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ABSTRACT

Dynamic Relationships between Criminal Offending and Victimization*

A stylized fact in criminology holds that those who commit crimes are more likely to be victims of crime, and vice versa. We use population-level administrative data of all police investigations in New Zealand to examine the possibility of this victim-offender overlap. Two-way fixed effects and dynamic panel models explore intertemporal relationships between victimization and offending. This analysis reveals that victim-offender overlap predominantly reflects population heterogeneity. However, a dynamic relationship does exist, and is primarily driven by 1) criminal incidents occurring close together in time and 2) simultaneous incidents where individuals are both offenders and victims (e.g., mutually combative assaults).

JEL Classification: C55, K14, K42

Keywords: victim-offender overlap, population heterogeneity, crime, victimization

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* These results are not official statistics. They have been created for research purposes from the Integrated Data Infrastructure (IDI) which is carefully managed by Stats NZ. For more information about the IDI please visit https://www.stats.govt.nz/integrated-data/. The results are based in part on tax data supplied by Inland Revenue to Stats NZ under the Tax Administration Act 1994 for statistical purposes. Any discussion of data limitations or weaknesses is in the context of using the IDI for statistical purposes and is not related to the data's ability to support Inland Revenue's core operational requirements. The authors are thankful to Anna Bindler as well as participants at the ESPE 2022 and AEA/ASSA 2022 conferences for valuable comments.

1. INTRODUCTION

Becker's (1968) seminal work in the economics of crime models the decision to engage in crime as a trade-off between expected costs and benefits. Building on this notion, theoretical and empirical work in the economics of crime has examined a range of determinants of criminal offending. This literature considers an array of factors, such as labor market conditions (Ihlanfeldt, 2007; Machin & Meghir, 2004; Phillips & Land, 2012; Recher, 2020), education (Åslund *et al*., 2018; Machin *et al*., 2011), as well as the likelihood of detection and severity of punishment (e.g. Bhuller et al., 2020; Chalfin & McCrary, 2017) as important drivers of criminal offending. More recently, economists have been increasingly interested in the consequences of victimization (e.g. Bharadwaj et al., 2021; Bindler et al., 2020; Bindler & Ketel, 2021). In some circumstances, however, individuals find themselves on both sides of the justice system as victims *and* offenders.

The overlap between victimization and offending is an important factor in understanding both the determinants and consequences of crime. Reciprocity between victims and offenders is a well-documented stylized fact in criminology with von Hentig (1940, p. 303) describing it as "*one of the most curious phenomena of criminal life*" in his seminal work over 80 years ago. Empirical assessment of this overlap is extensive in the criminology literature (see, for example, Berg et al., 2012; Berg & Mulford, 2020; Jennings et al., 2012; Lauritsen & Laub, 2007 for comprehensive literature overviews). However, existing literature has focused on identifying the descriptive relationship and role of time-invariant population heterogeneity in simultaneously determining victimization and offending. With improvements in the quality and availability of crime data over time, more studies have aimed at identifying dynamic relationships between victims and offenders (Deadman & MacDonald, 2004; Entorf, 2013; Ousey et al., 2011). Nonetheless, the economics of crime literature still lacks a clear and generalizable conclusion, particularly with respect to isolating population heterogeneity from dynamic causal effects. This dearth of evidence is due to the lack of reliable population-level data distinguishing the timing of victimization and offending events.

We use administrative data covering the universe of all police investigations in New Zealand to address this gap in the literature. Recorded at the incident-level, these data identify suspected victims and offenders, detail the offense type, and establish the relationship between suspected victims and offenders. These data not only enable us to establish the existence of victim-offender overlap, but to detail the nature of overlap by offense type, the relationship between victim and offender, and the timing between incidents. In particular, we ask whether overlap is driven by time-invariant population heterogeneity, or whether there is a dynamic causal component. That is, is it only an individual's characteristics, lifestyle, and/or neighborhood that make them more likely to be both a victim and offender? Or does being an offender *itself* increase the likely of becoming a victim in the future, and/or vice versa? Crime events in New Zealand are internationally relevant due to the country's moderate crime rates[7] as well as the similarity of its criminal justice system to the U.K. and other former British colonies, including the U.S., Canada, and Australia.[8] External validity is further bolstered by using administrative, rather than survey, data.

Administrative data afford us at least three advantages over survey data. First, our data cover the entire resident population of New Zealand, whereas existing research relies on surveys given to specific sub-groups of the population, such as students and young adults (Deadman & MacDonald, 2004; Entorf, 2013; Ousey et al., 2011). This renders our results more generalizable to the broader population. Second, administrative data do not rely on survey participants' recall of past victimization and offending over a specific time period, which may be subject to recall bias. Third, and most importantly, longitudinal administrative data allow us to exploit the precise timing of victimization and offending incidents—something not possible with cross-sectional survey data. We make use of the longitudinal structure of our data by removing individual-level sources of time-invariant heterogeneity in a two-way fixed effects model. We also appeal to dynamic panel methods to estimate intertemporal relationships between victimization and offending after accounting for individual fixed effects. This approach is in line with recent studies using administrative data to investigate determinants and consequences of victimization and criminal offending (e.g. Åslund et al., 2018; Bharadwaj et al., 2021; Bhuller et al., 2020; Bindler & Ketel, 2021; Doleac, 2018; Koppensteiner & Menezes, 2021). We do not estimate recursive bivariate

[7] Data from the United Nations Gallup World Poll shows that New Zealand has comparable crime rates for certain offenses compared to the U.S. and U.K.. Specifically, over the years 2006-2019 the estimated percent of the New Zealand population that was affected by theft and violence (i.e., assault/mugging) was 16 percent and 2 percent, respectively. For the U.S. these percentages were estimated to be 14 percent and 2 percent, respectively. Over the same period, the percentage of the population that was estimated to be victimized by theft and violence in northern Europe was 11 and 3 percent, respectively (van Dijk et al., 2021).

[8] Due to New Zealand's history as a British colony, the justice system is not just similar to the U.K. system but was actually modelled on it. For example, the New Zealand criminal justice system, like that of the U.K., U.S., Canada, and Australia, follows case law, based on a common law system (as opposed to a civil law system which is common in for example continental Europe).

probit models—the most common approach in the existing literature—as these require victimization and offending events to be pooled over time, which often results in misleading findings. Pooling would also prevent us from investigating the dynamic relationship between victimhood and criminality—our primary interest in this work.

Our econometric models estimate the link between past offending (victimization) and current victimization (offending). As in previous literature, we identify substantial victim-offender overlap in the data. Moreover, we find evidence of a causal relationship between previous victimization (offending) and current offending (victimization) after removing time-invariant heterogeneity. However, previous victimization (offending) is only positively linked to current offending (victimization) in the few months immediately preceding an incident. The dynamic relationship between victimization and offending is also driven by simultaneous events where an individual is both an alleged offender and victim (e.g., mutually combative assaults), and becomes weaker when these events are excluded. This result supports theoretical explanations that emphasize the importance of population heterogeneity as a driver of victim-offender overlap. Policy implications follow naturally: because much of victim-offender overlap results from population heterogeneity, this is consistent with the view that early life-course interventions would be most effective. However, because we find evidence that criminal behavior leads to victimization, and vice versa, this suggests that it may also be appropriate to time interventions at the point of the first offending or victimization event.

In examining the nature of the relationship between victimization and offending, we contribute to the existing literature in the economics of crime. Although the victim-offender overlap is a prominent topic in criminology, it has been less explored by economists. Nevertheless, the determinants and consequences of criminal behavior is a growing area in the economics of crime literature, and the victim-offender overlap is a potentially important component of both aspects. For example, Koppensteiner & Menezes (2021) finds that exposure to violence is associated with lower test scores and a higher probability of dropping out of school, and, in turn, other research, such as Åslund *et al.* (2018) and Machin *et al.* (2011), highlights that lower education levels are linked to a higher probability of engaging in criminal activity. In addition, Bindler & Ketel (2021) explicitly account for a potential victim-offender overlap in estimating the impact of victimization on subsequent labor market outcomes and find smaller estimated impacts of victimization for non-offenders. Understanding the dynamic association between offending and victimization is thus crucial for both strands of literature.

The remainder of the paper is as follows: Section 2 gives an overview of the existing literature and theoretical background behind victim-offender overlap; Section 3 describes administrative data used in the analysis; Section 4 summarizes two-way fixed effects and dynamic panel strategies; Section 5 presents results; and Section 6 concludes with a discussion of policy recommendations aimed at minimizing harm from victim-offender overlap.

2. EXISTING LITERATURE AND THEORETICAL BACKGROUND

The works of Hans von Hentig (1940; 1948) and Marvin E. Wolfgang (1958) were among the earliest and most influential contributions to the criminology literature, introducing the idea of a mutual and reciprocal relationship between offenders and victims. Since then, a significant body of literature has evolved on the link between victimization and offending, drawing a surprisingly clear picture: *"…we are unaware of any research that has examined the link between offending and victimization and failed to find a strong relationship. The relationship has been found across time, place, and for various subgroups"* (Lauritsen & Laub, 2007, p.60). Recognizing this stylized fact had a tremendous effect on the criminological literature and was a milestone for the research on the determinants of crime in general (Berg & Mulford, 2020; Reiss, 1981).

Criminology literature attempting to explain the association between victimization and offending can be divided into two types. The first type explains the association based on assumptions about population heterogeneity. These explanations highlight that victim-offender overlap exists due to (largely) time-invariant individual characteristics, but do not suggest a dynamic relationship whereby offending leads to subsequent victimization or victimization leads to subsequent offending. Second, a much smaller literature attempts to identify dynamic effects caused by state-dependent processes, whereby offending leads to an increased risk of subsequent victimization and vice versa.

Population heterogeneity in criminology

The analysis of population heterogeneity dominated the criminology literature for many years. This concept describes a relationship between victimization and offending driven by unobserved socio-demographic, economic, and psychological characteristics. The most prominent explanation is the so-called "lifestyle perspective" initiated by the work of Hindelang *et al.* (1978), which assumes an important role of differential exposure to crime. Based on this theory, the lifestyle and everyday activities of many offenders and victims are dominated by relatively risky behavior patterns which directly increase their chances of being exposed to crime (Cohen & Felson, 1979; Foreman-Peck & Moore, 2010; Osgood et al., 1996). These theoretical considerations were supported in multiple studies finding a strong link in the socio-demographic profiles of victims and offenders (Broidy et al., 2006; Sampson & Lauritsen, 1990; Silver et al., 2011; Singer, 1981; Turanovic et al., 2015; Wittebrood & Nieuwbeerta, 1999). Closely linked to this is the idea of "crime concentration" which was introduced by Weisburd and co-authors (2012, 2014). This suggests that neighborhoods are indispensable in explaining the overlap between victimization and offending. In addition to lifestyle and exposure explanations, a personality perspective has also been proposed. This suggests that individuals with certain personality traits, such as low self-control, are more likely to be offenders and victims, leading to a victim-offender overlap (Flexon et al., 2016; Gottfredson & Hirschi, 1990; Piquero et al., 2005; Turanovic et al., 2015; van Gelder et al., 2015).

