Profiling, Auditing and Public Policy: Applications in Labor and Health Economics

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CHAPTER 4

The Effect of Audit Regimes on Applications for Long-Term Care*

4.1 Introduction

In the provision of long-term care a trade-off has to be made between providing services quickly when needed and efficient spending of public resources. Long-term care expenditures have been expanding in many countries in the last decade. This raises the question how these costs can be controlled in an adequate way while maintaining or improving targeting efficiency in the provision of long-term care services. Some countries introduced a so-called gatekeeper for this purpose, which is an institution responsible for controlling access to long-term care services. The gatekeeper seeks for effective audit policies to manage the provision of long-term care.[1] In the design of audit policies the limited budgets for auditing and the ultimate goal of providing adequate care at the right place have to be balanced. How auditing budgets can be utilized in an efficient way for long-term care cost containment is a

*This chapter is based on Lindeboom et al. (2015b).

[1]Long-term care systems differ considerably across countries, not only in terms of the types of care covered, but also in terms of the systems implemented to manage expenditures. In particular, differences exist in terms of whether access decisions are made at the local (municipal or regional) or at the national level (Colombo et al., 2011). Secondly, in many countries, care providers make the assessment of needs, without an independent institution checking care needs prior to provision of services, whereas in other countries a separate institution makes need assessments. For instance, in Belgium, care providers make the assessment and this is combined with frequent ex-post random evaluations of the assessments (Willemé, 2010). For detailed cross-country comparisons of the long-term care systems, we refer to Colombo et al. (2011) (chapters 7 and 9 in particular) and Thomson et al. (2013).

question of importance for policy makers. For this, several audit policy instruments are available, such as audit frequency, degree of control, (financial) sanctions, and performance incentives. The choices of these instruments are important determinants of the speed at which an application can be approved and care can be provided and of the targeting efficiency in care provision.

Our study concerns the Netherlands, where long-term care involves chronic care, either institutionalized or home-based, for the elderly and the mentally or physically impaired. When a patient requires long-term care services, s/he approaches a care provider and, subsequently, the care provider files an application for (public) funding to the (national) gatekeeper. In the application, the care provider asks, amongst others, for a specific type and amount of care services and the way in which this is to be delivered to the patient (e.g., home-based or institutionalized). In addition, information on the health conditions of the patient should be provided by the care provider. The gatekeeper can either approve the application directly or perform an audit. In case of an audit, assessors employed by the gatekeeper study the actual care needs of the patient and compare this to care services that have been requested by the care provider. An audit can result in an approval, denial or adjustment of the application. An adjustment can imply that the amount of care for which the provider receives funding, differs from what has been requested by the provider. Care providers do not start the care provision until the gatekeeper has approved the application. As a consequence, audits delay the moment at which the patient receives care services. This is referred to as *ex-ante* auditing.

In this chapter, we conduct a large-scale field experiment to compare the ex-ante audit regime to two alternative audit regimes. The first is an *ex-post* audit regime, which allows that care provision starts immediately after filing the application. Only after the start of care provision, the gatekeeper selects applications for audit. Unlike after an ex-ante audit, the requested services cannot be denied or adjusted in an ex-post audit. The degree of control of the gatekeeper is thus smaller when applying ex-post auditing compared to ex-ante auditing, but ex-post auditing has the advantage of immediate provision of services to patients. Both ex-ante and ex-post auditing do not allow for (financial) sanctions in case of disapproval. Prior to our experiment ex-post auditing was already used for certain cheaper and less extensive types of long-term care (Lindeboom et al., 2015a). The main motivation for the field experiment studied in this chapter is that the gatekeeper believes that many care providers are intrinsically motivated to file applications correctly and in accordance with the actual care needs of patients. In the opinion of the gatekeeper ex-post auditing can therefore be equally effective as ex-ante auditing, while ensuring immediate access to care services.

In addition to the ex-ante and the ex-post audit regime, we introduce a *conditional* audit regime. In this regime, the timing of audits (i.e., ex-ante or ex-post auditing) is adjusted regularly during the experiment based on recent approval rates. The approval rate, which is the fraction of audited applications that is approved by the gatekeeper, serves as a proxy for application quality and performance of the care provider. Care providers that perform well, i.e., have high audit approval rates, are exposed to ex-post auditing, while care providers with low audit approval rates are subject to ex-ante auditing. Compared to the ex-ante audit regime, the conditional audit regime has a lower overall degree of control, but control is targeted towards poorly performing providers. This regime yields fewer delays in care provision than the ex-ante audit regime, but introduces incentives for care providers to comply because of the threat of switching between ex-ante and ex-post auditing. Care providers are randomized in one of the three audit regimes. We study the effects of the audit regime on application behavior of care providers, in particular the quantity and quality of applications. We are interested in behavioral responses of care providers to audit regimes. Therefore, we focus on quantity and quality of applications which could, in turn, have a direct effect on the long-term care budget. We do not consider effects on targeting efficiency of long-term care and health effects for patients. These health effects will only become visible in the long run, so beyond our current observation period.

The theoretical literature on auditing started with the traditional (tax) auditing model of Allingham and Sandmo (1972), which interprets the decision of agents to comply with the rules as an economic trade-off of the costs and benefits associated with noncompliance. In contrast to the traditional model, behavioral models focus on the role of intrinsic motivation and trust. Bohnet et al. (2001) conclude from a laboratory experiment that the longer agents are trusted (low enforcement probabilities), the more likely they are to act trustworthy. Mendoza and Wielhouwer (2015) show in a game-theoretical model that trust-based regulation is feasible if agents care sufficiently about the future and if there are some costs of control to the agent. Falk and Kosfeld (2006) find in a laboratory experiment that most agents reduce performance when the principal chooses to control. This suggests that control crowds out intrinsic motivation of agents, labeled the hidden costs of control. With only weak incentives available to the principal, trusting agents provides more favorable outcomes (Falk and Kosfeld, 2006). Although the studies above concern individuals, similar studies have been done for (non-profit) organizations. Bengtsson and Engström (2014) conduct a field experiment where they switch from a fully trust-based regime to one with increased monitoring for some non-profit organizations, but they do not find evidence for a crowding out effect.

Our study also relates to the literature on external interventions such as sanctions or rewards, or in our case a delay in provision, possibly crowding out intrinsic motivation. In multiple settings a crowding-out effect due to the presence of external (financial) interventions has been found. For instance, Frey and Oberholzer-Gee (1997) find that the willingness of individuals to agree with unwanted projects in their neighborhood decreases when compensation is offered. Gneezy and Rustichini (2000b) find that, even though higher monetary rewards increase performance, the introduction of financial incentives in itself may have detrimental effects on performance in IQ-tests and on doing volunteer work. Leuven et al. (2010) find a similar result when studying financial rewards for academic achievement. Finally, Gneezy and Rustichini (2000a) conclude that the introduction of a fine increases the number of parents picking up their children late from day care.[2]

These branches of the literature help us in defining hypotheses on the effects of the audit regime on care provider application behavior. Following the lines of thought of the traditional model, care providers should file fewer (invalid) applications of higher quality under the ex-ante and conditional audit regimes as compared to the ex-post audit regime. On the contrary, in our setting, intrinsic motivation can be interpreted as the willingness of providers to file applications according to the rules of the gatekeeper. This would lead us to hypothesize compliance to be highest in the ex-post audit regime, intermediate in the conditional audit regime and lowest in the ex-ante audit regime. This translates to fewer (invalid) applications and higher approval rates in the ex-post and, to a lesser extent, conditional audit regime compared to the ex-ante audit.

We use a large administrative data set containing information on long-term care applications filed by care providers, and find that when being exposed to the conditional audit regime care providers file 20% fewer applications than in the least strict ex-post audit regime. To a large extent, this seems to be the result of

[2]Reviews of this literature are provided by, for example, Fehr and Gächter (2000), Frey and Jegen (2001) and Gneezy et al. (2011). Various mechanisms are suggested for this effect (listed by, e.g., Falk and Kosfeld, 2006): agents may feel insulted (e.g., Gneezy and Rustichini, 2000b), the usage of incentives may provide new information on the importance or costs of the activity (e.g., Gneezy and Rustichini, 2000a; Bénabou and Tirole, 2003), or incentives may be in contradiction with social norms of fairness, trust or cooperation (e.g., Fehr and List, 2004; Sliwka, 2007).

substitution to other types of long-term care applications. Furthermore, we find an (insignificant) 10% decline in the number of applications in the ex-ante audit regime compared to the ex-post audit regime. Our results show some negative effects on the quality of applications (audit approval rate), for care providers in the ex-ante and conditional regimes compared to the ex-post audit regime. We provide some evidence that part of this effect can be attributed to behavior of assessors who screen ex-post audits less accurately.

The remainder of this chapter is structured as follows. The next section provides institutional details on the Dutch long-term care market. Section 4.3 describes the experiment and discusses hypotheses on the effects of the audit regimes. The data are described in section 4.4. The econometric analysis and results are discussed in section 4.5. Finally, section 4.6 concludes.

4.2 Institutional background

Each person living in the Netherlands is publicly insured for long-term care by means of the Exceptional Medical Expenses Act (AWBZ).[3] This covers chronic care, either institutionalized or home-based, for the elderly, the mentally and/or physically impaired, and chronic psychiatric patients. On July 1, 2013, around 4.8% of the Dutch population qualified for receiving long-term care. Long-term care expenditures are financed by means of general taxation and co-payments. As in many countries, long-term care expenditures in the Netherlands increased substantially in the past decade; real expenditures increased by 60% from 1998 to 2012.

Figure 4.1 illustrates the process of applying for long-term care services prior to the experiment. When a patient wants to receive long-term care services, she should contact her preferred care provider.[4] The care provider studies the situation and care needs of the patient and files a request to the gatekeeper, which is a national authority that has the task to manage access to long-term care services. The request states which types and amounts of care a patient needs, the way in which this care

[3]The description of the institutional details in this section is similar to Lindeboom et al. (2015a) and draws on Mot (2010), Nederlandse Zorgautoriteit (2013) and information from the Center for Care Assessment (CIZ).

[4]A patient can file a request directly to the gatekeeper and indicate the preferred care provider in this request, but around 85% of the applications are filed by care providers. Applications filed by the patient are always thoroughly checked by the gatekeeper before an assessment of needs is provided.

has to be delivered and for which period.[5] After having received the application, the gatekeeper decides about a so-called *assessment of the needs* stating amongst others the type and amount of care services the patient is eligible for. The decision can either be the consequence of an audit or the gatekeeper can simply follow the request of the care provider (without an audit). As soon as the decision is made, the provider can start delivering the care to the patient. This implies that a budget is available for providing all approved care. The care provider receives funding from this budget for all care delivered to the patient (within the restrictions set by the assessment of needs).

