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Boekbespreking van: Rational choice and political power

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– Amerikaanse burgers zouden onderworpen kunnen worden aan juridische vervolging in het buitenland, waar zij op minder bescherming zouden kunnen rekenen;

– ratificatie zou de Verenigde Staten al te zeer betrekken in internationale aangelegenheden ('increase international entanglements').

De lijst van opgesomde bezwaren, die door Kaufman uitvoerig aan de hand van de nodige citaten worden gepresenteerd, is overwegend afkomstig uit (ultra)-conservatieve hoek. De tegenstanders van ratificatie zijn er over het algemeen in geslaagd de bewijsvoering om te draaien: in plaats van de vraag waarom de verdragen *niet* zouden moeten worden goedgekeurd, wordt de vraag veelal gesteld waarom dat eigenlijk *wel* zou moeten gebeuren. Daarmee worden de voorstanders van ratificatie in de verdediging gedrongen en krijgen zij de taak op een groot aantal moeilijke politieke en juridische vragen een antwoord te vinden.

Kaufman beschouwt het nog steeds voortdurende verzet tegen de mensenrechtenverdragen als een erfenis van de jaren vijftig. Het begon allemaal met het zogeheten Bricker-amendement op de Amerikaanse Constitutie van 1953, dat erop gericht was de handelingsvrijheid van de Amerikaanse president in het sluiten van internationale overeenkomsten en verdragen in te perken. Kaufman laat zien dat de motivatie voor het amendement in belangrijke mate was ingegeven door angst voor de op stapel staande internationale mensenrechtenverdragen. De regering-Eisenhower was meer bezorgd over de dreigende inperking van de bevoegdheden van de president op het gebied van het buitenlands beleid in het algemeen dan over de bedreiging van de mensenrechtenverdragen. Dat gold ook voor de tegenstanders van het amendement in de Senaat. Uiteindelijk slaagde senator Bricker er niet in de vereiste tweederde meerderheid voor zijn voorstel te behalen: 52 senatoren stemden vóór en 40 tegen, maar dat betekende niet dat de laatste voorstanders waren van de mensenrechtenverdragen. In tegendeel: in de volgende jaren werden de argumenten, die tijdens de debatten over het Bricker-amendement ertegen waren aangevoerd, voortdurend herhaald.

Angst voor veranderingen – in het politieke en economische stelsel, in de traditionele Amerikaanse 'way of life' – was en is de belangrijkste reden die Kaufman geeft voor het verzet tegen de mensenrechtenverdragen. Dat verzet in de Senaat werd gevoed door een machtige lobby in de Amerikaanse Orde van Advocaten, de American Bar Association, waarbinnen zich conservatieve krachten bevonden die van al deze nieuwlichterij niets moesten hebben. Zij voorzagen de Senaat van de eerder genoemde argumenten en voerden het woord op de openbare hoorzittingen. De enige verdragen die *wél* zijn aangenomen betreffen een toevoeging aan de Slavernij Conventie, die niet als een nieuw verdrag werd beschouwd, de conventie over de politieke rechten van de vrouw en – na een verdragingsactie in de Senaat van meer dan dertig jaar – uiteindelijk het internationale verdrag tegen genocide. Maar dit laatste werd omgeven met vergaande voorbehouden en interpretaties die door een aantal andere partijen bij dit verdrag, waaronder Groot-Brittannië, Zweden en Nederland, als niet geldig worden beschouwd. (Zie voor een uitstekend overzicht van de Amerikaanse houding ten aanzien van het genocideverdrag: Lawrence J. LeBlanc, Duke University Press, *The United States and the*

Genocide Convention, Durham en Londen, 1991.) Ook de regering-Carter had zoveel voorbehouden ingebouwd bij de voorstellen tot goedkeuring van de beide internationale mensenrechtenverdragen dat de betekenis ervan sterk werd gerelativeerd – zelfs als de Senaat ze *wél* zou hebben goedgekeurd. De presidenten Reagan en Bush hebben zich trouwens niet erg ingespannen om de verdragen door de Senaat te loodsen.

