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Economic Theories and the
Science of Inter-Branch Relations

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Abstract

Once again, the relations between the branches of government attract the attention of increasing numbers of political scientists. The new literature raises questions such as the (a) determinants of the delegation of powers by one branch to another, (b) the resulting levels of discretion of the delegate, and (c) the control mechanisms available to the delegating branch. The analysis draws heavily from a handful of economic theories, such as principal-agent, the positive theory of agency, transactions cost economics, and incomplete contracts theory. This article (1) differentiates between those theories, (2) argues against mixing those theories, and (3) makes a strong and comparative point in favour of re-directing inter-branch relations studies towards transactions cost economics. Yet, a truly consistent political-scientific theory of transactions cost economics has not yet emerged. The conclusions point to the way forward for the construction of such a theory.

Keywords

Branches of government; Policy-making; Principal-Agent; Transaction cost economics

'Ultimately nothing rests on authority; the final court of appeal is intrinsic reasonableness.' (A. Whitehead 1929: 39)

'...complexity can and often does serve as an inducement rather than as a deterrent.' (O.E. Williamson, 1985: 15)

1. Introduction: The promise of theoretical unification*

A relatively new line of political-scientific literature analyzes inter-branch relations with the help of economic theories. Within that literature, the relation between the legislative and the executive branches of government is particularly salient. The main questions relate to the delegation of powers, the content of those powers, and the control of the executive by the legislative branch of government (e.g. Weingast & Moran 1983, Epstein & O'Halloran 1999, Franchino 2004). We have thereby gained valuable knowledge on legislative and bureaucratic politics, constitutional law, and democracy more generally.

Considering that literature, this article makes the following two points:

1. Inter-branch relations analyses must be based on, and aim at, careful theorizing. This implies a choice between different theories.¹ Regarding that choice, the most promising option is the theory of transaction costs—not principal-agent nor incomplete contracts theory; and
2. Regarding the logic of political transactions cost-economising, several authors have noted that legislators may aim at both production cost- and transaction cost-minimization. Yet, a considerable degree of ambiguity persists in these conceptualisations. Section 4 shows how we can rectify these approaches.

More analytically, the increasing number of scholars working in this field focus on the following two research questions. First, when do legislators delegate powers to the bureaucracy? This question can be formulated to apply to national, sub-national, or international legislators, and to the corresponding level of bureaucrats. We observe that in some cases legislators make policy themselves, while in others they delegate some responsibilities to bureaucrats. The goal is to explain that variation, and thereby to gain insight in constitutional law and politics, and in public policy-making.

Second, how much discretion does delegation afford to bureaucrats? We observe that some bureaucrats are offered the opportunity to develop policy autonomously, while others have to cope with constant and pervasive political interference. The goal is to understand both variations in the decisions of powerful legislators to tie their own hands, and variations in the outcomes of ex post delegation confrontations. The link between this and the first question should be clear: when legislators agree to tie their own hands, and when this leads to them loosing in subsequent confrontations with bureaucrats, delegation is truly meaningful—and vice versa.

The literature has been developing rather quickly. Important theoretical and empirical studies have been carried out in many empirical contexts, with national, international, inter-temporal, and inter-policy variations. The foremost purpose of this paper is to offer important theoretical clarifications that apply to all empirical settings. These clarifications regard (a) the role of theory, (b) the important but

* I acknowledge the financial support of the European Commission under its Marie Curie Programme. I thank Adrienne Héritier and Elia Marzal for their infallible support and their continuing encouragement. All remaining errors are mine.

1 A companion paper discusses the theoretical and epistemological issues raised by our consistent failure to proceed on the basis of conscious, research-specific, and explicit choice. It argues that we have been unduly trapped in a path-dependent sequence that favours the initially contingent and inefficient choice of principal-agent. Another companion paper presents the economic theories discussed here in greater detail.

largely unnoticed differences between various economic theories, and (c) the comparative advantage of transaction costs economics ('TCE')—though only if it is 'true' TCE.

Insofar as part of the newer literature is based on TCE, it should offer a promise of unification (or at least dialogue) between Americanist political science (e.g. Moe 1984, Lohmann & O'Halloran 1994, Epstein & O'Halloran 1994, 1999, Martin 1997, Huber & Shipan 2000, 2002), Europeanist political science (Majone 1996a, 1999, 2001, Moravcsik 1998, Franchino 2001, 2004), and perhaps even economics (Williamson 1990, 2000). Regarding Europeanist and Americanist political science, they both increasingly refer to TCE for the purpose of developing explanatory hypotheses on the variance of delegation of powers to, and discretion of, bureaucrats across policies. The resulting independent variables include (1) political expediency that benefits re-election minded politicians, (2) divided government and its ramifications, (3) decision rules in the legislature, (4) the technicality of a policy area, and (5) demand for credible commitments. (Some of these variables are sometimes presented separately and sometimes in a causal chain.) The dialogue between political science and economics represents a more elusive goal. Although the prospects for sustained inter-disciplinary dialogue look good, the Holy Grail of unification is still out of reach. Even within political science, unification is conditional on the clarification and classification of many conditions. This article is a first attempt to achieve this.

Section 2 argues for a return to careful theorizing. Most political scientists concentrate on empirical tests of previously developed hypotheses. Yet, we would greatly benefit from re-allocating our limited resources to more careful theorizing. Section 3 argues in favour of pursuing work in the TCE tradition. It first shows that inter-branch scholarship has referred to at least three distinct economic theories (often confusing them): principal-agent ('PA'), TCE, and incomplete contracts theory ('ICT'). It then argues that TCE is both sufficient and necessary as a theoretical informant to our endeavours, while the other two theories are not. Section 4 argues that existing works on transaction cost politics have made important contributions, but also that they are based on several problematic or outright false premises. On the positive side, it lists the minimum set of elements for a consistent theory of transaction cost politics. Section 5 offers some additional conceptual and methodological thoughts and concludes.

2. The place of theory in political science

The central question addressed in this article regards the choice of a theory among a menu of competing economic theories. But a preliminary question is, Why bother about theoretical details, and not let the data sort out the theory?

2.1. Limited resources and the choice between theory and method

Some influential scholars have contributed to downplaying the role of theory, arguing instead the primacy of method. For King, Keohane & Verba, *'we cannot develop a theory without knowledge of prior work on the subject and the collection of some data, since even the research questions would be unknown'* (1994: 19). Thus, *'the content is the method ... The content of 'science' is primarily the methods and rules, not the subject matter, since we can use these methods to study virtually anything.'* (9)

Some classic contributions [Lijphart (1971), Friedman (1953)], offer similar views. The most radical positivist view is that of Friedman: *'Truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions...'* (1953: 14; see also 19-20). Inasmuch as these authors prioritize the quantitative logic of inference, their argument implies that researchers with limited resources should opt for method, at the expense of theory.

A different view is that theory should take precedence over method. In that view, theory should be acknowledged as the most important element of (social) scientific inquiry. We start by framing a

research question in terms of a theory, and (ideally, but not necessarily) we finish with a generalisable argument that is a contribution to theory (Coase 1994: 16-17, Scharpf 1997: 22-34, Elster 1998, Johnson 2002). For example, Coase responded to Friedman's view that we could study leaves on a tree as if they '*knew the physical laws determining the amount of sunlight...*' (1953: 19) in the following terms: '*Let us suppose that it is true that the assumption that a leaf subscribes to Scientific American and the Journal of Molecular Biology and that it understands what is contained therein enables us to predict what the distribution of leaves around a tree will be. Such a theory nonetheless provides a very poor basis for thinking about leaves (or trees). Our problem is to explain how leaves come to be distributed on a tree given that a tree does not have a brain.*' (1994: 17)

Implicit in the second view is the assumption that we are not working in a theoretical vacuum in which we do not know our research questions. The social embeddedness of scientific effort means that we do not (need to) consider absurd hypotheses just because they may turn out to be predictively relevant. In addition, correlation analyses resting on several theoretical, coding, and other assumptions, it is difficult to find true causality by analysing qualitatively interesting cases that have been revealed to us from a correlation analysis. Students of inter-branch relations have long been aware of the pitfalls of poorly theorized correlation analyses that lead to ambiguous and uninterpretable results (Weingast & Moran 1983).

Whatever the current score in the precedence debate between empiricists and theoreticians, the mere play of the match shows that many political scientists care about the reasonableness of their theoretical assumptions and hypotheses.²

2.2. The choice of a theory and the choice criteria

Having established (or opted for assuming) the cardinal (or important) role of theory, we need to make a second important decision: which theory? For any research question there are usually plural alternatives. Whether such pluralism is good or bad is a larger debate that has been addressed elsewhere (see note 1). The fact is that we must choose.

Note first that, in practice, few scholars choose their theoretical informant on the basis of scientific criteria. Usually, several factors converge to create path-dependent sequences, whereby initially contingent choices are reinforced to create hysteresis effects. Such factors include herding, emulation, mentoring, and immediate precedent. Crucially, they also include resource limitations that render non-choice rational.

Note second that some particularly talented scholars may be able to shun choice and directly compare different theories against empirical data. But, most of should fear that such direct comparisons create more problems than they can possibly resolve (e.g. the test becomes one among competing assumptions, not hypotheses). We therefore usually acknowledge that our intellectual limitations oblige us to descend to less abstract levels of thought, and thus to modelize. This means that we have to exclude some theories, and so the question remains: how do we choose among theories?

The answer is tricky, thorny, risky, and, in our current practice, problematic. It is tricky because choice is difficult when we have not previously worked with all possible theories. It is thorny because understanding theories is an intellectual exercise that consumes many of our limited financial, temporal, intellectual, and possibly even psychological resources.³ It is risky because (a) one has to constantly acknowledge numerous and evolving nuances without yielding to rigid typologizing, and (b) once a choice is made, there is no short-run revisionist solution. Finally, in our current practice it is

2 Note also the limits of the theory/method dichotomy: if two theories support the same empirical observations, methodological tools will be needed to disentangle them.

3 E.g. it is not always easy to maintain intact morale when reading some of the founding fathers' equations – see Akerlof (1970), Banks (1990), McKelvey (1976), and even Williamson (1975, 1984). Whence the epigram from Williamson.

problematic, because too many works do not explicate (or outright skip) that crucial step—whence the epigram from Whitehead.

On the other hand, we do have guidelines that can assist our choice. Although not everybody agrees on the hierarchy of choice criteria, such criteria do exist. First, a theory needs to be internally consistent, logically complete, and falsifiable. It is internally consistent when its constitutive propositions are not mutually contradictory. It is logically complete when the resulting hypotheses flow from the assumptions. And it is falsifiable when it is non-tautological and specific enough to enable the deduction of hypotheses that can be proven wrong.⁴ The first step of any truly scientific inquiry consists in showing the satisfaction of these criteria. Then comes the second step, which consists in checking the tripartite fit between the theory, the research questions, and the specific empirical field in which these questions are asked. (See Coase 1972, Solow 2001, Williamson 2002. Philosophers call this ‘relevance’.) As demonstrated below, current practice in inter-branch relations does not only neglect these steps of the analysis: it often violates some, or all, of these criteria.

