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THEORY AND METHOD: THE SOCIAL EPIDEMIOLOGY OF CRIME VICTIMS

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Empirical victimology is concerned both with the study of people's experiences of crime victimisation and of the people who experience crime victimisation (i.e. victims). As such, it is fraught with conceptual problems, though many of these often tend to be over-looked or remain unexamined in empirical research. Most forms of social activity involve people interacting with their environment and crime victimisation is no exception. Each crime victimisation event is embedded within, and sets in train, complex webs of social action and meaning, stretching backwards and forwards in time for the people involved, and intersecting with the lives of others. Crime events also occur in specific instances of space-time – including 'cyberspace'. Yet we cannot readily observe the complexities and ramifications of these events and their settings – not just in the sense of being unable to observe people's motives towards them or their interpretations of them, but even being able to capture the full complexity of the systems of social interaction that converge upon and are brought about by the act itself. So, the data that we have to work with are typically only partial selections from these complexities, focussing on limited slices of time-space in which such events occur and, at any one time, usually upon only one of the many parties to the event – in this case, the crime's immediate victim. Inevitably, then, there are considerable difficulties of interpretation and inference, including problems of partiality, bias and distortion, raised by the essential *selectivity* of the available data.

This chapter sets out to review what we might know about the distribution of the general populations' experiences of crime victimisation from the data we have available. The term 'general population' is used here not necessarily in any normative or 'statistically average' sense but as a catch-all term to mean any population or group of people from which it is possible in principle to select a representative sample for study. Again, though, empirical research encounters the problem of selectivity. Studies of victims and victimisation (both of a quantitative and qualitative nature) invariably have been *opportunistic*, in the sense that they have sought to understand people's crime victimisation experiences from data usually assembled for other primary purposes: for example, from the respondents of social surveys designed to count crime, or amongst the users of social and clinical programmes and interventions. In this sense, much that we know empirically about victims is the result of secondary analysis of these data sources. It is rare indeed to encounter studies that set out specifically to investigate the phenomenology of crime victimisation, or test explanatory

hypotheses derived *a priori* from theory. Yet the selectivity and partiality engendered by the usual circumstances in which data is acquired (see Hope, 2005) also continues to limit the capacity of empirical research to support explanations of crime victimisation. Consequently, this chapter is as much concerned to describe some of the problems for explanation that arise from the various processes of selection which underpin the generation of data, as it is to advance explanation of the phenomenon of crime victimisation risk itself. Its emphasis is upon exploring what is, or should be, entailed in specifying hypothetical models that might be useful for exploring and describing the *data generating processes* (**dgp**) that presumably might underlie the observable data on crime victims and their experiences that are available.

SOME PROBLEMS OF STUDYING AETIOLOGY

Since there is no objective, impartial nor universally-applicable way of defining who is or who is not a 'victim' (as discussed in many contributions to this volume), and thus no agreed system for counting or measuring victims, it might be thought futile or disingenuous to proceed any further (as many do), especially to explore the distribution of victims within particular populations. Be that as it may, even where we might be reasonably confident on measurement issues – for instance, that persons who are prompted in the context of questionnaire surveys to report their experiences, broadly understand the questions put, and give truthful answers, and that our interpretations of these answers conform to commonly agreed notions as to what a victimisation event might look like - even so, substantial issues of selection bias remain to confront attempts at explanation. These reflect the fundamental conceptual and operational difficulty of framing and estimating the *counter-factual condition* for crime victimisation events, compounded by the consensus that such events are experienced as part of the victim's everyday or normal life (regardless of whether or not this might seem 'abnormal' to anyone else). The source of this difficulty stems from the problem posed for causal attribution by the nature of observation itself: namely, in this case, that an individual cannot be observed (or rather self-reported to have been) in both a victimised and a non-victimised condition simultaneously. We only have available the 'fact' (i.e. victimisation) but we cannot observe its precise 'counter-factual'. That is, since we cannot observe what would have happened if crime victimisation had not happened (i.e. the counter-factual condition); we have no empirically certain means of knowing what factors 'caused' the specific occurrence of the crime victimisation event. Because the counter-factual remains unobservable for 'singular events' of victimisation, we cannot evaluate the actual probability

of the event in question occurring against the unactualised probability of it not occurring¹.

The empirical social sciences, including the empirical study of victims, have sought generally to overcome this kind of problem by inference, particularly through observing regularities from variation in supposedly temporal antecedents of events or statuses, in order to deduce causal antecedents. In this, they are following Hume in the empirical tradition of causality (Russell, 1946; 2005: 603-612). With regard to victims, the method seeks to infer the causal antecedents of the likelihood of a person becoming a crime victim by tracing the biographical pathways leading towards crime events, or through comparison with ‘non-victims’, and preferably both. Nevertheless, for all its operationalisability, for a number of reasons the approach does not fully overcome the counterfactual problem with regard to the causes of crime victimisation:

1. Since it is generally felt that there is no *a priori* condition that leads axiomatically to crime victimisation², the acquisition of the status of ‘crime victim’ comes from people’s actual experiences of crime victimisation, over variably defined segments of their life-courses. Crime-victimhood is thus a status that is attached to people retrospectively, occasioned by the occurrence of a crime victimisation event; people *become* victims, attaining a status and label that they did not have before. Yet if we simply looked back over the circumstances of people’s lives that lead up to the event, we would be likely to risk teleological fallacy. This is because we would risk selecting only those past events that actually occurred (assuming they were recalled) and would have no means of evaluating whether they were the real reasons that lead up to the event, since we could not compare the observed routines and pathways with all the possible counterfactual alternatives that the victim could have taken but did not, for whatever reasons (including those that we would want to uncover).
2. A conventional scientific way of overcoming this problem is through some kind of prospective longitudinal ‘experiment’. In this case, such an experiment would set up conditions whereby people were randomly exposed (or not) to conditions that are presumed likely to give rise to a victimisation event. In the pure experimental design, those persons assigned to non-exposed conditions represent the counterfactual condition because the process of random assignment to the ‘exposed’ and ‘non-exposed’ conditions means that members of either group are ‘exchangeable’ and that the subsequently

observed differences between the two represents the difference between the factual and the counterfactual, and hence indicates causation (Hernán and Robins, 2006). Of course, such a research strategy risks rejection on ethical grounds in the case of crime victimisation, entailing as it would do the contrived exposure of human subjects to potential harm. Paradoxically, any experiment that contrived to minimise the risk of harm – say by somehow simulating victimisation in laboratory settings or other similarly constrained environments – would risk losing generalisability since such a set-up would be an artificial *abstraction* from the conditions in which the phenomenon of crime victimisation is embedded, that is, within everyday life.

3. Paradoxically, experiments that seek to expose people to benefits – in this case exposing people to conditions that would protect them against crime victimisation, likewise cannot be used to infer antecedent causes, if these are unknown, since they risk confounding the causes of the ‘dependent variable’ (i.e. crime victimisation) with the ‘causes’ of the intervention. For instance, an hypothetical experiment that offered intruder alarms for purchase to randomly selected households would not be able to assess the protective effect of alarms since it is known both that more affluent households are likely to purchase alarms and that they are less likely to be victimised from household property crime (Hope, 2002). Thus, in this case, it would be the condition of ‘affluence’ rather than the condition of ‘alarm’ that was the causal antecedent of victimisation. And ‘affluence’ would then *confound* observation of the effect of ‘alarm’, even though the experimental set-up was predicated on testing the latter effect. Again, the difficulty of abstracting victimisation from the realm of everyday life in which it is embedded makes it hard to identify its causal antecedents.
4. Given these problems, empirical research into the antecedents of crime victimisation has had to be content with inferential (statistical) methods of association – typically between victims and non-victims (as defined by self-reported admissions) – in order to estimate effects, usually by setting non-victimisation as the counterfactual condition. Yet as discussed below, this seemingly commonsensical model may well be a misleading depiction of the **dgp** of crime victimisation leading to mis-specification and mis-identification of explanatory models.
5. Some compensation might be had by building ‘controls’ into the design and analysis of

studies of observed data: for example, to establish temporal order (from which to infer causation) by implementing longitudinal ‘panel’ designs (i.e. repeated measurement on the same people at different points in time) and/or to control for extraneous or confounding effects by utilising appropriate, multivariate statistical models. Regrettably, longitudinal panel data is rarely available on victimisation³; ironically, an absence that severely hampers investigation of processes that have been seen of much policy-relevance, such as *repeat* (sic) victimisation – a concept that clearly implies a temporal causal ordering (but see Hope and Trickett, forthcoming; 2004).

