

# ***The Case for Causal Mechanisms***

***Exploring the Viability of a Mechanism-centred Research Strategy  
for Explaining Short-term Changes in Public Opinion***

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## **Forord**

Oppgaven er skrevet på engelsk av hensyn til problemene med å oversette tekniske termer, spesielt dem som presenteres i del 2, hvis tema opprinnelig var tenkt å utgjøre en større del av oppgaven. Jeg later likevel ikke som jeg skriver for et engelsk publikum, og forutsetter at leseren har grunnleggende kjennskap til norsk politikk.

Del 3.2.1 er delvis en videreutvikling av et argument jeg fremførte i en semesteroppgave i kurs STV4308 våren 2005: "Betraktninger om å påvise årsaksmekanismer gjennom samfunnsvitenskapelige eksperimenter".

En stor takk til min veileder Raino Malnes, som har bidratt med mange verdifulle tips, påpekninger av uklarheter, foreslo en fornuftig arbeidsplan og satte tidsfrister i perioden april 2006 til innleveringen.

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Oppgaven består av 34.920 ord, alt inkludert.

*“If you want truly to understand something, try to change it.”*

– *Kurt Lewin, pioneer of social psychology*

## **1. Introduction**

Norwegian research on public opinion consisted for many years essentially of efforts to develop and apply comprehensive theories describing politically manifested social cleavages. Notable theorists included Stein Rokkan (e.g. 1987) (partly in collaboration with Seymour Lipset) and Henry Valen (e.g. 1992). These cleavages made intelligible long-term patterns emerging in statistical data of party alignments. Indeed, party preferences seemed largely determined by stable geographic and demographic factors. Today these factors have lost much of the explanatory force they were once credited with (Aardal and Berglund 2004: 302). Traditional party loyalties have diminished significantly, and consequently, party preferences have become more fluctuating. No longer can these fluctuations be traced back to structural societal changes. In my view, this development has not merely exposed weaknesses in the traditional approach; it has simply made more apparent what has always been a fundamental failure to explain any change in public opinion, given a strict definition of the term “explain”, which I shall provide in section 2 below.<sup>1</sup>

An explanation – I shall contend – is more than the intelligibility obtained from an adequate fit between theory and observation; explanations require accounts of the *causal mechanisms* that produce observed associations between states and events perceived to be connected as causes and effects; and we shall hold those accounts to capture the *real* causal processes responsible for producing the observable effects that we wish to explain. This view is called “explanatory realism” – or, more broadly, “scientific realism” (see Van Fraassen 1980). The task of justifying this view, I shall leave to men much abler than myself. In this paper, it shall remain a guiding philosophical idea, whose intuitiveness I shall humbly attempt to strengthen. My

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<sup>1</sup> Subsequent developments by, notably, Stefano Bartolini and Peter Mair (1992) may have gone some way to provide the mechanisms needed in order to make the models of Rokkan and Lipset truly explanatory, but they do not represent what I shall label a “mechanism-approach”.

argument proceeds on the intuitiveness of explanatory realism and the desirability of explanatory mechanisms to the effect that a fundamentally new approach to the scientific study public opinion is necessary in order to achieve such explanations and with them, I shall argue, an ever better understanding of how people's political preferences change.<sup>2</sup>

Attention to short-term fluctuations in the popularity of political parties has, admittedly, increased in recent years (notably Aardal et al. 2004), but research designs and methods have remained much the same: predominantly *correlational* (see section 3 below). In this paper I shall propose a shift from the testing of sweeping theories to more *interdisciplinary* and *problem driven* research (cf. Bennett 2003) and outline a strategy for obtaining mechanism-based explanations.

My argument proceeds in three stages. In section 2 I shall justify the search for mechanisms by recounting the case for the *causal mechanical theory of explanation*, as well as pointing to several senses in which we may justifiably expect mechanism-based explanations to be *epistemologically more satisfactory* than mere statistical inferences (some of which are further fleshed out in section 4). Secondly, in section 3, I shall examine a notable collection of Norwegian studies on public opinion, including a *quasi-experiment* with the stated aim of identifying causal mechanisms. I shall argue that the experiment fails in this objective because its approach is fundamentally *correlational*. Thirdly, in section 4, I shall outline in more detail the alternative approach I am advocating, ultimately sketching a strategy for obtaining mechanism-based explanations in section 5. In order to clarify and illustrate my arguments, I devote another section to suggest how I would go about trying to explain the veritable surge of the Norwegian Progress Party (Fremskrittspartiet, henceforth Frp) in the spring of 2006.

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<sup>2</sup> It is customary to invoke a distinction between attitude *formation* and attitude *change* (cf. Oskamp et al.: 166). While clearly a useful analytical tool, I cannot see that there is any essential difference in terms of the proposed causal processes bringing each about. For this reason, I shall not invoke it in this paper.

## **1.1. Questions to Be Addressed**

This paper aims at answering the following three interconnected questions. Each need to be provided some kind of answer before the next on the list is addressed. Questions 2 and 3 are, strictly speaking, one and the same, insofar as the concept of “strategy” includes the choice of goal.

*Q1 How does the mechanism approach to the explanation of short-term changes in public opinion differ from traditional approaches, and in what sense are mechanism explanations of given changes in public opinion superior to traditional explanations based on statistical inferences? (Sections 1 through 4)*

*Q2 What insights and empirical findings may constitute epistemological justification for claims to have identified the most important causal mechanisms bringing about a given change in public opinion? (Section 4)*

*Q3 What research strategies – if any – can provide these insights and empirical findings? More specifically, how could one go about trying to explain the recent rise of Fremskrittspartiet by employing a mechanism strategy? (Sections 5 and 6)*

## **1.2. The Concept of “Causal Mechanism”**

The production of literature on the concept of “mechanisms” seems to be on the rise in the philosophy of social science – promoted, notably, by Jon Elster (1983; 1989; 1999) – but the debate has yet to influence actual social research in any significant manner. The amount of explicit references to mechanisms, vary widely among disciplines (Hedström et al. 1998: 3). Mechanisms are frequently invoked in psychology, especially research on cognition. Some economists think of the market as an explanatory mechanism. The concept is also sometimes employed in sociology, but has yet to compete with the notion of “institutions.” Nor has it gained any strong foothold in other disciplines of social science.

Though widely used, there is no standard definition of the concept of “causal mechanism”. Many authors employ the concept of “social mechanism”. It is not always clear whether this also includes intrapersonal, such as neurophysiologic, mechanisms. To me it seems unnatural that they do. In this paper “social mechanisms” shall denote causal mechanisms established by the social sciences, and I expect them to correspond largely to what I shall call “interpersonal mechanisms” (in section 4.1). In the spirit of cross-disciplinarity I shall not limit myself to these mechanisms when suggesting explanations of changes in public opinion.

My definition in section 1.2.1 below proceeds from this idea that mechanisms are made up of causal processes that are not directly observable but have observable effects in the world of human interaction, which is to say that mechanisms are something *real* to be identified in an objective reality, as opposed to merely an analytic tool (see section 2.1.2). Sociologist and philosopher Arthur S. Stinchcombe seems to intend a definition of the latter kind when referring to mechanisms as “pieces of scientific reasoning” (1993: 24-25). These two alternative concepts of “causal mechanism” correspond to the disagreement between *scientific realists* on the one hand, who believe the goal of science is to identify unobservable causal processes in the real world; and *antirealists* on the other, who believe science can only aim at achieving the best possible description of all observable phenomena (see section 2.1.). While Stinchcombe is an antirealist, I place myself squarely in the realist camp. Stinchcombe’s definition nonetheless points to some aspects that I shall want to take into account:

“Mechanism’ means (1) a piece of scientific reasoning which is *independently verifiable* and independently gives rise to theoretical reasoning, which (2) gives knowledge about a component process (generally one with units of analysis at a “*lower level*”) of another theory (ordinarily a theory with units at a different “*higher*” level), thereby (3) increasing the *suppleness, precision, complexity, elegance, or believability* of the higher level theory without excessive “multiplication of entities” in it, (4) without doing too much violence

(in the necessary simplification at the lower level to make the higher level theory go) to what we know as the main facts at the lower level” (my italics).

Although one should not play fast and loose with the concept of “verification” – “testability” is much preferable – I think clauses (1) and (2) capture the essence of the mechanism-centred research strategy that I shall propose. Rather than (merely) constructing one integrated theoretical description of the dynamics within a given field of research and tests its implications, researchers should take advantage of established “pieces of reasoning” from other – usually “lower level” – disciplines, in turn testing which relevant pieces are most important in the production of a given phenomenon. Suppleness and precision are obtained by establishing theory concerning the conditions under which mechanisms are efficacious or not.<sup>3</sup> Excessive multiplication of entities is avoided by including in an explanation only those mechanisms that are most important to the phenomenon we wish to explain.

Given explanatory realism, the only justifiable “violence” on descriptions of identified mechanisms is exclusion for purposes of economy or clarity. Revisions for any other purpose than to make theory correspond more closely to reality are untenable. Complex phenomena frequently have complex explanations. If one wish pragmatically to provide easily assessable *descriptions* of complex phenomena, that is another matter. The task of obtaining a true explanation is very different from making someone understand something, but it seems to me these two tasks are sometimes confounded by social theorists. My views in this respect are not on par with Stinchcombe’s. He explicitly accepts that a mechanism such as rationality explains market-phenomena, just as long as the hypothesis it generates fits well enough with empirical findings on the aggregate level (ibid: 25-26). Confidently brushing aside refutations by psychologists, Stinchcombe asserts that “mechanisms do not need to be true to be

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<sup>3</sup> By “efficacious” I shall mean *potentially* effective, while the “efficacy” of a mechanism shall refer to its actually being effective in a given situation. Efficacy is an ontological quality that decides the *truth* or an explanation: a mechanism *is* or is not effective. Efficaciousness is epistemological: it depends on the *justification* provided by the total amount of *evidence*.



useful” (ibid: 27; cf. references to Milton Friedman in section 2 below). In my view, they may be useful to describe, but not to explain.

There is another important distinction regarding social mechanisms, on which explanatory realism does not entail one stance or the other. In his essay “A Plea for Mechanisms” (1999), Jon Elster drew attention to two possible ways of defining the concept by himself substituting one for the other. In *Explaining Technical Change* (1984) he contrasted mechanisms with *black boxes* situated between causes and their effects, commonly expressed as regression coefficients. He saw the search for mechanisms as attempts at opening up these black boxes, in order to obtain “finer grained” theories of how events are connected as causes and effects. Fifteen years later Elster had abandoned this view. He had come to identify the quest for more fine grained theory with the impossible project of *reductionism* (1999: 4-5). Instead he contrasts mechanisms with *scientific laws*, arguing that mechanisms are something in between mere narratives and (unobtainable) laws (ibid: ix).<sup>4</sup> According to Elster’s new definition, mechanisms are “*frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with indeterminate consequences.*” Mechanisms, then, explain a phenomenon by subsuming it into a more general category of phenomena, without the universality that would allow us to make predictions.

Elster’s two concepts charge mechanisms with two quite distinct explanatory tasks. On the one hand: *reducing*; on the other: *generalising*. While Elster now seems to think it is impossible to obtain the kind of fine grained causal theories he once promoted, I believe it is the right goal for social science to pursue. I do not, however, believe the two concepts exclude one another. I believe that the relative generality of mechanisms (their frequency of occurrence) is an important aspect of their ability to explain. I realise that we may never achieve the kind of theory that would enable us to predict social phenomena with any accuracy; it remains true that mechanisms are “triggered

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<sup>4</sup> The idea of locating mechanisms between laws and descriptions is apparently adopted from Robert K. Merton. For a more comprehensive history of the concept, see Hedström and Swedberg 1998.

under generally unknown conditions or with indeterminate consequences,” but I think that attempts to clarify these conditions and consequences *as much as possible* should lie at the heart of a mechanism-centred research strategy. Thus I shall propose to accommodate both of Elster’s notions by conceiving reduction simply as finer grained generalisation (see section 4.2).

### 1.2.1. A Working Definition of “Mechanism”

Accordingly, I shall formulate a definition of “causal mechanisms” that pretends to take both of Elster’s concepts into account. By a “causal mechanism” I shall mean:

*A recurrently occurring pattern of causally connected events (causal processes), through which two or more events are related as cause and effect in some but not necessarily in all causal contexts.*<sup>5</sup>

The idea that such recurrent patterns really exist is central to the ontology upon which the causal mechanical theory of explanation is founded. It is an assumption that is strengthened each time a recurrent pattern is thought to be identified. Mechanisms are triggered by events in situations where relevant states – or *factors*, as statisticians call them – make up the “causal context”. Mechanisms, then, are not causes in themselves.<sup>6</sup> If, say, there is a mechanism of rationality, it would not be rationality itself that brings about a phenomenon, but an event in a specific causal context triggering a specific instance of rational deliberation.<sup>7</sup>

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<sup>5</sup> For all practical purposes this definition is equivalent to Andrew Bennett’s: “*Ultimately unobservable physical, social or psychological processes through which agents with causal capacities operate in specific contexts to transfer energy, information, or matter to other entities*” (2003, section II). Differences are merely of emphasis.

<sup>6</sup> There may be exceptions to the rule that mechanisms are not causes in themselves, though I cannot think of any. Possibly, self-sustainable biological mechanisms are of this kind.

<sup>7</sup> Triggers may be in principle be “mental events” – however one defines “mental.” There are, of course, good reasons to think mental events are themselves always triggered by outside events, but this causal regress is nothing peculiar to them. We are always forced to choose triggering events in a line of endless regress. It is a pragmatic choice, in which a scientist demonstrates explanatory emphasis and scientific judgment. Bas Van Fraassen’s “Pragmatics of explanation” is one way of responding to this difficulty (1980, chapter 5).

The fundamental structure of a causal mechanical explanation is as follows: *Event(s) e (or e1, ... en) caused/did not cause (an aspect of) our explanandum x through mechanism m (or m1, ..., mn) (and) because condition(s) c (or c1, ... cn) applied.* The explications of the mechanisms answer *how*-questions and the listing of conditions answer *why*-questions.<sup>8</sup>

By including the clause *recurrently* ( but not” frequently”) *occurring* I mean to indicate, firstly, that the frequency with which mechanisms occur has no definitional bearing upon their explanatory relevance in particular contexts. On the other hand, more frequently occurring mechanisms are more easily recognised and hence more likely to feature in explanations. This would be the case for an important subcategory of intrapersonal mechanisms that seem fixed by what Jerry Fodor has named “modules” (1988), a denomination adopted by evolutionary psychologists such as Jerome Barkow, John Tooby and Lean Cosmides (Barkow et al. 1992). Philosopher of mind Fred Dretske (1988: 42-43) refers to them simply as the “causal wiring” of the brain, and employs the more general concept of “structural causation” to describe a process commonly attributed to evolution. These mechanisms are fixed behavioural responses to given sets of sensory input and as such particularly promising explanations for human behaviour, including public opinion.

The clause *in some but not necessarily in all contexts* signals, on the other hand, mechanism explanations’ lack of universality – echoing Elster’s “under generally unknown conditions or with indeterminate consequences.” If a certain event produced a particular effect in *all* contexts, we would have a *law*. In such a case we would have reason to assume that we had stumbled upon a causal connection so intimate that it is not conditioned on the occurrence or non-occurrence of any third state or event. Such connections have yet to be discovered in the domain of social science (see section 4.2). I shall, however, go much further than Elster in requiring theories of mechanisms to contain explications of the conditions for their efficaciousness. In my view, this

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<sup>8</sup> My emphasis on explicating *why* something happened is partly inspired from Bas Van Fraassen’s “Pragmatics of Explanation”, where he expounds on the view that explanations are answers to questions of *why* something rather than something else happened (1980, chapter 5, in particular p. 134).

requirement is essential in order for mechanisms to be *epistemologically more satisfactory* than universal laws mended with *ceteris paribus* clauses (cf. section 2). Mechanisms should give an account of *why* something happens in one but not in another causal context.

On a purely philosophical note, I should like to invoke the distinction between *token* occurrences of mechanisms and *types* of mechanisms, as drawn more generally by Charles Sanders Peirce in the 19<sup>th</sup> century. Obviously, I speak of *types* of mechanisms recurrently occurring. But for a type of mechanism to be explanatory relevant to a particular phenomena, a token of this type must have been effective in bringing it about.

There are an insuperable number of mechanisms, states and events effective in the production of any particular social phenomenon. As mentioned, it is obviously necessary to restrict the number of mechanisms included in an explanation. In problem driven research, such priorities are guided by the question asked. In order to identify mechanisms responsible for very complex phenomena, such as the Wall Street Crash of 1929, scientists may find it more productive to begin by focusing on explaining constituent events. Whether an explanatory strategy beginning with a general theory of stock-market crashes might succeed, depends, it is implied by my arguments in section 5, on the suppleness of that general theory.

### **1.3. Explaining with Mechanisms as opposed to Variables<sup>9</sup>**

Practitioners in the study of public opinion might object to this dichotomy, arguing that variables presuppose mechanisms and, conversely, that mechanisms depend on variables to be tested. In reply to this, I shall, for now, have two comments. Firstly, there remain a crucial difference of *relative emphasis* on statistical inferences and other types of evidence, as explicated in sections 4 and 5 below. Secondly, arguments

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<sup>9</sup> I borrow this dichotomy from Hedström and Swedberg (1998), who in their “Introduction to Social Mechanisms” includes a section called “Variables versus social mechanisms,” where they invoke the same distinctions and some of the same points that I shall make.

in this paper are to the effect that explanations require (non-self-evident) mechanisms to be *explicit*. As implied by the typical structure of a causal mechanical explanations, the main philosophical point to be made is that a cause in itself does not itself fully explain any phenomenon. We need to know *why* and – to the extent that it is possible– *how* the cause had its effect. The *why* refers to the essential conditions for their ultimate effect, while the *how* refers to the constituent causal processes of the mechanisms themselves. As I shall argue in section 4, we do not always need to know the smallest particular processes of the mechanisms, but we do always need to know enough to identify its pattern in particular contexts.

What explains, are mechanisms in conjunction with triggering events and causal contexts. Granted, the mechanism is sometimes so obvious that we need only point to the cause, such as when lack of food causes hunger and eventually death. Other mechanisms are not so obvious, even when two states or events always occur together. This would be the case with the various effects of malnutrition. Lack of certain vitamins is associated with certain somatic effects, but it is not clear to everyone just how such deficiencies affect our bodies. This is simply to observe that explanations are always relative to background knowledge. Causes by themselves explain only when the associated mechanism is obvious and common knowledge.

It is easy to suggest better explanations than those we are accustomed to in the study of public opinion. It is harder to suggest viable alternative research designs and methods. From a purely epistemological point of view it is obviously desirable to identify and explicate causal mechanisms. Unfortunately, social scientists need to make more mundane consideration as well. Given methodological and financial restrictions, they may be justified in contending themselves with describing changes in public opinion, merely commenting upon their fit with relevant theories. To the best of my ability, I shall take such practical considerations into account, for I should like to make conclusions relevant to actual research. Indeed, the realisation that resources are limited is part of the motivation behind the proposed strategic shift away from the explanatory width associated with traditional theories to problem driven research, as

explicated in section 5. Furthermore, researchers choose methods and designs based on practical experience and know-how that outsider critics may not be able to take into account. I am certainly ignorant of any number of such practical considerations, and I shall try to make my arguments as invulnerable to them as possible. Complete abstraction, however, is impossible.

While causal mechanical explanations aim at answering *why* and *how* something happens, purely statistical explanations may at best answer questions pertaining to *what*, *who* and *when*. Norwegian research on public opinion abounds with two kinds of studies: Firstly there is the category of seemingly atheoretical surveys (e.g. Aardal 2003). Scientists ask respondents a broad range of questions with the aim of mapping opinions on various issues, then go on to construct variables, check correlations between them and a standard set of variables such as age, gender, income, region, education and political awareness. While survey questions may have been chosen on the grounds of more or less explicit theoretical expectations, they are not designed to test specific explanatory hypotheses. Political scientists, however, typically interpret their findings in the light of relevant theory, and if data fit sufficiently well with a specific theory, they might consider that theory an explanation. Of course, such general surveys are indispensable ways of establishing facts of public opinion, but, according to my arguments in sections 4 and 5, they cannot by themselves provide the necessary data to identify particular causal mechanisms.

The second common practise is construct surveys to test particular theories making sweeping claims on an aggregate level. Within the study of how mass media affects political participation, for instance, there are a handful of such theories suggesting, among them, the full spectrum of possible aggregate effects. The theory of “video-malaise” predicts that high exposure to mass media during an electoral campaign will trigger mechanisms contributing to *decreased* participation in elections; theories of mobilisation predict *increased* participation; and theories of “minimal effect” asserts that mass media exposure does not have much effect at all (Aardal and Waldahl 2004: 277-78).

