A Political Scientist's Unintentional Experiment with Critical Policy Studies: How Journals Make Themselves Look Bad

Krešimir Petković
kpetkovic@fpzg.hr
Faculty of Political Sciences, University of Zagreb, Croatia

The Plot: How It All Happened

Personal introductions make the usual ingredients of notorious internet spam scenarios. I will, however, take the risk of driving away the potential readers and first introduce myself, since the speaking subject is an important element in this story.

My name is Krešimir Petković.1 I just turned 28 years of age. I am political scientist, working as a research and teaching assistant at the Faculty of Political Sciences in Zagreb where, hopefully, I am also soon to defend my PhD thesis on penal policy and politics in Croatia from the vantage point of discourse analysis. I have a strong interest in political theory and in interpretive approaches to public policy analysis.

Last summer I participated in 4th International Conference in Interpretive Policy Analysis. It was held in German town of Kassel, 25-27th June, under the fairly broad name “Discourse, Power, and Politics”.2 I presented my paper in one in the last series of panels, along with Stephen Connelly, Alfred Moore, and John Dryzek. The paper was on the relationship between interpretive policy analysis and deliberative democracy, and it had a polemical subtitle: “Should we politicize analysis?”3 It all went very well. Frank Fischer, whose ideas I critiqued was not present, but his former pupil Navdeep Mathur was. He gave some poignant comments to which I replied the best I could.4 We then all sat in concluding plenary discussion, where we were called to submit our papers for publication in a new Routledge journal Critical Policy Studies.

So I did: I submitted my paper. After about a month I received an answer from Frank Fischer, the editor. Fischer politely explained how I misinterpreted him in some important aspects, but he also stated that the paper can be published if I radically reduce its size. It should also be reshaped as a polemical contribution to the forum section, and he promised that he will reply. (“A piece of about half the length would be suitable for section called Forum, devoted to controversial issues and debates. If that would be of interest to you, I would most happy and willing to reply--debate format--to your article.”).

I understood that as an editorial statement, and happily looked forward to a nice debate, where he would prove me wrong (or the other way around, the reader is of course the one to decide). Since my contribution was now classified as a part in a debate, I focused on

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1 This perhaps complicated Slav name is pronounced crash-e-mere (similar to cashmere) pet-co-witch.
2 http://www.ipa2009.uni-kassel.de
4 As far as I can remember, the part of it was ad hominem question about Lasswell who, as it is almost endlessly repeated, opted for policy science(s) of democracy. My response was, first, that it is not at all clear that Lasswell’s call means today what it meant then, in the Cold War context. Second, I explained that Lasswell was, methodologically speaking, chiefly behavioralistically oriented, and argued for the extended use of quantitative methods (“the battle for method is won”), etc. The other part of the comment was, I think, implying that I am opting for “technocracy”, thus imputing to me something what I never said (critique of one approach does not mean that one automatically subscribes to what the approach declaratively opposes).
the concluding polemical section of the paper, and I tried to elaborate my arguments to support my position. Although I did not find that I misinterpreted Fischer’s position, I found his letter friendly and encouraging. I wrote a reply in which I said that I accept the challenge, and that I’m willing to do what I was asked. I didn’t receive any answer. I rewrote my paper and resubmitted it after about little bit more than a month and a half passed.

Frank Fischer responded in a moment. I witnessed a complete transformation: friendly and cooperative editor became very hostile. Contrary to what he earlier said, Fischer immediately responded that my first essay was “altogether based on a misinterpretation” and that he “would not support the publishing”. This all referred to the first version, which he had for about a month before he replied; he couldn’t read the second version, since this volte face arrived only fifteen minutes after I’ve sent the new version. Perhaps the strategy had to be changed, I don’t know. I can only speculate: if you don’t succeed in getting rid of the disruptive element under the guise of politeness, then become aggressive. I reminded him what he said, and praised the importance of his work. I inserted a Millian reference on the value of public discussion and asked him to reply.

Fischer then became even more aggressive. Again, I can only speculate: blinded by his academic conceit, he might have thought: who is this provincial nobody to talk to Frank Fischer, established author and respected professor, like that. He accused me of intentionally misreading him and added that I didn’t understand “the nature of interpretation in policy analysis”. Fischer’s reply struck me as unbelievably narrow minded. He understood the notion of public debate literally (strangely excluding from it forum debates in journals, which are also part of public space), and he also took ironic Millian remark on “saying something stupid publicly” literally (“Normally, journal articles try to avoid publishing things that are stupid, as it makes them look bad.”). This is highly ironical: a lack of deliberative spirit and capacities for subtle understanding come from one of the leading scholars who advocates democratization of policy analysis and who supports interpretive approach. The cherry on top was comment that he would read the new version of my essay, but that his computer says it might do it harm (“my computer will not open it, as it says the file may damage my computer”). Hostility was obvious: banal technical message, which pops up concerning any attachment, infected or not, was now served to me as a figurative imputation of my bad intentions. By now, I realized that I was naïve almost as de Sade’s virtuous Justine who benevolently approaches her depraved future tormentors. Anyway, I replied very politely (although with few sarcastic remarks), sent him the document in a new format, and said that he shouldn’t take any of it personally.

I wished him “good luck with the journal”, since I now knew quite surely that nothing I write can ever be published in Critical Policy Studies. I received a final short message that he doesn’t decide on publishing. (“This is not in my hands. If we review the piece, it will be decided by reviewers.”) The circle was thus completed with the second contradiction: first he said that my contribution will be published (why would he say so, if he didn’t have such editorial authority), then he said he wouldn’t publish it (logical implication is that he decides on the matter), and finally he evokes procedure and says that he doesn’t decide on the matter (what is then the meaning of the whole correspondence, and his statements about “scarce journal space” and publishing agenda, which clearly implies that it is his decision…).

Few months then passed without any word from Critical Policy Studies. I sent an e-mail to journal’s secretary Helen, in which I expressed my skepticism on publishing of my work, but that I would anyway like to receive formal rejection, complemented with the reviews, just to close the matter. Very polite as usual, she sent me the formal rejection immediately together with two negative reviews.

I thought I was now beyond phase of naïveté à la Justine and I didn’t expect much from these reviews. But similarity with Sade’s notorious novel unfortunately didn’t end there.
The same as Justine, who meets new tormentors that become only worse as the story progresses, I now confronted a new unpleasant surprise. Comments I received were below any academic level, written in style of clearly political discourse, similar to the attacks on political dissidents by the totalitarian press propaganda (ironically, that confirmed my intuition about the problems of politicization of policy analysis was on the right track). Since anything that political enemy writes shouldn’t be taken seriously by definition of the genre, my arguments were not at all addressed in these “reviews”. The question was only to stick appropriate labels to me.

The first review spoke of “author’s avowed preference for the technical economics dominated version of policy analysis” and immediately added: “This preference is not so hidden in this paper.” That is simply insane. In both versions of the paper I clearly argue for interpretive approach. And if I didn’t, why should I hide it? This is conspiracy theory style in search for hidden enemies. Then, of course, the labels ensued. I found out that I have “conservative technocratic position”. Finally, it is all packed up with few elusive metaphors such as (“not… engaged… in any sufficient depth”). There was not a single sentence that took my argument seriously, and there was no effort at all to offer counterarguments. The only thing that was repeated ad nauseam, was that I did not interpret Fischer correctly, which is strange, given the frequency of his complete & unabridged quotations I used to support what I am saying that he was saying.

From the second review I found out that my position is “outmoded” (I admit, I like to be out of vogue) and “even latently technocratic” (God Save Us). By the way, this “latently” is so characteristic for discourse of political denunciations: latent tendencies and subversive forces have to be fought before they emerge and show their real face. E.g. you mustn’t say someone is just simply “Trotskyist”, you must accuse for “latent Trotskyism”.

