Practitioners often want and need scholarly support in addressing important policy problems. Social scientists, in turn, often believe and represent that they have special skills and standing in offering such policy analysis. In fact, the needs of practitioners and the research and analysis social scientists actually provide are often seriously mismatched. Some scholarly habits of mind rise to the level of “policy fallacies” and stand in the way of what could, and should, be much more congenial and productive relationships. These issues, and some simple, practical, and powerful ways to address them, will be explored using real-world examples drawn from public safety policy.

Scholars, practitioners, and public policy

There is a large and useful literature on the role of social science in supporting, shaping, and assessing public policy. I will set a tone here by, in the main, disregarding it. What follows is not a particularly scholarly piece of work. One of the main thrusts of this article will be the argument that scholarly conventions, and scholarly habits of mind, are in some important ways impediments to effective relationships between social scientists and practitioners in designing and implementing good public policy. It argues further that some simple, rather unsophisticated, and fundamentally common-sense conventions and habits of mind can change that, and make both social science and social scientists much more welcome, and much more influential, in the
policy arena. This article will model that position in practice by being, at least, unsophisticated and common-sense.

Rather than beginning with the literature, it will begin with, in scholarly language, ethnography. I have had, for the last twenty years or so, one foot in the academy and one foot in the world of criminal justice practice. I respect both very highly and have been part of a community aiming to improve public safety by working closely with both to craft and implement new interventions aimed at very serious crime: homicide, gun violence, chaotic drug markets, and other features of mostly extremely distressed urban neighborhoods. That work has given me extended, direct experience with some of the very finest scholars in a variety of fields, and to some of the very finest practitioners in a variety of agencies and settings. Ethnography is the study of culture through direct exposure, and it is an appropriate method to address how scholars and practitioners think and act, and how they do and do not do so well together.

For what we are dealing with here, and we should be clear about it, is a clash of cultures. I am going to neglect the existing literature, in part, because it tends to ignore, gloss over, or misinterpret this essential truth. To take a typical portrayal of the difference between academics and practitioners, we can look to the highly respected student of policy analysis Robert Behn:

The policy analysts are concerned with economic efficiency, with how public policies affect society as a whole. They emphasize explicitly defined policy objectives and conscious tradeoffs between competing objectives. They seek clear measures of outcomes – of how successfully these outcomes are being realized. With faith in their analytic techniques, the analysts draw their recommendations from abstract, mathematic models.

In contrast, the policy politicians are concerned with distribution, with how public policies affect individuals and groups. They emphasize negotiation, bargaining, and compromise, and the obscuration of competing interests to achieve consensus. They seek the resolution of conflict between competing interests – the allocation of limited resources between such interests. With a faith in the political process, the politicians draw their policy recommendations from the intensity of their constituents’ views.1

1 Behn, Robert Policy Analysis (7) 2 (Spring 1981) 199-226.
Behn is explicit that these two characterizations are caricatures, though with roots in real differences. But there is not much doubt about where one’s sympathies are to lie. Analysts are sophisticated, objective, and concerned with the common good; practitioners are unsophisticated, subjective, and concerned with narrow interests. The characterizations are themselves couched in mannered and specialist language, with a clear bow toward one camp: social scientists – some of them, at least – will have no difficulty recognizing themselves as treating in economic efficiency through the mobilization of advanced mathematics, while practitioners are unlikely either to recognize or to understand their roles as agents of distribution and obscurcation. The descriptions are fundamentally unbalanced: analysts may be amused by their personification as green-eyeshade technocrats, but practitioners will be insulted by theirs as slavishly beholden to the loudest voices. There is, if one is close enough to the academy to see it, the same stratification operating on the academic side: the honoring of “hard” quantitative and empirical vantages and methods over “soft” qualitative orientations.

