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How to sell power indices

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Abstract: There is no obvious answer to the problem about the right power measure. This paper suggests that we can learn "sales arguments" for power indices by looking at the history of the concepts of social welfare and, alternatively, the equilibrium notion in game theory. For instance, there isn't any unambiguous definition of social welfare. In general, we contend ourselves with a tautology: social welfare is what social welfare functions measure. Irrespective of the multitude of social welfare functions and thus by the multitude of welfare concepts, a standard paper in microeconomics has a section dedicated to "welfare analysis" - which follows the sections headlined "basic model" and "equilibrium analysis". This paper will also discuss the dominance of the Nash equilibrium concept in game theory. Although the Nash equilibrium is a questionable behavioral description for many game situations and often leads to inconclusive results, the large majority of game theorists agree that a game outcome has to concur with a Nash equilibrium - otherwise it does not make much sense. Why such a wide consensus does not exist for a specific power index?

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1. Introduction

Assume that there are three parties, A, B and C, which share the seats in parliament by 45%, 35%, and 20%. Given that decisions are made by simple majority it seems not very likely that the distribution of power, however defined, coincides with the distribution of votes.

Power indices have been developed to discuss issues of assigning power values to the resources (e.g., votes) of decision makers and to explain how these values change if the (vote) distribution changes or a new decision rule is applied. They seem to be valuable instruments to analyze institutional changes and effects of alternative institutional design. The two volumes, "Power, Voting, and Voting Power" (Holler, 1982a) and "Power indices and Coalition Formation" (Holler and Owen, 2001) not only contain original contributions to this discussion but also illustrate the development in this field over the last twenty years. A recent monography by Felsenthal and Machover (1998), "The Measurement of Voting Power", contains a formal treatment of the problem.

There is a growing interest in power measures such as the Shapley-Shubik index and the Banzhaf index, to name the two most popular measures, and their application to political institutions and, in particular, to the analysis of the European Union.[1] There are also new theoretical instruments and perspectives that support these applications. Of prime importance is the probabilistic model of coalition formation which is made operational by the multilinear extension of the characteristic function form of coalition games introduced by Owen (1972). This instrument triggered off a reinterpretation of existing power indices and the formulation of new ones.

This development has been accompanied by an intensive discussion of the concept of power in general - what do we measure when we apply power measures? - and the properties that an adequate measure of power has to satisfy. However, the question about which index is the right one is not conclusive. Selection criteria have

been proposed, derived from stories which accompany the indices but are not implicit to the formal measure concept. Other selection criteria refer to plausible properties - such as monotonicty - which are however (a) not unambiguously defined and (b) not necessarily implicit to the notion of power.

Given the multitude of power measures, a possible strategy is to choose a favorite power measure and to try to convince others to share this choice. An alternative is to accept the multitude of measures and their interpretations and select an appropriate measure in concurrence with the intuition possibly based on the accompanying stories. At the first glance, these two alternatives do not seem to be very promising strategies; they are likely to convince neither the experienced critics of the power measures nor the still hesitant newcomers to power research. A third alternative is to postulate discriminating properties which a power measure has to satisfy in order to qualify as an appropriate measure. Local monotonicity has been proposed to be such a property. Even if we accept this criteroin, a larger number of indices is left at our disposition. However, there are good arguments not to accept this criterion.

It seems that there is no obvious answer to the problem. In this paper, I suggest that we try to develop "sales arguments" for power indices by looking into the history of welfare economics and the discussion and application of social welfare functions. Alternatively, we can study the success of the Nash equilibrium notion in game theory. Irrespective of the multitude of social welfare functions and thus by the multitude of welfare concepts, a standard paper in microeconomics has a section dedicated to "welfare analysis" which follows the sections headlined "basic model" and "equilibrium analysis". In general, we contend ourselves with a tautology: social welfare is what social welfare functions measure. We will take a closer look at this practice in section 3 while section 4 deals with the dominance of the Nash equilibrium concept in game theory. Although the Nash equilibrium is questionable behavioral description for many game situation and often leads to inconclusive results, a large majority of game theorists agree that a game outcome has to concur with a Nash

equilibrium - otherwise it does not make much sense. The discussion of the Nash equilibrium, which will be sketched below, demonstrates that a wide consensus can be reached despite rather controversal arguments and obvious shortcomings of the selected standard. Why such a wide consensus does not exist for a specific power index? For example, is there an appropriate refinement strategy for power indices? Section 5 outlines research strategies which guaranteed the success of Nash equilibrium are outlined and suggests some principles which could be applied to develop a unifying approach to power measures.