I first thought just to let it go. Who really cares, if they work and speak in that way, they will destroy themselves anyway. They don’t need my help. Then I rethought the whole issue. Did I write and rewrite my article for nothing? I decided to take a small effort and just write what had happened, since it is not just a psychological matter (perhaps something like the following: author whose pride is hurt takes his revenge on the editor whose pride was hurt because he had been critiqued from the author…). The motives here are irrelevant: the guys from the CPS simply didn’t do the right thing, and it shouldn’t be left to silence.

In the next section I try to elaborate on why the whole affair is important. Thus it goes beyond the lesson from 1982’s Ted Kotcheff’s First Blood: of course, fascist sheriff should have known better than to push the war veteran and get “a war he would not believe”, but the question is to define his act as police brutality, to see what it means, and to find out where it comes from.

The Interpretation: Tiny Voices

I think this affair shows at least three things. First, it simply illustrates some bad publishing practices. Publishing process wasn’t lege artis at all. This journal did not respect the author. He was confronted with contradictory messages and his manuscript wasn’t treated seriously. What most of all seems troubling, reviews seem to be very far from the ideal double blind peer review process, and their anonymity is highly questionable (cf. appendices 2 & 3).

Second, the critique that sees some trends in interpretive policy analysis as a dubious political turn is confirmed by this episode. Just to be sure: unlike Sokal who wrote a parody and sent it to journal, my intentions were completely honest. Interpretive policy analysis in the broadest sense is what I do. It is what I am interested in and I had something to say on the

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5 The desperate plea for recognition and recompense... and if we don't confront them they will never go away. (Tiny Voices, a Bad Religion song from their 1994 Stranger than Fiction album).
matter. I reviewed the interpretive policy analysis literature, its theoretical roots, systematized it on the level of “science on science” meta-scientific discourse (ontology, epistemology, methodology). And, finally, I tried to show how turning analysis into a species of deliberative democracy can have some unintended side effects. When I tried to publish it I didn’t find an open forum, where my arguments should be confronted in publicly acceptable form. I instead met an aggressive, rude and contradictory editor whose agenda was to stop the publishing of the work which is critical of his position, before it gets to the public. That is perhaps psychologically understandable, but scientifically and professionally it is a complete disaster. The affair exposed not a vibrant scientific community, but an ideological sect, which cannot even follow its own proclaimed principles. When someone speaks something outside of the proclaimed dogmas, he is silenced and classified under worn out ideological labels. Thus, what I got is not a good debate, but only a bad experience. Retrospectively, it can be seen as an unintended experiment that made a journal and a politico-scientific project connected with it look bad. When science becomes politics, that outcome should be expected.

Finally, this tiny voice didn’t simply shut up because it seems that this affair altogether has a colonialist air. If these practices I was confronted with can be labeled as a sort of Jacobinism, from the other point of view they can also be interpreted as some form of colonialism. My sin was that I wrote solely on theoretical issues. That is not a position that marginal scientist from scientific periphery can afford. Theory is devised in the centre. In the colonies it can only be applied. Thus I cannot write about interpretive policy analysis and deliberative democracy as such. I can only analyze cases from my own country which are than presented as case studies in wider projects of social scientists from USA and UK. Said’s remark from Orientalism about scientists from periphery is instructive here: the scientist from colony can at best count on the status of “good informant”, who gathers info for the intellectual forces from the centre. There is, of course, no firm connection between my experience and this story, but it seems to me that I could have, much more easily than this theoretical one, published a text on e.g. some application of deliberative method in Croatia (my native country).

I will finish with some remarks on two books that the editor of CPS got published with the one of the strongest publishers, just to illustrate these double standards of publishing for Centre and Periphery. It seems that it is hard to get something in from the periphery, but that it is much easier to get anything out from the centre.

**Going Medieval: Some Lessons in Plagiarism and Sloppy Publishing**

Reference to Tarantino’s Pulp Fiction is appropriate here: if one side goes medieval on you (nothing less happened to me), then you can also apply the same methods of communication. *Unfortunately, this is the only language some understand.*

I remind the reader that I was accused of misinterpreting Fischer’s work. So I took my time, and studied it more thoroughly to see if I’m wrong. I didn’t find that I was wrong. But what I did find was not good at all. One might always say that an interpretation is missed, that quotes on which it is based are pulled out of context, etc. There is no definite interpretation that can be derived from an algorithm. Single solution simply does not exist. Almost like the old religious scripts, social every scientific work is more or less incoherent and it is open to different interpretations. But there are nonetheless some down to earth textual observations that can almost be seen as facts which are much less open to different and differing points of view.

First, what I found out via even more careful reading is that Fischer’s book *Reframing Public Policy: Discursive Politics and Deliberative Practices* (Oxford University Press, 2003) unfortunately contains blatant plagiarism, one of the grave sins in the academic world. Cf. the
Argumentation differs from formal demonstration in three important respects. First, demonstration is possible only within a formalized system of axioms and rules of inference. Argumentation does not start from axioms but from opinions, values, or contestable viewpoints; it makes use of logical inferences but is not exhausted in deductive systems of formal statements. Second, a demonstration is designed to convince anybody who has the requisite technical knowledge, while argumentation is always directed to particular audience and attempts to elicit or increase the adherence of the members of the audience to the theses that are presented for their consent. Finally, argumentation does not aim at gaining purely intellectual agreement but at inciting action, or at least at creating a disposition to act at the appropriate moment.

Now, confer the passage from Fischer’s 2003 book (p. 190):

Practical argumentation thus differs from formal demonstration in three important respects. Whereas formal demonstration is possible only within a formalized system of axioms and rules of inference, practical argumentation starts from opinions, values, or contestable viewpoints rather than axioms. It makes use of logical inferences but is not exhausted in deductive systems of formal statements. Second, a formal demonstration is intended to convince those who have the requisite technical knowledge, while informal argumentation always aims to elicit the adherence of the members of a particular audience to the claims presented for their consent. Finally, practical argumentation does not strive to achieve purely intellectual agreement but rather to provide acceptable reasons for choices relevant to action.

Fischer’s insertions and slight rephrasings are in bold format. Tracing is evident. Moreover, it’s a case of bad, unintelligent tracing characteristic for lazy students’ papers. Technology is perhaps the following: just copy the original and insert few words to cover your tracks. To be sure, Fischer mentions Majone’s book, but nowhere near this passage. Reference appears only three passages later, in the next subchapter (to p. 63 of Majone’s work). I suspect this is not the only case of plagiarism within this book.

The book also contains high frequency of misspellings of authors’ names. This raises unfortunate suspicion that Fischer didn’t read many of the works he mentions. Deleuze and Guattari thus become “DeLeuze” and “Guatani” (p. 240), Colin Gordon, the editor of the famous Foucault reader, becomes “Gordeon” (p. 243), etc. Poor Keohane from King-Keohane-Verba trio, the authors of seminal Designing Social Inquiry (Princeton University Press, 1994) has his name misspelled three times as “Koehane” (p. 156). Interestingly enough, it seems Fischer never learnt this name properly, since the same mistake is repeated in his other works. The chapter 7—not chapter 6, as the introduction has it—of Reframing Public Policy is reproduced from Fischer’s Policy Studies Journal article (vol. 26, no. 1, pp. 129-146). The same article is published as a chapter in Hajer’s and Wagenaar’s edited volume Deliberative Policy Analysis: Understanding Governance in the Network Society (Cambridge University Press, 2003). There, in a footnote, the same critique is copied with some slight changes and omissions, but with the same misspelling: it’s “Koehane” all over again (p. 219).

One can also find similar peculiarities in the treatment of Fischer’s theoretical opponents in his recent book Democracy and Expertise (Oxford University Press, 2009). Alan D. Sokal, to whose “hoax” I refer in this text, flatteringly labeled as on of “the intellectual reactionaries who have generated the 'science wars' of the late 1990s” (p. 139) is consistently misspelled as “Sokol” (which, interestingly enough means “hawk” in Croatian) – in the text (p. 139), in the references (p. 324) and in the index (p. 337).
Why mention all of this? Maybe I did misinterpret Fischer’s position, but some things I certainly did not misinterpret. That, I think, concludes this affair just right. For, how can you expect from someone who writes such books to edit a journal professionally?

