Audit regimes in long-term care

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Abstract

This paper studies the effects of various audit regimes used by a gatekeeper, differing in the degree of enforcement and the presence of performance incentives, on behavior of care providers filing applications for providing long-term care services to patients. We present evidence from a large-scale field experiment in the Dutch market for long-term care. We find that increasing the degree of enforcement reduces the number of applications and that introducing performance incentives reduces this even further. Finally, we find detrimental effects on audit approval rates, but we provide some results showing that assessors are less strict when audits do not have direct implications. This implies that an audit regime with performance incentives can support policy makers, who are faced with the trade-off between providing services quickly and the efficient spending of public resources.

Keywords: auditing, incentives, field experiment, long-term care, gatekeeper, enforcement
JEL-code: C93, H51, I18

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1 Introduction

The costs associated with health impairments are seldom born by the individual herself. Social insurance funds provide sick pay to individuals who report sick from work, insurers pay for the costs of doctor or hospital visits, and long-term care is often financed publicly. To reduce potential moral hazard, many countries have introduced gatekeepers, which are responsible for enforcing access to services.[1]

In this paper, we consider the gatekeeper for the public long-term care system in the Netherlands. In the Netherlands, as in most countries long-term care expenditures have been increasing steadily over the past decades.[2] This introduces a tension between adequate provision of care for those who need it and efficient spending of public resources. Therefore, the gatekeeper uses screening and auditing policies. Several audit policy instruments are available, such as audit frequency, degree of enforcement, (financial) sanctions, and performance incentives. These instruments are important determinants of the speed at which an application can be approved and care can be provided. Using a nationwide field experiment, we study the consequences of changing the audit regime.

In the Netherlands, when a patient requires long-term care services, she approaches a care provider and, subsequently, the care provider files an application for (public) funding to the gatekeeper. Long-term care involves chronic care, either facility-based or home-based, for the elderly and the mentally or physically impaired. Examples of care providers are nursing homes and companies delivering home-based care. In the application, the care provider specifies the care needs and asks for a specific type and amount of care services and the way in which this should be delivered to the patient (e.g., facility-based or home-based). The Dutch gatekeeper is a separate and independent institution entrusted with the task of assessing long-term care needs of patients. The gatekeeper can either approve the application directly or perform an audit. In case of an audit, assessors employed by the gatekeeper

[1] In many settings general practitioners (GPs) act as gatekeepers by giving prescriptions for medication and referring patients to specialized care. Hartman et al. (2013) show that GPs can be effective as gatekeepers, but their costs are high.

[2] 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). Second, 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 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).

study the actual care needs of the patient and compare this to care services that are requested by the care provider. Care providers are not allowed to start care provision before the gatekeeper has approved the application. As a consequence, audits delay the moment at which the patient receives care services. We refer to this as *ex-ante* auditing. Under ex-ante auditing, an audit can result in an approval, denial or adjustment of the application. An adjustment implies that the amount of care for which the provider receives funding, differs from what has been requested.

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. Assessors still determine whether the application should have been approved, adjusted or denied and this outcome is communicated to the care providers such that they learn from the feedback. However, no actual adjustment to, for instance, the type and amount of care takes place. The degree of enforcement 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.[3] In the context of the Netherlands, neither ex-ante nor ex-post auditing allow for (financial) sanctions in case of disapproval. Prior to our experiment ex-post auditing was already in use for cheaper and less extensive types of long-term care (Lindeboom et al., 2016). The main motivation for the gatekeeper to use ex-post auditing is that it believes that many care providers are intrinsically motivated to file applications correctly and in accordance with the actual care needs of patients. Ex-post auditing can then be equally effective as ex-ante auditing, while ensuring immediate access to care services and reducing the bureaucracy experienced by care providers.

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 of a care provider. 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 enforcement, but enforcement is targeted towards poorly performing providers. This regime yields fewer delays in care provision than the ex-ante audit regime, but introduces incen-

[3] In the US ex-post auditing is sometimes applied by insurers and is called "post-claims underwriting". There, sanctions are imposed in case of noncompliance.

tives 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. Our prime outcome of interest is the quantity of applications since this is the main determinant of the long-term care budget. If auditing is efficient, it should reduce moral hazard in demand for long-term care and thus the number of applications. We, therefore, also focus on substitution of applications to other types of long-term care where the strictness of auditing differs. In addition, we focus on the approval rate, but here we are more cautious because the type of auditing (ex-post or ex-ante) may also affect the stringency of the assessor employed by the gatekeeper. We do not consider health effects of providing patients access to long-term care. These health effects will only become visible in the long run, so beyond our current observation period.

Our field experiment is related to the field experiment described by Lindeboom et al. (2016). Both field experiments took place at the same time and within the same institutional setting. Lindeboom et al. (2016) study the effect of changing audit rates (either conditional or random). Because they consider a setting of ex-post auditing, the auditing result never has consequences. Varying the timing of audits as is done in this paper affects the degree of enforcement and therefore yields direct incentives to comply. Furthermore, the type of long-term care studied in this paper is much more extensive and thus more expensive than the cheap care studied by Lindeboom et al. (2016).

The theoretical literature on auditing and regulatory compliance starts 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. Whereas, in this traditional model auditing is random, later papers study conditional audit rules which reduce the number of audits but also find a lower compliance rate (e.g., Clark et al., 2004; Cason and Gangadharan, 2006). A more recent literature considers the performance of audit tournaments, in which the probability of being audited not only depends on one's own behavior, but also on the behavior of others (e.g., Cason et al., 2016; Gilpatric et al., 2011, 2015). The results come from laboratory experiments and show that dynamic audit tournaments have more favorable outcomes that go beyond more truthful reporting.

In contrast to the traditional model, behavioral models argue that enforcement crowds out intrinsic motivation of agents. With only weak incentives available to

the principal, trusting agents would provide more favorable outcomes.[4] However, Bengtsson and Engström (2014) do not find evidence for the crowding out effect for non-profit organizations in a field experiment where they switch from a fully trust-based regime to one with increased monitoring.

