ORIGINAL PAPER

Reciprocal Effects of Victimization and Routine Activities

Margit Averdijk

Abstract Although there is much research on the relationship between routine activities and victimization, we have little knowledge about the reciprocal effects of victimization and routine activities. The current paper is framed within the Once Bitten Twice Shy perspective proposed by Hindelang et al. (Victims of personal crime: an empirical foundation for a theory of personal victimization. Ballinger, Cambridge, 1978) which argues that victimization decreases risky routine activities and that this in turn decreases the risk of victimization. The current paper tests these propositions by using longitudinal data from the National Crime Victimization Survey, which allows us to tease out victimization and routine activities over time. Both violent and household victimization are examined. Variables pertaining to how often respondents go out for shopping, how often they go away at night and whether they have household devices are used as indicators for routine activities. Results indicate that the reciprocal effects of victimization and routine activities are limited. Consequences for routine activities theory are discussed.

Keywords Victimization · Routine activities · Longitudinal data

Introduction

A considerable amount of criminological literature suggests that people who adhere to risky lifestyles and engage in risky routine activities are more likely to become the victim of a crime (e.g. Cohen and Cantor 1980; Cohen and Felson 1979; Gottfredson 1984; Hindelang et al. 1978; Lynch 1991). The leisure activities one undertakes, the type of job one has, and the environments in which one moves around are all related to one's risk of victimization. However, causality should be expected to run in the opposite direction as well: one should expect victimization to influence routine activities. Victimization can be a very unpleasant, even traumatizing, experience. Victims should be expected to be motivated to avoid victimization in the future, to undertake preventive measures, and to

M. Averdijk (✉)
University of Zurich, Institute of Education, Büro F 15a, Freiestrasse 36, 8032 Zurich, Switzerland
e-mail: maverdijk@ife.uzh.ch

transform risky lifestyles and routine activities into safer ones where possible. Furthermore, due to changes in lifestyle and routine activities, one should expect victims to have reduced their risk of victimization. These expectations are captured in the saying 'Once bitten twice shy' (Hindelang et al. 1978).

However, empirical research consistently demonstrates that victims have a higher risk of victimization than non-victims, and that previous victimization is one of the best predictors of future victimization (Pease 1998). Why is this? Do victims not change their routine activities after victimization? Or do they change, but is the change not effective in preventing future victimization? Lifestyle theory implies that daily activities are influenced by structural factors such as demographics, role expectations and structural constraints (Hindelang et al. 1978). Hence, maybe victimization cannot be expected to have too large an influence on daily activities.

To date, the mechanisms articulated in the *Once bitten twice shy* perspective are still poorly understood. Most research has relied on cross-sectional data (see e.g. Skogan and Maxfield 1981; Ferraro 1995; Lurigio 1987; Lavrakas 1981; Rountree and Land 1996). Although cross-sectional research is very valuable, longitudinal data are indispensable to study changes in routine activities and change in victimization over time. As Hindelang et al. (1978) argued, 'many of the important linkages in the model, however, can only be tested adequately with longitudinal data' (p. 271). Therefore, to study the mutual effects of victimization and routine activities over time, the current paper uses panel-data from the National Crime Victimization Survey (NCVS), the major population survey on criminal victimization in the United States. Variables focusing on how often respondents go shopping, how often they spend the evening away from home, and whether they have household devices are used as indicators for routine activities.

This paper aims to advance current knowledge about the relationship between victimization and routine activities by investigating two questions. First, it is examined whether victims restrict their involvement in risky routine activities after victimization. Second, the effect of routine activities on the risk of victimization is investigated.

Theory and Prior Research

This section addresses the two relationships currently under investigation. First, the impact of victimization on routine activities is explored. The second part highlights how routine activities affect victimization risk.

The Impact of Victimization on Victims' Routine Activities

Criminal victimization can have various consequences. Not only does it inform victims on how unpleasant criminal victimization can be, but it also updates them on their risk of becoming victimized. Consequences of victimization can be classified into three types: emotional, practical and cognitive consequences.

First, emotional consequences are among the most extensively investigated consequences of crime (for an overview, see also Shapland and Hall 2007). Among the most likely emotional reactions to victimization are anger, fear, shock and insecurity (Mayhew 1993). A wide range of distressing symptoms has also been reported (Lurigio 1987). Much research has investigated the difference in fear levels between victims and non-victims, and although there has been debate on the conclusiveness of the victimization-fear link, findings indicated that victims show higher levels of fear of crime than non-victims

(Garofalo 1979; Heiskanen et al. 1991; Hindelang et al. 1978; Lurigio 1987; Quann and Hung 2002: Skogan and Maxfield 1981; Skogan 1987; Tseloni and Zarafonitou 2008).

Different crime types yield different emotional consequences. Victims of serious violent crimes report extensive emotional reactions (Gale and Coupe 2005; Lejeune and Alex 1973; Shapland and Hall 2007) but there is also evidence that property crimes, especially burglary, have emotional consequences (Mawby 2001; Mayhew 1993; Maguire 1980; Shapland and Hall 2007; Paap 1981). In a panel-study, Norris et al. (1997) found that victims of both violence and property crime were distressed, with violence victims being the most severely distressed. In another panel study, Finkelhor et al. (2007) found that experience with a range of different crime types was highly predictive of trauma symptoms among children.

The second type of consequences is of a more practical nature. These consequences include the inability to use stolen items, the trouble and monetary expense of replacing stolen items and the inability to work because of injuries. Mayhew (1993, p. 193) reports that overall 'inconvenience, nuisance and other practical problems' were perceived to be the worst problems of victimization by victims, especially in cases of property crimes that did not include burglary.

Third, victimization can have cognitive consequences in the sense that victimization can lead to higher levels of awareness regarding one's victimization risk. Rountree and Land (1996) found that previous burglary victimization was positively related to crime risk perception while Ditton and Chadee (2006) and Lurigio (1987) suggested this conclusion to extend to a wider array of crime types. In addition, Lejeune and Alex (1973) found that mugging victims displayed 'a new sense of vulnerability [and] an awareness of the self as a potential target' (p. 273). Tyler (1980) supported this evidence by reporting that victims experience elevated judgments of personal vulnerability.

Whether consequences of victimization are emotional, practical, or cognitive in nature, all are generally unpleasant—ranging from mildly unpleasant to traumatic—and therefore the *Once Bitten Twice Shy* (henceforth OBTS) perspective predicts that victims will reduce risky behavior in order to prevent future victimization. This rationale can be based on a psychologically oriented approach, by interpreting victimization as a learning event that leads victims to display preventive behavior. Moreover, Cook (1986) has argued that risk perception influences the extent to which individuals engage in self-protection efforts. According to Kahneman and Tversky's (1979) prospect theory, individuals do not always act fully rational in the face of rare risks. Risks with small probabilities (such as criminal victimization) are often overweighed. In general it has been found that 'losses loom larger than gains' (p. 279) and that people tend to prefer a small loss over a small probability of a large loss. It appears that in the face of a small probability of being victimized, people prefer a restriction of risky routine activities over the potentially large loss that victimization may cause.

Although intuitively it makes sense to say that victimization results in unpleasant consequences and that therefore victims will try to prevent future victimization, the relationship is not that clear-cut. There are several arguments to be made that victimization does *not* necessarily lead to behavioral change (see also Skogan 1981).

First, not all crimes are perceived as serious and therefore costs of criminal victimization may not always be evident. Consequently, there may not be many benefits to preventing re-victimization. Generally, surveys have found that people only report small numbers of serious crimes and many more minor incidents (Zedner 2002). Many crimes are seen as relatively minor experiences without serious consequences and long-lasting

effects (Fattah 1997; Mayhew 1993; Skogan and Maxfield 1981). Therefore, victimization cannot be expected to automatically induce (lasting) behavioral change.

Second, as Skogan (1981) pointed out, 'there is no such thing as cost-free crime avoidance' (p. 37), and 'it may be rational to choose to do nothing' (p. 19). Taking measures for crime prevention can be a costly enterprise and it may disrupt the structure of daily routines which revolve around much more complicated matters than crime prevention. As Skogan and Maxfield (1981) pointed out: 'The expectations of others based upon social role, the structures of race and class, the discipline of the time clock, the demands of family life, and neighborhood customs and physical design all shape what we can and cannot do about crime' (p. 188).

Third, and related to this, some people are better capable of undertaking preventive activities than others due to differences in opportunity structures (Skogan and Maxfield 1981; Skogan 1981). These opportunity structures are determined by social structures and stratification, but also by psychological and environmental factors. Schreck et al. (2006) for example, proposed that the extent to which victims change risky routine activities depends on levels of self-control.

