing on the community attributes that could either exacerbate or inoculate a jurisdiction’s potential for terror groups’ success.

LaFree and Bersani (2014) has several important implications that are accepted at face value. Their major implication, which serves as the basis of this policy essay, is that terrorist attacks can be predicted based on local demographic variables, and urban areas with greater population heterogeneity and language diversity are at a higher risk for terrorism.

LaFree and Bersani (2014) specify several important policy implications for police agencies. They astutely note research indicating that population heterogeneity can hinder gaining the trust and support of residents (Hill, Hubal, and Gowen, 2010). Furthermore, they note the difficulties police experience with recent immigrants in terms of communication (James, Hawkins, and Rowell, 2007) and emergency response (Marsella and Christopher, 2004). LaFree and Bersani encourage further exploration of the relationship among population heterogeneity, terrorism, and the role of law enforcement.

### **Police Role in Terrorism Prevention**

Although little published research has directly addressed terrorism prevalence and prevention, the role of law enforcement, and the complications of population heterogeneity, several published studies have been linked to these issues. Many authors have noted the complexity of merging the traditional police role with terrorism prevention or counterterrorism (Bayley and Weisburd, 2009; Mastrofski and Willis, 2010). In addition to their traditional responsibilities, police are expected to serve as the “eyes and ears” of federal agencies (White, 2002). Any number of variables influences a police organization’s orientation toward preparedness, including organizational philosophy (Kilburn, Costanza, Metchik, and Borgeson, 2011), emphasizing community policing (Randol, 2012), agency size and funding (Pelfrey, 2007), and perceptions of terrorism risk and consequences (Burruss, Giblin, and Schafer, 2010; Haynes and Giblin, 2014).

When studying terrorism, the inclination is to look at terrorists. Although some terrorists operate alone, others receive material support from accomplices. In an innovative study of the prosecution of those accomplices, Harms (in press) examined 59 cases encompassing 255 individuals that were charged in federal courts (between 2001 and 2007) under the Antiterrorism and Effective Death Penalty Act of 1996. Those charged tended to be male, educated (most had attended college, and nearly one third had graduate degrees), of Arabic descent (80% of defendants), and with families and jobs. Typically, these individuals live and function in American society but endorse a terrorist ideology and actively support terrorist efforts by providing money, weapons, and other resources. These findings have important implications, particularly relative to population heterogeneity, for police as they seek to identify, investigate, and prevent terrorist efforts. Police must build inroads in ethnically diverse communities if they wish to gain intelligence regarding potential terrorists. These access paths are hindered when ethnic groups distrust the police.

Although LaFree and Bersani (2014) wisely noted the role of law enforcement in ameliorating some of the negative effects of population heterogeneity, the main objective of this policy essay is to raise the level of enquiry from organizational levels, although important, and to begin the policy conversation at a grander level of cultural and social diversity.

## Addressing Population Diversity

Over the past two decades, European countries have experienced a dramatic influx of Muslim immigrants as North Africans have moved into France and Turks to Germany, bringing the Muslim population of Europe to 20 million (Moghaddam and Breckenridge, 2010). This rapid influx of peoples with different cultural mores, heritages, and religions has created intergroup tensions. Psychologists Moghaddam and Breckenridge noted that two approaches to integration have received the most attention in the scholarly literature: assimilation and multiculturalism (Fowers and Davidov, 2006; Verkuyten, 2005). *Assimilation* is the absorption of immigrants into a broader culture, much like a melting pot philosophy. This approach has characterized American immigration for many decades (Moghaddam, 2010). The traditional alternative to assimilation is *multiculturalism*, which is predicated on retention of strong ethnic identity and cultural concepts.

Moghaddam posited a third approach, *omniculturalism*. As he said in 2009 (p. 337):

Continuing intergroup conflicts, fundamentalism, and terrorism suggest a need to rethink traditional policies for managing diversity. The alternative policy of omniculturalism is put forward as a longer term solution to fundamentalism and intergroup conflict.

As he later described, omniculturalism advocates a two-stage socialization process (Moghaddam, 2010). During stage one, the object is to identify commonalities among people that supersede race, religion, and culture. In stage two, “intergroup differences and distinctiveness are introduced” (Moghaddam and Breckenridge, 2010: 2). Omniculturalism is predicated on superordinate goals—those goals that supersede any specific race or class.

