

## Interrogations #8. Methodological Pluralism

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### 1. Gocken Coskuner

Flyvberg suggests an alternative to the dominant form of social science research. He argues that social science should not be concerned about generating generalizable theories or should not try to emulate natural sciences but rather should seek to develop particular/situational practical solutions. He supports phronesis against scientism and argues that if scientism holds as a way of thinking social science will increasingly degenerate as a scholarly activity. He sees in terms of phronetic qualities social sciences are stronger than natural sciences. He suggests that social science should not emulate natural sciences and that social science cannot generate theories in the same sense that natural sciences do and that the two should not be compared. I agree with him that social research should be involved in relevant and practical research. However, there are some issues I would like to raise about his argument.

First, I am thinking instead of focusing on phronesis could he have suggested the epistem of social sciences are different than that of natural sciences. And I know that by saying this I might falling into the same mistake that Schram argues Laitin does which is understanding of phronesis as a form of knowledge that is not distinctive from episteme. But the point I would like to raise is related to the definition of epistem provided by Flyvberg. He defines epistem as scientific knowledge, universal, invariable, context independent. Defining epistem as such makes it impossible of talking about epistemology of social sciences. So again my question is if we were to define epistem different, i.e. variable, context dependent first, could we still talk about scientific knowledge, and second would there be need for such a demarcation focused on epistem (theory) and phronesis(practice)? **[I suppose one way of framing this is to see the scientific status of knowledge as a multidimensional continuum rather than a simple binary configuration. If one thought of it that way, then social science would fall short of physics in terms of the three dimensions of knowledge as universal, invariable, context independent, explanatory and predictive, but would still be somewhere above the zero-point on each of those. I agree with you that the sharpness of F's rejection of episteme for social science is not convincing.]**

The second point I want to raise is also related to the demarcation Flyvberg suggests between epistem and phronesis. While suggesting that social/political research should have phronesis orientation to have practical outcome Flyvberg seems to overrule any option that research focused on epistem cannot have any practical use. **[What he seems to argue is not so much that epistem has no practical use, but that it cannot contribute to value-rationality, to practical use infused with values. This assumes that value problems never hinge on any matters of fact about the world, which seems too strong a claim.]**

Third, in terms of sociological pluralism I think neither Laitin's tripartite methodology –which provides only methodological pluralism, nor Flyvberg's phronetic research are fully helpful if what Flyvberg proposes is to abandon the search for causality. Schram provides Toulmin's take on the issue saying : "...we do not want to simply replace an emphasis on abstract rationality with an exclusive focus on situated

reason. Instead, we want to redress the imbalance and allow for studies that enhance our ability to develop situated reason to have equal footing with those that encourage us to think in terms of abstract rationality and its related principles about generalizable relationships, casual or otherwise” (Politics and Society, p. 430). **[I am not completely sure about the “equal footing” claim here when stated independently of the specific problem being addressed. It would seem to me that there are some questions for which abstract rational is more important than situated reasoning, and others when the opposite is the case, and I have no idea whether or not this means that over all possible questions/problems they have equal footing.]**

Fourth, in terms of case methodology, I agree that it can allow for situated knowledge and a deep understanding of the experiences of the local however I think they provide better understanding when several cases are compared as opposed to say Harvard Business cases as noted by Flyvberg. In that sense although I did not read Scott’s work *Seeing Like a State* the methodology he applied seems noteworthy. **[Somehow Flyvberg seems to think that the practical judgment/wisdom one can derive from a case study will get diluted when the case study gets embedded in a comparative, multicase analysis. The very act of analytically reconstructing the cases – which is essential for their use in systematic comparisons – robs them of the phronetic capacity. There may be something to such a claim – perhaps simply to the character of the insight you get from reading a novel vs reading literary criticism about the novel. Both may be valuable, but often the latter detracts from the former.]**

Finally, Flyvberg asserts that in his analysis social science is nonparadigmatic, it is neither cumulative nor relatively stable. Suggesting non-cummulation does he also mean that phronetic research produced at one point in time and its results cannot be used or not relevant in other cases. I mean, although each case can be unique in terms of power dynamics, social actors etc. there may be some patterns that can be observed in other settings that allow the results of the research to be applied in others. Does Flyvberg overrule this option? **[I really don’t know precisely what he means by noncumulative. Taken literally it means that we never learn something from one case that enables us to begin the study of a second case in a better position than we were with the first case, for if we are in a better position to understand the second case then there is something cumulative. I don’t really understand his point here.]**

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## 2. Fabian Pfeffer

**“Where has all the knowledge gone”: Flyberg in 50 years from now.**

*Having read Flyvbjerg’s “Making Social Science Matter” and especially his response to Laitin’s fierce critique of it, it is not Flyvbjerg’s argument for a peculiar qualitative method that strikes me as the most challenging point in his work. Unlike Laitin claims, Flyvbjerg’s work is not mainly a challenge to and call for the replacement of standard quantitative research. As well as Laitin himself, he can also be seen to make a point for methodological pluralism, the peaceful coexistence of both approaches. I think this is*

why the theme of my interrogation unfortunately has to deviate from the headline given by the syllabus. Instead, my main puzzlement revolves around the possibility of cumulative knowledge in “phronetic social science” and progress in that kind of “science”, if we want to continue calling it that way. **[I agree that the core of the book is not the headline of the section – I concocted the session based on having read Laitin’s review and not yet the Flyvbjerg book. A professorial peril....]**

