

Research Workshop



25 - 26 May 2012, GWII / S 5

Experts and Consensus in Economics and the Social Sciences

www.martinicarlo.net/research/experts – DFG-funded workshop

Friday, 25 May

- 09.30 - 10.30 **Marcel Boumans** (Amsterdam and Rotterdam) – The problem of rational consensus in economics
- 10.45 - 11.45 **Filip Buekens** (Tilburg) – Truth, accuracy and expertise
- 12.00 - 13.00 **Maria Jimenez Buedo** (Madrid) – Attributing expertise in uncertain times
- 14.30 - 15.30 **Merel Lefevere & Eric Schliesser** (Ghent) – Expert responsibility and moral community
- 15.45 - 16.45 **Rafał Wierchosławski** (Lublin) – The role of experts in the condominium model of republican (re-)solution of social, economic, and political problems
- 17.00 - 18.00 **Frank den Butter** (Amsterdam) – The institutional economics of stakeholder consultation; reducing implementation costs through ‘matching zones’

Saturday, 26 May

- 09.00 - 10.00 **Robert Evans** (Cardiff) – Emperors, mavericks and children: consensus and its critics
- 10.15 - 11.15 **Amir Konigsberg** (Jerusalem) – The real problem of disagreement
- 11.30 - 12.30 **Aviezer Tucker** (Austin and Cologne) – Applying the Neyman-Rubin model of causal inference to the explanation of expert consensus
- 13.45 - 14.45 **Carlo Martini** (Bayreuth) – Expertise and institutional design in economics
- 15.00 - 16.00 **Laszlo Kosolovsky** (Ghent) – Explicating ways of consensus making in science and society: towards a social account of consensus formation
- 16.15 - 17.15 **J.D. Trout** (Chicago) – The horror and pretense of democratizing science: searching for alternatives
- 17.30 - 18.30 *Roundtable*: state of the art and open problems in a methodology of expertise

Experts and Consensus in Economics and the Social Sciences

University of Bayreuth, May 25-26 2012

GW-II (Humanities) - Room S5

WORKSHOP PROGRAM AND BOOK OF ABSTRACTS

FRIDAY 25TH

- 8.50 - 9.15 *mini-bus service from Hotel Ramada to Campus*
- 9.30 - 10.30 **Marcel Boumans** The problem of rational consensus in economics;
- 10.45 - 11.45 **Filip Buekens** Truth, accuracy and expertise;
- 12.00 - 13.00 **Maria Jimenez Buedo** Attributing expertise in uncertain times;
- 13.00 - 14.30 LUNCH BREAK
- 14.30 - 15.30 **Merel Lefevere & Eric Schliesser** Expert responsibility and moral community;
- 15.45 - 16.45 **Rafał Wierzchosławski** The role of experts in the condominium model of republican (re-)solution of social, economic, and political problems;
- 17.00 - 18.00 **Frank den Butter** The institutional economics of stakeholder consultation; reducing implementation costs through ‘matching zones’;
- 19.30 WORKSHOP DINNER *mini-bus service from the university to the restaurant*

SATURDAY 25TH

8.30 - 8.50 *mini-bus service from Hotel Ramada to Campus*

9.00 - 10.00 **Robert Evans** Emperors, mavericks and children: consensus and its critics;

10.15 - 11.15 **Amir Konigsberg** The real problem of disagreement;

11.30 - 12.30 **Aviezer Tucker** Applying the Neyman-Rubin model of causal inference to
the explanation of expert consensus;

12.30 - 13.45 LUNCH BREAK

13.45 - 14.45 **Carlo Martini** Expertise and institutional design in economics;

15.00 - 16.00 **Laszlo Kosolovsky** Explicating ways of consensus making in science
and society: towards a social account of consensus formation;

16.15 - 17.15 **J.D. Trout** The horror and pretense of democratizing science:
searching for alternatives;

17.30 - 18.30 **Roundtable:** State of the art and open problems in a methodology of expertise.