Overall, it should be noted that lifestyles and routine activities depend on role expectations and structural constraints which in turn are influenced by demographic characteristics such as age, marital status, and education (Hindelang et al. 1978). Hence, daily activities are influenced by structural factors that are not easily changed. An example of an important structural constraint is the neighborhood of residence. Prior research has shown that neighborhood characteristics such as affluence, ethnic heterogeneity, and social disorganization are related to victimization of several crime types in the US (Smith and Jarjoura 1989; Lauritsen 2001; Rountree et al. 1994), Canada (Kennedy and Forde 1990), England and Wales (Tseloni et al. 2002), Sweden (Estrada and Nilsson 2008), and the Netherlands (Van Wilsem et al. 2006). Effects of routine activities and prior victimization may also be mediated by area characteristics. Rountree et al. (1994) found that the effects of prevention strategies and guardianship are smaller in disadvantaged areas compared to advantaged areas: 'residents in more disadvantaged areas must "try harder" to obtain the same results as in less disadvantaged areas' (Rountree et al. 1994, p. 411). Tseloni and Pease (2004) found that the effects of prior victimization depend on size of the place of residence, with effects varying more in places that are densely populated compared to places that are less densely populated. Furthermore, for those who shop daily, prior non-victimization is less protective against subsequent victimization than for those who shop less frequently. In addition, for those who shop daily, living in a densely populated area is more risky than for those who shop less frequently. In terms of policy recommendations, Tseloni and Pease (2004) concluded that since victimization risk is linked to personal, household, and lifestyle characteristics 'Changing such characteristics to reduce victimization risk is unfeasible and probably unethical' (p. 944). Thus, structural constraints may play an important role in the extent to which victims can (effectively) change daily activities to prevent future victimization.

Summing up, the likelihood that victims adapt their behavior after a crime depends on the seriousness of the crime, the costs of changing behavior and the opportunity structures and structural constraints victims are faced with.

In terms of empirical evidence, research has found that victims change their routine activities in the aftermath of victimization. For example, based on survey data from Chicago, Philadelphia, and San Francisco, Skogan and Maxfield (1981) found that 62% of robbery victims and 52% of burglary victims changed activities in general 'because of crime', while 47% of non-victims did.

Ferraro (1995) found that out of several preventive behaviors (including buying weapons, learning self-defense, buying locks, and avoiding unsafe areas), victimization was only positively and significantly related to having a watchdog and to changing daily activities in the past year. In a telephone survey in Detroit, Lurigio (1987) found that victims were more likely to look out for suspicious people, avoid strangers during walks and check behind their front door before entering their home than non-victims. Skogan and Maxfield (1981) reported that '[a]bout 7% more victims than nonvictims reported having special protective devices on their windows, and taking certain steps to provide for the protection of their homes while absent was slightly more common among those who had been victimized' (p. 218). Lavrakas (1981) found that for renters (but not for home owners), burglary experiences were a significant predictor for home protection indices. Rountree and Land (1996, p. 174) reported that 'previous experience with crime or victimization and subsequent increases in perceived risk or feelings of being unsafe, in turn, are significant predictors of restricted routine activities in terms of safety precautions'. These safety precautions included locking doors, using extra locks, leaving lights on, being a member of a crime prevention program, owning a burglar alarm, owning a dog, having neighbors watch the house, and owning a weapon. In a case study, Paap (1981) related efforts to protect property such as installing an alarm system to burglary victimization.

In an interview study, Gale and Coupe (2005) found that victims of robbery reported taking precautions and restricting their behavior after victimization experiences, including 'changing social behavior, avoiding risky places or people, and modifying their appearance so that they would be less attractive targets' (:15). In their interview study with mugging victims, Lejeune and Alex (1973) reported examples of precautionary behavior such as avoiding the particular location where the crime took place, avoiding going out at night and considering moving.

While these studies form the cornerstone of our knowledge on the influence of victimization on routine activities and while they have provided us with much knowledge on the relationship, they have two limitations. First, not all of the quantitative studies use statistical tests to back up their findings. Second, they use cross-sectional data. To draw more robust conclusions, longitudinal data are indispensable. Two exceptions are Skogan (1987) and Dugan (1999). Skogan (1987) constructed a two-wave panel study, with pre- and post-test measures for 'defensive actions against personal crimes' (going out with others for safety reasons, avoiding certain areas, avoiding certain types of people, and staying home because of crime) and 'household crime prevention efforts' (installing special locks, lights, timers, etc.). Respondents came from relatively high-crime neighborhoods in Newark and Houston. Skogan found that victims reported higher levels of defensive behavior and that victims of property but not personal crime showed higher levels of household protection.

In another panel study, Dugan (1999) used the National Crime Survey (NCS; the former version of the NCVS) to investigate the relationship between victimization and moving. She reports that decisions to move were significantly related to recent property (but not violent) victimization. Xie and McDowall (2008a) replicated Dugan's results that property victimization significantly predicted moving, but violent victimization predicted moving more strongly than property victimization. These results are interesting in light of Hindelang et al.'s (1978) assertion that 'the behavioral effects of crime or the fear of crime appear more as *subtle adjustments* in behavior than as major shifts in what can be called "behavioral policies" (p. 224; italics in original). Dugan's and Xie and McDowall's results suggest that victim reactions need not be subtle but can be quite drastic. In addition, research by Ellingworth and Pease (1998) suggested that the risk of victimization is higher

both before and after moving, although the authors noted that due to the cross-sectional design of their data (British Crime Survey), they could not draw causal conclusions.

The Impact of Routine Activities on Victimization

The second research question examines how routine activities affect future victimization risk. According to the OBTS perspective, victims experience unpleasant consequences and will try to prevent future victimization by adopting preventive behavior. It is assumed that this preventive behavior is successful and that victims have a decreased risk of victimization after the crime.

According to routine activities theory and lifestyle theory, people with more risky daily activities run a higher risk of being victimized (Cohen and Felson 1979; Felson 1994; Hindelang et al. 1978). Consequently, it seems natural to think that changes in routine activities also lead to changes in victimization risk. More specifically, people who change their routine activities to less risky daily activities are expected to have a lower risk of victimization.

Empirical evidence for the link between routine activities and changes in (re-) victimization is scarce. Not many authors have investigated whether individual preventive behavior is effective. Although some studies present correlations between preventive behavior and reduced victimization risk, conclusions are often clouded by cross-sectional designs (DuBow et al. 1979). The study by Miethe et al. (1990) is an exception. They examined whether lifestyle changes were related to changes in victimization risk based on a two-wave panel from the NCS. They found that the adoption of more risky lifestyles over time was associated with a higher risk of victimization. However, an increase in precautionary actions did not reduce victimization risk. This is contrary to the OBTS assumption that preventive measures are effective in their goal.

Other empirical literature on the effect of prevention of repeat victimization has largely focused on area-based projects initiated by external agencies. Especially the prevention of repeat burglary through target hardening has gained attention. In their review of prevention initiatives of repeat burglary, Farrell and Pease (2006) concluded that 'There is evidence the repeat burglary can be prevented when a locally appropriate prevention effort is properly introduced (…)' (p. 175). Effects of initiatives to prevent vehicle crimes (Chenery et al. 1997), domestic violence (Lloyd et al. 1994), and racial victimization (Phillips and Sampson 1998) have also been promising. These initiatives have however been largely area-based, while the current analysis focuses mainly on individual prevention techniques.

In light of the discussed literature on the interaction between victimization and routine activities, what is needed is an extension of current research, using longitudinal data to separate out victimization and routine activities over time. The use of such data allows for a more thorough understanding of the mechanisms at work. An important source for this examination is the longitudinal component of the NCVS which has not been exploited in this way to date.

Hypotheses

The empirical part of this paper consists of two steps. The first step examines the extent to which victimization affects routine activities. The second step investigates whether routine activities affect victimization risk. This leads to the following hypotheses:

1. Individuals restrict risky routine activities after victimization.
2. Individuals who experience more serious victimizations are more likely to restrict risky routine activities after victimization than individuals who experience less serious victimization.
3. Individuals who undertake less risky routine activities reduce their victimization risk.

These hypotheses imply a temporal design with measures over multiple time points. To test whether victims restrict risky routine activities, we need to measure victimization at time T and routine activities at time $T + 1$. To test the impact of routine activities on victimization we need to measure routine activities at time T and victimization at time $T + 1$.

Data and Methods

Data

The temporal design of the current study requires panel-data. Therefore, this study uses the National Crime Victimization Survey (NCVS) which contains victimization data from the United States. The NCVS data are gathered by the Census Bureau and funded by the Bureau of Justice Statistics. The sample is designed to be nationally representative for households and non-institutionalized persons of 12 years and older. The survey uses a rotating panel design in which all age-eligible household members at a selected address become part of the panel and are interviewed every 6 months for a total period of 3 years. After seven interviews (with the first one used only for bounding purposes, minimizing the effects of forward telescoping) the address leaves the panel. The questionnaire focuses on victimization within the last 6 months prior to the first day of the month of the interview. The NCVS sampling procedure consists of a complex stratified, multi-stage cluster design. At the first stage, Primary Sampling Units (or 'PSU's', which consist of counties, groups of counties or large metropolitan areas) are selected. At the second stage, a sample of Enumeration Districts (ED's) is selected from the PSU's. Finally, the ED's are divided into segments, which are clusters of housing units (US Department of Justice 2006).