Flyvbjerg describes *phronesis* as the “knowledge of how to behave [in the 2004 paper: manage] in each particular circumstance that can never be equated with or reduced to knowledge of general truths [about managing]. Phronesis is a sense of the ethically practical rather than a kind of science” (p. 57). Phronetic social science is therefore “context-dependent” and “pragmatic”. It has “in the final instance nothing else to offer than concrete, context-dependent knowledge” (p. 72). The question then is: Through which mechanisms does the phronetic social scientist gain this specific form of knowledge? Flyvbjerg’s answer: context-bounded experience and learning through dialogue between “in principle anyone interested in and affected by the subject under study” (p. 408), i.e. contributing to and listening to the “desired polyphony of voices”. Taking into account Flyvbjerg’s accentuated reservation (if not hostility) towards the general, or the abstract because it “leaves us helpless in the face of the very differences we need to explore”, one might go on asking about the ways of transmission of this practical knowledge. As the experience gained from a specific project shall not lead into general knowledge, the only medium through which it could outlive the specific project is the *experience* of the respective scientist. Yet, this context-derived experience seems to be bound to the individual scientists and not transferable to other contemporary (leave alone succeeding generations of) scientists. **[Flyvbjerg does not rule out vicarious experience as a source of knowledge -- thus his advocacy of fine-grained, detailed narrative single case studies which are not overly structured analytically. Such writing can induce significant parts of experience needed to generate phronetic knowledge.]** In the framework given, I cannot conceive of a “phronetic scientific community” that outlives the specific project and the involved individuals. The *refusal of abstraction* on the other hand also implies the separation of phronetic social science from the existing scientific community which is seen as engaging in the unfruitful construction of universal knowledge (episteme). **[I think Flyvbjerg really takes a somewhat contradictory stand on this issue, for in some parts of his exposition he defends quantitative methods and claims that there is much to learn from survey research, thus suggesting that what is needed is a balance between phronetic and epistemic research. But in other places he seems to reject the very possibility of an epistemically coherent social science, so it would seem that there would be no point in quantitative large-N methods. Such methods cannot yield phronetic knowledge, and epistemic knowledge is impossible. So why bother?]** As we have seen, the purpose of Flyvbjerg’s social science is “not to develop theory” but “to produce input to the ongoing debate and praxis in politics and administration”. The creation of cumulative knowledge therefore seems to be neither desirable nor possible. At best, it would enrich the valueless treasure kept in the ivory tower of current social science.

Yet, here is where I see the key ground for a fruitful engagement in social science. Constant exchange and mutual fertilization between the sphere of general knowledge and the sphere of concrete, practical, context-sensitive knowledge seems to be the way of enhancing *theory that matters*. (Ideally this even does not have to be an exchange between two distinct camps but could be carried out by one and the same person). Let us for instance think of Merton's reference group theory. Isn't it the case that this potent theory bases on the generalization and abstraction of Stouffer's (context-specific) findings in "The American Soldier"? In the sequel, this theory was formalized (what a dreadful thing for a phronetic social scientist) and its applicability specified by Boudon. But also, its central component, the concept of relative deprivation, has been successfully applied to a range of further specific research questions, Davie's explanation of revolution still being one of the more general enterprises. Ultimately, Flyvbjerg's proposals seem to be guided by a general dissatisfaction with "grand theories" (see our discussion of Hedström/Swedberg). What about middle range theories which might draw on experience from specific cases and be a fruitful reference and inspiration for a range of connected specific (and yes, also practical) work?

While I can clearly see how Flyvbjerg's social science really does matter and by that surely is attractive, this approach can neither be the sole way to gain knowledge nor the sole source of generating relevant advice (I especially do not deny the great potential of this multi-level dialogue approach to policy advising). Due to its rejection of abstraction it forbids the accumulation of knowledge which is - in my opinion, probably not in Flyvbjerg's view - ultimately necessary for progress even in phronetic social science. On the other hand, "abstract" social theory will most likely profit from growing along specific cases and experiences. In sum, opposing *knowledge of how to manage to knowledge* does not seem promising for either of the two sides. One might also add that the mutual inspiration of the two might well be dependent on the subject matter under consideration. Who knows, probably social practitioners in the field of city planning can learn predominantly from the repeated polyphony of voices and less from abstract theoretical reasoning! On the other hand, the analysis of capitalist modes of production may call for stronger abstract contemplation and still be able to answer the three most salient "value-rational questions" raised by Flyvberg (p. 60-62). **[The tricky issue here is whether or not attempts at generating the abstract, rational, predictive/explanatory theory – even of middle range – subverts the effort at generating the kind of practical learning that Flyvberg sees as central to phronesis. Remember that the whole argument is framed in terms of the five stages of learning. I think – but am not completely sure – that F believes that analytical/abstract explanation contributes to blocking learning from moving from level 3 to 4. That is what he claimed in the mathematics-chess argument: too much mathematical knowledge, and the attempt at using that way of thinking in playing chess undermines the learning capacity of becoming an expert.]**

Two side issues:

1. One of Flyvberg's main suggestions is to include values and power-relations in every analysis. Is this really innovative? Has it been answered why case studies should be the only method to account for these?

2. Flyvberg claims to have found the golden way between foundationalism and relativism when focusing on values (p.130). I cannot see how the proposed “situational ethics”, also named “Sittlichkeit (ethics) rather than Moralität (morality)” (p. 136) in any way differ from ethical relativism?

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### 3. Matt Desmond

I have two questions this week revolving around the exchange between Laitin and Flyvbjerg. Although I find Flyvbjerg’s critique devastating **[One of the things that is interesting in this exchange of views is the difficulty in nailing down precisely what Flyvberg’s deep views are on certain crucial matters, and this invites misrepresentation. Here is what I mean: It is true that there are passages in which Flyvberg says things like “there s much that can be learned from survey research” and in which he says quantitative methods are important. Thus, when Laitin says Flyvberg rejects such methods and is hostile to large-N research, Flyvberg can certainly point to passages where he says the opposite. On the other hand, the strong, unequivocal attack on the possibility epistemic knowledge in the social sciences and his rejection of the ambition of scientific explanation and prediction as plausible goals of social science, suggests that such research is pretty useless. Since large-N quantitative research (let alone formal models) cannot generate phronesis, and since epistemic science is not possible for the social sciences, then in fact it seems Flyvberg does disparage such research. Laitin, therefore, may be correct in the substance of his attack on the book even though Flyvberg has covered his ass by formally including space for quantitative work]**, I would like take up the question of phronesis in social science by first inquiring about ideas of multiple methodologies. Both authors *rhetorically* agree that the ‘tripartite method’ can improve social science research by an incorporation of statistics, formal modeling, and narrative, although Laitin uses narrative more as a supplement to the ‘superior’ method of statistical analysis as Flyvbjerg correctly points (p. 395). **[I am not so sure that Laitin really does imply a hierarchy: he allows that narrative can correct the explanatory failures of formal models just as formal models can improve the knowledge content of narrative. Narrative isn’t just a “supplement” if, in the absence of narrative, false models will remain uncorrected.]** Most scholars (Laitin aside) would not claim that one well-founded research method should be done away with, but most would advance the notion that one should be done more often than another, have equal (or more) respectability than the other, or be incorporated more with the other (“mixed methods”). If we concede that different types of methodologies are useful for answering different types of questions, then there seems to be a serious problem when scientific capital is directly correlated with a single type of methodology manifest in funding sources, publishing opportunities in flagship journals, the number of scientists employing these methods, or, especially close to home, required classes. This is the central bone the perestroikans have to pick with political science, and I am very sympathetic to their arguments. **[I think you are right that if there was balance in funding and intellectual support, that there would be little impulse for the perestroikans to attack, but their attack is not just for a**