Tested in this manner, these theories will not contribute much by way of disclosing the how mass media influence the public (not strictly speaking its *opinion* in this case, but its aggregate *action*). If testing their total aggregate effect were all that was needed confirm the efficacy of the mechanisms implied by these theories, one of them would logically have to be true. Obviously, that cannot be. Each aggregate effect can be produced by any number of alternative mechanisms. The mechanism strategy that I shall outline in section 5 advocates the formulation of testable assertions about specific causal mechanisms. We are unlikely to identify mechanisms by merely examining the relation between two general events.

#### **1.4. The concept of “Public Opinion” and its Ontological Status<sup>10</sup>**

In an early contribution to the study of public opinion, Philip E. Converse (1964), in his famous *non-attitude thesis*, claimed there was no such thing as a public opinion. Analysing a number of panel surveys from the 1950s, Converse found that most people lacked even the most fundamental knowledge of political affairs and their answers to survey questions were highly unstable and even inconsistent. A minimum of stability and consistency must be expected from genuine attitudes. Without it, there is not much sense to the study of public opinion, argued Converse.

Though Converse’s conclusions have been widely contradicted, perhaps most notably by Christopher Achen (1975), Benjamin I. Page and Robert Y. Shapiro (1992), who have all pointed to various logical, methodological and theoretical problems in Converse’s argument, he undoubtedly expressed an important fact with which surveying researchers need to reckon. Later efforts have reinforced the idea that there are theoretical and methodological ways of handling certain degrees of instability and inconsistency. Indeed John R. Zaller’s “response axiom” not only accepts – it assumes nothing but top-of-the-head answers to survey questions (1992: 49; 277-79). Zaller’s so-called “RAS model” describes an assumed interaction among a few allegedly important factors in the reception and reproduction of political opinions. It proceeds

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<sup>10</sup> It seems that, in Norway at least, the concept of “public opinion” refer only to attitudes towards *issues*, not *political parties*. I shall not invoke this analytical distinction in this paper.

from the fundamental idea that public opinion is shaped predominantly by “elite supplied information” (ibid: 23). Statements of political opinions, as registered by public opinion surveys, are conceived as “the outcome of a process in which people receive new information [from elites], decide whether to accept it [as considerations<sup>11</sup>], and then sample [among a set of accepted political considerations] at the moment of answering questions” (p. 51, original emphasis), hence the name of “RAS”. Attitude change is understood as “a change in people’s long-term response probability” – i.e. the probability of an accepted consideration to be invoked in answering a given question. Changes in this probability result from “[changes] in the mix of ideas to which individuals are exposed” (p. 266).<sup>12</sup>

Thus Zaller justifies the fundamental assumption that changes in public attitudes are worthy of scientific study, even though they may only to a small degree be the result of deliberate individual changes of opinion. Another way for researcher to reconcile themselves with response instability is by attributing it to measurement error due to various distorting effects and the fundamental problem of translating attitudes into the unavoidably vague language of survey questions. Zaller makes a convincing case to the effect that “‘measurement error’ is closer to being a euphemism for ‘unexplained variance’ than it is to being a well understood phenomenon” (ibid: 31).

Individual attitudes and opinions, then, are not necessarily rational or internally consistent. Accepting Zaller’s response axiom, we should allow the possibility that some registered changes in public opinion – even if they are statistically significant – may merely be the product of fleeing influences or a mix of measurement effects contributing in the same direction and that the result would be significantly different if the survey were conducted only days later or in a slightly different manner. Not so for the category of changes with which I am presently concerned, which are by their nature sufficiently significant so as not possibly to be merely accidental. The surge of

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<sup>11</sup> The concept of “consideration” is a primitive term in Zaller’s model. A consideration is “a belief concerning and object and an evaluation of that belief” (1992: 40). The concept is meant to be general and to possess certain vagueness, in order to remain open to different theories of cognitive psychology.

<sup>12</sup> Thus the model seems compatible with John Petrocik’s theory of issue ownership and shifting agendas.



support for Frp in the spring of 2006 is one such undeniable change in public opinion. That is not to say that 30 percent would vote Frp if there were an election in April (see section 3.1), but the fact Frp rose so significantly on all polls, is in itself an event worth exploring.

## **2. Why Search for Mechanisms?**

My rationale for promoting the mechanism-centred strategy of explanation is two-fold. On the one hand there are conceptual philosophical considerations, as discussed in section 2.1 below. They proceed from a particular theory of what the concept of “scientific explanation” *means*, to assertions of what a proper explanation *must include*. Specifically, I shall attempt to translate the *causal mechanical theory of explanation* into a *mechanism-centred approach to the study of public opinion*. In strict philosophical terms, the causal mechanical theory constitutes an absolute (as opposed to relative) argument in favour of mechanism-based causal explanations, and as such it is not constrained to the category of short-term changes in public opinion. But the arguments that I shall present in its favour do not aim at strict philosophical validity; I shall attempt only to strengthen its intuitiveness as applied to the explanation of short-term changes in public opinion, relative to the alternative of regarding their explanation simply as the subsumption into a statistically established pattern. It is outside my scope to present technical philosophical arguments rebutting alternative theories of truth and explanation, which may possibly justify the traditional variable-centred approach to the study of longer-term changes in public opinion.

Secondly, there are epistemological considerations in favour of the mechanism-centred approach. They are fundamentally *relative* arguments in that they are to the effect that a mechanism-centred strategy will produce *epistemologically more satisfactory* explanations of short-term changes than do the traditional correlational approach. I intend this concept of “epistemological contentedness” to express the subjective feeling of having been provided with well justified assertions that satisfies the curiosity on a given question. I shall provide no strict definition, but assume that the

relative merits of mechanism-based explanations that follow below and in section 4 are all to the effect of increasing it. Thus the concept is wholly explicated in terms of an explanation's relative *completeness* and the relative *certainty* that we ascribe to its contentions. I might instead have taken the causal mechanical theory of explanation as fundamental, explicating the virtues of epistemological contentedness in terms of this theory, but I prefer the honesty of expecting the reader to agree on the intuitiveness of my considerations to the listlessness of appealing to a philosophical position that I have not the resources to fully defend.<sup>13</sup>

*Completeness* is the more tangible of the qualities contributing to epistemological contentedness. Of course, there is no reasonable sense in which we can obtain absolutely complete explanations, but an explanation may be *more complete* than another. There are at least two senses in which causal mechanical explanations are more complete than traditional statistical explanations. In the first place, they do not only answer questions of *what*, but also *why* and – to the degree that fine grained theory is available – *how*. While the conceptual argument explicated in section 2.1 will proceed on the *qualitative difference* between merely answering *what* and also answering *why*, it is in the present context simply the extra *quantity* of information that is thought to contribute to the increased completeness.

Essentially contributing to such quantitative completeness is the strategic shift towards problem driven research that I shall explicate in section 5. It comes at the price of the tentative explanatory range associated with variable-centred theories. The mechanism strategy will provide more complete accounts of a given explanandum by focusing attention on that specific phenomenon rather than on providing a more comprehensive account of the political situation in which it occurs. This strategy proceeds on the assumption – or rather the hope – that this trade-off between depth and width will only

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<sup>13</sup> Jon Elster makes a similar appeal to the readers' intuitions when stating that finer grained causal explanations are "*intrinsically more satisfactory to the mind*" (1999: 6). While Elster's contention concerns only mechanism explanations by *reduction* (cf. section 1.2) relative to *lawlike regularities*, I shall argue that also mechanism explanations by *generalisation* are epistemologically more satisfactory than mere statistical correlations. My concept also differs from Elster's in that I include the reduced risk of spurious explanation as a factor contributing to the contentedness.

be temporary. In the long run, scientists would be able to consider a number of established mechanisms relevant to a given explanandum and a theory of what contexts they would be efficacious. This is part of my rationale for advocating an emphasis on the distinction between developing *context independent* “mechanism theory” on the one hand and theories of particular contexts on the other (see section 5.1).

I propose a more general concept of “*certainty*” to replace the concept of “*statistical certainty*”, though I expect that the latter will frequently contribute significantly to the former. General “certainty” is intended to express an important aspect of epistemological contentedness that is difficult to capture by any numerically operationalised concept. Certainty is a state of mind, and it comes in degrees. To feel certain in an explanation is to feel certain that it is not *spurious*. Such certainty may or may not be well justified, but I shall assume that better justification increases certainty.<sup>14</sup> Certainty and quantitative completeness are not wholly independent qualities. Clearly, more relevant information will reduce the risk of spurious explanation, but certainty depends on more than just the quantity of information. In section 4 I shall argue that the mechanism-centred approach significantly reduces the risk of spurious explanation insofar as it manages to obtain support from relevant findings *across various causal contexts*, in the shape of intrinsically *interdisciplinary* “mechanism theory”. I shall argue that this is a more effective way of producing certainty than relying exclusively on evidence from the particular situation under investigation. According to John R. Zaller, this is in tune with the prevailing fashion in statistical theory:

“Contemporary statistical theory, though not most public opinion research, tends to downplay the importance of statistical significance as estimated from a

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<sup>14</sup> This concept of “certainty” is analogous to the “subjective” interpretation of *probability*. The assumption that such certainty is increased by better justification is analogous to the assumption that subjective probability is constrained by rational calculation. See: Hájek, Alan, "Interpretations of Probability", The Stanford Encyclopedia of Philosophy (Summer 2003 Edition), Edward N. Zalta (ed.): <http://plato.stanford.edu/archives/sum2003/entries/probability-interpret/>

single test of a model on a single dataset. It instead emphasizes the stability of results obtained across related problems and datasets [...]” (Zaller 1992: 290).

Certainty, then, unlike statistical significance, cannot be measured numerically. This increased flexibility as to what may be accepted as supporting evidence is a central idea to the causal mechanical approach. Increased flexibility does not necessarily involve increased leniency. To the extent that it does, it is a problem I shall take very seriously.

### **2.1. “Looking under the Hood”**

In an article rebutting the antirealist instrumentalism of the late Milton Friedman, Daniel Hausman (1992) advocates the necessity of “*looking under the hood*” of theories. The metaphor nicely summarises the dispute between *scientific realists* and *antirealists*. It also points to the rationale for seeking evidence across various causal contexts. To look under the hood of a theory is to examine whether its successful predictions are due to its correctly capturing real underlying causal processes, or whether it is just a coincidentally adequate description of all phenomena hitherto observed. Much like a driver who does not inquire into the workings of the engine until it breaks down, researchers tend not to question accepted theory as long as it yields reasonably accurate predictions. Nor should they, according to Friedman, in whose opinion it mattered only whether or not the theory “drives well” where it is intended to drive. And if it breaks down on such a road, it is time to get a new theory. Less metaphorically, theories are invariably founded upon a number of untested assumptions, and Friedman was adamant that the truth or falsity of these assumptions has no bearing upon the merits of the theory. The only possible criterion of the goodness of a theory is whether its predictions agree sufficiently well with subsequent observations. As long as economic theories assuming rational agents in perfectly competitive markets yield predictions of reasonable accuracy within its intended domain, we cannot reach for anything better. What is at stake here, fundamentally, is

whether *truth*, conceived as a quality that depends on establishable facts in a world independent of human minds, is a possible or even appropriate goal for science.<sup>15</sup>

It seems that it is the spirit of Friedman rather than Hausman in which most Norwegian research on public opinion is conducted. A typical study consists of a public survey whose results are interpreted in the light of some important theories (cf. section 1.3). If a theory seems to agree with the findings in one but not in another political context, this is simply stated to be the case, such as when Aardal and Waldahl mention that findings concerning media's agenda-setting influence on the voters in Norway are discrepant with those of corresponding surveys in Great Britain and theories tested in the USA (2003: 298-299). At best, such discrepancies are discussed on the grounds of *prima facie* saliences. I shall argue that this demonstrates a lack of proper attention to the causal mechanisms that are triggered in the one but not in the other political context. Social scientists need to *look under the hood* of their theories in order to assert that they really capture what is going on in the public they study.

### 2.1.1. Scientific Realism

The essence of *scientific realism* is the belief that unobservable causal processes are real and *in principle* within the grasp of scientific inquiry. *Explanatory* realism is the corresponding view that true explanations must refer to real entities. This is a fundamental premise of *the mechanical theory of explanation*. To have a true explanation, according to explanatory realists, is to have identified (and appropriately described) the real causes that produced the explanandum. Truth, then, is – at least essentially – defined by this correspondence between a belief and the external state that it concerns (cf. footnote 15). As already mentioned, it is beyond my scope to level technical philosophical arguments against alternative *antirealist* theories of

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<sup>15</sup> The most notable alternative to this *correspondence theory of truth* is the *coherence theory*, according to which truth is defined in terms of a relation between the belief being assessed and other beliefs, or this relation is held to be the only possible criterion upon which to assess truth. While scientific *antirealism* is per definition incompatible with any correspondence theory of truth, it seems quite possible that scientific realists accept an element of coherence when assessing the truth of a statement. Another possibility is, as does Richard Rorty, to define “truth” in terms of “usefulness”, which, frankly, I find unintuitive (cf. section 1.3 on the difference between *explaining* and to *make someone understand*.)

explanation. I shall defend explanatory realism and the causal mechanical theory of explanation only to the extent that I succeed in strengthening the intuitions upon which they are founded – which are also the intuitions that motivate my criticism of the traditional approach to the study of public opinion.

Hedström and Swedberg (1998: 14) assert that “[it is impossible to provide] theoretical accounts of what happens as it actually happens.” This is a rather pessimistic view of the possibilities of science. A few paragraphs later they seem merely to be asserting the impossibility of accounting for *everything* that happens, but, in their assessment, to ignore aspects of reality amounts to distorting that reality. From this they conclude that “truth value” is an impossible criterion upon which to choose among alternative scientific models. It is their “usefulness for the purpose at hand” that must be decisive. I disagree. This is essentially the kind of instrumentalism attributed to Milton Friedman. According to explanatory realism, on the other hand, explanation aspires towards truth, not only usefulness. And truth as correspondence is possible even without concepts corresponding to every aspect of reality. Simplification does not necessarily involve distortion. Even if we could *in principle* observe everything as it actually happens, we would not be able *in practise* to render it all. Choosing what to include in an explanation is a matter of good scientific sense, and asserting the impossibility of including everything is no argument against scientific realism.

Hedström and Swedberg’s argument is indicative of a common justification for an even more common kind of epistemological modesty among social theorists, namely, to aspire no further than to develop theories with a reasonably close fit to available data. Further considerations as to the actual correspondence between theory and reality, such as relevant insights from “lower level” sciences, are rarely taken into consideration. In this paper I take the view that scientific realism is the more appropriate idea upon which scientific enterprise should proceed. Presently I shall explain why.

First we need to have a clear idea of what “scientific realism” means. Bas Van Fraassen, an anti-realist of sorts, provides a minimal definition of scientific realism that captures its essence (or so I believe it is widely accepted):

“Science aims to give us, in its theories, a literary true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true” (1980: 8).

Importantly, this definition does not assert that science has hit upon the truth in their theories. Nor does it imply that it eventually will. His own position, Van Fraassen labels “constructive empiricism”. The only difference to his definition of scientific realism is the substitution of “literary true” for “empirically adequate”. Empirical adequacy includes not only all observed phenomena, but all *possibly observable* phenomena (ibid: 12).<sup>16</sup> Even this would fly in the face of instrumentalists like Friedman.

According to Van Fraassen, these positions are answers to the question of what it means to *accept* or *hold* a theory. Realism does not imply that scientists are ever in a position to assign the probability of 1 to any non-tautological statement. Contrary to what some seem to believe, scientific realism does not involve unwarranted confidence in the truth establishing capacity of science. It asserts that the criterion to decide the truth of an hypothesis is its correspondence to the reality it describes, but not that this is an easy criterion upon which to decide. Whether or not there is any relevant difference between scientific realism and Van Fraassen’s constructive empiricism to the topic of this paper hinges upon the definition of “observable”. That is a discussion into which I shall not venture.

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<sup>16</sup> According to Van Fraassen’s view, theories should not pretend to say anything about phenomena unobservable *in principle*. He concedes that we may never reach an agreement on where this line should be drawn, but the position seems tenable just as long as we agree that there are observable phenomena as well as phenomena that are in principle unobservable.

The realist idea that science may justifiably include unobserved and possibly unobservable entities in its theories is promoted by social scientist and philosopher of science Andrew Sayer of Lancaster University. In a book devoted to methodical implications of scientific realism, Sayer attributes to traditional social science the slogan “*never mind concepts, look at the techniques*” (1992: 2). In Sayer’s assessment, social scientists have uncritically accepted the (anti-realist) regularity view of causality, focusing singularly on the operationalisation of given concepts and establishing statistical correlations. Sayer’s book proceeds on the idea that – in addition to observable regularities – there are in the world certain causal powers and structures, which we may identify without directly observing them. Scientific concepts should aspire to mirroring these structures rather than just the regularities they may or may not produce:

“In view of this, less weight [should be] put on quantitative methods for discovering and assessing regularities and more on methods of establishing the qualitative nature of social objects and relations on which causal mechanisms depend” (1992: 2-3).

The requirement of explaining *why* something occurs (explicating necessary and, if possible, collectively sufficient conditions) follows from having rejected the regularity view of causality, and with it the idea that descriptions of empirical regularities explain the phenomena they subsume. When something does not always happen, as Sayer points out, we need to explain *why* it happens when it happens:

“The relationship between [causal mechanisms] and their effects is therefore not fixed, but contingent [...] explanation must include reference to the necessary conditions for the existence [and operation] of the mechanisms [...] Given the independence of mechanisms from their conditions, causations need not imply regularity in patterns and sequences of events (1992: 107; 111; 121).



This summarises nicely my own view, except that I would change “necessary conditions” for “essential conditions” (cf. section 2.3). The view that scientific concepts should aim at mirroring the underlying causal processes rather than observed regularities has reached its fullest and most coherent expression in the causal mechanical theory of explanation, as developed by Wesley Salmon.

### 2.1.2. The Causal Mechanical Theory of Explanation

Over more than four decades, Wesley Salmon has attempted to capture the essence of what it means to explain something scientifically. In the 1980s he reached the conclusion that any account of scientific explanation must include the notion of *causality* (Salmon: 1989), having accepted that “we need not commit ourselves to determinism in order to hold that there are causal influences in the world” (Salmon 1982: 166). In his latest efforts he takes as primitive the notions of a “*causal process*” and that of a “*spatiotemporal intersection of processes*” (Salmon 1998: 71). Mechanisms, then, are made up of these processes and intersections of processes.<sup>17</sup> Spatiotemporally continuous intersections are, according to Salmon, “precisely the kinds of causal connections Hume sought but could not find”. The great philosopher mistakenly sought “necessary connections”, thus, according to Salmon, overlooking “the one fundamental aspect of causal processes, namely, that they are capable of transmitting information” (1982: 166).

Salmon distinguishes three fundamentally distinct conceptions of scientific explanation: the “*modal*”, the “*epistemic*” and the “*ontic*” (1989: 118). *Modal* explanations are to the effect that the explanandum-event had to happen with *nomological* necessity, given its antecedents. This necessity derives from the *laws of nature*. Carl Hempel’s accounts of explanations, on the other hand, are *epistemic*. Both deductive-nomological and inductive-statistical explanations are inferences to the

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<sup>17</sup> Salmon does not, as far as I can see, employ the concept of “mechanism”. The Causal Mechanical Theory derives its name from its classical Newtonian emphasis on spatiotemporal connections.

effect that something was *logically (deductively or inductively)<sup>18</sup> to be expected* in virtue of the explanatory facts. Thus there are two kinds of necessity, derived from the laws of *nature* and *logic* respectively. This distinction is of no consequence to this paper, but the one between these two and the ontic is fundamental.

The *ontic* conception derives its name from its emphasis upon existent physical relationships. Crucially, its concept of scientific “law” does not imply any kind of necessity. It proceeds on the realist idea that laws either *are* regularities in the real world or *describe* such regularities. Thus construed, laws are just as universal as the patterns they describe. The causal mechanical theory is the principle ontic theory of explanation, according to which “*the explanation of events consists of fitting them into the patterns that exist in the objective world*” (ibid: 121); it will trace the causal processes and interactions leading up to the explanandum-event. Events are still explained by their being related to antecedent conditions by means of a law, but only in the sense that the laws make intelligible the real pattern into which they are fitted. Observed regularities, then, may help us identify the underlying pattern into which explanandum-events are fitted, but regularities themselves cannot explain their constitutive events. According to the ontic conception, there is no such thing as a statistical explanation *per se*, merely statistical *evidence* (Clark Glymour’s interpretation: 1982: 179). Indeed, Salmon later included “statistical relevance” as a requirement for causal processes to be explanatorily relevant, for without it we cannot say which specific *feature* of a causal process is responsible for the effect in question.<sup>19</sup>

These three ways of thinking about scientific explanations may seem equivalent as long as laws are *universal*, but a “striking divergence” will appear once we start considering explanations that invoke *statistical* laws (Salmon 1982: 160). When highly probable events occur, we may – in accordance with the epistemic account – assert

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<sup>18</sup> Deductive logic produces deductive-nomological (DN) explanations, while inductive logic produces inductive-statistical (IS) explanations (see e.g. Woodward 2003: section 2).