A longitudinal NCVS file for the years 1995–1998 was developed and provided in special arrangement by the Inter-university Consortium for Political and Social Research (ICPSR). The original data file contained 154,294 respondents. After removing the persons who did not have a bounding interview[1] (replacement households, new households, persons who moved into the household, and respondents who turned 12 during the panel) 108,208 cases remained.

---

[1] The first interview is also called the 'bounding interview'. This interview is typically only used to 'bound' the second interview, which means that victimization incidents reported in the second interview are compared to victimizations that were reported in the bounding interview. Duplicate victimization incidents are removed in the second interview. Thus, only victimization that occurred in the last 6 months are recorded by interviewers. This bounding procedure is necessary because individuals have been shown to have a tendency to misspecify the time period in which victimization occurred, leading them to report victimization incidents that occurred before the 6-month reference period (this is also called 'telescoping'). This leads to an overestimation of victimization. In general, this bounding procedure is an adequate technique against telescoping. However, not all respondents participate in the bounding interview, for example because they just moved into the household or because they reached the eligible age for participation in the NCVS after the bounding interview. Hence, we must correct for any unbounded interviews. This was achieved by removing all individuals who did not have a bounding interview.

Table 1 Average percentage of change in shopping between two adjacent time points ($T = 1$–6)

Shopping at time T (before)	Shopping at time $T + 1$ (after)					
	Almost every day (%)	At least once a week (%)	At least once a month (%)	Less often (%)	Never (%)	Total (%)
Almost every day	54.0	42.0	3.1	0.7	0.3	100.0
At least once a week	13.9	76.9	7.4	1.3	0.5	100.0
At least once a month	7.0	50.8	34.2	5.8	2.2	100.0
Less often	6.7	35.3	25.3	22.3	10.6	100.0
Never	4.1	20.7	14.5	13.2	47.5	100.0
Total	21.6	65.1	9.5	2.2	1.5	100.0

Key Dependent and Independent Variables for Hypotheses 1 and 2

The research questions require two different sets of dependent variables. For the first and second hypotheses routine activities are the dependent variables, operationalized through three items indicating how often respondents go **shopping**,[2] how often they spend the **evening away** from home,[3] and whether they have **household devices** against intruders.[4] The behavior expressed in the variables 'going shopping' and 'spending evenings away from home' can be interpreted as routine behavior; these are often recurring activities during daily life. While having household devices is not an activity in itself, it is the result of preventive activities aimed at safeguarding one's house.

Although conceptually different (less shopping and less evenings away may be categorized as avoidance behaviors, while installing more household devices is related to protective behavior), the three variables are all related to undertaking preventive behavior. Correlation statistics for the association between the dependent variables indicate that although the measures are significantly related, they are not inter-changeable (Pearson's r for shopping and evenings away is .28 ($p < .01$), while the Spearman's ρ for the relationship between both devices and shopping, and devices and evenings away is 0.06 ($p < .01$)).

Tables 1, 2 and 3 provide average percentages of change in routine activities between adjacent time points. For shopping, evenings away, and devices at time T the tables describe the average frequencies of these routine activities across the sample at the status quo. For shopping, evenings away, and devices at time $T + 1$ the tables describe the

---

[2] Exact wording of question: *V3028*: Before we get to the crime questions, I'd like to ask you about some of your usual activities. We have found that people with different lifestyles may be more or less likely to become victims of crime. On average, during the past 6 months, that is since (…), how often have you gone shopping? For example at drug, clothing, grocery, hardware, and convenience stores. Answer categories: 1. Almost every day (or more frequently); 2. At least once a week; 3. At least once a month; 4. Less often; 5. Never; 6. Don't know; 8. Residue; 9. Out of universe.

[3] Exact wording of question: *V3029*: (on average, during the last 6 months), how often have you spent the evening out away from home for work, school or entertainment? Answer categories: 1. Almost every evening (or more frequently); 2. At least once a week; 3. At least once a month; 4. Less often; 5. Never; 6. Don't know; 8. Residue; 9. Out of universe.

[4] Exact wording of question: *V2106*: We're interested in finding out if people we talk to do anything in particular to keep thieves or intruders out of their homes. Does your household have any special devices such as dead bolts, electric timers for lights, or an alarm system? Do not include animals. Answer categories: 1. Yes; 2. No.

Table 2 Average percentage of change in evenings away between two adjacent time points (T = 1–6)

Evenings away at time T (before)	Evenings away at time T + 1 (after)					
	Almost every evening (%)	At least once a week (%)	At least once a month (%)	Less often (%)	Never (%)	Total (%)
Almost every evening	51.7	37.7	6.1	2.8	1.7	100.0
At least once a week	13.8	66.9	12.7	4.3	2.3	100.0
At least once a month	6.7	39.3	35.8	12.4	5.9	100.0
Less often	5.7	25.3	23.5	30.0	15.5	100.0
Never	4.1	15.9	13.2	18.8	48.0	100.0
Total	17.8	49.8	16.4	8.7	7.3	100.0

Table 3 Average percentage of change in having devices against intruders between two adjacent time points (T = 1–6)

Devices at time T (before)	Devices at time T + 1 (after)		Total (%)
	No (%)	Yes (%)	
No	63.9	36.1	100.0
Yes	13.1	86.9	100.0
Total	27.0	73.0	100.0

average frequencies of these routine activities across the sample 6 months later. T represents all possible time points and for each time point it includes the respondents that participated at that particular time point.

Tables 1 and 2 indicate that most respondents report shopping and spending evenings away 'at least once a week' (65.1 and 49.8%, respectively) while small percentages report 'never' (1.5 and 7.3%, respectively) engaging in these activities. Most respondents stay within the same category over time. There is some tendency to report more shopping and more evenings away from home while time in panel advances. This could be due to a period effect, for example through nation-wide increases in consumption and spending time away from home, spurred by economic growth and rising wages at the time of data collection (see e.g. US Department of Commerce 2010). A time variable is introduced later on in this section to control for this effect.

The same tendency shown in Table 3 is less surprising: removing household devices is less likely than acquiring them. On average 36% of households that do not have devices at time T acquire devices at time T + 1. This seems much but is due to the percentages representing row percentages. It is somewhat surprising to see that 13% of households who did have devices report not having them in the subsequent period. This could be due to measurement error, since the survey question is quite broadly formulated. It could also be due however, to a change in the usage rather than the installation of the devices. For example, when people stop using deadbolt locks, though they are still installed on their doors, they may report not having them anymore. Finally, it can also be an effect of the devices being destroyed by burglars when they enter a dwelling.

The variables for shopping and evenings away are not continuous variables, nor are they normally distributed. In order to make the variables better suitable for the analyses, the following transformations were made. First, since the answer category 'never' was very rare, especially for shopping (see Table 1), the categories 'never' and 'less often' were collapsed. Second, the values of the variables were recoded to represent the approximate

Table 4 Number of time periods in which victimization occurred

Number of time periods in which victimization occurred	Violent crime in public places (respondents)	Household crime in home (households)
0	106,158 (98.1%)	46,136 (92.0%)
1	1,925 (1.8%)	3,440 (6.9%)
2	112 (0.1%)	447 (0.9%)
3	9 (0.0%)	90 (0.2%)
4	3 (0.0%)	6 (0.0%)
5	1 (0.0%)	5 (0.0%)
6	–	1 (0.0%)
Total	108,208 (100.0%)	50,125 (100.0%)

number of days. 'Almost every day' was set equal to 5 out of 7 days (5/7 = .714), 'at least once a week' was set equal to 1 out of 7 days (1/7 = .143), 'at least once a month' was set equal to twice per 30 days (2/30 = .067) and 'less often/never' was set equal to once per 45 days (1/45 = .022). Subsequently, the natural log was taken.

The key predictors for intra-individual differences over time in routine activities are victimization count variables for **violent** and **household crimes**. Violent crimes (rape, sexual assault, robbery, and assault) can be particularly unsettling and are therefore expected to influence routine activities. Since violence within the home is expected to have a different impact on routine activities compared to violence outside the home, all analyses are restricted to violence in public places. Household crimes (burglary, household property theft, and motor vehicle theft) that occurred *in* the respondent's home or lodging are expected to especially affect the protection of property. In the NCVS, household crimes are usually reported by the household respondent. However, since household crimes possibly affect everyone in the household, these victimizations were assigned to everyone in a household. Series crimes (series of at least six victimizations that are similar in nature) were counted as one. Additional analyses show that counting series crimes as six does not affect results. Table 4 shows the number of time periods in which victimization occurred.

