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## WHY PSYCHOSOCIAL CRIMINOLOGY?

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Our starting point is a concern, developed over many years of teaching criminology, that the individual criminal offender has long ceased to be of much interest to criminologists, especially to those of a radical persuasion. The result is that the subject (the conception of what it is to be a person) presupposed in existing theories of crime – whether psychological, sociological, psychosocial or integrated – is woefully inadequate, unrecognizable as the complex and contradictory human being operating in often difficult and cross-pressured social circumstances we know to be the reality of all our lives. In place of messily complex human subjects shot through with anxiety and self-doubt, conflictual feelings and unruly desires, we are offered depleted caricatures: individuals shorn of their social context, or who act – we are told – purely on the basis of reason or ‘choice’, interested only in the maximization of utility. Or, we are presented with individuals who are nothing but the products of their social circumstances who are not beset by any conflicts either in their inner or their outer worlds: pure social constructions, to use the fashionable jargon.

We think this matters for several reasons. At the level of theory, these inadequate conceptualizations of the subject are a contributory factor in criminology’s persistent failure to explain, convincingly, very much about the causes of crime. (There are other obvious reasons, such as the slightly absurd attempt by some to produce a general theory about something as diverse and context-bound as crime, but this only strengthens our general point.) This failure presents us with our primary objective, namely, to begin to rectify this situation. By replacing the caricatured subjects of criminological theorizing with recognizably ‘real’ (internally complex, socially situated) subjects and then examining particular cases in some detail, we hope to contribute to understanding the causes of particular crimes. Additionally, we believe that a more adequately theorized psychosocial subject can help us think more productively about other concerns within criminology: debates about victimization (in Chapter 5) and about particular kinds of punishments (in Chapters 10 and 11).

The failure to say something convincing about the causes of crime matters also at the level of student interest. What often intrigues many criminology students, especially those who are also studying psychology, is the question of motive: ‘why did

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he/she/they “do” it?”, especially if the crime is particularly unusual, extreme or bizarre. Indeed, the widespread popular interest in ‘true crime’ stories tells us that the fascination with unusual crimes extends far beyond the academy. One response to this interest is to see it as somehow unhealthy voyeuristic (which it may be, of course) and to redirect students to the ‘real’ concerns of criminology, which tends to mean matters to do with control and criminalization. It is certainly part of our job as criminologists to show students that criminology embraces questions of control and criminalization as well as crime, and that crime routinely involves the mundane and the petty rather than the serious and the extraordinary. But, it must surely also be part of our job to address what might colloquially be called ‘questions of criminality’: why it is that particular individuals commit particular crimes, including the very serious and relatively rare sort – *especially* as it is perennially fascinating. Isn’t part of our job to convert voyeurism into a proper understanding?

Part of criminology’s reluctance to stray too far from the routine and the mundane has to do, we think, with an approach to theorizing dominated by the ‘rule’, rather than any exceptions to it. Thus, if most delinquency is commonplace and short-lived – teenage fighting, vandalism, shoplifting and drunkenness, for example – then a theory that seems to account for much of this – the rule – tends to be seen as serviceable enough for all practical purposes (the serviceability of theory being an endemic concern of a discipline rooted in an eclectic pragmatism and with strong links to practice). Thus, theories of delinquency do not seem to feel the need also to embrace the less common delinquent activities such as teenage paedophilia, serial rape and extreme violence, for example. These can be sidelined as ‘exceptions’ – which, in practice, tends to mean largely left to the discourses of pathology (psychopath, sociopath, antisocial personality disorder) to ‘explain’. So, part of our interest in some of the more extreme crimes has to do with our feeling that theory must be able to encompass the exception as well as the rule, an approach to theorizing that relates to our commitment to the case study.

Ontologically speaking, our interest in explaining exceptional crimes stems from a view that all crime, including the most apparently bizarre, is normal in the sense that it can be understood in relation to the same psychosocial processes that affect us all – much in the way that Freud saw mental illness. We are all more or less neurotic and life, given certain psychosocial exigencies, can make psychotics of any one of us. This does not obviate the need for understanding, but it does require that we do so using understandings of psychic life and of the social world – and their interrelationship – that are applicable to all: pacifist church-goer as well as multiple murderer. This should humanize the criminal, however awful his or her deeds, and rescue him or her from the uncomprehending condescension of pathologizing discourses and the exclusionary practices these tend to promote. Which brings us to a further, political, reason why criminology’s failure to produce recognizable subjects plausibly committing particular crimes matters: those we do not understand we can more readily demonize, thus enabling ‘folk devils and moral panics’ to continue to figure prominently in the contemporary politics of law and order. The current demonization of the ‘asylum seeker’ and ‘terrorist’ may make some feel better. But, in moving away

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from a better understanding of those whom it scapegoats, demonizing discourses make the world a more fearful place than it is already. Since such scapegoating tends to be directed at the least powerful groups in society these demonizing discourses also make the world a less just place than it is already, demonstrating that tackling questions of criminality and motivation is not unrelated to questions of control and criminalization.

