A LONGITUDINAL TEST OF SOCIAL DISORGANIZATION THEORY: FEEDBACK EFFECTS BETWEEN COHESION, SOCIAL CONTROL AND DISORDER

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ABSTRACT

Social disorganization theory holds that neighborhoods with a greater population stability, higher socio-economic status and more ethnic homogeneity experience less disorder because these neighborhoods have higher social cohesion and exercise more social control. Recent extensions of the theory argue that disorder in turn affects these structural characteristics and mechanisms. Using a dataset on 74 neighborhoods in the city of Utrecht in the Netherlands spanning ten years, we tested the extended theory, which to date only few studies have been able to do because of unavailability of neighborhood-level longitudinal data. We also improve on previous studies by distinguishing between *potential* for social control (feelings of responsibility) and actual social control *behavior*. Cross-sectional analyses replicate earlier findings, but the results of longitudinal cross-lagged models suggest that disorder has large consequences for subsequent levels of social control and population turnover, thus leading to more disorder. This is in contrast to previous research, which sees disorder more as a consequence than a cause. This study underlines the importance of longitudinal data, allowing for simultaneously testing the causes and consequences of disorder, as well as the importance of breaking down social control into the potential for social control and actual social control behavior.

Keywords: social disorganization, social cohesion, social control, disorder, longitudinal

Bios

Wouter Steenbeek is a Postdoctoral Research Fellow at the Netherlands Institute for the Study of Crime and Law Enforcement (NSCR). This contribution was partly written when he was a graduate student in the department of Sociology and the ICS Graduate School at Utrecht University, and a visiting scholar at the University of California, Irvine. His research interests are the spatial-temporal distribution of crime and disorder, social cohesion and social control, and quantitative research methods.

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INTRODUCTION

There are various reasons why people want to avoid neighborhoods with high levels of social or physical disorder. Disorder or incivilities are the social and physical conditions in a neighborhood that are considered troublesome and potentially threatening (Perkins and Taylor, 1996; Taylor, 1999). Social disorder includes such undesirable behaviors as drinking in public, threatening behavior, or people getting bothered on the street, whereas physical disorder refers to physical deterioration of the neighborhood, including vandalism and graffiti. Although the seriousness of disorder is relatively low compared to other deviant acts (Rossi et al., 1974), the chance of encountering physical disorder (e.g., litter) in one's daily live is much higher than, e.g., witnessing a robbery. Moreover, signs of disorder may be a prelude to more serious crime (Skogan, 1990; Wilson and Kelling, 1982).

Social disorganization theory (Shaw and McKay, 1969) argues that neighborhoods with greater population turnover, lower socioeconomic status, and more ethnic heterogeneity are more likely to experience disorder. An important explanation for this relationship is the differential ability of residents to organize themselves to achieve common goals, e.g. a clean and safe neighborhood. Thus, the mechanism of informal social control and sanctioning is crucial for explaining the level of disorder in neighborhoods. Scholars argue that if neighborhood residents can organize themselves, this will result in 'informal social control'--the informal regulatory behavior of others--and therefore potential offenders will either refrain from offending or be stopped in the process.

This paper makes two important contributions to this current explanation of neighborhood disorder using recent neighborhood-level panel data from the Netherlands. First, we investigate two types of social control, namely (1) feelings of responsibility for the neighborhood and (2) actual social control activity. Prior research has not clearly delineated between *potential* for informal social control, and *actual* social control behavior. Instead, prior research often focused on the expectations of intervention by others (inter alia Sampson, Raudenbush, and Earls, 1997), or even the percentage of people reporting neighborhood satisfaction or organizational membership (Markowitz et al., 2001). In this study we have the unique opportunity to simultaneously include the shared feelings of responsibility for the neighborhood, as well as a direct measure of actions undertaken by respondents to improve the livability and safety of their neighborhood.

Second, by using a neighborhood-level panel dataset spanning ten years, we are able to better explore the social processes posited by these theories. Almost all studies have used cross-sectional data to make inferences about these causal effects. At best, occasional studies employ predictor variables which preceded the response variable by one year in time. Only a few papers, namely Sampson and Raudenbush (1999), Bellair (2000), Markowitz et al. (2001), and Robinson et al. (2003) explicitly modeled reciprocal relationships between crime and its social conditions. However, in these studies the number of time points is still very limited with just one time point (Sampson and Raudenbush, 1999) or two time points (Robinson et al., 2003), or have long intervals between time points of four and eight years (Markowitz et al., 2001). In short, the proposed mediating mechanisms of social disorganization theory have rarely been studied using proper longitudinal data, let alone taking into account the feedback effects of disorder back onto these mechanisms or the structural characteristics. Community-level panel data is lacking, and this has precluded the testing of the longitudinal theory of precisely the type that Shaw and McKay (1969) and Skogan (1990) envisioned. In this study, we are able to investigate these issues by using a community-level panel dataset spanning ten years. Thus, we are able to simultaneously investigate ‘feedback’ effects from disorder on subsequent population turnover, social cohesion and social control.

This paper takes the following course. In the upcoming theory section, we discuss the explanation of neighborhood disorder by social disorganization theory and discuss empirical findings of previous research. Specifically, we discuss the hypothesized mediation effects of social cohesion and social control, as well as possible feedback effects of disorder, and make note of the most obvious shortcomings. Next, we discuss the neighborhood-level panel data from the Netherlands which we use to improve upon previous research. The results section provides descriptive statistics of the data, as well as explanatory path analysis models. In the discussion we give general conclusions and reflect on our study, ending with implications and proposals for future research.

THEORY

SOCIAL DISORGANIZATION THEORY

Social disorganization theory, originally formulated by Shaw and McKay in 1942 which in turn was based on older ideas by Park and Burgess, has re-emerged as one of the major theoretical perspectives in the study of deviance (Markowitz et al., 2001; Pratt and Cullen, 2005), and has successfully been applied to explain violent crime (Sampson, Raudenbush, and Earls, 1997), delinquency (Sampson and Groves, 1989), and disorder (Sampson and Raudenbush, 1999).[1] [2] The basic premise of social disorganization theory is that neighborhoods with high population turnover, low socioeconomic status, and a high level of ethnic heterogeneity, experience more disorder than other neighborhoods (*Hypothesis 1*). The underlying mechanism is that people in these neighborhoods are less able to organize themselves against threats, e.g., disorderly behavior, than other neighborhoods. The residents themselves may move to and from the neighborhood, but the characteristics at the *neighborhood* level persist, and thus these neighborhoods remain socially disorganized (Shaw and McKay, 1969).

More precisely, the ability of neighborhood residents to combat collective problems is perceived to originate from *social cohesion* which fosters *social control* (see inter alia Bursik and Grasmick, 1993; Sampson and Groves, 1989; Sampson, Raudenbush, and Earls, 1997).[3] Bad housing quality causes residents to move out of undesirable neighborhoods when it is economically feasible for them to do so. Such residential mobility impedes the formation and maintenance of stable relationships, which are necessary for social control and social cohesion. Ethnic heterogeneity, partly a result of residential mobility, impedes communication between neighborhood groups, and thus shared norms cannot be established due to mistrust or the presence of different norms for the different groups of people. In addition, cultural transmission of deviant values occurs easily in such neighborhoods (Bursik and Grasmick, 1993; Pratt and Cullen, 2005; Shaw and McKay, 1969).

In summary, figure 1 shows that the neighborhood structural characteristics of ethnic heterogeneity, residential stability, and economic disadvantage affect disorder (*Hypothesis 1*). However, these effects are hypothesized to be mediated by social cohesion (*Hypothesis 2abc*), which in turn affects the social control (*Hypothesis 3*) which ultimately affects the level of disorder (*Hypothesis 5*). The arrows are numbered to respond to the hypotheses in question.[4]

----- Figure 1 about here -----

MEDIATION BY COHESION, POTENTIAL AND ACTUAL SOCIAL CONTROL

Previous research grounded in social disorganization theory generally draws similar conclusions, namely that cohesion and social control mediate the effects of structural neighborhood characteristics on deviance. However, there is considerable variation in how social cohesion and social control are measured in these studies. For example, Sampson and Groves (1989) find that neighborhoods with more 'social ties' and greater 'participation in organizations' experience less crime, while the presence of disorderly teenage groups was associated with more crime. Given that some would define the presence of teenagers hanging out on street corners as a measure of social disorder, we might have expected that social ties and organizational participation would have a causal effect upon the presence of such disorder. Bellair (1997) found that neighborhoods with more 'social interaction' (i.e., visiting with their neighbors) had lower levels of disorder. Warner and Rountree (1997) found that greater levels of 'neighboring activities' were related to lower assault rates, but found no evidence that these mediated the relationship between neighborhood structural characteristics and assault rates. Sampson, Raudenbush, and Earls (1997) found that a combined measure of cohesion, mutual trust, and expectations of intervention by others (which they labeled 'collective efficacy') reduced violent crime rates, and this also partly mediated the effect of neighborhood structural characteristics. Lastly, Markowitz et al. (2001) found significant relationships between neighborhood structural characteristics and disorder, which were mediated by 'cohesion' and 'social control'.