Kaufman wijst erop dat het niet goedkeuren van de beide mensenrechtenverdragen door de Verenigde Staten vooral van symbolische betekenis is. De Amerikaanse regering maakt zich minder geloofwaardig als zij kritiek uitoefent op schendingen van de mensenrechten door andere regeringen: 'The United States has put itself in the position of implying either that the violating government is bound by U.S. standards or that the government is bound by an international standard applicable to foreign governments but not to the United States' (p. 198).

Het boek bevat een uitstekende beschrijving van de behandeling van de mensenrechtenverdragen door de Senaat, de werking van de oppositionele lobbies en de gewisselde argumenten. De schrijfster heeft er weinig moeite mee de argumenten van de tegenstanders door te prikken. Het *hoe* van de gevoerde discussie komt dan ook uitstekend uit de verf. Minder bevredigend is de beantwoording van de *waarom*-vraag. Geloofden de tegenstanders werkelijk in hun eigen argumenten? Vanwaar toch die angst? Kaufman voelt zich duidelijk meer verbonden met de voorstanders van ratificatie en gaat de tegenstanders vooral met rationele overwegingen te lijf, terwijl de bezwaren toch blijkbaar sterk van emotionele aard zijn. Het boek zou nog aan waarde hebben gewonnen, als zij erin was geslaagd iets meer van de achterliggende motieven van de tegenpartij boven water te brengen. Daarvoor heeft zij zich echter te weinig in die motieven verdiept.

Deze kritiek neemt niet weg dat we hier te maken hebben met een goede studie over een belangrijk aspect van het Amerikaanse mensenrechtenbeleid, dat zelf weer een belangrijk deel uitmaakt van de buitenlandse politiek van de Verenigde Staten. Ook aan diegenen die wellicht wat minder geïnteresseerd zijn in het onderwerp als zodanig, maar wel belangstelling hebben voor 'congressional politics' in het algemeen, zij het boek als 'case study' van harte aanbevolen.

P.R. Baehr

K.M. Dowding, **Rational Choice and Political Power**. Edward Elgar, Aldershot 1991.

The U.S. Supreme Court has nine Justices. Every time a vacancy arises, and the President proposes a candidate, a heated debate breaks out about the political views – if not about other traits – of the proposed candidate, since many cases before the Court are coloured by ideology. Suppose that the eight incumbent Justices are equally divided in conservatives and liberals, and that the proposed candidate is an ideological nonconformist. His vote would then be decisive again and again, and

the new Justice would get his way most of the time. Does this mean that he is a powerful Justice? According to many empirical theorists of power, the answer would be 'yes', but not so according to Brian Barry, who relates the story (in 'The Uses of Power', recently reprinted in his *Democracy and Power*, 1991). The new Justice is merely lucky, to be taking a seat in a Court so divided and being a nonconformist himself. Luck, not power, distinguishes this Justice from his eight colleagues.

The distinction between power and luck is also central to Keith Dowding's book. What is real power? How is power related to preferences, interests, and ideology? Dowding cannot be accused of lacking courage, as he sets out to settle these questions by way of a 'conceptual exercise' (p. 175) in what must be the most overexercized concept in political science. His main points are, very briefly, that we should distinguish between power, luck, and systematic luck; that this distinction illuminates some old debates around power and society, and that a theory of power involves bargaining.

Dowding's argument is complicated, and as a result difficult to summarize. It is structured like a funnel, starting with a treatment of basic concepts in the first four chapters and concluding with the way in which these concepts are interrelated in chapter 7. Chapters 5 and 6 contain a critical analysis of previous research into power.