**balance; they denounce rational choice theory and formal models as pernicious, misleading, destructive, etc. They argue as if these should be dropped from the discipline, that Political Science would be better off without them.]**

Now, the conventional way to respond, demonstrated by both authors, is to vie for a multiple methods approach within each social scientist's research. Notice that neither one of the authors vie for a *discipline* that is more pluralistic with methods in general (in fact, Laitin criticizes—with a tinge of snobbery, I might add—this idea); rather they advance a claim that quantitative and qualitative (or whatever other arbitrarily polarized antimonies fit here) should be incorporated more into individual research agendas. So, here is my first question: Is there a danger to be found in the seemingly innocent cry for mixed methodology? Do all proper sociological questions require more than one method? Might we suspect that the multiple methodology charge might lead to research programs that include more methods that are watered-down? Would in-depth ethnographic investigations take a back seat to interviews if the researcher also incorporates statistics? Likewise, might we suspect that advanced statistical models might be replaced by less-advanced models if the researcher incorporates interviews? Although these questions sound a bit leading, I have not yet formulated an opinion here. **[I don't think anyone insists that each researcher must actually do all three of these, but rather than the process of knowledge-enhancement should take into account all of these. Thus, in the civil war in Sri Lanka example, the idea is not that Tambiah should have himself done a large-N statistical study of all violent ethnic conflict since WWII, but that his analysis and interpretation of his case would have been improved if he had included the results of that research in his own thinking and analysis. The research community needs to involve systematic learning across these genres of research, and this learning should then inform how each type is practiced: that is, large N studies should be different by virtue of the fact that the scholars have carefully read the narrative research, and both should be different because they have argued with the formal models.]**

Second, and I'll make this one shorter, if we agree with Flyvbjerg that social science needs to incorporate phronesis into its research enterprise, and phronesis-driven research is value driven-research, and the drive behind all social scientific research is the initial question, then where do phronetic questions come from? Who defines what values are important for social scientists? Although Flyvbjerg draws heavily off Bourdieu in *Making Social Science Matter*, he seems to sidestep Bourdieu's strong idea that "good intentions make for bad sociology." Do we allow the politicians to drive what is a valuable value question, much like the sociologists who in the past few years (with great publishing success at large press houses, surprise, surprise) have shifted their interests to terrorism? Do we allow voices from a said 'public sphere' to shape our values, and if so, whose voice is heard and whose drowned out? I do not think I need to further elaborate on the problems of turning *social* questions into *sociological* questions, but then, if we only rely on sociology to drive sociological questions, is this not a revert back to episteme? **[I don't know where Flyvberg feels the values should come from that drive the research question, but I suspect that it is embedded in his learning model – the five stages that lead to one being an expert in managing the practical tasks of a**



**situation. Perhaps he has some notion that the values are discovered through such value-rational expertise – that is part of what the wisdom and judgment of the expert entails. But I'm not sure.]**

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#### 4. Matt Dimick

As I recall, Flyvbjerg's main case for abandoning a logical, explanatory (and predictive, he often adds) social science is that such a social science would require the elaboration of context-independent rules; but that since human action is context dependent, non-rule based, intuitive, experiential, etc., such an elaboration is (likely) impossible. Flyvbjerg bases his case on a variety of research and examples, for instance, the research by the Dreyfuses' on experts and skill learning; he also mentioned chess playing and soccer playing. From these examples, he concludes that social action in general works this way, i.e., all kinds of social action is context-dependent, intuitive, etc. These arguments raised a number of questions for me.

First, I wondered if Flyvbjerg was generalizing wrongly from special cases. Why should the kind of social action we are interested in explaining be understood like these examples of highly skilled (and sometimes creative) kinds of action? My assumption would be that the more creative a kind of action is, the less possible it becomes to explain it in a logical, rule-based way. We've already discussed this semester the distinction between the context of discovery and the context of justification, and how the former defies efforts and logical, deductive explanation (although not completely beyond social scientific interest as Kuhn demonstrated). Action that is less creative but is highly skilled may occupy a middle position. In these cases, I may agree with Flyvbjerg. But whose to say that more mundane, quotidian social action is similar to creative or skilled action? If much of such action is the simple adapting of the best means known and available to achieve certain ends, for example, then it would be possible to describe such action and use it to explain social phenomena in a logical, rule-based way. **[I think Flyvbjerg would insist that even the most mundane of actions of the sort captured by Bourdieu's *habitus* involve expert-skills of the sort he describes, and thus are laden with the nonrule judgment/wisdom/subtlety that kills efforts at abstraction and analytical definition. What is less clear to me is why this in fact makes explanation and prediction in a relative sciencey way impossible. It all depends upon precisely what one is trying to explain and predict. All of these experience-intensive highly skilled capabilities may not be all that relevant for the forms-of-variation one is trying to explain.]** Second, even if social action occurs intuitively, or "bodily," such that the actors themselves typically don't describe their action in a logical, rule-based way, I'm not sure that would defeat attempts to model or explain that behavior in such a way. In fact, as a *descriptive* matter, I find Flyvbjerg's understanding of human action to be more "realistic" than, say, many rational choice accounts. But people's actions could be modeled with the use of rational choice theory, even if the actors themselves did not see their action in this way. A rational choice model, like all good models, makes certain assumptions and simplifications. (But I don't believe taking such a position commits you to the strong argument of Milton Friedman. He seems to say something more than just praising the benefits of simplifying models; that prediction is all that matters and that

unrealistic models are in principle better than realistic ones. My assumption is that simplifying models are still trying to say something accurate descriptively, just that one can't capture all of the realism; or all the realism one may like *if* one's goal was a descriptive understanding and not just or instead of an explanatory account. In *Social Mechanisms*, Hedstrom and Swedberg argued that even thick, narrative descriptions do this to some extent—any account ends up making some simplifying assumptions.) **[More generally, if the purpose of social science is to identify real mechanisms, I do not see why this is blocked by the fact that actors are phronetically skilled experts. Capitalist labor markets still constitute a mechanism through which people without means of production obtain their means of living, and this explains all sorts of things. Of course people become very skilled at this, they develop complex practical reasons as they navigate the labor market, and for some questions that would be of fundamental importance, but I don't see why a priori it is fundamental for every question.]**

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## 5. Matías D. Scaglione

*Towards a general critique of social science?*

I have followed and supported the Post–Autistic Economics network (PAE) since its inception, back in 2000. Like the Perestroika Movement (PM) in political science, the PAE channels the increasing discontent of students, professors and researchers with the present state of the discipline. Both movements' promotion of plurality and diversity of opinions within the production and reproduction of knowledge would seem to be a reasonable and laudable defense of free thinking, had not been for the intemperate responses of some representatives of the hegemonic traditions. The intemperance in the responses has to do, I believe, with the lack of critical debates in such closed “communities”. Although I am not acquainted with David Laitin's work, his review essay on Bent Flyvbjerg's *Making Social Science Matter* depicts him as an author more interested in defending his “method” than in critically confronting it with Flyvbjerg's proposal. In my opinion, this kind of attitudes reinforces the authoritarian and repressive aspects that *economics* is transmitting to the rest of the social sciences, in particular to political science (this is one hypothesis that I would try to prove elsewhere).