3. Economic theories and research in inter-branch relations

This section argues that inter-branch relations studies would benefit by adopting the lens of transaction costs-economizing, applied to politics. Nevertheless, this choice must be determined by purely scientific criteria. For this reason, I first present the three main economic theories that inform current research efforts (PA, TCE, and ICT), and then the reasons why TCE offers a more promising venue than the other two theories.

3.1. The menu of economic theories: PA, TCE, and ICT

Due to the benefits associated with careful theorizing, many political scientists have turned for inspiration to economics. In order to test Lowi’s ‘congressional abdication’ thesis (1969), a political-economic research agenda was defined. The resulting debates made a qualitative leap following the works of Fiorina 1977, Shepsle 1979, Weingast & Moran 1983, Weingast 1984, and Moe 1982, 1984. Eventually, rational choice institutionalists succeeded in furthering the intellectual frontiers of the discipline, and especially those of inter-branch relations studies. (The best review is Bendor 1988. See also Epstein & O’Halloran 1999: 14-33, Bendor, Glazer & Hammond 2001, Miller 2005.) Eventually, the initial appeal of some economic theories waned [as with the radical indolence and buccaneering theory of Niskanen (1971), or the blame-shifting thesis of Fiorina], while that of others waxed. This article focuses on theories in the latter category, and in particular on currently popular economic theories.

More specifically, the focus is on the following three theories: principal-agent (‘PA’), transactions-cost economics (‘TCE’), and incomplete contract theory (‘ICT’). Other sources of their appeal notwithstanding, these theories satisfy the aforementioned choice criteria, *at least in economics* (internal consistency, logical completeness, falsifiability, and relevance). Let us then turn to a brief presentation of each one of these theories, preceded by an indication of its current success *in political science*.

-Principal-Agent (‘PA’): asymmetric information, frictionless-ness, complete contracts, and ex ante optimization

PA models are the most popular economic theory among political scientists who work on inter-branch relations. (Bendor et al. 2001) As an example of that popularity, consider this non-exhaustive list of Europeanist works that explicitly refer to PA: Ballman, Epstein & O’Halloran 2002, Bergman 2000,

4 Trade-offs may easily creep in. For example, a theory that struggles to achieve internal consistency may introduce an ad hoc assumption, and by the same token loose in terms of logical completeness. As we shall see in Section 3, that is the case if incomplete contracts theory.

Blom-Hansen 2005, Doleys 2000, Egan 1998, Franchino 2000, 2001, Garrett 1992, Garrett & Weingast 1995, Gilardi 2001, Jun 2003, Kassim & Menon 2003, Lane & Ersson 2003, Majone 1996a, 1996b, 1997, 2001, Pierson 1996, Pollack 1997, 1998, 2002, 2003, 2006, Rasmussen 2005, Stone Sweet & Caporaso 1998, Tallberg 2002, Thatcher & Stone Sweet 2002, Tsebelis & Garrett 2001, Wilks & Bartle 2002. That list does not include works that simply refer to PA but are not applications of it, nor the dozens of Americanist studies, nor the new applications to international organizations.⁵ And, workshops on PA abound.⁶ So, PA is extremely popular and bound to develop even more. And yet, our understanding of PA seems severely flawed: popularity and ‘pop PA’ seem to develop hand-in-hand.

In economics, PA refers to a set of normative models that analyse the effects of asymmetric information on the design of resource allocation mechanisms aimed at securing efficient profit-sharing. (The intellectual foundations are Berle & Means 1932, Arrow 1964, and Akerlof 1970; the actual models are best described in Ross 1973, Holmström 1979, and Laffont & Martimort 2001.) Asymmetric information poses the potential problems of adverse selection and moral hazard, and hence leads to some necessarily positive amount of rent extraction. The principals have to distinguish between different types of agents by designing adequately discriminating incentives (i.e. incentives that have the property of minimising the value of rents). The tasks of the analyst are therefore the following: (1) treat the desired resource allocation mechanism as the unknown; (2) solve the simultaneous equations that correspond to the constraints on the principals’ profits, and, as applicable, to the agents’ participation, incentive, renegotiation-proofness, and coalition incentive compatibility constraints; and finally (3) propose a second-best contract that optimally resolves the many trade-offs faced by the principals.⁷ Once these three tasks are completed, PA analysis ends. In order to perform these tasks, PA works under a certain number of necessary assumptions:

First, all actors are assumed to be fully rational and individualistic. This implies that principals are Bayesian expected utility-maximizers, and therefore that they anticipate their agents’ subsequent behaviour, and optimize accordingly. It also implies that agents will strategically control their valuable information regarding their type and their moral hazard actions (usually, their effort variable⁸). For the purposes of analysis, this assumption means that principals are as hyper-rational as the expert analyst, and therefore that they will be able to perform the same backward induction operations as the expert analyst does. This rationality assumption has the crucial consequence of locating the action at the contracting stage, and not at the *ex post* contracting one. In other words, all potential contingent events will be fully insured at the time of contracting (i.e. all relevant premiums will be charged at the time of contracting). Hence, *ex post contracting agency losses cannot be explained in PA terms*.

Second, the main problem is the uneven distribution of information between voluntarily cooperating actors. In other words, the main issue concerns the effects of information asymmetry on market relationships. This implies that principals cannot *force* their agents to reveal information:

5 For the Americanist literature, see Miller 2005 and references therein. For applications to international organizations, see Hawkins, Lane, Nielson & Tierney 2006, and the individual chapters therein.

6 Counting only since 2002 and for top-50 Universities and associations, the list includes at least 8 conferences or workshops. More information is available on demand. (For rankings, see Hix 2004)

7 Space limitations impede the fuller explanation of these (and other) important concepts. I have just completed a project that clarifies all these economic concepts for political-scientific use. Readers who need urgent access to the research results are invited to contact me directly. The same applies to the other theories presented here, plus positive agency theory. (I have reviewed the genesis of these theories, their development, their current form, their comparative scopes, strengths and weaknesses, their applications in political science, and the problems in those applications).

8 It is important to know that most models of moral hazard are based on the assumption of egoistic indolence. Political scientists have often referred to other motives (i.e. to hidden action on variables other than effort). To the best of my knowledge, however, they have not worked out the intuitive, mathematical, and graphical implications of this. To give just one example, a second-best resource allocation mechanism would presumably differ if the agent systematically wants to do *more* rather than *less*.

agents have a credible outside option—attempt to squeeze them, and they will escape. Thus, principals have to figure out the best proposal that satisfies agents' participation, incentive, renegotiation-proofness, and coalition incentive compatibility constraints. Crucial in that respect is the fact that PA analysis does not admit that vertical integration should be assumed to reduce asymmetric information. (Though more recent efforts address the idea that different organizational forms generate different degrees and costs of asymmetric information—see Aghion & Tirole 1997.) Hence, *unless a situation is characterised by the presence of (a) demonstrable and relevant information asymmetries, and (b) a credible outside option for the agent, it is not a PA situation.*

Third, the principals may not know the private information of the agents, but the probability distribution of this information is common knowledge. This implies that the principals possess some knowledge that allows them to roughly evaluate the main trade-offs, to calculate the range of incentive feasible contracts, and to attempt to optimize accordingly. Nevertheless, the fact remains that PA models assume that the principals do not know, and cannot know, the private information of the agents. Hence, *the cognitive and behavioural assumptions of PA analysis are that actors are (a) perfectly rational, but (b) not omniscient. Assumptions of bounded rationality and/or omniscience place the analysis outside the realm of PA models.*

Fourth, since all actors are rational, both the principals and the agents have an infinite time horizon regarding their preferences. This implies that the principals already know what specific actions they would like to see being performed by the agents, and hence that their only problem is inducing those actions. Similarly, agents may vary in terms of their attitude towards risk, but they all know what functions they want to perform and what functions they don't. This assumption is intimately related to the three preceding ones, and it reinforces the focus on the contracting (as opposed to the ex post contracting) stage. Because principals (and agents) know (the probability distribution of) what they previously wanted, what they currently want, and what they might subsequently come to want, they are able to anticipate the probability of renegotiation risks, and thus include a corresponding premium in their original contract.

And fifth, the action is asymmetric: principals are modelled as active actors that evaluate feasibility and correspondingly make a take-it-or-leave-it offer to the agents, and agents are modelled as passive actors who take or leave the offer. This implies that the agents do not have agenda-setting or fence-keeping powers over the principals. Again, this assumption is intimately linked to the four preceding ones.

- Transactions-Cost Economics ('TCE'): opportunism, frictions, and ex ante and ex post transaction costs (with an emphasis on the latter)

TCE has been a relative late-comer in political science, developing about a decade later than the first Americanist PA models. Nevertheless, the pioneering efforts of Susan Lohmann, Sharyn O'Halloran, and David Epstein have attracted increasing attention, on both sides of the Atlantic. (For references, see the penultimate paragraph of Section 1.) TCE has become increasingly appealing, and it is bound to develop even more. Unlike PA, however, political TCE scholarship typically insists on the adaptations needed to translate economic theory for political-scientific use. As will be discussed below, some important issues remain unresolved.

In economics, TCE is a theory of contract that analyses the relationship between transaction costs and the need for continuous adaptation, and the effects of that relationship on the choice between different organizational forms. (The intellectual foundations are Coase 1937, Williamson 1967, 1968, 1975, and Arrow 1969; the current models are best described in Williamson 1985, 1996, 2000, 2002, Klein, Crawford & Alchian 1978, Joskow 1985, and Whinston 2002; the main political-scientific effort is Epstein & O'Halloran 1999: 34-51.) While PA analysis is a normative exercise geared towards the discovery of optimal contract solutions in different environments, TCE is a positive approach that aims at testing the theoretically-derived proposition that governance structures will vary

in accordance with the degree of hazard inherent in the corresponding transactions (i.e. firms will define their boundaries in view of economizing on transaction costs).

In TCE, the dependent variable measures the degree of vertical integration, which ranges from nil (as in so-called Walrasian auction markets and in spot markets) to full (as in outright mergers). The goal is to determine the causes of observed variations of the dependent variable. The tasks of the analyst are three: (1) name and explicate the critical dimensions with respect to which transactions differ; (2) name and explicate the critical attributes with respect to which governance structures differ; and (3) work out the logic of ex post contracting efficient alignment. To perform these tasks, TCE works under a certain number of clear and necessary assumptions, to which we now turn.

