Kill Method: A Provocation

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As criminologists we face two contemporary crises. The first is the unfolding crisis of global capitalism and state governance, and with it the spiraling social harms of dislocation, incarceration, impoverishment, and environmental degradation. Amidst these spiraling harms will surely emerge, sadly, a further host of phenomena demanding the critical attention of criminologists: new forms of acquisitive violence, new crimes attuned to economic and existential uncertainty, new moments of down-market corporate malfeasance, new strains on social and environmental sustainability, and new patterns of state surveillance and control. Perhaps this crisis holds the promise of progressive change—but if Marx and Merton were even half right, it most certainly contains the sorts of contradictions out of which new forms of crime and predation will emerge.

The second crisis is the crisis of criminology. Criminology is today crippled by its own methodology, its potential for analysis and critique lost within a welter of survey forms, data sets, and statistical manipulations. Worse, criminology has given itself over to a fetishism of these methodologies. Methods such as these are not only widely and uncritically utilized by contemporary criminologists—they are detailed and reified to the point that, for many criminologists, they have now replaced crime and crime control as the *de facto* subject matter of the discipline. The crisis of criminology doubles back on itself; criminology first embraces methods wholly inadequate and inappropriate for the study of human affairs, and then makes these methods its message.

This second crisis precludes criminology's progressive engagement with the first. Over the past few decades surveys, statistics, and other 'objective' methodologies have increasingly served to couple criminology to 'criminal justice' as both pseudo-discipline and state practice. Made the adjunct of criminal justice, criminology not only colludes in 'policing the crisis' and propping up the very institutions that underlie the crisis itself; criminology also finds itself pulled away from critical theory and into the realms of practical crime control, risk measurement, and data management. This trajectory in turn renders most criminological research impenetrable—not to mention off-putting and unusable—to everyday citizens, street-level progressive groups, young political activists, and others who might enlist criminology's aid in confronting the contemporary global crisis. Married to the criminal justice complex, divorced from the nuanced politics of everyday life, criminology narrows its view at the very moment that broader, critical criminological engagement is most needed.

As Rome burned, Nero fiddled. As our world conflagrates, criminology calculates.

These intertwined crises—the crisis of global politics, crime, and economy, and the crisis of criminology's methodological inability to engage this global situation critically—might be addressed in any number of ways. Seminars in advanced statistics or survey construction might be summarily emptied out, their participants sent out to establish urban gardens or no-cost daycare programs. Criminology as a discipline might be declared a failure and a fraud, with its graduate programs and publications reinvented as art, or history, or performance studies—or, if present orientations are maintained,

actuarial science. Alternatively, criminology could be continued as a discipline, but under this ongoing disciplinary cover its scholars could begin holding seminars in revolutionary political practice, do-it-yourself media operations, and economic self-sufficiency. Dangerous times, after all, require dangerous thinking.

Here, though, I offer a different sort of proposal and provocation for saving criminology, and for promoting its critical engagement with the current world crisis.

Kill method.

The Fetishism of the Methodology

'Fetishism' generally suggests two sorts of relationships between meaning and the material world. The first is the attribution of animating powers to an inanimate object, such that the object itself is seen to embody what otherwise might be understood as larger forces of human action or cultural practice. Second, and relatedly, is the notion of fetishism as a sort of unnatural preoccupation with some small dimension of a larger totality. For the anthropologist, then, fetishism can be investigated as a form of religious mysticism whereby various groups imbue fetish objects with spiritual powers. For Marxists, the 'fetishism of the commodity' implies not so much mysticism as mystification—an essential capitalist conceit where commodities are imagined to embody value in such a way that the creation of this value through human labor is forgotten. For the sexual fetishist, the toe or the earlobe emerges as the object of affection, a focused substitute for the broader dynamics of sexuality and allure.

The methodological culture of contemporary criminology operates in just this way. Orthodox criminologists imagine that survey research and statistical analysis are somehow mystically imbued with the power of 'objectivity', that they embody the spirit of scientific inquiry, mathematical precision, and dispassionate analysis. They imagine that these methods somehow operate independently of human emotion and human action—that such methods can drain objective 'data' and useful knowledge from those who are their targets, can produce results that are valid and 'replicable' no matter the researcher, can expunge 'error' and 'subjectivity' from the research process. And like the sexual fetishist, orthodox criminologists focus so tightly on the minutiae of their methodology, and on the social minutiae that their methods are designed to investigate, that they regularly forget larger dynamics of crime, transgression, knowledge, and power.