This is, if one does policy work and can see it from both inside and outside the academy, a very nice rendering of how the academy in fact looks at itself and at the world of practice. The academy feels privilege to say what is true and what should be done, based on superior theories, methods, and standards of evidence and evaluation. It honors the quantitative over the qualitative. It feels disregarded by the world of practice, and explains that lack of regard in terms of politics: the operation of special interests and attention to special interests over and against objective truth and the common good. It is sure that the world would be a better place if it had more influence. It believes that, in nearly every regard, the world of practice should move its way and become more like the world of scholarship. It recognizes, at least to some extent, that the world of practice operates in a web of real pressures and constraints, and recognizes the
influence of that web, without granting it any legitimacy: it is there to be overcome, or if not that reduced to the greatest extent possible.

There is not the same richness of position and narrative on the other side. Rather, for the most part, the world of practice simply doesn’t care very much about the academy. It isn’t relevant to what practitioners do or who they are. This too is an exaggeration – it’s too simple, and there are important exceptions – but as, again, a gloss, it’s not far off. In the world of practice, “academic” carries primarily one of its several ordinary-language connotations: “without practical import or utility.” To the extent practitioners do have thoughts about academics, it is that they are distant and detached; do not understand the realities of getting things done; are interested in things of little practical import; operate on glacial timelines; have their own, entrenched, and mostly liberal politics; and generally say that what practitioners have been doing didn’t work, long after any such input has ceased being meaningful.

An unusually direct manifestation of this is a blistering essay by police chiefs Charlie Beck and Bill Bratton and action-researcher George Kelling on the scholarly evaluation of their efforts to address skid row in Los Angeles. Called “Who Will Police the Criminologists,” it responds to academic judgments, published in a prominent peer-reviewed journal, that their work “demonize[d] persons,” was “poor craft,” and represented a “zero tolerance” approach “applied to putatively ‘broken people’ who apparently . . . are not to be fixed but instead are cited for the crime of brokenness and removed from sight.” In fact, the three wrote, had those commenting troubled to see what had been done on the ground, they would have found a robust, cooperative problem-solving effort aimed at, among other things, helping the homeless, with its strongest support coming from advocates for the homeless. “The consequences of confusing research and scholarship with hubris, polemics, and uncritical secondary-data analysis—in short, of
politicized social science,’’ they wrote, ‘‘are enormous: distrust between academia and policing, gross misrepresentations of policing, and, above all, widespread failure to understand what works and what doesn’t work in policing.’’²

Such differences are poisonous. To the extent that they are mistaken or unnecessary, they keep people from understanding each other, working together, improving both practice and scholarship, and achieving important public goals. And while there are unmistakeable grounds for both sets of beliefs, my own balance has shifted over the years toward the practitioners: I have, fundamentally, more sympathy for that view than for the other. At the same time, I see a deep and genuine desire on the part of many academics to contribute to practice and to be valuable in that world. That desire is often thwarted by very powerful habits of mind that are deeply engrained in the academy, are correctly central there, often do not translate to the world of practice, and frequently inflict on well-meaning scholars what are in effect self-inflicted wounds. This essay will address some of the most central of those themes.

They rise, in my view, to the policy equivalents of core and venerable logical fallacies. There are predictable ways in which discourse can go wrong, not in simple (or complex) error of fact but the core structure of the argument: they are errors of reasoning. We have names for many of these, from the long development and study of logic and rhetoric. We know better than to put forth an argument from authority: that somebody of standing says that something is true does not make it so. We know that absence of evidence is not evidence of absence: that we cannot prove something is true does not mean that it is not. We know not to argue that because something came before it caused what came after: post hoc ergo propter hoc. There are equivalents in the policy world: and they can be understood and guarded against. Much of what

academics are most deeply frustrated by when they engage with practitioners, and which leads to
the disregard from that world that is then interpreted as craven attention to politics, is in fact due
to academics’ commission of these fallacies: which are immediately evident to the practitioners
they are trying to influence. Understanding and avoiding them is therefore essential to more
productive relationships.