In order to investigate whether the seriousness of victimization affects routine activities, two additional variables are added. **Injuries** indicate whether the victim suffered from injuries after the crime. Injuries included rape injuries, attempted rape injuries, sexual assault injuries, knife or stab wounds, gun shot or bullet wounds, broken bones or teeth, internal injuries, unconsciousness, bruises and cuts, and other injuries. The **value of property taken** (including recovered property) is also included. The value of property taken was capped at 5 (which equals 5,000 dollars); capping at higher (e.g. 30 or 10) or lower (e.g. 1) values did not change results. For non-victims, the values for injury and value of property taken were set to zero.

In order to minimize the amount of temporal ambiguity in terms of impact of victimization on routine activities all victimization variables are lagged, meaning that models estimate the influence of victimization at time T on routine activities at time $T + 1$.

Key Dependent and Independent Variables for Hypothesis 3

For the third hypothesis the two victimization variables previously discussed are used as dependent variables. Both dependent variables are logarithmically transformed after replacing the zero values by 0.10. Replacing zero values by .5 or by 1 did not change the

results. As predictors, the three routine activities variables previously discussed are used. Lagged variable for routine activities were constructed, which means that models estimate the influence of routine activities at time T on victimization at time $T + 1$.

Control Variables

Since I focus on differences *within* individuals, time invariant control variables (that focus on stable differences *between* individuals) are not included in the analysis (see the Method of analysis section). A number of time-*variant* control variables are included.

Having a **job** relates to structures of routine activities. For example, employment status affects time spent on domestic work (Gershuny et al. 2005) and exercising (Nomaguchi and Bianci 2004). **Attending college** influences lifestyle and victimization risk (Wittebrood and Nieuwbeerta 2000).

Since victimization and potentially routine activities are related to financial resources, **household income** is included (Lauritsen 2001; Tseloni 2000). **Marital status** is associated with changes in routine activities and victimization risk (Lauritsen 2001; Sampson and Wooldredge 1987; Wittebrood and Nieuwbeerta 2000; Xie and McDowall 2008b). For example, marriage negatively affects time spent on exercising (Nomaguchi and Bianci 2004) and risky activities such as marijuana, alcohol use, and delinquency (Duncan, Wilkerson and England 2006). **Number of motor vehicles** affects mobility which is related to routine activities, and it affects victimization risk (Tseloni 2000; Xie and McDowall 2008b). **Number of household members** is related to a higher risk of household victimization arguably because having more children decreases the amount of parental supervision per child, and because larger population flows into and out of a home decrease guardianship (Xie and McDowall 2008b). In addition, a larger number of household members, especially adult household members, may indicate more valuable goods (Trickett et al. 1995).

Previous research has shown that the longer households stay in the NCVS, the less victimizations they report (Cantor 1989). In order to control for this, a variable for **time** (interview wave) is included.

Table 5 describes the minimum and maximum values, mean, variance within and between respondents, and metric of all variables in the analysis.

Limitations

The indicators used for routine activities are obviously only three out of a large variety of indicators for routine activities. In addition, they are measured according to frequency (*how often* have you gone shopping). The way in which people undertake their routine activities (e.g. what transport do they use, whether they are in companionship) is not measured. Especially these may be of importance. Furthermore, it should be noted that decreasing the frequency of certain types of risky behavior does not necessarily result in overall less risky behavior. Since decreasing one type of behavior is related, by definition, to increasing another type of behavior, one type of risky behavior can replace the other. It should be noted that the variables used to measure routine activities are indirect measures for prevention activities and that such proxy measures are known to have limitations (see also Kennedy and Forde 1990; Maxfield 1987).

In terms of missing data, it should be noted that not all respondents participated in all waves of data collection, which leads to potential problems with missing data. Of all respondents, 21% participated in only one interview, 12% in two interviews, 8% in three

Table 5 Summary statistics

Variable	Minimum (overall)	Maximum (overall)	Mean	Variance between	Variance within	Metric
Shopping	0.02	0.71	0.26	0.04	0.03	Approximate number of days
Evenings away	0.02	0.71	0.22	0.04	0.02	Approximate number of days
Devices	0.00	1.00	0.72	0.14	0.08	1 if true, 0 if not
Violent crime in public places	0.00	7.00	0.01	0.00	0.00	
Household crime in the home	0.00	7.00	0.03	0.02	0.03	
Injuries	0.00	1.00	0.00	0.00	0.00	1 if true, 0 if not
Value property taken	0.00	5.00	0.03	0.08	0.09	In dollars (divided by 1.000)
Job	0.00	1.00	0.61	0.20	0.04	1 if true, 0 if not
Attends college	0.00	1.00	0.05	0.05	0.02	1 if true, 0 if not
Household income	5.00	75.00	41.36	518.02	29.76	In $1,000
Married	0.00	1.00	0.57	0.24	0.01	1 if true, 0 if not
Number motor vehicles	0.00	4.00	2.12	1.09	0.22	
Number household members	1.00	17.00	3.04	2.43	0.15	
Time	1.00	6.00	3.50	0.00	2.92	

Unit of analysis for this table is time point within persons

interviews, 8% in four interviews, 12% in five interviews, and 40% participated in all six interviews. This results in an 'unbalanced panel', where many respondents have one or more waves of missing data. There is evidence for selective attrition, whereby those respondents who are most often victimized are most likely to drop out of the sample (Lohr and Sun 1998). Results show that the average number of reported victimizations in the first interview is 2.59 for those who drop out after the first interview, 2.50 for those who participate in the sample for two interviews, 2.35 for those who participate three times, 1.96 for those who participate four times, 2.08 for those who participate five times, and 1.45 for those who participate all six times. Hence, those who stay in sample six waves report $(2.59–1.45)/2.59 = 44\%$ less victimization in their first interview than those who were to drop out after the first interview. Half of the decrease can be contributed to the decrease between those who stay in the sample for five waves, and those that stay for six waves. The difference in T1 victimization reporting between those who drop out after the first interview and those who drop out after the fifth interview is 20%. It is not entirely clear how selective attrition should affect changes in routine activities. On the one hand, the more often one is victimized (and thus, the more likely one is to drop out of the sample), the more one could be motivated to prevent future victimization by changing routine activities (which we are less likely to know because this individual is more likely to drop out of the sample). Thus, by excluding individuals who drop out of the sample, we would underestimate the effect of victimization on routine activities. On the other hand, prior research by Schreck and colleagues has shown that the likelihood of victimization is predicted by low self-control (Schreck 1999), while in turn, those with low self-control are less likely to change risky routine activities (Schreck et al. 2006). This may suggest that those who are victimized more often (because they have low self-control) are also less likely to change routine activities (because they have low self-control). Since those who

are victimized more often are more likely to drop out of the sample, we might thus overestimate the effect of victimization on routine activities.

One of the sources for attrition is moving. The NCVS sampling strategy is based on addresses and thus households that move are not re-interviewed. Although individuals who move are kept in the analysis for as long as they remain in the panel, they cannot be analyzed once they move. This is important since those who move could be most subject to changes in routine activities. For example, moving has been found to be related to victimization experiences (Dugan 1999; Ellingworth and Pease 1998; Xie and McDowall 2008a). Additional analyses show that those who move out of the NCVS are more likely to be victimized. They are a little more likely to shop every day, more likely to go out every evening, and a little less likely to have household devices. Their within-individual variance in the routine activities variables however is lower than average. Although it is a weakness that those who move disappear from the panel, it should be noted that moving takes time, and so moving after victimization may be preceded by other behavioral changes that are included in the analysis. Hence, additional analyses on hypothesis one that include only those who move will be reported in this paper.

Finally, the time dimension of reactions to victimization has to be taken into account. Literature suggests that the strongest reactions to crime wear off as time passes (Maguire 1980). The fact that reactions to crime are measured up to a maximum of 6 months after the crime may be problematic in the sense that reactions may have ceased to exist after 6 months. In addition, when multiple victimizations occur within 6 months, reactions of the different victimizations may become entangled.

In order to assess the relationship between the routine activities measures and the victimization variables, bivariate cross-sectional ordinary least squares and logistic regressions were performed (not shown). Analyses yield mostly highly significant results when the routine activities and victimization variables are regressed on one another. However, no time-order or causal conclusions can be derived from these cross-sectional results. Therefore, the analysis now continues by making optimal use of the NCVS design which incorporates measures of both routine activities and victimization over time and therefore enables one to measure the interaction between routine activities and victimization.

Method of Analysis

Different methods for analyzing panel data exist. Unlike most other victimization studies using panel models, this study uses a dynamic panel model (for a rare application see Ousey et al. 2008). This model is related to Fixed Effects methods which allow one to cancel out differences *between* individuals (heterogeneity) and focus on differences *within* individuals. For example, using this method, effects of victimization on routine activities can be attributed to the victimization event itself, and not to unobserved pre-existing differences or risk heterogeneity between people (for example due to differences in place, upbringing, psychological factors, etc.; Halaby 2004).

