

VOODOO CRIMINOLOGY AND THE NUMBERS GAME

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Cultural criminology is of importance because it captures the phenomenology of crime – its adrenaline, its pleasure and panic, its excitement, and its anger, rage, and humiliation, its desperation and its edgework. I wish to argue that cultural criminology not only grasps the phenomenology of crime but, for that matter, is much more attuned to phenomenology of everyday life in general in this era of late modernity. We are confronted at this moment with an orthodox criminology which is denatured and desiccated. Its actors inhabit an arid planet where they are either driven into crime by social and psychological deficits or make opportunistic choices in the criminal marketplace. They are either miserable or mundane (see Hayward and Young, 2004). They are, furthermore, digital creatures of quantity, they obey probabilistic laws of deviancy – they can be represented by the statistical symbolism of lamda, chi and sigma, their behaviour can be captured in the intricacies of regression analysis and equation.

The structure of my argument is that, given human beings are culture-creating beings and are endowed with free will, albeit in circumstances not of their own making, then the *verstehen* of human meaning is, by definition, a necessity in any explanation of human activity, criminal or otherwise (see Ferrell 1997 for more on the notion of 'criminological *verstehen*'). It is in late modernity that such creativity and reflexivity becomes all the more apparent yet, and here is the irony, it is at precisely such a time of the cultural turn that a fundamentalist positivism occurs within the social 'sciences' with increasing strength and attempt at hegemony.

Let us first of all examine the intimate links between late modernity and cultural criminology. The late modern period is characterised by disruption of employment, of marital stability, by greater spatial mobility, by a pluralism of contested values, by the emergence of mediated virtual realities and reference points, and by the rise of consumerism. It embodies two fundamental contradictions: firstly a heightened emphasis on identity in a time when lack of social embeddedness serves to undermine ontological security and, secondly a stress on expressivity, excitement and immediacy at a time where the commodification of leisure and the rationalisation of work mitigates against this. This is a world where narratives are constantly broken and re-written, where values are contested, and where reflexivity is the order of the day (see Young, 1999). For all of these reasons a criminology which stresses the existential, which is focused upon subcultures of creativity and style, which emphasises the adrenalised excitement of human action, on one hand, and the tedium and commodification on the other, goes with the grain of everyday life. Moreover, and counter to claims to the contrary (see Garland, 2000), rational choice/routine activities theory does not fully mirror the texture of the time, but only one part of it. Let us start by looking at the situation of late modernity.

THE LOOSENING OF THE MOORINGS

In late modernity there becomes an increasing awareness of the social construction of boundaries and their contested nature. That is, any sense of the absolute, the reified, the natural, becomes exceedingly precarious. In this process people become more aware of their own role as actors in society. For, although the existential condition and the creation of human meaning has always been part of what we mean by social, this certainty becomes all the more apparent in late modernity. Why is this?

(a) **Voluntarism:** on the back of the movement towards flexible labour and the modern consumer society, with a myriad of choices, an individualistic society

arises where chance, expressivity, meaningful work and leisure becomes an ideal. The American Dream of the post-war period, with its stress on taken-for-granted ends of material comfort becomes overtaken by a new First World Dream, where meaning and expression are paramount and where lifestyles are to be created. Finding yourself becomes more important than arriving.

- (b) **Disembeddedness:** the flexibility and mobility of labour and the increased instability of the family result in people's lives becoming disembedded from work, family and community. This identity does not immediately and consistently present itself. The irony, then, is that just as there is a greater stress on creating one's identity, the building blocks of identity become less substantial. Furthermore, in a lifetime of broken narratives, constant re-invention becomes a central life task.
- (c) **Pluralism and Contest:** increased emigration creates a pluralism of value and this is augmented by the plurality of lifestyles that are created in more individualistic societies. People are, therefore, presented with a social world where values are contested and where there are alternatives of appropriate behaviour and aspiration.
- (d) **Mass Media and Virtual Realities:** In late modernity the mass media expands in terms of the percentage of time of a person's life that it takes up – in England and Wales, for example, television and radio alone take up an extraordinary 40% of the average person's waking life, or 60% of the free time of those in work. The media overall becomes more multi-mediated, diversifying and relating to wider audiences (see McRobbie and Thornton, 1995) As the physical community declines, the virtual community arises, carrying with it virtual realities with new and emerging role models, subcultures of value, vocabularies of motive and narratives both fictional and 'factional'.