First, all actors are assumed to be boundedly rational and opportunistic. Bounded rationality means that actors are intendedly rational, but only imperfectly so. They thus differ from their counterparts in the hyper-rational (and therefore fictitious) world of PA. This difference has important ramifications. Complex contracts are unavoidably incomplete: they *cannot* contain all the necessary clauses that would secure the transaction against any future contingency, and actors *cannot* accurately evaluate (and charge) all relevant premiums at the time of contracting. It follows that actors cannot readily invoke a common knowledge of payoffs. Coupled with opportunism, it also follows that actions are not as verifiable as in PA. (Note that non-verifiability complicates the problem not only of the parties to the transaction, but also of a third-party arbiter of potential disputes, such as a judge.) Actors are far-sighted, but boundedly rational, and so the assumption of bounded rationality leads to locating the action not only at the contracting stage, but also at the ex post contracting one. Hence, *the main mission of economic organization is to deal with this uncertain world by creating adaptation-enhancing institutions (i.e. governance structures, or 'private-ordering efforts') that allow parties to engage in voluntary, ordered, and mutually beneficial exchange. Contracts are 'entire but incomplete'.*

Second, TCE assumes that the use of the market mechanism is not costless. In other words, it assumes the existence of transaction costs. Intuitively, these transaction costs can be understood as the economic equivalent of physical friction when an activity is transposed from one technologically separable surface to another. More analytically, they have been broken down into the following categories: (a) search and information costs; (b) bargaining costs; (c) costs of executing multiple contracts; (d) monitoring and enforcement costs; (e) public policy incentives to avoid the market; and (f) costs of protecting trade secrets. Note that some of these categories contradict the fundamental assumptions of PA analysis. For example, bargaining costs presuppose that the parties actually bargain, i.e. that there is no simple take-it-or-leave-it offer by one of them. Similarly, the costs of executing multiple contracts, and monitoring and enforcement costs, presuppose that appeals to a third-party arbiter (commonly, the judiciary) is not only problematic (due to non-verifiability), but also costly. Hence, *TCE differs from PA in that it assumes the presence of positive transaction costs. Some of these transaction costs refer to the ex post contracting phase (the 'execution' phase).*

Third, as it transpires in the discussion of the first and second assumptions, the main problem is to tame the negative consequences of ex post contracting opportunism. Although opportunism may or may not be nefarious, it can lead to the collapse of the projected transaction, hence to the non-realization of mutual benefit, and hence to economic inefficiency. Two crucial points need to be highlighted in that respect:

- 1) Its insistence on opportunism leads TCE to assume that restrictive contractual agreements are not malignant (i.e. they should not be systematically explained by reference to the parties' monopolising instincts) but benign (i.e. they are explainable by reference to the notion of transactions-cost economizing). Thus, for example, vertical restraints between a supplier of a commodity and a seller of that commodity will not be unambiguously explained by their desire to foreclose the market to competitors, but by their desire to secure their transaction against each other's potentially opportunistic ex post contracting behaviour. Although this assumption may appear too technical at first glance to be relevant to political scientists, its relevance is easy to demonstrate. For example, where Lowi (1969) argued that legislators abdicate by delegating

powers to the bureaucracy, a TCE analyst would argue that such delegation may be explained by reference to transaction cost-economizing strategies. (What remains to be seen, however, is the nature of the relationship between the economic concept of integration and the political concept of delegation.)

- 2) Opportunism would not be problematic (and hence TCE would become less relevant), if only the market were competitive. As in neo-classical economics, competition would have a disciplinary effect on potentially opportunistic behaviour. Where TCE becomes relevant is where markets are not competitive, and hence where market power prevails. In addition, the source of market power may be not only structural, but also relational. In other words, even in otherwise competitive markets, contractual relationships themselves may lead to a certain degree of lock-in, and hence to bilateral monopoly.

Hence, TCE belongs to the group of theories that form the ‘efficiency branch of contract’, and which includes PA (Williamson 1985: 26-29). But TCE also differs from PA, in that it concentrates on the effects of in-built monopoly.

Fourth, there is a trade-off between production cost-economizing and transaction cost-economizing. Absent transaction costs, a firm would always seek to procure and contract in the open market—perhaps to the point of self-dilution. For example, all actors would permanently scan the market in search for the best deals, i.e. for production cost-minimizing deals. By virtue of the first and second assumptions, however, such scanning and searching activities are assumed to be costly: they contribute to a rise in transaction costs. Eventually, protracted scanning and searching efforts will result in transaction costs that will out-weigh the corresponding gains in terms of production costs. To illustrate this crucial point, take the example of a shoe manufacturer who needs to buy leather that she uses as raw material for her shoes. She will scan the market in search for the leather supplier who offers the best value for money. Unless she finds that supplier, she foregoes the opportunity to economize on production costs. And, unless she negotiates an optimal deal, she foregoes the opportunity of further economizing. Nevertheless, her market search (and subsequent negotiations) cannot go on forever: eventually, it will amount to a cost item that dwarfs any possible gains. Hence, *TCE assumes a trade-off between production cost-economizing efforts and transaction cost-economizing ones. The two categories of costs are analytically distinguishable, but they both refer to the production of the same good.*

And fifth, the development of lock-ins among trading partners leads the value of the relationship to exceed the value of the trading partners’ outside options (see also point (2) in the third assumption). In a bilateral relationship, that means that one trading partner will be able to extract quasi-rents from the other partner—this is commonly referred to as the ‘hold-up problem’. Given that contracts are incomplete and that promise is not self-enforcing, actors will become increasingly opportunistic, and will thus seek to increase their share of such quasi-rents. Eventually, such quasi-market transactions will become obviously inefficient: improvements in the welfare of one party will correspond to automatic deteriorations in the welfare of the other. Firms that find themselves in similar situations (and firms that *fear* finding themselves in similar situations) might opt for taking the matter to a court of law. But, non-verifiability precludes this. Such firms will therefore either outright not enter into the initial contractual relationship that degenerates, or opt for more securing governance.

-Incomplete Contracts Theory (‘ICT’, or the property rights approach)

The least we can say as political scientists is that we have a very incomplete knowledge of ICT. (But this may be a rational strategy that minimizes our own production and transaction costs!) There is actually no political-scientific literature, be it on delegation, discretion, and control, that is so labelled. Nevertheless, a simple summary of ICT will hopefully prove that ICT has had a profound impact on the literature—including in political TCE approaches (via Epstein & O’Halloran 1999: 42). As will be shown, that problem is due to our misunderstanding of the differences between ICT and TCE.

In economics, ICT is associated with the works of Sanford Grossman and Oliver Hart (Grossman & Hart 1982, 1986; Hart 1988, 1995). They started by expressing their sympathy with TCE, and by acknowledging the presence of ex ante transaction costs, asset specificities, opportunism, and incomplete contracts (whence differentiating themselves from PA models). They also differentiated themselves from the positive theory of agency⁹, defining the firm as consisting not of a nexus of contracts between voluntarily participating individuals, but as being composed of its assets.

On the other hand, Grossman & Hart drew a clear distinction between ICT and TCE, too. They argued that TCE exhibits mainly three important problems:

- a) It exaggerates the benefits of integration, by assuming that it automatically turns hostile suppliers into docile employees.¹⁰ Thus, TCE foregoes the opportunity to analyze power relationships, and their institutional objects and effects;
- b) It does not distinguish between (i) activities that are carried out via contract between separate owners, and (ii) activities that are carried out in a single ownership unit. Thus, TCE does not answer the question of what limits the size of the firm; and
- c) TCE is unable to distinguish between a structure composed only of in-house employees, and a structure composed only of exclusive agents. Thus, it fails to give concrete meaning to the term ‘integration’.

For these reasons, Grossman & Hart relied on the concepts developed in the classic literature on property rights (e.g. Alchian & Demsetz 1972). They distinguished between the situation where a contractor uses the production facilities that the firm provides to him, and the situation where the contractor uses his own production facilities. ‘*If the firm owned [all the] important assets of the independent agents, then we would say that such a company had the same degree of integration as a company in which the retail sales force was composed of ‘employees’.*’ (1986: 694)¹¹ On this basis, they made the following four groups of assumptions.

First, at the beginning there is a competitive market in identical potential trading partners. That market determines the ex ante division of surplus between the principal and the agent. Optimization involves maximizing the principal’s benefit subject to the agent’s participation constraint. (Unlike in PA models, there is neither uncertainty nor asymmetric information, and thus no incentive compatibility constraint. Unlike in TCE, there is common knowledge of payoffs, costless bargaining, and hence no ex post alignment problems.) Second, the payment method used by the principal to compensate her agent (whether he is an employee or an independent contractor) is some function of the observable states of nature and the observable performance of the parties to the contract. Third, contracts are incomplete, in the sense that ex post residual rights, ex ante investments, and ex post

9 The positive theory of agency (‘PTA’) is associated with the works of Michael Jensen and William Meckling (Jensen & Meckling 1976, 1979). PTA has also had a strong indirect (unconscious?) influence on political science. In a nutshell, PTA views (a) actors as varyingly rational; (b) the firm as an open nexus of voluntary contracts; (c) contracts as incomplete and subject to periodical re-negotiations; (d) the problem as consisting in defining property rights and calculating agency costs under conditions of informational asymmetry; and (e) the court system as an efficient dispute resolution mechanism. Based on these assumptions, the unit of analysis is the agent, and the temporal focus is the ex ante stage (i.e. not the allocation of ex post contracting costs). PTA influenced the political-scientific literature through several references in Moe 1984: 740. It was rejected by Epstein & O’Halloran on the grounds that, like any PA theory, it does not relate to the theory of the firm, and ‘*tells us little about the origin of this nexus or why two nexuses do not merge to form a single nexus.*’ (1999: 39). As with the rest of the economic theories discussed here, more information on PTA is available on demand. To the extent that most political scientists assume some degree of corporative-ness in the organizations that they study, this literature may not be the most relevant for our purposes.

10 This was a critique of Williamson’s assumption that the contract law that applies to private-ordering institutions is that of forbearance.

11 This important point can serve to clarify the difference between the concepts of integration and delegation. Integration means acquisition of property rights over assets, while delegation relates to contractual compensation. Delegation occurs independently of integration.

benefits to the respective parties, cannot be totally foreseen, specified and contracted ex ante. (Note that incompleteness means that some assets are not contracted upon, and hence that their owners retain full control over them). And fourth, integration in itself does not make any new variable observable to both parties,¹² does not change the cost of writing contracts (which is nil), and hence cannot be associated with complete contracting. On the other hand, integration does affect the identity of the person who has control over those provisions not included in the contract.