Although I do not share either the axiological tenets of Flyvbjerg's proposal nor his insistence in keeping the separation of political science from social sciences, I would like to discuss (the big issue of) the potentiality of *phronesis* towards a general critique of social science, i.e. the capacity of living experience and practical wisdom to transform social sciences. In my opinion, Flyvbjerg demonstrates that his proposal goes well beyond the topic of this session, “Narrative methods and Methodological pluralism”. **[You're absolutely right that the core argument in Flyvberg is much more than methodological pluralism. That was my characterization of the issues before I read the book. Still, if one takes his attack on the epistemic possibility of social science seriously, and embraces the phronetic model of knowledge, then it isn't all that clear**



**to me what contribution formal models and quantitative research would make to this phronetic anti-epistemic social science. ]**

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## 6. Elizabeth Holzer

Flyvbjerg argues that social phenomena is so contextual that researchers will not be able to use the idealized scientific method to understand it—I think that’s true. But Laitin raises a reasonable, if unsatisfactory objection: “Appealing to context is merely to say that we have not yet discovered the various factors or the interaction of factors that produced outcomes of significance” (Laitin 168). I find Flyvbjerg’s conclusion intuitively plausible but his argument is inadequate to counter Laitin’s criticism. How do we know that social explanations can’t be predictive? Does anyone have some more substantial support for this Flyvbjerg’s conclusion that I could add to my arsenal? On related note, what degree of generalization does Flyvbjerg think is attainable—he seems to be tacitly supporting the search for mechanisms, but I’d like to have a better sense of what he would consider adequate social research. How should we respond to the challenges that context and reflexivity present to social research? **[A couple of points here: I think Laitin and Flyvberg are using the term “context” differently. Laitin sees context as the concrete conjunctural interweaving of complex elements/factors. Context is the same as concrete contingent complexity. This is why he can characterize it as “interaction” and “nonlinearity”. Flyvberg means something much more specific than this in defining the *social* context: context is the practical action-relevant wisdom and judgment of actors in the situation. Context is primarily a way of describing a particular quality of subjectivity of actors (I think). It is something unobservable and can only be apprehended from ongoing experiences within the situation. This idea of context then is closely tied to his learning model with practical non-rule based expertise as the highest level. Because this kind of knowledge is noncodifiable and nonrule following, it cannot be turned into variables or stable components of explanatory theories. I think this is what it is about context that makes understanding of context impermeable to scientific abstraction and thus to epistemic science.]**

My second concern is about the claims to legitimacy that social scientists have within Flyvbjerg’s framework. Flyvbjerg sets a very laudible goal for social scientists: “The goal of phronetic political science is to produce input to the ongoing dialogue and praxis in politics and administration” (Flyvbjerg 408). But why should policymakers listen to social scientists? We no longer have the scientific kind of legitimacy, whatever that’s prefaced on. The framework places high value on experience—“More than anything else, *phronesis* requires *experience* (403). Is there something about the practice of social research that gives social scientists more experience? Otherwise, I don’t see what gives information collected through social research its weight, and I think Flyvbjerg is digging himself into a hole. On a related note, he’s opening up the possibility that the proper delivery of social research will be extremely varied—appeal to empathy and other emotions are no longer inconsistent with the ideals of social research. That is, in embracing values and judgments into social research, it seems to be that he is opposing

the hegemony of objectivity—which is nice. But that doesn't that imply a rejection of the (Habermasian) ideal of critical objectivity in public deliberation—unless values and ethics are just more “facts” to add to the equation? **[You reveal some pretty deep problems in Flyvberg's proposal, I think. It would seem to me that rather than deny the epistemic potential of social science one could say that social science has both an epistemic and a phronetic dimension of knowledge, and this is what makes it social science as opposed to social commentary. The key problem is to integrate the two, rather than to privilege phronesis or reject epistemological aspirations. This is why I think F's rejection of “theory” and his insistence on the impossibility (or at least implausibility) of paradigms is not convincing.]**

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## 7. Wayne Au

I'm not sure I like how the Dreyfus model constructed. It makes sense in the most linear way, but I do not like the rational/arational distinction between levels 3 and 4. **Isn't equally possible that arationality exists at levels 1, 2, & 3, but that lack of knowledge of procedure or lack of experience make arationality/intuition wrong or just so weak that at that point it takes a back seat to rationality? Similarly, could then the “expert” level means that experience, knowledge, and understanding have come together as an arational whole? Isn't it possible to construct arationality as a level of super-rationality that is so ingrained, so embedded, that understanding and action comes without having to be rational? [It is important that Flyvberg uses the term “rationality” rather than “reasonable” or “sensible” or something like that here. Rationality means calculating the best strategy for given ends. He really means instrumentally-rational here. The claim that expert knowledge is arational (but not irrational) is simply that there is no such calculation involved, no application of a rule to achieve the best result. In a different use of the idea of rationality, where all that is required is the selection of the best strategy or action, but not the selection being the result of calculative application of rules, then the expert is certainly rational. The Grand Master chess player selects the best strategy, even if intuition and tacit knowledge figure prominently in the gestalt perception and configurational reasoning of selecting a chess move. What is being contrasted here is a cognitive procedure of rational-calculation vs intuition, where good intuition is viewed as a high form of cultivated learning, rather than some lower-order cognition.]**

Flyvbjerg asserts that “predictive theories and universals cannot be found in the study of human affairs. Concrete, context-dependent knowledge is therefore more valuable than the vain search for predictive theories and universals.” (p. 73) **Does this mean, for instance, that we cannot assert that capitalist production and accumulation of wealth is based on exploitation because that is too universal of an assertion for something that is a human, socio-economic relationship?** (I think Flyvbjerg would argue that we could not make such an assertion, ever.) **Further, doesn't Flyvbjerg's assertion basically deny that there are concrete, external, social**