20.00 INFORMAL DINNER & SOCIAL GATHERING

EXTENDED ABSTRACTS

Marcel Boumans (Amsterdam School of Economics - University of Amsterdam and EIPE - Erasmus University)

The problem of rational consensus in economics. The aim of the method of rational consensus is to produce a mathematical aggregation of expert opinion by weighing the opinion of each expert on the basis of his or her knowledge and ability to judge relevant uncertainties. These relevant uncertainties are seed variables whose values are or will be known to the analyst but not to the expert. So, seed variables are important for assessing the performance of the experts assessments. To fulfill this role, a seed variable has to be a stable fact about a phenomenon, which is generally no problem in natural science, where this method became quite successful. The problem of economics, however, is that these stable facts hardly exist. They are rare because economic phenomena are not as stable as natural phenomena and because there is a lack of consensus in economics what these facts are, which make these facts even subjective. Therefore, as the paper will show, rational consensus in economics has to be model based, that is, the weighing of the expert's assessment has to be framed by the relevant empirical models instead of seed variables. Model-based facts are objective because they allow for empirical validation. In other words, in economics consensus on seed variables is replaced by consensus on empirically validated models.

Filip Buekens (Tilburg Center for Logic and Philosophy of Science (TiLPS) - Tilburg)

Truth, Accuracy and Expertise. When we evaluate outcomes of epistemic actions as justified or unjustified, good or bad, rational or irrational, we make, in a broad sense of that term, normative judgements about them. In this

paper, we look at accuracy as a desirable quality of beliefs and assertions. We explore operational accuracy, which focuses on a statement and its relation to an explicit or implicit standard set by participants involved in an epistemic exchange. While truth is a semantic property of propositions, the accuracy of an assertion that expresses a true proposition is informed by non-epistemic goals which act as filter with truths one has access to via one's epistemic methods available in the context of inquiry as its input, and accurate truths as output. Experts should be reliable as well as accurate informants. They are able to determine public standards of accuracy, tell accurate from inaccurate information and shift standards of accuracy in view of our epistemic and practical goals

Frank den Butter (Department of Economics - University of Amsterdam)

The Institutional Economics of Stakeholder Consultation; Reducing Implementations Costs through 'Matching Zones'. Complicated projects, policy plans and government regulation usually involve a number of stakeholders with diverging interests. A good infrastructure for the consultation of, and for the discussion between these stakeholders is needed in order to avoid high implementation costs. Following the theory of new institutional economics these implementation costs can be seen as transaction costs. This is especially relevant in G2B and G2C relationships where the projects and policy measures bring about (re)distribution problems. This paper discusses various ways to organise these consultations, so that a compromise agreement is reached on the solution of the (re)distribution problem. These institutionalised structures of consultation are referred to as "matching zones" here. Practical experiences, mainly from the Netherlands, provide guidelines for the effective institutional setup of such

“matching zones”. Specifically, the design of a “matching zone” should try to adhere to the following principles: (i) there should be a common interest and ample incentives for consensus; (ii) there should be the prospect of long, repeated interaction; (iii) there should be a balance between representation and efficiency; (iv) the constraints should be clear from the onset of the matching zone; (v) fairness should be strived for; (vi) IC technology should be utilised optimally; and (viii) informal contacts and an amicable atmosphere should be promoted.

Robert Evans (Cardiff School of Social Sciences - Cardiff University)

Emperors, Mavericks and Children: Consensus and its critics. Consensus is a beguiling idea. It implies an agreement that all can share and, with this, confers some epistemic robustness upon the outcome. The difficulty — or perhaps the danger — is that we focus on what lies in the centre of this shared space and fail to consider its boundaries. In particular, is there temptation to think that what the least controversial elements of the consensus (i.e. the centre) deserve extra weight. In this presentation I want to unpack the idea of a consensus and examine how we might value the range of views a consensus encompasses. The aim is to avoid discussing how consensus might be reached and focus on the logically prior question of ‘consensus amongst whom’?

In its ideal form the scientific community can be seen as a consensus-seeking activity and provides a standard argument for expert consensus: claims are made and evidence presented with a view to persuading others to agree. There is no coercion and interlocutors are persuaded by evidence and argument. Science and Technology Studies (STS) have shown that the process is not as pristine as some might imagine but as a normative ideal, its combination of evidence and reason seems hard to fault.