On these assumptions, Grossman & Hart built a two-periods model which applied equally to vertical and horizontal relationships, be they inside or outside the firm. In the ex ante period, two parties make relationship-specific investments; in the ex post period, some further (partly unforeseeable, unforeseen and thus non-contractible) production decisions are taken and the benefits from the relationship are realized. The incompleteness of the contract regarding (some) ex post decisions and benefits makes it necessary to allocate residual rights of control ex ante. In the ex post period, the state of the world is revealed, whatever information asymmetries existed disappear, further decisions are taken¹³, and the parties can renegotiate and re-contract on further actions and residual rights of control. [Such renegotiations and re-contracting are assumed to be costless; ex post costlessness is justified by the fact that the state of nature is now realized, and hence by the fact that each party now knows its own needs as well as the needs of the other. This possibility amounts to ex post efficient allocation, and renders the initial allocation of property rights ex post irrelevant. Crucially, however, the distribution of the ex post surplus (as opposed to its amount) is not insensitive to property rights: if the agent owns his production facilities, he may refuse to continue the relationship unless he extracts more surplus—or he may be prepared to give up his right for a side payment as part of a renegotiation. In turn, this will affect ex ante investment decisions on non-contractible assets.

Finally, assuming that the parties allocate ownership rights in a way that minimizes ex ante investment distortions, Grossman & Hart elaborated their view against the usual argument that integration can only expand the feasible set. Integration, they argued, distorts the incentives of the principal, and (given the unspecified nature of his residual rights) she cannot commit to intervene only selectively in her agent's operations. Hence, integration leads to overinvestment by the integrating firm, and underinvestment by the integrated one: it comes with benefits, but also with costs. Thus, the only solution consists in a clearer specification of property rights. Residual rights of control over assets (or actions) determine who owns which assets (or actions).

3.2. The science of inter-branch relations and the promise of political TCE theory.

The main argument in this sub-section is that our science of inter-branch relations could greatly profit by focusing on TCE, applied to political science. This is shown in three steps. The first step documents the proposition that political-scientists have not paid enough attention to the differences between the three economic theories. The second shows that the adoption of PA or ICT would force us to make too many heroic assumptions, and to unduly limit the scope of our investigations. The third shows that, if appropriately adapted, TCE is a necessary and sufficient theory for our purposes.

12 This was a direct criticism of Arrow (1975), who had argued that vertical integration economizes on communication costs, but who had not explained why (he had not explained why the incentives facing opportunistic agents change with integration).

13 For our purposes, it is necessary to note that each party takes such further decisions as a unitary actor: the choice on such decisions is (from an organizational point of view) unproblematic, because any subordinate can take it. In Grossman & Hart's words, '*no special skills are required ... [S]ince there are many subordinates available, none is in a position to refuse to carry out the owner's wishes or to argue about terms.*' (at 699) Note two things here: (1) This leaves the question of the existence of the firm unanswered (if no special skills are required, the corresponding decision could be sub-contracted externally without risk); and (2) This competition among subordinates is not necessarily found in all political settings.

-The political-scientific confusion of different economic theories

It is necessary to re-emphasize that PA, TCE, and ICT differ in many important respects. Each of these theories scores differently on the criteria of completeness, consistency, falsifiability, and relevance. Nevertheless, they have all been developed and applied profitably in different settings. But, start confusing those theories, and all their (varying) qualities are lost. Indeed, PA, TCE, and ICT make different assumptions about (a) actors' rationality, (b) the nature of the firm and the interaction between actors, (c) the role of non-verifiability, and thus of the judicial branch of government, and (d) the locus of action and the importance of potential ex post contracting issues. It seems fair to say that the only commonly shared assumption is that actors behave opportunistically, and therefore that promise is not self-enforcing. But, although this single common assumption results in a common interest in institutions and contracts, those theories differ.

Furthermore, due to their different assumptions, the three theories develop in distinct ways, and generate different hypotheses. PA sets out to describe optimal contracts, and thereby acquires a distinctively normative dimension.¹⁴ On the contrary, TCE sets out to analyse empirical differences in governance structures (with an emphasis on ex post delegation issues), and therefore maintains a markedly positive character. Halfway between PA and TCE, ICT sets out to discover the optimal allocation of risk-sharing and property rights, takes a positive perspective, and is limited to ex ante allocation issues.¹⁵ It should be obvious from this that these economic theories do not constitute interchangeable building blocks. On the contrary, each has its own logic and addresses its own research questions.

The classic joke about academics and light bulbs may be relevant here. (– 'How many academics are needed to fix a light bulb?' – 'What do you mean 'a light bulb'?') As things stand, the main question is not how much work is left to complete the economics-for-delegation research programme. Rather, the main question concerns what we have done with (and to) these economic theories. Only after sorting that out can we concentrate on the issue of what we can do with them. Yet, our use of these theories has consistently tended to corrupt, mix, and confuse them. Thus, however technically sophisticated our regression analyses or our analytic narratives might become, spotting a positive correlation or a plausible causal chain is (a) theoretically uninterpretable, and (b) methodologically non-generalisable.

Before going on to pinpoint specific instances of theoretical confusion in political-scientific scholarship, note that some economists have contributed to this confusion. For example, in his path-breaking (and otherwise fascinating and essential) empirical test of TCE, Joskow referred to TCE and PA, and noted that '*it would be productive to integrate the two approaches.*' (1985: 36 n.8)¹⁶ On the positive side, until recently only Williamson insisted on the differences between TCE, PA, ICT, and PTA. (Though all the foundational papers explicated their points of departure from previous approaches.) Today, the economic literature offers several rich and insightful reviews (e.g. Williamson 2000, 2002, Malin & Martimort 2000, Chiappori & Salante 2000, Whinston 2001, Brousseau & Glachant 2002, Martimort forthcoming). Unfortunately, the complex message does not seem to travel well across disciplines.

14 Extensive work in the 1980s and 90s relaxed some (but not all) of these assumptions: on the most important point (renegotiation costs due to the impossibility of full commitment) see Dewatripont 1988; Hart & Tirole 1988; Laffont & Tirole 1993: chapter 9. Interestingly, Hart & Tirole (1988) found that non-commitment may lead to the standard Coasian durable good model, and hence (in terms of the analysis) to the need to incorporate transaction costs.

15 For the sake of completeness, note also that PTA sets out to discover the consequences of 'complex information' in terms of optimal risk-sharing solutions.

16 See also Alchian 1984: 39, Milgrom & Roberts 1992, and Williamson 1996: 65. In general, many economists have argued for an integrated treatment. ... But none has fully achieved it, and recent efforts have focused on differentiating, rather than combining, theories (see the main text).

In the history of our discipline, formal deductive models of inter-branch relationships owe much to Barry Weingast and colleagues, and Terry Moe (see Bendor 1988, and Miller 2005). Space limitations prohibit an extensive epidemiologic argument here; but note that absolutely all volumes and articles on inter-branch relationships, be it in the USA, in Europe, or in the context of international organizations, refer (directly or one-step indirectly) to both Weingast and Moe. This path-dependent sequence unarguably shows that our efforts are indeed cumulative. On the other hand, it also shows our extensive reliance on past authority, and hence our vulnerability to past mistakes.

We owe to Weingast (alone or with Moran, Marshall, Noll, McCubbins, Calvert, Garrett, or Snyder) several important concepts such as ‘congressional dominance’, ‘observational equivalence’, ‘administrative procedures stacking the deck’, ‘authority vs. discretion’, etc. And yet, in his foundational work Weingast neglected to justify his choice of PA, rather than PTA, TCE, or ICT. He did not offer a justification for the assumptions of perfectly rational actors, passive executive agents, or executive agents with a credible outside option. (Though he did subsequently investigate appointment procedures, where the last two assumptions may be realistic – see Calvert, McCubbins & Weingast 1989, and Snyder & Weingast 2000.) Even more puzzling is the fact that this foundational work referred to PA (not TCE or ICT), but at the same time assumed the existence of ex post agency losses. Since, under PA assumptions, actors can only conclude complete contracts and rely for their enforcement on an efficient judiciary, Weingast’s original works exhibited important internal inconsistencies, and logical incompleteness. By the same token, they exhibited an important degree of irrelevance, since PA theory cannot be used to analyse the ex post control of the bureaucracy (i.e. one of the main questions in the field).

Turning to the other main figure in this field, Terry Moe (writing initially with Gary Miller), he also made a number of very significant contributions. Unfortunately, however, he also contributed to the confusion between the different economic theories. On the positive side, Moe consolidated the research agenda (1982), advanced our understanding of the consequences of the existence of multiple principles while engaging in significant empirical applications (1987), and divulged the economic theories while warning of potential translation problems for political scientists (1984). And yet, his work is both the exemplar and the main source of numerous confusions. He purported to present ‘*an approach perhaps best characterized by three elements: a contractual perspective on organizational relationships, a focus on hierarchical control, and formal analysis via principal-agent models.*’ (1984: 739) At the same time, however, he did not distinguish between PA, PTA, TCE, and ICT, but only between rational choice institutional approaches (including Williamsonian TCE), and the behavioural tradition in organizational analysis (i.e. Simon, March, Cyert and Olsen). Of course, Moe warned that his ‘*review is designed as an introduction, stressing the basic concepts and arguments without dwelling on complicating details.*’ (1984: 740) Unfortunately, however, he did not get the historical reconstruction of ideas right, and he therefore failed to explain the significance of these details. So much so that Williamson eventually warned Moe to revise his ‘*joinder of transaction-cost economics with agency theory...*’ (Williamson 1990: 264)

Indeed, Moe’s classification of TCE is truly problematic. He variously includes it in the ‘*New Economics of Organization*’ (which is contrasted with neo-classical economics and with behaviouralism), and partly excludes it from it, by arguing that Williamson attempted to ‘*integrate major components from the contractual and behavioural paradigms.*’ (1984: 753). Similarly, Moe did not pick up the central assumption of a trade-off between production cost-economizing and transaction cost-economizing. And, even more consequentially, he did not offer a consistent view on the behavioural assumption of TCE: at one stage he classified it as a theory of ‘*rational firms*’ (at 752), and at another stage he construed it as a theory of bounded rationality, ‘*in the Simon tradition.*’ (at 753) Again, Williamson warned political scientists about the perils of such confusions: ‘*Although transaction-cost economics and agency theory work out of a common framework ..., there are also differences (Williamson, 1988). For one thing, transaction-cost economics maintains that all complex contracts are unavoidably incomplete, whereupon a significant share of the contracting action is*

borne by the ex post governance stage of contract. By contrast, agency theory works out of a comprehensive contracting setup, whence all of the contracting action is concentrated on the ex ante incentive alignment.' (1990: 264)

Systematic failure to note that warning has resulted in a kind of disciplinary primeval soup. (Quite astonishingly, we call that soup principal-agent!) Take three illustrative examples from the fastest growing area in political science, European Union studies.¹⁷ First, Ballmann, Epstein & O'Halloran (2002) assert that comitology procedures used by Member States to supervise the European Commission is a typical PA relationship. At the same, however, they proceed to the analysis of ex post delegation maladaptations. Second, Pollack (1997, 2002, 2003, 2006) '*draws upon principal-agent models of delegation of powers by legislative principals ... to executive or judicial agents*' (2003: 4-5). At the same time, however, he discusses his transaction cost approach, its ramifications in terms of incomplete contracts and problems of credible commitments, and its empirical manifestations (e.g. 2003: 20-24). And third, Tsebelis & Garrett build a PA model of delegation, discretion, and control. At the same time, however, they assert that (a) delegation contracts between the Council of Ministers and the Commission are always incomplete (365), and (b) that their simplifying assumption that the Council is the sole legislator is a mere empirical inaccuracy (366).