Appendix 1  The Revised Version of the Article submitted for The Forum of Critical Policy Studies

Interpretive Policy Analysis and Deliberative Democracy: Must We Politicize Analysis?

KREŠIMIR PETKOVIĆ
Faculty of Political Sciences, University of Zagreb, Zagreb, Croatia
E-mail: kpetkovic@fpzg.hr

Mais, bien sûr, nous qui comprenons la vie, nous nous moquons bien des numéros!
Antoine de Saint-Exupéry, Le Petit Prince

Finally, the society we live in is techno-bureaucratic in form and there are big rewards for serving this system. Much of what goes on inside the university is aimed at facilitating the technocratic system of expertise—indeed, this is what is marketed to the students. At this stage, what is needed is a politics of expertise inside the disciplines that reflects the kinds of concerns raised in the chapters here, as well as the related issues of the progressive movements beyond the walls of the university. In all cases, these concerns and the politics associated with them should be taken on as part of the agenda of those in policy professions committed to democratic decision-making in a more egalitarian society. Making participatory policy inquiry poses, in this respect, one of the important challenges of our time.

The closing passage from Frank Fischer’s book Democracy and Expertise (emphases mine)

Interpretive policy analysis is one of the ways to do what the title of this new journal adroitly catches under the general label of critical policy studies. There is, of course, more than one legitimate way to understand and to practice interpretive policy analysis. Interpretive policy analysis is, perhaps, only an umbrella term for loosely connected theories and methodological tools which are all opposed to positivism and its research methods in policy studies. Thus, motto of interpretive policy analysis could well be the one above, from The Little Prince, about us who ‘understand life’ and can ‘make fun of numbers’, usually much adored by positivists.

Although some of the more general tendencies and trends within this broad and diverse field can be traced, and some schematizations can be made, it is not an aim of this short debate piece to make such a bold enterprise. The aim here is much more modest. I only

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6 A good review of the tradition of interpretive policy analysis is offered by three well known edited volumes (Fischer and Forester 1993, Hajer and Wagenaar 2003, Yanow and Schwartz-Shea 2006). In the paper I set forth at the 4th Annual Interpretive Policy Analysis Conference (Kassel, Germany, 25th-27th June 2009), I wrote more extensively on the tradition of interpretive policy analysis, i.e. on important theoretical influences by which it was shaped, and on its ontological, epistemological and methodological characteristics. Although such
want to make a little thought experiment. What I say, briefly, is the following. There is a broad tradition of interpretive policy analysis—a kind of a set of interpretive and critical theories and methods used in policy research. These theories and methods produce policy research work which can be used, and is used, in various ways to influence the policy making process. So far so good. But there is also a strong school in the field of interpretive policy analysis that sees it as a part of a political program of strengthening democracy, empowering citizens, and counterbalancing the technocratic tendencies at work in Western societies (and, for a long time now, in the whole globalised world). More specifically, this school advocates deliberative policy analysis and deliberative policy making as a model for good policy analysis. In that way, in my understanding, it—inadvertently or intentionally, it is of less importance—erases the boundaries between interpretive policy analysis and a specific political program of deliberative democracy. The fundamental turn is thus made: the analysis is not a separate input to politics anymore, but analysis becomes a species of politics. The boundary is broken. This is perhaps a good thing. At least from the point of emancipatory program of deliberative democracy it might well be. However, from the perspective of autonomous interpretive policy analysis, it could bring some problems, or at least, it could bring to life a few skeptical arguments, of which I tried to conjure some here. The reader can herself decide if they are prudent caveats or just hilarious exaggerations.

The following text is structured in four parts. First, I give a synoptic review of the idea of deliberative democracy in order to clarify what I exactly mean under the term (1). Second, I try to identify the political program of deliberative democracy as a model for policy analysis in Frank Fischer’s work (2). Then I offer three skeptical arguments which can be employed against such a program (3.1, 3.2, 3.3), and finally, I offer a short concluding remark (4).

(1) Variants of interpretive policy analysis which rely on citizen participation, and prescribe to the analyst a role of the mediator in the policy discussion, are firmly bound with the idea of deliberative democracy. Deliberative democracy appears as one of the variants of normative theories of democracy (that doesn’t, in any case, mean that the statements of the theorists of deliberative democracy do not contain empirical claims), and normative theories of democracy are usually opposed to the aggregative ones (Shapiro 2002). In contrast to the aggregative theories, which are primarily focused on the problems of aggregation of preferences of individuals and groups, deliberative democracy believes in reasonable discussion of citizens in which, by producing mutually acceptable reasonable arguments, initial preferences are changed and collective consensual decision is achieved.

Theory of deliberative democracy comes in many different variants. I shall mention some of them. From the perspective of abstract political theorizing, Joshua Cohen offered a strong normative model of deliberative democracy as an ‘association whose affairs are governed by the public deliberation of its members’ (Cohen 1989, p. 17). It is an independent and permanent association in which free and equal members make the decisions through reasonable deliberation, in accordance with the set of specific rules. Benjamin Barber, within the framework of his republican demands for a ‘strong democracy’, developed the conceptions of public seeing, political judgment and political-democratic talk, and made suggestions for concrete institutional reforms (for the USA, but they can be read as general recommendations for contemporary democracies as well). Those conceptions and expounding of the tradition of interpretive policy analysis would probably make my argumentation more convincing, I unfortunately don’t have the necessary space to do it here. Anyway, the paper is available for download at http://www.uni-kassel.de/hrz/db4/extern/ipa2009/ct/images/Petkovic.pdf. The argumentation I presented in the paper was based in research I did for my MSc thesis (Petković 2008a). I summed up the findings of the thesis in an article published in Croatian political science journal Politička misao [Political Thought] (Petković 2008b), and I made the Kassel paper by translating, revising and expanding the Politička misao article. This contribution consists of the revised and expanded last section of the Kassel paper.
recommendations also make his theory a species of deliberative democracy theory (Barber 1984). Jürgen Habermas, on the other hand, offered communicative action of civil society as his model of deliberative democracy. In his conception, civil society must amplify the lifeworld rationality and curb the misuse of administrative power and the excesses of market logic. His model does not demand rearrangement of existing political institutions according to formal demands of deliberative ideal. Instead, it requires enabling of the diffuse rational communication in the public sphere of existing liberal-democratic societies (Habermas 1998). As a final example of deliberative democratic theorizing I will mention John Dryzek’s theory. Dryzek offered an ideal of ‘discursive democracy’ as a creative contestation of discourses under somewhat more loose set of rules for deliberation. For example, in his model summoning of emotions is not forbidden, consensus achieving is not insisted on, and social equality is not a necessary condition for deliberation (Dryzek 2000). Beside the general theoretical considerations, deliberative democracy includes concrete deliberative designs, such as consensus conferences, deliberative polls and contemporary emulations of historical town meetings (Gastil and Levine 2005). Deliberative designs often try to emulate ‘mini-publics’ (Goodin and Dryzek 2006), i.e. they try to bring together limited number of individuals—who usually belong to the different social strata and must satisfy some other criteria—in controlled deliberation whose results are afterwards published in order to influence the political agenda and policy making.

In both variants—one of abstract theorizing and another of concrete practical experiments—deliberative democracy is an exercise in institutional design. Its proponents try to establish institutional settings to make policy and govern political life. In that sense, of course, deliberative democracy is politics: it devises rules of collective decision making, no matter how diverse are its theoretical visions and practical experiments. Why is that important for interpretive policy analysis?

(2) Deliberative democracy is important because proponents of deliberative policy analysis try to establish fora for analysis and creating of policies according to the model of such mini-publics in which citizens, according to a set of rules, deliberate policies and achieve consensual solution for policy problems. Their model of policy making and analysis is based on the theory of deliberative democracy. Their goal is to democratize the process of public policy making; hence they want to supplant the classical model of policy analysis with a species of deliberative democracy. Eminent representative of this deliberative program for policy analysis is Frank Fischer, also one of the main authors in the field of interpretive policy analysis in general. His well known books—one on ‘citizens and experts’, on the ‘reframing’ of public policy analysis (Fischer 2000, 2003), and the recent one on ‘democracy and expertise’ which further develops his thinking (Fischer 2009)—are important theoretical elaborations of this program. In this section I offer my reading of these works.