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. On the contrary, Ashraf et al. (2014) find that financial rewards can improve the performance of agents involved in delivery of public services when the financial incentive has a relatively high value. At the same time, they also find that non-financial rewards affect performance in a positive way. Boyer et al. (2016) find that imposing norms on contribution behavior crowds out intrinsic motivation. However, once a norm is in place, enforcement only modestly affects intrinsic motivation. 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.[5]

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 and conditional audit rules, 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

[4]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. Also in a laboratory experiment Falk and Kosfeld (2006) find that most agents reduce performance when the principal chooses to enforce.

[5]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).

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 regime.

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 percent fewer applications than in the least strict ex-post audit regime. This can be the result of substitution to other types of long-term care applications. Furthermore, we find an (insignificant) 10% percent 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. Finally, by comparing care providers assigned to participate in our experiment to those not part of our experiment, we do not find any evidence for a Hawthorne effect of being assigned to participation in the experiment.

The remainder of this paper is structured as follows. The next section provides institutional details on the Dutch long-term care market. Section 3 describes the experiment and discusses hypotheses on the effects of the audit regimes. The data are described in section 4. The econometric analysis and results are discussed in section 5. Finally, section 6 concludes.

2 Institutional background

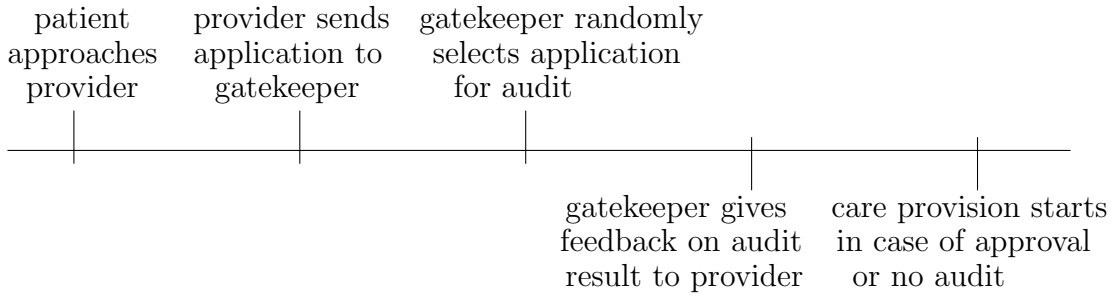
Each person living in the Netherlands is publicly insured for long-term care by means of the Exceptional Medical Expenses Act (AWBZ).[6] This covers chronic care, either facility-based or home-based, for the elderly, the mentally and/or physically impaired, and chronic psychiatric patients. On July 1, 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 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.[7] The care provider studies the care needs of

[6]The description of the institutional details in this section is similar to Lindeboom et al. (2016), who study a field experiment in the same Dutch long-term care market, and draws on Mot (2010), Nederlandse Zorgautoriteit (2013) and information from the Center for Care Assessment (CIZ).

[7]A patient can file a request directly to the gatekeeper and indicate the preferred care provider

Figure 1: Process of application for long-term care services (before the experiment).



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 has to be delivered and for which period.[8] 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 gatekeeper can approve the application immediately or initiate 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 home-based and 800 provided facility-based care. Facility-based care providers are required to be non-profit institutions, while home-based care providers can also be for-profit organizations. The care providers differ considerably in size. Some providers are large-scale nursing homes, whereas others consist of a very small number of care givers who visit patients at their homes. Next to goals of profit maximization, for home-based 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.[9]

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.

[8]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).

[9]In the period from July 2010 to July 2011, about 2.5% of those receiving home-based care through a re-assessment application switched provider and 8% of those receiving facility-based care switched (Nederlandse Zorgautoriteit, 2013).

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, the care provider should file an application type that is subject to 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 paper, we focus on the so-called re-assessment applications.[10] Re-assessment applications are used to prolong the provision of existing care services, but often request an adjustment in the intensity or type of services. This can concern facility-based and home-based 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. Re-assessment applications usually request care provision for a long period of up to 15 years.[11] 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, in practice this means that every fourth filed application gets 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.[12] 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 from the assessor, explaining what went wrong and how the quality

[10]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.

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

[12]Assessors are instructed to audit according to the rules set by the gatekeeper. They do not have targets on audit approval rates. In line with the goal of the gatekeeper, assessors should aim to audit in such a way that patients receive long-term care in the form and amount that is truly necessary.

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 postpone care provision until approval from the gatekeeper in that case. The outcome of the audit 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 choose 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*', '*facility-based 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.[13] Facility-based notifications concern institutionalized care for patients over 80 years of age. 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.

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 3.2. Subsection 3.3 states the hypothesized effects of the audit regimes on application behavior of care providers.

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

[13] Lindeboom et al. (2016) 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.

Table 1: Consequences of ex-ante and ex-post audits.

	ex-ante	**ex-post**
assessment of needs immediately provided	no	yes
care provisions starts directly after provider files application	no	yes
financial sanctions in case of disapproval	no	no
adjustment in allowed care provision in case of disapproval	yes	no
denial of application is possible	yes	no

services, but removes the possibility to actually adjust or deny applications. In particular, no (financial) sanctions can be applied in case of disapproval. Moreover, in case of ex-post auditing, care provision can continue without changes to, for example, the type or amount of care, even in case an assessor labels the application as disapproved. The care provider is still, as in ex-ante auditing, informed about the decision of the assessor, but the decision lacks consequences for care provision. Ex-post auditing is thus less strict than ex-ante auditing. Table 1 lists the implications of both ex-ante and ex-post audits.

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.[14] 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.[15] These updates were communicated to the care providers supported by a list of results of the relevant audits. All care providers in all treatment groups received the same detailed feedback about the outcomes of their own audits.

In all three 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.

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-

[14]The threshold of 0.92 is the gatekeeper's target approval rate.