This is of course neither the way criminology must be, nor the way it has always been. Historically, many of criminology's foundational works have emerged from idiosyncratic, impressionistic, and 'undisciplined' approaches that bear little resemblance to any sort of formalized methodology (Adler and Adler, 1998). When in the 1920s and 1930s Chicago School scholars conducted research, for example, they did so largely according to their own sentiments and schedules. The research for Frederic Thrasher's (1927: xiii, 79) 571 page book, *The Gang,* 'occupied a period of about seven years', and in the book he not only presents in fine detail his impressions of 'the thrilling street life of the gang', but includes his own *in situ* photos of gang rituals and juvenile gang life. In later decades, researchers associated with the National Deviancy Conference in Great Britain (e.g., Young, 1971, Cohen 1972), and American researchers like Howard Becker (1963) and Ned Polsky (1967), likewise undertook criminological research that remains

at the core of criminology—research that emerged not from pre-set methodologies, but from their own marginal lifestyles and (im)moral predilections.

Still, over the past six decades or so—and increasingly in recent decades—criminology has all but abandoned this tradition of engaged, fluid research for the fetishism of the methodology. This trajectory in part began after World War II, with the influx of governmental money into academia. "Ample funding, entrepreneurial professors and policy-makers thirsting for anything that looked like technical expertise provided a combustible mix," says historian Mark Mazower (2008: 36, 42). "Huge sums of money were suddenly pouring into the universities…. The social scientists who got the grants offered technical advice that simplified the world and made it governable, using behavioral science or mathematical economics models. They turned human affairs into data sets, cultural patterns into forms of behavioral response, and they replaced the messy multiplicity of words and tongues with the universal and quantifiable language of science." Within sociology, Chapoulie (1996:11) adds, "use of statistical instruments and the language of proof of the natural sciences was clearly a way to increase the scientific legitimacy of a discipline fully recognized neither in the university nor outside it.'

For criminology especially, this tendency has accelerated in recent years with the ascension of 'criminal justice', the funding imperatives of the National Institute of Justice and other bureaucracies, and the consequent demand for research that is methodologically marketable, politically fundable, and bureaucratically usable for policy makers and criminal justice agencies. In this criminological environment of rationalized knowledge and pecuniary aspiration, there is little room for research that is impressionistic, innovative, and emergent. In this environment, in fact, there is no room for the

foundational works of criminology. They would be not be funded, not allowed to go forward under Institutional Review Board guidelines, not accepted by journal editors for publication, not valued by tenure and promotion committees. They would instead be dismissed as field reporting, or speculative essay writing, or subjective interpretation, and all because of their lack of, well…methodological rigor.

And so, in place of the vivid ethnographies of Thrasher or Becker, in place of the innovative theorizing of Merton or Sutherland, what does orthodox criminology today offer? Put another way: What sort of discourse is produced by methodological rigor, by a criminology addicted to surveys and data sets and statistics? What are the ambient sounds of methodological fetishism? The answers aren't difficult to find; they're offered up in issue after issue of *Criminology*, the flagship journal of American criminology:

"First, household property crime committed against one's neighbors has an effect size of .132 that is comparable with that of one's own household victimization (.121)" (Xie and McDowall, 2008: 827).

"Recall that a key advantage of the tobit model is that it explicitly deals with the floor-value of the summative delinquency measure…" (Ousey and Wilcox, 2007: 340).

"A regression coefficient for role differentiation, such as $y_{11,}$indicates the increase in the tendency to be an offender versus a victim, with each unit increase in an explanatory variable (X_1), which is expressed as the extent to which log odds of offending exceed

those of victimization, adjusted for base-rate differences across all items" (Schreck, Stewart, and Osgood, 2008: 886).

"Is the poor performance of MM in part because of the lack of fit with the RC association model?" (Wadsworth and Roberts, 2008: 860).