In 2012, there were around 1,600 care providers in the Netherlands of which almost 1,400 provided extramural and 800 provided intramural care. Intramural care providers are required to be non-profit institutions, while extramural care providers can also be for-profit organizations. The care providers differ considerably in size. Some providers are large-scale nursing homes, whereas other care providing institutions consist of a very small number of care givers who visit patients at their homes. Next to goals of profit maximization, as can be the case for extramural care providers, incentives for the provision of high quality care exist. This is the case since patients are allowed to switch providers during care provision.[6]

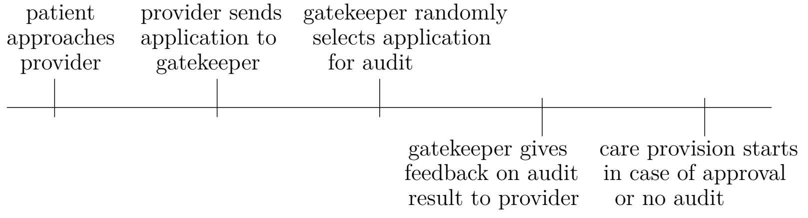
A provider can choose between various types of applications. Which application is suitable typically depends on the long-term care history of the patient and the care needs. In case of a first or complex application, an application type has to be filed that is subject to more intensive screening by the gatekeeper. On the contrary, if care is really quickly needed (in emergency cases), an application type for immediate care provision for a short duration could be used. In the field experiment studied in this chapter, we focus on the so-called re-assessment applications.[7] Re-assessment applications are used to prolong the provision of existing care services, possibly adjusted in, for instance, intensity. This can concern extramural or intramural care. It is not possible to use a re-assessment application in case of a first or a complex application. In 2012, around 70,000 re-assessment applications were filed.

[5]Types of care services are: personal care (e.g., help with showering), nursing (e.g., wound care), assistance (help in organizing practical matters in daily life), treatment (e.g., rehabilitation) and stay in an institution (e.g., nursing home).

[6]In the period from July 2010 to July 2011, about 2.5% of those receiving extramural care through a re-assessment application switched provider, while this was 8% for intramural care (Nederlandse Zorgautoriteit, 2013).

[7]The gatekeeper selected this application type for the experiment. This choice was amongst others related to the feasibility of implementation of the audit regimes in the software that is used for registration and auditing. The label re-assessment application is used by the gatekeeper.

Figure 4.1: Process of application for long-term care services (before the experiment).



Re-assessment applications usually result in care provision for a long period of up to 15 years.[8] Hence, the associated expenses are substantial and often much larger than for other types of applications. Therefore, although re-assessment applications are a specific type of application that do not form the majority of applications filed to the gatekeeper in terms of quantity, their importance in the long-term care budget is substantial.

A random sample of around 25% of the re-assessment applications are audited. Applications selected for audit are randomly divided among specialized assessors. In an audit, assessors make an assessment of the care needs of the patient in line with eligibility rules set by the government. The outcome of an audit is an 'approval' or 'disapproval'. An approval means that requested care is in line with the needs assessment made by the assessor. Requested care can then be delivered to the patient by the care provider. A disapproval can either be an adjustment, meaning that requested and adjudged care services can differ, or denial of the application. In the latter case, a new application for long-term care services has to be filed. Disapproval may result for various reasons. For instance, the type of limitations making the patient eligible for long-term care may differ, different types of care services may be needed or the necessary amount of care may be different. After an audit, care providers receive feedback on the audit from the assessor, explaining what went wrong and how the quality of future applications can be improved.

Only when an assessment of needs has been provided by the gatekeeper, a care provider is allowed to start care provision. Therefore, when a re-assessment application is selected for audit, care provision is delayed. The care provider has to wait with care provision until approval from the gatekeeper in that case. The audit

[8] The median duration of re-assessment applications in our data is 5478 days, i.e. approximately 15 years.

result should be made available within two weeks (on average, it is known within a week).

As briefly mentioned earlier, besides the re-assessment applications that are the main focus of our field experiment, providers can choice to file alternative application types. Although there are guidelines for the type of application to be used in a particular situation, the possibility for substitution exists. Alternative types of long-term care applications that providers can file are '*emergency care applications*', '*standard applications*', '*intramural notifications*', and '*regular applications*'. Emergency care applications make care immediately employable, but only for a period of two weeks after which another application is needed. Standard applications cover a limited set of services concerning cheaper care for a relatively short period. These applications are subject to ex-post auditing.[9] Intramural notifications concern intramural care for patients older than 80 years. Finally, regular applications are used for first and complex applications. Using this application type, care provision can start within six weeks, after completion of an audit.

4.3 Experimental design

The goal of our field experiment is to study the effects of being exposed to different audit regimes on application behavior of care providers. In the first subsection we describe the set-up of the field experiment. The randomization of care providers over the three treatment groups is discussed in subsection 4.3.2. Subsection 4.3.3 states the hypothesized effects of the audit regimes on application behavior of care providers.

4.3.1 Set-up

Prior to the start of our field experiment on September 17, 2012, re-assessment applications were subject to an ex-ante audit regime. During the experiment, which ran until April 7, 2013, we randomly assigned providers to (1) an ex-ante audit regime, (2) an ex-post audit regime, and (3) a conditional audit regime. Compared to ex-ante auditing, ex-post auditing allows immediate provision of requested care services, but removes the possibility to adjust or deny applications. In particular,

[9] Lindeboom et al. (2015a) study the effect of the audit frequency in this ex-post auditing regime for this particular type of application and found no effect of the audit rate on application behavior of care providers.

no (financial) sanctions can be applied in case of disapproval. Ex-post auditing is thus less strict than ex-ante auditing. In the conditional audit regime the timing of audit can switch between ex-ante and ex-post, depending on observed performance in the recent past. In particular, a provider is subject to ex-post auditing if the audit approval rate over the last eight weeks is at least 0.92. When the approval rate falls below 0.92, ex-ante auditing is used.[10] If, by chance, no audits took place, the audit regime remains unaltered. All care providers randomized in the conditional audit regime started at ex-ante auditing, the pre-experiment situation. The timing of audit was updated once every two weeks.[11] These updates were communicated to the care providers supported by a list of results of the audits. All care providers in all treatment groups received the same detailed feedback about the outcomes of their own audits.

In each of the treatment groups, the probability that an application is selected for audit is held constant. Assessors were instructed to audit along the same procedures as before the experiment.

4.3.2 Implementation

Of the approximately 1,600 long-term care providers in the Netherlands, we selected the 299 providers who filed at least four re-assessment applications in April 2012 for participation in the experiment. These providers are responsible for 80% of all re-assessment applications filed in April 2012. We randomly assigned care providers to one of the three treatment groups discussed in the previous section.[12] Participation was compulsory. Care providers were informed by letter and e-mail about the set-up of the experiment.[13] Finally, a brief notice of the experiment was posted in an online information bulletin of the gatekeeper in October 2012. More details on the information given to participants in the experiment are provided in Appendix 4.A.

Table 4.1 illustrates that the treatment groups are balanced in terms of pre-experiment outcomes and care-provider characteristics. On average, slightly more

[10]The threshold audit approval rate of 0.92 is determined by the target approval rate that the gatekeeper has in mind.

[11] In the first weeks of the experiment, implementation of regime-updating took some more time. As a consequence, the first adjustment was in place four weeks after the start of the experiment, and the second adjustment three weeks after the first update.

[12]The original proposal for the field experiment including power analysis is available at http://personal.vu.nl/b.vander.klaauw/OpzetCIZOnderzoek.pdf [in Dutch].

[13]An indication for the duration of the experiment was provided to the care providers. However, the experiment continued for some additional months, so that care providers were uninformed about the exact duration and could not anticipate on the end of the experiment.

Table 4.1: Balancing of pre-experiment outcomes and provider characteristics across treatment groups (January 1, 2012 - September 16, 2012).

	ex-ante regime	**ex-post regime**	**conditional regime**	**p-value**
Outcome measures				
re-assessment applications (per week)	3.34	3.56	3.26	0.308
	(0.35)	(0.36)	(0.41)	
re-assessment audits (per week)	0.76	0.83	0.70	0.301
	(0.08)	(0.09)	(0.08)	
re-assessment approval rate	0.84	0.83	0.83	0.935
	(0.01)	(0.01)	(0.01)	
Care provider characteristics				
nursing expertise	0.50	0.52	0.61	0.247
	(0.05)	(0.05)	(0.05)	
mental health expertise	0.22	0.25	0.23	0.845
	(0.04)	(0.04)	(0.04)	
care for handicapped expertise	0.26	0.21	0.15	0.181
	(0.04)	(0.04)	(0.04)	
# care providers	101	99	99	

Notes: Standard errors of means in parentheses. Reported p-values are for Kruskal-Wallis rank tests for equality of populations.

than three applications are filed per week and of the applications selected for audit, about 83% are approved. Furthermore, the table shows that half of the care providers in each group have an expertise in nursing care, a quarter are specialized in the provision of mental health care and another quarter in the provision of care for the handicapped. These fractions are not significantly different between treatment groups, as shown in the fourth column.

The gatekeeper assigned three assessors to perform the ex-post audits. These assessors also perform ex-ante audits, but they could distinguish whether the audit is ex-ante or ex-post. Table 4.2 provides descriptives on the pre-experiment fraction of approvals of these three assessors and assessors doing ex-ante audits only. There is no significant difference in the fractions of approval before the experiment and the fraction of approvals of ex-ante audits during the experiment. So results are not likely to be driven by differences between assessors. However, approval rates of ex-post audits are substantially higher than of ex-ante audits. We return to the role of the assessors in more detail in subsection 4.5.3.1.

Table 4.2: Assessor approval rates by group of assessors.

	before experiment	during experiment	
	ex-ante audits	*ex-ante audits*	*ex-post audits*
three assessors doing ex-post audits	0.832	0.777	0.872
	(0.018)	(0.022)	(0.023)
	[N = 3062]	[N = 747]	[N = 4239]
other assessors	0.824	0.811	-
	(0.028)	(0.026)	-
	[N = 4355]	[N = 3185]	-
p-value	0.817	0.364	-

Notes: N denotes the number of audited applications. Only audits by assessors who audited applications both before and during the experiment are included. That is, 12 out of 20 assessors auditing re-assessment applications. P-values are for t-tests for equality of the means (assuming unequal variances).

4.3.3 Hypotheses

In the institutional setting in which the field experiment takes place, the relationship between gatekeeper and care provider can be characterized as a principal-agent problem. The gatekeeper (principal) has the task to manage the long-term care budget and simultaneously ensure access to long-term care for targeted patients. Care providers (agents) advocate the wishes of their patients and compete for these patients. Since long-term care is publicly funded, both the patient and the care provider benefit from increasing the amount of care. Furthermore, patients typically would like to receive long-term care as soon as possible after contacting a long-term care provider. The gatekeeper is not able to make an assessment of needs based on a detailed audit for each patient, both since care services may be quickly needed and since auditing budgets are limited as well.

Therefore, care providers are mandated to determine the care needs of the patient themselves by filing a re-assessment application. Filing such an application is necessary to receive funding for the provision of long-term care. However, it also requires costly effort of the care provider, not only since the care needs of the patient have to be assessed, but also because of administrative actions associated with the application process. Because the assessment of needs provided by the gatekeeper limits the amount and type of care that can be delivered to the patient, care providers have incentives to ask for care services different from the actual care needs of the patient. The gatekeeper randomly selects a number of applications for audit and may disapprove the application if the requested care does not match the patient's actual needs or if information in the application is incomplete. Requesting too much care or not devoting enough effort to the application increases the probability of a disapproval during an audit.