In practice, however, the reality of science reveals some of the limits of a consensus reached by like-minded individuals. Whilst it seems reasonable to give special weight to the opinions of those who have studied a topic in some detail, there is a downside to such specialisation. For example, where consensus is reached by virtue of prior agreements about the way in which problems should be structured and/or different kinds of evidence evaluated is it fair to say all alternatives are considered equally? Or, does it make more sense to say that the use of shared analytic models and other standard practices facilitates the production of consensus amongst practitioners as they allow group members to reach similar conclusions quite quickly. In such circumstances, the risk is that a partial perspective becomes mistaken, particularly by its proponents, for a more complete understanding. To avoid such problems more participation from those outside the narrow consensus-forming world of the official experts is called for. In some cases, this leads to the calls for the democratisation of expertise. Again, there is some merit in this argument, but (again) some limits are needed. Including more voices raises more questions but the range of possible challenges is potentially infinite. It therefore follows that, where participation is based on knowledge some criteria for determining what is to count as a legitimate challenge are needed. Balancing these boundary problems of legitimacy and extension are thus central to the problem of consensus. If the boundary is too porous, then consensus will never be reached. If the boundary is drawn too tightly, reaching consensus may be easy but its usefulness may be highly curtailed. If we accept that non-coercive decision-making is appropriate, then the problem of group decision-making starts with the problem of defining the group. By drawing on notions of expertise and experience, the paper will conclude by setting out some criteria by which participants in consensus discussions might be selected.

Maria Jimenez Buedo (Logic, History and Philosophy of Science - UNED, Madrid)

Attributing expertise in uncertain times. In the last years, the nature of expert judgement and the role of the expert in the policy making process is becoming an issue of growing importance for several strands of literature that reflect upon the nature of social science knowledge and collective decision making processes (Collins and Evans 2007, Selinger and Crease 2006).

In relation to the role of experts in the social sciences, and especially in economics, a series of recent works, heir to a long-standing tradition in social psychology devoted to the systematic studying of experts' performance (Camerer and Johnson 1991), have been emphasizing the specialists' poor predictive record relative to that of statistical models or even to the amateur informed reader. Overconfidence and other cognitive biases and reasoning fallacies have been documented as being partly responsible for this relative underperformance (Tetlock 2005, Angner 2006).

Some of these works have also explored how the institutional framework in which economics experts operate may be partly responsible for the perpetuation of their poor predictive records, and have launched a series of recommendations with the intention of stirring public debate in ways that would improve experts' accountability (Reiss, 2007). The thrust and spirit of these recommendations are also shared by Goldman's well-known contribution to the problem of choosing amongst competing experts from a social epistemological perspective (Goldman 2001). These all suggest a series of actions that should be undertaken by the novice seeking advice from the expert and include, among others, the revision of the experts' records of past predictive successes, the necessity of well-defined, unambiguous predictions, and the checking of potential bias of the expert, stemming from her own personal stakes in the

matter under consultation.

The emphasis regarding this normative aspect of expertise has thus been put on the side of the novice, and though sometimes suggested in the literature, (see for example, by Angner 2006), very little has been said regarding how the predictive failures of experts may be related to their self-selection or whether the same characteristics that make an expert a poor performer may be the ones that make him or her postulate him or herself as expert. In this paper I argue that this crucial aspect of the problem of expertise has been neglected, in part, as a result of the downplaying of the relational aspect of expertise that is characteristic of the renewal in the study of expert knowledge (Collins and Evans 2007). The paper, by drawing on the literature on Comparative Political Economy that has studied the role of expert ideas in times of economic and political crises (Lindvall 2009, Blyth 2003) suggests new ways to look at the attributional and relational aspects of expertise.

Amir Konigsberg (Center for the Study of Rationality, The Hebrew University in Jerusalem)

The real problem of disagreement. Recent debates surrounding the epistemic significance of peer disagreement have sought to provide responses to cases in which peers disagree about the epistemic import of a shared body of evidence. Various responses have been suggested in the literature, such as the Equal Weight View, the Total Evidence View, the Common Sense View as well as a number of other closely related approaches. Typically, these address problems of the following general form: If persons A and B are epistemic peers - meaning roughly that it is equally probable that A and B will be correct in the domain in which they are peers - and on a particular unexpected occasion they happen to find out that they disagree about whether a particular proposition P is true given the

evidence equally available to them both, and assuming that neither party has any independent reason to discount the dissenting party's conclusion, they ought to respond to this discovery ... in such and such a way.