The core policy fallacies

The descriptive fallacy

Scholars believe deeply in reaching accurate, precise, and insofar as possible subtle and
nuanced portrayals of situations, issues, and problems. These correct descriptions of reality are
frequently guided by theory – theory cannot define facts in the world, but it can and often does
direct inquiry in very useful ways – supported by sophisticated methods, both quantitative and
qualitative; and governed by hard-won and subtle insights about how to ensure, as best as is
possible, accuracy of representation. Such inquiry and description can lead to entirely new
understandings of important fundamental facts and is a vital element not only in pure and applied
science but in any exercise of practical reasoning: one cannot be act intelligently about what one
fundamentally does not understand.

The converse, unfortunately, is not true: that fundamental descriptive understanding does
not necessarily lead, still less guarantee, intelligent prescriptions for action. The basic and
irreducible fact here is that knowing key facts about a problem almost never leads, in any simple
and deductive manner, to what to do about it, or even what direction to follow. The discovery of
geographic concentration of offending in criminology provides a classic example. Research
discovers shocking “hot spots,” as they came to be called.\textsuperscript{3} That this was new, different, and in some potential way important knowledge was obvious. Also obvious was that it had moved criminology ahead in an important way: the science had advanced significantly.

What was in no way obvious was what policy response this implied. Gun violence, for example, did indeed turn out to overwhelmingly concentrated in geographic hot spots: in the most dangerous neighborhoods, relatively few and small areas accounted for the overwhelming majority of incidents. As both a logical and a policy matter, however, any number of plausible actions might have been taken to address this fact. One might get rid of the guns in the country as a whole and that then made their way to and were used in those hot spots, a course that had considerable political passion behind it. One might enhance the carrying of guns by the good people of those hot spots so that they could defend themselves against the bad people, a course that also had considerable political passion behind it. One could work with the young men who lived in those hot spots, so that as they moved into their predictable years of high risk they did not use the guns available to them. One could work with the larger community around core issues of education and economic opportunity, so that underlying conditions no longer favored the development of those hot spots. And any number of other possibilities, with both reason and political appeal behind them.

The illustration is generalizable: as a policy matter one cannot deduce correct courses of action from descriptive fact. Scholars often think that one can, and act accordingly, but the logic by which they operate is unsupportable, and succeeds only by importing certain elements and ruling others out: both unsupportable moves, both intellectually and practically. In historical fact, the “hot spots” science and the literature that followed were both driven by a community of police-focused quantitative criminologists with a leaning toward random clinical trial evaluation

\textsuperscript{3} Hot spots refs.
methods. They came, therefore to favor focusing existing policing methods such as patrol on the hot spots, in carefully controlled ways: for example, locating police cars in the hot spots for prescribed periods, an intervention that could both be tested and shaped by strong quantitative evaluations. It should be obvious, however, that such a prescription – a public policy – in no way follows inevitably, or even naturally, from the underlying science. It is an importation to say that the response should be a police response; an importation to say that a police response should be a traditional one; an importation to say that a traditional police response should take a particular form. In fact, from only one step across the scholarly/practitioner divide, what looks clear and deductive can look absurd, as was the case here. To a police chief or a mayor, the idea that the best and highest use of fantastically expensive and scarce human and material resources is parking them on the street, doing nothing, is simply ludicrous. To add to which, they knew full well that their officers would not stand for it and their citizens would be outraged by it, both for good and understandable reasons.

The lesson is clear, nearly always applicable, and glaringly obvious to those with operational public policy responsibilities: *one cannot deduce courses of action from descriptive facts*. The fact that scholars often think and act as if one can substantially undercuts both their utility and credibility in the public policy process.

*The causal fallacy*

Scholars also believe deeply in reaching accurate, precise, and insofar as possible subtle and nuanced portrayals of how situations, issues, and problems came to be. These causal accounts are also guided by theory and sophisticated research methods, and also frequently lead to important new understandings and insights. If description attends to the “what” of a matter,
causal accounts penetrate further to the “why.” As both a logical concern and a matter of the culture of social science, it is even more true here that one cannot act intelligently about what one fundamentally does not understand. Acting without understanding of the causal pathways of an issue is to act blindly. Scholars frequently believe very strongly, as a result, that accurate causal understandings should be, and are, requisite predicates for effective action. They frequently believe further that when they have obtained, and can supply, such accurate causal understandings that they have made important contributions that should be respected in the policy process.