Thus, much of the available empirical data for studying the aetiological circumstances of crime victims are to be found in cross-sectional sample surveys. Since, as discussed below, the average prevalence of crime victims in populations (at least over periods of 12-months or less), is relatively low, large-scale samples are necessary, perforce leading to reliance upon government-supported surveys, often with national coverage such as the US National Crime Victimization Survey, or the British Crime Survey for England and Wales. Notwithstanding the fact that these surveys may lack adequate sets of measures of covariates of theoretical (aetiological) interest, due to possible conflicts with their public service functions⁴, all cross-sectional survey data remain vulnerable, nevertheless, to three kinds of selection bias:

1. *Sample selection bias* arising from failure of eligibility for inclusion in the sampling process;
2. *Response bias* arising from the failure of those selected to participate in answering some or any of the questions either accurately, truthfully or at all (for summaries of commonly observed sample and response biases see Hope, 2005; Mayhew, 2000); and
3. *Conditional selection bias*. This latter form of selection bias (sometimes called conditional censoring) again reflects the concerns of the counterfactual approach. As discussed below, in the case of testing theories of crime victimisation which rest upon the notion that people might be differentially exposed to the chance of becoming a victim, it is necessary to distinguish between those who are never likely to be exposed to the risk of crime, for example, because they never go out on foot, or to places where they might encounter street-robbers, from those that are so exposed to crime-risk but nevertheless do not become victimised (see Clarke *et al.*, 1985).

Generally, counterfactual reasoning suggests that explanatory models are likely to be incomplete, mis-specified or biased to the extent that they fail substantively to take each of these sources of selection bias into account (Heckman, 2001).

In sum, the nature of the available data, combined with the *ex post facto*, event-referenced nature of the definition of 'crime victim', present formidable problems for researching the aetiology of the condition of crime victimhood. Such research has had to proceed empirically by the method of statistical association, from which inferences about underlying data generation and aetiological processes might be deduced. Counterfactual reasoning suggests that such methods of empirical analysis are likely not only to be causally-weak (because of the difficulty of identifying satisfactory conditional counterfactuals) but may also lead to erroneous or biased results, - which might only be overcome, partially at least, by careful attention to *ex ante* theoretical development, model specification (including an incorporation of selection bias) and data collection. Nevertheless, empirical analysis of this kind has continued over the past thirty-odd years, though the difficulties and inconsistencies encountered in interpreting its results have been disappointing, on the whole, arguably through inattention to the issues of causal analysis identified here.

SPECIFYING THE DATA GENERATING PROCESS

Sample surveys of general populations generate *counts* of crime-victimisation events experienced by their respondents. Typically they ask their respondents to report the numbers of incidents of a set of offences that they or their households have experienced over a particular recall period (usually no more than the previous 12 months). Naturally, tabulations of responses to such questions yield frequency distributions for the number of crime victimisations experienced by the sampled respondents. Typically also, those experiencing one or more incidents over the recall period are usually considered to be 'victims', and thus distinguished from non-victims (i.e. people not reporting any incidents), while persons reporting more than one incident have come to be termed 'multiple victims' or, following Farrell and Pease (1993), 'repeat victims'. Because the surveys are representative samples, so also have these categories come to be thought of as representative of the crime victimisation experiences of the general population⁵. Since the surveys also ask a range of other questions about respondents' background, experiences and so on, these questions have been correlated with crime victimisation frequencies and, in turn, have been considered as covariate proxies

of causal antecedents, used to support, substantiate or test various explanatory theories of crime victimisation.

When the sampled count data of self-recalled crime victimisation are arranged as a frequency distribution, they invariably display two characteristics: first, that crime victimisation seems to be a probabilistically *rare event* – that is, the majority of the sampled population do not report victimisation over the recall period; and second, the population distribution is *over-dispersed* – that is, the sample variance exceeds the sample mean, for example, that there are more higher-frequency crime victims than would be expected, or conversely, that the zero response observation is over-inflated (see below). Table 1a and b shows that these characteristics appear across a variety of types of crime and types of victim. The similarity suggests that there might be a similar *data generating process (dgp)* underlying these different frequency distributions. The difficulty, however, lies in finding out what such a **dgp** might look like.

Table 1a-b about here

The Data Generating Process

While measurement issues are concerned with how we count crime victimisation, the form of the **dgp** is a particular stochastic process that best describes how the observed frequency distribution is produced. Any particular **dgp** cannot be observed itself but its presence and form can be identified by the degree to which a sample frequency distribution conforms to, or ‘fits’, a particular theoretical statistical distribution. If there is found to be a good fit, we can then infer that the unobserved **dgp** for the observed frequency distribution has the same general properties as the theoretical distribution which, in turn, provides us with basic building blocks for developing and testing theories about the processes that produce the frequencies observed.

In general, the **dgp** for crime victimisation distributions possesses three basic characteristics (Hope et al., 2001): first, *repetition* – that is, whether an observed frequency distribution implies a time-ordered sequence of crime victimisation events suffered by the same individual victim (which is the case in self-report, recall-based crime victimisation surveys); Second, *specificity*, that is, whether such an ordered sequence occurs within a specific crime

type or across crime types or both. Most research has focussed on victimisation within specific crime types. Exceptionally, Hope *et al.* (2001) model the conditional probabilities for cross-crime-type victimisation between household property crime and personal crime victimisation, also taking into account (self-reported) prior victimisation experiences of offences in both crime types. While the study is restricted to cross-sectional BCS data and cannot determine causal order between immediate victimisation experiences, it suggests a degree of *generality* present in crime victimisation experience: first, having taken into account covariate risk factors specific to each offence type (though there is also a degree of commonality in these too), there still remained a significant, positive association between the chance of becoming a property crime victim and that of becoming a victim of personal crime; and second, that prior victimisation experiences seem to affect immediate victimisation risk, both within (see also Ellingworth *et al.*, 1997) and also across crime-types.

This leads on to the third, and most elusive element, the existence and form of the *mechanism of risk-transmission* – that is, whether there is a non-random link between incidents suffered by the same individual over time and, if so, what might be the characteristics of the process linking incidents together? The Poisson model is regarded as the benchmark model for count data that consist of a number of discrete events occurring over a fixed time interval (Cameron and Trivedi, 1998). The simple Poisson model assumes that successive events for any individual occur independently of each other over time at a constant rate. However, in the case of crime victimisation data this would mean that the probability of victimisation would be the same for all persons in the population and that the probability of being victimised would not depend upon the number of previous victimisations (Nelson, 1980: 871). Yet not only has much recent research on the aetiology of crime victimisation proceeded as though neither of these conditions were true (see below) but the observable properties of the frequency distribution, particularly its over-dispersion, violate the assumptions of the simple Poisson distribution, leading to an extremely poor fit. Thus, early research on both American and British crime victimisation surveys suggested there was a need for an alternative to the simple-Poisson process that might be producing the underlying distribution of the observed condition of crime victimisation (Nelson, 1980; Sparks *et al.*, 1977). Yet, it has not been established precisely what form the **dgp** for the distribution actually takes. Empirically, there have been various strategies taken to overcome this problem when seeking to estimate covariates to stand as proxies for causal influences or risk factors.

