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DISORDER IN THE NEIGHBORHOOD
A Large-Scale Field Experiment on
Disorder, Norm Violation, and Pro-Social Behavior

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Abstract

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Chair of the Supervisory Committee:
Doctor Ross L. Matsueda
Sociology

A highly-visible paper, published in *Science*, using field experiments in a Groningen neighborhood found strong support for the broken windows hypothesis: disorder increases norm violations (Keizer et al. 2008). The study has been replicated in several other European countries. We attempt to replicate this study in the U.S. by embedding a mailbox field experiment in six Seattle neighborhoods that vary in social capital and collective efficacy. Our experiment places a lost letter with a visible \$5 bill near the mailbox. The treatment is graffiti and/or trash in the area. We examine three outcomes: passersby ignoring the letter, stealing the letter (a norm violation), or mailing the letter (a form of altruism). We use multinomial and nested logit methods to model the 2,786 cases. Results indicate physical disorder attenuates pro-social behavior while neighborhood collective efficacy and concentrated disadvantage are associated with rates of norm violation. Overall, we fail to replicate Keizer et al. but find evidence that disorder attenuates pro-social behavior.

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INTRODUCTION

One of criminology's most influential and controversial theories of the last few decades is broken windows theory (BWT), which asserts causal pathways from public disorder to serious crime (Welsh, Braga, and Bruinsma 2015). In the policy world, it is the theoretical basis for order maintenance policing—a law enforcement strategy that attempts to create or maintain social order by aggressively policing minor misdemeanors and norm violations—sometimes credited for New York City's precipitous crime decline. However, critics argue aggressive anti-disorder policing overwhelmingly targets poor minorities, contributes to mass incarceration, and injures public perceptions of the justice system (Harcourt 2001). In the academic literature, the theory has been subject to extensive empirical tests with equivocal findings (Welsh, Braga, and Bruinsma 2015). This relatively simple theory has evaded conclusive verification or refutation, as studies have produced evidence in both directions (Skogan 1990; Sampson and Raudenbush 1999; Xu, Fiedler, and Flaming 2005). This may in part be due to what Weisburd et al. (2015) describe as a “lack of conceptual clarity about the specifics of the theoretical model [which] has led to debate over how to appropriately test the broken windows thesis” (pp. 591).

Keizer, Lindenberg, and Steg's (2008) field experiments represent novel and compelling causal tests of the broken windows thesis, but they are limited in generalizability and neglect contextual effects. Thus from both academic and policy perspectives, a clear description and strong test of the causal pathways of broken windows theory, which acknowledges neighborhood context, remains much-needed to inform continuing debates. In this work, we first explain how Wilson and Kelling's (1982) original formulation of broken windows—in which feedback processes relate disorder and crime—makes it a daunting target for research, and then we describe a large scale field experiment to test the direct causal path between disorder

and crime in multiple neighborhoods.

Broken Windows Theory

First described in *The Atlantic* in 1982, Wilson and Kelling's broken windows theory (BWT) posits that disorder and crime are linked in a developmental sequence in which unchecked disorder spreads and promotes crime. According to BWT, physical and social disorder—such as the proverbial broken window or a loud group of unsupervised youths—signals an absence of social control. This disorder emboldens an individual presented with a criminal opportunity by signaling that the community is unwilling or unable to regulate itself. Thus the community is perceived as unlikely to stop or report a criminal act. Furthermore, for residents of the neighborhood, unchecked disorder increases fear of crime, and this fear leads residents to avoid unfamiliar faces and restrict their travel to safe spaces. Disengaged from the neighborhood, residents increasingly see responding to disorder as the duty of others; residents with resources begin to migrate out, and the informal social control that maintains a relatively consensual order in the community decays (Wilson and Kelling 1982). Together, the two interacting pathways of disorder relax both the self and social control that inhibit more crime in a neighborhood. This interrelationship is depicted in Figure 1.

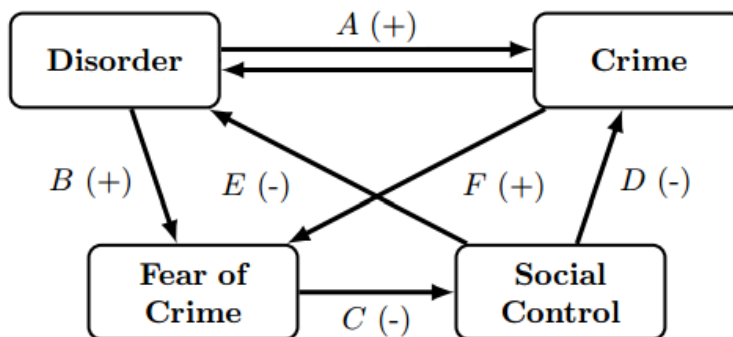


Figure 1: Broken Windows Theory

Arrow *A* represents the direct effect of disorder on crime caused by a reduction in a

potential offender’s perceived certainty of punishment. Disorder also acts through path *B*, elevating fear of crime in exposed residents. Fear of crime leads to disengagement and apathy, which reduce the willingness to engage in social control through path *C*. Crime is then increased via path *D*, in which reduced social control undermines actual certainty of punishment. This decline in social control also translates into reduced intervention against disorder, path *E*, which results in feedback loop *BCE*. Likewise, actual crime increases fear of crime, producing a second cycle—*FCD*—that further diminishes social control. In broken windows, these mutually reinforcing circular relationships drive the spiral of decay in neighborhoods and generate endogeneity that confounds empirical tests of the theory.

Disorder and Crime: Path A

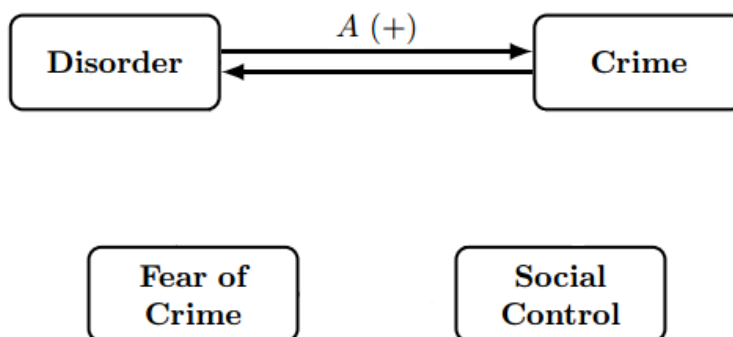


Figure 2: BWT: Disorder to Crime

The direct relationship between disorder and crime—Figure 2—represents emboldening of motivated offenders through a reduction in perceived certainty of sanctions: a neighborhood perceived as being unable to prevent disorderly conditions may be seen as even less likely to intervene against serious crime (Wilson and Kelling 1982). In the framework of deterrence, disorder promotes the commission of crime by signaling that the certainty of being sanctioned is low. Empirical evidence for the inhibitory effect of sanction certainty on crime is consistent and well established (see Nagin 2013 for a review). If disorder signals a lower probability of

apprehension, research on deterrence indicates a link should exist between the presence of disorder and criminal behavior. This effect of deterrence provides a plausible mechanism for both the spread of disorder and escalation of seriousness of minor norm violation to serious crimes. A general reduction in perceived certainty could produce continuous emboldening or growing severity in offenses as the threshold for commission of more serious crime is eroded by increasing signals of low social control. Likewise, disorder may also reinforce itself by signaling a low marginal impact of adding more disorder on apprehension or sanctioning: “one unrepaired broken window is a signal that no one cares, and so breaking more windows costs nothing” (Wilson and Kelling 1982:5).