This turns out not to be true, however, and in two different but very important ways.

One is that the idea that one needs to understand how an issue came to be in order to address it is simply wrong. Blind action can be dramatically effective. People with headaches and sore muscles take aspirin and ibuprofen. We need know neither what caused the headache and the soreness nor the mechanism of action for the intervention to know that it is effective and indicated. In medicine, fortunes have been made operating in this realm. The entire multi-billion dollar industry of antidepressants is built on these common-sense facts. Medicine does not know why depression arises (or even, factually speaking, what precisely it is). It does not know why SSRIs work to alleviate depression. It does know that SSRIs work for many people, and it has developed research and evaluation methods to advance the state of the art with respect to these medicines experimentally. Causation, in the sense that social scientists use the term, has no real place.

Faced with the clear fact that these dynamics are true, academics – particularly those coming from fields with roots in sociology, anthropology, public health in medicine, social work, and the like – frequently respond by arguing that responses rooted in more fundamental causal
explanations are inherently to be preferred. That aspirin-style responses to important public problems are “reactive” rather than “preventive,” and ignore “root causes,” frequently rise to the level of self-evident, stand-alone critiques: they are *prima facie* evidence that the policy track in question is inadequate. Many of these fields include, as part of the core teaching and culture of the discipline, that policy action ought, by definition, to act back along the causal pathway that generates problems. Public health and epidemiology, for example, enshrine this idea in the formal categorization of primary, secondary, and tertiary prevention.

A number of things need to be said about this. One is that this is simply not the way the world works. As I have written elsewhere:

Most of us don’t think upstream like this in our ordinary lives. We’d be dead if we did. When fall comes and it gets cold, you put on a sweater, build a fire, build a house. You don’t go to work on the root causes of winter.4

Many very important public policy problems respond beautifully to proximate actions that lie outside the causal pathway that generated the underlying issue. Vaccines, for example, can protect whole populations from serious diseases. They do nothing about the underlying diseases, the susceptibility of persons and societies to those diseases, or the social conditions that may make particular individuals more vulnerable to those diseases. They simply work.

Academics frequently respond that, while this may be so, more causally-oriented responses would be *better*, in part, usually, because they would be more efficient, effective, and persistent. Analogies are frequently drawn to situations in which this has been true, as with the famous example of the pump handle in epidemiology: faced with a devastating cholera epidemic in London, John Snow traced the pattern of infection back to a public water supply, removed the pump handle, and the plague was stayed. All this says, however, is that when such interventions

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4 David M. Kennedy, *Don’t Shoot: One Man, A Street Fellowship, and the End of Violence in Inner-City America* New York: Bloomsbury, 2011
are available – that is, effective, easy, and uncontroversial - they’re good options: they make no meaningful general point about the nature of good interventions as such. It may be true, as the public health community says, that gun manufacturers and gun culture represent the up-stream “pump” causing American gun violence. It might be true that addressing them would be effective. It is decidedly not true, however, that shutting down American gun manufacturers and changing American gun culture are either easy or uncontroversial. They have in fact proved so far, despite diligent effort on the part of many, to be impossible. Saying, as a matter of principle, that they are to be preferred to other, potentially more effective and actionable options is unjustified.

Such root-cause interventions also tend, by their nature, to carry important policy deficits. They tend to be expensive, because they operate across large social and geographic spaces; they tend to be slow, because they are addressing fundamental social dynamics; and they tend to be wasteful, since they address broad populations (gun owners) in the name of preventing rare events (gun homicides).

All of these issues are readily evident to practitioners, who have to consider and balance them in practice. They recognize the insistence on root-cause interventions for what it is – a matter of disciplinary and intellectual ideology – and, again, discount its adherents.

The cost-benefit, and related, fallacies

Scholars with roots in, or seeking to take advantage of, economics frequently deploy the argument that a favored policy provides benefits in excess of its costs. More sophisticated related arguments are that a proposed policy provides benefits in excess of other policy alternatives, or other variations on those general themes. They suffer, in the eyes of
practitioners, from a range of closely related problems; I will discuss them, and their related issues, as a whole.