Now it might be argued that these excerpts are here being unfairly made to represent and critique the studies from which they are taken, removed as they are from the larger substantive or theoretical contexts of the research. In reality, though, it is neither the substantive focus nor the theoretical framework that contextualizes this sort of criminology; it is the methodology of data manipulation and quantitative analysis. Statistical analysis drives and defines this research; as the authors make clear, the research stands or falls on measurement and calculation. As exhibited time and again in two-page tables, in elaborate mathematical equations that span the printed page, and in extended methodological expositions vis-à-vis brief 'discussion' or 'conclusions' sections, this sort of criminological research is primarily designed to construct towering statistical edifices—and to build these edifices on the shallowest of data and flimsiest of epistemic foundations.

The delinquency research excerpted above, for example, targeted 9,488 Kentucky seventh grade students, with less than 4000 of them eventually completing the surveys, which in turn offered only limited, pre-set responses to statements like "I talk to my mother" and "I can't seem to stop moving." In addition, the researchers admit that "our sample does seem to under-represent males," but that "without explicit data on the nonresponders, however, we cannot know with any certainty the extent to which they

differ from responders" (Ousey and Wilcox, 2007: 322-323, 351-352). In the 2008 offender/victim study, the researchers acquired from the Sociometrics Corporation data from a decade-old (1994-1996) survey of middle school and high school students. Despite noting the ambiguity of the survey —such that offending and victimization might be confounded within the survey questions themselves (!)—the researchers felt free to recode the original ordinal survey data into a yes/no dichotomy, and to "omit the measure of 'gang fighting' from our list" since "the gang-fighting measure seemed to produce greater confusion than clarity…" (Schreck, Stewart, and Osgood, 2008: 871, 881-2). Not to be outdone, the authors of the 2008 study on household crime/victimization and household mobility utilized decades-old (1980-1985) National Crime Survey data—data which excluded mobile homes, "hotels or motels occupied by transient guests," and "group quarters (such as dormitories or rooming houses) because too few observations are found to support separate analyses" (Xie and McDowall, 2008: 809, 816-17).

This is the information—a school kid's pencil mark in a little response box, a data set acquired from the Sociometrics Corporation, a decades-old survey rife with ambiguity and omission—that researchers then recode and manipulate to reach quantifiable conclusions about crime and crime control. Surveys answered by kids who may or may not understand the questions, who may or may not be willing or able to translate their memories onto coded answer sets, who may or may not represent those other kids not answering the survey; surveys then collated, stored away, and later salvaged for still more recoding and reinterpretation; these recordings then recoded again into charts, tables, and equations—*this* is imagined to constitute *knowledge* of crime and delinquency, and conclusions as to its causes and consequences? How immeasurably far is this

methodology of measurement from the lived experience of crime and victimization, and from criminologists' deep understanding of that experience?

Build thee more stately mansions, oh my soul (Holmes, 1858)—but not from numbers dense as bricks, and not on foundations of epistemic fraud.

At times, the foundations of epistemic fraud are even layered one on top of the other. As administered by the U.S. Department of Justice's Office of Juvenile Justice and Delinquency Prevention (OJJDP), the National Youth Gang Survey allegedly measures the number of gangs and gang members nationwide, and tracks significant trends in youth gang characteristics and activities—and, indeed, official reports based on the survey are suffused with summative tables, charts, figures, and formulae. To acquire this statistical and longitudinal knowledge, though, the OJJDP mails its surveys not to gang members or community researchers, but only to law enforcement agencies. Whoever might be assigned the task of completing the survey at each agency is then instructed to answer survey questions based on 'records or personal knowledge', though it is 'impossible to determine which'—and further, 'no definition [is] presented regarding what constitutes a gang member or a gang incident,' since 'little agreement has been reached on what constitutes a gang, gang member, or gang incident…' (OJJDP, 1999: 7). In this way, the OJJDP's methodology generates official criminological knowledge of a critical crime issue—gangs, gang members, and gang activities—that can perhaps best be summarized as follows: "That which is not to be studied directly can nonetheless be surveyed definitively, based on the records, or perhaps the personal perceptions, of those whose job it is to eradicate that which they cannot define accurately" (Ferrell, Hayward, and Young, 2008: 174).