**processes/mechanisms that are happening objectively outside of human perception?** I'm holding out for how he intends to work his way out of the subjective idealist/anti-realist blind alley, but for his constant attendance to the idea that he isn't there, I think he still is. **[I agree with you: there seems to me lots of social processes which have the required abstract quality to generate predictions and explanations, even if the actors within those social process have all of the phronetic capacities and practical wisdom that Flyvberg attributes to them. Our predictions may, of course, be contextual in a different sense from Flyvberg – they may be conditional upon an array of concrete elements of a situation. But that kind of context is certainly amenable to scientific exploration – as Laitin suggests.]**

In his discussion of the importance of case studies, Flyvbjerg asserts that "...experts do not use rules but operate on the basis of detailed case-experience. This is *real* expertise." (p. 85) **Couldn't we also make the argument that experts, while operating on the basis of case-experience, draw generalizations from their vast experience which allows them to act arationally?** If not, then wouldn't experts be caught referring back to every single case in order to piece together how to handle a specific situation? (This would be different than rationality because they would be referring to specific cases and not rules.) **[The point is that they cannot formulate the generalizations as abstract principles to be applied to new cases – they have those principles embedded in their phronetic capacity as experts, but the principle extracted from the many concrete cases cannot be formulated as abstract rules that can be applied by others.]**

In his section on methodology, Flyvberg states that, "...the socially and historically conditioned context...constitutes the most effective bulwark against relativism and nihilism. Phronetic researchers realize that our sociality and history is the only foundation we have, the only solid ground under our feet." (p. 130) **Does Flyvbjerg consider our "socio-historical foundation" to be real and stable enough to make that the standard framework for truth claims? Is he just saying that we cannot make universal claims across human history, but that we can make universal claims relative to our current "socio-historical foundation"? That there is rational truth, but only grounded in context? [The relativism he wants to combat here – I think – is moral relativism. And I don't see how he gets that from his contextually grounded knowledge, phronetic or otherwise.]**

## 8. Brett Burkhardt

What I find most interesting in Flyvbjerg's phronetic social science is its goal, which is "*to produce input to the ongoing social dialogue and praxis in a society*, rather than to generate ultimate, unequivocally verified knowledge (139, my italics)." I think Flyvbjerg is not sufficiently critical in discussing this goal, particularly when he presents his work on the Aalborg Project. The process of inputting discoveries into public dialogue is more problematic than Flyvbjerg states. In order to fully acknowledge these

difficulties, we should consider treatment of research discoveries once they arrive in public dialogue, and also access to the public dialogue.

Flyvbjerg does acknowledge that discoveries (or interpretations) will be subject to a wide range of reactions once they arrive in the public dialogue. “The significance of any given interpretation in a dialogue will depend on the extent to which the interpretation’s validity claims are accepted, and this acceptance typically occurs in competition with other validity claims and interpretations (139).” Interpretations may be refuted, slandered, ridiculed, falsified, etc. It is necessary for Flyvbjerg to acknowledge this, because these actions and responses within the public dialogue will have real consequences for peoples’ praxis.

Flyvbjerg, however, does not sufficiently address a related and perhaps more dangerous problem: highly restricted access of interpretations or research discoveries to the public dialogue, and consequently to praxis. Happily for Flyvbjerg in his struggles relating to the Aalborg Project, he seemed to have easy access to social dialogue. The project was an ongoing governmental venture, which in itself ensures some degree of public debate or deliberation. Additionally, Flyvbjerg (or members working on the Aalborg Project) seemed to have little trouble tapping into mass media (radio and newspaper). The only problem in these cases was how Flyvbjerg’s interpretation would be presented in the ongoing dialogue.

*But what if there is no ongoing dialogue?* What actions should phronetic researchers take? How far should a phronetic researcher go in his or her attempts to take research into the world of social dialogue and praxis? I was left unclear as to whether Flyvbjerg would go so far as to claim that drastic and openly political actions are among the proper tasks of a phronetic researcher.

**[I imagine that Flyvberg would reject the possibility of any general, abstract formulation of the problem of how to accomplish dialogue, since this must certainly be the sort of thing which is highly context dependent (in his sense of context). In any case, the specific strategy for entering into dialogue would vary with the nature of the setting and the nature of the relevant actors to constitute a “public.” Flyvberg’s approach is quite congenial to various forms of participatory research and action research in which the research process is itself embedded in struggles and activism, so I don’t think the fact of isolation from official public venues would matter that much.]**

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## 9 Mara Eisch-Schweitzer

Using the Dryfus model of *novice to expert*, Flyvbjerg effectively correlates the analytical rationality (which produces the epistemology of natural science) with the concrete, rule following skills of the *competent performer* (level three). He also equates the value-rationality (which evidences the ontology of the social sciences) with the *proficient* and *expert performer* (levels four and five). This seems to imply that hierarchically the social sciences use a higher level of thinking than the natural sciences.

This point does not seem to be well developed within the text. It would seem that the ‘power’ of the social sciences is in the recognition of this higher order thinking. **[Do you think it is plausible to claim that social sciences involve a “higher order” of thinking than, say biology? My sense is that there is a lot more total gibberish in the social sciences – the painful elaboration of the obvious – than in natural sciences, and also (uncharitably) a higher “bullshit quotient”. This could be consistent with the higher level thinking postulate – it could be precisely because it demands higher level thinking that most people fail to do interesting/insightful social science. In any case, my guess is that natural sciences just as much as social sciences also involve this kind of expert tacit knowledge – the ability to have a complex intuitive grasp of multidimensional complexity that is not strictly governed by codified rules. I imagine that this is crucial to the intellectual leaps that are involved in the “logic of discovery.”]**

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## 10 Mark Cooper

One concept that seems central to Flyvbjerg’s project, and to Laitin’s critique, is context. Laitin claims that Flyvbjerg “never actually defines it,” and proceeds to discuss a somewhat commonsensical notion of context. Laitin takes context to be, “a skein of interwoven factors,” and that, “one purpose of social science is to disentangle such skeins in order to trace the effects of separate strands, or to examine the impact of particular interactions among strands.” Laitin succinctly calls Flyvbjerg’s appeal to context, “a cop out.” (L168) Flyvbjerg seems to be working from a much different notion of context though; the details of which I found insufficiently clear in MSSM. In his reply to Laitin though, he calls the relevant context to social science “human beings’ everyday background skills.” (F396) The need for a *phronetic* social science is ultimately rooted in its objects of study being “self-interpretive.” It is unclear how this premise differs from the ideas of other theorists (especially Giddens and the double-hermeneutic) among which Flyvbjerg situates his project. (MSSM32-35) On page 47 Flyvbjerg reiterates the four arguments about theory in social science and locates within each a component of his idea of context; yet even here there is not an explicit demonstration of how his concept is more than an amalgamation of Giddens & Bourdieu or how the concept encompasses more than “background practices.” (F397) **[You give a nice characterization of Laitin and Flyvberg’s use of the idea of “context.” Frankly, it seems pretty odd to me to use the term “context” to describe the situated tacit knowledge and skills that constitute actors practical functioning. Laitin’s definition is the more standard one. I could understand Flyvberg’s usage more if he said something like: “the distinctive feature of social contexts, which make them so difficult to codify and subsume under any general rules, is the specific dimension of human tacit knowledge and practical skills.” This may be a particularly salient aspect of context, but it doesn’t seem like meaning of context. In any case, I agree with you that there is nothing particularly novel about this definition. What is perhaps novel is the claim that by virtue of this, explanation becomes impossible in the social science.]**