Scholars need a scientific methode to give a framework to their inquiry and debate. A rigorous methode enables us to see relationships that may be obscure without. But we should be aware that our expertise is defined by our methode. We should therefore be very careful when we narrow down the scope of our scientific tool box. I will argue in this paper that power indices can be successfully sold if we accept their multitude and make use it for power research.

2. The Local Monotonicity Yardstick

Felsenthal and Machover (1998, pp.221ff) are very explicit that any a priori measure that of power that violates Local Monotonicity (LM) is 'pathological' and should be disqualified as serving as a valid yeardstick to measuring power. For weighted voting games, LM implies that a voter i who controls a larger share of vote cannot have a smaller share of power than a voter j with a smaller voting weight.[2]

LM is an implication of desirability as proposed by Isbell (1958). This property implies that a voter i is at least as desirable as a voter j if for any coalition S, such that the union of S and {j} is a winning coalition, the union of S and {i} is also winning. Freixas and Gambarelli (1977) use desirability as a yardstick which defines reasonable power measures. Since both the Deegan-Packel index and Public Good Index violate LM (Holler and Packel ,1983), they also violate desirability.

In Holler (1998), I argued that when it comes to monotonicity of power with respect to voting weights, it is important to note that none of the existing measures guarantees that the power measure of player i will *not* decrease if his or her voting weight increases. Fischer and Schotter (1978) demonstrate this result (i.e., the paradox of redistribution) for the Shapley-Shubik index and the normalized Banzhaf index. This paradox stresses the fact that power is a social concept: if we discuss the power of an individual member of a group in isolation from his or her social context, i.e. related only to his or her individual resources, we may experience all sorts of paradoxical results.

It seems that sociologists are quite aware of this problem and nonmonotonicity of an individual's power with respect to his or her individual resources does not come as a surprise to them (see, e.g., Caplow, 1968). Political scientists, however, often see the nonmonotonicity of power as a threat to the principle of democracy. To them it is hard to accept that increasing the number of votes a group has could decrease its power, although it seems that there is ample empirical evidence for it. (See Brams and Fishburn (1995) for references.) In general, economists also assume that controlling more resources is more likely to mean more power than less. However, they also deal with concepts like monopoly power, bargaining, and exploitation which stress the social context of power and the social value of resources (assets, money, property, etc.).

In Holler et al. (2001), the authors analyse alternative constraints with respect to their consequences for the local monotonicity of the Public Good Index. For example, it is obvious that local monotonicity will not be violated by any of the known power measures, including the Public Good Index.and the Deegan-Packel index if there are n voters and n-2 voters are dummies. It is, however, less obvious that local monotonicity is also satisfied for the Public Good Index if we constrain the set of games so that there are only n-4 dummies.

In recent paper Braham and Steffen (2002) show that satisfying local monotonicity depends on what we assume to be a priori. More specific they

demonstrate that applications of Straffin's (1977) partial homogeneity approach do not always produce results which are consistent with LM. This is because partial homogeneity does not treat players symmetrically so that coalitions are not of equal weight: the power of a voter i depends not only upon the number of coalitions for which i is critical but also upon the probabilities by which the various coalitions arise. Of course, the larger the probability for a coalition S for which i is critical, the larger is the power which i derives from this coalition. However, in the extreme cases of partial homogeneity, defined by the Banzhaf index (in case of strictly independent voters) and the Shapley-Shubik index (in case of homogenous voters), LM is guaranteed.

Needless to say that this result depends on the probability interpretation of power and power measures. If we accept this interpretation, then Braham and Steffen (2002) argue that Straffin's homogeneity approach is not less a priori than the Banzhaf index and the Shapley-Shubik index. Since, originally, the Deegan-Packel index and the Public Good Index derive from an axiomatic approach, probability arguments do not necessarily appliy to these measures. However, applying the probability model to this measures, Braham and Steffen conclude that the arguments in Deegan and Packel (1978), Holler (1997, 1998) and Brams and Fishburn (1995) that the power must be accepted to be not locally monotonic "is not entirely correct either".