Population heterogeneity in the economics of crime

Although victim-offender overlap first emerged in the criminology literature, and the economics literature has not said much explicitly on this phenomenon, it is consistent with rational choice and behavioral economics literature in this area. The application of rational choice theory to the economics of crime maintains that individuals weigh the expected costs and benefits of crime. These costs and benefits will vary depending on the characteristics of the individual in terms of the opportunity cost of committing crimes, as well as differences in personality traits, individuals' degree of risk aversion and how heavily they discount the future. For example, those with lower relative incomes and less educated individuals will have less to lose and more to gain from crime (Lochner, 2004). This is supported by empirical studies which examine the relationship between income and income inequality and crime (Fleisher, 1966), education and crime (Ehrlich, 1975). The education and crime literature also includes quasi-experimental studies which exploit an increase in the school leaving age and find that increases in education reduce criminal activity (Lochner & Moretti, 2004; Machin et al., 2011). Existing literature also discusses the link between crime and personality traits, risk aversion and discount rates (Almlund et al., 2011; van Winden & Ash, 2012).

In terms of the overlap with victimization, this literature offers two conflicting possibilities. First, a rational offender will target victims who offer a high payoff, for example, higher wealth individuals. However, higher wealth individuals have more to lose and less to gain from committing crimes, leading to a clear difference in the characteristics of those that theory would predict are more likely to be offenders versus victims. On the other hand, those who are less risk averse and/or have higher discount rates are more likely to partake in riskier behavior and pay less attention to their personal safety, leaving them more exposed to being a potential victim. This suggests that victim-offender overlap will differ depending on crime type: overlap may be more prevalent in violent crimes, where population heterogeneity explanations may be more relevant, and less prevalent for property crimes, where the rational choice to target victims with higher expected payoffs would be more relevant.

While some insights into population heterogeneity explanations can be drawn from rational choice theory, there are only a handful of economic models which explicitly address the victim-offender overlap. These emerged early on and mostly fall under the umbrella of population heterogeneity. Balkin and McDonald (1981) suggested an economic model of crime which is based on the amount of time spent in public spaces which expose potential victims to the risk of crime. Closely related is the idea of a "subculture of violence" in which victims and offenders are exposed to similar crime-endorsing values and behaviors which again reinforce the same behavior among them as detection and informal punishment rates are low (Agnew, 1992; Akers, 2009; Berg et al., 2012; Jensen & Brownfield, 1986). An extreme example for this idea is the analysis of gang memberships and its role in explaining the victim-offender overlap (Pyrooz et al., 2014).

Dynamic relationships between victimization and offending

While descriptive literature on these different aspects of population heterogeneity is rich, few empirical studies attempt to identity the dynamic relationship between victimization and offending. As put forth by Lauritsen and Laub (2007), these dynamic relationships are caused by state-dependency whereby current experiences affect future risks. In line with the discussion of the lifestyle hypothesis above, a dynamic effect of offending on victimization and vice versa exists if the event causes the victim or offender to change aspects of their lifestyle, their risk-preferences, or their social environment. In addition to this indirect effect, a direct effect can be hypothesized especially from earlier offending on victimization risk in line with the arguments in Jensen and Brownfield (1986) as well as Deadman and McDonald (2004), if we assume that offending increases a person's vulnerability and exposure to future crime.

Behavioral economics also offers insights into the victim-offender overlap, particularly the possibility of a dynamic relationship in the direction of victimization leading to subsequent offending, as summarized in Entorf (2013). Humans seem to have an innate desire for fairness and a willingness to retaliate even if this is costly to themselves in the short run (Fehr & Gächter, 2002). This is confirmed by the findings of experimental economics (Fehr & Schmidt, 2006), suggesting that retaliation by victims results in a dynamic relationship whereby victimization leads to offending. This idea is also found in the criminological literature, where anger in response to being victimized triggers retaliation (for example, Agnew, 1992; Jacobs & Wright, 2010; Kubrin & Weitzer, 2003; Simons & Burt, 2011). However, the criminology literature suggests that this could be directed towards the perpetrator or undirected "lashing out" towards those who were not involved in the original perpetrating. Directed retaliation may also be considered rational in the context of repeated games where punishment reinforces cooperative behavior. This is consistent with results with experimental economics literature highlighting that altruistic punishment to maintain cooperation is only used when conditions are relatively favorable–that is, when costs to the punisher are relatively low and the impact on the punished is relatively high (Egas & Riedl, 2008). It should also be noted that these retaliatory motives explanations imply a dynamic relationship in one direction only: from victimization to offending, but not vice versa. Even more closely connecting victimization and offending than retaliation are simultaneous victim-offender events. For example, in mutually combative events such as bar fights, a direct causal link between victimization and offending can be observed (Daday et al., 2005).

Empirical evidence

To date, there has been little empirical testing of theoretical explanations of the victim-offender overlap, particularly in terms of the possibility of a dynamic relationship. One major reason for this gap in the literature is the lack of reliable longitudinal data. Lauritsen *et al.* (1991)

was among the first studies to use longitudinal survey data in order to identify the sequencing of victimization and offending in more detail. The authors applied OLS regressions which included one-period lagged victimization and delinquency measures as explanatory variables but did not employ panel-data econometric techniques. They found a strong dynamic relationship between victimization and offending even after controlling for sociodemographic and environmental characteristics. These findings have been supported by a number of subsequent empirical studies (see e.g. Jennings et al., 2010; Schreck et al., 2008).

More recent studies concentrate on sophisticated econometric models in combination with longitudinal data to identify the dynamic causal relationship between victimization and offending. For example, Deadman and MacDonald (2004) analyze data from the 1998 Youth Lifestyles Survey of about 4,000 people aged 12-30 in England and Wales. Using recursive bivariate probit analysis, they find that offenders are more likely to be victims, but not vice versa. Ousey *et al.* (2011) base their analysis on data from the Rural Substance Abuse and Violence Project (RSVP) which follows 4,102 students in Kentucky from 7th to 10th grade (13 – 16 years of age). Using fully simultaneous latent variable structural equation modelling, they also find that offenders are more likely to be victims but not vice versa. Finally, Entorf (2013) uses data from the German Crime Survey involving a selective sample of 960 adults above the age of 18.[9] Their recursive bivariate probit model comes to similar conclusions.

Nevertheless, these studies lack external validity as they are based on selective samples of teenagers and young adults, and lack internal validity as they rely on self-reported information about victimization and offending from survey data (Jennings et al., 2012). This limits the generalizability of the results. The timing of any offending and victimization also lacks precision, with the survey data only recording whether the respondent said they were a victim or offender within a certain time period (e.g., the last 12 months), and not whether the offending occurred before the victimization or vice versa. This prohibits the use of dynamic panel models that take account of whether observed offending occurred before or after victimization.

3. DATA

[9] The sample is highly selective as it was designed as a nationwide control group (of the non-incarcerated population) for the German Inmate Survey and thus resembles the prison population. For example, it is, on average, younger and less educated than the general German population as well as predominantly male.

Integrated Data Infrastructure

We use administrative data available within the Integrated Data Infrastructure (IDI) provided by New Zealand's national statistics agency, Stats NZ.[10] The IDI is a centralized research database containing individual-level data from a range of sources, including administrative databases, Stats NZ surveys, and non-governmental organizations. The IDI allows researchers to link across sources, bringing together individual-level data spanning several areas including justice, tax and income, welfare, health, and education outcomes via a unique individual identifier.

Our main data source is comprised of the Recorded Crime Offenders Statistics (RCOS) and the Recorded Crime Victims Statistics (RCVS) databases administered by New Zealand Police. RCOS collects information on every alleged offender reported from July 2009 to June 2020. Detailed information is available on each criminal incidence, including: the type of alleged offense committed[11], a standardized measure of its seriousness[12], and police action taken (i.e., whether the police proceeded with the offense and how, such as informal/formal warning, arrest, and prosecution, etc.). Similarly, RCVS includes information on all alleged victims of crime recorded by the police on an incident basis between July 2014 and June 2020. Offenders and victims are linked via unique individual identifier, allowing us to observe if a person is both an offender and victim. Moreover, each police incident has a unique identifier, allowing us to see who was involved in each incident as either an offender or victim (or both). Since police records are comprehensive, they include very minor infractions. We, therefore, exclude incidents involving very minor offenses that are not punishable by imprisonment, such as minor traffic offenses (i.e., those categorized as having the "lowest" seriousness).[13] We also exclude individuals who are under 18 years of age since they undergo a different judicial process.

Although these data cover the universe of all reported crimes in New Zealand over the study period, some limitations remain. Unreported offenses are of course not included, and survey data suggest that only about a quarter of crimes are reported to the police (Ministry of Justice,

[10] We use data from the October 2020 IDI refresh.
[11] Crime types are categorized based on the Australian and New Zealand Standard Offence Classification (ANZSOC).
[12] The New Zealand justice sector seriousness scores are based on the average sentences that such an offense would carry. For details, see McRae, Sullivan, and Ong (2017).
[13] Formally, we exclude Category 1 offenses, as defined by the Criminal Procedure Act 2011. Online at https://www.legislation.govt.nz/act/public/2011/0081/latest/dlm3359962.html (accessed 15 October 2021). Police data do not always contain information on the victims of burglaries.

2021). However, surveys of offending and victimization are also likely to involve significant reporting, recall, and perception errors. Moreover, under-reporting may be selective. Particularly relevant to victim-offender overlap is the possibility that offenders are less likely to report crimes as victims since they do not want to bring themselves to the attention of the police in any capacity. A further limitation is that offender data is likely to be more complete than victim data. This is for three reasons. First, there is no clear victim in many criminal incidents, such as incidents of public nuisance or in some burglaries, for example.[14] Second, information may not be collected from victims who are reluctant to supply it when it is unnecessary. Lastly, because we are only using seven years of data, we cannot rule out the possibility of earlier victimization leading to future offending, for example, in the case of being a victim during childhood. Importantly, the potential sources of reporting bias discussed above would result in fewer detected incidents of victim-offender overlap. In this sense, we consider our estimates of victim-offender overlap to be a lower bound of the true incidence.

Sample definition and variables of interest

To define our population of interest, we rely on New Zealand's Estimated Residential Population (ERP) between 2014 and 2020. The ERP includes individuals based on activity in administrative systems (i.e., taxes paid, health care receipt, receipt of social benefits, education enrollment, and data on migration and border movements) that indicates an individual is present in New Zealand during that year. It, therefore, removes individuals who left the population due to death or outmigration, and precludes tourists (Gibb et al., 2016).

For reasons of computational power, we draw a 10% random sample of the ERP as our spine. We then expand annual ERP observations to a monthly dataset based on the assumption that an individual is part of the NZ population in every month of the year in which they are observed in the ERP. We then merge the observed victimization and offending incidents in each month to the spine. A single month can involve multiple incidents and an incident can involve multiple alleged offenses. For example, an armed robbery may involve both theft and firearm offenses. To merge the victim and offender information to a monthly database of the NZ population, we thus collapse the information on the monthly level by only keeping the most severe offense per incident and the most severe incident per month. Based on this approach of aggregating the information on the monthly level, our key explanatory and dependent variables are indicators for at least one victimization or offense in each month.