- Why we do not (and should not) eat from our own primeval soup

The discrepancy between our nominal commitment to PA and our practice begs the question: Why do we not stick to what we profess? If PA is too normative, why don't we at least adopt ICT? The answer is two-fold: (a) PA and ICT make assumptions that are too strong for political-scientific purposes; and (b) applied to inter-branch relations, both theories exhibit an important degree of irrelevance. Each one of these limbs carries various smaller ones, and all deserve some consideration.

First, consider PA and its assumptions. (Note that strong assumptions can be just strong or outright heroic. In addition, a strong assumption for the analysis of one empirical reality may become heroic for the analysis of another empirical reality—but the opposite may also be true). The most fundamental and consequential assumption of PA is that of perfect rationality of all relevant actors. For inter-branch relations, this can only be considered as a heroic assumption. One reason is that we have long known that most legislative and public-bureaucratic environments are characterised by low-powered incentives and loose performance standards. Thus, although legislators may hope to engage in strategic manipulation of delegation to increase their re-election prospects, it remains true that '*the legislator's obvious powerlessness is itself a defence against constituency dissatisfaction. Lack of control over the bureaucracy can be as effective as complete control in a legislator's reelection strategy.*' (Miller 2005: 212) [Note in connection to Miller's lucid remark that it implies that politicians can always avoid the sanctions associated with (supposed) non-compliance.] A more fundamental reason is that we know from psychology that, in the absence of abundant cues and strong incentives, even deliberate modes of cognitive operation are plagued by anchoring and salience effects (Kahnemann 2003; see also Elster 1983). Combined with various political constraints to individual perfect rationality (e.g. party structures and ideologies), this means that cognitive rationality (if it operates) may not translate into instrumental rationality. And finally, in terms of empirical observations of the rationality hypothesis, we observe the opposite: complex contracts are incomplete, ex post alignment problems exist, and unintended consequences often appear. The bottom line is that the hyper-rational actors of PA are not the same as our political actors. (Absent the heroic assumption

17 By virtue of the identity of the publisher, the rank and affiliation of the authors, and the references made to them, those works form a biased sample. Nevertheless, the bias is towards well-known, top-quality, and most influential works. The empirical focus on the EU should not matter, since all authors are (or had been at the time of publication) based in top-ranking American universities. (For rankings see Hix 2004) And indeed, the same arguments are frequently heard regarding US inter-branch relations (e.g. Epstein & O'Halloran 1999: 28, Huber & Shipan 2000).

of PA-like rationality, discussion of other assumptions becomes superfluous; but let us proceed as if that were not so.)

Second, there may be good reason to believe that other PA assumptions are equally heroic, at least for our purposes. Consider, for example, the ability of the principal to make take-it-or-leave-it offers to the agent, the existence of a credible outside option for the agent, or the performance-based compensation contracts. Unless we focus on the limited topic of appointments (in which case we lose some relevance), there is no obvious reason to assume that incumbent agencies have nil negotiating powers, or that they have an option to exit the system, or that they even care about true performance. Of course, bureaucrats may have an outside option to follow the dictates of another principal, but this creates considerable analytical difficulties that we have not yet adequately addressed. Finally, regarding the assumption of a costless, efficient, and benevolent judiciary, plenty of evidence seems to prove that courts in politics are more often than not political courts. The exemplars here are the teleological interpretation of the Treaty of Rome by the European Court of Justice, and the progressive policy-making role of the Warren Court in the USA.

Note that PA assumptions are not unexplainable as such. Rather, they are all due to the essentially normative character of PA analysis. In PA, the analyst does not aim at developing empirical tests about past delegation, discretion and control patterns. Rather, her goal is to design an optimal resource allocation mechanism for the future. The most secure way of doing this (for methodological and substantive reasons that relate to the analyst's professional role) is to make these strong/heroic assumptions. By the same token, however, PA analysis becomes irrelevant to our positive political-scientific concerns. Inasmuch as ours is a positive-historical discipline that is not commissioned by a (benevolent) dictator in search for an optimal constitution, we should not be interested in making such heroic assumptions.¹⁸

Let us now turn to ICT. The appealing features of ICT are numerous. For example, its emphasis on ownership of physical assets might be easily transposed to the study of political property rights, as defined in a Constitution. (Though Grossman & Hart warn that *'the formal extension of our analysis to [the ex ante specification of the quality of input, as opposed to quantity] is by no means straightforward.'* 1986: 700, 717) In addition, ICT raises a useful critique of the TCE view that *'behaviour changes when one of the self-interested owners becomes an equally self-interested employee of the other owner.'* (Grossman & Hart 1986: 692) Furthermore, ICT raises an interesting question: *'if vertical integration always reduces transaction costs ... what limits the size of the firm?'* (692-93). Taken together, these points force us to think about the difference between integration and delegation. This difference might be the single most important conceptual point for the analysis of inter-branch relations. So, why have political scientists not bought the menu of ICT? And why should they not buy it?

One reason is that inter-branch scholarship does not, and should not, limit itself to the study of ex ante definitions of property rights. Doing so would unduly limit the scope of our inquiries to the issue of delegated discretion. It would thus leave the questions of the causes of delegation, and of ex post control, unaddressed. Yet Weingast's contributions (especially Weingast & Moran 1983) have defined an ambitious research agenda on ex post issues, and this agenda cannot just be assumed away. Inasmuch as ICT limits the scope of the analysis exactly like PA, it is equally irrelevant for our purposes.

A related reason is that ICT works on the assumption of the initial existence of two separate firms, each with its own assets. Two points need to be made in that respect:

¹⁸ A counter-argument could be that political scientists perform PA analysis for normative purposes; they therefore need to establish first whether there are currently agency losses, then identify their origin, and hence propose normative solutions. The problem with this argument is that it admits that actors are currently boundedly rational. Economic PA analysis does not make such assumptions. It is a 'toy theory'.

- a) ICT focuses on the types of contract that can be achieved given the features of pre-existing institutions (i.e. institutions are defined exogenously). That contrasts with TCE, which '*proposes a logically consistent theoretical framework to endogenize the forming of both institutions and governance structures.*' (Brousseau & Fares 2000) Given our interest in the explanation of institutions, TCE should be preferred.
- b) In ICT integration corresponds to the rational acquisition of one firm's assets by the other, where such acquisition is less costly than drafting fully contingent contracts. Two points need to be noted here. First, ICT suffers from a fundamental problem of inconsistency and completeness. It does not explain why rational actors cannot write complete contracts. More specifically, in order to explain the existence of writing costs, ICT refers to bounded rationality, in a world that is otherwise perfectly rational. Second, in ICT property rights are transferred through payment. It is payment that causes the selling party to lose its property rights, and the buying party to own them. ICT does not consider any other mechanism that has the property of securing a transaction and of mitigating conflict. In that world, there is no forbearance, cooperation, or ex post governance; there are only paid-for property rights (Grossman & Hart 1986: 698-99). Transposed to the study of inter-branch relations, this means that delegation decisions should be supported by inter-branch payments effectuated by the receiving branch to the benefit of the delegating branch. The difficulty in identifying such objective payments may be one of the chief justifications for political scientists' preference for other theories.

A third reason relates to the content given by ICT to the assumption of contractual incompleteness. Contrary to TCE, in ICT contractual incompleteness does not result from the parties' bounded rationality, nor from uncertainty.¹⁹ (This is consistent with the ICT assumption of symmetrically distributed information, which reduces uncertainty.) Rather, it results from the inabilities and inefficiencies of the judiciary. It is the courts' inability to enforce certain agreements that pushes the firms to agree on transfers and on particular renegotiation schemes. Applied to political science, this means that an ICT model would need to incorporate two components: (a) a judge that is unable to enforce an optimal level of transactions, this being due to problems of non-verifiability; and (b) a legislative and an executive branch that are perfectly rational. The cost of incorporating these views seems too high. First, politics may differ from economics in that contracts that produce observable effects for the parties produce observable effects for the judge, too (and vice versa). And second, the assumption of a boundedly rational judge in a world populated by other rational actors introduces a degree of ad hoc-ness, which jeopardizes the possibility of arriving at a logically complete model.

-The promise of TCE for the science of inter-branch relations

The previous sections have demonstrated why (a) incompatible assumptions derived from different economic theories should not be mixed; and (b) PA and ICT might not be as appropriate to our analyses of inter-branch relations as initially thought. Proceeding by exclusion, this points to the adoption of TCE. But can that choice be based on anything more than a default condition? This subsection presents a general argument in favour of the view that TCE is (almost) sufficient. The argument here is that most TCE assumptions are not only intuitively appealing, but also readily translatable into inter-branch relations studies. One darker point, however, is the TCE assumption that the contract law that applies within organizations is the law of forbearance. The next section elaborates on this view, placing more emphasis on the 'almost'.

¹⁹ To be more specific, ICT is extremely ambiguous regarding the concept of bounded rationality. In general, actors are assumed to be rational. Nevertheless, contractual incompleteness results from writing costs that are due to bounded rationality. The text here makes a 'notwithstanding' argument, giving ICT the benefit of the doubt regarding its internal consistency.

Consider first the assumption of bounded rationality, which serves to differentiate TCE from PA and PTA. Perfect rationality in the PA and PTA fashion means that principals are always able to predict the probability of occurrence of all future contingencies, and that they can always place themselves in the shoes of the agent. On the contrary, bounded rationality in the TCE fashion means that political actors do not know all the solutions to the problems they face, that they are unable to calculate all possible outcomes of potential solutions, and that they cannot perfectly order these outcomes in a preference space. (It also means, however, that political actors will try to gain knowledge regarding all these matters, and that they will be forward-looking.) Substantial empirical evidence supporting this second view notwithstanding, there is a fundamental methodological reason why it makes sense to adopt it: given that we are less interested in normative design and more interested in explaining past patterns of delegation, discretion, and control, assuming that all data reflect a strict correspondence between original intentions and actual outcomes makes history collapse to a single identifiable point in the past. Several independent variables whose importance has been corroborated by political scientists over the decades would thereby need to be assumed away. This would create an omitted variable bias, which only normative PA can afford.