According to the traditional model of Allingham and Sandmo (1972), care providers should weigh the benefits of requesting funding for more care and devoting more effort to filing the application against the expected costs of noncompliance (i.e., filing incomplete or incorrect applications) given the degree of control and the level of punishment imposed by the gatekeeper. This traditional model implies that sufficient control and extensive punishment by the gatekeeper can induce all care providers to comply. If the costs of noncompliance are substantial, care providers should file complete applications, should not request too much care and should not file invalid applications. In our setting, the gatekeeper can only choose between ex-ante and ex-post auditing. Neither of them includes direct financial sanctions in case of noncompliance, but ex-ante auditing is associated with a higher degree of control since adjudged care services can differ from requested care services. Besides, for the care provider ex-ante auditing has the disadvantage of not being able to provide services to the patients immediately. Since ex-post auditing does not provide any (financial) incentive for compliance, a prediction of the traditional model is that under ex-ante (and also conditional) auditing care providers should file fewer applications and have a higher approval rate.

This traditional model does not yield a clear prediction about how the conditional audit regime relates to the ex-ante audit regime. On the one hand, in the conditional audit regime care providers may have a lower degree of compliance during periods of ex-post auditing since the audits do not have immediate consequences for care provision. On the other hand, even when on ex-post auditing, the result of an audit in the conditional audit regime has consequences for how future audits will be performed. Therefore, it depends on the discount rate of the care providers, the strictness of audits and the costs of effort when filing an application, how behavior of care providers changes when being exposed to the conditional audit regime.

In contrast to the traditional model, behavioral theories focus on the role of intrinsic motivation. In our setting intrinsic motivation can be interpreted as the willingness of care providers to file applications according to the rules of the gatekeeper. It is often argued that external interventions, like control and punishment, can crowd out intrinsic motivation (see e.g., Gneezy et al. (2011) and other references in the introduction). Ex-post auditing has the least amount of control by the gatekeeper.

In the ex-ante audit regime the amount of control for the gatekeeper is largest. The conditional regime has a lower degree of control compared to the ex-ante regime, but incentives in the conditional regime are stronger. Increasing the degree of control may reduce effort put in filing applications and can reduce compliance. If intrinsic motivation is the driving factor and control crowds out intrinsic motivation, the prediction is that compliance is highest in the ex-post audit regime and lowest in the ex-ante audit regime. And if disapprovals are associated with invalid applications, then the number of applications is lowest in the ex-post audit regime.

Trust is often mentioned as a pathway through which external interventions crowd out intrinsic motivation. Mendoza and Wielhouwer (2015) mention two requirements for feasible trust-based auditing, i.e. non-decreasing compliance despite less control. They argue that trust-based auditing can be feasible when there are certain costs of control to agents and when agents sufficiently value the future. In our setting, control-based (cq. ex-ante) auditing is associated with a delay in care provision. Since care providers are competing for patients, they care about quick provision of services to patients. This delay implies a cost to the care provider. Regarding the second requirement, care providers typically have repeated interactions with the gatekeeper. Building a trust relation can, therefore, be valuable for providers. Hence, trust-based (cq. ex-post) auditing can be feasible in our setting and detrimental effects on compliance need not be expected.

Care providers are likely to be heterogeneous, some providers may behave according to the traditional model (referred to as traditional care providers in the remainder), while others may have a stronger intrinsic motivation. In case of such a mix of providers, the conditional audit regime would probably yield the largest compliance. Care providers that are intrinsically motivated may interpret this as a regime where trust can be built when applications are approved and dissolves after a disapproval. Once they become exposed to ex-post auditing, they may feel trusted and maintain a high level of compliance. For the traditional care providers this audit regime imposes the incentives to perform well since control will then be reduced and noncompliance becomes less costly. Besides, they can provide care quicker when their performance remains sufficiently high to be exposed to ex-post auditing. The crucial feature of the conditional audit regime is the performance threshold for switching between ex-ante and ex-post auditing. If the threshold is set too high, it is too costly for the traditional care providers to comply. Furthermore, the relatively large amount of control in case of a high threshold may crowd out intrinsic motivation of the intrinsically motivated care providers. This would reduce their performance (below the threshold) so that these providers will end up in ex-ante auditing.

4.4 Data

We have access to administrative data containing all applications filed by all care providers between January 1, 2012 and November 24, 2013.[14] This implies that we observe a pre-experiment period (January 1, 2012 - September 16, 2012), the experiment period (September 17, 2012 - April 7, 2013) and a post-experiment period (April 8, 2013 - November 24, 2013). In the empirical analysis we mainly consider the pre-experiment and the experiment period, but in subsection 4.5.2.2 we also look at behavior after the experiment and long-term consequences of the experimental variation in audit regimes. There are 642,921 applications filed by care providers participating in the experiment.[15]

Re-assessment applications, which are subject to the experimental variation, account for 17.3% of the applications of participating care providers. Of the 111,024 re-assessment applications, 22.2% (24,654 applications) were selected for audit. For each application we observe the application date, the date of audit, the care provider filing the application, the type of application, and the type, requested amount and maximum duration of care services. Furthermore, we observe the assessor who performed the audit, the result of the audit (approved or disapproved) and the reasons for this audit decision. Data on applications provided by the gatekeeper are complemented with linked micro-data from Statistics Netherlands, providing information on basic patient characteristics including age, gender and marital status.[16]

Descriptive statistics on application and patient characteristics for whom re-assessment applications are filed, are provided in Table 4.3. The first panel shows the relative importance of various types of re-assessment applications. By far the most frequently filed re-assessment application is changing an assessment for extramural care. A single application may be concerned with multiple types of care. The most frequent type of care is personal care, followed by assistance and institutionalized care. Finally, the table shows that more re-assessment applications are filed for women, and patients are, on average, approximately 64 years old. Around one-third of the patients are over 80 years of age and slightly over 20 percent are married. This may relate to the presence of informal care options, which is taken into account

[14]The data do not contain information on applications filed to the gatekeeper by patients themselves, so that we are not able to look into effects on the number of applications by patients, for instance.

[15]Only in subsection 4.5.2.3 we also consider applications of care providers not participating in the experiment.

[16]This linkage is available for all applications filed by participating care providers before and during the experiment, but not for applications filed after the experiment.

Table 4.3: Application characteristics and patient characteristics (January 1, 2012 - April 7, 2013).

	ex-ante	**ex-post**	**conditional**
Types of re-assessments			
change in extramural care	69%	73%	75%
prolong (unchanged) intramural care	16%	13%	11%
change in intramural care	12%	11%	11%
other	3%	3%	3%
Types of care			
personal care	48%	54%	58%
nursing care	17%	19%	20%
assistance	27%	27%	26%
special medical treatment	1%	1%	1%
institutionalized care	31%	27%	25%
Patient characteristics			
female	57%	58%	59%
age (in years)	62.26	64.30	66.16
older than 80	31%	33%	36%
married	22%	23%	24%
# applications	23,550	26,859	22,037

in determining eligibility for formal long-term care. Additional descriptives on outcomes during the experiment, by treatment group are provided in Table 4.D.1 in Appendix 4.D.

When studying the effects of audit regimes on the number of applications, we aggregate the data in periods of two weeks at the care provider level. The resulting panel data set is unbalanced, because providers may become inactive due to closing down activities or merging with another provider.[17] For ease of interpretation we normalize the number of applications to weekly averages for each care provider.

4.5 Results

Our key interests are the quantity and quality of applications, the latter being proxied by the audit approval rate. Subsection 4.5.1 provides descriptive evidence

[17] Details on the construction of the panel data set, such as the level of time aggregation and the way to deal with inactivity of care providers, are provided in Appendix 4.B. Tables 4.D.2 and 4.D.3 in Appendix 4.D show that results are robust with respect to these choices.

on the trends in these outcomes. In subsection 4.5.2 we quantify the effects of the audit regime on the number of applications. Subsection 4.5.3 looks at effects on the quality of applications.

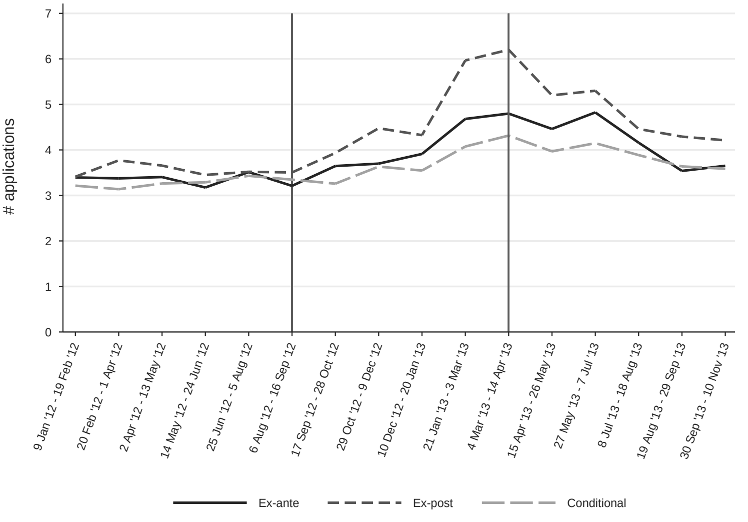
4.5.1 Descriptive evidence

Figure 4.2 shows the trend in the average number of applications in each treatment group before, during and after the experiment. There are no differences between the treatment groups before the experiment, but during the experiment the number of applications diverges. The increase in the number of applications is strongest in the ex-post audit group and smallest in the conditional audit group. After the experiment ended, the number of applications in the treatment groups converged again.

The change in the number of re-assessment applications might be explained by substitution with other application types. If substitution is present, it is most likely towards emergency care applications and regular applications. Other types of applications are very different in content from re-assessment applications. Figure 4.3 illustrates the trend in these two application types. The figure shows a decreasing pattern in the number of regular and emergency care applications before the experiment. We observe a change during the experiment, where the number of regular and emergency care applications slightly increases for providers in the ex-ante and conditional audit regimes, and remains relatively constant in the ex-post audit regime. This suggests that there could indeed be some substitution, although the differences in trends across regimes appear modest.

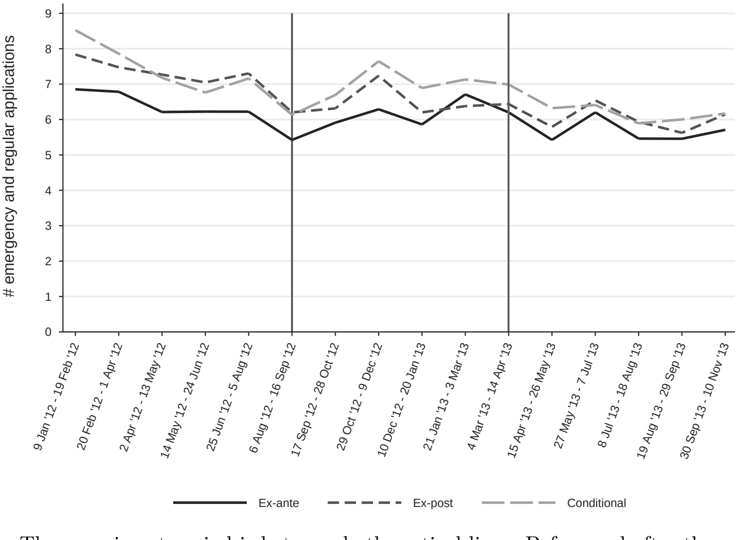
Our final outcome of interest is the audit approval rate. Figure 4.4 shows approval rates over time by treatment group. Despite the increase in the number of applications in the ex-post audit group, there are only modest differences across treatment groups with the approval rate being slightly higher in the ex-post audit group.