<sup>19</sup> This is another technical philosophical problem I shall shy away from. It is discussed in Woodward 2003, sections 4.2 and 4.4.).

that it *was to be expected* in virtue of the explanatory facts. But if laws are irreducibly statistical – which is to say that no matter how many relevant factors we are able to specify,<sup>20</sup> the improbable will in some cases occur – there can be no necessity involved. If, then, as the modal and the epistemic accounts assert, necessity is built into the concept of explanation, there would be no possibility of properly explaining anything at all in an indeterministic world.<sup>21</sup> The ontic account, on the other hand, does not have this consequence.

Much of Salmon's later writings have been devoted to establish a criterion to distinguishing causal processes from non-causal pseudo-processes. His efforts have focused on the ability of causal processes of transmitting information – or, more technically: a “mark” (Woodward 2003: section 4.1). Fortunately, I need not dig very deep into these accounts. What is important for my purposes is to understand why Salmon devotes so much of his time to this. In short, it is because this is also the criterion upon which we can decide whether a mechanism is possibly effective and hence truly explanatory. Such a criterion is necessary for the viability of the causal mechanical theory of explanation. Anti-realists, on the other hand, have no use for this criterion, because they refrain from appealing to anything but the regularities themselves. When regularity is the criterion for causality, nothing more is needed.

*The ideal gas law*, whereby there is a constant relationship between the volume, pressure and temperature of a given mass, provides excellent illustration for the distinctions now made. According to the epistemic and modal accounts, this law itself explains any changes in volume, pressure and temperature; while according to the ontic account, it does not explain anything. Rather, it is the underlying causal processes of molecules moving around and colliding that explains the lawlike regularity. Other examples of explanations that would be rejected by Salmon's criterion, while perfectly admissible according to some anti-realist positions, include

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<sup>20</sup> Laws thus specifying in detail the conditions for their applicability, may become indistinguishable from the kind of explanatory mechanisms I am promoting. But mechanisms are supposed to be theoretically better founded than laws ridden with *ceteris paribus* clauses (cf. sections 1.2 and 2.3).

<sup>21</sup> Quantum mechanics suggest that all laws are fundamentally statistical. If accepted, this will have an enormous impact on the philosophy of science.

explanations that invoke any ability of *rational deliberation* that people do not possess. The axioms of rationality may adequately describe observable dynamics of market behaviour, but unless they are also causally responsible for this behaviour, they are not, according to scientific realism, explanatorily relevant.

An explanation, then, requires assertions of causation, and causation occurs when there is a spatiotemporal intersection of processes capable of transmitting information. This may seem an impossible requirement for explanations of such complex phenomena as changes in public opinion. While it may be ontologically intuitive that such processes underlie observable phenomena, it is not always possible to identify them. Plainly, we cannot be required to identify spatiotemporally continuous processes from, say, a statement made by a politician to the reactions of each voter. But if we know the mechanism that produces the reaction of *one* person, we may try to identify that mechanism in other people as well or employ statistical methods to test its pervasiveness. As far as I can see, it is not necessary that we trace every particular causal process – as some critics of the causal mechanical theory suggest (see Woodward 2003: section 4.3) – but that the mechanisms with which we explain a phenomenon was demonstrably effective in producing it and that we are capable of measuring the degree to which it was. Analogously, in the case of a container for colliding molecules, an explanation of why temperature raises need not refer to all collisions. We need only know that there is a mechanism increasing the energy of each molecule as a result of collisions. The difference to the domain of public opinion, of course, is that people behave less uniformly than molecules. Explanations of changes in public opinion will have to identify several efficacious mechanisms that together explain a reasonably large part of a given change.

### **2.3. Concluding Remarks: Implications for Social Science**

The principal implication of a strictly realist concept of explanation, such as that expounded by Salmon's causal mechanical theory, is that there can be no assertion of a causal relationship between two events without having identified the spatiotemporally

continuous causal processes – i.e. *mechanisms* – that connect them. Now, my concept of “causal mechanism” includes patterns of causal processes that may not be spatiotemporally continuous, but still may be meaningfully highlighted as constituents in a more complex causal pattern. In section 4.2 I shall argue that such mechanisms explain essentially in virtue of subsuming explananda-events into more *general* and *familiar* patterns. These explanations are not directly justified by Salmon’s account, but I shall argue that they may nevertheless adhere to its realist requirement of correspondence to reality. While spatiotemporal continuity is obviously desirable, the eminently realist Andrew Bennett of Georgetown University notes in his paper “Causal Mechanisms and Causal Explanation” that:

“[The explanation via causal mechanisms] still allows for the possibility that theorizing and explanation can at times most usefully take place at the macro level, and can focus on emergent phenomena. [...] Not all macro-causal mechanisms are of a character that makes it necessary or possible in every study to explain or study them at the individual micro level. [...] *The key caveat here, however, is that macro-causal social mechanisms have to be based in principle on defensible micro level processes that explain individual behaviour.* [...] Explanation via causal mechanisms involves a commitment in principle to making our explanations and models consistent with the most continuous spatial-temporal sequences we can describe at the finest level of detail that we can observe” (2003; my italics).<sup>22</sup>

Not only must spatiotemporally non-continuous mechanisms be *compatible* with “defensible underlying processes”, we must, in my view, have established evidence to the effect that such recurrent patterns are indeed manifestations of one and the same underlying pattern of causal processes. If not, they are just unexplained empirical regularities. Valid evidence may be to the effect of rejecting other mechanisms

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<sup>22</sup> Evolutionary psychologists believe evolutionary theory should be to the social sciences what physics is to the natural science: “*To propose a psychological concept that is incompatible with evolutionary biology is as problematic as proposing a chemical reaction that violates the laws of physics*” (Barkow et al. 1992: 4). I believe it is a fundamentally sound idea to promote a social science better informed by evolutionary biology, but I am not presently prepared to argue the merits of evolutionary psychology as a foundation for social science.

possibly responsible for the regularity. I shall discuss what evidence we need in section 4.3 below. Secondly – and this is where I part with Elster’s account – having identified a mechanism involves having identified the essential conditions for its occurrence or non-occurrence – not just in the shape of *ceteris paribus* clauses, but as direct empirical implications of the mechanisms’ causal pattern (cf. sections 1.2.1; 2.1.1; 5.1; 5.2). In section 4.3 I shall argue that these requirements do not just amount to raising the bar for statistical verification and then claim that remaining explanations are better in virtue of being on average better supported by statistical evidence: there is a *qualitative* difference involved.

### **3. A Statistical Approach to Explaining Short-term Changes**

Unsurprisingly, there is any number of theories and theoretical frameworks available for explaining how the attitudes of individuals and public opinion change. For the purposes of this paper it is not necessary that I provide any systematic overview of significant theories.<sup>23</sup> I am not so much concerned with the proposed explanations themselves as with how researchers go about trying to corroborate them, and in particular the way in which theoretical statements are translated into testable empirical implications.

Empirical study of attitude change is carried out through three broad categories of methods: *correlational*, *experimental* and *quasi-experimental* (Oskamp and Schultz 2005: 210). Norwegian studies, inasmuch as they deserve the denomination of “variable-centred”, are, of course, predominantly *correlational*. In this section I shall have a look at a particular collection of studies into the 2001 Norwegian parliamentary election campaign, published in *I Valgkampens Hete* (2004) edited by, among others, Bernt Aardal of the Norwegian Institute of Social Research. The collection represents the first serious study into a Norwegian election campaign and as such it is the first systematic effort to study the kind of short-term changes with which I am concerned. Because, moreover, the studies are conducted by some of the best known Norwegian

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<sup>23</sup> One such overview is provided by Oskamp and Schultz 2005. See also Norris et al. 1999: 4-19.

researchers on public opinion, I believe I am well justified in holding them as exemplars of the more general approach that I am presently criticising.

I should, to be fair, emphasise that the studies in *I Valgkampens Hete* do not primarily aim at explaining fluctuations in polls during the campaign or the outcome election. Instead, they attempt to answer more specific questions pertaining to the role of the mass media in setting the *agenda* and *framing* the issues, thus deciding what issues and what aspect of those issues people will emphasise when assessing political parties and politicians running for office. But these very choices of topic and theoretical approach imply that changes in attitudes result essentially from agenda-setting and issue-framing provided by newspapers and television. Were the assumption not that these are the mechanisms through which people's political attitudes are influenced on the short term, it would make little sense to study them. And if the authors thought better theories were available, surely they would have employed them instead. I would in any case venture that any comprehensive study of an electoral campaign may reasonably be assessed by its ability to explain changes occurring in the course of it. Before I start assessing the ability of the methods and theories applied by Aardal et al. to explain short-term changes, I should clarify what I mean by "short-term".

### **3.1. Short-term vs. Long-term Change**

It is undoubtedly difficult to sort out the long-term factors from more abrupt ones purely on the criteria of time. I venture nevertheless that it makes sense to distinguish short-term from long-term changes, because there are good reasons to think there are changes stemming exclusively or almost exclusively from the dynamics of media conveyed current events, which in turn lays the foundations of communication and influence within and among subgroups of the population. Even in the age of the internet blogosphere, few statements or other politically relevant events reach mass audiences without at some point being reported in the mainstream media.

There is the possibility that long-term tendencies burst suddenly into effect, but it seems to me that some changes in public opinion are simply too big and abrupt to be triggered by anything but information brought by the mass media – perhaps with the exception of natural disasters and other apparent effects of global warming. For simplicity I shall define “short-term change” as being triggered *exclusively* by media conveyed current events. This seems a perfectly weak and reasonable assumption, which, incidentally, is also a fundamental premise for John R. Zaller’s RAS model<sup>24</sup> and, albeit implicitly, for the studies in Aardal et al. 2004. I shall nevertheless be careful not to base too much of my argument upon an assertion that I am, after all, unable to justify empirically. The assumption serves mostly to guide my attention, as it guides Aardal et al.’s. My argument does presuppose, however, that there is a species of change distinct from more gradual changes produced by long-term factors such as shifting social cleavages, and thus *in principle* inexplicable by the traditional theoretical framework.

In the absence of events with significant effects on the short term, public opinion will evolve slowly but constantly – not just in response to the sum of lesser events and the influence of opinion makers, but also as a result of demographic, technological, cultural and all other gradual changes in society. As for the Norwegian PP, empirical investigation support, according to Tor Bjørklund of the Norwegian Institute of Social Research, the thesis that “the emergence and surge of radical right in Scandinavia must be related to structural changes and new cleavages.” I have argued that the explanatory potential of this scheme by itself is either limited or *in principle* non-existent: the translation of social cleavages into party political affiliations is far from straightforward and it requires causal mechanisms. Such identified patterns may nevertheless tell us something important about long-term trends. Surely, Bjørklund is right to attribute Frp’s surge from 3.7 percent at the parliamentary election in 1985 to its 22.1 percent in 2005<sup>25</sup> – at least in part – to such long-term trends. Now consider its

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<sup>24</sup> Coverage of public affairs information in the mass media is “the dynamic element in my argument – the moving part, so to speak” writes Zaller (1992: 1), though he later stresses that this is not at strictly necessary premise for the model to work (ibid: 272).

<sup>25</sup> Graph at Bernt Aardal’s home page: <http://home.online.no/~b-aardal/>



7.5 percentage point growth from 2001 to 2005. Was that change caused by short-term or long-term factors? To claim it was exclusively the result of structural changes seems as dogmatic as to say it was simply due to the fact that the two elections fell on different points on a curve constantly fluctuating in response to current events.

There were great fluctuations in expressed support for Frp between the two elections, from a high point of 33 percent in October of 2002 to 18.3 percent in the beginning of September 2005,<sup>26</sup> but there is every reason not to equate polling data with election results. Voting plainly involves a greater commitment than does answering a pollster, and so we should expect that a hypothetical election at any given point of time between two elections would have produced a result closer to a straight line drawn between the outcomes of those elections than the actual polls' mean deviation from that line.<sup>27</sup> The co-reference of polls and election results is not strictly necessary in order to establish the rationale for studying short-term fluctuations.<sup>28</sup> We need only assume that responses to polls are not arbitrary, but the outcome of some information processing cognitive mechanism (cf. section 1.4), and that these processes are not essentially different from those effective when people decide what vote to cast. We assume, then, that polls and actual votes cast are both expressions of the same attitudes, though they are incommensurable measures of those attitudes.

In my conceptual scheme, long-term factors are conceived as relatively stable states providing context to causal mechanisms triggered by mass media events. Insofar as Bjørklund is right to invoke structural changes and new social cleavages, these will constitute (changed) contextual factors to mechanisms efficacious on the short-term. It is important to note, however, that there is nothing to prevent the triggering event of one mechanism, once it has occurred, to provide causal context to another. Indeed we

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<sup>26</sup> All references to polling data are averages of major polls, as calculated by Bernt Aardal: <http://home.online.no/~b-aardal/>

<sup>27</sup> The distinction between “party preference” and “party identification” is an analytical construct that possibly captures part of that deviation (see Aardal et al. 308-313).

<sup>28</sup> According to my calculations, the deviations between each party's average score on the polls in the last week preceding the election in 2005 and their share of the votes in the election itself varied between 0.2 (Venstre) and 3.1 percentage-points (Labour), or with 3.4 and 9.5 percent of their respective scores in the election. On average, the deviation was no more than 1.35 percentage points.

should expect that in some cases it will be hard to distinguish causal context from the causes themselves. Fundamentally, this has to do with the fact that there is no sharp real (ontological) difference between states and events.<sup>29</sup> In theory, contextual factors are distinguished from triggering causes in that they are present *before* mechanisms are triggered, without themselves triggering them.

As a matter of course, all changes in public opinion are sums of individual changes. Given explanatory realism, then, the explanation of net aggregate changes must refer to the most widespread explanations of individual changes. Long-term factors may change gradually on the individual level as well, such as growing up or changing media habits; other typically long-term factors occur quite abruptly on the individual level, such as moving from the countryside to a big city. Scepticism towards foreigners is a typically contextual factor evolving gradually on the aggregate level, but here is another route through which it may gain causal and explanatory import, namely through newspapers and television. When a deranged Somali refugee stabs a young man to death on a tram in Oslo or an Algerian asylum seeker kills his physician,<sup>30</sup> they become huge mass media events that set off a series of causal mechanisms that may abruptly alter the way many people think and talk about relevant policies. The effect of all this is commonplace, but it is often ignored that this is not inevitable. The effect is contingent upon innumerable contextual factors, such as existing sentiments towards African refugees, their perceived overrepresentation on crime statistics and the stances that political parties have taken on these issues. A murder in Norway by a deranged Brazilian immigrant might not have the same impact on public opinion, and in most Brazilian cities, a murder by a Somali refugee would probably have received much less attention.<sup>31</sup>

To some mechanisms, short-term factors may provide causal context. To illustrate this possibility, I shall postulate the hypothesis that the abovementioned murders in Oslo

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<sup>29</sup> “Phenomena” is a less precise concept than “states” and “events”, encompassing both these concepts.

<sup>30</sup> These incidents occurred in August of 2004 and in March of 2006 respectively.

<sup>31</sup> Rather unusually, Pipa Norris et al. takes this contingency into account when distinguishing “*conditional* effects” from “*actual* effects” (1999: 13-14).

created causal contexts with features significant in determining popular reactions to statements made by politicians in their immediate aftermaths. Let us assume that some Norwegian party politicians found themselves restrained by, say, the political incorrectness of accepting the premise that African immigrants constitute a particular violent group of people. These politicians may thus have been unable to express popular sentiments as effectively as politicians uninhibited by such a sense of political propriety. It seems plausible that the stabbing might have created a situation in which many Norwegians were tired of the political correctness of many party politicians. Such a state would have entered into a mechanism explanation as a contextual factor. Observers of Norwegian politics will concur that it is not a far-fetched idea that Frp politicians are less inhibited by political correctness than most other politicians.<sup>32</sup>

This conceptual mutability does not in my view constitute a paradox in the conceptual scheme that I am outlining, even though it aims at mirroring the real world dynamics of public opinion. If indeed it is a weakness, then it is one which is both unavoidable and possible to handle. We need to allow our concepts the flexibility to accommodate a fundamentally fluid social reality.

### ***3.2. Agenda-setting, Priming, Framing and Dynamic Issue Ownership***

In effect, significant short-term changes in public opinion are assumed to be triggered by information conveyed through the mass media. Aardal et al. assume, furthermore, that changes are essentially produced by the processes described by the concepts of “agenda-setting”, “issue ownership”, “priming” and “framing”. The studies concern changes occurring in the course of an election campaign, but I see no reason why theirs may not be considered a more general theoretical approach to short-term changes. While there are certainly features that make periods preceding elections exceptional contexts for attitudes to change – the most salient being the intensity of the information flow – they do not make these concepts inapplicable to other periods than

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<sup>32</sup> Gudmund Hernes makes a convincing argument to the effect that the Norwegian immigration policy was undemocratically established through a political consensus among parties other than Frp, and that this consensus was such that politically incorrect voices were subdued until they were expressed in the rhetoric of Frp (2006: 87-88).

the weeks preceding elections. I shall, accordingly, discuss this theoretical framework on the grounds of its ability to explain short-term changes in any situation.

The studies in Aardal et al. draw on substantial amounts of data on individual and aggregate changes in party preferences during the 2001 election campaign. According to these, the parties that gained most support during the campaign were the Socialist Left Party (SV) and Frp (Aardal and Berglund 2004: 308; 322). The only suggested explanatory mechanism is the two step process through which voters come to prefer a particular party by firstly having a more or less expressed opinion of *the priority of major political issues* (“personal agenda”) and, secondly, an opinion of *which party promotes the best policy* on those issues (“issue ownership”) or is *most concerned* with them (“issue-hegemony”).<sup>33</sup> The assumption is that these opinions are influenced essentially by the agenda set by the mass media and the way in which it frames the issues and political conflicts. The election campaign is conceived as a struggle to get onto the media’s agenda the issues on which they are judged most favourably by the voters, or, conversely, a struggle to be associated with popular solutions on issue areas already on the agenda.<sup>34</sup> In brief, this is the analytical framework in which Aardal et al. study changes in political party preferences (see Aardal et al.: 17-20; 289; 333 and 335).

*Issue ownership* was originally introduced as a “critical constant” in John R. Petrocik’s analysis (1996) of the American presidential campaign in 1980. It presupposes the theory of framing and priming, and that voters have made stable cognitive connections between particular parties, issues and policies. Presidential candidates are thus only able to influence the voters’ perceptions of which issues are in most urgent need of being addressed. In multiparty systems, issue ownership is thought to be more dynamic. In analyses of the Norwegian electorate, the concept has

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<sup>33</sup> The concept of “issue-hegemony” is introduced by Anders Todal Jenssen and Toril Aalberg (2004: 335; see section 3.2.1 below) on the assumption that an individual could prefer the solutions of one party while believing that another party is more preoccupied with the issue.

<sup>34</sup> On the individual level, statistical data show that people more frequently change the party in which they have most confidence on a given issue, than what issues they consider most important (Aardal et al.: 290).

been incorporated as a “critical *variable*”. Studies focus on how it changes (Aardal et al.: 18; 287-290 and 334).

The concept of “framing” denotes the way in which media present the issues. Framing is thought to be explanatory significant through its role in “priming” the voters to emphasise certain standards when evaluating the parties, politicians and policies. Narud and Waldahl (2004a and 2004b) do provide an analysis of the media content and the extent to which it primes the voters. This analysis does not, however, enter into any tentative explanations of why the SV and Frp won the campaign. Other than the obviously distinct effects of perceived positive and perceived negative framing (cf. Norris et al.: 137-141), the studies do not suggest any mechanism through which particular parties benefit from particular ways of presenting the issues – though we may detect a hint of the conventional wisdom that Frp benefit from a generally more superficial and “episodic” rather than “thematic” framing of the issues, i.e. an emphasis on particular consequences of policies rather than abstract effects on society (Aardal et al.: 21).

The mass media’s role in setting the political agenda, according to Aardal and Waldahl (2004: 280), is well established, but in the course of the campaign in 2001 it hardly seemed to change at all.<sup>35</sup> The campaign did, however, have an effect on *issue ownerships*. First of all, it increased awareness of the parties’ respective solutions (ibid: 208). Asked which party they thought had the best policy on nine given issue areas, the percentage of respondents answering that they did not know, decreased significantly in the course of the campaign (ibid: 283). This was particularly true for the issue areas that were most prominently featured on the media’s agenda. The Socialist Left Party (SV) increased its popularity on all nine issues, though only insignificantly on some. Frp strengthened its position on health and geriatric care. Høyre’s perceived ownership of the issues that came to dominate the campaign,

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<sup>35</sup> Norris et al. reached the same conclusion about the 1997 campaign in Great Britain (1999: 183). Agenda-setting effects have mainly been detected in the USA (ibid: 1982). The idea that the media is more effective in telling people *what to think about* than *what to think* is traceable to Walter Lippman’s Public Opinion (1922), according to Norris et al. (1999: 16).

namely *tax* and *elementary education*, as well as *heath* and *geriatric care*, decreased slightly in the course of the campaign – but Høyre enjoyed such a strong position on those issues at the outset so that it was still among the perceived winners of the election. As for the perception of other parties, there were only insignificant changes. From this Aardal and Waldahl conclude that the campaign had only a small effect on the voters’ assessment of the parties’ issue ownerships, but that significant changes *could* occur, as demonstrated by SV and Frp (ibid: 284).