What becomes clear from this overview of previous studies is that a conceptual overview of these different operationalizations is currently lacking. We identify five different ways in which previous studies have measured social control: (1) a respondent's 'expectation of intervention behavior on the part of other residents' aggregated to the neighborhood level; (2) 'participation in neighborhood organizations' (through which either the respondent is expected to exercise more social control himself due to his participation, or the level of participation in organizations is a proxy for the mobilization capacity of the neighborhood); (3) 'attachment, social ties, or cohesion' (assuming that these lead to more intervention or informal surveillance).[5] In addition, recent studies have included a combined measure of cohesion, trust in others and expectations of intervention by others, termed (4) 'collective efficacy' (see Sampson, Raudenbush, and Earls, 1997). Lastly, we define a 'miscellaneous' category in which social control is operationalized as some (5) combination of the previous four methods. For example, Markowitz et al. (2001) labeled their variable 'cohesion', though it is a combination of organization participation, helping behavior among neighbors, and satisfaction with the area. Though not quite the same as collective efficacy, as the latter concept requires a perception of what others will do, Markowitz' measure does combine cohesion and social control into one concept. Browning, Feinberg and Dietz (2004) proposed a negotiated coexistence model in which social networks are expected to contribute to collective efficacy, but at the same time also provide a source of social capital for offenders. Their empirical results, taking account of the average level of violence in previous years as well as spatial autocorrelation, showed that the effect of collective efficacy on violence is reduced in neighborhoods with high levels of social interaction. Mazerolle, Wickes and McBroom (2010) argued for cross-cultural testing of theories using data from a community survey in Brisbane, Australia and found that whereas collective efficacy reduced self-reported violent victimization, social ties in the neighborhood did not have such an effect.

Although cohesion and informal social control are empirically related, we argue that these are two distinct constructs. It is important to distinguish between (a) *determinants* of informal social control, (b) the *potential* for informal social control in neighborhoods, and (c) *actual* informal social control behavior. First, we argue that 'cohesion' causally affects, but is not a necessary precondition of 'social control'. For example, deprived neighborhoods with strong interpersonal networks and a sense of belonging may also have greater tolerance towards disorder and crime. In such neighborhoods, people may look out for others and help each other in times of need, but there are no 'shared expectations of intervention in deviant behavior'. Conversely, in upper-class neighborhoods residents may not know their neighbors very well, but the residents do share mutual expectations of intervention when deviant acts occur. Thus, we argue it is theoretically sound to keep cohesion and social control decomposed into separate measures.[6]

Second, we lament the fact that studies which aim to explain neighborhood differences in disorder frequently focus only on the *potential* of social control, but have not measured *actual behavior*, or 'direct intervention'. It is notable that the five operationalizations of social control we identified above do not focus on actual social control behavior to rid a neighborhood of disorder. We argue that it is important to distinguish between the potential for social control and actual social control behavior, because expectations that others will intervene (*potential* social control) need not necessarily result in people actually intervening more (*actual* social control behavior), even though this is implicitly assumed by social disorganization theory. Research on public good dilemmas provides arguments why expectations about intervention may lead to less actual intervention. Even though it is in the interest of each resident to exercise social control on others who misbehave, and thus help produce the collective good, it is rational to refrain from doing so if the resident expects others to exercise social control, because the individual resident then shares none of the burden but all of the profit (Coleman, 1990). And because each resident makes such a rational decision and comes to the same conclusion, then in the end no one will *actually* intervene.

While we do not investigate the theoretical discrepancy between potential social control and actual social control on the individual level in this paper, given that it would be outside the scope of the present study, we do explore it provisionally with aggregated measures of the percentage of residents who feel responsible for the livability and safety of the neighborhood, and the percentage of residents who have actually taken action to improve the livability and safety of the neighborhood. In line with previous social disorganization studies we hypothesize that cohesion positively affects the *potential* for social control, and we extend this hypothesis that cohesion also positively affects *actual* social control behavior (*Hypothesis 3*). In addition, we expect that a high *potential* for social control will result in more people *acting* on behalf of the common good, i.e., to reduce disorder (*Hypothesis 4*, see figure 2). We assume that potential offenders will infer from environmental cues that people have a shared sense of responsibility for the neighborhood (potential for social control) and thus they will not offend in the first place, and thus that the *potential* for social control will lead to less disorder.[7] In addition, we expect that more people actually doing something to decrease disorder (actual social control behavior) will lead to less disorder. Thus, both the *potential* for social control and *actual* social control behavior is expected to decrease disorder (*Hypothesis 5*).

FEEDBACK EFFECTS OF DISORDER

Wilson and Kelling (1982) argued in their ‘broken windows’ theory, as well as Skogan (1990) in the ‘disorder and decline’ model, that disorder in turn feeds back onto the development or maintenance of social ties and the extent to which residents exercise social control on deviants. That is, over time an increase in physical disorder may erode residents’ cohesion (*Hypothesis 6a*) as well as the processes of social control (*Hypotheses 6b and 6c*) (inter alia Markowitz et al., 2001; Robinson et al., 2003) and may also cause residents to exit the neighborhood (*Hypothesis 6d*) (Liska and Bellair, 1995). This in turn leads to more physical disorder and social disorder.

Very few studies have explicitly modeled these feedback effects from disorder on social control, social cohesion, and population turnover. Here we focus on neighborhood-level studies; we refer the reader to Robinson et al. (2003) for an overview of both multi-level and neighborhood-level studies. Cross-sectional ecological analyses generally support the hypothesis that disorder reduces cohesion (*Hypothesis 6a*), for streetblocks (Kurtz, Koons, and Taylor, 1998; Perkins and Taylor, 1996), for neighborhoods (Skogan, 1990) or other community areas (Liska and Warner, 1991; Rountree and Land, 1996).[8] In contrast, Hartnagel (1979) did not find that crime or fear of crime affected social cohesion or social activities. Taylor (2001) interviewed residents in Baltimore neighborhoods twelve years apart, and found that disorder led to more night-time fear as well as intentions to move away. Robinson et al. (2003) use data with two time points, and did not find significant lagged effects of disorder on block satisfaction or local safety trends. They could not test other outcomes, as they were impeded by the lack of between-neighborhood variation or block-level stability of the outcomes.

There is only minimal evidence for the effect of disorder on subsequent social control (*Hypothesis 6b/c*). Taylor (1996) used cross-sectional data and very cautiously concluded that in some neighborhoods, disorder may draw residents together. Sampson and Raudenbush (1999) found that more homicide and burglary both led to a decrease in collective efficacy. Bellair (2000) used cross-sectional data and found negative reciprocal relationships between robbery and assault and informal surveillance; however, after controlling for risk perception, results suggest that burglary increases surveillance behavior. Lastly, Markowitz et al. (2001) used three waves of the British Crime Survey using 300 or 151 neighborhoods (depending on the investigated time lag) and concluded that disorder (via fear) reduces cohesion (which they measured as a combination of the presence of neighborhood social ties, participation in voluntary organizations, and neighborhood satisfaction). Note that all but Bellair's study focused on the effect of disorder on subsequent *potential* for social control. Bellair's (2000) study suggests that some types of deviance may actually lead to more social control behavior – if there are more problems in the neighborhood, people will do more to solve those problems.

Although one study found no evidence that residents' *perceptions* about the amount of neighborhood crime affected subsequent moving (South and Deane, 1993), most studies do find such effects. Crime may affect the total rate of turnover (Dugan, 1999; Hipp, Tita, and Greenbaum, 2009; Sampson and Wooldredge, 1986), as well as the subsequent composition of the neighborhood (Liska and Bellair, 1995; Liska, Logan, and Bellair, 1998; Morenoff and Sampson, 1997). We expect that more disorder leads to greater population turnover (*Hypothesis 6d*).

CURRENT STUDY

The traditional explanations of social disorganization theory (figure 2, solid arrows) have received ample attention in the literature, but mostly by using cross-sectional data. One problem is that studies sometimes use very different operationalizations of social control. The 'disorder and decline' model, which extends the classical social disorganization theory by positing feedback effects back to the neighborhood demographics and cohesion and social control itself, has only occasionally been tested, and then mostly with regard to the effects of *crime* instead of disorder (figure 2, dashed arrows).[9] The primary reason for this state of affairs is that information on cohesion and control at the neighborhood level is not readily available. Ethnographic research is possible, but usually only focuses on a single neighborhood or the differences between a few neighborhoods. To quantify between-neighborhood differences and to assess mediation effects, we require surveys among many neighborhood residents per neighborhood and across many neighborhoods.

----- Figure 2 about here -----

This research attempts to address the above issues. Our research focuses on (1) the direct effect of neighborhood characteristics on disorder, as well as the mediation of this effect by social cohesion and social control. With respect to social control, we investigate both the effects of *actions taken* by the residents as well as of the *responsibility felt* for the neighborhood. In addition, we (2) investigate the feedback effects of disorder on turnover, cohesion, and control. We use a longitudinal neighborhood-level dataset spanning ten years, constructed from resident-based surveys (bi-annually: 1996-2006) coupled with official data on the neighborhoods (bi-annually: 1995-2005). Due to the relatively low number of neighborhoods (*n*=74) and the large number of parameters, we cannot fully investigate time lag issues; we assume the temporal effects to be either one year or two years.

The area of study is the city of Utrecht, the fourth largest city in the Netherlands (after Amsterdam, The Hague, and Rotterdam) with a growing population of about 235,000 to 275,000 in the years of this study. Located in the centre of the Netherlands, Utrecht is at the eastern corner of the major conurbation in the Netherlands (de Randstad). Crime and public disorder in Utrecht is comparable to that of the other three large Dutch cities; the police region to which Utrecht belongs (a larger area which encompasses the city proper) has slightly fewer victims of violent offenses, burglaries, and physical disorder than the police regions which encompass Amsterdam, The Hague, and Rotterdam.[10]

In contrast to the United States (see, e.g., Zimring, 2007), the Netherlands has not experienced an equally strong crime decline. In fact, similar to many other western European countries (see, e.g., Aebi, 2004), the Netherlands has experienced a slight increase in crime between 1990 and 2000, or a clear trend cannot be established (Wittebrood, 2001). Between 1996 and 2006, the years spanning our study, total crime in the Netherlands on average increased a bit at first, but decreased slightly from 2002 onwards (Bijl et al., 2009), with the exception of vandalism. Such a trend can also be found for perceptions of physical and social disorder, which consistently increased until 2002, decreased slightly afterwards, and seems to be rather stable between 2005 and 2008 (Van Noije and Wittebrood, 2009).