Second, consider the identity of the actors who are boundedly rational, which serves to differentiate TCE from ICT. In ICT, the legislators and the bureaucracy act under conditions of Bayesian uncertainty. They therefore behave in a perfectly rational manner. On the other hand, the benevolent judge is boundedly rational, and this eventually leads legislators and bureaucrats to an ex ante definition of property rights. On the contrary, in TCE all actors are boundedly rational. The prevailing condition is not Bayesian uncertainty, but radical uncertainty. That impedes the calculation of a probability function of future events, and this (not the judge's exceptional, and thus ad hoc, characteristics) creates contractual incompleteness. Political scientists who are not in a position to argue that (a) judges are neutral and/or less informed than other actors; nor that (b) actors implement an ex ante designed complete set of behavioural rules that solve all ex post coordination problems, correctly opt for TCE and not for ICT.

Third, the TCE assumptions regarding (a) opportunism, (b) costly spot market-like transactions, and (c) the trade-off between production costs-economizing and transaction costs-economizing do not seem to pose particular problems in political science. Assumption (a) is common to all economic theories that do not assume that actors can easily commit to their agreements, and to most rational choice institutionalist analyses. Assumptions (b) and (c) are more peculiar to TCE and ICT, but to the best of my knowledge few political scientists have openly objected to them. (We shall come back to the crucial assumption regarding the existence of transaction costs in Section 4.)

Fourth, it seems that we can readily agree with the TCE view that *'the principal dimensions for describing transactions are asset specificity, uncertainty, and frequency.'* (Williamson 1985: 72) Indeed, some political transactions are non-specific, while others are (e.g. Congress cannot demand a revision of the Merger Guidelines from anyone else than the two antitrust agencies). Some are more certain and some are more uncertain (e.g. those involving macro-economic policy may be more uncertain than those involving structural reforms). And some occur only occasionally, while others are recurrent (e.g. the Agriculture Council of Ministers of the European Union collaborates recurrently with the European Commission).

Finally, the crucial test for TCE is the assumption that the contract law of internal organization is that of forbearance. According to Arrow (1975), vertical integration might be motivated by its positive effects in terms of alleviating problems of informational asymmetry. According to Laffont & Martimort (2001), however, *'An obvious limitation of this approach is that it takes as exogenous the fact that vertical integration improves information.'* (47) But, according to Williamson, this can be accounted for by reference to the inaccessibility of courts and the resulting law of forbearance. Hence, *'courts will refuse to hear disputes between one internal division and another ... Access to the courts being denied, the parties must resolve their differences internally. Accordingly, hierarchy is its own court of ultimate appeal.'* (1996: 98) This point is crucial because it reveals the relative scope of, on

the one hand, PA, PAT, and ICT, and, on the other hand, TCE. A little more analysis is warranted to demonstrate the far-reaching consequences of that debate.

As already mentioned above, the assumption of forbearance was one of the main criticisms addressed by ICT and PTA to TCE. Based on Alchian & Demsetz's management-as-an-intellectual-deception thesis (1972: 777), Grossman & Hart (1986) argued that the TCE assumption of forbearance is illusional and deceptive because it serves to justify the unjustifiable: that integration creates immediate benefits by suppressing all internal principal-agent problems. For Alchian & Demsetz, and Grossman & Hart there is nothing, in contractual terms, to prove that the firm is different from the market. Absent intellectual artefacts, we should be able to proceed on the basis of a more realistic assumption, according to which simple integration does not guarantee any noticeable benefits.

Williamson responded with a carefully crafted legal argument, both for the private sector (Williamson 1996: 97-100), and for the public sector of the economy (Williamson 1990). Beyond its immediate technical significance, that response showed that economists argued over personnel disputes, i.e. over the workings of internal organizational hierarchies, strictly defined. For TCE, such hierarchies work by substituting the law of forbearance and fiat in otherwise opportunistic transactions. By implication, that means that TCE is not, as such, a theory of delegation, but a theory of integration. If the goal is to tame opportunism, the solution is to institutionalise fiat, and this is achieved through forbearance-enhancing integration. The general proposition is that more complex transactions will lead to more integration—not to more delegation—*because integration comes with forbearance*.

Consider now the innumerable political-scientific works that claim that legislators face a trade-off between control (where they do not delegate discretionary powers to bureaucrats) and expertise-based flexibility (where they do). Three facts stand out here. First, this is an essentially TCE approach—not a PA, PTA, or ICT one. For PA, PTA, and ICT, non-delegation does not suppress internal-to-the-organization principal-agent problems (the Irrelevance Theorem applies, see Malin & Martimort 2000). Second, in principle, that point should not pose additional problems to political scientists who study inter-branch relations. It can be readily translated into political science in the form of the following hypothesis: the more hazardous the political transactions that correspond to a policy area, the more legislators will integrate policy-making within the legislative branch of government. (As we are about to see, however, a lot revolves around the question of how we construe hazard and forbearance in politics.) And third, unless we do make that TCE assumption, the analysis of inter-branch relations collapses. In other words, unless we assume that some political organizations are indeed corporate actors (Scharpf 1997: 54), we cannot perform inter-branch analysis (whether with or without committees).

But are all legislative and bureaucratic actors truly corporate? Is there inter-national and inter-temporal variation in this condition? And, if so, what are its causes?²⁰ Preliminary results of a current research effort show that many constitutional regimes create incentives that can lead legislators to behave in ways that are not compatible with the TCE assumption of forbearance.

Clearly then, although TCE offers a promising venue for inter-branch relations research, that choice is not innocuous. All theoretical options facing inter-branch specialists come with an identifiable opportunity cost. Due to its reliance on the assumption that the contract law applicable to internal organization disputes is the law of forbearance, TCE is no exception. And yet, TCE seems to represent a choice with a lower opportunity cost than other economic theories.

Any true choice has an opportunity cost, and the same applies to the choice of a theory. Social-scientific effort should be based on permanent consideration of these opportunity costs. Yet, however

²⁰ This is the topic of a current research effort (in collaboration with Elia Marzal), which looks into constitutional- and parliamentary-law incentives for legislators to behave as if the law of forbearance applied.

well they score on completeness, consistency, and falsifiability, PA, PTA, and ICT do not score well enough on relevance.²¹ By invoking rationality, common knowledge of payoffs, and costless bargaining, these theories assume out ex post contracting maladaptations. Such ex post contracting issues being one of the main foci both of inter-branch relations studies and of TCE (which scores equally well on the other criteria), TCE is the theory to which we should be referring.

4. Transaction cost politics: the way forward

Fortunately, inter-branch scholarship has finally discovered TCE. The main reference is the pivotal work of David Epstein and Sharyn O'Halloran, *Delegating Powers: A Transaction Cost Politics Approach to Policy Making under Separate Powers* (1999). *Delegating Powers* ('DP') immediately caught professional attention, became an instant classic (Bendor et al. 2001) and easily crossed the Atlantic (Pollack 2003, Franchino 2004). But a lot remains to be done, especially in terms of theorizing. This section shows that, despite its authors' intentions, DP does not offer a TCE argument (Section 4.1). It then explicates the minimal set of elements required for a (falsifiable) TCE analysis of delegation and its effects (Section 4.2).

4.1. The unfulfilled promise of *Delegating Powers* ('DP')

DP is the first work that analyzes the possibility of developing a political theory of transaction costs.²² Yet, despite offering one of the best studies in inter-branch relations to date (in my view, ex aequo with Huber & Shipan 2002 and Weingast & Moran 1983), it falls short of its own target.

- A brief presentation of the argument in DP

The central puzzle addressed in DP is the delegation of powers from Congress to the executive, and in particular the variation of the level of delegated authority over time and across issue areas. This is an important topic, because it reflects the '*inherent tension between effectiveness and responsiveness*' in US policy-making. '*In many ways, the history of American political development from 1789 to the present can be viewed as an attempt to arrive at a manageable arrangement that allows government to be effective yet responsive.*' (5) In that context, responsiveness is enhanced when policy-making rests with Congress, and effectiveness when it rests with the executive.

The existence of the effectiveness-responsiveness trade-off explains the adoption of a TCE perspective: '*policy can be made in Congress, through delegation of authority to executive agencies, or by some mixture of these two. ... Thus, Congress' decision to delegate is similar to a firm's make-or-buy decision; hence our usage of the term 'transaction cost politics.'*' (7) In-house production has three drawbacks: (a) the cost of acquiring technical expertise; (b) constitutional veto points; and (c) '*legislative logrolls [that] tend to inflate the costs of even the simplest policy initiatives.*' (7-8)²³ But

21 In addition, PA being an essentially normative theory, it scores badly on falsifiability.

22 See also the contemporaneous paper by Huber & Shipan 2000. That paper is not reviewed here, because it is openly based on non-TCE assumptions: actors are seen as (a) cognitively rational, (b) maximizing, but (c) informationally bounded (26-7). Contrast this with TCE theory, where '*Bounded rationality ... is the cognitive assumption.*' (Williamson 2002: 440, my emphasis; see also Williamson 1985: 44-6, and Coase 1984: 231) In addition, Huber & Shipan confuse TCE and PA (2000: 27-8).

23 DP does not specify how these fixed parameters produce the alleged differential effects. Note, for example, that if logrolls are one of the foremost obstacles to in-house production, and if they are always present no matter the policy area, then we should not expect that factor to matter in terms of variation of delegation (a constant cannot explain a variable). A counter-argument could be that the absolute size of logrolls matters, and that this does vary. Yet, this would have far-reaching implications, which are not discussed in DP. Similarly, since constitutional rules and veto points are relatively

delegation is not optimal either: (d) bureaucrats may pursue their own policy goals; (e) they may seek to inflate their budgets; and (c) they may seek to increase their scope of control. (Again, there is no explication of cross-policy variance.)

The general predictions of DP are straightforward: *‘Our theory predicts, then, that policy will be made in the politically most efficient manner, be it through direct legislative action, through delegation to executive branch agencies, or through some combination of these two. Note the term ‘politically’ efficient; we make no claim that policy making under separate powers will be technically or economically efficient, allocating resources to their greatest advantage. ... Rather, we claim that policy will be made in such a way as to maximize legislators’ political goals, which we take to be reelection first and foremost.’* (9)²⁴

After (a) offering a literature review of PA models of delegation and control; (b) differentiating its own argument on delegation from three classic arguments (delegation for reasons of constituency relations, delegation as a regulatory lottery, and delegation as blame-shifting), and (c) presenting the economic theory of TCE and its adaptation to political science, DP turns to a game-theoretic analysis of the decision to delegate. Three general propositions are developed:

- 1) The closer the preferences of the committee to those of the median floor voter, the less likely Congress is to delegate authority to the executive.
- 2) The closer the preferences of the president to those of the median floor voter, the more likely Congress is to delegate authority to the executive. And
- 3) The more uncertainty associated with a policy area, the more likely Congress is to delegate authority to the executive.