Figure 4.2: Weekly average number of re-assessment applications in the three treatment groups.



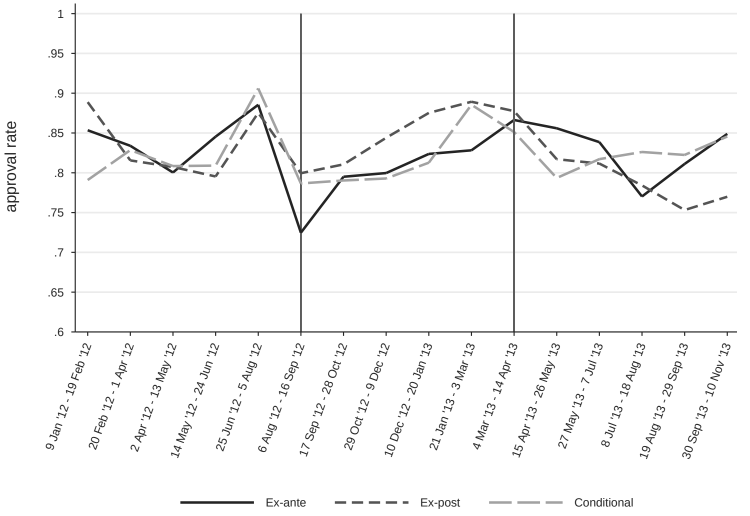
Note: The experiment period is between both vertical lines. Before and after the experiment the ex-ante audit regime was applied.

Figure 4.3: Weekly average number of emergency care and regular applications in the three treatment groups.



Note: The experiment period is between both vertical lines. Before and after the experiment the ex-ante audit regime was applied.

Figure 4.4: Average approval rates in the three treatment groups.



Note: The experiment period is between both vertical lines. Before and after the experiment the ex-ante audit regime was applied.

4.5.2 Number of applications

We estimate linear panel data models on data before and during the experiment to obtain the causal effects of the audit regime on the number of applications for each care provider within a period of two weeks,

$$Y_{c,t} = \alpha_c + \gamma_t + \delta_{\text{ex-ante}} T^{\text{ex-ante}}_{c,t} + \delta_{\text{conditional}} T^{\text{conditional}}_{c,t} + \varepsilon_{c,t} \tag{4.1}$$

Here c indexes the care provider and t is the time period. We include time fixed effects (γ_t) to account for common time trends.[18] Care provider fixed effects (α_c) are included to control for (time-invariant) characteristics of care providers. The treatment variables $T_{c,t}$ equal one if care provider c belongs to a treatment group in period t. The parameters of interest are the δ's, which describe the causal effects of the audit regime. Even though ex-ante auditing was applied before the experiment, we take the ex-post audit regime as the reference group in all estimations. The ex-post audit regime is the least strict regime and our hypotheses in subsection 4.3.3

[18] We also estimated models with alternative specifications of the common time trend (i.e., polynomials in time and quarter dummies combined with an indicator for the experiment period). Results are very similar to our baseline estimates and available on request.

are in comparison to this regime. The ex-ante regime increases control for all care providers irrespective of performance. The conditional audit regime increases control only for poorly performing care providers, but introduces an incentive to perform well. We report effects both in levels and in fractions. For the latter, we normalize the number of applications of a care provider in each period by dividing by the pre-treatment provider-specific average number of applications. This normalization eliminates provider fixed effects.

The baseline estimation results in Table 4.4 show a substantial, but insignificant, decline of 0.624 applications per week when providers are exposed to the ex-ante audit regime compared to the ex-post audit regime. The decline in the conditional audit regime is 0.924 applications per week and significant. When focussing on the fractional change, we see a 10% decline in the ex-ante audit regime and a 20% decline in the conditional audit regime compared to the ex-post audit regime, but only the latter is significant.[19] The larger effect of the conditional audit regime can be explained by larger opportunity costs of false applications in the conditional regime than in the ex-ante audit regime, due to the presence of the threat of switching from ex-post to ex-ante auditing.[20] An alternative explanation, as discussed in the hypotheses in subsection 4.3.3, may be the presence of a mix of traditional care providers and intrinsically motivated care providers, where both respond to the conditional audit regime but only the traditional care providers respond to the ex-ante audit regime.

Note that 10% and 20% of the pre-experiment mean in the ex-post audit regime are smaller than the effects in levels. This suggests that the effects of the audit regime changes are different for small and large care providers.[21] This may be caused by differences in the speed with which feedback on audits reaches the persons filing the applications within the organization, or it may relate to differences in the resources and time available for accurately filing applications. To study heterogeneous effects we interact the time trend and the treatment indicators in equation (4.1) with a

[19]Recall that in the beginning of the experiment regime-updating took some more time (see footnote 11). Results are robust to excluding the first two, four or six weeks of the experiment from the analysis.

[20]In Appendix 4.C we discuss the switches of providers between ex-ante and ex-post auditing in the conditional audit regime. There we find that sorting of providers, based on pre-experiment approval rates, can explain the switches that we observe.

[21]We also considered heterogeneity in effects for providers differing in terms of the share of re-assessment applications in total applications. We found some evidence for providers having a small share of re-assessment applications responding more strongly to the ex-ante and the conditional audit regimes. Results are available on request.

Table 4.4: Baseline estimates for the number of applications (per week) at the provider level (January 1, 2012 - April 7, 2013).

	# applications	
	level	*normalized*
	(1)	(2)
ex-ante audit regime	-0.624	-0.104
	(0.407)	(0.079)
conditional audit regime	-0.949^{***}	-0.203^{***}
	(0.394)	(0.078)
mean ex-post audit regime (before experiment)	3.557	1.000
	(0.362)	(0.000)
care provider fixed effects	yes	no
observations	9286	9286
# care providers	299	299

Notes: Pre-experiment mean and standard error of the outcome in the ex-post audit regime are reported as a reference. Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%.

dummy variable for the size of a provider.[22] The estimation results in Table 4.D.4 in Appendix 4.D show that large providers are most responsive to the audit regime. Large care providers file 37.9% and 36.8% fewer applications when being exposed to respectively the ex-ante audit regime and the conditional regime compared to the ex-post audit regime. Small providers respond less in general. They respond most strongly to the conditional audit regime. We do not find any evidence that the medium-size care providers adapt their behavior in response to the audit regime change.

4.5.2.1 Substitution effects

We find that the number of re-assessment applications in the ex-ante and conditional audit regimes declines compared to the ex-post audit regime. An explanation for this can be substitution towards other application types. The content of standard applications and intramural notifications is substantially different from the typical

[22] The model specification equals $Y_{c,t} = \alpha_c + \gamma_{d_c,t} + \delta_{\text{ex-ante},d_c} T_{c,t}^{\text{ex-ante}} + \delta_{\text{conditional},d_c} T_{c,t}^{\text{conditional}} + \varepsilon_{c,t}$, with d_c indicating care provider type.

re-assessment application, so if substitution is present, it is most likely towards emergency care applications and regular applications. Emergency care applications are audited less strict than re-assessment applications, while regular applications have a stricter audit regime.

To test for the presence of substitution effects, we estimate the baseline model in equation (4.1) having as outcomes the number of emergency care applications, the number of regular applications, the total number of applications other than re-assessment applications and the total number of applications including re-assessment applications.[23] Estimation results are reported in Table 4.5. We find positive effects on both the number of regular applications as well as the number of emergency care applications. Most of the effects are quite large but insignificant. The substitution effects are largest for the conditional audit regime, where we also found the largest decline in the number of re-assessment applications. From column (1) we observe that these substitution effects, especially for the conditional regime, have about the same magnitude as the negative effect on re-assessment applications reported in column (1) of Table 4.4, i.e., there seems to be almost one-to-one substitution although again the effects are insignificant. This is confirmed by the results in column (4) of Table 4.5 which point at a net effect on the total number of applications, including re-assessment applications, that is negative but small and not significantly different from zero. The results suggest that if auditing becomes more costly to the provider, providers choose the type of application more carefully. Stricter auditing hence seems to be less effective when only partially applied. The results are in agreement with the presence of traditional care providers searching for opportunities to minimize the effort of making applications.

Some re-assessment applications may be easier to substitute with emergency care and regular applications, which can cause compositional changes of the re-assessment applications. But also the audit regime itself may affect the composition of re-assessment applications. However, we find no compositional changes induced by the audit regimes, as shown in Table 4.D.5 in Appendix 4.D. In particular, there is no shift in patient characteristics (age and gender) and types of re-assessment applications.[24]

[23] In most cases, when an emergency care application is filed for a particular patient, two entries are recorded in the data: one for immediate care provision and another for care provision after two weeks. We consider this as a single application.

[24] We estimated the panel data model $A_{i,c,t} = \alpha_c + \gamma_t + \zeta_{\text{ex-ante}} T^{\text{ex-ante}}_{i,c,t} + \zeta_{\text{conditional}} T^{\text{conditional}}_{i,c,t} + \eta_{i,c,t}$ on re-assessment application-level data. Here $A_{i,c,t}$ is an indicator for whether or not application i filed by provider c at time t has a particular characteristic (e.g. application for an old

Table 4.5: Substitution effects at the provider level.

	all types	**regular**	**emergency**	**all types, incl. re-assessments**
ex-ante audit regime	0.382	0.304	0.126	−0.202
	(0.531)	(0.214)	(0.150)	(0.726)
conditional audit regime	0.740	0.301	0.463*	−0.189
	(0.960)	(0.241)	(0.247)	(1.070)
mean ex-post audit regime	16.973	4.485	2.701	18.230
(before experiment)	(2.768)	(0.579)	(0.572)	(2.455)
observations	9286	9286	9286	9286
# care providers	299	299	299	299

Notes: All effects are in levels. Each regression includes provider and time fixed effects. Pre-experiment mean and standard error of the outcome in the ex-post regime are reported as a reference. Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%.

4.5.2.2 Short-run, long-run and post-treatment effects

Figure 4.2 illustrated that the weekly number of applications was stable in the pre-experiment period, but increased in the fourth quarter of 2012. From the beginning of 2013, the levels remained relatively constant again. Furthermore, the figure showed a divergence of the average number of applications between the treatment groups during the experiment, and a subsequent convergence afterwards. Therefore, we extend our model to estimate separately short-run effects (fourth quarter 2012), long-run effects (first quarter 2013) and post-experiment effects,

$$\begin{aligned} Y_{c,t} = \alpha_c + \gamma_t &+ \delta_{\text{ex-ante,short}} T_{c,t}^{\text{ex-ante,2012Q4}} + \delta_{\text{conditional,short}} T_{c,t}^{\text{conditional,2012Q4}} \\ &+ \delta_{\text{ex-ante,long}} T_{c,t}^{\text{ex-ante,2013Q1}} + \delta_{\text{conditional,long}} T_{c,t}^{\text{conditional,2013Q1}} \\ &+ \theta_{\text{ex-ante}} P_{c,t}^{\text{ex-ante}} + \theta_{\text{conditional}} P_{c,t}^{\text{conditional}} + \varepsilon_{c,t} \end{aligned} \tag{4.2}$$

We distinguish between a treatment indicator for the fourth quarter of 2012 and for the first quarter of 2013. Furthermore, P is an indicator for the post-experiment period. The parameters θ describe possible treatment effects after the experiment ended and all care providers were again exposed to the ex-ante audit regime.

patient). We again include provider fixed effects, a general time trend and treatment indicators for the ex-ante and conditional audit regimes. The effects of interest, ζ, measure the change in the probability that the application has the characteristic A.