Descriptive statistics

Our 10% random sample includes 393,000 unique individuals with a total of 13,381,700 observation-months (on average about 34 observation months per individual).[15] Between 2014 and 2020, these individuals were involved in 19,000 reported offending and 24,300 recorded victimization events. As is shown in Table 1, most of these individuals (90.5%) were not involved in any event as either an offender or victim. About 5.1% were involved in at least one event as a victim, and 3.8% as an offender. Only 1% (4,000) were both offenders and victims.

Table 1. Bivariate frequencies and unadjusted conditional probabilities of any victimization or offending, 2014-2020

		victim			
		no	yes	total	
offender	no	353,800	20,200	374,000	$\Pr(V_i=1\|O_i=0)$
	(cell %)	*(90.53%)*	*(5.14%)*	*(95.17%)*	5.40%
	yes	15,000	4,000	19,000	$\Pr(V_i=1\|O_i=1)$
	(cell %)	*(3.82%)*	*(1.02%)*	*(4.83%)*	21.05%
	total	368,800	24,300	393,000	
	(cell %)	*(93.84%)*	*(6.18%)*		
		$\Pr(O_i=1\|V_i=0)$	$\Pr(O_i=1\|V_i=1)$		
		4.07%	16.46%		

Source: Authors' calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). Counts are from a random sample of 10 percent of the New Zealand estimated resident population from June 2014 to May 2020. Counts reflect all victims and offenders investigated for criminal incidents deemed "low," "moderate," or "high" seriousness. "Lowest" seriousness incidents are excluded. Counts have been rounded to the nearest 100 in accordance with the Stats NZ confidentiality protocol.

While the share of individuals who are both victims and offenders is small, conditional probabilities better highlight the degree of overlap between victimization and offending. For those who were not offenders over the 2014 to 2020 period, there is a 5.4% probability that they are victims. If the individual was an offender, this probability of being a victim increases almost

[15] Based on confidentiality requirements from Stats NZ, counts and observation numbers presented are rounded to the nearest 100.

fourfold to 21.1%. Similarly, for those who were not victims, the probability of offending is 4.1%, compared with a probability of offending of 16.5% for those who had been a victim.

Table 2 gives an overview of the characteristics of those who fall into the four groups 1) neither victim nor offender; 2) offender but not a victim; 3) victim but not an; and 4) both victim and offender. Females are most underrepresented among those who are offenders only, and are also underrepresented in the overlap group of those who are both victims and offenders. The overlap group has the lowest average age, followed by those who are offenders only, while those who are neither victims nor offenders are older on average. Those in the overlap group are less likely to be European or Asian and more likely to be Māori or Pacific Peoples. They also have lower average earnings and are much more likely to have had a parent who has been charged with a crime since court records began in 1992.

Table 2. Descriptive statistics

	$Vi = 0, Oi = 0$	$V_i = 0, O_i = 1$	$V_i = 1, O_i = 0$	$V_i = 1, O_i = 1$
	mean (s.d.)	*mean (s.d.)*	*mean (s.d.)*	*mean (s.d.)*
Female	.521	.167	.494	.398
Age	46.89 (19.18)	37.62 (13.64)	38.30 (15.42)	34.07 (11.68)
Ethnicity				
European	.644	.404	.541	.366
Māori	.125	.430	.222	.507
Pacific Peoples	.059	.110	.065	.074
Asian	.151	.045	.155	.040
MELAA	.015	.011	.016	.012
Other	.006	< .001	.001	< .001
Parent charged	.034	.091	.062	.110
Annual earnings	31,399 (40,736)	20,402 (24,392)	32,590 (38,697)	12,872 (19,015)
Observations	353,800	15,000	20,200	4,000

Source: Authors' calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS), Recorded Crime Offenders Statistics (RCOS), Inland Revenue, Stats NZ personal details and Ministry of Justice Court Charges data. "Parent charged" equals one if any parent was charged with a crime since 1992 (when the data series begins) and zero otherwise. Counts have been rounded to the nearest 100 in accordance with the Stats NZ confidentiality protocol. MELAA stands for Middle Eastern, Latin American, and African.

Table 3 describes observed criminal incidents separately for offenders only, victims only, and both victims and offenders. Characteristics of the overlap group are different for the offense and the victimization because the crime for which a person is an offender may differ from the crime for which they are a victim.

Table 3. Proportions of offense and victimization types

	Offender Only $V_i = 0, O_i = 1$	*Victim Only* $V_i = 1, O_i = 0$	*Overlap* $V_i = 1, O_i = 1$
Offender:			
Retaliatory	-	-	.056
Simultaneous victim/offender	-	-	.044
Repeat offending	.393	-	.522
Violent	.538	-	.571
Property	.263	-	.362
Family	.271	-	.306
Intimate partner violence	.211	-	.237
Sexual	.061	-	.042
Weapon	.172	-	.225
Victim:			
Retaliatory	-	-	.041
Simultaneous victim/offender	-	-	.026
Repeat victimization	-	.142	.309
Violent	-	.321	.610
Property	-	.714	.502
Family	-	.089	.204
Intimate partner violence	-	.090	.211
Sexual	-	.045	.050
Weapon	-	.063	.183
Observations	15,000	20,200	4,000

Source: Authors' calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS), Recorded Crime Offenders Statistics (RCOS).

In the overlap group, individuals are more likely to be repeated offenders (52.2%) than repeated victims (30.9%) and offenses are less likely to be violent (57.1%) than victimizations (61.0%). Repeat offending and victimization rates are higher in the overlap group compared to those who are only offenders or only victims. Those who are both offenders and victims are also more likely to be involved in violent crimes, intimate partner violence, crimes involving family members, and crimes involving weapons compared to those who are only victims or only offenders. As one might expect, violent crimes are the most prevalent type of incident among those who are offenders only or both victims and offenders, while property crimes are the most prevalent among those who are only victims.

Two special cases of criminal events warrant attention when examining victim-offender overlap. First, there are incidents of simultaneous victimization and offending which occur when a person is an alleged victim and offender within the same event. In these situations, victimization and offending does not necessarily have to involve the same people. For example, if Person A hits Person B in a bar fight, and then Person A is hit by Person C, then Person A would be recorded as both an offender and victim, although they offended against Person B and was victimized by Person C. About 4.4% of individuals in the overlap group have been involved in at least one such incident as offenders, and 2.6% individuals have been involved as victims.

The second special case is retaliatory crime. There is some overlap between these two special cases, however retaliatory incidents involve the same victim-offender dyad. Specifically, retaliatory incidents occur when Person A offends against Person B, and Person B also offends against Person A, either simultaneously or at a later date. Note that this is direct retaliation where the victim retaliates against the specific person who offended against them rather than retaliation involving the victim lashing out at any available victim, as described by Jacobs and Wright (2010). About 5.6% of individuals in the overlap group have been involved in at least one retaliatory incident as offenders and 4.1% as victims.

4. EMPIRICAL MODELS

We employ three approaches to examine the overlap between criminality and victimhood: multivariate regression analysis, two-way fixed effects models, and dynamic panel models. Each approach has its respective advantages and disadvantages which we discuss in detail.

Multivariate regression analysis

As a baseline, we estimate the association between past victimization (offending) on current offending (victimization) using multivariate regression models. These models can be represented as:

$$(1) \quad O_{it} = \alpha_0 + \sum_{j=1}^{12} \beta_j \, O_{i,t-j} + \sum_{k=0}^{12} \gamma_{k+1} \, V_{i,t-k} + \boldsymbol{X_{it}\delta_{it}} + \theta_t + \varepsilon_{it}.$$

$$(2) \quad V_{it} = \alpha_0 + \sum_{j=1}^{12} \beta_j \, V_{i,t-j} + \sum_{k=0}^{12} \gamma_{k+1} \, O_{i,t-k} + \boldsymbol{X_{it}\delta_{it}} + \theta_t + \varepsilon_{it}$$

where O_{it} is equal to one if individual i was a criminal offender in month t, and zero otherwise. Similarly, V_{it} is equal to one if individual i was the victim of a crime in month t, and zero otherwise. Equations (1) and (2) regress offending and victimization on current and/or lagged indicators of victimization and offending in a linear probability model. Models include a vector of covariates, $\boldsymbol{X_{it}}$, which includes income, age, and its square. A vector of monthly dummy variables to helps capture unobserved characteristics specific to certain months, such as police enforcement intensity, law enforcement resources, trends in certain crime types, as well as seasonal effects (e.g., more domestic disturbances during the holidays, more general crime during the summer, etc.). These models treat the data as a quasi-pooled cross-section and do not make full use of the panel structure of the data. (1) and (2) thus estimate the raw descriptive relationship between current offending (victimization) and past victimization (offending) without controlling for unobserved population heterogeneity. Potential time-invariant confounders at the individual-level include growing up in a high-crime neighborhood, family structure, risk preferences, and socioeconomic status, for example.

Two-way fixed effects models

In order to take full advantage of the panel structure of the data, we account for unobserved heterogeneity by including individual fixed effects. These models remove time-invariant individual-level characteristics from the analysis which may be correlated with both victimization and offending. These fixed effects models can be represented as:

$$(3) \quad O_{it} = \alpha_0 + \sum_{j=1}^{12} \beta_j \, O_{i,t-j} + \sum_{k=0}^{12} \gamma_{k+1} \, V_{i,t-k} + \boldsymbol{X_{it}\delta_{it}} + \theta_i + \theta_t + \varepsilon_{it}$$

$$(4) \quad V_{it} = \alpha_0 + \sum_{j=1}^{12} \beta_j \, V_{i,t-j} + \sum_{k=0}^{12} \gamma_{k+1} \, O_{i,t-k} + \boldsymbol{X_{it}\delta_{it}} + \theta_i + \theta_t + \varepsilon_{it}.$$

Models (3) and (4) are equivalent to those in (1) and (2) apart from θ_i representing individual fixed-effects. Unobserved time-variant individual characteristics (e.g. economic situation, changes in current living situation, changes in mental health) are still potentially confounding in two-way fixed effects models. Assuming that all time-variant characteristics are potentially endogenous to our models, as they might be caused by the initial exposure to crime, we assume them to be a crucial part of the dynamic relationship between both events.

Although insightful in terms of investigating the dynamics between criminality and victimhood, these models are not without their limitations. Specifically, introducing a lagged outcome variable on the right-hand side of the equation produces inconsistent results since the compound error term is correlated with the lagged dependent variable, although this is likely to impose a relatively small amount of bias given the size of our panel (Anderson & Hsiao, 1981, 1982). This limitation motivates our next approach.

Dynamic panel estimators

Our last empirical approach estimates dynamic panel models to address endogeneity in lagged outcome variables. These models would perhaps be the preferred vehicle in terms of capturing the relationship between victim and offender status as they address both heterogeneity and endogeneity concerns. However, they are subject to strict identification requirements and are not able to take advantage of the long nature of the panel data (Arellano & Bond, 1991).