Those propositions lead to nine hypotheses, most of which are derived from the combination of institutional and preference-proximity factors. The non-institutional hypothesis (Hypothesis 8) is particularly intriguing, due to the difficulty in testing it: *‘More authority will be delegated to the executive in informationally intense issue areas.’* (85) Since there is no easy definition of informational intensity and policy uncertainty, *‘We take several of these classifications and examine if any natural pattern emerges regarding informational intensity and delegation.’* (id.)

Turning to the empirical analysis, DP makes an impressive use of rich empirical data to which the authors were able to gain access. In terms of findings, the regression analyses support all the general propositions, and all the specific hypotheses. Given the wealth of data, one additional and informative test could have been made on whether legislators who knew they would not be running for re-election (due to old age, other office, etc) voted for delegation in the same way as the campaigning army of Pontius Pilates. That was not done—but enough *was* done to make DP an instant classic. Nevertheless, one question remains: does DP offer a true TCE approach?

(Contd.) _____

fixed, we should not be expecting much variation of the dependent variable. And yet, there is a *‘considerable amount of variation.’* (5)

24 Note here that the bounded rationality assumption of TCE may be progressively yielding to a rationalistic calculus approach, whereby legislators take advantage of the constitutional regime (separation of powers) in order to fool voters and thus secure their re-election. This is exactly equivalent to Weingast’s PA assumption: *‘If creating too many agencies implied that congressmen lost control over the policy decisions valuable for reelection, then they would not do so.’* (1984: 154.) To the extent that this rationalistic version of TCE and PA are (a) based on the same rationality assumption, and (b) lead to the same hypothesis, DP might have needed to explain that in greater detail.

Trouble seems here to stay if one considers the asymmetrical distribution of the rationality assumption (legislators can fool voters, but voters cannot respond by establishing corresponding performance standards). Epstein & O’Halloran deny that is the case (32-3). But, insofar as legislators do not opt for technical or economic efficiency, voters *are* systematically fooled, and hence the argument becomes inconsistent.

-Seven main problems with the argument in DP

The missing concept of integration: The most important problem with the argument in DP relates to the unspecified relation between the concept of in-house production and that of integration. In TCE, the dependent variable is allowed to take values that cover the whole range of a continuum defined, at one extreme, by spot market-like transactions, and, at the other extreme, by fully-fledged integration. In DP, by contrast, the dependent variable oscillates in a narrower continuum, which ranges from in-house production to external procurement, i.e. from congressional policy-making to delegation to the best external supplier of services. Since in-house production does not imply integration (i.e. there is not merger with an initially autonomous organization), the variation of the dependent variable is obviously not the same. DP truncates the variation of the dependent variable, excluding the value that is most crucial in TCE, i.e. integration. (Note that DP does acknowledge that delegation comes with different degrees of autonomy, where lower degrees of autonomy might be construed as partial mergers. Yet, that is not the case: autonomy is construed as inversely related to control instruments that apply to inter-firm relations, not to a merged entity.)

The missing concept of organization: DP focuses on the median floor voter and her lonely struggle to increase her re-election probabilities; it therefore describes a world of institutionally constrained choice, not a world of institution-building contract. Indeed, DP is based on the view that everything is decided by a median floor voter with fixed boundaries. First, the median voter decides everything: ‘After receiving the committee’s bill, the median floor voter, *F*, makes the key decision as to whether policy will be made through Congress alone or if substantive discretionary authority will be delegated to executive branch agencies as well.’ (57) This differentiates DP from TCE and its strong emphasis on cooperation and ‘private ordering efforts to infuse order, thereby to mitigate conflict and better realize the mutuality of advantage from voluntary exchange.’ (Williamson 2002: 440, calling this the ‘overarching argument’ of TCE.) Second, the actors in DP have fixed boundaries (see also the first objection to DP). Indeed, DP construes the committee, the agency, and the presidency as fixed and passive actors who only matter by virtue of their identity and the market signals that this sends. (For example, the model assumes out the possibility of agency entrepreneurship: 57 n.5.) The median floor voter does not engage in bi- or multi-lateral cooperation ‘of a conscious, deliberate, purposeful kind.’ (Williamson 2002: 441). Rather, she buys the best offer that is made available to her in the market. So, DP depicts a world of autonomous adaptation, and of response to varying relative prices—not to varying transaction costs and contractual (i.e. endogenous) dependencies.

The missing concept of asset specificity: DP focuses on the choice of the median floor voter between different suppliers of policy services (the committee, the Chief executive, or the independent agency²⁵), none of which is in the possession of specific assets that are indispensable to the median floor voter. In TCE, the main problem consists in the quasi-rents that can be extracted by one party in a bilateral relationship, when the other party has made relationship-specific investments and therefore has no credible option outside that relationship. In other words, TCE analysis considers what happens after the ‘fundamental transformation’, i.e. when asset specificity makes the relationship change from a ‘large numbers’ bidding situation to a ‘small numbers’ bargaining situation (Williamson 1985: chapters 2 and 4). In DP, by contrast, the median floor voter chooses to delegate to (or take back powers from) any of at least three agents, none of which has any relationship-specific degree of monopoly power. Thus, the median floor voter can choose her preferred partner, knowing that the selected partner faces competition by the other two potential partners. In that context, the fact that the chosen partner may not be able to offer credible commitments regarding a specific policy course should not raise worries of hold-ups, since potential competition creates high-powered incentives.

25 In fact, DP seems to consider several possible agents: independent agencies (which may or may not include independent commissions), the executive office of the president, a government corporation, and ‘one or more committees’ (46).

Trust your friends or trust them not? The central hypotheses in DP are all based on the intuition that legislators prefer delegating to non-enemies than to enemies (they choose to delegate to the actor whose preferences are closer to them). The idea is that the median floor voter in the legislature will delegate powers to political actors who have identical, similar, or at least non-conflicting preferences with her own preferences, because that way she minimizes the chances of being held up. As should be obvious from the previous point, that is quite at odds with TCE's 'Voltairean' view. (We attribute to Voltaire the phrase '*May God defend me from my friends; I can defend myself from my enemies.*'²⁶) In TCE, what matters most is the small numbers problem and the issue of relationship-specific investments in an opportunistic world. It is the combination of these factors that leads to the problem of hold-up and quasi-rents. And, in turn, that explains specific governance structures and, ultimately, integration. DP depicts a different world, where relationship-specific investments are secured by promise and initial preference homogeneity (friendship).²⁷ Further, the median floor voter faces a choice between different organizations to which she can delegate powers. Clearly, then, she lives in a competitive environment, and this should normally be enough to dissipate her worries about future hold-ups. The description of such a world, however elegant, mathematically- and statistically-loaded it may be, is both trivial (in the sense that it does not need to be based on inferential techniques) and far removed from TCE.

The ambiguous behavioural assumption: DP claims that transaction costs 'would seem to have relevance for any system characterized by a series of agreements between rational, utility-maximizing individuals.' (43) Nevertheless, it also acknowledges that one of the four crucial assumptions of TCE is bounded rationality, and that this inevitably leads to incomplete contracts. (45-7) But then, some confusion creeps in, and complete contracts seem to be feasible: '*Legislators can ... write detailed laws, in which case the executive branch will have little or no substantive input into policy...*' (47) And 'When legislators make all important policy decisions themselves, which is equivalent to congressional policy making, agencies have no discretion. When laws leave the details of public policy to the executive to fill in, then agencies have greater discretion. And it is Congress that chooses a point along this continuum by writing detailed or broad legislation.' (51) Inasmuch as this means that, in principle, a contract between the legislature and the executive can be complete (i.e. leave 'no discretion'), it is a far cry from the TCE assumption of necessarily incomplete contracts ... which DP also makes (e.g. '*the presence of incomplete contracts implies that Congress cannot specify the details of all future agency actions...*' at 48)²⁸

The missing historical horizon: DP starts on the assumption that the median floor voter can choose any 'agent' she wants, without regard to past relationships; similarly, it does not analyse what happens at the ex post contracting stage. Although it is argued that 'the model ... captures the key elements of policy making under separate powers and produces testable predictions' (52-3), there is actually very little in terms of actual policy-making issues. On the contrary, DP is self-confessedly limited to 'a formal model of Congress' decision to delegate' (53), where the 'model is one of sequential signalling and screening' (57). The median floor voter does not learn from the past, and she disappears in the future. The issue of adaptation at the ex post contracting stage (i.e. the central observable implication

26 Apparently, Bertolt Brecht wrote something similar: '*I don't trust him. We're friends.*' (Quoted in D. Carlton & J. Perloff, *Modern Industrial Organization*, Reading (Mass.): Addison-Wesley 2000: 396)

27 Note a further inconsistency in DP: decisions are made by the median floor voter, whose rule of thumb is partisan identity. But, her own party affiliation is not an issue (otherwise, DP would be based on legislative majorities). In one case party matters, and in the other it does not.

28 Note also in that respect that, while legislators behave in a strongly rational way, the electorate seems to be systematically fooled by the separation of powers trick. DP argues that this is not the case, since the electorate should be rational (32-3). But, if the electorate is rational (i.e. if delegation does not allow legislators to wash their hands and thereby secure electoral impunity despite being the ones responsible for delegation, monitoring, and organizational design) then what are the electoral benefits of delegation? Will not the electorate duly punish those who delegate in order to spare time for symbolic hand-shakes? All in all, there is an inconsistent assumption regarding the actors' cognitive condition.

that differentiates TCE from PA and ICT) is never mentioned in the whole book.²⁹ Thus, the ‘transaction costs’ of DP are those involved in a least-costly separating equilibrium game where the main problem is adverse selection—not those of TCE theory. Contrast this with the view that *‘Transaction cost economics maintains that it is impossible to concentrate all of the relevant bargaining action at the ex ante contracting stage. Instead, bargaining is pervasive—on which account the institutions of private ordering and the study of contracting in its entirety take on critical economic significance.’* (Williamson 1985: 29)

The Grand Betrayal:³⁰ DP does not take TCE to its logical conclusions, because at its core lies a ICT assumption that is incompatible with TCE. DP assumes that organization is inefficient, and that the political-market procurement creates PA-like problems: *‘There are political transaction costs associated with either option: the costs of making policy internally come from the inefficiencies of the committee system, while the problems of delegation stem mainly from Congress’ principal-agent problems of oversight and control, which we describe as a political hold-up problem.’* (49) It is obvious, however, that the theoretical informant of this proposition is a hybrid PA/PTA/TCE/ICT view. As Epstein & O’Halloran themselves note, *‘The importance of controlling the physical assets involved in production is the keystone of the property rights approach to vertical integration. In fact, this is the principal advantage of the property rights paradigm, as traditional transaction cost approaches [sic!] have difficulty explaining exactly why mergers can solve the hold-up problem.’* (42) It should now be obvious that this view is internally inconsistent (see Section 3). Suffice it to note Williamson’s view on the property rights approach: *‘it makes strange predictions (in that integration does not imply the unified ownership and management of two stages, A and B, but instead integration is ‘directional’: it matters whether A acquires B or B acquires A because unified [coordinated] decision-making is not attempted) and is very nearly untestable.’* (2002: 442) Thus, however fancy the statistical regressions in DP may be, and however significant the resulting coefficients, much of it may be futile. Given our current knowledge, there is no way we can meaningfully interpret the results of such an internally inconsistent model.

