

## Young Man in a Hurry: Recollections of a Soldier of Fortune\*

Mogens N. Pedersen, Odense University

Hamlet: Denmark's a prison.  
Rosencrantz: Then is the world one.  
Hamlet: A goodly one; in which there  
are many confines, wards, and  
dungeons, Denmark being one  
o' the worst.  
Rosencrantz: We think not so, my lord.  
Hamlet: Why, then, 'tis none to you; for  
there is nothing either good or  
bad, but thinking makes it so:  
to me it is a prison.  
Rosencrantz: Why, then your ambition  
makes it one. . . . (*HAMLET*, Act 2, Scene 2)

Graduating from Aarhus University in the summer of 1964, I happen to be the first professionally trained political scientist in Denmark. Well, it may be more to the point to say that I was the first young Dane to receive a political science degree from a Danish university.<sup>1</sup>

This fortunate position provides me with a unique vantage point. My memory is longer than the memory of most of my Danish colleagues. Since some of the early witnesses to the errors of my youth are no longer able to protest, and since colleagues, friends as well as foes, will probably shrug their shoulders anyway, it is within my powers to put myself into the picture in a most flattering way. This I say frankly, in order not to conceal that I harbour somewhat ambiguous feelings with respect to the genre of "Intellectual Autobiography" in general – and my own practice of it in particular. Being familiar with the stylistic technique of "confessions" (e.g. Rousseau 1781) I am fully aware of the great opportunity that I have to look better, nicer, and more successful than warranted.

\*Revised version of a paper presented at the ECPR Joint Sessions of Workshops, Leiden University 2-8 April 1993. Workshop on "The Intellectual Autobiography of Comparative European Politics".

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Nevertheless, I have decided to write a short account of some crucial years in my own career, roughly the time between my Freshman year at Aarhus University in 1958 and the year of 1970, when I went off to study and write at Stanford University.

The reason for concentrating on this short period is twofold. The first – and unintended one – is the discovery to my chagrin that many of the ideas, many of the recurrent themes, and many of the weaknesses of my later work were present already at this early stage, at least in embryonic form. This discovery provided an excuse to abandon further advancement into the autobiographical quagmire. Second, by selecting this period the dominant perspective becomes that of the interplay between an emergent Danish political science and a student and young scholar. Those who do not care about the personal part may find it worthwhile to read about “How Comparative Politics Came to Denmark”.

Fortunately – or unfortunately, as you like – this span of years has already been charted by other scholars, who have told the story of the emergent Danish political profession. The departments of political science at the Universities of Aarhus and Copenhagen have published their own versions of the history of the first years, certainly not without putting *their* emphasis on the story (Rasmussen 1978; Karup Pedersen 1980). Various attempts have even been made to present bits of the history to an English-speaking audience (Henningsen & Rasmussen 1966; Nannestad 1977; Rasmussen 1985). A major, unpublished, autobiographical – account by my late teacher and mentor Erik Rasmussen is also available (Rasmussen 1991). What I can add is only another account with a slightly egocentric bias. So much for warnings.

## The Pre-History

I entered Aarhus University in September 1958, intent on majoring in chemistry. Two weeks and an existential crisis later I enrolled as one of the first students in a political science programme that did not exist as a physical entity with teachers and facilities. It only existed as a freshly signed Royal Decree.

Literally, there was no such thing as a Danish political science in 1958.

The Danes cannot boast of a century-old tradition for studying “Rhetoric and Politics”, “Politices et Eloquentiae”. The man who gave his name to the recently created and coveted “Johan Skytte Prize” unfortunately did not donate a chair to the University of Copenhagen – as a matter of fact he instead participated in dictating the peace conditions to the Danish King in Brömsebro in 1645 (Lewin 1985). There had, of course, always been

scholars who engaged in studies related to this field, but there was no continuity and no institution-building.<sup>2</sup>

A professor of law at Copenhagen University, Sven Clausen, nowadays mostly remembered for his contributions as a playwright, had written a couple of compendia on the history of political theory. He published an *Outline of Political Science*, in which he diagnosed the situation in clear terms:

political science (never became) a traditional theme for thinking and scholarship by us, and most people do not even know what the term stands for. The plain fact is that political science in Denmark has to fulfil a demand that it first of all has to create (Clausen 1956, 11).

In saying so he also echoed the characterization that had been given by the Swedish scholar Elis Håstad in the early UNESCO survey (UNESCO 1950, 150).

When political science was introduced at last, it was not primarily in order to create a new research discipline. The major objective was to train a new type of civil servant, a genuine generalist, who would combine law, economics, politics and other social science disciplines. The name of the new academic programme was *Statskundskab*, a name that rings a bell in Scandinavia – and with those who know the German *Staatslehre*. How this new degree programme and the related research might develop was anyone's guess. It could have turned into an abortive and by now long-forgotten attempt. It did not. Why not? Probably for three reasons. First, a few fine scholars from other disciplines, but with an intent to nurture political science in Denmark, were recruited. Second, they were met by a small group of bright, motivated students. Third, this happened at the right time: the students were desperate to get the act going – and the political as well as the academic climate was optimal. The early history of political science in Denmark should be studied as a case that would support the fashionable “Garbage Can Theory” of decision-making (March & Olsen 1976). I am, however, in the following leaning more to the cruder theories of ambition (e.g. Schlesinger 1966).

## Students and Professors at the Frontier

My freshman year was spent digesting a heavy dose of public law and introductory economics – plus some philosophy. The small group of foolhardy students had to wait a year before the rather ineffective Danish university system had selected the first two professors of political science. In the meantime the professor of international law kept the eager youngsters “warm” by conducting a weekly seminar, in which an American introductory textbook was discussed.