Finally, our failure as criminologists to take seriously questions of individual subjects, as offenders or victims, will allow psychology – the long-time, disciplinary ‘poor relation’ in the criminological project – to reclaim some territory. The renewed interest shown by psychologists in criminology, evidenced by the emergence of textbooks (McGuire, 2004) and articles (Hollin, 2002) dedicated to psychological criminology, and the growth of university courses in forensic psychology, suggests this is already happening. Although it would no longer be fair to characterize all of this work as having a traditional ‘over-emphasis on the individual’ (McGuire, 2004: 1), it is still the case that the conception of the offender remains, in our terms, inadequate. When both criminologists and psychologists fail to explain particular crimes adequately, only the writer/journalists are left to plug the gap. Given that they are usually untrained in the social sciences, however interesting and thoughtful their work – and much good work on particular crimes, especially on murder, has stemmed from writer/journalists (e.g. Burn, 1984, 1998; Gilmore, 1994; Mailer, 1979; Masters, 1985, 1993; Morrison, 1998; Sereny, 1995, 1999; Smith, D. J., 1995; Smith, J., 1993) this is hardly a satisfactory state of affairs (a point we return to below, in the section ‘Why case studies?’).

So, this is a book that demonstrates the inadequacies of the presumed subject within some of the main theoretical approaches to explaining crime within criminology; then shows, through a series of case studies, how particular, relevant approaches fail to make adequate sense of a crime, victimization or particular punishment and how the use of an appropriately theorized psychosocial subject can better illuminate matters. This procedure makes two things crucial: the nature of our psychosocial subject and our reliance on case studies. We need, therefore, to say something here briefly in justification of both.

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**What is a psychosocial subject?**

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As any cursory literature search shows, ‘[Q]uite often ... the term “psychosocial” is used to refer to relatively conventional articles dealing with social adjustment or interpersonal relations, for example’ (Frosh, 2003: 1547). In the specific case of criminology, it tends to be used to describe an atheoretical combination of psychological and social measures – understood as ‘variables’ or ‘risk’ and ‘protective factors’ – to differentiate delinquents from non-delinquents, for example. Sir Cyril Burt’s *The Young Delinquent*, published in 1925 and often regarded as ‘the first major work of modern British criminology’ (Garland, 2002: 37), explained delinquency as the outcome of a plethora of psychosocial factors: ‘typically as many as nine or ten – operating at once upon a single individual’ (ibid: 38). Nearly 60 years later, Rutter and Giller

(1983: 219), after a comprehensive review of empirical research on juvenile delinquency originally undertaken for the Home Office and the DHSS, concluded that 'a wide range of psycho-social variables are associated with delinquency'. These variables ('family characteristics', 'films and television', 'the judicial response', 'school influences', 'area', 'the physical environment' and 'social change') were each then briefly discussed in terms of what was then known (usually not much, it seemed) about the impact of each and their relative strengths as causative factors.

This is not our meaning of the term psychosocial. Rather, our understanding is in line with Stephen Frosh's: 'a brand of "psychosocial studies" that adopts a critical attitude towards psychology as a whole, yet remains rooted in an attempt to theorise the "psychological subject"' (Frosh, 2003: 1545). Most of the initiatives have emerged, Frosh notes, 'primarily from disciplines that lie in a critical relationship with mainstream academic psychology – sociology and psychoanalysis, with applications such as social work and cultural studies' (ibid: 1549). Although these initiatives differ in precisely how they understand the psychosocial, they share several features that distinguish their approaches from conventional attempts interested only in identifying a range of unproblematically conceptualized psychosocial factors. The first is the need to understand human subjects as, simultaneously, the products of their own unique psychic worlds *and* a shared social world. This is not an easy notion to conceptualize. At one point Frosh talks of the psychosocial as being 'a seamless entity ... a space in which notions that are conventionally distinguished – "individual" and "society" being the main ones – are instead thought of together, as intimately connected or possibly even the same thing' (ibid: 1547). Elsewhere, he spells this out more specifically when he talks of the subject being 'both a centre of agency and action (a language-user, for example) and the subject *of* (or subjected *to*) forces operating from elsewhere – whether that be the "crown", the state, gender, "race" and class, or the unconscious ... it is ... a site, in which there are criss-crossing lines of force, and out of which that precious feature of human existence, subjectivity, emerges' (ibid: 1549, emphasis in original). The important point is how to hang on to both the psyche and the social, but without collapsing the one into the other.

