Designing Public Organizations and Institutions: Essays on Coordination and Incentives

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To my family

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Jens Dietrichson Lund, May 2013

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Chapter 1

Introduction

The substantial part of this thesis consists of four self-contained chapters. The first two use game-theoretical models to study issues of coordination: chapter 2 examines how incentives to coordinate and imperfect performance measures influence resource allocation in public sector organizations, and chapter 3 investigates how communication may (or may not) help groups coordinate their actions efficiently. Chapter 4 contains a theoretical model, but is primarily an empirical study of the relationship between budget institutions and fiscal performance in local governments. Chapter 5 examines empirically the effects of bailouts from central to local governments on the long-run fiscal discipline of the assisted local governments. Both chapters 4 and 5 use the Swedish municipalities as study objects.

While the methods are disparate, there are connections between the themes of the chapters. All the chapters treat questions of how formal and informal rules influence the behavior of agents, and how organizations and institutions should be designed in order to create adequate incentives and/or enable coordination. Another unifying theme is that all the chapters deal with agents that venture outside the traditional area of economics: markets. The agents either reside inside organizations, or are themselves organizations influenced by some institutional arrangement different from market institutions. Three of the essays focus directly on organizations in the public sector, and while the model in chapter 3 is not specific to any type of organization, the problems studied are certainly present in public sector organizations as well.

This introduction attempts to fit these four studies into the broader context of organizational, institutional, and public economics. Section 1.1 first defines the concepts of organizations and institutions, and elaborates on their close relationship. This section also compare theories of why markets, firms, and public sector organizations may be efficient, to examine why such different institutional arrangements are used to influence the behavior of agents. As the efficiency of these institutional arrangements seems to be justified by their ability to create incentives, coordinate agents, and enhance learning, I then briefly explore these three areas. The presentation in sections 1.2–1.4 is heavily skewed towards themes taken up in the later chapters; the sections should not be regarded as comprehensive surveys of the areas. The chapter ends with an overview of the main findings of each study.

1.1 Organizations and institutions

It is useful to begin with defining the subjects at hand. Allison and Zelikow (1999) describe organizations as "collections of human beings arranged systematically for harmonious or united action" (p. 145). They also separate between formal organizations and more informal ones. Formal organizations are "groups of individual human members assembled in regular ways, and established structures and procedures dividing and specializing labor, to perform a mission or achieve an objective" (p. 145). All the chapters deal with formal organizations in this sense.

I do not include organizations in my definition of institutions (as e.g. Greif, 2006), but instead follow North (1990, 2005) in seeing them as "the rules of the game" in a system. Institutions define the feasible set of choices for individuals and organizations, and shape the incentive structure. North also distinguishes between formal institutions, which are for example laws and regulations deliberately designed and imposed upon agents, and informal institutions such as norms and conventions.

One reason for studying organizations and institutions together is readily seen from these definitions: the "established structures and procedures" of organizations, such as rules, routines, and also cultures, are not much different from the laws and regulations (formal rules), and norms (informal rules) that make up institutions. Because of these similarities, organizations and institutions can be expected to influence the behavior of agents in similar ways. Throughout this introduction, I will use the term "rules" as an all encompassing term for the building blocks of both institutions and organizations, and point out when I specifically mean one or the other. A very important question for the design of both organizations and institutions is then what types of rules that should be in place in order for resources to be put to efficient use. The next section explores this question by comparing arguments for using markets and firms.

1.1.1 Markets or firms

To Hayek (1945), the question of the most efficient resource allocation system depends critically on what system that is able to use most of the existing knowledge. He argues that since much knowledge is not general, but refers to particular circumstances of time and place, decisions should be decentralized to agents that know these circumstances, and not centralized to a planner at the top of a hierarchy. Decentralization raises the question of how the actions of these decentralized decision-makers are to be coordinated. Hayek's answer to this problem is that decisions can be coordinated through the use of a price mechanism (Hayek, 1945, p. 527):

> ”But I fear that our theoretical habits of approaching the problem with the assumption of more or less perfect knowledge on the part of almost everyone has made us somewhat blind to the true function of the price mechanism and led us to apply rather misleading standards in judging its efficiency. The marvel is that in a case like that of a scarcity of one raw material, without an order being issued, without more than perhaps a handful of people knowing the cause, tens of thousands of people whose identity could not be ascertained by months of investigation, are made to use the material or its products more sparingly; i.e., they move in the right direction. This is enough of a marvel even if, in a constantly changing world, not all will hit it off so perfectly that their profit rates will always be maintained at the same constant or ’normal’ level.”

As Hayek points out, on a market every agent only needs to process a very limited amount of information, but the market is still able to move as a coordinated whole. Hayek also makes his case for the superior efficiency of markets compared to central planning by appealing to the limited information-processing abilities of agents, be it individuals or firms.

Basic microeconomic theory stresses the beneficial effects of competition on efficiency. Competition creates strong incentives for agents to provide effort in order to make profit. Competition also implies that agents whose products are not valued are driven out of business. Thus, resources in the form of physical and human capital are freed to be used in more efficient modes of production; that is, the process Schumpeter (1943, p. 81-86) named "creative destruction". In effect, competition yields strong incentives to learn how to produce new things and improve the production of existing products and services.

According to these arguments markets achieve efficiency by coordinating agents while limiting each agent's need to process information, and by providing strong incentives for effort, learning, and innovation.

Given these advantages of market production one may ask, as Coase (1937) did, why are not all transactions done on markets? Why are there firms, where transactions are not governed by the price mechanism and competition is often deliberately limited? Furthermore, not only do firms exist, but Simon (1991) claims that the overwhelming majority of transactions in an economy are not market transactions, but are made within firms and other organizations. An extensive survey of the theory of the firm or a comprehensive answer to this fundamental question is far beyond the scope of this introduction, but I will discuss a few representative examples.

Coase's answer to his own question is that there are costs of using the price mechanism: *transaction costs.* Firms exist when a transaction is more costly to perform on a market than within a firm. Williamson (1985) expresses the decision to vertically integrate transactions; that is, to organize them in a hierarchy instead of a market, as mostly dependent on asset specificity. When human and physical assets are specialized to certain usages, they are difficult to use and resell for other purposes. If complete contracts could be written, the problem of asset specificity would disappear. But if agents are not perfectly rational, contracts are necessarily incomplete and contracting parties will have incentives to capture the rents from specific investments. Costly hold-up problems such as haggling and renegotiation are likely to occur, which may give the hierarchical organizational form of firms the upper hand.

In line with transaction cost economics, property rights theory stresses the implications of incomplete contracts and ownerships of assets, and argues that integration into firms can be a means to reduce opportunistic behavior and hold-up problems (but can have costs associated with it too) (e.g. Hart and Moore, 1990). According to e.g. Holmström and Milgrom (1991) and Baker (1992), measurement problems may also cause inefficiences; if tasks are measurable to different degrees, agents' efforts may be excessively driven towards easy-to-measure tasks. The relatively low-powered incentives within firms compared to markets are therefore not necessarily a source of inefficiency, but a desirable trait.

These strands of theory emphasize incentive-based explanations for why there are firms. Grant (1996) instead explains the existence of firms as a response to a fundamental asymmetry in the economics of knowledge: the attainment of knowledge requires more specialization than is needed for the utilization of the same knowledge. Therefore, firms exist because they can create environments where individual specialists can integrate their knowledge. Simon (1991, 1996) and Radner (1993) point to the possibilities of organizations to localize and minimize information demands on each agent, in a similar way to markets, by decentralizing decisions. Simon also argues that the authority inherent in hierarchies may help agents cope with uncertainty by imposing rules that make situations predictable, and that firms have a different incentive advantage compared to markets, which is based on the human tendency to identify with group goals. For example, members of organizations frequently pursue organizational goals that are at odds with their own personal goals. This identification with organizational goals is a major source of motivation, and thus also of organizational efficiency.

Another advantage of organizations is the enhanced possibilities of learning and knowledge creation (Gavetti et al., 2007). If individuals have limited abilities to store and process knowledge, organizations as collections of individual agents provide possibilities of both greater storage and greater processing abilities, which then could be used in a coordinated way. Furthermore, innovations often arise from collaboration between individuals, something which is more difficult to achieve on a market consisting only of individual agents. Instead, innovation and collaborative problem-solving may require integration into larger entities (Marengo and Dosi, 2005).

These explanations of the relative efficiency of hierarchical organization are not necessarily dependent on any innate inefficiency of market organization, but more on the ability and efficiency of the organizations in question. As this ability changes with for example technological developments, such explanations may also provide part of the reason for the highly fluid boundary between markets and organizations that can be observed, both between societies and over time (Simon, 1991).

There are thus several, not mutually exclusive, explanations and arguments for why certain types of transactions will be made within organizations rather than on markets. However, these organizations – private firms – still *operate* on markets. The next section examines efficiency arguments for why the provision of services in some cases is governed by institutional arrangements that are deliberately designed to be different from markets.

1.1.2 Public sector organizations and institutions

Ultimately, questions of what types of services are suited for privatization and outsourcing should be settled by empirical studies, and there is an extensive empirical literature trying to do exactly this.[1] For many important services, including for example education and health, the evidence is far from conclusive though.[2] Here, I limit the discussion to some theoretical arguments.

Several arguments from the previous section can be quite straightforwardly extended to also justify why public organizations may be more efficient than firms and markets. First, sometimes low-powered incentives are an advantage. For instance, Acemoglu et al. (2008) argue that there are situations when governments may be the only owner that credibly can provide incentives that are muted enough for agents not to engage in excessive signalling. They use the example of teachers who respond to high-powered incentives connected to student achievement by "teaching to the test", rather than building up children's human capital in more productive ways. In Hart et al. (1997) private ownership does not necessarily foster increased quality as high-powered incentives may imply that private providers cut corners on things that are difficult to specify in contracts. Prendergast (2003) shows that consumer choice does not increase efficiency when consumers are unwilling or unable to make efficient choices. While there is always inefficiencies present in such situations, public organizations (or 'bureaucracies') are more efficient than private.

There may also be differences in the sources of motivation for agents in public and private organizations, or between for-profits and not-for-profits. This notion seems broadly consistent with results from empirical research of public service motivation in the field of public administration (e.g. Perry

[1] This is not meant to imply that the choice between public and private provision is necessarily made based on efficiency; political reasons may be at least as important.

[2] See e.g. Andersson and Jordahl (2011) who survey both the theoretical literature on outsourcing and empirical studies on a range of services outside the health and education sectors, Rouse and Barrow (2009) for a survey of school vouchers and student achievement, Comondore et al. (2009) for a review of private provision of elderly care, and Gaynor and Town (2011) for a survey of competition in health care markets.

and Wise, 1990; Houston, 2006) and in more recent studies by economists (Gregg et al., 2011; Kolstad and Lindkvist, 2012).[3] If agents are intrinsically motivated or identify with organizational goals to a higher degree in parts of the public sector, effort need not be lower in public organizations.

While competition creates strong incentives for improvements, it may also present obstacles for the diffusion of knowledge, for coordination among organizations, and for innovation. For example, knowledge of pedagogical methods that are firm (school) specific assets in a competitive school system, can be diffused much more freely in a non-competitive system. Thus, when the diffusion of innovations among units is more important than the rate of new innovations, non-competitive systems may have an advantage over more competitive ones. Similarly, if there are strong interdependencies among organizations in a field, competition may prevent the development of coordination mechanisms such as inter-organizational teams, as they often require an element of cooperation.[4]

Cyert and March (1963, p. 278-79) claim that certain types of innovations, such as significant technology improvements, are commonly made by firms with substantial slack. If a system is too competitive, there may not be enough slack, and hence a lower rate of significant innovations. Marengo and Dosi (2005) show that difficult problems, like the development of new technologies, are more likely to be solved within highly integrated organizations because of the need to control the strong interdependencies that characterize these kinds of problems. These arguments, while formulated about firms, are not hard to transfer to public sector organizations. Describing public universities as highly integrated organizations with substantial slack seems quite accurate for instance. In line with this argument, Aghion et al. (2008) claim that many significant technological innovations have their origins in the public sector, and supply one more argument in favour of public organizations/non-profits in regard to innovation: private firms cannot

[3]The results of both Gregg et al. (2011) and Kolstad and Lindkvist (2012) suggest that individuals with more pro-social motivation self-select into public/not-for profit organizations, as in models where organizational missions are matched to individual motivation (e.g. Besley and Ghatak, 2005).

[4]Note that this point pertains more to competition than private ownership per se. For example, a possible argument for having large private conglomerates produce publicly financed services is that this may strike the right balance between incentives to innovate and incentives to diffuse knowledge, as conglomerates would have strong incentives to spread successful innovations to all their sub-units at least.

commit to letting agents (e.g. scientists) retain the decision rights to what projects to explore and what methods to use. Thereby, fewer potentially successful options will be explored, and there are less resources left for research as firms must compensate agents for this lack of creative control with higher wages.[5]

Summing up, non-market institutions and public organizations are most likely to be more efficient than markets and firms when services require muted incentives, citizens cannot be counted on to make efficient choices, public sector organizations are more likely to attract motivated agents, and when competition may have undesirable consequences for the diffusion of knowledge, innovation, and coordination among organizations. The presented explanations of why markets, firms and public sector organizations function well thus center around similar concepts: because they provide appropriate *incentives*, because they enable agents to *coordinate* their activities, and because they enhance *learning*. The next three sections takes a deeper look into suggestions of how organizations and institutions should be designed in terms of these three concepts, focusing on public organizations and institutions, and on the themes of the subsequent chapters.

I should hasten to add that there are of course other factors that affect the efficiency of organizations and institutions. One obvious example would be the characters and skills of the individuals involved. However, the need for well-designed rules within the three areas are not likely to disappear completely, but only be mitigated by individuals that are "better" in some sense.

1.2 Incentives and motivation

Models of incentives in institutional and organizational economics primarily deal with the problem of how incentives can be created in order to align the interests of one or many agents to the interests of one or many principals, or to some societal objective.

In turn, the design of incentive schemes is very much affected by the nature of the information asymmetry between the parties to an economic relationship. A common classification is between *moral hazard models* and *adverse selection models*. In the former, the effort or actions of agents are

[5] In contrast, firms' ability to restrict the scope of agents' efforts will be more advantageous the closer the innovation is to a marketable state (Aghion et al., 2008).

not observable, whereas the outcome is to some degree observable, but not only dependent on the actions taken by the agents. In the latter, agents have information that is private, and the task is to design incentives in such a way that the agents truthfully reveal this information (Dixit, 2002). The incentive related problems studied in this thesis are more of moral hazard-type. The subsequent chapters deal with questions such as how to create incentives to coordinate resource allocation among public sector organizations, how to align the interests of planning and implementing units within organizations, and how to avoid the moral hazard problems inherent in the provision of financial assistance to troubled (local) governments. I therefore focus on suggestions from the earlier literature on how to induce effort and align motivation below.

Summing up the economic theory of organizations, Tirole (1994) mentions three ways to motivate self-interested economic agents: formal incentives such as piece wages, bonuses and relative performance evaluations; monitoring of work inputs; and lastly, career concerns. Since 1994, many other sources of motivation, primarily of intrinsic nature, have been explored.[6] In this respect, organizational economics has come closer to some strands of organizational theory, where for example identification with organizational goals has been a long-standing theme (e.g. March and Simon, 1958).

While intrinsic motivation is likely to be important, especially in the public sector, the more standard mechanisms mentioned by Tirole, which rely on extrinsic motivation, are still very much in use. As explicit, monetary incentive schemes are rare in the public sector (e.g. Heinrich and Marschke, 2010), I focus on other mechanisms here though. A simple example of a (possibly informal) rule, which may nevertheless be very effective in creating strong incentives, is the risk of getting fired for misconduct. This, and most other rules that reward or punish agents, requires that monitoring of agents is possible in some form; audits and oversights by third parties are two common forms of monitoring in the public sector. However, monitoring is costly, and may also affect agents' motivation. Thus, it may not be desirable to monitor agents to the full extent possible.

With regard to the former problem, career concerns have consequences for how organizations should structure tasks, and what types of agents

[6]Some of the most interesting models in this respect have been (co-)developed by Tirole, see e.g. Benabou and Tirole (2003, 2011).

should be hired. Dewatripont et al. (1999) find that, contrary to an explicit incentives model, the organizational principal faces a trade-off between reducing the riskiness of overall performance and enhancing effort. More specifically regarding government agencies, their model backs an argument made by for example Wilson (1989) that expanding the number of tasks typically reduces effort, and that unclear or 'fuzzy' missions reduce the incentives from career concerns and therefore also effort. These two results taken together imply that the hiring of specialists or professionals, whose talent is known to be low for all but a narrow set of tasks, will give more effort without monitoring by the principal.

Ellingsen and Johannesson (2007) describe several channels for employers to motivate their workers/agents by paying respect: symbolic rewards promoting desired worker traits; paying attention to good performance; building trust, which in turn promotes initiatives and trustworthiness; and becoming worthy managers and organizations. The latter two suggest for example that it is important for managers in organizations that in some sense build on idealism as a mechanism for motivation to be perceived as having a character in line with the ideals of the organization. For some of these sources of motivation – trust building and promoting initiative for example – it is easy to see how excessive monitoring may be contra-productive.

While non-pecuniary, these are still examples of extrinsic sources of motivation. Akerlof and Kranton (2005) include the possibilities of inducing *intrinsic* motivation by changing or affirming agents' preferences, or their *identity*, in order to motivate them to exert effort in line with organizational objectives. A particularly illuminating example used by the authors is the training of army officers. One of the main points of this training is to instill a new identity into the prospective officers, one which will make them "think of themselves, above all else, as officers in the U.S army. They will feel bad about themselves – they will lose utility – if they fall short of the ideals of such an officer" (p. 9).

Agents may also be inherently pro-socially motivated, either in the sense of deriving utility from producing (often called "warm-glow" altruism), or caring about the output directly ("output-oriented" or pure altruism) (Francois and Vlassopoulos, 2008). In models of pro-social motivation, more motivation does not necessarily imply more efficient outcomes. For increased efficiency, motivation need to be harnessed by for example having an organization commit to making the level of output dependent on an output-oriented worker's effort (Francois, 2000), or by matching agents with different motivations to types of organizations (e.g. for-profit, not-for-profit, and governmental as in Besley and Ghatak (2005)). The model developed in chapter 2 builds on the assumption that agents are motivated by some factor other than monetary incentives, but motivation is again not straightforwardly connected to better outcomes. Highly motivated agents may distort resource allocation more than less motivated agents if motivation is not properly aligned to the principal's interest.

As mentioned, extrinsic incentives may crowd out intrinsic motivation,[7] but it still seems useful to move away from an either/or view of the relationship between intrinsic and extrinsic motivation when thinking about designing public sector organizations and institutions to mitigate moral hazard problems. It seems very reasonable that most humans have a capacity for being both intrinsically and extrinsically motivated, and capable of both opportunistic and altruistic behavior depending on the circumstances (see Benabou and Tirole (2011) for a formal model that includes such changes). Judging by the suggestions of Osterloh and Frey (2000) and Ellingsen and Johannesson (2007), many mechanisms of both extrinsic (e.g. symbolic rewards and paying attention to good performances), and intrinsic (e.g. increasing worker participation to avoid creativity inhibition and building stronger personal relationships) motivation seem possible to use in public sector organizations without one necessary cancelling out the effect of the other.

While the theoretical suggestions are abundant, there is still a lack of empirically based best practice of how to design incentives and motivate employees in many, if not most, areas of the public sector. Chapter 4 contributes to one such area by examining the relationship between fiscal performance and several formal and informal budget institutions, intended to align within-organizational interests, in the Swedish municipalities. Some of these rules rely on extrinsic motivation (e.g. the risk of being replaced for managers and politicians), while others combine the two (e.g. result carry-over rules). The question whether central governments should bail out sub-

[7]See e.g. Kamenica (2012) for empirical examples. Benabou and Tirole (2003) develop a model where extrinsic motivation crowd out intrinsic when the principal has superior knowledge of the task at hand, so that the principal's choice of high-powered incentives is interpreted as a signal that the task is difficult, unpleasant or that the principal does not trust the agent to succeed.

units in fiscal distress, and if such bailout programs may be designed in a way that avoids moral hazard problems are other open questions. Chapter 5 empirically examines a bailout program where the financial assistance to municipalities was conditioned on costly efforts and the municipalities were monitored closely during the program.

1.3 Coordination

Coordination becomes an issue when the activities of one agent are affected by the activities of other agents. If so, the nature of the interdependence determines to a large extent how actively managed the interactions between agents have to be. When actions are in the agents' own interests and the consequences are easily anticipated, they do not have to be actively managed (March and Simon, 1958). But when these conditions do not hold, agents need incentives to act in the right direction, and mechanisms that help them solve the more cognitive parts of coordination problems (Hoopes and Postrel, 1999).

Motivation and incentives are thus also important for efficient coordination, but as these two subjects have been dealt with in the previous section, I focus on the more cognitive aspects of coordination problems here. A prerequisite for successful coordination in differentiated organizations seems to be that agents have a sufficient degree of *shared knowledge* (Kretschmer and Puranam, 2008), but it should arguably be important outside organizational contexts as well. In the absence of strategic considerations, shared knowledge ensures that an agent is able to anticipate the actions of other agents, and thus can adjust her own actions accordingly.

However, in many environments, shared knowledge is not present. Grant (1996) describes a taxonomy of coordination mechanisms that may help agents create shared knowledge: *rules and directives*, *sequencing*, *routines*, and *group-problem solving*. The use of more non-standardized, high interaction mechanisms in the fourth category, such as multidisciplinary teams, should increase with both task uncertainty and task complexity according to Grant. Other taxonomies (e.g. Sherman and Keller, 2011) prescribe a similar progression; circumstances that are sufficiently well-known can be *planned*, and then rule-based approaches generally work well. If contingencies arise, coordination requires transmission of situation specific information through some form of communication.

As communication and interactions between agents take time and may be costly in other regards, rule-based coordination may increase efficiency by economizing on the need for costly interactions. But rules (formal as well as informal) also help agents coordinate their actions by decreasing both uncertainty and complexity. Rules decrease the uncertainty about which actions other agents will choose, as this can be specified in the rules. Rules also reduce complexity by decreasing the need for information processing and the number of computations necessary; an agent have to know only her own part of a routine, not the whole routine.

One example that illustrates this principle is assembly line manufacturing, which is characterized by both extreme specialization and extreme task interdependence. As synchronization therefore is essential, all employees' actions are tightly scripted (Dessein and Santos, 2006). Despite the fact that the assembled product can be very complex, e.g. a car, no employee needs to know every detail of the whole assembly process. Through the rules – the separation and sequencing of tasks – the need for information transfer between employees is limited and the complexity of each employee's task is reduced.

When the environment is changing and activities need to be tailored to new information, rules are not enough to coordinate agents. Communication may then seem as an obvious way to create shared knowledge by simply transferring information about what agents intend to do. However, when agents act strategically it need not straightforwardly translate into efficient coordination, especially when communication is costly (e.g. Andersson and Holm, 2010; Kriss et al., 2012).

The effect of communication on a group of agents' ability to deal with a complex combination of tasks is not clear. Communication does not help agents cope with complexity by reducing the number of things to take into consideration, the opposite may even be the case. On the other hand, communication may allow less able agents to be helped by abler ones.

This description of how rules and communication affect coordination indicates that mechanisms which only use one or the other may not be optimal when the environment is uncertain and complex. But the taxonomies described previously, and, to the best of my knowledge, theory in general, do not give detailed guidance on how to mix rules and communication.[8]

[8] The connection between coordination mechanisms and the task environment have not been extensively tested empirically either (Sinha and Van de Ven, 2005).

For example, a communication-intensive mechanism such as a multidisciplinary team could be designed in many different ways regarding the rules governing participation, decision-making structure, and support functions.[9] The questions of when communication is able to aid coordination in groups, and how some simple rules that structure communication may help are precisely the questions investigated in chapter 3. Whether similar rules can be used in other organizational contexts, and how communication and rules should be mixed when the environment is uncertain and tasks are of greater complexity are interesting avenues for further research.

1.4 Learning

Simon (1996) describes learning as "any change in a system that produces a more or less permanent change in its capacity for adapting to its environment"(p. 100). Simon (1996), Grant (1996), and Foss and Mahnke (2011) all view learning as an inherently individual process; they argue that all knowledge is created by and stored in individuals. Consequently, organizations can only learn in two ways: either by the learning of its members, or by including new members who have knowledge that is new to the organization. March (1991), on the other hand, does not seem to agree that all knowledge is stored in individuals, but views organizational learning as a mutual process where "organizations store knowledge in their procedures, norms, rules, and forms" (p. 73). Similar views are expressed for example by Nelson and Winter (2002).

I will not settle this question here, but the distinction is not necessary for a description of how organizational agents may learn. Levitt and March (1988) distinguish between learning from direct experience, learning from the interpretation of experience, and learning from the experience of others. These three categories fit reasonably well with three concepts often featured in the literature on organizational learning: *feedback*, *innovation*, and *knowledge diffusion*. While they have been discussed in the previous sections, neither innovation nor diffusion processes are studied in the subsequent chapters. Therefore, I focus on feedback here. One could also argue

[9] In a review of the effectiveness of health care teams, Lemieux-Charles and McGuire (2006) conclude: "[u]nfortunately, taken as a whole, published studies do not provide clear direction on how to create or maintain high-functioning teams" (p. 295). Mathieu et al. (2008) contains a similar message for team processes in general.

that feedback is fundamental, in the sense that innovation and knowledge diffusion depend on feedback for their success.

The organizational learning literature often distinguishes between three learning types: *single-loop* learning occurs when outcomes that are not in line with intentions are detected and corrected, whereas *double-loop* learning corrects a similar mismatch but by changing the underlying rules, norms, and/or objectives related to the discrepancy between intended and actual outcomes. A third type, *deutero* learning, concerns learning about how to learn (Argyris and Schön, 1978; Vera et al., 2011).

A dynamically efficient organization (or a society for that matter) must arguably be good at all three types of learning. In turn, organizations require different types of feedback. Single-loop learning, the most basic type, requires just feedback on outcomes. Double-loop learning in addition requires feedback on how routines and norms connects to outcomes, and possibly feedback about the norms for what constitutes effective performance. Deutero learning adds a need for feedback on how successful different learning mechanisms are; that is, feedback from the organization's, and/or other organizations', past learning experiences (Argyris and Schön, 1978).

Successful learning of all three types requires of course more than just feedback, the incentives to learn, created by both formal and informal rules, are likely to be extremely important as well (Argyris and Schön, 1978). Combining rules for learning with appropriate incentives may often be a daunting task, especially since the nature of the feedback itself may be problematic. Levitt and March (1988) describe three problems with learning from experience: *a*) The experience provided by nature is often an inadequate guidance for future decision making, especially in a rapidly changing environment. *b*) As ordinary learning can imply stability of routines, it can lessen experimentation which could be necessary for an effective learning process. *c*) Organizational environments involve complicated causal systems and interactions among learning organizations; the complexity of such a system might produce learning outcomes that are hard to interpret.

While these three problems apply in full force to public sector organizations, the characteristics of their tasks imply that they may have even more fundamental problems. An army in peacetime is an (extreme) example of an organization that has trouble obtaining *any* relevant feedback (Wilson, 1989), i.e. even feedback necessary for 'single-loop learning'. More generally, outcomes are often not directly observable, but have to be measured by performance measures that are *distorted* and *imprecise* (Baker, 2002). In the case of the army, any peacetime feedback is likely to be a very poor indicator of how the army would measure up against the objective of defending its country for instance. In this sense, the obtainable feedback is likely to be severely distorted. An example of an imprecise measure may be the number of robberies as a measure of police efficiency. The measured number of robberies is not likely to be a very distorted measure of the true number of robberies, but it is an imprecise measure of police efficiency because it depends on many things outside the police organization's control.

Distortion and imprecision have been studied in terms of the implications for incentive schemes (see e.g. Baker, 2002; Heinrich and Marschke, 2010), but organizational learning is not a field where organizational economics has made much of a contribution (Foss and Mahnke, 2011). Indeed, in the overwhelming majority of theoretical models in organizational economics, it is assumed that the function determining how effort, resources, or some other input is turned into value, or production, is known by the agents (possibly with some error term, but then the distribution of this term is normally assumed to be known). Chapter 2 includes a simulation of how impediments – in the form of imprecise performance measures – to the feedback agents receive affect their possibilities to learn how to create value.

Chapter 2 does not make any progress regarding the question of how public organizations do learn, and what feedback mechanisms work in practice. According to Moynihan and Landuyt (2009) and Sanger (2013), these questions have not been extensively studied empirically for many types of public sector organizations, and large scale quantitative studies are especially rare. While some parts of organizational learning is bound to be specific to the nature of organization's task, the three learning types ought to apply more generally. The results of Moynihan and Landuyt (2009) point to what they call *structural* (e.g. resources, performance information systems) and *cultural* (e.g. mission orientation) factors, and some that combine them both (learning forums, employee decision flexibility), as being important for learning in public (US state) agencies. The mixture of structural and cultural mechanisms – formal and informal rules – is something that underpins the reasoning of how to achieve all three types of learning in Argyris and Schön (1978).

1.5 Overview

Chapter 2 uses a multi-task principal-agent model to examine two related reasons why coordination problems are common when public sector organizations share responsibilities: the incentives to coordinate resource allocation and the difficulties of measuring performance. The agents' task is to allocate resources between two types of activities, one which is the sole responsibility of one agent (core activities) and the other where responsibility is shared (joint activities). When targets are set individually for each agent, the resulting incentives may induce inefficient resource allocations, even if agents' motivation is fully in line with the principal's interests and measures of performance are perfect. If the principal impose shared targets among agents, this may improve the incentives to coordinate, but the success of this instrument depends on the imprecision and distortion of performance measures, as well as agent motivation and the interplay of distortion, motivation and the relative importance of the tasks. For activities that are complements (and vice versa for substitutes), the situations where shared targets have their best chance of succeeding are when agents are highly motivated by core activities, and/or performance measures overestimate the value of core activities, and the imprecision of performance measures is low.

Imprecise performance measures also affect value when agents have to learn the function that determines value. Simulations with a least squares learning rule show that the one-shot model is a good approximation in the short run when the imprecision of performance measures is low to moderate and one parameter is initially unknown. However, substantial and lengthy deviations from equilibrium values are frequent when three parameters have to be learned. Investing resources to develop more precise measures may thus be worthwhile, especially in new collaborations.

Chapter 3 (co-authored with Torsten Jochem) develops a model of costly communication with the weakest-link game as a basis and boundedly rational agents that choose myopic best replies, have limited information processing capabilities, and may occasionally experiment or make mistakes. Solving for the stochastically stable states, which can be interpreted as the most likely long-run states of the game, communication is seen to increase the possibilities for efficient coordination compared to a situation where agents cannot communicate. But as agents face a trade off between lowering the strategic uncertainty for the group and the costs of communication, the least efficient state is still the unique stochastically stable one for many parameter values. Making communication mandatory on the other hand induces efficient coordination, whereas letting a team leader handle the communication increase efficiency when the leader expects others to follow and has enough authority over the group. Simulations show that stochastically stable state is also overrepresented in the short run, especially if groups are large. The results are broadly consistent with recent experimental evidence of communication in weakest-link games.

Chapter 4 (co-authored with Lina Maria Ellegård) examines how conflicts of interest between the central and local levels regarding the importance of fiscal discipline create the need for budget institutions in hierarchic public organizations. The chapter first develops a simple model of the budget process as a motivating framework for the empirical investigation. The model suggests that, to reach the outcome desired by the central level, institutions that curb the bargaining power (that *centralize* the budget process) as well as institutions that align the incentives of the local level are needed. Moreover, budget institutions may have to be strengthened as the conflict of interests between the central and local level is intensified.

A survey is used to collect data on budget institutions and conflicts of interests in 265 out of 290 Swedish municipalities. The survey explicitly measures the conflict of interests between the central level, which is responsible for the municipality's overall fiscal performance, and the local-level committees, which are responsible for their respective sub-fields only. The regression results support the notion that the interactions between institutions and conflicts of interest are important, as the estimated correlations depend on the reported strength of conflicts. Centralization of the budget process, a credible threat of replacement of managers following systematic deficits, and surplus carry-over rules all appear beneficial to net revenues, but only in municipalities that report substantial conflicts of interest. For municipalities where the conflict is small, a deficit carry-over rule is positively correlated to net revenues. It remains to be explored whether the carry-over rules are also important in the absence of a centralized budget process, as fiscally successful municipalities employ to a large extent both centralized budget processes and carry-over rules.

Chapter 5 (co-authored with Lina Maria Ellegård) takes as its starting point that central government bailouts of local governments are commonly viewed as a recipe for local fiscal indiscipline, as local governments learn that the center will come to the rescue in times of trouble. Little is known however about whether such tendencies can be dampened if assistance is conditional on the local governments' own fiscal efforts. The chapter examines a case in which the Swedish central government provided conditional grants to 36 financially troubled municipalities. To deal with the obvious selection problem related to the participation in such a program, the synthetic control method (Abadie and Gardeazabal, 2003; Abadie et al., 2010) is used to identify suitable comparison units for each of the 36 municipalities. Using the resulting sample, fixed effects regressions then compare the development of costs and net revenues of admitted municipalities to that of their most similar counterparts during the decade after the program. For most of the admitted municipalities, costs seem to be largely unaffected by the program. However, a non-negligible share is able to hold back costs more than expected, and the development of net revenues is favourable for the group as a whole. Thus, participation in a conditional bailout program need not erode fiscal discipline, and may even induce a greater concern for fiscal discipline.

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Chapter 2

Coordination incentives, performance measurement, and resource allocation among public sector organizations

2.1 Introduction

The political scientist Harold Seidman once referred to the quest for coordination in public administration as being the "twentieth-century equivalent of the medieval search for the philosopher's stone" (quote from Wilson (1989, p. 268)); a colorful illustration of the recurring theme of coordination problems among public sector organizations.[1] There are reasons why coordination problems may be of higher general interest and more visible in public compared to private sector organizations – tax financing and relative openness of information are two – and that they therefore receive more attention. But are there also reasons to believe that coordination problems should be more common?

This paper uses a principal-agent model to scrutinize two reasons: the difficulties of accurately measuring performance and the incentives to coordinate resource allocation among organizations when responsibilities for activities are shared. These two reasons are tightly connected, as perfor-

[1] As an example, during a one-year period, articles about coordination problems among the following organizations appeared in the opinion pages of Sweden's largest daily newspaper ("Dagens Nyheter Debatt"): compulsory institutional and non-institutional psychiatric care (2009-08-09); schools and social services (2008-02-09); organizations treating substance abusers (2009-05-27); organizations handling land, sea and air-traffic infrastructure (2009-04-01); organizations handling fishing, sea resources and victual safety (2009-02-05); organizations supervising social services (2009-02-02); organizations involved in health and dental care for schoolchildren (2008-12-18); organizations responsible for psychiatric care of children (2008-11-19); and organizations working to stop football associated violence (2008-09-15).

mance measures influence coordination incentives, while such incentives in part determine the measured outcome.

While accurately measuring output and outcomes is a problem in all organizations, it is in general more difficult in public sector organizations than firms (Baker, 2002). Over the last two decades, the governing of public organizations in most Western countries has moved from a reliance on rules and procedures towards management by objectives (Propper and Wilson, 2003; Andersen et al., 2008; Verbeeten, 2008). As this development entails an increased reliance on performance measures, it is important to include the effects of imperfect measures in models of coordination in public sector organizations. One part of the measurement problem is the lack of summary measures of value in public sector organizations (Baker, 1992, 2002). Firms can be evaluated on the basis of firm/stock value, and such measures can also be used to align employees' interests with the firm's. Also, performance measures are often short term, with measurement following budget periods, whereas the relevant outcomes frequently materialize over longer periods of time. Due to these and other reasons, performance is often measured with considerable imperfection in terms of both distortion (bias) and imprecision in public sector organizations (Propper and Wilson, 2003).

Imperfect measures also affect the possibilities of designing incentive systems. Indeed, as measurement problems typically require more muted incentives, such problems are a justification for an activity to be the responsibility of a public sector organization (Acemoglu et al., 2008). Another important feature of the public sector in this respect is that most organizations do not sell their services and products at market prices, or make profits. Consequently, the price mechanism and cross-unit incentive schemes based on profit sharing, for example, is not available to coordinate activities between organizations. Public sector organizations are overall very limited in their use of monetary incentives (Burgess and Ratto, 2003; Propper and Wilson, 2003; Heinrich and Marschke, 2010).[2] The source of motivation is implicit and/or intrinsic rather than explicit incentive schemes (e.g. Wilson, 1989; Dewatripont et al., 1999).

Many, but not all, public sector organizations and some private orga-

[2]Another example, in a survey of the Swedish municipalities in 2010, only one municipality out of 256 respondents (there are 290 in total) stated that it uses bonus schemes related to budget surpluses for the manager of their largest sub-unit (Dietrichson and Ellegård, 2012).

nizations fit this description.[3] In the following, I use the term 'public sector organization' to denote organizations where non-market operation, motivated agents, and measurement problems are present, and develop a multi-task principal-agent model that includes these features. The principal determines performance measures and two agents determine resource allocation to two types of activities each – one where responsibility is shared (*joint activities*) and another for which one agent is solely responsible (*core activities*). The use of budgets to determine resource allocation and information asymmetries between principals and agents are also of consequence for the model, but these are key characteristics of both private and public sector organizations.

I first use a one-shot game to analyze coordination incentives when targets are set individually for each agent and measures are undistorted and precise. The results show that when activities are interdependent among agents incentives that distort the allocation of resources away from efficient levels are present, even if the agents' motivation is fully in line with the principal's interest. This suggests one potential remedy: sharing targets between agents. Public sector organizations may have a shared responsibility for vague, overarching goals, whereas the more specific goals on which performance is assessed are not usually shared (e.g. Knapp et al., 2006). Shared targets has been tried though as a part of for example the New Labour government's efforts to create "Joined-up Government" in the United Kingdom (Politt, 2003; Bogdanor, 2005; Moseley and James, 2008),[4] but I have neither found a quantitative, empirical examination, nor a formal, theoretical treatment of how sharing targets across organizational boundaries affect coordination incentives. Compared to other potential remedies such as vertical and horizontal integration, shared targets also have the advantage of being easily implemented.

Shared targets align incentives in a similar way to a profit sharing scheme – by rewarding performance ex post. An important difference to profit sharing is that the strength of the incentives created by shared targets is not controlled to the same extent by the principal, as the mechanism relies on

[3]Departments of larger corporations, such as research and development, and administrative departments, which do not sell anything directly to customers, are examples in the private sector.

[4]It is hardly a new idea though; Hood (2005, p. 35) mentions that already in 1650, imperial China introduced a practice of holding one officeholder responsible and punished (or rewarded) for the actions of another.

implicit and/or intrinsic motivation. The results show that shared targets always improve coordination incentives and efficiency when performance measures are undistorted and precise, and agents' motivation is aligned with the principal's interests. In general though, the effects depend on the interplay of motivation and the distortion of performance measures, as well as the relative importance of the tasks for value. Agents who are more motivated by core activities, and/or use performance measures that overestimate the value of such activities, will often allocate even lower shares of resources to joint activities. For activities that are complements, shared targets have the best chance of improving efficiency in such situations (and vice versa for substitutes). However, while the result holds for a broad range of parameter values, it does not hold for all permissible values of the parameters.

In line with results from similar models, imprecision in the form of variance of performance measures decreases value. Higher variance implies higher risk borne by agents, which leads risk-averse agents to demand higher wages. Higher wages in turn decrease the available resources and therefore also decrease the value created. As sharing targets implies responsibility for more performance measures and thus increases total variance, this decreases the usefulness of shared targets (at least in the cases where agents' wages constitute a non-negligible share of total resources). Imprecision may also have another consequence: if agents do not know their value functions in every detail, noisy measures may make it difficult for agents to learn these functions. To examine this possibility and to relax the assumption of common knowledge of the details of the one-shot game, I simulate a repeated version of the model where agents use a least squares learning procedure to estimate some parameters of their value functions (e.g. Sargent, 1993; Evans and Honkapohja, 2009). The results show that if performance measures are not too imprecise, the allocated shares are close to equilibrium values and the one-shot model is a rather good approximation in the short run. However, with three initially unknown parameters and more noisy measures there can be substantial and lengthy deviations from equilibrium values.

The next section provides some background to coordination, performance measurement and motivation, and relates earlier models of these concepts to my model. Section 3 describes the model and results. Section 4 contains concluding remarks.

2.2 Coordination, motivation, and measurement

When is coordination an issue? The crucial condition is whether activities of one agent are affected by the activities of other agents; i.e. if their actions are interdependent or not. Efficient coordination requires that agents are motivated to perform desired activities and that the value-maximizing activities are known and can be combined correctly (Hoopes and Postrel, 1999). Correspondingly, organizations need to create incentives for their members to take the right actions, as well as mechanisms that help members solve the cognitive parts of coordination problems.[5]

I focus on the incentive issues of coordination, mainly for the reason that mechanisms that mitigate cognitive coordination problems do not seem to differ in kind between public and private sector organizations. To borrow a taxonomy of coordination mechanisms from Grant (1996), nothing in the set up of public sector organizations prevent them from using similar rules and directives, sequencing, routines, and/or group problem solving in the same way as firms do.

In an early model of coordination incentives, Itoh (1991) considers the problem of when it is optimal for a principal to induce teamwork by making agents' wages dependent upon the outcome of other agents' tasks. The result is that the principal either wants a substantial amount of team work when agents' own effort and help from others are complementarities, or otherwise a strict division of labor (i.e. no team work). More recent studies include Kretschmer and Puranam (2008), who study if and how collaborative incentives, in the form of profit sharing among divisions, increase organizational value in the presence of task specialization. Specialization creates the need for collaborative incentives in order to fully realize gains from interdependence, but there exists a tradeoff as it also tends to make activities that are jointly undertaken between divisions less productive. Baiman and Baldenius (2009) examine how a bonus tied to the implementation of joint projects can manage the externality created by interdependence between two divisions of firm. In their model, an optimal implementation bonus

[5] A prerequisite for successfully solving the cognitive problems of coordination in differentiated organizations seems to be that the agents involved have a sufficient degree of shared knowledge of each other's actions (e.g. Kretschmer and Puranam, 2008, and references cited therein).

always enhances efficiency by inducing a better combination of ex ante investments and ex post implementation.[6]

The previously mentioned articles do not analyze measurement problems. Seminal models of multi-task, incomplete information environments by Holmström and Milgrom (1991) and Baker (1992) show that the optimal strength of incentive schemes is relatively low-powered when task outcomes are measurable to different degrees. Incentive-pay not only allocates risks and motivates agents but also serves as an effort allocation mechanism among different tasks. With market provision, the agent's effort is excessively driven towards the easy-to-measure task that can form the basis of incentive pay. Feltham and Xie (1994) show that in a single measure setting, loss of value is a function of distortion and imprecision. Additional measures are valuable as long as existing measures do not constitute a sufficient statistic for the additional measure with respect to a manager's action. Baker (2002) develops a model to study the effects of measurement imperfections in a multi-task environment, and show that many issues can be analyzed as a tradeoff between distortion and imprecision. Schnedler (2008) and Thiele (2010) show that when an agent's preferences or abilities are not equal across tasks, then optimal measures can be distorted. Distortion and imprecision should be supplemented with the agent's effort costs or preferences for activities in order to determine the value of a certain performance measure. Optimal measures may (but need not) be distorted in the model by Kaarbøe and Olsen (2008), which combines imperfect measures with monetary and implicit incentives, such as career concerns and ratchet effects.

These models examine monetary incentive schemes of some kind but, as mentioned, such schemes are uncommon in public sector organizations. Several alternative motivational factors have been suggested. In Dewatripont et al. (1999) and Acemoglu et al. (2008), the existence of career concerns implies that an agent will exert effort even in the absence of explicit monetary incentives, because the agent wants to convince employers of his/her talent. According to Wilson (1989), public sector organizations can use a sense of mission to economize on the need for monetary incentives. An organization has a sense of mission when it "has a culture that is widely shared

[6] Rantakari (2008), Alonso et al. (2008), and Dessein et al. (2010) also study similar questions of coordination, but are primarily interested in the optimal allocation of decision rights. This issue is abstracted from here.

and warmly endorsed by operators and managers alike" (Wilson, 1989, p. 95). Recent principal-agent models also include agents that are intrinsically motivated to exert effort: by for example identification with organizational objectives (Akerlof and Kranton, 2005), social esteem (Ellingsen and Johannesson, 2008), or by pro-social motivation, either in the sense that the agent derives utility from producing (often called "warm-glow" altruism) or that the agent cares about the output ("output-oriented" or pure altruism) (Francois and Vlassopoulos, 2008).[7]

To analyze coordination incentives in the presence of measurement problems, my model incorporates strategic interaction of agents in a Holmström and Milgrom (1991) type of model where measures can be imprecise and distorted. To fit the public sector context, I also assume that there are no monetary incentives and that agents are motivated, but motivation can be aligned to the principal's interests to different degrees. The source of this motivation may be interpreted as career concerns, identification, self-esteem, warm-glow or pure altruism.

The first part of the paper assumes that agents know their value function completely. If this assumption is relaxed, imprecise performance measures may also affect resource allocation by making it more difficult for agents to learn their value functions. To examine this issue, I need to add how agents learn. A problem is that there is no consensus in the earlier literature on which learning rules players actually use in games.[8] Furthermore, game-theoretical learning rules, like for example the experience-weighted attraction rule (Camerer and Ho, 1999) and individual evolutionary learning (Arifovic and Ledyard, 2004, 2011) normally include evaluation of hypothetical strategies. The connection between the choice of strategies and outcomes is thus known to the agents, but this connection is precisely what the agents in my game do not know and have to estimate. For these reasons, I use a rule that is similar to the adaptive learning models in the macroeconomic literature, where the agents behave as econometricians in order to estimate unknown parameters (e.g. Sargent, 1993; Evans and Honkapohja, 2009).

[7] Examples of models where agents are "warm-glow" altruists include Besley and Ghatak (2005); Prendergast (2007); Delfgaauw and Dur (2008) and Makris (2009), while models of agents with output-oriented altruism include Francois (2000); Glazer (2004), and Gailmard and Patty (2007).

[8] See Camerer (e.g. 2003) for an overview of experimental results, and Salmon (2001) and Wilcox (2006) for the difficulties of estimating learning rules in experiments.

2.3 A model of resource allocation

This section presents the basic set up for a model to analyze coordination incentives when responsibilities are shared between two organizations. The model includes three players, one principal and two agents $i = 1, 2$. For instance, the principal could be a political committee, and the agents two managers of sub-units, where some part of the services is a responsibility shared between the two. Examples include important public sector organizations such as schools and social services, and hospitals and primary care units (see footnote 1 for more public sector examples). While the principal has the authority to design the structure of resource allocation and rewards, the relationship between the agents is not hierarchical.

The principal is interested in maximizing the total value of services given the amount of resources available. Total resources are denoted R and are normalized to 1. The services provided by both agents consists of two parts: activities in set A_i (*core activities*) are directed towards target groups that are solely the responsibility of agent i, whereas activities in set B_i (*joint activities*) are directed towards target groups where responsibility is shared between the agents (e.g. all children of certain ages in contact with social services are also students in some school). It is not possible, due to information asymmetries, to contract directly upon delivery of specific activities. The principal therefore allocates resources (R_i) in advance to the agents, such that $R = R_1 + R_2$. The agents receive a fixed wage, w_i, which is taken out of R_i. Agents allocate the remainder of the resources, $r_i = R_i - w_i$, between activities in A_i and B_i. Let $a_i \in [0, r_i]$ be the share of agent i's resources allocated to core activities, and $b_i \in [0, r_i]$ the share allocated to joint activities. As R is normalized to 1, R_i, w_i, r_i, a_i and b_i should be interpreted as shares of total resources. For each set of activities, let the (real-valued) functions mapping resource allocations to value be

$$V(A_i) = \theta a_i + \tau_A a_i b_i \tag{2.1}$$

and

$$V(B_i) = \rho b_i + \tau_B a_i b_i + \varphi b_i b_j \tag{2.2}$$

which yields the combined value function for each agent i

$$V_i = V(A_i) + V(B_i) = \theta a_i + \rho b_i + \tau a_i b_i + \varphi b_i b_j \tag{2.3}$$

where $\tau = \tau_A + \tau_B$. Total value is $V = V_1 + V_2$. Variations of this formulation are fairly common in organizational economics and in models of interdependent agents.[9] For my purposes, I believe it captures important trade-offs faced by managers of public sector organizations, and how interdependence is of vital importance for coordination problems, as interactions of activities within an organization (the term $\tau a_i b_i$) and between organizations ($\varphi b_i b_j$) are included. Following e.g. Siggelkow (2002), two arguments of a value function are said to be interdependent if the cross-partial derivative is different from zero. Furthermore, they are complements if this derivative is positive and substitutes if it is negative. The interdependence between arguments x and y is stronger than between y and z if $|\frac{\partial^2 V}{\partial x \partial y}| > |\frac{\partial^2 V}{\partial y \partial z}|$. In (3), the stronger the interdependence between activities in A_i and B_i, the higher the $|\tau|$, and the stronger the interdependence between activities in B_i and B_j, the higher the $|\varphi|$. I assume everywhere, except where specifically mentioned, that agents are identical. To simplify notation, the indexes denoting agent i and j are subsequently omitted whenever possible.

I impose a few restrictions on the parameters: θ, ρ, τ are all > 0 and such that a strictly positive amount of resources is allocated: $a = b = 0$ is thus ruled out. φ can take on both positive and negative values, reflecting that activities b_1 and b_2 could be both complements and substitutes. I also assume $\tau \geq |\varphi|$, which rules out inefficiencies created because the basic division of labor is sub-optimal. That is, if $\tau < |\varphi|$ one could argue that it would be better to break up the organizations into three and pool activities in B_1 and B_2 into one organization.