Thus, Zygmunt Bauman contrasts the post-war modern world with the late modern world of liquid modernity. In the former there were:

patterns, codes and rules to which one could conform which one could select as stable orientation points and by which one could subsequently let oneself be guided, that are nowadays in increasingly short supply. It does not mean our contemporaries are guided solely by their imagination and resolve and are free to construct their mode of life from scratch and at will, or that they are no longer dependent on society for the building materials and design blueprints. But it does mean that we are presently moving from an era of pre-allocated 'reference groups' into an epoch of 'universal comparison', in which the destination of individual self-constructing labours is endemically and incurably underdetermined, is not given in advance, and tends to undergo numerous and profound changes before such labours reach their only genuine end: that is, the end of the individual's life.

These days patterns and configurations are no longer 'given', let alone 'self-evident'; there are just too many of them, clashing with each other and contradicting one another's commandments, so that each one is stripped of a good deal of compelling, constraining powers. (2000: 7)

Such changes accompanying the cultural turn have extraordinary implications for sociology, particularly for explanation, but also for measurement and research practice. Let us note at this juncture the significant changes in identity formation and with it the vocabulary of motives associated with given roles and structural positions. For the combination of the ideal of choice, disembeddedness and pluralism engenders a situation where vocabularies of motives begin to lose their fixed moorings in particular parts of the social structure and in specific social circumstances. That is, the old rigid moorings of Fordism, the demarcations of class, age and gender, the concentric demarcations of the Chicago model of space in the city begin to dissolve. Vocabularies of motive become loose and cast adrift from their structural sites; they can shift and be fixed elsewhere. This is not willy-nilly: there obviously has to be some fit between structural predicament and subcultural solution, but the level of determination and predictability diminishes. Furthermore, they can be bricolaged elsewhere in the system: they can be reinterpreted, transposed and hybridised. And, finally, and most crucially of course, they can be changed and innovated sometimes dramatically.

Let us, for a moment, look at the relationship between material and social predicaments, identity, vocabularies of motive and social action taking crime as an example (although we could as well focus, say, on educational achievement or sexual behaviour). There is an extraordinary tendency to suggest that the motive to commit crime springs fully fledged out of certain material predicaments (eg poverty, unemployment) or social circumstances (eg lack of control) or biological characteristics (e.g. youth and masculinity), almost as if no connecting narrative or human subjectivity were necessary (see Katz, 2002). In reality, a situation like poverty will result in totally different assessments and responses dependant on the narratives which the subjects use to interpret their predicament – indeed, the very assessment of whether one is poor or not will depend on social interpretation. There can be no causality in society without reference to meaning, and even high correlations – as all the methods textbooks tell us – do not necessitate causality (see Sayer, 1992). What is necessary is to understand that we live in a situation where such meanings change rapidly and do not adhere fixedly to particular social roles, or material predicaments. Concepts of what it is to be young, what it is to be female, what pleasures we should expect, our attitudes to work, sexuality and leisure have all been dramatically recast. None of these changes were predictable from the social and cultural “variables” present before these fundamental roles and values were reinvented. They are understandable, in retrospect, in terms of responses to material and social change, but they were re-fashioned by human actors who simply rewrote their narratives. Let me give an example, the teenage ‘revolution’ was one of the great changes of the late twentieth century – something so dramatic that, as so often with such sweeping changes, we can scarcely see it now. No one knew what youth were going to do with their new position and status. If you were attempting to predict crime rates from the ‘variables’ which seemed to explain crime rates in the fifties, one would have talked about inequalities, employment levels, educational achievement, percentage of adolescent males in the population, divorce rates, etc. But even the most sophisticated statistical analysis (very unlikely at that time) could not have predicted the extent of youth crime and the

reason for this is palpably simple: you could not have anticipated what was to happen to 'youth'.

Let me now turn to the problem of measurement and here I will repeat the structure of my argument. First I will note how the social and meaningful nature of human action make positivistic methods inappropriate, second I will indicate how the situation of late modernity heightens this situation. To do this I will turn first of all to a debate within the sociology of sex.

MEASUREMENT AND THE SEXOLOGISTS

In April and May 1995 the columns of *The New York Review of Books* were subject to a remarkable and, some would say, acrimonious debate. It was an argument which was, to my mind, one of the most significant examples of academic whistle-blowing, wide ranging in its critique, apposite in its target and reasoning, timely and badly needed yet falling, as we shall see, on stony ground.

On one side of this skirmish was Richard Lewontin, Professor of Zoology at Harvard, a distinguished geneticist and epidemiologist, on the other a team of sociologists led by Edward Laumann and John Gagnon from the University of Chicago, who had recently published *The Social Organisation of Sexuality* (1995), and its popular companion volume *Sex in America: A Definitive Survey* (1995). On the sides, chipping in with gusto, Richard Sennett, joint Professor of Sociology at the LSE and NYU.