Let us take a brief look at the two “Founding Fathers” – Poul Meyer and Erik Rasmussen. Their efforts were decisive and the effects were long-lasting, also in the sense of forming young minds. Even though their early students may have succeeded in cutting the umbilical cord, it would be wrong not to admit that their influence was pervasive on the first generations of political scientists in Denmark. Certainly on me.

Poul Meyer was a legal historian and constitutional lawyer, who had moved from dogmatic jurisprudence towards public administration – and who had also a background in journalism, civil service, the resistance movement – and – before 1940 – various right wing organizations. His book on *Administrative Organization* (1957) was acknowledged as a pioneering contribution. It was also a genuinely comparative book. The author did, however, retain some of the habits of the legal scholar. Concepts and conceptual analysis were for him the cornerstones of all scientific activity, and God forgive the poor student who was sloppy in this respect. Or in any other respect for that matter. His tongue was sharp. So was his pen. He sometimes found it difficult to keep his temper under control.

His companion – or counterweight – Erik Rasmussen, was trained as historian and humanist. He had also drifted a bit away from his field by taking up a more systematic study of cabinet government and the principle of parliamentarism in Denmark – along the lines laid out by Swedish political scientists. From him one could among other things learn the principles of sound scholarship – but also some academic tolerance plus bits of the art of political manipulation and negotiation that come from being active in Danish organization life, where consensus values are held in esteem.

It may sound a bit highflown, but it is nevertheless true that these two very different individuals and, dare one say, “outsiders” for a number of years held the fate of Danish political science in their hands. They could easily have given the development an unfortunate turn. They might have created the same kind of havoc that soon turned the equally young and experimental department of sociology in Copenhagen into an ideological battlefield and, subsequently, for years a smoking heap of ruins.

The truth is that one of them – if left alone – might have moved the infant Danish study of politics in a dogmatic direction and thus derailed it from international mainstream political science. It is also true that their relationship was not always happy. As a matter of fact Erik Rasmussen many years later spoke about “my marriage to Poul Meyer” (Rasmussen 1991). But since they were not only very different, but also scholars who had invested in a future in and for political science, they happened to keep each other at bay and at the same time keep the field moving.<sup>3</sup> First and foremost they delivered to Danish society the promised new type of civil servants – and a considerable number of politicians as well. Thirty years later they could look back at an unqualified success. Can one reach higher than to have

an article written in a leading Danish newspaper, in which the careers of the first hundred Danish political scientists were meticulously mapped, and in which the conclusion was that this department at Aarhus University had become the “hatching-place of the Danish power elite”? (*Jyllandsposten*, 26 December, 1988).

What made this experiment become also an academic success story is not as easy to tell. Perhaps the simplest way to tell the story, and in the process also to provide a glimpse of the early socialization pattern, is by emphasizing the term *eclecticism*. Constructive eclecticism was the hallmark of the new Danish political science, especially as espoused by Erik Rasmussen (Rasmussen 1968–69, cf. also Young 1968, 101ff.). Fortunately, neither of the new professors tried to invent the wheel once more. They borrowed and imported instead. Both of them were familiar with the old Swedish tradition as well as the new Norwegian political science, but they did not consider either of these ways a completely satisfactory model for a Danish political science. Neither would shun French or German scholarship.<sup>4</sup> It was, however, the vivid Anglo-Saxon tradition that attracted them the most. Their textbooks in Danish – of which they produced quite a few – introduced the students to the best of international scholarship, be it Scandinavian, European, or American (cf. Meyer 1959, 1962, 1965; Rasmussen 1968–69), and their students had to read the classics as well as the *gurus* of the day, Friedrich, Truman, Dahl, Almond, Easton, Lasswell, Simon, even the Norwegians Rokkan and Valen.

It was of crucial importance that political science was introduced in a coherent and systematic way, starting with the very basics. At a later stage in the development it became fashionable to snigger at the disproportionate amount of time spent in defining the concept of *politics*. It was slightly reminiscent of scholastic discussions in a medieval monastery. On the other hand, it was probably a necessary step for an emergent academic discipline which had to carve its niche, lay out and defend its boundaries. David Easton was the hero of the day – at least in this respect.<sup>5</sup>

The search for identity also consisted of a search for the proper normative position. The students early on became familiar with the important work of Gunnar Myrdal. In Denmark three great scholars of international renown, namely economist Frederik Zeuthen, legal philosopher Alf Ross and sociologist Theodor Geiger had discussed the value problem for years. First-year political science students had to master this discussion as well, even if it was difficult. Later on the reading list included Arnold Brecht’s monumental compendium on the foundations of 20th-century political thought (Brecht 1959) as well as the discussion generated by Leo Strauss, Voegelin and others. Max Weber and Karl Popper were also very much on the agenda. The “Logical Gulf Between Is and Ought” was thoroughly discussed, even to the point of indoctrination. I still do not see a valid

alternative to the carefully reasoned scientific value relativism of Arnold Brecht (cf. Pedersen 1990). It definitely was invaluable as a defence line during the skirmishes with the Marxists in the 1970s.

The third problem that was discussed at length was the great controversy between those who believed in “grand theory” and those who could at most envisage partial, “middle-range theory”. “Mainstream” Danish political science adhered to the latter. In these early days even the socialists could follow C. Wright Mills in his exorcism of “grand theory”. What else could a bunch of novices do?

The story has been told in such a way that the “frontier”-atmosphere is emphasized. Perhaps professors and students did not always get it right, when they immersed themselves in a literature that no one in Denmark had studied before. But they enjoyed studying together in a highly stimulating atmosphere – which unfortunately soon suffered, when students flooded the university around the mid-1960s. The spirit of the frontier disappeared for good during the tumults in 1968–69 (cf. Pedersen 1982, 1984).