DATA AND METHODS

DATA

This study uses a *neighborhood-level* panel dataset to test the hypotheses. We constructed the dataset by combining official neighborhood data from the Statistics Netherlands with the individual-level survey 'Nieuw Utrechts Peil' (NUP), provided by the Administrative Information Department, Adminstrative Affairs, City of Utrecht. Because this survey is a bi-annual cross-sectional survey, the same individuals were not interviewed in different years. This is not problematic because in this study we are interested in processes operating on the level of the neighborhood, and thus we aggregated individual-level responses to the neighborhood level. Neighborhoods are matched over time to create a neighborhood-level panel dataset.

What constitutes a neighborhood is always hard to define. In this particular survey, respondents were originally selected from each of the 29 districts or wards of Utrecht. However, more detailed information on respondent location was included as well, so we were able to use the 'natural areas' as defined by the Statistics Netherlands. These neighborhoods are smaller and more homogeneous than the 29 districts, and are more in line with what a citizen of Utrecht would delineate as his or her neighborhood. This definition of neighborhood is thus arguably better than either postal code areas or larger districts or wards. Lastly, the Statistics Netherlands also gathers data on this level of aggregation, so the survey responses can also be easily combined with official neighborhood data (e.g., the percentage of migrants living in the neighborhood) without encountering data merging problems.

The bi-annual cross sectional NUP survey was conducted between 1996 and 2006 by the municipality of Utrecht, the Netherlands. For each wave, a sample of respondents is drawn from the latest municipal records, of which all respondents are required to have a valid (non-classified) home address, be older than 16 years of age, and not currently living in an institution. In addition, respondents who also participate in other surveys are excluded. Only one person per address is asked (by letter) to participate in the survey, and after agreement, the written questionnaire itself is sent by mail. In 2006, respondents were able to choose themselves whether they prefer to fill out the questionnaire on paper, or fill out an internet-based questionnaire.

The response rate was between 75% and 45% in all years, with a larger response rate in the earlier years (about 70% in 1996 and about 45% in 2006). In total 42,220 respondents were interviewed. To maintain a consistent neighborhood sample across all years, we excluded respondents who lived in areas which were only included in the last wave (e.g., a newly constructed neighborhood quite some distance from the city proper). In addition, we excluded the inner-city neighborhood (the neighborhood comprising the central train station and adjacent mall), business districts and the area where the university is located (outside the city proper), because social disorganization theory's focus is on residential areas. This resulted in a final dataset of 37637 respondents nested within 74 neighborhoods (5604 respondents in 1996, 6321 respondents in 1998, 6565 respondents in 2000, 6414 respondents in 2002, 7486 respondents in 2004, and 5247 respondents in 2006).

Each of these 74 neighborhoods had a mean of at least 10 respondents across all time points. One neighborhood had 7 respondents at one time point (but 14 respondents at another time point, thus resulting in a mean equal to or higher than 10), but excluding this neighborhood from our analyses did not influence our results. All other neighborhoods have at least 10 or more respondents for each time point, one neighborhood having as many as 202 respondents in each survey year. On average the neighborhoods had 77 respondents in 1996, 85 respondents in 1998, 89 respondents in 2000, 87 respondents in 2002, 101 respondents in 2004, and 71 respondents in 2006. Thus, we are confident that we can aggregate individual-level responses to the neighborhood level.[11]

MEASUREMENTS

Disorder was measured by seven questions (on a three-point scale) with regard to how much of a problem a respondent perceived “graffiti on walls or buildings”, “litter on the street”, “dog feces on the street”, “vandalism of, e.g., bus stops”, “threatening behavior”, “drunk people on the street”, and “women and men getting bothered” on the street. As such, our disorder measure comprises both measurements of physical disorder as well as of social disorder, with an average Cronbach's alpha of .69 across all years and neighborhoods. Separate measures of social and physical disorder correlated too strongly to be justifiably separated.

Cohesion was reflected in eight different questions measured on a five-point scale ("completely disagree, disagree, not agree/not disagree, agree, completely agree"). The original cohesion survey questions were (translated from the original Dutch): "people in this neighborhood hardly know each other", "people in this neighborhood get along in nicely", "people in this neighborhood like to keep living here", "it is tedious to live in this neighborhood", "I will move out of this neighborhood if possible", "if you live in this neighborhood, you are lucky", and "I feel at home with the people living in this neighborhood". The eight items scaled very well together with an average Cronbach's alpha of .85 across all years and neighborhoods.

For both disorder and cohesion, we used the 'ecometrics' method (Raudenbush and Sampson, 1999), a hierarchical model to account for individual bias in the perception of the variables, to construct 'true' neighborhood measures. This hierarchical model has the response to the *i*th questionnaire item of the *j*th person in each neighborhood *k*, which depends on the 'difficulty' of an item and one's latent perception of the item plus error.[12] Thus, we are estimating a multilevel model with the following item-level equation:

$$y_{ijk} = \pi_{jk} + \Gamma D_{ijk} + \varepsilon_{ijk} \tag{1}$$

where y_{ijk} is the *i*th item of interest about the neighborhood reported by the *j*-th respondent of *J* respondents in the *k*-th neighborhood, D_{ijk} is the matrix of questionnaire items, Γ shows the 'difficulty' of these items in the data, and π_{jk} is the 'true' score for person *jk*, and ε_{ijk} is a disturbance term. Level 2 reflects the respondent-level:

$$\pi_{jk} = \eta_k + \Lambda X_{jk} + r_{jk} \tag{2}$$

where X_{jk} is a matrix of exogenous predictors with values for each individual *j* in neighborhood *k* that take into account possible biasing effects, Λ shows the effect of these predictors on the subjective assessment, η_k is the random neighborhood-level version of the measure, and r_{jk} is a disturbance term.[13] The third level equation is:

(3) $$\eta_k = \gamma + u_k$$

where η_k represents the overall measure in neighborhood *k*, and u_k is a disturbance for neighborhood *k*. The empirical Bayes estimates at the neighborhood level, or posterior means, are then assumed to be the 'true' neighborhood-level value.[14]

For *social control* we used two different measures. First, we used the percentage of people who said they "felt co-responsible for the livability and safety of the neighborhood", which reflects *potential* for neighborhood-level social control. In addition, we used the percentage of people who said that they had "been active to improve the livability and safety of the neighborhood" in the last year, which reflects *actual* social control behavior. Because this question is asked retrospectively, we assume that respondents making a statement about their behavior in the NUP survey of, e.g., 1998, are talking about their actions to improve the neighborhood in the period of 1997-1998.

Lastly, we included the traditional explanatory variables of social disorganization theory. For *residential instability*, we used the percentage of respondents who had been living there for less than two years, constructed from the individual-level data (see also Markowitz et al., 2001). *Socioeconomic status* was measured by the mean income per neighborhood.[15] *Ethnic heterogeneity* was measured as the percentage of non-western migrants (which excludes: Europe, North-America, Oceania, and Indonesia) living in each neighborhood, here recoded so that an increase of 1 reflects a 10% increase.[16] The socioeconomic status and ethnic heterogeneity measures were provided by the Statistics Netherlands, and are measured a year before each of the NUP survey time points: '95-'97-'99-'01-'03-'05.

METHOD OF ANALYSIS

We accounted for missing data at the individual-level through a multiple imputation (MI) approach. MI requires the less stringent assumption of missing at random than do approaches using listwise deletion, and MI is also more efficient. We included all of the individual-level measures used in the analyses in the imputation procedure. These variables included the various individual-level characteristics that we included as possible biasing effects when constructing our neighborhood-level measures, the various measures that comprised the cohesion and disorder scales, and our measures of shared feelings of responsibility, and actual social control behavior. Given the rate of missingness in our data, we imputed the dataset five times using an MCMC procedure implemented in Stata 9. All analyses were then conducted on these five datasets, and the results were combined and the standard errors were correctly computed (Rubin, 1987: 2361).

After constructing the neighborhood-level panel datasets, we estimated the cross-lagged models using a full information maximum likelihood estimator in Mplus 5.21.[17] In our data, each row contains one neighborhood, and separate variables are created for each time point (e.g., disorder in time point 1, disorder in time point 3, etc).[18] This approach allows us to estimate separate intercepts and disturbance term variances over years; the former allows us to capture changes in crime levels over time, the latter allows us to capture heteroskedasticity over time (the technique is described in numerous sources, such as Finkel, 1995; for an earlier example of a study with spatial autocorrelation using this cross-lagged approach, see Hipp, Tita, and Greenbaum, 2009).[19] Given that we have no reason to expect that the *size* of the effects will change over the relatively short time period of the study, we constrained these coefficients to be equal over years. That is, although there are certainly reasons to expect that the level of the various variables will change from year to year, we have no theoretical reason to expect that the parameter values we are estimating will change during the relatively short study period. We are estimating the model pictured in figure 3. These equations are estimated simultaneously using the maximum likelihood estimator.[20]

For all equations, we allowed for one or two year lags to appropriately capture the temporality of these processes. These lags occur in part because of (1) the different waves of the survey; (2) the two different sources of data; and (3) the form of the question (whereas some questions to the respondents are retrospective over the previous year, some ask about current attitudes and opinions). Figure 3 presents the substantive relationships we estimate in the cross-lagged model.[21] As can be seen, there are one- or two-year lags between all constructs, including the feedback paths. The structural characteristics as well as whether the respondents have taken action to improve the neighborhood were measured in the odd years from 1995 to 2005. Social cohesion, feelings of responsibility and disorder were measured in the even years from 1996 to 2006.