[15] 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.

assessment applications filed in April 2012. We randomly assigned care providers to one of the three treatment groups discussed in the previous section.[16] Participation was compulsory. Care providers were informed by letter and e-mail about the set-up of the experiment.[17]

Table 2 illustrates that the treatment groups are balanced in terms of pre-experiment outcomes and care-provider characteristics. On average, slightly more 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 3 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 the 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 5.3.1.

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. Patients typically like to receive long-term care as soon as possible after contacting a long-term care provider. Since long-term care is publicly funded, both the patient and the care provider benefit from increasing the amount of care. The gatekeeper is not able to make an assessment of needs based on a detailed audit

[16]The original proposal for the field experiment including power analysis is available at http://personal.vu.nl/b.vander.klaauw/OpzetCIZOnderzoek.pdf [in Dutch]. The pre-experiment data reveal that about 300 care providers, on average, file over four applications per month and the power analysis show that this sample size is sufficient.

[17]An indication of 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 2: 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	

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

for each patient, both since care services may be quickly needed and since auditing budgets are limited as well.

Care providers are mandated to determine the care needs of the patient themselves by filing a re-assessment application. Filing an application is necessary to receive funding for the provision of long-term care. However, it 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 devoting an insufficient amount of effort to the application increases the probability of a disapproval.

According to the traditional model of Allingham and Sandmo (1972), care providers weigh the benefits of requesting funding for more care and devoting more effort

Table 3: 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	-

Standard errors of means in parentheses. 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).

to filing the application against the expected costs of noncompliance (i.e., filing incomplete or incorrect applications) given the degree of enforcement and the level of punishment imposed by the gatekeeper. This traditional model implies that if the costs of noncompliance are substantial, care providers file complete applications, do not request too much care and do not file invalid applications. In our setting, the gatekeeper can choose between ex-ante and ex-post auditing. Neither includes direct financial sanctions in case of noncompliance. Despite the lack of financial sanctions, care providers may still dislike disapprovals since they will receive feedback from the care provider, which takes time to discuss with the gatekeeper. Ex-ante auditing is associated with a higher degree of enforcement 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. A prediction of the traditional model is that under ex-ante (and also conditional) auditing care providers file fewer applications and have a higher approval rate.

The traditional model does not yield a clear prediction as to 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 are 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. External interventions, like enforcement and punishment, can crowd out intrinsic motivation (e.g., Gneezy et al. (2011) and other references in the introduction). Ex-post auditing has the lowest amount of enforcement by the gatekeeper. In the ex-ante audit regime the amount of enforcement is largest. The conditional audit regime has a lower degree of enforcement compared to the ex-ante regime, but incentives in the conditional regime are stronger. Increasing the degree of enforcement may reduce effort put in filing applications and can reduce compliance. If intrinsic motivation is the driving force and enforcement 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 lower enforcement. They argue that trust-based auditing can be feasible when there are certain costs of enforcement to agents and when agents sufficiently value the future. In our setting, enforcement-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 behave according to the traditional model (referred to as traditional care providers in the remainder), while others 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 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 enforcement 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 this threshold is set too high, it is too costly for the traditional care providers to comply. Furthermore, the relatively large amount

of enforcement in case of a high threshold crowds out intrinsic motivation of the intrinsically motivated care providers. This reduces their performance (below the threshold) so that these providers will end up in ex-ante auditing.

4 Data

We have access to administrative data containing all applications filed by all care providers between January 1, 2012 and November 24, 2013.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 5.2.1 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.[18]

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.[19]

Descriptive statistics on application and patient characteristics for whom re-assessment applications are filed, are provided in Table 4. 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 home-based care. A single application can be concerned with multiple types of care. The most frequent type of care is personal care, followed by assistance and facility-based 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% are married. This may relate to the presence of informal care options, which is taken into account in determining

[18]Only in subsection 5.2.2 we also consider applications of care providers not participating in the experiment.

[19]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: Application characteristics and patient characteristics (January 1, 2012 - April 7, 2013).

	ex-ante	**ex-post**	**conditional**
Types of re-assessments			
change in home-based care	69%	73%	75%
prolong (unchanged) facility-based care	16%	13%	11%
change in facility-based 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%
facility-based 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

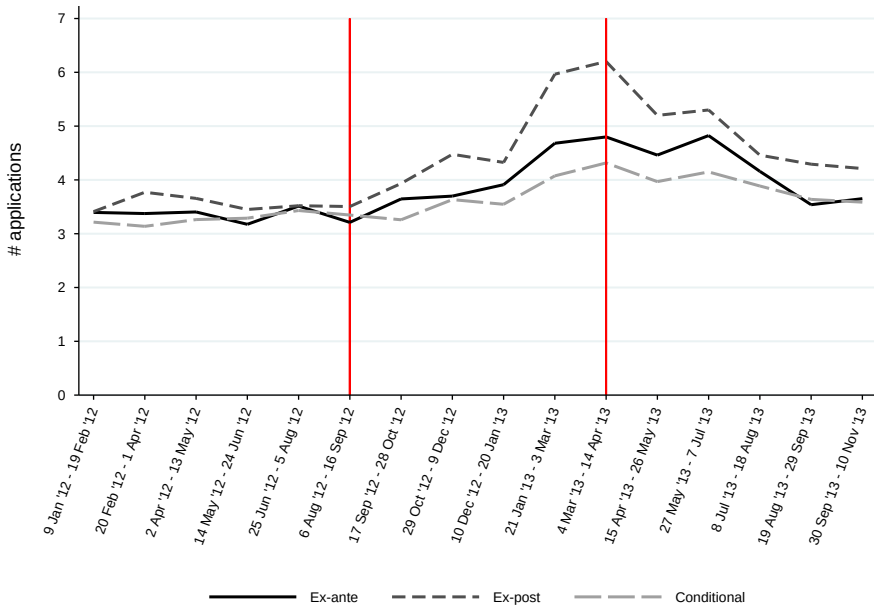
eligibility for formal long-term care. Additional descriptives on outcomes during the experiment are provided in Table A.3 in the appendix.