The discrete outcome model

By far the most common approach taken to date has been to conceptualise the distribution as a ‘discrete outcome’ model⁶ where the observed frequency distribution is truncated into two outcomes: typically, zero to indicate non-victimisation, and a positive value (usually 1) to indicate victimisation frequencies of one or more incidents over the recall period – commonly designated as ‘victim’. This approach has a number of advantages: it conforms to ‘common-sense’ and policy priorities – that is, interest in the process of acquiring the status of victim; it reflects legal distinctions that operate on an individual case-basis, which assigns status to the different parties to a criminal event (i.e. ‘victim’ and ‘offender’); and it obviates the problem of the uncertain nature of the underlying **dgp** by converting it into a simple discrete binary outcome. It also capitalises on empirical observation – that the majority of sampled populations are not victimised over the typical recall period (Tables 1a-b). Thus, by truncating the positive values of the count of victimisation incidents, the discrete outcome approach brings the average risk rate, called *prevalence* (defined as the number of victimisation incidents divided by the number in the sampled population), closer to an assumed true mean of the distribution, approximating to zero. The approach removes the explanatory problems posed by over-dispersion by ignoring the contribution to explanation of the statistically rare sub-population of higher-frequency ‘multiple victims’, thereby ‘normalising’ the distribution, ‘fixing’ the **dgp**, and thus making the distribution easier to analyse, without apparently much loss to explanation (Nelson, 1980).

The theoretical assumption of the discrete outcome approach is thus to focus on the ‘normal’ condition – that is, close to a zero likelihood of victimisation for the majority of the population. Victimisation is thus conceptualised as a *deviation* from normality – a relatively rare risk that individuals encounter in their daily lives and routine activities. The empirical task is thus to look for those risk factors present in the environments in which people normally find themselves (Miethe and Meier, 1994). The primary reason for asking the question in this way is to identify the factors that might contribute to, and thus predict on the basis of *a priori* risk, the general population’s likelihood of *exposure* to crime victimisation – an approach first developed by Hindelang, Gottfredson and Garofalo in their book *Victims of Violent Crime* (1978). To the extent to which patterns are found, this indicates the risk of *exposure* to the likelihood of crime victimisation that is part of the structure of the social environment. The null hypothesis (the counter-factual) is thus that crime victimisation – in

the rare chance that it should occur – is a random event, at least from the point of view of the victim. This approach has been termed *positivist victimology* reflecting an empirical strategy of analysing population distributions in terms of deviations from norms, with the theoretical intention of identifying and explaining what those norms might be (Mawby and Walklate, 1994).

Exposure to risk

Central to the discrete outcome empirical approach is the concept of exposure *to risk*. Bearing in mind the conventional *ex post facto* definition of victim status, it is not surprising that when first encountering these distributions, researchers gravitated towards the concept of exposure – that persons who became victims did so because they were likely to be exposed to criminogenic risks, typically through social and/or physical propinquity to the carriers of the ‘disease of crime’, that is, those who offend (Hindelang, Gottfredson and Garofalo, 1978). In part, this may have arisen because criminology came first, reflecting a primary preoccupation with explaining crime and offending (Lauritsen and Laub, 2006). Thus, it was assumed that the observed distribution of crime victimisation mirrored the data generating process of differential exposure to criminogenic sources and conditions in the population. Further support for this assumption came from Cohen and Felson’s (1979) specification of the necessary conditions for the occurrence of criminal incidents ⁷. The probability of these convergences occurring could be explained primarily by reference to common, socially-structured *routine activity patterns*, such as travelling to work, leaving dwellings unoccupied, frequenting public spaces or places of entertainment. And these patterns could be extended to residential proximity to neighbourhoods also plentiful in the supply of ‘offenders’ (Cohen et al., 1981).

Correspondingly, at the individual-level of analysis, a *lifestyle explanation* of exposure to criminal victimisation was originally proposed by Hindelang et al. (1978) as a way of accounting for apparently non-random differences in victimisation risk observed between different demographic groups (i.e. groups distinguished by age, gender, race, income, etc.) in survey data. In an effort to explain these observations, individuals' status characteristics - which are presumed to be measured by their demographic characteristics - were held to imply role expectations and structural constraints which result in differing routine activities and patterns of social relations. These activities and associations may also entail differing levels

of 'exposure' to others who might victimise them (Miethe and Meier, 1994). The analysis has been essentially inductive – with researchers seeking to provide *ex post facto* explanations for observations obtained from survey data⁸.

Within this tradition of research, four central concepts have been used to explain the risk of individual victimisation (Miethe and Meier, 1994):

1. *Proximity to crime*. This might be thought of as the degree of permanent, physical proximity to places where offences might be committed. Risks of victimisation may be heightened in many kinds of place where people who are likely to commit crimes might congregate, and where circumstances are conducive to their being encouraged to commit crimes (Cohen and Felson, 1979); in the residential context, proximity is generally taken to mean a victim's residence in an area which is also likely to have a high rate of resident offenders whom it is presumed may be likely to victimise their fellow residents.

2. *Exposure to crime*. Aside from physical propinquity, certain individuals and households may also have a heightened risk of victimisation by virtue of their lifestyles and routine activities which provide them with additional exposure to the likelihood of risk. Their routine activities and lifestyle choices may take them to risky places, or amongst dangerous people (as in the proximity hypothesis), and may take them away from the location of their residential and mobile property, which then becomes more exposed to the risk of expropriation or damage. Alternatively, their routines might allow them to avoid such risks.

3. *Attractiveness*. In addition to proximity and exposure, some targets (property or persons) may be seen by offenders as more attractive or worthwhile to attack or steal than other targets. Possibly, it may be the different values or subjective utilities attached to particular targets within a range of suitably 'exposed' targets in any residential community which shapes the decisions of offenders to select particular targets (Miethe and Meier, 1994).

4. *Capable guardianship*. Finally, targets may or may not be selected depending upon the degree of effective protection – what Cohen and Felson (1979) call capable guardianship - which is available to them. Such protection might take the form of physical security measures, and activities by residents or owners carried out either individually (i.e. locking-up, keeping a dog) or collectively (i.e. participating in watching actions with neighbours).

Individuals may also benefit from guardianship 'services' available collectively in the locales in which they find themselves, including physical-environmental opportunities facilitating surveillance (i.e. street-layout, building design, street-lighting), informal surveillance by other residents, organised citizen surveillance (block watches, citizen patrols), and public or private police patrolling.

Is crime victimisation risk a discrete or continuous process?

Much of the research effort which has sought to apportion individual likelihood of risk between these elements has been hampered by inadequacies both in the selection and measurement of indicators, the specification of appropriate analytical models, and underlying difficulties in conceptualising the way in which these effects might operate and the relationships between them (see Miethe and Meier, 1994). Research has tended to produce discrepancies and difficulties of interpretation, and it is difficult to work out whether these are substantive or merely artefactual. At least part of the difficulty may reside in conceptualising crime victimisation as a discrete, binary choice. If the underlying **dgp** for crime victimisation is actually a continuous process (or if there is actually a non-random link between events), then this cut-off point is essentially arbitrary. Other or more salient thresholds may exist than the distinction between no incidents and one incident, or that there might be an underlying continuous distribution with no empirically meaningful thresholds at all. Theoretically, the discrete outcome approach reifies the *status* of 'victim' as a stable quality at the expense of conceptualising the *process* of victimisation – which may be one reason for the apparent confusion of empirical research, or the lack of covariate consistency between samples.

Osborn *et al.* (1996) specifically test the possibility that crime victimisation is a discrete or a continuous distribution by testing a 'double-hurdle' model of household property crime victimisation: first, the transition from non-victim to victim household; and second, whether the victimised household progressed to become a multiple (two or more) victim. It was assumed that the probability of a second or subsequent victimisation would not be independent of the probability of an initial victimisation. This allows both for the possibility of a contagious link between victimisation incidents (see below) and that the process of victimisation risk may be similar for each of the hurdles, that is, a continuous process of *heightened exposure to risk*. Having taken into account the (individual and area) risk factors

associated with the first hurdle, no further or additionally significant risk factors could be identified for the second hurdle (of repeat household crime victimisation). The inference was that there were no measured *a priori* predictors that would distinguish multiple victims from victims generally, other than those that are common to all victims and distinguish them from non-victims (Osborn et al., 1996). Two interpretations were invited by this result: first, that there might be *unmeasured* risk factors that would distinguish higher from lower frequency victims (Tseloni et al., 2002)⁹ – that is, that would substantiate a discrete outcome model with a further cut-off between ‘single’ and ‘multiple’ victims; but second, that there remained the probability that a victim experiencing higher frequency victimisation was dependent to some extent upon the initial (or rather *a priori*) risk of victimisation – that is, that victimisation was a continuous process.