The Fixed Effects method eliminates unobserved heterogeneity. Consider a typical panel regression model of the form:

$$y_{it} = \beta x_{it} + \gamma_i + \varepsilon_{it} \qquad (1)$$

where the outcome variable y_{it} and predictor x_{it} vary over both individuals and time. Variable γ_i represents time-invariant unobserved individual effects, while ε_{it} is an idiosyncratic disturbance.

In order to cancel out the individual effects γ_i, the mean scores of time-varying variables for each i (Eq. 2) are subtracted from Eq. 1, resulting in Eq. 3:

$$\bar{y}_i = \beta\bar{x}_i + \gamma_i + \bar{\varepsilon}_i \tag{2}$$

$$(y_{it} - \bar{y}_i) = \beta(x_{it} - \bar{x}_i) + \varepsilon_{it} - \bar{\varepsilon}_i \tag{3}$$

The individual effects (γ_i) become zero, and hence time-invariant unobserved heterogeneity is eliminated.

There has been some discussion on the tradeoff between Random Effects (RE) and Fixed Effects (FE) models (Halaby 2004; Bushway et al. 1999). RE models allow for the inclusion of time-invariant predictors and are more efficient than FE models, provided that they meet the assumption that time-invariant unobserved heterogeneity is random and uncorrelated with the regressors (Halaby 2004). This assumption however is hardly ever met. Hausman tests were performed to investigate whether the data met the random effects assumption. Highly significant outcomes indicated that the assumption was not met. Differences were confirmed when results from RE models differed substantially from FE models. Therefore, Fixed Effects regression models were preferred.

It is expected that prior victimization has a direct effect on the risk of future victimization. According to the Once Bitten Twice Shy perspective, this effect should be negative. However, earlier research has shown support for a positive state dependence effect (Ousey et al. 2008; Tseloni and Pease 2003; Wittebrood and Nieuwbeerta 2000; Lauritsen and Davis Quinet 1995).

For routine activities it is also to be expected that there is a direct effect of previous behavior on future behavior. For example, if one joins a sports club and starts spending his evenings there, it is to be suspected that this affects his evening activities in the following period.

To assess these effects of prior events or behaviors (also called 'state dependence effects') on future events or behaviors, one would want to include the lagged value of the dependent variable in the equation. This leads to a so-called autoregressive model of the form:

$$y_{it} = \alpha y_{i,t-1} + \beta x_{it} + \gamma_i + \varepsilon_{it} \tag{4}$$

However, including the lagged dependent variable as a predictor in the regression equation is problematic since it is not exogenous or uncorrelated with the error term (Arellano and Bond 1991). The resulting bias can be substantial and can lead to a negative effect of the lagged dependent variable when in fact it should be positive (Bond 2002; Ousey et al. 2008). In order to correct for this a number of dynamic panel models have been developed that typically first transform the model to eliminate heterogeneity and then use instruments for the lagged y to correct for endogeneity (Halaby 2004). The Anderson and Hsiao (1982) 2SLS estimator with instrumental variables for AR(1) panel data provides such a model. Anderson and Hsiao first eliminate the individual effects by applying first-differences:

$$\Delta y_{it} = \alpha\Delta y_{i,t-1} + \beta\Delta x_{it} + \Delta\varepsilon_{it} \tag{5}$$

Because this equation violates the exogeneity assumption, since the endogenous variable term $\Delta y_{i,t-1}$ (or $y_{i,t-1} - y_{i,t-2}$) is correlated with the error term $\Delta\varepsilon_{i,t-1}$ (or $\varepsilon_{i,t-1} - \varepsilon_{i,t-2}$), the Anderson-Hsiao method uses $y_{i,t-2}$ or $\Delta y_{i,t-2}$ as an instrument for $\Delta y_{i,t-1}$ (Bond 2002; Hsiao 2003; Roodman 2006).

A second type of dynamic panel model was developed by Arellano and Bond (1991). They argue that the Anderson-Hsiao procedure is not efficient. As an alternative procedure, they developed a first-difference procedure based on the Generalized Method of Moments

(GMM). Instead of using $\Delta y_{i,t-2}$ as an instrument, this method uses all available lags to instrument the lagged y.

Although dynamic panel models work well for linear fixed effects models, the case for logit fixed effects regressions is much more problematic (Maddala 1987). Since four of the five dependent variables presented in this paper can be treated as continuous and the remaining dependent variable is binary in nature (household devices), this paper uses two analytic approaches.

First, for the four 'continuous' dependent variables (shopping, evenings out, violent victimization, and household victimization), Arellano-Bond estimation procedures are performed. The two-step generalized method-of-moments (GMM) estimator with Windmeijer corrected standard errors is used.[5] These four analyses include state dependence effects since the Arellano Bond procedure corrects for endogeneity. As an additional 'sensitivity' analysis, Anderson-Hsiao analyses were performed (results not shown); no substantially different results were obtained. It should be noted that although a logarithmic transformation was applied in order to treat the count victimization variables as continuous, these variables are not technically continuous. Therefore, additional analyses were performed in which the victimization variables are treated as binary (0 = not victimized, 1 = victimized). Applying non-dynamic logit fixed effects models without state dependence effects to these binary variables did not lead to substantially different results.

Second, for the binary dependent variable (household devices), a non-dynamic logit fixed effects model was estimated. State dependence effects could not be included, since this would cause endogeneity. This bias in binary fixed effects models can currently not be corrected for in the major statistical packages. To see whether inclusion of a state dependence effect would have influenced the results, two additional analyses were performed. First, a logit fixed effect model with state dependence effect was run. As predicted the state dependence effect was negative and highly significant, but did not substantially influence results on the key predictors. Second, a linear Arellano-Bond model was performed on the binary dependent variable. The sign of the state dependence effect flipped, making it positive and highly significant, but again this did not greatly influence the effects of the key predictors.

As noted, the NCVS sample is drawn based on a multi-stage cluster design. In addition, the NCVS datafiles include weights. Therefore, additional analyses were performed in which results were corrected for clustering within PSU's and ED's and in which weights were included. Results were very similar.

Results

Results 1: Impact of Victimization on Routine Activities

This section presents the results for the first and second hypothesis which state that victimization, and especially serious victimization, is related to restriction of risky routine activities. Table 6 shows the mean values of routine activities before and after victimization.

These descriptive bivariate results suggest a very limited impact of victimization on routine activities. In order to check whether differences are significant, whether they hold

---

[5] In finite samples, estimated asymptotic standard errors of the two-step GMM estimation procedure can be biased downwardly. The robust estimator of Windmeijer corrects for this (as shown by Windmeijer, 2005).

Table 6 Mean values of routine activities before and after victimization

	Mean values routine activities		
	Shopping	Evenings away	Devices
Before violent victimization	.30	.33	.76
After violent victimization	.28	.32	.78
Before household victimization	.27	.25	.71
After household victimization	.27	.24	.72

in a multivariate environment, and whether suppressor variables are at work, regression analyses are performed. Results are shown in Table 7. Results are presented for all three dependent variables: shopping, evenings away, and household devices. For each dependent variable, three models are presented. The first model contains only the state dependence effect and the time-variable, the second model adds the victimization variables, while the third model adds time-varying control variables. To avoid unnecessary large N, the analyses on 'Devices' were performed on only household respondents, however including victimizations of any household member. The decrease in the number of cases across models is largely due to inclusion of the income variable which has relatively many missing values. Removing income from the analyses does not lead to substantially different results.

Model 1 shows that state dependence effects for both shopping and evenings away are positive, meaning that earlier shopping behavior and evenings away lead to more shopping and evenings away later on. The results for Model 2 indicate one significant effect of victimization on routine activities. Violent victimization in public places is significantly associated with less shopping which is according to the hypothesis.

The full models (Model 3) show that this significant relationship holds. In addition, victimizations with injuries are related to spending less evenings away from home. The absence of other significant results on the key predictors leads to the conclusion that although violent victimization in public places is related to less shopping[6] and although victimization with injuries is related to less evenings away, in general victimization does not appear to influence routine activities. The effect size of violent victimization on shopping shows that for every victimization experienced, shopping decreases by .069. Given the range of the log of shopping (−.337 to −3.817) the effect size is not very large. The same can be said about the effect size of injuries on evenings away, although this effect is somewhat larger. Although the effect sizes do not seem very large, it is worth noting that not only are effect sizes dependent on measurement and design, the practical meaning of statistical effects also needs to be considered (McCartney and Rosenthal 2000). According to Belsky (2001), 'even small effects, when experienced by many […], may have broadscale consequences' (p. 856).