Of course this is all laughably absurd, unless your job is to believe its not—that is, unless your job hinges on a politically expedient methodological fetish for figures and formulae in place of on-the-ground criminological knowledge. And by the way: What if your job is, in addition, to induce masturbation among college students? In his 2006 Sutherland Address to the American Society of Criminology, Professor of Public Policy and Statistics Daniel Nagin (2007: 265) outlined a recent clinical experiment in which male undergraduates were "instructed to masturbate but not to the point of ejaculation while responding to a series of sex-related questions." Citing this study as 'the type of research on decision making that I believe will illuminate important issues in criminology" and help in "moving choice to center stage in criminological theory and empirical research," Nagin (2007: 262, 266, 269) went on to raise an interesting methodological issue. "Tests of wide-ranging theories," he noted, "often rely on survey data in which people respond in a 'cool', non-aroused state…." Yet the masturbatory experiment "suggests that responses in a cool state to choice relevant considerations, such as moral judgments and social attachments, may provide very poor measurements of that factor in the aroused emotional states that commonly accompany criminal behavior."

Putting aside the question of what Nagin himself might actually know about the "emotional states" of either survey respondents or criminals, his comments nonetheless offer a telling insider critique of criminology's fetishized methodologies. Even a criminologists who is a professor of statistics and author of more than one article on "semiparametric mixed Poisson models," even a criminologist comfortable with the forced artificiality of clinical trials and controlled masturbation, can't help asking a foundational question of the statistically manipulated survey data that orthodox

criminologists use to test theory and to measure crime: What emotional connection do such data have to the reality of crime and victimization? Not only is such 'data' profoundly suspect in terms of clarity, perception, memory, and knowledge–it is likely emanating from the wrong emotional register as well.

Nagin and his clinical masturbators confirm the masturbatory fetish that is orthodox criminological methodology. Against all logic, orthodox criminologists attribute to their survey data and statistical summaries powers that are in fact not present: precision, insight, and objective evaluation. Like other fetishists, they in turn focus on these fetish objects with a sort of unnatural intensity, obsessively turning them over in their minds and in their computers, and so forgetting the world beyond answer sets and data sheets. And indeed all of this is mostly masturbatory—mostly for the pleasure of a small circle of journal editors, tenure committees, and governmental operatives—and mostly unusable in that big and increasingly dangerous world beyond academic careerism and bureaucratic policymaking.

Criminology, and that big world beyond, would be better off without it.

Alternative…Method?

An alternative to all this—an alternative that can perhaps foster criminology's critical engagement with the contemporary world and its crises—has already been glimpsed: the tradition of deeply engaged, impressionistic ethnographic field work, as practiced by early Chicago School researchers, later generations of qualitative methodologists, and a few criminologists today. Yet even here, the debilitating effects of fetishism remain a danger. In the decades since Thrasher and Becker, ethnographic

research has often been characterized as a sort of thoroughgoing, self-contained alternative methodology, a carefully calibrated counterweight to the methods of survey and statistic. This reification of qualitative method in part reflects a tendency that philosopher of science Paul Feyerabend (1975) has identified: the tendency to imagine, post hoc, that earlier disciplinary work must surely have anticipated the discipline today, and so to assign to it a rationality and teleological certainty that it never had. This totalizing sense of ethnographic field research derives also, ironically, from the disciplinary stigma with which ethnographic researchers are currently saddled; like Al Cohen's (1955) delinquent boys, ethnographers well know that the measuring rod of orthodox criminology finds them lacking, and so they invert it, and claim instead that ethnography constitutes the real standard of rigorous method. For still other qualitative researchers, disciplinary stigma induces not inversion by imitation—a hope that if qualitative research can somehow be made to seem more like quantitative research, can be more infused with scientific schemata and self-avowed validity, then it might be made legitimate.

For a variety of historical and disciplinary reasons, then, ethnography risks becoming just another methodological fetish, just another chapter in the methods textbooks, just one more set of deployable research procedures deserving of endless reification, refinement, and discussion. Yet as all good ethnographers know—though won't always put into print, for reasons of disciplinary survival—the field researcher's deep engagement with subjects and settings renders any preordained methodological prescriptions provisional at best. Ethnographic research techniques are in reality not deployed; they are negotiated with subjects of study, invented or reinvented on the spot,

and not infrequently discarded in the dangerous, ambiguous, interactive process of field research (Ferrell and Hamm, 1998). "OLS regression" may be a technique of statistical analysis, and "taking good field notes" a technique of ethnography—but where the first follows procedural protocols for manipulating data, the second follows the uncertain rhythms of human interaction beneath bridges, in back alleys, and inside courtrooms. Methods textbooks and graduate qualitative research seminars aside, viable ethnography remains closer to the "impressionistic, humanistic, and artistic" (Adler and Adler, 1998: xii) undertakings of Becker (1963), swinging high with jazz musicians, or Polsky (1967), hustling his way through seedy pool halls, than it does the formalized methodologies of survey research or clinical experimentation.