----- Figure 3 about here -----

According to Waldo Tobler's first law of geography "everything is related to everything else, but near things are more related than distant things". Because most statistical techniques are based on the assumption of independence between observations, results may be biased when data have such spatial dependence (Bernasco and Elffers, 2010). Applying spatial models is a common way to account for spatial dependence in clustered observations (Anselin, 1988). In our models we accounted for global spatial autocorrelation over neighborhoods by including one spatial lag component for each dependent variable. We computed an inverse exponential distance decay weight matrix based on the five closest neighbors.[22]Given our longitudinal modeling framework, these spatially weighted variables were constructed based on the temporally lagged versions of these variables. Thus, e.g., the spatial variable for 'cohesion' refers to the effect of cohesion in surrounding neighborhoods at the previous time point. We are therefore testing whether cohesion in the focal neighborhood, and cohesion in the surrounding neighborhoods, at *t-2* affect social control *behavior* at *t-1*, disorder at *t-0*, etc.[23]

RESULTS

DESCRIPTIVE STATISTICS

Table 1 shows descriptive statistics of the variables in this study for each time point, averaged across the 74 neighborhoods. Ethnic heterogeneity ranges from 1% to 57% in 1995 and from 3% to 78% in 2005 – thus, there are big differences between neighborhoods with respect to the percentage of residents of foreign descent. On average neighborhoods have about 14% foreign residents in 1995 and 19% in 2005. The mean average income increases slightly over the years, and it ranges from about 9,000 Euros to 13,000 Euros per neighborhood resident, also indicating large variation between neighborhoods. Our measure of residential mobility, the percentage of respondents who have lived less than two years in each neighborhood, ranges from 0% to 50%. Because we used 'ecometrics' analysis (Raudenbush and Sampson, 1999) to construct our measure of disorder, this measure has by definition a mean of 0 across all neighborhoods across all years. During the course of time, disorder in the worst neighborhood seems to decrease somewhat, from 0.53 in 1996 to 0.32 in 2006.

The mediating variables, the percent of respondents who feel responsible for the livability and safety of the neighborhood, the percent of respondents who have taken actions to make the neighborhood more livable and safe, and the constructed measure of social cohesion also vary both between neighborhoods and between waves of data. The percentage of respondents feeling responsible for the livability and safety of the neighborhood seems relatively stable over time, with 84% in 1996 and 86% in 2006. However, between neighborhoods within each year, there is quite some variation: for example, in one neighborhood in 1996 67% of the respondents felt responsible compared to 96% of the respondents in another neighborhood. Similarly, across all waves the mean percentage of respondents who have taken actions on behalf of the neighborhood range from about 22% to 33%, while the range of this measure varies considerably across waves.

----- Table 1 about here -----

In table 2 we present the change over time formally by estimating the correlation coefficients between the measurements of each variable between the time points. Table 2 shows that residential mobility and the two measures of social control vary between the different time points. Although there is ample between-neighborhood variation of ethnic heterogeneity, socioeconomic status, and social cohesion, table 2 illustrates that the neighborhoods themselves are very stable over time on these latter characteristics.

Our measure of disorder both has a similar mean and range across neighborhoods across data waves (table 1), and remains stable over time for each neighborhood (table 2).[24] These results confirm Shaw and McKay's (1969) original thesis that neighborhoods remain relatively stable over time, which was the basis of their search for neighborhood explanations of crime and disorder. This will also have consequences for our models below, as this stability over time combined with our modeling approach of including lagged versions of the measures reduces our statistical power.

CROSS-SECTIONAL ANALYSES

We begin by estimating a recursive model in which we treat our data as cross-sectional. Thus, we pooled the data together and estimate a model as if all variables had been measured at the same point in time. With this approach we mimic previous studies, so that we can assess whether the general pattern observed in this setting in the Netherlands matches that discovered in prior research conducted in U.S. cities.[25]

----- Table 2 about here -----

As shown in table 3, we find that neighborhoods with a higher percentage of foreign residents (B= -.126, β= -.754, p < .001) and more population turnover (B= -.027, β= -.096, p < .1) have less social cohesion, this cohesion is associated with higher levels of feeling responsible for the neighborhood (B=1.212, β=.478, p < .001), and both social cohesion (B= -.205, β= -.314, p < .01) and feelings of responsibility (B= -.037, β= -.145, p < .05) appear to translate into lower levels of disorder. Thus, both of these structural measures appear to be mediated by cohesion and informal social control, as hypothesized. Furthermore, neighborhoods with higher income (B=.064, β=.295, p<.001) have a higher percentage of people feeling responsible for the neighborhood, which then appears to result in lower levels of disorder, also consistent with the hypothesized mediating effect. Moreover, neighborhoods with more population turnover (B=.051, β=.279, p < .001) appear to have a direct effect on experiencing more disorder.

----- Table 3 about here -----

Thus, our cross-sectional analyses replicate previous findings: certain structural neighborhood characteristics go hand in hand with less cohesion and less social control, and both of these mediate the relationship with disorder. We next turn to our longitudinal models that allow for temporal differentiation in these effects.

EXPLANATORY ANALYSES

Population turnover, income, and ethnic heterogeneity. To test hypothesis 1, we estimated a separate model which only takes into account that the neighborhood structural characteristics precede disorder in time, but in which we do not estimate the mediating effects of social cohesion or social control. We control for previous levels of all variables, including disorder, and we also control for spatial autocorrelation of all variables. Thus we can test, given the previous level of disorder, the additional effect of the three structural characteristics. Table 4 presents the results of this model.

----- Table 4 about here -----

Table 4 shows that the strongest predictor of disorder is the level of disorder at the previous time point, as expected given the correlations over time (B=.841, β=.886, p < .01).[26] Furthermore, the level of disorder in surrounding neighborhoods at the previous time point have a further positive effect on disorder in the focal neighborhood (B=.117, β=.075, p <.01). Thus, we see evidence of a clustering effect in which larger areas of multiple neighborhoods experience a worsening effect of disorder over time based on a diffusion effect from nearby neighborhoods. Note, however, that the relative importance of this spatial clustering effect is very small in comparison to the effect of disorder in the focal neighborhood at the previous time point. Population instability as measured by the percentage of new residents in the neighborhood as well as neighborhood income do not significantly affect subsequent disorder.[27] Higher ethnic heterogeneity leads to more subsequent disorder (*B*=.012, β=.088, $p < .01$), thus finding support for Hypothesis 1 only for this variable.[28]

We next turn to the full extended social disorganization model by adding mediation effects and feedback effects. Note that, in addition to the hypothesized effects in figure 2, we also estimated the direct effects of population stability, SES, ethnic heterogeneity, and cohesion on disorder. Thus we can test whether these variables retain a significant direct effect on disorder after controlling for the mediating variables. Although relationships are simultaneously estimated using full information maximum likelihood estimation in Mplus version 5.21, we discuss them in three separate sections. Table 5 presents the parameter estimates of the model.[29]

Cohesion and social control. Table 5 shows that population turnover has no significant effect on subsequent social cohesion, and thus we do not see support for *Hypotheses 2a*. When controlling for all other variables, higher neighborhood income seems to lead to less social cohesion (*B*= -.005, β= -.042, $p < .1$), contrary to *Hypothesis 2b*. A higher percentage of migrants living in the neighborhood leads to less cohesion one year later (*B*= -.021, β= -0.111, $p < .01$), which supports *Hypothesis 2c*. Due to the stability of social cohesion over time, the previous level of social cohesion is the strongest predictor of current cohesion (*B*=.870, β=.870, $p < .01$). The level of cohesion clusters positively in space: neighborhoods with high cohesion tend to cluster together, as do neighborhoods with low cohesion. A higher level of cohesion in surrounding neighborhoods leads to more cohesion in the focal neighborhood at a later time point (B=.130, β=.081, p<.01), although this effect is weaker than the effect of ethnic heterogeneity, let alone the cohesion in the focal neighborhood at the previous time point.

Table 5 further shows that cohesion is a strong predictor of the percentage of people feeling responsible for the livability and safety of the neighborhood (B=.541, β=.212, p < .01), thus finding support for *Hypothesis 3* for *potential* for social control. The effect of ethnic heterogeneity on *potential* for social control is mediated by cohesion. In addition, socioeconomic status directly affects the percentage of respondents that feel responsible for the livability and safety of the neighborhood (B=.041, β=.129, p < .01). The previous level of feelings of responsibility also affects subsequent levels of responsibility after controlling for the other variables (B=.138, β=.140, p < .05). Notably, we also find evidence that greater potential for social control in surrounding neighborhoods leads to a greater potential for social control in the focal neighborhood (B=.299, β=.173, p<.01).

----- Table 5 about here -----

Finally, table 5 presents support for a direct positive effect of social cohesion on the *actual* social control behavior, measured as the percentage of people taking actions to improve the neighborhood (B=1.021, β=.385, p < .01), similar to but even stronger than the effect of cohesion on the *potential* for social control. Thus we again find support for *Hypothesis 3*. Interestingly, we do not find a significant effect of *potential* social control on *actual* social control behavior. So, the percentage of people feeling responsible for the neighborhood has no relationship with the percentage of people actually taking action to improve the neighborhood, and we reject *Hypothesis 4*. In addition, the percentage of people in surrounding neighborhoods which takes action to improve their neighborhoods is not significantly related to the percentage of people taking action in the focal neighborhood.