Conceptualizing this psychosocial subject non-reductively implies that the complexities of both the inner and the outer world are taken seriously. Taking the social world seriously means thinking about questions to do with structure, power and discourse in such a way that 'the socially constructed subject can be theorized as more than just a "dupe" of ideology; that is, ... [as] more than the social conditions which give rise to them' (ibid: 1552). Taking the inner world seriously involves an engagement with contemporary psychoanalytic theorizing because only there, in our view, are unconscious as well as conscious processes, and the resulting conflicts and contradictions among reason, anxiety and desire, subjected to any sustained, critical attention. Crucial to linking the psychoanalytic subject to the social domain of structured power and discourse is 'the psychoanalytic concept of "fantasy" because "fantasy is not "just" something that occupies an internal space as a kind of mediation of reality, but ... it also has material effects' (ibid: 1554). Here Frosh's use of the term 'fantasy' incorporates not only the wildly outlandish – our more bizarre sexual

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fantasies, for example – but also ordinary people’s everyday imaginings and ruminations. To illustrate, think of how we sometimes imagine quarrels with those who have upset us in our heads (in internal space) without actually verbalizing our disquiet, and then how this fantasy quarrel can influence how we later relate to that same person when we next speak to them (in external space), even though we think we have forgotten all about it (Segal, 2000). What psychoanalysis teaches us is that people’s feelings about and investments in particular experiences and everyday activities are directed by this kind of fantasising. In other words,

the social is [always] psychically invested and the psychological is socially formed, neither has an essence apart from the other. Just as we need a theory of how ‘otherness’ enters what is usually taken as the ‘self’, so we need concepts which will address the ways in which what is ‘subjective’ is also found out there.

(Frosh, 2003: 1555)

One final point about how our notion of the psychosocial differs from that of the conventional: if we take the psychosocial seriously in the way just suggested, this necessarily reduces the utility of cross-sectional studies, where factors abstracted from context and person are analysed statistically to assess their correlative impact (which is so easily assumed to be causative). This reduction in the usefulness of cross-sectional studies is because the psychosocial in our sense is ‘*always social*’ (ibid: 1551; emphasis in original) and *always biographical*. Therefore, to abstract psychosocial factors from particular biographies in order to conduct a cross-sectional analysis is to denude the factors of any real meaning since such factors only operate in the way that they do in the context of a particular life; within another life with its own peculiar psychosocial contingencies, their meanings inevitably differ somewhat. It is this feature of the psychosocial that makes our attention to individual case studies not simply an idiosyncratic preference but, as we argue in the next section, theoretically important too.

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**Why case studies?**

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As with the recent emergence of interest in psychosocial studies, so, too, there would appear to be a burgeoning interest in case study methodology, as the annotated bibliography at the end of the excellent anthology, *Case Study Method* edited by Gomm, Hammersley and Foster, attests. Although there is a sense in which all research can be called case study work because ‘there is always some unit, or set of units, in relation to which data are collected and/or analysed’ (Gomm et al., 2000: 2), usually the term has a more restricted meaning. In contrast to the experiment or the social survey (two influential forms of modern social research) Hammersley and Gomm (2000: 4) suggest several defining characteristics of case study research. Broadly, a lot of information is collected about only a few ‘naturally occurring cases’, ‘sometimes just one’, which is

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usually analysed qualitatively. Given our interest in the complex processes involved in thinking about the psychosocial subject, a commitment to the case study as a method of demonstration/explication seems not only appropriate but probably unavoidable. Despite their own rather different methods, Rutter and Giller endorse this. In their psychosocial overview of the 'causes and correlates' of delinquency referred to earlier, they spell out 'the family characteristics most strongly associated with delinquency', namely, 'parental criminality, ineffective supervision and discipline, familial discord and disharmony, weak parent-child relationships, large family size, and psycho-social disadvantage' (Rutter and Giller, 1983: 219). They go on to say:

Less is known about the precise mechanisms by which these family variables have their effects, but recent observational studies of interaction in the home offer promise of progress on this question. More research of that type [i.e. case studies] is required.

(ibid)

If we *need* detailed observational case studies of everyday interactions of real families to make sense of family factors in delinquency, why has so much criminological time and money been spent reproducing lists of factors that seem to have altered little from the time of Burt's psychosocial enquiries?