It is also theoretically possible that truncating the distribution at the zero (positive) value masks important qualitative distinctions amongst those classified as members of the ‘non-victims’ sub-population. For example, there may be persons who, for various reasons, are not ‘at risk’ of victimisation and those who are theoretically ‘at risk’ but who have not been victimised over the recall period. If the frequency distribution of crime victimisation is held to measure the risk of victimisation, then the zero category may also be masking both discrete differences – for example, those never likely to be at risk – and continuous differences – that is, those who depart with increasing degrees from the likelihood of being victimised; in this latter case, not only would the victimisation risk process be continuous but that there should also be *negative* exposure values on such a victimisation risk continuum. Yet these cannot be observed empirically, especially if the risk continuum is measured by actual events of victimisation. Again, this points up the limitations to explanation of an inadequate or incomplete conceptualisation of the counter-factual (or antonym) to the experience of crime victimisation. So, while the measurement of victimisation can only be positive, the underlying **dgp** may be capable, theoretically, of producing unobservable negative risk values. As we shall return to below, the possibility not only of continuing, incremental risk but also of possible negative values, poses difficulties both for the discrete outcome model, and the concept of exposure that underpins it.

CONCEPTUALISING RISK

The concept of exposure also presupposes that there is something that victims are exposed to – that risk is a property of their environment (rather than something intrinsic to them). Nevertheless, there are problems in conceptualising the role of environmental effects and how they might be distinguished from individual risk factors. For example, numerous empirical studies in both Britain and America, have found a correlation between lone (female-headed) household status and the risk of property crime victimization (Hope, 2001). A variable 'lone-parent household' is an individual-level characteristic, while a variable 'proportion of single-parent households in the resident population of a neighbourhood' is a characteristic of an environment, in this case the residential neighbourhood in which the household is located. Correlations between *rates* of lone-parent households and *rates* of victimization have been found across neighbourhoods; and lone-parent households have also been found to be at greater risk of victimization compared to other household types.

But what does this mean? Are these correlations indicators of:

1. *individual vulnerability* - for example, lone-parents may have to leave their property unguarded more often because they go out to work (Cohen and Felson, 1979), or are victimised more often by intimates, including past and estranged partners (Maxfield, 1987; Tseloni, 1994; Genn, 1988), or their children's friends (Sampson and Lauritsen, 1990) ? In other words, are these correlations evidence of an essentially *micro-level effect*? or
2. *neighbourhood-level vulnerability* - for example, high proportions of lone-parent households in a neighbourhood might mean that there are fewer adults to carry out day-to-day surveillance of property (guardianship), or to supervise local youth activities (social control) (Sampson and Groves, 1989). In other words, providing evidence of a *macro-level effect*; or even
3. *context-specific vulnerability* - for example, lone-parents who live in areas of predominantly lone parents are not only more vulnerable than other types of household (in 1,

above) but are also more vulnerable than single parents who live in other kinds of area (in 2 above). Here, individual vulnerability is enhanced by the particular neighbourhood-level context of risk (Miether and McDowall, 1993). In other words, providing evidence of a *macro-micro interaction effect*.

Why these different interpretations matter is that, if one or the other is true, and the others false, it would then help to support different theories of crime victimisation, particularly those that give different explanatory weight to one or the other levels of analysis. For example, if the true interpretation operated at the micro-level rather than the macro-level, it would be evidence of so-called *life-style/routine activity theories* of certain households' and individuals' (lone parents') differential likelihood of exposure to criminal victimisation risk (Smith and Jarjoura, 1989; Maxfield, 1987). In contrast, the macro-level correlate could indicate diminished levels in the community of informal social control over children and teenagers, and would be supportive of theories of *social disorganisation* (Sampson and Groves, 1989; Sampson, 1985).

Cross-level misspecification

Yet the problem remains, in the example given above, that lone-parent household status appears related empirically to the risk of victimisation at *both* levels of analysis (Hope, 2001). So, how could we set about sorting out which one is right? In the context of differentiating whether an effect operates at the macro-level or the micro-level, we encounter the general theoretical problem of *cross-level misspecification* – that is, of misinterpreting effects measured at one level as representing also explanations operating at another level, and *vice versa* (Sampson and Lauritsen, 1994), and thus committing errors of inference. Each source of variation - whether attributed to micro-level or macro-level sources - constitutes a threat to the validity of explanation couched at the other level.

There are different kinds of potential cross-level misspecification error, depending on whether one is seeking to explain micro-level phenomena from macro-level observations, or *vice versa*. Mouzelis (1991) sees both of these as problems of reductionism (of reducing explanation to one or the other of the two levels), and are the pitfalls of methodological individualism. One type of reductionism – *downward reductionism* – lies in erroneously explaining micro-level phenomena from macro-level observation. Here one might encounter

two kinds of misspecification error:

1. *The ecological fallacy* - that is, inferring that unobserved individual actions can be deduced from aggregate-level observations. Following Robinson's classic exposition (Robinson, 1950), it is possible to demonstrate logically that, say, an observed correlation between rates of lone-parent households and criminal victimisation at the macro-level does not *necessarily* mean that, within any given community, it is the lone-parent households who are being victimised more than other types of household.

2. *The contextual fallacy* - that is, what might be asserted to be contextual effects - requiring macro-level explanations - may turn out to be merely unmeasured, residual variance at the micro-level which might have disappeared had the micro-level variables been better or more comprehensively measured. For example, that the capacities and abilities of families to supervise their teenaged children have not been properly measured. If they had, then the seemingly contextual effect produced by the lone parent proportion indicator could be explained by the distribution of families with variable supervision capabilities.

Similarly, when seeking to move from micro-level evidence to macro-level explanation there is the risk of *upward reductionism* (Mouzelis, 1991), again in various forms:

1. *The aggregation fallacy* – this is the opposite side of the coin from the contextual fallacy – that what might purport to be genuine contextual (macro-level) effects are merely biased aggregations of individual-level measurement. For example, in a frequently-cited study, Sampson and Groves (1989) aggregate data collected from individual respondents in the British Crime Survey and, taking advantage of its sampling design, aggregate these response together to form macro-level data on ‘pseudo-communities’. Thus, they derive contextual variables – such as the presence of teenagers on the streets – from respondents individual perceptions of neighbourhood teenagers as a problem. The risk in this approach is that the macro-level variables so constructed may compound the sampling error of the Survey, leading to biased estimates of the macro-level parameters used in analysis at the macro-level – in preference, it may be better to use genuinely contextual variables about the area in which the respondents lived, derived from an independent source, such as the national Census (Osborn *et al.* (1992). Unfortunately, in this case, the Census does not contain information on the kinds of concepts of substantive significance for Sampson and Groves’ (1989) analysis.

2. *The individualistic fallacy* – it follows from the aggregation fallacy that analysts may refuse to countenance that the effect of a variable observed at the individual level, or based on an individual's response, is anything other than an attribute of the individual. In this sense, it assumes that what is important is the way that individuals interpret or respond to their environment, which can be gauged by asking or observing them, without needing to look at what it is in the environment that may be prompting, shaping or structuring their responses, nor at the role that the environment might play in mediating these influences to shape individual propensities to victimisation. In simple form, this may amount to saying that a neighbourhood crime rate is merely due to features of its population's composition, such as the number of families that fail to supervise their teenage children.

3. *The selection-compositional fallacy* - the chief threat of individual-level variance for macro-level explanation is that observed community-level variation may simply be the product of *selective population composition*. In other words, it may be that the *differential selection* of particular vulnerable individuals to certain communities may be the source of any observed community effects. Thus, systematic (macro-level) social selection processes could be the main way in which individuals with similar characteristics (such as propensity to crime victimisation) are brought together in specific spatial areas. If so, macro-level explanations would become substantively trivial or spurious, for what we might observe in the aggregate in neighbourhoods may be merely individual characteristics writ large - simply the *compositional effect* of bringing individuals together (Sampson and Lauritsen, 1994). Thus, neighbourhoods with high crime rates also have high rates of lone parent households, and many other problems besides, merely because they are unpopular or undesirable so that only 'problem populations' find themselves living there, and thus express their problems, including youth crime and consequently victimisation.