Through survey research of a nationally representative population, Moghaddam and Breckenridge (2010) found broad support for omniculturalism, favored over assimilation or multiculturalism. Those endorsing omniculturalism also reported lower fear of terrorism. Moghaddam and Breckenridge (2010) suggested that if communities endorse and implement an omnicultural approach, by noting the commonalities of those in America rather than by celebrating our differences, then this approach will likely diminish intergroup tensions. If communities take steps to minimize intergroup tensions, then they could reduce ethnically motivated violence, perceived disparity, and the hatred that foments radicalization. As an example, Pickett, Baker, Metcalfe, Gertz, and Bellandi (2014) found that quality contact between Israeli Jews and Arabs led to a greater desire among Jewish residents for compromise and reduced fear of a Palestinian threat.

Implementing omniculturalism is a two-stage process in which identification of commonalities precedes recognition of distinctiveness. The identification of commonalities can occur through a variety of approaches. Moghaddam (2010) described efforts such as providing a voice to all groups (perhaps through community forums and city/policy advisory councils), presenting formal recognition of ethnic and cultural groups within communities,

and explaining superordinate goals (opportunities for one's children, social justice, access to employment, etc.).

The second stage of omniculturalism is predicated on noting, and celebrating, group distinctiveness, something that astute politicians do instinctively. Social identity theory (Tajfel and Turner, 1979) explains individual behavior as people self-identify into groups, and their behavior is largely an artifact of perceived group status differences. The *Black-is-beautiful* movement of the 1970s was noted by Tajfel and Turner (1979) as an example of social identity theory, in which persons self-identified with a broader movement. Social identity theory has important implications for terrorism, particularly with disenchanted or marginalized persons and groups who may take steps toward self-radicalization or who used perceived disparity as the basis for radicalizing terrorists (Schwartz, Dunkel, and Waterman, 2009). Recognizing and celebrating group distinctiveness can obviate the perceived group disparity that social identity theory notes as a foundation for terrorist ideology.

Omniculturalism is not without critics and critiques. Bilewicz and Bilewicz (2012) questioned who is entitled to "define the content of humanity" and identify superordinate goals (p. 334). It falls to communities, prompted by community leaders (potentially including police leaders), to identify superordinate goals. Community policing and omniculturalism are thematically linked as they both seek to leverage community values, norms, and cultural imperatives to reduce violence, conflict, and discord. Dahrendorf (1959) would embrace both in addressing social exclusion and inclusion based on authority.

### **Policing for Omniculturalism**

LaFree and Bersani (2014) note the importance of local variables such as population heterogeneity, urban concentration, and language diversity as predictors of terrorism. The juxtaposition of cultural diversity with terrorism has important implications for police, particularly relative to the prevention of terrorism. Local police can do little to influence foreign policy, international conflicts, or economic deprivation in other countries. They can, however, address cultural diversity and subsequent intergroup tension in their locales. Many of the lessons from community policing are applicable to terrorism prevention (Pel-frey, 2005). The concept of community policing is predicated on a proactive police agency that uses community interaction that informs the prevention of crime (Cordner, 1997). Local law enforcement should not abdicate all terrorism prevention responsibilities to federal agencies. Although those federal agencies have international intelligence networks, vast data mining efforts, and substantial budgets, local police have local knowledge, possess the opportunity to build local alliances, and are heavily vested in local success. For several years, London police have built alliances with Muslim communities through police contact units (Spalek and Lambert, 2007). These relationships have produced important community relations outcomes, although they raise complicated political questions. London police were confronted with a report that one of their allies had ties to al-Qaeda and radical Islamists (Fielding and Tahir, 2005). Differentiating between safe and dangerous

allies will be a difficult issue as police agencies seek to build productive and meaningful relationships.

The following two hypotheticals present police efforts, or lack thereof, in two imagined cities—Utopia and Dystopia. Both are characterized by diverse populations, unemployment, urban concentration, racial and ethnic tension, and a history of conflict between the police and the population. These could be any number of medium-to-large cities in the United States.

In Utopia, the police adopt proactive efforts to engage the community in a variety of initiatives. Following the logic of omniculturalism, police work with schools, community groups, and cultural bodies to identify and espouse superordinate goals that connect peoples irrespective of their racial, ethnic, or religious backgrounds. For example, the city of Chicago recently experienced great public support for its Little League national champions. That support went beyond race and religion, unifying peoples of many backgrounds (Byrne, 2014). To unite people, police leaders could encourage community advisory councils populated by representatives of different religions, ethnicities, immigrant groups, and public/private agents. The community advisory council represents a forum for both conveying information to the police and channeling information from the police to the community (on new initiatives, concerns, needs, etc.). The police work through the representatives on the community advisory council to create and cultivate local intelligence networks that ask leaders to identify persons who might become threats. Those individuals can then be approached by both the police and community leaders to address concerns and, if possible, interrupt the radicalization process. The police also encourage, and take part in, community activities that celebrate cultural heritage (i.e., ethnic festivals). Consistent with community policing, these goals are shared by the other departments in the city—fire, EMS, public works, emergency communications, and public health.