Table 4.6: Short-run, long-run and post-treatment effects (January 1, 2012 - November 24, 2013).

	# applications	
	level	*normalized*
	(1)	(2)
ex-ante regime, short-run	-0.319	-0.053
	(0.287)	(0.064)
ex-ante regime, long-run	-1.095^{*}	-0.222^{*}
	(0.619)	(0.134)
ex-ante regime, post-experiment	-0.487	-0.037
	(0.361)	(0.112)
conditional regime, short-run	-0.491^{*}	-0.090
	(0.277)	(0.065)
conditional regime, long-run	-1.571^{***}	-0.354^{***}
	(0.605)	(0.133)
conditional regime, post-experiment	-0.658	0.013
	(0.436)	(0.114)
mean ex-post audit regime (before experiment)	3.557	1.000
	(0.362)	(0.000)
p-value, short-run	0.165	0.382
p-value, long-run	0.026	0.027
p-value, post-experiment	0.285	0.916
care provider fixed effects	yes	yes
time fixed effects	yes	yes
care characteristics	no	no
patient characteristics	no	no
assessor indicators	no	no
observations	13,730	13,730
# care providers	299	299

Notes: Pre-experiment mean and standard error of the outcome in the ex-post audit regime are reported as a reference. Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%. Reported p-values are for F-tests of the null hypothesis of no effect of any of the audit regimes in the short-run, long-run and after the experiment.

The estimation results in Table 4.6 show that the treatment effects are considerably more pronounced in the long-run than just after the start of the experiment. It may have taken some time for care providers to become fully aware of the implications of the audit regime change, to search for suitable alternatives for filing applications and to adapt their behavior accordingly. This is especially true for the conditional audit regime, which includes specific performance incentives. After the experiment, there is no longer a significant difference in the number of applications filed by providers in the ex-ante and conditional regimes relative to providers in the ex-post regime. Care providers thus reversed their application behavior after the experiment, indicating that the temporary change in audit regime did not cause habit formation.

4.5.2.3 Hawthorne effect

Before the start of the experiment all care providers were exposed to the ex-ante audit regime. Recall from Figure 4.2 that after the start of the experiment the average number of applications of care providers randomized in the ex-ante audit regime started increasing. This might be a Hawthorne effect, implying that participation in the experiment already affects outcomes. A possible explanation might be that informing providers about the experimental variation in auditing regimes for re-assessment applications draws more attention to the possibility of filing this application type, thereby influencing the number of re-assessment applications. To test for a Hawthorne effect, we compare participants in the experiment to non-participants. Recall that we selected all providers having filed at least four re-assessment applications in April 2012 to participate in the experiment. For each care provider, participating in the experiment or not, we compute the total number of re-assessment applications between February and June 2012, so the pre-experiment period. The total number of applications ranges from 6 to 642 among the participating care providers and from 1 to 64 among the non-participating care providers. Figure 4.5 shows the distributions of the total number of applications filed by participants and non-participants.

We select among both groups the care providers filing between 6 and 45 applications, so the range in which there is an overlapping support.[25] Next, we weigh the non-participating care providers, such that the weighted distribution of the total number of applications is the same as the observed distribution among the participating care providers. When an increasing trend in the number of applications similar to the trend for the ex-ante audited providers is found for the (weighted) group of non-participating providers, a Hawthorne effect is unlikely to explain the

[25]That implies that we focus on 150 of 299 participating providers and 266 of 580 non-participating providers.

Figure 4.5: Distribution of number of re-assessment applications for participating and non-participating care providers, February 2012 - June 2012.

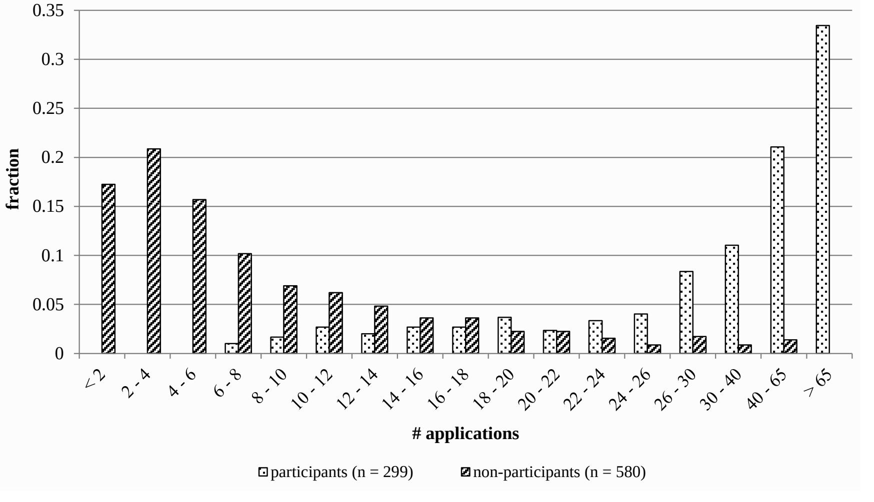
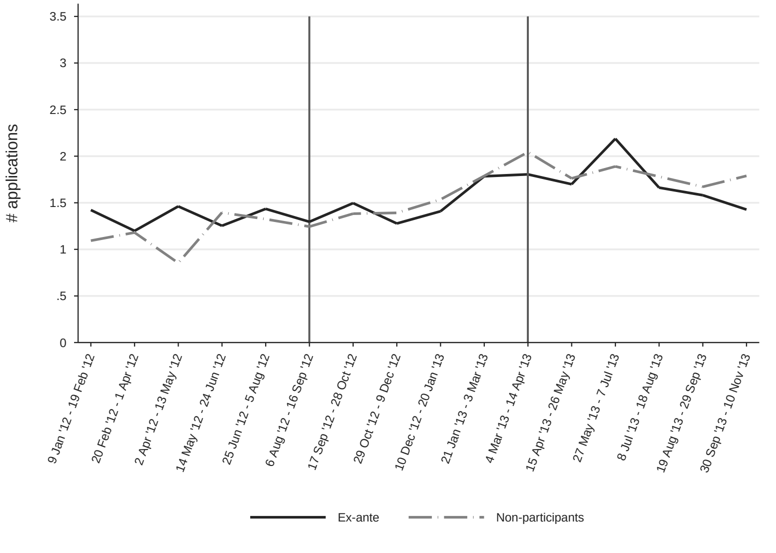


Figure 4.6: Average number of re-assessment applications, providers in ex-ante regime and non-participants.



Note: The experiment period is between both vertical lines. Before and after the experiment the ex-ante audit regime was applied.

findings in Figure 4.2. Figure 4.6 illustrates the trends in the number of applications over time for the ex-ante regime and the non-participants. These trends are very comparable.[26] This suggests that participation in the experiment itself did not cause behavioral responses. The increase in the number of applications in the fourth quarter of 2012 thus seems a genuine time trend rather than an effect of the experiment.

4.5.3 Quality of applications

For the audit approval rate we estimate a linear panel data model on application-level data,

$$Q_{i,c,t} = \alpha_c + \gamma_t + \delta_{\text{ex-ante}} T_{i,c,t}^{\text{ex-ante}} + \delta_{\text{conditional}} T_{i,c,t}^{\text{conditional}} + \beta X_{i,c,t} + \eta_{i,c,t} \tag{4.3}$$

$Q_{i,c,t}$ is a dummy for approval of application i filed by provider c at time period t. Again we include provider fixed effects α_c and time fixed effects γ_t. Furthermore, we sequentially include various sets of application characteristics, $X_{i,c,t}$. These include indicators for the type of re-assessment, indicators for types and amounts of care, patient characteristics and indicators for the assessor performing the audit. The causal effect estimates (δ) represent percentage point changes in the approval probability. Again, the ex-post audit regime, which is the least strict, is the reference group.

The estimation results are presented in Table 4.7. The approval probability declines by about four percentage points when providers are exposed to the ex-ante audit regime instead of the ex-post audit regime. We estimate a three percentage points decrease for switching to the conditional audit regime.[27] The direction of the effects is consistent with the presence of intrinsically motivated care providers, i.e. increasing the degree of control crowds out intrinsic motivation thereby reducing

[26] To test this more formally, we estimate weighted panel fixed effects models similar to our baseline specification in equation (4.1) for the number of applications filed by non-participants and providers in the ex-ante audit regime. We take non-participants as the reference group and include a treatment indicator for the ex-ante audit regime. As weights we use the weights assigned to the non-participants in order to equalize the distribution of the total number of applications to that of the participating providers. The estimation results in Table 4.D.6 in Appendix 4.D show that there is no significant difference in the (normalized) number of applications in the ex-ante audit regime compared to the non-participants.

[27] As for the number of applications, we also estimated short-run, long-run and post-treatment effects on the audit approval rate. We do not find differences in short-run and long-run effects. The effects on the quality of applications are immediate and quite persistent. This may be explained by the potential role of the assessors in the effect on application quality studied in subsection 4.5.3.1. The effects are no longer present after the experiment, which again suggests the absence of habit formation. The estimation results are shown in Table 4.D.9 in Appendix 4.D.

Table 4.7: Baseline estimates for the audit approval rate at the application level (January 1, 2012 - April 7, 2013).

	approval			
	(1)	(2)	(3)	(4)
ex-ante audit regime	-0.038^{**}	-0.039^{**}	-0.039^{**}	-0.047^{***}
	(0.015)	(0.016)	(0.016)	(0.017)
conditional audit regime	-0.026^{*}	-0.028^{*}	-0.028^{*}	-0.029^{*}
	(0.015)	(0.015)	(0.015)	(0.016)
intercept	0.884^{***}	0.956^{***}	0.956^{***}	0.924^{***}
	(0.023)	(0.026)	(0.026)	(0.043)
care provider fixed effects	yes	yes	yes	yes
time fixed effects	yes	yes	yes	yes
care characteristics	no	yes	yes	yes
patient characteristics	no	no	yes	yes
assessor indicators	no	no	no	yes
# applications	16,876	16,875	16,872	16,872
# care providers	299	299	299	299

Notes: Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%. The general time trend is accounted for by two week period dummies. Care characteristics include indicators for type of re-assessment, indicators for all care types (personal care, nursing care, individual assistance, group assistance, treatment and intramural care). Furthermore, indicators for various levels of intensity of personal care, and similarly for nursing care, individual and group assistance are included. Patient characteristics include a gender dummy, a marital status dummy, and an indicator for whether an individual is older than 80 years.

compliance. The estimates for the treatment effects are robust against including covariates. Only including the assessor indicators in column (4) causes the effects to increase slightly. This might be influenced by the fact that eight of the 20 assessors performed audits only in the pre-experiment period or only during the experiment. We discuss the role of the assessor in explaining application quality in ex-post versus ex-ante audits in more detail below.