Those wedded to the notion that there is a 'normal' or 'rational' subject that can best be grasped through decontextualized aggregation often answer this question defensively, asserting that nothing, aside from conjecture, can be gleaned from the analysis of single cases. Yet, for advocates of the case study approach, it is the working through of the entirety and complexity of the data, as it applies in very particular contexts, that enables 'how?' and 'why?' questions to be adequately broached. Inevitably, views on what case studies can contribute differ even amongst those who advocate case study methods. Stake, for example, (2000: 22) advocates the case study approach as a means of ascertaining 'a full and thorough knowledge of the particular'. Such knowledge can provide the basis for what Stake calls 'naturalistic generalization', by which he means that, armed with such knowledge, the reader is in a position to make generalizations based on their own experiences. Is this a good enough answer? Is an experientially-based recognition of the applicability of a particular case study 'in new and foreign contexts' (ibid) what we find valuable in single cases and therefore what we hope to achieve with our chosen examples? Although we are not against the thoughtful use of experience as a basis for generalization, we are mindful of the fact that, given our notion of a conflicted psychosocial subject whose self-knowledge is always less than complete – indeed who is often motivated to defend against painful self-knowledge – experience can deceive as well as inform. In other words, experience is never transparent (or natural) but always subject to interpretative work. For us, then, Stake's idea that the purpose of the case study method is to facilitate naturalistic generalization is insufficient.

An alternative account of the use of case studies can be found in Lincoln and Guba (2000). From a similar epistemological starting point to Stake, Lincoln and Guba

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argue that generalization is intrinsically reductive. However, they would prefer 'a new formulation proposed by Cronbach (1975): *the working hypothesis*' (p. 38, emphasis in original) to the idea of generalization. Does the change in terminology alter matters much? Lincoln and Guba go on to suggest that the basis of '*transferability*' across contexts 'is a direct function of the *similarity* between the two contexts, what we shall call *fittingness*' (ibid: 40, emphases in original), a concept they go on to define as 'the degree of congruence between sending and receiving contexts' (ibid). How is such 'fittingness' to be judged? To make an informed judgement about this we will need an 'appropriate base of information' (ibid) about both contexts. By this they mean, following Geertz (1973), a 'thick description' of both. Whilst such an approach is more systematic than a reliance on 'naturalistic' experience alone, it remains wedded to the empirical since the similarity between contexts is said to be an empirical issue. Given our focus on interpretation, it is hard to see how 'fittingness' can be arrived at entirely empirically.

Lincoln and Guba also follow up Schwartz and Ogilvy's (1979) suggestion that, as Lincoln and Guba (2000: 41) put it, 'the metaphor for the world is changing from the machine to the hologram'. This enables them to suggest that it matters little where we start or what we sample because 'full information about a whole is stored in its parts' (ibid: 43), a notion not dissimilar to the psychological idea of Gestalt (Hollway and Jefferson, 2000: 68). It also brings to mind the psychoanalytic idea, with which we have much sympathy, that symptoms, 'free associations', slips of the tongue, dreams, etc. reveal more about the 'whole' (person) than is apparent at first blush. But, and here is the crucial point, the information contained in the part is not self-revelatory: we have to know how to interpret it (ibid: 43). And, just as the psychoanalytic interpretation of dreams, etc. is reliant on an elaborate theoretical edifice, so too will any 'part' need theoretical assistance, an interpretative schema, before it can be used to illuminate the 'whole'.

Now we are in a position to offer our answer to the question: what is it we can learn from a single case study? The nub of the answer is that cases assist theory-building. Indeed, they are essential to it. All theories need testing to see how well they explain particular cases. When applied to a new case, the theory may be confirmed, or only partially, in which situation the unexplained parts of the case act as a stimulus to refining or developing the theory. Mitchell (2000: 170) makes the point very strongly: 'A case study is essentially heuristic; it reflects in the events portrayed features which may be construed as a manifestation of some general, abstract theoretical principle'. However, it is important to distinguish this idea from that of typicality. For, when people ask, 'what is it we can learn from a single case?', what they often mean is 'how typical is it?'. This implies that 'atypical' cases can reveal nothing of value. Mitchell's response to this is to argue that to ask such a question of a case study is to be guilty of confusing two different inferential processes: the statistical one, which is aimed at answering the question of how representative, or typical, is the phenomenon under study, and the very different theoretical one, which is aimed at uncovering logical or causal connections. What this means in terms of generalization is that a case is generalizable to the extent that the new case confirms the theoretical

framework informing the analysis; the empirical question of how often this will happen is simply not something case studies are designed to answer.