This debate is of interest because it represents a direct confrontation of natural science with sociology or social science, as it is often hopefully and optimistically called. Such encounters are relatively rare and tend to occur when particularly politically distasteful findings are presented to the public as cast iron and embellished with the primatur of science. A recent example of this was the publication of Richard Herrnstein and Charles Murray's *The Bell Curve* (1994),

accompanied by pages of statistical tables which purported to present the scientific evidence for the link between race, IQ and, indeed, crime. At that time many prominent scientists including Steven Rose, Stephen Gould were moved to intervene but normally the walls between disciplines remain intact: Indeed a collegial atmosphere of mutual respect coupled with lack of interest ensures that parallel and contradictory literatures about the same subject can occur in departments separated sometimes by a corridor or, more frequently, a faculty block. In the case of the natural and social sciences this is complicated by a unidirectional admiration – a one-sided love affair, one might say – or at least a state of acute physics envy – between the aspiring social scientist and the natural sciences. Be that as it may, a considerable proportion of sociologists, the vast majority of psychologists and an increasing number of criminologists embrace, without thought or reservation, a positivistic path. Namely, that natural scientific methods can be applied to human action, that behaviour is causally determined, that incontestable objectivity is attainable and that precise quantitative measurement is possible, and indeed preferable. In the case of criminology, this entails the belief that the crimes of individuals can be predicted from risk factors and that rates of crime can be explained by the changes in the proportion of causal factors in the population.

Richard Lewontin sets out to review the two books. They arose on the back of the AIDS crisis and the need to understand the epidemiology of its spread. The survey was eventually well funded by research foundations, was conducted by NORC, the premiere social survey research organisation in North America. The project involved a sample of 3,432 people representing 200 million post-pubertal Americans. Just for a minute let us think of the audacity of the sample survey – and this one was more thoroughgoing than most - to claim to generalise from such a small number to such a large population of individuals. Lewontin's critique is on two levels, one the problem of representativeness and two – and more substantially – the problem of truth.

Let us first of all examine the problem of representation. An initial criticism is that the random sample was not actually from the total population. It is based on a sample of addresses drawn from the census, but it excluded households where there were no English-speakers, nor anyone between the ages of 15-59. Most crucially it excludes the 3% of Americans (some 7.5 million) who do not live in households because they are institutionalised or are homeless. This latter point is, as Lewontin indicates, scarcely trivial in understanding the epidemiology of AIDS as it excludes the most vulnerable group in the population, including those likely to be victims of homosexual rape in prison, prostitution, reckless drug use, the sexually 'free' college aged adolescents etc. The random sample is not, therefore, drawn from the population as a whole: a *very* atypical population is omitted. Such a restriction in population sampled is a usual preliminary in survey research.

However, once this somewhat restricted sample was made, the research team did not stint in their efforts to get as large a response rate as possible. After repeated visits, telephone calls and financial inducements ranging from \$10 to \$100, the result was a response rate of 79% - of which they were duly pleased. But, as Lewontin points out: 'It is almost always the case that those who do not respond are a non-random sample of those who are asked' (1995a, p.28). In this case it could well be prudishness, but in the case of other surveys equally non-random causes of non-responses. For example, in our own experience of over fifteen large-scale crime and victimisation studies which we ran at The Centre for Criminology, Middlesex University (see for example Jones *et al*, 1986; Crawford *et al*, 1990) we made every effort to reduce non-response but never managed better than 83%. Indeed, criminal victimisation surveys as a whole have between one fifth to a quarter of respondents whose victimisation is unknown. As I remarked at the time, in the thick of quantitative research:

It goes without saying that such a large unknown population could easily skew every finding we victimologists present. At the most obvious level, it probably includes a disproportionate number of transients, of lower

working class people hostile to officials with clipboards attempting to ask them about their lives, and of those who are most frightened to answer the door because of fear of crime. (Young, 1988: 169).¹

Lewontin's first point ('the problem of representativeness') is, therefore, clear and is as applicable to criminology as to sociology. Let me at this point remind the reader of Quetelet's warning. Adolphe Quetelet, the founder of scientific statistics, and a pioneer in analysing the social and physical determinants of crime, introduced into academic discussion in the 1830s the problem of the unknown figure of crime. That is crime not revealed in the official statistics:

'This is also the place to examine a difficulty ... it is that our observations can only refer to *a certain number of known and tried offenders out of the unknown sum total of crimes committed*. Since this sum total of crimes committed will probably ever continue unknown, all the reasoning of which it is the basis will be more or less defective. I do not hesitate to say, that all the knowledge which we possess on the statistics of crimes and offences will be of no utility whatsoever, unless we admit without question *that there is a ratio, nearly invariably the same, between known and tried offences and the unknown sum total of crimes committed*. This ratio is necessary, and if it did not really exist, everything which, until the present time, has been said on the statistical documents of crime, would be false and absurd.' (A Quetelet, 1842: 82)