It is important to emphasize that we find direct positive effects of all structural neighborhood characteristics on the subsequent percentage of people who take action to improve the neighborhood.

Higher levels of population turnover reduce this direct social control behavior (*B*= -.195, β= -.233, *p* < .01), whereas higher levels of income (*B*=.059, β=.177, *p* < .01) and ethnic heterogeneity (*B*=.159, β=.324, *p* < .01) increase the provision of such social control, when taking into account the other measures in the model. Shared *feelings* of responsibility do not seem to be a necessary precondition for actions on behalf of the neighborhood. This highlights the need to decompose social control into its *potential* and the *actual* social control behavior.

Causes of disorder. Table 5 shows that, as expected, previous levels of disorder strongly predict subsequent levels of disorder, net of the other explanatory variables (*B*=.832, β=.874, *p* < .01). Moreover, we find that controlling for all other variables, higher levels of disorder in surrounding neighborhoods lead to more disorder in the focal neighborhood (*B*=.127, β=.081, *p*<.01), although this effect is relatively very small.

In addition, we only find that more ethnic heterogeneity results in lower levels of disorder (*B*=.008, β=.057, *p* < .05). No other structural neighborhood characteristics or cohesion and social control have significant effects on subsequent disorder, which contradicts the hypotheses of social disorganization theory.

Feedback effects of disorder. We hypothesized that disorder would affect subsequent levels of cohesion, social control, and population turnover. As table 5 shows, we did not find empirical support for *Hypothesis 6a*, that disorder affects the level of social cohesion. However, all other hypotheses of feedback effects of disorder are supported. First, we find that with a high level of disorder, subsequently a lower percentage of people feel responsible for the livability and safety of the neighborhood (*B*= -.352, β= -.108, *p* < .05), thus supporting *Hypothesis 6b*. Second, we find support for *Hypothesis 6c*, as disorder leads to more subsequent *actual* social control behavior, measured as the percentage of people taking action to improve the livability and safety of the neighborhood (B=.977, β=.283, p < .01). Third, more disorder leads to a higher population turnover, supporting *Hypothesis 6d* (B=.620, β=.121, p < .01).[30]

SENSITIVITY ANALYSES

To assess the robustness of our outcomes, we estimated several ancillary models beyond the ancillary models already reported in various footnotes throughout the paper. First, we replaced the lagged variables of social cohesion and feelings of responsibility by these variables measured *in the same year* to the equation predicting disorder.[31] We see no evidence that a higher percentage of respondents feeling responsible for the neighborhood (B= -.006, p = .31) will reduce disorder at the same time point. With a different lag period we still get similar results for *Hypothesis 5*. We do find a significant effect for social cohesion: higher levels of social cohesion (B= -.070, p < .01) are related to lower levels of disorder at the same time point. However, because social cohesion and disorder are now measured at the same time point, we can no longer disentangle cause and effect. Lastly, we note that ethnic heterogeneity no longer affects the level of disorder at the next time point (B=.002, p=.52). Not specifying the lag period during which these processes should occur, would lead us to conclude that the neighborhood characteristics are fully mediated by social cohesion and social control, whereas the time-lagged model in table 5 suggests that ethnic heterogeneity does have a direct effect.

Second, we investigated whether the results of the longitudinal analyses were greatly influenced by including the *actual* social control behavior in our model. That is, the final model (table 5) not only differs from the initial cross-sectional models (table 3) in being longitudinal and taking account of spatial autocorrelation, but it also includes the percentage of respondents reporting they had been active to improve the livability and safety of the neighborhood. Might this somehow have caused the non-significant effect of *potential* for social control on disorder? To assess this, we estimated a model which did not include the *actual* social control behavior variable, but which otherwise was the same as the model in table 5. We still did not find evidence that the *potential* for social control directly affects disorder (*B*= -.004, p = .49). There are no substantive differences between this model and our results in table 5. Thus, we do not find any evidence that the *potential* for social control directly affects disorder when we use proper longitudinal data and take spatial autocorrelation into account.

CONCLUSION

This study used a longitudinal neighborhood-level dataset to test the central hypotheses of social disorganization theory (Shaw and McKay, 1969), namely that low socioeconomic status, high ethnic heterogeneity, and high population turnover lead to more neighborhood disorder. We also tested mediation effects of cohesion and social control as proposed by more recent revitalizations of this classic theory (inter alia Bursik and Grasmick, 1993; Markowitz et al., 2001; Sampson and Groves, 1989; Sampson, Raudenbush, and Earls, 1997). In addition, we simultaneously tested for feedback effects of disorder back to the structural neighborhood characteristics, cohesion, and social control at a later point in time (Robinson et al., 2003; Skogan, 1990). We next describe the main findings.

The first important theme of our findings was disentangling social control into the *potential* for social control and *actual* social control behavior. Prior scholarship generally does not make such a distinction. Controlling for all other effects, the *potential* for social control (i.e., shared feelings of responsibility) has no impact on subsequent disorder. This lack of effect also seems reasonable as potential offenders use visual cues to assess the neighborhood (St. Jean, 2007), which are partly provided by the level of disorder. Our results indicate that disorder itself leads to a population turnover and a breakdown of social control.

A second important theme was the striking difference in the results using neighborhood-level longitudinal data and controlling for previous levels of every variable in the models compared to cross-sectional models. In particular, we found no evidence that social cohesion, the percentage of respondents who feel responsible for the safety and livability of the neighborhood, or the percentage of people actually taking action to improve the neighborhood significantly affect levels of disorder at a later time point. Our measures of cohesion, *potential* and *actual* social control are quite similar to the measures used in previous studies, yet we did not replicate previous findings when estimating longitudinal models. It is important to highlight that our findings estimating cross-sectional models mimicked those of neighborhoods in other countries, suggesting that our differences are not due to the setting, but rather due to more appropriately allowing for a temporal lag when estimating these purported causal relationships. Thus we argue that our data are indeed comparable to previous studies. Proper neighborhood-level longitudinal data allows for controlling for previous levels of all variables, as well as allowing for time lags to take account of causality. As a result, we conclude that results from previous studies may partly have resulted from inappropriate assumptions about the directions of these relationships.

A third theme of this study was viewing the mediating effects of cohesion and informal social control (both the *potential*, and the *actual* behavior) between neighborhood structural characteristics and disorder. Controlling for previous levels of all explanatory and dependent variables, we found that only ethnic heterogeneity leads to lower levels of subsequent social cohesion. Higher average income of the neighborhood as well as a higher level of social cohesion increases the *potential* for social control.

The neighborhood structural characteristics consistently affected the *actual* social control behavior, measured as the percentage of respondents who said they had taken action to improve the livability or safety of the neighborhood. Higher income levels and a more stable population led to more action. In contrast to predictions of social disorganization theory, higher ethnic heterogeneity also resulted in more action. This may be due to more non-natives living in areas where there is something to 'improve' in the first place. In addition to these neighborhood structural characteristics, higher levels of cohesion lead to more people taking action to improve the neighborhood. However, we did not find that the *potential* for social control is related to more *actual* social control. As such, the *potential* for social control is not a necessary precondition for taking action to *actually* improve a neighborhood.

A fourth theme was the important role that disorder plays in generating neighborhood change. Because of the neighborhood-level panel dataset used in this study, we had the opportunity to also test feedback effects of disorder. We tested the effects of disorder on all of the explanatory variables at subsequent points in time: when controlling for lagged versions of each variable, we find support for the hypotheses that neighborhood disorder leads to less *potential* social control, more *actual* social control, and more population turnover. This suggests that disorder could be directly detrimental to informal social control (both directly and indirectly through population turnover), but not for the formation and maintenance of social cohesion.

In summary, the results suggest a cyclical model in which neighborhoods have relatively stable levels of disorder over time, and the processes which lead to disorderly neighborhoods are difficult to turn around. Neighborhoods with high levels of disorder cause more people to move out, and higher population turnover leads to a lower percentage of people taking action to improve the livability and safety of the neighborhood. Neighborhood disorder thus has cumulative effects over and above the direct effect on population turnover by reinforcing itself via a weakening of community processes of social control. Our simple descriptive statistics about the stability of neighborhood disorder through time underscore this conclusion.