Importantly, the studies do not establish any unequivocal correlation between the changes in the voters’ assessment of the parties’ issue ownerships and the media’s relevant coverage, i.e. the extent of it and whether articles or news bits were positively or negatively framed (ibid: 200 and 284). This is a particularly serious failure, inasmuch as the framework presupposed that any explanation of changes in party preference could be traced back to the media coverage. The studies established that the increased support of the SV and Frp coincided with a certain gain in the confidence that the public expressed in them on important issues, but they cannot identify a way in which media coverage may have contributed to this. Nor are the studies able to discard the notable possibility that increased support and increased ownership on certain issues are both effects of another confounding factor. In conclusion, the correlational method as applied by Aardal et al. failed to determine causes and to identify causal mechanisms. Therefore it fails to explain why these parties “won” the election campaign. The authors themselves admit as much in their introduction:

“Even if the analyses of the previous chapters shed light on important aspects of the interaction among parties, the media and voters: the question of causality remains moot. We have asserted but not shown that voters are influenced by media politics.” (Aardal et al. 2004: 28; my translation.)

### 3.2.1. A Variable-centred Quasi-Experiment

In order to address questions of causality, *I valgkampens hete* rounds up by recapitulating two quasi-experimental studies conducted by Anders Todal Jenssen and Toril Aalberg of the University of Trondheim. The experiment most relevant to the topic of this paper was set up in order, they say, to identify the effects of agenda-setting, framing and priming in the actual campaign and to “increase our understanding” of the causal mechanism involved (Jenssen and Aalberg 2004: 336). They wish to achieve this by measuring changes in the attitudes of high school students watching two televised party leader debates on economic policy and education respectively.

The setting of the experiment is justifiably described as quasi-experimental inasmuch as students were interviewed immediately before and after watching the debates live in a university auditorium, thus tentatively eliminating all other influences than those of the debates themselves.<sup>36</sup> Participants were randomly divided into two groups, one of which watched the debates, while the others were shown irrelevant entertainment movies. Effects were measured by comparing aggregate changes within the two groups. This so-called *non-equivalent control group design* seems to ensure that the study measures only the effects of the debate (cf. Oskamp and Schultz: 210). The fact that the debates were shown live, seems to ensure the findings strong external validity.

Students were asked questions pertaining to possible changes in four attitude dimensions: (i) their *awareness* of the issues debated; (ii) their perceptions of which political party were *most concerned* with those issues; (iii) their perceptions of which party had the *best policies* on those issues and (iv) whether the students became more disposed to *hold the government responsible* for the current state of affairs. The theoretical concepts guiding the experiment are familiar: *agenda-setting effects* (i); *issue-hegemony* (ii); *issue ownership* (iii) and *priming/framing* (iv). The latter

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<sup>36</sup> "[Experimental studies] are studies in which researchers achieve a high degree of planned manipulation of variables, and participants are randomly assigned to experimental conditions. [In Quasi-Experiments] researchers may have partial control over the independent variables, but may not be able to assign participants randomly to conditions" (Oskamp et al.: 210).

dimension was included on the assumption that the debates would to a large extent concern the record of the current government.

Todal Jenssen and Aalberg wish to flesh out these concepts by going back to their roots – that is, defining them in terms of psychological theory on *cognitive schemas*. A “cognitive schema”, according to a standard definition within public opinion research, is a “hierarchical organisation of information within a particular domain” (Lau and Sears 1986: 349). The fundamental idea is that our perception of the world is guided by various such cognitive schemas, which categorise information in the brain and suggest causal- and other types of relationships among categories of events and people – in short, how everything in the world is connected (or not connected). Of particular importance is the assumption that cognitive schemas decide what information is most readily accessible to the perceiver, i.e. which considerations comes first to people’s minds in any given situation and is thus taken into account when they make up their opinions. In psychological literature, this is called an “accessibility bias” (Iyengar 1990).

Available schemas are activated by cues in the environment. “Priming” is the technical term for such activation. The studies in *I Valgkampens hete* proceeds on the assumption that people are essentially primed by how political issues and actors are framed in newspaper articles and on television. In particular, the media is seen as instrumental in deciding the criteria on which people assess politicians and policies, either by giving cues activating schemas that people already possess, or by providing readymade frames internalised as schemas by people whose attitudes are consistent with them. Thus Shanto Iyengar and Donald Kinder assert that “priming refers to the changes in the standards that people use to make political evaluations” (quoted in Jenssen and Aalberg 2004: 329).

Given these references to schema theory, it would seem that the experiment is founded upon a comprehensive body of well tested theory on a lower level than that on which the problem is formulated – just as prescribed by the mechanism approach. Indeed, it



is the stated objective of Jenssen and Aalberg to suggest how insights from cognitive psychology can “make the concepts above more fruitful” (ibid: 328), and they devote several pages to rendering them (ibid: 328-336). Effects of agenda-setting and framing are thus conceived as the activation of particular cognitive schemas or parts of such schemas (ibid: 329). Furthermore, debates may also influence the schemas themselves. The experiment focuses upon a specific type of framing, namely the attempts made by politicians to associate themselves with particular issues and solutions. This particular brand of priming is thought to produce the “*association effects*” that in turn decide issue ownerships and issue hegemonies.

But theoretical explication alone is not enough to “increase our understanding” of causal mechanisms that are efficacious in a particular context. We need to apply what we believe to be known of them in designing experiments intended to identify them. We will have to put to the test the finest grained theory available in order to determine whether these mechanisms – as they are theoretically described– are truly the ones mediating the stimuli of the debates into effects on the participants in the experiment. Clearly, the causes that make the attitudes of viewers change are to be found among the specific statements and the general behaviour of the participants in the debates. It is also plain that their effects will depend on the viewers’ prior cognitive wiring and other personal characteristics (not only opinions on the four abovementioned dimensions). According to the schema theory, the debates will provide cues activating or entrenching the viewers’ ideas of how the world works. Possible hypotheses might include the idea that particularly passionate articulations may sway viewers in either of two diametrically opposite directions, depending on their prior attitudes. The attitudes of viewers already sympathetic to the politician may become more entrenched, while unsympathetic viewers may find the pathos sickening. In technical terms, the effect will depend on the viewers’ prior *role* or *person schema* (see Milburn: 73-75).

In this experiment, concepts of cognitive psychology are simply used to denote empirically identified aggregate changes. They have no role in steering the experiment towards identifying the mechanisms that bring these changes about. Notably, the

concept of “priming” does not generate any testable hypotheses concerning the cognitive processes involved. It refers simply to the total aggregate influence of the experimental condition (the televised debates) in changing the way the viewers associate particular issues with particular parties. The concepts of “association effects”, “issue ownership” and “issue hegemony” are simply different names for these same aggregate changes. The experiment does not break the debates down to particular possible cues triggering mechanisms of priming. Each debate is treated as a single unified cause, and the hypotheses on which the experiment proceed are simply to the effect that the viewers are generally influenced in the direction of the politicians they find most convincing. Any number of mechanisms may have been responsible for this effect.

My main misgivings about the experiment, then, are as follows: it does not properly analyse the debates themselves; it does not include into the statistical analysis a sufficient number of personal characteristics and it measured only aggregate changes in the samples. In spite of a quasi-experimental design, it applies fundamentally correlational methods, and even though the authors take pains to explicate the cognitive mechanisms of schema theory, they end up applying a typically variable-centred theoretical framework in which concepts simply denote aggregate changes. Consequently, the study fails to yield sufficiently fine grained hypotheses to test the efficacy of particular mechanisms (cf. section 4.3 below). In order to have done so, the empirical implications of its theoretical description would have had to be formulated in such a way that the experiment may have ruled out plausible alternative mechanisms. In section 6.3.1 I shall sketch an experimental design better suited to identify particular causal mechanisms.

### ***3.3. Concluding Remarks: A Deficit of Causation***

In this section I have defined short-term changes in public opinion as being triggered by events conveyed by mass media. I have argued that the first systematic Norwegian effort to explain such short-term changes was essentially variable-centred in its

approach. Even quasi-experiments studies applied essentially correlational methods. Consequently, these studies failed to tell us *why* or *how* people's attitudes change. In the next section I shall outline a fundamentally different approach to explaining short-term changes and in section 5 I shall try to translate that approach into a more concrete research strategy.

#### **4. A Mechanism-centred Approach to the Study of Attitude change**

Before possibly explaining short-term changes in public opinion, they will have to be disclosed – some might say *generated* (Zaller 1992: 28-30; 76) – by surveys. The research strategy that I am proposing proceeds from established facts of public opinion, trying to identify the causal mechanisms that brought them about. Remember the typical structure of a mechanism explanation from section 1.2.1: *Event(s) e (or e1, ... en) caused/did not cause (an aspect of) explanandum x through mechanism m (or m1, ..., mn), because condition(s) c (or c1, ... cn) applied.* Within the statistical approach, the first part of this structure would by itself constitute an explanation: *x was caused by e*; while in a mechanism explanation, statistically established causes are as much part of the explanandum as they are part of the explananda. It is the mechanisms we identify (the *how*) and the explication of the essential conditions for the efficaciousness of those mechanisms (the *why*) that do the explaining.

In section 2 I argued that an account of *how* and *why* something occurs is intrinsically desirable. In this section I shall clarify what I hold to constitute such accounts and discuss what we need to know in order to justifiably assert that we have obtained them, thereby addressing question two (*Q2*) in section 1.1. First I shall concretise the concept of a “causal mechanism” by suggesting categories and examples of mechanisms. In section 4.2 I shall go on to clarify the sense in which mechanisms explain. Having done so – while keeping in mind the realist demand for reference in explanations, as explicated in section 2 – I am in a better position to discuss what information is required in order to justifiably assert that a (context independently) established mechanism really is effective in a particular context. This is the second goal (*G2*) in

section 5 below. In section 4.3 I shall make the argument that obtaining such information significantly reduces the risk of spurious explanation. How we should go about trying to obtain that information is the topic of section 5.

#### **4.1. Causal Mechanisms**

To remind ourselves, *mechanisms* are composed of *intersecting causal processes* (see sections 1.2.2 and 2.1.2) that make up recurrent patterns. There is nothing to prevent the processes of one mechanism from intersecting with processes that are part of other mechanisms. What processes constitute a mechanism is a matter of definition. A set of intersecting mechanisms may themselves make up a recurrent pattern that we may choose to define as a (larger scale) mechanism. I think these would be what Jon Elster calls “*molecular mechanisms*” (1999: 32). I cannot think of any universal criterion upon which we may always decide whether a set of intersecting causal processes constitute one single molecular mechanism or several simpler ones. Fortunately, there does not seem to be any immediate need of such a criterion, just as long as the definitions of particular mechanisms are sufficiently precise and well-founded. The crucial requirement is that we know what to look for when trying to identify effective mechanisms, i.e. that we have obtained descriptions of recurrent patterns that allow for reasonably unequivocal empirical implications

In section 1.2.1 I pointed out that my definition of “mechanisms” did not specify a level on which changes in public opinion are to be explained, though it may frequently be the case that explanatory mechanisms are identified in lower level sciences, such as evolutionary theory, cognitive psychology or even neurophysiology. Mechanisms manifesting themselves on an aggregate level may in many cases be reduced to descriptions in these sciences, such as when Hedström and Swedberg suggest that the sociological theories of *self-fulfilling prophecies* and *threshold-based behaviour* are both founded upon the same cognitive mechanism of belief formation by inferring the beliefs of others (1998: 18-19). Other mechanisms may be irreducibly social in that the causal patterns are essentially interpersonal. This will be the case if mechanisms

include references to interactions among different types of personalities or individuals occupying different social roles, as does the two-step process wherein so-called “opinion leaders” are supposed to receive messages and pass them on to others in their personal networks (Katz and Lazarsfeld 1955). Thus I propose a distinction between *intrapersonal* and *interpersonal* mechanisms. While there is no reason *in principle* that we should be unable to reduce interpersonal mechanisms to descriptions in lower level sciences, we should expect them to resist reduction to the extent that interpersonal causal processes are more complex and thus harder to describe in lower level terms.

It is important to note, however, that – given explanatory realism – any mechanism explaining behaviour (such as expressed attitudes) must be physically realised in the brain of individuals. This is the only way it can be causally efficacious in bringing that behaviour about. Obviously, a mechanism generating the diffusion of attitudes through “opinion leaders” will consist of a conglomerate of single mechanisms. Whether such complex patterns are reasonably described as one mechanism depends on the identifiable recurrence of that pattern and whether its theoretical description is “based in principle on defensible micro level processes that explain individual behaviour” (Bennett 2003; cf. section 2.3 above).

#### **4.1.1. A Typology of Mechanisms**

Based on James Coleman’s (1986) macro-micro-macro model for conceptualising collective social action, Hedström and Swedberg (1998: 21-23) distinguishes three categories of mechanisms (the first parts of the parentheses refer to the level on which the events triggering the mechanisms occur; the second part to the level on which the effects are best observable): (i) *situational* mechanisms (macro-micro); (ii) *action formation* mechanisms (micro-micro) and (iii) *transformational* mechanisms (micro-macro).

The idea is to emphasise the cyclic interdependence between phenomena on the micro- (or individual) level and the macro- (or *aggregate*) level. While it remains true, as I

have emphasised, that all aggregate phenomena are the sum of individual phenomena, they may have causal import in virtue of their effects on the aggregate level, through those being perceived by individuals. All this is nicely illustrated by the mechanism of *threshold-based behaviour* (ibid: 294), which in my view is best described as a *macro-micro-macro-mechanism*. It may reasonably be said to be triggered by macro-phenomena, as when, say, the number of visitors to a restaurant – or, strictly speaking, *the perception* of its popularity– causes an individual to sit down and eat at that restaurant. The cognitive mechanism that generates a preference from the observation of its pervasiveness is of course fully realised on the individual level, but its outcome as described by threshold models of collective behaviour is manifested only on an aggregate level. I shall return to this example when discussing ways of identifying such irreducibly social mechanisms in section 4.3.

Hedström and Swedberg include all mechanisms “[linking] a social structure or other macro sociological states to the beliefs, desires and opportunities of individual actors” in the category of “situational mechanisms” (ibid: 297). The concept of “situational” seems to be derived from Karl Popper’s notion of “situational analysis” and as such it is slightly misleading. To Popper the concept was near synonymous with *rational choice theory* (Hausman 2006: section 4.1), but in Hedström and Swedberg’s schema, mechanisms of rational deliberation would more naturally belong to the category of *action formation mechanisms* (ii), which include “the plurality of psychological and social-psychological mechanisms,” such as Leon Festinger’s mechanism of “cognitive dissonance reduction” (1957), through which people are thought to adapt their beliefs to their behaviour, as opposed to the more conventional idea of a contrary relationship. In my two-fold schema, the salient feature of these mechanisms would be that they are fully manifested in individual persons, and thus they are likely candidates for reduction into physiological descriptions.

*Transformational* mechanisms are those that produce a distinctive aggregate outcome from a set of individual actions: “Here a number of individuals interact with one another and the specific mechanism (which depends upon the type of interaction)

*describes* how these individual actions are transformed into a collective outcome” (ibid: 298; my emphasis). The prototypic transformational mechanisms are those described by *game theory*. Interestingly, Hedström and Swedberg refrain from asserting that such mechanisms *explain* the collective outcomes. Most agree that game theoretical models cannot themselves explain the outcomes they predict. In order to assert otherwise, as Don Ross points out (2006: 2.1): “we must suppose that agents at least sometimes do what they do [...] *because* game theoretic logic recommends certain actions as the rational ones” (my emphasis). There is little reason to believe that such strict reasoning is widespread. In order to explain a collective outcome with reference to only one causal mechanism, we need evidence to the effect that this mechanism was effective in a significant portion of the individuals involved.

#### **4.2. Two Explanatory Roles**

Roughly corresponding to, but not strictly depending on, this level-based categorisation of mechanisms we should expect a shift in the relative importance of the two non-mutually exclusive explanatory roles assigned to mechanisms in section 1.2. Some mechanisms – mostly *micro-micro* mechanisms, I expect – explain essentially in the sense that they “reduce” the causal relationship under investigation to component causal processes among more specific states and events described in terms of a lower level science, thereby explicating how and why the effect came about. This is what Elster thought of as opening up the “black box” between correlating variables (cf. section 1.2). Other mechanisms explain by *generalisation*, which is to identify, between the phenomena contained in the explanans and the explanandum, a pattern of causal processes that, given a set of conditions, are commonly associated with this type of phenomenon (or so we have reason to believe). In this case, according to Elster, “the mechanism provides an explanation because it is *more general* than the phenomenon it subsumes” (1999: 6; original emphasis). I shall argue that they are explanations fundamentally in virtue of their successfully appealing to (patterns of) causal processes with which the inquirer is *familiar*.

While explanation by generalisation is possible without reduction, it seems that reduction always involves generalisation. Hedström and Swedberg convincingly assert that “simply making up an ad hoc story tailored to a specific case does not constitute an acceptable sociological explanation” (1996: 289). Indeed the very notion of causal mechanisms suggests that reduction is by definition accompanied by generalisation, inasmuch as mechanisms are recurrent patterns, not uniquely occurring events. There are good reasons to believe that such patterns exist (cf. section 1.2.1), and the more we know about the causal processes of a mechanism, the more widely we shall be able to apply it. I propose to accommodate both of Elster’s notions (cf. section 1.2) in my concept of “explanatory mechanism” by arguing that reduction is in effect finer grained generalisation; the upshot being that we may explain without reduction, but not without generalisation.

Jon Elster asserts, correctly in my view, but also quite vaguely, that: “Explanation by mechanisms works when and because we can identify a particular causal pattern that we can recognise across various situations and that provides an intelligible answer to the question, ‘Why did he do that?’” (1999: 10). As far as it goes, this is perfectly compatible with both explanatory roles that I am attempting to define, but the exact nature of an “intelligible answer” may be a matter of personal opinion. It seems easier to say in the case of reduction, where an intelligible answer is obtained by explicating the causal processes between relevant events. The insights thus obtained are much like those we would get from opening up an electronic device and having someone point to the component parts, telling us their respective functions. The inquirer may, of course, keep asking further questions about smaller processes until the explicator has no further answer. It seems ultimately a matter of subjective taste when to stop asking. We should expect this taste to evolve with the introduction of new methods of observation and new evidence on lower levels of analysis. As I shall argue in section 4.3, the crucial requirement is not that explanations identify causal connections on the lowest possible level, but that they be *in principle consistent* with, as Andrew Bennett (2003) puts it, “the most continuous spatial-temporal sequences we can describe at the finest level of detail that we can observe.”



Reduction, of course, presupposes a lower level science to which we may reduce. Certain disciplines within physics have no possibility of explaining through reduction, simply because there exists at present no lower level science. There is a sense in which possibly irreducible interpersonal mechanisms are similar to irreducible laws of physics. In both cases we need to assume an unobservable chain of events that – for want of methods or even concepts – we cannot identify. One important difference is the lack of universality in social regularities. The fact that lower level physical laws are usually perfectly universal constitutes a reason to believe that they have hit upon causal connections on such a fine grained level that the posited mechanism is the only one possible to connect the phenomena in question. When we explain social regularities, on the other hand, we will need evidence to identify the effective mechanism(s) among several possible candidates.

Pinpointing the essence of explanation by generalisation is slightly more difficult. In my view they must always appeal to the sense that a particular causal connection fit into a *familiar* pattern. I am unable to define very precisely this concept of “familiarity”. It is intended to capture the epistemological contentedness that stops an inquirer from asking further questions. As such it depends crucially on that inquirer’s prior knowledge (cf. requirements for *explicitness* in section 1.3). The perceived intelligibility of invoking the mechanism of, say, *wishful thinking* to explain a particular action, without further explication of the neurological causal processes, depends on whether the perceiver is familiar with the mechanism through personal experience. This sort of explanation goes on implicitly among human beings all the time.<sup>37</sup> Without it, we would be unable to interact as we do. In a proper mechanism explanation, however, the mechanism need to be explicit and we must ensure that it actually refer to effective mechanisms by applying some sort of scientific method. Bennett’s requirement of lowest level consistency applies to higher level

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<sup>37</sup> The set of assumptions guiding this most significant of human abilities, namely to make other people’s actions intelligible, is referred to by brain scientists as our “theory of mind” (Carr 2006: 9). There is a potential distinction, as Raino Malnes pointed out to me, between the familiarity involved when we immediately recognise the motives for an action (through this innate theory of mind) and the familiarity that comes with *having experienced* the same thing. Both types of recognitions will leave us satisfied that we have an explanation and both require further evidence in order to qualify as *scientific* explanations.

generalisations as well as to reduction. The difference is that appeals to familiarity do not require the identification of particular processes. We need not know whether the mechanism of *belief formation by observing the beliefs of others* is evolutionarily conditioned or culturally learned in order to invoke it for explanatory purposes, just as long as the aggregate empirical outcome is sufficiently distinct to be identified in various contexts as being produced by one and the same mechanism and that mechanism is *consistent* with lower level sciences.