As value cannot be directly observed, agents maximize value as measured by a number of performance measures. Following Baker (2002), performance measures have two dimensions of imperfection: *imprecision* and *distortion*.[10] A measure is imprecise if it is measured with noise, but is otherwise unbiased. A distorted measure is biased. Let $P = \{p^{A_1}, p^{B_1}, p^{A_2}, p^{B_2}\}$ be the set of available performance measures and

$$p^k = d^k V(k) + \varepsilon^k, k \in \{A_i, B_i\}; \tag{2.4}$$

[9] See for example Marschak and Radner (1972); Cremer (1990); Siggelkow (2002), and Kretschmer and Puranam (2008). All results in the paper hold qualitatively for a value function with negative, squared terms of a and b, which are often added to model decreasing returns in models that lack a budget constraint.

[10] In the accounting literature distortion is often called incongruity (e.g. Feltham and Xie, 1994; Budde, 2007), while others have used the term alignment (e.g. Schnedler, 2008). The formal definitions are often equivalent though.

where $d^k \in [0, D], D \in \mathbb{R}_+$, is a measure of distortion and ε^k is a normally distributed random term with mean zero, $\varepsilon^k \sim N(0, v^k)$. The random error terms represents influences on the performance measure that are outside an agent's control. Measures are undistorted when $d^k = 1$, whereas a measure where $d^k < 1 (d^k > 1)$ underestimates (overestimates) value.

The principal specifies a subset $P^C \subset P, C \in \{I, S\}$ of these measures for each agent, where $P^I = \{p^{A_i}, p^{B_i}\}$ is each agent's set of performance measures under *individual targets*, and $P^S = \{p^{A_i}, p^{B_i}, p^{B_j}\}$ is the corresponding set under *shared targets*. I assume that the measures are independent, in the sense that the presence of one measure does not affect the other measures. This assumption implies that the (measured) marginal value of core activities ($\partial p^k / \partial a$) is not changed by the introduction of shared targets.

For each performance measure in the chosen subset, the principal also specifies a level that should be attained, i.e. a benchmark or a standard, denoted $\bar{p}^k$. Explicit benchmarks are common in all types of organizations. Moreover, it is difficult to conceive of performance measures that are not at least implicitly evaluated against some standard. This assumption also has the technical advantage that an agent's utility does not automatically increase with the number of (nonnegative) performance measures. I do not model the process of determining this benchmark, but assume that it is some fixed, positive number, set by the principal to signal to the agents what is expected of them. I assume throughout that the principal knows that the agents are identical and splits the initial allocation in half.

The agents' expected utility depends on a fixed wage ($w_1 = w_2 = w$) and the created value as measured by the performance measures in comparison to the benchmarks:

$$\mathbb{E}(u) = \mathbb{E}[-exp(-\delta(w + \boldsymbol{m}'(\boldsymbol{p} - \bar{\boldsymbol{p}})))] \tag{2.5}$$

where $\delta > 0$ measures the agent's risk aversion, $\boldsymbol{p}$ is the performance measures of a certain subset P^C arranged in a (column) vector with typical element p^k, $\bar{\boldsymbol{p}}$ is a (column) vector of fixed, positive benchmarks with typical element $\bar{p}^k$, and $\boldsymbol{m}$ is a (column) vector with typical element m^k.[11]

The vector $\boldsymbol{m}$ signifies the extent to which an agent is motivated – higher m^k implies that the agent cares more about performance measure p^k and the corresponding set of activities – and also the extent of the organization's

[11] This formulation is a variant of the canonical model of constant absolute risk-aversion developed by Holmström and Milgrom (1987, 1991).

sense of mission. An agent for whom $m^k = m^l > 0$ for all $l, k \in \{A_i, B_i, B_j\}$ fully shares the organizational mission; that is, it is only the marginal value of each allocation that guides the agent's choice of allocation, and the agent's motivation is not in conflict with the principal's interest.[12] In this case, $\boldsymbol{m}$ is just a scalar.

As the benchmarks are fixed, their levels do not directly affect the the marginal value of resource allocation. But the levels of the benchmarks affect the allocations indirectly. To see how, first define each agent's participation constraint as

$$\mathbb{E}(u) \geq \bar{u} \tag{2.6}$$

where $\bar{u}$ is the outside option available to the agents. Furthermore, agents maximize utility subject to a budget constraint:

$$r \geq a + b. \tag{2.7}$$

As resources are dependent on wages, and wages are determined by equation (2.5) and (2.6), the benchmarks affect the allocation through the constraints. Another thing to note is the effect of the benchmarks in combination with motivation. If $\bar{p}^k < \max_{a,b} \mathbb{E}(p^k)$, higher m^k implies that the principal can set a lower wage all else equal, whereas if $\bar{p}^k > \max_{a,b} \mathbb{E}(p^k)$, more motivated agents require a higher wage. If agents are motivated by career concerns, this seems reasonable. That is, if agents exceed what is expected of them, this reflects positively on their future career possibilities, and vice versa. Similarly, agents driven by desire for social esteem or identification with the organizational mission could also be expected to demand compensation for not being able to achieve what is expected of them. The timing of the model is:

1. The principal learns total resources, R.
2. The principal specifies the agents' tasks, i.e. the performance measures and benchmarks, and offers a fixed wage.
3. If each agent's participation constraint is met, the principal allocates resources to the agents. Otherwise, return to step 2 and let the principal offer a new wage level.

[12] I treat motivation as exogenously given throughout. See Rob and Zemsky (2002) for a model where the utility of cooperation and the corporate culture is endogenously determined by the incentive structure and the history of cooperation in the organization.

4. Agents decide how to allocate the given resources between a and b, which determines total value.

2.4 Complete information, perfect measures, and aligned motivation

To get some benchmark results, this section compares the resource allocations of agents with that of an informed principal under conditions when the agents fully share the organization's mission, and each individual performance measure is precise and undistorted. Therefore, assume $v^k = 0, d^k = 1$, and $m^k = m^l > 0$ for all $k, l \in \{A_i, B_i, B_j\}$. Assume also that the details of the model as laid out above, including the effect of their own and the other agent's allocation on the performance measures, are common knowledge among the two agents. Given the procedure stipulated in the previous section and that the agents' utility functions are strictly concave, their allocations constitute a unique sub-game perfect Nash equilibrium. An informed principal chooses an allocation to directly maximize

$$V^* = p^{A_1} + p^{B_1} + p^{A_2} + p^{B_2} \tag{2.8}$$

subject to

$$2r^* \geq 2a^* + 2b^*. \tag{2.9}$$

Proposition 1 compares V^* to V^I, the value created by two identical agents with individual targets. To make the comparison interesting, I assume that the available resources are the same for agents with individual targets and the informed principal, so $r = r^*$.[13] All calculations are found in the Appendix.

Proposition 1: Suppose agents are identical and $\varphi \neq 0$. If

(i) $\theta \geq \rho + r\tau$, then all resources are allocated to activities in A_1 and A_2, $(a, b) = (r, 0)$ and $V^* = V^I$;

(ii) $\theta \leq \rho - r(\tau - \varphi)$, then all resources are allocated to activities in B_1 and B_2, $(a, b) = (0, r)$ and $V^* = V^I$;

[13] In principle, with an informed principal there is no need for agents in the model, as their only task is to allocate resources. The principal could therefore choose not to hire any agents and save the wages. This comparison is not very informative though.

(*iii*) $\rho - r(\tau - \varphi) < \theta < \rho + r\tau$, then $a, b > 0$ and $a + b = r, V^* > V^I$. Moreover, the difference in value is increasing in $|\varphi|$.

The reason for (*i*) and (*ii*) is of course that the value of a dominates the value of b and vice versa, so interdependence need not be taken into account.[14] From here on I analyze only the case where strictly positive shares of resources are allocated to both tasks.

As shown by (*iii*), whenever there is interdependence between the two agents and a and b are positive, with individual targets there exist incentives to allocate resources in a sub-optimal way. Thus, even when favorable (indeed, implausible) assumptions of agent motivation and performance measures are made, some mechanism needs to be in place to manage interdependencies. This result is in line with results from models of coordination incentives in the literature on private firms (e.g. Rantakari, 2008; Kretschmer and Puranam, 2008; Baiman and Baldenius, 2009), but the result does not depend on agents having different preferences to the principal. The allocations in this case are

$$(a^I, b^I) = \left(r - \frac{\rho + r\tau - \theta}{2\tau - \varphi}, \frac{\rho + r\tau - \theta}{2\tau - \varphi} \right) \tag{2.10}$$

$$(a^*, b^*) = \left(r - \frac{\rho + r\tau - \theta}{2(\tau - \varphi)}, \frac{\rho + r\tau - \theta}{2(\tau - \varphi)} \right) \tag{2.11}$$

which implies that $b^I < b^*$ when joint activities are complements ($\varphi > 0$) and $b^I > b^*$ when they are substitutes ($\varphi < 0$).

Corollary 1 describes how the agents in public organizations can be made to internalize these interdependencies with the help of shared targets. Then, $P^S = \{p^{A_i}, p^{B_i}, p^{B_j}\}$ and the resulting value is denoted V^S.

Corollary 1: Suppose $\varphi \neq 0$ and the agents are subject to shared targets, then $V^* = V^S$.

Thus, first-best can be achieved by letting agents share targets when performance measures are precise and undistorted, and agents' motivation is in line with the principal's interest. The next sections relax some of the

[14] Note that the parameter values in the proposition hold for the individual targets case, but the parameter condition for $(a, b) = (0, r)$ is different when the principal is fully informed: $\rho - r(\tau - 2\varphi) < \theta < \rho + r\tau$. That is, the principal allocates positive shares to both a and b for a narrower range of parameters.

assumptions made in this section and examine if and when shared targets can improve upon individual targets.

2.5 Distortion and misaligned motivation

Performance measures are of course seldom, if ever, "perfect" and agents do not necessarily share the mission of their organization in the sense assumed in the previous section. This section examines the effect of distorted performance measures and misaligned motivation, while keeping the assumption of common knowledge, as well as precise performance measures. As discussed in section 2.3, the wage level w, and in turn available resources r, depend on the difference between $\max_{a,b} p^k$ and $\bar{p}^k$. This difference also influences how changes in m^k and d^k affect w and r; it is easy to show that $\partial r/\partial m^k = p^k - \bar{p}^k$ while $\partial r/\partial d^k = m^k V(k)$.[15] In order to focus on the "pure" effects of motivation and distortion on the choice of allocations, I abstract from the resource effects here and assume that r is fixed, or equivalently that $\max_{a,b} p^k = \bar{p}^k$ in this section. If wages for managers are a small share of total resources, this abstraction is likely to be inconsequential.

How does distortion affect the allocations? It is *not* necessarily true in the model that a distorted performance measure decrease value, even if r is fixed. Recall that individual targets with undistorted and precise measures yield an inefficient allocation, b being too low in the case of complements and too high in the case of substitutes. Thus, a distorted measure that either overestimates the marginal value of b, or underestimates the marginal value by the "right" amount, could induce a first-best allocation. Solving a similar maximization problem under individual targets as in section 2.4 but with $d^k > 0 \, \forall \, k \in \{A_i, B_i\}$, i.e. maximizing

$$\mathbb{E}(u) = -exp\left[-\delta\left(w + m \sum_{p^k \in P^I} \left(d^k V(k) - \bar{p}^k\right)\right)\right] \tag{2.12}$$

[15] If agents are motivated enough, or measures overestimate value enough, wages may be driven to zero. While this does not seem to be a very common state of affairs in the public sector or for managers in general, it is not an unthinkable concept for other types of agents. For instance, internships with zero or very low compensation are common in many industries.

yields the following allocation

$$b^I = \frac{d^{B_i}(\rho + \tau_B r) + d^{A_i}(r\tau_A - \theta)}{d^{A_i}2\tau_A + d^{B_i}(2\tau_B - \varphi)} \tag{2.13}$$

If we compare this expression to (2.11), it can be shown that if

$$\frac{d^{B_i}}{d^{A_i}} = \frac{\theta + b^* 2\tau_A - r\tau_A}{\rho + \tau_B r - b^*(2\tau_B - \varphi)} \tag{2.14}$$

then $b^I = b^*$ and there is no loss of value even with individual targets. The point is that it is the ratio of measured marginal values that matters for the resource allocation, and therefore it is the *combination* of performance measures that is important, rather than the individual measures. This implies that distortion works differently here compared to e.g. Baker (2002), where distortion is always negative. Kaarbøe and Olsen (2008), Schnedler (2008) and Thiele (2010) also show that distortion may increase value. In their models, this is driven by distortion of non-verifiable measures, by different effort costs, and by different ability over tasks, respectively; whereas the explanation in my model is the interdependence of the agents.

To see how motivation that is not fully in line with the organizational mission affects the results, let the elements of the motivation vector be $m^k \geq 0 \; \forall \; k \; \in \{A_i, B_i, B_j\}$ and not necessarily equal. The agent then maximizes

$$\mathbb{E}(u) = -exp\left[-\delta\left(w + \sum_{k \in \{A_i, B_i, B_j\}} m^k\left(p^k - \bar{p}^k\right)\right)\right] \tag{2.15}$$

subject to the same restrictions as before. Compare this expression to (2.12) to see that motivation affects the allocation in a similar way to distorted performance measures. In a general formulation, with distortion included, the allocation to joint activities with shared targets becomes

$$b^S = \frac{m^{B_i}d^{B_i}(\rho + r\tau_B) + m^{A_i}d^{A_i}(r\tau_A - \theta)}{m^{A_i}d^{A_i}2\tau_A + m^{B_i}d^{B_i}(2\tau_B - \varphi) - m^{B_j}d^{B_j}\varphi} \tag{2.16}$$

As long as $m^{B_j}, d^{B_j} > 0$, expression (2.16) shows that shared targets always imply a higher b when φ is positive, and a lower b when φ is negative, compared to individual targets (when either m^{B_j}, d^{B_j} or φ is zero, the allocation is equal to the one with individual targets). Shared targets can therefore only be an improvement when individual targets result in $b^I < b^*$ for $\varphi > 0$ (complements), and $b^I > b^*$ for $\varphi < 0$ (substitutes).[16]

For complements (and reversed for substitutes) it may seem as if increased motivation for core activities, or distorted measures that overestimate the value of such activities, should imply a higher a and increase the possibility that shared targets improve the allocation. Similarly, increased motivation for, or overestimation of, the value of joint activities should have the opposite effect. However, proposition 2 shows that while this intuition holds for a broad range of parameter values, it does not hold for all:

Proposition 2: Let b be given by

$$b^I = \frac{m^{B_i} d^{B_i} (\rho + r\tau_B) + m^{A_i} d^{A_i} (r\tau_A - \theta)}{m^{A_i} d^{A_i} 2\tau_A + m^{B_i} d^{B_i} (2\tau_B - \varphi)},$$

then i):

$$\frac{\partial b}{\partial m^{A_i}} < 0, \frac{\partial b}{\partial d^{A_i}} < 0 \tag{2.17}$$

when

$$\theta (2\tau_B - \varphi r) + \tau_A (2\rho + \varphi r) > 0; \tag{2.18}$$

and ii):

$$\frac{\partial b}{\partial m^{B_i}} < 0, \frac{\partial b}{\partial d^{B_i}} < 0 \tag{2.19}$$

when

$$\theta (2\tau_B - \varphi) > \tau_A (2\rho + r (4\tau_B - \varphi)). \tag{2.20}$$

Regarding i), increased motivation for core activities, m^{A_i} (or increased distortion, d^{A_i}), normally decreases b and increases the possibility for shared targets to work. But as there are no parameter restrictions set on τ_B and τ_A individually (only on $\tau = \tau_A + \tau_B > 0$), the inequality can be reversed when τ_B is small enough relative to φr. This would require that allocations to joint activities have a relatively large effect on core activities, but not the other way around (τ_A is large relative to τ_B).

About ii), increased motivation for joint activities m^{B_i} (or increased d^{B_i}) may increase b as there are many parameter values for which the inequality is reversed. The inequality holds when a affects the value of b

[16] I still assume that $r = a + b$ and $a, b > 0$, so the changes to the allocation from misaligned motivation and distortion do not warrant a corner solution.

strongly (τ_B is high), but b does not have a positive effect on a (τ_A is relatively low, zero, or negative). Then, increased motivation for joint activities may decrease the share of resources allocated to these activities.

In sum, Proposition 1 and Corollary 1 do not hold generally. There are instances when distorted measures and agent motivation may neutralize the inefficiency found with individual targets. However, for a broad range of parameter values, shared targets are more likely to improve coordination incentives for complements when agents are highly motivated by core activities, or performance measures overestimate the value of core activities (vice versa for substitutes).

2.6 Imprecision

To see the first effect of imprecise measures on resource allocation clearly, let $\boldsymbol{p}$ be composed of the undistorted performance measures under shared targets and $\boldsymbol{m}$ be a scalar, so that the loss of value would be zero absent noise. The size of the imprecision of a performance measure depends on the variance, v^k. When $v^k > 0$ and the error term is normally distributed, the agents' expected utility functions can be shown to be

$$\begin{aligned}\mathbb{E}(u) &= \mathbb{E}\left[-exp\left(-\delta\left(w + m \sum_{p^k \in P^S}\left(V(k) + \varepsilon^k - \bar{p}^k\right)\right)\right)\right] \\ &= -exp\left[-\delta\left(w + m \sum_{p^k \in P^S}\left(V(k) - \bar{p}^k - \frac{\delta m v^k}{2}\right)\right)\right]. \end{aligned} \tag{2.21}$$

The agent's utility is still increasing in the fixed wage and in measured value, but is always decreasing in the variance of the performance measures because the (risk-averse) agents are forced to bear more risk. A negative influence on agents' utility must increase the wages paid. As wages have to be taken out of available resources, this decreases the amount that can be allocated to produce value. It is also evident that all else equal, more motivated agents will require more compensation for bearing risk, which seems reasonable especially if motivation derives from career concerns. This also shows that the relationship between motivation and wages (and in turn resources) is again not straightforward. It is not simply the case that highly motivated agents demand lower wages.

A thing to note is that the introduction of noise may affect the relative allocation of resources of each agent, as well as total resources available. This implies that noise affects the results in a different way to models of private firms because of the budget constraint, which is typically absent in such models (e.g. Feltham and Xie, 1994; Baker, 2002). Recall from expressions (10) and (11) that r is included in the expressions for a and b. A change in resources is not necessarily neutral in these expressions, so the ratio of a to b may change and in turn affect V.[17]

As an added measure can only increase variance, this implies that shared targets often require higher wages. As discussed in previous sections, the exception may be if $\max_{a,b} \mathbb{E}(p^{B_j}) > \bar{p}^{B_j}$ which may partly or wholly offset the effect of increased risk on wages and resources.

In any case, as long as the wages of agents in charge of resource allocation are a small share of total resources, these effects of imprecision are likely to be small problems in practice. The next section examines a potentially more problematic consequence of imprecision.

2.6.1 Learning with imprecise performance measures

The one-shot game relies on assumptions that agents know how resource allocations determine value, both for themselves and for the other agent. As such, the one-shot equilibrium is perhaps best interpreted as a long run outcome. To examine how imprecise performance measures affect the agents' possibilities of learning their value function, this section simulates a repeated version of the model. The simulations also shed more light on when the one-shot model is a reasonable approximation in the short run, as the rather strict assumption of common knowledge is relaxed.

I assume that the agents still have some knowledge of how their own allocations affect the performance measures (as the agents would not be needed otherwise). In particular, I assume that they know the functional form of the mapping from shares of resources to measured value, but must learn some of the parameters. It seems reasonable, and is supported by empirical evidence, that the values of interdependent activities are more difficult to assess (e.g. Sherman and Keller, 2011), so I let first φ, and

[17] In fact, a change in r is only neutral if $\rho = \theta$. To see this, differentiate the ratio of a/b with respect to r, which yields $\partial(a/b)/\partial r = ((\tau - \varphi)(\rho - \theta + r\tau) - \tau(r(\tau - \varphi) - \rho + \theta))/(\rho - \theta + r\tau)^2 = (2\tau - \varphi)(\rho - \theta)/(\rho - \theta + r\tau)^2$. As the denominator and $2\tau - \varphi$ must be greater than zero, the expression is only zero when $\rho = \theta$.

then all of φ, τ_A and τ_B, be unknown. The resource allocation of the other agent is also unknown beforehand, but revealed after each period. When agents choose their best replies, they use the other agents choice in the previous period, b_{jt-1}; i.e. they assume that the other agent's choice of b is stationary. Given this uncertainty, I also assume that agents choose myopic best replies, i.e. they are not forward looking. Myopia can be motivated by the fact that agents may be replaced. If agents know that they are learning over time, it may similarly be regarded as rational to only take the current period into account. From a different point of view, it may instead reflect an aspect of bounded rationality. Both stationarity and myopic best responses are common in game-theoretic learning models (e.g. Fudenberg and Levine, 2009).

2.6.2 The learning rule

For simplicity, I use a regime of individual targets and exemplify the rule below with the situation where φ, τ_A and τ_B are unknown. This implies that agents use p^{A_i}, p^{B_i} and what they know about the parameters and allocations in their own value function to "back out" the values of the unknown parameters. In period 1, agents use initial beliefs of the unknown parameters to make their choice. For τ_A in periods $t > 1$ agents use p^{A_i} and the known terms θ, a_i, b_i to get an estimate:

$$
\begin{aligned}
(\hat{\tau}_A)_t &= \frac{1}{t-1}\sum_{s=1}^{t-1}\frac{1}{a_{is}b_{is}}\left(p_s^{A_i} - \theta a_{is}\right) \\
&= \frac{1}{t-1}\sum_{s=1}^{t-1}\frac{1}{a_{is}b_{is}}\left(\theta a_{is} + \tau_A a_{is}b_{is} + \varepsilon_s^{A_i} - \theta a_{is}\right) \\
&= \frac{1}{t-1}\sum_{s=1}^{t-1}\left(\tau_A + \frac{\varepsilon_s^{A_i}}{a_{is}b_{is}}\right). \qquad (2.22)
\end{aligned}
$$

That is, agents take the average of the backed out values of τ_A and the error term of the performance measure over the past periods. Effectively, agents regard the error terms as having mean zero. The error term is scaled up by the term $a_i b_i$, which implies that the lower the values of a_i and b_i, the more the error term influences the estimation. This is so since τ_A is not observed separately from $a_i b_i$. If $a_{it} = 0$ or $b_{it} = 0$, the performance measure contains no information about the value of the interdependence and I assume that $(\hat{\tau}_A)_t = (\hat{\tau}_A)_{t-1}$. When φ is the only unknown, agents use a similar rule to estimate that parameter but instead use p^{B_i} and the known terms ρ, a_i, b_i, b_j and τ_B.

For τ_B and φ things are a bit more complicated. As

$$p_t^{B_i} = \rho b_{it} + \tau_B a_{it} b_{it} + \varphi b_{it} b_{jt} + \varepsilon_t^{B_i} \tag{2.23}$$

contains two unknown parameters to be estimated, agents need to estimate these parameters jointly over several periods. Therefore, in periods 1 and 2 I assume that agents do not update their beliefs about φ, but use their initial beliefs $\hat{\varphi}_0$ to estimate τ_B in the same way as τ_A. That is, agents focus on the within organization interaction between a_i and b_i first. In the first period, choices are made based on initial beliefs. In the second, there is one observation to estimate τ_B from, which yields an estimate for $t = 2$ equal to

$$\begin{aligned}(\hat{\tau}_B)_2 &= p_1^{B_i} - \rho b_{i1} - \tau_B a_{i1} b_{i1} - \hat{\varphi}_0 b_{i1} b_{j1} \\ &= \tau_B + \frac{1}{a_{i1} b_{i1}} \left((\varphi - \hat{\varphi}_0)\, b_{i1} b_{j1} + \varepsilon_1^{B_i} \right)\end{aligned} \tag{2.24}$$

For periods $t > 2$, I assume that the agents in every period estimate the parameters by an ordinary least squares (OLS) estimation. In each period t, combine the performance measure and the known terms ρ and b_i into

$$y_{it} = p_{t-1}^{B_i} - \rho b_{it-1}. \tag{2.25}$$

Then, define the matrix $\mathbf{X}_{it}$ and the vector $\mathbf{y}_{it}$ as

$$\mathbf{X}_{it} = \begin{bmatrix} (a_{i1} b_{i1}) & (b_{i1} b_{j1}) \\ \vdots & \vdots \\ (a_{it-1} b_{it-1}) & (b_{it-1} b_{jt-1}) \end{bmatrix}, \mathbf{y}_{it} = \begin{bmatrix} y_{i1} \\ \vdots \\ y_{it-1} \end{bmatrix} \tag{2.26}$$

and let agent i's point estimate of the parameters at time t be written as the OLS estimator:

$$\hat{\boldsymbol{\beta}}_{it} = \begin{bmatrix} \hat{\tau_B}_{it} \\ \hat{\varphi}_{it} \end{bmatrix} = \left(\mathbf{X}'_{it} \mathbf{X}_{it} \right)^{-1} \mathbf{X}'_{it} \mathbf{y}_{it}. \tag{2.27}$$

Note that the above learning rules imply that the initial beliefs of the unknown parameters are discarded after the first observation (with the exception of $\hat{\varphi}_0$ when all three parameters are unknown).

2.6.3 Simulation set-up and results

For simplicity, I assume that total resources and wages, as well as distortion and motivation – factors that co-determined the equilibrium of the one-shot game – are time invariant. As in the previous sections, I study the case when $a + b = r$. The true parameter values in all versions of the simulation model are exogenous and time invariant, and such that both a and b are greater than zero in equilibrium. Furthermore, the level of distortion is not important for the analysis in this section, so I exemplify only with undistorted performance measures.

The stage game is repeated for T periods. In each period $t = 1, 2, ..., T$, the two agents choose a myopic best reply allocation, using the estimations of the unknown parameters. In $t = 1$, there is no history, so I assume that the players maximize, taking just their initial beliefs and the constraints into account.

In periods $t > 1$, all players observe the outcome of their performance measures in the previous period. Using the learning rule, agents update their assessments of the unknown parameters. Agents then decide how to allocate the given resources between a and b by choosing a myopic best reply conditional on their beliefs. Using the last period's play by agent j and solving a similar program as in the one-shot model yields the following best reply function for b:

$$b_{it} = \frac{\rho + \bar{r}\left((\hat{\tau_A})_{it} + (\hat{\tau_B})_{it}\right) - \theta + \hat{\varphi}_{it} b_{jt-1}}{2\left((\hat{\tau_A})_{it} + (\hat{\tau_B})_{it}\right)}. \tag{2.28}$$

whereas a_{it} is determined as $a_{it} = \bar{r} - b_{it}$. The simulations run for $T = 30$ periods and each variation is repeated 10,000 times. I use the following values of the true parameters: $\theta = 1.1, \rho = 1.004, \tau_A = 0.4, \tau_B = 0.4, \varphi = 0.4$ and $\bar{r} = 0.48$, which yields $b^I = 0.24$ in equilibrium.

In each repetition, a value for all unknown parameters in the initial period is selected by a uniform randomization. The range of permissible initial beliefs about φ is $\hat{\varphi}_0 \in [0, \tau]$. That is, the agents are assumed to believe that the interdependence within their organization is at least not less "important" than the interdependence among the organizations. I only consider complements in the simulation, therefore the lower bound is 0 and agents are not initially allowed to incorrectly perceive inputs into joint activities as substitutes. When unknown, $(\hat{\tau_A})_0$ and $(\hat{\tau_B})_0$ are also in $[0, \tau]$.

There are no restrictions on the parameters after the initial round, in order to let the learning rule run its course. However, there may be situations where it is unreasonable to assume that agents would always let the parameter estimate fully determine resource allocation. This may be the case when, for example, allocating no resources to an area of activities is not an option, if agents have strong priors about the parameters, or if they realize that measures are noisy. To model this, I let the play of b_{it} be confined to three intervals: 1) $b_{it} \in [0, 0.48]$; 2) $b_{it} \in [0.01, 0.47]$; and 3) $b_{it} \in [0.1, 0.38]$. That is, if the parameter estimates imply a choice of b (and as a consequence a) outside the specified range, the upper or lower bound is chosen instead. The first scenario implies essentially no restrictions except that agents cannot spend more than available resources, while the second imposes mild restrictions that rule out situations where no learning occurs (recall that the learning rule provides no information about parameters when $b_{it} = 0$ or $b_{it} = \bar{r}$). The third imposes more substantial restrictions.

The results reported in table 2.1 are the total absolute differences in percent between each agent's choice of b_{it} and the equilibrium value as given by the parameters (b^I),[18] averaged over the 10,000 repetitions. That is, the value for a period $t \in T$ is

$$\frac{1}{10000} \sum_{rep=1}^{10000} \left(\frac{|b_{1t}^{rep} - b^I| + |b_{2t}^{rep} - b^I|}{b^I} \right) \times 100. \tag{2.29}$$

Columns (1)-(3) of table 2.1 show the results of simulations where only φ is unknown, while τ_A, τ_B and φ are unknown in columns (4)-(6). Panels 1-3 correspond to the three ranges for b_{it} discussed above. The error terms are normally distributed with mean zero and a standard deviation, $\sigma^k, k \in \{A_i, B_i\}$, of 0.1 (columns (1) and (4)), 1 (columns (2) and (5)), and 5 percent (columns (3) and (6)) of the equilibrium value of the performance measures, given by $p^{A_i} = V(A_i) = 0.287$ and $p^{B_i} = V(B_i) = 0.287$.

The results show that when there is only one unknown parameter, the agents' assessments converge fast to the true value of b. There are more deviations from equilibrium values of b when the standard deviation is quite high, i.e $\sigma^k = 0.05 \times V(k)$, but the absolute difference added over both agents is still less than 8 percent in periods 11-20, and less that 6 percent in periods 21-30. There are also practically no situations where the parameter

[18] Note that this is the equilibrium value under individual targets, not the efficient share b^*.

Table 2.1: Average total absolute differences (%)

Panel 1: $b_{it} \in [0, \bar{r}]$						
	Unknown parameter: φ			Unknown parameters: τ_A, τ_B, φ		
Periods	(1)	(2)	(3)	(4)	(5)	(6)
	$\sigma = 0.001$	$\sigma = 0.01$	$\sigma = 0.05$	$\sigma = 0.001$	$\sigma = 0.01$	$\sigma = 0.05$
All	1.73 (0.79)	3.12(0.97)	10.0(3.55)	7.84(17.8)	21.5(27.1)	41.9(31.0)
1-5	9.62 (4.75)	11.4(4.60)	22.9(8.23)	23.3(18.2)	40.8(22.8)	62.3(23.4)
6-10	0.22 (0.11)	2.16(1.15)	11.0(6.00)	4.96(18.6)	19.2(30.1)	46.2(38.5)
11-20	0.15 (0.08)	1.48(0.77)	7.51(3.99)	4.72(18.6)	17.5(29.8)	37.2(36.8)
21-30	0.11 (0.06)	1.12(0.61)	5.66(3.12)	4.66(18.6)	17.0(29.7)	34.3(35.7)

Panel 2: $b_{it} \in [0.01, \bar{r} - 0.01]$						
	Unknown parameter: φ			Unknown parameters: τ_A, τ_B, φ		
Periods	(1)	(2)	(3)	(4)	(5)	(6)
All	1.73(0.79)	3.12(0.97)	10.0(3.55)	5.14(1.97)	14.8(7.33)	33.4(16.2)
1-5	9.62(4.75)	11.4(4.60)	22.9(8.23)	21.1(11.2)	38.5(18.3)	58.0(18.5)
6-10	0.22(0.11)	2.16(1.15)	11.0(6.00)	2.14(2.07)	12.2(12.1)	37.8(25.9)
11-20	0.15(0.08)	1.48(0.77)	7.51(3.99)	1.92(1.91)	9.78(8.48)	27.8(20.8)
21-30	0.11(0.06)	1.12(0.61)	5.66(3.12)	1.86(1.86)	9.31(7.93)	24.4(18.5)

Panel 3: $b_{it} \in [0.1, \bar{r} - 0.1]$						
	Unknown parameter: φ			Unknown parameters: τ_A, τ_B, φ		
Periods	(1)	(2)	(3)	(4)	(5)	(6)
All	1.72(0.78)	3.12(0.96)	10.0(3.55)	4.87(3.02)	14.3(6.72)	32.4(13.6)
1-5	9.58(4.66)	11.3(4.52)	22.9(8.19)	18.9(8.07)	31.8(9.99)	49.7(12.6)
6-10	0.22(0.11)	2.16(1.15)	11.0(6.00)	2.25(3.29)	11.9(8.50)	34.8(18.8)
11-20	0.15(0.08)	1.48(0.77)	7.51(3.99)	2.04(3.19)	10.7(7.86)	28.3(16.5)
21-30	0.11(0.06)	1.12(0.61)	5.66(3.12)	1.97(3.16)	10.3(7.70)	26.5(15.8)

Standard errors in parentheses.
In column (1) and (4), $\sigma^k = 0.001 \times V(k) = 0.000287$.
In column (2) and (5), $\sigma^k = 0.01 \times V(k) = 0.00287$.
In column (3) and (6), $\sigma^k = 0.05 \times V(k) = 0.01435$.

estimates result in values of $b_{it} = 0$ or $b_{it} = \bar{r}$ (which precludes learning in the next period according to the learning rule), which is shown by the fact that the differences are not affected by the change of permissible range for b_{it}. The values are almost identical over Panels 1-3.

Things look worse when there are three unknown parameters. When $\sigma^k = 0.001 \times V(k)$, agents manage to learn rather fast, and it is only in period 1-5 where difference to equilibrium values is really substantial in all panels. But with more imprecision the differences become quite large, especially when $\sigma^k = 0.05 \times V(k)$, and the differences even in periods 21-30 are between 24-34 percent. One explanation is that the noisy performance measures cause agents to choose $b = 0$ or $b = \bar{r}$, as can be seen by the difference between Panel 1 on the one hand, and Panel 2 and 3 on the other. Such situations may be interpreted as coordination breakdowns, or the discontinuing of a new project, but in many instances it may not be plausible that all of the resources go to one area even if agents were to believe that the other type of activity is not worth doing. However, this source of deviation is ruled out in Panels 2 and 3, and there are still substantial deviations from equilibrium values left after 30 periods. Moreover, the differences between these two panels are small for most periods, which indicates that restrictions on b_{it} do not further learning over extended periods of time.

A concern may that the deviations are due to too wide ranges for the initial beliefs. This does not seem to be the case though; while the deviations decrease in all periods, the average total absolute difference in periods 21-30 is still over 25 percent when I use $\hat{\tau_{A0}}, \hat{\tau_{B0}}, \hat{\varphi}_0 \in [0.3, 0.5]$ and $b_{j0} \in [0.2, 0.28]$ and $\sigma^k = 0.05 \times V(k)$ (results available on request). The level of imprecision thus seems to have a much larger impact on the possibilities for agents to learn to play equilibrium values than their initial beliefs.

It is of course difficult to say in general what a reasonable amount of noise is, since it depends on the activity and the measure in question. But the results provide another potential explanation of coordination problems in public sector organizations: if agents have to use noisy performance measures to estimate the value of resource allocations, it may take a long time to learn the equilibrium allocations even if the agents use a very efficient learning procedure such as OLS. If a period is taken to be one year (a very common budget period), 30 periods is a substantial amount of time. If the equilibrium allocation corresponds to the efficient allocation, then this also implies substantial inefficiency. The interdependence among activities provides an added dimension of difficulty as agents are affected by each other's learning. Therefore, imprecise performance measures in one organization may be a concern for other, interdependent organizations.

2.7 Concluding remarks

This paper examines two related reasons why coordination problems are common among public sector organizations, and why they may be difficult to solve. The analysis suggests that coordination problems ought to be more common in public sector organizations than in private sector organizations; not because organizational coordination problems differ in kind, but because performance measurement problems are more severe and the instruments available to create coordination incentives are more limited and blunt.

First of all, unless the interdependencies of agents are managed somehow, resource allocation is likely to be inefficient. The model shows that interdependencies may lead to inefficient resource allocations when measures are assigned individually, even if agents' motivation is aligned with the principal, and performance measures are undistorted and precise. Shared targets solve the coordination problem with perfect measures and aligned motivation. They may also improve incentives to coordinate when measures are distorted and motivation misaligned, but the success depend on the interplay of distortion, motivation, and the relative importance of core and joint activities for value. For complements (and vice versa for substitutes), such situations are most likely to arise when agents are highly motivated by core activities, and/or performance measures overestimate the value of core activities.

An interesting question for the usefulness of shared targets is therefore whether motivation can be expected go in any particular direction? I would argue that we should expect agents to normally give higher priority to core activities. This could be for reasons of career concerns or because of identification with organizational missions, or both. If performance measures indicate the ability of a manager to potential employers and core activities are the manager's own responsibility whereas responsibilities for joint activities are shared, then measures of core activities reasonably constitute a more informative indication of the manager's ability. The manager would thus have incentives to give core activities higher priority. It is also reasonable to expect managers to be more likely to choose professions where they identify with the core activities of their organization; e.g. people who are interested in teaching are more likely to become teachers and subsequently headmasters, than to be self-selecting into the social services. If this reasoning is correct, complements are likely to present more severe coordination problems than substitutes.

Imprecision in the form of variance of the performance measures has two distinct effects, both potentially adverse. First, if agents are risk-averse (which they are assumed to be here) noisy measures increase the risk borne by agents, risk for which they demand compensation. Compensation, in the form of wages, is taken out of available resources and there is consequently less resources to allocate to productive activities. As adding measures increases total variance, this channel affects the choice between individual and shared targets as well. Second, if the agents have to learn at least some of the parameters of their value function, noisy measures may result in a very long learning period. In the simulations presented here, agents use a least squares learning rule to estimate the parameters. Although this rule is likely to be a very idealized way of learning, allocations with noisy measures are frequently quite far from equilibrium values after 30 periods when three parameters have to be learned. None of these effects of imprecision are of course particular to public sector organizations but may be aggravated in such organizations, due to the relative difficulty of measuring outcomes. Investing resources to develop more precise performance measures would thus seem worthwhile, especially for new collaborative activities.

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2.A Calculations

2.A.1 Proposition 1

Four scenarios may be possible depending on the parameter values in the value functions (the scenario where $a_i = b_i = 0$ is ruled out by assumption):

1. $a_i > 0, b_i = 0$
2. $a_i = 0, b_i > 0$
3. $a_i > 0, b_i > 0, a_i + b_i = r$
4. $a_i > 0, b_i > 0, a_i + b_i < r$

The agents' and the principal's maximization problems are described first below; and then the value created is compared in each of the four cases.

The agents' problem

As the agents are assumed to be identical, it is enough to show the solutions for one agent. As $v^k = 0$ and there is no uncertainty, the expectations operator is dropped and as all elements of $\boldsymbol{m}$ are equal, this vector is reduced to the scalar m. Under individual targets an agent maximize:

$$\max_{a_i,b_i} u_i = -exp\left[-\delta\left(w + m\left(\sum_{p^k \in P_i^I} p^k - \bar{p}^k\right)\right)\right] \tag{2.30}$$

This expression is maximized subject to

$$r_i \geq a_i + b_i \tag{2.31}$$

$$a_i, b_i \geq 0 \tag{2.32}$$

which yields the following Lagranian:

$$\begin{aligned} L = -exp\left[-\delta\left(w + m(\theta a_i + \rho b_i + \tau a_i b_i + \varphi b_i b_j - \bar{p}^k)\right)\right] \\ +\lambda\left(r_i - a_i - b_i\right) - \mu_a\left(-a_i\right) - \mu_b\left(-b_i\right) \end{aligned} \tag{2.33}$$

The corresponding Kuhn-Tucker conditions are:

$$\frac{\partial L}{\partial a_i} = m\left(\theta + \tau b_i\right) exp(\cdot) - \lambda + \mu_a = 0 \tag{2.34}$$

$$\frac{\partial L}{\partial b_i} = m\left(\rho + \tau a_i + \varphi b_j\right) exp(\cdot) - \lambda + \mu_b = 0 \tag{2.35}$$

$$\lambda \geq 0, \lambda = 0 \text{ if } a_i + b_i < r_i \tag{2.36}$$

$$\mu_a \geq 0, \mu_a = 0 \text{ if } a_i > 0 \tag{2.37}$$

$$\mu_b \geq 0, \mu_b = 0 \text{ if } b_i > 0 \tag{2.38}$$

(2.34) and (2.37) imply

$$m\left(\theta + \tau b_i\right) exp\left(\cdot\right) - \lambda \leq 0 \tag{2.39}$$

where (2.39) is equal to 0 if $a_i > 0$. (2.35) and (2.38) imply

$$m\left(\rho + \tau a_i + \varphi b_j\right) exp\left(\cdot\right) - \lambda \leq 0 \tag{2.40}$$

where (2.40) is equal to 0 if $b_i > 0$.

The principal's problem

An perfectly informed and risk neutral principal would maximize value directly according to

$$V^* = V_1 + V_2 = \theta(a_1 + a_2) + \rho(b_1 + b_2) + \tau(a_1 b_1 + a_2 b_2) + \varphi(b_1 b_2 + b_1 b_2) \tag{2.41}$$

subject to

$$1 \geq r_1 + r_2 + w_1 + w_2 \tag{2.42}$$

$$a_i, b_i \geq 0 \tag{2.43}$$

Agents are identical so resources allocated to each agent are $r_1 = r_2 = r \geq a_i + b_i$. This yields the following Lagranian

$$L = \theta(a_1 + a_2) + \rho(b_1 + b_2) + \tau(a_1 b_1 + a_2 b_2) + \varphi(b_1 b_2 + b_1 b_2) \tag{2.44}$$

$$+\lambda(2r - a_1 - b_1 - a_2 - b_2) - \mu_1(-a_1) - \mu_2(-a_2) - \mu_3(-b_1) - \mu_4(-b_2) \tag{2.45}$$

and the following Kuhn-Tucker conditions

$$\frac{\partial L}{\partial a_1} = \theta + \tau b_1 - \lambda + \mu_1 = 0 \tag{2.46}$$

$$\frac{\partial L}{\partial b_1} = \rho + \tau a_1 + 2\varphi b_2 - \lambda + \mu_2 = 0 \tag{2.47}$$

$$\frac{\partial L}{\partial a_2} = \theta + \tau b_2 - \lambda + \mu_3 = 0 \tag{2.48}$$

$$\frac{\partial L}{\partial b_2} = \rho + \tau a_2 + 2\varphi b_1 - \lambda + \mu_4 = 0 \tag{2.49}$$

$$\lambda \geq 0, \lambda = 0 \text{ if } a_i + b_i < r_i \tag{2.50}$$

$$\mu_1 \geq 0, \mu_1 = 0 \text{ if } a_1 > 0 \tag{2.51}$$

$$\mu_2 \geq 0, \mu_2 = 0 \text{ if } b_1 > 0 \tag{2.52}$$

$$\mu_3 \geq 0, \mu_3 = 0 \text{ if } a_2 > 0 \tag{2.53}$$

$$\mu_4 \geq 0, \mu_4 = 0 \text{ if } b_2 > 0 \tag{2.54}$$

In turn, these equations implies that the following conditions hold

$$\theta + \tau b_1 - \lambda \leq 0 \; (= 0 \text{ if } a_1 > 0) \tag{2.55}$$

$$\rho + \tau a_1 + 2\varphi b_2 - \lambda \leq 0 \; (= 0 \text{ if } b_1 > 0) \tag{2.56}$$

$$\theta + \tau b_2 - \lambda \leq 0 \; (= 0 \text{ if } a_2 > 0) \tag{2.57}$$

$$\rho + \tau a_2 + 2\varphi b_1 - \lambda \leq 0 \; (= 0 \text{ if } b_2 > 0) \tag{2.58}$$

Value in scenario 1-4

Below, the derived conditions for the principal and the agents are compared in the four scenarios:

1. *Agents*: As $\rho, \tau > 0$, (2.40) implies that $\lambda > 0$, i.e. there is a positive marginal value of allocating additional resources to agent i. Thus, $(a_i, b_i) = (r_i, 0)$ is the candidate for a maximum point in this scenario. For each agent, $V^I(r_i, 0) = \theta r_i$.

Principal: Use (2.56) and (2.58) to see that as $\rho, \tau > 0, \lambda > 0$. Thus, for each agent $(a_i^*, b_i^*) = (r_i, 0)$ is the candidate for a maximum point in this scenario. Each agent produce a value of $V^*(r_i, 0) = \theta r_i$.

2. *Agents*: As $\theta, \tau > 0$, (2.39) implies that $\lambda > 0$. Thus, in max $(a_i, b_i) = (0, r_i)$, which yields $V^I(0, r_i) = \rho r_i + r_i^2 \varphi$ for each agent.

Principal: $\theta, \tau > 0$, so $\lambda > 0$ according to (2.55) and (2.57). Therefore, $(a_i^*, b_i^*) = (0, r_i)$ is the candidate for the maximum point. Value per agent is $V^*(0, r_i) = \rho r_i + r_i^2 \varphi$.

As $V^I = V^*$ in both scenarios, this concludes (i) and (ii). See scenario 3 for the parameter values that imply that max is in (i) and (ii).

3. *Agents*: Here (2.39) and (2.40) holds with equality, which makes the first-order conditions for agent $i = 1, 2$ equal to

$$m(\theta + \tau b_i) exp(\cdot) - \lambda = 0 \tag{2.59}$$

$$m(\rho + \tau a_i + \varphi b_j) exp(\cdot) - \lambda = 0 \tag{2.60}$$

$$r_i - a_i - b_i = 0 \tag{2.61}$$

Using that $b_i = b_j$ and the three conditions to solve for a_i, b_i:

$$b_i = \frac{\rho + r_i \tau - \theta}{2\tau - \varphi} \tag{2.62}$$

$$a_i = r_i - \frac{\rho + r_i \tau - \theta}{2\tau - \varphi} \tag{2.63}$$

To get these allocations the following must hold $2\tau > \varphi$, $\theta + \tau b_i \geq 0$, $\rho + r_i \tau > \theta$ and $(\rho + r_i \tau - \theta)/(2\tau - \varphi) < r_i \Leftrightarrow \rho - r_i(\tau - \varphi) < \theta$. The first two hold by definition, whereas the second two are the conditions stated in the proposition, which we thus assume hold in this case.

Principal: The candidate point can be solved from the fact that (2.55)-(2.58) holds with equality and that $r_i - a_i - b_i = 0$. The resulting allocations are:

$$b_i^* = \frac{\rho + r_i\tau - \theta}{2(\tau - \varphi)} \tag{2.64}$$

$$a_i^* = r_i - \frac{\rho + r_i\tau - \theta}{2(\tau - \varphi)} \tag{2.65}$$

For these to hold, $\varphi < \tau$ and $\theta + \tau b^* \geq 0$, which holds by assumption, and $\theta < \rho + r_i\tau$ and $(\rho + r_i\tau) - \theta)/2(\tau - \varphi) < r_i$ which corresponds to the conditions of the proposition in the principal's case. To compare the principal's allocations to the agents, I use a loss function l, the fact that $a_i = r_i - b_i$ and compare allocations for one agent as follows (and drop the indexes as there should not be any risk of confusion):

$$\begin{aligned}
l(b,r) &= V^I - V^* = \theta(a - a^*) + \rho(b - b^*) + \tau(ab - a^*b^*) + \varphi(b^2 - b^{*2}) \\
&= \theta(r - b - r^* + b^*) + \rho(b - b^*) + \tau((r - b)b - (r^* - b^*)b^*) + \varphi(b^2 - b^{*2}) \\
&= (b - b^*)(\rho - \theta) - (b^2 - b^{*2})(\tau - \varphi) + (rb - r^*b^*)\tau + (r - r^*)\theta \\
&= (b - b^*)(\rho + r\tau - \theta) - (b^2 - b^{*2})(\tau - \varphi)
\end{aligned}$$

Where the last equality is the result of $r = r^*$, which holds according to the stated assumptions. $V^* > V^I$ when $l < 0$, which is the case if

$$(b - b^*)(\rho + r\tau - \theta) < (b^2 - b^{*2})(\tau - \varphi) \Leftrightarrow (b - b^*)\frac{\rho + r\tau - \theta}{\tau - \varphi} < b^2 - b^{*2}$$

Let $(\Delta b) = b - b^*$. Then, as

$$b^* = \frac{\rho + r\tau - \theta}{2(\tau - \varphi)} \Leftrightarrow \frac{\rho + r\tau - \theta}{\tau - \varphi} = 2b^*$$

write

$$(b - b^*)\frac{\rho + r\tau - \theta}{\tau - \varphi} < (b^2 - b^{*2}) \Leftrightarrow 2b^*(\Delta b) < (b^* + (\Delta b))^2 - b^{*2}$$

$$\Leftrightarrow$$

$$2b^*(\Delta b) < 2b^*(\Delta b) + (\Delta b)^2 \Leftrightarrow 0 < (\Delta b)^2$$

which holds for all $(\Delta b)^2 \neq 0$. As

$$\begin{aligned}
(\Delta b) = \frac{\rho + r\tau - \theta}{2\tau - \varphi} - \frac{\rho + r\tau - \theta}{2(\tau - \varphi)} &= \\
-\frac{(\rho + r\tau - \theta)\varphi}{(2\tau - \varphi)2(\tau - \varphi)} &= -\frac{b^*\varphi}{2\tau - \varphi}
\end{aligned}$$

(Δb) is only zero when $\varphi = 0$ and/or $b^* = 0$, which would be a contradiction to the stated assumptions. Moreover, as derivative of the expression with respect to φ is strictly negative if $\varphi > 0$ (complements) and strictly positive if $\varphi < 0$ (substitutes) the difference in value to the optimal allocation is increasing in all permissible absolute values of φ.