Quetelet's fixed ratios are, of course, a pipe dream, as unlikely as they would be convenient. His warning, written in 1835 (English translation 1842) has echoed throughout the criminology academy for the last one hundred and seventy years. If we do not know the true rate of crime all our theories are built on quicksand. They will be of 'no utility', 'false', and indeed 'absurd'. Legions of theorists from Robert K Merton through to James Q Wilson have committed Giffen's paradox: expressing their doubts about the accuracy of the data and then proceeding to use the crime figures with seeming abandon, this is particularly true in recent years when the advent of sophisticated statistical analysis is, somehow, seen to grant permission to skate over the thin ice of insubstantiality (Giffen, 1965; Oosthoek, 1978). Others have put their faith in statistics generated by the social

scientist, whether self-report studies or victimisation surveys, as if Quetelet's warning no longer concerned them and the era of pre-scientific' data was over.

Indeed, Richard Sparks and his associates, in the introduction to their groundbreaking British victimisation study, summarised the decade of American research prior to their own with a note of jubilation: 'Within a decade ... some of the oldest problems of criminology have come at least within reach of a solution.' (Sparks *et al*, 1977: 1). As we have seen, the problem of non-response means that such a resolution of the age-old problem of measurement is not resolved. It would be so, of course, if the non-respondents were just – or almost – like the respondents and indeed such an excuse is often invoked with as much likelihood of validity as Quetelet's ratios. As it is, the atypicality of non-respondents is likely to overturn the significance levels of any probabilistic sampling. Richard Sparks was quite clear about this in his assessment of the potential of victimisation studies. His initial excitement became tempered by considerable caution. Thus he writes, ten years later:

Much too much fuss is made, in practically all official NCS publications, about statistical significance (i.e. allowance for sampling variability). A variety of standard errors and confidence intervals for NCS data are now routinely quoted in those publications. Yet it is clear that *nonsampling* error is of far greater magnitude in the NCS; adjustments ... may offset some of this nonsampling error, though only in a ballpark way, which makes questions of sampling variability virtually irrelevant. My own view (not shared by all) is that if after commonsensical adjustment a trend or pattern appears which makes some sense, then it ought not to be disregarded even if it does not attain some magical level of statistical significance. (1981: 44, *n.42*)

TELLING THE TRUTH?

But let us go on to Lewontin's next criticism: the problem of truth. And here the problem is even more important and substantial than that of non-response and the dark figure. This revolves around the key question of whether those who

responded to the questionnaire were in fact telling the truth. That is, that social surveys may not only have dark figures of non-respondents, but a dark figure of non-response – and indeed ‘over-response’ – amongst the respondents themselves.

It is rare for surveys of attitude or self-reported behaviour to have any internal check as to validity. After all, if people say they would rather live by work than on welfare, if they profess liberal attitudes on racial matters, or if they tell you that they were assaulted twice last year, how is one to know that this is not true? One may have one’s suspicions, of course, but there are few cast iron checks. Every now and then, however, anomalies stare you in the face. In the case of the sexual behaviour survey there is a particularly blatant example. For the average number of heterosexual partners reported by men over the last five years is 75% greater than the average number reported by women. This is an obvious anomaly. It is, as Lewontin points out, like a violation of the only law in economics that the number of sales must be equal to the number of purchases. What is startling is that the researchers are well aware of this. Indeed, they devote considerable time to debating the reasons this ‘discrepancy’ might have occurred and conclude that the most likely explanation is that ‘either men may exaggerate or women may underestimate’. As Lewontin remarks,

So in the single case where one can actually test the truth, the investigators themselves think it most likely that people are telling themselves and others enormous lies. If one takes the authors at their word, it would seem futile to take seriously the other results of the study. The report that 5.3 percent of conventional Protestants, 3.3 percent of fundamentalists, 2.8 percent of Catholics, and 10.7 percent of the non-religious have ever had a same-sex partner may show the effect of religion on practice or it may be nothing but hypocrisy. What is billed as a study of “Sexual Practices in the United States” is, after all, a study of an indissoluble jumble of practices, attitudes, personal myths, and posturing. (1995a: 29)

What is of interest here is the awareness of thin ice, yet the ineluctable desire to keep on skating. Just as with Giffen’s paradox, where the weakness of the

statistics is plain to the researchers yet they continue on to force-feed inadequate data into their personal computers, here the problem of lying, whether by exaggeration or concealment, does not stop the researchers, for more than a moment, in their scientific task. Of course, as a sociologist, such findings are not irrelevant: they inform you much about differences in male and female attitudes to sex – what they *don't* tell you is about differences in sexual behaviour. Yet what Richard Lewontin is telling us is that interview situations are social relationships – that results are a product of a social interaction and will vary with the gender, class, and age of the interviewer and of the interviewee. But here we have it: it needs a Professor of Biology to tell sociologists to be sociologists. Thus, he concludes:

The answer, surely, is to be less ambitious and stop trying to make sociology into a natural science although it is, indeed, the study of natural objects. There are some things in the world that we will never know and many that we will never know exactly. Each domain of phenomena has its characteristic grain of knowability. Biology is not physics, because organisms are such complex physical objects, and sociology is not biology because human societies are made by self-conscious organisms. By pretending to a kind of knowledge that it cannot achieve, social science can only engender the scorn of natural scientists and the cynicism of humanists. (*ibid.*: 29)

Of course this is not the end of it. Edward Laumann and his colleagues are outraged. They do not think it 'appropriate for a biologist ' to be reviewing their work, he does not have the right 'professional qualifications' – 'his review is a pastiche of ill-informed personal opinion that makes unfounded claims of relevant scientific authority and expertise' (1995b: 43). Lewontin, in reply, notes caustically that it is understandable that the team

would have preferred to have their own work reviewed by a member of their own school of sociology, someone sharing the same unexamined methodological assumptions. They could avoid the always unpleasant necessity of justifying the epistemic basis on which the entire structure of their work depends. (1995b: 43)

As to his incompetence with regards to statistical analysis, he points to being a bit disturbed to have to reveal his CV, but that he has a graduate degree in mathematical statistics which he has taught for forty years and this is the subject of about one-tenth of his publications including a textbook of statistics!

And, of course, such a process of believing in the objectivity of data is fostered by the habit of researchers of *not* conducting their own interviews, of employing agencies such as NORC, or second-hand, in terms of using older datasets or even a meta-analysis of past datasets. So the data arrives at their computers already punched, sanitised: it is a series of numbers with scientific-looking decimal points. Human contact is minimised and a barrier of printout and digits occurs between them and human life.

But let us leave the last remarks of this section to Richard Sennett. He congratulates Lewantin on the brilliance of his analysis, he laments the current fashion of scientific sociology, and concurs with Lewontin's remark that, if work such as this is typical, then the discipline must be in 'deep trouble'. That's putting it mildly, suggests Sennett, 'American sociology has become a refuge for the academically challenged' (1995: 43). But he adds that mere stupidity itself cannot alone explain the analytic weakness of such studies for 'sociology in its dumbed-down condition is emblematic of a society that doesn't want to know much about itself' (*ibid*).

LESSONS FOR CRIMINOLOGY

But what has all of this to do with criminology? A great deal and more, for it is probably criminology, of all the branches of sociology and psychology, where the problem of unchecked positivism is greatest. The expansion of academic criminology was a consequence of the exponential increase in the size of the criminal justice system just as the shift from students studying social policy/administration to criminology parallels the shift from governmental interventions

through the welfare state to those utilising criminal justice. The war on crime followed by the war on drugs and then on terror. This has been accompanied by an expansion in funding designed to evaluate and assess governmental interventions and programmes. The material basis for the revitalisation of positivist criminology is considerable and, certainly within the United States, approaching hegemonic. (see Hayward and Young, 2004)

Embarrassing Findings

Criminological research is replete with findings that range from the very unlikely to the ridiculous. I will give just a few examples:

Rarity of Serious Crimes

Victimisation studies consistently report levels of serious crime that are gross underestimations and are *freely admitted* as such. For example, the first British Crime Survey of England & Wales in 1982 found only one rape and that attempted.

Variability of Findings with Different Instruments

If we take sensitive topics such as incidence of domestic violence, the range of figures are extraordinarily wide – and, in no doubt, underestimates. Thus in 1998 the percentage of women experiencing domestic violence, defined as physical assault with injury, was 0.5% in the police figures, 1% in the British Crime Survey, and 2.2% when Computer Assisted Self-Interviewing was used. An independent survey found a rate in the region of 8%. (Mooney,1999) Which figure in this range is one going to feed into one's PC? What sort of science is it where estimations of a variable vary sixteen fold?

Self-Report Studies

Consistently come up with results showing that there is little variation between the levels of juvenile delinquency between the working class and the middle class, between black and white, and produce a considerably reduced gap between males and females. Hence Tittle and Villemeys's (1977) extraordinary claim that there is no relationship between class and crime which has been taken

at face value by many theorists. All one can record about this surmise is John Braithwaite's pithy remark:

it is hardly plausible that one can totally explain away the higher risks of being mugged and raped in lower class areas as the consequence of the activities of middle class people who come into the area to perpetrate such acts. (1981: 37)

The Assault Rate on White Men

The United States NCVS regularly comes up with results which show that the assault rate reported by white men is higher than or just about equal to that of black men. For example, in 1999 the rate was 32.3 per, 1000 for whites compared to 31.0 per 1000 for blacks. This is totally against any evidence from homicide rates or other indices of violence which would suggest a much higher rate for blacks (see commentary in Sparks, 1981).