With the emergence of postmodern and reflexive/auto-ethnographic orientations over the past couple of decades, at least some researchers and theorists now embrace this admission that ethnography is by nature a fluid and idiosyncratic undertaking. In this context there is among some scholars increasing acknowledgement—even celebration—of ethnography as an enterprise distinctly and inherently different from the abstract rigidity of social 'scientific' method. This sense of fluidity and openness applies to the subject of ethnographic research as well; where it was once imagined that rigorous ethnography could capture the totality of a clearly delineated group or setting, many researchers now speak of ethnographic research as an uncertain process attuned to the parallel uncertainty of contemporary groups and their circumstances. Contrasting the tradition of "technique-driven" social science research with postmodern notions of fluidity and ambiguity, Peter Manning (1995: 246) argues that these notions can usefully orient ethnography to the 'emergent, fragile, and reflexive character of modern life…."

Ethnographies of such circumstances, Manning (1995: 249-251, emphasis in original) notes, can in turn account for the "fundamental perversity and *unpredictability* of human conduct" by gathering "fragments and shards of events" into an open-ended "ethnography of experience". Unpredictability, fragility, fragmentation—all constitute a refreshing counter to any fetishizing of ethnography as a magical set of technical maneuvers.

Along with revitalizing ethnographic research in general, the emergence of cultural criminology has likewise spawned a certain reimagining of ethnographic practice. In their recently published *Cultural Criminology: An Invitation*, for example, Ferrell, Hayward and Young (2008) outline two sorts of ethnography emerging within cultural criminology, and in response to the swirling possibilities of late, liquid modernity (Bauman, 2000). *Instant ethnography* denotes an ethnography of moments and ephemeral meanings and in so doing confronts yet another conventional assumption underlying the sense of ethnographic method as a totalizing enterprise: the notion that durable social groups and situations are to be studied through enduring ethnographic research. Echoing Katz's (1988) conceptualization of crime's situational seductions, and Lyng (1990, 2005) and Ferrell's (1996, 2005) work on illicit moments of edgework and adrenalin, instant ethnography suggests documenting not groups or organizations, but instead the situated moments in which crime and crime control are negotiated. It further implies that the ethnographer must become part of those negotiations, must go "inside the immediacy of crime" (Ferrell, 1997), inside instants so fleeting and fragile that those involved often believe them to be both ephemeral and ineffable (Ferrell, Milovanovic, and Lyng, 2001). In this way, instant ethnography also becomes an ethnography of the performances

(Conquergood, 2002, Garot, 2007) whereby gang members, police officers, or victims construct and affirm their identities.

Liquid ethnography likewise suggests a useful destabilization of ethnographic "method," and of the boundary separating researcher from research subjects. Liquid ethnography is the ethnography of populations cut loose from stabilities of time and space through global immigration, short-term employment, and virtual communications; it is ethnography attuned less to durable affiliations than to transitory allegiances. Further, liquid ethnography flows with the shifting interplay of images in media-saturated environments, and with the interplay of ethnographer, ethnographic subjects, and social activism that animates the best of field research. Ferrell's (2006: 1) recent mixing of "field research and free form survival" as an unemployed urban scrounger might constitute one example. The research of David Brotherton, Luis Barrios, and their associates certainly constitutes another (Brotherton and Barrios, 2004, 2009). Through this engaged research they document the ongoing political interplay of "street gangs," U.S. immigration policy, street-level organizing, and global activism—all while critiquing conventional images of gangs and incorporating alternative visual representations. Similarly, Maggie O'Neill (2004: 220) and her associates utilize "participatory action research," "performative praxis," and various forms of art and photography in their collaborative work with prostitutes, immigrants, asylum seekers and other marginalized populations, thereby inventing forms of field work that can affiliate the researcher with even the most transitory and contingent of communities.