I also find Flyvbjerg's negotiation between of value-questions between the poles of foundationalism and relativism to be unsatisfactory. While I find the existence of a situated condition in which social scientists do their work as unproblematic, the aim of doing research that provides "better" interpretation based on shifting notions of better seems underspecified and undifferentiated from relativism. While Flyvbjerg describes his value orientation as "pragmatic," it is unclear to me that such a description does not merely conceal unequal power relations that structure these sets of validity claims. While this is not relativism in the 'anything goes' sense, it shares relativism's failure to meet any standard not produced by those in care of some about of discrete, and often hegemonic, power. How then, can a dialogical and phronetic social inquiry generate critiques of dominant modes of social organization and discourse when the evaluation of such inquiry is inevitably judged by the same social organizations and discourses? **[I am not sure that F's use of the term "pragmatic" implies that the validity claims of the phronetic research have to be acceptable to powers-that-be. I would assume that Flyvberg would want to allow pragmatic value-rational research to emerge from dialogue with popular social forces and marginalized groups. I agree that this is all pretty vague, but to the extent that there is a Habermasian undercurrent here, perhaps the ideal is pragmatic validation in the context of something like "ideal speech conditions"? ]**

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## 11 Ana Cristina Collares

I enjoyed reading the controversy between Laitin, Flyvbjerg and Schram, and got enthusiastic about Flyvbjerg's ideas on phronesis.

Laitin accuses Flyvbjerg of creating normative science (or "non science") with his proposal of focusing in science as a "reflexive analysis and discussion of values and interests aimed at praxis".

In his answer, Flyvbjerg uses Weber's concept of value rationality to claim that phronesis and says that "The goal is to help restore social and political science to its classical position as a practical, intellectual activity aimed at clarifying the problems, risks and possibilities we face as humans ... and at contributing to social and political praxis". Although I disagree with most of Laitin's criticisms, in this case I see a conception of "good" and "bad" society implicit in Flyvbjerg's proposal, that makes it normative. To make value judgments about the political praxis is a different thing than to use your value-judgments to delimit the object of scientific study, as Weber proposes. The first case is a normative proposition, and I do not think phronesis could escape being normative without falling the extreme opposite, i.e., being totally relativist. If the focus on values recommended by Flyvbjerg is not a normative proposal, then or it is relativistic or it is not different at all from *episteme* - in the sense that questions about "Where are we going?" or "Who gains and who loses" can perfectly be asked by formal scientific reasoning. The problem is with the last "core" question of phronetic political



science: “What, if anything, should we do about it?”. My question here is: *How can we answer this question without being normative? Is Flyvbjerg really rejecting this normative character of his phronetic science, or does he accept it as an unavoidable consequence of dealing with human subjects that react to scientific findings about themselves?*

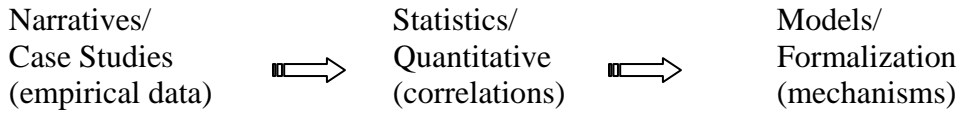
I would like to understand better the difference between having “situational ethics” and relativism, because, sincerely, I don’t see much difference here. **[I agree that this is murky, and I am really not sure what is being claimed. Flyvberg somewhere does invoke the idea of things which are “harmful to humans” (I couldn’t find it, but I recall reading in the book, I think....) as if this is something that we could learn phronetically – through wisdom and judgment in experiencing the concrete circumstances of people’s lives. What this means, perhaps, is that phronetic knowledge of a social context enables one to make valid normative judgments that are distinct from foundationalist judgments (i.e. derived from abstract rules) or relativist judgments (derived from simple description of what people consciously think). This notion of practical Judgment is central to the phronetic enterprise, and I think the notion is that this can be applied to normative issues yielding normative answers that are neither arbitrary nor abstractly derivable. But this may not be what is being claimed.]**

A second point that I would like to clarify is related to Laitin’s proposal of a tripartite scientific methodology, i.e., the interdependence between formalism, statistical calculation and narratives. Although I am very favorable to the idea of combining different methodologies in order to grow in depth and precision of a sociological study, I do not see a strong point in Laitin’s proposal. Is he saying that a “scientific frame” can only be attained by the use of these three methodological approaches in a sequence, directed to the same phenomenon? **[I don’t think there is any claim to a unique sequence. Rather the claim is that all three are needed as part of the scientific community of mutual correction of errors and deepening of insights.]** It seems to me that there is a widespread consensus in social sciences about the idea that certain facts can be better approached with certain methodologies, and some others with different methodologies. I do not know how Laitin would answer to this. **[The argument is not about facts but about explanations and theories. This is well illustrated in the example of the Sri Lankan violence, where explanatory errors are made by the ethnographic research because it was not informed by the large-N study. In another example he shows how an error in a formal model was corrected by the insights of a narrative case study. I think these are genuinely a tripartite method of mutual critique and correction]**

But my question is more simple than this. It is about the constant association, in Laitin’s writings, between the ideas of formalism and mechanisms. I also found the association between formalization and hypothesis-testing. Can we say, at least in Laitin’s view, that *the formalizing a sociological subject/phenomenon is equal to creating a hypothesis about its mechanisms?* **[I think this is probably right: what the formal model does is**

**explicitly identify the mechanisms that turn a set of initial conditions into some outcome.]**

Can we say that the three methodologies that he proposes are hierarchically organized into levels of abstraction, like in the sequence represented below?