Although Fischer generally argues for the employment of discursive and critical theories in policy analysis, and recognizes the instrumental employment of deliberative methods, as a means of approaching to local knowledge, his main demand is focused on democratic deliberatizing of policy analysis, and is primarily of political nature. The idea that runs through all three books is that ordinary citizens are capable to participate in policy analysis and in political decision making, and that their knowledge and participation is an important democratic task. In his work Fischer tries to give a contemporary answer to initial Lasswell’s challenge of building policy sciences of democracy:

‘But the policy-analytic enterprise as a whole failed to take up Lasswell’s bold vision, instead following a constricted part of evolution. Not only has it emerged more along technocratic rather than democratic lines, the empirical and practical pay-offs have been far smaller than the original promise’ (Fischer 2003, pp. 3–4).
According to Fischer, the chief problem of the classical policy analysis—the one which is performed by policy experts, and which often employs statistics, mathematical models, and has an implicit rational choice ontology of individual utility maximizers—is that it is technocratic and it helps the reproducing of the order in which the experts govern, often at the expense of citizens’ real interests and the good of the political community as a whole. Policy analysis must instead promote political inclusion and emancipation of citizens (Fischer 2000, pp. 14, 32). Although Fischer does not advocate the abolishing of the expertise, he maintains that the experts have too big and uncontrolled influence on decision making and that ‘citizens and experts can strike a much more democratic balance between knowledge and participation’ (Fischer 2000, p. 40). For him, the real participatory analysis has to be ‘egalitarian—even radically egalitarian’ (Fischer 2000, p. 188), which roughly means that citizens and experts have the equal legitimacy in creating of the final conclusions of the analysis. Fischer thus welcomes different experiments of deliberative democrats, such as the aforementioned deliberative polls, citizens’ juries, and different workshops of citizens and experts. He especially stresses the value of the consensus conferences, conducted according to the model introduced in the 1980s by the Danish Board of Technology (Fischer 2003, pp. 210-219), and different variants of participatory policy analysis: both the spontaneous ones, such as the one which came in the first plan in the case of popular epidemiology case in Woburn, Massachusetts, and the state and civil society sponsored ones, such as systemic consulting of local knowledge in the development policy of the economically underdeveloped Indian state of Kerala (Fischer 2000, pp. 151-169).

On the bases of such positive experiences with policy deliberation, Fischer advocates ‘a vigorous participatory democracy capable of supporting representative democracy’ (Fischer 2000, p. 37). In his opinion

‘(...) a great deal more participation is both possible and necessary than presently exists in Western democratic systems and that, among other things, this means rethinking the relationship between experts and citizens’ (Fischer 2000, p. 37).

Fischer maintains that these nascent participatory practices have a threefold positive effect: they enhance the practices of democracy, they help to legitimate the policy, and finally they are good for policy science itself, because they bring to it the important input of local knowledge that only citizens possess (Fischer 2000, p. 243-244). In the first two books, two lines of argumentation are clearly discernable. The first one is epistemological. It is contained in the last of the three enumerated positive effects of citizen participation. It insists on citizens’ inquiry because only they themselves can give appropriate account of their own social perspective. It is a methodological concern of policy inquiry, which convincingly argues that one cannot access local knowledge and perspectives of social actors without actually consulting them. The second line of argument is political. It includes the first two positive effects. It calls for the strengthening of democracy, the building of political legitimacy of policy, and curbing the excesses of technocracy.7 Already in the first two books, the second line of argument tends to be more important. In Fischer’s opinion, quest for policy

7 Fischer himself makes the distinction between these two lines: ‘Politically, participatory research's dedication to democratic practices provides a dramatic departure from the mainstream to the corporate-bureaucratic state. On the epistemological level, its emphasis on collaborative research and the methodologies of problem posing, discourse, and social learning confront the most pressing and sophisticated epistemological issues facing social sciences’ (Fischer 2000, p. 191; emphases mine). Cf. also: ‘Inherent to these discourse-analytic and interpretive methods has been an emphasis on participatory democracy, derived as much from the requirements of a postempiricist epistemology as from the values and norms of democratic governance’ (Fischer 2003, p. 17; emphases mine).
deliberation is more a quest of building democracy and finding the political will to do it, than a question of epistemological or methodological discussion:

‘In this respect, the contemporary problem seems to be less a question of methods than one of the political will to introduce and experiment with such practices on a larger societal scale’ (Fischer 2000, p. 259; emphases mine).

‘Rather than taking scientific practices to be ideal for politics, we asked to what degree scientific practices might be democratized. (...) By transforming citizens' ways of knowing and acting, participatory deliberation can build new political cultures capable of preserving and extending decision-making capabilities’ (Fischer 2003, p. 219; emphases mine).

While the two lines of argumentation—epistemological and political—are clearly distinguishable in the first two books, the third book focuses on the political quests of democratization of science and building of participatory policy making, leaving the epistemological line in the second plan. In Democracy and Expertise, the ‘approach is primarily normative’ (Fischer 2009, p. 1). The introductory quotation exemplifies Fischer’s quest. It is a quest for more participation of citizens and against technocracy. Policy sciences of democracy demand:

‘(...) participatory institutions and practices that open democratic spaces for citizen deliberation of empirical outcomes, contextual assumptions, and the social meanings of conclusions’ (Fischer 2009, p. 299).

To be sure, the participation is neither ultimate goal of politics, nor is it required of every citizen on every issue. Fischer also criticizes deliberative democracy (Fischer 2009, p. 82). In Democracy and Expertise, deliberative democracy is criticized because it underestimates power of expertise. It underestimates the real interests, complexity of real world decision making, and it underestimates the role of power. But deliberative democracy isn’t discarded for the usual reasons. In special cases of policy analysis, Fischer, in fact, proposes a more radical version of deliberative democracy as a model of policy analysis and making, that puts citizens and experts on equal level. It is not employed everywhere, but when it is employed in policy making, it is ‘radically egalitarian’, and it is clearly political in the above elaborated sense (cf. the end of section 1). Analysis becomes a collective action in forum, that is, a species of deliberative democracy. Instead of political form for collective decision making, deliberation here becomes a model for democratic shaping of analysis,

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8 The epistemological line is, however, present in the third book. E.g.: ‘To conceptualize this role, we need to move beyond a technical theory of knowledge to a postempiricist, interpretive understanding of communication and argumentation’ (Fischer 2009, p. 159; emphasis mine). But it takes a more instrumental role, as a legitimating tool for the building of stronger democratic participation. It becomes secondary to political project of participatory democracy; it becomes something that is ‘known’, about the primary role of politics: ‘The ultimate success of a postempiricist policy science will depend upon political and institutional reforms’ (Fischer 2009, p. 299). In Fischer’s theory, it is politics that forms knowledge, not vice-versa: ‘From social constructivism in science studies we now know that it is necessary to draw on extra-scientific factors to bring about the closure of scientific and technical debates—scientific method, experiments, observations, and theories are not enough’ (Fischer 2009, p. 140; emphases mine). It strikes me as a bit strange to say that we now ‘know’ anything from a perspective of social constructivism. In that perspective, it is more likely that we ‘construct’, ‘reach political consensus’, ‘decide’, or ‘influence with power’, which is a quite different thing to do.

9 Cf.: ‘It [Democracy and Expertise, KP] has worked, as such, from a realistic assumption—namely, that not everyone must—or even can—participate in the deliberation of all matters. In a complex socio-technical society this is as impossible as it is undesirable’ (Fischer 2009, p. 296).
which seems to delegitimize the autonomous policy research and analysis conducted by the experts themselves.