It is interesting that violent victimization leads to less shopping but not to spending less evenings away from home. A potential reason for this finding could be that entertainment, schooling, and work activities generally hold more value for individuals than going shopping to the extent that shopping is experienced as a household task rather than a leisure activity. In this sense, giving up entertainment activities could lead to lower perceived

---

[6] Note that this result only holds for violent victimizations that occurred in public places. The same was not found when other locations were included.

Table 7 Regression results for impact of victimization on natural logarithm of routine activities

	Ln(shopping $T + 1$) (linear regression)			Ln(evenings away $T + 1$) (linear regression)			Devices $T + 1$ (logistic regression)		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
State dependence (=lagged Y)	.051**	.051**	.044**	.044**	.044**	.042**	n.a.	n.a.	n.a.
Key predictors									
Violent crime T		−.061**	−.069**		.008	.025		.004	−.016
Household crime T		−.009	−.007		−.008	−.007		−.060	−.054
Injuries T		−.035	−.033		−.076	−.100*		.081	−.089
Value of property taken T		.004	.003		.004	.005		.004	−.040
Control variables									
Job			.002			.135**			.073
Attends College			−.032*			.087**			−.231**
Household income			.000			.001*			.000
Married			.039*			.000			−.012
No. of motor vehicles			.022**			.007			.008
No. of household members			−.014*			−.019**			−.075**
Time	.005**	.005**	.002	.005**	.005**	.001		.070**	.075**
Number of observations	170,528	170,528	133,558	169,421	169,421	132,919		52,051	39,318
Number of individuals/ households[a]	59,440	59,440	47,695	59,299	59,299	47,621		12,116	9,536

** $p < .01$; * $p < .05$ (one-tailed)

[a] This row indicates 'number of individuals' for the analyses on shopping and evenings away, and 'number of households' for the analysis on devices

quality of life. On the other hand, going shopping, especially for groceries, is much more of an activity that is guided by the needs and requirements of everyday life, while the activity itself is less related to 'fun' and quality of life and might even be regarded as a nuisance. Going shopping less often, or buying larger quantities of groceries at once instead of smaller quantities more frequently, may be a much more feasible adaptation after victimization than giving up highly valued entertainment activities. This interpretation is somewhat supported by the finding that only in cases of injuries are victims willing to give up evening entertainment activities.

As discussed in the Limitations section, additional analyses were performed in order to see whether the effects change when only those who move are included. No significant effects were obtained. Thus, although it is found that in general violent victimization is related to less shopping and injuries are related to spending less evenings away, these findings are not obtained when only those who move are included. A first potential explanation is that the power of the analyses on those who move is lower since less individuals and time periods can be included. A second potential explanation for this difference may be obtained through the characteristics of these individuals. In the Limitations section, it was reported that those who move are generally more likely to be

victimized, to shop every day, to go out every evening and less likely to have household devices. These characteristics suggest that those who move have a more risky lifestyle than those who do not move. Risky lifestyles are linked to personality characteristics such as low self-control (Gottfredson and Hirschi 1990; Schreck 1999). Individuals with low self-control tend to be adventurous and to have little interest in long-term commitment. Although it is not possible to test whether those who move have low self-control, being adventurous and having little interest in long-term commitment may correspond with the profile of being likely to move more often. Research by Schreck et al. (2006) suggests that individuals with lower levels of self-control may be less likely to change risky behaviors after victimization. Thus, given their characteristics, movers may be more likely to have low levels of self-control and they may thus be less likely to change risky activities after victimization. These speculations may be problematic in light of findings that moving can be a reaction to victimization (Dugan 1999; Xie and McDowall 2008a). Thus, a third potential explanation is that those who move after victimization do also change their activities after victimization, but since they drop out of the sample, we do not observe these changes.

Results in Table 7 suggest that other life events influence routine activities more strongly than criminal victimization does. Model 3 reveals that some of the control variables (all measured at the same time point as the dependent variables) are significantly related to the dependent variables. Having a job is related to more evenings away, which can be due to the higher amount of financial resources associated with having a job. Also interesting is that getting married is associated with more shopping. Attending college is associated with spending more evenings away from home, less shopping and less devices. Obtaining motor vehicles means a higher degree of mobility and hence more shopping. An increase in the amount of household members is related to less shopping and evenings away, which can be due to dividing up tasks between household members or to the arrival of newborns. Also interesting is the negative relationship between the number of household members and household devices. Perhaps natural surveillance in the form of household members decreases the need for more formal prevention measures.

A final analysis checked whether results differed by gender and race. Such differences would shed more light on how differential opportunity structures faced by victims influence the extent to which victims take precautions after victimization. For example, since ethnic minorities are more likely to live in disadvantaged neighborhoods than Caucasian Americans, the structural constraints on their behaviors, as explained in the introduction, may limit the extent to which they can take (effective) precautionary measures against repeat victimization. Similarly, since men traditionally spend less time in the home than women (Hindelang et al. 1978), men have more 'to change' in terms of out-home-activities than women. On the other hand, men might feel they should be less affected by victimization than women, and thus be less likely to change their activities than women. To explore whether differences exist in precautionary measures between genders and races, interaction terms between victimization and gender, and between victimization and race were constructed. No significant differences in precautionary behaviors were found between genders and races.

Results 2: Impact of Routine Activities on Victimization

In this section, the effectiveness of preventive behavior in reducing victimization risk is considered. Results are presented in Table 8. Similar to Table 7, three Models are presented for each dependent variable.

Table 8 Regression results for impact of routine activities on natural logarithm of victimization

	Ln(violent crime $T + 1$)			Ln(household crime $T + 1$)		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
State dependence (= lagged Y)	.013	.014	.003	.027**	.023**	.015*
Key predictors						
Shopping T		.002	.000		.006	.007
Evenings away T		−.002	.002		−.008	−.009
Devices T		.000	.000		−.005*	−.005*
Control variables						
Job			−.001			.008*
Attends college			−.002			−.003
Household income			.000			.000
Married			−.002			−.028*
No. of motor vehicles			.001			.003
No. of household members			.002			.011**
Time	−.001**	−.001**	−.001**	−.003**	−.003**	−.002**
No. of observations	224,629	184,240	136,954	260,439	188,661	136,954
No. of individuals	70,518	63,238	48,028	78,454	65,000	48,028

** $p < .01$; * $p < .05$ (one-tailed)

Table 8 reveals a positive state dependence effect for household crime, meaning that previous victimization has a positive and direct impact on future victimization. This is in line with previous literature, and at first sight seems to reject the OBTS hypothesis since OBTS predicts that previous victimization leads to a lower risk of future victimization. The effect of prior violent victimization in public places on future violent victimization is not significant, which might be due to the rarity of this type of victimization.[7] Surprisingly, no significant results are obtained on the effect of routine activities on violent victimization. The analysis on household property crimes yields one significant result. Obtaining devices leads to less household crimes, as hypothesized, although the effect size is small.

Model 3 presents interesting results on the control variables (again measured at the same time point as the dependent variables), especially in light of some long standing axioms on the relationship between life circumstances and victimization risk. One of the most interesting results is that getting married is associated with less household victimization. This suggests a routine activities association. Getting married is conceptually related to decreasing risky routine activities, to spending more time with a partner at home, thereby increasing natural surveillance. Getting a job is related to household crime. This suggests an opportunity effect: having a job usually means that one is away from home during the day, which decreases guardianship of the house. In addition, having a job increases purchasing power of the occupant in terms of buying attractive goods and therefore increases target attractiveness. Finally, the positive relationship between the number of household members and household crime is as expected, as explained in the Data section.

---

[7] Note that the variable for violent victimization as used has very little variance (see Table 5). When a larger category of violent victimization with more variance is used (i.e. not limited to violence in public places), the state dependence effect of prior victimization becomes significant, as do some of the control variables. However, the results on the key routine activities variables remain insignificant.

Conclusion & Discussion

The current paper investigated (1) whether victimization restricts risky routine activities and (2) whether restricting risky routine activities reduces victimization risk. Panel data from the NCVS were used in conjunction with dynamic panel models that cancel out unobserved heterogeneity.

Regarding the first research question, it was found that victimization affects routine activities to a limited extent. Violent victimization was related to less shopping. In addition, victimizations with injuries were related to spending less evenings away, which indicates that seriousness of victimization is important in the extent to which victims change their routine activities after victimization.

Regarding the second research question, one significant result was found. Household devices lead to less household crime, which is as hypothesized. In terms of control variables, one of the most interesting findings was that marriage is related to a lower risk of household victimization. It is interesting that some of the control variables showed significant effects even though they had very little within-individual variation. For example, Table 5 showed that the variables indicating job, attending college, and married vary little within individuals. Thus, it is all the more surprising that some of these variables in fact show significant results. For example, getting a job is significantly related to evenings away and household crime, starting college is significantly related to all three routine activities variables, and getting married is significantly related to shopping and household crime. This suggests that changes in these activities are quite powerfully related to victimization risk. The method that was used effectively rules out that these findings can be explained by unobserved heterogeneity.