4. *Agents*: This scenario implies that $\lambda = 0$ and $r_i - a_i - b_i > 0$. The first-order conditions are

$$\theta + \tau b_i = 0 \tag{2.66}$$

$$\rho + \tau a_i + \varphi b_j = 0 \tag{2.67}$$

As θ, τ and b_i are all positive by assumption, (2.66) cannot hold, and this scenario cannot occur.■

2.A.2 Corollary 1

Using P^S, agent i maximizes:

$$\max_{a_i, b_i} u_i = -exp\left(-\delta\left(w + m\sum_{p^k \in P_i^S} p^k - \bar{p}^k\right)\right) \tag{2.68}$$

subject to

$$r_i \geq a_i + b_i$$
$$a_i, b_i \geq 0$$

This yields the following Lagranian:

$$L = -exp\left[-\delta\left(w + m(\theta a_i + \rho(b_i + b_j) + \tau a_i b_i + \tau_B a_j b_j + 2\varphi b_i b_j - \bar{p}^k)\right)\right]$$
$$+ \lambda(r_i - a_i - b_i) - \mu_a(-a_i) - \mu_b(-b_i)$$

Solving this problem in the same way as the agents' problem in scenario 3 above results in

$$b_i = \frac{\rho + r_i\tau - \theta}{2(\tau - \varphi)}$$

which is equal to the share allocated to $b_i^*, i = 1, 2$ in proposition 1. Therefore, $V^* = V^S$.■

2.A.3 Proposition 2

The share allocated to joint activities under individual targets is

$$b^I = \frac{m^{B_i} d^{B_i} (\rho + r\tau_B) + m^{A_i} d^{A_i} (r\tau_A - \theta)}{m^{A_i} d^{A_i} 2\tau_A + m^{B_i} d^{B_i} (2\tau_B - \varphi)}.$$

As m^{A_i} and d^{A_i}, and m^{B_i} and d^{B_i} have similar derivatives, I exemplify with m^{A_i} and m^{B_i}. Differentiating b^I with respect to m^{A_i}, yields

$$\frac{\partial b^I}{\partial m^{A_i}} = \frac{d^{A_i} (r\tau_A - \theta) \left(m^{A_i} d^{A_i} 2\tau_A + m^{B_i} d^{B_i} (2\tau_B - \varphi)\right)}{\left(m^{A_i} d^{A_i} 2\tau_A + m^{B_i} d^{B_i} (2\tau_B - \varphi)\right)^2} - \frac{d^{A_i} 2\tau_A \left(m^{B_i} d^{B_i} (\rho + r\tau_B) + m^{A_i} d^{A_i} (r\tau_A - \theta)\right)}{\left(m^{A_i} d^{A_i} 2\tau_A + m^{B_i} d^{B_i} (2\tau_B - \varphi)\right)^2}$$

As the denominator, as well as d^{A_i} and $m^{B_i} d^{B_i}$ are strictly positive, and the term

$$(r\tau_A - \theta)\, m^{A_i} d^{A_i} 2\tau_A$$

is present on both side of the minus sign and thus cancel out, $\frac{\partial b^I}{\partial m^{A_i}}$ is negative when

$$2\tau_A (\rho + r\tau_B) > (r\tau_A - \theta)(2\tau_B - \varphi) \Leftrightarrow \theta (2\tau_B - \varphi r) + \tau_A (2\rho + \varphi r) > 0.$$

Differentiating with respect to m^{B_i} yield

$$\frac{\partial b^I}{\partial m^{B_i}} = \frac{d^{B_i} (\rho + r\tau_B) \left(m^{A_i} d^{A_i} 2\tau_A + m^{B_i} d^{B_i} (2\tau_B - \varphi)\right)}{\left(m^{A_i} d^{A_i} 2\tau_A + m^{B_i} d^{B_i} (2\tau_B - \varphi)\right)^2} - \frac{\left(m^{B_i} d^{B_i} (\rho + r\tau_B) + m^{A_i} d^{A_i} (r\tau_A - \theta)\right) d^{B_i} (2\tau_B - \varphi)}{\left(m^{A_i} d^{A_i} 2\tau_A + m^{B_i} d^{B_i} (2\tau_B - \varphi)\right)^2}$$

As the denominator, d^{B_i} and $m^{A_i} d^{A_i}$ are strictly positive, and the term

$$(\rho + r\tau_B)\, m^{B_i} d^{B_i} (2\tau_B - \varphi)$$

appears on both sides of the minus sign, the derivative is negative when

$$\begin{aligned} &2\tau_A (\rho + r\tau_B) + (r\tau_A - \theta)(2\tau_B - \varphi) < 0 \\ &\Leftrightarrow \\ &\theta (2\tau_B - \varphi) > \tau_A (2\rho + r (4\tau_B - \varphi)) . \blacksquare \end{aligned}$$

Chapter 3

Organizational coordination and costly communication with boundedly rational agents

with Torsten Jochem

3.1 Introduction

When agents are specialized and interdependent, coordination becomes an important task and the ability to facilitate coordination has been argued to be a major reason for the existence of organizations (e.g. Simon, 1991; Grant, 1996). Consequently, understanding why groups of agents may or may not be able to coordinate their actions, and how coordination mechanisms should be designed, is one of the keys to explaining and improving organizational efficiency.[1]

Along with the motivation to cooperate, organizational theory lists a sufficient degree of *shared knowledge* among agents as a necessary prerequisite for successful coordination in differentiated organizations (Hoopes and Postrel, 1999; Kretschmer and Puranam, 2008). Communication may seem as an obvious way to create shared knowledge by simply transferring information about what agents intend to do. However, when agents act strategically, communication is not straightforwardly translated into efficient coordination, as is shown by experimental results in the so-called weakest-link game (e.g. Weber et al., 2001; Kriss et al., 2012).[2]

[1]See Lawrence and Lorsch (1967), Sinha and Van de Ven (2005), Grandori and Soda (2006) and Sherman and Keller (2011) for evidence of the difficulties in choosing and/or designing appropriate coordination mechanisms in organizations.

[2]The game is also known as the "minimum effort game" (e.g. Van Huyck et al., 1990) and the "weak-link game" (e.g. Camerer, 2003).

In a weakest-link game, subjects simultaneously choose an action, represented by an integer, from a set of available actions ranging from 1 to some highest integer K. A subject's payoff increases with the minimum action chosen in the group and decreases with the deviation of the subject's own choice from this minimum. The subjects' actions are therefore highly interdependent and they have a common interest to coordinate their actions. In particular, combinations where all agents choose the same action are strict Nash equilibria and everyone choosing the highest ranked action is the payoff dominant combination. However, starting with Van Huyck et al. (1990), a large experimental literature shows that play in groups of more than three almost invariably converges towards the least efficient equilibrium when subjects are not helped by any coordination mechanism. Later studies show that costless or mandatory communication may substantially increase efficiency (Blume and Ortmann, 2007; Kriss et al., 2012). Much smaller or no gains compared to the no communication benchmark are observed in two treatments with *costly* communication in Kriss et al. (2012) though, despite the fact that the costs are very small in relation to the potential gains of efficient coordination.

As arguably all organizational communication, regardless of the form, takes time and is thus in some sense costly, this phenomenon is important to understand. However, to the best of our knowledge, there are no previous theoretical models of communication that analyze similar problems of organizational coordination. The cheap-talk literature examines the effect of costless pre-play communication on outcomes in a variety of games (e.g. Crawford and Sobel, 1982; Farrell and Rabin, 1996).[3] Models of costly communication analyze sender-receiver games and examine how outcomes vary with the degree of private information and/or conflicts of interest between sender and receiver (e.g. Austen-Smith, 1994; Dewatripont and Tirole, 2005; Gossner et al., 2006; Calvo-Armengol et al., 2009; Wilson, 2012). But as subjects have a common interest in achieving efficient coordination and the parameters of the experimental game are common knowledge, pri-

[3] More recently, several studies have modelled cheap-talk among boundedly rational players by using level-k models of strategic thinking. Ellingsen and Östling (2010) find that as long as truth-telling is lexicographically preferred to lying, both one-way and two-way communication facilitate coordination in all n-player common interest games where there are also positive spillovers and strategic complementarities, as in the weakest-link game. See also e.g. Crawford (2003) and Wengström (2008) for results in hide-and-seek and price competition games, respectively.

vate information and conflicts of interests are unlikely explanations of the coordination difficulties in the situations we are interested in.

We use the weakest-link game and the experimental conditions of Kriss et al. (2012) as the basis for our model. Agents are boundedly rational in a similar sense to that in models by Young (1993, 1998) and Kandori et al. (1993): they choose *myopic* best replies given their expectations, have *limited information processing capabilities*, and may occasionally *experiment* or make *mistakes*. Agents are myopic in that they choose best replies for just one period at a time, i.e. they are not forward-looking. Agents are limited in their information processing as we assume that they only use the previous period's information to form their expectations about communication and actions in the present period. Mistakes and experiments are modelled by introducing a small probability that agents, instead of choosing a best reply, randomize uniformly over the set of available messages or actions. Our game is also repeated in contrast to the mostly one-shot settings used in the costly communication literature (Gossner et al. (2006) is an exception). Compared to e.g. Dewatripont and Tirole (2005), Calvo-Armengol et al. (2009) and Wilson (2012), we simplify and treat only the sending of information as costly.

We believe these behavioral assumptions are a reasonable approximation of the behavior of real world organizational members in settings characterized by high levels of strategic uncertainty. Moreover, we think the highly interdependent actions in the weakest-link game capture some of the essence of organizational coordination problems and the game is arguably a good stylized description of many organizational situations of interest.[4] Additionally, the stochastically stable states – the solution concept used by Kandori et al. (1993) and Young (1993, 1998) – may also be able to select among the strict Nash equilibria of the weakest-link game. As groups in the experiments seem to end up coordinated on certain equilibria in non-random ways, this is an advantage.

Stochastically stable states can be interpreted as the likely long-run state of a system or a process. However, experimental studies often use only a few periods (8-10 periods are common). To be able to say more about the short-

[4]Camerer (2003) mentions for example airplanes before departure, joint production of documents in law firms, accounting firms, and investments banks, and that production functions like the Cobb-Douglas with large exponents or Leontief functions also have similar properties (see e.g. Knez and Camerer (1994) for more examples).

run properties of the model and to relax some of the assumptions made, we simulate a version of the model. This also allows us to use parameter values that have not been used in experiments so far.[5]

Our main results are broadly consistent with the experimental literature. As a benchmark, we show that when agents are not allowed to communicate the unique stochastically stable state is the least efficient equilibrium. When communication is allowed, this may solve the coordination problem by helping agents to break out of inefficient states, but only if the cost of communication is small enough and/or the incentives to coordinate on the efficient action combination are strong enough. If so, the unique stochastically stable state is the payoff dominant equilibrium; otherwise the least efficient equilibrium is the only stochastically stable state. The reason why communication in our case may fail to solve coordination problems is that agents face a trade-off between lowering the strategic uncertainty for the group and the costs of communication. Therefore, in a sense there are also incentives to free ride on other agents' communication. Such behavior seems intuitively plausible and is also congruent with behavior in recent experiments, not only in weakest-link games with communication (Kriss et al., 2012), but also in other coordination games (Andersson and Holm, 2010b). The simulations show that the stochastically stable state is not only a long run phenomena, but also tend to be overrepresented in the short run. We also provide results that indicate that larger groups will find it harder to use communication to coordinate on efficient actions, but due to this may actually coordinate faster; that the exact level of message costs is not very important in our model; and that the effect of changing the strength of incentives does not have to be monotonic.

Communication may not only be possible in an organization, the organization may also have the authority to structure communication by imposing rules or routines for how its members should communicate. We examine analytically how two such routines – making communication mandatory and assigning one agent to be the team leader – change the outcome. In line with the experimental results in Kriss et al. (2012), mandatory communication leads to the result that the state where all agents coordinate on the

[5] Our model therefore also relates to the literature on agent-based models of organizations. See Chang and Harrington (2006) for a review of agent-based models of organizations and Shoham and Leyton-Brown (2009) for a review of agent-based models of communication.

payoff dominant action is the unique stochastically stable state. A team leader may improve coordination, but the team leader must both expect other agents to choose the communicated action and have enough authority over the group for efficient coordination to occur (see Weber et al. (2001) and Brandts and Cooper (2007) for experiments where communication by team leaders produces mixed results).

We proceed in the following way: section 3.2 reviews the experimental literature on weakest-link games. Section 3.3 outlines the model and presents the analytical results. Section 3.4 describes the simulation model and results, while section 3.5 contains concluding remarks.

3.2 Experiments with weakest link games

Given the difficulty of performing experiments in real organizations, laboratory experiments using simulated organizational environments are an important source of knowledge about the relationship between coordination mechanisms and efficiency. We review the experimental literature on weakest-link games below, with focus on studies that allow for communication between their subjects in some form.[6]

The weakest-link game was first used in an experimental setting by Van Huyck et al. (1990). Their main result, that large groups generally find it very difficult to coordinate on efficient equilibria and instead tend to converge to the least efficient equilibria, has since been replicated in almost all subsequent studies.[7] Van Huyck et al. (1990) used group sizes of 14-16 subjects and 7 actions (or effort levels) to choose from, but even groups of 4-5 subjects and a smaller number of actions produce similar results (e.g. Brandts and Cooper, 2007). Three-person groups, as in Knez and Camerer (1994), seem to do better, but are not able to fully coordinate on the efficient equilibrium. Two-person groups that are repeatedly matched to each other seem on the other hand to be able to coordinate on the efficient equilibrium

[6] See Devetag and Ortmann (2007) for a more comprehensive review that also includes experiments with other coordination games, e.g. median-action and stag hunt games.

[7] Engelmann and Normann (2010), in a study conducted in Denmark, present the one exception we have found. Using similar treatment conditions as in the original Van Huyck et al. (1990) study, but varying the group size, groups of 4 and 6 frequently coordinate on the most efficient equilibria. Their results seem to be driven by the share of native Danes in the groups and the authors hypothesize that cultural factors, such as high levels of trust, may be the reason behind the results.

most of the time (Van Huyck et al., 1990; Camerer, 2003).[8]

Costless and mandatory communication by electronic messages significantly improve coordination, whether it is the players who communicate intended actions among themselves as in Blume and Ortmann (2007) (who use groups of 9 subjects), or an external manager as in Brandts and Cooper (2007) (who use groups of 5 subjects, including the manager). In the latter experiment, the largest gain is achieved in a two-way communication treatment when employees can also send messages to the manager (but not to each other). However, a speech by a randomly selected leader after two periods is not enough to ensure coordination on the more efficient equilibria in the ensuing six periods in Weber et al. (2001): their groups of 9-10 subjects receive the minimum payoff in 75 percent of the trials. Chaudhuri et al. (2009) use (free-form) advice from one non-overlapping generation of players to another successor generation. Coordination on more efficient equilibria is frequent in treatments when all advice is made public, and the predecessor generation unanimously urges subjects to play the payoff dominant action, but otherwise not. Private advice between one predecessor and one successor consistently fails to promote efficiency.

Few experiments have been conducted where direct and *costly* communication between subjects is allowed. The only study we have found using the weakest-link game is a recent experiment by Kriss et al. (2012).[9] When messages are mandatory or without cost, an overwhelming majority of subjects in their groups of 9 send messages indicating the efficient action (which is 7) in the first period. In one high and one low message cost treatment,

[8] To overcome the problems found in larger groups, other types of coordination mechanisms have also been tried: higher financial incentives raise efficiency in Brandts and Cooper (2006a) and Hamman et al. (2007), and non-monetary incentives in the form of disapproval ratings do so even more in Dugar (2010). Earlier experience of successful coordination is not enough to stop inefficient coordination when two smaller groups are merged into one larger in Knez and Camerer (1994), but helps in Devetag (2005). Weber (2006) shows that more efficient equilibria are attainable when groups start small and then slowly grow up to 12 members, although many groups still fail to sustain efficient coordination. Riedl et al. (2011) find that efficiency is much higher when groups can exclude members. Information about individual actions is beneficial but no panacea in Berninghaus and Ehrhart (2001) and Brandts and Cooper (2006b), and not efficiency improving in the original experiments of Van Huyck et al. (1990), in Devetag (2005), and in Hamman et al. (2007). Recently, Deck and Nikiforakis (2012) has shown that real-time monitoring of other subjects actions helps coordination.

[9] For experiments with costly communication in other games, see e.g Andersson and Holm (2010a) and Wilson (2012).

significantly fewer send a message and the modal behavior is to not send a message. Those who do send messages mostly send message 7. Over time, these patterns are reinforced. The first period average minimum actions are similar across treatments, whereas average actions and average minimum actions in the last four rounds out of eight are highest in the mandatory communications treatment (5.46 and 4.68), almost as high in the treatment with costless communication (5.13 and 4.17), lower in the low cost treatment (3.41 and 2.75), and much lower in the high cost treatment (1.59 and 1.00). The high cost treatment therefore yields practically the same results as when communication is not possible. In the two costly communication treatments, only 4 out 14 groups manage to have a higher minimum effort than 1 in the last period (all in the low cost treatment), and only one group coordinates on 7.

3.3 The model

This section presents the model and analytical results. We start in section 3.3.1 with a description of the weakest-link game and how agents choose messages and actions, while sections 3.3.2 and 3.3.3 present results with and without communication as well as when simple rules are used to structure the agents' communication.

3.3.1 A model of communication in weakest-link games

We consider a finite set of agents $N = \{1, 2, ..., n\}$, $n \geq 2$. Let $A_i = \{1, 2, ..., K\}$ be the set of actions for agent i. Actions are represented by integers where 1 is the lowest ranked action and K is the highest ranked. Let $M_i = A_i \cup \{\varnothing\}$ be the set of available messages, where the empty message represents the case of no communication. The set of all possible combinations of messages is denoted $M = \prod_{i \in N} M_i$ and the corresponding set of actions $A = \prod_{i \in N} A_i$. Agents' tasks in every period $t = 1, 2, ...$ of the infinitely repeated game is to choose a message $m_i^t \in M_i$ in the communication stage, and an action $a_i^t \in A_i$ in the action stage.

To start with, we structure communication and actions in a way similar to the experimental conditions of Blume and Ortmann (2007) and Kriss et al. (2012): agents send one message per period and this message is sent to all other agents. Furthermore, messages are sent simultaneously so agents do not learn the other agents' messages before sending their own. Let $m^t = \{m_1^t, m_2^t, ..., m_n^t\}$ be sent messages and m_{-i}^t agent i's received messages in period t, while the collected choices of actions in period t are denoted $a^t = \{a_1^t, a_2^t, ..., a_n^t\}$. The cost of sending message m_i is $c(m_i)$ and we assume that $c(m_i) = c(m_j)\ \forall i, j \in N$, and that it is constant over time, while $c(m_i) = c > 0$ for all $m_i \neq \varnothing$, and $c_i(\varnothing) = 0$ (i.e. not communicating is costless). Receiving messages is not costly.

After the action stage, payoffs in the weakest-link game with costly communication are given by a function $\pi : M \times A \to \mathbb{R}$, defined for each agent i in period t as

$$\pi_i(a^t, m_i^t) = \alpha \min_{j \in N}\{a_j^t\} - \beta a_i^t - c(m_i^t) \tag{3.1}$$

where α and β are parameters of the game, $\alpha > \beta > 0$, and $\min_{j \in N}\{a_j^t\}$ is the lowest ranked (minimum) action played by some $j \in N$. We assume that the payoff function is common knowledge and the same for all agents in every period.

The next step is to describe how agents choose messages and actions. First, we assume that all agents follow the same decision-making process, characterized by myopic best replies, limited information processing, and mistakes and experiments. Note that we do not assume that agents know they are identical, or that the details of the decision-making process is common knowledge. As in the experiments of Blume and Ortmann (2007) and Kriss et al. (2012), we let agents observe all messages whereas they are only informed about the minimum action in each period.[10] To form their expectations about play in current period agents use the previous period, that is in t agent i uses the history of play in $t-1$, $h^{t-1} = \big(m^{t-1}, \min_{j \in N}\{a_j^{t-1}\}\big)$.

Starting with how expectations are formed in the communication stage, let agent i's subjective probability of action k becoming the minimum action in period t before any message is sent be given by a function $q : M \times A \to [0, 1]$. We assume that the subjective probability put on k is influenced by i) the prospective content of i's own message m_i^t; ii) other agents' messages in the previous period; and iii) if $\min_{j \in N}\{a_j^{t-1}\} = k$, i.e. if k was the minimum action in the previous period or not.

[10] That agents can only observe the minimum action and not individual actions of other agents is the most commonly used informational condition also in the experimental literature without communication, see Devetag and Ortmann (2007) for exceptions.

Let $q_i^k(m_i^t)$ be short for $q\left(k|m_i^t, h^{t-1}\right)$ and let $\sum_{k=1}^{K} q_i^k(m_i^t) = 1$ for each m_i^t. We make four more specific assumptions about the $q_i^k(m_i^t)$, which are further discussed below:

Assumption 1: Agents form expectations based on a distribution they believe is stationary; i.e. they expect the empirical frequencies of other agents' messages in $t-1$ to be the same in period t.

Assumption 2: For $t > 1$ and all $i \in N$ and all $k, l \in A_i$, if there is no message $m_j^{t-1} = k$ and $\min_{j\in N}\{a_j^{t-1}\} \neq k$, then in t, $q_i^k(\varnothing) = q_i^k(l) = 0$. So, besides the initial period,[11] if there is no indication of k, either by communication or by earlier play, then agent i places probability 0 on action k being the minimum in period t, unless agent i herself sends $m_i^t = k$.

Assumption 3: The subjective probabilities are influenced by the frequencies of messages, not their labels. That is, if we change the labels on messages and actions equal to l in h^{t-1} to k and call this new history $\hat{h}^{t-1}$, then sending $m_i^t = l$ given h^{t-1} affect q_i^l exactly as sending $m_i^t = k$ affect q_i^k given $\hat{h}^{t-1}$.

Assumption 4: $q_i^k(m_i^t)$ is non-decreasing in the number of $m_j^{t-1} = k$, if $m_i^t = k$, and if $m_i^t = \varnothing$ and $\min_{j\in N}\{a_j^{t-1}\} = k$. In addition, if $m_i^t = k$ and $\min_{j\in N}\{a_j^t\} \neq k$, then $(q_i^k(k))^{t+1} \leq (q_i^k(k))^t \; \forall\, i \in N$ and strictly smaller when $(q_i^k(k))^t > 0$.

The first two assumptions have counterparts in several other game-theoretical learning models. Agents are assumed to treat the empirical distribution of play as stationary in fictitious play for example (Fudenberg and Levine, 2009). Many models in which expectations are based on empirical frequencies of past play include an assumption similar to the second (e.g. Young, 1998). The third assumption simply states that there is nothing intrinsically special about certain actions in terms of how expectations change due to agent i's own communication. If some actions are focal points, so that messages indicating such actions are expected to influence other agents' choice of action more than others, this would be a violation of the assumption.[12]

The fourth assumption adds some more structure to the conditional expectations about minimum actions. We think that it is reasonable that

[11] Given the assumptions about the agents' decision-making process, the initial expectations are of no consequence for our first results, but we discuss this issue in section 3.4.2.

[12] If the highest ranked action K is a focal point in this sense, this would not change the main message of proposition 2-4.

agents are at least not less likely to expect an action to be the minimum if it is indicated by messages, or that sending the empty message may be interpreted as an indication that the agent in question will continue to play last period's minimum action (rather than some other action). The second part of assumption 4 rules out the possibility of agents forever sending messages that subsequently never becomes the minimum action. A mismatch between a sent message and the subsequent minimum does however not imply that the conditional expectation of the sent message is necessarily forever lower. In period $t' > t' - 1 > t$, $(q_i^k(k))^{t'}$ is only determined by events in $t' - 1$ and not in t.

Given these assumptions and the common knowledge of the payoff function, we formulate the expected payoff of action k in time t conditional on agent i sending message m_i^t as follows:

$$\mathbb{E}\left(\pi_i(k)|m_i^t, h^{t-1}\right) = \sum_{g=k}^{K} q_i^g(m_i^t)k\left(\alpha - \beta\right) + \sum_{d=1}^{k-1} q_i^d\left(m_i^t\right)\left(\alpha d - \beta k\right) - c(m_i^t). \tag{3.2}$$

Because the lowest ranked action played by any agent is always payoff-determining, the risk associated with playing k decreases when the subjective probabilities of k and all higher ranked actions increase. Therefore, the expected payoff of k becoming the minimum increases with *all* q_i^g, such that $g \geq k$ (i.e. the term $\sum_{g=k}^{K} q_i^g(m_i^t)$ in equation (3.2)). Consequently, $\mathbb{E}\left(\pi_i(1)|m_i^t, h^{t-1}\right) = \alpha - \beta - c(m_i^t)$, regardless of the history. As all actions are higher ranked than 1, if played by any agent, action 1 always determines payoffs. For this reason, it can never be a best reply message to send $m_i^t = 1$.

To determine a best reply message, we are interested in the total or aggregate expected payoff conditional on a certain message and the history of play, denoted $\mathbb{E}(\pi_i|m_i^t, h^{t-1})$. What we have in mind is a procedure where agents contemplate each possible message, compare the expected payoffs, and then choose the message that yields the highest expected payoff. However, the expected payoffs for single actions can be aggregated into $\mathbb{E}(\pi_i|m_i^t, h^{t-1})$ in several different ways. For our first results, we assume the following:

$$\mathbb{E}(\pi_i|m_i^t, h^{t-1}) = \sum_{k=1}^{K} \mathbb{E}(\pi_i(k)|m_i^t, h^{t-1}). \tag{3.3}$$

That is, the agent sums the expected payoffs for the individual actions. The best reply correspondence for messages is then

$$BR_i^m = \{m_i^t \in M_i : \mathbb{E}\left(\pi_i | m_i^t, h^{t-1}\right) \geq \mathbb{E}\left(\pi_i | \hat{m}_i^t, h^{t-1}\right) \; \forall \, \hat{m}_i^t \in M_i\}. \quad (3.4)$$

If there is more than one message that is a best reply, we assume that the agents choose between these messages by randomizing uniformly. Mistakes and experiments are also possible: agent i chooses a best reply message according to the above procedure with probability $1 - \epsilon$, and with a (small) probability ϵ chooses a message in M_i by uniform randomization.

In the action stage, we assume that agents best-reply to expectations given by the frequencies of received messages and the minimum action in the previous period. When an agent receives messages from some but not all other agents, agents assume that the non-communicating agents will play the minimum action in the previous period. The subjective probabilities are given by a function $p : M \times A \to [0, 1]$, where p_i^k is the probability assigned by agent i to k being the minimum action. The expected payoff of an action k in period t is then

$$\mathbb{E}(\pi_i(k) | m_{-i}^t, \min_{j \in N}\{a_j^{t-1}\}) = \sum_{g=k}^{K} p_i^g k(\alpha - \beta) + \sum_{d=1}^{k-1} p_i^d (\alpha d - \beta k). \quad (3.5)$$

where $p_i^g = \frac{1}{n-1} \sum_{j \in N \setminus \{i\}} p_{ij}^g$ and $p_i^d = \frac{1}{n-1} \sum_{j \in N \setminus \{i\}} p_{ij}^d$, and

$$p_{ij}^g = \begin{cases} 1 & \text{if } m_j^t = g \\ 1 & \text{if } m_j^t = \varnothing \, \wedge \, \min_{j \in N}\left\{a_j^{t-1}\right\} = g \\ 0 & \text{otherwise} \end{cases}$$

and

$$p_{ij}^d = \begin{cases} 1 & \text{if } m_j^t = d \\ 1 & \text{if } m_j^t = \varnothing \, \wedge \, \min_{j \in N}\left\{a_j^{t-1}\right\} = d \\ 0 & \text{otherwise.} \end{cases}$$

The procedure implies that $\sum_{k=1}^{K} p_i^k = 1 \, \forall \, i \in N$. As in the communication stage, we assume that the expected payoff of any action k increases in the sum of subjective probabilities put on all higher ranked actions and the action itself, i.e. the term $\sum_{g=k}^{K} p_i^g$. Agents thus use the frequencies of messages to determine the subjective probabilities of actions, so again it is only the number of messages that counts, not their labels. The procedure implies that agents disregard their own message's effect on other agents,[13] and that agents do not expect that others always choose the action indicated by their message. If agents were to expect that other agents always choose actions in accordance with their messages, then the best reply action would be $a_i^t \leq \min(m_j^t)$ and lower only if at least one other agent send the empty message and the minimum action in period $t-1$ was some $k < \min(m_j^t)$.

With probability $1-\varepsilon$ agents choose an action in the best reply correspondence for actions

$$BR_i^a = \{k \in A_i : \mathbb{E}(\pi_i(k)|m_{-i}^t, \min_{j\in N}\{a_j^t\}) \geq \mathbb{E}(\pi_i(l)|m_{-i}^t, \min_{j\in N}\{a_j^t\})\ \forall\, l \in A_i\} \tag{3.6}$$

and with probability ε agents use a uniform randomization over all actions in A_i. We make the following assumption about the probabilities of mistakes/experiments:

Assumption 5: $\epsilon = \varepsilon$. That is, an agent is as likely to make a mistake or experiment in the communication stage as in the action stage. Furthermore, we assume that both ϵ and ε are identical for all agents and independent both across agents and over time.

The decision-making process in this section concerns agents that are myopic, have limited ability to process information, and may occasionally make mistakes or experiment with messages and actions that are not best replies. The model is a variant of the adaptive learning process developed by Young (1993, 1998) and Kandori et al. (1993), and the decision-making process forms what Young (e.g. 1993) calls a regular, perturbed Markov process.[14] For the results in the next two sections, we first find the *absorbing*

[13] Allowing agents to take their own message into consideration by letting $p_i^k = \frac{1}{n}\sum_{j\in N} p_{ij}^k$ does not alter the results for most values of the parameters. However, when $n(\beta/\alpha) < 1$, proposition 2 would also contain cases where more states in between 1 and K, such that no agent communicates and everyone chooses the same action, are stochastically stable.

[14] We depart from Young (1993, 1998) and others that have used a similar framework (e.g. Jackson and Watts, 2002; Goyal and Vega-Redondo, 2005) in that all agents choose an action in every period, instead of only one agent updating at t. Kandori et al. (1993) and Robles (1997) also let all players update their strategies in every period. The agents in Young (1993, 1998) furthermore use an individual random sample of the remembered history of play, which can be longer than one period, whereas Robles (1997) and Riedl et al. (2011) also let their agents use only the previous period. None of these models however include communication between agents.

states of the process – states that the process cannot leave without mistakes or experimentation – and second the *stochastically stable states*, which are roughly the absorbing states for which the number of mistakes/experiments needed for the process to leave is the highest. Stochastically stable states can be interpreted as the states where the process is most likely to be in the long run; they may but need not be unique, but there is always at least one (Young, 1993). We describe this and related concepts more in detail in section 3.A.1 in the Appendix.

3.3.2 No communication benchmark

To derive some benchmark results, we first assume that agents cannot communicate. Best reply actions are then as defined by equations (3.5) and(3.6); i.e. since $m_i^t = \varnothing$ for all $i \in N$, $p_{ij}^g = 1$ if $\min_{j \in N}\{a_j^{t-1}\} = g$ and 0 otherwise, and $p_{ij}^d = 1$ if $\min_{j \in N}\{a_j^{t-1}\} = d$ and 0 otherwise. This yields a Markov process P^ε on the state space A.

The assumption that $\alpha > \beta > 0$ implies that the combinations where all players choose the same action constitute the strict Nash equilibria of the game. Call the set of strict Nash equilibria $E = \{E_1, E_2, .., E_K\}$, where $1, ..., K$ corresponds to the ranking of actions. With this apparatus in place, we have the following result (all proofs are found in Appendix 3.A):

Proposition 1: Let the agents' decision-making process be defined by P^ε and let the state space be A. Then the unique stochastically stable state in the weakest-link game without communication is E_1.

The proposition indicates that the least efficient equilibrium, corresponding to all agents choosing action 1, is the most likely long-run outcome of the weakest-link game. For a similar result, see Robles (1997, proposition 3).[15] While this is in line with much of the experimental evidence reviewed in section 3.2, note that the result holds regardless of the number of players and of the incentives to choose the payoff dominant action (the ratio of α to β). This seems intuitively less convincing and is at odds with for example some of the results of experiments with two players.[16]

[15] See also Crawford (1995) for a different model of adaptive learning without communication that matches the short/medium run dynamics of the weakest-link and median action experiments run in Van Huyck et al. (1990) and Van Huyck et al. (1991).

[16] There are more general results available if agents have longer memory. For small groups

3.3.3 Results in the weakest-link game with communication

The relevant state space for the perturbed Markov process $P^{\varepsilon,\epsilon}$ defined by the decision-making procedure with communication is $S = M \times A$, and we denote a strategy profile $s \in S$ in period t as $s^t = (s_1^t, s_2^t, ..., s_n^t)$ where $s_i^t = (m_i^t, a_i^t)$. Let states where strategies are such that $s_i^t = (\varnothing, k)\ \forall\, i \in N$ be denoted E_k and the set of such states be E, i.e. we use, hopefully without any risk of confusion, the same notation for strategies corresponding to the strict Nash equilibria in the game without communication. This yields the following proposition:

Proposition 2: Let the agents' decision-making process be defined by $P^{\varepsilon,\epsilon}$ and let the state space be S. Then, $E_k \in E$ are the only candidates for stochastically stable states and in any $E_k \in E$, $q_i^l(l) = q_j^l(l)$ for all $i, j \in N$ and all $l \in A_i$. If

(*i*) $q_i^l(l) < \frac{\beta}{\alpha} + \frac{c}{\alpha(l-k)}$ in all $E_k \in E$ and for all pairs $k, l \in A_i$, then E_1 is the unique stochastically stable state;

(*ii*) $q_i^l(l) > \frac{\beta}{\alpha} + \frac{c}{\alpha(l-k)}$ in at least one $E_k \in E$ and for at least one pair $k, l \in A_i$, then E_K is the unique stochastically stable state.

The proposition implies that only states where no agent communicates and all choose the same action can be absorbing states. Conditions (*i*) and (*ii*)[17] in turn imply that either the lowest ranked of these states (all agents play action 1) or the highest (all agents play action K) is the unique stochastically stable state. The latter will be the case when agents' expectations that their message will sway the others to a higher ranked action, when "stuck" in some absorbing state, are high enough, so that the costs of communication are dominated by the higher expected payoff. Messages can then be used to break out of an inefficient absorbing state. As all agents use the same information in states like $E_k \in E$, if condition (*i*) holds for one agent it

and/or low β/α-ratios, E_K is also a common stochastically stable state (results available upon request). See also Honda (2012) for an illuminating theoretical explanation of equilibrium selection in the two-player version of the weakest-link game without communication (and other coordination games).

[17] If neither (*i*) nor (*ii*) hold, this implies that $q_i^K(K) = \frac{\beta}{\alpha} + \frac{c}{\alpha(K-1)}$. By adding a tie-breaking rule, we could make either E_1 or E_K the stochastically stable state in this, most likely exceedingly rare, situation.

also holds for the others. Another key to the result is that the condition in (ii) can only hold for $l > k$, and whenever it holds for some $l > k$, it also holds for K. Moreover, in any E_k, the expected payoff of sending $m_i^t = K$ is strictly higher than for any other message, except possibly the empty message.

The conditions in (i) and (ii) have the intuitive implications that a) the higher the costs of communication, b) the lower the α and the higher the β (i.e. the weaker the incentives to coordinate on higher ranked actions), and c) the smaller the difference between l and k, the harder it is to break out of an inefficient state using communication. In sum, although communication is not part of any stochastically stable state, the possibility of communication may help coordinate play on the most efficient action.

We have so far not made any assumptions on how $q_i^k(m_i^t)$ depends on the number of agents. It seems reasonable that more agents would make agents less likely to expect that their message would affect the minimum action. If we add an assumption that $q_i^k(m_i^t)$ is decreasing in the number of agents, the results and thresholds in proposition 2 still hold, but E_K would be less likely to be the stochastically stable state when the group is larger.

Comparing these results to the experimental results in Kriss et al. (2012), we can note that most subjects either send the empty message or $m_i = K = 7$ when they can choose whether to communicate or not. Both the decline of communication over periods seen in the experiment, and the dominance of messages indicating the highest ranked action when the subjects communicate, are in line with proposition 2. As mentioned in section 3.2, only 4 out of 14 groups in the costly communication treatments manage to achieve a higher ranked minimum action than 1 in the eighth and final round of the experiment. Of these, only one group is coordinated on the highest ranked action. The threshold in the proposition also indicates that it would be difficult; using the experimental parameters in Kriss et al. (2012) implies that in E_1 (which yields the lowest possible threshold for the condition in (ii)), $q_i^K(K) = q_i^7(7) > 0.51$ would be needed in the low cost treatment and $q_i^7(7) > 0.54$ in the high cost treatment. That is, for the highest ranked action to be the stochastically stable state, an agent must expect that there is a larger than a 50 percent probability that the group will switch to a minimum action equal to 7, should she send $m_i^t = 7$. However, as the proposition should be interpreted as the likely long-run state and Kriss et al.'s experiment runs for eight periods, we should perhaps not make too much of the quantitative comparison and settle for the results being a fairly good qualitative match.

Proposition 2 implies that just allowing agents to communicate may not be enough to induce coordination on efficient states. One of Kriss et al.'s conclusions states that "in some cases, communication may be effective only if its use by employees is mandatory" (p. 21). Our next result shows that making communication mandatory will help agents solve the coordination problem in our model as well. To create the routine *mandatory communication*, restrict the choice of messages to be $m_i^t \in A_i$ for all $i \in N$; that is, the empty message is not an option any more.[18] Choices of actions are made simultaneously as described by equations (3.5) and (3.6), and assumptions (1)-(5) still hold.

Proposition 3: If *mandatory communication* is in place and messages affect subjective probabilities, then the unique stochastically stable state in the weakest-link game with communication is $s_i = (K, K)\ \forall i \in N$.

The stated assumptions do not imply that agents must expect a message to have an effect on the subjective probabilities in the communication stage, but if it does, then proposition 3 implies that we are most likely to see agents coordinate on the highest ranked action. This is in line with the experimental results of Blume and Ortmann (2007). The intuition for the result is that once the empty message is no longer available, message costs are not important because of the assumption that they are equal, and the highest ranked action K is always one of the best reply messages. Thus, agents do not risk getting stuck on lower ranked actions and once the minimum action in period $t-1$ is K, $m_i^t = K$ is the unique best reply message for all agents.

Another way to coordinate agents is to impose restrictions on who gets to communicate. As tried experimentally in different ways by Weber et al. (2001) and Brandts and Cooper (2007), an agent may therefore be assigned to the role of communicator (interpreted as a manager or a team leader). The experiments in these two studies use more free-form communication, so we do not exactly match the set-up in their experiments but model the routine *team leader* as follows: let the team leader be agent 1 and let the

[18]It does not matter for this result whether the empty message still can be sent by mistake or not.

communication stage consist of agent 1 sending $m_1^t \in A_1$, while no other agent communicates. Agent 1 chooses a best reply message according to equations (3.2) and (3.4). Mistakes and experiments are still possible and equally probable in both stages of the game but only agent 1 can make them in the communication stage. As only agent 1 sends messages, assumption (1) is not in play any more, whereas we assume that agent 1's expectations in the communication stage follow assumptions (2)-(4). In the action stage, all agents choose actions simultaneously: agent 1 chooses $a_1^t = m_1^t$ and agents $i \in \{2, ..., n\}$ choose actions according to equations (3.5) and (3.6).

A team leader can have different levels of authority or credibility. We incorporate this notion by making an assumption about the probability that the other agents assign to the action indicated by agent 1's message subsequently becoming the minimum action. Let $p_{i1}^k \in [1, n-1]$ be the weight assigned to action k by agent i if $m_1^t = k$. Again, let p_{i1}^k not be influenced by the labels of messages, so that $m_1^t = k$ has the same influence on p_{i1}^k as $m_1^t = l$ has on p_{i1}^l for all $k, l \in A_i$. Furthermore, $\sum_{k=1}^{K} p_i^k = 1$ for all $i \in N \setminus \{1\}$. These assumptions imply that if $p_{i1}^k = n-1$, then $p_{ij}^k = 0$ for all $j > 1$ and $p_i^l = 0$ for all $l \neq k$. That is, when $p_{i1}^k = n-1$, the team leader has absolute authority and previous period's minimum action does not influence the expectations of the other agents. If $p_{i1}^k = 1$ agent i does not assign a higher probability to the team leader's message than to the actions of other agents, which can be interpreted as the team leader having no more authority or credibility than any other agent.

Proposition 4: Let the routine *team leader* be in place. If

(*i*) $q_1^K(K) > \frac{\beta}{\alpha}$ and $p_{i1}^K > (n-1)\beta/\alpha$ for all $i \in N \setminus \{1\}$, then the unique stochastically stable state is $s = ((K, K)_1, (\varnothing, K)_2, ..., (\varnothing, K)_n)$;

(*ii*) $q_1^K(K) < \frac{\beta}{\alpha}$ and/or $p_{i1}^K < (n-1)\beta/\alpha$ for some $i \in N \setminus \{1\}$, then the unique stochastically stable state is $s = ((1, 1)_1, (\varnothing, 1)_2, ..., (\varnothing, 1)_n)$.

For the routine to induce coordination on the highest ranked action, the team leader must both expect a message to result in the indicated action and have enough authority over the team members. Here, we also have the intuitive result that it is more difficult to lead a larger group to an efficient outcome.

3.4 Simulation

To examine the short-run properties of the model and to relax some of the assumptions, we use a version of the model as the basis for simulations. However, there is a trade-off in this latter regard, as the simulation requires more detailed assumptions about how agents choose to communicate and update their expectations. In the next section 3.4.1, we describe the version of the model used in the simulation. Section 3.4.2 contains the parameter configurations we use, and the results.

3.4.1 Model of communication for simulation

The model of communication described in section 3.3.1 assumes certain properties about the conditional expectations of agents, i.e. we assign probabilities to actions given messages and minimum actions in the form of $q_i^k(m_i^t)$, but the model is otherwise silent about how agents reason to reach these expectations. Here, we describe a process where agents reason about how other agents react to their messages, which we then use in the simulations.

Let $q_{ij} : M \times A \to [0,1]$ be a function that represents i's expectation over j's subjective probabilities. Let $h \in N \setminus \{i,j\}$ denote agents not i or j, and let $q_{ij}^k(m_i^t)$ be short for $q_{ij}\left(k|m_i^t, m_h^{t-1}, \min_{j\in N}\{a^{t-1}\}\right)$, while all other terms are defined as before. Thus, i uses i's own prospective message in period t, the empirical distribution of messages, and the minimum action in $t-1$ to form expectations of j's subjective probabilities in t. More specifically, let

$$\begin{aligned} q_{ij}^k(m_i^t) = \frac{1}{n-1}(\mathbf{1}\left(m_i^t = k\right) + \sum_{h\in N\setminus\{i,j\}} \mathbf{1}\left(m_h^{t-1} = k\right) \\ + |\varnothing| \times \mathbf{1}(\min_{j\in N}\{a^{t-1}\} = k)) \end{aligned} \tag{3.7}$$

be i's expectation over j's subjective probability of action k becoming the minimum in period t, conditional on message m_i^t. $\mathbf{1}(\cdot)$ are indicator functions. Unless agent i makes a change from communication to non-communication or the other way around, the term $|\varnothing|$ is just the number of empty messages sent by agents other than j in the last period. If i changes from a substantive to the empty message (or from the empty to a substantive message), $|\varnothing|$ decreases (increases) by one. That is, if

$$m_i^t = k \wedge m_i^{t-1} = \varnothing \Rightarrow |\varnothing| = \left(|\varnothing| \in m_{-j}^{t-1}\right) - 1 \tag{3.8}$$

$$m_i^t = \varnothing \wedge m_i^{t-1} = k \Rightarrow |\varnothing| = \left(|\varnothing| \in m_{-j}^{t-1}\right) + 1 \tag{3.9}$$

where $|\varnothing| \in m_{-j}^{t-1}$ denotes the number of empty messages received by agent j in period $t-1$. This formulation constrains $\sum_{k=1}^{K} q_{ij}^k(m_i^t) = 1$ for each $m_i \in M_i$ and all t, except for the initial period. We describe the initial expectations used in section 3.4.2. Thus, we can represent agent i's expectations in period t by the $(K+1) \times K$ matrix Q_{ij}^t (there are always $K+1$ possible messages, one for every action plus the empty message). Then, agent i can calculate each agent j's expected payoff for $k > 1$ as

$$\mathbb{E}\left(\pi_{ij}(k)|m_i^t\right) = \sum_{g=k}^{K} q_{ij}^g(m_i^t)k(\alpha - \beta) + \sum_{d=1}^{k-1} q_{ij}^d(m_i^t)\left(\alpha d - \beta k\right). \tag{3.10}$$

As before, each agent's payoff of $k = 1$ is always safe, and equal to $\alpha - \beta$. Agent i does not have to take into account any message costs for agent j, as these represent sunk costs in the action stage for j and are not considered when choosing a best reply action.

Now, using the the expected payoff $\mathbb{E}(\pi_{ij}(k)|m_i^t)$, agent i can evaluate the expected minimum action by checking each agent j's best reply to each of i's messages, and then choose the message that induces the highest ranked minimum action of the other agents. More formally, let

$$\Pi_j(m_i^t) = \{k \in A_i : \mathbb{E}\left(\pi_{ij}(k)|m_i^t\right) \geq \mathbb{E}\left(\pi_{ij}(l)|m_i^t\right) \forall\, l \in A_i\} \tag{3.11}$$

be the set of actions such that they are an expected best reply to message m_i^t for agent j (from the point of view of agent i). If $\mathbb{E}\left(\pi_{ij}(k)|m_i^t\right) = \mathbb{E}\left(\pi_{ij}(l)|m_i^t\right)$ for some k, l agents randomize uniformly among them to decide which is the expected action given a certain message m_i^t (so $\Pi_j(m_i^t)$ becomes a singleton). Let $\Pi_{-i}(m_i^t) = \Pi_1(m_i^t) \cup ... \cup \Pi_{i-1}(m_i^t) \cup \Pi_{i+1}(m_i^t) \cup ... \cup \Pi_n(m_i^t)$ be the union of all agents' $j \neq i$ expected best reply sets. There is thus one $\Pi_{-i}(m_i^t)$ for each $m_i \in M_i$ and $K+1$ in total for every agent i. Agent i then compares the payoffs of the lowest ranked action in each $\Pi_{-i}(m_i^t)$ – the minimum, denoted k_{min} – and then chooses the message corresponding to the set with the minimum yielding the highest payoff. We denote this collected set of minimum actions by Π_i^{min}. The best reply message is found in

$$BR_i^m = \{m_i \in M_i : \pi_i\left(k_{min}\right) \geq \pi_i\left(l_{min}\right) \; \forall \, k_{min}, l_{min} \in \Pi_i^{min}\} \tag{3.12}$$

where

$$\pi_i\left(k_{min}\right) = k_{min}\left(\alpha - \beta\right) - c\left(m_i^t\right). \tag{3.13}$$

If there is more than one message in this best reply correspondence, we assume that agents randomize uniformly between them. The implication of the above procedure is that the only probabilistic judgement is made when assessing the impact of a certain message on other agents' choice of best replies. In the action stage, the decision-making is exactly as described by equations (3.5) and (3.6).

Example: As an example, we use the following set-up: let the number of agents $= 3$, $A_i = \{1, 2, 3\}$, $\alpha = 1.5, \beta = 1$, and $c = 0.1$. Assume that the minimum action in period $t-1$ was 2, and that $m^{t-1} = \{\varnothing, 3, 2\}$. Using this we can calculate the q_{ij}^k's as in equation (3.7). For example, $q_{23}^2(m_2^t = 3)$, agent 2's expectation of agent 3's assessment of action $k = 2$ being the minimum action, conditional on agent 2 sending a message indicating $k = 3$, is calculated as

$$\begin{aligned} q_{23}^2(m_2^t = 3) &= \frac{1}{n-1}\left(\mathbf{1}\left(m_2^t = 2\right) + \sum_{h \in N} \mathbf{1}\left(m_h^{t-1} = 2\right) + \left(\varnothing| \times 1\right)\right) \\ &= \frac{1}{2}\left(0 + 0 + (1 \times 1)\right) = \frac{1}{2} \end{aligned}$$

The full calculations are shown in table 3.1.[19] In turn, this yields i's expectation of the expected payoffs for agent j given i's message, calculated according to equation (3.7) and collected in table 3.2 (remember that any message sent by j is seen as sunk costs by i and is thus disregarded). For example, $\pi_{12}(2|m_1^t = 2)$, agent 1's expectation of agent 2's expected payoff of choosing $k = 2$ conditional on agent 1 sending $m_1^t = 2$, is calculated as

$$\begin{aligned} \pi_{12}(2|m_1^t = 2) &= \sum_{g \geq 2} q_{12}^g 2(\alpha - \beta) + \sum_{d < 2} q_{12}^d\left(\alpha d - \beta 2\right) \\ &= (1/2 + 1/2)\, 2(1.5 - 1) + 0 = 1.00 \end{aligned}$$

[19] As the procedure implies that $q_i^k(m_i^t)$ is mostly either 0 or 1 and otherwise equal to one over the number of messages in the best reply correspondence, we refrain from listing them here.