Changes to the timing of audit for care providers in the conditional regime are registered in the data every two weeks. During the experiment, there were twelve opportunities for providers to switch between ex-ante and ex-post auditing. On average a provider switches 1.8 times during the experiment. Appendix A.2 provides details switching behavior and plots the distribution of switches.

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. For ease of interpretation we normalize the number of applications to weekly averages for each care provider. The resulting panel data set is unbalanced, because providers may become inactive due to closing down activities or merging with another provider.[20]

[20]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 the appendix. Tables A.4 and A.5 in the appendix show that results are robust with respect to these choices.

Figure 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.

5 Results

Our key interest is the number of applications. But we also study the quality proxied by the audit approval rate. Subsection 5.1 provides descriptive evidence on the trends in these outcomes. In subsection 5.2 we quantify the effects of the audit regime on the number of applications. Subsection 5.3 looks at effects on the quality of applications. Finally, in subsection 5.4 we link our results to the hypotheses formulated above.

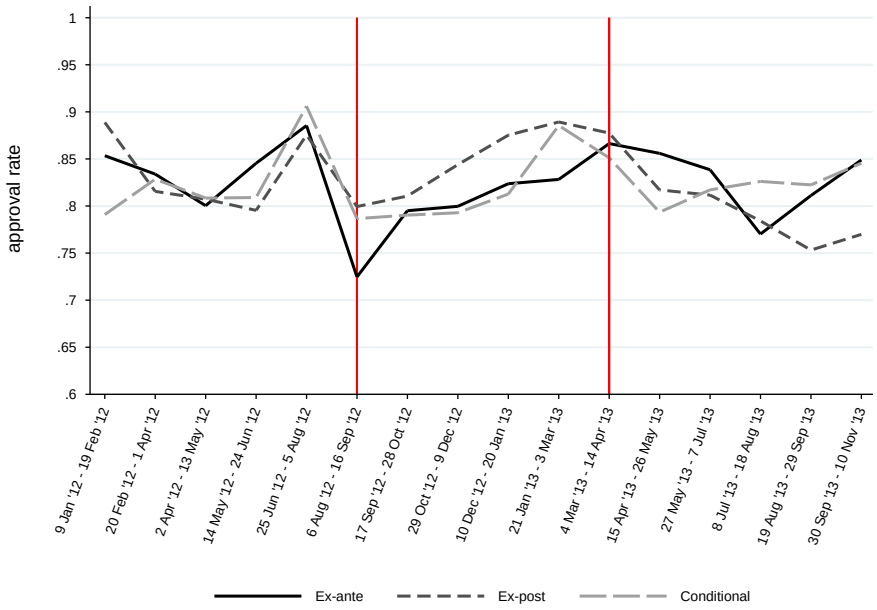
5.1 Descriptive evidence

Figure 2 shows the trend in the average number of re-assessment 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.

Furthermore, we are interested in the audit approval rate as an outcome measure. Figure 3 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 3: 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.

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,

$$Y_{c,t} = \alpha + \gamma_t + \delta_{\text{ex-ante}} T_{c,t}^{\text{ex-ante}} + \delta_{\text{conditional}} T_{c,t}^{\text{conditional}} + \eta_c + \varepsilon_{c,t} \tag{1}$$

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.[21] Because data are from a randomized experiment, the treatment status is independent from the care provider specific effects η_c, which we, therefore, model as random effects.[22] The treatment variables $T_{c,t}$ equal one if care provider c belongs to a treatment group in period

[21] 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. Furthermore, recall that in the beginning of the experiment regime-updating took some more time (see footnote 15). Results are robust to excluding the first two, four or six weeks of the experiment from the analysis.

[22] Fixed effects estimation gives almost the exact same estimates and standard errors.

t.[23] 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 3.3 are in comparison to this regime. The ex-ante regime increases enforcement for all care providers irrespective of performance. The conditional audit regime increases enforcement 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. Because of this normalization we eliminate provider specific effects.

The baseline estimation results in Table 5 show a substantial, but insignificant, decline of 0.626 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 0.954 applications per week and significant. This corresponds to a 20% decline of the number of applications in the conditional audit regime compared to the ex-post audit regime. 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.[24] An alternative explanation, as discussed in the hypotheses in subsection 3.3, is 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. We reject the null hypothesis of no effect of any of the treatments as indicated by the F-test p-values in the table.

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. 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 (1) with dummy

[23]For the estimation we use data from both the pre-treatment and the experiment period. Only using the experiment period only modestly changes the estimates, but slightly increases the standard errors.

[24]In section A.2 in the appendix we discuss the switches of providers between ex-ante and ex-post auditing in the conditional audit regime. We find that sorting of providers, based on pre-experiment approval rates, can explain the switches that we observe.

Table 5: Baseline estimates for the number of applications (per week) at the provider level (January 1, 2012 - April 7, 2013).

	# applications		**substitution**	
	level	*normalized*	*all types, excl. re-assessments*	*emergency*
	(1)	(2)	(3)	(4)
ex-ante audit regime	−0.626	−0.104	0.377	0.125
	(0.411)	(0.079)	(0.530)	(0.149)
conditional audit regime	−0.954**	−0.203***	0.742	0.464*
	(0.398)	(0.078)	(0.960)	(0.247)
mean ex-post audit regime	3.557	1.000	16.973	2.701
(before experiment)	(0.362)	(0.000)	(2.768)	(0.572)
care provider random effects	yes	no	yes	yes
p-value joint significance	0.032	0.030	0.668	0.171
observations	9286	9286	9286	9286
# care providers	299	299	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%. The p-value for the F-test is for joint significance of the coefficient on the ex-ante audit regime and the conditional audit regime.

variables for the size of a provider.[25,26] The estimation results in Table A.6 in the appendix 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.

