So what do we do with it?

First, let me answer a question not asked by the title: What do we not do with CR? I said it at the end of the last chapter, and Christian Smith says I always say it. I say it again here: CR is not a theory; therefore do not treat it as such. Strictly speaking, there is no CR explanation of anything. No more than there is a positivist or post-positivist account of this or that. What is the positivist theory of social stratification? There may be multiple theories of stratification all positivist in nature, but there is no particular theory that is singularly the positivist theory of stratification or of anything. The same goes for CR.

Like positivism, postmodernism, or pragmatism, CR is a metatheory rather than a theory. As such it parameterizes – from its criteria – what good theories are. Thus, it provides grounding for what it considers tenable research and challenge to research that departs from its premises. But that is all it does.

Is that enough? Enough to explain anything? No, but it is still something. Something needed and something important.

So how do we use CR? In the first place we use it for defense and for attack. We know that from the positivist point of view, which still reigns in sociology, that ethnography and historical narrative are suspect. Not so from the standpoint of CR. From the standpoint of CR, neither of these approaches is second best. Instead, from a CR perspective, even purely descriptive work is scientifically important. It is important to know what is out there, especially if attention has not previously been drawn to what is described and especially if, in the process, what is identified is described in theoretical language that highlights the significance of what is identified.

From a CR perspective as well, neither ethnography nor historical narrative need encompass only description. Insofar as CR separates causation from generality, ethnographic description of operative mechanisms can contribute to causal explanation even if what operates does so in only one place and time. Historical narrative, on the other
hand, is the only way to bring together the causal effects of conjunc-
tures of mechanisms. As I said before, narrative, not a regression
equation, is the canonical form of a CR explanation as it is of any
scientific account of anything in the real world outside the laboratory.

Narrative is particularly apt, in fact, for showing the combined effect
of structure, culture, and agency. On the one hand, the effects of
structure and culture show up mainly in the thoughts and actions of
individuals. But because those socially structured thoughts and actions
remain creative, they do not necessarily follow regular patterns. Given
what is perhaps a unique conjuncture of structural and cultural mech-
anisms, the ensuing thought and behavior may well itself be unique.
Unlike a statistical correlation, narrative can trace how thought
and action, while remaining creative, were still nevertheless a (non-
deterministically) caused cultural reaction to structural forces.1 This
whole explanatory possibility is opened up by CR in a way that is just
precluded by positivism, or even postmodernism, which denies both
coherent agents and objective structures – not to mention coherent
narratives.2 It is not, however, just defense or philosophical warrant
that is being served here – as important as that function is – but also a
greater understanding of all that intensive research approaches can
afford.

It is not only ethnography and narrative history that receive philo-
sophical legitimacy from CR, but value-laden research as well. From
the positivist perspective, activist research is highly suspect. The posi-
tivist image of the proper scientific observer is one who is absolutely
neutral about whatever he or she is proposing to study. That stance
precludes study of a social movement in which one participates oneself.
Not so from a CR perspective. CR expressly challenges the equation of
objectivity with neutrality. From the CR perspective, it is not neutrality
that is to be demanded of competent scientific researchers, but intellec-
tual honesty – the honesty to admit when their own expectations go
unmet, the honesty to admit when rivals have the better argument. Of
course, given the ever-present possibility of counterargument, it is not

1 Again, I take this to be part of the point of Iddo Tavory and Stephen

2 For a fuller account of narrative as the way to reconcile structure and agency, see
Douglas Porpora (1980) The Concept of Social Structure (Westport, CT:
Greenwood Press).
always easy to say when these moments have arrived, when, that is, the available defensive arguments lose their plausibility. That moment will be different for different researchers, but all must maintain an openness to its arrival.

Defense is not the only function served by CR. The various ontological commitments of CR also afford resources for attack. Sociologists often don’t think in terms of attack and defense, but in part science is a contentious practice, a matter of conjectures and refutations. A conjecture or claim is put forward and stands only if it resists criticism – which is a form of attack.