Table 3.1: Expectations in the communication stage (Q_{ij}^t)

Agent 1	q_{12}^1	q_{12}^2	q_{12}^3	q_{13}^1	q_{13}^2	q_{13}^3
$m_1^t = \varnothing$	0	1/2	1/2	0	1	0
$m_1^t = 1$	1/2	0	1/2	1/2	1/2	0
$m_1^t = 2$	0	1/2	1/2	0	1	0
$m_1^t = 3$	0	0	1	0	1/2	1/2
Agent 2	q_{21}^1	q_{21}^2	q_{21}^3	q_{23}^1	q_{23}^2	q_{23}^3
$m_1^t = \varnothing$	0	1/2	1/2	0	1	0
$m_1^t = 1$	1/2	0	1/2	1/2	1/2	0
$m_1^t = 2$	0	1/2	1/2	0	1	0
$m_1^t = 3$	0	0	1	0	1/2	1/2
Agent 3	q_{31}^1	q_{31}^2	q_{31}^3	q_{31}^1	q_{31}^2	q_{31}^3
$m_3^t = \varnothing$	0	1	0	0	1	0
$m_3^t = 1$	1/2	1/2	0	1/2	1/2	0
$m_3^t = 2$	0	1	0	0	1	0
$m_3^t = 3$	0	1/2	1/2	0	1/2	1/2

Examining the payoffs in table 3.2 we can see for example that for agent 1, $\Pi_{-1}(m_1^t = \varnothing) = \{2, 2\}, \Pi_{-1}(m_1^t = 1) = \{1, 1\}, \Pi_{-1}(m_1^t = 2) = \{2, 2\}$, and $\Pi_{-1}(m_1^t = 3) = \{3, 2\}$. This results in $\Pi_1^{min} = \{2, 1, 2, 2\}$. Of these, choosing to send message $m_1^t = \varnothing$ will result in the highest expected payoff, as this induces the same minimum action (action 2) as sending messages $m_1^t = 2$ and $m_1^t = 3$ and there is no cost of the empty message. Performing the same calculation for agents 2 and 3 yields the same result and thus no agent communicates in period t (barring probabilistic mistakes and experiments which we disregard in this example).

In turn, this implies that all agents have to use the previous period's play to determine their best reply action. As the minimum action in $t-1$ was $k = 2$, using the procedure given by equations (3.5) and (3.6) results in $k = 2$ being the best reply for all agents. The best reply strategy is thus $s_i^t = (\varnothing, 2)$ for $i = 1, 2, 3$; given the parameters this is also a best reply to itself in the next and coming periods.

3.4.2 Simulation results

We start by comparing our results to Kriss et al. (2012), and then examine the model at a more general level. Each configuration of the parameters run for eight periods, as in the Kriss et al. (2012) experiments. When there are non-zero probabilities of mistake and experiments, we run each configuration 100 times. For the regression results below, we report averages

Table 3.2: Agent i's beliefs about agent j's expected payoffs in t

Agent 1	$\pi_{12}(k=1)$	$\pi_{12}(k=2)$	$\pi_{12}(k=3)$	$\pi_{13}(k=1)$	$\pi_{13}(k=2)$	$\pi_{13}(k=3)$
$m_1^t = \varnothing$	0.50	1.00	0.75	0.50	1.00	0.00
$m_1^t = 1$	0.50	0.25	0.00	0.50	0.25	-0.75
$m_1^t = 2$	0.50	1.00	0.75	0.50	1.00	0.00
$m_1^t = 3$	0.50	1.00	1.50	0.50	1.00	0.75
Agent 2	$\pi_{21}(k=1)$	$\pi_{21}(k=2)$	$\pi_{21}(k=3)$	$\pi_{23}(k=1)$	$\pi_{23}(k=2)$	$\pi_{23}(k=3)$
$m_1^t = \varnothing$	0.50	1.00	0.75	0.50	1.00	0.00
$m_1^t = 1$	0.50	0.25	0.00	0.50	0.25	-0.75
$m_1^t = 2$	0.50	1.00	0.75	0.50	1.00	0.00
$m_1^t = 3$	0.50	1.00	1.50	0.50	1.00	0.75
Agent 3	$\pi_{31}(k=1)$	$\pi_{31}(k=2)$	$\pi_{31}(k=3)$	$\pi_{32}(k=1)$	$\pi_{32}(k=2)$	$\pi_{32}(k=3)$
$m_3^t = \varnothing$	0.50	1.00	0.00	0.50	1.00	0.00
$m_3^t = 1$	0.50	0.25	-0.75	0.50	0.25	-0.75
$m_3^t = 2$	0.50	1.00	0.00	0.50	1.00	0.00
$m_3^t = 3$	0.50	1.00	0.75	0.50	1.00	0.75

of these 100 repetitions. The simulations without mistakes and experiments are not repeated.[20]

In the initial round, we use a uniform randomization to create a vector of non-empty messages that agents use to form expectations about which messages they think other agents will send in period 1. Agents then send best reply messages conditional on these expectations as described in section 3.4.1. In the action stage, agents best reply to sent messages as before, but as there is no minimum action in the previous round agents use only the messages to form their expectations and the empty message puts equal weight on all actions. So if all messages are empty in the initial round, which may happen, agents randomize uniformly over all available actions.

The following variables determine the configurations. *Number of agents* and *Number of actions*: both the number of agents and actions are varied between 2-10 in increments of two for the regressions. These are denoted for example as $agents_2$ if the configuration uses 2 agents. When we compare our results to Kriss et al. (2012) we use 7 actions and 9 agents as they do.

Message costs: the cost of sending messages is increased in increments of two, starting from 1 and up to 9. *Action mistake probabilities* and *Communication mistake probabilities*: we use three different levels of mis-

[20]Note that there is a chance component also when mistakes/experiment probabilities are zero, as agents resolve the choice between best reply messages/actions with equal expected payoff by randomizing uniformly. As these ties are rare and to keep the number of simulations at a manageable size, we choose not to repeat these runs.

take/experiment probabilities in both the communication and action stage: 0, 10, and 20 percent. In contrast to assumption 5, mistake and experiment probabilities does not have to be same in the simulation.

Action mistake type and *Communication mistake type*: These two are dummy variables indicating a different distribution of mistakes and experiments compared to the uniform distribution, which was assumed for the analytical results. The alternative distribution captures the idea that experiments and mistakes may be more likely to be close to the originally intended action. The probability of a mistake/experiment is thus the same as with a uniform distribution, but doubling the distance from the best reply message/action reduces the probability of being mistakenly chosen by half. Assume for example that there are 4 messages and that the best reply message is 2. Under uniform probability, each message has a 25 percent chance of being chosen when a mistake/experiment occurs. Under the "double-distance-half-likely" type, 0 would have the probability of 11.1 percent, 1 of 22.2 percent, 2 of 44.4 percent, 3 a chance of 22.2 percent, and 4 a 11.1 percent chance. The distribution works identically for actions (but action 0 does not exist of course). The variables *actmistaketype* and *commistaketype* equal 1 when this distribution is used in a configuration, and 0 when we use the uniform distribution.

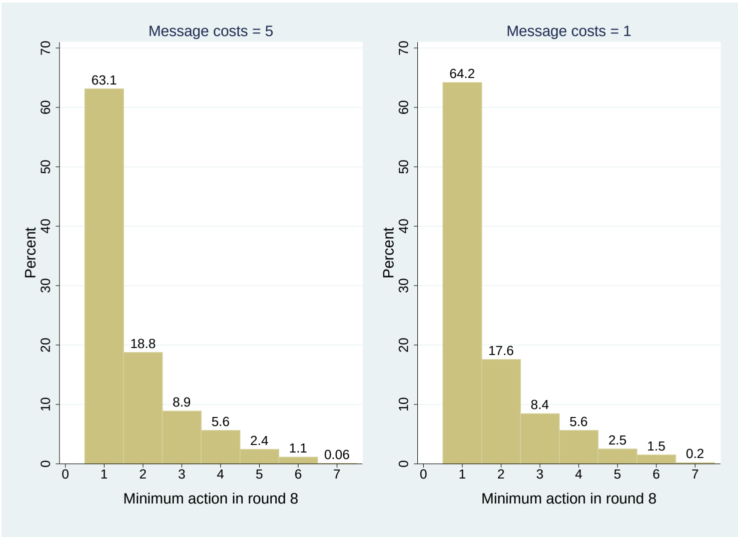
β/α-ratio: We keep α constant at 20, while β varies between 8-12 in increments of one, so $ratio_r \in \{0.4, 0.45, 0.5, 0.55, 0.60\}$, $r = 1, 2, 3, 4, 5$. The mid-point 0.5 is the most commonly used ratio in the experimental literature.

The two treatments in Kriss et al. (2012) use a ratio of $\beta/\alpha = 10/20$, 9 agents, and 7 actions and let message costs be equal to 1 or 5. In Figure 3.1 we show the full empirical distributions of minimum actions in round eight when message costs = 5 (left part) and message costs = 1, and there are mistake/experiment probabilities greater than zero in both the communication and action stages.[21] While it is evident that the distributions are wide-ranging (all seven actions are represented as the minimum action in both), it is also clear that the stochastically stable state (action 1) is massively overrepresented already in round 8 with both message costs. The overrepresentation is somewhat less pronounced if we only allow for a 10

[21] For each message cost, there are 1600 rounds, which include 16 different configurations (4 combinations of mistake/experiment distributions $\times$ 4 combinations of mistake/experiment probabilities) repeated 100 times each.

percent probability of mistakes and experiments, but the distributions are otherwise similar (results not shown). Furthermore, the results are similar regardless of message costs.

Figure 3.1: Distribution of minimum actions in round 8, Kriss et al. (2012) configuration



In Kriss et al.'s treatment with message costs = 5, all six groups have a minimum action of 1 in the eighth round. With message costs = 1, the distribution is the following: four groups play action 1, two groups play 3, and one group play action 5 and action 7, respectively.[22] Thus, action 1 is overrepresented as the minimum action also in these experiments.

With message costs = 5, the average action over the last four rounds (round 5-8) is 1.59 and the average minimum action is 1.00. In the low message cost treatment, these averages are 3.41 and 2.75. For both the average action and the average minimum action we are reasonably close with message costs = 5. Our estimates for the average action in round 8 range from 1.66 to 2.35, while the range for the average minimum action in round 8 is 1.23 to 2.28. We are further away when we use message costs = 1, the same estimates range from 1.56 to 2.37 (the average action) and from 1.17 to 2.29 (the average minimum action).

[22]Information about the average action in round 8 is not included in Kriss et al. (2012), so we cannot compare the distribution of the average action.

These results reflect a feature seen also below in the regressions: our estimates are not very sensitive to increases in the cost of messages. Kriss et al.'s subjects do on the other hand seem to react to the different costs. One reason may be that the salience of costs differ, e.g. message costs = 1 may be treated as negligibly small, while higher message costs may loom larger and enter into the calculations of subjects' expected payoffs. In relation to salience we have also, as mentioned, ruled out the highest ranked action as a focal point by assumption. Recall though that the total number of experimental groups in these two treatments are 14 in Kriss et al. (2012) (8 in the low message cost treatment and 6 in the high message cost treatment), so we are comparing averages for the minimum actions over very small samples.

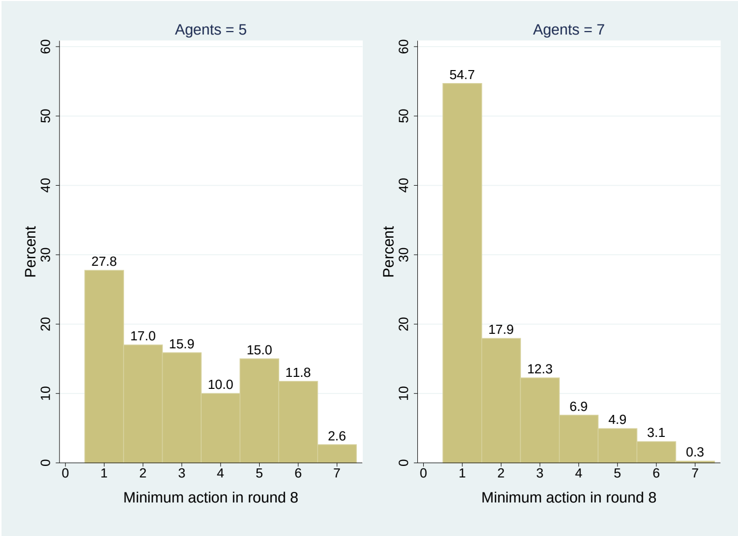
In Figure 3.2, we look more closely into the question of group size, which may be interesting for the design of future experiments. The figure shows the results when we run a similar configuration to the one used by Kriss et al. (2012) in their high message cost treatment, but change the number of agents to 5 (left part) and 7 (right part). While the stochastically stable state in both these configurations is the same (action 1), there is a clearly visible short-term difference. While action 1 is the most common minimum action with groups of 5 agents, all other actions except 7 have substantial representation as well. With groups of 7, we instead get a more similar distribution to groups of 9.

To be able to separate the effects from different variables and to report the general results in a succinct way, we run OLS regressions with the *average action*, the *average minimum action*, and the *percent coordinated* games in round eight as dependent variables. A game counts as coordinated if all agents *intend* to play the same action. Games where some agent chooses a different action than the group by mistake or experiment in the last round thus still counts as coordinated.

As independent variables, we include dummy variables for each increment of the variables used to determine the configurations, using the category with the lowest value as reference category throughout. Using the ranges described above yields 22,500 configurations (and a total number of runs well over 2 million).

The results of the OLS regressions are shown in table 3.3. Columns (1)-(2) use the average action in round 8 as the dependent variable and columns (3)-(4) use the average minimum action in round 8. Columns (5)-(6) use

Figure 3.2: Distribution of minimum actions in round 8, agents = 5 and 7



the percent of coordinated games in round 8. Columns (1), (3), and (5) contains all configurations regardless of whether mistakes and experiments are possible, while columns (2), (4), and (6) contain specifications where there are non-zero probabilities of mistakes and experiments in both stages.[23] In these specifications, we use the categories where mistake and experiment probabilities are 10 percent as reference categories.

We start by discussing the results for the average and minimum actions, which are similar for most variables over the two types of specifications. We expect that increasing the *number of agents* should make it more difficult to use communication to break out of inefficient states, and to increase the probability that mistakes or experiments are made. Both effects should therefore imply lower average actions and average minimum actions. This is clearly reflected in the estimates as well, which gets progressively more negative as we increase the number of agents. The largest change is the jump from 2 to 4 agents.

The results for the number of actions are perhaps less interesting as when more actions are available, the average and minimum actions increase more or less mechanically. As the stochastically stable state often is the

[23] Using an intermediate specification where mistakes and experiments are possible in at least one stage does not change the results much.

Table 3.3: *Average action*, *Average minimum action* and *Percent coordinated*

VARIABLES	(1) Avg	(2) Avg	(3) Min	(4) Min	(5) Pct	(6) Pct
$agents_4$	-2.604***	-2.752***	-2.863***	-3.056***	-0.0283***	0.00812***
	(0.0260)	(0.0364)	(0.0284)	(0.0395)	(0.00229)	(0.00236)
$agents_6$	-2.752***	-3.006***	-2.900***	-3.183***	0.0452***	0.0856***
	(0.0260)	(0.0372)	(0.0287)	(0.0405)	(0.00180)	(0.00201)
$agents_8$	-2.963***	-3.330***	-3.099***	-3.496***	0.0709***	0.120***
	(0.0269)	(0.0375)	(0.0295)	(0.0405)	(0.00163)	(0.00183)
$agents_{10}$	-3.211***	-3.567***	-3.344***	-3.734***	0.0799***	0.132***
	(0.0271)	(0.0382)	(0.0297)	(0.0410)	(0.00156)	(0.00179)
$actions_4$	1.055***	0.831***	0.983***	0.738***	-0.0375***	-0.0437***
	(0.0210)	(0.0304)	(0.0224)	(0.0325)	(0.00118)	(0.00147)
$actions_6$	2.057***	1.659***	1.912***	1.472***	-0.0643***	-0.0774***
	(0.0214)	(0.0294)	(0.0229)	(0.0315)	(0.00140)	(0.00163)
$actions_8$	3.123***	2.577***	2.897***	2.292***	-0.0879***	-0.103***
	(0.0249)	(0.0330)	(0.0267)	(0.0353)	(0.00179)	(0.00191)
$actions_{10}$	4.231***	3.563***	3.919***	3.174***	-0.103***	-0.123***
	(0.0303)	(0.0398)	(0.0325)	(0.0426)	(0.00189)	(0.00227)
$msgcosts_3$	0.00291	0.000355	0.00593	0.00483	0.000856	0.000560
	(0.0234)	(0.0282)	(0.0251)	(0.0302)	(0.00184)	(0.00201)
$msgcosts_5$	0.00319	0.00391	0.00285	0.00412	-7.78e-05	0.000180
	(0.0234)	(0.0281)	(0.0251)	(0.0301)	(0.00187)	(0.00202)
$msgcosts_7$	-0.0102	0.00197	-0.00930	0.00712	0.000371	0.00114
	(0.0234)	(0.0281)	(0.0250)	(0.0301)	(0.00187)	(0.00201)
$msgcosts_9$	-0.0780***	-0.0431	-0.0729***	-0.0385	0.00444**	0.00528***
	(0.0235)	(0.0284)	(0.0251)	(0.0303)	(0.00183)	(0.00198)
$actionmistake_1$	-0.992***		-1.097***		-0.0217***	
	(0.0191)		(0.0199)		(0.00150)	
$actionmistake_2$	-1.191***	-0.193***	-1.411***	-0.300***	-0.0379***	-0.0154***
	(0.0193)	(0.0179)	(0.0208)	(0.0191)	(0.00155)	(0.00125)
$commistake_1$	-0.0752***		-0.0773***		-0.00651***	
	(0.0194)		(0.0209)		(0.00153)	
$commistake_2$	-0.130***	-0.0154	-0.168***	-0.0464**	-0.0378***	-0.0317***
	(0.0192)	(0.0179)	(0.0206)	(0.0191)	(0.00163)	(0.00125)
$actmistaketype$	0.152***	0.193***	0.271***	0.359***	0.0113***	0.0179***
	(0.0148)	(0.0179)	(0.0159)	(0.0191)	(0.00116)	(0.00125)
$commistaketype$	-0.0905***	-0.0999***	-0.101***	-0.129***	0.00969***	0.0127***
	(0.0148)	(0.0179)	(0.0159)	(0.0191)	(0.00116)	(0.00125)
$ratio_2$	-0.423***	-0.356***	-0.470***	-0.405***	-0.00808***	0.00234
	(0.0249)	(0.0292)	(0.0265)	(0.0311)	(0.00138)	(0.00186)
$ratio_3$	0.00590	-0.0548**	-0.104***	-0.149***	-0.0518***	-0.0341***
	(0.0233)	(0.0278)	(0.0253)	(0.0301)	(0.00199)	(0.00220)
$ratio_4$	-0.234***	-0.214***	-0.322***	-0.292***	-0.0348***	-0.0184***
	(0.0235)	(0.0281)	(0.0253)	(0.0303)	(0.00176)	(0.00191)
$ratio_5$	-0.543***	-0.427***	-0.582***	-0.468***	-0.00296*	0.0117***
	(0.0250)	(0.0302)	(0.0263)	(0.0319)	(0.00154)	(0.00197)
Observations	22,500	10,000	22,500	10,000	22,500	10,000
R^2	0.761	0.806	0.732	0.779	0.348	0.591

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

lowest ranked, the increase is not one to one though.

Message costs are not a large influence on either the average or minimum action in round 8. Except for the highest cost category in the specification with all configurations included, the estimates are very close to zero (and the magnitudes are also small for message costs = 9). This can actually be seen also in the conditions of proposition 2: while the stochastically stable state depends to some degree on message costs, the ratio and the number of actions are more potent influences. Beyond the introduction of costly communication, message costs are therefore not a huge influence on the outcome in either version of our model.

Mistakes and experiments have a predictable negative effect on the average and minimum action. The coefficients on mistakes/experiments in the action stage are an order of magnitude larger than the coefficients in the communication stage though. This is actually in line with how we prove the propositions, in all proofs except for proposition 3 (when communication is mandatory) it turns out that it is mistakes/experiments in the action stage that determine the stochastically stable state when messages cannot be used to break out of inefficient absorbing states. Almost all of the effect comes from going from no mistakes/experiments to having at least some, as the coefficients on the variables are of very similar magnitude for both *action-mistake* and *commistake*. The type of distribution used for mistakes affect the results differently depending on whether we are in the communication or action stage. The coefficients are small though, which indicates that the new distribution does not yield drastically different results compared to the uniform distribution.

Lastly, the coefficients for *ratios* are non-monotonic in a way that may seem unintuitive. A higher ratio represents weaker incentives to play a higher ranked action, so one may expect to see progressively more negative coefficients going from $ratio_2$ to $ratio_5$ ($ratio_1$ is the reference category). However, both $ratio_3$ and $ratio_4$ are always less negative than $ratio_2$, and $ratio_3$ is even positive in column (1) (although not significantly different from zero). Note also that the standard errors are larger for $ratio_2$. We see two explanations, not mutually exclusive, for the non-monotonicity of the coefficients:

First, the stochastically stable state – the state that we expect to see most often in the long run – is the same for a large share of the configurations, regardless of the ratio. So we should see convergence over time.

In Figures 3.1 - 3.2 above the stochastically stable state, which is action 1 for the depicted configurations, is clearly already overrepresented after 8 periods. The convergence effect is therefore evident also in the short run for many configurations.

The *second* explanation is more subtle and depends on the messages that are chosen in the first round. In the initial round, we randomize a vector of messages that agents use to form expectations about other agents' messages. In configurations that use $ratio_1$ and $ratio_2$ the resulting expected payoffs often yield the empty message as a best reply. That is, agents expect the empty message to yield a high enough action from the others in response, high enough for them to think that it is not worth it to incur the cost of sending a message. However, if many or all agents send the empty message this strategy may backfire. If agents do not receive any message in the first round, our procedure specifies that agents place equal probability on all actions and thus have to randomize between them. As the payoff structure of the game is such that minimum action determines payoffs, this frequently leads to a low ranked action being the minimum in the first round. Play in the first period has a persistent influence for many configurations, which yields low ranked actions in period 8 as well. This randomization/uncertainty also explains the higher standard errors. For the higher ratios agents more often find it worth sending a message in the first round.

Together, the two different influences explain why we get the somewhat non-intuitive results. While the last effect mentioned is a direct consequence of how we choose to specify agents' choices in the initial round, we do not think that it is implausible that agents may reason in this way (nor do we think that it is the only way agents may reason of course). A behavioral interpretation of the results could be that with a higher ratio, agents are relatively certain that others will indicate the highest ranked action with a message, and therefore abstain from doing so themselves (i.e. taking the chance to free ride on other agents' messages).[24] However, if many or all agents think in this way, few will actually send a message. Therefore, when faced with an unexpected situation in the action stage, e.g. no one indicates

[24]In Kriss et al. (2012), the modal message in the costly communication treatments is the empty message; 45.8 and 53.7 percent of the subjects send this message in low and high cost treatment respectively, so it is not uncommon that agents choose not to communicate in the first round.

any action, placing a similar probability on all actions may be a reasonable thing to do.

For the results when *percent coordinated* is the dependent variable, note first that the share of configurations that are coordinated is high in general, and only slightly lower when there are positive probabilities of mistakes and experiments in both the communication and action stage (93.3 percent and 91.4 percent of the configurations are coordinated, respectively). As with *average action* and *average minimum action*, the differences between the two specifications are in general small, so we comment on both unless specifically mentioned.

The coefficients on actions become monotonically more negative as there are more available actions, which seems intuitive. Message costs on the other hand have again little influence (with the highest level as a partial exception, but the magnitude of the coefficient is small), but as these costs have little influence on the average and minimum actions chosen it seems reasonable that they should not affect coordination much either. Mistake and experiment probabilities have a predictably negative effect on coordination, while we see more coordination when we do not use the uniform distribution. This is also intuitive, as mistakes and experiments then tend to end up closer to the previous best reply, they are less likely to entail a change of action for the rest of the group.

That $ratio_3$ and $ratio_4$ are negative for coordination is in line with the results for *average action* and *average minimum action*. As there are more messages of different types in these configurations, it should also take more periods to coordinate. That more agents should make it *more* likely that play is coordinated, as indicated by the coefficients on the agent-dummies, may seem unintuitive. However, this reflects that it becomes harder to affect the minimum action with messages when the group is large. Thus, the empty message should be more common, which leads to faster coordination.

3.5 Concluding remarks

This paper develops a model to examine how communication affects organizational coordination when actions are highly interdependent among agents. In line with the experimental results reviewed in section 3.2, the results imply that efficient coordination may be difficult to achieve when communication is costly. Even if communication costs are small compared to the potential gains of efficient coordination, the costs introduce a trade-off for agents between lowering the strategic uncertainty for the group and the costs of communication. Costly communication also introduces incentives to free ride on other agents' communication. These effects of communication costs may explain the contrasting results in experiments with costly and costless communication.

Such results furthermore suggests two reasons for the existence of organizations: first, they may lower communication costs. Second, organizations may have the authority to implement formal rules and routines that structure communication in a way that can be difficult for more informal and non-hierarchical groups of agents. Rules and routines may under certain conditions be necessary for efficient coordination. We examine two such routines, *mandatory communication*, and the assignment of a *team leader*. Mandatory communication implies that sending and choosing the payoff dominant action is the unique stochastically stable state. A team leader may also induce efficient coordination but only when he or she has enough authority or credibility, and expects to be able to persuade the group to choose the communicated action.

We also use a version of the model in simulations to examine its short run properties. The stochastically stable states often have considerable explanatory power also in the short run, as these states are overrepresented in the empirical distribution of minimum actions in round 8 (especially when groups are large). In this respect, the model produces results in line with earlier experiments. The difficulties experienced by experimental subjects to coordinate on efficient states when communication is costly is clearly present also in the short run in our model. However, the agents in our model seem to be less sensitive to message costs than real world subjects. We furthermore provide results that indicate that larger groups will find it harder to use communication to coordinate on efficient actions, but due to this may actually coordinate faster, and that the effect of changing the strength of incentives does not have to be monotonic.

We think that the modelling of costly communication is one step towards richer game-theoretical models of organizational coordination, models that allow for more general ways of communication and are informative about how communication and routines can be mixed to achieve efficient coordination. Interesting future developments in this direction would be to let agents communicate sequentially, to generalize the number of periods agents remember, and also to apply the model to other games that resemble other situations where coordination is important, for example the median action game.

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3.A Proofs of propositions

We start in the next section by defining the concept of stochastic stability and how stochastically stable states can be computed, as well as some properties of unperturbed and perturbed Markov processes that we use in the proofs. A fuller description of these concepts can be found in for example

Young (1998) (especially chapter 3, which we follow closely below). The proofs of propositions 1 – 4 follow in sections 3.A.2 – 3.A.5.

3.A.1 Stochastic stability

A discrete-time Markov process on a finite state space X specifies the probability that the process changes from state x to state y from one period to the next for each state $x, y \in X$ (Young, 1998). In our model, the largest state space we use is $S = M \times A$, which is clearly finite. The transition probability of moving from state $s = (m^{t+1}, a^{t+1})$ in period $t+1$ conditional on being in $s' = (m^t, a^t)$ in t is determined by the frequencies of messages and the minimum action in t, as well as the probabilities of mistakes and experiments. As long as the mistake/experiment probabilities are non-zero but small, they imply that the process can be regarded as a perturbed Markov process, in the sense that the transition probabilities are slightly distorted versions of some original process, called P^0. Young (1993, 1998) calls such processes *regular perturbed Markov processes*, denote them P^ε, and define them to have certain characteristics, which we describe below.

Definition: P^ε is aperiodic and irreducible for all $\varepsilon \in (0, \varepsilon^*]$, where $\varepsilon^* > 0$.

Aperiodic means that the process can return to a state x at irregular times. A process is irreducible if there is a positive probability of moving from any state to any other state in a finite number of periods. Because mistakes and experiments are possible in every period in our setting, any state can be reached with positive probability from any other state.

As P^ε is irreducible for every $\varepsilon > 0$, it has a unique stationary distribution μ^ε (Young, 1993). Again following Young (1993, 1998), a state x is *stochastically stable* if

$$\lim_{\varepsilon \to 0} \mu^\varepsilon(x) > 0, \tag{3.14}$$

i.e. any state that the limiting distribution puts positive probability on is a stochastically stable state. The limit $\lim_{\varepsilon \to 0} \mu^\varepsilon(x) = \mu^0(x)$ exists for every x, and the limiting distribution μ^0 is a stationary distribution of P^0. It follows in particular that every regular perturbed Markov process has at least one stochastically stable state. To describe a way find this state or states, we need to define some other concepts as well.

Definition: A *recurrent class* of P^0 is a collection of states such that no state outside the class is accessible from any state inside it, i.e. the probability of leaving a recurrent class is zero. A state is called *absorbing* if it constitutes a singleton recurrent class.

We denote the set of recurrent classes/absorbing states of the unperturbed process $E = \{E_1, E_2, ..., E_K\}$. An irreducible process, like the perturbed one, have only one recurrent class, which consists of the whole state space. There is in general several different ways of reaching every E_k.

Definition: A *kl-path* is a sequence of states $\zeta = (E_k = z_1, z_2, ..., z_q = E_l)$ that start in E_k and end in E_l.

Next, we introduce a concept for how "difficult" it is for the process to move from a certain state to another:

Definition: The *resistance* of a one-period transition between two states z_i, z_j in a perturbed process, denoted $r(z_i, z_j)$, is the minimum number of mistakes or experiments required to make the transition, i.e. $r(z_i, z_j)$ is a positive integer, or zero if no mistakes or experiments are needed. The resistance of a kl-path is the sum of the resistances on the path, i.e. $r(\zeta) = r(z_1, z_2) + r(z_2, z_3) + ... + r(z_{q-1}, z_q)$.

As it is impossible to leave an recurrent class or an absorbing state without mistakes/experiments, the resistance of a transition from a recurrent class E_k to another E_l is always positive. There can in general be many kl-paths, but to find the stochastically stable we are going to be interested in the ones with the least resistance.

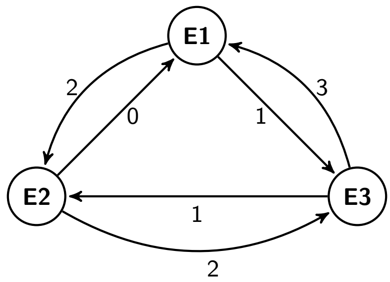
Definition: $r_{kl} = \min r(\zeta)$ is the *minimum total resistance* needed to transition from E_k to E_l for all possible kl-paths ζ.

Note that r_{kl} need not be equal to r_{lk}. Young (1998, p. 55-56) describes how the stochastically stable states can be computed in a simple way: first, construct a complete directed graph with K nodes, one for each recurrent class. The directed edge $k \to l$ from E_k to E_l is called kl and the weight on the edge is equal to r_{kl}. A rooted tree T is a set of $K-1$ directed edges such that from every node different from E_k, there is a unique directed path in the tree to E_k. The total resistance of T is the sum of the minimum resistances r_{lk} on the $K-1$ edges that compose it.

Definition: The *stochastic potential* $\gamma(E_k)$ of the recurrent class E_k is defined as the minimum resistance over all trees rooted at k. That is, denote the set of all trees rooted at E_k with $T(k)$, then the stochastic potential is

$$\gamma(E_k) = \min_{T \in T(k)} \sum_{l,l' \in T} r_{ll'}. \tag{3.15}$$

Figure 3.A.1: Pairwise resistances between recurrent classes



Stochastically stable states are the states that have the minimum stochastic potential, i.e. $\min_{E_k \in E} \gamma(E_k)$ (Young, 1993, Theorem 2).

Example: Consider the complete graph in Figure 3.A.1, where the three recurrent classes E_1, E_2, and E_3 are represented by the three nodes and the resistances between these classes are shown by the adjoining numbers to the edges.

This example has nine rooted trees, three for each node. For example, the three trees rooted at E_1 have the following directed edges: $(23, 31)$; $(21, 31)$; $(32; 21)$. The stochastic potentials – the summed resistances on the tree with the minimum resistance for each E_k – are :

$$\gamma(E_1) = r_{32} + r_{21} = 1 + 0 = 1$$
$$\gamma(E_2) = r_{13} + r_{32} = 1 + 1 = 2$$
$$\gamma(E_3) = r_{21} + r_{13} = 0 + 1 = 1$$

Consequently, $\gamma(E_1)$ and $\gamma(E_3)$ have the same minimum stochastic potential and are therefore the stochastically stable states.

We also use some additional results and ideas in the proofs. The only recurrent classes, or rather the only absorbing states, of the unperturbed Markov process in the weakest-link game without communication are the strict Nash equilibria (see e.g. Proposition A.6 in Riedl et al. (2011) for weakest-link games. See also Young (1998, p. 106-109)). These states are therefore our only candidates for the stochastically stable states according to the procedure described above. As it turns out, the corresponding states, i.e. $s = ((\varnothing, k)_1, ..., (\varnothing, k)_n)$, are also the only candidates in the weakest-link game with communication, but we show this in the proof of proposition 2.

All proofs use the idea from Riedl et al. (2011) that if a state can be reached with a number of uncoordinated mistakes, it can also be reached with the same number of "coordinated" mistakes. That is, if we for example assume that all mistakes are made in the action stage, one of the least resistance kl-paths between any E_k and E_l is always one where all mistakes/experiments are of action l (the path need not be unique). This is so since all combinations of the same number of mistakes/experiments have same probability, as the distribution of mistakes/experiments is uniform. Also, for all E_k except E_1 and E_K, it is possible to move to both higher and lower ranked absorbing states. Thus, if it always requires less mistakes/experiments to move to a lower ranked state, then $\gamma(E_1) < \gamma(E_k)$ for all $E_k \in E$. If it always requires more mistakes/experiments to move to a lower ranked state, then E_K has the minimum stochastic potential, i.e $\gamma(E_K) < \gamma(E_k)$ for all $E_k \in E$.

3.A.2 Proof of proposition 1

Assume $\min_{j\in N}\{a^{t-1}\} = k$. After play in t, the following must hold for some $g > k$ to be a best response in $t+1$,

$$p_i^g\left(g(\alpha-\beta)\right) + \left(1-p_i^g\right)\left(\alpha k - \beta g)\right) \geq k(\alpha-\beta) \Rightarrow p_i^g \geq \frac{\beta}{\alpha} \tag{3.16}$$

This holds as we only need to consider coordinated mistakes/experiments, which imply that the frequencies of all other actions not g or k are zero. Therefore, agent i consider k a certain payoff, as k is lower ranked than g. As $\alpha > \beta > 0$, $\beta/\alpha \in (0,1)$. By equation (3.5) and the assumption of no communication, if $\min_{j\in N}\{a_j^t\} = g$, then $p_i^g = 1$ and otherwise 0. That is, $a_i^t \geq g \,\forall\, i \in N$ must hold for g to be a best reply in $t+1$. As this can only happen by mistake/experiment in t, it takes at least n mistakes/experiment to move to a higher ranked equilibrium.

For a lower ranked action d to be a best response in $t+1$, the following condition must hold

$$d(\alpha-\beta) \geq \left(1-p_i^d\right)\left(k(\alpha-\beta)\right) + p_i^d\left(\alpha d - \beta k)\right) \Rightarrow p_i^d \geq 1-\frac{\beta}{\alpha} \tag{3.17}$$

because, again, the payoff when moving unilaterally to a lower ranked equilibria is deemed certain when we compare equilibria pairwise and consider "coordinated" mistakes/experiments. Similarly, $p_i^d = 1$ in $t+1$ if $\min_{j\in N}\{a_j^t\} = d$. This requires only 1 mistake/experiment for any $d < k$. Thus, $\gamma(E_1) < \gamma(E_k)\ \forall E_k \in E$ such that $k \neq 1$ and E_1 is the unique stochastically stable state.■

3.A.3 Proof of proposition 2

We start by stating a lemma, which shows that the expected payoff for all actions of sending $m_i^t = K$ is always weakly greater than all other messages except possibly the empty message and that sending a message that is lower ranked than the previous period's minimum is never a best reply. The lemma is subsequently used also in the proofs of proposition 3 and 4.

Lemma 1: For all $i \in N$ and all t

(*i*) $\mathbb{E}\left(\pi_i(k)|m_i^t = K, h^{t-1}\right) \geq \mathbb{E}\left(\pi_i(k)|m_i^t = l, h^{t-1}\right)$ for all $k, l \in A_i$;

(*ii*) for all $d < \min_{j\in N}\{a_j^{t-1}\}, m_i^t = d \notin BR_i^m$.

Proof: For (*i*): First, $\sum_{g=k}^K q_i^g(K) \geq \sum_{g=k}^K q_i^g(l)$ for all $k, l \in A_i$ as the number of messages indicating actions ranked higher than or equal to k is at least as many in the first term and messages affect probabilities only by their frequencies according to assumption 3. In turn, $\sum_{d=1}^{k-1} q_i^d(K) \leq \sum_{d=1}^{k-1} q_i^d(l)$ since $\sum_{k=1}^K q_i^k(k) = 1$. From equation (3.2), we can see that $m_i^t = K$ thus always implies at least as much weight on $k(\alpha-\beta)$, and as $k(\alpha-\beta) > \alpha d - \beta k$ for all $k, d \in A_i$ such that $k > d$, $\mathbb{E}\left(\pi_i(k)|m_i^t = K, h^{t-1}\right) \geq \mathbb{E}\left(\pi_i(k)|m_i^t = l, h^{t-1}\right)\ \forall\, k, l \in A_i$ in all periods, which proves part (*ii*).

To prove (*ii*), assume $\min_{j\in N}\{a_j^{t-1}\} = k$. For $m_i^t = d \in BR_i^m$, by equation (3.4), $\mathbb{E}(\pi_i|m_i^t = d, h^{t-1}) \geq \mathbb{E}(\pi_i|m_i^t = K, h^{t-1})$. Part (*i*) implies that this can hold with at best equality. If messages are able to affect subjective probabilities in t, then by assumption 3: $\mathbb{E}\left(\pi_i(l)|m_i^t = K, h^{t-1}\right) = \mathbb{E}\left(\pi_i(l)|m_i^t = l, h^{t-1}\right)$ for all $l \leq d$ and

$$\mathbb{E}\left(\pi_i(g)|m_i^t = K, h^{t-1}\right) \geq \mathbb{E}\left(\pi_i(g)|m_i^t = l, h^{t-1}\right)$$

for all $g > d$ as the frequency of higher ranked messages is equal for d and lower ranked actions, while higher for all $g > d$ so that $\sum_{g>d}^K q_i^g(K) \geq \sum_{g>d}^K q_i^g(d)$. Furthermore, this implies that $\sum_{g>d}^K q_i^g(K) = \sum_{g>d}^K q_i^g(d)$ only if $m_i^t = K$ does not increase q_i^K and therefore does not increase $\sum_{g>d}^K q_i^g$

(which $m_i^t = d$ never does by assumption 4). But then as q_i^k is non-decreasing in $m_i^t = \varnothing$ by assumption 4, $\sum_{g>d}^{K} q_i^g(\varnothing) \geq \sum_{g>d}^{K} q_i^g(d)$ for all $g > d$. Therefore, as $c(\varnothing) = 0$ and $c(d) > 0$, $\mathbb{E}(\pi_i | m_i^t = \varnothing, h^{t-1}) > \mathbb{E}(\pi_i | m_i^t = d, h^{t-1})$ and the empty message is the best reply if $\mathbb{E}(\pi_i | m_i^t = d, h^{t-1}) = \mathbb{E}(\pi_i | m_i^t = K, h^{t-1})$, as well as if messages do not affect subjective probabilities at all.$\blacksquare$

The rest of the proof consists of the following segments: we first show that there are no best reply cycles and that all absorbing states must be coordinated in both stages of the game. Second, we show that strategy profiles such that $s_i = (k, k)\ \forall\ i \in N$ cannot be absorbing states (and therefore not stochastically stable states). This implies that all candidates for stochastically stable states are such that $s_i = (\varnothing, k)\ \forall\, i \in N$. Third, we derive a condition for when communication can be used to move from one absorbing state to another. Lastly, we show which the stochastically stable states are when this condition hold and when it does not.

Assume that period $t-1$ yielded a minimum action of k. Lemma 1 implies that no agent sends $m_i^t = d < k$. Then, as $\sum_{g=l}^{K} p_i^g = 1$ for all $l \leq k$ and all $i \in N$ according to equation (3.5), and $k\,(\alpha - \beta) > d\,(\alpha - \beta)$ for all $d < k$, playing a lower ranked action cannot be a best reply for any agent in t. This implies that we cannot go back to lower ranked actions being minimum actions in the unperturbed process. Either enough agents send $m_i^t = h \geq k$ so that all agents' best replies are of higher rank than k, or k is the minimum action also in t and onwards. But as there is a finite number of actions $h \geq k$, the process must stop in some period $t' > t$ at some h such that $k \leq h \leq K$.

Once there are not enough best reply messages to change the minimum action to $g > h$, we can use assumption 4 to see that agents cannot forever send messages of other actions $g > h$. If $m_i^t = g$ and $\min_{j \in N}\{a_j^t\} \neq g$, then $(q_i^g(g))^{t+1} \leq (q_i^g(g))^t\ \forall\, i \in N$ and strictly smaller when $(q_i^g(g))^t > 0$. At some point $g > h$ is not a best reply message for any agent and agents must switch to sending h or $\varnothing$.

Furthermore, assume that $h^{t-1} = ((k,k)_1, ..., (k,k)_n)$. Then, by assumption 1, in t all agents expect the same distribution of other agents' messages. As $q_i^l(\varnothing) = 0\ \forall\, l \neq k$ due to assumption 2, this and assumption 4 implies $q_i^k(k) = q_i^k(\varnothing) = 1$, and as $c(k) > c(\varnothing)$, $m_i^t = \varnothing$ must be a best reply for all $i \in N$. By equation (3.5), indicating action k or no communication has the same effect on others choice of action in this case. Then, as the minimum action is of rank k, $a_i^t = k$ is a best reply action for all $i \in N$. Thus, communication is never a part of a strategy that is a best reply to itself and therefore not a part of an absorbing state of the unperturbed Markov process.

That leaves $E_k \in E$ as candidates for the stochastically stable states. Assume we are in E_k in $t-1$. In t all agents use exactly the same information as no one communicated in $t-1$, it therefore suffices to check for one agent when communication can be used to move from E_k. By assumption 1, no agent expects any other to send a message, so by assumption 2 $q_i^k(\varnothing) = 1$ and $q_i^l(\varnothing) = 0 \, \forall \, l \neq k$. By the same assumptions, if i sends $m_i^t = l > k$, then $\sum_{g=l}^{K} q_i^g(l) = q_i^l(l)$ and $\sum_{d=1}^{l-1} q_i^d(l) = q_i^k(l) = 1 - q_i^l(l)$. Note that, as above, in this situation there can be no change to any action $l \neq k$ without communication, as $p_i^k = 1$ for all i if no agent communicates, and lower ranked messages cannot be best replies. Thus, whenever

$$
\begin{aligned}
& k(\alpha-\beta) > q_i^l(l)l(\alpha-\beta) + \left(1-q_i^l(l)\right)(\alpha k - \beta l) - c \Rightarrow \\
& \frac{\beta}{\alpha} + \frac{c}{\alpha\,(l-k)} > q_i^l(l)
\end{aligned}
\tag{3.18}
$$

hold for all $k, l \in A_i$ there is no better reply to $s_i = (\varnothing, k)$ than itself for any i.

To separate between the candidates for stochastically stable states whenever equation (3.18) hold, we need to check how many mistakes/experiments that are needed to move from one absorbing state to another. As in the proof of proposition 1, we need $p_i^g \geq \beta/\alpha$ for a move to a higher ranked action g, and $p_i^d \geq 1 - \beta/\alpha$ for a move to a lower ranked action d. Assume first that all mistakes/experiments are made in the communication stage, then given the definitions of p_i^g and p_i^d we can rewrite these conditions as

$$
p_i^g = \frac{1}{n-1} \sum_{j \in N \setminus i} p_{ij}^g \Rightarrow \sum_{j \in N \setminus i} p_{ij}^g \geq (n-1)\left(\frac{\beta}{\alpha}\right) \tag{3.19}
$$

$$
p_i^d = \frac{1}{n-1} \sum_{j \in N \setminus i} p_{ij}^d \Rightarrow \sum_{j \in N \setminus i} p_{ij}^d \geq (n-1)\left(1-\frac{\beta}{\alpha}\right) \tag{3.20}
$$

where p_{ij}^g and p_{ij}^d is 1 if j makes a mistake/experiment and otherwise 0. Also, recall that for a higher ranked action to become the minimum action, $p_i^g \geq \beta/\alpha$ for all $i \in N$, whereas $p_i^d \geq 1 - \beta/\alpha$ only needs to hold for one agent in the case of a lower ranked action. This implies that at least two mistakes/experiments are needed to ensure a move to a higher ranked absorbing state, even if $(n-1)\left(\frac{\beta}{\alpha}\right) < 1$. The reason is that in the case of just one mistake/experiment, the mistaken/experimenting agent, say agent i, does not receive any $m_j^t = g$, so $\sum_{j \in N \backslash \{i\}} p_{ij}^g = 0$ and k is still a best reply action for this agent.

Assume instead that all mistakes/experiments are made in the action stage, then recall from the proof of proposition 1 that it takes only one mistake/experiment to "create" a lower ranked minimum action d as a lower ranked action is payoff determinant, but n mistakes/experiments for a higher ranked $g > k$. Thus, since $n \geq 2$, E_1 is the only stochastically stable state. This proves part (i) of proposition 2.

For part (ii), assume first that $q_i^l(l) > \frac{\beta}{\alpha} + \frac{c}{\alpha(l-k)}$ for all k, l such that $l > k$. By lemma 1, if this condition hold for $l > k$, it must also hold for K. If i's message is either $m_i^t = l$ or $m_i^t = K$, then under assumption 2 the only other action with positive probability is k. In turn, by assumption 3 $q_i^K(K) = q_i^l(l)$, $q_i^k(l) = 1 - q_i^l(l)$, and $q_i^k(K) = 1 - q_i^g(K)$ which implies that $q_i^k(l) = q_i^k(K)$. Then we can write the difference between the expected conditional payoffs of messages K and l as

$$q_i^K(K)K(\alpha - \beta) + \sum_{d=1}^{K-1} q_i^d(K)(\alpha d - \beta K) - q_i^l(l)l(\alpha - \beta)$$
$$- \sum_{d=1}^{l-1} q_i^d(l)(\alpha d - \beta l) = (K - l)\left(q_i^K(K)\alpha - \beta\right). \tag{3.21}$$

Expression (3.21) is always positive as $q_i^K(K)\alpha > \beta$ if $\frac{\beta}{\alpha} + \frac{c}{\alpha(K-k)} < q_i^K(K)$. So if equation (3.18) does not hold, agents send $m_i^t = K$. As this holds for all agents, if communication is ever part of a best reply strategy in a situation such that $s_i^{t-1} = (\varnothing, k)\, \forall\, i \in N$, that strategy is $s_i^t = (K, K)$ for all agents. In turn, the best reply to $s_i^t = (K, K)$, for all i, is $s_i^{t+1} = (\varnothing, K)$, which is the only best reply to itself and the only absorbing state and stochastically stable state.

For any $l > k$, $\frac{\beta}{\alpha} + \frac{c}{\alpha(l-k)}$ is smallest when $k = 1$ and increases with k. Thus, whenever $q_i^l(l) > \frac{\beta}{\alpha} + \frac{c}{\alpha(l-k)}$ holds for some k, l, it also holds for l and $k = 1$. Assume that $q_i^l(l) > \frac{\beta}{\alpha} + \frac{c}{\alpha(l-k)}$ hold for $l = K$ and $k = 1$, but not for any other pair $l, k \in A_i$. This implies that all $E_2, ..., E_K$ are absorbing states. By the proof of part (i), from any E_k, the move to E_1 requires the fewest mistakes/experiments among all $E_k \in E$. Whenever in

E_1, $s_i = (K, K)$ is the unique best reply for all $i \in N$, i.e. the resistance between the states is zero. A similar argument can be made when $q_i^l(l) > \frac{\beta}{\alpha} + \frac{c}{\alpha(l-k)}$ hold also for $k > 1$, which implies that E_K has the minimum stochastic potential whenever $q_i^l(l) > \frac{\beta}{\alpha} + \frac{c}{\alpha(l-k)}$ holds for some pair k, l and is thus the unique stochastically stable state. This proves part (ii) of proposition 2 and concludes the proof.■

3.A.4 Proof of proposition 3

Assume $\min_{j \in N}\{a^{t-1}\} = k$. By Lemma 1, $m_i^t = K$ is at least weakly preferred to all other $m_i^t = l$ in any period. If BR_i^m contains more than one message, agents randomize uniformly between these. As $m_i^t = K \in BR_i^m$ holds for all t and all i and there are finitely many messages, at some $t' \geq t$ enough agents will send $m_i^{t'} = K$ so that $\min_{j \in N}\{a_j^{t'}\} = K$. If messages affect subjective probabilities, $\sum_{g=l}^{K} q_i^l(K) \geq \sum_{g=l}^{K} q_i^l(l) \,\forall\, l \in A_i$ and $q_i^K(K) > q_i^K(l)$. Therefore, in $t'+1$, $\mathbb{E}(\pi(l)|m_i^{t'+1} = K, h^{t'}) \geq \mathbb{E}(\pi(l)|m_i^{t'+1} = l, h^{t'})$ for all $l \in A_i$, while $\mathbb{E}(\pi(K)|m_i^{t'+1} = K, h^{t'}) > \mathbb{E}(\pi(K)|m_i^{t'+1} = l, h^{t'})$ for all agents, so the sum of expected payoffs is greater for $m_i^{t'+1} = K$, which is then the only best reply message. K is consequently the only best reply action possible for all i in $t'+1$. As $s = ((K, K)_1, ..., (K, K)_n)$ is for the same reasons the best reply to itself, it is the unique absorbing and stochastically stable state.■

3.A.5 Proof of proposition 4

Assume $\min_{j \in N}\{a^{t-1}\} = k$ and that $m_1^{t-1} = g > k$. Then agent 1's message was not enough to make k into a best reply for all agents in $t-1$. As in equation (3.19) in the proof of proposition 2, this implies that $p_i^g < \frac{\beta}{\alpha}$ for at least one $i \in N \setminus \{1\}$. According to equation (3.5), $p_i^g(m_1^t = g) = \frac{p_{i1}^g}{n-1}$ when $\min_{j \in N}\{a_j^{t-1}\} = k$, which together with the assumption that $m_1^{t-1} = g$ did not change the minimum action into g implies that $p_{i1}^g < (n-1)\beta/\alpha$. If $p_{i1}^g < (n-1)\beta/\alpha$ holds for all $g > k$, messages do not change the minimum action and then by assumption 4 $(q_1^g(g))^t < (q_1^g(g))^{t-1}$. Thus, in some period $t' > t$, no other message than $m_1^{t'} = k$ remain a best reply message for agent 1.