Evidence from previous studies for the direct path between disorder and crime is equivocal, mainly observational, and often the subject of debate (Welsh et al. 2015). For example, Skogan’s (1990) analysis depicted a robust relationship between disorder and crime, in which disorder mediated the effect of social and economic factors on crime. In a critical reanalysis, Harcourt (1998) showed that Skogan’s results were reliant on the inclusion of a small number of neighborhoods with high disorder and crime, though others found this critique unconvincing (Eck and Maguire 2006). Most research on the effects of disorder on crime has come from studying order maintenance policing interventions and not attempted to separate direct and indirect effects of disorder on crime (Braga et al. 2015).

Disorder and Crime to Fear of Crime: Paths B and F

Empirical research offers extensive support for the link between fear of crime—usually measured by survey—and both resident-perceived and researcher-observed disorder (for a review, see Skogan 2012). Additionally, there is consistent evidence that disorder has a greater impact on fear of crime than actual crime or victimization in neighborhoods (Bideman et al. 1967; Xu et al. 2005; Skogan 1990). From the perspective of broken windows, the condition of a neighborhood provides individuals with information that shapes their behavior: “In pluralistic cosmopolitan areas where interaction with strangers is frequent or commonplace, we cannot be advised about those we meet by personal knowledge, history, or perhaps

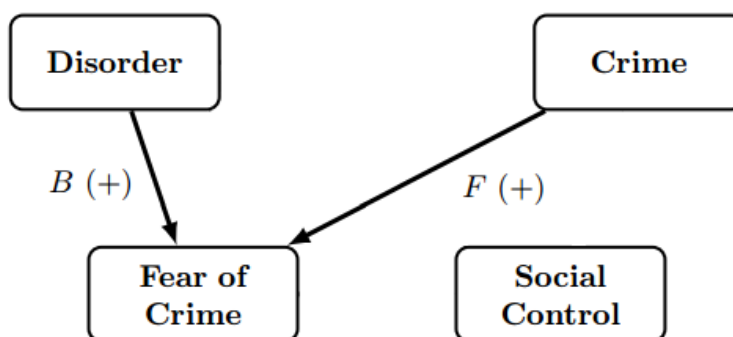


Figure 3: BWT: Disorder, Crime to Fear of Crime

even reputation. Instead, we take our cues from activities observed on the street and shape our public behavior accordingly if we are the strangers” (Kelling and Coles 1996:15). That is, disorder may impact behavior—possibly inhibiting adherence to some social norms even when the exposure is short in duration, as in Keizer et al. (2008)—and possibly even more so when out of place, such as in neighborhoods with high social capital capable of maintaining low levels of disorder (Keuschnigg and Wolbring 2015). Over longer periods, when disorder is present, “many residents will think that crime, especially violent crime, is on the rise, and they will modify their behavior accordingly. They will use the streets less often, and when on the streets will stay apart from their fellows...” (Wilson and Kelling 1982). This is the essence of the relationship between disorder, crime, and fear of crime. The diffuse nature of disorder’s effects mean that they manifest as an externality from the tolerance of norm violations: “in cases where behavior that is tolerable to one person is intolerable to many others, the reactions of the others—fear, withdrawal, flight—may ultimately make matters worse for everyone, including the individual who first professed his indifference” (Wilson and Kelling 1982:16). Fear of crime emerges and grows when indifference to disorder permits its accumulation and eventual pervasiveness.

The fact that residents experience and react to crime and disorder similarly—that is with fear—does not contradict the division between the two in broken windows, as Wilson and Kelling note that they can be equally frightening and unpleasant, particularly to the

vulnerable: “outside observers should not assume they know how much of the anxiety now endemic in many big-city neighborhoods stems from a fear of ‘real’ crime and how much from a sense that the street is disorderly, a source of distasteful, worrisome encounters” (1982:4). Consequently, as seen in paths *B* and *F*, both disorder and crime impact social control through fear of crime. In BWT, “fear of crime” is primarily about *fear* not crime, about what *can* happen not what does. It is fear of the unexpected, awareness of vulnerability, and alienation from one’s surroundings, which disempower able guardians, drive families out of neighborhoods, and circumscribe the routine interactions of residents from which collective efficacy emerges.

Disorder, Fear of Crime, Social Control, and Crime: Path BCD

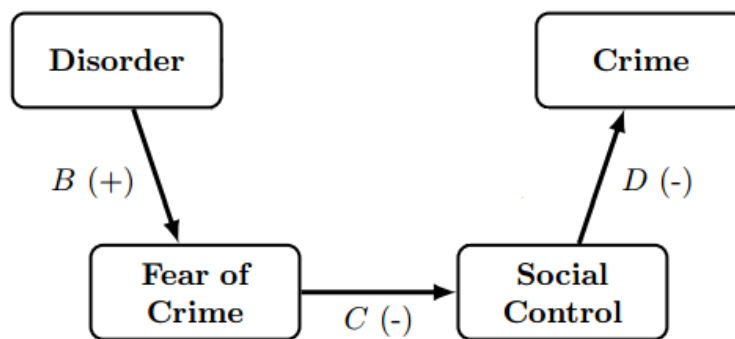


Figure 4: BWT: Indirect Path from Disorder to Crime

Just as disorder and crime are similar with regard to their impact on fear of crime, so too are they as outcomes of neighborhood characteristics such as social control; the indirect pathway between disorder and crime—likely the dominant pathway—hinges on a negative relationship between fear of crime and social control. Social control is used here in the classical sense: social control is the capacity of a group to regulate itself in accordance with shared norms (Janowitz 1975). In the case of BWT, it is assumed that residents share a desire for a neighborhood free of crime and disorder, which research finds holds nearly universally in

the United States, even among perpetrators of crime (Kornhauser 1978; Hearn 1997; Skogan and Harnett 1997). The interactions that produce social control in the neighborhood are primarily informal interactions, such as verbal censuring or “calling your parents.” While the police do play an important role, they “cannot, without committing extraordinary resources, provide a substitute for that informal control” (Wilson and Kelling 1982). It is important to note that it is the threat of these actions, rather than their use, that forms the main deterrent to proscribed behaviors (Shaw and McKay 1942; Sampson 2012). As Wilson and Kelling (1982) note, the safest and best regulated areas often lack any obvious signs of social control—such as police patrols—because the certainty of intervention against offenders is high. For residents to take actions that promote social control, they require both social ties that make informal interventions effective and freedom from the threat of retaliation.

By circumscribing the social behavior of residents and evoking feelings of danger, fear of crime impedes the routine interactions between group members that inhibit crime and disorder: “In response to fear people avoid one another, weakening controls” (Wilson and Kelling 1982:8). Kelling and Coles summarize this relationship of disorder, fear, social control, and crime: “those who might seek to oppose or curtail an individual act become increasingly fearful and less likely to do so as the number of perpetrators and acts grows. At some point disorderly behavior reaches a tipping point so that fear on the part of citizens turns to avoidance and retreat, and serious crime begins to make inroads into the neighborhood” (Kelling and Coles 1996:35). As the process advances, disorder and fear of crime further destabilize these neighborhoods by fostering outmigration of residents able to move, compromising the reputation of the neighborhood, which in turn discourages in-migration and investment. This residential instability undermines social control through the dissolution of social ties (Skogan 1990; Sampson 2012). Over the long term, this self-sustaining pathological process of withdrawal and disinvestment destroys the capacity of a neighborhood to regulate itself or attract outside assistance.