Several things follow from this. First, the atypical case is as useful as any other providing that 'the theoretical base is sufficiently well-developed to enable the analyst to identify within these events the operation of the general principles incorporated in the theory' (ibid: 180). This relates back to our earlier point about our intended use of 'exceptional' cases and is, hopefully, now properly justified. Second, the theoretical purpose of the case studies will inform the interpretation, which will mean, inevitably, some loss of the case's complexity. In particular, this leads to some simplification of the case's context. However, Mitchell (ibid: 182) draws on the earlier work of Gluckman (1964) to make the point that this 'is perfectly justified ... provided that the impact of the features of that context on the events being considered in the analysis is incorporated rigorously into the analysis'. Third, and following on from this, whatever contextual features are suppressed in the interests of the theoretical analysis (and brevity), it is vital 'to provide readers with a minimal account of the context to enable them to judge for themselves the validity of treating other things as equal in that instance' (ibid).

We started this section with Stake's idea that the purpose of a case study is to provide 'a full and thorough knowledge of the particular' (2000: 22). In the light of our journey through this section, we can now be more specific about our relationship to this notion. Basically, we see such particularistic versions of case studies, which for us mean detailed, *descriptive* accounts ('thick descriptions') of particular events, as our starting point. Thus, for example, we often use journalistic accounts of particular crimes, which may indeed be book-length accounts, as the basis, or part of the bases, of our own case studies. But what we will be interested in is how such particular cases, each in their different ways, manifest, to use Mitchell's phrase again, the 'general, abstract theoretical principle', of, in our case, psychosocial subjectivity and how without such a principle the cases cannot be fully understood. In presenting our cases we hope to abide by the strictures outlined in the previous paragraph, namely, to try to ensure that any reductions in complexity and decontextualization are both theoretically justified and visible to the reader. At the end of our endeavours, what we hope to have demonstrated is something of the generalizability of our concept of the psychosocial subject.

Before leaving this chapter we have one final task, namely, to say something about how criminology, which started out being very interested in the criminal subject, has become almost indifferent to the topic. More precisely, in deference to those criminologists who have shown theoretical interest in offenders, why has criminology been so little affected by the new psychosocial developments of the past 20 years?

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### What happened to the criminal subject?

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An implicit answer to this question, as applied to the British context, is to be found in David Garland's (2002) historiography of criminology's 'governmental' and 'Lombrosian' projects. What Garland (ibid: 8) has in mind in talking of 'the governmental project' is

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the long series of empirical inquiries, which, since the eighteenth century, have sought to enhance the efficient and equitable administration of justice by charting the patterns of crime and monitoring the practice of police and prisons ... [It] exerts the pragmatic force of a policy-oriented, administrative project, seeking to use science in the service of management and control.

His idea of 'the Lombrosian project' is very different. This

refers to a form of inquiry which aims to develop an etiological, explanatory science, based on the premise that criminals can somehow be scientifically differentiated from non-criminals ... [It is] an ambitious ... (and, ... deeply flawed) theoretical project seeking to build a science of causes.

(ibid)

What happened to the originally continentally-based Lombrosian project within the UK is that it overlapped with, or found echoes in, 'a new quasi-medical specialism which ... came to be known as psychological medicine or psychiatry' (ibid: 22) focussed on bio-psychological explanations of insanity and the late nineteenth and early twentieth century penal and forensic psychiatric work of psychiatrists and prison doctors who were diagnosing, classifying and treating criminals. Indeed, Garland suggests that the psychiatrist Henry Maudsley and the prison doctor J. Bruce Thomson were "Lombrosian" before Lombroso' having 'written about "the genuine criminal" and "the criminal class"' as early as the 1860s. However, the general thrust of his argument is that the British approach – concerned with therapy and with classifying mental disorders not criminal types; practically connected to the criminal justice system – softened Lombroso's idea of the criminal as a natural type with the result that the interfusion of the two projects produced 'a scientific movement which was much more eclectic and much more "practical" than the original criminal anthropology had been' (ibid: 26). The resulting 'new science of criminology' (ibid: 26) was, thus, also more acceptable to the British project.