(3) Is that a standpoint we can accept without hesitation? I don’t think so. Surely, technocratic tendencies of contemporary western societies are worrying and deliberative democratic experimenting may help to contain them in certain policy cases. It is not disputable that policy deliberation can bring new insights and experiences, and that it can serve as a handy political means to create a counterbalance to technocracy. There is no problem in advocating (policy) deliberation as a special political program, which can then be estimated according to its advantages and shortcomings. However, a problem appears if it is stated or implied that deliberative policy analysis is the only legitimate approach to analysis and creation of policies, and that the primary purpose of policy analysis is politicization of passive citizenry, and creating of strong democracy in a more and more expert driven society (Fischer 2000, p. 2). In that way, autonomous research, conducted with the help of different ideas and research methods is implicitly characterized as an illegitimate scientific version of ‘art for art’s sake’. As if it is claimed that there can be no neutrality in policy analysis while the battle for more democracy is raging. Those who are not with us, they are against us!

My characterization perhaps sharpens the position I discuss, and that Fischer’s work cannot unambiguously be subsumed under it. However, his ideas which I briefly sketched, and tendencies they contain, are undoubtedly existent, and they are gaining more prominence. When that position is driven to its ultimate consequences—and these are summarized in the drawing of equation sign between interpretive policy analysis and deliberative democracy—at least three problems emerge, which I want to point out in this section.

(3.1) The first problem is that, by equating interpretive policy analysis and deliberative democracy, interpretive policy analysis takes over all of the problems which still burden the theory of deliberative democracy, which are usually discussed in the field of political theory. Deliberative democracy may lead to change of initial preferences and thus surpass the blockage of decision making; reasonable deliberation can build consensus and maybe even bridge over the deeper differences. Certainly, communicative rationality must not be underestimated. On the other hand, there is no guarantee that it will achieve its proclaimed goals. Discussion can lead to the blockage of decision process. It can lower the quality of the decision made, and even sharpen the conflict between the stakeholders. If analysis and policy making are subjected to deliberative ideal, neither a good analysis, nor a good decision must necessarily result from that. Deliberative democracy has ambiguous consequences. If existing liberal democratic institutions provide enough space for deliberation and democratic control of decision making—and many good arguments can be made to support that position—it is not at all clear why shouldn’t we leave political decision making to them, and enable the analytic work to the ‘self-reliant’ policy scientists? To sum up, entering the field of political theory could here be too expensive a maneuver with unclear gains.

(3.2) The second, more general problem, is that—by accepting of specific political program as a solution for the problems of analysis—the existing equilibrium between politics and analysis is changed in favor of politics, also with somewhat dubious consequences. Of course, in the wider sense, every social-scientific analysis is politics. Being aware of the possible accusations for entering the slippery terrain of metaphysics, the following can none the less be stated: in spite of all constraints, which usually determine action in concrete life-

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10 E.g. one of the interesting critiques of deliberative democracy can be drawn from Hayekian positions, cf. Pennington (2003).

11 Barber maintains that political deliberation can in practice solve many hard problems, including the intriguing paradoxes, theoretical elaboration of which has earned Nobel Prize to some: ‘When asked for a choice, we get the paradox of transitivity; when asked for a clarification, we may get information that leads out of the paradox…’ (Barber 1984, p. 205).
histories (economy, psychology, discourse, etc.), an individual has some possibility to choose. For example, she can analyze politics from social-scientific positions or she can be a problem solving oriented policy analyst. She can instead be a politician in practice or a political activist. Or, she can ignore politics and just ‘manage her private garden’. She can also intervene in behalf of subjecting politics to religious norms, involve herself into Dadaistic provocations against the existing social and political order (or perform them just for aesthetic reasons). All the taken courses of action are choices, and have some (sometimes hardly calculable) consequences. All of them are, in wider sense, politics. Activity we know as (social) science is also politics in that wider sense. And one must say a politics that is worked out in detail—one that has by long tradition established rules, methods and specific scientific ethos, and that works pretty good. However, science is not politics in the narrow sense of collective decision making. Equating policy analysis as a scientific program with deliberative democracy, it becomes politics in the narrow sense, again with somewhat ambiguous consequences. What if deliberative democracy starts pressuring science? According to its presuppositions, scientist has to—when larger group, by stating ‘mutually acceptable arguments’, concludes differently—concede that she was wrong. Could it be the case that deliberative democracy opens up a possibility of interpretive policy analysis limiting itself to a possibly conservative or vulgar common sense of the societal context? Instead of placing it into deliberative forum, it may be that analysis should be left to scientists and their own conscious; and that critique and counter-argumentation should be left to a wider, diffuse process in scientific and general public; and that creating of policies should be left to classical democratic process. In other words, hardly acquired scientific autonomy in the discussed case seems too valuable to be so easily left to politics. If there are expert driven societies, the opposite possibility of radically egalitarian and strongly anti-scientifically disposed societies could also be imagined. Historical approximations certainly exist, not just in the infamous inquisition processes against scientists conducted by post-medieval Church loosing its ideological supremacy, but in a more recent and disturbing examples of totalitarian regimes such as Khmer Rouge Cambodia or China during the Cultural Revolution, just to name the radical examples. It seems to me that in order to be able to ‘speak truth to power’ (Wildavsky 1979), social science and policy analysis should be given a certain amount of autonomy from political power. Deliberative democracy undoubtedly is a form of political power, functioning in a historical socio-political context, so equating policy analysis with deliberative politics, in my understanding, erases an important distinction between politics and science.

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12 Sometimes these proceedings were based on arguments quite reasonable in the context of the then society and scientific knowledge, which lead Paul Feyerabend to radical conclusion that only appropriate scientific methodology is the anti-methodology of ‘anything goes’ and scientific anarchism. But note that this position can also be used against ‘the chauvinism of science’ and for ‘political interference’ (Feyerabend 1993, pp. 33–38).

13 A curious example comes to mind here. One of the most famous and controversial works of Croatian social science written during the former Communist Yugoslavia was a critique of industrial self-management written by a sociologist Josip Županov (Županov 1969, Fanuko 2005). Županov’s critique diagnosed an ‘egalitarian syndrome’ as an important problem of then Yugoslav society. Županov defined egalitarian syndrome as a system of cultural values which contributed to economic inefficiency of the then economic system. Egalitarian syndrome included the demands for equal distribution of all goods, so called ‘uravnilovka’ (roughly translatable as ‘equalization’ or ‘leveling’), ‘the theory of equal stomachs’. But egalitarian syndrome also included animosity towards general intellectual activity. ‘Intellectual uravnilovka’, anti-professionalism and anti-intellectualism were also widespread cultural values. Županov’s work was (for him) a potentially dangerous regime critique, which went against politically determined ‘better arguments’ of his time. I concede that in the times of irresponsible and power driven experts of neoliberalism the story may seem quite anachronistic, but it makes sense to mention it within the framework of this argument. That kind of critical social science work certainly could not be produced within the deliberative councils of the former Yugoslav regime, namely the strongly politicized self-management councils and different Communist party fora. It’s certainly an interesting twist worthy of consideration: in a regime with Marxist ideology as an official religion, empirical sociology functions as a radical critique, while in the capitalist liberal democracies empirical work is often labeled as in essence
Finally, the third problem appears, closely connected to the second one. If the demarcation line between science and deliberative politics is erased, it can be concluded that all the other forms of analysis are politically unacceptable. The pluralism of different positions and usage of different research techniques bring many innovative insights—every different approach for itself and even more through their mutual communication and critique. One does not need to specially demonstrate that scientific pluralism is a ‘good thing’ for science. Possible abolishing of theoretical and methodological pluralism, which could follow from the demands for specific politicization of analysis, could bring about the unproductive theoretical ‘totalitarianism’. Thus, if second point is made from the vantage point of the importance of the autonomy of research, the third one is made from the point of taking in account the importance of research success and scientific development which are usually connected with scientific pluralism.