The most fundamental issue with this line of argument is that it frequently simply doesn’t matter to those responsible for important public problems: nor should it, at the most concrete level. The cost-benefit position is an efficiency position: it says, this is a good and defensible use of public resources. The argument is that using these resources in this way will provide benefits in excess of their value, or in excess of any competing use, or a conclusion to some similar effect. It is entirely possible for that to be true and for the resulting outcome to be entirely unacceptable in public policy terms. Practitioners are most fundamentally interested not in efficiency but in effectiveness: they want important problems to be solved, at some at least minimum threshold level. An investment in a problem may pay for itself, or be the most productive amongst existing alternatives, and simply not work nearly well enough to pass that threshold test. It may not produce a large enough effect; it may take too long to do so; or fail to satisfy any number of other legitimate concerns.

These facts highlight that for practitioners public policy is frequently a matter of inventing new policy rather than assessing existing policy options. If existing policy options are satisfactory along whatever dimensions matter – that is to say, they will work well enough, however that is to be judged – then return on investment, and returns on competing investments, matter. But if they are fundamentally not satisfactory, then what practitioners need is something that will be satisfactory. Shades of difference amongst unsatisfactory options will then be of no great interest. Trading one bad option for a different and more efficient bad option will rarely seem worthwhile.
A second core issue is the accrual of any benefits, and what that can mean, and not mean, in practice. Cost-benefit and related analyses tend to look at the overall public good that may result from a public policy. Given the nature of policy and its impact, those goods may be both real and utterly inconsequential to practitioners. Practitioners have work to do and bills to pay. If they cannot support the work and pay the bills, they cannot operate. Let us say we have an effective early-childhood intervention to reduce serious violence. Cost-effectiveness evaluation suggests that it would make economic sense to take investment out of police and prison and direct it to that intervention. The practitioner who declines to do so will look both irrational and cruel. What she sees, however, is that the benefits will accrue in reduced health care costs to victims, and twenty years from now. She cannot, because it is literally impossible, draw on those funds to pay the necessary police and prison bills today. We do not do our public business in ways that allow her to act.

And, given the contempt often directed from the academy toward practitioners who behave this way, it’s worth pointing out that we all behave this way. It would be socially more useful for most of us to use our resources differently. Where the common good is concerned, many of us would do better to paint our neighbor’s house than mow our own yard, and to send their children to private school rather than our own to summer camp. We do not do so, for good and obvious reasons.

The evidence-based fallacies

A closely related suite of fallacies can be collected under the banner of evidence-based. It has become an article of faith in scholarly circles, and in governmental and other funding circles, that policy should be “evidence based.” As such, there can (or should be) be no quarrel;
the alternatives – “ignorance based”? – do not suggest themselves in any positive way. In practice, however, “evidence based” has come to have some particular meanings, in theory and in practice, and some of them are deeply flawed.

One is that “evidence based” means, in practice, “evaluated according to particular, strict, and ideally random-assignment methods,” with the explicit corollary that interventions not so evaluated do not count and should not be employed. This has, in practice, a range of very serious problems.

One is, to say it again, that the world does not work this way. Proponents of this meaning of “evidence-based” acknowledge this, and then argue that the world should work this way. There has been a fitful conversation within the academy arguing the point, without much moving the center of the conversation. Outside the academy, in the world of getting work done, this position is seen as lunatic, for a variety of reasons: and correctly.