Dynamic panel models are a class of estimators designed to provide consistent estimates when the dependent variable is at least partially dependent on its own past values. These models are specifically tailored to situations where the number of panel members, N, is large and the number of time periods, T, is small. The earliest models were developed by Holtz-Eakin, Newey, and Rosen (1988) and were popularized by Arellano and Bond (1991). These models use first differencing to remove heterogeneity, then apply instrumental variables (IV) methods to consistently estimate parameters on lagged dependent variables. The instruments considered are "deeper" lags of the dependent (also independent) variables in the model. The idea is that deep lags of the dependent variable are likely correlated with more recent values of the independent variable itself, but uncorrelated with current values of the dependent variable. These assumptions are testable.

Recognizing that Arellano-Bond estimators often suffer from weak instruments, multiple improvements have been made to original estimators in order to increase precision (Arellano & Bover, 1995; Blundell & Bond, 1998). We utilize generalized method of moments (GMM) estimation following Blundell and Bond (1998) to increase the relevancy of IVs used in the analysis. These estimates control for time-invariant individual characteristics and time trends. However, there remains a risk that certain unobservable individual-level time-variant characteristics remain unaccounted for. In fact, items such as family structure, neighborhood, and socio-economic status may change over time, although it can be argued they are slow to change and therefore relatively stable, especially over reasonably short time periods. As mentioned above, we only use 12 months of data for dynamic panel estimates, which means these models only use a small fraction data available. Results report Windmeijer (2005) WC-robust standard errors and tests of no error correlation—the identifying assumption of the Arellano-Bond estimator.

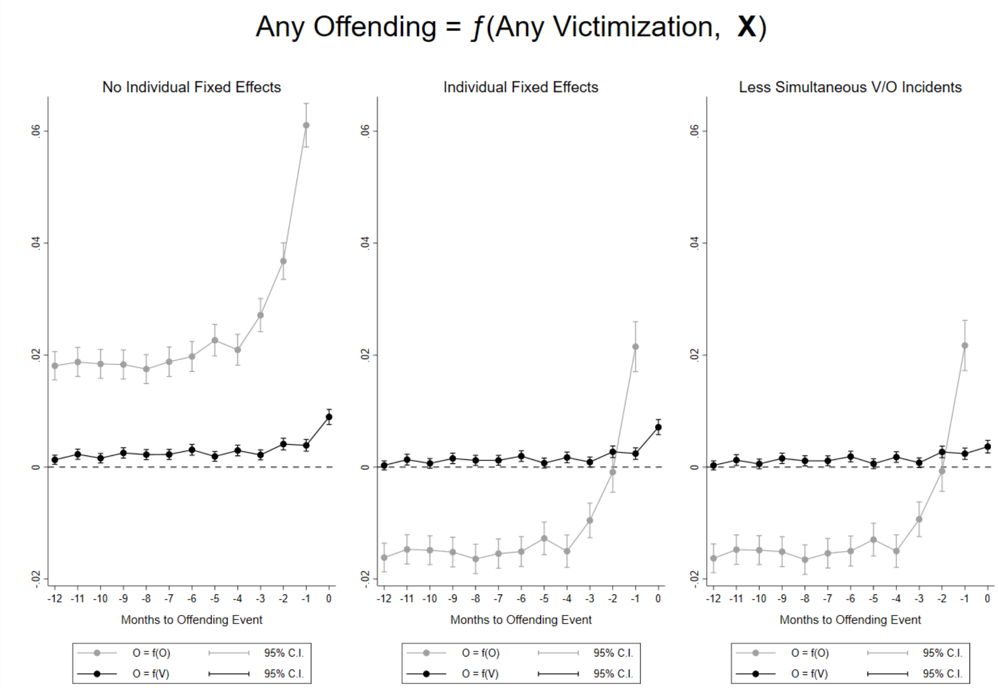
5. RESULTS

Multivariate regression and two-way fixed effects models

Figure 1 presents results for equations (1) and (3), where current offending (at time zero) is a function of current and lagged victimization (black), as well as lagged offending (grey). Vertical bars represent 95% confidence intervals based on robust standard errors. The first panel (left) of Figure 1 estimates the model with no fixed effects; the middle panel estimates the same equation with individual-level and time fixed effects; and the third panel (right) removes simultaneous victimization/offending incidents. Similarly, Figure 2 presents results for equations (2) and (4), where current victimization (at time zero) is a function of current and lagged offending (black), as well as lagged victimization (grey). Full estimation results are shown in Tables A1 and A2 of Appendix A.

Ignoring population heterogeneity, Figure 1 shows that previous offending in any of the previous 12 months increases the likelihood of offending in the current period by between two and six percentage points, with the magnitude of estimates increasing closer to the current period. That is, there is a positive and statistically significant relationship between current and past offending. In terms of victimization, there is also an increased likelihood of victimization in the months

Figure 1. Victim-offender overlap, any offending

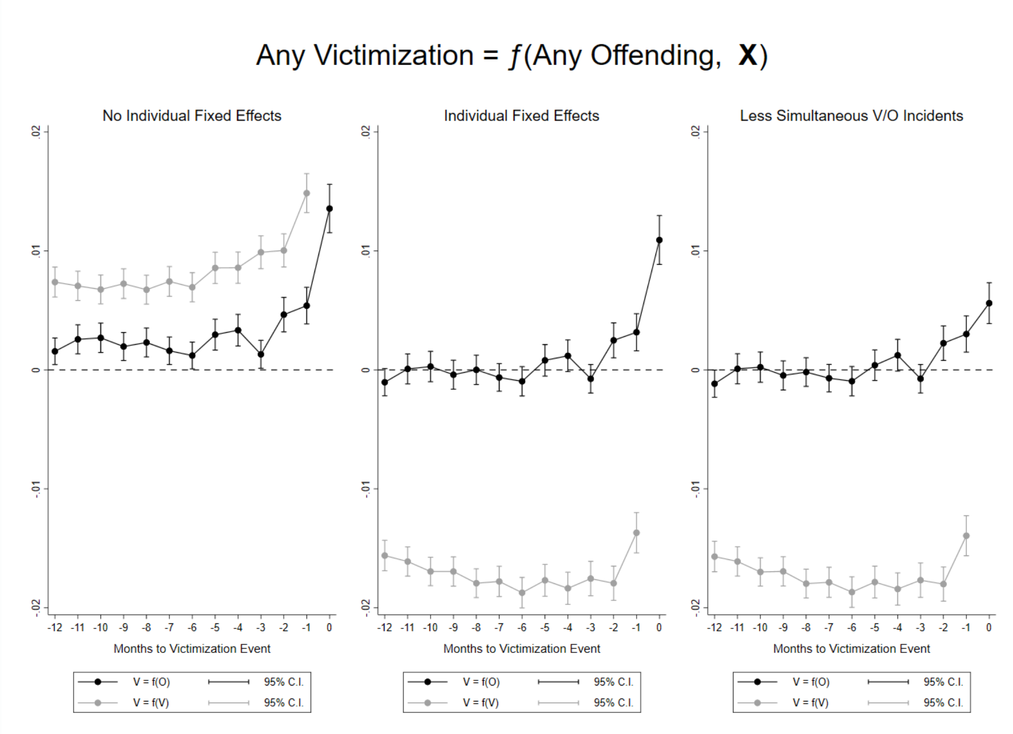


Source: Authors' illustration and calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). All models include month fixed effects. Robust standard errors used to construct 95% confidence intervals.

leading up to, and in the month of, the offending event, with the likelihood increasing as the current period draws closer. Although consistent with previous studies of victim-offender overlap, these results mask the effects of population heterogeneity.

The inclusion of fixed effects in Figure 1 changes results substantially, suggesting that population heterogeneity is an important factor explaining the dynamic relationship between victimization and offending. In terms of the relationship between current and past offending, there is now a negative relationship until two months before the offending event. A positive relationship between past and current offending is now detected only in the month preceding the offending event. In the month prior to the offending event, the coefficient on previous offending decreases from 6.11 to 2.15 percentage points. Given that the average offending rate per month is roughly 0.07 percent over our sample period, this means that individuals offending in month *t-1* are roughly 30 times more likely to offend in month *t* compared to those that did not commit crimes in the previous month. Although this estimated effect may seem quite large in magnitude, recall that most

Figure 2. Victim-offender overlap, any victimization



Source: Authors' illustration and calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). All models include month fixed effects. Robust standard errors used to construct 95% confidence intervals.

individuals neither committed any crimes nor were victimized over the sample period (90.53 percent) and between 39 and 52 percent of offenders were repeat offenders. The negative dynamic relationship between long run past offending and current offending may be explained by incarcerations or increased monitoring (such as supervised remand) which decrease offending opportunities or deter further criminal activity by increasing the probability of detection if another crime is committed. It may also be explained by an updating of beliefs in light of new information. This would be in line with Lochner (2007), which finds that beliefs about the probability of arrest are largely unresponsive to most outside influences, but they do respond to an individual's own experiences. Those who commit crimes and are not caught reduce their perceived probability of being arrested, while those who are caught increase their perceived probability. This would lead to a negative relationship between past and current offending as updated beliefs increase an individual's perceived likelihood of detection and, therefore, their estimation of the expected costs of committing a crime. We refer to negative relationship between previous offending and current offending as the "punitive effect."

The inclusion of individual fixed effects also lowers coefficients on current and previous victimization. Those who are victimized in month *t* are 0.71 percentage points more likely to also offend in month *t*. Relative to an average monthly victimization rate of 0.07 percent over the sample, this means that individuals victimized in month *t* are roughly ten times more likely to also offend in month *t*. Individuals victimized in month *t-1* are nearly 3.5 times more likely to criminally offend in month *t*, an effect that is both statistically and economically significant. Thus, although much of the relationship between current offending and past victimization, and indeed current offending and past offending, is driven by population heterogeneity, we find evidence of a true dynamic causal relationship between victimization and offending. Yet, the positive dynamic relationship between victimization and offending appears to be relatively ephemeral in nature.

To explore the possibility that the remaining relationship is driven by simultaneous incidents where the individual is both an alleged offender and victim, these events are removed in the third panel (right) in Figure 1. The same patterns emerge, however point estimates in the current period decrease. For example, the effect of current victimization on current offending is cut nearly in half from 0.71 to 0.37 percentage points yet remains highly statistically significant. This suggests that even after accounting for population heterogeneity and events where individuals are simultaneously cast as offenders and victims, the phenomenon of victim-offender overlap persists. Appendix Tables A1 and A2 presents results in detail and include models where potential retaliatory events are removed from the analysis in column (4). The removal of potential retaliatory incidents does not make a material difference in results.

Figure 2 shows the relationship between current victimization and past offending (black) and victimization (grey). Without individual fixed effects, Figure 2 shows a positive dynamic relationship between past victimization and current victimization, with the strength of the relationship (again) increasing markedly as the current period approaches. A similar pattern emerges between past and current offending and victimization, with previous offending increasing the likelihood of current victimization by between 0.1 and 1.4 percentage points. The inclusion of individual fixed effects vertically shifts both estimated curves downwards. Accounting for population heterogeneity now suggests that previous victims of crime are less likely to be victims in the current period. This "once bitten, twice shy" phenomenon demonstrates the importance of accounting for unobserved time-invariant factors when studying the dynamics of criminal behavior. For example, after removing population heterogeneity, and accounting for an average monthly victimization rate of 0.11 percent over the sample period, individuals victimized in month *t-1* are more than 12 times *less* likely to be victimized again in month *t* compared to individuals that were not victimized in the previous month. Estimates of victim-offender overlap are also significantly different after the inclusion of individual fixed effects: previous offending three months (and further) before the current period now has no significant effect on current victimization. However, individuals committing crimes in month *t-1* are estimated to be 2.9 times more likely to be victimized in month *t,* an estimate that is statistically significant at the one percent level. Individuals offending in month *t-2* are estimated to be twice as likely to be victimized in month *t*. When simultaneous victim-offender events are removed from the analysis, the magnitude of the coefficient on current period victimization decreases from 1.1 to 0.6 percentage points but remains highly statistically significant.