From whence does criticism come? Sometimes it comes from our guts, which means one of two things. One thing our guts might reflect is our own lived experience. Our lived experience, like our passion, is a theoretical resource. It directs us to issues. Sociology often trains us out of it. As Christian Smith observes in *What is a Person?*, we have been trained to accept as sociological truths theories that run completely counter to the way we live our lives.3

In some important ways, our critical thinking is actually turned off by disciplinary conventions. Turn it back on! Like the child in the fable of *The Emperor’s New Clothes*, start asking the embarrassing questions from your own lived experience that sociology prefers you not to ask. And don’t just ask those questions: Turn them into research projects, projects that challenge disciplinary shibboleths.

It is not, however, always just our lived experience to which our guts direct us. Our guts may also signal our tacit reliance on one or another theoretical framework we have absorbed without ever having articulated it to ourselves. How much better, how much more deployable to articulate it?

As a formal metatheory or philosophy of science, CR articulates a range of ontological commitments that foster a corresponding range of theoretical questions to be lodged against any theory that is encountered. Does the theory – say a Bourdieusian account of academia – involve a defensible understanding of agency? Does it account for structure? How does it understand causality?

Similarly, can we effectively in Foucauldian fashion treat discourse as if it were not produced by actors motivated to produce it from

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specific structured sites? Can we, as per Branwyn Davies and Rom Harré’s social positioning theory, talk of how actors position each other in conversation without similarly assuming that there are such things as actors who do such positioning work? And can there be positions without relations? Or, per relational sociology, relations without relata?

These questions are not just ammunition for the delivery of interparadigmatic bombs. To the extent that science progresses through conjectures and refutations, it grows through scientific practitioners knowing with which questions to challenge each other and from there what further research might be fruitful. Thus do sites of attack turn into potential research projects, projects that lead somewhere theoretically interesting and not just to record another county heard from. CR in particular prompts questions that sociology – and especially American sociology – does not currently ask.

In the end, however, although CR is a valuable heuristic for asking important research questions, CR is not generally, in my opinion, the place to begin formulating a research study. Those looking for a place to begin should begin with whatever passionately engages them.

That advice is not necessarily easy to follow. Undergraduates often come to me asking my advice about graduate programs in sociology. “Well,” I ask them, “What are you interested in?”

“Sociology,” they tell me. Not a great answer. I have to do deep therapy with them to elicit something more helpful. Perhaps I can get them to say, “gender.” They are interested in gender.

“Well, what about gender?” It is like pulling teeth. The promising researcher, however, eventually learns to pull these teeth himself or herself. The promising researcher learns to internalize a voice like mine. To internalize it and to answer it.

These questions are not particularly CR questions. As I said, I do not think CR is the place to start. I advise against asking oneself how to do a CR study of gender. That question is not what one should be asking.

What should beginning CR researchers be asking? Well, they should be asking first whether the literature on gender or whatever it is they are interested in has answered all the questions that are of interest. Have all questions been answered satisfactorily? Here, as I suggested

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above, CR can help as a heuristic. Do the answers to the questions employ defensible accounts of agency? Has social structure been adequately taken into account. Culture? What remains to be done?

Once prospective researchers determine what remains to be answered, they still are not necessarily ready to bring in CR in any formal way. The next step is to determine with what theoretical perspective they resonate. Is it Symbolic Interactionism? Marxism? Bourdieu or Foucault? Perhaps, then, prospective researchers should accept Bourdieu and Wacquant’s invitation to a reflexive sociology so as to ask themselves whether their favored perspective gives us a full picture and whether it entails any philosophically untenable positions that at least need repair.

The repair may itself be the point of a research project aimed at bolstering a perspective to which one is overall attracted. I personally think, for example, that Davies and Harré’s account of social positioning is a wonderful piece of work that provides almost an algorithm for future study. But I also think they are seriously wrong in what they say about structure and about agency. So someone attracted to this perspective might, following CR, do some empirical research from the social positioning perspective that brings to empirical light how important to that perspective is structure or agency or both. Attack, therefore, need not be deployed just to undermine but also to build up.

So a beginning question is whether theoretical repair is to be part of the work the prospective researchers plan to undertake or whether the prospective work will just entail a philosophical assumption different from that perspective’s philosophical mainstream. That type of question, stemming from CR considerations, is the type of issue that prospective researchers should consider.