The responses to this problem in the literature vary, and can, I believe, be divided into three kinds: 1) the bootstrap response; 2) the conciliatory response, and 3) the egalitarian response. I think that some of these responses have considerable appeal. But I also think that some make sense theoretically, but are not practically sensible, and that others are simply unreasonable. All the same, I do not believe that establishing which of the responses presented in the literature is better, as a good deal of the recent debate has been focused on doing, actually addresses the normative problem of disagreement.

The problem of disagreement asks about the appropriate response (typically the response of a peer) upon encountering a disagreement between peers. The responses proposed in the literature offer different solutions to the problem, each of which has more or less normative appeal. Yet none of these seems to engage with what seems to be the real problem of disagreement. It is my aim in this paper to highlight what I think the real problem of disagreement is. It is, roughly, the problem of deciding whether a revisionary tactic is appropriate following the discovery of disagreement as well as deciding which revisionary tactic is appropriate. This, I will show, is a slippery and inevitable problem that any discussion of disagreement ought to deal with. Moreover, it is a problem that, once recognized, also impinges on the question of which revisionary tactic is appropriate.

The above-mentioned approaches (1-3 above) are characterized by the different tactics that they propose for dealing with disagreement. But these tactics only appear to be relevant after the truly hard work of deciding whether they are relevant in each actual case of disagreement has been done. And this, I believe, is a huge problem that

has not been adequately recognized in the literature or has even largely been missed until now. Moreover, the epistemic significance of this problem extends far beyond debates surrounding disagreement. It involves the subjective appreciation of evidence about the reliability of inferences from evidence in general. It is my aim here to draw attention to this problem which I believe lies at the heart of debates surrounding disagreement. It is my contention that actual cases of disagreement, as opposed to possible cases of disagreement, must deal with this inevitable situation.

Laszlo Kosolosky (Center for Logic and Philosophy of Science, Ghent University)

Explicating ways of consensus making in science and society: Towards a social account of consensus formation. Worldwide matters of concern, such as climate change and the economic crisis, have shown that science is not an isle in society, where scientists should (solely) pursue science for its own sake. The underlying value-free ideal of science is a hot topic of debate in contemporary philosophy of science (Lacey, 1999; Longino, 2002; Douglas, 2009; Elliott, 2011). Scientists are more often being called upon as experts to give advice to policymakers and (governmental) organizations. Society, however, finds itself confronted with experts disagreeing and making mistakes. This puts a burden on both science, as it seems to be hit in its core, and society, which (often blindly) relies upon the advice of scientists for policy making. In general, scientific research does not produce logically firm evidence about the natural world. And thus, the corresponding idea of science being able to deliver undisputable evidence upon which policy decisions could be made, is flawed. In a scientific community, different individuals can weigh evidence in different manners through the use of different standards. In the best case, science puts forward a robust consensus based on

a research process that allows continued scrutiny, re-examination, and revision (Oreskes, 2004). On the one hand, when push comes to shove, establishing a scientific consensus is imperative to solve controversies, such as global warming. Establishing a consensus on the causes and extent of global warming could facilitate policymaking and, moreover, send a convincing signal that doing nothing will have dire consequences. On the other hand, studies carrying attention for plurality and heterodoxy have raised questions concerning the ideal of the scientific consensus and, connected to it, the neglect of dissent (Solomon, 2006). For instance, dissent is epistemically valuable, because it evokes discussion, which makes deliberation possible (Longino, 2002) and because dissenting views carry interesting insights that would otherwise get lost in the process of consensus formation (Solomon, 2006).