It is concluded that although some results indicate that victimization is related to restricting routine activities, overall the evidence in support of the OBTS claim is limited. In the theoretical framework I indicated some of the reasons why victimization may not lead to intra-individual differences in routine activities: most victimizations are not very traumatic, cost-benefit analyses do not necessarily favor preventive behavior, and opportunity structures and structural constraints determine the extent to which one is capable of changing routine activities. In addition, it has been argued that decisions about preventive behavior are influenced by the perceived effectiveness of measures (Skogan 1981). Respondents may not perceive a lower frequency of shopping and evenings away as effective prevention measures. The finding that victimization does not decrease risky activities implies a need for further research on why this is the case. In addition, it calls for a more proactive effort from victim services to reduce the risk for repeat victimization.

The results also somewhat counter the OBTS assumption that restricting risky behavior is successful in its preventive goal. Apparently, restrictive intra-individual differences are not always associated with a lower risk of victimization. More in general, these results point to the difficulty of predicting changes over time within individuals (see also Lauritsen 1998).

Rather than assessing the consequences of the results for the OBTS perspective, it is appropriate to view them in the larger framework in which the OBTS hypothesis is framed: routine activities theory. What do the results mean for routine activities theory?

The results on the first research question indicate that routine activities must be seen in a far larger framework than just in terms of victimization. Although routine activities were proposed to be intricately linked with the risk of victimization, routine activities are much more than that. They are built through the structures of lives, driven by formal obligations such as work and school, more informal activities such as sports clubs and going out with

friends, and personal preferences about how to spend one's time. Routine activities encompass the way one lives one's life, and seen from that perspective victimization *can* only be expected to influence these structures to a limited extent. Therefore, routine activities theory may be seen as a general proposition on life structure. Results of the current paper suggest that routine activities vary largely due to life circumstances such as having a job, attending college, being married, household size, and mobility, and less with victimization. This is not surprising when routine activities are viewed in a larger perspective on life structure.

Matters may off course be different when the most injurious and serious forms of victimization are considered. This is supported by the finding that victimizations with injuries lead to spending less evenings away. It is sometimes argued that the NCVS may undercount these types of victimizations, although this claim is countered by Rand and Rennison (2005). The OBTS notion that victimization influences routine activities may have been set in too general a perspective, and may only be applicable to these most serious crime types.

In this perspective, the proposition cast by Hindelang et al. (1978) that changes in routine activities may occur mostly in the form of subtle adjustments could be highly important in studying the effect of victimization on routine activities. Apart from the most serious and injurious crime types, we cannot expect victimization to cause drastic changes in routine activities. Therefore, in order to extend the knowledge about this linkage we would need panel data on these subtle aspects of routine activities.

The results for the second research question also provide food for thought. Routine activities should influence victimization risk, but most of the results remained insignificant. How can this be explained? We cannot just conclude that the measures used are not good measures of routine activities. Although routine activities did not influence *intra-individual differences* in victimization risk, they do influence *inter-individual differences* in victimization risk: as mentioned, cross-sectional regressions models (not shown) showed highly significant results when the routine activities and victimization variables were regressed on one another.

Hence, results lead us to believe that the effects of routine activities on victimization are much more complicated. For example, Miethe (1991) argued that protected targets may increase the thrill of offending, especially juveniles. This balances out the decrease in interest from more experienced offenders who may be put off by increased safety precautions, thus resulting in a null-finding.

There are two paths in which future research could proceed. First, to extend the current research, we would benefit from very detailed descriptions of routine activities to improve our understanding of which routine activities are best related to victimization risk. A good example is provided by the British Crime Survey which includes detailed questions on the presence and use of a range of different security measures such as burglar alarms, locks, property marking, and sensor switches for lights. It also includes questions on carrying personal alarms or weapons when going out after dark, vehicle security measures, frequency of going to a variety of venues in the evening, and detailed questions on the specific venue visited and transportation used when going out (Home Office 2006, 2007). Such detailed measures combined with a rotating panel design would be a large step forward in the study of OBTS and the reciprocal effects of routine activities and victimization. Improving the understanding and measurement of this scale of 'risky' activities seems highly important for understanding the impact of routine activities on victimization.

Although this path should provide us with more detailed information about the link between routine activities and victimization within individuals, a second path of future

research is also proposed. This follows from the observation by Lauritsen et al. (1992) that the effect of direct measures of adolescent activities (such as time studying, time in school activities, family activities, sports activities, time with (delinquent) peers, partying or dating, going to movies, watching television, spending time alone) on victimization was substantially mediated when a respondent's delinquency was considered. This prompted the authors to conclude: 'Perhaps if we were able to examine other lifestyle factors we might have identified activities that reduce the risk of victimization. However, based on our extensive analyses of two national data bases on adolescent victimization [National Youth Survey and Monitoring the Future, MA], we were unable to identify a set of conventional lifestyle activities that are truly "protective" of adolescent victimization. These results cast doubt that such a set of activities could even be found' (p. 102). Lauritsen et al. therefore emphasized that research should focus on the relationship between victimization and truly dangerous activities, or the overlap between victimization and delinquency. Studying both of these paths would provide us with a better idea of the link between routine activities and victimization within individuals.

References

Anderson TW, Hsiao C (1982) Formulation and estimation of dynamic models using panel data. J Econ 18:47–82

Arellano Manuel, Bond S (1991) Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations. Rev Econ Stud 58:277–297

Belsky J (2001) Emmanuel Miller lecture. Developmental risks (still) associated with early child care. J Child Psychol Psychiatry 42(7):845–859

Bond S (2002) Dynamic panel data models: a guide to micro data methods and practice. Working paper CWP09/02: CEMMAP

Bushway S, Brame R, Paternoster R (1999) Assessing stability and change in criminal offending: a comparison of random effects, semiparametric, and fixed effects modeling strategies. J Quant Criminol 15:23–61

Cantor D (1989) Substantive implications of longitudinal design features: the national crime survey as a case study. In: Kasprzyk D, Duncan GJ, Kalton G, Singh MP (eds) Panel surveys. Wiley, New York

Chenery S, Holt J, Pease K (1997) Biting back II: reducing repeat victimisation in Huddersfield. Home Office Police Research Group, London

Cohen LE, Cantor D (1980) Residential burglary in the United States: life-style and demographic factors associated with the probability of victimization. J Res Crime Delinq 17:140–159

Cohen LE, Felson M (1979) Social change and crime rate trends: a routine activity approach. Am Sociol Rev 44:588–608

Cook PJ (1986) The demand and supply of criminal opportunities. Crime and justice. Annu Rev Res 7:1–27

Ditton J, Chadee D (2006) People's perceptions of their likely future risk of criminal victimization. Br J Criminol 46:505–518

DuBow F, McCabe E, Kaplan G (1979) Reactions to crime. A critical review of the literature. US Department of Justice, National Institute of Justice, Washington, DC

Dugan L (1999) The effect of criminal victimization on a household's moving decision. Criminology 37:903–930

Duncan GJ, Wilkerson B, England P (2006) Cleaning up their act: the effects of marriage and cohabitation on licit and illicit drug use. Demography 43:691–710

Ellingworth D, Pease K (1998) Movers and breakers: household property crime against those moving home. Int J Risk Secur Crime Prev 3:35–42

Estrada F, Nilsson A (2008) Segregation and victimization: neighbourhood resources, individual risk factors and exposure to property crime. Eur J Criminol 5:193–216

Farrell G, Pease K (2006) Preventing repeat residential burglary victimization. In: Welsh BC, Farrington DP (eds) Preventing crime: what works for children, offenders, victims and places. Springer, New York

Fattah EA (1997) Criminology: past, present and future. A critical overview. MacMillan Press, Basingstoke

Felson M (1994) Crime and everyday life. Insights and implications for society. Pine Forge Press, Thousand Oaks

Ferraro KF (1995) Fear of crime. Interpreting victimization risk. State University of New York, New York

Finkelhor D, Ormrod RK, Turner HA (2007) Polyvictimization and trauma in a national longitudinal cohort. Dev Psychopathol 19:149–166

Gale J-A, Coupe T (2005) The behavioural, emotional and psychological effects of street robbery on victims. Int Rev Victimol 12:1–22

Garofalo J (1979) Victimization and the fear of crime. J Res Crime Delinq 16:80–97

Gershuny J, Bittman M, Brice J (2005) Exit, voice, and suffering: do couples adapt to changing employment patterns. J Marriage Fam 67:656–665

Gottfredson MR (1984) Victims of crime: the dimensions of risk. HM Stationary Office, London

Gottfredson MR, Hirschi T (1990) A general theory of crime. Stanford University Press, Stanford

Halaby CN (2004) Panel models in sociological research: theory into practice. Annu Rev Sociol 30:507–544