While we might suspect cross-level effects in cross-sectional data, we may be able to do little to sort out the appropriate levels of analysis. This problem is compounded by the inherent, socially structured heterogeneity of the social world that renders it impossible in natural settings to 'abstract' individuals from their environments¹⁰. For example, Trickett, Osborn and Ellingworth (1995) estimate a multivariate statistical model of household property crime victimisation that includes both variables representing household characteristics (micro-level) and characteristics of households' area of residence, taken from the Census (macro-level). Again, pursuing our illustration of the lone parent household, when the significant influence

of area-level variables on households' likelihood of property crime victimisation is taken into account - including the separate influences of the proportions of adult residents who may be living as lone-parents, single-person households, and who are adult women; alongside other socio-economic and demographic indicators – we find that lone parent households do *not* have any greater risk of victimisation than any other type of household.

While this example could be taken as evidence that households' risk of property crime victimisation is a consequence of the social environment or context of different kinds of neighbourhood – and hence would seem to explain the apparently greater vulnerability of lone-parent households - it does not rule out a relationship between lone-parent household composition and crime rates, via social selection processes at the macro-level. Thus, using the same data, although this time aggregated to represent 'pseudo-neighbourhoods', Osborn, Trickett and Elder (1992) find not only that the proportion of lone-parent households is significantly related to area-level property crime rates but also that lone-parent households are likely to be found disproportionately in areas with other social characteristics related to crime rates, including a larger teenage/young adult population, single adult households, and households in non-self-contained accommodation; taken together the effect of these neighbourhood characteristics is substantial. Yet, in neither analysis does the data itself explain:

1. the macro-level social-selection processes that allocate lone-parent households to particular kinds of neighbourhood;
2. and why it is that such neighbourhoods with particular social compositions have an influence on the crime rates experienced there - i.e. a contextual (structural) influence that is not reducible simply to the fact that they may contain household types or individuals prone to be involved in crime in one way or another.

Both these processes are relevant to shaping the crime victimisation rate at an observed macro-level but nevertheless involve different *movements in explanation* going from the macro-level to micro-level and back again. Thus, point (1) above, needs to explain a macro-level observation by a process that links micro-level actors (lone-parent households) to macro-level locations (neighbourhoods); while point (2) is a process that seeks to interpret a macro-level observation by collective, micro-level processes – i.e. the effect of the compositional structure of the neighbourhood upon individual actions or events (offending or

victimisation). Not only do both require a theory but also the resulting explanations depend upon moving between the levels of analysis. Thus, a more complete explanation requires not only explanation at each level of analysis but also *cross-level explanation* that links each level with the other in a causal sequence (Coleman, 1990). Otherwise, it becomes very difficult to disentangle individual victimisation risks *produced* by environments from the *contribution* to risk that individuals make to their environments.

UNDERSTANDING THE DATA GENERATING PROCESS

An obstacle that has stood in the way of developing aetiological understanding of crime victims on the basis of the discrete outcome approach has been the perhaps unwarranted readiness of empirical researchers to make assumptions as to the nature of the **dgp** that might underpin observed frequency distributions, and thence to interpret covariate associations as indicators of aetiology in the light of their assumptions. The discrete outcome approach naturally leads to normalising the distribution around non-victimisation and focussing explanation on deviations from that condition. While, on the one hand, research suggests that there is nothing to distinguish a subsequent hurdle (of multiple victimisation) from the primary hurdle (Osborn et al, 1996), there nevertheless remained an unmeasured dependency between the probability of becoming a multiple victim and that of becoming a victim at all. Additionally, this study estimated that, irrespective of initial differences in risk, once victimised, the probabilities of subsequent victimisation converged. So while there may be only one hurdle (Between non-victim and victim), victimisation itself may be more of a continuous process – as indicated by the entire frequency distribution - than the discrete outcome approach allows.

As illustrated in Tables 1a and 1b, the discrete outcome approach recognises one part of the distribution – the preponderance of non-victimisation – that would seem common to all observed victimisation distributions, including inter-personal violence (Table 1a) and domestic violence (Table 1b). Yet it ignores the second consistent element of the distribution – the high frequency of victimisation of the apparent minority of ‘multiple victims’. Although these frequency rates differ between crime types – with property crime at one end of the spectrum and domestic violence at the other – the survey data nevertheless suggest a common pattern of multiple victimisation, ranging from a fifth of victims of burglary to around three-quarters of victims of domestic violence Table 1a-b). Yet while the assumption of deviation

from normality inherent in the concept of exposure might fit common-sense explanations of household property crime – committed apparently by predatory strangers – they hardly fit common-sense understandings of acquaintance and domestic violence – committed by familiars who are party to a routine relationship, with *persistent frequency* of risk over time.

Not surprisingly, from the outset, victimology has seen a polarisation between explanations of property crime, on the one hand, and domestic violence, on the other. Inter-personal violence has had an uneasy position between them, splitting primarily on the fact of the prior relationship between the two parties, resting on a distinction between ‘stranger’ and ‘acquaintance’. While the discrete outcome empirical model has suited quantitative survey-based methods, the study of domestic violence has proceeded primarily on the basis of qualitative methods. Similarly, the discrete outcome model focuses exclusively on issues of *onset* – why people become victims, and completely ignores issues of *process*, including the duration of victimisation experience and its cessation (see Miether and Meier, 1994). In contrast, the latter are of paramount interest in the victimology of domestic violence. Issues of onset are at best irrelevant and at worst inappropriate, since domestic violence is a condition that develops *after* the ‘onset’ of a relationship. Ostensibly, different normative counterfactuals apply: in the case of domestic burglary, it is the expectation of privacy and protection from trespass that underpin property norms (Hope, 1999); in the case of domestic violence, it is the social and cultural norms of intimate human relationships.

Yet the empirical similarity of all observed crime victimisation distributions (Tables 1a and b) suggests that there might be a common data generating process uniting all forms of crime victimisation. Conceptually, what would be required for a general theory to express the commonality of the crime victimisation data generating process would be to bring the respective counter-factual conditions into common alignment. Yet, since it is both archaic and unacceptable to apply the norms of property relations to the conduct of interpersonal relationships¹¹, the only possible strategy to unite the two empirical traditions of victimology is to apply the norms of personal relationships to the explanation of property crime and ‘stranger’ victimisation, especially to consider issues of victimisation duration and cessation, which have been central the study of domestic violence.

The conceptual strategy that would need to be applied to property crime would be to mimic more precisely the ‘relational’ or processual characteristics found in the phenomenology of

domestic violence. Two broad strategies are possible: first, to focus on the duration of victimisation. Central to this line of enquiry is whether there could exist a property of *contagion* between victimisation events – that is, that the probability of one event influences the probability of subsequent events. This is the case in domestic violence where violence becomes established in a relationship and may escalate in severity as a result of repetition. A second strategy is to focus particularly on the conditions that lead to *immunity* – both those things that lead to the cessation of victimisation once it has started – for instance by removing victims to a place of refuge - as well as those things that inhibit it from starting in the first place – which in the case of violence means addressing factors leading men to perpetrate violence in relationships.

However, the specialism of victimology (as distinct from criminology) suggests that the focus remains upon *individual victims and their specific time-space locations and trajectories*. Part of the confusion that has grown up recently has been a result of shifting the focus away from the specific perspective of victims. For example, an interest in repeat victimisation can include an interest in working out relationships between victimisation *incidents*, particularly from a crime prevention perspective. Such studies may be concerned with whether there is any contagion between incidents occurring amongst neighbouring properties (e.g. Townsley et al. , 2003), or within residential areas (Morgan, 2001). Similarly, some (perhaps most) studies of repeat victimisation are concerned primarily to study the target-selection activities of *offenders*, again with a concern to intervene in these activities to reduce crime, again defined as consisting of the quantity of incidents (Pease, 1998; 1993; Farrall and Pease, 1995). Yet these interests tend to divert attention away from exploring the *position and agency of the victim*, who remains a passive ‘host’ to these phenomena which are, strictly speaking, occurrences emanating from their external environment. But notwithstanding the conceptual and moral difficulties of introducing the concept of victims’ agency in their victimisation [see elsewhere in this volume], the aetiological analysis of victims necessarily requires some notion of victims’ agency, particularly how victims *interact* with their environments. In so doing, it may be possible to move beyond the normative and explanatory inadequacies of the discrete outcome model of exposure to risk (Walklate, 1997).