Overlap by crime type

We expect the drivers of victim-offender overlap to differ by crime type. Each crime type is unique in ways which affect the expected relationship between victimization and offending. For example, as discussed in Section 2, overlap may be more prevalent in violent crimes, where population heterogeneity explanations may be more relevant, and less prevalent for property crimes. In addition, property crimes are often targeted at strangers, thus there may be little evidence of overlap for this crime type. If gang crimes are more likely to involve weapons and gang-affiliated offenders often target rival gangs, crimes involving weapons may exhibit a large amount of victim-offender overlap in both directions. This subsection, therefore, previews estimates for various types of crimes in order to more fully understand the dynamic relationships in these special cases.

Located in Appendix B, Figures B1 through B10 present graphical results for two-way fixed effects models of five crime types: violent crimes, property crimes, intimate partner violence, sex crimes, and crimes involving weapons. Violent crime offending in Figure B1 reveals substantial punitive effects of previous violent offending on current violent offending. Specifically, after removing individual fixed effects, previous violent offending decreases the likelihood of violent offending in the current month by between 1.23 and 2.15 percentage points. In other words, individuals that were violent offenders in the previous 12 months were between 40 and 68 times less likely to violently offend in the current period (relative to individuals that did not violently offend in the past 12 months). Estimates of victim-offender overlap are statistically significant in the two months prior to the current period. For example, Figure B1 estimates that violent victims in month *t-1* are roughly five times as likely to commit a violent offense in month *t* as individuals that were not violently victimized in the previous month. Figure B2 shows little evidence of violent offending leading to violent victimization, except in the current month. After removing population heterogeneity, violent offending in month *t* increases the likelihood of being the victim of a violent crime in month *t* by 1.67 percentage points. This estimate is reduced to 0.5 percentage points after removing events where individuals are simultaneously recorded as a victim and offender within the same incident—nearly a twelvefold increase compared to the baseline violent victimization rate.

Figures B3 and B4 estimate the dynamic relationship between being a victim of property crime and being a property crime offender. Figure B3 clearly demonstrates that there is no effect of previous or current property crime victimization on property crime offending in the current period. As we mentioned, this may result from the fact that many victims of property damage, theft, and burglary are unknown to their perpetrators. Figure B4 estimates property crime victimization as a function of current and previous property crime offending. Because there are no instances of simultaneous property crime incidents in the New Zealand Police data, the second and third panels are identical. After controlling for population heterogeneity, property offending in month *t-1* increases the likelihood of being a victim of property crime in month *t* by 0.36 percentage points (553.6%).

Intimate partner violence is defined as a violent offense against a current or ex-partner. In Figures B5 and B6, there is no evidence that previous intimate-partner-violence victimization results in current period intimate-partner-violence offending. The coefficient on current period victimization is 1.68 percentage points, meaning that those who are victims of intimate partner violence in the current month are nearly 130 times more likely to be intimate-partner-violence offenders in the same month (compared to individuals that were not intimate-partner-violence offenders in the current month). However, this effect disappears when we remove events where individuals are simultaneously categorized as both victim and offender. This suggests that the intimate-partner-violence overlap observed in the data stems from incidents such as a mutually combative assault, for example. In Figures B7 and B8, we find zero evidence of any victim-offender overlap for crimes of a sexual nature—both overlap plots, including their 95% confidence intervals, sit directly atop the zero line.

Sex crimes include sexual assault, sexual offenses against minors, and sex trafficking, amongst others. In Figures B7 and B8, we find zero evidence of any victim-offender overlap for crimes of a sexual nature—both overlap plots, including their 95% confidence intervals, sit directly atop the zero line.

Plots for crimes involving weapons are shown in Figures B9 and B10. After removing population heterogeneity, Figure B9 reveals that weapons victimization in month *t* is associated with a 1.11 percentage points increase in the likelihood of weapons offending in month *t*. This estimate is reduced to 0.3 percentage points after removing simultaneous victim/offender incidents, suggesting that weapons victims in month *t* are roughly 35 times as likely to be weapons offenders in month *t* compared to individuals that did not offend in the current month. Weapons victimization in month *t-1* results in a 0.18 percentage point increase, or a twentyfold percent increase, in the likelihood of weapons offending in month *t*. This result is statistically significant at the five percent level.

Dynamic panel models

We now turn to the Arellano-Bond estimator for consistency in the presence of lagged dependent variables and serially correlated errors. Arellano-Bond removes individual and time fixed effects by first differencing equations (3) and (4) and using a set of lagged regressors as instruments. These dynamic panel models allow correlation in the dependent variable over time to be driven by three separate sources: 1) directly through lagged values of the dependent variable, known as true state dependence; 2) directly through observed independent variables, known as observed heterogeneity; and 3) indirectly through individual and time fixed effects, known as unobserved heterogeneity (Cameron & Trivedi, 2010). Because dynamic panel models require a short panel (i.e., large number of groups, N, and small number of time periods, T), the analysis only uses 2019 data. Because models are overidentified, more efficient estimation is achieved by using the optimal generalized method of moments (GMM) two-step estimator. Windmeijer (2005) WC-robust standard errors are reported in parentheses.

Results of Arellano-Bond dynamic panel models are presented in Table 4. However, before interpreting coefficients, we first examine tests of the identifying assumption of no serial correlation in the error terms. The null hypothesis of this test is no serial correlation in the first differenced residuals. From Table 4, we cannot reject the null hypothesis of no serial correlation for lags of order two (and higher). This provides evidence that Arellano-Bond model assumptions are satisfied. Results for offending as a function of current and past victimization are presented in column (1) of Table 4.

Table 4. Dynamic panel estimates of victim-offender overlap, 2019

	(1)	(2)
Variable	*Offender(t)*	*Victim(t)*
Offender(*t*)		.014*** (.004)
Offender (*t-1*)	.066*** (.007)	.010*** (.005)
Offender (*t-2*)	.027*** (.005)	.013*** (.003)
Offender (*t*-3)	.012*** (.004)	-.004 (.004)
Victim(*t*)	.006** (.002)	
Victim (*t-1*)	.009*** (.002)	.010*** (. 003)
Victim (*t-2*)	-.003 (.002)	.008*** (.003)
Victim (*t-3*)	.0004 (.002)	.006** (.003)
Order	*p*-value	*p*-value
1	.000	.000
2	.570	.665

Month Effects	YES	YES
Individual Effects	YES	YES
Number of Instruments		
Observations	2,926,600	2,926,600

Source: Authors' calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). In order to satisfy the requirement of having a "short" panel, only 2019 data are considered. Two-step estimators are computed with Windmeijer (2005) WC-robust standard errors reported in parentheses. *, **, and *** denote statistical significance at the 10, 5, and 1 percent-levels, respectively. The null hypothesis for autocorrelation tests is no autocorrelation in first-differenced errors.

Results are similar to estimates from two-way fixed effects models in Table 1. For example, current victimization increases the likelihood of current offending by 0.6 percentage points in the Arellano-Bond model, compared to 0.7 percentage points in the two-way fixed effects model. Coefficients for victimization in month *t-1* on offending in month *t* are 0.9 percent and 0.24 percentage points, respectively. However, coefficients on lags of orders two and three are not significantly different from zero in the Arellano-Bond model, while coefficients are positive and significant in two-way fixed effects models.

Results for victimization as a function of current and past offending are presented in column (2). These results are (again) similar in sign and significance to those from the two-way fixed effects approach, with larger coefficient magnitudes. In column (2), current offending is estimated to increase the likelihood of current victimization by 1.4 percentage points (compared to 1.1 percentage points in two-way fixed effects models). Coefficients on lags of orders two and three are 1.0 and 1.3 percentage points compared to 0.32 and 0.25 percentage points, respectively. In both models, coefficients on third-order lags are not statistically different from zero.

Overall, Arellano-Bond estimates, which have the advantage of controlling for endogeneity and individual and time fixed effects, produce results that are broadly similar to two-way fixed effects models. The difference in coefficient magnitudes can easily be explained by the short panel requirements of the Arellano-Bond estimator. Because Arellano-Bond models require a short panel, and deeper lags of dependent variables are needed as instruments, variation in the dependent variables is attributed to a smaller set of lags compared to two-way fixed effects models.

6. CONCLUSION

In this paper, we used administrative data to explore the relationship between criminal offending and victimization. Using a census of all investigated police incidents in New Zealand from 2014 to 2020, we employed panel data techniques to 1) examine the role of population heterogeneity in explaining victim-offender overlap and 2) search for evidence of a dynamic causal relationship after removing population heterogeneity from econometric models. This was done through constructing two-way fixed effects and dynamic panel models. Fixed effects models regress current month offending and victimization on lagged dependent variables, lagged and current independent variables, and time-varying individual characteristics such as age, its square, and income using the full 72-month panel. Dynamic panel models use deeper lags of dependent variables as instruments to consistently estimate the effect of offending on victimization (and vice versa) using a shortened panel limited to 2019 data. Both have their advantages and disadvantages, but largely agree in terms of findings.

We find that population heterogeneity plays an indispensable role in understanding the dynamics between victimization and offending. For instance, when we ignore population heterogeneity, we consistently find that previous offending is associated with a higher likelihood of offending in the current period. This positive relationship also holds for victimization. However, when population heterogeneity is removed using individual fixed effects, coefficients turn negative, and we find evidence of punitive effects and once-bitten-twice-shy effects. That is, in the absence of population heterogeneity, previous offending (victimization) is negatively associated with offending (victimization) in the current period. Removing population heterogeneity also changes the estimated relationship between victimization and offending. Whereas estimates ignoring population heterogeneity show a stable positive relationship between previous offending (victimization) and current victimization (offending), the use of individual fixed effects erases many of these positive relationships. After controlling for individual and time fixed effects, the dynamic relationship that remains between victimization (offending) and offending (victimization) is driven primarily by 1) criminal incidents occurring close together in time and 2) simultaneous incidents where individuals are both offenders and victims.

Upon exploring intertemporal relationships for specific crime types, we find that the nature of victim-offender overlap varies substantially. Violent crime largely mirrors relationships found in the main analysis, with overlap observed going back two months from the current period.

For property crimes, there is no evidence that victims become offenders, however the converse is not true. Not surprisingly, there is no evidence of victim-offender overlap when it comes to crimes of a sexual nature. For both intimate partner violence and crimes involving weapons, any evidence of victim-offender overlap disappears after removing incidents where individuals are found to be both a victim and an offender—population heterogeneity does not play a significant role in overlap for these crime types.