Disorder

The definition of disorder is critical to broken windows theory and long a point of contention for critics, due both to its ambiguous distinction from crime and its ostensible subjectivity (Sampson and Raudenbush 2004). Key works in the broken windows literature have addressed these challenges. Kelling and Coles (1996:4) describe disorder as “behavior that violates widely accepted standards and norms of behavior, and about which a broad consensus exists, in spite of racial, ethnic, and class differences.” Skogan (1990) distinguishes disorder from crime by the diffuse and cumulative nature of its harm: Crimes are isolated actions or events, while social disorder is episodic. Physical disorder is an ongoing state, often non-criminal, which is much more frequently encountered. It is evidence of flagrant disregard for people and public spaces—and of unpoliced activity—which differs widely in form but is reacted to similarly by residents. Typically lacking individual victims, its damage comes as an emergent property of variety and volume rather than isolated instances. The problem with disorder is not harm from individual acts but rather “the fear that such disorderly behavior engenders in the local community when it reaches a critical mass” (Kelling and Coles 1996:16). Nonetheless, Kelling and Wilson (1982) indicate that isolated disorder provides an immediate signal of the absence of social control to motivated offenders, inducing criminal behavior; the norm-inhibiting effect of localized physical disorder described by Keizer et al. (2008) presents evidence of this local effect. Our experiment focuses on testing the causal link between disorder and crime, and thus attempts to elicit the immediate emboldening—or norm-inhibiting—effect of disorder rather than the long-term effect of Skogan’s (1990) “spiral of decay.”

Critics have also attacked the concept of disorder as being subjective and entangled with factors like race and class (Harcourt 2001). Evidence indicates that residents disagree over what kind of disorder exists in their neighborhoods and these disparate evaluations are tied to resident characteristics, activities, and neighborhood attachment (Wallace, Louton, and Fornango 2015). In other words, subjective and objective measures of disorder may be dis-

connected (Sampson 1999). Nevertheless, there is generally consensus among individuals on what constitutes disorder regardless of ethnicity, class, or other characteristics—including vandalism and litter specifically (Skogan 1990)—and when individuals feel it is present in some form (Wallace et al. 2015; Perkins and Taylor 2002). Recognizing the importance of distinguishing crime from disorder, this study follows Keizer et al. (2008) by operationalizing disorder with litter and crude graffiti—minor but unambiguous signals of disregard for property and community—and crime with theft of a letter containing money. One might argue that letter theft is also a minor crime of simple disregard for others; however in this case the theft is for clear material gain, likely to provoke a police response if observed, and potentially subject to considerable penalty including imprisonment (United States Code).

Broken Windows as a System

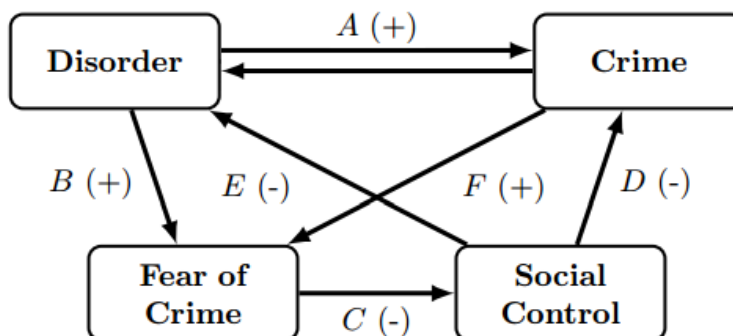


Figure 5: Broken Windows Theory

From the above discussion, it should be clear that BWT describes a highly interdependent system of neighborhood-level factors, as seen again in Figure 5. This model suggests that disorder and crime both reinforce one another and propagate through community disengagement and subsequent collapse of social control. The broken windows thesis is defined by one reciprocal path between disorder and crime (A) and two feedback loops (BCE and FCD). This is an under-identified system, and it is statistically impossible to simultaneously

estimate all parameters represented by arrows in Figure 5. Due to the reciprocal structural relationships between disorder, crime, fear, and social control, observational research is hampered by endogeneity that prohibits estimation of the model depicted in Figure 5.

In Raudenbush and Sampson's (1999) highly-visible critique of BWT, the authors claim that disorder and serious crime differ mainly in severity, both being the product of low collective efficacy—that is the neighborhood's capacity for social control. According to this perspective, the link between disorder and crime is spurious, and collective efficacy precedes both in causal order. Using survey measures of collective efficacy to operationalize social control and measures of disorder based on video footage of block faces in neighborhoods, they found no evidence for a direct relationship between disorder and crime—net of collective efficacy—except in the case of robbery. They acknowledge, however, that “fear of crime, especially the fear of being accosted by strangers in public and attacked, may thus undermine neighborhood collective efficacy” (Sampson and Raudenbush 1999:631). They also find empirical support for a “feedback loop, whereby disorder entices robbery, which in turn undermines collective efficacy, leading over time to yet more robbery” (1999:637), though this relationship is not found for homicide. Given that collective efficacy has been considered a construct that captures a neighborhood's capacity for social control (Sampson et al. 1997; Sampson and Raudenbush 1999), these statements can be interpreted as limited support for the model depicted in Figure 5.

Xu, Fiedler, and Flaming (2005), however, point out that Sampson and Raudenbush (1999) conducted their analyses under the assumption that crime does not feed back on disorder and without estimating the effect of disorder on collective efficacy. Xu et al. (2005) claim that Sampson and Raudenbush's results provide evidence for the indirect pathway between disorder and crime through collective efficacy; in addition to collective efficacy predicting disorder, Sampson and Raudenbush found a reverse effect from crime to collective efficacy indicative of the feedback processes seen in Figure 5. Without estimating the effect of disorder on collective efficacy, Sampson and Raudenbush cannot rule out the indirect path of BWT, nor account for endogeneity that may obscure the direct effect of disorder on crime.

Xu et al. (2005) also present a structural equation model that provides limited evidence for both direct and indirect effects of disorder on crime. However, their study uses individual-level survey data with perceptual measures of disorder and crime, which, as noted earlier, may be weakly related to objective measures and entangled with other neighborhood and resident characteristics (Sampson 1999; Wallace, Louton, and Fornango 2015).

In each of these two studies, conclusive observational evidence for the direct effect of disorder on crime is elusive, as BWT is not structurally amenable to the examination of its component pathways in isolation. To conduct a strong test of BWT—in particular the direct path between disorder and crime (*A*)—the reciprocal relationships and feedback loops must be broken. This can be accomplished with field experiments, which permit measurement of causal effects by holding fixed neighborhood-level characteristics while manipulating focal predictors such as disorder.

Experimental Evidence

Experimental research has offered stronger support for the direct effect of disorder on crime than observational research. By manipulating the disorder condition through a controlled experiment, the potential spuriousness created by social control can be eliminated (e.g. Keizer et al. 2008). Wilson and Kelling’s (1982) original essay on broken windows cites Zimbardo’s famous abandoned car experiment (Zimbardo 1973), in which a disabled vehicle in a disorderly neighborhood in the Bronx was rapidly dismantled but one in an affluent orderly neighborhood in Palo Alto was left alone until first vandalized by the researcher (Wilson and Kelling 1982). Consistent with path *A* in Figure 5, the implication is that the potential for crime exists everywhere but in orderly places a signal of disregard is needed to catalyze crime.