If we generalize the partial homogeneity approach to the Public Good Index and apply zero probabilities to winning coalitions with surplus players then the a priori argument which Braham and Steffen (2002) derive for the partial homogeneity measure should also be available for the Public Good Index. Brueckner (2001) demonstrates that we can extend the probabilistic characterization so that the Public Good Index follows from the homogeneity assumption if we consider strict minimum winning coalitions only. That is, there is a axiomatic approach and probability interpretation for the Public Good Index. From this point of view there is no difference to the Banzhaf index or the Shapley-Shubik index - and the question of a priori cannot be answered on this basis.

From the analysis of Straffin's partial homogeneity approach we can conlude that there is the possibility of a violatrion of LM whenever coalitions (or permutations) are not taken into consideration for the calculation of the power measure with equal probability. This, of course, holds for *a posteriori measures* which derive probabilities for coalitions from empirical (or historical) data.[3] It would be interesting to see whether the extensions of the Shapley-Shubik index and Banzhaf index, proposed in Owen (1977) and Owen (1982), satisfy LM.

3. Welfare and Welfare Functions

In general, we contend ourselves with a tautology: social welfare is what social welfare functions measure.
Samuelson (1972) on Kenneth Arrow
Winch (1971)

4. The Success Story of the Nash equilibrium

Van Damme (1987, p.3) writes: "the solution of a non-cooperative game has to be a Nash equilibrium since every other strategy combination is self-destabilizing if binding agreements are not possible." However, are utility maximizing (rational) players interested in stability per se? Does stability of choices contribute to the well-being of a player?

The Nash equilibrium became the standard solution for decribing game outcomes. Nash (1951) argued that any theory of games, including cooperative games, should be reducible to equilibrium analysis. "With this step, Nash carried social science into a new world where a unified analytical structure can be found for studying all situations of conflict and cooperation" (Myerson, 1999, 1074). The so-called Nash

program unified cooperative and non-cooperative game theory: "Instead of taking a proposed cooperative solution concept on trust, Nash proposed that it be *tested* with the help of noncooperative bargaining models constructed to capture the essence of the bargaining procedures whose outcome the cooperative solution concept supposedly predicts" (Binmore, 1998, p.44). By this, the Nash equilibrium became not only the unifying approach to game theory but the unifying general structure for economic analysis. (See Myerson, 1999).

Like many other eminent game theorists, Binmore (1998, p. 25f.) seems to subscribe to the Nash equilibrium "The idea of a Nash equilibrium is basic to noncooperative game theory. An authoritative game theory book cannot possibly recommend a strategy pair as the solution to a game unless the pair is a Nash equilibrium. If the book recommends a strategy to me that is not a best reply to the strategy it recommends to my opponent, then I will not follow its recommendation if I believe that my opponent will." But why should the opponent follow the prescribed strategy? In general, game theory books do not suggest strategy pairs but solution concepts - and the Nash equilibrium is highly recommended. Surprisingly, Binmore (1998, p.26) askes himself "why did von Neumann and Morgenstern not formulate this extension themselves?" Binmore's guess is "that they recognized that it is often not very helpful to say that the solution of a game must be a Nash equilibrium. Interesting games typically have many different Nash equilibria." So it seems that the multiplicity of the Nash equilibrium is the problem. " Still Binmore maintains that "a proper extension of the Von Neumann Morgenstern maximin criteria simply says that the solution of a noncooperative game lies among its Nash equilibria" (p.26). Moreover, "For a two-person, zero-sum game, the equilibrium selection problem is irrelevant, because all the Nash equilibria in such a game are equally satisfactory. I think that von Neumann and Morgenstern saw that the same is not true in general and therefore said nothing at all rather than saying something they perceived as being unsatisfactory" (p.26).

However, there is this remarkable footnote to this latter statement. "As they would have put it, the defense given above of the nash equilibrium concept is entirely negative. It only says that nothing other than a Nash equilibrium can be the solution of a game. But a player needs positive reasons for choosing one strategy rather than another" (p.26). The latter condition may not hold even when the Nash equilibrium is unique. There is a class of variable-sum 2-by-2 games such that both the Nash equilibrium and the maximin solution are mixed. In this case, the two solution concepts assign the identical payoffs to each player, but prescribe different strategy choices. The fact that the Nash equilibrium strategy of player i is exclusively determined by the payoffs of player j, and not by its own, seems to be at odds with the utility maxmization behavior hypothesis governing game theory. (See Holler, 1990, 1993).