The decline in the number of re-assessment applications in the ex-ante and conditional audit regimes compared to the ex-post audit regime may be explained by sub-

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

[26]In addition, heterogeity in effects for providers differing in terms of the share of re-assessment applications in total applications is studied. We find some evidence that providers having a small share of re-assessment applications, respond more strongly to the ex-ante and conditional audit regimes. Results are available on request.

stitution towards other application types. The content of standard applications and facility-based notifications is substantially different from the typical re-assessment application. Regular applications are subject to more stringent screening than re-assessment applications, but emergency care applications are audited less strictly than re-assessment applications. Hence, if care providers substitute, they most likely will substitute to emergency care applications.

To test for the presence of substitution effects, we estimate the baseline model in equation (1) having as outcomes the number of emergency care applications and the total number of applications other than re-assessment applications.[27] Estimation results are reported in columns (3) and (4) of Table 5. The estimated substitution effects are quite substantial, but hardly significant. Only for providers in the conditional audit regime we find a marginally significant substitution effect: the decline in the number of re-assessment applications is accompanied by an increase in the number of emergency care applications.

Some re-assessment applications may be easier to substitute with emergency care applications than others, which could 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 A.7 in the appendix. In particular, there is no shift in patient characteristics (age and gender) and types of re-assessment applications.[28]

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

Figure 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 shows a divergence of the average number of applications between the treatment groups during the experiment, and a subsequent convergence afterwards. Therefore, we

[27] 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. In all empirical analyses, we ignore this second application.

[28] We estimated the panel data model in equation (2) on re-assessment application-level data.

$$A_{i,c,t} = \alpha + \gamma_t + \zeta_{\text{ex-ante}} T_{i,c,t}^{\text{ex-ante}} + \zeta_{\text{conditional}} T_{i,c,t}^{\text{conditional}} + \eta_c + \upsilon_{i,c,t} \tag{2}$$

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 patient). We again include provider random 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.

extend our model to estimate separately short-run effects (fourth quarter 2012), long-run effects (first quarter 2013) and post-experiment effects,

$$\begin{aligned} Y_{c,t} = \alpha + \gamma_t &+ \delta_{\text{ex-ante,short}} T_{c,t}^{\text{ex-ante,2012Q4}} + \delta_{\text{conditional,short}} T_{c,t}^{\text{conditional,2012Q4}} \\ &+ \delta_{\text{ex-ante,long}} T_{c,t}^{\text{ex-ante,2013Q1}} + \delta_{\text{conditional,long}} T_{c,t}^{\text{conditional,2013Q1}} \\ &+ \theta_{\text{ex-ante}} P_{c,t}^{\text{ex-ante}} + \theta_{\text{conditional}} P_{c,t}^{\text{conditional}} + \eta_c + \varepsilon_{c,t} \end{aligned} \tag{3}$$

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.

The estimation results in Table 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.

Table 6: Short-run, long-run and post-treatment effects (January 1, 2012 - November 24, 2013).

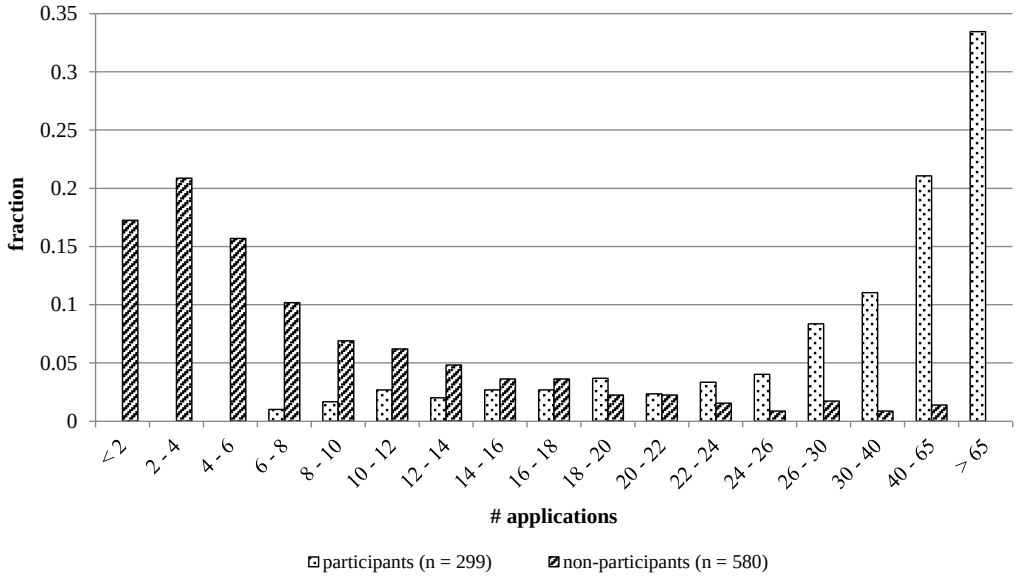
	# applications	
	level	*normalized*
	(1)	(2)
ex-ante regime, short-run	−0.322	−0.053
	(0.290)	(0.064)
ex-ante regime, long-run	−1.096*	−0.222*
	(0.624)	(0.134)
ex-ante regime, post-experiment	−0.489	−0.037
	(0.364)	(0.112)
conditional regime, short-run	−0.496*	−0.090
	(0.279)	(0.065)
conditional regime, long-run	−1.575***	−0.354***
	(0.609)	(0.133)
conditional regime, post-experiment	−0.662	0.013
	(0.437)	(0.114)
mean ex-post audit regime (before experiment)	3.557	1.000
	(0.362)	(0.000)
p-value, short-run	0.161	0.382
p-value, long-run	0.026	0.027
p-value, post-experiment	0.283	0.916
care provider random effects	yes	no
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.

5.2.2 Hawthorne effect

Before the start of the experiment all care providers were exposed to the ex-ante audit regime. Recall from Figure 2 that after the start of the experiment the average number of applications of care providers randomized in the ex-ante audit regime

Figure 4: Distribution of number of re-assessment applications for participating and non-participating care providers, February 2012 - June 2012.