Relative to Zimbardo, Keizer et al.’s (2008) highly visible paper in *Science* presents a more systematic and modern exploration of the direct broken windows effect. The foundation of their study is Cialdini, Reno, and Kallgren’s (1990) distinction between injunctive norms (what “ought to be,” or is commonly accepted as proper) and descriptive norms (what is

actually observed). Cialdini et al. (1990) presented evidence that the presence of conflicting information from one type of norm can inhibit adherence to another type of norm. For example, the presence of litter (descriptive) may increase the probability of an individual littering (injunctive). Keizer et al. (2008) extend this to test whether the presence of a descriptive norm of disorder, which conflicts with a widely-held injunctive norm, inhibits adherence to a second injunctive norm. For example, the presence of litter may increase the likelihood of theft rather than just littering. They call this a cross-norm inhibition effect, and present it as a potential mechanism for BWT.

In Keizer et al.'s study, the introduction of a disorder treatment was found to promote the commission of a variety of norm violations. In one experiment, researchers placed an envelope with a visible five euro note on a mail box, and, in the experimental condition, introduced either graffiti or litter. The disorder conditions were associated with approximately double the rate of envelope theft versus the baseline, an exceptionally powerful treatment effect. This experiment provides the clearest evidence to date for the direct broken windows effect, path *A*, in which visible signals of disregard for public space appear to promote the commission of crime. The external validity of the study may be limited due to the experiments being restricted to a single site in Groningen, the Netherlands: it is uncertain to what degree Keizer et al.'s results hold in other neighborhoods in Groningen or different cultural contexts, such as major cities in the United States which experience the higher levels of crime and disorder that prompted the development of BWT.

Keuschnigg and Wolbring (2015) replicated Keizer et al.'s (2008) experiment near two university dormitories in Germany that differed in levels of social capital as measured by surveys. The authors found general support for Keizer et al. (2008), though the effect of disorder appeared stronger in areas with high social capital. While this is evidence of a contextual effect, it remains unclear whether these results would be replicated in other cities or countries or in other neighborhoods: college dormitories are unlikely to be similar to typical urban locations in population composition or the type of activities that occur. The two prior studies also utilize relatively few trials and do not record participant characteristics, which

prevent the use of more advanced statistical analyses and controls for potentially confounding covariates. Additional research is needed to replicate the results of Keizer et al.'s (2008) and Keuschnigg and Wolbring (2015) while extending their scope to a range of social contexts.

EXPERIMENTAL DESIGN

The present study closely replicates the experimental method of Keizer et al. (2008) in Seattle, WA—a U.S. city which displays much higher rates of property crime than Groningen (Seattle Police Department 2010; Sociaal Planbureau Groningen 2013) including vandalism which is likely indicative of physical disorder being more commonplace. The present study expanded the number of trials, recorded more detailed data, and varied sites to examine neighborhood context. We used a field intervention based on the approach of Keizer et al. (2008) in which pedestrians encountered a stamped envelope containing a five-dollar bill lying on the sidewalk near a mailbox. This presented the opportunity to behave altruistically (pro-socially) by mailing the letter, act criminally (anti-socially) by stealing, or respond indifferently (apathetically) by walking by without acting. In contrast to Keizer et al., we observed and recorded inactions of participants, which comprise the majority of our observations. Keizer et al. (2008) focused on the outcome of norm violation and combined inactions—of which they indicate there were relatively few—with mailings. We separated these outcomes to evaluate variation in both conforming and norm-violating behavior. By separating inactions from conforming behavior, we can capture how disorder and neighborhood characteristics influence the relative probability of individuals acting pro-socially, anti-socially, or apathetically, rather than assuming that an observed increase in norm-violation necessarily equates to a reduction in pro-social behavior or inaction.

In the treatment condition, we manipulated disorder by introducing graffiti and litter. A sandwich board with graffiti—copied from graffiti on local refuse bins—was used in place of the non-graffitied control sign, and a number of items of litter were added to the sidewalk surrounding the USPS box, within a radius of approximately 25 feet. An example of a disorder condition is depicted in Figure 9 in Appendix 1. These items were typically “indigenous

litter” collected from nearby trash receptacles or the street and included any items removed earlier if the treatment was preceded by a control trial. No locations in which trials were conducted initially contained sufficient litter to match a treatment condition. Investigators avoided introducing “excessive” litter in the treatment condition which would appear dramatically out of place. Realistic quantities of local litter were used to reduce the possibility of the disorder treatment eliciting confusion or suspicion in participants which may compromise external validity. By manipulating the level of disorder around the mailbox, in the form of litter and graffiti on a yard sale sandwich board, we intended to test the immediate effect of local physical disorder (litter and graffiti) on participant criminal behavior (theft of the letter)—Path *A*—while eliminating reciprocal and endogenous effects (Path *BCE*, and the reciprocal component of *A*). We accomplish the latter through the use of a disorder treatment that is exogenous to both local disorder and social control. Additionally, by conducting experiments in neighborhoods varying on collective efficacy, we can estimate the relationship between neighborhood social control and pro-social and norm-violating behavior (Path *D*). As described by Wilson and Kelling (1982) and made clear by Weisburd et al. (2015), the short duration of our disorder treatment condition relative to the slow pace of neighborhood change renders macro-level indirect effects of disorder on crime through fear and social control unlikely. Figure 6 below depicts the examined paths in solid arrows and paths held constant through experimental design in dashed arrows.

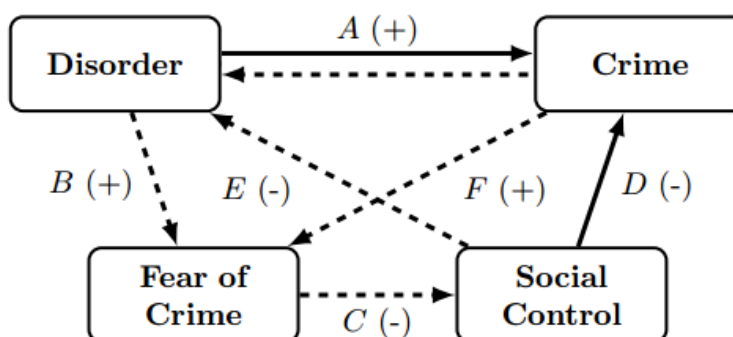


Figure 6: Pathways Under Examination

All trials were conducted in the summer, in dry and sunny or partly-cloudy weather conditions, and on week days between 10 AM and 5 PM to avoid large numbers of commuters or weekend pedestrians that may be atypical of a given location. Care was taken to mitigate any systematic bias related to time-of-day or contagion between trials. Each trial type typically required an entire morning or afternoon during these hours, so treatment assignment was accomplished by alternating the order of control and treatment trials day-to-day and without repeating trials in the same location in consecutive days when more than one day was required to complete a site. No statistically significant associations were found between participant actions and either time of day or day of week for either trial type.