Providers in the conditional audit regime who had performance levels around the target approval rate of 0.92 before the experiment may be more likely to respond to the audit regime changes by changing application quality compared to providers far below or far above the threshold. Therefore, we expect effects on the audit approval rate to be heterogeneous with initial approval rate. We interact the time trend and the treatment indicators in equation (4.3) with a dummy variable for the initial approval rate category.[28] The estimation results in Table 4.D.4 in Appendix 4.D show that care providers with intermediate and, to a lesser extent, high initial approval rates reduce their number of applications in response to the conditional audit regime. Furthermore, only providers with low initial approval rates have a significant reduction in audit approval rate in response to the conditional audit regime.[29]

4.5.3.1 The role of the assessor

The absence of possibilities for adjustments to the type and/or amount of care to be delivered after an ex-post audit may cause that assessors value ex-post audits differently than ex-ante audits. Even though assessors were explicitly instructed not to change anything in the way audits were done, differences in attitude towards ex-ante and ex-post audits can have consequences for their strictness. Table 4.8 shows descriptives on the reasons for disapproval in ex-ante audits and ex-post audits before and during the experiment, where multiple reasons can be given for a single disapproval. Comparing ex-ante audited applications before and during the experiment, the table illustrates only modest differences. More substantial differences are observed between ex-ante and ex-post audited applications during the experiment. For ex-post audited applications the average number of registered reasons for disapproval is considerably lower. Furthermore, we see a substantial reduction in *too little care requested* and *disorder or limitations not appropriate*. Recall that ex-post audits occur both in the ex-post audit regime and in the conditional audit regime. If ex-post audits are less rigorous, this can inflate approval rates in the ex-post regime and, to a lesser extent, the conditional audit regime.

Of the 16 assessors auditing re-assessment applications during the experiment, only three performed both ex-post and ex-ante audits. We can exploit this to

[28]We estimate the panel data model $Q_{i,c,t} = \alpha_c + \gamma_{d_c,t} + \delta_{\text{ex-ante},d_c} T_{i,c,t}^{\text{ex-ante}} + \delta_{\text{conditional},d_c} T_{i,c,t}^{\text{conditional}} + \beta X_{i,c,t} + \varepsilon_{i,c,t}$.

[29]There can also be heterogeneity across types of patients for whom an application is filed. For some types of patients it may be easier to make an accurate assessment of care needs and as a result to file an application that will be approved in an audit. We considered heterogeneous effects by age and gender and found no heterogeneity by gender. Approval rates decrease considerably more for the elderly, both in the ex-ante and the conditional audit regime. The estimation results are available on request.

Table 4.8: Reasons for audit disapproval before and during the experiment.

	before	during	
	ex-ante audited	*ex-ante audited*	*ex-post audited*
number of registered reasons	1.761	1.668	0.897
different type of care required	0.261	0.274	0.261
too much care requested	0.328	0.344	0.376
too little care requested	0.048	0.077	0.014
disorder and/or limitations not appropriate	0.592	0.544	0.179
other reasons	0.357	0.283	0.261
# audited applications	1383	788	574

Notes: Fractions do not sum to one as there may be multiple reasons underlying one disapproval. Ex-ante (ex-post) audited applications may be filed either by care providers in the ex-ante (ex-post) regime or care providers in the conditional regime. Obviously, ex-post audited applications are necessarily filed during the experiment.

investigate whether ex-post audits are less strict than ex-ante audits. For this purpose we consider the panel data model,

$$Q_{i,a,c,t} = \alpha_c + \gamma_t + \xi_a + \theta P_{i,a,c,t} + \eta_{i,a,c,t} \tag{4.4}$$

where $Q_{i,a,c,t}$ is the dummy for approval in an audit of application i filed by provider c at time t and audited by assessor a. We again include provider fixed effects and a general time trend. ξ_a is an assessor fixed effect accounting for differences in audit strictness between assessors. $P_{i,a,c,t}$ indicates whether application i filed by provider c was audited ex-post ($P_{i,a,c,t} = 1$) or ex-ante by assessor a. The effect of interest θ measures the change in the probability of approval in case of ex-post instead of an ex-ante audit.

Table 4.9 shows the estimation results. In column (1) assessor fixed effects are excluded and estimation is done on data for all 16 assessors. This shows that the overall difference in approval probability between ex-ante and ex-post audited applications is very small. When adding the assessor fixed effects in column (2), we identify the effect on those assessors having performed both ex-ante and ex-post audits. Now, ex-post audits have about a four percentage point higher approval rate, which is consistent with our earlier finding that approval rates are highest in the ex-post audit regime. The effect on the audit approval rate may be partly attributed to a different attitude of assessors towards ex-ante and ex-post audits. Nevertheless,

Table 4.9: Effect of ex-post auditing on approval probability.

	approval	
	(1)	(2)
ex-post audit	-0.004	0.040
	(0.025)	(0.027)
constant	0.827^{***}	0.856^{***}
	(0.024)	(0.062)
provider fixed effects	yes	yes
time fixed effects	yes	yes
assessor fixed effects	no	yes
# audited applications	8358	8358
# care providers	287	287
# assessors	16	16

Notes: For 163 out of 8,521 audited applications (during the experiment) it is unknown which auditor performed the audit. These observations are excluded from the analyses. Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%.

the estimated effect of ex-post audit is insignificant, which prevents us from drawing strong conclusions on this.

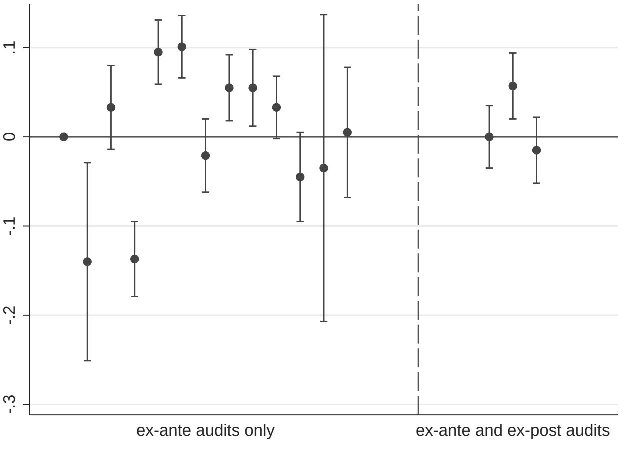
To characterize the three assessors doing ex-post audits during the experiment, we estimate the following model using only pre-experiment data.

$$Q_{i,a,c,t} = \alpha_c + \gamma_t + \xi_a + \eta_{i,a,c,t} \tag{4.5}$$

Figure 4.7 shows the resulting point estimates and 95%-confidence intervals for the assessor fixed effects ξ_a. The figure illustrates that, although there are differences in approval probabilities across assessors, the three assessors who performed ex-post audits, are not very different in terms of approval rates from other assessors.

Hence, we do not find evidence that the lower approval probability in the ex-ante and conditional audit regimes can be attributed to differences in the strictness of assessors.

Figure 4.7: Estimated assessor fixed effects.



4.6 Conclusion

Public resources for long-term care services can be managed by a gatekeeper, which decides about rewarding applications for these services. However, often care is needed quickly, which limits the gatekeeper in its possibilities to audit applications. In this chapter we discussed a large-scale field experiment to study the effectiveness of different audit policies in the Dutch market for long-term care. These audit policies differ in the degree of control, which is operationalized by the timing of audit and the presence of performance incentives. In particular, the timing of an audit can be ex-ante (before the start of care provision) or ex-post (after the start of care provision). With ex-ante auditing the gatekeeper makes the final decision on the care services that can be provided to the patient. The gatekeeper then has a large amount of control, but this comes at the cost of delay in provision. Ex-post auditing allows for immediate provision of care services, but comes at the risk of inefficiency in spending.

Our results show that increasing the degree of control (the ex-ante compared to the ex-post audit regime) reduces the number of applications by 10%. This effect is large in magnitude, but insignificant. A more substantial and significant reduction of 20% is found for the conditional audit regime as compared to the ex-post regime. In this conditional audit regime control (ex-ante auditing) is targeted towards poorly performing care providers, which yields as a performance incentive that control is low (ex-post auditing) when the performance of a care provider is sufficiently good.

A large part of these declines in the number of applications can be attributed to substitution between types of applications. Furthermore, we found negative effects of both the ex-ante and the conditional audit regime on the quality of applications. We found (weak) evidence that this can partly be explained by assessors valuing ex-post audits less than ex-ante audits. The estimated effects on both quantity and quality of applications disappear shortly after the end of the experiment, so the temporary change in the audit regime did not cause any habit formation.

More control and incentives reduce the number of applications, which supports the traditional model by Allingham and Sandmo (1972). But the results for the audit approval rate suggest that care providers are (often) intrinsically motivated to file applications correctly. However, differences in the attitude of assessors towards ex-ante and ex-post audits provide an alternative explanation. Providers could in principle also accidentally make mistakes in filing applications, for instance as a result of not understanding the rules or when rules are not as clear-cut that they translate into one 'correct' application. The former could lead to a larger amount of variation in approval rates, the latter would create a range of care services corresponding to actual care needs. Nevertheless, in both cases providers could still differ systematically in terms of compliance. When only focussing on long-term care expenditures the conditional audit regime should be preferred because of the reduction in the number of applications and the shift towards other application types, which are typically associated with less extensive care. Furthermore, in the conditional audit regime the gatekeeper causes fewer delays in care provision due to auditing. Nevertheless, the choice of audit policy should ultimately also take into account targeting efficiency of funding, in particular whether the decline in the number of applications does not come at the cost of the availability of care services to patients truly in need of those services.

4.A Information provided to participants

Before the start of the experiment, care providers for whom audit procedures would change during the experiment, were informed that a study on the effects of various ways of auditing applications would start. They received an information letter and were contacted by phone or personally by case managers employed at the gatekeeper.

The information letter informed providers about the goal of the experiment. In particular, it was indicated that the research aimed to investigate which auditing procedures would maximize approval rates. Importantly, the letter stated that the reason for doing this research was to critically evaluate and possibly improve audit procedures applied by the gatekeeper.

Moreover, the providers were informed about all groups that existed in the experiment and the audit procedures that would apply to these groups during the experiment. However, the exact details of updating audit procedures in the conditional regime were not provided in the letter.

Finally, the letter informed providers about the group they were assigned to. This included mentioning the exact audit procedure in the regime they were assigned to. Providers in the conditional regime were also told about the frequency of updating audit rules and that they would receive feedback when updates would be implemented.

A short notice in an online information bulletin published in October 2012 ensured that all providers were informed about the ongoing experiment. The goal of the experiment was mentioned in the same way as in the letters sent to the providers. Furthermore, the experimental groups were listed, although exact audit rates were left out.

4.B Construction of the panel data set

Starting from application-level data containing the exact date of each application, a few choices are made to construct an aggregated panel data set. We want the start of the experiment at the beginning of a time period. This affects the observation period when we only want to use full time periods. Alternatively, we could weigh the numbers of applications and audits in the incomplete periods by the number of weeks that these periods cover. Comparing the results in Tables 4.D.7 (weighted) and 4.D.2 (exclusive) in Appendix 4.D, we find results to be robust with respect to this choice. Furthermore, the end of the experiment does not necessarily fall exactly at the beginning of a new time period when aggregating the data. Therefore, we drop the period containing the end of the experiment in the regression analysis.