True Contagion

Many instances of count data, like those of crime victimisation (Table 1a-b), are characterised by over-dispersion. The standard parametric model used to account for over-dispersion is the *negative binomial model* (Cameron and Trivedi, 1998), which Nelson found to have an “astoundingly good fit” to the observed sample distribution of crime victimisation (1981: 872). Yet, despite its empirical robustness, the negative binomial model is theoretically ambiguous since its assumptions can support a variety of probability mechanisms that might be producing the data generating process (**dgp**)¹². Because of this, although multivariate regression models based on the negative binomial distribution can be used to estimate the predictors of the distribution of crime victimisation by allowing for over-dispersion (Osborn and Tseloni, 1998; Tseloni, 2006), there is not a unique specification of the **dgp** for the negative binomial model and hence the problem of identifying a general specification of the distribution of crime victimisation remains unresolved.

There are two archetypical processes producing over-dispersion in count data (Cameron and Trivedi, 1998): first, a dynamic dependence between the occurrence of successive events which are reported by each respondent – what is termed here as *event-dependency*. Event-dependency can be regarded as an instance of *true contagion* between victimisation experiences. The hypothetical illustration often used is where crime victimisation events occur over time in a *series* against a target (or victim) because they are committed by the same person, who returns to the scene on a number of occasions because he or she is attracted to the target in some way (Farrell *et al.*, 1995). A second source of over-dispersion in the observed sample distribution is where there is unmeasured variability in *a priori* risk between respondents that affects the observed distribution in a systematic way – what is termed here as *risk-heterogeneity*. For example, two households reporting different frequencies of victimisation may possess different characteristics, signifying differential risk or vulnerability, that render them *differentially prone* to being victimised. This interpretation conforms more to the assumptions of the risk-exposure model. Thus, any repetition of victimisation over time for any individual may merely indicate separate occurrences (even if these consist of separate series or spells of victimisation occurrences) of a stable and persistent risk. Crucially, in the risk-heterogeneity interpretation, incidents occur randomly over time with no direct link between one incident and the next.

As Pease (1998) helpfully puts it, the repeat victimisation of a person or household over time may serve either as a *flag* for a stable probability of victimisation risk or indicate that victimisation events can serve to *boost* the likelihood of subsequent events over and above chance. Yet merely aggregating risk occurrences across heterogeneous individuals can yield an *apparent* contagion effect which is indistinguishable in cross-sectional data from the effect produced as if true contagion (event dependency) existed. So, despite the fit yielded by the negative binomial distribution, the underlying **dgp** cannot be identified, nor can true contagion (event-dependency), using only cross-sectional data (Cameron and Trivedi, 1998). Both Nelson (1980) and (Sparks, 1981) rejected as implausible the idea that true contagion characterised the **dgp** since this would mean, contrary to the risk-exposure model, that everyone faced the same *a priori* risk. Nelson (1980) substantiated this recommendation by showing that the correlates of victimisation differed very little whether one used individual probabilities (i.e. whether a person will experience one or more victimisations over the recall period) or rates (the frequency-rate of victimisation over the period).

Nevertheless, theoretically, the negative binomial distribution is capable of accommodating different *mixtures* of flag and boost processes (Cameron and Trivedi, 1998). In particular, the hypothesis that victimisation is repeated by virtue of event-dependency has been justified more recently by arguments in terms of ‘routine situational risk-transmission’ (Hope *et al.*, 2001), primarily by hypothesising that re-victimisation is a consequence of re-visits by the same offender or his/her direct associates over the short-term (Farrell *et al.*, 1995). In this, there is an attempt to incorporate elements of true contagion into the risk-exposure model. As Pease (see also Farrell *et al.*, 1995) puts it:

“...the key reasons for repeats are...the presence of good, and lack of bad, consequences of the first crime for the offender, and the stability of the situation which presents itself to an offender on the first and subsequent visits to the scene of his or her crime” (Pease, 1998: 6).

Two pieces of evidence are often cited as suggestive of the existence of true contagion: first, analysis of aggregate recorded crime data appears to suggest a short time-period between offences, if they repeat (Polvi *et al.*, 1990)¹³; and second, *series offences* comprise a very large proportion of offences reported by victims to the British Crime Survey (Chenery *et al.*,

1996)¹⁴. The emphasis on short-term, event-dependent repetition lends itself congenially to the application of immediate situational control methods of crime prevention (Laycock, 2001; Pease, 1998).

Osborn and Tseloni (1998) were able to estimate a multivariate negative binomial model for the whole frequency-distribution of household property crimes using British Crime Survey (BCS) data with the assumption that over-dispersion arises through unexplained risk-heterogeneity. Using *a priori* risk factors identified from their model, they were able to predict the probabilities for the various frequency-levels of victimisation observed in the data. Comparisons of predictions from the negative binomial model were made with predictions derived from a simple Poisson model: for low levels of predicted *a priori* victimisation risk, the two distributions gave very similar results. However, for higher risk levels, the negative binomial model diverged from the Poisson model, more closely resembling the observed distribution of crime victimisation, with fewer victims than under a Poisson assumption but correspondingly more multiple-victim households (Osborn and Tseloni, 1998: 325), and suggesting a much better fit. They conclude that "...our models indicate that crime is more concentrated [on multiple victims] than random events would predict...the effect of this concentration is most marked for those whose household and area characteristics make them most vulnerable" (Osborn and Tseloni, 1998: 328).

Nevertheless, despite further model refinement (Tseloni, 2006), the use of negative binomial regression methods cannot *prove* the presence of true contagion. That is to say, it does not imply that multiple victims are 'repeat' victims, in the sense that each event is dependent upon prior events. If anything, the reverse is more likely, since the model specification utilised in applications to date tests specifically for risk-heterogeneity and relegates any possible contagion effects to residual error¹⁵. Even so, the limitations of cross-sectional data also limit efforts to test properly for the existence of event-dependency. In practice, while modelling refinements, such as those introduced by Tseloni (2006), can help further to identify the extent of risk-heterogeneity – for instance, by estimating the degree of unmeasured heterogeneity – by the same token they reduce further the possibility of event-dependency. Thus, while considerable confidence can be had that the distribution of crime victimisation fits the negative binomial model, less can be said as to which generalisation of the model is the most appropriate description of the underlying process generating the

distribution, even though the success of the risk-heterogeneity version to date reduces support for true event-dependent (i.e. repeat victimisation) contagion (Tseloni and Pease, 2003).

Vulnerability

Nevertheless, even if there may be no event-dependent link between incidents, that does not mean that the occurrence of events are not related, though the mechanism linking them may reside in the biography or 'life-course' of the victim – including relationships with others – rather than intrinsically in the victimisation events themselves (Hope *et al.*, 2001). Risk-transmission via the life-course becomes more persuasive when incidents are contextualised as part of people's lives – for example, when they re-occur over the longer-term, or reoccur in variant or generic situations, or are clearly part of a long-term life-course attribute, such as a cohabiting relationship. In a 'life-course' explanation, the 'carrier' of risk is the victim. People might become vulnerable to crime victimisation by virtue of some predisposing risk-factors acquired early on, which renders them abnormally and persistently vulnerable to being victimised during their subsequent life-course¹⁶. This view of vulnerability, however, lends itself too readily to the reification of unwarranted and immeasurable traits such as victim-proneness, which logically must precede any instance of victimisation, must constitute a prior cause of subsequent victimisation but which must not be defined by reference to them. Here, again, problems are raised by the difficulty in deciding upon what would be an appropriate counter-factual condition for life-course victim-vulnerability.

Immunity

In preference, the continuing occurrence of incidents that persistently instantiate the victims' vulnerability may eventually become part of victims' biographies. Regular experiences of victimisation may shape their life-course to such a degree that crime victimisation itself becomes part of the 'normal' pattern of their everyday lives (Genn, 1988). These patterns may then continue into the future, unless and until something happens to *rupture* the expected life-course – for example, the abused woman leaves the abusive relationship, or the victimised household moves to a safer neighbourhood. Yet while they remain in such victimising circumstances, victims will simply continue to experience victimisation. The frequency of which being largely in the hands of offenders. So, while the concept of risk-exposure has been deployed to account for the onset of crime victimisation, the possibility of

life-course persistence (duration) calls, in turn, for an explanation also of the *cessation* of victimisation.