Letters were dropped within approximately eight feet of a USPS mailbox, typically on a well-trafficked street corner that had a minimum of loitering bystanders. In the control condition, any litter on the same sidewalk and side(s) of street as the USPS box and within approximately 50 feet was removed to produce a clean baseline context. A hand-made graffiti-free sandwich board painted with the text “Yard Sale” was placed within eight feet of the USPS post box where it would not interfere with the use of the sidewalk or obstruct the view of the envelope from either direction; this sandwich board was included to provide a control for the treatment version covered in graffiti.¹

Tract Data and Sampling

This study uses data from the Seattle Neighborhoods and Crime Study (SNCS)—a survey of 4,994 residents from all 123 census tracts in Seattle conducted in 2002 and early 2003—to inform sampling and permit analysis of the relationship between rates of mailing and theft and neighborhood conditions. The SNCS taps into collective efficacy using a series of five questions regarding child-centered social control modeled on Sampson (Sampson, Raudenbush, and Earls 1997; Sampson 2012), such as “Would your neighbors do something about

¹Any graffiti on the USPS box or in the surrounding area was recorded by the investigators; this graffiti could not be covered or removed and thus could potentially bias the estimated difference between control and treatment. A series of trials were run with only a graffiti treatment and without litter to estimate the independent effect of graffiti: it was found to have no distinguishable treatment effect.

kids fighting in the neighborhood?” Matsueda and Drakulich (2016) found strong measurement properties for collective efficacy in the SNCS. While these survey data were collected a decade prior to this study, research suggests collective efficacy—our main neighborhood measure of interest—is highly stable over time (Sampson 2012). Seattle has, however, undergone non-trivial socioeconomic change since these data were collected, such as widespread development, moderate population growth, and increases in income. Nonetheless, due to the expense and complexity in acquiring neighborhood-level measures of collective efficacy, the existing data are the best available to measure this concept. In addition, we include census and American Community Survey data on socioeconomic conditions from 2000 to 2015 to facilitate sampling and provide a more recent measure of neighborhood context in the form of indices of concentrated disadvantage. This measure is a combination of percent of households under the poverty line, percent female-headed households, percent on public assistance, and percent unemployed (Land, McCall, and Cohen 1990)².

While the effects of existing neighborhood context are not the primary focus of this study, we include a measure of neighborhood collective efficacy from the 2002-2003 SNCS to examine how the construct relates to rates of pro-social or anti-social behavior in our experiment. In our study, we treat collective efficacy as a fixed capacity of the neighborhood—exogenous to our treatment—to respond to norm violations that deters those violations through potential offenders’ awareness of this capacity (rooted, presumably, in past instances of effective responses to norm violation). That is, a potential offender is deterred from crime in an efficacious neighborhood due to the perception that the community is likely to respond to a violation in a manner resulting in apprehension and sanction. In our study, if collective efficacy restrained anti-social behavior while the disorder treatment exerted no influence, it would be compelling evidence against broken windows. This would indicate that passersby are cognizant of the neighborhood’s capacity for social control and that they do not interpret

²Due to increases in population, eight year-2000 census tracts were split into pairs in 2010. Consequently, we calculated concentrated disadvantage measures for these eight year-2000 tracts using the population-weighted means of the year-2010 tract pair values.

the litter and graffiti in the disorder treatment as a signal of a low capacity for social control.

The nature of our experimental design places constraints on sampling, requiring a USPS mailbox in an area that has sufficient foot traffic to generate observations in a timely fashion yet does not feature long term loiterers which would compromise repeated trials. Further, mailboxes are unequally distributed in the city in a fashion that is correlated with concentrated disadvantage and collective efficacy, neighborhood characteristics which may affect participant actions. These challenges required non-systematic selection of 13 locations based on our neighborhood statistics and field scouting of locations. Figure 7 below depicts the sampled tracts and the full distribution of Seattle tracts on collective efficacy and concentrated disadvantage as measured during the 2002-2003 survey. The diagonal line depicts a linear regression line of collective efficacy on concentrated disadvantage; the slope is indicative of the moderate negative correlation between concentrated disadvantage and collective efficacy. Figure 8 depicts the geographical distribution of these tracts. Regarding change since the 2002-2003 SNCS, the census tracts under examination were largely stable from 2000 to 2015 on the composite measure of concentrated disadvantage (see Figure 11 in Appendix).

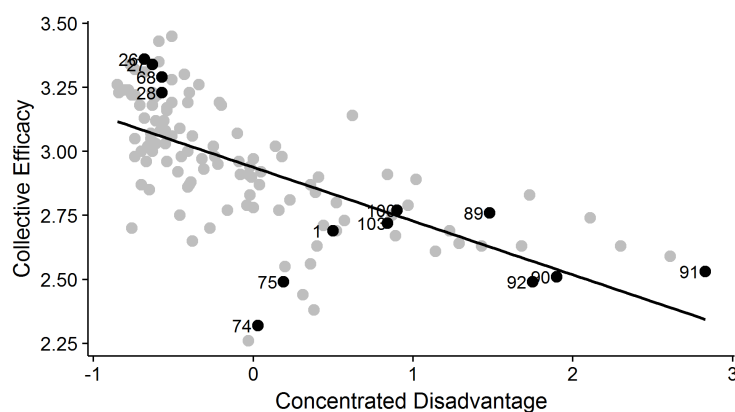


Figure 7: Sampled Tracts by Collective Efficacy and Concentrated Disadvantage

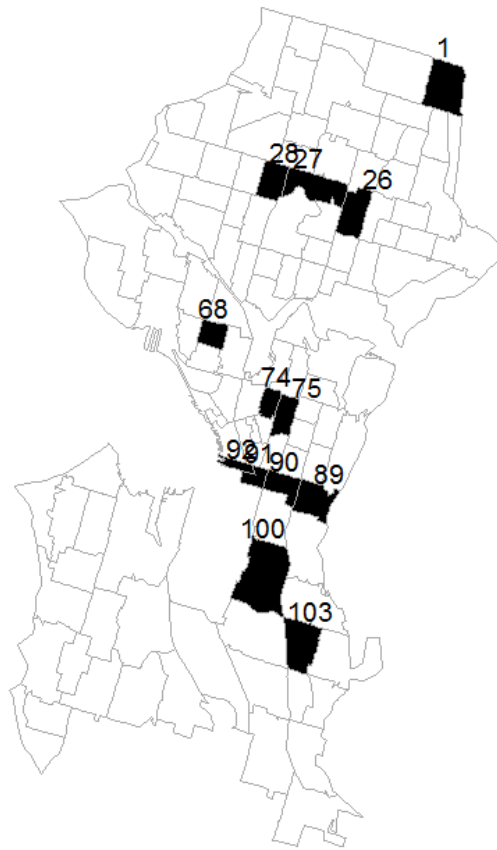


Figure 8: Geographical Distribution of Sampled Tracts

Data Recording

To record interactions with the envelope as well as visible traits of participants, a pair of investigators sat posing as bystanders—such as in outdoor seating at a café—in sight and within one block of the mailbox. The majority of mailboxes were located near, but not immediately adjacent to, cafés or restaurants where long stays by a pair of individuals are normative. Locations were chosen to enable the pair to covertly observe participants passing through the experiment area from either direction. The experiment area was defined as the immediate vicinity of the mailbox and dropped envelope, such that participants were those pedestrians passing directly by the mailbox and envelope. Tasks were divided such

that the first observer recorded counts of participants entering the experiment area cross-classified by approximate age, race, sex, and membership in a group. Participants that appeared to be under the age of 18 were excluded from data collection, resulting in the removal of only one action. Groups were separately classified by race and sex, however age was not recorded for individuals in groups due to complexity in recording. For both individuals and groups, recording of race was limited to “White” or “Non-White” due to the limitations of investigator evaluations of race. The second investigator covertly placed the envelopes by the mailbox, frequently leaving the immediate area for some time before returning to the observation point. After a participant interacted with the envelope, the investigators verified the participant left the area and waited approximately five minutes before the second investigator attempted to drop a new envelope. Care was taken to avoid being observed by passersby or attracting attention prior to drops, including by dropping envelopes while walking past the USPS box.