By the 1920s and 1930s, 'clinically-based psychiatric studies' (ibid: 35) undertaken in the service of treatment and prevention, constituted the criminological mainstream in Britain. Typifying this work was that of W. Norwood East, 'a psychiatrically trained prison medical officer', who, according to Garland, was highly influential despite his 'subsequent neglect' (ibid: 34). Although 'a proponent of a psychological approach to crime' (ibid) and the co-author of a report (with Hubert) 'on *The Psychological Treatment of Crime* (1939)' (ibid: 35), he 'consistently warned against the dangers and absurdities of exaggerating its claims' (ibid), was more interested in practice than 'theoretical speculation' (ibid), and thought that '80 per cent of offenders were psychologically normal' and therefore required 'routine punishment', not 'psychological treatment' (ibid). Although East himself was hostile to psychoanalysis, others were not. Maurice Hamblin Smith, for example, 'Britain's first authorized teacher of "criminology", and ... the first individual to use the title of "criminologist"' (ibid: 33). He wrote a book called *The Psychology of the Criminal* (1922) and 'was ... one of the first criminological workers in

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Britain to profess an interest in psychoanalysis ... to assess the personality of offenders [and] ... for treating the mental conflicts which, he claimed, lay behind the criminal act' (ibid). However, despite some important institutional developments stemming from this interest in psychoanalysis – the Tavistock (1921) and Maudsley (1923) clinics, 'new child guidance centres' (ibid: 34), the Institute for the Scientific Treatment of Delinquency (ISTD) (1932) and its 'own Psychopathic Clinic [1933]' (later to become, in 1937, the Portman Clinic), *The British Journal of Delinquency* (1950) (since 1960, *The British Journal of Criminology*) – psychoanalysis remained 'an important tributary' (ibid: 37) rather than mainstream.

A very different type of criminology was opened up by the publication, in 1913, of Charles Goring's (1913) *The English Convict*. Sponsored by the Home Office and the Prison Commission and based upon a large sample and statistical measurement, and a starting assumption that crime was normal (i.e. common to all, the difference between 'men' being one of degree not a difference of type), it purported to refute 'the old Lombrosian claim that criminals exhibited a particular physical type', only to invent 'a quite new way of differentiating criminals from non-criminals' (ibid: 35). After finding 'a significant ... association between criminality and two heritable characteristics, namely low intelligence and poor physique', but no close association between 'family and other environmental conditions' and crime, Goring went on to draw 'a series of practical, eugenic conclusions': criminals were 'unfit' and their propagation should be strictly regulated. As Garland suggests (ibid: 36), this effectively brought back Lombroso 'in some new, revised form' (ibid: 36). Although Goring's eugenicist conclusions seemed to undercut the possibility of reform, and were rejected by the Prison Commissioners, the statistical analysis of mass data as a form of criminological research gradually overtook the clinically-based psychiatric study as the preferred form of government-sponsored research, especially after World War II.

Somewhere between the two was a third stream 'best represented by the eclectic, multi-factorial, social-psychological research of Cyril Burt' (ibid: 37), especially his 1925 study of *The Young Delinquent*, which was seen by 'later criminologists such as Mannheim and Radzinowicz ... as the first major work of modern British criminology' (ibid). This study

was based upon the detailed clinical examination of 400 schoolchildren (a delinquent or quasi-delinquent group and a control group), using a battery of techniques that included biometric measurement, mental testing, temperament testing, and psychoanalytic and social inquiries, together with the most up-to-date statistical methods of factor analysis and correlation. Its findings were expansively eclectic, identifying some 170 causative factors that were in some way associated with delinquency, and showing, by way of narrative case histories, how each factor might typically operate. From his analysis, Burt concluded that certain factors, such as defective discipline, defective family relationships, and particular types of temperament, were highly correlated with delinquency, while the influence of other factors, such as poverty or low intelligence ... had been seriously overstated in the past.

(ibid: 37–8)

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This early period of British criminology – ‘between the 1890s and the Second World War’ (ibid: 38) – whether clinical, statistical or eclectic was the period of psychology’s dominance of the developing field of knowledge. The focus was on individual offenders and their treatment and Durkheim’s pioneering sociological work in France, like that of the new Chicago School of Sociology, ‘was virtually absent’ (ibid). Rather, ‘the “social dimension” of crime was conceived as one factor among many others operating upon the individual’ (ibid). Garland sees this take-up of sociology as ‘a good example of how the criminological project transforms the elements which it “borrows” from other disciplines’ (ibid); more charitably, from our perspective, it might be seen as an early, albeit inadequate, attempt to hang on to both the psychological and the sociological dimensions: to be, in our terms, psychosocial. Be that as it may, the crucial point for understanding how this early interest in the criminal subject all but disappeared in the later part of the twentieth century can be traced to the dominance at this stage of ‘the governmental project’. As Garland makes clear:

The governmental project dominated almost to the point of monopolization, and Lombroso’s science of the criminal was taken up only in so far as it could be shown to be directly relevant to the governance of crime and criminals.

