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## Boekbespreking van: *Verdeelde Macht. Een onderzoek naar invloed op rijksbesluitvorming in Nederland*

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development of a systematic comparative presentation (see, Table 15.2, p. 254) could be interesting, by creating more complex indicators for describing the crucial dimensions of variation (for instance, the degree of organizational consolidation of PPGs). Nevertheless, given the little information currently available on the PPGs, and given the enormous costs of such an extensive comparison, the book by Heidar and Koole is destined to provoke very favourable reactions among political scientists both for its findings and as a good example of comparative research.

Luca Verzichelli

Klaartje Peters, *Verdeelde Macht. Een onderzoek naar invloed op rijksbesluitvorming in Nederland [Diffused Power. A study of the influence on political decision-making in the Netherlands]*. Amsterdam: Boom 1999. ISBN 90-5352-559-9, f 52,-.

Peters' book, for which she received the 2000 Dutch Political Science Dissertation Award, deals with the question, which actors succeed in influencing political decision-making in the Netherlands. Three cases are analysed over the period 1985-1995. The first one deals with childcare policies and the need for state subsidies, which has been a fiercely disputed issue in the Netherlands. The second case concerns the reorganization of the entire Dutch police system, which, because of bureaucratic strife, turned out to be a very sensitive issue. And the third case deals with rural area policies and the 'distribution of space' between agriculture, housing, infrastructure, tourism and nature, each sector having its own claims. In analysing these three cases, the book tests several mainstream theories on the distribution of power in Dutch politics.

The resulting book is very readable and interesting. It is readable because, in the first part of the book, Peters describes 30 years of Dutch research on political power in a very comprehensive and transparent way. A fine text, I would say, for both students and seniors interested in power studies. Moreover, her analysis of the three cases in the second part of the book is accessible without becoming superficial. The book is also interesting, because Peters confronts her own conclusions on the way power is divided in the three cases of Dutch political decision-making with eleven hypotheses, which she deduces from the overview of Dutch research on political power. In doing so, she is able to verify some hypotheses on power, falsify others, and formulate some new ones. This approach enables Peters to put her research into broader perspective, and to transcend the status of merely a collection of case studies, although some methodological risks are involved here.

In her overview of Dutch research on political power, Peters discusses several classical views on power structures in the Netherlands. These include the weakness of Dutch parliament vis-à-vis the power of government, the power of civil departments (the so-called *fourth* power), the power of interest groups (the so-called *iron ring*), and the

corporatist power structure in agriculture (the so-called *iron triangle*). In the final part of her study, Peters confronts these views with the conclusions of her own study, and consequently falsifies most of them. According to her, Dutch parliament is generally not a weak institution. Moreover, neither an iron ring nor an iron triangle can be said to exist. On the other hand, the hypotheses concerning the power of government, especially of individual ministers, and concerning the 'fourth power' are confirmed. However, because these conclusions are based on a generalization of the findings in only three case studies, Peters has become vulnerable to criticism. Some accuse her of violating the rules of external validity. She wrongly extrapolates her conclusions, derived from a few cases, to the entire Dutch political arena. Yet this criticism is, in my opinion, not entirely justified, all the more so since Peters carefully deals with this problem in the methodological part of her study (pp. 90-91). Following Yin and others, she makes the distinction between statistical and theoretical generalizations. The former refers to quantitative research, the latter to case studies. In most case studies, a complex pattern of a phenomenon is studied, not a few variables of a large population. Therefore, the problem of generalization is different. The premise of theoretical generalization is that as long as a specific pattern of a phenomenon is reproduced in any new case, or in a number of new cases, theoretical generalization is valid. This means that the underlying hypothesis can be maintained *for the time being*. However, as soon as a pattern is not found, generalization is not possible, and the hypothesis concerned should be dismissed, or at least put into perspective. The problem with Peters' analysis is that she is aware of this validity issue, but does not deal with it properly in the second part of her book. The care she employs in the methodological chapter, is lacking in the concluding chapters.