Findings of the International Crime and Victimization Studies

The International Crime and Victimization Study (van Kesteren *et al*, 2000) frequently finds rates of reported violence between nations which are almost the inverse of the homicide rates. (See Young 2004)

THE PLURALISM OF THE DARK FIGURE

Up till now we have discussed either technical problems of non-response or the more substantive problem of exaggeration or lying. I want now to turn to a third problem which generates even greater and more impenetrable barriers for scientific quantification. The first two problems – which Lewontin addresses – presume that there is an objective data there to be registered. However, there is a profound difference between measurement in the natural world and in society, namely that the definitions of social phenomena are constructed by individuals and in this they will vary with the social constructs of the actors involved. If one hands out a dozen metre rules to students and asks them to measure the length of the seminar room, they will come to a common agreement with a little variation

for accuracy. If one asks the room full of students to measure levels of violence they are, so to speak, already equipped with a dozen rules of different gauge and length. They will come out of the exercise with different amounts of violence because their definitions of violence will vary. And the same will be true of the respondents to a victimisation study. All of us may agree that a stab wound is violence, but where along the continuum does violence begin: is it a shove (if so, how hard?), is it a tap (if so, how weighty?), or perhaps it is a harsh word, an obscenity, a threat? People vary in their definitions and tolerance of violence: there is a pluralism of measures.

Let us look at two 'anomalies' in this light. The peculiar results of the International Crime and Victimisation Studies where the rates of violence reported are approximately the inverse of the rates of violence occurring if we are to trust the homicide figures, may well be not only that reporting to strangers distorts the level of violence. It may well be but that countries with low levels of violence have low levels of tolerance of violence and thus report acts which other, more tolerant/violent nations, might ignore. Similarly the comparatively higher rate of violence against white compared to black men may reflect differences in definition as to what constitutes 'real' violence. Once we have acknowledged the pluralism of human definition, we can then return to the dark figure with even greater doubts and trepidations. For the dark figure will expand and contract not merely with the technical means we bring to it, but with the values of the respondents and indeed the categories of the interviewers. And the social rather than the merely technical permeates our measurement on all three levels: whether it is the respondents who refuse to talk to us, to those that in their relationship with a stranger (of class, gender, age, and perhaps ethnicity) will attempt to convey an impression of themselves (a product of their own personal narrative which they have woven around the 'facts' of their lives), to the values and meanings which the interviewer brings to the table.

EYSENCK'S DILEMMA

It is important to stress how damaging such findings are for the positivist, for the scientific project of studying humanity. For positivism needs fixed categories, agreed measurements, objective and uncontested figures. The late Hans Eysenck, the doyen of psychological behaviourism, recognised this quite clearly in the last book he wrote on criminology with his colleague Gisli Gudjonsson. For, in *The Causes and Cures of Criminality* (1989) they began by taking issue with the authors of *The New Criminology* (I Taylor *et al*, 1975) in their assertion that crime is not an objective category but a product of varying legal fiat – Eysenck and Gudjonsson quite clearly recognise this as an obstacle to science and get round the argument by differentiating two types of crime: victimless and victimful crime. Victimless crime – and they give examples from prostitution to anal sex – they concede, are subjectively and pluralistically defined. These are eliminated from the realm of objectivity – but victimful crimes, and here they list such phenomena as theft, assault, murder, rape, are, they argue, universally condemned and, therefore, clearly objective. This is obviously untrue: all of these crimes are subject to varying definitions – to talk of them having a fixed nature is to teeter on the brink of tautology. Rape is, of course, universally condemned because it is an illegal sexual attack, but what constitutes rape varies and, indeed, expands with time, witness the acknowledgement of marital rape as rape. And assault, as we have seen, is greatly dependent on our tolerance of violence.

THE IMPACT OF LATE MODERNITY ON MEASUREMENT

In the introduction to this essay I noted how in late modernity the causes of crime become loosened from their fixed structural moorings, that the ‘same’ circumstances become imbued with different meanings. The causes of crime, in the sense of a narrative leading from personal circumstances to crime committed, are more varied; they are in less in a fixed relationship. We have seen in our discussion of measurement how the effects - the outcome of such a

narrative - are subject also to differential interpretations. They are part of this constructed narrative of meaning both of the actor and in the actor's relationship to the interviewer in the act of measurement. But here once again such a social construction is also subject in late modernity to greater contest and pluralism of definition. So the hidden figure expands and contracts with the values we bring to it. In a pluralist society it is no longer possible to talk of a hidden figure x with which we can attempt to measure, but a whole series of hidden figures x, y, z etc.