(ibid)

This domination of the governmental project was to cast its shadow on all subsequent developments in the new discipline right up until the radical challenges of the 1960s and 1970s, spearheaded by the formation of the National Deviancy Conference (NDC) in 1968 and the publication in 1973 of Taylor, Walton and Young’s *The New Criminology*. This was broadly true of the institution-building that took place between the 1930s and 1950s, during which time criminology-teaching in universities expanded: the ISTD and its specialist journal was established; and the first British criminology textbook (Jones, 1956) appeared. It is even more evident with the advent ‘in the late 1950s’ of government ‘support and funding’ (ibid: 39). Garland sees this development – crucially, the establishment of the government-funded Home Office Research Unit in 1957 and the Cambridge Institute of Criminology in 1959 as ‘a key moment in the creation of a viable, independent discipline of criminology in Britain’ (ibid: 40). This is because such governmental commitment ‘to support criminological research, both as an in-house activity and as a university-based specialism ... marked the point of convergence between criminology as an administrative aid and criminology as a scientific undertaking – the consolidation of the governmental and Lombrosian projects’ (ibid). From our point of view, the importance of this convergence is twofold: first, it spelt the death of any serious interest in questions of aetiology and, second, it ensured that when the challenge from sociology eventually came, in the 1960s, the aim was to overthrow the dominant governmental project – specifically its narrow, positivistic, policy-led, correctionalist focus – in the light of the new US-led developments in sociology and the revival of interest in a revisionist Marxism. It was not interested in revamping the Lombrosian project. Let us take both of these points in turn.

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Garland notes the divergence that emerged within the Scientific Group for the Discussion of Delinquency (first established in 1953 under the auspices of the ISTD) between the older, clinically-minded members and some of the younger ones who 'grew dissatisfied with the clinical and psychoanalytical emphasis of leading (if controversial) figures such as Glover and split off to found the more academically oriented British Society of Criminology' (ibid: 39–40). The ISTD itself, with its commitment to psychoanalysis and 'open hostility to much official penal policy ... remained essentially an outsider body' (ibid: 34), which left it out of the running when it came to the establishment of the first funded university-based Institute of Criminology. The 1959 White Paper which led to the establishment of the Cambridge Institute of Criminology was quite explicit in its rejection of aetiological research – because the problems involved were too complex, answers would not be easy to come by and "progress is bound to be slow" – and its espousal of "research into the uses of various forms of treatment and the measurement of their results, since this is concerned with matters that can be analysed more precisely" (Home Office 1959: 5) (quoted in Garland, 2002: 43). Unsurprisingly perhaps, this emphasis matched that of Leon Radzinowicz, the Institute's first director. He 'argued in 1961' that "the attempt to elucidate the causes of crime should be put aside" in favour of more modest, descriptive studies which indicate the kinds of factors and circumstances with which offending is associated' (quoted in Garland, 2002: 43). This preference for 'modest descriptive studies' was indeed borne out in practice. As Garland says, 'the prediction research that claimed so much attention in the late 1950s ... made little use of clinical information about the offender, and actually discredited to some extent the whole project of etiological research' (ibid: 40). Thus the aetiological project was undermined, from within: as the younger ISTD members, some influenced by the more sociological teachings of Herman Mannheim at the London School of Economics, turned away from clinical and psychoanalytic work; and from without: as government sponsorship put its support (and funding) behind a pragmatic, policy-driven correctionalism.

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**Why no critical criminological subject?**

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Opposition to this 'administrative criminology', as it would later be called, was immediate: from 'the psychoanalysts at the ISTD', on the one hand, 'and the group of sociological criminologists that was forming around Mannheim at the LSE' (ibid: 43), on the other. But, it was not until sociology as a university discipline had firmly taken root (massively, a post-World War II phenomenon) that a generation of criminologists would emerge, radicalized by the many critical currents of the 1960s, able to mount a successful challenge to the dominance of traditional, administrative criminology. Not that this new criminology ever supplanted the old. Indeed, the questions asked and the theoretical frameworks deployed were largely very different. Thus, for a while at least, there were two criminologies – old and new; administrative and critical – existing side by side. Those interested in the details of criminology in

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the 1960s and 1970s should consult Cohen (1981), who picks up the story where Garland finishes. For our purposes, this turn to sociology had many strands: the importance of labelling and social reaction; the politics of crime and crime control; and ethnographic studies of deviants and delinquents designed to 'appreciate' – understand from the inside – their subjective, life-worlds. These latter studies, influenced by symbolic interactionism and the social psychology of George Herbert Mead ([1934] 1967), were where a fresh and properly psychosocial interest in questions of aetiology might have developed. But the move was the other way as symbolic interactionism came under attack from a renewed Marxism. The result was a certain shifting of attention: from micro to macro concerns; from empirically-based ethnographic studies to theoretically-driven political analyses in which questions of structure and history, not individual biographies, loomed large; from the 'sociology of deviance' to the relations among crime, law and the state. The challenge of feminism, with its concern to establish that 'the personal is political', might have been another opportunity to resuscitate interest in subjectivity, but its main efforts were directed elsewhere: towards establishing the structural or discursive importance of gender rather than its subjective significance.