In Dystopia, the police are primarily a reactive agency that addresses traditional crime as evidenced through calls for service. Interaction with the community happens pursuant to investigations and standard police work. When (or if) community leaders come to agency leaders to voice their concerns, the issues are investigated and action is taken if criminal activity is evident. Addressing cultural diversity and potential conflicts is not viewed as a police responsibility until it becomes a crime. Similarly, other departments “stay in their lanes” and address only those narrowly defined events for which they are responsible. Symptomatic of the community problems, these departments and agencies are in constant conflict with one another for fear of losing resources and power.

If a major event happens in Utopia or Dystopia, such as the shooting of an unarmed minority teenage male by a White police officer, the repercussions of the police approach to diversity will be most evident. In Utopia, such an event would lead to a press conference with police, elected leaders, and community leaders, calling for a joint investigation into the shooting. After the investigation, irrespective of whether the shooting was founded or unfounded, the outcome and all sanctions would be made public and steps would be

taken to repair any damaged community relations. Key to all of these events is the level of trust and communication, particularly communications representing respect for cultural differences. The goal, as Moghaddam (2012) stated, is a community “in which people are knowledgeable about, and give priority to, human commonalities, but also leave some room for the recognition and further development of group distinctiveness” (p. 306).

In Dystopia, the same event would likely lead to violent conflict between the police and the community as the police might be viewed as disconnected from the community, particularly the minority community. That is, the community may view the Dystopia police as enforcers of the law rather than as persons mutually pursuing community stability. Conflict and tension between the police and community members, manifested as demonstrations, would likely produce a backlash from the police including militaristic approaches to quell protests rather than hearing from the complainers. These community relations issues play a significant role in police/community trust and subsequently the development and fruition of intelligence networks. Although Utopia and Dystopia are hypotheticals, it would not be difficult to find major American cities that closely mirror each. When the Utopia and Dystopia hypotheticals are extended to a terrorist threat, either “homegrown” or international, similar patterns emerge. Those plotting the attack are likely part of self-characterized aggressive “out-groups” either embraced and included or ignored and allowed to foment their perceived (or real) lack of inclusion.

The purpose of presenting these hypotheticals is to highlight the role of police in addressing population heterogeneity and the potential repercussions of action, or inaction. Given the statistical significance of population heterogeneity as a predictor of terrorism, police who serve disparate communities need to assess their own efforts and initiatives relative to cultural, racial, and ethnic diversity. This is about much more than simply having a demographically representative police agency. The premise of omniculturalism provides an action plan for police to secure the support of ethnic groups, thereby enhancing opportunities for intelligence and, consequently, terrorism prevention.

## Conclusion

The police role has always been complicated and amorphous, particularly during periods of change. The current paradigm of policing is characterized by traditional responsibilities coupled with information, intelligence, community policing, and terrorism prevention. The factors that predict terrorism are gaining clarity, as shown by LaFree and Bersani (2014). Urban concentration, language diversity, and population heterogeneity were linked significantly to terrorist attacks. Local government and police decision makers can leverage these findings into action plans that could obviate future terrorist activities. Those action plans would also strengthen communities and reduce intergroup tension.

One path to addressing intergroup tension is omniculturalism, an alternative to traditional integrative approaches of assimilation (the melting pot approach) and multiculturalism (where enclaves of ethnicities exist concurrently). Omniculturalism (Moghaddam,

2010) suggests that first we should point to the commonalities that unite peoples (superordinate goals) and then celebrate the differences that make groups distinctive. This approach has several repercussions, including a reduction of intergroup tension and reduced fear of terrorism. Police involvement in building omniculturalism should enhance trust in the police and provide in-roads for the collection of intelligence.

Few American cities have the ethnic diversity of New York City or the risk of terrorism. In 2008, the New York Police Department created the NYPD Cricket League—a police-sponsored sports league for teenagers. The purpose of this league was to make in-roads with central Asian (particularly Muslim) communities by winning over kids, their parents, and subsequently communities (A. Kosseim, personal communication, July 2010). Many teams were coached by NYPD officers of Asian descent. Not every city can, or should, support a cricket league, but innovative initiatives, led by police, to ameliorate intergroup tension and build trust will almost certainly yield valuable community relations, intelligence, and long-term benefits, ideally including the prevention of terrorism and crime.