Once population heterogeneity is removed, what is the possible explanation for the remaining short-run positive dynamic relationship between offending and victimization? Retaliatory events deserve consideration here. In Tables A1 and A2, we investigated the possibility of retaliatory events that were directed—where the victim retaliates against the specific offender who victimized them—and found the short-run positive relationship persisted even when these events were removed from the analysis. However, undirected retaliation is still a possibility, whereby a victim lashes out more generally at others who were not involved in the original incident. Theory suggests these retaliatory events are motivated by anger, which likely subsides over time and therefore leads to a concentration of these events in the near term. Directed retaliation against a group of individuals rather than a specific individual may also be possible, such as with gangs where a crime committed against one member of a gang leads to retaliatory action against any member of the rival gang.

Findings could also be partly explained by crime detection in general. Since we can observe only crimes that come to the attention of police, it may be that those who have had a recent offending or victimization event are more likely to be monitored by police, and therefore, their subsequent offending or victimization is more likely to be detected, at least in the near term. The timing of detection could also play an important role. For example, suppose an offender goes on a crime spree over a two-month period but is not immediately caught by police. If they are eventually caught, they may be charged with the earlier crimes, evidence permitting. A similar explanation could be given for the relationship between previous offending and current victimization. While recording someone as a victim, police could make a connection to previous (unsolved) crimes.

Although the work in this paper can be viewed as an interesting academic exercise, important policy implications follow. Empirical findings confirm the notion that population heterogeneity accounts for much of victim-offender overlap and reinforces the commonly held view that early life course interventions would be most effective in reducing the incidence and costs of crime. However, because we still observe positive victim-offender overlap after removing population heterogeneity, this suggests that public interventions may also be effective later in life, specifically at an individual's first entering the justice system as either a victim or an offender. Targeted interventions are not likely to work for crimes where overlap is driven mostly by simultaneous victim-offender events (e.g., offenses involving weapons, IPV), but may be effective for crimes where overlap occurs as a result of criminal incidents grouped together in time (e.g., violent crime). Our findings suggest that interventions to educate first time offenders and victims on how to prevent future offending and victimization (and its costs) may be worth considering.

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APPENDIX A. Full model results, any offending or victimization

Table A1. Full estimation results, any offending as the dependent variable

	(1) No Individual Fixed Effects	(2) Individual Fixed Effects	(3) Less Simultaneous V/O Offenders	(4) Less Potential Retaliatory Offenders
offending				
$t-1$	.0611***	.0215***	.0217***	.0219***
	(.0020)	(.0023)	(.0023)	(.0023)
$t-2$	.0368***	-.0009	-.0007	-.0007
	(.0017)	(.0018)	(.0018)	(.0019)
$t-3$	.0272***	-.0095***	-.0007	-.0096***
	(.0015)	(.0016)	(.0016)	(.0015)
$t-4$	.0210***	.0150***	-.0150***	-.0150***
	(.0014)	(.0015)	(.0015)	(.0015)
$t-5$	.0227***	-.0127***	-.0130***	-.0129***
	(.0014)	(.0015)	(.0015)	(.0015)
$t-6$	.0198***	-.0151***	-.0150***	-.0150***
	(.0014)	(.0014)	(.0014)	(.0014)
$t-7$	.0189***	-.0155***	-.0154***	-.0152***
	(.0013)	(.0013)	(.0013)	(.0014)
$t-8$	.0175***	.0164***	-.0165***	-.0166***
	(.0013)	(.0013)	(.0013)	(.0013)
$t-9$	.0183***	-.0152***	-.0151***	-.0150***
	(.0013)	(.0014)	(.0014)	(.0014)
$t-10$	.0184***	-.0148***	-.0148***	-.0149***
	(.0013)	(.0013)	(.0013)	(.0013)
$t-11$	.0188***	-.0147***	-.0148***	-.0148***
	(.0013)	(.0013)	(.0013)	(.0013)
$t-12$	.0181***	-.0162***	-.0163***	-.0162***
	(.0013)	(.0013)	(.0013)	(.0013)
victim				
t	.0090***	.0071***	.0037***	.0034***
	(.0007)	(.0007)	(.0006)	(.0006)
$t-1$	.0039***	.0024***	.0024***	.0022***
	(.0005)	(.0005)	(.0005)	(.0005)
$t-2$	.0041***	.0027***	.0027***	.0025***

	(.0005)	(.0005)	(.0005)	(.0005)
t – 3	.0022***	.0009**	.0008*	.0007*
	(.0005)	(.0005)	(.0004)	(.0004)
t – 4	.0030***	.0017***	.0018***	.0018***
	(.0005)	(.0005)	(.0005)	(.0005)
t – 5	.0019***	-.0007	.0006	.0005
	(.0004)	(.0005)	(.0004)	(.0004)
t – 6	.0031**	.0020***	.0019***	.0018***
	(.0005)	(.0005)	(.0005)	(.0005)
t – 7	.0023***	.0012***	.0011**	.0011**
	(.0005)	(.0005)	(.0004)	(.0004)
t – 8	.0022***	.0012***	.0011**	.0009**
	(.0005)	(.0005)	(.0005)	(.0004)
t – 9	.0025***	.0016***	.0016***	.0014***
	(.0005)	(.0005)	(.0005)	(.0005)
t – 10	.0016***	.0007	.0006	.0006
	(.0004)	(.0004)	(.0004)	(.0004)
t – 11	.0023***	.0013***	.0013**	.0014***
	(.0005)	(.0005)	(.0005)	(.0005)
t – 12	.0013***	.0003	.0003	.0002
	(.0004)	(.0004)	(.0004)	(.0004)
Individual Fixed Effects	NO	YES	YES	YES
Monthly Fixed Effects	YES	YES	YES	YES
Age and income	YES	YES	YES	YES
observations	20,467,500	20,467,500	20,461,500	20,456,900

Source: New Zealand Police Recorded Crime – Victims Statistics (RCVS) and Recorded Crime – Offenders Statistics (RCOS). Robust standard errors are shown in parentheses. Marginal effects are calculated at variable means. *, **, and *** denote statistical significance at the 10, 5, and 1 percent-levels, respectively.

Table A2. Full estimation results, any victimization as the dependent variable

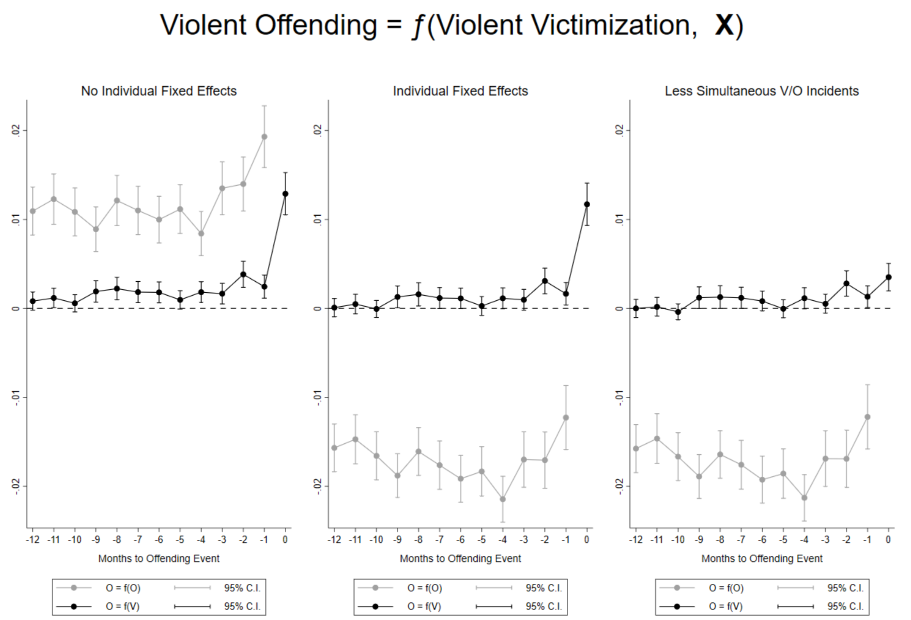
	(1) No Individual Fixed Effects	(2) Individual Fixed Effects	(3) Less Simultaneous V/O Offenders	(4) Less Potential Retaliatory Offenders
<u>offending</u>				
t	.0136*** (.0010)	.0109*** (.0010)	.0056*** (.0009)	.0052*** (.0009)
t – 1	.0054*** (.0008)	.0032*** (.0008)	.0030*** (.0008)	.0030*** (.0008)
t – 2	.0046*** (.0007)	.0025*** (.0007)	.0022*** (.0007)	.0021*** (.0007)
t – 3	.0013** (.0006)	-.0007 (.0006)	-.0007 (.0006)	-.0008 (.0006)
t – 4	.0033*** (.0007)	.0012* (.0007)	.0012* (.0007)	.0009 (.0007)
t – 5	.0030*** (.0007)	.0008 (.0007)	.0004 (.0007)	.0005 (.0007)
t – 6	.0012** (.0006)	-.0010 (.0006)	-.0009 (.0006)	-.0010 (.0006)
t – 7	.0016** (.0006)	-.0006 (.0006)	-.0007 (.0006)	-.0004 (.0006)
t – 8	.0023*** (.0006)	< .0001 (.0006)	-.0002 (.0006)	.0001 (.0006)
t – 9	.0020*** (.0006)	-.0004 (.0006)	-.0005 (.0006)	-.0005 (.0006)
t – 10	.0027*** (.0006)	.0003 (.0007)	.0002 (.0006)	.0003 (.0006)
t – 11	.026*** (.0006)	< .0001 (.0006)	< .0001 (.0006)	.0002 (.0006)
t – 12	.0016*** (.0006)	-.0010* (.0006)	-.0012** (.0006)	-.0013** (.0006)
<u>victim</u>				
t – 1	.0149*** (.0008)	-.0137*** (.0009)	-.0139*** (.0009)	-.0140*** (.0009)
t – 2	.0100***	-.0179***	-.0180***	-.0180***

	(.0007)	(.0007)	(.0007)	(.0007)
$t-3$	.0099**	-.0175***	-.0177***	-.0179***
	(.0007)	(.0007)	(.0007)	(.0007)
$t-4$	.0086***	-.0184***	-.0184***	-.0184***
	(.0007)	(.0007)	(.0007)	(.0007)
$t-5$	.0086***	-.0177***	-.0178***	-.0179***
	(.0007)	(.0007)	(.0007)	(.0007)
$t-6$	.0069**	-.0187***	-.0187***	-.0187***
	(.0006)	(.0007)	(.0007)	(.0007)
$t-7$	.0074***	-.0178***	-.0179***	-.0180***
	(.0006)	(.0006)	(.0006)	(.0006)
$t-8$	.0067***	-.0179***	-.0180***	-.0180***
	(.0006)	(.0006)	(.0006)	(.0006)
$t-9$	.0073***	-.0169***	-.0169***	-.0171***
	(.0006)	(.0006)	(.0006)	(.0006)
$t-10$	.0068***	-.0169***	-.0170***	-.0170***
	(.0006)	(.0006)	(.0006)	(.0006)
$t-11$	.0071***	-.0161***	-.0161***	-.0163***
	(.0006)	(.0006)	(.0006)	(.0006)
$t-12$	.0074***	-.0156***	-.0157***	-.0156***
	(.0006)	(.0007)	(.0007)	(.0007)
Individual Fixed Effects	NO	YES	YES	YES
Monthly Fixed Effects	YES	YES	YES	YES
Age and income	YES	YES	YES	YES
Observations	20,467,500	20,467,500	20,461,500	20,456,900

Source: Authors' calculations using New Zealand Police Recorded Crime – Victims Statistics (RCVS) and Recorded Crime – Offenders Statistics (RCOS). Robust standard errors are shown in parentheses. Marginal effects are calculated at variable means. *, **, and *** denote statistical significance at the 10, 5, and 1 percent-level.