DISCUSSION

Of course this study also has several limitations, and we can suggest several possibilities for further research. First, we did not simultaneously investigate the effect of social cohesion and social control on crime as well as on disorder. Research suggests that crime and disorder are both caused by the same community-level processes (Sampson and Raudenbush, 1999). The visual cues of disorder may lead to more crime in some neighborhoods and not in other neighborhoods, thus affecting population turnover, social cohesion and social control even more via crime. Second, our measure of disorder was provided by the neighborhood residents themselves, which undoubtedly introduces measurement error because of individual differences. Even though we employed the 'ecometrics' method (Sampson and Raudenbush, 1999) to control for some such individual differences, disorder may be even more objectively measured by systematic social observation. Third, due to our cross-lagged path models using 74 neighborhoods over a period of ten years, we were unable to estimate the relationships between variables for each time point separately; many more neighborhoods are needed to allow estimating this number of parameters. Fourth, our study focused on the city of Utrecht, one of the four largest cities of the Netherlands. Even though we feel this is a strength of our study, since we test social disorganization theory and feedback loops of disorder in a different context than the United States, the results are also less comparable to previous studies. Disorder, for one thing, is present less frequently in the Netherlands than in the average large U.S. city. We cannot determine whether the differences between our study and previous studies are caused by our longitudinal framework, cultural differences, or other unobserved differences. Nonetheless, the fact that the causes of disorder as proposed by social disorganization theory (i.e., structural neighborhood characteristics affecting social cohesion and social control) were not supported by our results in the Dutch context at the very least suggests certain scope conditions for the theory. Although the theory is posited to work in all places at all points, we found little evidence that disorder is fostered by these conditions within this cultural context. Fifth and finally, we stress that the interplay of different *actors* and their *actions* in the neighborhood is important to explain disorder. Even though social disorganization is a neighborhood-level theory, a focus on an individual level theory of action with regard to social control may be the most fruitful area of further research. Do people who *feel* responsible for the neighborhood actually *act* to improve it? From our own individual level data, only about 30 percent of these 'responsible' people have actually taken action to improve the neighborhood. Even in neighborhoods with a lot of disorder (> +2 standard deviations of disorder) more than two thirds of the respondents who state that they feel responsible for the neighborhood do *not* take action to improve it. Individual level analyses are needed to shed light on our neighborhood level outcomes.

Although this study has highlighted the importance of approaching these questions using longitudinal data, an implication is the importance of considering the proper temporal lag when viewing these processes. There is little theoretical guidance regarding this issue, and even less empirical evidence given the limited number of studies with longitudinal data allowing addressing this question. Although the call for longitudinal data is important, it is also necessary to consider the proper time period in which these processes occur such that data are collected at useful intervals. Intervals that are too long will essentially reduce the modeling to that of cross-sectional data, necessitating the modeling of simultaneous relations. Intervals that are too short will not be able to capture how the neighborhood changes in response to the particular stimulus. This is an important issue, and will necessarily be a key area for future research.

In conclusion, this study has made two main contributions to previous research: disentangling social control into the *potential* for social control and *actual* social control behavior, and taking into account the important role disorder plays in neighborhood dynamics. First, that residents *share feelings* of responsibility ultimately does not affect subsequent disorder: *actions* are necessary to improve the neighborhood. Although it is difficult to observe social control —it only occurs *in response* to problems— it nonetheless is important to not simply assume that intentions to provide social control will in fact be translated into action during times of difficulty. Second, our results using longitudinal data showed that disorder frequently appears to play a stronger role in how neighborhood composition and social control change over time than how these characteristics affect neighborhood disorder. The fact that we obtained results that mimicked the existing literature when treating our data in a cross-sectional manner provides a particularly strong challenge to the existing large body of prior research studying these processes with cross-sectional data. Thus, we highlight the need for much more concerted effort to study neighborhood processes with longitudinal data.

Figure 1.

Traditional Social Disorganization Model

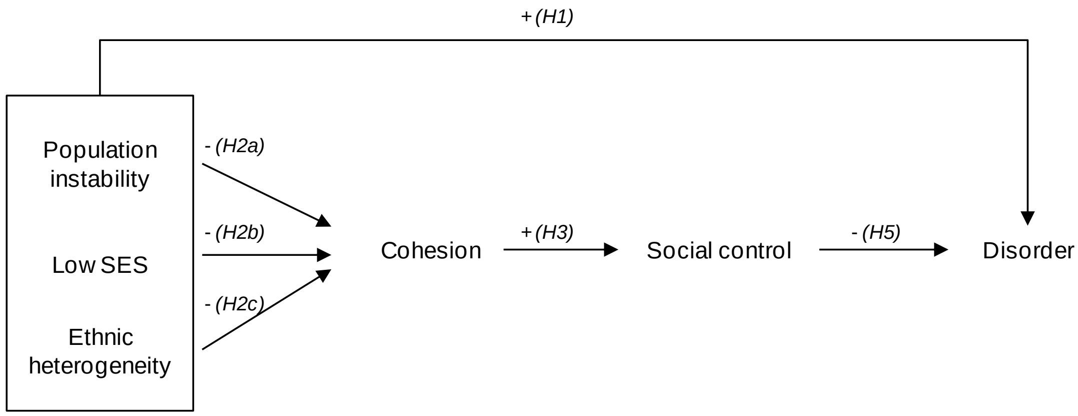
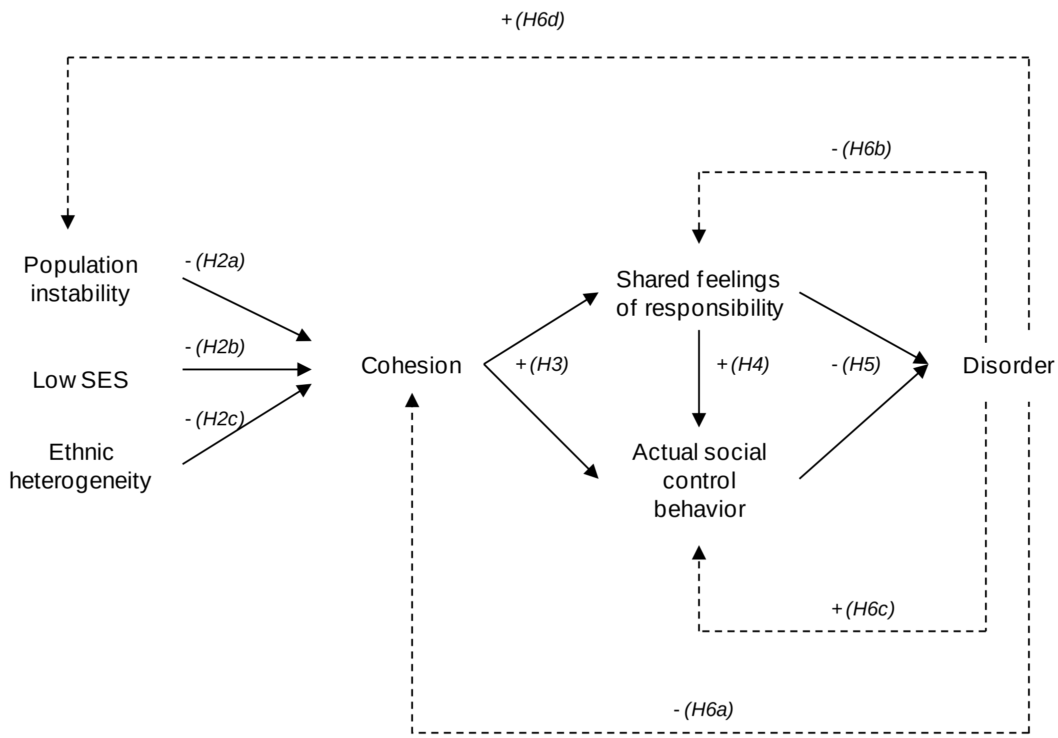


Figure 2.

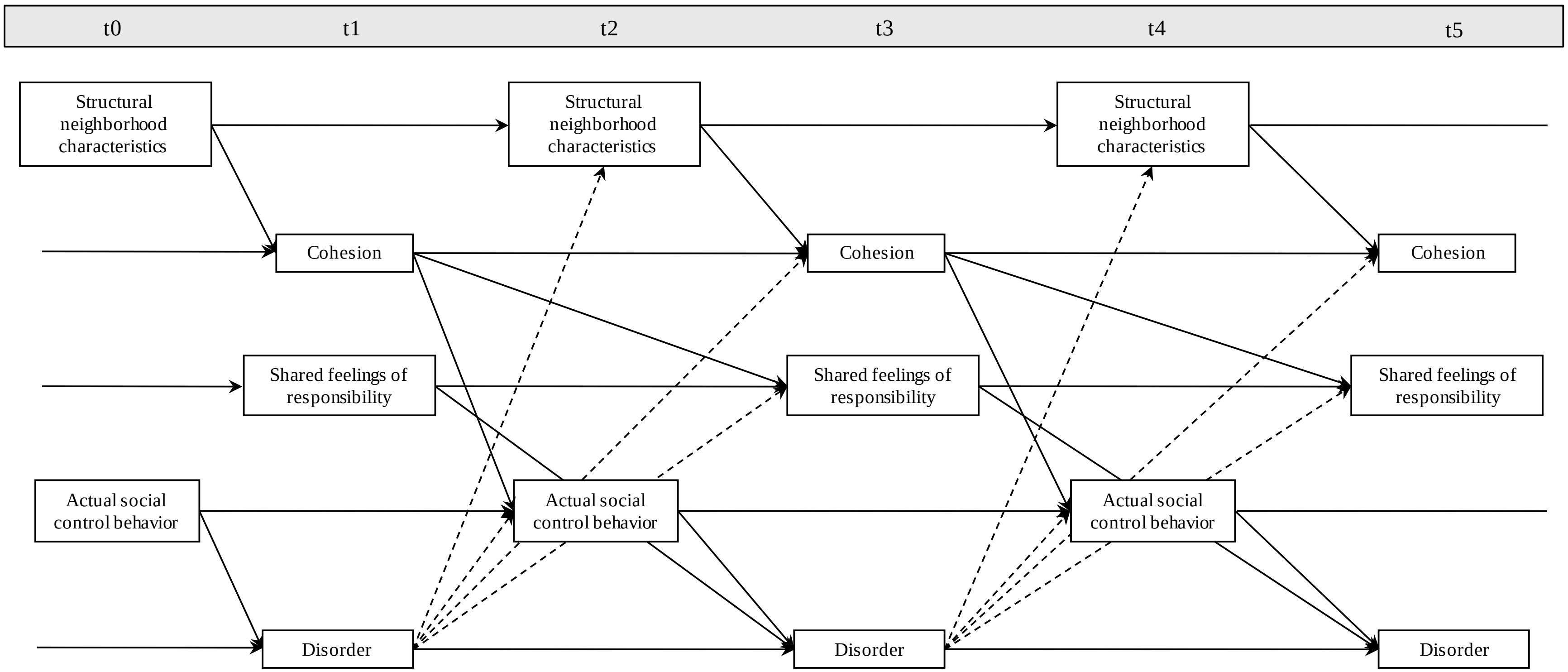
Extended Social Disorganization Model, including Feedback Effects



Note: arrows reflect temporal lags; this figure is for illustrative purposes only. See figure 3 for the estimated cross-lagged model.