(4) How to assess this thought experiment of mine? The presented argumentation by no means wants to state that variants of policy analysis which advocate deliberation and participation are illegitimate. Those are worthy analytico-political practices, which can bring out valuable knowledge for policy analysis. They can strengthen democracy and help to legitimate policy. However, autonomous analytical work (e.g. ethnographic studies, contextually sensitive interpretive approach, analysis of narratives, tropes, and policy discourse) is also legitimate. These techniques and theories that inform them are more than enough to constitute interpretive policy analysis. The thing I wanted to express my concern about in this debate piece is just the possibly undesirable tendency of totalizing of just one approach. If interpretive policy analysis would politicize itself in too excessive way, and rely exclusively on citizen deliberation, the undesirable probability of losing itself in a specific political project and disappearing as an autonomous analytical research technique—which can be employed for different purposes, obviously including the betterment of the policy making process—would appear.

Instead of a somewhat constricted species of deliberative democracy, interpretive policy analysis perhaps better suits us as a broad scientific program devised for contextually sensitive understanding of policy process, functioning as a policy advice that can be used for different political purposes in the policy making process. It can include deliberative analysis, not as a condition of, but as an option of research. Good interpretive research, deliberative or not, will enter and influence political arena in a diffuse way. It will be published, read and used to legitimate different visions of policy and policy change. Good research can be done autonomously. No doubt, we should build democracy. No doubt, citizens should participate. No doubt, that can be good for analysis and policy. And no doubt, deliberative policy analysis/making is sometimes a good tool for analysis and a good mode of politics. But not always, not everywhere, and not necessarily. To replace analysis with a form of politics, to completely abolish the separation of politics and science, may not always be good. Politics and social science/policy analysis are not, at least in my opinion, always and not necessarily the same stories. Understanding (and mocking the numbers) could be the best what we, interpretive policy analysts, have for now. If discourse of political agitation totally replaces discourse of understanding, we could loose analysis. Is that a good thing?

References

conservative performance, implicitly legitimizing the structures of the regime by ‘describing’ them. (Just to be sure, Županov also published critical works on nationalist authoritarian regime that emerged in the Croatia of the 1990s, especially on its criminal capitalist privatization of ‘societal property’.)


Appendix 2  The Anonymous Reviews of the Revised Version

Review 1: IPA and DD: Must we Politicise Analysis?

The paper takes issue with what the author constructs to be a central objective of interpretive policy analysis - politicizing the more technocratic quantitative form of policy analysis, and replacing it with deliberative democracy. In doing so, the author while nodding to other writers overwhelmingly places the work of Frank Fischer as the central piece of the literature that creates the 'equation between IPA and DD' (paraphrasing the author here).

There are two major problems with this paper, due to which I feel it should be rejected for publication in CPS. The problems are not a matter of revision but of basic understanding of the postpositivist literature in policy analysis, including that of interpretive policy analysis which the author treats as the centre-piece and finds that its consequences would replace the 'standard' mode of policy analysis with some kind of democratic participation, while not having really engaged much with deliberative democracy in any sufficient depth.

1. The paper starts out with and continues to reinforce the fact/value dichotomy, counterposing more substantively technocratic understanding as superior to more socially grounded methods. In doing so, the author (mis)treats interpretive policy analysis as a program of 'political agitation' (authors' words) than a set of methods, and therefore doesn't lay out how these methods are able to offer counter-expertise in policy, a basic fundamental element of IPA. This treatment develops a contrived and thin version of IPA. That these methods remain grounded in facts, and empirical knowledge simply misses the author's critical view. or the author dismisses these methods due to the author's avowed preference for the technical economics dominated version of policy analysis. This preference is not so hidden in this paper. This is a fundamental misunderstanding of the family of IPA methods, and the IPA enterprise. While a critical treatment would be very welcome in the journal, it should not only have sufficient depth but also evidence that the literature has been engaged with. This paper unfortunately does not bear out that promise.

2. Fischer and deliberative democracy. As one familiar with the postpositivist literature of which Fischer's contribution is a part, this reviewer notes that Fischer's work has been mischaracterized and used selectively out of proportion. It is quite clear in my reading that Fischer's work does not suggest the politicization of policy analysis, in the way the author lays out. Fischer has illustrated the more neglected element in Lasswell's call for policy science of democracy, which the technocratic orientation has dominated (whether for good or for bad is not up for judgment in my review, it is simply that). Moreover Fischer's own work on argumentation and evaluation of policy analysis shows how the normative and empirical (even technical) are brought together through a 4 level system of discourses. Where Fischer refers to politics, is a call for a departure of one form of reason (technocratic) to another kind (socially, culturally grounded), and attention to the contestations and conflicts that are so easily ignored by the technocratic orientation. It is clear that the author takes issue with Fischer's critique of the technocratic orientation rather than substantively engaging with what he actually does. Here the paper is polemic, without the counter-position illustrated in any depth or engagement, but simply falls back on a conservative technocratic position that has been around already.

The paper simply does not add anything to a critique either with IPA or DD and is based on a misreading of the work of Fischer. Much of the critique rests of selective reading, and out of context arguments, without engaging with wider theoretical and methodological strategies advocated by Fischer, with DD being a specific type of methodology in some cases, this doesn't occupy every space in Fischer's vision, but where it does he offers a way of how to understand its utility in situations of socio-technical complexity. More fundamental is the poorly understood enterprise of IPA within the CPS orientation, with which the author has yet to show any critical engagement, even in summative form for the forum section of this journal.
Review of "Interpretive Policy Analysis and Deliberative Democracy: Why we Politicize Analysis."

This paper raises an important question but offers a problematic answer. In my view, it is the wrong answer. There are two different types of problems. The first has to do with the author's reading of Fischer's work, with which I am long familiar. In some cases, it selectively and/or carelessly characterizes that work; in others it is simply wrong. Consider the following points.

Fischer is not a deliberative democrat in the sense attributed to him. As his book on Democracy and Expertise makes clear, he is in fact critical of the approaches that the author outlines (see author's point 1, page 2) as representative of the direction in which he moves. Fischer is in many ways closer to writers such as Young, Walzer and Shapiro, major critics of that tradition spelled out by people like Cohen (Habermas and Dryzek in many ways as well). Fischer's interest is in promoting deliberative politics rather than deliberative democracy per se. He emphasizes what Mansbridge calls the deliberative system.

Fischer does not call for the politicization of policy analysis as such, although he does see policy analysis as regularly being used politically. He seeks to ask what Lasswell's "Policy Science of Democracy" would actually look like, operationalizes so to speak. He clearly indicates that he doesn't advocate everybody participating on everything. Instead, he calls for a "policy epistemics," which is specified as a research program, specifically designed to determine when participation should be used and when not.

His own model is not called "interpretive policy analysis." Indeed, he finds that framework too thin, as it doesn't do the full job of policy analysis. His postpositivist approach, called "deliberative policy analysis, revolves around a four level discursive model, one that integrates standard empirical approaches with normative inquiry (spelled out in great detail in Evaluating Public Policy).

With regard to carelessness, the second quotation on page 4 offers an example. The passage as written by Fischer does not imply a wholesale turn to democracy and participation in policy analysis, as indicated by the call for experimentation to determine what might be possible. The passage is falsely cited (out of the larger context) to support an argument that the underlying goal is political at the expense of methodology. Fischer was arguing that we already know a great deal about empirical-analytic methods and that the reluctance to open up the process is more a matter of a lack of political commitment to using them for democratic ends, which the technocratic tendencies (which have shaped policy analysis) have neglected (in significant part) intentionally (as technocrats do no believe in participatory decision processes).

These points illustrate the author's eagerness to use Fischer as a foil without carefully examining what he is actually arguing. But this is not all. Equally problematic is the author's support of a politics/science dichotomy. In the discussion, he or she seems not to understand what an interpretive policy analysis is about. Missing is a recognition that interpretation is involved in the scientific as well as the political process. While social science is not the same of politics, it has political assumptions imbedded in it through various social and value judgments that are made during the analytical process, even if unwittingly. Thus, parts of the process need to be opened to a more deliberative/participatory discussions, as social scientists have no privileged position in this matter. This is made clear throughout Fischer's work, which is founded on a social constructivist understanding of social and policy science (one that recognizes the scientific community to itself be a social group, functioning in many ways like any other group—witness the recent controversy about emails related to climate change). This is importantly different from erasing the demarcation between science and politics.