Hope and Trickett (2004) propose an *immunity model* to explain the **dgp** driving the observed micro-level frequency distribution. Rather than see ‘repeat victimisation’ as a consequence of excessive selective exposure to crime risk – primarily selection by prolific offenders (Pease, 1998) – frequent (multiple) victimisation could also reflect certain victims’ *inability to remove themselves from risk*, by virtue of their relative powerlessness to change their life-circumstances. In support of the concept of *powerlessness*, Hope *et al.*, (2001), found that multiple crime-type victims experienced also other forms of social vulnerability – such as suffering a domestic fire in the past two years (also a rare occurrence) – and were also more likely to be younger adults, living with children, renting from the local (social) housing authority and living in poorer, urban areas. Thus multiple victims shared many of the social characteristics of economically marginal social groups – sectors of society that are also likely to suffer other misfortunes, including ill-health.

Yet, unlike many forms of ill-health, the source of crime victimisation risk (i.e. motivated offenders) comes primarily from victims’ environments. Thus very vulnerable residents in high-risk environments continue to be victimised because they are unable to attain immunity – that is, to remove or protect themselves from risk within those environments. Rather than being selected specifically for repeat victimisation on the basis of prior victimisation (Pease, 1998), victims in these environments may only *appear* to have a non-random probability of repeat crime victimisation over time because they are more likely to remain unprotected in an environment where the probability of victimisation itself remains high and constant. Their contagiousness is apparent rather than true. In contrast, those who have attained ‘immunity’ by virtue of their removal from risk have a censored (negative) exposure to crime risk – that is, they are no longer eligible for selection as victims and thus unavailable to register at higher frequency levels. Thus, while repeat victims do not possess any additional risk factors that mark out their excessive risk (Osborn *et al.*, 1996), their continued vulnerability may indicate an (as yet unmeasured) incapacity to remove themselves from risk; while the category of non-victims may mask a variety of types of immunity, again unmeasured¹⁷. As such, powerlessness to avoid the probability of exposure may allow the conceptualisation of negative values of risk that are missing from the more positive conception of exposure discussed above.

As described in most of its technical reports, the Primary Sampling Unit (PSU) of the BCS (however it has been defined) is a *nested cluster sample* of respondents living in close, spatial proximity to each other. From these it is possible to aggregate individual responses according to the strata and clusters present within the BCS sampling structure – for instance, to form ‘pseudo-neighbourhoods’ based on such clusters. Trickett *et al.* (1992) estimated expected prevalence from observed prevalence rates, over the deciles of the distribution of pseudo-neighbourhood crime victimisation rates for both personal and household property crime victimisation. As illustrated in Figure 1, observed prevalence rates differed significantly from that expected, over all decile groups, though dramatically so in higher rate areas ¹⁸.

Figure 1 about here

Their analysis implied that significantly *fewer* people are victimised than would be anticipated if the chance of crime victimisation was distributed randomly amongst the population. Yet neither is this likely to be random selection: while the disparity described in Figure 1 has been interpreted as evidence of excessive exposure of a minority of people (repeat victims), it can also be seen just as easily as indicating a non-random prevalence of immunity (i.e. non-victimisation) amongst other residents. Thus ‘what is different about high crime areas’ is not only non-random repeat victimisation but also non-random immunity. The possible non-random co-presence of *both* immune and chronic groups within the same risk producing environment has not hitherto greatly influenced either theory or policy. Even so, it is possible that the social environment of any high crime community may be composed at any one time of a *segmented order* comprising *both* the extremely vulnerable and the highly immune. The resulting neighbourhood risk environment may reflect the outcome of ‘conflicting forces’ of exposure and immunity – a process documented during a crime reduction ‘experiment’ in Hope and Foster (1992) - resulting in a particular macro-level pattern of crime-flux (Hope, 1995) and a distinctive micro-level frequency distribution.

Very little is known about either the varieties of immunity or the forms of relationship between immune and victimised groups or, needless to say, how these relationships shape the data generating process of observed crime victimisation distributions. Some people, e.g. the elderly, may be immune because they are never exposed to risk, or because of moral inhibition (see Clarke *et al.*, 1985). Some may be immune to victimisation because they are

themselves offenders, or members of family networks, capable of retribution should they be attacked. And some people may be victimised excessively because they do not conform to the prevailing culture, or because they ‘stand-out’ in some way (Walklate and Evans, 1999). In any event, detailed ethnography reveals a variegated, micro-social pattern of group relations, social networks and contrasting cultures that warns against reliance upon a ‘black box’ conceptualisation of the environment of crime victimisation¹⁹. Whatever the conceptualisation, it seems likely that all victims have *relationships* of one kind or another with the sources of their victimisation risk, even if these are not intimate or identifiable.

CONCLUSION

Crime victimisation is conceptualised as a phenomenon that is embedded in everyday life. ‘Victims’ are members of populations in society and experience victimisation while being ‘normal’. Nevertheless, the empirical study of crime victims, especially derived from population sample surveys, has suffered from the illusion of actuality fostered by the ‘real’ and ‘normal’ appearance of these data. It is not as though such data were not real themselves nor that they do not represent real phenomena but that the embeddedness of victimisation in everyday life makes it hard, if not impossible, to *abstract* crime victimisation so that its true causes and processes might be observed. Victimisation is defined by its observed actual occurrence which makes it impossible to observe the data generating processes that underlie the production of these observations, including continuous processes of risk that might be inherent in victimisation. And it is also impossible to observe the counterfactual conditions that give meaning to the factual condition of crime victimisation. In sum, we are hindered in explanation because we cannot observe non-victimisation in the same way that we observe victimisation and we cannot therefore observe the complete manifestation of the phenomenon.

Nevertheless, the positive perspective has dominated empirical work, leading analysts too readily to interpret observations as positive evidence of crime victimisation – especially, that crime victimisation is a product of exposure to abnormal risk, and that victims deviate from the normal condition. Yet efforts to apply statistical models to the data reveal problems and difficulties with the approach, in part because positive data is being used to prove the normal condition – non-victimisation – which, paradoxically, is defined by its absence, or as an unobservable negative. It may be impossible to overcome the limits to observing the data

generating process of crime victimisation. Operational research efforts to abstract victimisation from everyday life risk both selection bias and artificiality. But equally so does unreflexive 'realism' based on direct observation. The only viable research strategy, then, is to work with the observations of crime victimisation that we are able to gather but to approach these in a fundamentally reflexive and hypothetical manner. And this requires careful attention to how we frame and test our hypotheses, not just about the facts of victimisation that we can observe but also about the necessary counterfactuals, which we must infer.

FURTHER READING

Many of the sources for this chapter are contained in the references, though these tend to be in the form of rather technical academic journal articles. Somewhat dated books concerning analyses of victimisation survey data include: R. F. Sparks, H. Genn, and D. Dodd Surveying Victims. (1977, London: Wiley); M. S. Hindelang, M. Gottfredson, and J. Garofalo Victims of Violent Crime. (1978, Cambridge, MA.: Ballinger); and T. D. Miethe and R. F. Meier Crime and Its Social Context. ((1994, Albany, NY: SUNY Press). A useful textbook, again somewhat dated, is C. Coleman, and J. Moynihan Understanding Crime Data. (1996 Buckingham: Open University Press).

More recent overview chapters on crime surveys are: P. Mayhew (2000). 'Researching the state of crime: local, national and international victim surveys' (in R.D. King and E. Wincup (Eds.). Doing Research on Crime and Justice. Oxford: Oxford University Press); and T. Hope (2005). 'What do crime statistics tell us?' (in C. Hale, K. Hayward, A. Wahidin and E. Wincup (Eds.). Criminology. Oxford: Oxford University Press. Details about the British Crime Survey can be found in annual reports published in the Home Office Statistical Bulletin series (<http://www.homeoffice.gov.uk/rds/pubsstatistical.html>). Information on the social and political context of crime victimisation surveys in Britain can be found in Crime Statistics: User Perspectives. Statistics Commission Report No. 30, 2006 (London: Statistics Commission. www.statscom.org.uk).