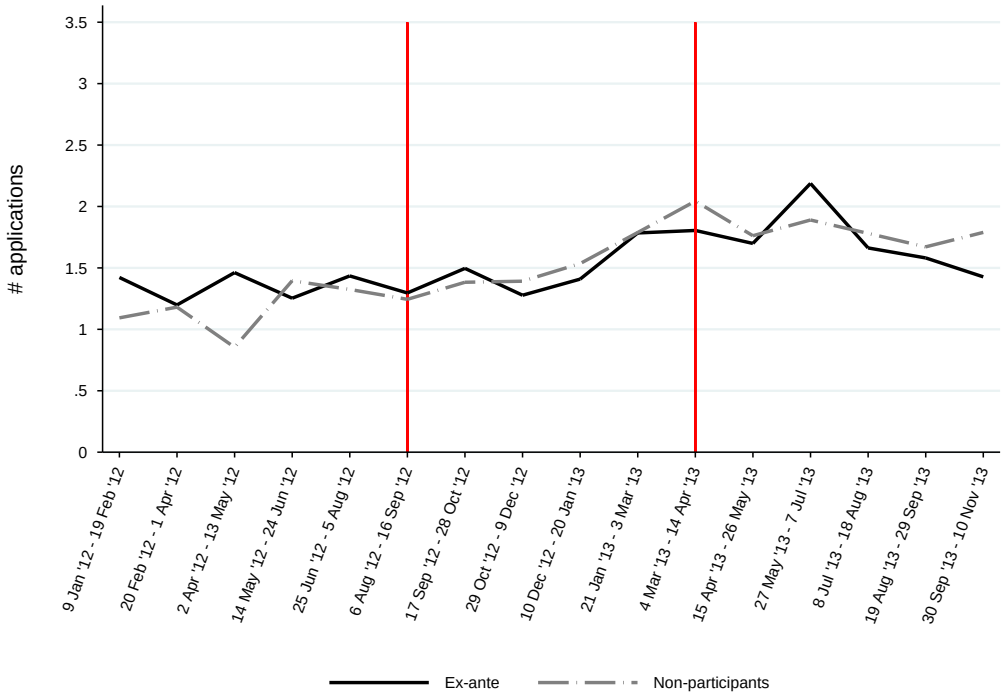


started increasing. This might be a Hawthorne effect, implying that participation in the experiment already affects outcomes. A possible explanation might be that informing providers about the experimental variation in auditing regimes for re-assessment applications draws more attention to the possibility 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 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.[29] 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. Thus within each cell shown in Figure 4 we give each non-participating care provider a weight based on the ratio of the number of parti-

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

Figure 5: Average number of re-assessment applications, providers in ex-ante regime and non-participants.



pating and non-participating care providers in the cell. 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 findings in Figure 2. Figure 5 illustrates the trends in the number of applications over time for the ex-ante regime and the non-participants. These trends are very comparable.[30] 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.

[30]To test this more formally, we estimate weighted panel fixed effects models similar to our baseline specification in equation (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 A.8 in the appendix show that there is no significant difference in the (normalized) number of applications in the ex-ante audit regime compared to the non-participants.

5.3 Quality of applications

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

$$Q_{i,c,t} = \alpha + \gamma_t + \delta_{\text{ex-ante}} T_{i,c,t}^{\text{ex-ante}} + \delta_{\text{conditional}} T_{i,c,t}^{\text{conditional}} + \beta X_{i,c,t} + \eta_c + \upsilon_{i,c,t} \quad (4)$$

$Q_{i,c,t}$ is a dummy for approval of application i filed by provider c at time period t. Again we include provider random effects η_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.

The estimation results are presented in Table 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 2.5 percentage points decrease for switching to the conditional audit regime.[31] We can reject the null hypothesis that both the ex-ante audit regime and the conditional audit regime simultaneously have no effect on the approval probability. The direction of the effects is consistent with the presence of intrinsically motivated care providers, i.e. increasing the degree of enforcement crowds out intrinsic motivation thereby reducing compliance. The estimates for the treatment effects are robust against including covariates. Only including the assessor indicators in column (4) causes the effects to change 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.

[31] 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 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 A.11 in the appendix.

Table 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	-0.039^{***}	-0.040^{***}	-0.040^{***}	-0.045^{***}
	(0.013)	(0.013)	(0.013)	(0.014)
conditional audit regime	-0.027^{**}	-0.025^{*}	-0.024^{*}	-0.019
	(0.013)	(0.013)	(0.013)	(0.015)
mean ex-post audit regime	0.827	0.827	0.827	0.827
(before experiment)	(0.014)	(0.014)	(0.014)	(0.014)
F-test p-values	0.008	0.007	0.006	0.006
care provider random 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: 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%. F-test p-values are reported for testing whether the coefficient on the ex-ante audit regime and the conditional audit regime are simultaneously equal to zero. 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 facility-based 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.

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) with a dummy variable for the initial approval rate category.[32] The estimation results in Table A.6 in the appendix 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.[33]

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. Which application is in fact audited, cannot be influenced by the assessor, but the strictness of auditing can. Table 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*.[34] Recall that ex-post audits occur both in the ex-post audit regime and

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

[33] There can be heterogeneity across types of patients for whom an application is filed. For some types of patients it is 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 consider heterogeneous effects by age and gender and find 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.

[34] These shifts can be (partly) attributed to differences in data quality of registrations of disapproval reasons for ex-ante and ex-post audits and hence do not relate to behavioral responses of care providers. Because of this we do not present more formal analyses of the effect of the audit

Table 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.

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 16 assessors auditing re-assessment applications during the experiment, only three performed both ex-post and ex-ante audits. We exploit this to investigate whether ex-post audits are less strict than ex-ante audits. For this purpose we consider the panel data model,

$$Q_{i,a,c,t} = \alpha_c + \gamma_t + \xi_a + \theta P_{i,a,c,t} + \eta_{i,a,c,t} \tag{5}$$

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 include provider fixed effects and a general time trend. Provider fixed effects are required because in the conditional audit regime, the choice for ex-ante or ex-post audit is not random. 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 ex-ante audit.