Figure 3.

Cross-Lagged Model of Extended Social Disorganization Theory (t0-t5; substantive relationships).



Note: t0=1995, t1=1996, etc. Solid arrows indicate traditional social disorganization model; dashed arrows indicate feedback effects.

Table 1. Descriptive Statistics, 74 Dutch Neighborhoods, 1995-2006

	Min	Max	Mean	S.D.
disorder				
t1: 1996	-0.31	0.53	0.03	0.18
t3: 1998	-0.29	0.51	0.03	0.17
t5: 2000	-0.36	0.43	0.02	0.16
t7: 2002	-0.35	0.42	0.04	0.16
t9: 2004	-0.32	0.29	-0.02	0.14
t11: 2006	-0.39	0.32	-0.04	0.16
population turnover (% new residents)				
t0: 1995	3.8	39.5	19.1	7.6
t2: 1997	0.0	38.2	16.8	7.4
t4: 1999	0.0	50.0	19.0	7.7
t6: 2001	2.5	36.8	17.3	7.0
t8: 2003	5.9	36.4	22.6	7.3
t10: 2005	4.8	47.8	24.3	8.8
socioeconomic status (average income/1000 euro)				
t0: 1995	6.1	14.3	9.0	1.7
t2: 1997	6.1	15.5	9.7	1.9
t4: 1999	6.8	16.0	10.7	1.9
t6: 2001	7.3	18.3	11.8	2.2
t8: 2003	8.5	23.7	13.9	2.8
t10: 2005	7.7	20.8	13.0	2.6
ethnic heterogeneity (% foreigners)				
t0: 1995	1.0	57.0	13.6	11.7
t2: 1997	1.0	61.0	13.1	12.1
t4: 1999	3.0	71.0	16.7	13.8
t6: 2001	3.0	75.0	17.5	14.4
t8: 2003	3.0	76.0	18.1	14.9
t10: 2005	3.0	78.0	18.8	15.4
cohesion				
t1: 1996	-0.59	0.46	0.00	0.23
t3: 1998	-0.58	0.38	0.00	0.22
t5: 2000	-0.58	0.39	0.01	0.22
t7: 2002	-0.57	0.35	0.00	0.21
t9: 2004	-0.58	0.49	0.01	0.26
t11: 2006	-0.82	0.38	-0.08	0.29
potential for social control (% feel responsible)				
t1: 1996	66.7	96.0	84.2	6.0
t3: 1998	70.7	100.0	85.1	6.0
t5: 2000	72.7	100.0	87.4	5.5
t7: 2002	71.3	96.4	87.2	5.9
t9: 2004	80.0	100.0	90.3	4.6
t11: 2006	50.0	96.7	86.0	8.2
actual social control behavior (% active to improve neighborhood)				
t0: 1995	5.3	50.0	21.5	7.9
t2: 1997	8.1	37.5	21.5	5.7
t4: 1999	13.0	57.1	26.2	8.6
t6: 2001	13.6	73.7	30.6	8.7
t8: 2003	12.6	39.0	26.2	5.8
t10: 2005	14.3	56.0	32.7	7.9

Note: population turnover, ethnic heterogeneity, potential for social control (i.e., % feeling responsible), and actual social control behavior (i.e., % active to improve the neighborhood) were divided by 10 in the models for estimation purposes. Source: NUP, first imputation.

Table 2. Correlations over Time for all Variables, 74 Neighborhoods, 1995-2006

disorder

	t1	t3	t5	t7	t9
t3	.957**				
t5	.935**	.943**			
t7	.931**	.940**	.957**		
t9	.899**	.922**	.922**	.917**	
t11	.830**	.859**	.865**	.869**	.938**

cohesion

	t1	t3	t5	t7	t9
t3	.960**				
t5	.934**	.947**			
t7	.932**	.948**	.962**		
t9	.921**	.930**	.962**	.956**	
t11	.876**	.895**	.936**	.926**	.960**

socioeconomic status

	t0	t2	t4	t6	t8
t2	.966**				
t4	.934**	.969**			
t6	.912**	.924**	.962**		
t8	.893**	.916**	.955**	.975**	
t10	.893**	.920**	.940**	.939**	.957**

ethnic heterogeneity

	t0	t2	t4	t6	t8
t2	.987**				
t4	.966**	.987**			
t6	.932**	.963**	.988**		
t8	.901**	.939**	.970**	.984**	
t10	.870**	.917**	.954**	.968**	.991**

potential for social control (% feel responsible)

	t1	t3	t5	t7	t9
t3	.532**				
t5	.479**	.365**			
t7	.241*	.382**	.520**		
t9	.466**	.481**	.443**	.501**	
t11	.488**	.393**	.633**	.388**	.529**

population turnover

	t0	t2	t4	t6	t8
t2	.534**				
t4	.339**	.458**			
t6	.225	.281*	.556**		
t8	.534**	.549**	.633**	.463**	
t10	.428**	.420**	.621**	.516**	.715**

actual social control behavior (% active to improve)

	t0	t2	t4	t6	t8
t2	.239*				
t4	.174	.368**			
t6	.251*	.357**	.244*		
t8	.293*	.513**	.335**	.350**	
t10	.212	.186	.316**	.161	.426**

** $p<0.01$; * $p<0.05$ (2-tailed). Source: NUP, first imputation.

Table 3. Full Information Ml Parameter Estimates of Cross-Sectional Analyses of the Traditional Social Disorganization Model, 74 Neighborhoods, 1995-2006

	cohesion		potential for social control (% feel responsible)		disorder	
	B	SE	B	SE	B	SE
intercept	0.154	0.146	7.817	0.438**	0.249	0.188
population turnover	-0.027	0.015+	-0.010	0.055	0.051	0.014**
socioeconomic status	0.005	0.009	0.064	0.021**	-0.008	0.008
ethnic heterogeneity	-0.126	0.015**	0.010	0.039	-0.001	0.015
cohesion			1.212	0.176**	-0.205	0.074**
potential for social control					-0.037	0.019*
yr1998	0.005	0.042	0.068	0.167	0.094	0.037*
yr2000	0.062	0.028*	0.225	0.119+	0.087	0.028**
yr2002	0.045	0.024+	0.155	0.123	0.120	0.024**
yr2004	0.068	0.013**	0.318	0.088**	0.066	0.015**

** *p*<0.01; * *p*<0.05; + *p*<0.1 (2-tailed).

Table 4. Full Information ML Parameter Estimates of Structural Neighborhoods Characteristics on Disorder, 74 Neighborhoods, 1995-2006

	disorder	
	B	SE
socioeconomic status	-0.001	0.002
ethnic heterogeneity	0.012	0.003**
population turnover	0.000	0.004
disorder at previous time point	0.841	0.025**
spatial coefficient	0.117	0.037**

** *p*<0.01; * *p*<0.05; + *p*<0.1 (2-tailed).
Note: cross-lagged model, including spatially lagged measure of temporally lagged disorder.

Table 5. Full Information Ml Parameter Estimates of Extended Social Disorganization Model, 74 Neighborhoods, 1995-2006

	cohesion		potential for social control (% feel responsible)		actual social control behavior (% taken actions)		disorder		population turnover	
	B	*SE*	*B*	*SE*	*B*	*SE*	*B*	*SE*	*B*	*SE*
population turnover	0.002	0.006	0.001	0.034	-0.195	0.070**	0.002	0.005		
socioeconomic status	-0.005	0.003+	0.041	0.015**	0.059	0.022**	-0.001	0.002		
ethnic heterogeneity	-0.021	0.005**	-0.030	0.033	0.159	0.047**	0.008	0.004*		
lagged variables										
cohesion	0.870	0.030**	0.541	0.205**	1.021	0.264**	-0.015	0.021		
potential for social control			0.138	0.056*	0.082	0.080	-0.005	0.006		
actual social control behavior					0.211	0.057**	0.005	0.004		
disorder	0.008	0.026	-0.352	0.174*	0.977	0.269**	0.832	0.025**	0.620	0.242*
population turnover									0.418	0.062**
spatial coefficient	0.130	0.033**	0.299	0.111**	-0.037	0.099	0.127	0.036**	0.354	0.122**

** $p<0.01$; * $p<0.05$; + $p<0.1$ (2-tailed).
Note: cross-lagged models, including spatially lagged measure of temporally lagged outcome.

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NOTES

[1] Sampson and Groves (1989) used "the percentage of residents who reported that disorderly teenage peer groups were a 'very common' neighborhood problem" as a mediating variable to explain crime rates, which they proposed as an indicator of the extent to which the community is unable to control peer-group dynamics. In later research, this measure was used not as an indicator of community organizational ability, but as an outcome in itself: delinquency. Here we follow the more recent convention and thus describe their paper as a test of social disorganization theory with respect to delinquency.