I do not want to discuss here whether the assumption of rational behavior is adequate when looking at economic and social behavior and applying game theory to analyze it (see Rubinstein, 2000, chapter 5, for a discussion). In any case, Tan and da Costa Werlang (1988) demonstrate that it needs additional, rather strong assumption, to derive Nash equilibrium behavior from utility maximization. Similarly, Myerson (1999) argues that the Nash equilibrium is a *necessary*, but *not sufficient* condition for rational behavior. The case of the mixed strategy Nash equilibrium in variable-sum 2-by-2 games, however, demonstrates that the relationship between the two Nash equilibrium ans utility maximization can be rather "weak".[4]

If the payoff of player is the same for Nash equilibrium and maximin then the game is "unprofitable". In case of unprofitable mixed-strategy equilibria, Harsanyi (1977, p.125) strongly suggests that players choose maximin strategies instead of trying to reach an equilibrium. Aumann (1985, p.668) concludes from studying an

[4]The mixed strategy Nash equilibrium in variable-sum 2-by-2 games is weak: unilateral deviation does not to the disadvantage of a player. Obviously, Nash equilibrium does not exhaust strategic behavior. Schelling's (1960) focal point theory assumes that games with multiple equilibria should be understood as games where common cultural perceptions or historical traditions can have a decisive effect. Harsanyi (1973) teaches us that when equilibria are in mixed strategies, "each player's behavior may depend critically on something that he knows

unprofitable mixed strategy equilibrium: "Under these circumstances, it is hard to see why the players would use their equilibrium strategies."

There is an other argument which questions the use of the Nash equilibrium concept: How myopic is the Nash equilibrium? If players look further ahead, they might see that it could be, on the one hand, in their interests to destabilize a Nash equilibrium or, on the other, to choose strategies which are not best replies, given that only one move of reaction is possible (as assumed in the Nash equilibrium), which are however best replies if, in principle, an infinite sequence of reactions and counter-reactions are possible. Brams's "Theory of Moves" (see Brams and Wittman, 1981; Holler, 1993) takes care of this type of strategic interaction. For instance, that it predicts for the Chicken Game that one of the two strategy pairs is stable which do not form a Nash equilibrium, there is a potential of a sequence of iterating moves.

So far we have collected a series of arguments which make the Nash equilibrium a rather dubious solution concept. There are. however, also strong arguments in favor of it and which, in the end, support the Nash equilibrium as the standard solution concept for non-cooperative games. Perhaps the strongest arguments derive from the evolutionary game approach. Mailath (1998) discusses Nash equilibria as rest points of evolutionary games and their relationship to asymptotic stability and evoltionary stable strategies. These results support the Nash equilibrium as outcome of decentralized decisions which are coordinated via a Darwinian selection process (for instance, captured by a replicator function). More specifically, Andreozzi (2002) demonstrates for the inspection game that the evolutionary process oscillates around the Nash equilibrium of the static one-shot game. This supports the Nash equilibrium as "average of the possible outcomes". However, Andreozzi also shows that players who choose the mixed strategy maximin solution (see above) will not "die out", i.e. "are strong enough to survive".

5. Refinements and Standards

The application of evolutionary game concepts, like in Andreozzi (2002), had to aspects: (a) the modelling of the interaction of individual decisions when agents do not form rational expectations, i.e., if they are not fully aware of how their socio-economic environment works and therefore the rationality of their decision is "bounded"; and (b) the support it gives to the Nash equilibrium and equilibrium selection. For instance, Mailath (1998) focuses on the second aspect as can be readily seen from the title of his contribution. This aspect is closely related to the refinement approach which has neen developed (a) to reduce the multiplicity of Nash equilibria and (b) to sort out counter-intuitive (implausible) equilibria. In general, the objectives (a) and (b) are met simultaneously. As such, refinements (such as subgame and trempling-hand perfectness, sequential equilibrium, the intuitive criterion) were helpful tools to support the Nash equilibrium as the exclusive solution concepts.