In his introductory chapter, Dowding argues for a dispositional over a causal definition of power. He prefers to steer a middle course between individualist and structuralist accounts of power, since social structure is an important resource for individual and group power. After a short exposition of the theory of rational choice in chapter 2, Dowding in chapter 3 goes into the relations between preferences and (objective) interests. Needs and interests do not always coincide with preferences. But, Dowding argues, the fact that people may misspecify their preferences does not imply that another person 'is pulling the strings' (p. 46). This 'blame-fallacy' is a familiar phenomenon in many community power studies, as he further explains in chapter 5.

The concept of political power is clarified at the outset of chapter 4. Dowding distinguishes social power (my ability to deliberately change your incentive structure to bring about or help to bring about outcomes) from mere outcome power (my ability to bring about or help to bring about outcomes). As politics is very broadly defined as a conflict-related concept, social power may be identified as political power. Social power depends on the resources that actors may use in bargaining with other actors. Dowding presents an informal account of Harsanyi's work, in which he emphasizes the importance of these resources and of a reputation for stubbornness in the bargaining process.

Groups as well as individuals may be powerful and/or lucky. The question how much power and how much luck is involved, is central to Dowding's re-analysis of (parts of) the debate between pluralists, elitists and statists in chapters 5 and 6. Chapter 5 is mainly devoted to studies in local politics. Following Barry, luck is defined as the probability of getting what you want without trying. Community power studies have usually been more concerned with luck than with power, and

their results therefore cannot be considered valid. Dowding rightly claims that many social or interest groups which fail to further their group interests are powerless 'on their own', i.e. they are not necessarily being oppressed by other, vested interest groups. The state level is considered in chapter 6, where another important body of political science literature illustrates Dowding's argument that social power is enhanced by Harsanyi's four categories of bargaining resources: information, legitimate authority, unconditional incentives to alter others' incentive structure, and conditional incentives to alter others' incentive structure.

Chapter 7 connects with chapter 3, and is about the relationships between preferences, resources, ideology and power. As Dowding argued in chapter 3, interests may only indirectly be related to preferences. When the structure of society tends to favour some groups' interests, as for example in the case of farmers' unions, these groups may be called *systematically lucky*. Dowding readily admits the apparent contradiction in terms, but maintains that it is only an oxymoron, i.e. a figure of speech, and that the concept itself is a useful one. Systematic luck differs from power (since it gets you what you want without trying) as well as from mere luck (since 'it attaches to certain locations within the institutional and social structure', p. 137).

Ideologies are regarded as cost-saving belief systems. They help people to evaluate their interests, and may thus be counted as a source of luck. But ideologies also contribute to power, since they serve as an informational power resource.

In the final chapter 8, Dowding again underlines the need for conceptual clarity. An important argument against efforts like his, is that power is 'essentially contested'. Connolly and Lukes, among others, have argued that every effort to define 'power' empirically, is obstructed by the normative load of any definition of power. Dowding holds this view to be unnecessarily anti-positivist, and restates Popper's claim that for an empirical scientific debate hypotheses should be verifiable or falsifiable *in principle*. 'Non-neutrality does not entail any lack of objectivity' (p. 173).

As I said before, Dowding's argument is complicated. It is sustained by an unremitting exchange with authors as divergent as Woody Allen and Ludwig Wittgenstein. Dowding could perhaps have written another book, in which his central points (the distinction between power, luck, and systematic luck, and its consequences for the interpretation of power studies) could have been made in a much simpler fashion. But he did not, and so his readers will have to bear with him. However, I found the exercise on the whole rewarding. The book is generally well-written. Another of its good qualities is, that in his effort to solve conceptual problems, Dowding connects concepts which sometimes have very different traditions. Power, luck, bargaining, interests, collective action, pluralism, elitism, marxism are brought together in a framework of rational choice. I think that Dowding has done everything one could expect from a 'conceptual exercise'.

This does not imply that I agree with all of his points. For reasons of space, I shall confine myself to two central problems: the 'essential contestability' of power and interests, and the 'oxymoron' of systematic luck.