There is another category of question to which CR may prompt us. Our discipline goes through sensibility cycles. I came up in an era when many of the New Left entered the academy, a Left that was pretty much a political economic Left. A prominent research agenda then was directed at social change, political economy, corporate power, inequality, and war. The questions asked and perspectives addressed to them reflected those interests. In sociology, functionalism was abandoned in favor of a whole range of new perspectives better able to address social change and human action. Shortly thereafter, the political economic Left was eclipsed by a new cohort of graduate students reflecting a more cultural Left, reflecting social identity movements.
The dominant research agenda accordingly changed, and with that change there changed too the theoretical perspectives sociology came to emphasize. In America, the culture section mushroomed and the Marxist section withered.

Like lay people, even we sociologists tend to reify the present, to suppose in a Whiggish fashion that what prevails now is the culmination of best sense and not just a historically accidental sensibility. So one question that researchers might ask themselves is what questions the discipline is not or no longer even asking and whether they are important. Canvassing the various ontological categories represented by CR – agency, structure, truth, values, and so forth – may help to prompt such larger questioning of the discipline and the research direction one comes to pursue. It would be salutary for the discipline if more sociologists asked what important questions were going unanswered in the discipline, if more would traverse roads less followed.

As any dissertation chair will advise, whatever question one decides to pursue, it is necessary for the prospective researcher to narrow down the prospective topic of study, to formulate a project doable in the time to be allotted to it. Again, although the issue is not specific to CR, it may be that what one thinks one wants to study actually depends on prior findings that need to be studied first. Here, CR can help by orienting one toward interacting mechanisms and their conjuncture. It may even be a version of CR transcendental argument that prospective researchers need to entertain: What needs to be true for the phenomenon a researcher wants to study even to be a possibility? Does the researcher know that those prior possibilities obtain? If not, then maybe the existence of those prior conditions are actually what needs to be studied.

I stated the above issue in terms that are highly abstract. Can I supply a concrete example? Yes, sort of. At the risk of boring, I refer once again to my own work for Post-Ethical Society. I set out wanting to know what explains the privatization of morality, the retreat of moral reasoning from the public to the private domain. What causes that retreat? That question is a very good one if I do say so myself, but the prior question is whether there truly is such a privatization of morality. Certainly, if there is no such phenomenon, then nothing is going to explain it. So prior even to searching for the causes of the thing – in this case the privatization of morality, I needed to do
something that had not originally occurred to me as a research project: Determine whether what I think I want to explain even obtains. As I mentioned in Chapter 2, following Isaac Reed, who follows Merton, often the first thing we need to do is “establish the phenomenon.”

That prior task meant I actually needed to reformulate the research work I had to do.

It is such self-critical process that moves us back a step that is what I am talking about. What is required to do here is less empirical than careful conceptual analysis and a knowledge of what the literature on the topic so far has shown or failed to show.

Speaking of the literature on a topic, whether one plans statistical work or ethnography, one should enter research with two different conversations in one’s head – and actually keep those conversations going throughout. One conversation is a dialogue between the theoretical literature and one’s own theoretical expectations. How does what I intend to say add to what others have said? Whose positions am I supporting and whose am I challenging? The other conversation is between what one thinks one is going to say, come the time to report in, and what one actually finds, whether the findings come in the form of statistics, ethnographic field notes, or interviews.

Within this double dialectic, there is constant need for revision: “That is what I thought was happening, but now I find this. What is the relevance of this finding to the literature? What can I now say and is it still important?” This double conversation between self and data and self and literature needs to go on constantly. It is a version of grounded theory, although not the original, simplistic, positivist version that imagined we could go into research as blank states and just let the data affect us.

So this double dialectic of the research process is not specific to those who subscribe to a CR perspective. What CR can allow one, though, should time permit, is to think in terms of mixed methods. Because there are no canonical CR methods – at least from my perspective, one is free to entertain and utilize all methodologies.

Yet, although CR countenances analytical statistics, it must always be remembered that it does not do so in service to general laws. What that comment means is that for us, statistical equations never become the

\[ 5 \text{ See Isaac Reed (2011) Interpretation and Social Knowledge: On the Use of Theory in the Human Sciences (Chicago, IL: University of Chicago), pp. 15–16.} \]
explanation for what is happening but only evidence that one thing or another is happening with some frequency or perhaps that more of one thing is happening than another. Even when we insert an intervening variable into our regression equations, the new, more complex equation does not attain the status of an explanation. The intervening variable may signal the operation of a mechanism, but what that mechanism is and how it operates still need to be explained discursively.