When the application of a refinement concept let pass Nash equilibria which were not convincing from the point of view of rational decision making - or standard economics and results of empirical (experimental) research -, a new refinement concept was formalized which helped to cure the case. If considered necessary, the Nash equilibrium concept was even extended. For instance, Bernheim (1984) and Pearce (1984) simultaneously introduced the concept of rationalizable strategies. In short, a strategy is rationalizable if it is a best reply to a strategy which is rationalizable. Rationalizable strategies are not necessarily equilibrium strategies, however, the mutually best-reply strategies of a Nash equilibrium are always rationalizable. This consequence contributes to a further justification of the Nash equilibrium and, in the eyes of many game theorists, bridge the gap between Nash equilibrium and the utility maximization hypothesis of rationality.

Rationalizability is the starting point of *New Game Theory* which considers the Nash equilibrium and its refinements as merely a short-cut in the epistemic game about forming beliefs and expectations about the other players' beliefs and expectations in a game situation (see Holler, 2002). So far related models are very demanding and the results are not always ready for generalizations. As a consequence

the Nash equilibrium and its refinements are still the standard tool to analyze game situations. If there is incomplete information then Harsanyi's *consistent Bayesian games* are the standard analytical framework.[5]

Is the Public Good Index a refinement of the Banzhaf index, and if so in what sense?

The discussion of power indices is characterized by the search of the "right index"[6] and the claims that non-favored indices are inadequate, inappropriate, or pathological. To outsiders this looks very puzzling - and not very attractive. Some insiders of the discussion suffer personally from the negative appreciation which their propositions earned from other researchers in this area. (This is quite different from what we see in the discussion of the Nash equilibrium and its refinements.) There seems to be a "tradition of exclusion" and an "urge for uniqueness and orginality" in this discussion. Perhaps power as a subject asks its toll.

Contrary to the Nash equilibrium and its refinements the relations between the various power measures is as yet not clarified. However, there is work which tries to contribute to this program. Allingham (1975) has shown that the Dahl measure is simply the Shapley value without weights. More recently, Widgrén (2001) analyzed the probabilistic relationship of the Public Good Index (h_i) and the normalized Banzhaf index (β_i) and demonstrates that β_i can be written as a linear function of h_i such that $\beta_i = (1-\pi)h_i + \pi\varepsilon_i$. Here $(1-\pi)$ represents the share of strict minimum winning coalitions, compared to all minimum winning coalitions, i.e. coalitions that contain at least one swinger; and ε_i expresses the share of minimum winning coalitions which have i as a member, but are not strict, compared to the number of all minimum

[5]In a consistent Bayesian game the differences in the players' belief systems "at the beginning of the game could have been caused by their having observed different random variables, about which all players had common prior beliefs" (Myerson, 1999, p.1076). Informational differences thus can be explained by differences in players' experiences.

[6]Perhaps Holler and Packel (1983) were the first who explicitly raised the question of the "right index". This, however, was a misleading question.

winning coalitions which are not strict. Obviously, the larger these two shares the more β_i and h_i deviate from each other.

Widgrén interprets the part of the function that is independent of the Public Good Iindex, $\pi\varepsilon_i$, as an expression of a special type of luck in the sense of Barry (1980). If the institutional setting is such that minimum winning coalitions form which are not necessarily strictly minimal, and the corresponding coalition goods are produced, then the normalized Banzhaf index seems an appropriate measure. In this case, local monotonicity is guaranteed. This implies that the institutions are such that the fundamental free-rider problem, of which the Public Good Index takes care (Holler, 1982b), does not apply.

At the first glance, a comparison of the axioms underlying the various power measures seems to be alternative approach to clarify the relationship between the various power measures. However, since most axiom sets differ by only one axiom one has to conclude that it is the combinations which brings about the variance which results in a multitude of measures.

Another source of variance is the difference in the notion of power which the authors of the various measure propose. Is power a probability, capacity, or potential - or merely a theoretical concept? Does power depend on preferences - and, if so, on which preferences? Unfortunately, the power index community is far from finding a standard answer to these questions. However, can we sell the power indices without developing a standardized scheme of discussion and application? I suggest that we try to learn from the application of welfare economics and the discussion of the Nash equilibrium and its refinements.

6. Conclusion

Myerson (1999, p.1080) argues that "the task for economic theorists in the generations after Nash has been to identify the game models that yield the most useful insights into economic problems. The ultimate goal of this work will be to build a canon of some

dozens of game models, such that a student who has worked threough the analysis of these canonical examples should be prepared to understand the subtleties of competitive forces in the widest variety of real social situations".

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