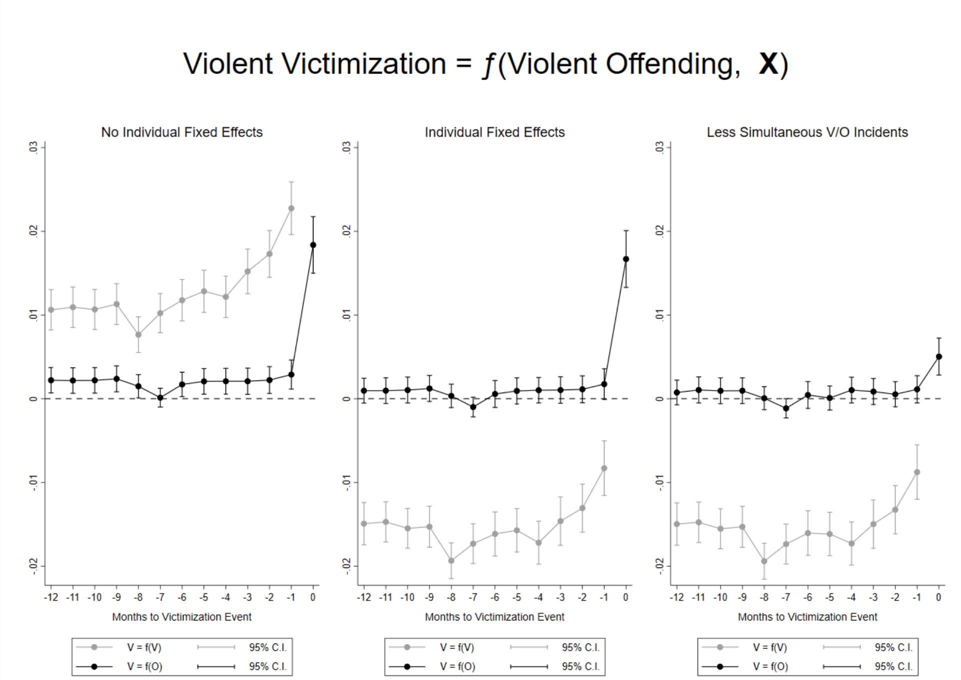
APPENDIX B. Victim-offender overlap by offense type

Figure B1. Victim-offender overlap: violent offending



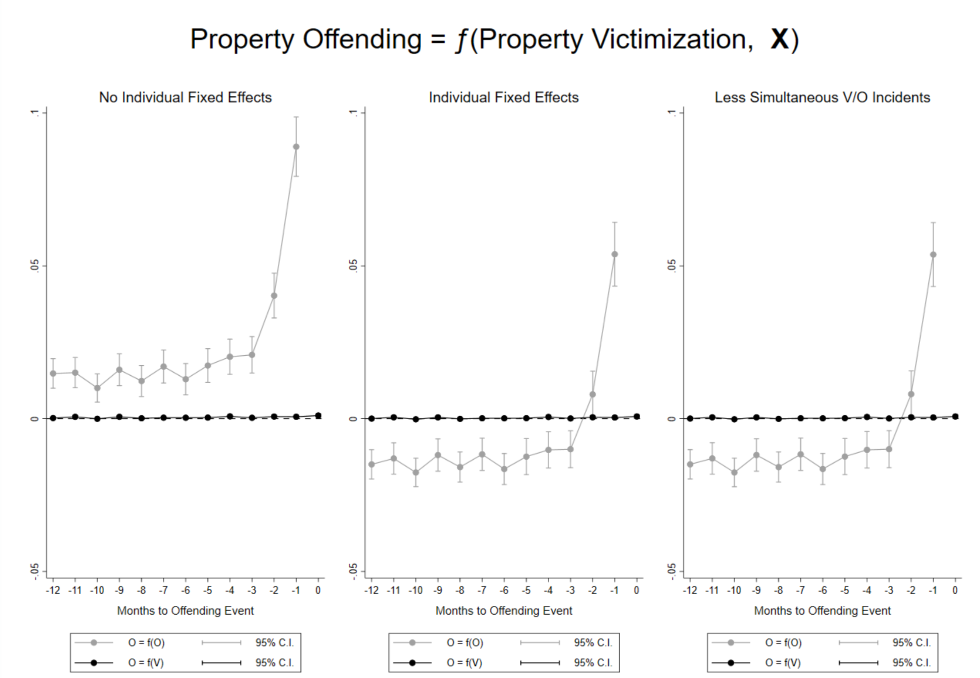
Source: Authors' illustration and calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). All models include month fixed effects. Robust standard errors used to construct 95% confidence intervals.

Figure B2. Victim-offender overlap: violent victimization



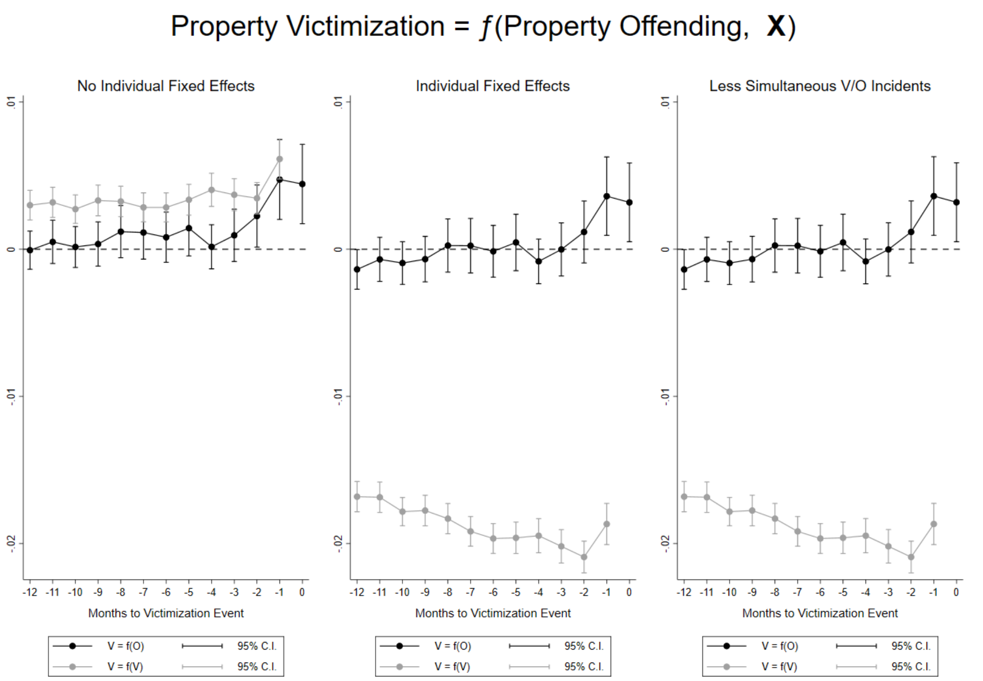
Source: Authors' illustration and calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). All models include month fixed effects. Robust standard errors used to construct 95% confidence intervals.

Figure B3. Victim-offender overlap: property crime offending



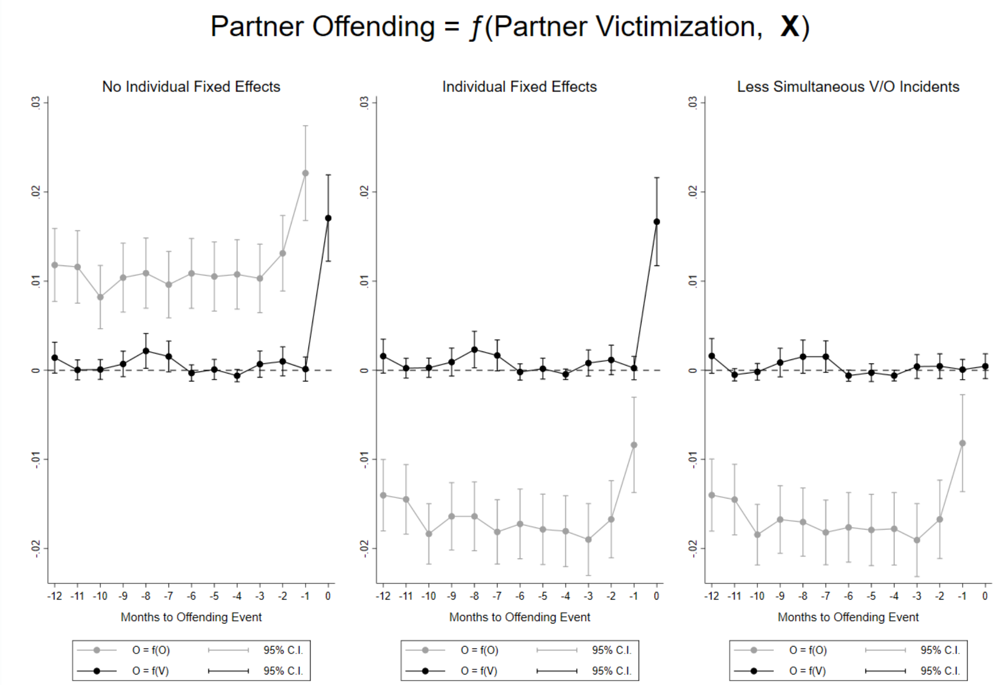
Source: Authors' illustration and calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). All models include month fixed effects. Robust standard errors used to construct 95% confidence intervals.

Figure B4. Victim-offender overlap: property crime victimization



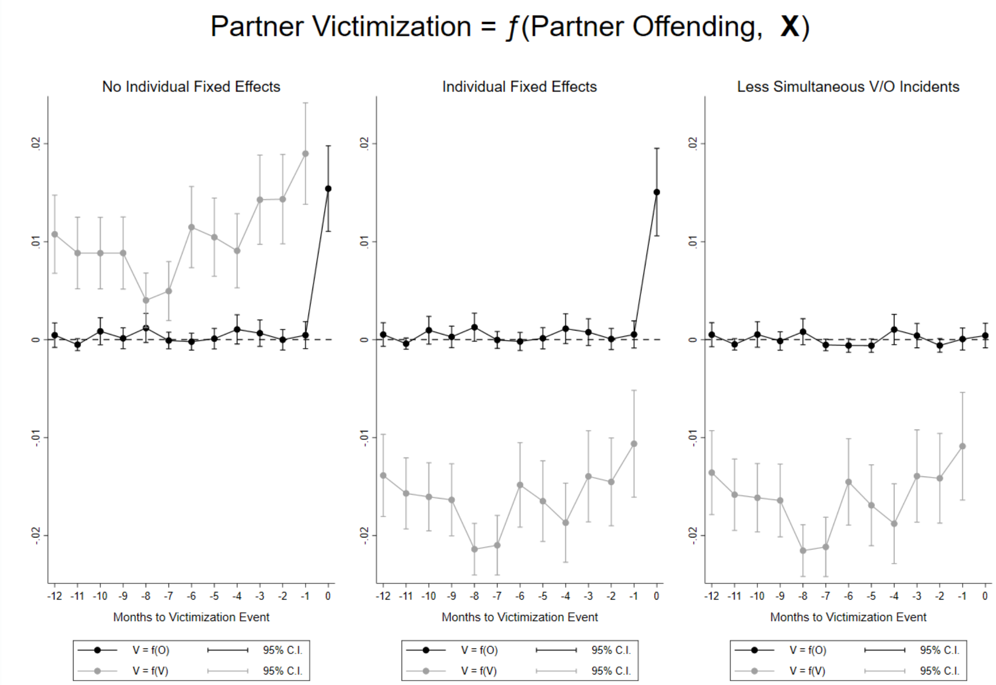
Source: Authors' illustration and calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). All models include month fixed effects. Robust standard errors used to construct 95% confidence intervals.