THE BOGUS OF POSITIVISM

The positivist dream of a scientific sociology of crime, which attempts to objectively relate cause and effect, becomes all the more impossible in late modernity. As we have seen, both the causes of crime and the definitions of crime, that is the outcome or effects, become problematised. To move from, say, unemployment to crime, or deprivation to crime, you need narratives; correlation alone cannot assure causality, it is only the narratives which link factors to outcomes that can do this. People turn 'factors' into narratives – they are even capable of turning such factors on their heads. Furthermore, what is crime itself is part of this narrative. It is a variable dependant on subcultural definition and assessment.

The bogus of positivism is that it only *seemed* to work when the world was reasonably static, where vocabularies of motive seemed organically linked to points in the social structure and where definitions of crime were consensual and unproblematised. The loosening of moorings in late modernity, and the multiple problematisation consequent on pluralism destroys this illusion. As Martin Nicolaus exclaimed in his famous article in *Antioch Review* so many years ago, 'What kind of science is this, which holds true only when men hold still?' (1969: 387)

We live in a time of rapid change. In these times, rather than the variables determining the change, it is almost as if the change occurs and the factors seem

to scuttle after them. Prediction of real life events of any consequence has always been a lamentable failure in the 'social sciences', just think of the collapse of communism and look at the writings of political scientists prior to the days of *glasnost*. In criminology we have witnessed in our lifetimes two dramatic changes completely contrary to our scientific predictions. First of all, in the period from the sixties onwards, the crime rate increased remorselessly in the majority of industrial countries despite the fact that all the factors which previously had been identified as reducing crime, were on the increase (e.g. wealth, education, employment, housing). I have termed this elsewhere the 'aetiological crisis' in criminological theory (1994) and this set in motion an intense debate amongst criminologists and is the basis for the extraordinary creativity and plethora of theories that occurred in the last thirty years. But having spent the whole of our professional lives researching why crime should almost inexorably go up (whether by relative deprivation, broken homes, social disorganisation, breakdown of controls, labelling, etc.) we find ourselves in the infuriating position of the crime rate in very many industrial countries (including the US and the UK) begin to go down, against all predictions that I know of. Here we have a double trauma or whammy, if you want!

THE CRIME DROP IN AMERICA AND THE CRISIS OF POSITIVISM

On November 16 2000, in San Francisco, a packed meeting of the American Society of Criminology gathered together to discuss a most extraordinary happening in the world of crime. For from 1991 onwards, violent crime in the United States, which had led the advanced world by far in rates of murder and robbery, had begun to fall. Homicide dropped by 35.7 per cent from 1991 to 1998 (from 9.8 to 6.3 per 100,000) (Blumstein and Wallman, 2000). Al Blumstein, of the National Consortium on Violence Research had brought together a dazzling array of experts: demographers, economists, sociologists and criminologists, all contributing their views on the change with graphic charts and probing statistical analysis. I listened with fascination to how they factored

each of the developments over the period to explain the phenomenon, from changes in the distribution of handguns, the extraordinary prison expansion, zero-tolerance policing, down to changes in crack-culture and technology. At the end of the session they asked for comments from the audience, no doubt expecting some detailed remark about policing levels or the influence of handgun availability, or such like; but the first question, from a Canadian woman, was something of a revelation. She pointed out, ironically, how Canadians were supposed to be condemned to culturally lag behind their American cousins, but that they too had had a drop in violence, despite the fact that they had not experienced such a period of rapid prison expansion, that zero-tolerance policing was not *de rigueur* and that Canada had only a small problem of crack-cocaine. (see commentary in Ouimet, 2002). A Spanish woman, who said something very similar about her country, followed her. In fact there was a crime drop in 13 out of 21 industrial countries during 1997-98 (Barclay *et al.*, 2001; Young, 2004)

Blumstein's team focused on the relationship between variable changes and the drop of violence, once international data is examined one must seriously question whether they were looking at the correct variables and furthermore, to cap it all, they traced their line of correlation between these variables and the level of violence when, in fact, property crimes were also declining. The most immediate explanation of this is that we are encountering 'spurious causality'. (See Andrew Sayer, 1992: 193) But the enigma of the crime drop takes us far beyond the world of technical mistakes. The usual procedure in such analysis is to take the demographics and other factors which correlate with crime in the past and attempt to explain the present or predict the future levels of crime in terms of changes in these variables. The problem here is that people (and young people in particular) might well change independently of these variables. For in the last analysis the factors do not add up and the social scientists begin to have to admit the ghost in the machine. Thus, Richard Rosenfeld of Blumstein's team writes ruefully:

If the church is the last refuge of scoundrels, “culture” is the final recourse of social scientists in search of explanations when existing economic, social and political theories have been exhausted. (2000: 157)

So there we have it, subculture becomes the final refuge of scoundrels! And Rosenfeld comments, ‘It is possible that American adults are becoming, in a word, civilised’ (*ibid*: 156).