Moghaddam and Breckenridge (2010: 14) concluded their research with the following:

The findings of this study highlight the value of exploring alternative policies for managing diversity, as well as critically re-thinking links between both alternative and traditional policies and homeland security.

LaFree and Bersani (2014) could have ended their article with this same statement. Whether omniculturalism is the desired philosophy or community policing the desired vehicle for implementing it, the findings on the relationship between diversity (heterogeneity) and terrorism demand strategic attention and policy action.

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## Developing Policies for Countering Terrorism

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The conclusion that county-level predictions of terrorist attacks can be developed using attributes of social disorganization theory provides a tempting basis for developing policies that would focus counterterrorism strategies on those counties that have the highest levels of heterogeneity, residential instability, and urbanization. A somewhat similar concept emerged in the immediate years after the creation of the Department of Homeland Security as a response to the tragedies of September 11, 2001 when the department struggled to define the areas in the nation of “highest risk” of experiencing a terrorist attack. Lacking this kind of competent research and having very little basis for determining the risk to any given county or city, the risk assessments were not based on much more than guesswork, but there was some notion of grading locations according to at least the opportunity to make major negative impacts on America’s economy or sense of security. New York City appeared on most lists as the greatest risk and received federal funds accordingly.

The decision to focus resources on the most affected counties may not be the wisest choice. Our policy options today include defining what we want intelligence and law enforcement agencies to do regarding terrorism, deciding how we want to draw boundaries on what they do in support of their missions, and allocating resources to the most productive and effective efforts.

Policy issues regarding the nation’s response to terrorism are much different today than in the immediate aftermath of 9/11. Our failure as a nation to predict these events was to some extent overshadowed by our national inability to respond coherently, so that much of the policy debate was then focused on how we might better respond to such disasters. We spent considerable energy and applied funding around issues such as communications

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interoperability and building state and local capacity to respond to major catastrophic events. This period was also a time of rebuilding our national intelligence apparatus to seek a more reliable form of knowing when such events may occur. A lot of this work was based on the idea of building capacity at all levels to respond to a distinctive set of scenarios that might befall us in the future.

In recent years, policy makers across the government have come to understand that the most important purpose of our counterterrorism work is prevention. Although our ability as a nation to respond to major catastrophic events is still important, the development of a national capability to detect and deter terrorist actions is the more significant focus of all counterterrorism policy. We have, therefore, been engaged in the development of national intelligence-gathering efforts that are designed to help us identify behaviors or events that can be precursors to terrorist activities so that interventions become possible. An example is the National Suspicious Activity Reporting Initiative, which is aimed at educating local police and other public safety workers to report suspicious activity (behaviors) that may represent a precursor. This program calls for local officials to report such observations to state fusion centers and, where justified, to both state and national agencies that will be empowered to take action.

The challenge in this new emphasis is that the sharing of information regarding behaviors that might or might not be linked to a potential terrorist act raises serious policy questions. Such information is radically different from that which is collected and shared regarding the commission of a crime for which there is a well-defined societal sanction that justifies the intervention as well as constrains the use of such data. Many precursor events that have been identified as possibly being linked to a terrorist incident are not conclusive in their own context. Photographing the infrastructure of a major bridge could be a student architect's class assignment or the first step in planning a bomb placement, but the mere observation of the behavior tells us little about the purpose of the act.

Considerable work still needs to be done in developing the policies regarding the collection, dissemination, and application of such data about behaviors that might or might not link to a terrorist event. We have not determined the value of collecting such information, nor have we defined in legislative terms the specific rules about discarding it after some period of time or a finding that it is not relevant. Furthermore, we have not conceived consistent penalties for misuse or abuse of such data.

The research on the incidence of terrorist attacks in counties suggests that it might be less fraught with political overtones to impose more surveillance or other policies in the counties that are characterized by the social disorganization factors because they are not representative of the kinds of places that are characterized by higher crime rates—the socioeconomically disadvantaged counties. Because the counties where terrorist attacks seem to be more prevalent are more affluent, possibly more educated, and so on, there might be less of a political risk of focusing attention on potential behaviors.

In this post-Snowden era, the opposite is likely to be true. Civil rights activists have already mounted objections to the National SAR Initiative and are continually concerned about police intelligence gathering as an invasion of privacy. It is highly likely that these concerns will influence policy at the national level even more as incidents such as the Michael Brown shooting raise questions about the militarization of policing. The calls for ending the provision of surplus military equipment to police agencies could easily expand to the equipment used in surveillance activities. We also have seen instances of police agencies overreaching in their attempt to collect data on potential terrorist activities.