The total number of participants and the duration of each trial were also used to calculate a total rate of passersby—in persons per minute—to control for pedestrian density. This was calculated as the number of recorded participants passing through the experiment area during an active trial divided by the length of the trial in minutes. Participants passing by while no envelope was on the ground, and thus while no trial was active, were not included in this calculation. Altogether, trials were completed in 13 census tracts in the Seattle, yielding a combined 2786 observations of participants (1537 control, 1249 treatment) featuring 230 mailings, 47 thefts, and 2509 inactions. Table 1 depicts descriptive statistics of Seattle census tracts in which trials were conducted.

Table 1. Descriptive Statistics of Tracts

Tract	N	Inaction	Mailing	Theft	Coll. Eff.	Walk-By Rate
1	79	0.785	0.215	0.000	2.69	0.90
26	95	0.821	0.168	0.011	3.36	0.52
27	288	0.868	0.125	0.007	3.34	0.71
28	247	0.887	0.101	0.012	3.23	1.07
68	406	0.943	0.049	0.007	3.29	2.34
74	122	0.869	0.123	0.008	2.32	0.70
75	242	0.946	0.033	0.021	2.49	3.28
89	218	0.844	0.110	0.046	2.76	0.56
90	160	0.912	0.044	0.044	2.51	1.11
91	392	0.929	0.051	0.020	2.53	2.11
92	425	0.962	0.033	0.005	2.49	3.46
100	74	0.784	0.162	0.054	2.77	0.54
103	38	0.553	0.421	0.026	2.72	0.66
Mean	214	0.854	0.126	0.020	2.83	1.38

RESULTS

Bivariate Analysis

Table 2. Frequency of Action by Treatment Status

	Inaction	Mail	Theft	Total
Control	1359 (0.884)	153 (0.100)	25 (0.016)	1537 (0.552)
Treatment	1150 (0.921)	77 (0.062)	22 (0.018)	1249 (0.448)
Total	2509 (0.901)	230 (0.083)	47 (0.017)	2786
$\chi^2, df = 2$	13.08	$p < 0.001$		

Table 2 depicts a cross-tabulation of treatment condition and outcome. Whole numbers depict counts of cases and the parentheses contain proportions for actions within each type of trial (control and treatment). A statistically significant χ^2 statistic indicates the presence of a treatment effect, but it is apparent that the substantial increase in theft observed by Keizer et al. (2008) is not observed in our sample. Column 3 of Table 2 reveals almost no difference in the proportion of thefts between treatment (0.016) and control (0.018) conditions. However, there is approximately a 40% reduction in mailing actions attributable to the treatment and a commensurate increase in inactions. Our manipulations of graffiti and litter appear to inhibit the pro-social behavior of mailing yet exert no influence on theft.

Multivariate Analysis

Next we consider our results in a regression framework. While the experimental design of our study allows us to make accurate estimates of the causal impact of our treatment without the need for covariates, multivariate analysis permits the examination of the relationship

between our outcomes and participant and neighborhood characteristics. This allows for comparison of the relative importance of the disorder treatment compared to contextual and individual factors. It would be noteworthy, for instance, if disorder exerts some impact but is trivial when compared to neighborhood characteristics, or if the treatment suppresses these relationships.

Table 3 depicts log-odds coefficient estimates of three multinomial regression models: Model 1 is a simple model including only our treatment; Model 2 introduces neighborhood characteristics; Model 3 adds participant characteristics. Model 2 features collective efficacy, as obtained from the 2002-2003 Seattle Neighborhoods and Crime Survey (Matsueda 2010), and the rate in participants per minute entering the trial area. The latter may influence the likelihood of either mailing or theft through a number of mechanisms, not limited to the bystander effect (Darley and Latane 1968), the presence of capable guardians (Cohen and Felson 1979), or perceptual load (Lavie 1995). The bystander effect may reduce the likelihood for individuals to behave pro-socially (mailing the envelope) as the presence of others reduces their perceived responsibility to act. According to routine activities theory (Cohen and Felson 1979), one of the requisite conditions for the commission of a criminal act is the absence of capable guardians that might intervene: a potential offender may be deterred from stealing the envelope when there are more passersby who may intervene against their norm violation. Lastly, it is possible that the presence of litter and graffiti—which increase the amount of information in the environment and thus the perceptual load of participants—may reduce the likelihood of individuals noticing the envelope on the ground (Lavie 1995).

Model 3 introduces participant sex and whether or not they appeared to be walking with others. It is possible that the actions of participants might be influenced by being in a group through social pressure or desire to please others. We would also expect to see an elevated rate of theft for males who exhibit higher rates of crime and deviance in the population than females (Smith and Visher 1980). While we would anticipate higher rates of theft by youth (Gottfredson and Hirschi 1990), dummies for age (young, middle-age, older)

were excluded as they were not statistically significant, had no impact on other estimates, and failed to improve model fit as measured with BIC (Raftery 1995). Appendix 2 includes discussion of tests and appropriate re-specifications for violation of the independence of irrelevant alternatives assumption of multinomial logit. Those models provide substantively identical results without improving BIC, so the original multinomial logit is presented here for ease of interpretation.

As expected, the effect of the disorder treatment on mailing is strongly negative and stable across all three models while there is no apparent impact on theft. In Model 2 neighborhood collective efficacy is associated with a reduction in probabilities of theft but not with mailing, but it drops below significance in Model 3 due to the inclusion of individual race which has a -0.33 biserial correlation with collective efficacy. Given that minorities in Seattle are clustered in disadvantaged areas and also more likely to be poor than whites even in areas with similar economic conditions, we cannot rule out that the effect of race is attributable to socioeconomic disadvantage or other individual characteristics for which we cannot control. The rate of participants entering the trial is negatively associated with both mailing and theft; busier areas have lower individual probabilities of actions, which is compatible all of the explanations discussed previously (the bystander effect, capable guardians, and perceptual load) but does not permit us to adjudicate between them. For individual characteristics, males are more likely to commit theft but just as likely to mail as females relative to walking by the letter. Non-white individuals were less likely to mail the letter and possibly more likely to commit theft, but the latter relationship is not statistically significant. Lastly, participants in groups are less likely to interact with the envelope in any way. One of many potential explanation is that those traveling in a group may be more likely to be engaged in conversation or otherwise distracted and thus less likely to see the dropped envelope.