In our main analysis, we aggregate the data in time periods of two weeks. Aggregating the data to weekly periods leads to many provider-period observations with zero re-assessment applications (24%), while this is limited to 12% of provider-period observations when aggregating the data for two week time periods. For the number of audits this is even more pronounced with zero audits in 56% (38%) of the provider-period observations when aggregating to weeks (two weeks). We checked the robustness of our estimated effects on the number of applications to this aggregation choice. Results can be found in Table 4.D.2 in Appendix 4.D.

Related to this aggregation issue, a further robustness check is concerned with the issue of potentially serially correlated errors stressed by Bertrand et al. (2004). In our main analyses, we account for this by reporting cluster-robust standard errors. An alternative solution, mentioned by Bertrand et al. (2004), is to collapse the data into one pre-treatment and one post-treatment period and estimate a simple difference-in-differences specification on this collapsed data set. The estimation results are presented in Table 4.D.8 in Appendix 4.D. Columns (1) and (2) show coefficients similar in magnitude to our baseline estimates reported in Table 4.4. We observe some differences in the magnitude of the estimated effects on the audit approval rate (columns (3) and (4)). Including a pre/during-experiment dummy instead of dummies for two week periods yields estimates that are comparable to our baseline results (Table 4.7 column (1)). However, fixed effects estimation on the pre/during-experiment aggregated data set yields larger coefficients and standard errors.

A second choice relates to the treatment of providers that were inactive at the beginning or at the end of the time range for which we have data available. We do not observe the exact date at which a provider became (in)active. We approximate this by comparing the provider-specific length of a period at the beginning/end without applications to the length of intermediate periods of zero applications. If the length of a period of inactivity at the beginning (end) exceeds the length of the longest intermediate period of inactivity by more than two weeks, we define the provider as inactive at the beginning (end) of our data range. There are four ways to treat inactivity of these providers in the construction of the panel data set.

(a) Disregard inactivity by putting numbers of applications and audits to zero whenever no applications have been filed in a particular time period (balanced panel).

(b) Include providers as soon as an application is filed.
(c) Include providers from the time period $t + 1$ ($t - 1$) onwards if activity starts (ends) within time period t.
(d) Weigh the number of applications and audits by the number of weeks of activity in the first (last) time period of activity.

In our main analyses we include providers as soon as they filed an application, irrespective of whether or not this coincided with the start of a new time period (option (b)). We investigated the robustness of results to the alternative ways of dealing with inactivity. The estimation results for our baseline model are reported in Table 4.D.3 in Appendix 4.D. Estimates are only modestly affected when using the balanced panel data set. Estimates obtained using procedures (c) and (d) yield coefficients similar to our baseline results reported in section 4.5. As a final robustness check, the table also shows the results of regressions on a data set from which providers inactive at the end of the data range have been completely removed. Note that for the effects on the audit approval rate, which are estimated on application-level data, this issue does not show up.

4.C Dynamics in the conditional audit regime

The conditional audit regime successfully reduced the number of re-assessment applications. Furthermore, audit approval rates in the conditional regime are somewhat lower. Behavioral responses can be investigated in more detail by looking at dynamics between ex-ante and ex-post auditing of providers in the conditional audit regime. Recall that providers were audited ex-ante (ex-post) as long as their approval rate is less than (at least) 0.92. There were twelve occasions for switching the timing of audit. Descriptives on these switches are provided in Table 4.C.1. Over time, a declining fraction of providers is subject to ex-ante auditing. This illustrates that, on average, care providers had performance levels above the 0.92-threshold. However, in each update we observe providers switching in both directions.

The light-colored bars in Figure 4.C.1a show the distribution of the actual number of switches for providers (over all twelve updates). Providers switched at most five times, but on average a provider has 1.8 switches during the experiment. Figure 4.C.1b shows the distribution of the actual number of update rounds in which a care provider was subject to ex-ante auditing. The figure illustrates that only 11% of care providers performed consistently well; they moved to ex-post auditing immediately after the start of the experiment and remained being audited ex-post.

Table 4.C.1: Updates of the timing of audit in the conditional audit regime.

update	% ex-ante	% ex-ante → ex-post	% ex-post → ex-ante
0 (17 Sep ’12)	100.0	-	-
1 (15 Oct ’12)	57.9	42.1	0.0
2 (5 Nov ’12)	61.1	9.5	12.6
3 (19 Nov ’12)	56.8	5.3	1.1
4 (3 Dec ’12)	56.8	6.3	6.3
5 (17 Dec ’12)	52.6	6.3	2.1
6 (31 Dec ’12)	53.7	6.3	7.4
7 (14 Jan ’13)	53.7	3.2	3.2
8 (28 Jan ’13)	55.8	7.4	9.5
9 (11 Feb ’13)	55.8	5.3	5.3
10 (25 Feb ’13)	50.5	8.4	3.2
11 (11 Mar ’13)	51.6	3.2	4.2
12 (25 Mar ’13)	48.4	10.5	7.4

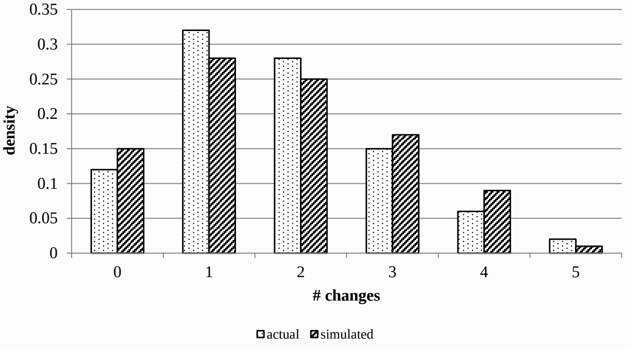
Notes: In parentheses in the first column is the date at which the update became effective. Reported are percentages of providers in the conditional audit regime. Four care providers did not have any audited application during the experiment. For them the timing of audit remained at ex-ante without this being due to poor performance. We excluded these providers from the figures in this table.

On the other hand, 12% of care providers performed consistently poor: they have always been subject to ex-ante auditing.

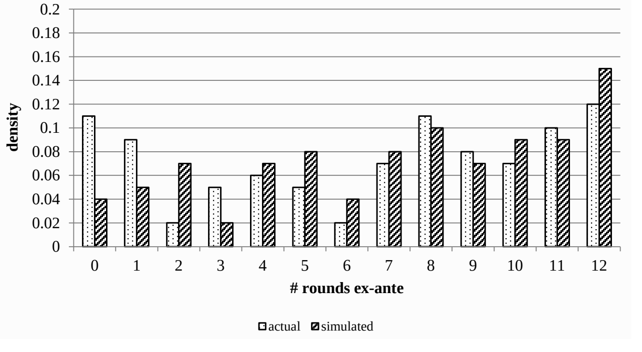
Switches between ex-ante and ex-post auditing in the conditional audit regime can be the result of sorting based on care providers’ intrinsic motivation and/or ability to file applications accurately, or could reflect actual changes in performance as a result of the audit regime change. To determine whether the full variation in the timing of audit can be attributed to sorting, we consider simulated conditional updates for providers in the ex-ante audit regime. That is, we apply the rules used for updating the timing of audit in the conditional audit regime to providers in the ex-ante audit regime using application data on the experiment period. This exploits the absence of actual behavioral responses to performance incentives in the conditional regime by care providers in the ex-ante audit regime. As a result, simulated variation in the timing of audit for these providers is the result of sorting only. Table 4.C.2 shows statistics on the simulated updates for providers in the ex-ante audit regime. For each update we test for equality of the actual and simulated fraction of providers subject to ex-ante auditing and for equality of the actual and simulated fraction of providers switching in a particular direction by means of a χ^2-test and report

Figure 4.C.1: Actual and simulated dynamics in the timing of audit.

(a) Changes in the timing of audit



(b) Frequency of ex-ante auditing



p-values in the table. The results illustrate some differences for the first update, with fewer providers switching to ex-post auditing in the simulated updates. However, in subsequent updates we do not observe significant differences. Hence, the results suggest observed dynamics in the timing of audit in the conditional audit regime to be largely the result of sorting on pre-experiment approval rates.

In addition, Figure 4.C.1a shows that the actual and simulated distributions of the number of switches are quite similar. This is confirmed by a formal χ^2-test for equality of the actual and simulated distributions (p-value = 0.872). Figure 4.C.1b shows some differences in the actual and simulated distributions of the number of rounds of ex-ante auditing. The actual distribution shows a larger fraction of providers with only very few rounds (zero or one) of ex-ante auditing. Nevertheless, a formal χ^2-test cannot reject the null hypothesis of equality of the distributions

Table 4.C.2: Descriptives on simulated updates of the timing of audit in the ex-ante audit regime.

update	**% ex-ante**		**% ex-ante → ex-post**		**% ex-post → ex-ante**	
		p-value		*p-value*		*p-value*
0 (17 Sep '12)	100.0		-		-	
1 (15 Oct '12)	70.5	[0.069]	29.5	[0.069]	0.0	
2 (5 Nov '12)	65.3	[0.547]	12.6	[0.488]	7.4	[0.227]
3 (19 Nov '12)	65.3	[0.234]	7.4	[0.551]	7.4	[0.030]
4 (3 Dec '12)	67.4	[0.135]	6.3	[1.000]	8.4	[0.579]
5 (17 Dec '12)	66.3	[0.055]	4.2	[0.516]	3.2	[0.650]
6 (31 Dec '12)	64.2	[0.140]	5.3	[0.756]	3.2	[0.194]
7 (14 Jan '13)	56.8	[0.662]	11.6	[0.026]	4.2	[0.700]
8 (28 Jan '13)	54.7	[0.884]	9.5	[0.601]	7.4	[0.601]
9 (11 Feb '13)	52.6	[0.662]	9.5	[0.267]	7.4	[0.551]
10 (25 Feb '13)	51.6	[0.885]	7.4	[0.788]	6.3	[0.306]
11 (11 Mar '13)	54.7	[0.663]	2.1	[0.650]	5.3	[0.733]
12 (25 Mar '13)	51.6	[0.663]	8.4	[0.620]	5.3	[0.551]

Notes: Reported are percentages of providers in the ex-ante audit regime. Six care providers did not have any audited application during the experiment. We excluded these providers from the figures in this table. In square brackets are χ^2-test p-values for equality of actual and simulated statistics.

(p-value = 0.553). We thus conclude that observed dynamics in the timing of audit in the conditional audit regime can largely be attributed to sorting based on unobserved intrinsic quality of providers.

4.D Additional tables

Table 4.D.1: Descriptive statistics on outcomes during the experiment (September 17, 2012 - April 7, 2013).

	ex-ante	**ex-post**	**conditional**
re-assessments			
applications (per week)	4.13	4.88	3.70
	(0.46)	(0.61)	(0.47)
audits (per week)	0.96	1.23	0.87
	(0.10)	(0.15)	(0.11)
approval rate	0.82	0.87	0.81
	(0.01)	(0.01)	(0.02)
other applications (per week)			
standard applications	6.62	7.22	8.75
	(1.48)	(1.64)	(1.87)
intramural notifications	1.74	2.16	1.73
	(0.33)	(0.41)	(0.35)
emergency care applications	4.11	4.54	5.86
	(0.76)	(0.97)	(1.33)
regular applications	3.93	4.11	3.80
	(0.37)	(0.48)	(0.45)
# care providers	96	99	96

Notes: Standard error of means in parentheses. Reported p-values are for Kruskal-Wallis rank tests for equality of populations.