Then we are back to mistakes/experiments in the action stage[25] being

[25] Mistakes and experiments in the communication stage cannot be important here as if

the only source of change, and $s = ((1,1)_1, (\varnothing,1)_2, ..., (\varnothing,1)_n)$ is the unique stochastically stable state for the same reason as in the proofs of proposition 1 and 2: it takes only one mistake/experiment to move to lowest ranked action from any other state and more to move higher ranked.

Assume instead that $p_{i1}^{l} > (n-1)\beta/\alpha$ holds for all $l \in A_i$ and all $i \in N \setminus \{1\}$. Agent 1 sends a higher ranked message if it is expected to change the choices of the other agents. As Lemma 1 and equation (3.21) from the proof of proposition 2 holds for agent 1, the best reply message for agent 1 is $m_1^t = K$ in such a case. That is, agent 1 sends $m_1^t = K$ when

$$q_1^K(K)K(\alpha-\beta)+(1-q_1^K(K))(\alpha k-\beta K) > k(\alpha-\beta) \Rightarrow q_1^K(K) > \frac{\beta}{\alpha} \quad (3.22)$$

Note that as agent 1 must send a message, we can disregard the message costs since these are always incurred. Thus, if $p_{i1}^K > (n-1)\beta/\alpha \forall\, i \in N \setminus \{i\}$ and $q_1^K(K) > \frac{\beta}{\alpha}$, then the only absorbing state and the unique stochastically stable state is

$$s = ((K,K)_1, (\varnothing,K)_2, ..., (\varnothing,K)_n).\blacksquare$$

$p_{i1}^{g} < (n-1)\beta/\alpha$ for at least one agent i and for all $g \neq k$, then the minimum action does not change because of agent 1's mistaken/experimental message.

Chapter 4

Institutions promoting fiscal discipline: evidence from Swedish municipalities

with Lina Maria Ellegård

4.1 Introduction

How to maintain fiscal discipline is a persistent challenge at all levels of government. The importance of this challenge has been all the more evident in the aftermath of the financial crisis in 2008, as the recession has severely strained the finances of many municipalities, regions, and countries, and even resulted in bailout-programs and defaults. One suggested response to the challenge, reflected in the European Union's fiscal pact for example, is to improve budget institutions – that is, the formal rules and informal norms related to the drafting, approval and implementation of the budget. The idea that budget institutions improve fiscal discipline finds support in earlier research, which indicates that features such as the transparency of the budget (e.g. Eslava, 2011), the centralization of the budget process (e.g. von Hagen and Harden, 1995; Hallerberg and von Hagen, 1999), and, if properly enforced, balanced budget rules (e.g. Bohn and Inman, 1996) seem to increase fiscal performance in some contexts.[1]

However, the literature is still far from a consensus on best practice in several respects. We contribute by addressing three issues: *First*, although conflicts of interest between agents within government are at the core of the political economy literature on fiscal discipline, few empirical studies have tried to quantify such conflicts. To the best of our knowledge, none have done so using field data.[2] Omitting the degree of conflict between agents

[1]Poterba (1996); Alesina and Perotti (1999) and Eslava (2011) survey this literature.
[2]Ehrhart et al. (2007) tests predictions of the Ferejohn and Krehbiel (1987) models of

makes it harder to detect the effect of budget institutions, since these should only play a role when there is a conflict for them to solve. *Second*, due to the often small number of observations, many studies represent budget institutions by index measures. The index formulation implies that the effect of particular institutions is obscured and thus inhibits straightforward policy recommendations (Poterba, 1996). Index measures moreover preclude the study of interdependence between different institutions, while the specific combination of institutions has been argued to be of importance (e.g. von Hagen, 2006; Eslava, 2011). *Third*, although the need to control the whole budget process – from formulation to implementation – has been previously acknowledged (e.g. von Hagen and Harden, 1995; von Hagen, 1998; Hallerberg and von Hagen, 1999), empirical studies have largely overlooked the implementation stage, or included it in index measures capturing features of the whole budget process (von Hagen and Harden, 1995; Fabrizio and Mody, 2006). Knowledge is therefore scant regarding how the incentives of local-level agents, to whom the responsibility of implementing the budget is delegated, can be aligned to the interests of the central level, that formulates the budget.

Our study addresses these three issues in an analysis of budget institutions and fiscal performance in the Swedish municipalities, thus adding to the literature on sub-national budget institutions.[3] Besides the fact that the municipalities – like local governments in general – constitute a large part of the national economy, certain attributes make them attractive study objects. They all operate under the same legal system and in the same cultural context, which mitigates the risk of mistakenly attributing the effect of these factors to institutions – a prominent concern in cross-country studies. Moreover, all municipalities have the same fundamental areas of responsibility, which dampens the influence of differences in ambition. Still, the municipalities have considerable freedom to choose how activities should be

top-down and bottom-up budgeting in a laboratory experiment, and show that there is no straightforward relationship between the sequence of the budget decisions and the size of the budget; the outcome also depends on the preferences of players.

[3]See e.g. Poterba (1994), Bohn and Inman (1996), Strauch and von Hagen (2001), and Krogstrup and Wälti (2008) who find that self-imposed balanced-budget rules are correlated to lower deficits; Foremny (2011) and Grembi et al. (2012) who find positive effects of fiscal rules imposed by the central government on fiscal performance; and Feld and Kirchgässner (1999), Hagen and Vabo (2005), Tovmo (2007), and Jochimsen and Nuscheler (2011) who find that centralization of the budget process is positively associated to (some) measures of fiscal performance.

organized and financed, so there is heterogeneity to study.

We first develop a simple model of the municipal budget process as a motivating framework for our empirical investigation. The model suggests that to reach the outcome desired by the central level, institutions that curb the bargaining power (that *centralizes* the budget process) as well as institutions that align the incentives of the local level are needed. Moreover, budget institutions may have to be strengthened as the conflict of interests between the central and local level is intensified. To obtain data on budget institutions and conflicts of interests, we construct a survey and collect a unique dataset covering 265 out of 290 municipalities. The survey explicitly measures the conflicts of interest between the central level, which is responsible for the municipality's overall fiscal performance, and the local-level committees, who are responsible for their respective sub-fields only. The survey data indicates that substantial conflicts of interest regarding the importance of fiscal discipline prevail in roughly half of the municipalities.

The comparatively large number of cross-sectional observations enable us to analyze a diverse set of budget institutions without resorting to index measures. Besides the centralization of the planning stage of the budget process, we examine two types of institutions that may allow the central level to influence the local level's spending decisions: result carry-over rules and threats to replace managers and politicians running systematic deficits.[4]

Our regression estimates confirm the importance of taking the interaction between institutions and fiscal preferences of different levels into account, as the estimated correlations depend on the degree of conflict. Like many previous studies, we find that a centralized budget process is beneficial for fiscal performance (measured as operating revenues net of costs), though only for municipalities where there is a substantial conflict of interest – that is, only in the circumstances where centralization should have a role to play. For this group of municipalities, we furthermore find that fiscal performance correlates positively to the use of a surplus carry-over rule and to a credible threat of replacement of local-level managers. For municipalities with less intense conflicts, the use of a deficit carry-over rule is positively correlated to performance. While the data does not allow us to study the effect of either carry-over rule in the absence of a centralized

[4]Dahlberg et al. (2005) find no correlation between result carry-over rules and fiscal performance in a study of the Swedish municipalities. To the best of our knowledge, replacement threats have not been studied before in the context of local governments.

budget process – most municipalities with carry-over rules also have a relatively centralized process – we do find that municipalities that combine carry-over rules with a centralized budget process have higher performance than municipalities that employ at most one of these institutions.

The next section gives some background information about the Swedish municipalities. We present our theoretical framework in Section 3. Section 4 describes the survey, the construction of our institutional variables and the other variables in the analysis, while Section 5 describes our empirical strategy. Section 6 is devoted to the presentation and interpretation of our results; and section 7 contains a discussion of identification issues and concluding remarks.

4.2 The Swedish municipalities

Sweden is divided into 290 municipalities: geographically separated units for local government. Municipal expenditures accounted for approximately 14 percent of Swedish GDP in 2010, almost half of the public sector's total expenditures for final consumption and investments (Statistics Sweden, 2011). All municipalities have the same fundamental responsibilities, e.g. the pre- to upper secondary school system, elderly care, social services, building and planning issues, environmental protection, and fire department services (Brorström et al., 1999). Nonetheless, the principle of municipal self-government, written into Sweden's constitutional laws, implies considerable freedom to choose how activities should be organized and financed (Berlin and Carlström, 2003).

Swedish law stipulates that each municipality must have a council and an executive committee. The council is appointed through general elections, held every four years, and the executive committee is elected by the council (Brorström and Siverbo, 2001). Most municipalities employ an organizational structure in which the council delegates the responsibility for different services to lower-level political committees, generally defined by function (e.g schools) and/or by geography (e.g. a district). Administrative units with civil servants are connected to each political committee.

The municipalities are obliged to annually specify a budget, which should contain a plan for the coming year, and a long-term budget for the subsequent two years. The balanced budget law, enacted in 2000, moreover states that a budget deficit one year must be followed by an equally large surplus over the next three years. Nevertheless, the law allows for exceptions[5] and in practice is not enforced by any sanctions.

According to the bills preparing the legislation, the balanced budget requirement should be regarded as a minimal demand (Swedish Government, 2004). Empirically, nearly all municipalities have formulated more ambitious financial goals (Dahlberg et al., 2005; Brorström et al., 2009). The main reason to strive for surpluses is that the municipalities have separate operating and capital budgets. Investments in capital generate expenditures immediately, but they only become costs in the form of write-offs. As investment expenditures normally are higher than write-offs, municipalities need to run surpluses to be able to finance investments without taking on more debt.

4.3 Theoretical framework

This section draws on the most relevant earlier literature to construct a motivating framework for our empirical investigation of what institutions, and what combinations of these are conducive to fiscal discipline in situations characterized by conflicts of interest.[6] We sketch the budget process as a simple game, and, following North (1990; 2005), identify institutions with the (formal and informal) rules and enforcement characteristics of this game. We design the budget game with the budget process of the Swedish municipalities in mind, but the main features apply to public budget processes in general.

The budget game has two types of players, the central player (C) and the local player (L). Translated to the context of Swedish municipalities, C corresponds to the council and the executive committee and its administration, while each L corresponds to an operating branch at a lower level, for example the committee and administration of public schools. For simplicity, we assume only one single L in the game.[7] To focus on the relation between

[5]E.g. if the deficit is caused by unconverted losses in stocks and bonds, or if the municipality has previously amassed large amounts of wealth (Swedish Government, 2004).

[6]We focus on budget institutions and thus disregard the large and related literature emanating from Roubini and Sachs (1989), that examines the effect of weak governments on fiscal performance. See e.g. Ashworth et al. (2005) for a review of the (mixed) results of this literature. We do however acknowledge strength of government in the empirical analysis, see section 4.4.

[7]Treating the central and local levels as unitary players abstracts from the possibility

the central and the local level, we do not model how voter preferences influence the political game of deciding the tax rate and the level of fees.[8] In effect, we assume that the level of revenues is exogenous to the game.

The game focuses on two stages of the budget process: the planning stage, during which the budget is drafted and approved, and the implementation stage, during which it is executed. Previous research, as well as the features of the two stages, suggests that each stage has its own crucial institutional features. At the planning stage, the degree of centralization of the drafting and approval process is a crucial feature. At the implementation stage, the prevalence (or absence) of institutions that restrain the spending of the local level is important. As these latter institutions align the incentives of the local level to the interests of the central level, we henceforth refer to them as *incentive-aligning institutions*. In the game, we represent this type of institution with a possibility that C may punish L for not complying with the budget. The timing of the game is as follows:

1. The players receive information about the level of revenues.
2. In the planning stage, C and L bargain about the budget: L puts forward a budget proposal b_L and C determines the final budget b.
3. In the implementation stage, L chooses a spending level $x > 0$.
4. If the spending level exceeds the budget, C may punish L. Otherwise, the game ends after step 3.

The next three sections describe players' preferences and the planning and implementation stages in more detail. Throughout, information about preferences, payoffs, probabilities, and strategies are assumed to be common knowledge.

4.3.1 Players' preferences

Each player has preferences over fiscal performance; that is, the difference between revenues and spending. As the level of revenues is exogenous, we

that politicians and civil servants within each level have different preferences. For the purposes of this paper, we think that central-local conflicts of interests are more important.

[8] This choice precludes a theoretical treatment of the transparency of the budget process, suggested to be important by Eslava (2011). See section 4.7.1 for a discussion about this matter in relation to our results.

can translate, without any further loss of generality, preferences over fiscal performance into preferences over spending. For every level of revenue, we also assume that preferences are single-peaked; that is, there exists an optimal level of spending for each player, denoted x_C^* and x_L^* respectively. Due to the different roles and responsibilities of C and L, we assume that L's preferred level of spending is at least as high as C's, $x_L^* \geq x_C^*$, and say that conflicts of interest over spending increase as the difference between x_L^* and x_C^* increase. Although we assume that L is relatively spending-prone, L's optimal level of spending is not likely to be completely unrestrained. We rely here on the argument in Wildavsky (1975, p. 6-8) that there must be an element of cooperation and a shared understanding of the limits for budgetary proposals if an organization is to be able to function at all. Thus, L may but need not be a budget maximizer as the bureaus in for example Niskanen (1968).

4.3.2 The planning stage

Bargaining over the budget draft is a key feature of the planning stage. Weingast et al. (1981) were the first to suggest that excessively high (and Pareto-dominated) levels of spending can be explained by a common-pool problem present at the planning stage. von Hagen and Harden (1995) show that centralization of the budget process addresses the problem by changing the balance of bargaining power in favour of a centrally appointed finance minister (a player who, in contrast to ministers with specific portfolios, takes the full costs of each proposal into account).[9]

In our game, C bargains with L over a budgeted level of spending. We denote L's budget proposal b_L, and the approved budget, which is determined by C, is denoted b. To retain focus on the interplay between the institutions at the two stages, we refrain from explicitly modelling the bargaining process. Instead, to incorporate the insights from the earlier literature regarding centralization and bargaining power of L, we let C incur a

[9] Empirically, positive associations of centralization with fiscal performance have been found in the EU (von Hagen and Harden, 1995), Asia (Lao-Karaya, 1997), Latin America (Alesina et al., 1999; Stein et al., 1999), Africa (Gollwitzer, 2010), American states (Strauch and von Hagen, 2001), and in Norwegian municipalities (Hagen and Vabo, 2005; Tovmo, 2007). However, Dahlberg et al. (2005) and Perotti and Kontopoulos (2002) find no significance of centralization-type institutions in Swedish municipalities and OECD countries, respectively.

cost, ($h \geq 0$) if the final budget proposal is lower than L's proposed level. We also assume that h is increasing in L's bargaining power. Following von Hagen and Harden (1995), we call the planning stage *centralized* when the bargaining power of L is constrained in some way, for example by restrictions on the possibilities of proposing amendments,[10] or on the share of resources bargained over. That the possibility of making proposals is connected to the bargaining power of L can be rationalized by the fact that budget proposals of local committees are typically made publicly known through the media in Swedish municipalities. Thus, popular proposals are costly to decline for the central level. In other words, restricted possibilities of making proposals decrease the bargaining power of local committees and vice versa. Therefore, increasing centralization decreases h, and a fully centralized planning stage implies $h = 0$.

4.3.3 The implementation stage

If the fiscal preferences of the central level differ from those of the local level, which takes the actual spending decisions, incentive-aligning institutions at the implementation stage are necessary to prevent the local level from spending in excess of the budget (Hallerberg and von Hagen, 1999). Balanced budget rules and other numerical targets are examples of institutions intended to constrain agents. Poterba (1996) and Eslava (2011), who review the literature on numerical targets, underline that, although several studies find a positive correlation to fiscal performance, rules are only effective if enforced.[11] Acknowledging these results, we incorporate a generic incentive-aligning institution into the game – a threat of punishment for budget non-compliance – that varies in credibility and strength.

At the implementation stage, L first chooses the level of spending (x). After having learnt the realized level of spending and compared it to the budgeted level b, C decides whether or not to punish L. A punishing institution affects L's actions in the previous step by making deviations from the

[10] Agenda-setting is often associated with bargaining power in political economy-models (e.g. Persson and Tabellini, 2000; Tovmo, 2007).

[11] Bohn and Inman (1996) find that balanced budget rules in American states that are enforced by the state supreme court have a positive impact on fiscal performance, and that the rule is more binding in appointed, as opposed to elected, supreme courts. The results in Debrun et al. (2008) for the countries in the European Union suggest that features such as statutory basis, independent monitoring and enforcement, automatic correction mechanisms, and media coverage are all important.

budgeted level of spending costly. The deterring effect of the institution depends on the size of punishment, $p \geq 0$ (the strength), and on L's subjective assessment of the probability that the punishment is carried out, $q \in [0, 1]$ (the credibility). We assume that this probability is known by C and that it is strictly increasing in the size of the deviation from the budgeted level. Furthermore, if $x = b$ then $q = 0$.

4.3.4 Payoffs and results

The payoffs (utility) for the two utility maximizing players are given by the following functions:

$$U_C = u_C(x) - h$$

$$E(U_L) = u_L(x) - q(x)p$$

where the utility of spending for each player i, $u_i(x)$, is a continuous and strictly concave function with a single optimum $x = x_i^* \in (0, \infty)$. Assume also that if $u_L(x' = b) = u_L(x) - q(x)p$, then L prefer to comply with the budget, i.e. choose x' rather than risk punishment. Within this setup, we look for the sub-game perfect equilibrium level of spending of the game (x^e) and state the following propositions (see Appendix 4.A for proofs):

Proposition 1: (i) $qp = 0 \Rightarrow x^e = x_L^*$, (ii) $x^e \in [x_C^*, x_L^*]$, (iii) If $x_L^* = x_C^*$, then $x^e = x_L^* = x_C^*$.

Part (i) shows that an incentive-aligning institution has to possess some credibility and strength ($qp > 0$) to be effective. It follows that h is unimportant if $qp = 0$; if the threat of punishment is not credible, L can simply disregard the budget and centralization becomes unimportant. Part (ii) captures that neither C nor L is interested in spending less than x_C^* or more than x_L^*. The interesting implication of part (iii) is that if there are no conflicts of interest, then the institutional structure is not important for the level of spending, and in turn not for fiscal performance. In the following, we assume $x_L^* > x_C^*$.

Proposition 2: Suppose $h > 0$. Then, (i) $x^e > x_C^*$ for any $qp \geq 0$; and (ii) for any $qp > 0$, $x^e \in (x_C^*, x_L^*)$ strictly increases in h.

Part (i) shows that incentive-aligning institutions are not sufficient to reach the outcome desired by C; in fact, whenever the planning stage is not fully centralized, C's optimal level of spending is unattainable. The reason is that C then has to take the cost h of proposing a budget $b < b_L$ into account, which gives L enough bargaining power to force C to set $b > x_C^*$. Part (ii) states that for a given level of incentive-alignment, a more decentralized budget process always implies a higher level of spending. (ii) also implies that for sufficiently large h, a given incentive-aligning institution will not be able to curb spending at all. The reason is that when C experiences sufficiently high costs of deviating from L's proposal, L can make C propose $b = x_L^*$ at the planning stage and thus obviate the threat of punishment at the implementation stage.

Proposition 3: For any $h \geq 0$, (i) there is a lowest feasible level of spending $\underline{x} \in [x_C^*, x_L^*]$ and (ii) $x^e \in (\underline{x}, x_L^*]$ strictly decreases in q and p.

Part *(i)* highlights that the level of centralization implies a lower bound for the attainable level of spending C can force L to choose. Part (ii) adds that the equilibrium level of spending will be closer to x_C^*, the stronger and/or more credible the incentive-aligning institutions are (until the lower bound defined by L's bargaining power is reached).

In the last proposition, we take the point of view of C and show that when conflicts of interests over spending increase, i.e. when x_L^* increases, C may need stronger institutions to retain the earlier level of spending. Notably, increased conflicts of interest do not imply higher levels of spending if punishments are sufficiently credible and severe to start with.

Proposition 4: For given x_C^* and $h > 0$, (i) x^e is non-decreasing in x_L^*; and (ii) if the strength (p) and credibility (q) of the incentive-aligning institution is sufficiently weak, then x^e is strictly increasing in x_L^*.

If we instead take the opposite view and fix x_L^*, the equilibrium level of spending may similarly be decreasing in x_C^* (that is, decreasing in the level of conflict), but the strength and credibility of the institutions play the same crucial role in the determination of the equilibrium level of spending. Viewing C's bliss point as a measure of its motivation for high fiscal performance, henceforth referred to as its *fiscal motivation*, proposition 4 indicates that C

has incentives to strengthen its budget institutions as its bliss point moves farther away from L's. This suggests that the realized level of spending and the institutional framework may be jointly determined, which is challenging for our empirical investigation. We discuss this matter further in section 4.7.1.

In sum, if there are differences in preferences over spending, both centralization and incentive-aligning institutions may be required to reach a level of spending that implies an outcome close to that desired by the central level. The need for stronger institutions also increases when conflicts of interests over spending increase. We use these results and the earlier literature to guide our data collection and econometric analysis, which we describe in the next sections.

4.4 Data

For the empirical investigation we require information on fiscal preferences, the degree of centralization, and on candidates for incentive-aligning institutions. To obtain such data, we constructed a survey that was sent to all 290 Swedish municipalities in June 2010.[12] The electronic survey was addressed to the civil servant in charge of planning and implementing the overall budget, i.e. the budget manager. Respondents were promised confidentiality.

We modelled the survey after a similar survey conducted by the Swedish Association of Local Authorities and Regions (SALAR) in 2004.[13] Our survey differs from the 2004 survey in important respects though; in particular, the older survey does not record whether there are differences in the fiscal preferences of the central and local levels. To validate the survey questions, we discussed them with the budget manager and one of his close co-workers in the municipality of Helsingborg (the 9th largest municipality), and with representatives of SALAR.

As many as 91 percent of the municipalities responded to the survey, although the response rates were lower for certain questions (for an analysis of differences in response rates between questions, see Appendix 4.B). Compared to the responding municipalities, the 25 non-responding municipalities are significantly smaller and have higher tax rates, smaller income

[12] See Appendix 4.E for a translation of the survey questions.

[13] Dahlberg et al. (2005) analyze this survey.

tax bases and lower net revenues.

Data on fiscal performance and additional control variables is obtained from Statistics Sweden (2011).

4.4.1 Measuring conflicts of interest

To measure conflicts of interests over fiscal matters between the central and local levels, the budget managers were asked to indicate the situation that best describes their municipality:[14]

1. the executive committee and the municipal council are more concerned about fiscal discipline than local committees;

2. the executive committee, the municipal council and the local committees do not differ significantly in their concerns about fiscal discipline;

3. local committees are more concerned about fiscal discipline than the executive committee and the municipal council.

The survey answers are translated into the dummy variable *pd*, which equals 1 if the executive committee/municipal council are more concerned about fiscal discipline (alternative 1) and 0 otherwise.[15] 56 percent of the 239 municipalities that responded to the question chose alternative 1, i.e. the budget manager estimated that there were conflicts of interest of some substance. To relate the survey question to our theoretical model, which considers preferences over spending levels, note that the level of revenues is fixed in the model. The preferences over spending levels in the model are therefore closely related to preferences regarding fiscal discipline as measured here. Fiscal discipline is of course a more long-term concept than the model reveals, but local committees that are concerned about overall discipline should also be more likely to respect their own short-term budget balance.

We are confident that the budget manager is the most suitable person to judge the situation, as the manager has a coordinating role in the budget

[14] The translation of the Swedish survey question into English is not perfect, the question uses an idiom ("en ekonomi i balans") in use in the municipalities, which does not literally translate as "fiscal discipline". We think that fiscal discipline conveys the meaning of the idiom better than the literal translation ("a balanced economy").

[15] Only two municipalities indicated alternative 3. The results are not affected by putting them in the same category as those who chose alternative 2.

process and closely follows the local level throughout the budget year. It is moreover important to note that the budget manager has little interest in stating a certain response in order to look better her-/himself, the question regards the committees.

The variable is a crude measure of the degree of conflicts though, as respondents' individual cut-off points for choosing one alternative over another are subjective and likely to differ. Therefore, some municipalities that according to an objective measure would be categorized as having substantial conflicts may choose alternative 2, and vice versa. Such mis-categorizations decrease the difference between the groups in terms of real conflicts of interest, which makes it more difficult to empirically detect between-group differences in how budget institutions work.

The concern for fiscal discipline likely differs somewhat between the two levels even in municipalities that chose alternative 2, as each local committee is responsible for only one part of the municipality's services and moreover partly functions as advocate for its own area. For the empirical analysis, this implies that the expected difference in the workings of the budget institutions becomes a matter of degree; effective institutions are not unthinkable in municipalities that chose alternative 2, but we expect them to be less important.

4.4.2 Incentive-aligning institutions

Our theoretical model considers a generic type of incentive-aligning institution, but empirically they can take various shapes. Monetary bonus schemes readily come to mind, but such schemes are virtually non-existent in Swedish municipalities.[16] We therefore examine a few other institutions, to see whether they possess incentive-aligning properties.

As a first candidate, we consider *result carry-over rules*: rules specifying that local level surpluses/deficits are to be transferred to the next budget year. Note that we do not mean rules regarding whether deficits are at all *allowed* or not, which is a common use of the term (see Alt and Lowry (1994) for a discussion of this in relation to US states). In the context of countries and states, result carry-over rules in our sense of the term have been hypothesized to decrease fiscal discipline (e.g. Alt and Lowry, 1994;

[16] Only one municipality in our survey reports the use of bonus schemes related to surpluses, despite the nearly universal prevalence of surplus targets.

von Hagen and Harden, 1996; Fabrizio and Mody, 2006), but there are several reasons why we think such rules restrain local level spending within municipalities. The reward (punishment) of forwarding a surplus (deficit) increases (decreases) the autonomy of the local level, as it implies greater (smaller) possibilities of allocating its resources as it sees fit over time.[17] A surplus carry-over rule reduces the local level's incentives to spend its entire budget each year, as unspent resources one year does not equal "wasted money" if it can be carried over to the next year's budget. The surplus rule moreover sends a signal of trust and thus of respect.[18]

The variables *keep surplus* and *keep deficit* indicate the presence of either carry-over rule. *Keep surplus* equals 1 if local committees/administrations carry over surpluses (wholly or partly) from one fiscal year to another, and 0 otherwise. *Keep deficit* equals 1 if local committees/administrations carry deficits over to subsequent fiscal years, and 0 otherwise.

A second way to punish non-complying committees and managers is to replace them.[19] For example, the municipal council has the authority to dissolve or reorganize a local committee, or change its responsibilities. Two dummy variables measure the risk of dismissal: *committee risk* and *manager risk*. To construct *committee risk*, we ask respondents whether a scenario of non-incidental and repeated deficits would constitute a sufficient reason to replace the members of the largest local committee.[20] A positive answer implies a value of 1 on the variable, which otherwise is coded as 0. *Manager risk* is constructed in a slightly different way.[21] We first ask

[17] Wilson (1989, pp. 179-195) argues that public organizations often value autonomy as much as, or more than, additional resources.

[18] See e.g. Ellingsen and Johannesson (2007) and the references therein for how esteem and respect may align interests between principals and agents.

[19] Hallerberg and von Hagen (1999, p. 218) write that "the ultimate punishment is dismissal from office". Although they discuss spending ministers, it should be equally true for civil servants.

[20] The "largest" administration/committee refers to the one with the highest level of spending. As spending levels vary greatly among the different local committees/administrations in a municipality, there is substantial heterogeneity in their impacts on the overall fiscal performance, and it is therefore unlikely that all committees/administrations are treated similarly with respect to deficits/surpluses. We restrict attention to the largest committee as the question would be difficult to answer if framed in a more general way, due to the heterogeneity.

[21] We would have preferred to construct the two variables in this way, but to limit the number of survey questions, we specified *committee risk* – which we ex ante believed to be less effective – in a simpler way.

Table 4.1: Incentive-aligning institutions

Variable	N	Mean	S.d.	Min	Max
keep surplus	255	0.45	0.50	0	1
keep deficit	256	0.33	0.47	0	1
committee risk	174	0.68	0.47	0	1
manager risk	191	0.78	0.42	0	1

whether a scenario of non-incidental and repeated deficits would constitute a sufficient, or a conducive but not sufficient, reason to replace the manager of the largest local administration. Respondents who answer that such a situation could be a conducive but not a sufficient reason are presented with a similar scenario, with the modification that the administration has made efforts to reduce the deficit. *Manager risk* equals 1 for those municipalities who answered that either of the two scenarios would constitute a sufficient reason to replace the manager, and 0 for the others.[22]

Table 4.1 shows descriptive features of our candidate incentive-aligning institutions. Almost 50 percent of municipalities employ a surplus carry-over rule, while one out of three employs the corresponding rule for deficits. The correlation between the two rules is quite high, $\rho = 0.64$. It can moreover be noted that the regulations of surpluses and deficits have changed in 25 (surplus rule) and 28 percent (deficit rule) of the responding municipalities between the 2004 and 2010 surveys.

68 percent of the respondents state that systematic deficits increase the risk that a local committee will be replaced. The risk is even higher for local managers – 78 percent of the respondents indicate the presence of such a risk. The two institutions are moreover highly correlated, $\rho = 0.68$. As seen in the table, the questions making up *committee risk* and *manager risk* have relatively low response rates (66 and 72 percent, respectively). Non-respondents are significantly different from respondents in some respects; for example, they have better fiscal performance (see Appendix 4.B). There were no corresponding questions about risk of replacement in the 2004 survey.

[22]Note that our survey is not a direct measure of q, L's beliefs about C's propensity to punish in the budget game, as the respondents are centrally placed administrators. It was simply not possible to send the survey to 290$\times$number of local administrators.

4.4.3 Centralization

We use three survey questions to measure the degree of centralization. The first asks whether the budget process is initiated by the executive committee or by the local committees. If the executive committee initiates the process, a follow-up question asks whether the local committees have large, limited, or no possibilities of proposing adjustments to the executive committee's budget proposal. The third question asks whether demographic factors and pre-set unit costs (e.g. schooling costs per pupil) govern the resource allocation to a large extent, to some extent or to a small extent. This question is an attempt to measure the size of the resources that are bargained over (if bargaining possibilities exist). The possibility of making budget proposals should make little difference for the local committees, if they only bargain over a negligible share of total resources.

We divide the municipalities into four categories, summarized in Table 4.2. Category 1, which refers to the highest degree of centralization, contains municipalities where the local committees hardly influence the budget process at all: where *a*) the central level initiates the process, there is no room for adjustment proposals and/or only a small share of total resources is bargained over; or *b*), the local level initiates the process but the scope for bargaining is small. In category 2 we put municipalities where the executive committee initiates the budget process, the local committees have some limited possibilities of making adjustment proposals, and there are some resources to be bargained over. Category 3 contains municipalities where *a*) local committees initiate the budget process and there are some resources to be bargained over; *b*) the executive committee initiates the budget process, there are large possibilities of making adjustment proposals, and there are some resources to be bargained over; or *c*) the executive committee initiates the budget process, there are some possibilities of making adjustment proposals, and a large share of total resources is bargained over. Category 4, the most decentralized category, contains municipalities where a large share of total resources is bargained over and either the local committees initiate the budget process, or the executive committee initiates the budget process but local committees have large possibilities of making adjustment proposals.

Table 4.3 shows how the municipalities are distributed over the four categories. Of the 249 responding municipalities, 18 percent are categorized

Table 4.2: Classification of degrees of centralization

Budget initiation	Adjustment proposal	Scope for bargaining	Centralization category
Central	Large	Large	4
Central	Large	Some	3
Central	Large	Small	1
Central	Limited	Large	3
Central	Limited	Some	2
Central	Limited	Small	1
Central	None	Large	1
Central	None	Some	1
Central	None	Small	1
Local	N.A.	Large	4
Local	N.A.	Some	3
Local	N.A.	Small	1

Table 4.3: Distribution of centralization variable

Degree of centralization	Frequency	Percent
1 (Most centralized)	45	18
2	35	14
3	111	45
4 (Most decentralized)	58	23
Total	249	100

as highly centralized, 23 percent are highly decentralized and 59 percent lie in between.[23]

4.4.4 Dependent variable

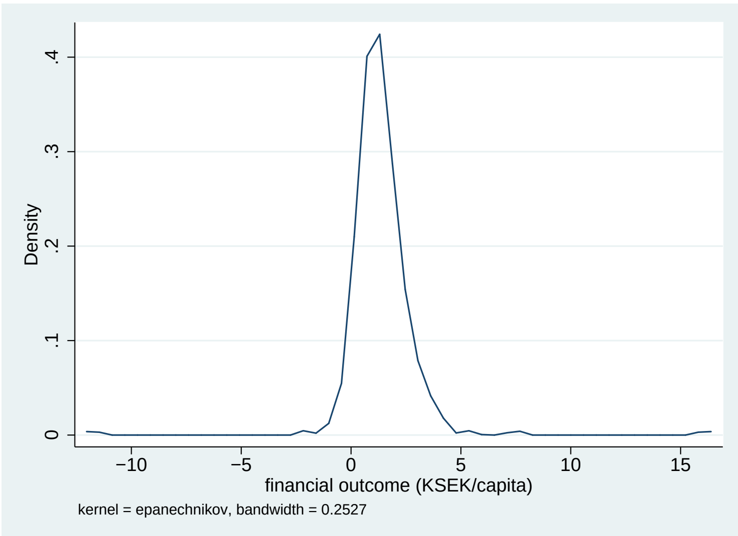
As our measure of fiscal performance, we use the per capita operating revenues net of costs (*net revenues*). This and all other economic variables are measured in 2010 prices. We focus on surpluses/deficits rather than balance sheet measures such as debt per capita or the equity ratio because our institutions are only indirectly connected to the balance sheet. Moreover, stock measures, debts and equity ratios are heavily influenced by extraordinary historical events (e.g. sales of large public companies) and can thus be misleading in a cross-sectional setting. There are also differences in accounting

[23] Our measure of centralization is not directly comparable to any measure in the 2004 survey. The first two questions are similar to those used to measure centralization in Tovmo (2007). Tovmo does not include any measure of the share of resources open to bargaining though.

practices, notably in regards to the accounting of pensions. In addition, the balanced budget law shows the lawmakers' focus on the revenues and costs statement, rather than the balance sheet (Brorström et al., 1999, pp. 54-61).

The distribution of net revenues in 2010 (Figure 4.4.1, Table 4.5) is centered around 1 360 SEK per capita (approximately 160 EUR). This may seem high, but recall from section 4.2 that surplus targets are the norm due to separate operating and capital budgets. Moreover, to dampen the consequences of the concurrent recession, the central government made extra transfers (proportional to population size) to all municipalities in 2009 and 2010; therefore, the recession did not have a large impact on revenues these years. Notably though, despite the balanced budget law, 6 percent of the municipalities ran deficits in 2010. This is somewhat below the average share running deficits during 2003-2009, which is 18 percent.

Figure 4.4.1: Per capita net revenues 2010



The measure of fiscal performance does not include so-called *extraordinary revenues/costs*.[24] This suits our purposes well, as we want to capture

[24]Note that the extra transfer from the central government is *not* counted as extraordinary. Generally, almost all revenues and costs are regarded as ordinary; extraordinary is reserved for e.g. natural disasters and sales of firms owned by the municipality (Council for Municipal Accounting, 2006).

systematic components of the municipalities' fiscal performance, rather than large exogenous shocks. However, the chosen measure is certainly not an indisputable measure of fiscal performance. For instance, discretion over the timing of accounting for certain costs and revenues can be used to manipulate the reported figures to some extent, and there are reasons why municipalities might wish to do so: the balanced budget law creates incentives to avoid showing deficits and "too large" net revenues may cause unsustainable demands for spending and/or tax cuts. We therefore expect the distribution of the dependent variable to be "compressed" compared to what it would be if the municipalities had no discretion regarding the timing of accounting. As a robustness check, we also perform regressions with per capita operating costs (i.e. excluding financial costs) as the dependent variable, thus excluding some manipulable posts. In order not to classify municipalities that temporarily run deficits to reduce previous high surpluses as irresponsible, we moreover include the equity ratio and mean net revenues over the period 2000-2007 in the estimations (the chosen period corresponds approximately to the latest completed business cycle in Sweden).

A remaining drawback of our approach is that neither net revenues nor costs are unambiguous measures of "better" fiscal performance in a normative sense; municipalities are supposed to provide adequate services in a fiscally responsible way, not to maximize profits. While these variables are indicative of fiscal performance, they need not be linearly related. One way to circumvent this problem would be to relate the actual net revenues of each municipality to the level specified in the budget (assuming that the budgeted level represents a fiscally sustainable level). By including the budgeted net revenue as a control variable, we do not completely avoid the problem that higher does not equal better, but we at least avoid comparing apples with oranges in terms of level of ambition.

We have manually collected information on the budgeted level of net revenues from the 2010 annual reports of almost all municipalities in our sample. It turns out that the average difference between actual and budgeted level of net revenues is very large, 898 SEK per capita (66 percent of the average actual level of net revenues). This large difference likely reflects the extra transfers from the central government, which suggests that many municipalities did not adjust their budgets after the transfer was announced.

The difference may also reflect factors such as bad forecasting, caution, or a desire to surpass expectations, and may therefore not be strongly related to a fiscally sustainable level. It is moreover conceivable that the budgeted level depends on what is deemed feasible given the set of budget institutions and the degree of conflict of interests. Because of these issues, we do not include the budgeted level in our baseline specifications, though a specification including this variable is available in Appendix 4.D.

4.4.5 Control variables

Following e.g. Tovmo (2007) and Krogstrup and Wälti (2008), we acknowledge that some municipalities may be more likely to employ budget institutions than others. The carry-over rules are more common and the budget process more centralized the larger, richer (in terms of personal income), younger, better educated and more right-wing the population (significant at 10 percent level in t-tests).[25] Although these municipalities have a relatively strong income tax base, their per capita total municipal revenues are significantly lower. This is explained by the intergovernmental equalization system, which favours municipalities with smaller income tax bases and unfavourable demography. Nonetheless, the municipalities using carry-over rules and/or a centralized budget process have higher equity ratios than other municipalities. The prevalence of *manager risk* and *committee risk* is higher in the same type of municipality, although there are no statistically significant differences in the share of right-wing voters, and the differences with regard to economic or political control variables are smaller and often statistically insignificant.

As these background variables are also likely to be correlated with the realized level of net revenues, we control for them in the analysis. Definitions of these control variables, as well as some structural controls, are found in Table 4.4 and Table 4.5 shows descriptive statistics for 2010.

Because of high collinearity, we cannot simultaneously include *total revenues* and *income tax base* in the analysis. In the choice between the two, we settle for the former, which makes our empirical model come closer to the theoretical model. One may argue that revenues, in contrast to the income tax base, are endogenously determined. However, almost all discretionary parameters (i.e. tax and fee rates) are fixed in the budget before the start of

[25] As the education level is highly collinear to the population size, we do not include the education level among the control variables.

Table 4.4: Description of control variables

Variable	Type	Description
total revenues	Economic	Per capita total revenues (KSEK)
relative change in total revenues	Economic	Change, tot. rev. between t and $t-1$ (%)
fixed asset revenues	Economic	Realization of fixed assets (% of tot. rev.)
financial costs	Economic	Per capita interest, asset write-downs etc
equity ratio	Economic	Private equity/total assets in $t-1$
mean net revenues 00-07	Economic	Mean net revenues 2000-2007 (KSEK)
share right-wing parties	Political	Right-wing seats in municipal council (%)
herfindahl	Political	$h = \sum_i (\text{vote share of party i})^2$
long-term budget	Political	LTB viewed as important
population	Demographic	Population (log)
population 20-79	Demographic	Population share in ages 20-79 (%)
cities	Structural	Dummy for larger cities
rural	Structural	Rural location
suburb/commuter/ manufacturing	Structural	Municipality either suburban, or large share commuters/manufacturing industries

the fiscal year; during the fiscal year, local committees/administrations have little influence over revenues and mainly affect fiscal performance through their spending decisions.[26]

We also include *fixed asset revenues* and *financial costs* as control variables, not because we believe that these are related to the institutional structure, but to reduce variation in the dependent variable, which stems largely from rare events that do not say much about fiscal discipline.

Importantly, the five budget institutions are more common in municipalities where the *long-term budget* is viewed as important according to the survey.[27] As *long-term budget* is a plausible proxy for central level fiscal motivation, we include it in the empirical specification to partly deal with the problem that budget institutions and net revenues may be simultaneously determined by such fiscal preferences (c.f. proposition 4 and Bohn and Inman, 1996; Eslava, 2011). Notably, *long-term budget* is the only significant variable in a similar analysis performed in Dahlberg et al. (2005).[28] As

[26] Revenue from income taxes make up approximately 65 percent of total municipal revenues; fees (21 percent), and government grants (12 percent) are the other two main sources of revenue (Statistics Sweden, 2011).

[27] Contrary to what its name suggests, this variable does not indicate whether the municipality employs long-term budgeting or not; all municipalities are obliged to.

[28] The survey question is a translation of an item in von Hagen's 1991 survey (von Hagen and Harden, 1995; de Haan et al., 1999).

Table 4.5: Descriptive statistics, dependent and control variables in 2010

Variable	Mean	Std. Dev.	Min.	Max.	N
*net revenues**	1.36	1.62	-11.78	16.13	265
*total revenues**	59.63	7.42	43.23	88.41	265
relative change in total revenues	2.32	2.82	-9.44	31.17	265
*fixed asset revenues**	2.34	8.61	0	90.0	265
*financial costs**	0.41	0.58	-0.09	6.32	265
equity ratio	50.08	17.72	-13.69	81.83	265
*mean fiscal performance 00-07**	0.53	0.53	-1.29	2.58	265
share right-wing parties	44.92	11.64	6.45	88.89	265
herfindahl	0.25	0.05	0.16	0.43	265
long-term budget	0.53	0.5	0	1	257
population (log)	9.87	0.95	7.81	13.65	265
population 20-79	70.80	1.62	64.44	77.17	265
cities	0.11	0.31	0	1	265
rural	0.13	0.34	0	1	265
suburb/commuter/manufacturing	0.41	0.49	0	1	265

*KSEK per capita

mean fiscal performance 00-07 and *equity ratio* too reflect the central level's fiscal motivation, the inclusion of these variables also addresses this omitted variables problem to some extent. It can lastly be noted that the variable capturing conflicts of interests, *pd*, shows no strong pairwise correlation to the mentioned background variables (although Ellegård (2013) shows that conflicts are slightly more common in smaller municipalities). We discuss issues of endogeneity and identification more in-depth in section 4.7.1.

4.5 Empirical strategy

Our first estimations explore whether the potentially incentive-aligning institutions (*keep surplus, keep deficit, manager risk* and *committee risk*) and/or *centralization* correlate positively to the fiscal performance of the municipalities. All institutions are included in one regression, in order not to confound their effects. As previously noted, non-response is relatively high for the two *risk* variables. Instead of dropping these observations – and thereby lose efficiency in the estimation of the effect of the carry-over rules – we include dummies for non-response to the *risk* questions. Recalling from the budget game that the expected positive effect of incentive-aligning institutions on fiscal performance is contingent on the degree of conflicts of interest, we interact each institution with the indicator for a difference between the fiscal preferences of the two levels of hierarchy (pd). We thus estimate

$$\begin{aligned}&\textit{fiscal performance}_i = \alpha + \mathbf{institution}_i'\boldsymbol{\beta}_0 + (pd_i \times \mathbf{institution}_i)'\boldsymbol{\beta}_1 \\ &+ \beta_2 pd_i + \mathbf{x}_i'\boldsymbol{\gamma} + \varepsilon_i \end{aligned} \tag{4.1}$$

where **institution** is a 5×1 vector including the four potentially incentive-aligning institutions and the centralization variable, $\mathbf{x}$ is the vector of control variables, and ε is a random error term. $\boldsymbol{\beta}_0$ and $\boldsymbol{\beta}_1$ are 5×1 vectors of parameters for the institutional variables and their interactions with pd. In the following, we refer to $\beta_n^j, n = 0, 1$ and $j = ks, kd, mr, cr, c$, when discussing the parameter estimates for *keep surplus, keep deficit, manager risk, committee risk* and *centralization*, respectively.

The theoretical framework suggests that the effectiveness of centralization depends on the incentive-aligning institutions and vice versa. To examine this suggestion empirically, we use the results from the estimations of Equation 4.1 to identify candidates for effective incentive-aligning institutions. To explore whether municipalities that employ *both* centralization and effective incentive-aligning institutions perform better than municipalities that employ only centralization, only incentive-alignment, or neither, we then partition the municipalities into groups. This analysis tells us whether relatively well-performing municipalities employ more types of institutions, but it does not say whether, for example, centralization is more effective in the presence of incentive-aligning institutions. To examine such interaction effects, we would need to estimate a model where each effective incentive-aligning institution is interacted with the centralization variable. As we shall see, the pursuit of this strategy is largely prevented by the fact that some of our candidates for effective incentive-aligning institutions and centralization are very often used together.

The baseline specifications are estimated on data for 2010 (except for *equity ratio*, which is lagged one year as the ratio one year is directly affected by the net revenues the same year). There are some influential observations (Cook's distance $> 4/n$) in the estimations, typically characterized by extreme values in terms of *net revenues*. By investigating the annual financial report of each outlier, we detect whether their extreme outcomes can be explained by rare events and/or book-keeping technicalities. As this is not the kind of behavior we seek to explain, we estimate each model twice: first including and then excluding the outliers whose extreme outcomes can be explained by such factors (3 in 2010). Note however that rare events and book-keeping technicalities can be hidden behind the more "modest" fiscal performances of other municipalities as well. That is, the real basis for exclusion of the outliers is not the extreme events in themselves, but the fact that they result in overall extreme outcomes. Thus, the sample excluding the outliers is not unambiguously a more valid basis for conclusions.

4.6 Results

4.6.1 Institutions and fiscal performance

Table 4.6 shows the baseline estimation results. The odd-numbered columns include all observations in the estimation sample, and the even-numbered columns show estimates for the sample excluding the three outliers. To illustrate how the level of conflict (*pd*) influences the results, the first two columns display coefficients from regressions without interaction terms between *pd* and the institutional variables, while our main specifications (i.e. Equation 4.1) are shown in the last two columns.[29] The centralization variable is coded as a dummy variable with category 4 as reference, as a Wald test suggests that the coefficients of categories 1, 2 and 3 are indistinguishable from each other.[30]

It can first be noted that municipalities with substantial conflicts of interests appear to have somewhat lower net revenues than municipalities with smaller conflicts. Though the estimates from the model without interactions are small and statistically insignificant, the coefficient of *pd* in the interaction specifications indicates that net revenues are about 500 SEK per capita lower in municipalities with a substantial conflict of interest – a large number in relation to the mean net revenues of 1 360 SEK. This suggests that the conflicts of interests measured by the survey do inhibit fiscal performance.

The question is whether budget institutions are helpful in closing this gap. The estimates give some support to the idea that the reward for

[29] See Appendix 4.C Table 4.10 for control variables. The indicators for non-response to the risk questions are left out of the table, as the coefficients for non-respondents are insignificant at conventional levels in most specifications and have no meaningful interpretation.

[30] In Appendix 4.D we examine a less broad classification.