## Teaching at the Frontier

The reader may ask, was it all so rosy? Of course not. One could probably have had a better political science education in Bergen, Norway. But not much better, as far as I know. Below the quiet surface there was, however, a current. It had a lot to do with the contrasting personalities of the two founding professors. Perhaps it also had a bit to do with my own youthfulness and lack of tolerance.

The inherent structural problems only became visible – but quickly started nagging – when upon graduation I was immediately hired as assistant (*amanuensis*) to Poul Meyer, i.e. a university position situated between apprentice and slave (cf. Pedersen 1984). Now it dawned upon me that it was a mixed blessing to have studied with two professors who were as different as night and day. Could one – and should one – choose between the primarily legalistic and conceptual, not to say highly personal, approach of Poul Meyer and the basically historical, somewhat more empirical and much more eclectic approach of Erik Rasmussen? What about other ways of practising political science? It is to these difficult questions that I turn now. It took me 3–4 years to sort this out – and choose a side. It was not an easy decision, the more difficult, since it also involved a shift in personal allegiance – an aspect that is only mentioned here in passing.<sup>6</sup>

One might say that the first couple of years of teaching and research also were a kind of graduate school. There was no school in physical terms, and certainly not many “graduate” students around. The professors took some interest in one’s research ideas, but there was no pressure, sometimes

perhaps even a bit of what one could call benign indifference – like the atmosphere of many a real graduate school. My immediate boss, Poul Meyer, needed me primarily for teaching ever bigger classes of introductory political science according to his own ideas, which were laid out in a small, and somewhat idiosyncratic, textbook (Meyer 1962 cf. Pedersen 1984).

This professor had some clear, not to say indisputable ideas about academic behaviour. One of them came out in the open, when one day I asked him, how he would behave if a student asked him a smart question that he was not able to answer himself immediately? His answer was that one could as a respectable university teacher not afford to end up in a corner like that more than, say, once every second month. In another blast of his temper I was told in no uncertain terms to lecture according to his understanding of the basic concepts, even if these – in my opinion – represented a complete misreading of the primary text.

These were the conditions. I still admire this great scholar, but I have to admit that he was not an easy master to serve. Little wonder that I had to create my own “graduate” programme. Since this damned little textbook was based upon a very broad reading, these studies had to include important books from all the social sciences. I still remember immersing myself in the analysis of social norms (Newcomb, Rommetveit, Parsons); groups (Bentley, Truman); power (Lasswell & Kaplan); behavioural models (Simon, Shubik, Lipset, and so on); values (Myrdal, Ross, Zeuthen, Weber); philosophical/conceptual analysis (Ross, Hart, Ayer, Hartnack); ethics (Toulmin, Lögstrup, Hare, Nowell-Smith) to say nothing of a lot of semantic theory (Hayakawa, Ogden, etc.). After having read and digested all this and more, and made plenty of laborious excerpts, one was able to answer all sorts of simple-minded questions from students, who were only a few years younger. In the long run the investment may have paid off – although one sometimes has doubts. . . .

So much about the teaching side and what is normally called general education. I still wonder if one could have had a better, not to say tougher training in one of the “real” graduate schools?

## Young Man in a Hurry: Where to Begin?

What was it like to start doing research in those days? The next – unabashed egocentric – pages will deal with this aspect. Since the teaching load was rather light at the time, the question was not as “academic” as it has turned out to be in more mature years.

Let us suppose for the sake of argument that a research career is based upon rational deliberations and decisions – and is not just a messy product of “garbage-can” processes.



What is rational behaviour for a young scholar-to-be, who has been educated in the kind of milieu just described? That is: on the very “frontier” of the national discipline – confronted with a vast Virgin Land, but with neither funds nor other resources available?

If the young man is either an absolutely brilliant scholar or exactly the opposite he might consider following in the footsteps of his “own” professors. The reader may himself produce the argument for such behaviour. The dangers inherent in this strategy are, however, obvious, especially in the old times of omnipotent professors. Anyone, who is familiar with the sociology of apprenticeship, will know the structural problems.

So: If the youngster is not so sure about his own intellectual powers it is probably advisable to try to carve out a niche for himself, somewhat removed from the primary interests of his elders, but still within communication distance. If, at the same time, he also tries to build up, what the negotiation theorists call a BATNA (Fisher & Ury 1981), so much the better.

This may sound a bit cynical, as if one’s research is primarily a function of naked ambition. I would never defend a statement to that effect. But I have to admit that this sociological model fits my own situation much better than models that are based entirely on the nice idea that scholars are searching for The Scientific Truth from the very first day of their career. The crux of the matter is that one’s academic career depends upon a lot more than one’s bright and shining intellect. It certainly also depends a lot upon timing; idiosyncrasies; whom you meet and how you meet them; and there are also the unexpected luck and mishaps. . . .

My early career contained it all, but I have of course tried to “dress it up”, as if I learned something all along the way. Scholars “often take down the scaffolding after putting up their intellectual buildings, leaving little trace of agony and uncertainty of construction” (King et al. 1994, 13). I am no exception to this rule.

My first choice was to decide upon a field of specialization. As will be clear in a moment that decision may have contained an element of the unconscious: it happened over an extended period, at the end of which I had already invested a lot of time and energy studying parliaments and literature on this institution which was supposedly uninteresting and in decline.

The second step – much more consequential – consisted of picking the behavioural approach as the preferred “paradigm”. Although I had never heard of Thomas Kuhn, I may have grasped his notion. The decision was made with my eyes wide open, in fact it was a quite deliberate act. Many of the books and articles that guided me into this new world had one name on the title page: Heinz Eulau (e.g. Eulau et al. 1956; Wahlke & Eulau 1959; Wahlke et al. 1962; Eulau 1963; plus many of the articles later reprinted in Eulau 1969). I did not know this gentleman at the time, but I certainly

admired his work for its elegant empirical analyses as well as its erudition. It did not escape me that this American apparently was also a smart academic entrepreneur and a powerful figure in the profession.