Dowding maintains that it is possible to reach general agreement on empirical definitions of interests and power: '... if we can understand what would show the truth of one theory and the falsity of another, then we do not have essential contest-

ability of concepts. We have contestability in the actual world, but we do not have contestability in all possible worlds; for in some of them we can see which theory is true and which false ... A realist may well accept that there is no fixed point by which to judge all moralities, but does not accept that they cannot therefore be judged at all' (p. 171-172).

In my opinion, Dowding here misjudges the claims of 'essential contestability'. Concepts such as power and interests are essentially contested, or 'irreducibly evaluative' (Lukes) because every empirical application of them is based on an outsider's normative judgement. For example, collective action models assume that a certain group of persons has a common interest, which should serve as a normative guideline for its behaviour. Problems of contestability arise when we try to put such a model to an empirical test. The test may succeed or fail, but we will never know whether its success or failure is due to the research design (the researcher's decision to investigate this particular interest for that particular group) or to the quality of the model itself (the prediction that, given an interest in *a*, an actor will under specified circumstances take action *b*). Persons may have interests, or be subject to power relationships, *without themselves being aware of them*, which obstructs direct empirical research into these concepts. After the test, we are thus left with the researcher's normative assumptions, and some empirical knowledge which may or may not support these assumptions, but can never (verify or) refute them. In terms of Carnap's logic, we have a definiens (group interest, circumstances) and a consequent (collective action), and we know that our theoretical prediction about their interrelationship is false only when the definiens is true, and the consequent is false. But we have no way of finding out *whether* the definiens is true or false. Therefore, concepts like power and interests are by their nature contestable, and consequently cannot be judged decisively against empirical evidence.

My other point of criticism concerns 'systematic luck'. Dowding introduces systematic luck to describe the phenomenon that some social groups get what they want without trying, as a result of the structure of society. A society organized around free enterprise, will generally favour business organizations. Dowding is determined to show the usefulness of the concept of systematic luck, since according to him it clearly differs from more personal forms of luck (p. 64-65). However, I wonder whether there is any difference between systematic luck and influence through anticipation. What Dowding describes as systematic luck, conforms by and large to Friedrich's 'rule of anticipated reactions' (in *Man and His Government*). Power, not luck is involved here. The antithesis between power and luck has been known since the Renaissance, and systematic luck is in my opinion closer to *virtù* than to *fortuna*. By insisting on the term 'systematic luck', Dowding seems to immunize many power relations from the theoretical problems associated with tacit influence.

Notwithstanding these and other comments, Dowding's book is worth reading for those who are involved in power or collective action research. It does not answer all the questions one could ask, but it does offer an interesting perspective on some of the central problems of empirical political science.

C.W.A.M. Aarts

Summaries

The level of political knowledge of the Dutch citizen

Karin Wittebrood

Political scientists often point at differences in the level of political knowledge of citizens as an explanatory variable of political behaviour, as do theorists of mass belief systems. Nevertheless, they hardly pay attention to explaining this factor itself. In this article, a theoretical model has been constructed to explain differences in citizens' political knowledge. The model assumes that political interest is a motivating factor in obtaining political knowledge by use of the media. Interest refers to both advantage and profit, and curiosity. Furthermore, it is assumed that a higher level of political knowledge leads to a better perception of political advantage and profit, or that it increases political curiosity. Thus, there should be an interaction between knowledge and interest. The results of the research presented show, however, that this model does not hold. In the first place, political knowledge is obtained not only by use of the media, but also in other ways. In the second place, the perception of political advantage and profit can hardly explain the level of political knowledge. The results also show, however, that the way in which the concept 'political knowledge' is measured deserves more attention in political science.

Civil Society and the Revolutions of 1989. The Study of Eastern Europe in Search of a New Paradigm

Tom Casier

The article tries to provide a critical analysis of the civil society paradigm as a dominant model in the explanation of the revolutions in Central and Eastern Europe in 1989. The civil society paradigm replaced former models (such as the totalitarian, group and corporatist approaches), which proved insufficient to explain these changes as they underestimated the seriousness of social developments. Con-