From a more sympathetic perspective, Andrew Karmen in his meticulous analysis of the New York crime drop – *New York Murder Mystery* (2000) casts his eyes across all the various explanations judiciously giving them various explanatory weightings, but at the end of the book talks of ‘the final demographic factor which might be the most important of all’ (2000: 249). But then, he reflects, ‘the shift is not even strictly demographic in nature: it is attitudinal and behavioral as well as generational’ (*ibid*). And, he adds, ‘Unfortunately the existence of this suspected evolution in subcultural values defies precise statistical measurement. It is not clear what kind of evidence and statistics could prove or disprove it.’ Karmen points to the possibility of profound changes in the norms of urban youth culture. And here he refers to the pioneering work of Ric Curtis, the New York urban anthropologist who talks of the ‘little brother syndrome’. That is, where younger children, having witnessed the devastating effects of hard drugs, gun culture, intensive crime on their older brethren decide that these things are not for them, they are no longer hip and cool – the culture evolves and turns its face against the past. This observation has ready resonance with, for example, any attempt to understand changes in drug use. These do not seem to relate to changes in social factors or the impact of the war against drugs. They seem to relate to changes in fashion – although this is perhaps too light a word for it – changes in subcultural project would probably be more fitting.

Curtis relates these changes closely to the development of late modernity and to the loosening of the moorings which I referred to at the beginning of this article.

Thus he writes: 'The postmodern global economy is one which identity formation is less dependent upon the influence of family, neighborhoods, race/ethnicity, nationality and history, and more than anywhere else the inner city is an empty canvass an open frontier where new structures, institutions and conventions are waiting to be built.' (1998: 1276).

AN OPEN SEASON ON NUMBERS?

Am I suggesting an open season on numbers? Not quite: there are obviously(as Sennett points out in the Sex in America debate) numbers which are indispensable to sociological analysis. Figures of infant mortality, age, marriage, common economic indicators are cases to point as are, for example, numbers of police, imprisonment rates and homicide incidences in criminology. Others such as income or ethnicity are of great utility but must be used with caution. There are things in the social landscape which are distinct, definite and measurable, there are many others that are blurred because we do not know them -- some because we are unlikely ever to know them, others, more importantly, which are blurred because it is their nature to be blurred. Precision must be constantly eyed with suspicion, decimal points with raised eyebrows. There are very many cases where statistical testing is inappropriate because the data is technically weak -- it will simply not bear the weight of such analysis. There are many other instances where the data is blurred and contested and where such testing is simply wrong. Over the last decade there has grown up a peculiar formula for writing journal articles. The introduction usually presents two theories in competition but they are strange one-dimensional creatures almost unrecognizable compared to the real thing by virtue of being rendered simple and decontextualized for the purposes of operationalisation. This acephalous introduction, this headless chicken of an argument is then followed by an extensive discussion of measures whilst the data itself is usually outsourced from some past study or bought in from a survey firm, an obligatory recession analysis follows, an erudite statistical equation is a definite plus and then, the usually

inconclusive results are paraded before us. The criminologists themselves are far distant from crime out there hidden behind a wall of verbiage and computer print out , the barrier graphited with the Greek letters of statistical manipulation.

What can we do to get out of this sanitised redoubt? What is needed is a theoretical position which can enter in to the real world of existential joy, fear, false certainty and doubt; which can seek to understand the subcultural projects of people in a world riven with inequalities of wealth and uncertainties of identity. What we need is an ethnographic method that can deal with reflexivity, contradiction, tentativeness, change of opinion, posturing and concealment. A method which is sensitive to the way people write and rewrite their personal narratives. Our problems will not be solved by a fake scientificity but by a critical ethnography honed to the potentialities of human creativity and meaning.

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ⁱ Even panel studies which follow a given population over time suffer from this problem. To take the famous Seattle Social Development Project as an example (see Farrington *et al*, 2003). This is a prospective longitudinal survey of 808 children. To start with, these are the children/parents who consented to be included out of the population of 1,053 fifth-grade students targeted – that is, it has a 70% response rate from the outset – with 30% refusing consent. Secondly, youths dropped out over time so, for example, by the age of 12 the sample fell to 52%. There is, of course, every reason to suspect that those who initially did not consent and those who fell out of the panel might have different delinquency patterns to those who consented and remained within the panel.