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TABLE 1A

Recalled-frequency crime distributions, previous 12 months – British Crime Survey (2002/03)
General adult population (percentages)

	Proportion of Non-victims in population	Proportion of victims experiencing two or more incidents
Property		
Burglary – households	96.6	18
Vehicle-related theft – vehicle owning households	89.2	19
Violence		
Mugging	99.9	10
Stranger	98.4	21
Acquaintance	98.6	28
Domestic	99.4	45

Notes

Adults in the BCS are persons over 15 years of age.
Source: Simmons and Dodd (ed) HOSB 07/03

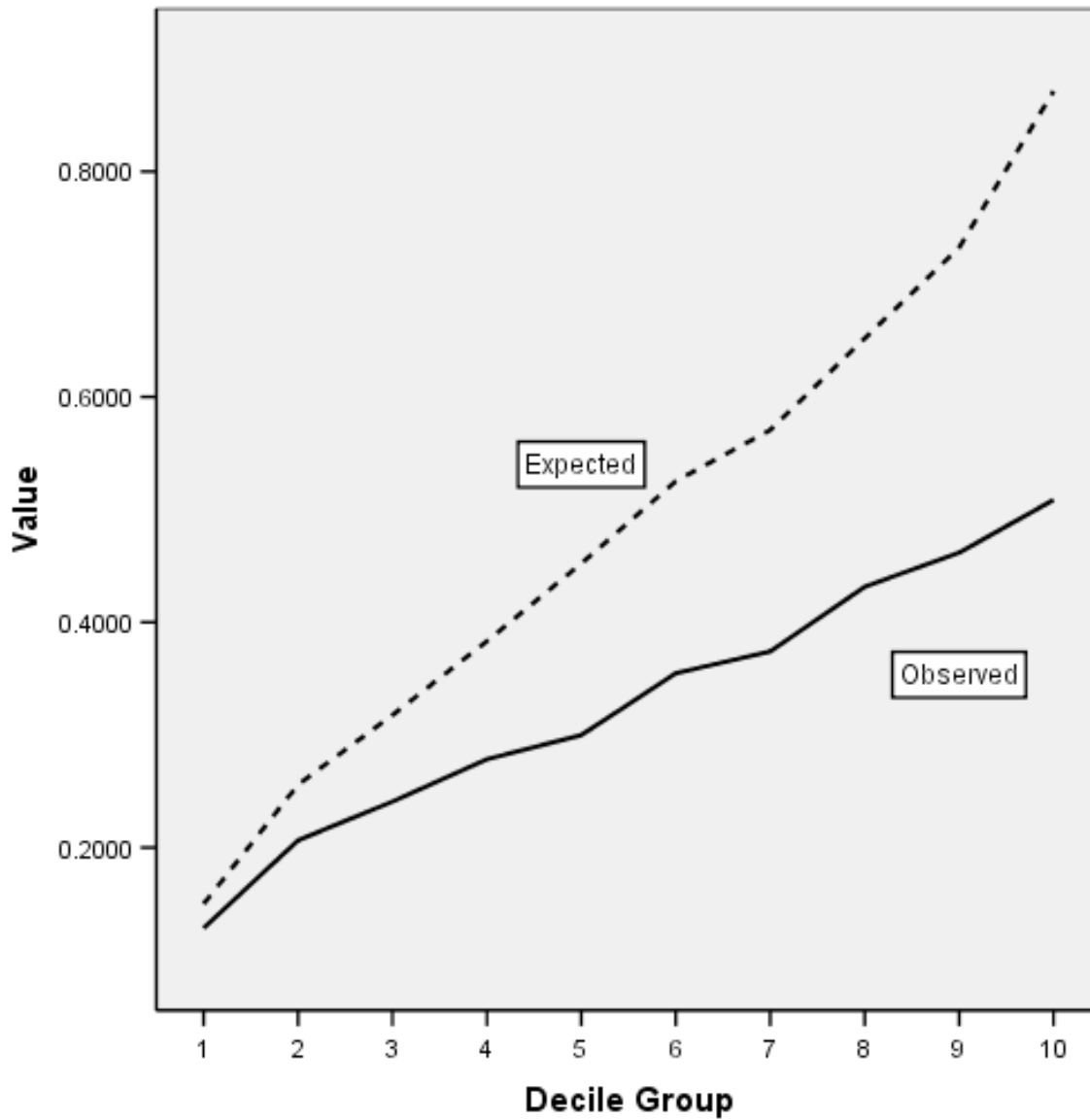
TABLE 1B

Recalled-frequency domestic violence distributions,
previous 12 months – British Crime Survey (2001)
Adult women (percentages)

	Proportion of Non-victims amongst adult women	Proportion of victims experiencing two or more incidents
Domestic violence – threat or force	95.8	72
Domestic force	96.6	68
- minor	97.4	63
- severe	98.4	73

Source: Walby and Allen (2004)

Figure 1

Observed and expected prevalence - household property crime victimisation

From: Trickett et al (1992: Table 1, p. 86)

Notes

¹ Counterfactual theories of causation have emerged as (arguably) a more useful way of conceptualising causation than the traditional Humean empirical regularity approach, particularly for observable phenomena. As Menzies (2001) puts it, the basic idea of counterfactual theories is that "...the meaning of a singular causal claim of the form "Event *c* caused event *e*" can be explained in terms of counterfactual conditionals of the form "If *c* had not occurred, *e* would not have occurred" (Menzies, 2001). Increasing interest in, and use of, counterfactual reasoning is occurring, for example, in the cognate fields of epidemiology (e.g. Maldano and Greenland, 2002), micro-econometrics (Heckman, 2001), quantitative sociology (e.g. Harding, 2003) and, recently, criminology (Sampson *et al.*, 2006).

² For example, no hereditary proneness to crime victimisation has yet been identified.

³ Presumably due to the point made above, that much data is collected for other primary purposes, for example, in the case of national victimisation surveys such as the British Crime Survey, to estimate annual (cross-sectional) prevalence and incidence rates for its jurisdiction (England and Wales).

⁴ For example, competition for questionnaire space with apparently more 'policy-relevant' variables.

⁵ Specifically, national victimisation surveys such as the British Crime Survey (BCS) sample representative adult populations of permanent residence. In the case of the BCS, respondents are aged 16 years or over and answer questions about themselves and their households. Respondents are selected using a complex multi-stage sample design and are interviewed in person using Computer Assisted Personal Interviewing (CAPI) techniques.

⁶ Referred to in the econometrics literature as 'discrete choice' models.

⁷ "...the probability that a violation will occur at any specific time and place might be taken as a function of the convergence of likely offenders and suitable targets in the absence of capable guardians (1979:590)".

⁸ Marsh (1982) disparages such an approach as 'face-sheet sociology'.

⁹ These could either be similar or different to hypothetical unmeasured differences distinguishing generally victims from non-victims. These possibilities have been explored recently using US National Crime Victimization Survey data by Tseloni and Pease (2003).

¹⁰ Which are further compounded by the problems of 'experimentation' noted above.

¹¹ Indeed, this has been the paramount project of feminist victimology

¹² Cameron and Trivedi (1998: 102) cite thirteen distinct stochastic mechanisms.

¹³ Although this assumption has been questioned by employment of alternative stochastic models (Spellman, 1995).

¹⁴ A 'series offence' is defined in the British Crime Survey by respondents answering the question 'were any of these very similar incidents, when the same thing was done under the same circumstances and probably by the same people?'

¹⁵ i.e. implementation as a risk-heterogeneity model – a characterisation of the Negative Binomial as a Poisson-gamma mixture (Cameron and Trivedi, 1998; Tseloni, 2006; Osborn and Tseloni, 1998).

¹⁶ Of course, this approach has been adopted by developmental perspectives on offending.

¹⁷ This interpretation is consistent with a compound-Poisson specification of the negative binomial distribution (Cameron and Trivedi, 1998) which has been found via multivariate modelling to have both a good fit to the micro-level distribution (Osborn and Tseloni, 1998), and to indicate substantial unmeasured risk-heterogeneity in the population (Tseloni, 2006).

¹⁸ This finding was replicated using data from the 1988 BCS (Trickett *et al.*, 1995).

¹⁹ See Merry (1981); Foster and Hope (1993); Walklate and Evans (1999)