One particular category of posited micro-micro – or possibly macro-micro – mechanisms deserves special attention in this discussion. The *evolved psychological mechanisms* or *modules* of evolutionary psychology are defined by their *functions* rather than their physical manifestations. They are “adaptations, constructed by natural selection over evolutionary time” producing behavioural responses to given environmental stimuli (Barkow et al. 1992: 5). As all systems processing information, they may be described in at least two different ways: either by how their physical components interact; or by characterising the input on which they operate, the algorithm through which they process that input and the output they generate (ibid: 7). One and the same information processing mechanism might thus have various physical manifestations and its description might be highly fine grained and perfectly testable without referring to particular physical entities.

According to my concepts, a functionally defined mechanism might only generate explanations by appealing to the familiar. That which we call the “function” of an evolved psychological mechanism is not something physical, but a concept that summarises the adaptive advantages that the various physical manifestations of the functionally defined mechanism have bestowed upon individuals possessing them. If a mechanism cannot be defined by a distinct pattern of causal processes, it remains a linguistic construct that depends on our familiarity with the function it performs in order for us to accept an explanation with reference to it.<sup>38</sup> Evolutionary psychology

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<sup>38</sup> Barkow et al. draw a distinction between neuroscience and cognitive psychology that would classify all explanations of the former as *reductions* and all of the latter as *generalisations*: “A neuroscience description characterizes the ways in which its physical components interact; a cognitive, or information-processing,

aims to provide an account of “how and why the information processing organization of the nervous system came to have the functional properties that it does” (ibid: 8), and I will say that I cannot think of a more satisfactory explanation for my behaviour than the identification of mechanisms with demonstrable adaptive advantages to the environment where humans had time to evolve (the *Pleistocene* era, according to evolutionary psychologists).

When explaining by appealing to the familiar, then, it is not necessary that we identify and describe the particular causal processes and intersections involved in the production of a phenomenon (a full account of the *how*), but even when we are unable to do so, we still need to know something about the conditions under which the mechanisms produce given phenomena. This is because the ultimate effects of triggered mechanisms depend on the causal context in which they are triggered, including their interaction with other mechanisms. Appealing to a familiar mechanism, therefore, is only necessary, but not sufficient to explain their effects (cf. footnote 7 on Van Fraassen in section 1.2.1 and quotations from Sayer 1992 in 2.1.1). In this respect, the difference between reduction and generalisation is that in the former case these conditions are contained in the very description of the mechanism’s constituent processes, while in the latter we may have to infer them from our understanding of the mechanism and corroborate them without knowing the particular processes. Significantly, if the conditions are not in any way made intelligible by theory of the mechanism itself, we have merely a law with *ceteris paribus* clauses. The requirement of such clarification makes explanatory mechanisms fundamentally distinct from *laws*, which by themselves imply their effects with necessity (cf. the “ontic” vs. the “modal” and “epistemic” theories of explanation in section 2).<sup>39</sup>

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*description characterizes the 'programs' that govern its operation. In cognitive psychology, the term mind is used to refer to an information processing description of the function of the brain [...]*” (1992: 7).

<sup>39</sup>Studies of the relation between attitudes and behaviour made by cognitive psychologists in the 1980s were prototypic collective scientific efforts to develop such conditioned mechanisms, proceeding from the crucial question: “Under what conditions do what kinds of attitudes held by what kinds of individuals predict what kinds of behaviour?” They made considerable progress that would pave the way for the integration of various theoretical perspectives (Sorrentino et al. 1986; cf. “mechanism theory” and “theoretical integration” in section 5).

Fundamentally, the relative importance of these two explanatory roles does not depend on the category of mechanism, but on the type of data that is available about the mechanism. In the case of *wishful thinking* – I am sure – we would be able to draw on a comprehensive body of fine grained theory in cognitive psychology and neurophysiology. To the extent that we have the means of obtaining sufficiently precise data to test the empirical implications of that theory in a particular context, we may be able to explain the belief by identifying the chain of causal processes. Where such theory or data is not available, we will have find alternative means of establishing that the mechanism with which we are familiar is indeed effective in the individual that we study, as discussed in section 4.3.2.

### **4.3. Avoiding Spurious Explanation**

Whether the explanation is mainly by reduction or generalisation, then, we must have obtained evidence to the effect that we have identified a real and effective mechanism. It is not enough to establish correlation or the presence of a conventionally posited cause. As in the case of wishful thinking, when someone desire that which he erroneously believes to obtain, he might just as well be making a mistake that, by coincidence, leads to the preferred conclusion. Further investigation is necessary in order to justifiably conclude that we are indeed dealing with the mechanism of wishful thinking.

#### **4.3.1. Establishing General Reference**

First of all we need to know that the mechanism we have posited really exists. This is a significant further step from the conventional statistical requirements for asserting causal connections. If there is no known mechanism making desires the causes of corresponding beliefs, researchers seeking a mechanism-based explanation will have to suspend their judgement. If the posited mechanism is positively demonstrated not to exist, the proposed explanation must be discarded. Having committed themselves to explanatory realism, no amount of statistical evidence will make any difference. As Andrew Bennett (2003) points out:

“In contrast to Friedman’s view, researchers seeking to explain phenomena via causal mechanisms must frankly acknowledge that their theories are in trouble if it can be shown that the mechanisms their theories posit are not consistent with the observed processes at the next level of analysis down.”

I assume that Bennett does not merely require *logical* consistency – which would exclude few explanations – but also that *plausible (spatiotemporally continuous) causal accounts* be available (cf. section 2.3). The following example provided by Hedström and Swedberg is illustrating: Epidemiological studies have found empirical association between exposure to low frequency electromagnetic fields and childhood leukaemia, but the correlation fails Bennett’s consistency requirement, because there is no known biological mechanism that can possibly explain how magnetic fields induce cancer. If, furthermore, it were known that any such mechanism would violate established physical principle, the posited explanation would also fail the weaker requirement of *logical* inconsistency – which in this case it has indeed been suggested that it does (1996: 287). If it were well established common knowledge that electromagnetic fields affect human cells and that *given certain conditions* it induces cancer, but that the exact mechanism through which it does so was not identified, then, in my view, it would qualify as an explanation by *appeal to the familiar* – once, of course, possibly confounding factors had been excluded. If, furthermore we could identify and explicate the particular causal processes, we would have explanation by *reduction*.

Arthur Stinchcombe (cf. section 1.2) as we have seen, follows Friedman in asserting the view that mechanisms do not need to refer to real processes in order to be useful. An explanatory model need only describe adequately the observable dynamics within its domain. Writing on the assumptions of economics, he states that:

“[As long as] the mechanism of rational choice [can be used] to derive hypotheses about how the market works, [it does not matter that] the

assumption of rationality can be refuted as a training exercise by any promising young cognitive psychologist” (1993: 25).

In my opinion, Stinchcombe fails to distinguish between at least two senses in which assumptions may be false. They may either posit entirely *fictitious* mechanisms or they may just be *simplifications* of mechanisms that really exist. In the case of rational deliberation, we do not know which the case is. It might just be that our emotional responses sometimes make us behave in approximately rational ways. Indeed, brain scanning has revealed that certain decisions are made before we are consciously aware of having made up our minds. Such findings would seem to severely undermine the possibility of efficacious rational deliberation (Carr 2006: 12). If, on the other hand, there exist a cognitive “module” (cf. Barkow et al. 1992 and Fodor 1988) determining behaviour by weighing pros and cons in a complex manner hitherto not mapped, assumptions of rationality might be acceptable simplifications. Such simplifications are acceptable insofar as the abstractions involved do not make the descriptions so context-specific as to make the theory difficult to carry across disciplines (cf. section 5.1).

When in the introduction to section 2 I asserted that mechanism approach would significantly reduce the risk of spurious explanation insofar as it managed to obtain support from relevant findings across *a broad range of contexts*, I was thinking of this interdisciplinary enterprise of establishing mechanisms independently of particular contexts (the “mechanism theory” of section 5.1 below). This requirement amounts to more than just raising the bar for statistically inferring causation and then claiming they are more probably true explanations. I am advocating the consideration of information entirely distinct from statistical evidence, in part, as I have argued, because it will make any given statistically significant correlation less likely to be spurious; and in part because of its potential role in guiding empirical research, as discussed in the following subsection.

### 4.3.2. Establishing Particular Reference

I mentioned in section 1.2.1 that social phenomena are usually the results of innumerable causal mechanisms interacting in indeterminable ways. This is why mechanisms are not fit for purposes of prediction. Only *ex post facto* may we try to identify the mechanisms that were most important in producing the explananda (cf. section 1.2). According to Jon Elster, mechanisms often come in pairs with opposite effects (1999: 6). He distinguishes two types of such pairs according to how the indeterminacy of their net effect arises. Type A occurs when “the indeterminacy concerns which (if any) of several causal chains will be triggered”, while type B arise “when we can predict the triggering of two causal chains that affect an independent variable in opposite directions, leaving the net effect indeterminate” (ibid: 3).

These categories illustrate the range of possibly confounding factors in a statistical causal analysis. Plainly, we cannot hope to identify actually effective mechanisms in this jungle without theory guiding us to disparate empirical implications of the various possible mechanisms. This is why we want theories describing mechanisms to be as “fine grained” as possible, which is to say that they deal with entities, states and events defined as narrowly and as clearly as possible, so as to limit the number of possible causal processes connecting these states and events. Many causal mechanisms may account for a correlation between the general level of education and a simple preference for Frp – whatever its motivation. Fewer possibilities exist between any particular reason for uttering a preference for Frp and, say, a more narrowly defined type of higher education; a specific feature associated or correlated with it, such as an ability to critically evaluate political statements; any socialisation involved in attending a university or a stronger “need for cognition” (cf. Oskamp and Schultz: 223). At any given degree of fit between data and theory, the more fine grained the theory and preciseness of observation the better justification for believing that we have identified real mechanisms.

When fine grained lower level theory is not available or we cannot obtain the data necessary to test it, the desirability of context independently established mechanisms

still remain, as do the requirement that plausible alternative mechanism and possibly confounding factors are discarded. We still need to infer and identify empirical patterns with as little likelihood as possible of being produced by other mechanisms. One way of achieving this is by including as many relevant factors as possible in a statistical analysis. Theories of mechanisms shall include statements of *why* the mechanism is efficacious in certain contexts and not in others (cf. sections 2.1.1 and 2.3). From such theory we may infer empirical implications concerning a number of intervening and contextual factors.

This strategy would perhaps require the inclusion of more factors than today's techniques for statistical inferences are designed to handle. The number of respondents in a survey restricts the number of factors that can be included in the causal analysis. For each extra variable we wish to keep constant, fewer respondents remain to establish the causal connection we are trying to isolate. Elster's categories suggest (1999: 6), moreover, the possibility that causal connections are such that ordinary cross tabulation either cannot identify their effects (type B), or cannot distinguish the actual cause from a spurious cause (type A). We should realise that introducing the concept of "mechanism" – and the causal processes they describe – into the explanation of complex social phenomena, involves methodical challenges commonly avoided by social scientists, and they are avoided for a reason. Several techniques for handling those challenges are suggested in section 5.3.2 below.

#### **4.4. Concluding Remarks: Avoiding Spuriousness by Specificity**

In this section I have attempted to further justify the contention that mechanism-based explanations of changes in public opinion are superior to the traditional practise in social science of explaining by making assertions of causation based solely almost solely on statistical covariance. In section 2 I presented a philosophical argument to the effect that they are intrinsically better explanations in the sense that they include an account of how and why the cause produced the effect. In this section I have argued that context-independently established mechanisms may reduce the risk of spurious



explanations by, firstly, ensuring that there is a plausible mechanism and, secondly, by inferring more precise empirical implications. In the following section I shall propose methods to obtain such explanations.

## 5. A Mechanism-centred Research Strategy

I expect many social scientists would subscribe to the desirability of finer grained causal theory, but then go on to lament, as does Jon Elster, that such theory might not be available in the study of complex phenomena such as collective human behaviour. Indeed it might not, but if social science is to achieve the best theory possible, surely it needs to start taking into account relevant insights from lower level disciplines such as evolutionary theory and neurophysiology whose tremendous explanatory force keeps instilling the idea that we are, albeit magnificently complex, fully physical products of evolution. Social theory developed in detachment from these insights might adequately describe aggregate variation, but it will not even in a simplified way mirror the underlying causal patterns producing these variations, barring by accident. From the mechanism-centred approach to explanation I shall sketch a research strategy with a fundamental aspiration towards increased *interdisciplinary exchange*<sup>40</sup> and *conceptual integration* – i.e. “the principle that the various disciplines within the behavioural sciences and social sciences should make themselves mutually consistent and consistent with what is known in the natural sciences as well” (Barkow et al. 1992: 4).<sup>41</sup>

The conceptual fragmentation of social science is not something inherent; it is not entailed, as some might say, by the fact that its objects of study are thinking human beings. Conceptual fragmentation is the main obstacle social science need to overcome in order to become a more collective enterprise and come closer to achieving the level of knowledge accumulation that has made the natural sciences so successful. In section

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<sup>40</sup> The inherent interdisciplinarity of the mechanism-centred approach is also pointed out by Hedström and Swedberg (1998: 2).

<sup>41</sup> *Conceptual integration* within the social sciences (also called “vertical integration”) is thoroughly and convincingly advocated in the introduction to *The Adapted Mind* (Barkow et al. 1992). The merits of conceptual integration are independent of the merits of evolutionary psychology.

5.1 I suggest that the development of “mechanism theory” is the way to go to achieve this. In sections 5.2 and 5.3 I discuss some methodical problems that arise from these increased explanatory ambitions.

Interdisciplinary exchange is naturally promoted by the emphasis on identifying mechanisms inasmuch as descriptions of mechanisms frequently cut across disciplinary boundaries. Also promoted by the mechanism approach is the strategy of *problem driven research*, according to which the researcher set out to *answer questions* rather than *testing theories*, thus warranted to conduct inquiries independently of disciplinary boundaries. As Andrew Bennett (2003) points out, the focus in political science has been on testing which of various contending schools of thought best describe particular political environments. While this may have been useful in order to clarify these alternative theories, Bennett believes it has diverted attention from more productive “empirical puzzle solving and problem driven research.” Consequently, general scientific progress has suffered.

The present challenge, according to Bennett is “to allow for complexity and eclecticism at the level of causal explanation [...] while still maintaining sufficient structure in the field to retain a common set of concepts [...] to communicate within and across subfields.” I believe we may reasonably hope that the challenge of maintaining commensurable concepts will partly sort itself out given the realist requirement of correspondence between concepts and reality.<sup>42</sup> Insofar as there is a natural taxonomy of things, disparately obtained concepts will refer to the same entities.<sup>43</sup> Conceptual integration, then, will follow naturally if all disciplines stress the need for consistency with the observed processes at the next level of analysis down. The realist requirements thus beget a natural standardisation of data.

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<sup>42</sup> The realist intuition that the unity of the real world should eventually produce a unified body of science gives rise to the “unificationist” account of scientific explanation. Unificationism has come to be most closely associated with the efforts of Philip Kitcher (“1981; 1985c” in Kitcher 1989: 410).

<sup>43</sup> The philosophy of Willard V. O. Quine may be read as an argument to the opposite effect, but he does invoke the ancient term of “*natural kinds*” to distinguish *good scientific categories* from *bad* (1969), which is all my argument requires.

This may seem a bit enthusiastic on behalf of scientific realism. A couple of qualifications are certainly in order. Firstly, concepts may refer to the same entities without necessarily being *synonymous*. Assertions to the contrary would imply that the meaning of a concept is fully determined by its reference. I shall not want to deny that the connotations of exactly co-referring concepts may vary, but this is just to say that one true statement may be expressed in more than one way. Secondly, and more fundamentally, this all depends on the realist assumption that the world is indeed independent of the way that we describe it (cf. section 2). This assumption is much more plausible as regards the objects of the natural sciences than the phenomena with which social scientists are concerned.<sup>44</sup>

Still, I believe it is valid enough. While there is little doubt that, say, economic theories influence how economies function, the problems thus arising for social science are easily overestimated. I do not see why we should we not admit that ideas and theories have causal powers and thus include them in our explanations. In any case, changes produced by theories do not affect the ability of those theories to explain how things worked *before* the changes occurred. Social theories may change the reality they describe only to the extent that they affect people's beliefs, and few ideas will spread so fast as to make impossible testing before changes occur – if we are not making long-term predictions, in which case we will have to consider the possibility of *self-fulfilling prophecies* (see Hedström and Swedberg 1996: 293-294). Of course, the theory may be *disproved* by the very observation that the dynamics changed, but then it never really explained in the first place. A wholly different matter is whether the phenomena we study change by being observed. This is a methodological problem, to be addressed by those conducting surveys and experiments.

Of course a problem driven inquiry cannot start from theoretical scratch. Without some sort of theory to guide the search for explanations, scientists will fumble in the

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<sup>44</sup> Charles Taylor of McGill University has written extensively and interestingly on the manner in which he believes language forms reality (see *Philosophical Papers* vol. 1 and 2). While I realise there are significant peculiarities to “the study of man”, I do not believe, as Taylor does, that it is inappropriate to model this study on the natural sciences.

dark. It is a conventional idea that theories are the “lenses” through which we study reality. Indeed all observations are inevitably guided by prior beliefs. In political science, Graham T. Allison’s analysis of the Cuban Missile Crisis (1971) is a seminal illustration of how different theories help highlight different aspects of a situation, without any one theory fully explaining the outcome. If Allison’s point was to promote multi-theoretical approaches to produce trans-theoretical explanations, it does not seem to have stuck. Political science is by and large still a contest between competing schools of thought, as any textbook will make apparent (e. g. Oskamp and Schultz’s *Attitudes and Opinions*). In my view, social science would do well to tailor-make “theoretical lenses” to particular situations, suggesting what mechanisms are likely to be effective from what is known of the conditions for their efficaciousness. The point will not be to corroborate or disprove such “context theories”, but to revise them in light of which mechanisms are actually identified.

I shall organise this section around what I regard as the three basic goals of the mechanism-centred research strategy:

*G1 Identifying and describing mechanisms on the individual or interpersonal level, i.e. developing mechanism theory.*

*G2 Identifying these mechanisms in specific contexts and determining the effect of each on an aggregate outcome.*

*G3 Developing context theory suggesting what mechanisms are efficacious in particular contexts and how they interact with each other.*

*G1* and *G2* together make up mechanism-based explanations. *G2* and *G3* are, as hinted, highly interdependent and it is not clear which comes first. In the following I shall flesh out the concepts of “mechanism theory” and “context theory” and discuss possible designs and methods for achieving each of the three goals.

### **5.1. Two Types of Theory**

I have cited a tendency in social science to develop sweeping theories intended to explain broad ranges of phenomena. Much more emphasis is placed on internal coherence than on correspondence to reality. When theories run into serious empirical anomalies, they are replaced once better alternatives emerge. Because of the abstractions and simplifications involved in obtaining a level of descriptive adequacy across these broad ranges of phenomena, theories become so far removed from the actual causal processes so as to rob them of relevancy to the theories for which they are substituted. Thus it is not only for pedagogical reasons that textbooks such as Oskamp and Schultz (2005) present alternative theories as if they were chronologically replacing each other in clean breaks. Textbooks convey the impression that social research is in large part a quest for the one comprehensive theory that explains as much as possible.

I am proposing an alternative approach to the enterprise of theorising, which starts by developing smaller pieces of theory by identifying, examining and describing particular causal mechanisms independently of a more comprehensive and systematic theoretical framework. This first goal (*GI*) of a mechanism-centred approach involves an increased emphasis on detailed examination and description, in order to obtain a stricter correspondence between descriptions and actual causal patterns, and thereby ensure that these pieces of theory have continuing relevance across changing conceptual paradigms. This shift away from the emphasis on developing a systematic theory is not a new idea. Carl Hovland and his group of researchers at Yale University, who in the 1950s produced very influential body of literature on changes in public opinion, explicitly stated they were not presenting a systematic theory, but an “initial framework of working assumptions about factors affecting attitude change” (attributed in Oskamp and Schultz 2005: 218). The motivation for a mechanism theory also seems essentially akin to that of the notion of “*middle range theory*” in sociology, as introduced by Robert K. Merton in an essay named “On Sociological Theories of the Middle Range” (1967).