Both the U.S. Congress and the president have recognized the concern over such policies, and they have implemented by statute and executive order the requirement to evaluate closely any such policies in the light of protecting civil rights and liberties. The program manager for the Information Sharing Environment under the Director of National Intelligence must confirm that the policies that are related to information sharing also include safeguarding and the protection of civil rights.

The geographic dispersion of terrorist attacks shown by LaFree and Bersani (2014, this issue) reveals another principle that must be considered in the development of policy on this issue. As LaFree and Bersani show, attacks have taken place in every state except Delaware, Hawaii, and Alaska. Digging a little further shows that many of the worst terrorist attacks actually were planned and developed by terrorists residing in or passing through small suburban or rural communities. The opportunity to detect behaviors potentially linked to terrorism in these cases is not in the major urban counties. As an example, consider the wanderings through small towns in the United States by Mohamed Atta, who crashed the plane into the North Tower of the World Trade Center.<sup>1</sup>

Also, it is incumbent on policy makers to consider the geography in a bit more detail than the data allow given the small number of incidents. A body of thinking emerging now in policing circles indicates that it is more fruitful to develop strategies and policies around neighborhoods rather than around whole cities, let alone counties. Braga and Weisburd (2010) have shown that even within major cities, less than 5% of the areas in a city account for more than 80% of the crime, and no serious crime was observed in many areas within what might be characterized as a “crime-ridden city” (Braga and Weisburd, 2010). The same phenomenon is likely to be found in consideration of terrorist activity, particularly if we were to concentrate as much on where the planning and organizing of terrorist attacks take place as on the location of the resulting incidents.

The policies and strategies aimed at preventing crime including terrorism are different from the policies on the response to such events. As an example, many progressive police agencies recognized even before 9/11 that their relationships with specific ethnic groups and enclaves needed work, particularly if such relationships were to become helpful in preventing

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1. “Mohamed Atta.” Retrieved September 17, 2014 from [en.wikipedia.org/wiki/Mohamed\\_Atta](http://en.wikipedia.org/wiki/Mohamed_Atta).

crime as well as responding to it. The events of 9/11 emphasized the importance of true community-based policing models and the mandate of developing better relationships particularly in those communities that fit the characterization given by the model of social disorganization.

Having considered these issues in shaping policies in the counties most affected, it is still reasonable to allocate resources in proportion to the risk, and counties where the risk has been measurably higher should logically be the focus of a proportionally greater investment in prevention. Such actions as fully documenting the vulnerabilities of critical infrastructure, monitoring the purchasing of certain chemicals, deploying sensors capable of detecting weapons of mass destruction, increasing intelligence efforts, and training local police and other first responders to observe and report suspicious incidents are all obvious strategies that would seem to be worth more emphasis in counties where a higher incidence of terrorist attacks has been measured.

One of the most important policies to develop and apply is that information sharing between the affected counties and the various strategic and tactical units attempting to prevent these acts. Sharing knowledge about precursor events, radicalization efforts, methods and means employed to execute the attacks, and all other information that may shed light on the potential for further attacks must be shared in a timely fashion. A major move toward this end is the creation of the program manager for the Information Sharing Environment that seeks to work with federal, state, local, tribal, and international partners to make relevant information available to decision makers.<sup>2</sup>

The president's strategy for information sharing and safeguarding calls on the program manager for the Information Sharing Environment and every other agency involved in counterterrorism to make the right information discoverable and to make that information available to the right person at the right time (The White House, 2012). This ideal has not been realized, but the need is indisputable. Our ability to prevent terrorist acts, or crimes for that matter, will rest on our success in sharing the necessary information.

The most fearful terrorist threats to our society are widely acknowledged to be home-grown violent extremists committing acts of terror and cybercrime. As nation states become more sophisticated in cyberwarfare, we will observe events that we have not come close to anticipating as possible scenarios.

To help us prepare for this uncertain future, research on the origins of the previous attacks, taking the data down to the neighborhood level, would be very helpful. If we can better understand the social conditions (contexts) of the neighborhoods in which the planning of attacks was carried out, we may be able to define more accurately the kinds of precursor measurement and monitoring that could allow us to detect and prevent such

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2. For more information, visit [ise.gov](http://ise.gov).

acts. It will be essential to build collaborative efforts in the future and to learn to share the knowledge faster.

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