If the main enemy for the sociologically-inspired new deviancy theorists of the late 1960s and 1970s was the governmental project of administrative criminology and its offender-based focus, this goes a long way in explaining their lack of theoretical interest in understanding criminal subjects. But, this is not the whole story. It is also relevant that the emergence of a critical psychology and a feminist-inspired renewal of interest in psychoanalysis came later than the emergence of a critical sociology and was much less far-reaching. In other words, whereas Cohen could talk of 'a whole range of [critical] sociological connections' at the end of the 1970s 'for students of crime and deviance: 'Education', 'Mass media', 'Cultural studies', 'Medicine and psychiatry', 'Law', 'Social policy and welfare' (Cohen, 1981: 238–9) – the same could not be said about critical psychological connections. Thus, one reason for not taking the criminal subject seriously in the 1960s and 1970s was the absence of any adequate (new, critical) theoretical tools from within psychology for so doing.

The rise of Thatcherism in the 1980s and the subsequent upheavals in the funding and administration of higher education in general and the social sciences in particular, as well as more general developments like the collapse of Communism and the inevitable crisis of Marxism that accompanied it, led to further reconfigurings of the intellectual landscape. In broad terms, critical criminologists found themselves pushed, pulled and persuaded towards more policy-relevant research, while policy-relevant research was having to take at least some cognizance of the radical agenda. In other words, the 1980s and beyond have seen some convergence between administrative and critical criminology. The demise of the NDC and the resuscitation of a single body, the British Society of Criminology, with its well-attended Annual Conference acting as an umbrella body for all types of criminology, is some indication of this rapprochement. Illustrative of this new convergence, and particularly relevant for our purposes, was the (short-lived but influential) emergence of 'new' or 'left realism'. Developed from critical criminology, with one of the authors of *The New Criminology*, Jock Young, a leading proponent, its theoretical

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starting point was the idea that the crisis of criminology was its failure to take aetiology seriously. Coming from someone with impeccable radical credentials, and given that aetiological questions had ceased to interest even the traditionalists, this was a surprising claim. According to Young, this mattered because during the 1970s crime was continuing to rise despite rising incomes, a fact that ran counter to all criminological assumptions about the links between deprivation/disadvantage and crime. Additionally, there was widespread pessimism about treatment: 'nothing', apparently, 'worked' and rehabilitation had ceased to be a goal of Her Majesty's Prison Service. Most alarmingly, neither critical criminology (now referred to as 'left idealism' in contrast to its new 'realist' incarnation) nor what Young called the 'new administrative criminology' (presumably to distinguish it from the old version with its vestigial interest in aetiology, and exemplified by situational crime prevention) had any interest in aetiology.

Young's solution to this crisis was to argue for the development of a:

realist theory of crime which adequately encompasses the scope of the criminal act. That is, it must deal with both macro and micro levels, with the causes of criminal action and social reaction, and with the triangular inter-relationship between offender, victim and the state.

([1986] 2003: 323–4)

Confusingly, Young later went on to talk about a 'square' of crime with 'the public' being the additional element (Young, 1997: 485–6). But, more interestingly for us, 'the criminal act', which obviously entails some attention to the criminal subject, disappeared from view in the actual 'new realist' research that was undertaken. Essentially, these researches consisted of local victimization studies, modelled on the British Crime Survey but with certain changes designed, for example, to improve the returns for sexual offences. In other words, the victim was fairly exhaustively, if conventionally, researched while the state and the offender remain unexamined (except as they manifest in the answers of victims). Here, then, 'taking crime seriously', the project's political starting point, reduced to a now standard element of the governmental project, namely taking victims seriously; and the much-trumpeted aetiological question remained, as before, unaddressed. This expressed concern combined with a practical failure to do anything about it echoes a similar failure in an earlier text co-authored by Young, namely *The New Criminology*. In the final chapter of that book, Young and his collaborators explained the need, for 'a fully social theory of deviance' (Taylor, Walton and Young, 1973: 269), to deal with action and reaction, and to do so at three levels, the actual act (the level of social dynamics), immediate origins (the level of social psychology) and the wider origins (the level of political economy). Yet, 30 years on, realist criminology has provided no real assistance for thinking about the relations among the levels and nothing of substance that might be deemed social psychological. Perhaps, in the light of this brief, schematic history of criminology, it is possible to glimpse why.