One reason is that practitioners know a lot, act on it, and should act on it, in the near-entire absence of knowledge gained through random clinical trials. It’s easy to in effect count coup on this point, as through the observation that parachutes have not been proved to be effective through trials including a control group not employing parachutes, but it is a real and central issue, and the fact that it is a real position and real debate within academia is enough for many practitioners to write academia off, full stop. To take one example of which I have close personal knowledge, I have participated in the development of an intervention designed to shut down “overt” – public and flagrant – street drug markets in troubled American communities. Such markets often persist for decades and are subject to very large, sustained volumes of law

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5 Sparrow, Kennedy and Moore.
enforcement, social service, and other official attention. The intervention largely eliminates them at a stroke, with large and immediate reductions in crime and increases in various kinds of community well-being. The formal social science evaluations lagged behind the first applications of the intervention by several years. During this time, police executives and other practitioners heard from social scientists that nothing was known about the impact of the approach, and that in fact nothing could be said about whether it had any impact at all, because the intervention had not been subject to a random trial. This meant that scholars were telling these practitioners that it was highly likely that a stand-out community problem, which those practitioners knew intimately, which had resisted decades of various kinds of official attention, and that had vanished exactly at the moment a new approach to make it vanish had been implemented, had likely done so by coincidence: and that the scholars’ judgement that it was likely coincidence should trump the practitioners’ judgement that it was not. Not surprisingly, those practitioners wanted little more to do with those scholars, or with scholars in general.

Closely attached is the converse of this standard: the position of academics that practitioners should limit themselves to interventions that have met this level of proof. Had we a robust portfolio of such interventions, this might be reasonable: but we don’t. There is, in criminal justice, to pick a few from my own area, no such intervention that works to prevent gang membership or overall gang crime; drug use by chronic heavy-use offenders; or overall recidivism by those under active criminal justice supervision. Such issues are of central importance to working professionals; they must engage with them, of necessity and by choice; and to be told that they should do only what is known to work, when nothing is known to work, is not only not useful but alienating.
The scholarly standard for what counts as “evidence-based” impact in random-trial and even milder evaluation designs carries with it other issues. That standard is almost invariably statistical significance attached to some level of impact. What matters here is that those two dimensions are independent. One can, and often does, have statistically significant results at a low level of impact. Such results will count as “evidence-based.” And so they are. They fall, however, into the same area addressed in the discussion of cost-effectiveness: they’re likely to be of no real interest to practitioners. Practitioners, in the main, are interested in the ordinary meaning of “significance:” results that matter. Minor impact, no matter how well grounded in evaluation, will often be seen as beside the point.

Finally, there is the issue that built into the mainstream academic idea of “evidence-based,” particularly when attached to random clinical trials, that it should focus on tightly defined, “transportable” interventions. An intervention that cannot be well-specified cannot be, and is not worth, rigorous evaluation; if that intervention is not carefully replicated elsewhere it is not the same intervention and we should have no confidence that it will work; “evidence-based” practice is thus limited to such interventions. It is the nature of both the problems on the ground and of effective responses to them in important areas of social policy, however, that both the problems and the environment in which interventions are applied can be meaningfully different in different settings. In, again, my own area, gangs are different in Chicago and Los Angeles; so, for that matter, is law enforcement; so, in fact, is the law, at the level of state statute. Will a gang intervention that has been proved to work in Chicago work in Los Angeles?

Since both the problem on the ground and the operational environment are both different, it is likely that the specifics of the intervention will also be different. How are we to judge whether the intervention in Los Angeles is sufficiently true to the original in Chicago? In
practice, one moves to the level of the theory and logic of the original intervention: here is what was essential to the original intervention – this is why it worked – and it is therefore that that must be replicated elsewhere.

The trouble here is that the higher one climbs on the scale of evaluation-methodology rigor, the less one knows about theory, logic, and the actual mechanism of impact. This is true even for the medical clinical trials beloved of public-policy evaluation methodologists: as noted previously, it can be said with great accuracy, given what is possible in a double-blind random-assignment clinical study of a new medication, that it works, and with what degree of impact. That trial need not, and usually does not, tell us anything at all about how and why it works. These are black-box assessments: they do not tell us what is within the black box, or why it matters, or if some of it matters and some does not, which is which. So when practitioners in Los Angeles say, our gangs are different, our communities are different, our law enforcement capacities and legal capacities are different – all of which are true – the evaluations give no basis on which to say, that doesn’t matter, or, that does matter and here is what you should do about it. They give no real help at all, and once again the academics and academic standards held up as preeminent are shown to be beside the point.