Table 4.6: Baseline results, OLS on 2010 sample

	(1)	(2)	(3)	(4)
VARIABLES		Ex. outliers		Ex. outliers
keep surplus	0.463**	0.168	0.249	-0.063
	(0.195)	(0.164)	(0.280)	(0.236)
pd×*keep surplus*			0.353	0.332
			(0.341)	(0.306)
keep deficit	0.240	0.266	0.481*	0.602**
	(0.188)	(0.169)	(0.290)	(0.252)
pd×*keep deficit*			-0.390	-0.545
			(0.374)	(0.332)
manager risk	0.424*	0.407*	0.243	0.349
	(0.253)	(0.225)	(0.398)	(0.345)
pd×*manager risk*			0.357	0.105
			(0.406)	(0.363)
committee risk	-0.005	-0.113	0.012	-0.164
	(0.258)	(0.220)	(0.397)	(0.346)
pd×*committee risk*			-0.037	0.101
			(0.407)	(0.360)
cent123	0.194	0.220	0.054	-0.021
	(0.164)	(0.157)	(0.240)	(0.233)
pd×*cent123*			0.225	0.410
			(0.339)	(0.313)
pd	-0.118	-0.110	-0.507*	-0.499*
	(0.138)	(0.124)	(0.289)	(0.285)
Constant	-6.382*	-6.427**	-6.227*	-6.536**
	(3.598)	(3.197)	(3.615)	(3.248)
Incremental effect of institution j for municipalities where $pd = 1$				
$\beta_0^{ks} + \beta_1^{ks}$			0.602**	0.269
$\beta_0^{kd} + \beta_1^{kd}$			0.091	0.058
$\beta_0^{mr} + \beta_1^{mr}$			0.599**	0.454*
$\beta_0^{cr} + \beta_1^{cr}$			-0.025	-0.063
$\beta_0^{c} + \beta_1^{c}$			0.278	0.389*
Controls	Yes	Yes	Yes	Yes
Observations	225	222	225	222
R^2	0.567	0.256	0.574	0.275
F	4.300	3.710	3.661	3.294

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
See Table 4.10 in Appendix 4.C for control variable estimates.

being able to carry surpluses over to the next fiscal year promotes fiscal discipline, as the coefficient of *keep surplus* is positive and significant in column (1). Moving on to the interaction specification in (3), we can note that the positive and significant incremental effect derives from the group of municipalities that report substantial conflicts of interest. That is, β_0^{ks} is not significantly different from zero but $\beta_0^{ks} + \beta_1^{ks} > 0$ (see lower part of table). This is in line with our expectation that the institution should only make a difference where there is a problem to be solved. However, columns (2) and (4) reveal that the magnitude, as well as the statistical significance of the surplus rule, is partly driven by the three outliers. This calls for some caution in drawing conclusions regarding this variable – though the results in Section 4.6.2 below seem to indicate that there is more than an outlier effect.

We find no significance of the corresponding carry-over rule for deficits (*keep deficit*) in the specifications without interactions. The interaction specifications entail a challenge for our theoretical framework, as we find a reversed result from what we expect: *keep deficit* is significantly and positively related to fiscal performance (with large magnitude, 480-600 SEK per capita), but not in municipalities with a substantial conflict of interest. For them, $\beta_0^{kd}+\beta_1^{kd}$ is not distinguishable from zero in any specification. One possible interpretation is that the effect is non-linear; where there are major conflicts, the employment of a punishment rule may send out a signal that reinforces the conflict and increases non-compliance, while where conflicts are small, the rule may simply serve its restraining purpose. It can be noted that the outliers do not affect the estimate of *keep deficit*.

manager risk is strongly and positively related to net revenues according to the specification without interactions in columns (1) and (2). Moving to the interaction specification reveals that the results for *manager risk* are in line with the theoretical framework: β_0^{mr} is insignificant for municipalities that do not report substantial conflicts of interests, but it is positively related to the fiscal performance of municipalities with a substantial conflict to be resolved ($\beta_0^{mr} + \beta_1^{mr} > 0$). A credible threat that local managers who misbehave will be replaced is associated with approximately 450-600 SEK higher per capita net revenues, which also implies substantial economic significance.

By contrast, none of the estimations suggest that *committee risk* is useful, as the coefficient for this variable is insignificant in all specifications.

Thus, we find no support for the idea that the threat of replacement deters politicians of local boards from overspending. Multicollinearity with *manager risk* may cause the insignificance, but we also see another plausible explanation: the risk is connected to more severe consequences for the managers, for whom the risk applies to their main occupation, than to local politicians, who usually only have part time commissions or devote leisure time to politics.

The coefficient of *cent123* is insignificant in all samples, so, according to expectations, there is no evidence that a centralized process influences outcomes in municipalities where central and local levels agree on the importance of fiscal discipline. However, the incremental effect of centralization is positive and significant for municipalities that report substantial conflicts of interest in the sample excluding outliers (the coefficients imply 275-390 SEK per capita higher net revenues). Thus, although not as clear as in other studies, we do find indications of a beneficial effect of centralization in circumstances where it should make a difference.

Of the control variables (Appendix 4.C Table 4.10), we restrict our discussion to the political variables and the emphasis put on long term budgets, as these factors are most related to the institutional variables and the previous literature. Neither the *share of right-wing parties* nor the fragmentation of the municipal council (*herfindahl*) are significant in any specification. The negative coefficient of *herfindahl* contrasts with the theoretical predictions of fragmentation (Tovmo, 2007; Eslava, 2011); still, its statistical insignificance suggests that our focus on the interaction between central and local levels is more important for performance than the composition of the council. The importance assigned to the *long-term budget* is significantly associated with higher fiscal performance, just as found in the 2004 data by Dahlberg et al. (2005). A reasonable interpretation is that the variable partly captures the emphasis on fiscal discipline at the central level.

In Appendix 4.D we examine the robustness of the baseline results to 1) using *costs of services per capita* as the dependent variable; 2) removing and adding control variables; 3) using alternative, less endogenous, revenue measures; 4) other categorizations of the centralization variable and 5) including budgeted net revenues as control variable. We also run a first-difference regression for the carry-over rules, on which we have information for 2004. In sum, the baseline results seem rather robust; the key issue seems to be that the results for *keep surplus* and *cent123* are sensitive to outliers. In our view, the results indicate that all institutional variables except *committee risk* may be beneficial for net revenues. In the next section, we examine the theoretical suggestion that the combination of centralization and incentive-aligning institutions improve fiscal performance.

4.6.2 Combinations of budget institutions

We begin by investigating whether municipalities that employ combinations of good institutions (according to the above results) are relatively well-performing. Our previous estimations suggest that *keep surplus, manager risk* and *cent123* are important for municipalities where there is a conflict of interest (*pd*=1), and that *keep deficit* is important for the group where *pd*=0, whereas *cent123* is mostly positive, but insignificant for this group. We therefore partition the municipalities into four groups – A, B, C and D – as follows. Groups A (76 municipalities) and D (24 municipalities) contain the municipalities where *pd*=1; group A consists of those who also employ both *cent123* and either of *keep surplus* and *manager risk* (or both), and group D consists of the complementary subset that employs at most one type of institution (centralization or incentive-aligning). Similarly, groups B (24 municipalities) and C (50 municipalities) contains the municipalities where *pd*=0; group B consists of those who employ both *cent123* and *keep deficit*, while group C consists of those who employ at most one of these two institutions. Odd-numbered columns show the sample including outliers, while even-numbered columns exclude these municipalities.

Using group D as the reference category, columns (1) and (2) of Table 4.7 show that well-performing municipalities are overrepresented in the groups that combine several beneficial institutions (A and B).[31] For example, the coefficient on A is positive and significant both statistically and economically (the coefficients imply 450-560 SEK per capita higher net revenues). Similarly, the coefficient on group B is positive and larger than the coefficient on group C (and larger than the reference group D) with magnitudes of 670-730 SEK per capita. The difference between B and C is significant at the five percent level according to Wald tests (shown in the lower part of the table).

This analysis indicates that well-performing municipalities employ both

[31] The number of observations decreases because the partition rules out the inclusion of non-respondents to the *manager risk*-questions.

Table 4.7: Combinations of institutions

	(1)	(2)	(3)	(4)
		Excl outliers		Excl outliers
Group A	0.566**	0.458**		
	(0.250)	(0.223)		
Group B	0.741**	0.680**	0.693**	0.565**
	(0.290)	(0.269)	(0.302)	(0.250)
Group C	0.222	0.212	0.159	0.0948
	(0.231)	(0.220)	(0.204)	(0.185)
Group a			0.727***	0.440**
			(0.242)	(0.214)
manager risk			0.432**	0.326*
			(0.175)	(0.169)
Constant	-6.618	-8.478**	-7.063	-8.416**
	(4.937)	(4.254)	(4.656)	(4.179)
Test B≠C	p=0.040	p= 0.035	p= 0.049	p= 0.044
Control variables	Yes	Yes	Yes	Yes
Observations	174	171	174	171
R^2	0.620	0.334	0.641	0.348
F	7.682	6.803	8.208	6.958

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Group A: *pd*=1, *cent123*=1 and either *keep surplus*=1.
or *manager risk*=1, or both.
Group B: *pd*=0, *cent123*=1 and *keep deficit*=1.
Group C: *pd*=0, at most one of *cent123* and *keep deficit* = 1.
Group a: *pd*=1, *cent123*=1 and *keep surplus*=1.
Group D: reference category.

centralization and incentive-aligning institutions, but we would also like to examine whether centralization, for example, is more effective in the presence of certain incentive-aligning institutions. We are unable to address this question for the combination of carry-over rules and centralization, as almost all municipalities that employ a carry-over rule also have a centralized budget process. For instance, only 7 municipalities employ *keep surplus*, have a substantial conflict of interest ($pd = 1$) *and* are centralized to the lowest degree ($cent123 = 0$), and only 3 municipalities that use *keep deficit* have small conflicts of interests ($pd = 0$) *and* lack a centralized process.[32]

In addition to preventing us from estimating a meaningful interaction model, this implies that the baseline estimates for the carry-over rules (Table 4.6) mostly capture their influence conditional on using a centralized budget procedure. Collinearity with centralization is less of an issue for *manager risk*. If we run a specification with the *manager risk* interacted with centralization (results available on request), *manager risk* is positive and significant regardless of whether centralization is employed or not, whereas the interaction of the two is never significant. Thus, while the risk of replacement seems influential in itself, it neither affects nor is affected by centralization.

To see whether the results for group A are driven by *manager risk*, we create a new group a, which contains the municipalities that report a substantial conflict of interest ($pd = 1$) and also employ both *cent123* and *keep surplus*. The results in column (3) and (4) of Table 4.7, where groups B and C are kept the same and we include *manager risk* as a control variable, show that these fears are unwarranted. The coefficient on group a is positive, significant, and of comparable size to the coefficient on group A. Notably, the estimates for group a strengthen our belief in the importance of the surplus rule and centralization, especially since the coefficient is significant regardless of whether outliers are included or not.

[32] The prevalence of municipalities that combine result carry-over rules and centralization is not surprising from the point of view of our theoretical results. If the game approximates the municipal budget process and these institutions are effective, the central level of municipalities should be expected to employ both.

4.7 Discussion and conclusions

4.7.1 Causality and identification

In her review, Eslava (2011) mentions several shortcomings of the empirical research on political and institutional determinants of fiscal performance. In short, due to reverse causality and omitted variables, most studies fail to discriminate between competing explanations for observed phenomena. How does this study fare in these dimensions?

In our view, reverse causality from fiscal performance to institutions is not very plausible in our setting: budget institutions are unlikely to be reformed very often and we control for previous fiscal performance by several variables. We moreover argue that reverse causality mainly would serve to strengthen our case. In their search for ways to reduce deficits, high-deficit municipalities should be more likely to experiment with the institutional structure, while low-deficit municipalities have no reasons to rock the boat.[33] According to this argument, deficit-prone municipalities are over-represented in the pool of observations with "good" institutions, thus contributing *negatively* to the correlation between our conjecturally good institutions and fiscal performance. However, the opposite case can also been made; in particular, Fabrizio and Mody (2010) find that countries with higher deficits are less likely to reform their budget institutions, and argue that a war of attrition between different policy fields impedes institutional reforms. It can be noted from Table 4.8 below that the raw correlations between our institutions and the measures of previous fiscal performance – *mean net revenues 00-07* and *equity ratio* – are positive (though only significantly so for the surplus rules). In any case, since we control for exactly these variables in the analysis and institutions infrequently change, reverse causality is no prominent ground for concern.

The same control variables also decrease the risk of reverse causality from performance to *pd*, which otherwise may be suspected to reflect respondents' explanations for observed unsatisfying fiscal performance. But notably, even if the negative correlation between *pd* and fiscal performance is due to reverse causality, the correlation is evidently weaker in municipal-

[33] Alesina and Perotti (1999) argue that as institutions are costly to change, they have to be unsatisfactory to be changed. Alt and Lassen (2006) and de Haan et al. (1999) also note that fiscal crises often precede institutional reform.

ities that employ some of the budget institutions.

A more relevant concern is that the budget institutions may proxy for omitted factors that affect both fiscal performance and the institutional structure. Voter preferences over fiscal discipline is one often mentioned factor (e.g. Poterba, 1996; Krogstrup and Wälti, 2008; Eslava, 2011); for instance, fiscally responsible politicians may implement balanced budget rules to win the votes of fiscally conservative voters. There are three reasons to believe that voter preferences are sufficiently taken into account in our estimations: *first*, we control for voter preferences to some extent through the variable *share of right-wing parties*; *second*, voters' preferences for fiscal discipline are likely correlated to the equity ratio (a long-run measure) rather than to the yearly fiscal performance, and the equity ratio is included as a control variable; *third*, the details of governance captured by our institutions are unlikely to buy many votes; for instance, we suspect that few voters know whether their municipality employs result carry-over rules.[34]

The transparency of the budget process is another much-discussed factor (Alt and Lassen, 2006; Eslava, 2011). For politicians, a transparent budget process increases the risk of being punished at the polls due to fiscally irresponsible behaviour (Eslava, 2011). Budget transparency also relates to the institutional structure, specifically to centralization; more transparency may make the central level more adherent to local level budget proposals, as information about deviations from popular proposals becomes more widespread. However, as all budget documents must be made publicly available, there are national standards for municipal accounting and almost all municipalities publish their annual reports on their websites, there are reasons to believe that the between-municipality variation in transparency is low.

In our view, insufficient control for the central level's fiscal motivation is the key impediment to a causal interpretation of our results. It is conceivable – although far from indisputable – that conflicts of interests are more likely if the central level is relatively prudent. Fiscal motivation is moreover likely to be positively related to the achieved level of net revenues *and*

[34] With regard to other features of the political landscape, it can be noted that the within-country setting rules out any confounding of the influence of budget institutions with the influence of the electoral system (Eslava, 2011), and that the *herfindahl* variable accounts for confounding effects of political fragmentation (Hallerberg and von Hagen, 1999).

to the propensity to use budget institutions that are believed to promote fiscal discipline. For sure, it is difficult to explain why fiscally successful municipalities would bother to use these institutions if unmeasured fiscal motivation accounts for *all* of the positive association between institutions and performance – using ineffective rules seems rather pointless (especially if they are costly to implement). Moreover, the problem should be somewhat dampened by the inclusion of *equity ratio*, the *mean net revenues* 2000-2007 and *long-term budget* – all of which can be thought of as proxies for fiscal motivation (these proxies are indeed correlated to net revenues as well as the institutional structure, see Table 4.8). Nevertheless, we cannot rule out the possibility that we fail to exhaustively control for fiscal motivation, and we thus abstain from making causal claims.

In fact, the estimated significance of *manager risk* cannot be explained without making reference to central level fiscal motivation – for *manager risk* to be greater than zero, it is *necessary* that someone with the authority to replace managers is concerned about fiscal performance. The case for *manager risk* having an effect in itself is nevertheless rather strong; facing a conflict, the central level needs to apply some incentive-aligning measure in order to enforce the budget, and replacement of non-complying agents seems like a plausible choice.

This omitted variables problem is shared by most of the related literature. More generally, central level fiscal motivation is intrinsically connected to the enforceability of budget institutions. Thus, credible causal claims are more likely to be possible in settings with super-imposed budget institutions (e.g. fiscal rules imposed by the central government, as in Grembi et al. (2012) and Foremny (2011)), than in settings like ours where local governments themselves choose institutions.[35]

[35] Note that the few studies finding positive correlations between institutions and fiscal performance when using fixed effects, e.g. Fabrizio and Mody (2006), do not fully circumvent the omitted variables problem. Since politicians and party majorities change over time, it is quite likely that fiscal motivation is not fully captured by the fixed effects. Attempts to correct the problem using lags of the institutional structure as instrumental variables (Debrun et al., 2008; Hallerberg et al., 2007) rest on the assumption that fiscal motivation show no persistence at all. See Acemoglu (2005) for an enlightening discussion of the feasibility of IV in the analysis of institutions.

Table 4.8: Correlations for *pd*, institutions and proxies for central level fiscal motivation

Variables	*pd*	*keep surp.*	*keep def.*	*man. risk*	*cent123*	*mean...00-07*	*equity ratio*	*long-term b.*
pd	1.000							
keep surplus	0.120	1.000						
	(0.065)							
keep deficit	0.005	0.644	1.000					
	(0.936)	(0.000)						
manager risk	0.084	0.098	0.082	1.000				
	(0.197)	(0.118)	(0.193)					
cent123	-0.008	0.271	0.125	0.155	1.000			
	(0.900)	(0.000)	(0.049)	(0.014)				
mean...00-07	-0.012	0.215	0.200	0.094	0.078	1.000		
	(0.853)	(0.001)	(0.001)	(0.125)	(0.222)			
equity ratio	0.002	0.213	0.078	-0.037	0.003	0.473	1.000	
	(0.975)	(0.001)	(0.214)	(0.546)	(0.967)	(0.000)		
long-term budget	0.054	0.157	0.126	0.105	0.086	0.151	0.043	1.000
	(0.412)	(0.013)	(0.045)	(0.092)	(0.178)	(0.015)	(0.495)	

p-values in parentheses.

4.7.2 Concluding remarks

Our estimations underline the importance of controlling for conflicts of interest between central and local levels, as the relationship between budget institutions and fiscal performance depends on the degree of such conflicts. For instance, the positive correlation between a centralized budget process and the level of net revenues is concealed when we do not take into account our measure of conflicts of interest between central and local levels.

Apart from centralization, our analysis points out other specific institutions that may promote budget discipline. As one of few studies examining carry-over rules individually, rather than as part of an index, we find that total net revenues are higher if the local committees are allowed to carry over surpluses between fiscal years. The detected correlation is not entirely robust though and requires further investigation. A natural next step would be to relate the rule to the outcomes of actual local committees, for whom the rule is more likely to be exogenous. We also find that systematic carry-over of deficits correlates positively to fiscal performance, though only in municipalities that report small conflicts of interest. While the data does not allow us to conclude that the carry-over rules are also influential in the absence of a centralized budget process, it should be noted that municipalities combining carry-over rules with centralization have higher net revenues than municipalities employing centralization only. Furthermore, it is interesting to note that our findings run counter to the argument that carry-over rules undermine fiscal discipline, which has been put forward in studies of European countries and US states (e.g. Alt and Lowry, 1994; von Hagen and Harden, 1996; Fabrizio and Mody, 2006).

Net revenues are higher in municipalities where managers face a relatively high risk of dismissal as a consequence of budget deficits. Though this is an informal institution, its implementation goes hand in hand with a strong commitment to fiscal discipline at the central level. This result also has interesting policy implications for the national government. For instance, to alleviate soft budget constraint problems (e.g. Kornai, 1979), the government may condition grants and bailouts on a strict treatment of local managers in the face of repeated deficits.

Like most researchers in this area, we cannot make convincing causality claims due to the possible endogeneity of budget institutions. Nonetheless, the results clearly suggest that conflicts of interests, as well as centralization and incentive-aligning institutions, ought to be considered when examining the causes of variability in fiscal performance.

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4.A Proofs of propositions

Proposition 1: (i) $qp = 0 \Rightarrow x^e = x^*_L$ (where x^e is the equilibrium level of spending), (ii) $x^e \in [x^*_C, x^*_L]$, (iii) If $x^*_L = x^*_C$, then $x^e = x^*_L = x^*_C$.

Proof $qp = 0 \Rightarrow E(U_L) = u_L$ and by definition, x^*_L solves $\max u_L(x)$. This proves (i). To prove part (ii), we have to show that each of the proposed bounds is a feasible realized level of spending and that it is a bound. Suppose L chooses $x' > x^*_L$. Then, $u_L(x') - q(x')p > u_L(x^*_L) - q(x^*_L)p$, which implies that $p(q(x') - q(x^*_L)) < 0$ as $u_L(x') - u_L(x^*_L) < 0$ by definition. Consequently, $q(x') < q(x^*_L)$. In turn, this implies that $|b - x'| < |b - x^*_L|$ and that $b > x^*_L$. As $x' > x^*_L \geq x^*_C$ and thus $u_C(x') < u_C(x^*_L) \leq u_C(x^*_C)$, for C to choose $b > x^*_L$, it must hold that $u_C(x') > u_C(x^*_L) - h$ and therefore that $b_L > x^*_L$. But this is a contradiction to x^*_L being L's optimal level, as if h is large enough to make C set $b = b_L > x^*_L$, then $b'_L = b' = x^*_L$ is also feasible as well as preferred by L. Thus, x^*_L is the upper bound.

For the lower bound: suppose L chooses $x' < x^*_C$. Then, $q(x') < q(x^*_C)$ because $u_L(x^*_C) > u_L(x')$ as $x' < x^*_C \leq x^*_L$; which implies that $|b - x'| < |b - x^*_C| \Rightarrow b < x^*_C \Rightarrow u_C(x') > u_C(x^*_C) - h \Rightarrow b_L < x^*_C$. But this is a contradiction because if p is large enough to make L choose x', then L can set $b_L = x^*_C$ and guarantee a higher payoff, as C then sets $b = b_L$. Finally, it follows from the players' utility functions that x^*_C is feasible for $h = 0$ and a sufficiently large qp, such that $u_L(x^*_C) - u_L(x) > p(q(x^*_C) - q(x)) \forall x > x^*_C$. (iii) follows directly from (ii).■

Proposition 2: Suppose $x^*_L > x^*_C$ and $h > 0$. Then, (i) $x^e > x^*_C$ for any $qp \geq 0$; and (ii) for any $qp > 0$, $x^e \in (x^*_C, x^*_L)$ strictly increases in h.

Proof First note that when $qp = 0$, (i) holds by part (i) of proposition 1.

The following proves the proposition for $qp > 0$. At step 3, L chooses the x that solves $\frac{\partial u_L}{\partial x} = \frac{\partial q}{\partial x}p$, unless $\frac{\partial q}{\partial x}p > \frac{\partial u_L}{\partial x}$ at $x = b$, in which case L chooses $x = b$. As u_L, p and $q(x)$ are common knowledge, C knows the threshold level of spending x' for which L rather complies to a budget $b' = x'$ than chooses another level of spending:

$$u_L(x') \geq u_L(x) - q(x)p \quad \forall x \neq b' = x' \tag{4.2}$$

L complies to any budget $b > x'$; $b' = x'$ is simply the lowest budget that C can enforce. It is easy to see that Proposition 2*i)* holds in case $x' > x_C^*$. For the case where $x' \leq x_C^*$, we must show that $h > 0$ implies that C will propose a budget $b > x_C^*$ *despite* that C is able to enforce a smaller budget in this case.

As $h > 0$ and $u_C(x)$ is continuous, there are $x'' \in (x_C^*, x_L^*]$ such that

$$u_C(x'') > u_C(x_C^*) - h. \tag{4.3}$$

As $u_C(x)$ is common knowledge, L can identify the largest proposal $\overline{b_L} = \overline{x''}$ from which C will not deviate at step 2. By setting $b_L = \overline{b_L}$, L will make C set the final budget to $b'' = \overline{b_L} = \overline{x''} > x_C^*$. As $\overline{x''} > x_C^* \geq x'$, L will comply to this budget at Step 3. Thus, $x^e > x_C^*$ also in this case. Finally, (ii) follows from C's utility function: larger h implies that (4.3) holds for larger x''.■

Proposition 3: For any $h \geq 0$, (i) there is a lowest feasible level of spending $\underline{x} \in [x_C^*, x_L^*]$ and (ii) $x^e \in (\underline{x}, x_L^*]$ strictly decreases in q and p.

Proof By proposition 1, $x^e \in [x_C^*, x_L^*]$, so any feasible level of spending belongs to this interval. The concavity of u_C together with the inequality in equation (4.3) in the proof of proposition 2 shows that there is a lowest feasible level that depends on h. (ii) follows from $E(U_L)$ being decreasing in qp.■

Proposition 4: For given x_C^* and $h > 0$, (i) x^e is non-decreasing in x_L^*; and (ii) if the strength (p) and credibility (q) of the incentive-aligning institution is sufficiently weak, then x^e is strictly increasing in x_L^*.

Proof First, note that since $u_L(x)$ has a single optimum, equation (4.2) holds for larger x' if x_L^* increases. In words, the minimal budget to which

C can make L comply increases when L's bliss point moves further away from C's bliss point. Whether this affects the equilibrium level of spending x^e or not depends on whether x'' in equation (4.3), the maximal budget proposal b_L from which C will not deviate, is larger than x' or not. As long as $x'' \geq x'$, the logic behind the optimality of $b_L = x'', b = x''$, and $x = x''$ explained in the proof of proposition 2 holds. Thus, the equilibrium level of spending is neither increasing nor decreasing in x_L^* when $x'' \geq x'$. To prove (ii), note that $x' > x''$ implies that L's bargaining power is to weak to make C set a larger budget than x'. Also, for all $b < x'$, L would choose $x > x'$ by equation (4.2), which is worse for C. Thus, it is optimal for L propose $b_L = x'$ and for C to choose $b = x'$ in step 2, as then no cost h is incurred for C and L does not get punished for choosing $x = x'$ (this assumes that $x' \geq x_C^*$, which is true because $x'' > x_C^*$ when $h > 0$ as shown in proposition 2). Thus, $x' > x'' \Rightarrow x^e = x'$, which is increasing in x_L^* by equation (4.2).■

4.B Analysis of response rates

Many municipalities replied to some, but not all, of the survey questions. Table 4.9 summarizes the response rates for the central survey questions.[36] Regarding the carry-over rules and centralization, we do not consider the levels of non-response to be a problem. For *manager risk*, *committee risk*, and *pd*, which have lower response rates, we perform a series of Wilcoxon rank sum tests with respect to the independent variables in the baseline estimations. The rank sum tests compare those that responded to the specific survey question to those that did not respond to this question, but have responded to other questions. Applying 10 percent as the significance level yields the following results: 1) There are no significant differences between respondents and non-respondents regarding the question that we base the *pd* variable on; 2) Non-respondents to *committee risk* have lower financial costs and higher equity ratios; and 3) Non-respondents to *manager risk* have lower financial costs, higher equity ratios, and are over-represented in the municipalities categorized as suburban, dominated by commuters or by manufacturing industries.

[36] The denominator is 265, i.e. the number of municipalities who did respond to at least one question. That is, these figures overestimate the "real" response rates. However, as we already know that the drop-outs differ from the respondents, we leave the out of the comparison so the table gives the relevant rates.

Table 4.9: Response rates

Variable	Response rate
committee risk	66%
manager risk	72%
pd	90%
centralization	95%
keep surplus	96%
keep deficit	97%

The similarity between respondents and non-respondents with regard to *pd* is reassuring.[37] For *manager risk* and *committee risk*, we include dummy variables for non-response to these questions to increase precision. Reassuringly, leaving out these dummies does not substantially affect the results. The only noteworthy difference to the baseline estimation is that the incremental effect of *cent123* × *pd* is no longer significant in the specification including outliers. The sign and magnitude of the coefficients are still similar though, so we interpret this as indicative of low precision (results available upon request).

[37] It may be noted that *pd* is negatively associated to population in a multiple regression setting, see Ellegård (2013).

4.C Control variable estimates

In table 4.10, we show the coefficients for the control variables included in the baseline estimation shown in table 4.6.

Table 4.10: Baseline results, control variables

	(1)	(2)	(3)	(4)
		Ex. outliers		Ex. outliers
total revenues	0.0114	0.00622	0.0100	0.00503
	(0.0122)	(0.0121)	(0.0120)	(0.0121)
change in revenues	0.171***	0.100***	0.174***	0.100***
	(0.0501)	(0.0303)	(0.0499)	(0.0305)
fixed asset revenues	0.0815***	-0.0152	0.0818***	-0.0177
	(0.0233)	(0.0112)	(0.0237)	(0.0115)
financial costs	0.229*	0.177	0.250*	0.197
	(0.136)	(0.134)	(0.141)	(0.138)
equity ratio	0.00546	0.00771	0.00703	0.00972
	(0.00584)	(0.00564)	(0.00611)	(0.00589)
mean fiscal perf. 00-07	0.103	0.152	0.0677	0.114
	(0.142)	(0.129)	(0.146)	(0.127)
share right-wing	-0.00506	-0.00182	-0.00442	-0.000410
	(0.00686)	(0.00584)	(0.00709)	(0.00610)
herfindahl	-1.554	-1.649	-1.276	-1.227
	(1.457)	(1.485)	(1.484)	(1.506)
long-term budget	0.312**	0.278**	0.290**	0.251*
	(0.139)	(0.128)	(0.138)	(0.128)
log(*population*)	0.0164	0.0125	0.0361	0.0213
	(0.0988)	(0.0888)	(0.0994)	(0.0908)
share 20-79	0.0758*	0.0847**	0.0724	0.0857**
	(0.0435)	(0.0390)	(0.0448)	(0.0411)
cities	-0.271	-0.135	-0.319	-0.133
	(0.284)	(0.251)	(0.280)	(0.248)
rural	0.506*	0.417	0.500*	0.384
	(0.272)	(0.264)	(0.259)	(0.251)
suburb/commuter/manufactural	0.519***	0.534***	0.551***	0.564***
	(0.170)	(0.144)	(0.177)	(0.153)
Observations	225	222	225	222
R^2	0.567	0.256	0.574	0.275
F	4.300	3.710	3.661	3.294

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

4.D Robustness checks

In Table 4.11, we examine the robustness of the results presented in column (3) and (4) of Table 4.6. Columns (1)-(5) of Table 4.11 show estimates of Equation (4.1) from the full sample (including outliers), but the results excluding outliers are commented on in the text.[38]

In column (1) we change the dependent variable to per capita costs of services. Note that the expected signs are reversed, e.g. that negative coefficients imply lower costs in municipalities that employ a certain institution. The results are therefore qualitatively similar to the baseline estimations (the incremental effect $\beta_0^c + \beta_1^c$ even becomes more significant). However, the cost regression is sensitive to changes in the control variables; in particular *total revenues* explains a very large share of the variation in costs (the high R^2 value mainly derives from this variable).

A second concern is that our large set of control variables may influence the estimates. In column (2), we show that the results are similar when we only control for *relative change in revenues*.[39] This holds when outliers are excluded as well, with the exception that the incremental effect of *cent123* $\times$ *pd* (i.e. $\beta_0^c + \beta_1^c$) becomes insignificant (though with a p-value of 0.13). The estimates for the institutions are moreover robust to the inclusion of only controls that are significant at the 10 percent level (results not shown).

We have also tested specifications where the following variables are added one at a time to our baseline control variables (results available on request): the number of committees, indicators for whether the local committees are chaired by members of the executive committee; an indicator for whether the executive director (highest ranked civil servant) is the manager of local administration managers (instead of local committees doing the hiring and firing of managers); indicators for whether the political majority changed from left to right or vice versa in the election of 2006; an indicator for having no shift of political majority in the three elections 1998, 2002, and 2006; and an indicator for whether the municipality reports that it may not put forward a balanced budget in a recession. With the exception of the

[38] Control variables are included in the regressions but are suppressed in the table. For the first-difference estimation, the control variables consist of all time-varying controls in the baseline cross-sectional estimation.

[39] The results for *manager risk* and *keep deficit* also remain in a specification without *any* controls. The results for *keep surplus* and *cent123* are qualitatively similar but lose significance when all controls are removed.

indicator for having had no shift of political majority (which is positive and significant in the sample including outliers) none of these variables come out significant on conventional levels. More importantly, their inclusion leaves the institutional variables largely unchanged.

In column (3), we exclude *total revenues*, *changes in net revenues*, *fixed asset revenues*, and *financial costs*, and instead include the tax base size and the level of government grants, which are exogenous in the short run. The results are qualitatively similar, but the incremental effect of *keep surplus* × *pd* and *cent123* × *pd* are no longer significant in any sample.

In column (4), we examine a less broad classification of centralization by separating category 3 from categories 1 and 2. First of all, we note that there are no important implications for the other four institutional variables when we change the categorization. However, although none of the centralization coefficients are significant, the correlation between centralization and fiscal performance is quantitatively different for category 3 than for category 12, and the magnitude depends on *pd.* For the most centralized category (*cent12*), the correlation is positive regardless of the value of *pd* and slightly larger for those where *pd*=1. For category 3, the correlation is negative if *pd*=0 but positive if *pd*=1. A Wald test of equality of the coefficients on *cent12* and *cent3* suggests that the correlations differ between the categories (p=0.0398), although neither coefficient is distinguishable from zero. But the interesting question according to our framework is whether the influence of centralization is positive when there are conflicts of interest. Looking at the municipalities that report *pd*=1, there is notably no significant difference (p=0.438) between the incremental effect of centralization for category 12 ($\beta_0^{cent12} + \beta_1^{cent12} = 0.405$) and for category 3 ($\beta_0^{cent3} + \beta_1^{cent3} = 0.210$). When excluding the three outliers, the estimated incremental effects in the two categories are even more similar. Thus, as the incremental effects are indistinguishable from zero when *pd*=0 and similar for category 12 and 3 when *pd*=1, it seems reasonable to merge the two categories as done in the baseline.

Column (5) contains results where we control for *budgeted net revenues.* The coefficient for *keep deficit* becomes smaller and is no longer significant (although almost so in the sample excluding outliers, p-value = 0.116). The results for the other institutions are qualitatively similar to the baseline. The coefficient on *budgeted net revenues* is positive (250-360 SEK per capita) and significant at the 5 percent level in the sample excluding outliers, and

Table 4.11: Robustness estimations

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
keep surplus	-0.324	0.000485	0.136	0.351	0.383	0.370
	(0.327)	(0.249)	(0.403)	(0.291)	(0.265)	(0.298)
pd×*keep surplus*	-0.320	0.441	0.151	0.224	0.216	
	(0.397)	(0.346)	(0.410)	(0.357)	(0.324)	
keep deficit	-0.307	0.683**	0.772**	0.449	0.294	0.496*
	(0.344)	(0.273)	(0.344)	(0.287)	(0.283)	(0.287)
pd×*keep deficit*	0.159	-0.469	-0.682	-0.350	-0.286	
	(0.434)	(0.383)	(0.428)	(0.377)	(0.366)	
manager risk	-0.218	0.515	0.644	0.237	0.294	
	(0.541)	(0.398)	(0.420)	(0.377)	(0.386)	
pd×*manager risk*	-0.403	0.0725	-0.152	0.342	0.331	
	(0.566)	(0.414)	(0.438)	(0.397)	(0.397)	
committee risk	0.241	-0.0322	-0.0338	-0.0168	-0.0625	
	(0.549)	(0.417)	(0.401)	(0.375)	(0.400)	
pd×*committee risk*	-0.226	0.0508	-0.0475	0.0111	0.0209	
	(0.569)	(0.433)	(0.397)	(0.397)	(0.411)	
cent123	-0.227	0.109	0.117		0.0520	
	(0.295)	(0.225)	(0.264)		(0.232)	
pd×*cent123*	-0.328	0.288	0.216		0.215	
	(0.421)	(0.317)	(0.364)		(0.321)	
pd	0.758**	-0.445	-0.227	-0.473	-0.517*	
	(0.355)	(0.292)	(0.408)	(0.293)	(0.288)	
cent12				0.364		
				(0.291)		
pd×*cent12*				0.0403		
				(0.411)		
cent3				-0.124		
				(0.250)		
pd×*cent3*				0.333		
				(0.351)		
budgeted net rev.					0.255	
					(0.156)	
Constant	7.668*	0.0668	-8.760*	-6.360*	-4.651	-17.19
	(4.447)	(0.330)	(4.420)	(3.627)	(3.490)	(26.12)
Incremental effect of institution j for municipalities where $pd = 1$						
$\beta_0^{ks} + \beta_1^{ks}$	-0.644**	0.442*	0.286	0.575**	0.600**	
$\beta_0^{kd} + \beta_1^{kd}$	-0.148	0.213	0.090	0.099	0.007	
$\beta_0^{mr} + \beta_1^{mr}$	-0.621**	0.587**	0.492**	0.579**	0.625**	
$\beta_0^{cr} + \beta_1^{cr}$	0.014	0.019	-0.081	-0.006	-0.042	
$\beta_0^{c} + \beta_1^{c}$	-0.555*	0.397*	0.334		0.267	
Controls	Baseline	Reduced set	Reduced set	Baseline	Baseline	Time variant
Observations	225	227	225	225	219	456
R^2	0.974	0.397	0.452	0.584	0.604	0.521
F	365.9	2.520	3.666	3.673	4.667	14.84

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Column (1) use per capita costs of services as dependent variable. Column (2) include only *relative changes in revenues* as control variable. Column (3) include *tax base* and *government grants* while excluding *total revenues*, *relative changes in net revenues*, *fixed asset revenues*, and *financial costs*. Column (5) adds *budgeted net revenues* to the baseline control variables. Column (6) show a first-difference specification on the years 2004 and 2010, including *keep surplus*, *keep deficit*, and the time-variant baseline control variables.

close to significant (p-value 0.104) in the sample including outliers.

As the use of carry-over rules was surveyed both in 2004 and 2010, we also run a regression in differences (column (6)). The main virtue of this first-difference (FD) model is that it controls for time-invariant omitted factors at the municipality level, but for several reasons its usefulness as a robustness check is limited. First, as we lack information on *pd* for 2004, insignificant coefficients may result from the failure to model the need for incentive-alignment rather than from ineffectiveness of the rules. Second, as we cannot control for either *manager risk* or centralization in the FD regression, these institutions are omitted. Third, and perhaps most important, identification of the coefficients comes only from those who switched rules between 2004 and 2010. But there must be a reason why rules are changed, and this reason is likely related to changes in the importance assigned to fiscal discipline or in the central-local relation – thus, the municipality fixed effects, that only control for time-invariant factors, do not wipe out the confounding heterogeneity. Finally, it is uncertain whether it is appropriate to model the effect of introducing the rules as quantitatively similar to the effect of abolishing the rules, as implied by the FD model. With those caveats in mind, we interpret the FD model in (6) as follows. First, we cannot determine whether the insignificant (but positive) coefficient on *keep surplus* is due to lack of relevance (lack of conflicts of interests) or due to the rule being ineffective. The FD model thus does not overturn our previous tentative conclusion that *keep surplus* may be effective. Second, the positive and significant coefficient on *keep deficit* cannot conclusively be interpreted as a causal effect, as there may well be unobserved changes related both to the change in rules and to the change in fiscal performance.

Except for the FD estimation, all estimations use the sample from 2010. Running regressions on a pooled sample over the period 2009-2011 or using the mean of the variables over the same period yields results that are in general qualitatively similar to the baseline, while the results are less stable for the single years 2009 and 2011. As we do not know whether the institutions have changed between the years, we think that these specifications are less reliable and refrain from showing them (they are available on request).

4.E Survey questions

The survey was constructed with the help of the electronic survey program Easyresearch. A link to the survey was sent to the official e-mail address of every municipality with a note asking for the survey to be forwarded to the chief financial officer/budget manager. Note that several of the questions below were not directly used in the econometric analysis in the paper. For completeness, we have included all questions here. The original survey in Swedish and (anonymous) data over the municipalities' answers are available upon request. All questions included a 'Do not know'-alternative, which we have omitted below for brevity.

1. *When does the council decide on the overall budget for the coming fiscal year?*

- In spring before the fiscal year
- In fall before the fiscal year

2. *Which of the following alternatives bear most resemblance to the beginning of the budget process in your municipality? (The concept of committees is intended to include all governing bodies that consist of politically elected representatives and are placed organizationally directly under the municipal council. The executive committee and as well as other boards with responsibilities for tax- and fees financed activities are thus included in the concept).*

- The budget process begins with a budget proposal from each committee regarding their own activities
- The budget process begins with a budget proposal from the executive committee for all committees

[Question 3 was only posed to municipalities that indicated the second or the 'Do not know' alternative in Question 2.]

3. *Which of the following alternatives bear most resemblance to the continued participation of the committees in the budget process?*

- The committees have relatively large possibilities to propose changes to the executive committees budget proposal
- The committees have limited possibilities to propose changes to the executive committees budget proposal
- The committees have no possibilities to propose changes to the executive committees budget proposal

4. *Indicate the alternative below that best describe how the municipality allocates its resources:*

- The resource allocation is to a large extent governed by centrally established unit costs for different services (SEK/student etc) and demographic variables

- The resource allocation is partly governed by centrally established unit costs for different services (SEK/student etc) and demographic variables
- The resource allocation is to a small extent or not at all governed by centrally established unit costs for different services (SEK/student etc) and demographic variables

5. *How are forecasts of tax revenues produced in the municipality?*

- The municipality uses the Swedish Association of Local Authorities and Regions' forecasts
- The municipality uses the Swedish Association of Local Authorities and Regions' forecasts as a point of departure, but produces an independent assessment of the tax revenues as well
- The municipality does not use the Swedish Association of Local Authorities and Regions' forecasts, but produces an independent assessment of the tax revenues

[Question 6 was only posed to municipalities that indicated the second, third and the 'Do not know' alternative in Question 5.]

6. *Indicate the alternative below that best describe the municipality's independent assessment of the tax revenues:*

- The municipality's independent assessment is in general higher than the Swedish Association of Local Authorities and Regions'
- There is in general no or a small difference between the municipality's independent assessment and the Swedish Association of Local Authorities and Regions'
- The municipality's independent assessment is in general lower than the Swedish Association of Local Authorities and Regions'

7. *Which alternative does best describe the situation in your municipality regarding long-term budgets?*

- Long-term budgets are lacking entirely
- Long-term budgets have the character of a pure forecast
- Long-term budgets are indicative decisions
- Long-term budgets constitute important political commitments

8. *How often during the fiscal year are follow-ups of the overall financial outcome performed by the executive committee?*

- 8-12 times/year
- 5-7 times/year
- 3-4 times/year
- 1-2 times/year

9. *Are the committee chairmen members of the executive committee?*
(Yes, all/Yes, some/No)

10. *How many of the last 5 years has one/some committees been given extra appropriations during the year, over and above their budgeted resource allocation?*
(0/1/2/3/4/5)

11. *How are budget surpluses handled?*
    - The committees can carry-over the surplus to the next fiscal year
    - The committees can carry-over some of the surplus to the next fiscal year
    - The committees have no possibility to carry-over the surplus to the next fiscal year

[Question 12 was only posed to municipalities that indicated the first alternative in Question 11.]

12. *This question concerns only the municipality's, in terms of gross costs, largest committee. Could the committee, in violation of the principle, be deprived of some of the surplus if it amounted to 3-5% of total resources allocated to the committee?*
(Yes/No)

[Question 13 was only posed to municipalities that indicated the first alternative in Question 12.]

13. *This question concerns only the municipality's, in terms of gross costs, largest committee. Could the committee, in violation of the principle, be deprived of some of the surplus if it amounted to 1-2% of total resources allocated to the committee?*
(Yes/No)

[Question 14 was only posed to municipalities that indicated the second or the 'Do not know' alternative in Question 12.]

14. *This question concerns only the municipality's, in terms of gross costs, largest committee. Could the committee, in violation of the principle, be deprived of some of the surplus if it amounted to 6-10% of total resources allocated to the committee?*
(Yes/No)

15. *Does your municipality have a principle of forcing committees to carry over budget deficits from one year to another?*
(Yes/No)

[Question 16 was only posed to municipalities that indicated the first alternative in Question 15.]

16. *This question concerns only the municipality's, in terms of gross costs, largest committee. Could the committee, in violation of the principle, be remitted some of the deficit if it amounted to 3-5% of total resources allocated to the committee?*
(Yes/No)

[Question 17 was only posed to municipalities that indicated the first alternative in Question 16.]

17. *This question concerns only the municipality's, in terms of gross costs, largest committee. Could the committee, in violation of the principle, be remitted some of the deficit if it amounted to 1-2% of total resources allocated to the committee?*

(Yes/No)

[Question 18 was only posed to municipalities that indicated the second or the 'Do not know' alternative in Question 16.]

18. *This question concerns only the municipality's, in terms of gross costs, largest committee.Could the committee, in violation of the principle, be remitted some of the deficit if it amounted to 6-10% of total resources allocated to the committee?*
(Yes/No)

19. *This question concerns only to the municipality's, in terms of gross costs, largest committee. Consider a scenario where the committee for some years has run budget deficits, which are not caused by incidental circumstances. In this situation, which of the alternatives below best describe your municipality?*

- The deficits would possibly be a sufficient reason to replace the leadership of the committee
- The deficits would possibly be a contributing but not a sufficient reason to replace the leadership of the committee
- The deficits would not be a reason to replace the leadership of the committee

20. *Is the chief executive officer in your municipality the head over the managers for the respective administrations?*
(Yes/No)

21. *Does it occur in your municipality that managers of the administrations receive some form of bonus if the administration runs surpluses?*
(Yes/No)

22. *This question concerns only the municipality's, in terms of gross costs, largest administration. Consider a scenario where the administration for some years has run budget deficits, which are not caused by incidental circumstances. In this situation, which of the alternatives below best describe your municipality?*

- The deficits would possibly be a sufficient reason to replace the to replace the manager of the administration
- The deficits would possibly be a contributing but not a sufficient reason to replace the to replace the manager of the administration
- The deficits would not be a reason to replace the to replace the manager of the administration

[Question 23 was only posed to municipalities that indicated the second or the 'Do not know' alternative in Question 22.]

23. *This question concerns only to the municipality's, in terms of gross costs, largest administration. Consider a similar scenario as in the previous question: the administration has for some years run budget deficits not due to incidental circumstances. Furthermore, the administration has to a large extent planned and carried out measures to come to terms with the deficit, but these measures have not succeeded in reducing*

the deficit. Would this situation be a sufficient reason to replace the manager of the administration?
(Yes/No)

[Question 24 was only posed to municipalities that indicated the second or the 'Do not know' alternative in Question 23.]

24. *This question concerns only the municipality's, in terms of gross costs, largest administration. Consider a similar scenario as in the previous question: the administration has for some years run budget deficits not due to incidental circumstances. Furthermore, the administration has to a small extent planned and carried out measures to come to terms with the deficit, but these measures have not succeeded in reducing the deficit. Would this situation be a sufficient reason to replace the manager of the administration?*
(Yes/No)

25. *Suppose that the forecasted revenues in your municipality decreases due to a considerable recession. Is it possible that such a scenario would imply that the municipal council would decide on an underbalanced budget?*
(Yes/No)

[Question 26 was only posed to municipalities that indicated the first or the 'Do not know' alternative in Question 25.]

26. *Would the municipal council decide on an underbalanced budget if the forecasted revenues decreased by 3-5%?*
(Yes/No)

[Question 27 was only posed to municipalities that indicated the first alternative in Question 26.]

27. *Would the municipal council decide on an underbalanced budget if the forecasted revenues decreased by 1-2%?*
(Yes/No)

[Question 28 was only posed to municipalities that indicated the second or the 'Do not know' alternative in Question 26.]

28. *Would the municipal council decide on an underbalanced budget if the forecasted revenues decreased by 6-10%?*
(Yes/No)

29. *Which alternative best describe your municipality?*

- The executive committee and the municipal council are more concerned about fiscal discipline than the local committees
- The executive committee, the municipal council and the local committees do not

differ significantly in their concerns about fiscal discipline

- The local committees more concerned about fiscal discipline than the municipal council and the executive committee

Chapter 5

Assist or desist? Conditional bailouts and fiscal discipline in local governments

with Lina Maria Ellegård

5.1 Introduction

Whenever a central government faces a sub-unit in financial distress, the unpleasant question that arises is whether to assist the unit or not. On the one hand, neglecting to bail out the unit may lead to default or bankruptcy, which could be very costly both economically and politically. On the other hand, bailouts may create problems of soft budget constraints: noting that the central government steps in in times of trouble, sub-units may come to expect that bailouts will be available when needed. Thereby, their incentive for fiscal discipline is eroded (Kornai, 1979; Wildasin, 1997; Goodspeed, 2002; Inman, 2003). The current situation in regions and countries within the EMU provides a clear illustration of the dilemma, but the empirical relevance of the problem is also backed up by more systematic evidence from studies of fiscally decentralized countries.[1]

A possible way out of the dilemma may be to grant the sub-unit assistance, but condition payment on actions that lay the ground for fiscal discipline. We investigate a case in which the Swedish central government provided conditional bailouts to 36 municipalities in fiscal distress.[2] The

[1]See Rodden (2002); Rodden et al. (2003); Plekhanov (2006); Bordignon and Turati (2009); Pettersson-Lidbom (2010); Baskaran (2012); Fink and Stratmann (2011), and Lusinyan and Eyraud (2011). Kornai et al. (2003) survey the theoretical literature and provides further empirical examples.

[2]The transfers were not last minute rescue attempts in the face of imminent defaults. We

36 municipalities were granted extra funds, but payment was contingent on them first cutting certain costs and achieving budgetary balance. At the closure of the program, it was evident that there was a short-term effect on fiscal performance, as all admitted municipalities managed to meet the conditions. But the more interesting question is whether this newly acquired fiscal discipline was retained after the program, when there was no longer an explicit incentive for such behavior. To address this question, we analyze the evolution of per capita costs as well as revenues net of costs (henceforth referred to as *net revenues*) during the decade after the launch of the program.