[2] Sampson and Raudenbush (1999) argue that the logic of their analytic approach shares more affinity with routine activity theory than with social disorganization theory. Indeed, (Shaw and McKay, 1969) thesis involved the production of *offenders* by neighborhood conditions. However, more recent developments of the theory also generalize the theory to the neighborhoods themselves as units of control or guardianship, regardless of where offenders may reside. Indeed, Sampson and Groves (1989) discuss the theory in this manner without ever referring to routine activity theory.

[3] Informal social control is informal regulatory behavior of the actions of others. More specifically, the relevant research that aims to explain neighborhood differences in disorder is concerned with *parochial* control, which refers to "relationships among residents that do not have the same sentimental basis as affective networks" (Bursik and Grasmick, 1993: 35). Three types of such social control are identified: (1) informal surveillance: that is, observation of neighborhood streets that is engaged in by individuals during daily activities, (2) movement-governing rules with regard to which areas to avoid, and (3) direct intervention, e.g., questioning strangers about suspicious activities or chastening children for unacceptable behavior. In the body of research relevant to the present study, authors generally refer to social control of the first or the third type.

[4] Note: the hypothesis concerning the effect of social control on disorder is hypothesis number 5, because the model is currently incomplete. In the next section, we will extend the model depicted in figure 1. An extra effect, which precedes the effect of social control on disorder in time, will then be captured by hypothesis number 4.

[5] Participation in organizations is arguably a form of 'public' social control (Bursik and Grasmick, 1993: 17). However, it is often unclear, and untested, if participation in organizations leads to more actions by each of the organization's members, or that the organization itself is expected to help combat collective problems.

[6] Bellair and Browning (2010) make similar arguments, although they refer to "community networks" instead of cohesion. Their analyses show that indicators of community networks and informal control reflect different dimensions of a multitrait construct, and that networks exert indirect effects on crime rates through informal control. However, these results are not based on longitudinal data.

[7] Note that there are many reasons *why* a resident might not be willing to intervene in such instances. One reason could be a fear of retribution, that could occur in instances of informal social control (directly intervening) or from formal social control (if actors become aware who notified authorities). Assessing such motivations is outside the scope of our study.

[8] Some studies (e.g., Rountree and Land, 1996) suggest a complex relationship between crime and cohesion, in which mediation by perceived risk as well as fear of specific types of crime plays a role. However, we are not able to test such fear-related or risk-related mechanisms in our study.

[9] Here we will assume, based on theoretical notion by Skogan (1990) and empirical work by Sampson and Raudenbush (1999) that the results of these studies might also apply to the explanation of disorder.

[10] For crime and disorder, the Statistics Netherlands (http://www.cbs.nl) only provides freely available data which is aggregated to the police region level.

[11] In addition, we used 'ecometrics' analysis to create some of our neighborhood level variables, thus explicitly taking sample size as well as individual perceptual bias into account. This is explained more fully in the 'measures' section.

[12] We also tried different ways to construct neighborhood-level measures. We performed confirmatory factor analysis (CFA) to construct factor scores for disorder and cohesion at the individual level (these CFA models were estimated with a weighted least squares estimator on the polychoric correlation matrix in Mplus 5.21 to account for the ordinal nature of the data). We used the regression scoring technique to output these factor scores from Mplus.

Then, we used (1) a multilevel model mirroring 'ecometrics', but without the extra item-level, to construct neighborhood-level scores; and, (2) a fixed-effects model. The correlations between the different approaches are more than .96 for both cohesion and disorder. Therefore we decided to continue with the accepted 'ecometrics' approach described in the main text.

[13] We accounted for possible biasing effects by including several individual- and household-level measures. These include the following; gender, age, age squared, length of residence, household income, education level, home ownership, employment status, Moroccan, Turkish, other race, single parent household, married with no children, married with children, and other marital status.

[14] We also included a separate 'survey year' level, because items are nested within respondents nested within years nested within neighborhoods. Then the empirical Bayes estimate at the neighborhood-level plus the estimate at the year-level is the 'true' score. We compared this method to a three level (item-individual-neighborhod) model which simply includes an indicator (dummy) variable per survey year. The resultant neighborhood-level measures correlated with $r > .9$.

[15] Previous studies have also used the median and/or variance of income, or a composite measure reflecting the SES of the neighborhood. However, such measures were not available to us for each time point.

[16] We did not use the formula $1-\Sigma p_i^2$, an index capturing the diversity in the neighborhood, because in such a diversity measure a neighborhood with 80% natives and 20% non-natives will receive the same value as a neighborhood with 20% natives and 80% non-natives. Therefore, we chose to use the original percentage values of the ethnic group.

[17] As discussed, we used MI to deal with missing individual-level values and then constructed neighborhood-level scores. However, we also used measures reflecting socioeconomic status and ethnic heterogeneity which were directly provided by the Statistics Netherlands on the neighborhood-level. These neighborhood-level measures contained a few missing values, and therefore we used the Full Information Maximum Likelihood procedure to estimate the cross-lagged path models.

[18] One problem we encountered was that our full model contained more parameters than observations (given that there are just 75 neighborhoods), which calls into question the estimated standard errors. To assess the effect of this on our results, we estimated ancillary models that were subsets of our full model (not containing various mediating measures). For example, one estimated cross-lagged model contained disorder, cohesion, and feelings of responsibility, whereas another contained disorder, cohesion, and action to improve the neighborhood (and treating the other neighborhood structural variables in each of these models as exogenous measures). The parameters and standard errors in each of these models were very similar to those in our full models, increasing confidence in the results.

[19] We also estimated the models with the data stacked such that each row in the data represented a neighborhood time point. A disadvantage with this approach is that it does not allow directly addressing the endogeneity of the measures at time points and possible autocorrelation. Nonetheless, it is reassuring that these alternative model specifications resulted in similar substantive results as the models reported in this paper. The main difference was that actual social control behavior actually had a significant positive effect on subsequent disorder in these alternative models (though it was still smaller than the effect from disorder to subsequent actual social control behavior). Thus, our results are robust across different across different methods of analysis.

[20] Another approach with such data would estimate latent trajectory models. This approach is very appropriate when testing for long-term trends. However, we do not think it is theoretically plausible that the level of cohesion at the beginning of the study period then monotonically affects the trajectory of disorder in neighborhoods over the entire study period. We think it is more appropriate to model the relationship between cohesion or informal social control and disorder in a much shorter temporal framework: cohesion levels likely affect disorder in the near future of the next year or so, but we explicitly hypothesize that levels of disorder then impact levels of cohesion in an equally short time period. A latent trajectory model would ignore all of these short-term reciprocal effects.

[21] Note: for example, we also estimate direct effects from the structural neighborhood characteristics and social cohesion on disorder. All estimated relationships are presented in the tables in the Results section.

[22] We also constructed spatially weighted versions of the temporally lagged *independent* variables and re-estimated the models in this paper including those variables. In most equations the spatially lagged independent variables were insignificant, and, importantly, the substantive results of the models showed no change in the equations in which the spatially lagged variables had a significant effect. Therefore we decided to present the most parsimonious models in the tables, in which we only include spatially lagged version of (the temporally lagged) dependent variables.

[23] We explored two additional model specifications. In one, we specified these spatially lagged variables as endogenous (predicted by the spatially lagged versions of the variables in our main structural equations). In the other, we specified models that did not include these spatially lagged measures. In each instance, the substantive results were essentially the same as those in our main models. Thus, whether or not we include these spatially lagged measures, and whether or not we specify them as endogenous, does not change the substantive results in any way.

[24] The stability of disorder could partly be the result of using perceptions of disorder measured on a five-point scale instead of observations of disorder. In addition, the stability of our measure is also partly the result of the more precise 'ecometrics' method with which the variable was constructed: neighborhood-year combinations are assumed to be a sample of the true population and the measures of disorder are shrunk towards to mean based on the number of respondents within each neighborhood. Studies which have not employed the ecometrics strategy to create neighborhood-level variables from individuals' perceptions may have underestimated the stability of neighborhood characteristics over time due to failing to account for measurement error.

[25] In principle, we could have used the data from all of the years: 1996-2006. However, in our longitudinal analyses we do not include disorder as an outcome variable in 1996 because we cannot control for the previous level of disorder, in 1994. Therefore, to maintain comparability, the cross-sectional analyses were performed with the exact same time points as our longitudinal analyses: 1998-2006. In addition, 2006 was used as the reference category of time point, so that we report the parameter estimates of years 1998, 2000, 2002, and 2004.

[26] The standardized parameter estimates differ slightly for each time point. We only present the standardized parameter estimates of the first time point in the text, because the standardized coefficients change only slightly between time points, and the substantive interpretation of relative importance does not change between time points.

[27] We get similar results for a model that does not control for spatial autocorrelation, although the effect of neighborhood income on disorder in that model is more significant (B=-.002, $p < .1$). All other parameter estimates are comparable.

[28] Note that while we do not show these controls in the table, the previous levels of the explanatory variables also strongly predict subsequent levels of these variables. This holds for all variables: population turnover (B=.462, p<.001), income (B=1.008, p<.001), and ethnic heterogeneity (B=1.034, p<.001). In addition, we find evidence for spatial clustering effects over time for neighborhood income (B=.085, p<.01) and population turnover (B=.408, p<.01).

[29] We also tested various interaction effects in an exploratory fashion, but these were not significant.

[30] We also tested whether the disorder of surrounding neighborhoods (i.e., a spatially weighted version of the temporally lagged disorder) affected these neighborhood characteristics at a later time point. We did not find any evidence for such effects.

[31] Due to multicollinearity, it was not possible to include both the lagged versions of social cohesion and social control and the non-lagged versions of these variables in the same model.