If the lived practice of ethnographic field research moves criminology away from "method" as a set of deployable protocols and techniques, these recent

reconceptualizations push it away further still. Blurring researcher and research setting, confounding engaged research with political engagement, destabilizing both the temporal and spatial frames of the research process, they harbinger the death of method itself. They suggest that ethnography, rather than existing as a "method" of research, in fact operates most usefully as a *sensibility* about the external world and a *sensitivity* to its nuanced ambiguities. This ethnographic sensibility orients the criminologist to the ongoing, symbolic construction of meaning, and to the shared emotional environments in which such meaning is made—all while promoting a research sensitivity of openness, attentiveness, and epistemic humility. Conceptualizing ethnography as intertwined with performance and persuasion in turn repositions it as part of a larger goal: the ethnographer's ultimate goal of communicating with others—readers, viewers, community members—in the interest of humanistic engagement and progressive social change. As experienced by ethnographers themselves and as embodied in new orientations toward instant inquiry and liquid research, ethnography disappears as a method and reemerges as a way of living and being in the world, more ontological orientation than methodological technique.

One further reorientation surely applies the *coup de grace* to any fetishism of the methodology, to any lingering belief that control groups and carefully quantified data sets—or for that matter, pre-ordained techniques of ethnographic research—constitute appropriate avenues to criminological understanding. Simply put, this reorientation involves acknowledging that, more times than most anyone cares to admit, good research constitutes what we might call a "gorgeous mistake" (O'Connor and Pirroni, 1990). Ethnographer Stephanie Kane (1998: 142-43), for example, echoes Manning's sense of

the "fundamental perversity and unpredictability of human conduct" in arguing that chaotic "moments of extreme or unusual conditions" often offer invaluable insights into social situations. As such, she adds, mistakes and misdirections in research, moments of stumbling serendipity, are to be valued—maybe even sought—for their criminological insights. No less a criminologist (and sociologist of science) than Robert K. Merton (in Cullen and Messner, 2007: 6) likewise notes the "differences between the finished versions of scientific works as they appear in print and the actual course of inquiry…. Typically, the scientific paper or monograph presents an immaculate appearance which reproduces little or nothing of the intuitive leaps, false starts, mistakes, loose ends, and happy accidents that actually cluttered up the inquiry." And then there are Kandinsky, Man Ray, Duchamp, Rauschenberg, Pollock, and de Kooning, all artists whose breakthrough works, we now know, emerged out of mistakes and misperceptions, out of cracked printing presses and broken picture tubes—though as art critic Carter Ratcliff says, "it wasn't part of the way they presented themselves to acknowledge this" (in Lovelace, 1996: 119).

Kill method indeed—these examples suggest, as does Feyerabend (1975) in his critical history of modern science that "method," and those who fetishize it as a safeguard against error and subjectivity, regularly ignore insight and inhibit creativity. The failure of method, it turns out, may often be the best method (Ferrell, 2004, 2009). Or as the anonymous punk petty thief author of the book *Evasion* (2003:12) put it, "I always secretly looked forward to nothing going as planned. That way, I wasn't limited by my imagination. That way anything can, and always did, happen."

Coda, as Performed by the Schwarze Kapelle

Then again, sometimes when nothing goes as planned, you end up in prison.

During World War II a group of German citizens, diplomats, and military officers decided that the only hope for Germany, and for the world, was to depose Hitler, through assassination if necessary. The Gestapo's name for this informal group was the Schwarze Kapelle—the Black Orchestra—and among them was the brilliant German theologian Dietrich Bonhoeffer. Through a series of unlucky accidents their plan to kill Hitler failed, though, and members of the group were rounded up, imprisoned, tortured, and murdered.

Even after his capture, sitting in his prison cell, awaiting execution, Bonhoeffer (1972: 280) didn't doubt his commitment to the plan—but he did began to reconsider the nature of religion, faith, and existence. In a letter to his friend Eberhard Bethge, he wrote that perhaps religion was only "a historically conditioned and transient form of human self-expression," not to be confused with Christianity itself. And so he asked Bethge a question: "If religion is only a garment of Christianity—and even the garment has looked very different at different times—then what is religionless Chistianity?"

Nothing on the order of life and death at stake in this essay; no theological considerations either. But still, the parallel appeals. Sitting in the intellectual prison that is orthodox criminology today, one can't help but wonder: Is "method" only a garment of criminology, a transient construction, and not to be confused with the criminological imagination itself? And if so, what might criminology become without it?

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