*The “not-A” fallacy*

Finally, we will end with a move beloved of authors of peer-reviewed journal articles, but not uncommonly encountered in the field as well. This is familiar from the world of formal logic and its taxonomy of logical fallacies, where it is called “false dichotomy” or the “excluded middle:” the inappropriate consideration of only two in a wider range of possibilities. In this context I’ve come to think of it as the “not-A” fallacy, because of the form it typically takes in
the journals. There, there will be meticulous attention to the core standards of scholarly inquiry: the definition of the question at hand, previous treatment in the literature, the design of the current research, data sources and their strengths and weaknesses, the methods used and their strengths and weaknesses, findings, discussion of the findings: that, for example, a law intended to control firearms and thus gun homicide did neither.

There will then be wild-eyed move to “policy implications:” saying, typically, that this means that the authorities should address the needs of at-risk youth, or rebuild the inner cities, or address media violence, or whatever most appeals to the authors. There is no argument made, no consideration of the range of alternatives, their cost, their impact, their pace of action, their possible unintended consequences, the rational for choosing amonst them: all of the things that make real policy work the challenge that it is. Such “policy implications” are based on nothing except that the intervention at issue failed - “not A” – matched with the preferences or prejudices of the authors. As a logical matter, if “not A” implies any given “B,” it implies all given “Bs”: if the gun law failed, we should drink orange juice, or convert to Scientology, or hold our breath. It is as if the world sums to two received choices, and with the elimination of one, only the other is left. The world, needless to say, is not like that: something that practitioners know well. In fact, the fact that the gun law didn’t work means only that the gun law didn’t work. What to do instead is an entirely different matter, and where the real work begins.

Conclusion

None of this is cause for despair. Far from it, in fact. The common-sense view of practitioners – that they are good at doing things, and probably could use some help thinking about things – is basically correct. The common sense view of scholars – that they are good at
thinking about things, and probably could use some help doing things – is also basically correct. As someone principally in the thinking-about-things business, and devoted to the idea that thinking about things should matter in the world, it pains me to see my extraordinarily smart and able scholarly colleagues unintentionally undercut what they are trying to do out in the world. It causes them pain and frustration, diminishes their standing and influence among those who should value and respect them, and stands in the way of public progress. It seemed worth mapping where some of the tiger traps lie.

What to do about it is less clear, and beyond the scope of this paper. But by way of suggestion, let me echo a comment I’ve heard made by Carnegie-Mellon’s Alfred Blumstein, one of America’s most prominent criminologists, who has a long history of addressing important public policy problems. That comment is to the effect that while practitioners don’t necessarily need a lot of social science, they absolutely need social scientists. What I have taken that to mean is that while important public problems are unlikely to fall to surgical deployments of social science – no single discipline, methodology, study, or key finding is probably going to resolve or dissolve a problem that has persisted this long – we still have a lot to offer. We know a lot of hard-won things. We have studied and can draw on some very advanced ways of thinking about the world and how it works. We are inclined, and trained, to figure out important questions. We are inclined, and trained, in how to find answers. We have standards and traditions that have been designed, and do a good job, to protect against a wide range of likely and often serious mistakes. As much as anything else, we do not have agencies to run, services to deliver, or, usually, life-and-death crises to respond to, which means we have the inestimable resource of time.
It is only when we elevate those virtues to fetishes that we get in trouble. Practitioners don’t need to be told that they’re incompetent and can’t think; they aren’t, and they can. They don’t need to be told that we have methods that will crank out answers to problems they’ve spent careers struggling with; we don’t. They don’t need to be told that everything they know is beside the point: it isn’t. They don’t need to be told that matters they know as infinitely complicated are simple: they’re not.

What practitioners need is partners who can help them think things through. As academics, since we don’t have public budgets, run agencies, or command large numbers of personnel, we need partners who can get things done. There is a natural alliance evident here, which when realized in practice has frequently been pretty successful. The academic policy fallacies, with the distance, separation, and frank hubris they imply, are impediments to that common-sense partnership. Like their equivalent logical fallacies, they should not be tolerated in legitimate discourse.

7 The New Criminal Justice, other refs