In a short time I decided that the behavioural persuasion was going to be my epistemological platform, not in its vulgar “behaviouristic” form, but as a framework that would allow me to study political institutions in a broader perspective than I had been brought up with, and without having to move too far away from the Danish professorial “mainstream”. I even defended my platform in public (Pedersen 1972b).

The third important decision that I am conscious of having made consisted in deciding to specialize in using primarily a certain type of data: “process-produced”, “unobtrusive”, and “ecological” data. Again, one can with hindsight see that this was a natural, perhaps even necessary – and therefore rational – choice, given the place and the times. At the time no surveys had yet been conducted in Denmark; without a strong organization and a department capable of backing up a major research project, one simply had to opt for “cheap” data. Public research money was also scarce at this juncture. A good training in historical methodology and the conspicuous absence of training in “quantitative” methods also supported this preference for “process-produced” electoral statistics, roll-call data and other remnants from the legislative process, biographical information, etc. It stuck. It is only in recent years that survey data have become part of my daily life.

## Some Crucial and Traumatic Experiences

Looking back it is easy to identify subjective experiences that were of importance, because they prompted the above-mentioned decisions and formed my thinking. If you are burdened with a reflective mind in which *pros* and *cons* are being balanced all the time, it is not difficult to remember these kinds of experience, especially if they are also of a traumatic nature.

The first such occasion was connected with my thesis for the *cand. scient. pol.* degree. I had been working for a long time and intensively with a project on the internal border disputes in the Danish trade union movement. Since I wanted to master the details, I had even narrowed down the work to the crucial period between ca. 1890 and 1920. Archives had been rummaged. Data from every damned trade union – almost a hundred of them – had been collected and interpreted. This was before the invention of xeroxing and other technical facilities. The research protocols were tomes of learning and piles of garbage.

My professor – the one who was a historian by training – had advised and urged me to delve into the archives. He had also plainly told me that he was not himself an expert on the history of the Labour movement. I could not

care less. I was not afraid of hard work, and since I was also – at the time – a young and hot-headed socialist, who knew *my* movement by heart, it was at least in the beginning a great learning experience. After a while I did, however, become uneasy. I had some vague ideas about how to generalize my findings, but I could not find any literature that could give me the crucial clue. An otherwise very fine discussion of the Danish Labour movement by the American economist Walter Galenson (1952) was almost the only guidance at hand. I did not find Jörgen Westerståhl's dissertation (1945) very helpful. In the end I could not see the wood for myriads of trees. . . .

All this work was done two years before Mancur Olson's seminal book on the *Logic of Collective Action* (1965) and eight years before Albert Hirschman published his analysis of *Exit, Voice, and Loyalty* (1970). Neither had I heard about Thomas Schelling's important work on bargaining theory which had just appeared (1960). Too bad those books were not there!

I mention this because *Nadir* was reached, when the thesis was turned in for grading. The grade was high, but not the top one that an ambitious student hopes for. Apparently – so I was told at least – the *censor*, i.e. the external examiner, had not found the work to be a genuine political science thesis, but “just” a piece of work that might as well have been written by an economist or a historian. No top grade for this!

What did I learn from this?

First of all, I became even more confused about the proper character of political science within the family of social sciences. More importantly, I decided to move on to new challenges and forget entirely about the study of the Labour movement.<sup>7</sup> Another lesson was that it is a great challenge to cross disciplinary borders, but it is not only fun and easy. It is also a risky business. Thus I learned the hard way that one should never dare such crossings without bringing along some theoretical notions from political science – or at the very least a smart idea.

The second traumatic experience happened a few months after I had been appointed to the first position at Aarhus University. My very first publication dealt with the political mobilization of Danish women. It was just a short article, done *con amore*, in which were presented some time series of participation indices – with some discussions added to these new facts. It was published in Danish in a relatively obscure journal (Pedersen 1965), and the young author was infinitely pleased when the Association of Danish Women asked for permission to reprint it in a journal mostly read by middle-aged bourgeois ladies. The rest was silence. A few years later, when the feminists took over gender research, it was of course completely forgotten. If this piece had only been written a couple of years later, the author might have become a guru – or might have been given harsh treatment as a Male Chauvinist. . . .

More importantly: there is really nothing so depressing for a young scholar as lack of response. Especially if you sit in the middle of nowhere. The moral is that timing as well as channel of publication is of vital importance, if one is keen to reach an audience. “Marketing” efforts may help somewhat, but sheer luck and lots of patience may also be of importance.<sup>8</sup>

A third traumatic experience occurred when my first “real” article – on preferential voting – had been accepted for publication in the very first volume of *Scandinavian Political Studies*. It had been conceived in Danish. To have it translated into English by a semi-professional translator was a tortuous process. To have it ruined by a Finnish printing company and only salvaged by chance in the final, third proofs was another memorable experience. I vowed that I would write in English in the future even if it were to cost a lot in terms of time and effort. As a matter of fact, this was one of the better decisions I have made in my professional life. With usual lack of foresight I also decided that I would never ever trust an editor.

### *To Be or Not to Be – That is the Question*

I cannot escape it, even if most of the time I succeed in forgetting everything about it, especially since I destroyed the incriminating piles of data some years ago, when I moved to a new office. We now have to talk about the most traumatic experience in my years as a young scholar.