It is conventional wisdom among social theorists that greater theoretical detail is bought at the expense of wideness in applicability. The difficulty of balancing the opposing considerations of explanatory preciseness and span constitutes a classical dilemma to scientists in all disciplines. It is indeed true *ceteris paribus* that digging deeper means having to make a narrower hole. Research projects will have to budget with limited resources, as well as bounded human ability to work with large numbers of intervening factors. I shall presently explain how the distinction between mechanism theory and context theory addresses this dilemma.

*Mechanism theory* describes the patterns of causal processes that make up a mechanism and the conditions for its efficacy. Such theory can in principle be formulated in any vocabulary on any analytic level. The only requirements are that it depicts a real mechanism and that it be formulated in a way that makes it possible to determine whether the mechanism it describes was effective in a particular context. Mechanism theory need not respect any disciplinary boundaries, because the real world is not so partitioned. Nor does it need to be incorporated into a larger and more general body of theory. Non-scientific observers, such as politicians or journalists, may well come up with theories of real mechanisms, though the formulation of testable empirical implications might require scientific training. I venture that this flexibility will radically improve the possibility of sharing of insights across scientific disciplines, which is essential to the eventual success of this approach.

I have already mentioned the fact that complex social phenomena, such as the growth of Frp, are brought about by innumerable causes and mechanisms, though they are obviously not all equally important. If only a fraction these mechanisms were theoretically established, the task of testing their actual efficacy in a specific context would be insuperable. Thus the need for theory that guides researchers towards those mechanisms most likely to be relevant in the production of phenomena they wish to explain. To *context theory* I assign essentially the same job as Andrew Bennett (2003) assigns to what he calls “*typological theorising*”, which he hopes will get a

mechanism-centred research strategy “back from complexity to cumulation” using what I have called “mechanism theories” as building blocks:

“With theories on each of these types of mechanisms as building blocks, scholars can develop typological theories of how mechanisms interact with one another in recurrent combinations, which addresses Elster’s challenge of outlining in a structured and cumulative way the conditions under which different mechanisms prevail.”

As opposed to mechanism theory, there are good reasons that context theory should be formulated in the language of one scientific discipline. Interdisciplinarity presupposes, crucially, the existence of scientific disciplines. In order to be fruitful, human investigation needs to be guided by the concepts and conventional ways of questioning that are embedded in specific scientific traditions. Even if every efficacious and thus explanatorily relevant mechanism in the production of changes in public opinion may eventually be reduced to neurophysiological descriptions, we cannot formulate questions pertaining to these changes in the language of neurophysiology. Political science and sociology will still provide the concepts we use to organise the study of aggregate changes in public opinion.

## **5.2. The Development of Mechanism Theory**

I have defined a mechanism as *"a recurrently occurring pattern of causally connected events through which two or more states and/or events are related as cause and effect in some but not necessarily in all causal contexts"*. Mechanism theory aims to describe these recurring patterns. As Stinchcombe pointed out (cf. section 1.2), mechanism theories are pieces of scientific reasoning *usually* in units at a lower level of analysis than the phenomena to which they contribute. I expect this to remain true, but I have stressed that my definition is in principle flexible as to the conceptual level on which the mechanism is described and the distance between the cause and effect (see sections 2.1., 2.2. and 4.2). The crucial requirement is that mechanisms are identified as real

recurring patterns and that their descriptions allow for their efficacy to be tested in particular contexts. Mechanisms theories are also expected to list the essential conditions for the efficaciousness of the mechanism it describes, which in turn will help us develop context theory and – when the mechanism is demonstrably efficacious – ultimately make up the *why*-part of a the explanation.

The fundamental prescription of this section is that an answering questions pertaining to short-term changes in public opinion requires *experiments* and *quasi-experiments* (cf. footnote 36 in 3.2.1). Because of the uncontrollably complex causal environment of social interaction, experimental designs are the only way to study the causal potencies that underlies the empirical patterns identifiable by correlational studies. Different causal mechanisms may – as Jon Elster’s typology suggested (cf. section 4.3.2 above) – produce indistinguishable effects; and largely similar causal antecedents may produce widely different effects depending on an easily ignorable but crucial condition. Consequently, as Andrew Sayer notes in *Realism and Social Science* (2000: 14):

"Consistent regularities are only likely to occur under special conditions, in 'closed systems'. The conditions for closure are first that the object possessing the causal power in question is stable (the intrinsic condition), and second that external conditions in which it is situated are constant (the extrinsic conditions). Such 'closed systems' do not occur spontaneously in the social world."

Given the difficulty of making sound statistical inferences to assertions of causality (cf. sections 2.1; 3.2 and 4.3), there is an uncontroversial preferment of experiments as methods to establish causes for attitude changes – *when they are possible* (Oskamp and Schultz: 210). The problem is that as ways of addressing questions pertaining to *actual* changes in public opinion, they never are possible. Plainly, the manipulation necessary to obtain closed causal systems is impossible without sacrificing *external validity*, i.e. the validity of inferring from experimental findings to facts of the world outside the closed experimental setting. Possibly, some quasi-experimental designs



may obtain such validity, but then they shall be required to ensure that the subjects of the experiment behave just as they would in a natural setting. Fulfilling that requirement will prove difficult, at best. However, external validity is not necessary for the purposes of developing mechanism theory. Rather than lamenting the fact that experiments cannot provide direct access to the situations they wish to study, researcher should consider the potential of experimental studies to make correlational studies more effective in identifying underlying causes in natural environments.<sup>45</sup> The mechanism approach suggests such a two-step strategy in which experiments identifies the particular causal processes of mechanisms; clarifies their interaction and the various conditions for their efficacy, and then the knowledge thus obtained is used to design better correlational studies.

Of course it is wildly unrealistic to expect every study in the field of attitude research to begin with experiments. Nor is it necessary, granting the justification I have provided for applying the theory of lower level sciences. But testing the aggregate prevalence of mechanisms posited by lower level sciences will rarely be a matter of straightforward translation into survey questions. As cognitive mechanisms tend to be, those posited by schema theory consist of complex processes that are highly conditioned on the individuals' prior cognitive wiring as well as contextual factors. They may regularly but not always produce aggregate changes in the direction that was expected given the nature of the stimuli. Correlations thus consistent with expectations were established by the quasi-experiment summarised in section 3.2.1 above, but I argued that those findings did not provide grounds for asserting that schema theory could explain those changes. When used as in Todal Jensen and Aalberg's experiment, lower level theories are not actually put to the test; they serve simply as inspirations for hypotheses concerning aggregate changes that may or may not be produced by those posited mechanisms. In order to identify mechanisms responsible for those changes, we need to derive empirical implications that will

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<sup>45</sup> While this is certainly not a new idea – fruitful experiments were conducted by Carl Hovland and his colleagues at Yale (see Oskamp et al.: 218), it was not until the 1990s that experiments entered the “standard repertoire” of political science (according to Norris et al.: 50).

distinguish the various mechanisms possibly responsible for given regularities – either on the individual or aggregate level.

An empirical implication may be more than just the assertion that an event  $e$  will or will not occur – or, more specifically but equivalently, that a statistically significant proportion of a sample will change their attitudes in the expected direction given certain stimuli. The ultimate effects of any communication potentially triggering attitude change depend on a series of factors, commonly classified by the following categories: “*source characteristics*”; “*message characteristics*” (the content); *media characteristics* and “*audience characteristics*” (see Norris et al.: 9 and Oskamp and Schultz: 219-220). Thus we may arrive at statistically testable hypotheses such as: individuals with characteristic(s)  $c$  (or  $c_1, \dots, c_n$ ) are swayed to have a more (or less) favourable attitude towards particular politicians or political solutions having heard a particular politician deliver a certain phrase (“soundbite”) in a particular manner on a particular program; or that a particular phrase delivered by a particular politician will sway individuals with characteristic(s)  $c$  (or  $c_1, \dots, c_n$ ) to hold a more favourable view of a particular political solution, while in other recipients it merely confirms their prior positive or negative attitudes to the politician and the solution. Insofar as these hypotheses are sufficiently corroborated, several alternative mechanisms may be discarded. Mechanisms may have distinct empirical implications on the aggregate level as well. Through research on the mechanism of threshold-based behaviour (cf. section 4.1) we may, for instance, arrive at hypotheses asserting particular threshold for particular behaviours.

The priority in time of experimental studies or correlational studies on random representative samples need not be as suggested above. Individual media events such as political gaffes or particularly successful speeches may trigger significant short-term effects on public opinion. If such effects are statistically well recorded, researchers may subsequently inquire into how and why they came about. By manipulating the message variable in an experimental setting we might be able to identify particular cognitive mechanisms through which a particular phrase of gaffe

influenced people the way they did and disclose the essential conditions for those effects, provided we know enough about who was influenced in the first place. While scientists in the field of public opinion have increasingly centred their efforts to explain changes on what goes on in mass media, they do not seem to have realised the possibilities this narrowing down has created for pinpointing particular causes. Skilled media observers will have plenty of ideas as to which particular events were instrumental in triggering any substantial change in public opinion. Their insights, I venture, are grossly underexploited for scientific purposes (see section 6.2).

### **5.3. The Identification of Mechanisms in Specific Contexts**

In this section I shall discuss some methods through which we may use mechanism theory and context theory to identify mechanisms in particular contexts thus confirming posited explanations. In section 2 I rejected the concept of statistical significance as sole criterion for deciding whether to accept an explanation. In the process of stripping our knowledge of everything that cannot be expressed in numbers, I argued, we lose much of that which contributes to our sense of certainty that an explanation is correct. I went on to assert that a broadly supported theoretical basis for expecting the efficacy of the posited mechanism contributes essentially to that sense of certainty. Such theoretical justification requires that we have been able to exclude the possibility of equally plausible mechanisms. That requirement was part of the rationale for preferring fine grained theory – or, alternatively, for including as many relevant factors as possible in the statistical analysis (cf. section 4.3.2).<sup>46</sup>

I have not the resources to list or discuss in any detail the various possible designs and particular methodical venues to identify mechanisms, but there are a few ideas I should like to mention, however briefly. In 5.3.1, I shall discuss some problems arising with the adoption of *finer grained* mechanism theory from lower level sciences. In section

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<sup>46</sup> The rest of that rationale was the increased epistemological contentedness of knowing more of the particular causal processes (the *how*) or obtaining a better understanding of the conditions for the efficacy of the mechanism (the *why*) (cf. section 2).

5.3.2, I shall address more technical problems arising from including *several interacting factors* into a statistical analysis.

In the study of changes in public opinion, I argued in section 2, we should aim to explain by identifying the mechanisms producing *individual* changes in attitude. As for the popularity of political parties, insofar as the number of people holding a party preference does not change significantly, any increase in popularity is always associated with the decreased popularity of at least one other party. Thus to explain the increased popularity of one party is in effect to explain at least part of the decreased popularity of another. In order to explain such aggregate changes in public opinion, then, we need first to identify the relative pervasiveness of its constituent individual changes from one party preference to another. Given the unreliability of human memory (see Oskamp and Schultz: 125; 194), we shall prefer *time series data*, i.e. data from the same individuals across various points of time, to measure changes.

The *Danish General Survey of Power and Democracy* included an exemplary design to generate time series data. As part of it, a revolving probability panel of Danes was asked the same set of questions every fourth months between May 1999 and September 2002. One third of the interviewees were substituted before every survey. Each respondent would thus participate in three or four consecutive surveys, making it possible to study both individual and aggregate changes. Questions were chosen so as to cover most possible topics in Danish politics, such as immigration, the EU, the welfare state and the new bridge across the Öresund to Sweden. Some issues did not receive any significant media attention when surveys began, but would foreseeably do so, such as a general election campaign or the opening of a bridge. Other issues would be affected by unforeseeable events, such as the September 11<sup>th</sup> 2001 terrorist attacks. In both cases surveys provided data on the impacts of these events on public opinion. This justifies to some extent the characterisation of *natural experiments*, argues its researcher-in-charge, Lise Togeby (2004: 16; see section 5.3.1 below).

The *Danish General Survey* also included qualitative interviews, focus groups and analyses of official documents and press clippings. It seems to me that such comprehensive multi methodical surveys are the way of the future for public opinion research. Not only do time series data of attitudes help define more precisely the aggregate changes we wish to explain; held together with simultaneous analyses of press and TV clippings may help us identify triggering causes and the supplementary qualitative interviews may provide the kind of information we need to mechanisms not easily identifiable by simpler statistical surveys. Moreover, such monitoring provides a valuable basis of information upon which other studies may proceed, as we shall see in section 6 below.

### **5.3.1. Testing Fine grained Theory**

Finer grained theory, I have argued, involves getting closer to conceptually capturing the particular connections between causal processes (of which causal mechanisms are constituted: cf. sections 1.2; 2; 4.2 and 4.3). The less that mediates between two causally connected events, the more regular their correlation, but then the more factors we need to keep constant in order to identify and measure them statistically and the smaller the subsets of the samples in which the connection will be found, thus making it harder it is to identify them through inferring from simple random representative samples.

One way of addressing this problem is to *stratify* the population before sampling from it, i.e. categorising people into subgroups (“strata”) according to relevant characteristics and then make random samples from each of these subgroups. In order for such targeted surveys to work, we need to have an idea of what mechanisms to expect and the relevant characteristics to their efficaciousness. We need theory to guide us each step of the way from the choice of a research question to decisions concerning who to study and how to do it. Firstly, context theory should help us translate characteristics of the phenomenon under inquiry and cues in its environment into predictions of what mechanisms brought that phenomenon about. Having obtained

a set of expected mechanisms, we shall then apply what we know of them in the design of the study – of which a notable part, in the case of a stratified survey, is to choose the most relevant strata.

While surveys are certainly irreplaceable as a method to measure the prevalence of mechanisms, it should be clear from my discussions in section 2 that I am not simply hoping that more sophisticated statistical techniques will solve all methodological challenges arising from testing finer grained theory. However finely targeted, surveys themselves may not suffice to make causal inferences, but they may still provide evidence that *in conjunction with what we know of the conditions for the mechanisms we believe to have identified* may serve to exclude other known plausible mechanisms – assuming, as I do, that in order to achieve a given degree of certainty, having a better context independent theoretical justification for believing a particular mechanisms to be efficacious means *ceteris paribus* that the statistical evidence in favour of that mechanism may be correspondingly weaker.

Indeed, if we come close to identifying the full set of essential conditions for the efficaciousness of a mechanism, the very presence of these conditions will go a long way to establish their actual efficacy in situations where they may explain an observed phenomenon. Comparisons among the largely similar political systems in Norway, Denmark and Sweden may thus contribute to explaining the relative popularity of their respective radical right parties. Today, the Danish People's Party (Dansk Folkeparti) enjoys popularity on par with the Norwegian PP, while the Swedish counterpart New Democracy (Ny Demokrati) was a short-lived phenomenon of the 1980s. Tor Bjørklund and Jørgen Goul Andersen (2006) identifies several apparent preconditions for their success in Norway and Denmark: *turbulence in the party systems* with referendum campaigns generating massive grassroots activity, considerable *polarisation* and distrust between voters and party elite consequently *severing ties of loyalty*; an expansive period for the welfare state demanding an unprecedented *raise of taxes*; coalition governments of non-socialist parties that after a long period of social democratic governments did not significantly change policies, thus creating *frustration*

*and disillusionment among non-socialist voters.* None of these preconditions were present in Sweden until the late 1980s.

Importantly, the presence or non-presence of these conditions cannot count as evidence corroborating any explanation without prior context independently established theory of the implied mechanisms. Without this requirement, any conditions present in both Norway and Denmark but not in Sweden during the 1970s may *ipso facto* be held as explanatory relevant. Of course we all have an idea of which are the more important conditions, but the theories guiding those ideas need to be explicit in order to make proper testing possible. Also, any valid comparison must proceed on strictly defined shared characteristics and differences among the three parties.

At rare opportunities, scientists may get to conduct such comparisons in settings approaching *natural experiments*. These are naturally occurring situations particularly favourable to identifying the effects of one single or a set of related factors. One well known example is the study of Helena, Montana where a smoking ban was in effect in all public spaces from June of 2002 to December 2002. The city is geographically isolated and served by only one hospital. It was observed that the rate of heart attacks dropped by 60 percent while the smoking ban was in effect (Sargent et al. 2004). Better developed theory may make for more frequent identifications of situations allowing for natural experiments.

Apart from everything else contributing to the unreliability of *self-reporting* (several are discussed in Oskamp and Schultz: 211-214), there arises a further problem with borrowing fine grained theory from lower level sciences such as evolutionary theory or cognitive psychology: we are trying to identify mechanisms of which the subjects are themselves often unaware. In section 3.2.1, I pointed to the inadequacy of merely asking subjects questions concerning the broadly expected effects of posited cognitive mechanisms. In order to identify a mechanism we need to infer and identify more distinct empirical implications. Machines for measuring activity in the brain may not anytime soon be routinely available for social scientists, but we may find that there are

other ways of identifying cognitive processes of which the subjects are wholly or partly unaware. There are, of course, branches of psychology based on the very idea that statements such as “slips of the tongue” provide more direct access to unconscious processes, but unlike Freud and his followers, we shall require actual falsifiability.

Thus provided we are able to infer distinct empirical implications of cognitive mechanisms that are testable by responses to open-ended questions, there are a variety of methods available (see Oskamp and Schultz: 67-86; 207-263 and section 6.3 below). One method I find particularly promising to obtain more reliable data on how people react to political messages in an experimental setting is the so-called “*thought-listing technique*”. This method prescribes that participants are given a limited amount of time (usually three minutes) to write down brief statements of the thoughts they had as they listened to these messages (Oskamp and Schultz: 256). Their statements are then coded by independent raters according to whatever aspects we believe to be indicative of the mechanism in question. In conjunction with thorough interviewing and perhaps other tests to establish significant features of the participants prior cognitive apparatus, we may find that differently primed individuals perceive messages in significantly different ways (cf. section 3.2). If such is the case, we shall have obtained evidence much more relevant to decide the efficacy of priming than if we merely knew the ultimate effect the messages had upon the attitudes of the participants.

### **5.3.2. Zaller on the Problem of Highly Interactive Models**

John R. Zaller’s theory of attitude formation, as laid out in *The Nature and Origins of Mass Opinion* (1992) is of great interest to this paper because it encounters several of the problems I expect will arise with the identification of mechanisms, among them the fact that its inclusion of several interacting factors makes it hard to test by traditional statistical means. The main point of this subsection is to show that these problems are not widely believed to be insuperable, but being addressed by contemporary statistical theory.



Before discussing these methodical issues, I should say a few words on the RAS model itself. Zaller and I share the opinion that the fragmented state of current public opinion research both can and should be mended. However, the remedy Zaller proposes is in some respects the very antithesis to the approach I am advocating. Zaller wants, in his own words, “to integrate as much as possible of the dynamics of public opinion within a cohesive theoretical system” (Zaller 1992: 1). Significantly, Zaller’s idea of theoretical integration does not merely involve an effort to achieve conceptual integration and mutual compatibility among theories, but to construct a new all-encompassing theory. With the justification that “mass opinion change [...] seems to conform to the same principles in whatever context it occurs,” Zaller sets out to replace “domain-specific theories [with] a unified theory of its major empirical regularities.” The so-called RAS model is intended to explain the formation of almost any political preference, thus ostensibly exemplify the very explanatory exuberance of the traditional approach that I have ventured to criticise.

The RAS model describes an assumed interaction among a four allegedly important factors in the reception and reproduction of political opinions (cf. section 1.4).<sup>47</sup> It proceeds from the fundamental idea that public opinion is shaped predominantly by “elite supplied information” (ibid: 23). Statements of political opinions, as registered by public opinion surveys, are conceived as “the outcome of a process in which people *receive* new information [from elites], decide whether to *accept* it [as considerations], and then *sample* [among a set of accepted political considerations] at the moment of answering questions” (ibid: 51, original emphasis), hence the name of “RAS”. Attitude change is understood as “a change in people’s long-term response probability” (cf.

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<sup>47</sup> These four fundamental assumptions, or “axioms”, are as follows:

**A1. The Reception Axiom:** “The greater a person’s level of cognitive engagement with an issue [political awareness], the more likely he or she is to be exposed to and comprehend – in a word, to receive – political messages concerning that issue” (1992: 42).

**A2. The Resistance Axiom:** “People tend to resist arguments that are inconsistent with their political predispositions, but they do so only to the extent that they possess the contextual information necessary to perceive a relationship between the message and their predispositions” (ibid: 44).

**A3. Accessibility Axiom:** “The more recently a consideration has been called to mind or thought about, the less time it takes to retrieve that consideration or related considerations from memory and bring them to the top of the head for use” (ibid: 48).