Table 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

regime on disapproval reasons.

Table 9: Effect of ex-post auditing on approval probability (September 17, 2012 - April 7, 2013).

	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: Both models are estimated on audits during the experiment only. 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%.

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

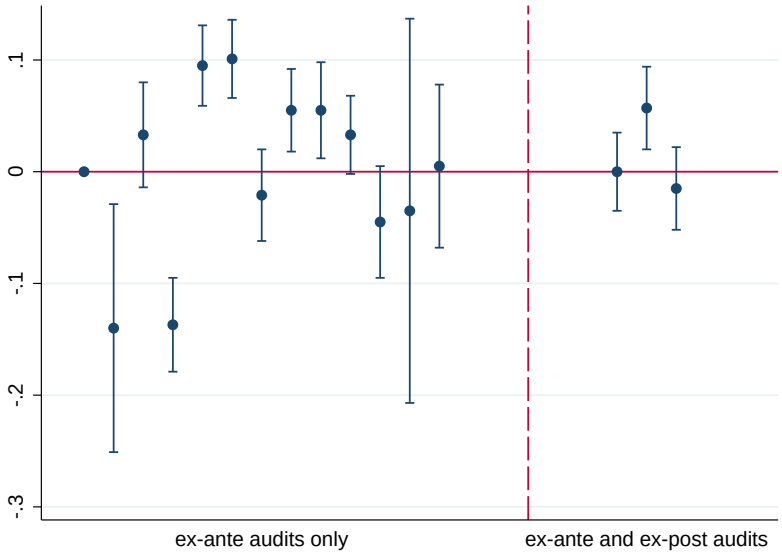
To characterize the three assessors doing ex-post audits during the experiment, we estimate the following model using only pre-experiment data.

$$Q_{i,a,c,t} = \alpha_c + \gamma_t + \xi_a + \eta_{i,a,c,t} \tag{6}$$

Figure 6 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 strong 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 6: Estimated assessor fixed effects.



5.4 Link to hypotheses

In this subsection we briefly relate our empirical results to the hypotheses formulated in subsection 3.3. Our results show that the number of applications are lowest in the conditional audit regime and highest in the ex-post audit regime. Ex-post auditing, relying on a large degree of trust, does not seem to work in this context. This rejects the idea of intrinsic motivation. The results are more consistent with the traditional auditing model, which predicts that increasing the degree of enforcement (the ex-ante compared to the ex-post audit regime) and providing incentives (the conditional to both other audit regimes) reduce the number of applications

The decrease in the number of applications is partly offset by substitution towards other, cheaper application types. This in line with the traditional model, where providers search for alternative opportunities when the costs of (false) applications increase. In the trust-based auditing model one would not expect to observe substitution between alternative application types. The substitution is towards cheaper types of care, which implies that ex-post auditing yields the highest long-term care expenditures.

The main goal of the gatekeeper is not to reduce expenditures, but instead to minimize moral hazard without rejecting valid applications. The question is whether the decrease in the number of applications involves applications that would actually be valid, cannot be answered by our analysis. However, the approval rate is lower in the ex-ante and conditional audit regimes compared to the ex-post audit regime, which may be considered as evidence against the traditional audit model. But there is some evidence that this outcome is partly explained by the assessors valuing ex-

post audits less than ex-ante audits. Because, in the ex-post audit regime also the invalid applications receive funding, this regime is the most costly regime for the public funds.

6 Conclusion

Public resources for long-term care services can be managed by a gatekeeper deciding on the approval of applications for these services. However, often care is needed quickly, which limits the gatekeeper in its possibilities to audit applications. In this paper we discuss 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 enforcement, 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). Ex-ante auditing means that the gatekeeper makes the final decision on the care services that can be provided to the patient. The gatekeeper then has a high degree of enforcement, 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 enforcement (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 enforcement (ex-ante auditing) is targeted towards poorly performing care providers, which yields as a performance incentive that enforcement is low (ex-post auditing) when the performance of a care provider is sufficiently good. Furthermore, we find negative effects of both the ex-ante and the conditional audit regime on the quality of applications. We find (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 enforcement 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. The main policy implication is that audit rules can affect applications for long-term care ser-

vices. So, a gatekeeper is important when aiming to limit the expansion of long-term care expenditures. In particular, auditing is most efficient when there are incentives such as in the conditional audit regime (as compared to the unincentivized ex-post audit regime). This regime is also efficient in terms that it causes fewer delays in access to care services than the ex-ante regime. However, the auditing efficiency might be somewhat limited by a shift towards other application types, but these types are typically associated with less extensive care. The lesson for policy makers is thus that (some) care providers make applications to types of care where access is most easily obtained.

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Appendices

A.1 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 A.9 (weighted) and A.4 (exclusive) in the appendix, 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 A.4 in appendix A.3.

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 A.10 in appendix A.3. Columns (1) and (2) show coefficients similar in magnitude to our baseline estimates reported in Table 5. 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 7 column (1)). However, random 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 A.5 in appendix A.3. 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 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.

A.2 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 A.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 A.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 A.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. 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

Table A.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

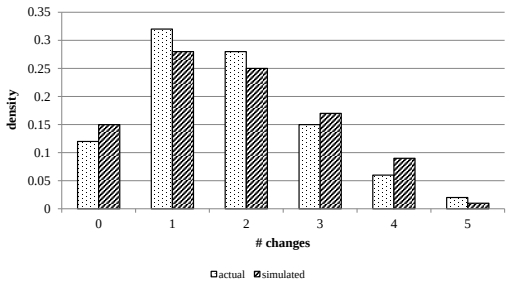
In parentheses in the first column is the date at which the update became effective. Reported are percentages of providers in the conditional audit regime. Four care providers did not have any audited application during the experiment. For them the timing of audit remained at ex-ante without this being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

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 A.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 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.