In an early description of Danish political science the reader may find a note in which two leading Danish professors were informing the English-speaking world that “Mogens N. Pedersen is undertaking an analysis of the question hours of the Danish Folketing. His aim is to analyse the development of parliamentary questioning since its introduction in 1947, and to examine its function as a species of political control under the changing conditions of Parliament-Cabinet relations.” (Henningsen & Rasmussen 1966, 256).

This was an example of how the Danish professors at the time had to include even “ongoing research” in their reporting in order to make an impression on the outside world – in the process leaving in the annals an unintended trace of an abortive project, long forgotten.

The fact is that my first major independent attempt to do serious empirical research consisted in coding – by hand and all by myself – all parliamentary questions in the Danish *Folketing*, since this procedure had been introduced in 1947. After a modest beginning the number of these questions started to grow, and in the mid-1960s the annual number was approx. 200. This exciting development called for an explanation, and the relatively limited amount of data made it ideal for empirical analysis performed by an individual scholar. Every question was coded according to an elaborate

scheme, and a huge pile of coding sheets was accumulating, only waiting to be punched and analysed. And there it stopped!

Why did nothing come of all this labour? Apart from a number of explanations that are not flattering for the ego, the following come to mind:

To begin with, other, more exciting research options lured me away. But it also should be taken into account that in Denmark it was difficult at that time to get the kind of professional advice that might have salvaged the project, when it went sour. And advice was necessary, since it gradually dawned upon the young scholar, first, that the project was perhaps theoretically as well as empirically unsound, and, second, that he himself needed training in a great number of difficult methodological subjects.

The international literature on parliamentary questions was small, traditionalist, and not very helpful (e.g. Chester & Bowring 1962; Ameller 1964). The tricky problem that I could not solve in a valid way was how to code the “uses” or “functions” of the parliamentary questions. The reading of Robert Merton’s definitive analysis of the ambiguities of functional vocabulary in the social sciences founded a still lingering distrust in the “functionalist” language, at least in its more vulgar appearances (cf. Pedersen 1993). The problem also became increasingly difficult the more I understood the language of politicians. The strategic use of this language in connection with posing questions and with the evasion of questions was far too complex and sophisticated to be treated by means of simple-minded punch-card-oriented coding. It is with some pleasure that I have recently noticed that the methodological problems that almost destroyed me back in 1965 still baffle not only thoughtful students of parliaments (Wiberg 1994) but social psychologists and linguistic scholars as well (cf. Wilson 1990, 131–78).

Equally important was the painful discovery that the formal political science curriculum had not equipped the novice with an adequate methodological and statistical training, especially not if one drifted towards empirical work in the behavioural fashion. The Aarhus curriculum included traditional historical methodology plus some passive reading of a Swedish textbook on sociological methods (Karlsson 1961). It was of vital importance to read up not only content analysis, but also the most elementary and some not so elementary statistics and general methodology. Soon one was into finite mathematics, game theory, econometrics, factor analysis, survey analysis techniques – and what later proved to be important – some, then in Europe, not well-known techniques for legislative roll-call analysis.

Fortunately this disaster and waste of time and energy happened in a distant past, when academic pressure was tolerable, and no one expected results here and now. Had it happened today, I would have become a dropout.

### *Metodenstreit – The Danish Way*

A final experience, somewhat happier, but still a mixed one, was a bitter *Metodenstreit* that took place in 1967. At the time a fragile tradition for ecological voting studies was developing along the lines laid out by Herbert Tingsten three decades earlier (Tingsten 1937). Poul Meyer (Jeppesen & Meyer 1964), as well as my slightly older colleagues Jan Stehouwer and Ole Borre (e.g. Stehouwer 1967; Borre & Stehouwer 1968) were breaking new ground. We were all amazed to see the wonders that could be produced by means of, first, a counter-sorter machine, and, later on, a main-frame computer. Not the university computer, which did not exist at the time, but a commercial one, the services of which could be hired for a fee to produce huge piles of endless printout.

At this point a knowledgeable political journalist entered our circles. In an ambitious book he professed to understand the phenomenon of “floating voters” in Denmark as well as in the neighbouring countries (Lassen 1967). He made quite a number of interesting points, but his knowledge was not entirely up to the high standards of the new and great political science profession. His results were not entirely “intersubjectively transmissible” as demanded by the great hero Arnold Brecht (1959, 113ff.) and, *eo ipso*, he was not living up to the demanding requirements of the Scientific Method.

The junior member of the department of political science had recruited himself to do – or was prompted to do – the necessary job of the hatchet-man. The unfortunate book was split into atoms and its author left bleeding (Pedersen 1967b). The notion of the ecological fallacy was spelled out in minute details for everyone to understand and avoid, and so devastating was the criticism that a historian some fifteen years later would claim that I had paralysed ecological analysis in Denmark (Noack 1981). Fortunately it has recovered since then.

The lasting effect of this not so pleasant skirmish on the mind of the hatchet-man himself was a continuing interest in – phrased negatively – the underlying and widespread problem of logical fallacies in the social sciences, or – phrased positively – the great challenge of making inferences across analytical levels – especially in those parts of political science, where data are not clean and nice. C. Wright Mills, Heinz Eulau, Erwin Scheuch, Stein Rokkan, and Johan Galtung were the ones from whom one could learn a lot about these epistemological problems. The “level-of-analysis” problems and the “micro-macro” problems are still among the most intriguing and exciting, and I have grappled with them ever since, in studies of political recruitment (e.g. Pedersen 1976, 1977a) as well as in discussions of electoral volatility (e.g. Pedersen 1979, 1980, 1983), and, especially, the linkages between the electorate, the party system, and the mechanisms of elite transformation (e.g. Pedersen 1994).