**[Narratives are certainly more concrete than models – that much is sure. And I guess it is also the case that quantitative analysis is more abstract than narratives because of the kind of descriptive details that are lost in quantification. So there does seem some kind of hierarchy of abstraction here. However, the Narrative case study involves positing explanations – that is what Tambiah did in the Sri Lankan – and these necessarily invoke abstract mechanisms, albeit mechanisms that have not been formalized in an explicit model. This implies that narratives will have quite abstract elements within them, at least when they propose explanations. The same is true for quantitative analyses. So the partition between the three methods is not quite so simple, but still I think you’ve identified a dimension of the problem.]**

## 12 Dan Warshawsky

### Narrative Methods and Methodological Pluralism

This week’s readings are especially challenging, in terms of content and theoretical implications for social science within the academy and at large. Although obviously quite nuanced, Flyvbjerg and Laitlin provide quite opposing views for the future of social science research. Amidst the powerful critiques provided by post-structuralism, post-modernism, standpoint epistemologies, among others, social scientists have been forced to question the value and strength of their research methodologies.

Two issues interest me most. First, I will detail the Flyvbjerg-Laitlin debate as I see it, including areas that need clarification. Secondly, is the distinction between the social sciences and natural sciences a useful distinction?

I was quite impressed by Flyvbjerg’s *Making Social Science Matter*. He provided a thorough yet straightforward analysis as to why we should abandon the current social science mode of research in favor of another more potent and sustainable method. From the beginning of his work, he set out the focus of the ideal social science: one that emphasized phronesis over episteme.

First, we must drop the fruitless efforts to emulate natural science’s success in producing cumulative and predictive theory; this approach simply does not work in social science. Second, we must take up

problems that matter to the local, national, and global communities in which we live, and we must do it in ways that matter; we must focus on issues of values and power like great social scientists have advocated from Aristotle and Machiavelli to Max Weber and Pierre Bourdieu. Finally, we must effectively communicate the results of our research to fellow citizens. (Flyvbjerg 166)

Although I am not sure if I believe Flyvbjerg's work is flawless, I do think it presents a very admirable and workable goal for social scientists. In contrast, Laitlin's criticism seems somewhat out of place. Although he criticizes Flyvbjerg for pursuing his 'agenda,' it seems that Laitlin is the one who misrepresents Flyvbjerg's ideas to pursue his 'agenda.' Additionally, I do not think that Laitlin's tripartite method is truly sustainable or ideal. **[You need to say something more than just an assertion that it is not sustainable or ideal – you have to give reasons for this. What is wrong with the tripartite method? What would you propose – a one dimensional method?]** It seems that Laitlin is trying to hold onto to a structure that fails to encompass new powerful critiques that can only add to social science. **[Surely it is not the case that these new and powerful critiques can “only add” to social science. They could seriously deflect scholars from empirical research in ways that could be quite harmful to social science. This has been (sometimes) the effect of post-modernist critiques of science.]** True, post-structuralism and post-modernism could destroy social science research if interpreted and 'used' without judicious forethought as to their implications, Flyvbjerg's utilization of Foucault's ideas on power, for example, are not only helpful but essential. Thus, as Flyvbjerg states, social can and must integrate the tools set forth by post-structuralist and post-modernist critiques and combine them into a workable whole.

Although I admire most of the conceptual 'moves' that Flyvbjerg emphasizes in his work, I am unsure of one central issue. As a human geographer, who is constantly in contact with physical geographers (of the natural sciences), I wonder what, if any, overlap there can be for both human and physical geographers (across the content and theoretical 'divide'). Should there be any common methodological tools used and common theoretical agendas that can or should be achieved? I believe Flyvbjerg's work provides great clarity for me as a social scientist, but not necessarily as a geographer. At the beginning of his work he starts from the social science-natural science binary.

We will see that in their role as phronesis, the social sciences are strongest where the natural sciences are weakest: just as the social sciences have not contributed much to explanatory and predictive theory, neither have the natural sciences contributed to the reflexive analysis and discussion of values and interests, which is the prerequisite for an enlightened political, economic, and cultural development in any society, and which is at the core of phronesis. This should be at the core of social science if we want to transcend the current malaise of the Science Wars. (Flyvbjerg 3)

He seems to say that the social sciences should not try to imitate the natural sciences, so does this imply that different methodologies are best for a human geographer

and physical geographer, but one who is a human-physical geographer must choose one framework over another? Crossing my disciplinary 'divide' is a central theoretical issue for me when contemplating how Flyvbjerg and Laitlin fit into my academic interests. **[I personally don't accept Flyvbjerg's sharp demarcation here. Social science generate lots of good explanations and even predictions – albeit predictions that are always have strong conditions on them. And the explanations of natural sciences figure strongly in discussions about values and priorities. Can you imagine environmental sociology making any kind of serious critique of environmental practices in the absence of scientific knowledge about global warming, pollution, environmental degradation, etc.? There seems to me a much tighter dialogue here than a gulf.]**

Thus, I have two main questions this week. Is Laitlin an artifact, or does he provide a useful critique of Flyvbjerg's work? Secondly, how does one 'betwixt' and 'between' (from the anthropologist Victor Turner) the social sciences-natural sciences divide respond to Flyvbjerg's work (such as those straddling the human geography-physical geography 'divide' in geography)?

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### 13 G.C.

I'm not too clear as to what the conflict really is between Laitlin and Flyvbjerg; it seems to have to do with normative values. Plato hoped to assimilate ethics to mathematics and geometry, the inscription above the entrance to the Academy read: "Let no one ignorant of geometry enter here". He was trying to give mathematical certainty to practical questions, by saying that the idea of absolute mathematical equality was the same sort of transcendental, necessary truth, as was the idea of absolute goodness, justice, uprightness, etc. Thus, in whatever he particularly judged to be an instance of justice or goodness, his judgment would be reinforced with the character of mathematical certainty, for absolute ideas are directly perceived by the soul while in some transcendent realm before being born into this mortal life. Thus it can be said that act x is just, just as all the points on the circumference of a circle are equidistant from the center; these things simply are true. Nietzsche has something to say about this, namely that it is a reification of plebian resentment. Aristotle, rather than insisting that each instance of goodness or justice must partake of the form of the good or the just, holds that observation of particulars can reveal their nature (whereas for Plato, the nature or essence of a thing is perceived in the transcendent realm and cannot be discovered through mere empirical observation). Thus he describes certain acts as "taken to be just" or "punished as being unjust", rather than asserting that something simply counts as just, due to its metaphysical nature. He abandoned Plato's project of assimilating ethics to episteme, while continuing to use the language of goodness and justice. Goodness and justice operate in societies; sometimes differently. As Voltaire quipped of God: even if he doesn't exist, he still managed to get himself invented. This is congruent with Nietzsche's view of ethics (and God) as after-the-fact rationalizations of deep-seated psychological desires. This makes ethics rather more relativist or situationalist. Still, the task of social science isn't to discover what the good is, in itself, before proceeding to examine how the good and people's conception of the good operates in society. Ethical questions might be taken to be important; or not. I