To draw firm conclusions about the program effect, we would ideally have wanted municipalities to be randomly assigned to the program. However, non-random assignment is an inescapable feature of bailout programs since, by design, such programs are directed to a selected sample of units, namely those in fiscal distress. In the current context, this is illustrated by the fact that all 290 municipalities had the option to apply to the program, but only 36 of the 59 that chose to apply were judged to be eligible. The experience of being denied participation in the program is a kind of treatment in its own, and we analyze the fiscal performance also of the rejected municipalities.[3]

Instrumental variable estimation would overcome the selection problem in principle. As the program was explicitly directed to municipalities with poor fiscal performance, it is difficult to envision variables that are correlated to program status, but uncorrelated to our outcome variables, and even harder to come up with separate instruments for admission and rejection. Instead, we use the synthetic control method for case studies, developed in Abadie and Gardeazabal (2003) and Abadie et al. (2010), to identify appropriate comparison units for each of the municipalities affected by the program. This algorithm constructs a synthetic control municipality for each affected municipality as a weighted average of untreated municipalities. The weights are chosen to make the synthetic control match the actual municipality in terms of observable pre-program characteristics, including the pre-program development of costs.

use the term "bailout" to comply with the terminology in the literature on soft budget constraints, where the term is also used to denote discretionary transfers to cover deficits (see e.g. Fink and Stratmann (2011, p. 367)).

[3] As most municipalities do not end up in fiscal distress, we are interested in the (conditional) average treatment effect on the treated for both groups (Imbens and Wooldridge, 2009).

Two assumptions are needed to interpret differences in the fiscal performance of actual and synthetic municipalities as causal effects of the program. *First*, program participation must be independent of potential outcomes, conditional on covariates (Imbens and Wooldridge, 2009).[4] That is, a causal interpretation assumes that all post-program differences derive from the program, rather than from differences in unobservable characteristics, in the reaction to post-program shocks, or in the set of shocks experienced. To increase the credibility of this assumption, we estimate fixed effects regressions on the samples of admitted and rejected municipalities and their synthetic controls. Thereby, we explicitly control for time-invariant unobservables and can include covariates to capture post-program changes in observables.[5]

The *second* assumption is the Stable-Unit-Treatment-Value assumption (SUTVA) (e.g. Rubin, 2005); that is, the comparison units should be unaffected by the existence of the program. In this regard, we are most concerned about the municipalities that are neighbours to the admitted. Pettersson-Lidbom (2010) used the frequency of deficit grants to neighbouring municipalities as an instrumental variable for expectations of future grants, and showed that such expectations led to higher debt levels during an earlier regime of discretionary transfers in Sweden. However, neighbouring municipalities are also likely to be similar to the treated municipalities in many important dimensions and to experience the same shocks. In a nutshell, the comparison group that would make the first assumption most likely to hold is exactly the group for which the second assumption is most questionable. We therefore run the synthetic control algorithm twice, once including and once excluding neighbours in the "donor pool" of possible comparison units.

We use per capita costs of services as our main measure of fiscal performance and let the synthetic control algorithm search for comparison units based on this variable. For the rejected municipalities, costs appear to be unaffected by the program regardless of whether neighbours are included in the donor pool or not. For the admitted municipalities, we find permanent cost reductions on average when neighbours are allowed to contribute to

[4]The assumption is often called "unconfoundedness" in the program evaluation literature. Another assumption needed for selection on observables to work is that there should be overlap between the distribution of covariates for treated and untreated units (Imbens and Wooldridge, 2009). We see the synthetic control method as a way to increase the chances that this assumption holds as well.

[5]See e.g. Fitzpatrick (2008); Hudson (2010) for similar estimation strategies.

the synthetic controls, whereas the estimated average effects are insignificant when neighbours are excluded from the donor pool. An examination of the actual-synthetic cost difference for each municipality further reveals that the average cost reduction found when neighbours are included in the donor pool is driven by a third of the admitted municipalities; the remaining two-thirds show no divergence from their synthetic control. A tentative exploration of this heterogeneity suggests that the incumbent politicians in the former group were initially more certain to be re-elected; they could thus afford to hold back costs without fear of losing the next election. The latter group on the other hand increased their revenues more, which indicates that they chose another strategy to deal with their fiscal problems.

In accordance with these findings, we find positive, significant and large average effects on the net revenues of admitted municipalities for many post-program years when estimating similar fixed effects specifications on the sample of actual and synthetic municipalities. For net revenues, we find positive effects regardless of whether neighbours are included in the sample or not. For the rejected municipalities, the estimates for net revenues are often positive but less often significant.

Taken together, our results indicate that the program has not undermined the fiscal discipline of municipalities participating in the program; it may even have had a beneficial impact. The two identifying assumptions are basically untestable though; we cannot rule out that the results reflect differences in (time-variant) unobserved motivation for improving fiscal discipline that is unrelated to the participation in the program. However, the fact that the turn towards more fiscal discipline coincides with the initiation of the program suggests that the experience of being in program *had* a beneficial effect *per se*.

To the best of our knowledge, this is the first attempt to investigate the impact of conditional bailouts on the fiscal performance of local governments. Our results stand in contrast to findings from settings with unconditional bailouts (see footnote 1), which suggests that conditions may be key to dampening the soft-budget effect of central government bailouts.

The rest of the chapter is structured as follows: section 5.2 outlines the institutional background. Section 5.3 presents the data and discusses the choice of fiscal performance measure. Section 5.4 describes our estimation strategy and introduces the synthetic control method, while section 5.5 contains the estimation results. Section 5.6 explores potential sources of the heterogeneity in program effects. Section 5.7 concludes.

5.2 Institutional background

The 290 Swedish municipalities are responsible for the financing and provision of several important public services such as primary to upper secondary schooling, and elderly care. Municipal expenditures accounted for approximately 14 percent of Swedish GDP in 2010, almost half of the public sector's total expenditures for final consumption and investments (Statistics Sweden, 2012b). Revenues mainly derive from a proportional income tax, with the tax rate set freely by each municipality. On average, about 12 percent of revenues come from a rule-based equalization system.[6] Central government *discretionary* transfers, which are more likely to lead to soft budget constraint problems (Rodden and Eskeland, 2003), have varied in prevalence over time. Before 1993, municipalities could apply for unconditional grants to cover deficits each year. Since a major reform of the grant system in 1993, the central government has been considerably more restrictive with discretionary transfers. Still, it is unlikely that municipalities view their budget constraints as binding under all circumstances. Equal access to public services in the whole country is an important objective for the central government and municipalities are prohibited by law to default on debt; thus, the national government would likely step in if a municipality was threatened by insolvency (Dahlberg and von Hagen, 2004).

The program under study was announced in August 1999, in connection to the approaching implementation of the Balanced Budget Act (which would come into effect in the year 2000). The act states that municipalities have to attain budgetary balance each year, and if deficits occur, they have to be recovered within the subsequent three years.[7] However, in 1999 the central government noted that quite a few municipalities would have substantial problems with achieving budgetary balance on time, due to structural factors perceived to be beyond the control of local politicians.

[6] In 2010, revenue from income taxes made up approximately 65 percent of total municipal revenues, fees 21 percent, and government grants from the equalization system 12 percent (Statistics Sweden, 2010).

[7] Nevertheless, the law allows for exceptions, for example if the deficit is caused by unconverted losses in stocks and bonds, or if the municipality has previously amassed large amounts of wealth. It is in practice not enforced by any sanctions either (Swedish Government, 2004).

In the fall of 1999, the government therefore decided to install a committee, *Kommundelegationen*, to investigate whether some municipalities should be granted financial assistance to mitigate their problems. To be considered for the program, municipalities had to apply in November 1999 at the latest; in all, 59 municipalities applied.[8]

Compared to the municipalities that did not apply, the applicants had higher costs, higher debt and a lower equity ratio in 1998, and had witnessed a larger population decline between 1994-1998 (see Appendix 5.A, tables 5.A.1-5.A.3). They and their neighbours moreover received more discretionary transfers before 1993; they may thus have had higher expectations about receiving the grant (Pettersson-Lidbom, 2010).

During the spring of year 2000, the delegation held an initial meeting with each applicant and discussed its situation. According to the official report, the delegation used the following criteria to decide whether each applicant should be considered further or not (SOU, 2003):

- Structural problems, e.g. demographic changes and low employment rates.
- Projected deficits over the coming three years.
- Weak balance sheet, in particular a high level of debt.
- Limited possibilities of increasing revenues.

The municipalities whose applications were not rejected were asked to come up with a proposal of cost reductions. These proposals formed the basis for a discussion of the necessary conditions to be fulfilled in order to receive the grant. The resulting agreements were approved by the respective municipal councils (SOU, 2003).

In early October 2000, the government took the formal decision about admission, in accordance with the delegation's proposal (SOU, 2003, Appendix 1). Surprisingly, given the above criteria, there are no significant differences between the admitted and the rejected with regards to the cost structure, debt level and demographic changes (Tables 5.A.1 and 5.A.2). This suggests that projected future revenues was the most important of

[8]Two more municipalities initially applied but withdrew their application before the government made its decision. These two are not included in the rejected group in our specifications.

the selection criteria and the official motivations for rejection support this interpretation (Swedish Ministry of Finance, 2000).[9]

The size of the grant was non-negligible; on average, it amounted to four percent of the program municipalities' cost level in the year 2000. The grant was supposed to be set as a fixed (i.e., same for all admitted municipalities) share of the cost reductions in the agreement; however, it is not entirely clear from the official documentation whether this practice was strictly applied (SOU, 2003).

To receive the full grant, the 36 admitted municipalities had to meet two conditions by the end of year 2002. *First*, they would have to cut the costs specified in their agreement with the government. *Second*, they would have to achieve budgetary balance. According to the committee's report to the government, the actions of the municipalities were continuously monitored during the program period (SOU, 2003).[10]

In 2002, the admitted municipalities received 25 percent of the grant given that they could show that they had started to cut costs in 2001. Ten municipalities succeeded to fulfil all conditions in their agreements already in 2001, and therefore received the whole grant in 2002. Of the remaining 26, all but two municipalities fulfilled the program conditions in 2002 and thus received the remaining part of their grants in 2003. The last two received the remaining part of their grants in 2004, after having achieved budgetary balance in 2003.

Though all 36 sooner or later fulfilled the conditions, a follow-up study from 2004 points at relatively large cost increases in the admitted municipalities between 2002 and 2003 (Siverbo, 2004) (i.e. after most of them had received the whole grant). Interviews with representatives from some of the admitted municipalities moreover suggest that the program succeeded to make a substantial change in only some municipalities, while other indicated that they had not succeeded to make the turn towards fiscal responsibility (Siverbo, 2004; SOU, 2003).

[9] The three committee members were politicians; two were social democrats and the third was from the Centre party. As Dahlberg and Rattsø (2010) note, political factors such as key voter districts or party concerns do not seem to explain selection into the program.

[10] Whether the central government would actually be tough and apply the conditions, or give in and pay the whole sum anyway, was uncertain at the beginning of the program. For example, an audit report from 2000 raises concerns about the central government's toughness and encourages the government to terminate the program (Swedish National Audit Office, 2000, p. 9).

A related program complicates the story somewhat. In several of the Swedish municipalities, the real estate boom-and-bust in the beginning of the 1990s left the publicly owned housing companies highly indebted and with a large over-supply of apartments. In the late 1990s, several municipalities called for help from the central government, which installed a committee (*Bostadsdelegationen*) to assist with the reconstruction of insolvent housing companies. Together with each municipality in the housing program, this committee decided on the number of apartments that would be phased out,[11] and a cost-sharing arrangement between the central and local government, typically a 50-50 split. Other conditions forced municipalities to increase equity in housing companies to balance write-downs of assets and prohibited dividends for several years.

During 1998-2005, as many as 52 municipalities were in the housing program at some time. In fact, 23 out of the 36 in *Kommundelegationen* also received assistance from the housing program (Swedish National Board of Housing, Building and Planning, 2005).[12] For these 23 cases, we can only estimate the combined effect of the two programs. We do not view this as very problematic, as the two programs were similar in spirit, but discuss the issue more in sections 5.4.2 and 5.6.[13]

5.3 Data

We obtain municipality-level data on a set of economic, political and structural variables for all 290 municipalities and for each year between 1993-2010 from Statistics Sweden. The reform of the intergovernmental equalization grant system is the prime reason why we do not collect data further back than 1993. Besides, there were other major reforms put in place about the same time; specifically, the school system and the provision of long-term care to the elderly and disabled came under municipal responsibility

[11] In several cases phasing out implied tearing down fully functional houses.

[12] Of these 23, 6 entered the housing program in 1999, before they were admitted by Kommundelegationen, and 4 entered the housing program after 2002.

[13] We focus on *Kommundelegationen* as it was directly connected to the overall fiscal performance of the municipalities. Housing is just one part of municipal services and far from the largest in terms of operating costs; it is also a non-obligatory part. *Kommundelegationen* in principle addressed all of the municipal administration. For a short term evaluation of the housing program, see Swedish National Board of Housing, Building and Planning (2005).

in 1992. Comparisons further back in time may thus be misleading.

5.3.1 Dependent variable

Of the available measures of fiscal performance, we find the two prime candidate measures from the balance sheet – the debt level and the equity ratio – unsatisfactory for two reasons. First and most importantly, there were substantial differences among municipalities in the accounting of debt before the Municipal Accounting Act came into effect in 1998. Some important differences still remain today, notably in regard to the accounting of pensions. Second, balance sheet measures are heavily influenced by extraordinary historical events, such as sales of e.g. public companies and real estate. We therefore delimit our choice set to the items on the revenues and costs statement, and settle for the (log of) *per capita operating costs* as the main dependent variable.[14] We also provide results with revenues net of costs (henceforth referred to as *net revenues*) as the outcome variable. A technical reason to focus on costs rather than net revenues is that the latter variable fluctuates a lot from year to year (for idiosyncratic reasons), which makes the synthetic control method more difficult to apply.

5.3.2 Covariates

The dataset contains several potential cost predictors which are used as inputs in the synthetic control matching algorithm and covariates in the fixed effects regressions. The ability to raise revenues is accounted for by the *tax base size* (taxable income per capita), *per capita central government grants*, and the *employment rate* (for the population +16 years). We account for the demographic structure by the *population size*, the *share of children* (0-14 years) and the *share of elderly* (+65 years). We moreover account for differences in policy preferences and political landscape by the *share of*

[14]We log costs to obtain better fit in the regressions and for interpretational ease. All economic variables are in 2010 prices. Financial costs are not included in the cost measure, partly because this item fluctuates a lot from year to year, and partly because financial costs are to some extent beyond the control of the municipalities.

right-wing parties,[15] the *Herfindahl index of political concentration*,[16] and the *number of seats in the municipal council*.[17] Summary statistics for the year 1999 can be found in Appendix 5.A. Tables 5.A.4 and 5.A.5 show that the differences between the groups of admitted and rejected municipalities in terms of the covariates are small (and not significant). On the other hand, compared to those who did not apply (Table 5.A.6), all of the variables are significantly different on at least the 10 percent level for both groups of applicants. Applicants on average had smaller tax bases, received larger equalization grants, had lower employment rates, had smaller and older populations, more left-wing voters, and a municipal council that was less fragmented and had fewer seats.

The data also contains two proxies for initial bailout expectations: (i) the number of deficit grants from the central government received during 1979-1992, and (ii) the average share of each municipality's neighbours that received discretionary grants over the period 1979-1992.[18] In accordance with the results in Pettersson-Lidbom (2010), both the number of discretionary grants and the share of neighbours with grants is significantly higher for applicants than for non-applicants. The former variable is not significantly different between the admitted and rejected groups, while the latter is; a larger share of neighbours of admitted municipalities received transfers during the earlier regime.

5.4 Empirical strategy

The non-random selection into the program means that a simple regression of per capita costs on program status on the sample of all municipalities is unlikely to capture the causal effect of the program (Angrist and Pischke, 2008; Dahlberg et al., 2008). As high costs and poor fiscal performance in general were reasons to apply for the program, it is difficult to envision an

[15] Pettersson-Lidbom (2008) find that municipalities with left-wing governments have higher levels of spending. However, in line with the model of Persson and Svensson (1989), right-wing municipal governments accumulate more debt when their probability of electoral defeat is high (Pettersson-Lidbom, 2001).

[16] Defined as $H = \sum_i (\text{vote share of party } i)^2$ (see e.g. Borge, 2005).

[17] In the political economy literature, the size of the decision making body has been argued to influence costs (Weingast et al., 1981). See e.g. Perotti and Kontopoulos (2002) and Pettersson-Lidbom (2011) for (conflicting) empirical evidence.

[18] Neighbours are defined as sharing land borders.

instrumental variable that would be correlated to program status but uncorrelated to performance (conditional on program status). Consequently, it is even more difficult to find two separate instruments for admission and rejection.

Instead, we use the synthetic control method, which is described in more detail in Section 5.4.1, to select a comparison group that contains only units that are similar to the affected municipalities from the larger group of municipalities that did not apply to the program (the "donor pool"). To study the average effects of the program, we then estimate fixed effects (FE) regressions on the resulting samples of admitted or rejected municipalities and their respective synthetic controls for the period 1999-2010 (see section 5.4.3 for details). The FE framework has some advantages over a simple comparison of the developments in actual and synthetic municipalities:[19] *First*, it allows us to explicitly control for time-invariant unobservables when comparing the actual and synthetic costs in the post-program period. In particular, since we include the year 1999 in the sample, the fixed effects capture unobserved initial motivation for fiscal discipline, which is otherwise one of the key confounders. *Second*, the FE frameworks allows us to include a set of covariates to examine to what extent the actual-synthetic differences are driven by post-program changes in observables.

For a causal interpretation, we need to assume that comparison units are not affected by the program; i.e. that the Stable-Unit-Treatment-Value assumption (SUTVA) holds (Rubin, 2005). The validity of this assumption depends crucially on the choice of donor pool, which we discuss further in section 5.4.2.

As the synthetic control algorithm estimates the yearly actual-synthetic difference in costs for each municipality affected by the program, we lastly take the opportunity to explore the heterogeneity in responses to the program. To draw inference on the significance of each municipality's average difference, i.e. to classify the change in costs as a reduction, no change, or an increase, we create empirical distributions of placebo effects by estimating synthetic controls for the municipalities in the donor pool as well (see Section 5.4.4 for a fuller description).

[19] The potential drawbacks are stronger assumptions on functional form and the distribution of residuals. We provide estimates of the "raw" actual-synthetic differences as well as inference from a method based on the empirical distribution of placebo tests in Appendix 5.B.

5.4.1 The synthetic control method

The synthetic control method for case studies was first used in Abadie and Gardeazabal (2003) and further developed in Abadie et al. (2010).[20] For each municipality i affected by the program, a synthetic control municipality is constructed as a weighted combination of the j municipalities not affected by the program (the "donor pool"). The weights are chosen so as to make the synthetic control similar to the program municipality in terms of some relevant characteristics (cost predictors in our case) during the pre-program period, and to make the synthetic control reproduce the pre-program outcome path for the program municipality. Technically, let the donor pool be of size j, let w denote a $j \times 1$ vector of weights, Z^{dp} a $k \times j$ matrix of k cost predictors and y_t^{dp} a $j \times 1$ vector of pre-program outcomes at time t. Let T_0 denote the period when the program starts. The synthetic control algorithm searches for weights w that make

$$\begin{cases} Z_i = Z^{dp} w \\ y_{i,t} = \sum_j w_j y_{j,t}^{dp} \qquad \forall t < T_0 \end{cases} \tag{5.1}$$

hold, where Z_i are the cost predictors and y_i is the time-t pre-program outcome for a municipality affected by the program. In case there is no w that make these equations hold exactly, the weights are chosen to make the synthetic control as similar to the actual municipality as possible. To do this, the algorithm minimizes the Mean Square Prediction Error (MSPE) over the pre-program period.

A large pre-program MSPE implies that the pre-program similarity of the actual and the synthetic unit is poor. As the method then has failed to construct a valid counterfactual, using such estimates for inference can be questioned (Abadie et al., 2010). However, there is no convention developed regarding the MSPE cut-off of a "sufficiently good" synthetic control. We evaluate our results at several different cut-offs for the pre-program root MSPE (RMSPE). For municipalities whose pre-RMSPE exceeds each threshold, the effect is classified as indeterminate at the given threshold. Note that the RMSPE can be interpreted as a difference in percent (because the dependent variable is logged); thus, if pre-RMSPE is below 0.05, the absolute difference between actual and synthetic unit costs is lower than 5 percent on average during the pre-program period.

[20] For earlier applications, see also e.g. Moser (2005); Fitzpatrick (2008); Hudson (2010); Hinrichs (2012).

Estimation is performed by the *synth* package for Stata.[21] In Z, we include the debt level and equity ratio in 1998, population growth between 1994 and 1998, the average share of neighbours receiving a discretionary transfer in 1978-1992, and the average over the whole pre-treatment period (the default option in *synth*) of the following variables: taxable income per capita, central government grants per capita, employment rate, population size, share of population of age 0-14 and over 65, share of right-wing parties, Herfindahl index and the number of seats in the municipal council. These characteristics are statistically significant in initial regressions of costs for the whole sample of municipalities (results available on request). We also include three lags of the dependent variable (1993, 1996 and 1998) in Z.

Two features of the synthetic control method are potentially problematic in our setting. As the risk of bias decreases with the number of pre-program periods (Abadie et al., 2010), there may be too few pre-program years to produce good controls. Moreover, the method may fail to construct good controls for units that are extreme in terms of pre-program characteristics, as it is difficult (or even impossible) to find suitable combinations of the donors for such units.[22] Recalling the descriptive statistics (Appendix 5.A), the municipalities applying for the program are quite likely to be extreme. Importantly, though, the relevance of these two concerns can be judged after the estimation, as it is possible to examine the pre-program fit of each synthetic control.

5.4.2 Selection of donor pool

One advantage of the synthetic control method is that it implies a data-driven choice of comparison group (Abadie et al., 2010). Nevertheless, this does not imply that any municipality should be included in the donor pool. First, we exclude the admitted and the rejected municipalities from the donor pool, as they were directly affected by the program and thus violate SUTVA. A case can be made that the rejected should be included in the

[21] Unlike the September 2012 version of this paper, we now use the *nested allopt* option of the algorithm. This reduces the pre-program RMSPE's, especially when using the donor pool excluding neighbours.

[22] More formally, this may be the case if the set of pre-program predictors of a unit falls far from the convex hull of the set of predictors of the units making up the synthetic control, in which case the identifying assumptions of the synthetic control method may not even hold approximately (Abadie et al., 2010).

donor pool for the admitted – or even that they should constitute the whole donor pool. As seen from Tables 5.A.1- 5.A.2, the admitted and rejected are very similar in many dimensions and we also know that they both showed the intention to be treated. However, given that rejection is a kind of treatment in its own, it is uncertain to what extent a difference between the admitted and rejected would reflect the effect of being in the program.

Because the concurrent housing program (see section 5.2) may have affected costs directly as well as indirectly (through bailout expectations), we exclude municipalities that were admitted to or rejected from the housing program. We also exclude large cities (as defined by the official classification from Statistics Sweden), which, due to their different cost structure and labour market, are unlikely to be suitable comparison units, and the municipality of Gotland, which has a broader set of responsibilities than the other municipalities. Other municipalities are excluded for more technical reasons, namely municipalities that were formed during or after the pre-program period and two municipalities that were formed in 1992 (for which we lack data on some matching variables).

A particularly difficult choice is whether or not to include neighbouring (to the admitted) municipalities in the donor pool. As neighbours are likely to share the same economic, political, and structural characteristics, and experience similar shocks, they are likely to be important contributors to the synthetic controls and thus make the assumption of unconfoundedness more likely to hold. However, if neighbours keep track of what is going on in bordering municipalities, it is possible that the neighbours of admitted municipalities interpreted the admission of their neighbours as a general softening of the municipal budget constraint and thus relaxed their fiscal efforts. If so, SUTVA does not hold. The results in Pettersson-Lidbom (2010) provide a reason for such suspicions, though we would argue that spillover effects on neighbours are less likely in the current context: in contrast to what was the case for the earlier deficit grants, the program studied here was limited in time, employed relatively clear selection criteria and rejected a large share of applications (almost 40 percent). It is therefore far from obvious that other municipalities, including neighbours, interpreted the program as a significant softening of the budget constraint.

To sum up, if we could prove that there was no spillover effect of the program on the neighbours, we would most definitely want to include them in the donor pool. Since it is impossible to prove this, we estimate synthetic controls twice: once including and once excluding the neighbours of admitted municipalities from the donor pool. The donor pool consists of 136 municipalities when neighbours are included, and 103 when neighbours are excluded.[23]

5.4.3 Fixed effects estimations

Our general estimation equation is

$$y_{it} = \alpha + \beta X_{it} + \sum_{t=2000}^{2010} \gamma_t D_{it} + \lambda_t + \mu_i + \varepsilon_{it} \tag{5.2}$$

where X_{it} is a vector of cost determinants[24] and D_{it} is a dummy variable that capture the year-specific program effect; i.e. the t'th dummy equals 1 for admitted (rejected) municipalities all years $t \geq 2000$ and are zero for all other observations – in particular, it is always zero for the synthetic municipalities. λ_t is a vector of time dummies, μ_i is a vector of fixed effects for each municipality – note that the actual and synthetic versions of municipality i have separate fixed effects – while ε_{it} is an idiosyncratic error term. To compute the values of the covariates and the dependent variable for the synthetic municipalities, we use the weights obtained from the synthetic control estimation. For each variable, the value for the synthetic control is the weighted sum of the values for the municipalities that comprise the synthetic control.

The chosen specification, with separate program dummies for each post-program year, has two advantages over a specification with only one single program dummy for the post-program period. *First*, we can compare the average effect for each year with the raw difference from the synthetic control estimations. *Second*, Laporte and Windmeijer (2005) show that if the yearly effects differ, then a single-dummy version may be biased.

y_{it} is either the log of per capita costs or the per capita net revenues. It should be noted that we then assume that the municipalities contributing

[23]The number of neighbours, defined as sharing a land border with an admitted municipality, is larger than 33, but many neighbours are already excluded for the other reasons mentioned above.

[24]We include the time-variant controls used in the synthetic control estimation. This includes the central government grants variable, though the program grant may have ended up in this post for the admitted municipalities. However, the estimates of the coefficients of interests are not much affected by leaving this variable out.

to the synthetic control for *costs* are also suitable comparison units for net revenues. This seems like a reasonable assumption given that they are similar in terms of cost structure as well as political, economic and demographic characteristics.

5.4.4 Heterogeneity and placebo tests

In our exploration of the heterogeneity in responses to the program, we use placebo tests to classify each affected municipality's average effect (computed over 2000-2010) as a cost increase, a cost reduction or no change.

To obtain a placebo distribution of effects, we follow Abadie et al. (2010) and construct synthetic controls for each municipality in the donor pool. The average effect for each admitted (or rejected) municipality is then compared to this distribution of placebo effects. A municipality's average effect is classified as significant if either one or both of the following two statistics lie in the extreme deciles of their respective placebo distributions: (i) the average actual-synthetic difference in per capita costs 2000-2010, i.e.

$$average_i = \frac{1}{T} \sum_{t=2000}^{2010} (y_{it}^{actual} - y_{it}^{synthetic}); \tag{5.3}$$

and, (ii) the ratio between the post-program RMSPE and the pre-program RMSPE. The first statistic has the advantage of capturing the sign of the effect, while the other has the advantage that it acknowledges the effect size in relation to the fit of the synthetic control. An estimated effect of 0.03 (i.e. 3 percent) is arguably more indicative of a significant effect if the pre-program RMSPE is 0.01 than if it is 0.1.

5.5 Results

5.5.1 Estimations and fit

As the program was announced in the fall of 1999 and the admission decision was not made until one year later, we suspect that there was not much time to implement changes due to the announcement in 1999. Therefore, we let the *synth* algorithm minimize the MSPE over 1993-1999.

The donor pool contains more than 100 municipalities, but the synthetic controls generally consist of only a handful of municipalities.[25] A comparison of the pre-program predictor values within each actual-synthetic pair shows that the algorithm generally produces controls that are similar to their actual counterparts, although the equity ratio and the share of right-wing parties seem to have been relatively difficult to match (results available on request).[26] A visual inspection of the pre-program evolution of costs in actual and synthetic municipalities also suggests that the algorithm yields controls with adequate fit for most municipalities, though large pre-program fluctuations in actual costs are a complicating factor in some cases.

Table 5.1: Average pre-RMSPE per synthetic control estimation

	Admitted		Rejected	
pre-RMSPE cut-off level	Incl neighbours (1)	Excl neighbours (2)	Incl neighbours (3)	Excl neighbours (4)
None	0.0189	0.0261	0.0251	0.0323
	(35)	(34)	(22)	(22)
0.05	0.0180	0.0218	0.0222	0.0285
	(34)	(30)	(21)	(20)
0.03	0.0140	0.0159	0.0184	0.0228
	(28)	(22)	(16)	(10)
0.02	0.0117	0.0137	0.0128	0.0134
	(23)	(17)	(9)	(7)

In parentheses: number of municipalities whose pre-RMSPE<cut-off.

Table 5.1 shows the average pre-program RMSPE in each of the four estimations (admitted vs. rejected, including vs. excluding neighbours in donor pool) at different cut-off levels.[27] The pre-program RMSPEs are in the order of 0.01-0.03, i.e. the prediction errors during 1993-1999 typically amount to 1-3 percent of the yearly cost level. At most cut-offs, the synthetic controls of admitted municipalities have a better fit than those of the rejected. The number of municipalities passing the cut-off criterion (pre-RMSPE<cut-off) naturally decreases as the cut-off becomes stricter. The decrease is especially drastic in the estimations where neighbours are ex-

[25] For the admitted, the median number of contributing donors is 6. 75 percent of the admitted have more than 4 but fewer than 9 contributing donors.

[26] We were unable to construct synthetic controls for admitted municipality Älvdalen and rejected municipality Gullspång, due to missing data for some years.

[27] Lowering the cut-off even further to 0.01 reduces the number of placebo municipalities substantially (from 97 when pre-RMSPE $<$ 0.02 to 37) and 26 out of 36 program municipalities are categorized as indeterminate. Using 0.04 as a cut-off yields results that are in between the results for 0.03 and 0.05.

cluded from the donor pool, which confirms that neighbours are important contributors to the synthetic controls.

5.5.2 Average program effects

Figures 5.5.1 and 5.5.2 present the results from the synthetic control estimations for admitted and rejected municipalities, respectively. The figures show, for each of the years 1993-2010, the average of the raw differences between actual and synthetic log costs per capita. The dashed vertical line indicates the start of the post-program period, i.e. year 2000. The black (dashed) line represents the average actual-synthetic cost difference when neighbours are included in (excluded from) the donor pool.[28] In the upper right part of Figure 5.5.1 (Figure 5.5.2), the yearly averages are computed over all 36 (22) admitted (rejected) municipalities, regardless of pre-program fit; in the other parts of the figure, the averages are computed over the municipalities that pass the pre-program RMSPE cut-offs of 0.05, 0.03 and 0.02, respectively.

For both admitted and rejected municipalities, the estimated average differences are sensitive to whether neighbours are included in the donor pool or not. Starting with the admitted, the upper part of Figure 5.5.1 shows that the average actual-synthetic differences are positive most years from 1999 and onwards when neighbours are excluded from the donor pool. For the municipalities passing the lower RMSPE cut-offs (bottom row of figure), there is more or less no difference between actual and synthetic costs. When neighbours are allowed to enter the donor pool, the admitted municipalities have *lower* costs than their synthetic controls from 2001 onwards for all RMSPE cut-offs. The rejected (Figure 5.5.2) show roughly the same pattern as the admitted; unexpectedly high costs when neighbours are excluded from the donor pool disappear when neighbours are included in the donor pool (as well as when applying lower cut-offs). However, unlike the admitted, the rejected never show any sign of *reducing* their costs in relation to their synthetic controls.

The figures give us a hint of the reason for the deterioration of pre-program RMSPE when neighbours are excluded from the donor pool (c.f.

[28] The point estimates and bootstrapped p-values for the raw differences in 2000-2010 are also shown in Appendix 5.B, Table 5.B.3 (including neighbours) and Table 5.B.4 (excluding neighbours) respectively.

Figure 5.5.1: Average actual-synthetic difference, admitted

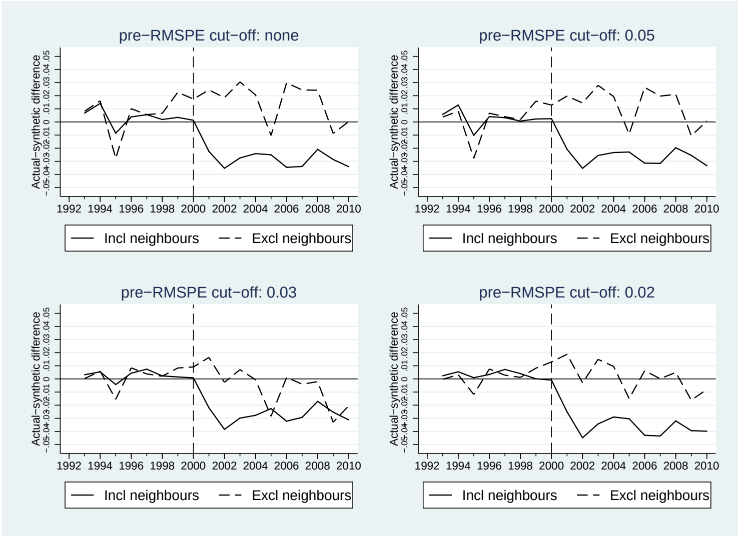


Table 5.1) as much of this deterioration arises due to bad fit in 1999. The sensitivity to the inclusion of neighbours motivates a further investigation. In Appendix 5.C, we therefore estimate synthetic controls for the 33 neighbours as well. In brief, we get a very poor fit for three of the municipalities that figure prominently in the synthetic controls mentioned above. We are unable to sign the effect for two of these, while the third has higher costs than its synthetic control during the post-program period. The average effect is positive; however, most neighbours follow their synthetic controls closely during the post-program period so neighbours in general do not seem to be affected by the program.[29]

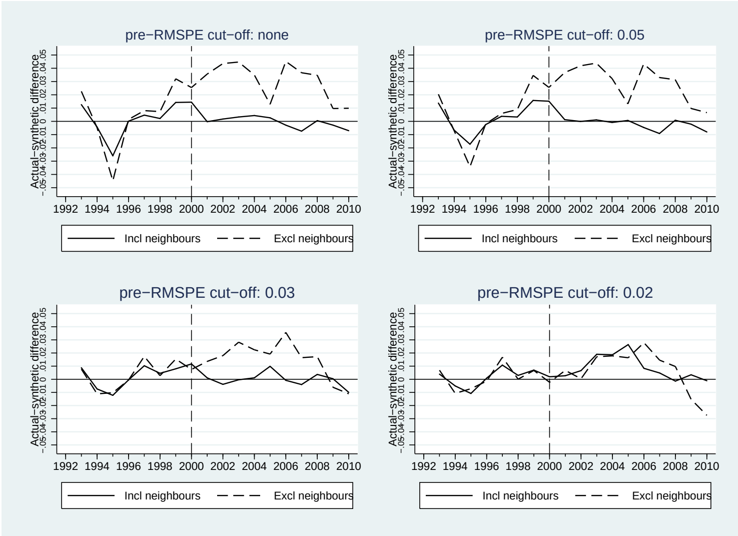
We next turn to the fixed effects (FE) estimations on the samples including admitted (rejected) municipalities and their synthetic controls over the period 1999-2010.[30] Tables 5.2 and 5.3 show the results for the samples of admitted and rejected, respectively. All actual-synthetic pairs enter the estimation; i.e. no pre-RMSPE cut-off is applied.[31] Neighbours are

[29] Note that our identifying assumptions carry over to the estimation for neighbours: i.e., just because some of the neighbours increase their costs unexpectedly after the program, we cannot be sure that it is due to the program rather than to something else.

[30] See Appendix 5.B for results for covariates.

[31] Our conclusions do not change if we instead include only municipalities with pre-

Figure 5.5.2: Average actual-synthetic difference, rejected



allowed to contribute to the synthetic controls in columns (1)-(2), but not in columns (3)-(4). Columns (1) and (3) show the yearly average cost differences conditional only on municipality-specific and year-specific effects, while columns (2) and (4) show the results conditional also on covariates.

When neighbours are included in the donor pool (column 1 of Table 5.2), the admitted municipalities show a significantly lower cost level than their synthetic counterparts from 2001, the first full year of the program,[32] and onwards. When neighbours are excluded from the donor pool (column 3), the estimates are much closer to zero and only significantly negative a few years. None of the coefficients are *positive* and significant though, contrary to what may be expected from the upper row of Figure 5.5.1. Apparently, the inclusion of municipality-fixed effects entails a downward adjustment of the differences.[33]

For the rejected (Table 5.3), there are almost no significant differences between actual and synthetic costs, regardless of whether neighbours are included in the donor pool or not. As for the admitted, the fixed effects

RMSPE < 0.03, the results are in general very similar (results available on request).

[32]Recall that applications were not approved/rejected until late 2000.

[33]The actual-synthetic differences shown in Figure 5.5.1, i.e. the differences not accounting for municipality-specific effects or covariates, appear to be significantly positive according to the bootstrap p-values in Appendix 5.B.

seem to erase the seemingly positive effects in Figure 5.5.2 for the sample excluding neighbours.

Changes in the included covariates do not appear to drive the detected differences, as seen from a comparison of column (1) with column (2) and column (3) with column (4) (for each of Tables 5.2 and 5.3); the changes in the magnitude and significance of the coefficients for both groups are mostly small for both groups.

In columns (5)-(8) of Tables 5.2 and 5.3, we use *net revenues per capita* as the dependent variable; column (5) corresponds to the specification used in column (1) etc. It can be noted that the coefficients now are expressed in thousands of SEK per capita, so a coefficient of 1 implies that admitted municipalities had 1 000 SEK higher net revenues per capita that year.

Three things stand out regarding the estimates for net revenues. *First*, the magnitudes of the yearly differences in Table 5.2 are very large. The estimated marginal effects for admitted municipalities amount to about 1000 SEK per capita, which is a little bit less than one standard deviation of the average for the period,[34] and the coefficients are highly significant most years. *Second*, we find little indications of a similar effect on the rejected municipalities (Table 5.3), though there are a few positive significant years (especially at the end of the period). A *third* and final observation is that the estimates are more or less insensitive to the exclusion of neighbours.

To sum up, the estimates for costs show decreased cost levels for admitted municipalities when neighbours are included in the sample, and hardly any differences to the comparison group when they are excluded. Rejected municipalities are close to the cost level of their comparison group, regardless of sample. The consistently positive estimates for the admitted, as well as the difference in the estimates for admitted and rejected, suggest that program participation is associated with a relatively favourable development of net revenues. In Appendix 5.B, we show that similar results are obtained also when we estimate fixed effects models on the unweighted sample of municipalities (i.e. not applying the weights from the synthetic control algorithm). We also include results that indicate that the results in the sample *excluding* neighbours are sensitive to the chosen length of the period, especially for net revenues and for the rejected group. Importantly, per capita costs are still not significantly different from zero and net rev-

[34] The standard deviation is about the same in the group of actual and synthetic as for the whole group of 290 municipalities.

Table 5.2: Fixed effects estimations of program effects, admitted municipalities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent:	Costs	Costs	Costs	Costs	Net rev.	Net rev.	Net rev.	Net rev.
admitted×2000	-0.00221	0.000653	-0.00525	-0.000214	-0.387	-0.322	-0.317	-0.246
	(0.00517)	(0.00533)	(0.00423)	(0.00458)	(0.325)	(0.331)	(0.287)	(0.290)
admitted×2001	-0.0259***	-0.0232***	0.00176	0.00665	0.796**	0.877**	0.784**	0.907**
	(0.00649)	(0.00684)	(0.00624)	(0.00673)	(0.396)	(0.405)	(0.375)	(0.389)
admitted×2002	-0.0388***	-0.0476***	-0.00411	-0.0135*	2.381***	2.443***	1.694***	1.808***
	(0.00786)	(0.00730)	(0.00815)	(0.00682)	(0.365)	(0.382)	(0.351)	(0.379)
admitted×2003	-0.0309***	-0.0420***	0.00785	-0.00453	1.103***	1.184***	1.020***	1.170***
	(0.00954)	(0.0105)	(0.00987)	(0.0111)	(0.272)	(0.294)	(0.268)	(0.323)
admitted×2004	-0.0276***	-0.0383***	-0.00202	-0.00813	0.516*	0.599*	1.412***	1.609***
	(0.00988)	(0.0104)	(0.0102)	(0.0103)	(0.286)	(0.300)	(0.276)	(0.290)
admitted×2005	-0.0285***	-0.0339***	-0.0330***	-0.0376***	1.027***	1.166***	2.560***	2.719***
	(0.0102)	(0.0117)	(0.0106)	(0.0103)	(0.281)	(0.315)	(0.341)	(0.383)
admitted×2006	-0.0381***	-0.0415***	0.00715	0.00452	1.332***	1.488***	0.840***	1.019***
	(0.0109)	(0.0135)	(0.0114)	(0.0123)	(0.248)	(0.302)	(0.216)	(0.295)
admitted×2007	-0.0374***	-0.0444***	0.00173	0.000818	1.634***	1.773***	1.382***	1.534***
	(0.0113)	(0.0141)	(0.0119)	(0.0126)	(0.298)	(0.336)	(0.266)	(0.297)
admitted×2008	-0.0244**	-0.0282*	0.00152	0.00253	1.840***	2.019***	1.194***	1.389***
	(0.0116)	(0.0159)	(0.0123)	(0.0132)	(0.331)	(0.370)	(0.262)	(0.322)
admitted×2009	-0.0321**	-0.0318*	-0.0314**	-0.0280**	1.397***	1.636***	1.939***	2.143***
	(0.0124)	(0.0175)	(0.0120)	(0.0139)	(0.303)	(0.361)	(0.265)	(0.291)
admitted×2010	-0.0376***	-0.0370*	-0.0224*	-0.0205	1.556***	1.738***	1.244***	1.416***
	(0.0130)	(0.0186)	(0.0132)	(0.0153)	(0.307)	(0.365)	(0.269)	(0.312)
Constant	3.875***	0.135	3.863***	-0.338	-0.147	-76.78**	-0.363***	-64.24*
	(0.00413)	(1.890)	(0.00426)	(1.849)	(0.123)	(37.75)	(0.0927)	(34.17)
Covariates	N	Y	N	Y	N	Y	N	Y
Neighbours in d.p.	Y	Y	N	N	Y	Y	N	N
Observations	840	840	816	816	840	840	816	816
Nr of municipalities	70	70	68	68	70	70	68	68
R^2	0.917	0.930	0.912	0.923	0.440	0.451	0.447	0.458

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Year fixed effects included in all models. Älvdalen is excluded in all samples. Dorotea is excluded in samples excluding neighbours from the donor pool.

Table 5.3: Fixed effects estimations of program effects, rejected municipalities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent:	Costs	Costs	Costs	Costs	Net rev.	Net rev.	Net rev.	Net rev.
rejected×2000	0.000225	0.00217	-0.00660	-0.00225	0.0238	0.0459	0.136	0.117
	(0.00737)	(0.00788)	(0.00725)	(0.00776)	(0.439)	(0.442)	(0.417)	(0.426)
rejected×2001	-0.0145	-0.0122	0.00356	0.00493	0.513	0.526	0.594	0.562
	(0.00893)	(0.00911)	(0.00889)	(0.00870)	(0.422)	(0.426)	(0.415)	(0.411)
rejected×2002	-0.0125	-0.0153	0.0115	0.00393	0.581	0.899*	0.209	0.363
	(0.0111)	(0.0108)	(0.0116)	(0.0104)	(0.534)	(0.534)	(0.540)	(0.517)
rejected×2003	-0.0109	-0.0127	0.0126	0.00495	0.251	0.541	0.124	0.217
	(0.0114)	(0.00991)	(0.0117)	(0.00908)	(0.396)	(0.384)	(0.387)	(0.350)
rejected×2004	-0.00985	-0.0200	0.00288	-0.00628	0.0957	0.391	0.838*	0.916**
	(0.0146)	(0.0137)	(0.0145)	(0.0124)	(0.404)	(0.348)	(0.416)	(0.354)
rejected×2005	-0.0116	-0.0173	-0.0192	-0.0290***	0.468	0.798**	1.640***	1.696***
	(0.0144)	(0.0119)	(0.0142)	(0.0107)	(0.365)	(0.350)	(0.437)	(0.424)
rejected×2006	-0.0171	-0.0145	0.0131	0.00863	0.581	0.750*	0.197	0.242
	(0.0150)	(0.0124)	(0.0157)	(0.0123)	(0.374)	(0.373)	(0.338)	(0.373)
rejected×2007	-0.0216	-0.0189	0.00455	0.00519	0.570	0.783*	0.410	0.476
	(0.0153)	(0.0128)	(0.0160)	(0.0136)	(0.412)	(0.410)	(0.387)	(0.455)
rejected×2008	-0.0137	-0.00521	0.00266	0.00498	0.662	0.904**	0.310	0.368
	(0.0160)	(0.0126)	(0.0166)	(0.0134)	(0.451)	(0.424)	(0.408)	(0.450)
rejected×2009	-0.0171	-0.00682	-0.0224	-0.0197	0.712**	0.953***	1.111***	1.189***
	(0.0180)	(0.0136)	(0.0179)	(0.0141)	(0.349)	(0.307)	(0.332)	(0.370)
rejected×2010	-0.0213	-0.0185	-0.0222	-0.0239	1.891**	2.256***	1.762**	2.007**
	(0.0175)	(0.0143)	(0.0174)	(0.0147)	(0.719)	(0.756)	(0.667)	(0.836)
Constant	3.849***	-2.716*	3.846***	-3.578**	0.274	-5.479	0.0874	43.64
	(0.00544)	(1.474)	(0.00563)	(1.516)	(0.181)	(48.36)	(0.159)	(56.15)
Controls	N	Y	N	Y	N	Y	N	Y
Neighbours in d.p.	Y	Y	N	N	Y	Y	N	N
Observations	528	528	528	528	528	528	528	528
Number of municipalities	44	44	44	44	44	44	44	44
R^2	0.919	0.941	0.915	0.937	0.316	0.363	0.339	0.386

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
Year fixed effects included in all models. Gullspång excluded from all samples.

enues are positive and significant most years 2002-2010 for the admitted group when we use the whole period 1993-2010. Thus, these results do not change our conclusions about the average effect for admitted municipalities, and the difference to the rejected group becomes, if anything, more marked.

5.5.3 Heterogeneous effects

The yearly average cost differences discussed in the previous section may hide substantial variation between municipalities. To examine this possibility, we investigate the actual-synthetic cost differences of each affected municipality (averaged over 2000-2010, see Equation (5.3)). We restrict our attention to the municipalities passing pre-program RMSPE cut-off of 0.05, to strike a balance between fit on the one hand and representativeness with respect to the whole group of affected (admitted or rejected) municipalities on the other. In order to classify each of the average cost differences as positive (cost increase), negative, or zero, we perform the placebo tests described in Section 5.4.4.

In the estimations where neighbours are *included* in the donor pool (Table 5.4, Panel A), admitted municipalities are over-represented in the lowest decile of a placebo distribution: out of the 34 municipalities passing the RMSPE criterion, 32 percent (11 municipalities) are classified as having reduced costs. The average cost reduction of these 11 municipalities is 7 percent, which can be compared to their average pre-program RMSPE of 2 percent.[35] 6 percent of the admitted appear to increase costs. For the rejected, the distribution is pretty similar to the placebo distributions: of 22 rejected municipalities, 14 percent (3 municipalities) are classified as having increased and 14 percent as having reduced their costs.

According to the estimates *excluding* neighbours from the donor pool (Table 5.4, Panel B), 8 out of 30 admitted and 6 out of 20 rejected are classified as having increased their costs, while the number reducing costs are fewer (4 admitted, 1 rejected). However, we would like to stress that the fit of the synthetic controls decrease noticeably with this donor pool and that the incorporation of fixed effects thus makes a large difference for the estimated average effects. Given the relatively poor fit with this donor pool,

[35] The one admitted municipality not passing the pre-RMSPE criterion of 0.05 (its pre-RMSPE is 0.0503) is also in the lowest decile of the placebo distribution. Its reduction amounts to 8 percent.

we believe that the fixed effects pick up important unobserved heterogeneity and thus do not view the raw actual-synthetic differences as equally reliable as for the sample including neighbours in the donor pool. With this caveat in mind, it may however be noted that the raw actual-synthetic differences for *neighbours* show a similar pattern of heterogeneity, with 20 percent (6 municipalities) in the highest decile of a placebo distribution and 6 percent (2 municipalities) in the lowest decile.

This analysis reveals great heterogeneity in the post-program differences.[36] In particular, the average negative cost differences for admitted municipalities when neighbours are included in the donor pool appear to be driven by a subset of this group, while two thirds of the admitted show no indication of a program effect. Regardless of which donor pool one prefers, it seems reasonable to conclude that *for most municipalities, there is little evidence that the program implies increased costs in the long run.*

Table 5.4: Distribution of individual program effects

Panel A	Donor pool: including neighbours (130 municipalities)		
	(1)	(2)	(3)
Group	Cost reduction	No change	Cost increase
Admitted	11	21	2
Rejected	3	15	3
Panel B	Donor pool: excluding neighbours (98 municipalities)		
	(1)	(2)	(3)
Group	Cost reduction	No change	Cost increase
Admitted	4	18	8
Rejected	1	13	6

5.6 Exploring sources of response heterogeneity

We finally examine whether certain structural characteristics, institutions, and attitudes can explain why some of the admitted municipalities managed to hold back costs more than others. Restricting our attention to the estimations where neighbours are included in the donor pool, we compare the 12 municipalities that appear to have reduced costs (the cost-reducer group)

[36] We cannot perform the same analysis for net revenues, but looking at the raw averages over the period 2000-2010 for the admitted, these range from -1.2 to 2.6 percent of gross tax revenues. Thus, there seems to be great heterogeneity also for this variable.

to the 23 municipalities that do not reduce costs (the non-reducer group) according to the placebo analysis.[37] As the sample size is very small, we foremost interpret differences between the two groups as potentially fruitful directions for future investigations.