In retrospect I have called all these early experiences traumatic. Is it too bombastic? Was I just naive? Before the reader makes his judgement, he should consider the times and the basic fact that a Danish university in the 1960s was not at all like an American graduate school. It was more like an American frontier in the 19th century. Fortunately, one was allowed to learn by trial and error – and was not shot at dawn.

## Denmark as a Small Democracy: Joining a New Network

By far the most decisive influence on the Young Man in A Hurry was the encounter with Stein Rokkan and his international friends. A great deal has already been written about this towering figure, about his intellect and about his generosity. I am unable to add much. He just “discovered” the young Dane, and suddenly everything was different.

The background was, probably, that the article on preferential voting (Pedersen 1966) had been noticed by the voracious Norwegian reader. For the *First Nordic Congress of Political Science* in Oslo a more audacious paper had been concocted. The title was “Consensus and Conflict in the Danish Folketing 1945–65”. It contained an analysis of all divisions and roll-calls in the Danish parliament over this long period. The analysis was done entirely by hand. The labour had been considerable, a fact which the reader would not notice since the analysis was parsimonious, not to say simplistic. The young scholar was, however, quite satisfied with his job. *En route* he had learned a lot about legislative behaviour methodology and about basic statistics, and he had read everything from Stuart Rice’s classical book (1928) to the most recent scaling exercises. He had also invented his first descriptive gimmick, an “index of distance” – a measure that was later on used frequently by Danish scholars and also used and quoted widely for some time outside Denmark.<sup>9</sup>

Stein Rokkan just came up to the blushing youngster and talked to him, as if they had known each other for years. It is well known that Stein was a modest man. He knew his worth in the scholarly world, but he did not talk much about it. This mixture of modesty and self-assuredness was fascinating from the very first moment.

One of the first things one would be impressed with was his tremendous breadth of knowledge. Stein would always tell you about the latest interesting book or article that he had read.<sup>10</sup> A minute later he would inquire, if you had yourself read – or thought of – “something”. You could drink freely of his fountain of wisdom, but you were also expected to inform his mind from time to time, sometimes at a very early breakfast meeting after a long Nordic night. He was not just a very personable Norwegian from the

northern periphery, but also a demanding international scholar, from whom certain professional standards could – and should – be learned.

All this I did not know at the time of our first encounter. I was just deeply fascinated, primarily because I instantly liked the man, whose articles I had read with admiration. But, perhaps, some deeper, “Freudian” forces were at play here as well. I felt sort of “adopted”. It may also have counted that Stein with his mixed background in history, philosophy, sociology and Scandinavian thinking in general might serve as a kind of mental “bridge” between my more traditionally oriented Danish mentors and the promised land of behavioural American political science. Who can tell?

This famous Norwegian professor – who, by the way, at the time of our first encounter had only just been given a full chair in his own country – took an interest in the young Dane, as said. In quick succession this meant being hired as a subcontractor, i.e. part-time research assistant, to the great and hopeless *Small European Democracies* project. There was still a little money left at the time, and Stein Rokkan as well as Hans Daalder needed basic data about this strange little country, Denmark. They gathered some facts and analyses on the complicated electoral system and I also did my first pilot study of recruitment phenomena in connection with this engagement (Pedersen 1968).

A meeting of the entire SED group in a pleasant hotel in the Hague in 1967 was a revelation and a boost. The core group, Robert A. Dahl, Stein Rokkan, Hans Daalder and Val Lorwin, created a friendly and warm symposium, and if there were status differences between the project leaders and the younger scholars, they were not easily observable, and they did not hinder communication at all.

In this group of people one could learn a lot, just by sitting in. As a matter of fact one had of course to contribute as well. The very first lesson had to do with what was called the “Zanzibar Ploy”. You could expect very tired faces in the group of senior scholars, if you objected to one of the many attempts at generalizing by suggesting that their Great Scheme did not work, because Zanzibar (read: Denmark) did not fit in. But if you suggested that some specific – but generalizable – characteristics (which you happened to know from back home) might be plausible and interesting to integrate into the scheme, then you could expect an instant response or at least tolerance. When Przeworski & Teune some years later (in 1970) made their important methodological point about treating particular social systems – like nations – as “residua of theoretical variables”, it certainly rang a bell. Another thing that I learned from the witty Val Lorwin was the power of words. When he had told the true story about when and how he invented – and marketed – a brand new term *Allgemeinkoalitionsfähigkeit* (Lorwin 1966, 178), one would for the rest of one’s life be aware of the magic power of terminology in a theory-poor social science.



For a marginal Dane the most intriguing observation was that the Danish political system did not play a role either as a “contributor” of theoretical variables, or as an object of study. It could in fact be argued that Denmark was not on the map of comparative politics. This may sound like a thesis manufactured for dubious and self-serving purposes. Therefore I will let the facts speak for themselves for a little while, before proceeding with the subjective narrative.

## Denmark on the Map of Comparative Politics ca. 1965

If we go back to early comparative scholarship such as the writings of James Bryce and Carl J. Friedrich, only fleeting and flimsy references to Danish politics can be found. Mostly the country was mentioned alongside with Norway and Sweden as if differences did not exist – or at least did not matter. With the illustrious exception of Maurice Duverger’s *Political Parties* (1952) it was only at the end of the 1950s that the proper name of this more than a thousand-year-old monarchy started to appear in the systematic literature.

Dankwart A. Rustow wrote a book about Sweden in 1955, and a year later he contributed a chapter on Scandinavian parties to the influential Neumann volume on *Modern Political Parties* (1956). In this he coined the catchy term “Working Multiparty Systems” about the Scandinavian party systems – a phrase that sticks (cf. Pedersen 1987). This was about all that existed in print about Danish politics in the English language before 1960, at least if one searched the notes and references of the early – American – comparative politics literature. The doubtful reader is referred to the excellent bibliography by Miller (1968).