In my opinion, the *internal* validity of Peters' study is a greater problem than the external validity. To measure power, she uses the so-called *intensive process analysis*, developed by Huberts, which heavily draws upon Dahl's decision-making method. Essentially, the intensive process analysis is a detailed reconstruction of decision-making in terms of relevant authorities, relevant participants, their preferences, their interventions, and the eventual decision itself. Its aim is to reconstruct causal linkages between preferences and acts of participants on the one hand and the contents of the decision taken by the authorities on the other. If such linkages are found, the actors concerned are considered powerful. A tool to make such causal analyses is the *timetable*, in which the change of preferences of the several actors over time is reconstructed. On the basis of such a table, actors can be 'eliminated', because they appear not to be influential. For example, if decision-maker X's preference remains different from participant Y's preference during the entire period of decision-making, then Y should not be considered influential. Finally, of those who last, a score on political influence is determined.

Although the intensive process analysis is a well-elaborated tool, as I have experienced myself, in my opinion, Peters does not fully acknowledge problems with regard to internal validity. Firstly, a basic precondition for a successful analysis is knowledge of the 'true' preferences of actors, as Peters herself suggests (p. 77).

However, she does not reflect on this issue sufficiently. After all, to what extent can she know the 'true' preferences of her study objects, given the fact that political decision-making processes are *strategic* settings? Do the respondents involved know their exact preferences themselves? These questions suggest that a reconstruction of preferences, and the changes over time, is not unproblematic. Secondly, a *correlation* of preference development of participants on the one hand and of decision-makers on the other does not necessarily imply *causation*. Peters is fully aware of that, but does not sufficiently put her conclusions into this perspective. Thirdly, it remains totally unclear which criteria have been used to ascribe a certain *extent* of influence to actors. While other political scientists have developed variables and formulae to assess the extent of power in political decision-making, even in the context of qualitative case studies, Peters remains silent on this issue. We simply have to believe her scores. All in all, it would have been better if Peters had considered her conclusions *well-educated guesses*, as other students of power do, instead of real, objective statements, as she seems to suggest.

Irrespective of the above criticism, which all point to a lack of reflection on the internal validity of the study, the empirical analyses themselves are interesting. As far as the first case is concerned (childcare), the minister involved, one political party (Christian-Democrats), civil servants and parliament have all been influential actors, while for example regional and local governments as well as interest groups seem to have failed to affect decision-making here. The same is more or less true for the second case (police system). The third case (rural areas), however, is deviant, in the sense that parliament and the relevant minister are less influential, while the Department of Agriculture, Nature and Fisheries is very dominant. Also, interest groups are a bit more influential in this case. Peters explains this difference by the technical character of this policy domain. All in all, Peters concludes that the distribution of influence in Dutch politics is as follows. The political influence of parliament is *substantial*, as is the one of the national government. The influence of civil departments amounts to *much*, while regional and local governments exert *some* influence. Finally, interest groups exert only *marginal* influence.

Peters' general conclusion brings me to my final criticism: she is quite silent on structural power and on the duality of agent and structural power. Apart from a short theoretical analysis (pp. 61-62), she does not put her empirical conclusions on agent power into a more structural perspective. This is striking, as many of her hypotheses deal with *structures* (fourth power, iron ring, iron triangle, etc.). Had she done so, by asking herself what consequences her conclusions on agent power in three cases of policy-making imply for structural power in Dutch politics, she might have prevented much of the criticism with regard to external validity. After all, she could have discussed the duality of agent power in specific cases of policy-making on the one hand and structural power in Dutch politics on the other, without suggesting a linear, aggregate relationship.

Despite these points of criticism, I liked the book very much. Given the fact that, firstly, the study of political power is a complex, scientific battlefield and that, secondly,

the concept of power is essentially contested, any solution for methodological and conceptual problems raises new ones, at which critics may target. Therefore, my criticism should not overshadow the fact that I believe that Peters has written a fine book. After all, it teaches political scientists that in-depth empirical research is very much needed to underpin hypotheses on power, which we tend to reify in the literature so often, while political practices unfold otherwise. At the same time, the book makes it obvious that such empirical research remains problematic. Many of the theoretical and methodological problems that started with Dahl – e.g., generalization, causation and structuration – have to a large extent remained unresolved so far, also in Peters' book.

Bas Arts