am curious how important and central everyone in the group, as social scientists, rates ethical questions. This isn't the nexus of the Laitin -Flyvbjerg argument, but I don't exactly see what that nexus is (if there is one). **[I think Flyvberg is saying something fairly strong about the capacity of a properly constructed social science to intervene in ethical issues. If the deep problem in ethical problems are the complexities of trade-offs, dilemmas and contradictions among a series of competing ethical requirements, then the core of the task of resolving ethical disputes involves these illusive ideas of "wisdom" and "judgment", rather than simply abstract reasoning or concretely given culturally-dictated prescriptions. The argument, then, is that this kind of judgment/wisdom is a form of tacit knowledge and skill, learned through experience and comprehensible through a particular kind of social science engagement with experience. I still find this pretty elusive as a guide to sociological practice, but I think it implies a deeper link of social science to normative problems than is usually claimed.]**

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#### 14. Matt Nichter

Why can't we have a scientific theory of human behavior that relies in part on non-propositional 'contextual' knowledge? In places Flyvbjerg suggests it is simply because the knowledge is non-propositional. But this seems wrong. We have prima facie scientific theories of all sorts of things, from billiard balls to the human unconscious, which are not governed in any obvious way by 'beliefs that X.' It seems that the crucial extra ingredient that Flyvbjerg appeals to is (to put it in much cruder terms than he does) free will. It is this which is supposed to make a predictive theory of human behavior impossible. **[The key content of the notion of "context" is not just that it involves nonpropositional knowledge, but that the knowledge has this tacit-skill quality to it. I don't think this is just a free will issue, although it may be connected to that. The issue is the way this kind of knowledge is embodied and embedded in human action. It is still unclear to me why this has such a devastating impact on the possibility of explanation and prediction.]**

Whether or not one accepts this argument, there are at least two ways the science-ist could accomodate it. One is to go reductionist, and try to explain the gaps in intentionalist theories of human action in terms of sub-intentional brain processes and so forth. Another is to indentify macro-social mechanisms which are not particulatly sensitive to the specific micro-behavior of agents.

Moreover, it seems to me that the argument that it is important to encourage ethical reflexiveness is entirely separable from the argument that human sciences cannot be genuinely scientific. **[I agree with you, of course. But the link in Flyvberg is that this reflexiveness is part of the social world being studied, part of this peculiar form of "context", and not just part of the desirable**

**practice social scientists. The fact that actors in the world are reflexive and this reflexivity is part of what renders them expert performers (apparently) contributes to the inability to make real predictions.]**

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15 Martín Santos.

### GOING BEYOND “MR. PERESTROIKAS” AND “MR. ANTI-PERESTROIKAS”

I am very sympathetic with the idea of a “tripartite method” (or “triangulation of methodologies” in other versions) for Social Science because it helps to overcome the weaknesses of any methodology working in isolation. Laitin suggests a *feedback* relationship between statistical models, formal models and “narrative”. Thus, statistical models can help *reconstruct* narratives and narratives can lead to the *re-specification* or replacement of a given formal model for another one. Moreover, Laitin states that all of these methods are equally important in his tripartite method (“*I see narrative as a co-equal to the statistical and formal elements of the tripartite method...*”, p. 176). But is this really the case? Does narrative occupies a symmetric place vis-à-vis statistical and formal models in the tripartite method? **[They could be co-equal without being entirely symmetrical, I think. That is, each method could correct distinctive failings of the other method, distinctive ways in which they generate error, and yet all three may be necessary, and by virtue of being necessary are of equal importance. Only if you could establish a clear sense in which some kinds of errors were “worse” than others would the asymmetry imply an inequality.]**

In what follows, I will do a “mini-discursive” analysis of Laitin’s reconstruction of Flyvbjerg’s narrative to make the case for the importance of *phronesis as a way of unpacking power relations*.

Flyvbjerg’s narrative is organized around a primary idea: methodological approaches in Social Science are the product of our (social scientists) *social ontologies*. Thus, intuition, body, experience, the *ambiguity of meaning*, the multilayered character of discourse, and the indissoluble connection between meaning, *time* and power, are crucial elements in his theory of social action. *Context* can be understood as the web of discourses and power relationships that make social action possible, unlikely, and in some cases, unpredictable. **[I am not sure that this is precisely what Flyvberg means by “context”. He seems to emphasize the tacit knowledge aspect of practice rather than “discourses” and power as such.]** The crucial issue for Flyvbjerg is that, following Bourdieu, “*practice has a logic which is not that of logic*”. Thus, the question is: can Social Science succeed in studying social action using a logic (rule-based, context-independent) which is different from that deployed by social actors (intuition-based; context-dependent)? **[This is a nice formulation. At first glance I do not see any inherent reason why there needs to be a homology between the logic of inquiry and the logic of action. There is no reason why, for example, one has to adopt of a logic of habit in our research in order to study habits in people. I am not sure why there needs to be any correspondence here.]**



Laitin's reconstruction of Flyvbjerg's narrative does not take into account the arguments just presented. He frames Flyvbjerg's ideas as if they were only about "methodology". Moreover, he *translates* many of Flyvbjerg's notions (such as "context") into *his variable-oriented language*: "appealing to context is merely to say that we have not yet discovered the various factors or the interaction of factors that produced outcomes of significance", p. 168). Laitin criticizes Flyvbjerg for not providing mechanisms by which power produces its effects. However, the latter does provide (mention) some kind of mechanisms (for instance, in p. 153). I suggest that these two authors speak different languages (social ontologies) even though they think they speak only one. **[But if Flyvbjerg does specify mechanisms, then why can't these be studied in the standard "scientific" manner? ]**

Finally, despite his statement about the equal importance of methodologies in his tripartite method, narratives appear and work in Laitin's narrative *to complement* statistical and formal models (to test statistical hypothesis, to provide mechanisms to link independent and dependent variables, to analyze "residuals") and *not vice versa*. **[Specifying a mechanism that links independent to dependent variables is not simply a "complement" – this is the heart of the matter in generating explanations, and it is the kind of intervention that can demonstrate a false causal claim about the relationship between those variables. I don't know if these uses of narrative necessarily imply that it is subordinate to models or large-N studies.]** *There is implicit a hierarchy between these methodologies, and the relationship between them is framed in terms of the language, reasoning and logic of the social ontology underlying statistical and formal models.*

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