What struck me so much, when I myself read into this mushrooming literature, was the relative lack of references to solid scholarship on a number of smaller European countries, certainly my own in particular. In some cases, for example the work by Gabriel Almond, one could readily suspect that the author knew very little about the Scandinavian political systems. The problem apparently was a simple one: since these scholars did not know the more esoteric European languages, and since the British have never cared much about the smaller countries on the so-called Continent, the emergent comparativists had to rely on newspaper-reading and whatever flimsy English-language documentation they could get hold of in the United States. The European scholars may have had a better “feel” for the European situation and heritage, but they were, with the exception of Stein Rokkan and a few electoral system specialists (e.g. Hermens, Lakeman & Lambert, Duverger), as ignorant as the Americans. In retrospect one becomes increasingly aware of the greatness of Herbert Tingsten’s early contributions.

Let me illustrate my point by taking a look at the documentation available to some of the major figures in the movement – as it appears in their own work – when they deal with Danish politics.

One of the most prolific writers in the early days was Seymour Martin Lipset whose *Political Man* (1960) was on the reading list also in Denmark. Lipset's primary source on Denmark was the already mentioned Walter Galenson, who, among other things, had summarized some thought-provoking hypotheses of the differential development patterns of Labour movements in Scandinavia, put forward by the Norwegian historian Edvard Bull. This theme had first appeared in the book – with Bendix – on *Social Mobility in Industrial Society* (1959), and it was replayed also in later books by the same author (e.g. Lipset 1963, 1968). Lipset's footnotes demonstrated considerable knowledge of the writings of Nordic scholars, and he would even quote an unpublished dissertation – in Danish – from the University of Copenhagen.

Gabriel Almond, whose “preliminary sorting operation” – as he has himself named his influential attempts at classifying political systems – started in the 1950s, without fuss placed Denmark and the other Scandinavian systems “somewhere in between the Continental pattern and the Anglo-Saxon” (Almond 1956). His knowledge of Scandinavian politics was not profound.

Robert A. Dahl was no different. He was very conscious of the fact that knowledge of smaller European nations rarely entered into comparative political analysis, as witnessed already in the *Political Oppositions* volume from 1966, an edited volume from which Denmark was conspicuously absent. In his later writings he sometimes hinted at the puzzling Danish political system, but he apparently had decided to let it stay with that.<sup>11</sup>

One could go on like this for some time. Harry Eckstein based his very general statements about Scandinavian politics on a relatively brief “field trip” to Norway (Eckstein 1966). Arend Lijphart, who did more than many others to come to grips theoretically with Denmark and the other Nordic countries, even as late as 1977 would rely on an unpublished American doctoral dissertation (McDaniel 1963), when he attempted to fit Danish politics into his well-known classification (Lijphart 1977, 111).

My point is that this little political system was gladly, grandly and without much documentation characterized as possessing a stabilizing “homogeneous and secularized culture” (Almond & Verba 1963). It was a “consensus system” (Eckstein 1966). Its party system was a “working” one (Rustow 1956) in which “centripetal” forces were at play (Sartori 1966). It also looked as if it was on its way to becoming a “depoliticized democracy” (Lijphart 1968).

## Denmark: An Interesting Case?

Was there a better role for the Young Man to carve out for himself being a marginal young scholar from a marginal country, than to become a subcontractor to this burgeoning movement, specializing in the “translation” of Danish politics into the general lingo of modern comparative politics? No. At the very time, when – we are now told – the aims and pretensions of comparative research were reduced from large-scale programmes to “something like putting national results into perspective” (Van Deth 1995, 445), it was a timely decision to specialize in this direction.

No sooner said, than done.

It is, however, easier said than done. One could end up doing the “Zanzibar-plot” for a lifetime. As an editor of an international journal I have encountered quite a few colleagues who ended up doing this sort of thing again and again. Fortunately, the “case-method” saved me – or, at least, provided peace of mind. This is how it worked:

The 1967 article on “Consensus and Conflict” (Pedersen 1967a) turned out to be a godsend. Not only did a Danish historical journal publish a revised and enlarged version, but the English version apparently was noticed by some of the greater minds within the exclusive legislative behaviour section of the political science profession, among them Samuel C. Patterson, John C. Wahlke – and Heinz Eulau. An exchange of information started, and suddenly came the *accolade*: an invitation to present a paper at the 1969 *Shambaugh Conference on Comparative Legislative Behavior Research* at the University of Iowa.

This time I had learned what to do. First of all to present something that would be interesting, and, second, not to go beyond the limits set by my modest skills in statistics.

If you go to the United States for an important meeting, where you are likely to meet the very top of the profession in your branch of enquiry, you had better do something that will interest the participants. They might know that there was some little nation called Denmark somewhere – but would they be as interested in Danish legislative phenomena as I had been in role perceptions in the state legislature of Ohio, when I read *The Legislative System?*

I decided to do something bordering on the foolhardy, namely addressing one of the classical problems in political recruitment theory, and by doing so also criticizing the work of one of the prominent scholars, Heinz Eulau himself. *In casu*, the theory of professional convergence as proposed by Eulau & Sprague (1961). This conference paper was titled “Lawyers in Politics: The Danish Folketing and United States Legislatures” (cf. Pedersen 1972a). As the title suggests, I was familiar with the problem of unduly “stretching” of concepts that Giovanni Sartori (1970) has warned against,

but thanks to Max Weber's seminal analysis of the political role of Juristen I also knew how to handle the conceptual problem (Weber 1920, 396–450).