Figure B5. Victim-offender overlap: intimate partner violence offending



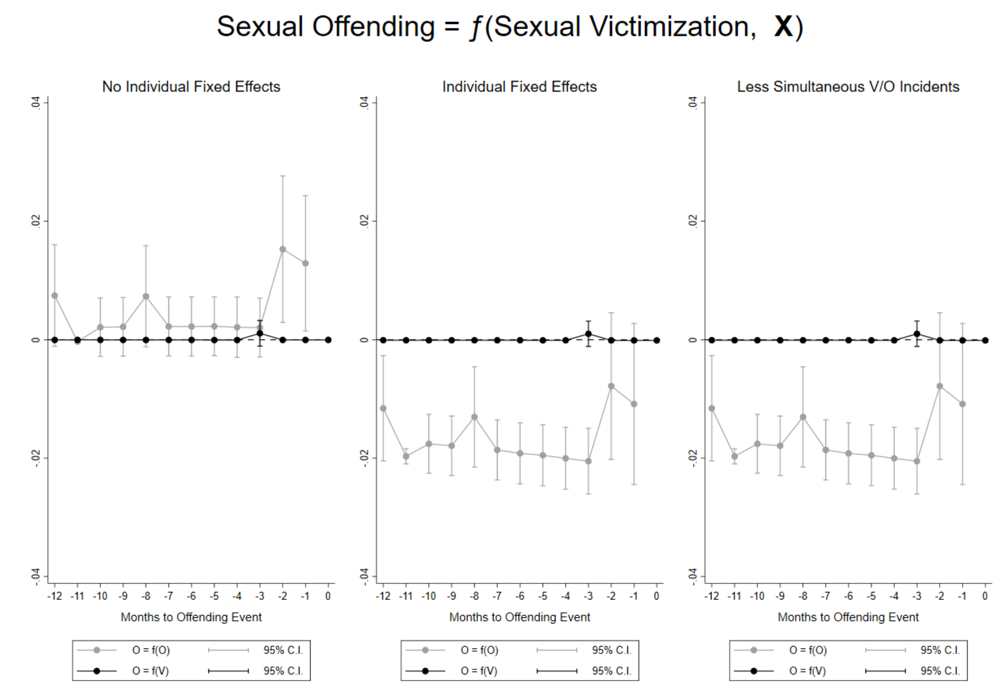
Source: Authors' illustration and calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). All models include month fixed effects. Robust standard errors used to construct 95% confidence intervals.

Figure B6. Victim-offender overlap: intimate partner violence victimization



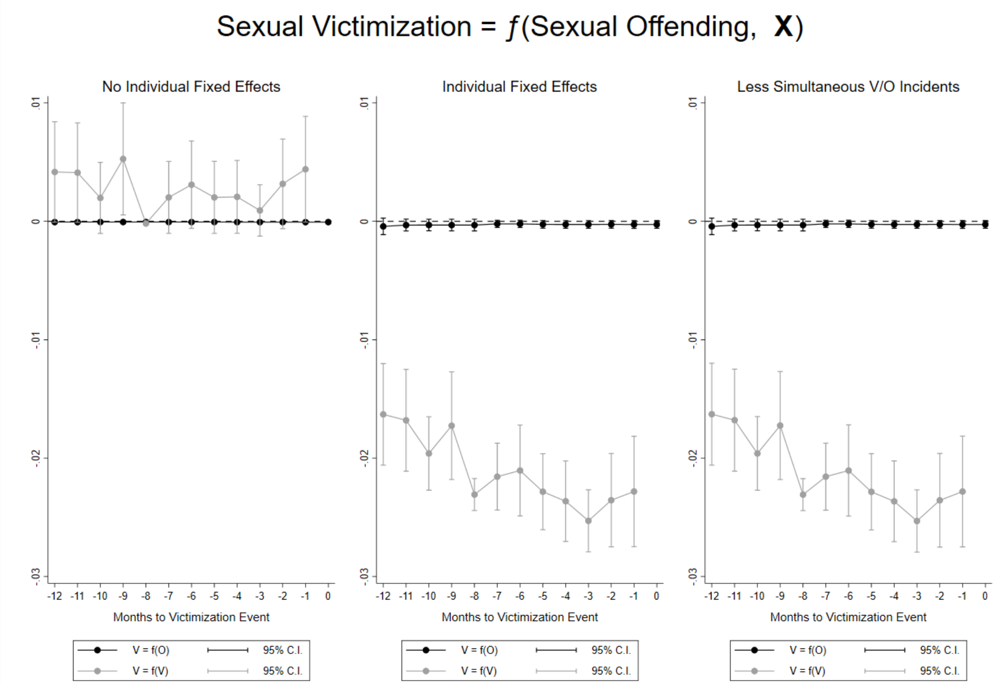
Source: Authors' illustration and calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). All models include month fixed effects. Robust standard errors used to construct 95% confidence intervals.

Figure B7. Victim-offender overlap: sex crime offending



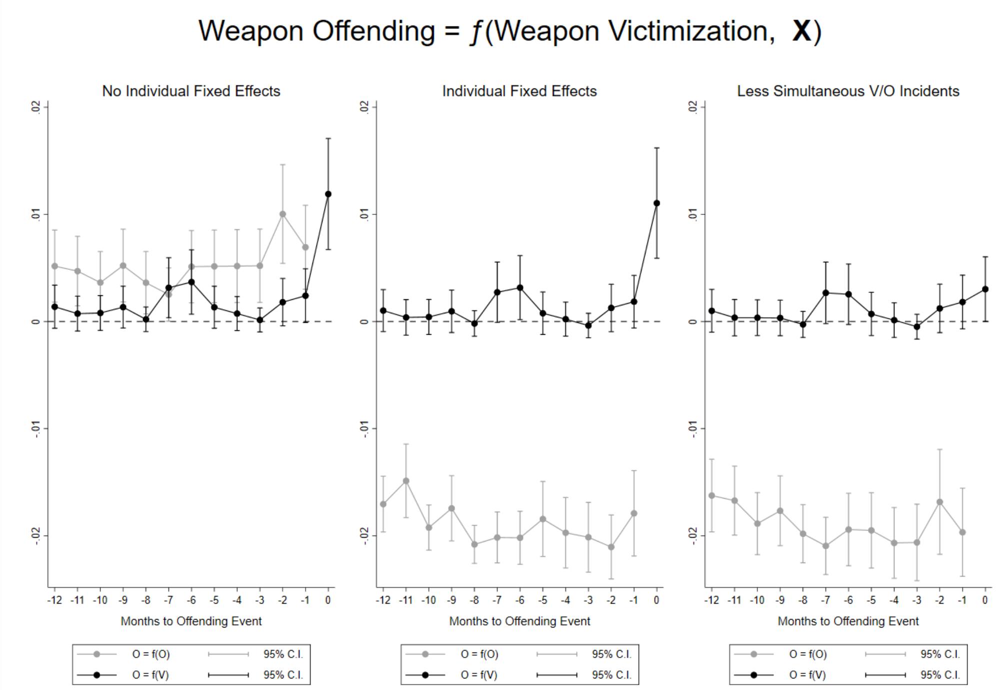
Source: Authors' illustration and calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). All models include month fixed effects. Robust standard errors used to construct 95% confidence intervals.

Figure B8. Victim-offender overlap: sex crime victimization



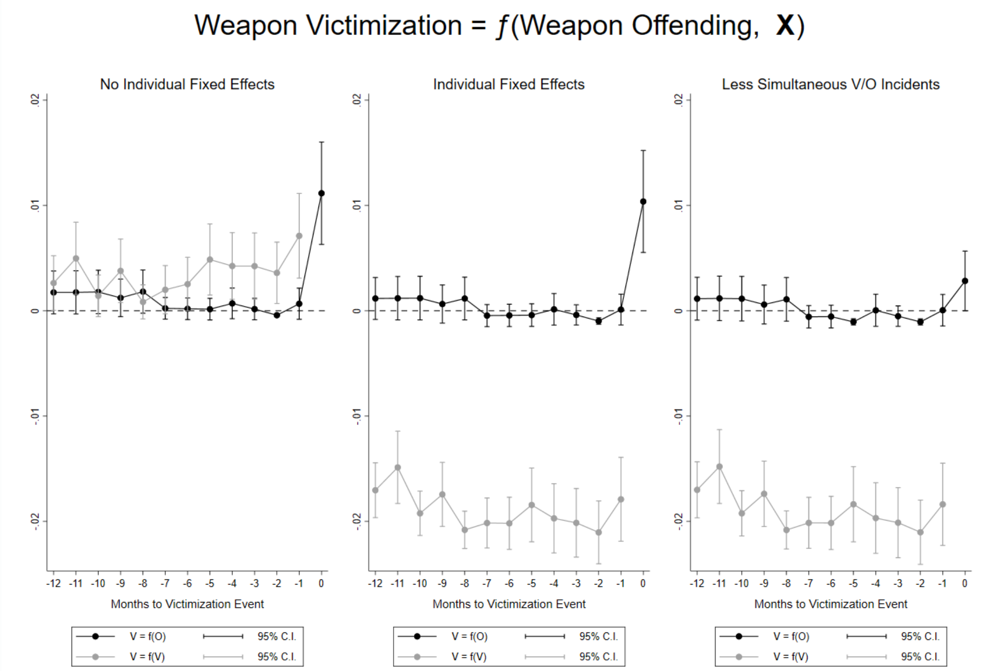
Source: Authors' illustration and calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). All models include month fixed effects. Robust standard errors used to construct 95% confidence intervals.

Figure B9. Victim-offender overlap: weaponized offending



Source: Authors' illustration and calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). All models include month fixed effects. Robust standard errors used to construct 95% confidence intervals.

Figure B10. Victim-offender overlap: weaponized victimization



Source: Authors' illustration and calculations using New Zealand Police Recorded Crime Victims Statistics (RCVS) and Recorded Crime Offenders Statistics (RCOS). All models include month fixed effects. Robust standard errors used to construct 95% confidence intervals.