Table 5.D.1 in Appendix 5.D shows (two-sided) t-tests for equal means (or equal proportions, where applicable) between the cost-reducer and non-reducer groups for a set of candidate explanatory variables. In the interest of space, we delimit the discussion here to variables that differ significantly between the groups or are of particular interest for other reasons.

As a primarily methodological check, we examine whether the different developments of costs in the two groups relate to the importance of neighbours in their respective synthetic controls. For each synthetic control, we compute the share of the total weight that derives from neighbours to the admitted municipalities. This share is rather large for most of the 35 municipalities – the mean is 0.64 and the median is 0.74. Though the mean share is higher in the group of reducers than in the group of non-reducers (0.74 vs. 0.60), the difference between the two means is not statistically significant (p-value=0.41). Moreover, the correlation between the share of neighbours and the average actual-synthetic cost difference ($average_i$) is small (-0.093) and insignificant (p-value=0.59).

A notable difference between the groups is that the share receiving assistance from the contemporary housing program is higher in the group of cost-reducers (83 percent) than in the non-reducer group (52 percent) (p-value=0.070). This difference may indicate that participation in two programs – both of which coupled grants with costly efforts – was necessary to enable a turn towards fiscal discipline. It may likewise mean that the general program did not affect fiscal discipline at all, but that the housing program was the real wake-up call.[38] Another possibility is that the cost reductions only capture that the municipalities whose housing companies had been reconstructed no longer had to transfer funds to their housing compa-

[37] We do not apply a pre-RMSPE cut-off; hence there are 12 instead of 11 cost-reducers. The twelfth municipality has a pre-RMSPE of 0.0503, which is not strikingly larger than the 0.05 cut-off applied in section 5.4.

[38] Interviews with representatives from a few of the admitted municipalities shortly after they received their grant give some support for the idea that the housing program was a wake-up call; some express that it was no longer possible to ignore the severity of the municipality's financial problems when fully functional apartments were destroyed as part of the housing program (SOU, 2003).

nies. In Appendix 5.B, we show however that costs were not only reduced in areas where such transfers would be recorded.[39] Furthermore, there is no indication that the municipalities admitted *only* to the housing program reduce costs in other areas. Thus, for whatever reason, the cost-reducers appear to have engaged in a rather broad cost reduction effort.

Another significant difference between cost-reducers and non-reducers relates to the size of the grants received within the bailout program (*Kommundelegationen*): on average, the grant amounted to 6 percent of total costs for the cost-reducers in 2000, but to 4 percent for the non-reducers (p-value=0.067). As the cost-reducers are over-represented in the housing program, there is also a large difference in the ratio of grants received from *both* programs to total costs; on average, total grants amount to 17 percent of total costs for the cost-reducers but to 8 percent for the non-reducers (p-value=0.011). These findings may relate to between-group differences in motivation and/or ability to reduce costs, as the size of the grant was positively related to the size of the cost reductions in the agreement (SOU, 2003).

An argument in favour of differences in ability rather than motivation is that the cost-reducers historically have received relatively many deficit grants from the central government: on average, municipalities in this group received deficit grants from the central government in 10 of the years 1979-1992. The corresponding average is 6 in the non-reducer group and the difference is statistically significant (p-value=0.013). Moreover, the average proportion of neighbours receiving deficit grants (again during 1979-92) is higher for the cost-reducers (0.56) than for the non-reducers (0.46)(p-value of difference = 0.068). It certainly seems counter-intuitive that municipalities that are used to relying on the central government suddenly (i.e. at the time of application to the program) would be particularly motivated to increase fiscal discipline. In fact, Pettersson-Lidbom (2010) shows that municipalities that received many grants in the 1980s were more likely to apply for the program under study here, and interpret this result as a sign that the applicants were particularly likely to believe that the central government would come to their rescue – hardly a sign of pre-program mo-

[39] Moreover and importantly, a majority of book-keeping posts in these two areas are also unrelated to housing (Statistics Sweden, 2012a) and the areas are small in comparison total costs (on average for all municipalities, the two categories amount to 13 percent of total costs in 2010).

tivation.[40] Moreover, both groups have bought consultant services from the Swedish Association of Local Authorities and Regions to a similar degree (SALAR has a special unit that, against a fee, helps municipalities to improve their fiscal situation), and the political commitment to long-term budgets is also not different. Both these variables are reasonable proxies for fiscal motivation.

There are on the other hand between-group differences that supports ability as an explanation for the heterogeneity. The average (over 2000-2010) share of right-wing parties in the municipal council is lower in the cost-reducer group, 30 percent versus 42 percent for the non-reducers (p-value=0.010). This difference also reflect differences in electoral uncertainty: in the most recent election before the program was initiated (held in 1998), the right-wing parties had between 45 and 55 percent of the votes in one third (8) of the non-reducer municipalities, while there were no such close elections in the cost-reducer group (p-value of difference = 0.020). The cost-reducers could thus implement cost reductions with less fear of losing the next election, while the situation was different in the other group.

The relative increase in fees and total revenues between 2000-2010 is significantly higher in the non-reducer group (p-value = 0.016 and 0.002 respectively). This group has also increased their tax rates more (although not significantly so, p-value = 0.137). These differences may be related to the differences in electoral uncertainty between the two groups. It may be less costly (in terms of votes) to raise taxes and fees than to cut spending on popular services; thus, municipalities with close elections may opt for the strategy to increase revenues, while municipalities with more certain majorities can afford to choose the cost-reducing strategy. In relation to this possibility, it can be noted that the positive and significant coefficients in the FE regressions on net revenues are not driven by the group of cost-reducers (results available on request). There seem to be less heterogeneity when it comes to net revenues than when it comes to costs.

Apart from these variables, we find no significant between-group differences for any of the examined demographic, economic, political, and institutional variables. Missing values for the institutional variables is a concern

[40] The grant was reasonably the prime incentive to participate in the program. Any actions taken to increase fiscal discipline during the program would in principle be possible to implement without involvement of the central government or the program committee.

however; thus, we do not rule out that institutions may be a channel for the differences between the groups.

5.7 Conclusions

None of our main specifications indicate that the admitted municipalities on average have increased costs significantly, and all specifications indicate that they on average have increased net revenues significantly. There is heterogeneity behind the average results though; some are more prone to cut costs while others mainly increase revenues. A cautious interpretation is that conditional discretionary intergovernmental grants need not have negative effects on fiscal discipline. A stronger claim is that the program even increased fiscal discipline in several municipalities.

The assumptions needed to identify causal effects of the program are untestable, but we can discuss their validity in relation to the two interpretations. Of the municipalities in the comparison group, we believe that neighbours to the admitted are the most likely to be influenced by the program and we find evidence consistent with such spillover effects in a few cases. SUTVA is thus least likely to be violated when we exclude neighbours from the comparison group. In these estimations, we find no significant effects on the post-program costs of the admitted; thus, there is support for the more cautious of our interpretations. As the admitted have significantly higher net revenues in this sample, there is even support for the stronger claim. It should be pointed out though that the estimates for net revenues rely on the additional (and in our view reasonable) assumption that the synthetic control municipalities constructed for costs are valid also for net revenues.

The admitted and their neighbours are similar in many respects. While increasing the credibility of SUTVA, the exclusion of neighbours therefore simultaneously reduces the credibility of the unconfoundedness assumption. For the sample including neighbours, the admitted on average have significantly lower costs and higher net revenues than their synthetic controls. If SUTVA holds, these results support the stronger claim. Notably though, even if SUTVA does not hold and the neighbours *are* affected by the program, the results suggest that fiscal discipline benefited less from, or was harmed more by, non-participation than from participation in the program. Whether fiscal discipline overall benefited from or was harmed by the program can however not be established in this case.

Though we compare very similar units and control for time-invariant characteristics, unconfoundedness may still be threatened by unobserved time-variant characteristics. In relation to the cautious interpretation, it is for instance conceivable that the program harmed the admitted municipalities' motivation for fiscal discipline and that they would have displayed even better outcomes if the program had not existed. In relation to the stronger claim, the most concerning confounder is that the admitted municipalities *for reasons unrelated to the program* have become more motivated to come to terms with their fiscal situation. We find unobserved fiscal motivation less worrying for two reasons:

First, in most samples and for both outcome variables, there is a visible turn towards more discipline in 2001. This was the first year when admitted municipalities had time and explicit incentives to react to the content of the program (rather than just to its announcement). Among all conceivable explanations for the timing of the turn, a program effect appears most plausible.

Second, we find little evidence of improvements for the municipalities that were denied to participate in the program, who were similar to the admitted in many respects and obviously were motivated enough to apply to the program. We cannot rule out that the program committee was able to discern and admit only the most motivated applicants. Motivation at the time of admission should however be captured by the fixed effects, and thus cannot explain the different results for the two groups. The most plausible explanation instead relates to participation in the program: while the admitted could use a pending grant to convince the opposition and/or the public about the necessity of improving discipline, the rejected had no such means at hand.[41]

We do not intend to downplay the importance of motivation for the establishment of fiscal discipline. As long as debt roll-over is possible, motivation is a prerequisite for fiscal discipline. It is also the only channel through which the program possibly may have affected the municipalities' behaviour after its closure. Our point is rather that it is hard to explain the change on average for the admitted without referring to their participation in the program. On balance, we think that the most plausible interpretation of our results is that the program did not reduce the fiscal discipline of

[41] We thank Magnus Henreksson for suggesting this explanation.

the admitted, and that it even had beneficial effects on fiscal discipline in several cases.

Only some of the admitted municipalities reduce costs significantly compared to their synthetic controls. This group does not appear to drive the results for net revenues and we find no differences in motivation between the two groups of admitted municipalities. A tentative explanation is instead that the incumbent politicians in municipalities opting for the cost reducing strategy had more certain majorities, and thus could afford to cut costs without fear of losing the next election.

The contrast between our results and the message from previous studies suggests that the conditions attached to the grants, a distinguishing factor of the program under study, may be a key component in dampening the soft-budget effect of discretionary intergovernmental grants. If the government clearly announces that harsh conditions will be applied, negative spillover effects on other units may moreover be mitigated. This is important as previous research (Pettersson-Lidbom, 2010) as well as our findings are consistent with a spillover interpretation. However, to claim more conclusively that conditions are crucial we would need larger samples and more variation in the conditions. This presents an interesting avenue for future research in other contexts.

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5.A Descriptive statistics

This section shows descriptive statistics for the municipalities, divided into admitted, rejected, and others. Table 5.A.1-5.A.3 display variables corresponding to the selection criteria for the program, as well as the number of bailouts and share of neighbours with at least one bailout during the earlier regime of discretionary transfers. Table 5.A.4-5.A.6 display summary statistics for the time-varying covariates in 1999. Economic variables are in 2010 prices.

Table 5.A.1: Selection criteria and initial bailout expectations, admitted municipalities

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Total costs 1998 (KSEK/capita)	45.5	5.7	29.9	57.5	36
Debt 1998, incl pensions (KSEK/capita)	37.3	9.4	24.9	65.7	36
Equity ratio 1998 (%)	50.4	17.0	12.7	78.6	36
Pop growth 94-98 (%)	-4.7	1.9	-8.2	1.8	36
Number of bailouts 79-92	7.9	4.1	0	14	36
Share neighbours with bailout 79-92 (%)	50.0	16.6	8.6	77.1	36

Table 5.A.2: Selection criteria and initial bailout expectations, rejected municipalities

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Total costs 1998 (KSEK/capita)	43.8	4.6	34.9	51.5	23
Debt 1998, incl pensions (KSEK/capita)	40.1	14.1	23.0	92.8	23
Equity ratio 1998 (%)	47.3	21.7	-5.5	82.2	23
Pop growth 94-98 (%)	-4.8	2.5	-8.3	4.7	23
Number of bailouts 79-92	7.7	3.3	0	13	23
Share neighbours with bailout 79-92 (%)	40.8	11.8	17.9	57.1	23

Table 5.A.3: Selection criteria and initial bailout expectations, others

Variable	Mean	Std. Dev.	Min.	Max.	N
Total costs 1998 (KSEK/capita)	39.9	4.6	30.8	57.3	229
Debt 1998, incl pensions (KSEK/capita)	31.7	11.7	11.4	84.8	229
Equity ratio 1998 (%)	59.1	17.9	-4.4	92.7	229
Pop growth 94-98 (%)	-1.2	3.3	-8.4	13.3	227
Number of bailouts 79-92	4.2	3.8	0	14	226
Share neighbours with bailout 79-92 (%)	30.3	19.7	0	100	224

Table 5.A.4: Summary statistics, admitted municipalities

Variable	Mean	Std. Dev.	Min.	Max.	N
Tax base (KSEK/capita)	112.0	10.1	90.4	139.5	36
Central gov. grant (KSEK/capita)	10.3	5.1	-1.1	23.2	35
Employment rate, 16+ (%)	50.5	5.4	37.6	69.4	36
Population size	12177.8	6498.7	2746	28872	36
Share 0-14 (%)	17.9	1.5	15.6	23.0	36
Share +65 (%)	21.7	3.9	8.1	28.8	36
Share right-wing (%)	35.5	13.8	8.6	67.7	36
Herfindahl	0.26	0.05	0.18	0.36	36
Number of seats	40.1	7.4	31	61	36

Table 5.A.5: Summary statistics, rejected municipalities

Variable	Mean	Std. Dev.	Min.	Max.	N
Tax base (KSEK/capita)	111.7	11.3	97.9	135.6	23
Central gov. grant (KSEK/capita)	9.2	4.6	1.0	21.6	23
Employment rate, 16+ (%)	52.1	4.4	41.3	64.5	23
Population size	14658.4	15755.4	4304	64096	23
Share 0-14 (%)	18.5	1.5	15.8	22.6	23
Share +65 (%)	20.9	2.5	13.4	26.1	23
Share right-wing (%)	39.7	13.9	22.6	66.7	23
Herfindahl	0.26	0.05	0.18	0.38	23
Number of seats	40.6	9.3	31	61	23

Table 5.A.6: Summary statistics, others

Variable	Mean	Std. Dev.	Min.	Max.	N
Tax base (KSEK/capita)	117.0	15.6	94.5	215.7	230
Central gov. grant (KSEK/capita)	6.8	3.9	-7.0	20.7	230
Employment rate, 16+ (%)	55.9	5.0	45.0	69.9	230
Population size	35156.0	63524.9	3244	743703	230
Share 0-14 (%)	19.1	1.7	13.5	24.2	230
Share 65+ (%)	18.4	3.7	8.6	28.1	230
Share right-wing (%)	45.9	11.4	13.7	86.7	230
Herfindahl	0.24	.04	0.17	0.51	230
Number of seats	47.9	11.9	31	101	230

5.B Sensitivity tests and covariate estimates

For comparison, this Appendix includes estimates from FE specifications on the "raw" sample of municipalities, i.e. not applying the weights obtained from the synthetic control method. We also estimate similar FE specifications as in the main text, but include more pre-program years, and present estimates where the dependent variable is disaggregated into costs possibly related to housing and costs unrelated to housing. Finally, we present the raw actual-synthetic cost differences, as well as bootstrap estimates of the significance of these differences.

Table 5.B.1 shows results from fixed effects regressions where we do not apply the weights obtained from the synthetic control method. The estimation samples cover the whole period 1993-2010. To capture the long-run effect for admitted and rejected municipalities in the same regression, we use two dummy variables (*admitted* and *rejected*) that take on the value 1 from 2000 and onwards for the respective groups.

In column (1)-(4) we use per capita operating costs as dependent variable. In column (1) the full sample of 290 municipalities is included. The admitted coefficient is negative, significant and amounts to about 2 percent lower cost level on average, while the rejected coefficient is positive and insignificant. In column (2), we let the dummy variables take the value 1 already in 1999. The admitted coefficient is still negative but now insignificant. The rejected coefficient becomes somewhat more positive, but is still insignificant. In column (3) and (4) we let the samples mimic the donor pools used in the synthetic control estimation: (3) includes the 33 neighbours of admitted municipalities that were not excluded for other reasons, while (4) excludes this group. In these two estimations, we also exclude the admitted and rejected municipalities that we were unable to develop synthetic controls for; i.e. column (3) excludes Älvdalen and Gullspång and column (4) excludes also Dorotea. In line with the baseline estimates presented in section 5.5, the coefficient for the admitted group is negative and significant when neighbours are included, and more or less of the same size as in the full sample, while less negative and insignificant when neighbours are excluded. The coefficient on rejected municipalities is positive, but small and insignificant in both columns which is also in line with our baseline estimates. Furthermore, the coefficient on admitted municipalities is significantly lower than the rejected coefficient on at least the 10 percent

Table 5.B.1: Fixed effects specifications, 1993-2010

Dependent	(1) Full sample *costs*	(2) Prog. 1999 *costs*	(3) Incl neigh. *costs*	(4) Excl neigh. *costs*	(5) Full sample *net rev*	(6) Incl neigh. *net rev*	(7) Excl neigh. *net rev*
admitted	-0.021**	-0.015	-0.020**	-0.014	0.493***	0.554***	0.437**
	(0.010)	(0.010)	(0.010)	(0.011)	(0.152)	(0.172)	(0.181)
rejected	0.006	0.012	0.004	0.007	0.235	0.267	0.194
	(0.010)	(0.010)	(0.010)	(0.011)	(0.205)	(0.210)	(0.220)
$log(taxbase)$	0.652***	0.657***	0.483***	0.509***	-0.465	-0.249	-2.260
	(0.093)	(0.093)	(0.084)	(0.099)	(1.323)	(1.734)	(1.844)
eq.grant	0.0066***	0.0065***	0.0034**	0.0027	0.117***	0.105***	0.0794***
	(0.0020)	(0.0021)	(0.0017)	(0.0016)	(0.024)	(0.027)	(0.025)
$eq.grant^2$	0.0001	0.0001	0.0003***	0.0003***	-0.0013	-0.0021	-0.0011
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0011)	(0.0013)	(0.0014)
employment	-0.0039***	-0.0040***	-0.0030**	-0.0035**	0.055**	0.049	0.058*
	(0.0015)	(0.0015)	(0.0014)	(0.0016)	(0.022)	(0.030)	(0.031)
$log(pop.)$	-0.041	-0.033	0.061	0.075	4.25***	3.62***	3.31**
	(0.064)	(0.064)	(0.073)	(0.084)	(0.89)	(1.19)	(1.36)
share 0-14	0.0065	0.0065	0.0029	0.0034	-0.0058	0.050	0.012
	(0.0041)	(0.0041)	(0.0043)	(0.0049)	(0.057)	(0.069)	(0.073)
share 65+	0.011***	0.010***	0.0058**	0.0065**	-0.0077	0.032	0.035
	(0.0032)	(0.0032)	(0.0025)	(0.0027)	(0.035)	(0.047)	(0.049)
rightwing	-0.00013	-0.00013	-0.00033	-0.00061	0.012**	0.018***	0.020***
	(0.00037)	(0.00037)	(0.00044)	(0.00049)	(0.0056)	(0.0068)	(0.0074)
herfindahl	0.148***	0.147***	0.105*	0.0891	1.168	1.781	1.668
	(0.0509)	(0.0507)	(0.0579)	(0.0641)	(0.842)	(1.094)	(1.175)
seats	0.00025	0.00031	-0.0000	-0.0000	-0.00851	-0.00555	0.00533
	(0.0006)	(0.0006)	(0.0008)	(0.0008)	(0.0105)	(0.0130)	(0.0146)
Constant	0.861	0.760	0.813	0.567	-43.43***	-39.92***	-27.68*
	(0.714)	(0.716)	(0.791)	(0.884)	(10.22)	(14.08)	(14.72)
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,198	5,198	3,474	2,862	5,198	3,474	2,862
Municipalities	290	290	193	159	290	193	159
F	483.8	473.2	403.5	298.0	44.86	36.05	33.23
R^2	0.929	0.929	0.944	0.942	0.237	0.244	0.252

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Column (1) and (2) includes all 290 municipalities.
Column (3): 35 admitted, 22 rejected, and the donor pool of 136 municipalities.
Column (4): 34 admitted, 22 rejected, and the donor pool of 103 municipalities.
Column (5)-(7) use the same sample as column (1), (3), and (4) respectively.

level in all columns (1)-(4).

Column (5)-(7) instead use per capita net revenues as dependent variable. In column (5) we again use the full sample, while column (6) and (7) corresponds to the sample used in columns (3) and (4) respectively. The admitted coefficient is positive, significant and large in all samples: the magnitude corresponds to 500 SEK per capita higher net revenues on average (for comparison, the mean and standard deviation for all municipalities 2000-2010 is 621 and 1,272 SEK per capita respectively). The rejected coefficient is positive, insignificant and about half the size of the admitted coefficient. The difference between the two groups is not significant in any sample.[42]

As in the baseline estimation, we include government grants and its square in these estimations, although this variable may have been directly affected by the program. There is however, just as in the baseline, hardly any effect on the admitted and rejected coefficients if we instead exclude these two variables (results available on request).

In our baseline FE estimations, we use a short sample from 1999-2010 to capture more of the unobserved heterogeneity (as more things should be fixed over a shorter period). This is especially important in the specifications where the fit of the synthetic controls is less good as in the samples excluding neighbours, but also for the estimations of net revenues. However, this approach may be problematic if the difference between actual and synthetic municipalities is large for some idiosyncratic reason in 1999.

To see if this is a problem, we re-run our baseline FE regressions with the samples of actual and synthetic municipalities but use the whole period 1993-2010. When neighbours are included in the sample, this yields similar results for both admitted and rejected – very much alike for costs, somewhat more attenuated coefficients for net revenues but still large and highly significant most years (results not shown). This is fully in line with the view that the fixed effects are less important in these samples. In column (1) and (2) of table 5.B.2, we replicate the potentially more problematic specifications that excludes neighbours for the admitted group. Column (1) shows coefficients using per capita costs as dependent variable and including covariates (compare column (4) of Table 5.2). There are some significant and

[42] To save space, we do not include estimates with the program taking effect in 1999 for net revenues in the table, but both coefficients become smaller and are insignificant in this specification (results available on request).

Table 5.B.2: Fixed effects on longer samples and housing/non-housing related costs

	(1) *costs*	(2) *net rev.*	(3) *housing*	(4) *non-housing*	(5) *housing*	(6) *non-housing*
admitted×2000	0.0172**	-1.147***	0.0124	0.00508		
	(0.00720)	(0.307)	(0.0235)	(0.00574)		
admitted×2001	0.0245***	-0.0762	-0.00536	-0.0134*		
	(0.00863)	(0.362)	(0.0249)	(0.00777)		
admitted×2002	0.00683	0.814**	-0.0963**	-0.0315***		
	(0.00876)	(0.339)	(0.0372)	(0.00826)		
admitted×2003	0.0216*	0.0206	-0.0606	-0.0212***		
	(0.0123)	(0.266)	(0.0468)	(0.00805)		
admitted×2004	0.0141	0.490*	-0.0745	-0.0164*		
	(0.0116)	(0.260)	(0.0704)	(0.00885)		
admitted×2005	-0.0151	1.648***	-0.0657	-0.0213**		
	(0.0115)	(0.286)	(0.0645)	(0.00956)		
admitted×2006	0.0243*	-0.0675	-0.0572	-0.0235**		
	(0.0137)	(0.212)	(0.0670)	(0.0107)		
admitted×2007	0.0180	0.546**	-0.0805	-0.0205*		
	(0.0138)	(0.241)	(0.0654)	(0.0116)		
admitted×2008	0.0213	0.336	-0.0826	-0.0179		
	(0.0142)	(0.248)	(0.0671)	(0.0114)		
admitted×2009	-0.00994	1.124***	-0.0499	-0.0217*		
	(0.0147)	(0.268)	(0.0632)	(0.0122)		
admitted×2010	-0.00145	0.372	-0.0650	-0.0215		
	(0.0159)	(0.259)	(0.0680)	(0.0133)		
admitted					-0.0397	-0.0195***
					(0.0388)	(0.00688)
housing program					-0.0683	0.00760
					(0.0415)	(0.00981)
rejected					-0.103**	0.00308
					(0.0444)	(0.00964)
Constant	1.163	-49.19***	-3.487	0.869	-6.423*	1.354**
			(5.724)	(0.774)	(3.602)	(0.585)
Covariates	Y	Y	Y	Y	Y	Y
Observations	1,224	1,224	2,235	2,235	3,762	3,762
Municipalities	68	68	172	172	290	290
F	1146.3	141.2	6.430	279.3	8.290	475.1
R^2	0.958	0.439	0.182	0.932	0.134	0.925

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
housing includes costs recorded as "infrastructure" (*Infrastruktur*) or ("business activities" *Särskilt riktade insatser*); *non-housing* includes all other costs.

positive years for costs but most are insignificant, especially towards the end of the period where there are also some negative coefficients. In column (2), we show the coefficients for a similar specification with net revenues as dependent variable (compare column (8) of Table 5.2). These are smaller and less significant, but still positive all years except one during 2002-2010, and large and significant for several of these years. For both costs and net revenues we get closer to the coefficients from the estimation on the 1999-2010 sample as we progressively shorten the sample (results available on request). Thus, we do not think that these results should change our main conclusion that fiscal discipline for the admitted group have not deteriorated on average, and have increased for several municipalities.

The changes for the rejected group are larger when we exclude neighbours, especially for costs. The $rejected \times year$ coefficients using costs as dependent variable are consistently positive, larger than in the baseline, and significant for a majority of the post-program years in the 1993-2010 sample. The results for 1999-2010 also seems more special compared to the results for the admitted group, as there are still many positive and significant coefficients for the intermediate sample lengths as well. When we use net revenues as the dependent variable, the coefficients are also smaller and some are negative (although never significant), while there are still some large, positive and significant years in the 1993-2010 sample (all results available on requests). As the synthetic controls have worse fit for the rejected group, we are more reluctant to draw firm conclusions from these results, but the difference to the admitted group definitely seem to remain also in these specifications.

Columns (3)-(6) in Table 5.B.2 show FE models with the per capita costs variable disaggregated into two: costs potentially related to housing and costs unrelated to housing. As discussed in Section 5.6, we want to examine whether the cost reductions of admitted municipalities are only a mechanical implication of having reconstructed their troubled housing companies.[43] This may be the case if municipalities made transfers to their troubled housing companies before the reconstruction began, but no longer

[43] It is common practice to have municipally owned commercial real estate and apartments for rent in a separate limited liability company, and not as a part of the regular municipal administration. All municipalities admitted to both programs except one (a non-reducer) followed this common practice already before the two programs started, the cost reductions should thus not be caused by reducers simply moving housing costs off the revenues and cost statement and into a separate company.

have a reason to do so after the reconstruction. The cost reductions we find in our synthetic control estimations are then unrelated to changes in fiscal discipline. The dependent variable *housing* covers the bookkeeping posts where transfers to housing companies should be recorded (Statistics Sweden, 2012a, p. 41 and 50);[44] it should however be noted that these posts contain a lot more than just housing related costs. *non-housing* covers all other bookkeeping posts. In columns (3) and (4), the estimation sample consists of admitted municipalities and the donor pool including neighbours during the period 1998-2010 (we do not have data over the different areas of costs further back). The estimates show that the admitted municipalities have had significantly lower values of *non-housing* during most of the post-program period, while the level of (potentially) housing-related costs is not significantly different except in 2002 (although the point estimates are sometimes large).

In columns (5) and (6) of Table 5.B.2, we estimate a FE model for the full sample of municipalities while including single-dummies for admitted and rejected. This allows us to study also the municipalities that were in the housing program but that did not apply to the bailout program. *housing program*, a dummy equal to one from the year a municipality was admitted to the housing program and onwards, is insignificant for both types of costs (although very close to significant for potentially housing related costs). The admitted dummy is negative but insignificant for housing related costs, while negative and significant for non-housing related costs. This result does not support the hypothesis that the program effect for the cost-reducers was only due to their participation in the housing program.

5.B.1 Synthetic control estimates and inference

This section displays the yearly averages of the raw actual-synthetic difference in costs. Starting with the results when neighbours are *included* in the donor pool, the solid black lines in Figure 5.B.1 shows average per capita costs for admitted (left panel) and rejected (right panel) municipalities; the dashed black lines show the corresponding averages for the synthetic controls. The gray lines display the corresponding graphs for the placebo group, that is, the donor pool (note that admitted and rejected have the same placebo group); evidently and reassuringly, there are no signs of any

[44] *Infrastruktur* and *Affärsverksamhet*.

program effect for the placebo group. Only observations with a pre-RMSPE lower than the 0.05 cut-off are included in the figure. Results for each RMSPE cut-off are shown in Table 5.B.3.

The inference on the yearly average program effects in Section 5.5 relies on standard errors from the fixed effects estimations. As an alternative way to evaluate the statistical significance of the yearly average program effect, we use a variant of the method recently suggested by Cavallo et al. (2011). Let $N_p, p = a, r$ be the number of units affected by the program, where a denotes admitted municipalities and r denotes rejected. The average of the difference in per capita costs between each actual municipality and its synthetic control in year t is then

$$\bar{\alpha}_t = \frac{\sum_{i=1}^{N^p} y_{it} - y_{it}^{synth}}{N_p}. \tag{5.4}$$

Cavallo et al. ask how rare it is to encounter an average effect, computed over N_p units, amounting to the estimated program effect. They thus calculate the average effects for each possible combination of N_p-sized samples drawn from the donor pool, and check where the program effect ends up in this distribution.

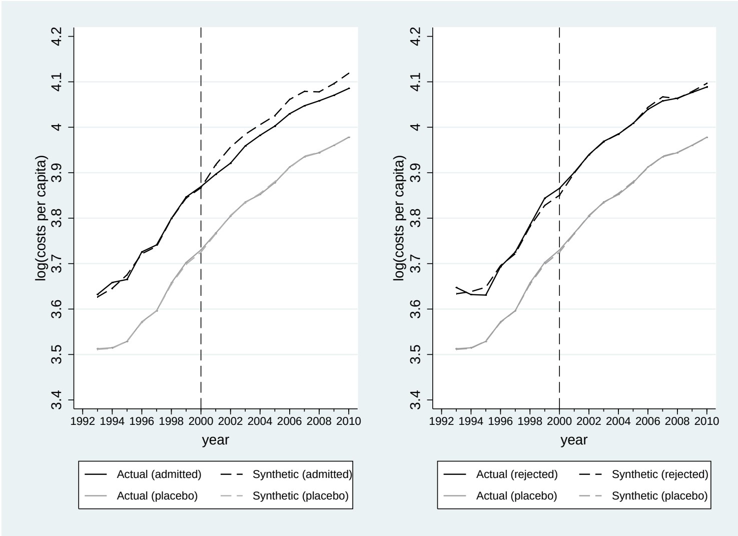
We modify the method slightly because of our large donor pool. We choose to draw (with replacement) 10 000 bootstrap samples of size N_p from the donor pool for each of the eleven years during and after the program. We then compute the "p-value" of the average program effect in year $t \geq T_0$, i.e. the probability to observe such a large/small effect in the absence of program, as

$$p - value_t = \frac{\sum_{dp=1}^{10000} \mathbf{1}\left(\bar{\alpha}_t^{dp} < \bar{\alpha}_t\right)}{10000} \tag{5.5}$$

where $\bar{\alpha}_t$ is defined as in equation (5.4), $\bar{\alpha}_t^{dp}$ is the average placebo effect in bootstrap sample dp_t, and $\mathbf{1}(\cdot)$ is an indicator function taking the value 1 whenever an average from the donor pool is lower than the program average, if we are doing inference about negative point estimates (vice versa for positive estimates). The p-values can be interpreted as an estimate of whether a certain average program effect is large compared to the placebo effects and therefore also tells us if the effect is likely to be due to chance.

As would be expected given the small magnitudes, the actual-synthetic differences are rarely significant for the *rejected*. For the *admitted* municipalities, however, the bootstrap p-values suggest that the effects are unlikely

Figure 5.B.1: Actual and synthetic average per capita (log) costs of services for admitted, rejected, and placebo municipalities, pre-RMSPE < 0.05 (incl. neighbours in donor pool)



to be due to chance: from 2001 and onwards, the p-values are well below 0.05.

Figure 5.B.2 (again for municipalities with pre-RMSPE < 0.05) and Table 5.B.4 show the results when neighbours are *excluded* from the donor pool. As discussed in the main text, the estimates are not as stable over the different cut-offs as when neighbours were included in the donor pool. For the admitted, the average differences are now positive and significant until 2009 when looking at columns (1) and (3), where the relatively lax pre-RMSPE cutoffs are applied. For the observations with lower pre-program prediction error than 0.03 (column (5) and (7)), the estimates are positive and significant in the first years but turns towards zero already in 2004; the differences in 2005 and 2009 are even significantly negative in column (5). For the rejected, we see positive and significant effects until 2009 at most cut-offs, though it should be noted that more than half of the rejected municipalities fail to pass the lower pre-RMSPE cut-offs.

There are some discrepancies between the results reported here and the ones reported in Tables 5.2 and 5.3 when we exclude neighbours from the donor pool. This is not surprising: it becomes more important to control

Table 5.B.3: Average program effects by year ($\bar{\alpha}_t$) incl neighbours in donor pool

	All		pre-RMSPE < 0.05		pre-RMSPE < 0.03		pre-RMSPE < 0.02	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Adm.	Rej.	Adm.	Rej.	Adm.	Rej.	Adm.	Rej.
Year	$N_a = 35$	$N_r = 22$	$N_a = 34$	$N_r = 21$	$N_a = 28$	$N_r = 16$	$N_a = 23$	$N_r = 9$
2000	0.001	0.015*	0.003	0.015*	0.001	0.012	-0.001	0.002
	(0.640)	(0.058)	(0.683)	(0.065)	(0.752)	(0.144)	(0.256)	(0.547)
2001	-0.022***	-0.000	-0.021***	0.001	-0.022***	0.001	-0.025***	0.003
	(0.000)	(0.472)	(0.000)	(0.515)	(0.000)	(0.495)	(0.000)	(0.381)
2002	-0.035***	0.002	-0.035***	-0.000	-0.039***	-0.004	-0.045***	0.007
	(0.000)	(0.323)	(0.000)	(0.567)	(0.000)	(0.421)	(0.000)	(0.180)
2003	-0.027***	0.003	-0.026***	0.001	-0.030***	-0.000	-0.034***	0.019**
	(0.000)	(0.354)	(0.000)	(0.499)	(0.000)	(0.486)	(0.000)	(0.024)
2004	-0.024***	0.004	-0.023***	-0.001	-0.028***	0.001	-0.029***	0.019*
	(0.004)	(0.215)	(0.002)	(0.562)	(0.002)	(0.356)	(0.002)	(0.03)
2005	-0.025***	0.003	-0.023***	0.001	-0.023***	0.010	-0.030***	0.026***
	(0.002)	(0.255)	(0.002)	(0.384)	(0.001)	(0.101)	(0.001)	(0.008)
2006	-0.035***	-0.003	-0.031***	-0.005	-0.032***	-0.001	-0.043***	0.008
	(0.000)	(0.422)	(0.001)	(0.334)	(0.000)	(0.501)	(0.000)	(0.275)
2007	-0.034***	-0.007	-0.032***	-0.009	-0.029***	-0.004	-0.044***	0.005
	(0.000)	(0.329)	(0.001)	(0.268)	(0.000)	(0.471)	(0.000)	(0.271)
2008	-0.021**	0.001	-0.020**	0.001	-0.017***	0.004	-0.032***	-0.001
	(0.017)	(0.455)	(0.024)	(0.440)	(0.006)	(0.305)	(0.006)	(0.582)
2009	-0.029***	-0.003	-0.026***	-0.002	-0.025***	0.000	-0.039***	0.003
	(0.002)	(0.426)	(0.003)	(0.442)	(0.000)	(0.434)	(0.000)	(0.341)
2010	-0.034***	-0.007	-0.033***	-0.008	-0.031***	-0.010	-0.040***	-0.001
	(0.000)	(0.298)	(0.000)	(0.260)	(0.001)	(0.255)	(0.001)	(0.564)

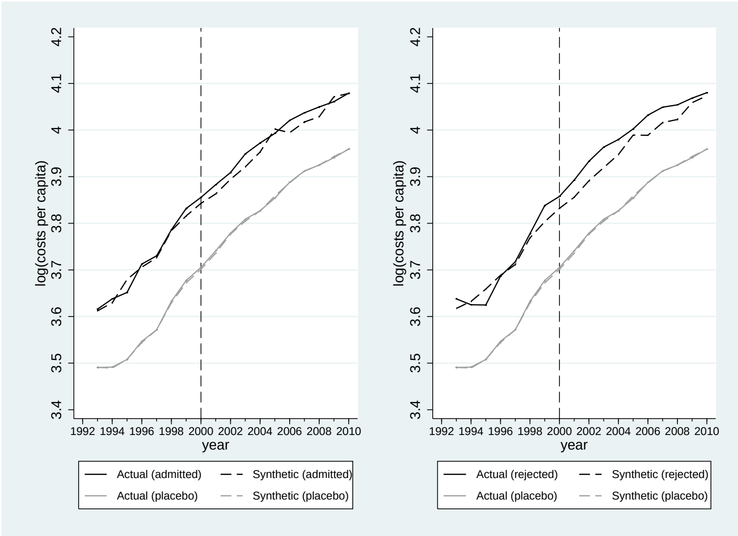
p-values in parentheses.
N_a = number of accepted municipalities with pre-RMSPE < cut-off
N_r = number of rejected municipalities with pre-RMSPE < cut-off

Table 5.B.4: Average program effects by year ($\bar{\alpha}_t$) excl neighbours from donor pool

	All		pre-RMSPE < 0.05		pre-RMSPE < 0.03		pre-RMSPE < 0.02	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Adm.	Rej.	Adm.	Rej.	Adm.	Rej.	Adm.	Rej.
Year	$N_a = 34$	$N_r = 22$	$N_a = 30$	$N_r = 20$	$N_a = 22$	$N_r = 10$	$N_a = 17$	$N_r = 7$
2000	0.017***	0.025***	0.013*	0.026***	0.009	0.007	0.013*	-0.002
	(0.003)	(0.000)	(0.061)	(0.001)	(0.190)	(0.349)	(0.085)	(0.298)
2001	0.024***	0.036***	0.020***	0.037***	0.016**	0.014	0.019**	0.007
	(0.000)	(0.000)	(0.010)	(0.000)	(0.025)	(0.150)	(0.012)	(0.352)
2002	0.019***	0.044***	0.015*	0.042***	-0.003	0.018*	-0.003	0.000
	(0.010)	(0.000)	(0.056)	(0.000)	(0.436)	(0.060)	(0.408)	(0.471)
2003	0.031***	0.045***	0.028***	0.044***	0.007	0.028**	0.015**	0.017*
	(0.000)	(0.000)	(0.001)	(0.000)	(0.234)	(0.014)	(0.045)	(0.100)
2004	0.021***	0.035***	0.019**	0.032***	-0.001	0.022**	0.010	0.018*
	(0.007)	(0.000)	(0.014)	(0.002)	(0.554)	(0.039)	(0.130)	(0.094)
2005	-0.010	0.013**	-0.009	0.013**	-0.028***	0.019**	-0.015	0.016*
	(0.250)	(0.050)	(0.285)	(0.044)	(0.004)	(0.038)	(0.141)	(0.063)
2006	0.030***	0.045***	0.026**	0.043***	0.001	0.036***	0.006	0.028**
	(0.003)	(0.001)	(0.013)	(0.002)	(0.319)	(0.005)	(0.222)	(0.049)
2007	0.024***	0.037***	0.020**	0.033***	-0.004	0.016*	0.000	0.015
	(0.008)	(0.003)	(0.038)	(0.009)	(0.530)	(0.088)	(0.391)	(0.165)
2008	0.024**	0.035***	0.021**	0.031**	-0.002	0.017	0.005	0.010
	(0.012)	(0.007)	(0.038)	(0.016)	(0.604)	(0.103)	(0.262)	(0.250)
2009	-0.009	0.010	-0.011	0.010	-0.033**	-0.006	-0.016	-0.015
	(0.329)	(0.177)	(0.249)	(0.198)	(0.017)	(0.496)	(0.208)	(0.317)
2010	0.000	0.010	0.001	0.007	-0.021	-0.011	-0.008	-0.027
	(0.433)	(0.204)	(0.467)	(0.309)	(0.121)	(0.383)	(0.375)	(0.138)

p-values in parentheses.
N_a = number of accepted municipalities with pre-RMSPE < cut-off
N_r = number of rejected municipalities with pre-RMSPE < cut-off

Figure 5.B.2: Actual and synthetic average per capita (log) costs of services for admitted, rejected, and placebo municipalities, pre-RMSPE < 0.05 (excl. neighbours in donor pool)

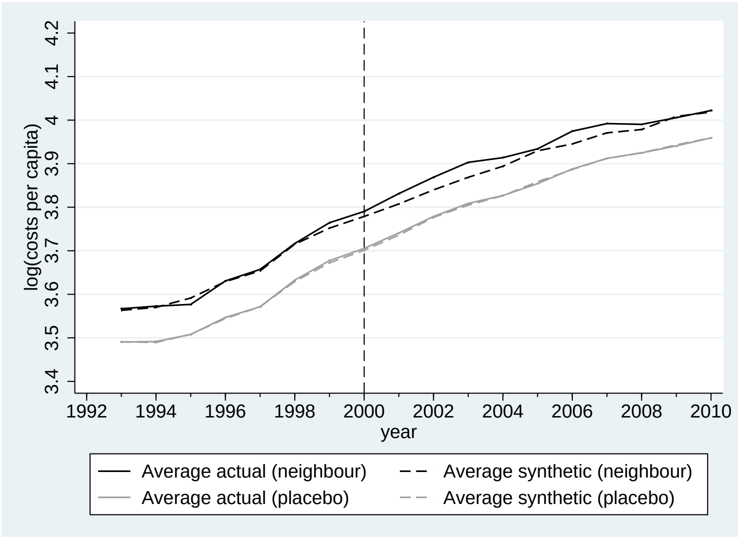


for unobservable, time-invariant characteristics and observable time-variant characteristics when the match between actual and synthetic controls is worse. That the estimates are similar, especially for the admitted group, for the samples including neighbours are reassuring.

5.C Synthetic controls for neighbours

Here, we report results from the estimation of synthetic controls for the 33 municipalities that are neighbours to at least one municipality admitted to the program and not excluded from the donor pool for other reasons. The donor pool consists of 103 municipalities as described in Section 5.4.2. Apart from 1995 and 1999, pre-program fit is in general good for the neighbours (average pre-RMSPE is 0.020). However, there are some prominent exceptions for which the algorithm fails to find good controls, especially Lycksele (pre-RMSPE = 0.079), Vilhelmina (0.065) and Åmål (0.049). Notably, Lycksele contributes to the synthetic control (i.e. has a weight>0) of 14 admitted municipalities, Vilhelmina contributes to 13, and Åmål to 4 (Lycksele's average weight is 0.115, Vilhelmina's is 0.337, and Åmål's is

Figure 5.C.1: Actual and synthetic average log costs per capita, neighbours and placebo group



0.197). It is therefore unfortunate that we do not get very precise estimates of the "program effect" for these municipalities.

Figure 5.C.1 shows that the average of neighbours' actual costs are higher than the average of synthetic costs for several of the post-program years (as well as for 1999). Only the 31 municipalities with a pre-RMSPE < 0.05 were included in the computation of the average shown in the figure. An examination of the average (over 2000-2010) difference of each individual neighbour suggests that the positive differences found on average are driven by 6 municipalities (including Åmål). 2 neighbours have instead reduced their costs relative to their synthetic controls. It is worth emphasizing that 23 of the 31 neighbours with pre-RMSPE<0.05, i.e. an overwhelming majority, are quite close to their synthetic controls; in other words, seemingly unaffected by the program.

5.D Tests of equal means and equal proportions

Table 5.D.1 shows the group means (or proportions) and two-sided tests of equal means (proportions) for a set of explanatory variables.

neighbours' weight indicate the proportion of a municipality's synthetic control that derives from neighbours. I.e. if two donors contribute to a synthetic control and one of them is a neighbour with weight 0.7, then *neighbours' weight* equals 0.7 for this synthetic control (recall that the total weight is normalized to 1). *housing program* is a dummy equal to one if the municipality was ever in the housing program, and zero otherwise. The next two variables relate the grant received from *Kommundelegationen* and, respectively, the total grants from the bailout program (Kommundelegationen) *and* (if applicable) the housing program, to the municipality's total costs of services in 2000. The variables *number of bailouts* and *share of neighbours bailouts* were presented in Section 5.3.2; note that they concern the period 1979-1992. Regarding the political variables, *close election in 1998* is a dummy equal to one if right-wing parties got between 45 and 55 percent of the council seats after the 1998 election and *years, left majority* counts the number of years (during 2000-2010) that the Leftist party and the Social Democrats together have had more than 50 percent of the council seats.

There are also some self-explanatory structural variables (see also Section 5.3.2); here, Δ-variables measure the relative change over 2000-2010. The mean (over 2000-2010) of population density (inhabitants/km^2) is included because it may be more difficult to reduce costs if the population is more spread out (due to fixed costs).

We also set out to examine institutional features of the budget process and some measures of motivation for fiscal discipline, using survey data collected by the Swedish Association of Local Authorities and Regions (SALAR) in 2004 and by ourselves in 2010 (Dietrichson and Ellegård, 2012). From these surveys, we take some institutional variables that were significantly correlated with better fiscal performance in the Swedish municipalities in Dietrichson and Ellegård (2012). The third survey was conducted by Statistics Sweden in the election years 1998 and 2002. The variable *help from SALAR* 2000-2010 tests for differences between the groups in their propensity to buy consultant services from SALAR that have a special unit that, against a fee, helps municipalities to improve their fiscal situation (personal communication).

The *surplus/deficit rules*-variable, measured in 1998 and 2002, indicate whether there *are* regulations regarding local committees' surpluses and deficits, but does not specify what type of regulation. *centralization*, which is available only for 2010, measures the presence of restrictions on the bargaining power of local committees and administrations in the budget process. *centralization* is an ordinal variable with four categories, where 1 implies most centralized and 4 implies least centralized. The dummy variables *keep surplus* and *keep deficit*, measured in 2004 and 2010, indicate whether local committees are allowed to carry over surpluses/have to carry over deficits to the next fiscal year or not. *manager risk*, measured in 2010, is a dummy variable that equals 1 if managers of local administrations run a relatively high risk of being replaced if they repeatedly run deficits. The dummy *long term budget* indicates whether the multi-year budget is viewed as an important commitment by politicians or not. The last dummy variable, *conflicts of interests* (also this from 2010), equals 1 if a municipality reports that the executive committee and the municipal council assign higher importance to fiscal discipline than local committees.

Table 5.D.1: Sources of heterogeneity

VARIABLE	Reducers (n=12) Mean/Prop.	Non-reducers (n=23) Mean/Prop.	z/t	p-value
neighbours' weight	0.742	0.602	0.83	0.408
housing program	0.833	0.522	1.81	0.070
grant Kommundelegationen/total costs	0.055	-0.037	-1.97	0.067
total program grants/total costs	0.166	0.075	2.86	0.011
nr of bailouts	10.42	6.480	2.76	0.013
share of neighbour bailouts	0.565	0.465	1.90	0.068
mean, share right-wing 2000-10	30.40	41.70	-2.80	0.010
years, left majority 2000-10	8	6	1.23	0.230
close election in 1998	0	0.348	-2.33	0.020
mean, herfindahl 2000-10	0.277	0.275	0.15	0.880
debt incl pensions 1998	39.31	35.94	0.85	0.409
fees mean 2000-10	12.01	11.89	0.10	0.918
Δ *fees* 2000-10	0.104	0.369	-2.56	0.016
total revenues mean 2000-10	59.98	57.36	0.96	0.346
Δ *total revenues* 2000-10	0.218	0.298	-3.40	0.002
tax rate mean 2000-10	22.37	22.16	0.98	0.338
Δ *tax rate* 2000-10	0.012	0.023	-1.54	0.137
tax base mean 2000-10	138.4	134.7	1.09	0.287
Δ *tax base* 2000-10	0.350	0.356	-0.29	0.776
employment rate mean 2000-10	52.76	51.84	0.63	0.537
Δ *employment rate* 2000-10	0.022	0.012	0.47	0.641
equalization grants mean 2000-10	13.10	12.09	0.51	0.619
Δ *equalization grants* 2000-10	0.464	0.361	0.64	0.527
population size mean 2000-10	12047	11682	0.15	0.879
Δ *population size* 2000-10	-0.075	-0.057	-0.89	0.384
mean, population density 2000-10	11.76	20.66	-1.32	0.196
share 0-14 mean 2000-10	16.06	16.04	0.03	0.981
Δ *share 0-14* 2000-10	-0.180	-0.179	-0.03	0.974
share 65+ mean 2000-10	22.45	22.93	-0.38	0.705
Δ *share 65+* 2000-10	0.114	0.146	-0.93	0.362
help from SALAR 2000-10	0.417	0.478	-0.24	0.810
centralization	3	2.94	0.17	0.863
keep surplus 2004	0.181	0.227	-0.30	0.763
keep surplus 2010	0.300	0.333	-0.18	0.856
keep deficit 2004	0.091	0	1.40	0.160
keep deficit 2010	0.200	0.111	0.64	0.520
surplus/deficit rules 1998	0.500	0.522	-0.12	0.903
surplus/deficit rules 2002	0.333	0.500	-0.94	0.350
manager risk	0.667	0.800	-0.73	0.465
long-term budget 2004	0.272	0.363	-0.522	0.601
long-term budget 2010	0.200	0.389	-1.03	0.305
conflicts of interest	0.800	0.611	1.03	0.305