**A4. The Response Axiom:** “*Individuals answer survey questions by averaging across the considerations that are immediately salient or accessible to them.*” (ibid: 49)

section 1.4). Changes in this probability result from “[changes] in the mix of ideas to which individuals are exposed” (ibid: 266).<sup>48</sup>

The empirical support in favour of the RAS model is, frankly, unimpressive. Zaller himself admits its failure to obtain “statistical robustness, [which manifests] itself in large standard errors for coefficients and the need to constrain parameters in models in order to obtain conventionally significant estimates” (ibid: 290). When defending his model in spite of this, Zaller invokes alternative criteria to the empirical assessment of theories. In section 2, I quoted Zaller’s apparent dismissal of traditional statistical significance testing. Instead, he promotes an emphasis on “stability of results obtained across related problems and datasets and [on] the substantive magnitude of particular effects and the theoretical pattern into which they fit” (ibid: 290, with reference to Achen 1983). Note the similarity to my emphasis on the role of mechanism theory in increasing *certainty* (cf. sections 2; 4.3; 5; 5.1 and 5.2). On these criteria the RAS model apparently fare much better.

Zaller identifies three sources of the problem (ibid: 291). The principal one is the highly *interactive nature* of the RAS model. This creates a large amount of *multicollinearity*,<sup>49</sup> and thereby a loose statistical fit to the data. Another source of statistical imprecision is that much of what the model tries to capture involves the behaviour of small subgroups of the overall sample. In section 5.3.1 above, I suggested *stratified samples* in response to this problem. The presence of multicollinearity will require even larger samples in order to obtain an acceptable level of statistical precision. The third difficulty is the presence of “effects that may be either subtle or invisible in particular contexts” (ibid: 292). Each condition a mechanism requires in order to be efficacious, the more subtle and less visible we may expect its effects to be. With the other two problems, detecting subtle effects seems close to impossible.

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<sup>48</sup> Thus the model seems compatible with John Petrocik’s theory of issue ownership and shifting agendas.

<sup>49</sup> Multicollinearity occurs when the independent variables in a regression-model are themselves highly correlated. Consequently, the estimates of each independent variable’s impact on the dependent variable (while controlling for the others) tend to be less precise.

Is there anything to be done? Yes, says Zaller. He suggests four general improvements of statistical analysis. First of all, there is the fundamental significance of having *reliable measures*. As a slogan, this squares perfectly with the programmatic implications of scientific realism, as expounded in section 2. It implies the advocacy of a shift of emphasis from the internal consistency and elegance of a causal model to the operationalisation and measurement of its concepts. The requirement of reliability gets more acute when faced with the abovementioned problems. When measuring correlation among very general characteristics possessed by all participants in surveys, measures need not be as reliable in order to obtain statistical significance. Relevant results to Zaller's model depend on the behaviour of relatively small subgroups of very high or very low political awareness (cf. A1 in footnote 50). Thus it is essential that he can reliably distinguish those subgroups.

Reliability, of course, is of little worth without *valid* operationalisations, i.e. the requirement that measures capture the characteristics described by our concepts. Validity seems implied by Zaller's concept of "reliability". He prefers simple tests of *knowledge* to the alternatives of self-reports of *interest* or *media exposure* and *education*. (ibid: 292-293), but it is not clear whether this is mainly because of its validity or reliability. In the latter case, it would be symptomatic of a general orientation among social scientists, as summarised in the slogan attributed to them by Andrew Sayer: "Never mind concepts, look at the techniques" (1992: 2; cf. section 2.1.1 above). Scientific realism requires that any valid scientific inquiry proceed on concepts that denote real and relevant taxonomies in the external world. Failures to do so will become more evident when dealing with subtle effects concerning the behaviour of well defined subgroups – *which is exactly why we wish to study them* (cf. section 4.3). Statistical analyses, I venture, should fail to establish significant regression coefficients much more frequently.

Secondly, Zaller discusses the possibility of increasing the *size of surveys*. This is not easy when designing surveys intended to capture attitude change, he concedes, because "it is rarely possible to anticipate in advance when attitude change will occur

and what questions will be necessary to capture it” (ibid: 293). This was a fundamental problem facing Todal Jenssen and Aalberg when designing the quasi-experiment discussed in section 3.2.1. When resources allow, a continuous revolving panel such as that set up by the Danish General Survey (see 5.3 above) may help both to indicate changing attitudes as well as making it practically possible to collect data with greater speed.

Even more important than large datasets and reliable measures, argues Zaller, is “*good variance* on the principal independent variable in the model” (ibid: 294; cf. section 6.3 below). In order to achieve this, it is necessary to identify situations and conduct surveys where there is such variance. As with otherwise motivated stratification, we may be helped by well-developed mechanism theory. Fourthly, according to Zaller, *modelling technology* (complicated non-linear regression models) can “help squeeze as much meaning as possible from the available evidence” (ibid: 294).

### **5.3. Concluding Remarks: A More Unified Social Science?**

In public opinion research, there is no equivalent to the hugely complex economic models employed by central banks, statistical agencies etc. While the axioms and explanations of economics are philosophically contentious, the discipline boasts a high degree of internal – if not external – conceptual integration, which is a prerequisite for these comprehensive programs. The dynamics governing economies are certainly more predictable than the factors moving public opinion, but it seems to me appropriate to ask whether similarly comprehensive computer programs would be possible to predict changes in public opinion. Theories of mechanisms shall have logical structures fundamentally similar to the algorithms of which such programs consist: *if (and only if) a, b and not c, then d, etc.* Given the possible complexity of the interaction among mechanisms, computer programs may be required for inferring any empirical implications at all. At the very least we may envisage programs into which one may punch information about a certain situation and then get out the mechanisms most likely to be effective, thereby guiding research.

The point of this section was to suggest that a more cumulative and collective social science is possible and sketch a strategy through which it may achieve a shared flexible theoretical approach to any political situation, following the example of economics, but putting much more emphasis on lower level consistency.

## **6. Explaining the Rise of Fremskrittspartiet**

Ever since its conception as the Anders Lange's Party in 1973, Frp has been controversial. It is somewhat of a pariah among political parties in Norway, accused of sins such as racial intolerance and economical irresponsibility. Consequently, its steep climb on the polls in the spring of 2006 gave rise to extensive debate. In accordance with the strategy of problem driven research, I have chosen this particular explanandum-event to illustrate and flesh out my arguments. The aim of this section is not to test particular explanations of that rise, but to sketch some ways in which the conceptual and methodical ideas proposed in the previous sections may be employed in testing the efficacy of some mechanisms that have been (implicitly) suggested as having played a part in bringing it about.

In section 6.2, I shall discuss how one might go about examining and describing these hypotheses with an eye to making the implied mechanisms as identifiable as possible in given contexts. This is the development of what I have called "mechanism theory" (goal G2 in section 5). In section 6.3 I shall go on to discuss how one might go about trying to identify effective and hence explanatorily relevant mechanisms in the context of contemporary Norwegian politics, and in particular the post-election months of 2005 and the spring of 2006. As explicated in section 5, the identification of mechanisms should be guided by some sort of "context theory" – i.e. tentative description of a situation indicating what mechanisms we should expect to be prevalently effective. No such comprehensive context theory is currently available, and it is far beyond my scope and ability to go about formulating one. In the following discussions I shall make some assumptions that might – were they properly developed, organised and justified – have been part of such a theory. Incidentally, this is in

accordance with the implicitness of such assumptions in much of contemporary social research. First of all I should like to provide some background by outlining the dominant theoretical perspective on the longer-term growth of radical right parties in Western Europe.

### **6.1. Alleged Long-term Tendencies Favouring *Fremskrittspartiet***

In the conclusion of his highly influential *Political Man* (1960), Seymour Lipset asserted that: “in virtually every economically developed country the lower income groups vote mainly for the parties of the left, while the higher income groups vote mainly for the parties of the right.” As Lipset himself observed in his introduction to Karvonen et al. 2001, this is no longer true. From 1973 to present day, the class composition of the radical right have changed significantly (Bjørklund and Andersen 2006<sup>50</sup>).

According to Ronald Inglehart (1997), this change is a consequence of the transition from the industrial to the post-industrial society. Today’s left wing voters are “postmaterialists”, emphasising non-materialistic values issues such as self-expression and environmentalism. These issues have but a weak appeal among the so-called “working classes” who feel threatened by change, either economically, socially or culturally. Such ideas suggest that the radical right has a special appeal to those who do not benefit from social change. Indeed, for both Frp and the Danish People’s Party, young men with working class background constitute a stronghold (Bjørklund and Andersen 2006). According to polls cited by Mr Magnus E. Marsdal (2006: 67) Frp won 36 percent of the votes among unskilled workers in 2005, more than any other party, including Labour. While the class composition of the radical right changed from 1973 to 2005 the majority of men have remained a stable factor (Bjørklund and Andersen).

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<sup>50</sup> I was provided an unpaginated manuscript that was to be translated into Spanish for inclusion in Simon 2006.

The typical losers are the unemployed, but, significantly, there is no empirical evidence to support the thesis that they are a source of votes to the radical right in Norway (Bjørklund and Andersen). Frp does exhibit a clear profile of protest against the established political elite, but this may merely be due to the fact that it has never been part of any government (ibid). There is nevertheless empirical evidence to support the general thesis that:

“The emergence and surge of radical right in Scandinavia must be related to *structural changes and new cleavages*. As the industrial society is transformed to a post-industrial society new conflicts arise. The radical right in Scandinavia is an answer to all the changes in an area of globalization: economic (free trade), cultural (immigration) and political (concerning the sovereignty of the nation state)” (ibid; my emphasis).

The emergence of Frp’s predecessor coincided with the dissolution of party political loyalty bonds, the significance of which I mentioned in the introduction of this paper (cf. also section 5.3.1 above). This trend was accelerated, assert Bjørklund and Andersen, by the collapse of communism, the end of the cold war and other global trends, notably increased emigration. The influx of immigration and asylum seekers to Scandinavia during the second half of the 1980s gradually generated strong political divisions. From 1988 onwards all distributions have located the voters of Frp in Norway and Denmark on the one pole and those of the socialist left at the other (ibid). While the immigration issue has been Frp’s best “recruiting plank”, it has profited by other issues as well, partly by successfully linking immigration to problems of the welfare state. Money spent to accommodate asylum seekers, they suggest, is grabbed out of the hands of elderly and sick people of Norwegian origin (ibid).

Even if we identify the mechanisms that make structural societal changes efficacious, they can only go so far in explaining short-term changes, such as the post election surges in 2005 and 2006. Among those who have attempted to come to grips with this phenomenon, is Gudmund Hernes. A former labour minister, he begins by noting that

no country has ever been as well off as Norway is today, and never has a country with a similar level of material welfare had a protest party of such a size (2006: 81). Hernes discusses five proposed explanations, which are the topic of section 6.2.1.

## **6.2. Developing Theory**

Let me begin by what is known of the explananda. During the five weeks of the 2005 campaign, Frp rose on the polls by 1.8 percentage points to 20.5 percent a few days before the election. In the election itself, they won 22.1 of the votes.<sup>51</sup> During December of 2005 they were at an average of 23.6. Beginning the following January, they rose steadily until the peak of 31 percent in April of 2006.<sup>52</sup> Interestingly, Frp experienced a similar surge after the parliamentary election of 2001. Having achieved 14.6 percent in the election, they were at a top average of 32.5 percent on the polls a little over a year later, to be succeeded by a steady downward slope.

At a time when the final analyses were still pending (in an e-mail on October 10<sup>th</sup> 2006), Bernt Aardal informed me that surveys from the post election months showed that Høyre was leaking substantially to Frp. Part of the explanation, he suggested with due provisions, may have been that Høyre was disadvantaged by having spent four years in government and thus vulnerable to attacks from Frp. Possibly most important, in Aardal's opinion, is that Frp remained the clearest oppositional party. It could not be held responsible for previous governments' policies, and having never been in position, meant never having had to break any promises.

There are two main venues through which Norwegian scholars have tried to explain the rise of Frp – or, for that matter, the rise or decline of any other party. Firstly there is *informed speculative reasoning* of the kind that Aardal engaged in above. To this category belong the contributions of Gudmund Hernes and the left-leaning author and journalist Mr Magnus E. Marsdal (2006). The latter is worth mentioning because it

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<sup>51</sup> Given the controversiality of Frp, underreporting may occur. A rise in popularity may thus have been partly self-propelling by providing acceptability – explicable, perhaps, in terms of a macro-micro mechanism. I have not the space to discuss this possibility.

<sup>52</sup> For more statistics, see Bern Aardal's home pages: <http://home.online.no/~b-aardal/>



expresses some ideas common among political observers, notably that Frp has managed to appear the radical, down-to-earth and folksy alternative to a grey mass of arrogant parties in the centre (Marsdal 2006: 66). The Labour Party's alleged turn to the right has in effect made it a party of the elites, thus leaving an electoral void among the working classes, into which Carl I. Hagen has cleverly manoeuvred Frp (ibid: 72). Such explanations rely mainly on their authors' personal understanding of Norwegian politics. Only in passing do they cite statistical evidence.

At the other end there are attempts at *statistical inferences*. Within this approach belong the authors of *I valgkampens hete* (cf. section 3.2). Several hypotheses concerning the rise of Frp have thus been rejected, among them the idea that more wide-spread scepticism towards an extensive welfare state have moved voters rightwards on the political spectrum. Quite contrarily, Frp has managed to promote itself as the chief protector of health services and geriatric care (cf. Bjørklund and Andersen). This has lead some to believe its rise may have coincided with an increased public preoccupation with this issue. But there is no evidence to support that thesis, concludes a master thesis by Helge Magnus Stang Opsahl at the University of Oslo (2005). Statistical inferences, I have argued, are indeed better suited to disprove hypotheses (as well as indicating the pervasiveness of demonstrably efficacious mechanisms) than themselves corroborating causal connections. Mere correlations are not, strictly defined, explanatory – nor, I venture, epistemologically satisfactory to any serious inquirer (cf. section 2).

The challenge, then, is to put informed speculation to the test. In order to do so, we must first separate the real beef, as it were, from essayistic seasoning and turn claims into testable hypotheses. Is it explanatorily relevant, say, that Frp politicians appear more “folksy” than other politicians? Quite possibly it is, but I have yet to see its effect scientifically tested.<sup>53</sup> Surveys frequently ask whether voters have *confidence* in

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<sup>53</sup> The efficaciousness of Frp's “folksy” profile and the invaluable help condescending cultural celebrities have offered to its cultivation of that profile, appears to be an important thesis in Magnus Marsdal's *Frp-koden – Hemmeligheten bak Frps suksess*, which was published in mid April 2007, but not in time for its claims to be considered in this paper. It is clearly not a strictly scientific work.

politicians. I have not seen surveys asking whether they *like* them or whether they *identify* with them, the relevance of which is implied by Marsdal emphasis on the fact that Hagen wore very casual training equipment while campaigning instead of the more conventional sports jacket (2006: 67).

The fundamental thesis of this paper is that lower level theory capturing patterns of causal processes in the human mind or precise renderings in general terms of demonstrably efficacious mechanisms with which we are familiar (cf. section 4.2), may help bring forth unclear ideas hitherto expressed mainly in unscientific but tentatively explanatory essays and turn them into more subtle and fine grained scientific hypotheses than those presently dominating the field. I would have liked to in the following discussions to have systematically rendered relevant lower level theory, but, unfortunately, I do not have the necessary overview. For the sake of illustration, I shall suggest possible mechanisms without reference to actual lower level theory – while minding the requirement of, at least logical, consistency with neurophysiology and evolutionary theory (which need not necessarily rest on *biological* mechanisms; cf. section 6.2.2).

### **6.2.1. Hernes' Five Proposed Explanations**

At the height of Frp's popularity in the spring of 2006, Gudmund Hernes wrote an essay called "Det er noe i det... Om Fremskrittspartiets vekst" (2006). In an essentially non scientific language, he discusses five frequently proposed explanations for its rise on the polls. The explanations do not refer to any exceptional factors of the months after the election of 2005, but the implication is that the factors he mentions were particularly potent in that period.

The first proposed explanation Hernes labels the "*pendulum explanation*". It suggests that in the minds of the voters, different economic situations require different regimes, so that when unemployment goes up, the popularity of the Labour Party does too (Hernes 2006: 81). Statistically, according to Hernes, this may account for about a

third of the variation in support for Frp and Høyre taken together. This explanation is also discussed by Bjørklund and Andersen (2006):

“In Scandinavia, prosperous times seem to have nurtured the right radical parties. Perhaps it is in prosperous times one can more easily overlook the advice from authorities. In hard and difficult times appeals to “*Keine Experimente*” may more easily receive support. People choose the well-known alternative and listen to the message from responsible authorities.”

As it stands, this remains speculation. Significantly, the very opposite effect is also conceivable (in accordance with Jon Elster’s expectations (1999: 6); cf. section 4.3.2 and 6.1 above): High unemployment may produce feelings of protest that channel into support for parties at odds with the prescriptions of the governing authorities. This hypothesis has not been supported by empirical evidence in Scandinavia (Bjørklund and Andersen). Insofar as the implied mechanism is context independently established and demonstrably efficacious in other political contexts, we might wish to know *why* it was not in Norway. Clarifying the conditions for the efficacy of a mechanism producing a preference for safe choices and (ostensibly) responsible politicians in times of insecurity will thus make identifying it easier, as well as *ceteris paribus* justifying a higher degree of certainty that it was indeed responsible for the observable effects (cf. section 4.3 and 5.3). If we manage to obtain a satisfactory degree of certainty, such clarification will also contribute to our sense of epistemological contentedness (cf. section 2).

Now, whatever, the conditions, it is perfectly plausible that they were more pervasively present after than before the election of 2005. Aardal’s statistics suggested that many of the voters switching to Frp after the election had voted Høyre,<sup>54</sup> which I believe most people would consider a “safer bet” in government than Frp. We may thus hypothesise that in the period preceding the election, people realised that their own choice of a party, however insignificant, would be counted in the decision of who

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<sup>54</sup> As I pointed out in section 5.3, when our explananda is the increased support of one particular political party *a*, we shall, in multi party systems, usually need to break it down to its constituent aggregate changes from each of the other political party to party *a*.

should lead the country, thus making them more cautious and prone to go with the safer bet. Having thus cast a vote for Høyre and it being four years to the next parliamentary election, they might start deliberating with less sense of caution and responsibility, knowing they will have plenty of time to change their minds.

Hernes then discusses what he calls the “*the Hamelin-explanation*” (ibid: 81), comparing Carl I. Hagen (and Siv Jensen<sup>55</sup>) to the rat catching Pied Piper of Hamelin, luring voters with charm and cunning on television and at rallies, speaking the language of ordinary people and appearing to take their worries seriously. Hagen’s style is one of eloquent common sense complaints concerning the follies and inconsideration of other parties. He has successfully situated himself outside the establishment, in spite of being the most seasoned of parliamentarians.

While Hagen’s charisma has undoubtedly been instrumental to the present popularity of Frp, the assertion is merely the suggestion of an explanation. It presupposes that Hagen’s language is the kind of language that people like politicians to speak, but the success of his rhetorical strategy was never guaranteed. If people are fed up with the talk of other politicians, why should they not be fed up with Hagen’s? Having identified essential characteristics of Hagen’s rhetoric, we may inquire into when people are more susceptible to it, thus establishing the conditions for its success: When do people vote for outsiders? When do they want politicians to speak like themselves?

His third, Hernes labels “*the explanation from stupidity*”, according to which people do not understand what the policies of Frp really entail. It may appear a paradox, then, that the level of education has never been higher in Norway; though perhaps not so much, considering that highly educated people tend not to vote Frp (ibid: 82). Insofar as education explains the rise of Frp, argues Hernes, it is because a generally higher level of education has bred a more pronounced cultural divide between those who have it and those who have not. Such polarisation might certainly have contributed to the general attraction of a radical protest party among the so-called working classes, as

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<sup>55</sup> Hagen retired from the chairmanship in March of 2006 leaving Siv Jensen in charge.

discussed by Bjørklund and Andersen, as well as having clear relevance to the four other explanations proposed by Hernes.

It seems that Hernes' fourth, the "*explanation from haughtiness*" invokes this very cultural divide, but approaches it from the other direction. While Hagen and Jensen are perceived to speak on behalf of "ordinary people", other politicians belong to a class of *removed haughty media figures* who have lost touch with – and even appear to despise – the lives of those they represent.<sup>56</sup> A frequently condescending attitude among other parties towards Frp might have helped Frp to cultivate this role as victimised outsider, persecuted for its efforts to help ordinary people. Hagen, it is often said, is an expert in portraying himself as a man persecuted by his political enemies for having ordinary people's best interests at heart.