In solving this tension between plurality and consensus, which is not always made explicit in accounts of consensus as knowledge-based (Gilbert, 1987), there is a meta-consensus or a meta-agreement in play. Therefore, instead of focusing on consensus on the simple level (that is, as the result of alternative theories/models tested against one another eventually thought to be leading to some consensus outcome, we should shift to analyzing the meta-consensus that stipulates the procedure to be followed. Understandings of consensus-making differ in how much weight they place on procedures relative to substantive considerations about the quality or characteristics of the outcomes of these processes. The resulting account of consensus will be a social one (not stipulating the characteristics the outcome should have, but stipulating the social procedure that has to be followed) — analogous to the social account of objectivity developed by Helen Longino (2002). Obviously, it might be self-evident that an account of consensus is social. Consensus is always a product of sociality. However, we want to shift the focus from the product to the

epistemic processes constituting an outcome. Taking this route, could also imply that consensus comes in degrees, depending on the extent to which the procedure has been followed, repeated, etc. Further elaborating on the details, advantages and pitfalls of such an account in understanding the complex relation between science and society in general and consensus and plurality in particular, is the aim of this paper. The author will moreover draw on current debates in Social Epistemology to present his own findings accordingly (Kosolovsky, 2010 & 2011 & under review; Kosolovsky & Van Bouwel, in preparation; Seselja & Kosolovsky, 2012).

Merel Lefevere & Eric Schliesser (Center for Logic and Philosophy of Science, Ghent University)

Expert responsibility and Moral Community. In her 2009 book Douglas reflects on the moral responsibilities of scientists, and claims that scientists need to consider the consequences of error in their work. We expect reasonable foresight and careful deliberation from scientists. Reasonable foresight and careful deliberation are to be measured against the benchmarks of the scientific community. Failure of this foresight and deliberation can cause two unwanted situations: negligence and recklessness. We point to at least two serious flaws in her description of moral responsibility in science.

First, Douglas does not provide a moral theory about the character of the scientific community; we argue that this omission in Douglas' theory leaves it without adequate response to the "The?Everybody-Did?It (TEDI), Syndrome". Second, in a system where the scientific community sets the standards, there is a danger of self-affirming reasoning. The community consists, of course, of individual scientists. During communication through journals, conferences, etc. they develop a benchmark for what reasonable scientists should foresee and

how they should deliberate. In practice, this means that alternative models and even long-standing objections get suppressed from a discipline's collective toolkit and memory. The members of the community create the conditions by which the community creates the vice of close-minded and there can be collective negligence. One possible solution is to demand pluralism of scientists who are working on the aggregate level (editor, teacher, etc.). Efforts should be focused on structuring scientific community in such a way that it reflects the diversity at the individual level.

Martini Carlo (Philosophy & Economics - Bayreuth and TiLPS - Tilburg)

Expertise and Institutional Design in Economics. When economists are concerned with practical applications of their science, it is widely acknowledged that analytical theory and objective evidence alone are often not sufficient for giving practical institutional directives. In those cases subjective expert judgment is recognized as an important source of evidence whenever analytical or experimental methods fail to provide enough support to a specific policy stance (Cooke 1991; Reiss 2008). Recourse to subjective expert judgment is in fact the norm in most economic institutions. It is because they require human expertise that the quality and performance of institutions depend heavily on the type of decision making procedures, the membership structure, accountability, transparency, and several other factors which, together, make up the design of an institution. Providing the tools that make for good institutional design is a central interest of much interdisciplinary research at the intersection of political sciences, economics, and philosophy. In this paper I evaluate six principles of institutional design for economic committees. The principles are meant to provide a normative theory against which to gauge the contribution of

expertise and subjective expert judgment to economic committees. I identify two kinds of committees: one satisfying broad-focus problem-solving tasks, the other dedicated to narrow-focus forecasting and assessment, and two types of expertise, with similar characteristics. The six principles, which are repeatedly found in much of the literature on expertise, are more or less compelling depending on whether one or the other of the two kinds of committees and expertise are involved. I assess each of them on the basis of two case studies: the Boskin Commission and the Bank of England's Monetary Policy Committee.