In sum, this piece that should be rejected by Critical Policy Studies. Not only is there no basis for recommending revisions—as the work is founded on false premises related to Fischer's work, there is no reason to publish an outmoded—perhaps even latent technocratic-understanding of policy analysis.
Hello to all Kassel conference participants:

We hope you have all managed to rest up after the busy but interesting conference last week. As the editors of Critical Policy Studies, we are writing to ask all of you who are interested in contributing to a special issue of the conference papers (perhaps a double issue) and have papers that are ready—or very near ready—for review to please send us your papers.

We realize that most of these papers are online, but we also need a statement of both status and intention.

Please send the papers to Helen Hancock, the journal administrator, whose address is H.I.Hancock@bham.ac.uk.

Best regards,

Frank, Steven, and Navdeep

Dear Helen,

I received Frank Fischer's call for papers for special issue of CPS, so I'm submitting my paper in the review procedure. Please find it attached to this message.

Best,

Kresimir Petkovic

Thanks. Frank

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14 The messages are reproduced exactly as they were (pasted from the original). The time is Central European (CET). Original files are available on request. Their authenticity can easily be checked among the actors of the affair. However, since their credibility is reduced, I reassure the reader that some specialized CSI forensic unit would surely ascertain their utmost credibility.
Dear Kresimir Petkovic

Thank you for submitting your paper for publication in Critical Policy Studies.

I am passing the paper to the three editors of the Journal, who will consider whether it meets their criteria of quality and relevance. If it does, it will be sent to one or more referees whose reports the Editors will draw on in order to decide whether or not to offer publication. This process usually takes two to three months.

I look forward to being in touch with you when I have some progress to report.

Yours

Helen Hancock

Helen Hancock
Journals Administrator
College of Social Sciences
JG Smith Building
University of Birmingham
BIRMINGHAM B15 2TT
Tel: 0121 414 2835
Email: H.I.Hancock@bham.ac.uk

From:  H.I.Hancock@bham.ac.uk
Subject:  Acknowledgement of CPS IPA12
To:  kresimir.petkovic@inet.hr
Sent:  Wednesday 1st July 2009 14:53

Dear Kresimir Petkovic

CPS IPA12: Interpretive Policy Analysis and Deliberative Democracy: Must We Politicize Analysis?

Thank you for sending in your paper. I have passed it on to the CPS Editors, and I shall let you know about its progress through our review system in due course.

With best wishes

Helen Hancock

Journals Administrator
College of Social Sciences
JG Smith Building
University of Birmingham
BIRMINGHAM B15 2TT
Tel: 0121 414 2835
Email: H.I.Hancock@bham.ac.uk
Dear Kresimir,

I apologize for having taken so long to read your Kassel paper (submitted as well to Critical Policy Studies). But I have now managed to read it and would begin by noting that somewhere in you paper you say that you may have characterized my position. In fact, I think you have.

Nowhere do I support deliberative democracy in all cases. Indeed, I am somewhat skeptical about deliberative democracy, depending on how it is defined (see Democracy and Expertise 2009). Indeed, my call for "policy epistemics" (see Reframing Public Policy and Democracy and Expertise) focuses on the question of when we might use participatory policy analysis and when we might not. I see it as being useful, but not in all cases.

Moreover, I do not argue that citizens and experts can equally judge all policy assessments, including the technical components (exempting in some cases citizens local knowledges of technically related issues, for example the agricultural knowledge of farmers). What I do say is that policy, by its nature, always rests on normative components and here citizens have something to say, sometimes important things to say. In this regard, policy analysts should always consult a wider range of understandings, including those of citizens, as they have no legitimate base to determine these social constructions themselves.

But such consultation is not in and of itself deliberative democracy. Nor does getting the opinions of other members of society mean that policy analysis is politicized. Interpretive policy analysis, by definition, is required to take these positions into consideration.

With regard to Critical Policy Studies, your paper as it now stands is way too long for publication as a journal essay. A piece of about half the length would be suitable for section called Forum, devoted to controversial issues and debates. If that would be of interest to you, I would most happy and willing to reply--debate format-- to your article.

With regards,

Frank

---

Dear Frank,

Thank you very much for taking your time and reading my paper. I should myself apologize to you for taking so long to respond to your e-mail (I'm currently on vacation in Sibenik-a coastal city in Croatia-and I tend to check my e-mail just once or twice a week...).

I also thank you for the clarification of your position you made, which was certainly very helpful. It seems to me that I now have (at least) three possible moves in the game we are playing.

1. The first one is to simply admit that I have missed my mark. Fischer just isn't saying what I said/thought he was saying. Since I made an ad hominem argument (what the man said - although I'm very careful with that, as you've noticed, I hope...) and the man himself says he didn't say what I said he said, then the argument fails. The rebuttal is there and I can only say, with humility and all apologies: "Sorry for misinterpreting your position!"
2. The second one is to say: "Wait a minute, what Fischer just said, isn't what he really said in his books!" You know that in C&E, RPP and D&E you have written more bold and more moderate statements and arguments, that you changed your position as time has passed, etc. The authors aren't the sole masters of their work (and perhaps-as Foucault has suggested in his inaugural lecture at College de France-maybe they even shouldn't be...). So, I could reread your works and try to establish a firmer and better corroboration of my claim that you are, in fact, saying what I said you were saying.

3. The third one - and I think that would be the most interesting and the most constructive option - is to accept what you said and try to analyse it more carefully. It seems, at first, that you are starting from the constructivist and interpretive position as a methodological position. If you want to interpret what people's positions are (and that is IPA), you have to talk to them to approach their "social constructions". As you say, "IPA, by definition, is required to take these positions in consideration." So, it's simply a methodological request implied by the very nature of IPA as a research program.

But, when what you've written is carefully reread, one notices words "normative" and "legitimate base", which are not just metaphorical. Normativity and legitimacy are certainly questions of politics or, let's say, of juridico-political order (if you let me to coin such a syntagma in the spirit of Foucault's "Society Should Be Defended" CdF lectures - to mention F. just for one more & also for the last time...). It isn't politicizing in the narrow sense of party politics, or whatever, but it's a political request certainly, whether you like it or not, even on the level of it's vocabulary (and if I might add, the vocabulary of your books is similar...). So, we could discuss what kind of politics it is, what kind of DD and when, etc.

Then, you see, we already have a debate. (This all is sketchy, I know: I really written it as a first reaction here in the Sibenik's public library...)

If your offer for publishing my paper still stands, I gladly accept it! (You say that the article is too long to be published as a journal essay, but I understand that there is also a problem of quality, since the length reduction is an option for the Forum but you don't mention it as an option for the other section. I would certainly-as I guess every author would-like it to be published as a full article, not just as a controversial position in a debate, but I have no problem if it's seen as such. If publishing it in the Forum is the last and only option-you being the editor and offering it, I guess it is...-I certainly, gladly, and thankfully accept it! And of course, I assume here that your mail is authorized position of the journal, that we continue on this track of communication, that I now don't wait for the three anonymous reviews, etc.)

Of course, we know come to technical questions:

1. How to cut the article to appropriate size, what is the exact upper size limit, and what are your suggestions? (I guess the best option would be to radically reduce the first part with the general stuff, the first three sections, and elaborate a bit on the last one, where the interesting argument is; I would like to keep the table, and rewrite a short intro fort the last section perhaps...)

2. What is the exact form of the debate? (Do you just respond and that's it, or do I get a rejoinder? If I don't, then I should take your position from the mail a priori, and perhaps rewrite the argument a bit, etc.)

3. What are the deadlines?

That's it from me for now. Sorry for the long e-mail, but then again, I feel that we have already started the debate, which is not bad at all.