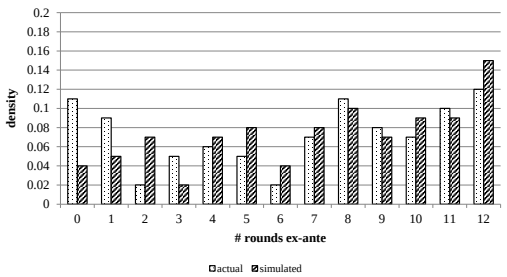
In addition, Figure A.1a shows that the actual and simulated distributions of the number of switches are quite similar. This is confirmed by a formal χ^2-test for equality of the actual and simulated distributions (p-value = 0.872). Figure A.1b

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

(a) Changes in the timing of audit



(b) Frequency of ex-ante auditing



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 (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.

A.3 Additional tables

Table A.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]

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.

Table A.3: 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)
facility-based 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

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

Table A.4: Robustness check: level of time aggregation.

	# applications				
	(1)	(2)	(3)	(4)	(5)
ex-ante audit regime	−0.626	−0.652	−0.614	−0.534	−0.505
	(0.411)	(0.419)	(0.413)	(0.379)	(0.390)
conditional audit regime	−0.954**	−0.963**	−0.938**	−0.829**	−0.766**
	(0.398)	(0.404)	(0.399)	(0.366)	(0.376)
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 A.5: Robustness check: inactive care providers.

	# applications				
	baseline	*balanced*	*excluded*	*weighted*	*removed*
	(b)	(a)	(c)	(d)	
ex-ante audit regime	−0.626	−0.708*	−0.623	−0.622	−0.639
	(0.411)	(0.417)	(0.412)	(0.411)	(0.419)
conditional audit regime	−0.954**	−0.915**	−0.949**	−0.957**	−0.982**
	(0.398)	(0.403)	(0.398)	(0.398)	(0.403)
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	*balanced*	*excluded*	*weighted*	*removed*
	(b)	(a)	(c)	(d)	
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 A.1. 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 A.6: Heterogeneous effect estimates.

Panel A: heterogeneity in provider size			**Panel B: heterogeneity in initial approval rates**		
	# applications (norm.)	***approval***		***# applications (norm.)***	***approval***
	(1)	(2)		(3)	(4)
ex-ante × small	−0.148	−0.080**	ex-ante × low	−0.024	−0.039**
	(0.169)	(0.041)		(0.091)	(0.017)
ex-ante × medium	0.046	−0.018	ex-ante × intermediate	−0.302**	−0.040*
	(0.091)	(0.022)		(0.149)	(0.021)
ex-ante × large	−0.379**	−0.042**	ex-ante × high	−0.126	−0.044
	(0.157)	(0.019)		(0.246)	(0.039)
conditional × small	−0.280*	−0.050	conditional × low	−0.087	−0.030*
	(0.153)	(0.035)		(0.095)	(0.018)
conditional × medium	0.002	−0.008	conditional × intermediate	−0.419***	−0.018
	(0.104)	(0.023)		(0.159)	(0.022)
conditional × large	−0.368**	−0.029*	conditional × high	−0.341*	0.018
	(0.150)	(0.017)		(0.203)	(0.033)
	(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	0.127	p-value, low	0.610	0.057
p-value medium	0.849	0.674	p-value, intermediate	0.027	0.170
p-value, large	0.042	0.058	p-value, high	0.178	0.334
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. The models include dummies for small and large providers (medium-sized providers are the reference) or dummies for low and high initial approval rates (intermediate initial approval rates is the reference). 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 A.7: 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	0.007	0.006
	(0.010)	(0.009)	(0.013)	(0.015)	(0.009)
conditional audit regime	−0.009	0.006	0.007	0.008	−0.014
	(0.009)	(0.007)	(0.012)	(0.016)	(0.011)
intercept	0.585***	0.281***	0.709***	0.118***	0.124***
	(0.018)	(0.020)	(0.020)	(0.011)	(0.013)
# 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 home-based care, type 2 applications concern prolongation of facility-based care without changes, and type 3 re-assessments concern changes of facility-based care assessments. Each model includes care provider random 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 A.8: 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 A.9: Robustness check: including incomplete periods.

	# applications			
	(1)	(2)	(3)	(4)
ex-ante audit regime	−0.682	−0.721	−0.704	−0.813
	(0.429)	(0.449)	(0.450)	(0.502)
conditional audit regime	−0.976**	−0.981**	−1.000**	−0.996**
	(0.413)	(0.431)	(0.433)	(0.479)
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	−0.107	−0.107	−0.114	−0.131
	(0.083)	(0.089)	(0.089)	(0.116)
conditional audit regime	−0.196**	−0.184**	−0.204**	−0.192*
	(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. Provider random effects and time fixed effects are included. Cluster-robust standard errors in parentheses, clustered by care provider. * significant at 10%, ** significant at 5% and *** significant at 1%.

Table A.10: Robustness check: accounting for serial correlation.

	# applications		**approval**	
	level	*normalized*	*application-level*	*aggregated*
	(1)	(2)	(3)	(4)
ex-ante audit regime	−0.656	−0.114	−0.042***	−0.051***
	(0.461)	(0.084)	(0.013)	(0.019)
conditional audit regime	−0.913**	−0.171**	−0.028**	−0.056**
	(0.443)	(0.082)	(0.013)	(0.024)
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 random 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 random 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 A.11: 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^{***}
	(0.017)
ex-ante regime, long-run	-0.034
	(0.016)
ex-ante regime, post-experiment	0.005
	(0.015)
conditional regime, short-run	-0.024
	(0.021)
conditional regime, long-run	-0.013
	(0.018)
conditional regime, post-experiment	0.017
	(0.014)
mean ex-post audit regime (before experiment)	0.827
	(0.014)
p-value, short-run	0.022
p-value, long-run	0.102
p-value, post-experiment	0.489
care provider random 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.