J.D. Trout (Department of Philosophy - Loyola University Chicago)

The Horror and Pretense of Democratizing Science: Searching for Alternatives. In democracies, citizens are expected to make decisions that bear on their own well-being and that of others. But some of those decisions — especially decisions based on technical scientific information — go well beyond their knowledge and training. As an epistemic matter, should an English literature professor decide on responsible reactions to ocean acidification? A roofer to stem cell generation? A pastor to electric car technology? A computer engineer to the disposal of nuclear waste, batteries, or wood preservatives? In democracies, these gaps in expertise are usually spanned by voting for a representative rather than on an issue. And in these democratic settings, there are two chief models for deciding on matters requiring scientific expertise: (1) Assemble expert panels to advise inexpert legislators on expert matters, or (2) allocate decisions about scientific matters to scientific experts who are civil servants. The first option is a reasonable arrangement only when either (a) the representatives interests are aligned with those of the citizens they represent, or (b) when

violations of their allegiances are met with swift punishment. Neither of these conditions obtains when legislators presume to decide scientific issues. To cite a characteristic instance, in the U.S. California Representative Rohrabacher (R-CA) and Diaz-Balart (R-FL) did not suffer for their very public misrepresentations of House Resolution 3247 considered by the Committee on Science, Space, and Technology. With little knowledge of science, some media savvy, and no requirement that you attend expert panels, members of Congress can kill a science bill like HR 3247 on purely political grounds, so that it never gets a scientific hearing. We will take a look at how this happens, and how it circumvents the legislative charge. Another tactic used by scientifically inexpert political leaders is to force a reappraisal of specific scientific findings, as Toomey (R-PA) did in the mid-1990s when he introduced a bill to block funding for a number of expert, peer-approved scientific proposals at the National Institutes of Health. Neither of these tactics can go unpunished in a society that prioritizes evidence-based policies over an unseemly gumbo of patently corruptive influences on policy-making. That there are these noxious influences is not surprising. The political representatives themselves have interests that conflict with the political obligations of their roles, and they can ignore or explore these conflicts with impunity. In light of the severe and persistent problems of (1), I examine approaches that treat scientific matters in administrative law, and advance preliminary proposals about civil servants of science whose tenure spans administrations.

Aviezer Tucker (University of Texas in Austin and University of Cologne)

Applying the Neyman-Rubin model of causal inference to the explanation of expert consensus. I propose to draw epistemic conclusions from consensus among experts by applying the

statistical method known as the Neyman-Rubin model of causal inference. This inference is common in the Social Sciences, Medicine and Agronomy. The method begins with a hypothesis that connects types of causes with correlated types of effects. The effects in the epistemic context that concerns me are correlation of opinions among experts, a consensus. The suggested cause is that they possess knowledge, that the best explanation of the consensus is shared knowledge.

In the first stage, epistemologists need to prove that the correlations between the opinions of experts (the consensus of opinions) are more likely given the common cause type (knowledge) than given different and separate types of causes, possible biases like interests, prejudices, ignorance, power relations and so on. When we use the Neyman-Rubin model of causal inference we specify the properties of the common cause type we propose (knowledge in this case), but do not specify the properties of the alternative types of separate causes (confounders whose properties are specified are alternative common causes). The method for achieving a significant gap between the likelihoods of the correlations given the common cause type and the unspecified separate causes that may be many, varied and unknown, is the random assignment of members to two populations to make them nearly identical in sharing the same types of (unknown or unspecified) variables with the exception of the common cause type (sometimes called the treatment) that all the members of one group share and none of the members of the other (control) group are affected by. Significant differences between the two populations are likely then to be the result of that common cause type. For example, social scientists may choose a random sample of persons representing a population and divide the random sample into two randomly assigned equal and sufficiently large groups, whose only difference is the presence or absence of the hypothetical common cause type. Then, social scientists would measure

the difference in the putative effects between the two populations and see if there is a significant gap between the two groups. If so, the correlation between the types of effects is more likely given the common cause type than given unspecified separate types of causes. In the case of experts, we want to have a randomly assigned population sample that shares everything with the experts except their expertise.

The second stage is the elimination of confounders, alternative common causes that all the experts share but have nothing to do with knowledge, for example gender bias if all the experts are of the same gender or national bias if they are all of the same nation and so on. In this way, the epistemology of consensus can be given a firm methodological statistical and empirical basis, using a fruitful method, extensively used in the social sciences (as well as medicine, agronomy and so on).