Today, it is difficult to remember exactly the timing and how the various ideas became intermingled. Arend Lijphart's influential 1971 article was not at my disposal in 1968–69 – it was presented for the first time at an IPSA conference in late 1969. What later on became his 1968 article on political typologies was, however, in circulation, and it may have guided my first steps into the sociological literature on cases, especially deviant cases, as a methodological tool. The only thing about which I am absolutely certain is that the most important source for my own development in this field was a book which turned up all the time in Eulau's work: Cohen & Nagel's *An Introduction to Logic and Scientific Method* (1934). Its exposition of John Stuart Mills's methods of Agreement and Difference was most informative, when in the winter of 1968–69 I wrote my first truly comparative paper. The methodological possibilities and problems of case methodology have intrigued me ever since. Quite apart from an attempt to generalize the methodology of historical case-work (Pedersen 1977b), considerable parts of my work since then have belonged to the categories of interpretative or hypothesis-generating case studies. Another long-term effect could probably be traced in my efforts as editor of the *European Journal of Political Research* 1981–94. These efforts frequently consisted in convincing authors that they could – and should – reformulate their often a-theoretical one-country-focused research in terms of a theoretically interesting case study.

The Lawyer-in-Politics paper had a very favourable reception, even from the formidable and not always pleasant target of my attack. It resulted not only in publication, but also in an invitation to spend a year at Stanford with Heinz Eulau and Gabriel Almond. In the meantime, Stein Rokkan and Hans Daalder had been instrumental in acquiring a Ford Foundation Scholarship for me, and off I went into the wide world to learn more about comparative politics, about Danish politics, and – possibly – also about my own ego. . . .

I left behind in Denmark the *Institut for Statskundskab* which at the end of the 1960s had grown to the incredible size of four full professors, more than a dozen assistant and associate professors, and more than 500 students. The days of the Frontier were gone for good.

#### NOTES

1. In order to be absolutely honest, at least on the first page, I have to confess that in the very first “class of 1964”, which consisted of two students, a good friend passed his final exams 30 minutes before the author of these lines. But since Jørgen Flindt Pedersen left the academic world immediately for a distinguished career in journalism, I am permitted to project myself a little.
2. At the Academy of Sorø, which existed as a school for the young nobility during the 17th and 18th century, politics was taught to some extent in connection with rhetoric as

- well as ethics. A few Copenhagen scholars were also doing related work, the most prominent perhaps being the great historian and playwright Ludvig Holberg. The University of Copenhagen opened a still-existing *Statsvidenskab* programme in the mid-19th century. It did, however, quickly degenerate into economics.
3. About this topic I could say a lot more. A little has already been said in my contributions to *Dansk Biografisk Leksikon*; in an introduction that I wrote for Poul Meyer, *Nederlag* (1992), and in another little memoir (Pedersen 1984). Rasmussen (1991) is the most important source.
  4. Even in the mid-1960s Maurice Duverger was invited to do a guest lecture, and the students, who had been forced to read not only his great party book but also his somewhat duller tome on *Institutions Politiques et Droit Constitutionnel* (1960), found him difficult to follow. So did probably some of the professors present. A witty account of this as well as other episodes from early Danish political science can be found in Heltberg (1979).
  5. The first generation of students read his first book from 1953 and his important 1957 article. As his systems analysis emerged in larger and ever more pompous instalments during the 1960s, his fame grew, but the readership diminished. He still has a few staunch disciples in Denmark.
  6. For a full account of some internal “crises” in Danish political science during the late 1960s, the reader is referred to the unpublished manuscript by Erik Rasmussen (1991). Since these events were more important for me than for Comparative Politics in Europe, I have decided not to deal with them at length.
  7. Twenty-five years later I was for some reason taken back to the old data (Pedersen 1988). Now I finally discovered that a crucially important political decision made by one of the union movement leaders at the time could be interpreted nicely as a perfect case of the theoretical notion of a “tactical commitment” (cf. Thomas C. Schelling 1960). As will be clear from the following, the idea of thinking in terms of “cases” was something that was acquired later.
  8. Some years later, in 1974, I presented a paper at an ECPR workshop directed by Stein Rokkan. The paper was very well received. It was, in all modesty, also well executed, containing some bright ideas that might interest political scientists as well as political geographers. I even played around with some kind of rational choice thinking at a rather early stage in the development of that *malaise*. A leading US scholar – and old friend – had done a lot in order to make the reading easier for American scholars. This masterpiece was published in the *European Journal of Political Research* as the lead article in volume 3 (Pedersen 1975). I am still waiting to see if someone someday will notice its existence. Well, to be absolutely honest I have cajoled a colleague into mentioning this long-forgotten article in a review of Scandinavian political economy literature (Nannestad 1995). He called it “a single voice in the wilderness”. Not bad after all those years. . . .
  9. It was only somewhat later, that the proud young inventor discovered that his newly acquired friend Arend Lijphart had already some years earlier invented an almost similar index, – fortunately in connection with the analysis of roll-calls in the UN General Assembly, an even more esoteric arena than the Danish parliament (Lijphart 1963). We have here a perfect illustration of a fascinating problem in political science: the lack of cumulation (cf. Sjöblom 1977).
  10. Those who met him, when he had just read Albert Hirschman’s slim but brilliant *Exit, Voice and Loyalty*, will have a vivid memory of his approach to books and knowledge. Over the years many other great books were introduced to the Nordic social science public through his incessant and obsessive reading, most of which must have taken place at airports.
  11. I invite the reader to study a lengthy footnote on page 37 in Dahl (1970). Also, I cannot help repeating the great quote from *After the Revolution*: “Some of the most profound changes in the world take place in a quiet country like Denmark, where hardly anyone raises his voice and the rhetoric of revolution finds few admirers” (Dahl 1970, 4). It was fun to read, especially when one had to spend the 1970s in a Danish university, analysing the Glistrup movement and other fascinating topics.

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