The bottom line is to question the methodological type of research question we are asking, and this meta-question does take a CR form. In particular, is our research question a matter of how and why or is it more a matter of whether and where?

Again, the above formulation, meant to be pneumonic, is a bit too compact and abstract. Let me elaborate. If our question is a matter of how and why, then we are asking how or why it happens that something occurs. In that case, we are asking for the identification of a mechanism or a conjuncture of mechanisms. Our answer, then, if it is not to be just conjecture, which is sometimes appropriate, suggests research that is more intensive or qualitative in nature. We may want to show ethnographically, as in Tally’s Corner, how a particular structure exerts its effects on behavior or, as in Talking of Love, how people draw on culture differently all to like effect. If conjunctures are at issue, then the research will likely take the form of narrative history.

As I have said, in my own research for Post-Ethical Society, my colleagues and I eventually realized we needed to show how in the secular press moral matters are consistently reframed as matters of strategy or prudence. That was a how question that accordingly called for a mechanism. The operative mechanism is textual reframing, not something that can be explicated via statistics. The how of textual reframing needs to be shown descriptively. It requires qualitative discourse analysis.

What did constitute a statistical question, because it was a matter of counts, was whether such textual reframing was general to secular American discourse. And when our question turned to whether such reframing occurred more in secular than in religious discourse, our question became an issue of where: Where (or in which discourse) did such framing appear with greater frequency and where (in which discourse) did it occur with less? Because that question was a matter of comparative counts, it too called for quantitative, statistical analysis.
The quantitative question is important too. Still, however, the statistical analysis did not explain why – why, as we found, secular publications are much less likely than religious publications to engage in moral reasoning. Our answer there we did leave as a conjecture, which means it is left for someone else potentially to research further.

The research questions with which the prospective researcher enters the field depend in part on the theoretical state of play in the literature on that topic. There seems only a very few sociologists concerned with macro-moral discourse, i.e., discourse about macro-moral matters like war, genocide, terrorism, and torture. Because there had been little written on this topic by anybody, when my colleagues and I began our research for Post-Ethical Society, we did not have much by way of expectation. A number of people, like Stephen Hart, Michèle Lamont, and Thomas Luckmann, had suggested that, like religion, morality might be privatized. That was about it.

As I said, with that state of play, it was premature to ask a causal question like “What is causing the privatization of morality?” We first had to confirm whether indeed morality is privatized. That task called not for causal analysis but description, appropriate description, and operationalization: What validly counts as the privatization of morality?

My point is that we enter the research process informed by the theoretical literature on the topic and with questions that result from the state of play in that literature. Actually, though, the state of play determines more than just the research questions we ask. From a CR perspective, it also determines to an extent what counts as good research.

Positivism sets up an ideal, decontextualized standard for all research. Being a foundational perspective, for positivism, if there is no certainty, there is no truth. Of course, no research study, particularly no non-experimental study in the social sciences, can hope to avoid all the threats to validity that we learn. So in reality, it is the rare social science study that comes close to the ideal, that establishes anything with certainty. I say rare, but I actually don’t know of any.

The question is how far can your study depart from the ideal and still be good? Although positivism offers no answer to that question, in practice, the answer depends on two factors: (1) how hard-assed peer reviewers are likely to be; and (2) what passes for good research in that area, given the theoretical and scholarly state of play and the research difficulties involved in the topic’s study.
On its own, the first criterion is clearly arbitrary. There is no empirical research study – not even one that is experimental in form – that can escape criticism. As the sociologists of science have shown us, even the best experimental findings are fodder for debate. The second criterion – what passes for good research in an area – is a much fairer criterion. Suppose it is difficult for anyone to administer a truly randomized opinion poll somewhere on some topic. It may be, then, that all scholars who research that topic in that place rely on convenience samples. In such cases, it would seem inappropriate to reject for publication an individual paper on the grounds that it failed to employ random sampling.