4.2. Minimum set of elements for a political TCE analysis

First, the behavioural assumption is universal bounded rationality. Inter-branch studies usually focus on the relation between legislators and the executive (however defined). It follows that those two organizational actors (and any identifiable organizational actors therein) must be assumed to be boundedly rational. In addition, for reasons pertaining to consistency and completeness, and because a lot of inter-branch politics depends on the capabilities of the ultimate dispute resolution mechanism, this assumption must also apply to courts. Note that bounded rationality means that all actors are intendedly rational, but only limitedly so: *‘the rational spirit approach is not coterminous with hyperrationality.’* (Williamson 1996: 8) This assumption serves to differentiate a TCE approach from PA and PTA ones. Note also that it is logically possible to postulate that boundedly rational actors are fully rational about the existence of future problems, but boundedly rational about the specific nature and timing of those problems.

Second, because of bounded rationality, contracts are inevitably incomplete. Unlike in DP, contractual incompleteness is not only due to staff and other resource limitations that supposedly distinguish legislatures from business firms. On the contrary, contractual incompleteness is also due to the prevalence of bounded rationality and to the radically uncertain character of the world in which we live. So, complete contracting is impossible in principle, not only in practice. (The concept comes

29 Of course, one could argue that DP operates on the basis of a Cartesian division of political-scientific work, and that its focus on delegation is part of a larger story. (In more technical terms, the game-theoretical model may be understood as a reduced form presentation of a more complex model that does not yet exist.) But, as the other points show, that is not the case.

30 I use the phrase ‘The Grand Betrayal’ to paraphrase Epstein & O’Halloran’s *‘The Grand Regression’* (233-36).

from economics, which proves that exceptionalist arguments about the nature of political organizations, such as in DP, fail.) Crucial in that respect is the fact that the analysis must not be limited to the contracting stage: the main part of TCE consists in investigating patterns of ex post contracting cooperation, as misalignments between means and goals gradually appear.

Third, promise is not self-enforcing. This assumption is due to bounded rationality, but also, and mainly, to the second behavioural assumption: opportunism. Opportunism means that politics (like business) is a rather bleak world, where commitment is problematic. In that world, no one is safe by virtue of gentlemanly agreements, partisan affiliation, or ideological congruence. Progress (i.e. exchange) can only be achieved by devising appropriate safeguards. Thus, opportunism leads to organization. ‘Organization’ refers to conscious, deliberate, and purposeful cooperation, if necessary with exchanges of hostages (Williamson 1996: 120-44), and, if that is still not enough, with mergers. Organization must thus be distinguished from spontaneous/autonomous adaptation to changes in relative political prices. Hence, it includes constitutions and institutions, of course, but also, and mainly, governance structures.

Fourth, TCE is a second-order economizing theory (Williamson 2000). It focuses on rules which are constrained by two higher levels of rules. The highest level consists of socially embedded rules (i.e. secular macro-level customs, traditions, etc). Given their secular character and their spontaneous appearance, these rules are not economizing ones.³¹ The second level consists of formal rules of the game (i.e. the constitutional and institutional environment, which is slightly less fixed). Here, opportunities for purposeful action may arise: that is the level of first-order economizing. The third level consists of governance structures and quasi-continuous re-alignments to match such structures with the transactions that they support. That is where TCE operates: the main dependent variable is governance structures. Finally, a fourth level consists of permanent resource allocation and employment relations, where third-order economizing occurs (and where PA or PTA are the most suitable options, provided that they are not exported by the analyst to higher levels of rules).

Fifth, TCE may be applicable to first-order economizing rules, but only if it can be shown that design opportunities at that level are more important than macro-level evolutionary processes. In other words, a big difference between economics and political science is that political actors are more focused on such rules than business firms are. It follows that political scientists may assume that adaptive economizing occurs at the level of formal rules of the game. But, again, not all political environments exhibit a predominantly economizing character at that level. For example, there is a big difference between the longevity of the US Constitution and the unstable quasi-federal constitutions of Spain, the UK, or the European Union.

Sixth, the dependent variable (i.e. governance structures, unless the conditions in the previous point apply, in which case it also includes institutions) must be allowed to vary without assuming that some values signify inefficiency. In that respect, note first that in-house production must not be considered as the maximum potential value, which should then be compared with market transactions (i.e. ‘lower’ values of the dependent variable). On the contrary, careful consideration must be given to the political equivalents of the economic concepts of integration (mergers). Note second that, unlike in DP, where the assumption is that internal organization implies inefficiencies, a TCE view must be based on the assumption that such inefficiencies become comparatively expensive only when ‘*asset specificity is insubstantial*.’ (Williamson 1985: 132) More generally, the issue of asset specificity (i.e. relationship-specific investments) is probably the single most important departing point for the advancement of falsifiable TCE hypotheses.

31 Herein lies the main problem of voluntaristic political analyses, which assume that there can be first-order economizing at the level of embedded rules. The exemplar is the neo-functionalist conjecture on European integration, which assumed ever since the late 1950s that the sunny weather of Messina was a good test of rainwear for Brussels and the Ruhr industrial heartland.

Finally, *'operationalization entails naming and explicating the critical dimensions with respect to which transactions differ, naming and explicating the critical attributes with respect to which governance structures differ, and working out the logic of efficient alignment. The general argument is this: more complex forms of governance are reserved for more hazardous transactions.'* (Williamson 2002: 441) Two points need to be clarified here:

- a) First, TCE in a contractarian theory that focuses on governance *structures*—not on the nominal identity of potential co-operators. Actors are not looking for friends, but for credible commitments. In times of trouble, exchange of hostages, or even integration, with an enemy is safer than the promise of a friend.
- b) Second, the most critical dimensions with respect to which transactions differ do not necessarily relate to the nature of the final product. Rather, there is a more general logical category that applies first, that of strong bilateral inter-dependence (i.e. asset specificity).

5. Conclusion

Of course, these elements are only the starting point of a political theory of TCE. As things stand today, a political scientist who contemplates using TCE must also decide on the following more 'open-ended' questions:

- 1) How much can/should governance structures and/or institutions be assumed to be functional; and conversely, how much can/should they be assumed to reflect power politics? (On that issue, see Elster 1989: 71-81 and 99, Moe 1990, Fiorina 1990). Note here that TCE is explicitly located on the *'efficiency branch of contract'* (Williamson 1985: 26-9), and therefore that it is an essentially functional theory. The central explanation is derived from exchange, transactions cost-economizing, and mutuality of advantage, and not from power and power politics. On the one hand, TCE acknowledges that even functional solutions have an opportunity cost, which is measured in terms of production cost-economizing. A political TCE theory should aim at quantifying this opportunity cost in relative (i.e. comparative) terms.
- 2) What is the exact balance between, on the one hand, the 'rationality' component, and, on the other hand, the 'bounded' component of the notion of 'bounded rationality'? Although the assumption of bounded rationality is a sine qua non condition for TCE analysis, the analyst has to decide how much forward-looking actors can factor future hazards back into the initial contract. That decision will have a crucial effect on the choice of the analytic technique: if the analyst assumes that actors can meaningfully screen current and future types of partners, then game theory might be applicable (at least up to the point that it does not produce results that contradict the social embeddedness of certain values, and thus the non-economizing nature of certain institutions); if, on the other hand, the analyst believes that the actors cannot meaningfully analyze and/or predict the identity of current and future partners, then non-evolutionary game theory will not suffice, and may even damage the quality of the analysis by introducing some inconsistent elements of hyper-rationality.
- 3) Which actors can be assumed to be corporate actors, and which should be treated as collective actors? As shown above, in their seminal contribution, Epstein and O'Halloran (1999) make the assumption that every legislator will vote, and that voting will clear the political market. That implies that neither Congress as a whole, nor congressional parties, are corporate actors—which may be a reasonable assumption for the American political system, but not for other systems. On the other hand, TCE is based on the assumption that actors within an organization engage in sustained co-operation and permanent bargaining, and thus that they do not often resort to such radical measures (see also Elster 1989: 74: n.4, and Shepsle 2006 34). It follows that the analyst must carefully define the attributes of each organizational actor, most evidently by reference to the notion of forbearance.

Having explicated her answers to those questions, the analyst has to choose what to compare. As already mentioned in the text, the dependent variable of a TCE analysis measures the degree of integration between two or more contracting parties. The values of the dependent variable range from spot market-like transactions, to fully-fledged integration, with quasi-market transactions, and strong governance structures lying within that continuum. But, what causes a variation of the dependent variable? Again, as mentioned in the text, the key independent variables identified by TCE measure (a) the searching costs of using the market mechanism, and (b) the hazardous character of transactions.³² It is the combination of these two variables that leads to particular values of the dependent variable. But one question remains: where should we look for comparisons? In other words, how should we identify cases of variation of the independent variables?

There are three possible answers. The first answer consists in comparing different policies, in the hope that policies differ in an essential and systematic way, so that '*policies determine politics*' (Lowi 1972: 299), and so that politics determines transaction costs (as in DP). The second answer consists in comparing the same policy across nations, in the hope that institutions determine transaction costs (e.g. Huber & Shipan 2002). And finally, the third answer consists in comparing the same policy in the same national setting, but across different periods of time, in the hope that these periods correspond to different levels of transaction costs. Clearly, each option has its own advantages and disadvantages. Given the current stage of development of political TCE, it may be premature to argue strongly for one comparative approach and against the other two. Nevertheless, any TCE analysis must explicate the choice made between these options.

Clearly, then, TCE is neither a theory that readily translates into political science, nor a theoretical choice that can be consciously made without acknowledging its own opportunity costs. On the contrary, lots of translating work remains to be done, and some important uncertainties persist. Where we can be certain, however, is that (a) by virtue of its relevance to the questions of delegation, discretion, and control, TCE offers a promising basis on which to continue political-scientific research in inter-branch relations, (b) the theory is consistent, complete, and falsifiable, and more so than other economic theories on which inter-branch relations research has been based, and (c) these qualities would be lost if we decided to confuse TCE with neighbouring but plainly different theories, such as PA, PTA, and ICT.

32 As mentioned repeatedly in the text, according to Williamson (1983) and Joskow (1985), the main issue here should be the degree to which a transaction leads to relationship-specific sunk investments.

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