Heiskanen M, Lättilä R, Seppänen S (1991) Interim results of a Finnish national survey. Victims and criminal justice. In: Kaiser G, Kury H, Albrecht H-J (eds) Victimological research: stocktaking and prospects. Max Planck Institute for Foreign and International Penal Law, Freiburg

Hindelang MJ, Gottfredson MR, Garofalo J (1978) Victims of personal crime: an empirical foundation for a theory of personal victimization. Ballinger, Cambridge

Home Office (2006) British crime survey 2006/2007. Final questionnaire

Home Office (2007) British crime survey 2007/2008. Final questionnaire

Hsiao C (2003) Analysis of panel data. University of Cambridge Press, Cambridge

Kahneman D, Tversky A (1979) Prospect theory: an analysis of decision under risk. Econometrica 47:263–292

Kennedy LW, Forde DR (1990) Routine activities and crime: an analysis of victimization in Canada. Criminology 28:137–152

Lauritsen JL (1998) The age-crime debate: assessing the limits of longitudinal self-report data. Soc Forces 77:127–155

Lauritsen JL (2001) The social ecology of violent victimization: individual and contextual effects in the NCVS. J Quant Criminol 17:3–32

Lauritsen JL, Davis Quinet KF (1995) Repeat victimization among adolescents and young adults. J Quant Criminol 11:143–166

Lauritsen JL, Laub JH, Sampson RJ (1992) Conventional and delinquent activities: implications for the prevention of violent victimization among adolescents. Violence Vict 7:91–108

Lavrakas PJ (1981) On households. In: Lewis DA (ed) Reactions to crime. Sage, Beverly Hills

Lejeune R, Alex N (1973) On being mugged. The event and its aftermath. Urban Life Cult 2:259–287

Lloyd S, Farrell G, Pease K (1994) Preventing repeated domestic violence: a demonstration project on merseyside. Home Office Police Research Group, London

Lohr S, Sun S (1998) Probability of victimization over time: results from the US national crime victimization survey. In: Proceedings of statistics canada symposium 98. Longitudinal analysis for complex surveys, pp 161–166

Lurigio AJ (1987) Are all victims alike? The adverse, generalized, and differential impact of crime. Crime Delinq 33:452–467

Lynch JP (1991) Victim behavior and the risk of victimization: implications of activity-specific victimization rates. In: Kaiser G, Kury H, Albrecht H-J (eds) Victims and criminal justice. Victimological research: stocktaking and prospects. Max Planck Institute for Foreign and International Penal Law, Freiburg

Maddala GS (1987) Limited dependent variable models using panel data. J Hum Res 22:307–338

Maguire M (1980) The impact of burglary upon victims. Br J Criminol 20:261–275

Mawby RI (2001) Burglary. Willan, Cullompton

Maxfield MG (1987) Lifestyle and routine activity theories of crime: empirical studies of victimization, delinquency, and offender decision-making. J Quant Criminol 3:275–282

Mayhew P (1993) Measuring the effects of crime in victimization surveys. In: Bilsky W, Pfeiffer C, Wetzels P (eds) Fear of crime and criminal victimization. Ferdinand Enke, Stuttgart

McCartney K, Rosenthal R (2000) Effect size, practical importance, and social policy for children. Child Dev 71(1):173–180

Miethe TD (1991) Citizen-based crime control activity and victimization risks: an examination of displacement and free-rider effects. Criminology 29:419–439

Miethe TD, Stafford MC, Sloane D (1990) Lifestyle changes and risks of criminal victimization. J Quant Criminol 6:357–376

Nomaguchi KM, Bianci SM (2004) Exercise time: gender differences in the effects of marriage, parenthood, and employment. J Marriage Fam 66:413–430

Norris FH, Kaniasty K, Thompson MP (1997) The psychological consequences of crime. In: Davis RC, Lurigio AJ, Skogan WG (eds) Victims of crime. Sage, Thousand Oaks

Ousey GC, Wilcox P, Brummel S (2008) Déjà vu all over again: investigating temporal continuity of adolescent victimization. J Quant Criminol 24:307–335

Paap WR (1981) Being burglarized: an account of victimization. Victimology 6:297–305

Pease K (1998) Repeat victimisation: taking stock. Home Office, London

Phillips C, Sampson A (1998) Preventing repeated racial victimization. An action research project. Br J Criminol 38:124–144

Quann N, Hung K (2002) Victimization experiences and the fear of crime. In: Nieuwbeerta P (ed) Crime victimization in comparative perspective. Results from the international crime victims survey, 1989–2000. Boom Juridische uitgevers, Den Haag, pp 301–316

Rand MR, Rennison CM (2005) Bigger is not necessarily better: an analysis of violence against women estimates from the national crime victimization survey and the national violence against women survey. J Quant Criminol 21:267–291

Roodman D (2006) How to do xtabond2: an introduction to "difference" and "system" GMM in Stata. Working paper number 103. Center for Global Development, Washington, DC

Rountree PW, Land KC (1996) Burglary victimization, perceptions of crime risk, and routine activities: a multilevel analysis across Seattle neighborhoods and census tracts. J Res Crime Delinq 33:147–180

Rountree PW, Land KC, Miethe TD (1994) Macro-micro integration in the study of victimization: a hierarchical logistic model analysis across Seattle neighborhoods. Criminology 32:387–414

Sampson RJ, Wooldredge JD (1987) Linking the micro- and macro-level dimensions of lifestyle-routine activity and opportunity models of predatory victimization. J Quant Criminol 3:371–393

Schreck CJ (1999) Criminal victimization and low self-control: an extension and test of a general theory of crime. Justice Q 16:633–654

Schreck CJ, Stewart EA, Fisher BS (2006) Self-control, victimization, and their influence on risky lifestyles: a longitudinal analysis using panel data. J Quant Criminol 22:319–340

Shapland J, Hall M (2007) What do we know about the effects of crime on victims? Int Rev Victimol 14:175–217

Skogan WG (1981) On attitudes and behaviors. In: Lewis DA (ed) Reactions to crime. Sage, Beverly Hills

Skogan WG (1987) The impact of victimization on fear. Crime Delinq 33:135–154

Skogan W, Maxfield MG (1981) Coping with Crime. Individual and neighborhood reactions. Sage, Beverly Hills

Smith DA, Jarjoura GR (1989) Household characteristics, neighborhood composition and victimization risk. Soc Forces 68:621–640

Trickett A, Osborn DR, Ellingworth D (1995) Property crime victimisation: the roles of individual and area influences. Int Rev Victimol 3:273–295

Tseloni A (2000) Personal criminal victimization in the United States: fixed and random effects on individual and household characteristics. J Quant Criminol 16:415–442

Tseloni A, Pease K (2003) Repeat personal victimization: 'boosts' or 'flags'? Br J Criminol 43:196–212

Tseloni A, Pease K (2004) Repeat personal victimization. Random effects, event dependence and unexplained heterogeneity. Br J Criminol 44:931–945

Tseloni A, Zarafonitou C (2008) Fear of crime and victimization: a multivariate multilevel analysis of competing measurements. Eur J Criminol 5:387–409

Tseloni A, Osborn DR, Trickett A, Pease K (2002) Modelling property crime using the British crime survey. Br J Criminol 42:109–128

Tyler TR (1980) Impact of directly and indirectly experienced events: the origin of crime-related judgments and behaviors. J Pers Soc Psychol 39:13–28

US Department of Commerce (2010) National income and product accounts table. Table 1.1.5. Gross domestic product. Retrieved 29 July 2010, from http://www.bea.gov/national/nipaweb/TableView.asp?SelectedTable=5&ViewSeries=NO&Java=no&Request3Place=N&3Place=N&FromView=YES&Freq=Qtr&FirstYear=1995&LastYear=1998&3Place=N&Update=Update&JavaBox=no#Mid

US Department of Justice (2006) National crime victimization survey, 1992–2004. Codebook. Inter-university Consortium for Political and Social Sciences, Ann Arbor

Van Wilsem J, Wittebrood K, De Graaf ND (2006) Socioeconomic dynamics of neighborhoods and the risk of crime victimization: a multilevel study of improving, declining, and stable areas in the Netherlands. Soc Probl 53:226–247

Windmeijer F (2005) A finite sample correction for the variance of linear efficient two-step GMM estimators. J Econ 126:25–51

Wittebrood K, Nieuwbeerta P (2000) Criminal victimization during one's life course: the effects of previous victimization and patterns of routine activities. J Res Crime Delinq 37:91–122

Xie M, McDowall D (2008a) Escaping crime: the effects of direct and indirect victimization on moving. Criminology 46:809–840

Xie M, McDowall D (2008b) The effects of residential turnover on household victimization. Criminology 46:539–575

Zedner L (2002) Victims. In: Maguire M, Morgan R, Reiner R (eds) The Oxford handbook of criminology. Oxford University Press, Oxford, pp 419–456