Table 4.D.2: Robustness check: level of time aggregation.

	# applications				
	(1)	(2)	(3)	(4)	(5)
ex-ante audit regime	−0.624	−0.651	−0.611	−0.528	−0.496
	(0.407)	(0.416)	(0.407)	(0.369)	(0.379)
conditional audit regime	−0.949**	−0.960**	−0.931**	−0.818**	−0.751**
	(0.394)	(0.402)	(0.394)	(0.357)	(0.367)
mean ex-post audit regime (before experiment)	3.557	3.551	3.557	3.554	3.552
	(0.362)	(0.362)	(0.362)	(0.362)	(0.362)
	# applications (normalized)				
	(1)	(2)	(3)	(4)	(5)
ex-ante audit regime	−0.104	−0.105	−0.103	−0.089	−0.080
	(0.079)	(0.080)	(0.079)	(0.070)	(0.074)
conditional audit regime	−0.203***	−0.200**	−0.198**	−0.173**	−0.162**
	(0.078)	(0.079)	(0.078)	(0.072)	(0.074)
mean ex-post audit regime (before experiment)	1.000	1.000	1.000	1.000	1.000
period length	2 weeks	1 week	4 weeks	8 weeks	12 weeks
observations	9286	19127	4650	2048	1466
# care providers	299	299	299	299	299
# time periods	32	66	16	7	5

Notes: Column (1) repeats the baseline results. Pre-experiment mean of the outcome variable in the ex-post audit regime is reported as a reference. Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%.

Table 4.D.3: Robustness check: inactive care providers.

	# applications				
	baseline (b)	*balanced* (a)	*excluded* (c)	*weighted* (d)	*removed*
ex-ante audit regime	−0.624	−0.703*	−0.622	−0.620	−0.639
	(0.407)	(0.414)	(0.408)	(0.407)	(0.415)
conditional audit regime	−0.949**	−0.908**	−0.944**	−0.953**	−0.979**
	(0.394)	(0.400)	(0.395)	(0.394)	(0.400)
mean ex-post audit regime (before experiment)	3.552	3.557	3.559	3.558	3.514
	(0.362)	(0.362)	(0.362)	(0.362)	(0.360)
	# applications (normalized)				
	baseline (b)	*balanced* (a)	*excluded* (c)	*weighted* (d)	*removed*
ex-ante audit regime	−0.104	−0.171**	−0.102	−0.103	−0.102
	(0.079)	(0.085)	(0.079)	(0.079)	(0.080)
conditional audit regime	−0.203***	−0.211**	−0.196**	−0.208***	−0.204**
	(0.078)	(0.082)	(0.078)	(0.078)	(0.079)
mean ex-post audit regime (before experiment)	1.000	1.000	1.000	1.000	1.000
observations	9286	9568	9273	9286	8952
# care providers	299	299	299	299	281

Notes: Column (1) repeats the baseline results, where a care provider is included in the data as soon as an application is filed. In column (2) we disregard inactivity and set the number of applications to zero when no application has been filed in a particular period, resulting in a balanced panel. Column (3) additionally, compared to the baseline, excludes periods in which care providers start (stop) to be active. In column (4) the number of applications in the first (last) period of activity is weighted by the number of weeks in which the provider was active. For details on these alternatives, see Appendix 4.B. Pre-experiment mean of the outcome variable in the ex-post audit regime is reported as a reference. Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%.

Table 4.D.4: Heterogeneous effect estimates.

Panel A: heterogeneity in provider size			**Panel B: heterogeneity in initial approval rates**		
	# applications (norm.) (1)	*approval* (2)		*# applications (norm.)* (3)	*approval* (4)
ex-ante × small	−0.148	−0.071	ex-ante × low	−0.024	−0.045***
	(0.169)	(0.047)		(0.091)	(0.017)
ex-ante × medium	0.046	−0.010	ex-ante × intermediate	−0.302**	−0.047*
	(0.091)	(0.031)		(0.149)	(0.026)
ex-ante × large	−0.379**	−0.044**	ex-ante × high	−0.126	−0.076*
	(0.157)	(0.022)		(0.246)	(0.044)
conditional × small	−0.280*	−0.059	conditional × low	−0.087	−0.043**
	(0.153)	(0.044)		(0.095)	(0.020)
conditional × medium	0.002	0.000	conditional × intermediate	−0.419***	−0.022
	(0.104)	(0.033)		(0.159)	(0.026)
conditional × large	−0.368**	−0.035*	conditional × high	−0.341*	0.027
	(0.150)	(0.018)		(0.203)	(0.042)
mean ex-post, small	1.000	0.801	mean ex-post, low	1.000	0.752
	(0.000)	(0.034)		(0.000)	(0.016)
mean ex-post, medium	1.000	0.851	mean ex-post, intermediate	1.000	0.892
	(0.000)	(0.017)		(0.000)	(0.005)
mean ex-post, large	1.000	0.822	mean ex-post, high	1.000	0.988
	(0.000)	(0.014)		(0.000)	(0.005)
p-value, small	0.165	0.243	p-value, low	0.610	0.015
p-value medium	0.849	0.897	p-value, intermediate	0.027	0.203
p-value, large	0.042	0.069	p-value, high	0.178	0.056
controls	no	yes	controls	no	yes
observations	9286	16,872	observations	9286	16,872
# providers	299	299	# providers	299	299

Notes: 34% (21%) of participating providers are categorized as filing few (many) re-assessment applications in the pre-experiment period, the remaining 45% of providers file an intermediate number of applications; 59% (22%) of participating providers are categorized as having low (intermediate) initial approval rates. In columns (2) and (4) care and patient characteristics are included as controls. Pre-experiment mean (over providers) and standard error of the outcome in the ex-post audit regime by provider type are reported as a reference. Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%. Reported p-values are for F-tests testing the null hypothesis of no effect of any regime for a particular type of provider.

Table 4.D.5: Composition effects in the number of re-assessment applications at the application level.

	patient types		re-assessment types		
	women	*older than 80*	*type 1*	*type 2*	*type 3*
ex-ante audit regime	−0.006	0.001	−0.014	0.006	0.006
	(0.010)	(0.009)	(0.013)	(0.015)	(0.009)
conditional audit regime	−0.011	0.006	0.007	0.009	−0.014
	(0.009)	(0.007)	(0.011)	(0.016)	(0.011)
intercept	0.598***	0.302***	0.754***	0.102***	0.101***
	(0.015)	(0.013)	(0.011)	(0.008)	(0.010)
# applications	72,446	72,446	72,446	72,446	72,446
# care providers	299	299	299	299	299

Notes: Type 1 re-assessment applications concern changes of extramural care, type 2 applications concern prolongation of intramural care without changes, and type 3 re-assessments concern changes of intramural care assessments. Each model includes care provider fixed effects. The general time trend is accounted for by including dummy variables for each period of two weeks. Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%.

Table 4.D.6: Estimates of a Hawthorne effect for the number of applications.

	# applications	
	level	*normalized*
	(1)	(2)
ex-ante audit regime	0.119	−0.014
	(0.135)	(0.099)
mean (weighted) non-participants, before	1.293	1.000
observations	9545	9545
# care providers	314	314

Notes: The results reported are obtained from weighted regression, using weights assigned to non-participants to equalize their distribution of applications filed to those for the participants in the experiment. Time and provider fixed effects are included. Pre-experiment mean of the outcome for the non-participants is reported as a reference. Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%.

Table 4.D.7: Robustness check: including incomplete periods.

	# applications			
	(1)	(2)	(3)	(4)
ex-ante audit regime	−0.681	−0.720	−0.700	−0.812*
	(0.425)	(0.444)	(0.441)	(0.491)
conditional audit regime	−0.971**	−0.973**	−0.989**	−0.973**
	(0.409)	(0.426)	(0.425)	(0.469)
mean ex-post audit regime (before experiment)	3.551	3.531	3.548	3.489
	(0.362)	(0.363)	(0.363)	(0.370)
period length	2 weeks	4 weeks	8 weeks	12 weeks
observations	9854	5218	2616	2034
# care providers	299	299	299	299
# time periods	34	18	9	7
	# applications (normalized)			
	(1)	(2)	(3)	(4)
ex-ante audit regime	−0.106	−0.106	−0.113	−0.129
	(0.083)	(0.089)	(0.089)	(0.116)
conditional audit regime	−0.196**	−0.184**	−0.204**	−0.190*
	(0.081)	(0.086)	(0.085)	(0.110)
mean ex-post audit regime (before experiment)	1.000	1.000	1.000	1.000
period length	2 weeks	4 weeks	8 weeks	12 weeks
observations	9854	5218	2616	2034
# care providers	299	299	299	299
# time periods	34	18	9	7

Notes: Pre-experiment mean of the outcome variable in the ex-post audit regime is reported as a reference. Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%.

Table 4.D.8: Robustness check: accounting for serial correlation.

	# applications		**approval**	
	level	*normalized*	*application-level*	*aggregated*
	(1)	(2)	(3)	(4)
ex-ante audit regime	−0.631	−0.114	−0.042***	−0.058**
	(0.424)	(0.084)	(0.015)	(0.024)
conditional audit regime	−0.868**	−0.171**	−0.027*	−0.064**
	(0.409)	(0.082)	(0.015)	(0.028)
mean ex-post audit regime (before experiment)	3.545	1.000	0.829	0.829
	(0.362)	(0.000)	(0.014)	(0.014)
observations	590	590	16,876	586
# care providers	299	299	299	299

Notes: The models include provider fixed effects, a treatment period indicator and interactions between treatment group and the treatment period indicator. As in the main analyses, the model in column (2) does not include provider fixed effects. No additional controls are included. Pre-experiment mean of the outcome variable in the ex-post audit regime is reported as a reference. Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%.

Table 4.D.9: Short-run, long-run and post-treatment effects on the audit approval probability (January 1, 2012 - November 24, 2013).

	approval
ex-ante regime, short-run	-0.048^{***}
	(0.018)
ex-ante regime, long-run	-0.028
	(0.019)
ex-ante regime, post-experiment	0.012
	(0.015)
conditional regime, short-run	-0.033
	(0.020)
conditional regime, long-run	-0.021
	(0.019)
conditional regime, post-experiment	0.005
	(0.015)
mean ex-post audit regime (before experiment)	0.827
	(0.014)
p-value, short-run	0.023
p-value, long-run	0.309
p-value, post-experiment	0.741
care provider fixed effects	yes
time fixed effects	yes
care characteristics	yes
patient characteristics	yes
assessor indicators	no
observations	24,170
# care providers	299

Notes: Pre-experiment mean and standard error of the outcome in the ex-post audit regime are reported as a reference. Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%. Reported p-values are for F-tests of the null hypothesis of no effect of any of the audit regimes in the short-run, long-run and after the experiment.