Rafał Wierzchoślawski (Faculty of Philosophy, John Paul II Catholic University of Lublin)

The role of experts in the condominium model of republican (re-)solution of social, economic, and political problems. Usually the experts problem is considered in epistemological and sociological contexts, i.e. deals with privileged epistemic position of experts against that of laics, and their specific place in social stratification. Experts' considerations are often extended onto such topics like the role of science in democratic societies and experts contribution in social and political decision making, and as the effect they provides typologies of experts (Collins, Evans, 2007, Stephen P. Turner 2003). The aim of my presentation is to examine how analysis of expertizing question (typologies) applies to the problems of politics and by consequence to resolving social and economic problems. The experimental field is going to be modern neo-republican tradition.

Republican idea of freedom and government has a venerable tradition, and it has enjoyed a significant revival in recent years, competing with other genres in reflection on politics. There are different types of neo-republican restorations Athenian and Roman one, or in other formulations Mediterranean-Atlantic and Franco-Prussian (Pettit 2010). I will consider the second interpretation of republican heritage, which got its modern formulation in writings of Quentin Skinner and Philip Pettit and their followers (Skinner 1998, 2002, Pettit 1996, 1997). The republican idea of freedom claims that we are free when we are not dominated i.e. we are not under mastery and possibility of capricious intervention in the domain of our actions, by a possible master — we are not in potestate domini, but we live in sui juris (Pettit 2010: 37). The domination in question can happen in two areas: imperium and dominium: the first one deals with relation state-individual (institutional and public) and the other one deals with relations between individuals in social realm (i.e. family), as well as in economy domain (i.e. work conditions and regulations). We can avoid domination when we can point it out and when we are able to protect our freedom applying various mechanisms, which can eliminate, or at least diminish possibility of domination. In the first context of non-domination the main aim is to ensure that institutions are limited and controlled by non-dominating law (contestatory principle) and constitutional devices of mixed-constitution, which ensures individual liberty. In the other context, the aim of civic republican is to formulate social-democratic idea of progressive intervention in favor of the most dominated groups of society a postulate, which has got a full voice in the recent stage of development of the republican interpretation of dominium (Martí, Pettit 2010). “The state should intervene so as to guard against the private domination and should organize itself so as to guard against public domination. The civic republican project is to translate those principles

into specific designs for the civic control of public power and into specific policies for the establishment of a social order in which even the poorest citizens can command the respect of their fellows, conscious of not being exposed to private power in the basic domains of human choice (Pettit 2010: 52-53).

In my presentation I would like to provide an analysis of the condominium model as a method to resolve decision-making problem of diverse opinion presented by its participants, which has been proposed by Philip Pettit in his recent publications (Martí, Pettit 2010; Pettit, 2008, 2010, 2011). Pettit refers to independent bodies, auditors, solicitors as experts and prescribe them essential and important role to play in the condominium model: “Regulatory constraints that subjects the committee to monitoring by independent officials and bodies, appointed from among owners or from outside, such as auditors and solicitors.”

Outsourcing constraints that require committee to outsource decisions to an independent arbitrator or advisor in arenas where rival, individual interests are engaged, Tie-breaking constraints that ensure that decisions between equally acceptable policies are not indefinitely delayed and are made by procedures that themselves fit with common concerns: depending on the case, these might authorize a committee vote, or a referendum, or referral to an expert or impartial body. (Martí, Pettit 2010: 64). The main presumption of Pettit deal with experts impartiality and independence as far as resolution of conflicting situations, which may happen in the condominium. However it seems that he omits the problem how experts position might be dependent on the type of expertizing she/he belongs to. In my opinion this presumption should be examined in the light of expert studies, and I will do it applying Stephen Turner’s experts taxonomy (Turner 2003).

I will focus my particular attention on (1) Pettit’s understanding of common concerns vs. sectarian interests as far as public goods are

concerned; (2) the condominium model which should “have to conduct its life on the pattern of the mixed constitution and the contestatory citizenry [...] give an important role to civic deliberation [...] in public decision-making” (Martí, Pettit 2010: 68); (3) Turner’s taxonomy of experts to the requirements the condominium model and (4) consideration of profits and limits of experts’ referral demand for the condominium model. What makes the project interesting is that Pettit’s theoretical project has been in certain measure tested in actual application in Zapatero’s Spain, and by that token it can be considered the accountability test has been passed or failed (Martí, Pettit 2010, Pettit 2011).