Table 3. Multinomial Regression, Log-Odds Coefficients

	Model 1		Model 2		Model 3	
	Mail	Theft	Mail	Theft	Mail	Theft
Intercept	-2.184	-3.995	-1.085	0.133	-0.614	-1.327
	(0.085)	(0.202)	(0.605)	(1.314)	(0.651)	(1.483)
Treatment	-0.520	0.038	-0.414	0.039	-0.414	0.036
	(0.145)	(0.295)	(0.149)	(0.298)	(0.151)	(0.300)
Coll. Eff.			-0.058	-1.174	-0.100	-0.837
			(0.195)	(0.446)	(0.203)	(0.478)
Walk-By Rate			-0.644	-0.526	-0.599	-0.452
			(0.080)	(0.150)	(0.080)	(0.151)
Male					-0.043	0.651
					(0.143)	(0.325)
Non-White					-0.485	0.477
					(0.169)	(0.306)
In Group					-1.616	-0.994
					(0.266)	(0.481)
Log-Likelihood	-1021.6		-972.4		-934.7	
BIC	2074.9		2008.2		1980.4	
N	2786		2786		2786	

DISCUSSION

Using the largest sample size to date for a field experiment testing the direct effect of local disorder on norm violation, we find no evidence for a causal effect and fail to replicate Keizer et al. (2008). Conversely, by accounting for participant inaction, we find evidence that disorder reduces the likelihood that participants will engage in the pro-social behavior of mailing a dropped envelope. We also find that the effect of disorder holds across a range of neighborhood contexts, and that neighborhood conditions—in this case collective efficacy—exert an effect on the probability of theft independent of the treatment. These findings have implications for our understanding of BWT and the effect of context on social behaviors.

While our findings do not explicitly refute Keizer et al. (2008), they suggest possible new interpretations for their findings: Keizer et al.’s results may hold in the context in which their study was conducted, that of Groningen, the Netherlands, but not in other nations or cities. Cultural differences between study locations could be responsible for our disparate findings: Cialdini et al. (1990) note that cultural and contextual norms are likely to influence behavior if they are salient. This would suggest additional research is needed to determine which cultural or contextual factors influence the effects of disorder on social behaviors and what conditions make those factors primary in a situation. It is also possible that participant behavior is highly sensitive to experimental design: Keizer et al. (2008) place their envelope hanging from the mailbox while the design of American mailboxes required that the envelopes be placed on the sidewalk. If this difference is sufficient to eliminate the effect of disorder it would raise serious concerns about the use of all similar approaches to studying these phenomena. We ran additional trials with envelopes placed on the mailbox and did not obtain a higher proportion of actions.

Alternatively, it is possible that this discrepancy is due to our inclusion of inaction as an

outcome or its relative profusion; where the results of this study are dominated by inactions, Keizer et al. (2008) state that inactions were so uncommon in their study that they were pooled with mailing. It is uncertain if this difference in the frequency of inaction is due to experimental design or cultural differences between Groningen and Seattle. Omitting inactions from our results produces apparent evidence of a treatment effect in the direction of Keizer et al. (2008) though smaller in magnitude. However, this is only due to the omission of inactions obscuring our treatment’s inhibitory effect on the pro-social behavior of mailing—which we argue is the key finding in this study. By removing inactions, any reduction in mailing increases the relative proportion of thefts, even in the absence of any actual increase in theft.

Table 4. Frequency of Action by Treatment Status

	Mail	Theft	Total
Control	153 (0.552)	25 (0.090)	178 (0.642)
Treatment	77 (0.278)	22 (0.079)	99 (0.357)
Total	230 (0.830)	47 (0.170)	277
$\chi^2, df = 1$	3.02	$p = 0.082$	

Table 4 depicts frequencies of theft and mailing by treatment status, omitting inactions. Cell proportions, rather than row proportions, are shown in parentheses to control for the decrease in marginal counts from removing inactions. In Table 4, what could be interpreted as a 50% increase in theft from 14% (25/178) in the control to 22% (22/99) in the treatment is only due to a decrease in pro-social actions, namely mailing the letter. In reality, we see very similar proportions of theft in the control (0.090) and treatment (0.079) when we control for the change in marginal counts due to the omission of inactions. As the treatment appears to have no measurable impact on theft, a letter left under the disorder condition is relatively more likely to be stolen versus mailed only because participants are more likely to ignore it. This finding highlights the importance of considering the impact of treatment,

and thus disorder itself, on both pro-social and anti-social behavior. With respect to broken windows theory, this finding may be interpreted in two ways: (1) as evidence against the direct pathway between physical disorder and the commission of crime, at least for localized disorder, and (2) as support for Wilson and Kelling's indirect pathway, as the presence of disorder appears to inhibit other-regarding behavior in its immediate vicinity. If disorder inhibits individuals from intervening to remove a criminal opportunity, such as the lost letter, that opportunity is more likely to eventually be exploited by a motivated offender due to availability alone. Lack of a treatment effect on theft also indicates that disorder is not merely causing individuals to be less likely to see the envelope, unless it is the case that potential mailers and thieves differ in awareness of surroundings.

Our key finding is that if disorder hardens individuals into inaction, an opportunity to commit crime can remain in place longer, permitting more crime even without an increased individual propensity to commit crime. Disorder thus may facilitate crime through an increase in the exposure time of criminal opportunities rather than through direct inducement. Interestingly, this would be compatible with St. Jean's (2007) finding that criminals are responsive to opportunity rather than the presence of disorder. Even if disorder signals a reduction in the certainty of sanctions, without an opportunity to commit crime none will occur. If disorder similarly reduces the likelihood of individuals engaging in social control actions, such as calling the police or censuring minor offender, it would be analogous to the indirect pathway operating through fear of crime in broken windows. Additionally, extending Keizer et al.'s (2008) concept of cross-norm violation, if one form of disorder inhibits the pro-social act of cleaning up another type of disorder—or an increase in the quantity of disorder reduces intervention—it is suggestive of the potential for cascade effects or tipping points as described by Wilson and Kelling (1982). If this holds, then in a context where disorder is commonly introduced—such as sporadic littering—but normally removed rapidly, a sudden influx of disorder could overwhelm the area's capacity for removal. Further disorder would then begin to accumulate only through the reduced propensity of residents to intervene pro-socially. Together, if these suppositions are true, no infusion of motivated

offenders from outside the neighborhood or emboldening of those present would be required to generate Skogan's "spiral of decay" (1990). Instead a neighborhood's decline would be partially the result of growing inaction of residents—the collapse of informal social control or collective efficacy. As BWT scholars have long asserted, neighborhoods at or beyond this tipping point may represent opportunities for targeted interventions by the state aimed at restoring residents' ability to regulate their neighborhood (Wilson and Kelling 1982; Kelling and Coles 1996).

While we believe this study makes a strong contribution to the literature on broken windows, we must note a number of limitations which offer opportunities for further research. First, this study has notable caveats as a test of BWT. First, BWT describes a developmental sequence between disorder and serious—particularly violent—crime, our experiment examines the impact of a transient intervention on a non-serious crime. It is possible disorder may promote the commission of more serious crimes, though this cannot practically or ethically be examined experimentally. Similarly, the observed negative impact on pro-social behavior may be limited to particular behaviors; future research might establish the scope of this inhibitory effect. We are also unable to test the longer-term effects of persistent disorder on fear of crime and social control that are integral to Wilson and Kelling's (1982) original theory. It is, however, conceivable that the inhibitory effect of disorder on pro-social behavior could, as described earlier, serve as a mechanism for lasting and self-reinforcing reductions in interventions against disorder and crime.

Second, while the experimental method used in this study should ensure high internal validity and permits us to make causal statements regarding the effect of disorder, the degree to which our results are externally valid—that is generalizable to other contexts or non-contrived situations—is unknown. By conducting trials in a wide variety of neighborhoods we have done our best to address this limitation but it is an inescapable feature of social field experiment methods.

Third, while our measure of neighborhood collective efficacy predicts lower thefts, it is entangled with other neighborhood conditions—such as economic disadvantage. Consequently,

measurement error may be attenuating the effect of collective efficacy on theft and mailing. Unlike our treatment effect, the relationship of collective efficacy and theft and mailing should not be interpreted as unambiguous let alone causal.