All the best & looking forward to your answer,
Kreso
Dear Helen,

I finally edited my paper on interpretive policy analysis and deliberative democracy. I adjusted it for the forum section of Critical Policy Studies, as Frank has suggested to me. I radically reduced its size, from about 12 000 words to below 5 000 words – the cap for the forum contributions.

I tried to conform to style guidelines of Taylor & Francis Journals as much as I could in this second version. The reference style is adjusted, quoting, etc. However, reading the instructions, I wasn't sure if all the rules that apply to the peer reviewed articles also apply to the forum contributions. For example, I wasn't sure if the acknowledgements, the abstract and key words, and a short biographical note, are also required for the forum papers. I included them in a separate document just in case; please find it attached to this mail. (I also used footnotes, because I am not familiar with using endnotes in MS Word.)

I hope you are still interested in the paper and that it conforms to the CPS quality standards. If some revisions concerning content are required, I readily await further suggestions. Also, if rejoinder is expected/allowed, I'll be happy to produce one. I don't know if the forum section is designed that way, i.e. does debate have just two or three turns.

Finally, I hope Frank will reply now...

Sincerely Yours,
Kresimir Petkovic

Frank:

I did reply. Frank

Yes you did. But not to the second mail.

Kresimir

Frank
I'm not aware of a second email. The first essay seemed to be altogether based on a misrepresentation of my work. If that is still in there, I would not support publishing the article. Frank

From: kresimir.petkovic@inet.hr
Subject: RE: Kresimir Petkovic - 'IPA & DD: Must We Politicize Analysis?'
To: frankfischer24@aol.com
Sent: Wednesday 23rd September 2009 23:14

I'm pretty tidy with my correspondence, so I saved the reply I sent you. It is in the attachment, you can read it if you like.

'A piece of about half the length would be suitable for section called Forum, devoted to controversial issues and debates. If that would be of interest to you, I would most happy and willing to reply--debate format-- to your article.'

That sounds a bit different. Anyway, you have your own conscience and decisions, and good reasons for them, of course. I respect that. But consider this: it seems to me that I took your work pretty seriously. At least, I closely read three of your books (and many more articles that I don't mention in the text...), and thought about what you said carefully. I think it's good and important work. But I also have a critique.

Perhaps what I came with is only 'a misrepresentation'. But you could at least read what I wrote, and write your reply in which you could give arguments for your claim. I won't cry, if my arguments turn to be weak, and publicly exposed as such. I gave my best. (On the contrary, I would be happy, for the same reason old John Stuart Mill was happy when somebody said something stupid publicly.) Perhaps I could even try to reply to your reply?

Does that sound fair?

Or, on the other hand, you could just discard my 'controversial debate piece' as a misrepresentation...

Cheers,
K.

From: frankfischer24@aol.com
Subject: Re: Kresimir Petkovic - 'IPA & DD: Must We Politicize Analysis?'
To: kresimir.petkovic@inet.hr
Sent: Wednesday 23rd September 2009 23:25

I read the other piece. It was a misreading of my work, clear and simple. How or why am I supposed to support a misreading, which at times struck me as intentional. You paper need me as a strawman; otherwise it had nothing new to say. And equally problematic, in this later regard, it illustrated that you do not understand the nature of interpretation in policy analysis.

I am not particularly inclined to devoting scarce space in the journal to something that is simply wrong, despite whatever John Stuart Mill might have said. For that matter, I think he was talking about public discussion, not journal articles. Normally, journal articles try to avoid publishing things that are stupid, as it makes them look bad.

I will look at your new version, but my computer will not open it, as it says the file may damage my computer.

FF
Dear Frank,

Stupid was used in a metaphorical sense. Certainly, journals shouldn't publish 'something stupid' (as the song goes...), if they like to look good. So, in the last mail, 'stupid' = 'something you disagree about'. That much about subtleties of interpretation and the nature of understanding.

It certainly wasn't my intention to use you as a straw man. I'm sorry if you felt that way. It certainly wasn't my intention to misread your work (and it is certainly not my intention to harm your computer, I think there are no viruses on mine, sorry if the file is infected, I really didn't know, everything looks OK here... perhaps it is the MS file, so I send you the pdf converted file in this mail...).

And if you think of my work what you wrote in the last e-mail, than you certainly shouldn't publish it. I you have such a bad opinion on the first version, I doubt you'll like this one. In any case, you should know that anything I wrote wasn't written with any bad intentions. It wasn't personal, so you shouldn't take it as such.

It is past midnight where I am. Since this isn't a very good deliberation, and I feel it is altogether becoming more of a misunderstanding, than of an understanding - whatever the nature of it could be, hmm... - I think I should go to sleep.

Good night & good luck with the journal, K.

This is not in my hands. If we review the piece, it will be decided by reviewers. FF

Dear Kresimir

Thank you for your revised paper. I can understand that you are now waiting with great interest to read Frank's response.

I note that you have sent Frank (and Dvora) a copy of the paper, and I am now sending copies to the other two editors, Steven Griggs and Navdeep Mathur. As the editor responsible for the Forum section, Navdeep will be particularly keen to read the paper - and the response when it comes.

Thank you for including an abstract, key words and biographical note. These are needed, so that's very helpful. (I usually end up rushing round contributors at the last minute asking for their biographical note.)

I expect that Frank will respond directly to you, but if you have any queries about the publication process at any point, please get in touch.
Dear Helen,

It has been more than a few months now, and I haven't still received any formal decision on my paper I submitted in September (please see the attached message).

I also noticed two issues of CPS have been published in the meantime. So, naturally, I'm wondering about the status of my paper now.

I didn't receive any reviews or comments from Steven Griggs, Navdeep Mathur, or anyone else. Frank Fischer - let's put it mildly - didn't quite agree with my argument (although he initially gave explicit consent to publish it with his reply if I reduce the size of the text and adjust it to the Forum section, which I did, as you know), but he said it wasn't on him to decide. To my knowledge, nobody decided yet, or I wasn't informed about the decision.

Although I have good reasons to believe that the decision is negative, I would still like to receive it in some kind of form other than a long silence period, and possibly complemented with a review of some kind, so that I can decide what to do next with my text. So, just to be sure...

Best wishes,
Kresimir

From:  kresimir.petkovic@inet.hr
Subject:  RE: CPS IPA12 Forum piece for Kassel Issue
To:  H.I.Hancock@bham.ac.uk
Sent:  Thursday 7th January 2010 12:13

Dear Helen,

The editors did email me at the beginning of this week asking me to write to you, but I came back to a mountain of emails on Monday, and I had not got around to dealing with this one when your email arrived.

The editors (Steven, Navdeep and Frank) obtained two reports on your manuscript, and these are attached. Having considered these reports, they asked me to apologize for the time it has taken to get back to you, but to tell you that they do not feel they can accept the paper for publication in CPS.

Best wishes,

Kresimir

From:  H.I.Hancock@bham.ac.uk
Subject:  RE: CPS IPA12 Forum piece for Kassel Issue
To:  kresimir.petkovic@inet.hr
Sent:  Thursday 7th January 2010 17:29
I do apologize for my own tardiness in passing on these reports and the editors’ decision, and thus delaying your own decision as to what you do next with your text.

With best wishes

Yours

Helen

Helen Hancock
Journals Administrator
College of Social Sciences
Room 1149, Muirhead Tower
University of Birmingham
BIRMINGHAM B15 2TT
Tel: +44 (0) 121 414 2835
Fax: +44 (0) 121 414 4954
Email: H.I.Hancock@bham.ac.uk

From: kresimir.petkovic@inet.hr
Subject: RE: CPS IPA12 Forum piece for Kassel Issue
To: H.I.Hancock@bham.ac.uk
Sent: Thursday 7th January 2010 18:05

Dear Helen,

Thank you very much for sending these stimulating reviews.

And no need for apologies, I know it is not your fault. Anyway, I hope you successfully dealt with the mountain in the end: usually, one can simply walk back, or perhaps go around, but the administrative staff always has to climb over, no matter how steep the climb...

Send my regards to Frank & co.

Yours,
Kresimir