Any deficiency applies instead to the sub-field as a whole rather than any individual paper. Of course, it may be quite appropriate to fault the entire sub-field on such grounds, particularly if better methods are possible. In the absence of better methods, the question is whether faulty data are better than no data.

Positivistically minded thinkers may say no, feeling that, unlike no data, faulty data can lead to faulty conclusions. But faulty conclusions can be corrected whereas silence cannot. Instead of being cowed by those who worry that our account harbors some unknown errors somewhere, we should ask in turn what error they think there is and invite them to do their own research to confirm it. The point is that truth does not spring full-blown from our methods like Athena from the head of Zeus. Truth is instead something toward which we often grope, and as such something we often obtain only through slow, individually erroneous steps.

In practice, sociologists are always working with faulty data. It would seem better to embrace a philosophy of science that actually admits or allows for that clear reality. CR does. CR epistemology, remember, is alethic. According to that perspective, the truth is what it is apart from our methods, although we only know what is true through our methods. Or, to be more precise, we have only our best chance of knowing what the truth is through our methods, because our knowledge of the truth is always fallible.

Thus, in science, CR does not expect the truth to emerge necessarily from a single study. It may take a range of different studies on an issue,

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all with different faults but all pointing the same way before we begin to get some confidence about what the truth might be. Such being the case, the CR advice to the individual researcher is to do the best one can methodologically, minimally as well as what passes for good research in that field of study, better if one can, perhaps correcting for methodological deficiencies in the literature even if it means incurring others, and not to sweat the remaining deficiencies over much. Of course, if one can employ a new method that comprehensively challenges all that came before, better still, but that prospect does not often present itself.

There is a final way to use CR that is truly distinctive and which brings us back to my opening remarks in this book. It is the most subversive way to use CR so far and not for the faint of heart. As I said at the very beginning of this book, sociology – American sociology particularly – socializes its new recruits into normal science, routine work within a paradigm.

There is absolutely nothing wrong with normal science. Science would not get done without it. The problem is when absolutely everyone is steered exclusively to normal science, when they are counseled against asking the big questions that cut across paradigms. It is especially a problem when, as in sociology, what we have is a plethora of paradigms of unclear compatibility. Can they all be completely right? Is there no way to work toward larger synthesis? Sociology has dubbed itself a multi-paradigm discipline, and we all nod our heads as if that christening completely legitimates the state of affairs.

By rights, however, our current multi-paradigm condition is a scandal. It suggests the discipline’s inability to offer any comprehensive account that can cut across particular issues. It is not a state of affairs in which to take pride but one that betokens synthetic failure. In physics, the reconciliation of quantum mechanics and general relativity is a matter of keen concern. In sociology, the lack of reconciliation among our different paradigms leaves us indifferent. That lack of concern signals the reign of abject empiricism and a disciplinary inability to stomach long conceptual questions that cannot be answered with more data. Unfortunately, as the postmodernists in general and social constructionists in particular have amply demonstrated, data alone cannot adjudicate across paradigms.

What is needed to adjudicate across paradigms is not more data but deeper conceptual analysis. What is needed is a philosophical step back...
from each particular paradigm so as to examine their taken-for-
granted assumptions. That is a metatheoretical task that often moves us outside of empirical sociology into metaphysics. The very word evokes horror in empiricists, which includes both positivists and postmodernists alike. Yet, as long as we decline to enter into metaphysical debate about fundamental ontology, the postmodernists are right that what we are left with is a relativist aporia.

Whereas empiricism in either its positivist or postmodern form discourages metaphysical inquiry, CR positively encourages it. It is CR therefore that distinctly promotes what is needed for interparadigm communication and for the great synthetic task left undone by a multi-paradigm discipline.

Yet, as the needed conceptual work does not conform to the positivist picture of science, our discipline actively discourages it. Were sociologists physicists, they would have complained about Schrödinger’s cat and the Einstein–Rosen–Podolsky argument: “Away with all these silly thought experiments! Serve us more data.” Then quantum mechanics would have been left in the same condition as sociology today. Fortunately, although real scientists think they are positivists, they actually do not act that way. Unfortunately, sociologists do. We need more intrepid souls. For them, CR is waiting.