Fourth, we are unable to identify the mechanism by which disorder reduces pro-social interventions, and this may be an important avenue of future research. It is possible, for instance, that increasing disorder reduces the perceived marginal benefit of pro-social behavior. That is, when a small quantity of disorder is present, a small pro-social intervention is perceived to produce a relatively large improvement. On the other hand, if a large quantity of disorder is present, individuals may feel disempowered because a great deal of effort is required to make a visible difference. This implies a non-linearity between disorder and interventions against it. If this is the case, individual behavior should be sensitive to variation in the form and level of disorder. It is not difficult to imagine that this would apply broadly to disorder and crime, as even in the absence of danger to oneself, continually intervening is likely to be prohibitively time intensive and fatigue-inducing; it is easy to behave pro-socially when one must do so only rarely.

Nonetheless, this study makes a number of contributions to the literature. Using a large-scale field experiment across multiple sites, we fail to replicate Keizer et al. (2008) and find no evidence that disorder induces norm-violation in the form of theft of a letter containing money. Conversely, we find that disorder inhibits pro-social behavior—that is, mailing of the letter. This inhibitory effect results in the letter remaining on the ground longer, and thus represents the prolonging of a criminal opportunity that might otherwise be removed. Disorder may thus facilitate crime by increasing exposure to criminal opportunities rather than by emboldening offenders. Furthermore, if this inhibition of pro-social behavior applies also to acts of social control, these findings are compatible with the indirect pathway of BWT under which disorder reduces the capacity of a neighborhood to combat norm violation such as disorder and crime.

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APPENDIX

Appendix 1: Disorder Condition



Figure 9: Disorder Condition

Appendix 2: Assessing Model Assumptions

It is possible that the estimates of the above multinomial logistic regression model are compromised by the similarity between theft and stealing as outcomes. As both require that a participant notice the envelope prior to acting, while inactions can occur when a participant either notices the envelope but ignores it or never notices the envelope in the first place. This process is depicted in Figure 10 below. The path that lacks a participant decision process could violate the assumption of independence of irrelevant alternatives (IIA) for multinomial logistic regression. Statistically significant Hausman-McFadden tests between the full model above and models omitting the mailing outcome ($\chi^2 = 55.729$, $df = 7$, $p < .001$) or omitting the inaction outcome ($\chi^2 = 1169.9$, $df = 7$, $p < .001$) provide evidence for a IIA violations. Nested logit and multinomial probit are regression techniques for categorical outcomes that relax the IIA assumption. Both were estimated and produced results that are substantively identical to the original multinomial logit, consequently they have been omitted for brevity but are available upon request.

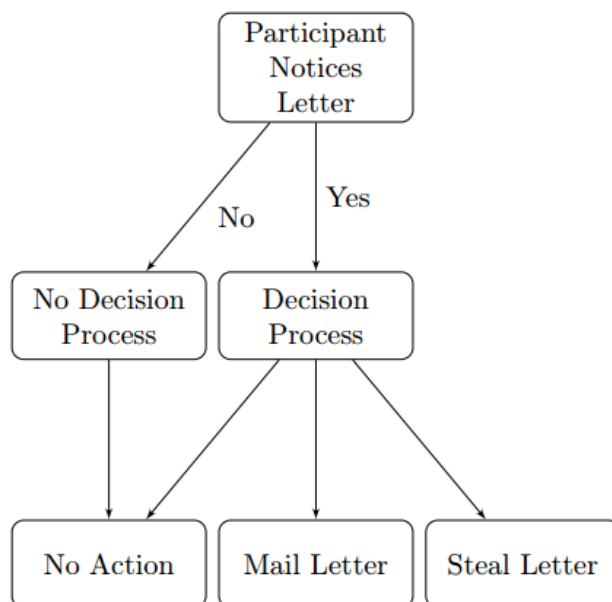


Figure 10: Action Process

Appendix 3: Concentrated Disadvantage Over Time

This study uses neighborhood measures obtained from the 2000 decennial Census and 2002-2003 SNCS. The validity of these measures would be compromised if the neighborhoods under examination experienced substantial change between 2000 and 2014-2017 when our experiments were being conducted. Figure 11 depicts levels of concentrated disadvantage over time in the tracts under examination as captured by our composite measure. This updated scale was constructed using the same variables obtained from the 2000 and 2010 Censuses and the 2010 and 2015 American Community Surveys. This graph is notable mainly for the great consistency over time exhibited in these tracts; only tract 91, located near downtown Seattle, experienced a dramatic change in the 15 year span. Overall, most higher disadvantage tracts experienced small declines—to be expected with Seattle’s rapid growth in the last decade—while low disadvantage tracts remained stable. Given the strong correlation between socioeconomic disadvantage and collective efficacy (Sampson 2012), this observed stability increases our confidence in the collective efficacy measures from the 2002-2003 SNCS.

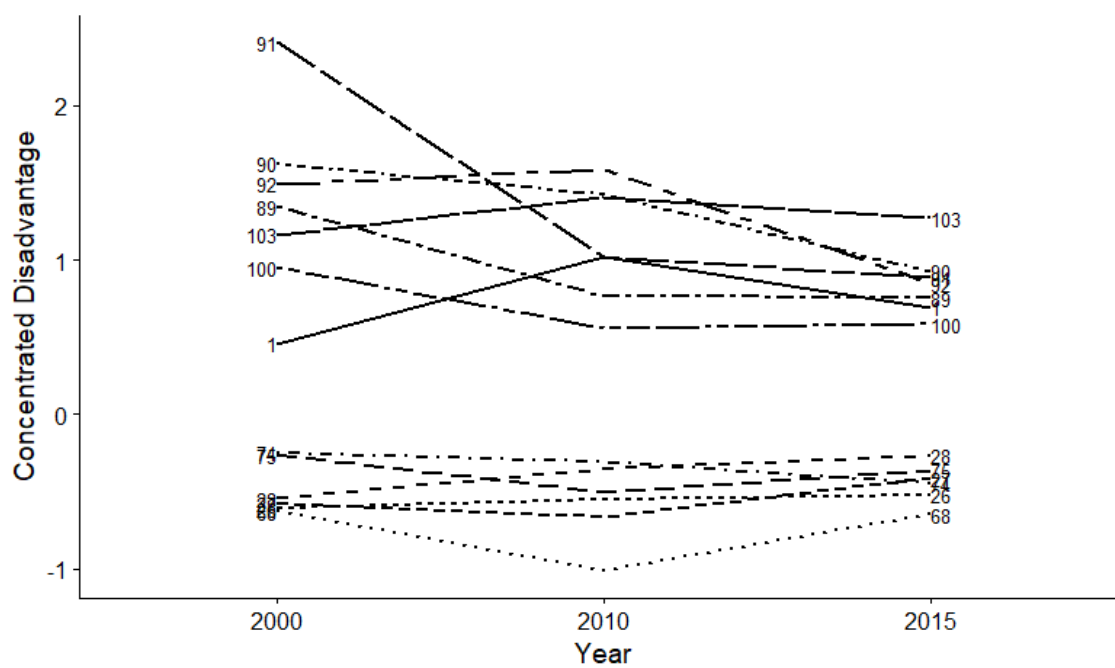


Figure 11: Concentrated Disadvantage over Time in Selected Tracts