Lastly, there is the "*the honey jar explanation*". Petroleum, of course, has made Norway an extraordinarily rich country. While all other parties have adhered to the so-called "rule of operation" ("handlingsregelen") stipulating limited use of revenues from accumulated capital so as not to cause inflation and loss of international commercial competitiveness, there are no such worries among Frp politicians, who promise to spend much more of the oil-generated wealth than other party. This *petroleum populism* proceeds on a logic that may seem irresistible to some: "Here is a problem; we have the money to solve it; why has it not been solved?" Full of seemingly genuine indignation, Hagen and Jensen habitually demand why the government is not spending only a fraction of the nation's vast fortunes to help its sick and elderly – and why a country of such wealth needs to have toll roads (when Sweden

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<sup>56</sup> As I am writing this on April the 16<sup>th</sup> 2007, Frp's Øystein Hedstrøm is saying on TV2s *Tabloid* that his is the only party taking "ordinary folks" seriously ("Vi tar vanlige folk på alvor") and that they are being harassed by other parties for it. Video available at: <http://pub.tv2.no/TV2/magasiner/tabloid/> Later that same night on NRKs *Redaksjon EN*, Siv Jensen claimed that critics of Frp frequently exhibit a detestation towards her voters. Magnus Marsdal agreed, calling cultural pundits "useful idiots" to Frp. Author Erik Fosnes Hansen posited that Norwegians are "allergic to" and possess an "antenna" detecting such self-important pundits ("viktigperer"), whom they despise. See: <http://www.nrk.no/programmer/tv/redaksjonen/1.2258594>

does not<sup>57</sup>). Other politicians, they insinuate, are in the pockets of cold hearted bureaucrats at the Finance department.

Given that we manage to test the effect of such rhetoric, we should again ask what the conditions are for its success. There are at least two ways of going at it. First we may ask why other politicians fail to convince voters of the necessity of the rule of operation. Theirs is essentially an *argument by fear appeal*. Plenty of research has been conducted on such arguments (see Oskamp and Schultz: 221-222). Among the more relevant findings is the fact that at very low fear levels, the negativism carried by the message motivates the audience to discount it altogether. The strongest effects of fear appeals occur when the fear is immediately relevant and there are specific behavioural instructions (ibid: 222). A mechanism for receptivity and acceptance of arguments by fear appeals may help explain both the failures to advocate curbed public spending and the apparently effective warnings of climate change. While there are in Norway few obvious reasons to worry about the economy, there have been several signs of climatic change.

Secondly, we may ask *who* are more receptive to immoderate political promises and *when*. Carl Hovland et al. (1953) have conducted studies on how persuasibility correlates with *gender; need for cognition; self-esteem* (see Oskamp and Schultz: 223); and to a lesser degree, *age; anxiety; dogmatism; self-monitoring*, which is essentially the degree of emphasis on *consistency of belief* (ibid: 271) and *intelligence*. Whatever the results, research on these factors is highly relevant to both the *explanation from stupidity* and *the honey jar explanation*. We may find, say, that women (and of course anxious individuals of both sexes) are more susceptible to fear appeals; or that the *need for cognition* and *self-esteem* are more relevant than both *education* and *intelligence*. While inclusion of these factors will make statistical analyses more complicated (as discussed in section 5.3.2), it will, *ceteris paribus* and given the *reliability and validity of the measures* (also discussed in 5.3.2.), justify a greater

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<sup>57</sup> There is but one toll road in Sweden, claimed Hagen at a rally I attended during the 2005 election campaign. Apparently, it is on the road entering Norway.

degree of certainty in causal inferences. Theoretical assumptions favouring the inclusion of *education, income, political awareness*, etc. (cf. section 1.3) are, in any case, rarely explicit and may not be well-founded.

As for when, I have already mentioned the fact that Frp has never been in government, and therefore has never had to break any promises. This may be essential to its success. Another important factor, I expect, is the extent to which people believe the promises are obtainable. Norwegians enjoy favourable international comparisons, and the fact that Norway regularly tops UN rankings of human development and quality of life, make headlines. Yet there is a lot of discontent. Clearly, expectations are sky high. Former Swedish Prime minister Tage Erlander famously described it as *the discontent of rising expectations*. Variations of this mechanism have been invoked to explain revolutionary uprisings throughout history. It affirms that it is the variance between their perceived prosperity and their expectations that decides when people will rise in protest, not their absolute level of welfare. In Norway, it seems, actual prosperity has not risen as fast as expectations.

### **6.2.2. Formulating Hypotheses**

The sum of all this is to suggest that any explanation for the popularity of Frp must proceed from the fact that it has successfully set itself apart from the group of other parties, notably less removed and haughty. Empirical research should try to identify mechanisms translating this perception into an expressed preference for Frp and identify the contextual conditions for their efficacy. Combining elements from these explanations I shall propose two hypotheses, closely related in that the conditions for the efficacy of their implied mechanisms are thought to be essentially the same and their effects are thought to be mutually enhancing. The testing of these hypotheses is the topic of section 6.3.

I believe the author Erik Fosnes Hansen touched upon an interesting possibility when suggesting on NRKs *Redaksjon EN*<sup>58</sup> that people might vote for Hagen out of sympathy – or, more specifically, because they *identify* with his victimhood: “[When Hagen is being verbally lambasted in television debates] people feel sorry for him, because they have an uncle Gunnar just like him. And they feel he is expressing their own opinions.” In all its simplicity, I believe this is as plausible an hypothesis as any of those that were tested by the quasi-experiment of Todal Jensen and Aalberg, and I shall make it mine:

*H1 People who feel marginalised by “good society” tend to identify with politicians whom they perceive to be similarly left out of the “good society” of parliamentary party politics and sanction politicians they perceive as arrogant.*

*H2 People who feel they are not receiving their entitled share of the national wealth will wish to sanction political parties they perceive to deny them that share and, conversely, reward parties they believe not to be.*

*Under certain, largely overlapping conditions, either of these feelings will transform into an expressed preference for the party they perceive to be excluded and/or sympathetic to their feelings of entitlement. Events triggering these mechanisms will essentially occur in televised debates where Frp’s ostracisation becomes apparent.*

One essential condition is that *arguments by fear appeal* – in this case the fear of what will happen with Frp in government – are left largely ineffective. That they might well have been after the election of 2005, when that fear was no longer *immediately relevant* and the *specific behavioural instruction* to vote for a “safer” alternative no longer applied (Oskamp and Schultz: 221-222). Another important condition is that the constituency be minimally sympathetic to the actual political messages expressed by Frp. People who consider themselves ideologically at odds with Frp are, of course, less likely to give sympathy votes – especially the more *dogmatic* voters. So are *cautious*

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<sup>58</sup> On April the 16<sup>th</sup>, see footnote 61 above.



and *anxious* people, who might in any case go for the “safer” alternative. Higher *age* is sometimes associated with being more cautious, as is the *female gender*. Insofar as the justification for the *rule of operation* appears valid on closer inspection, we should expect a greater *need for cognition* (see Oskamp and Schultz: 223) to contribute to its acceptance.

As it stands, this comes close to being an idiographic narrative told in general terms, the folly of which has been pointed out by Jon Elster (1999: 1-2). What is needed is that the mechanism – as recurrent causal patterns – be established independently of the context, thus ensuring its explanatory potential (cf. sections 2 and 4). In this case we are looking for a lower level mechanism for *recognition and sanction of haughty, self-important and condescending behaviour* (and establish its conditions for efficacy). This mechanism is apparently persistent and ubiquitous – at least in Norway (Norwegians are *allergic to self-importance*, suggested Erik Fosnes Hansen, cf. footnote 60 above) and in Denmark, according to Axel Sandemose, who in *A Refugee Crosses his Tracks* (1933) labelled it “the Law of Jante”.

This mechanism seems – at least logically – consistent with evolutionary theory. Given the desirability of power and status within a group and the threat self-assured people pose to such aspirations, such sanctioning might well have been beneficial to individuals. Evolutionary game theory proposes models of cultural learning according to the principles of natural selection, which may explain the persistence and pervasiveness of such practises (see Alexander 2003 and Barkow et al. 1992). Importantly, such sanctioning is not beneficial when the group faces external threats and the haughty individual possesses qualities – say, a talent for leadership – that might help the survival of the group. Times of crisis might thus make voters more tolerant of haughty but capable leaders, such as Sir Winston S. Churchill. I am sure that psychiatrists and psychologist have much more to say on this mechanism and the conditions for its efficacy. In the following section, I shall make do with what is intuitively plausible and outline possible designs for testing it, among them an

experimental design similar to the quasi-experiment conducted by Todal Jensen and Aalberg (cf. section 3.2.1).

### **6.3. Testing the Hypotheses**

On a general note, any number of considerations might contribute to the preference of Frp. No one of them might alone be sufficient for a person to state that preference, but several might be necessary or at least essential in causing that preference. Open ended qualitative interviews might provide insight into the considerations that the interviewee's himself perceives to be important. Experiments might help us see through possible self-deception, insincerity or other sources of unreliability.

While such methods might provide valuable insights applicable outside the situation of the interviews or experimental setting, we will of course never be able to establish all the reasons of all those who states a (newfound) preference for Frp. Given the resources available to the *Danish General Survey of Power and Democracy* (see section 5.3), we might nevertheless establish the *pervasiveness over time* of each of the considerations we have identified and examined through qualitative and experimental research, and we might find that an ebb or flow in their pervasiveness coincides with the change in public opinion that we wish to explain. If there is a plausible mechanism connecting each of these considerations with conditions present in the environment and with the explanandum, then we have with reasonable certainty obtained a mechanism-based explanation.

Of course such time series data indicating the pervasiveness of a particular consideration are rarely available. Indeed it is not in the case for the hypothesis suggested in 6.2.2. We might try to formulate survey questions intended to make the respondents imagine the relevantly distinct situations, scattering them appropriately

throughout a questionnaire, but I would not expect practitioners to put much faith in the reliability of those answers.<sup>59</sup>

Better, it might seem, to prepare and wait until a similar situation occurs again. The broader mechanism-centred strategy was outlined in order to promote accumulation of context independently established mechanism theory, which would include the conditions for the efficacy of the mechanisms, upon which supple context-theory would be developed to guide research by suggesting what mechanisms are efficacious in particular political contexts (cf. section 5.3). With proper such theory, it might have been foreseen that a victimised Frp would perhaps more easily obtain support from sympathy or protest after the election. Comprehensive context-theory might also help determine whether relevant aspects of the situations before and after the parliamentary election in 2009 will turn out similar to those in 2005. If so, and if Frp gets a similar post election surge in 2009-2010, evidence gathered at that point will reasonably, in my view, corroborate the same hypothesis in 2005-2006. Conversely, if evidence were available to establish a similar situation in 2001 and 2002 and we had news clips and clippings to compare, we might be able to explain why the post election surge in 2002 did not reach its summit until one year after the election. On that *same principle of trans-contextual validity* based on relevant similarities – made possible to establish by detailed mechanism-theories – I shall posit the possible external validity of the experiment outlined in the following section.

### **6.3.1. A Tailor-made Experiment**

I believe the most effective way to establish evidence to support my hypotheses would be an experimental study designed to identify the posited mechanism in the situation it is thought to be most pervasively efficacious, namely when Frp was at the height of its popularity. Participants would be subjected to the posited triggering causes, namely televised political debates. While the evidence would not be directly indicative of the

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<sup>59</sup> Questions might include: *If there were an election today, which party would you vote for? If Norway were in the midst of an economic crisis, which party would you vote for? If given a choice, who would you prefer as prime minister of Norway today? If Norway were in the midst of an economic crisis, who would you prefer as prime minister? If Norway were at war, who would you prefer as prime minister?*

processes through which party preferences actually changed, it might confirm that the posited mechanism is efficacious in an experimental setting design to mimic real life situations. Now, if part of the participants could somehow be primed to deliberate as in the situation when they have actually to cast their votes, we would be able to compare the effects of the posited mechanism given varying efficacy of arguments by fear appeal, and so, I venture, obtain evidence to corroborate H1 and H2.

If there is reason to believe that the effects of the posited mechanism are heavily conditioned on specific personal characteristics, such as those mentioned in 6.2.2 above, there might be a case for choosing participants according to those characteristics. Significantly, the hypothesis is not well suited to be tested on university students. While there are certainly outsiders among them who might readily identify with the plight of an ostracised party chairman, we should expect there to be fewer than in the population in general. Given that the goal of the study is merely to *identify* the mechanism given certain conditions, not measure its pervasiveness, there is no requirement that the sample of participants mirror the general population. There cannot, however, be any unknown systematic skewness that might affect results and we shall require good variance in the factors that are important to their efficaciousness of the posited mechanism (cf. Zaller's fourth proposed improvement in section 5.3.2). Researchers might find that these requirements are achievable by posting and promoting a non-anonymous questionnaire on the internet, and then choose participants according to those responses.

As opposed to Todal Jensen and Aalberg (cf. section 3.2.1), I would have participants watch carefully preselected televised debates (they cannot be so old as to deal with outdated issues) in which Hagen is subjected to verbal bashing of varying degrees of severity and in which he exhibits varying degrees of his countenance of victimhood, of which he is so famous. I would expect, then, that posited effect were strongest among the participants who had watched the severest bashings. In order to mimic as closely as possible the actual situation in which people watch debates at home, I would not have them answer any questions beforehand, but make it possible to identify each

participant's previously completed questionnaire, thus knowing their previous party preference. I would have each participant watch only one debate in its entirety, not only theoretically relevant parts. Obviously, this is an ideal setting. Given limited resources and a maximum number of participants, one shall have to compromise on some conditions.

Given the nature of the hypothesis, notably the implication that viewers are swayed not by explicitly weighed substantial arguments, but by affective reactions of which they are not necessarily conscious, novel measures seem required (cf. last paragraph of section 5.3.1). Various indirect methods of measuring such "implicit attitudes", are discussed in Oskamp and Schultz' textbook *Attitudes and Opinions* (2005: 67-87).<sup>60</sup> In section 5.3.1 I mentioned one: the method of *thought-listing*. The possibility of measuring attitudes without relying on self-report is a venue not much explored in Norwegian research on public opinion, possibly for good reasons. There are also ways to measure physiological responses, such as heart rates and blood pressure, to indicate the intensity of an emotion (ibid: 71-72). Issues of measurement validity and reliability remain unsolved (ibid: 87), but they are being addressed.

If we could establish unequivocal evidence to the effect that participants who react most strongly to the bashing of Hagen are also most prone thereafter to express a preference for Frp; and furthermore that this effect was strongest for those who were not primed to deliberate as when casting actual votes, it would, I believe, strongly corroborate my two hypotheses. The external validity of this experiment is not based on the sample's representativeness of the general population, nor, as with Todal Jensen and Aalberg's quasi-experiment, based on showing participants authentic debates in real time (2004: 336); it is based on identifying relevant conditions in the real world that correlated with the changes we wish to explain and having those conditions vary sufficiently similarly in an experimental setting.

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<sup>60</sup> Psychologist Mahzarin Banaji at Harvard University is among the most important contributor to the study of these implicit processes involved in *social perception* (Oskamp and Schultz: 68). See e.g. Banaji and Bhaskar 2000.

#### **6.4. Concluding Remarks: A Viable Strategy?**

It is clear that the possibility of obtaining a well corroborated mechanism-based explanation for the rise of Frp depends in part on a collective adoption of the broader strategy outlined in section 5. Without well developed and broadly supported theory to guide, setting up costly research designs to identify particular mechanisms may not be economically justified. A collective effort to develop a body of conceptually integrated theories of mechanisms will help develop statistical surveys more relevant to specific contexts, upon which experiments may be conducted to examine particularly subtle or important mechanisms.

Some may object to the lack of emphasis on actual policy differences in my discussion of the Norwegian PP. The diminution is deliberate. I consider actual policies to be a subset of many factors possibly contributing to the explanation of parties' popularity, one which I chose not to discuss in this paper.

#### **7. Recapitulation and Conclusions**

I have taken it upon myself to pronounce a sentence on the explanatory potential of correlational studies that may seem both harsh and somewhat ambitious. I am acutely aware of my shortcomings as an appraiser of scientific practises and the impertinence with which practitioners may charge a student coming down with abstract philosophical concepts to affirm the failure of practises that may have been decades of trials and errors in the making. I would not dare did I not allow the possibility that the daily preoccupation with finding practical solutions to test conventionally posed questions might not make it slightly harder to have a critical look at those conventions. The unfettered views of an outsider might in this case, I hope, justify their utterance.

I have stressed that my criticism proceed on a strictly defined concept of "explanation", which does not correspond to its ordinary use in the discipline of public opinion research. If the concept implied by that use is at all thought to be grounded on any philosophical theory, I cannot think of any other candidate than Carl Hempel's DN

model – or, more precisely, the *theory of inductive statistical explanation*, according to which an explanation is successful to the extent that it confers high likelihood on the occurrence of its explanandum event. This theory is beset with philosophical problems, some of which I mentioned in section 2.1. An overview of further criticisms is provided in Woodward 2003: section 2.

It is not the technical philosophical problems of this theory that motivates my criticism. While my arguments correspond largely to Wesley Salmon's, they are not strictly philosophical, but appeal ultimately to an intuitive dissatisfaction with purely statistical explanations. By way of addressing Q1, I have argued to the effect that by answering questions of *how* and *why* mechanism-based explanations will make inquirers *epistemological more content*. I have often heard it claimed in lectures and elsewhere that a given factor “may explain” another, in effect because they are correlated and because it is theoretically reasoned that the explanandum comes first. For some time I accepted that they *might explain*, expecting eventually to be provided with more satisfactory accounts. These I have found only outside strictly conducted social science, in ruminative essays and journalism, where ostensible explanatory force is bought at the expense of testability.

It is easier, I have noted, to criticise existing practises than to propose viable new ones. The fundamental thesis of this paper is, as stated in section 6.2, that *lower level theory capturing patterns of causal processes in the human mind or precise renderings in general terms of demonstrably efficacious mechanisms with which we are familiar, may help bring forth unclear ideas hitherto expressed mainly in unscientific but tentatively explanatory essays and turn them into more subtle and fine grained scientific hypotheses than those presently dominating the field*. This formulation contains the idea that the mechanism theories to which it refers – whose corroboration will produce epistemologically more satisfactory explanations, but whose testing also beget the very problems addressed by Q3 (cf. section 1.1) in sections 5 and 6 – not only present these methodical *problems*; they are also part of the possible *solutions*.

It is part of my proposed solutions, firstly, by helping to generating better hypotheses. In section 5 I discussed the possibility of making social science a more collective and cumulative for explanatory insight. To that end I have proposed an alternative strategy for developing theory, beginning with detailed examination and descriptions of particular mechanisms independently of any particular discipline, upon which, in turn, the particular discipline of public opinion research may piece together a more supple common theory to guide research towards explanations of any phenomenon. Secondly, more detailed theory may help to guide both correlational and experimental studies to identify mechanisms – once they are hypothesised as explaining a given phenomenon (cf. section 4.3.2 and 5.3).

When addressing Q2 in section 4.2 I distinguished two ways in which mechanisms may explain: through *reduction*, i.e. explicating the way in which a phenomenon contributes causally to another by identifying the particular processes through which that causation occurs; or through *generalisation*, i.e. by identifying a general pattern of causal processes with which we are familiar connecting the phenomena included in the explanans and the explanandum. Which of these explanatory roles an explanatory mechanism may fulfil, does not, I argued, depend on the type of mechanism, but the data available on it and the situation in which we wish to identify it (cf. section 4.1). Reduction, in short, is possible when we have sufficiently precise theory and data. The respective problems of corroborating these two types of explanations were the topic of sections 5.3.1 and 5.3.2. The main point of those sections was to show that alternative designs are available and relevant methodical problems are not unique to the mechanism-centred strategy, but seem to be addressed by contemporary statistical theory.

In section 6 I have attempted to employ the ideas I have laid out in suggesting testable mechanism-based hypothesis explaining the rise of Frp, in particular during the post election period of 2005 and 2006. I believe I have succeeded in conferring greater plausibility and explanatory force to otherwise purely idiographic and narrative hypotheses – insofar as the mechanisms to which they refer are real. The *practical*



viability of the mechanism-centred strategy, however, remains undemonstrated. I have not been able to show conclusively that the hypotheses in section 6.2.2 are actually testable with methods reasonably available. I have only pointed to reasons to believe the methodical problems arising with such hypothesis are surmountable.

I pointed out in the conclusion of section 6 that the possibility of obtaining a well corroborated mechanism-based explanation for the rise of Frp depends in part on a collective adoption of the broader strategy outlined in section 5. This proposed strategy constitutes the main and most novel idea of this paper. One may dismiss the whole idea of focusing research on the task of identifying causal mechanisms, but still accept that social science needs to become a more concerted and interdisciplinary oriented effort to accumulate knowledge. With the introduction of mechanism theory and context theory I have, however, outlined what seems a perfectly viable strategy for obtaining this, without, at least not in the long run, having to buy epistemologically more satisfactory explanations at the expense of more general theories to approach political situations.

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<sup>61</sup> This introduction is largely identical to Hedström, Peter and Richard Swedberg; 1996: “Social Mechanisms”, in *Acta Sociologica* vol. 39, no. 33.

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