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Physicians' Multitasking and Incentives: Empirical Evidence from a Natural Experiment

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Abstract:
We analyse how physicians respond to contractual changes and incentives within a multitasking environment. In 1999 the Quebec government (Canada) introduced an optimal mixed compensation system, combining a fixed *per diem* with a discounted (relative to the traditional fee-for-service system) fee for services provided. We combine panel survey and administrative data on Quebec physicians to evaluate the impact of this change in incentives on their practice choices. We highlight the differentiated impact of incentives on various dimensions of physician behaviour by considering a wide range of labour supply variables: time spent on seeing patients, time devoted to teaching, administrative tasks or research, as well as the volume of clinical services and average time per clinical service. Our results show that, on average, the reform induced physicians who changed from FFS to MC to reduce their volume of (billable) services by 6.15% and to reduce their hours of work spent on seeing patients by 2.57%. Their average time spent per service increased by 3.58%, suggesting a potential quality-quantity substitution. Also the reform induced these physicians to increase their time spent on teaching and administrative duties (tasks not remunerated under the fee-for-service system) by 7.9%.

Keywords: Physician payment mechanisms, multitasking, mixed-payment systems, incentive contracts, labour supply, self-selection, panel estimation

JEL Classification: I10, J22

1 Introduction

The labour-supply behaviour of physicians is an important determinant of the performance of any health-care system. Physicians make decisions on both the extensive margin, affecting their time spent at work, and the intensive margin, affecting the number of services provided. What is more, physician services are provided within a multitasking environment (Ma 1994; Ma and McGuire 1997) – decisions over the number of patients treated, the time spent seeing patients and the time devoted to teaching and to administrative tasks affect the quantity as well as the quality of health care supplied. Consequently, employers of physician services (in both the private and public sector) must be concerned with the efficient allocation of physician time and effort across different tasks.

One policy instrument which is available to address these issues is compensation design. Economists have written extensively on payment systems and their effects on individual behaviour and performance (*e.g.*, Hart and Holmstrom 1987; Milgrom and Roberts 1992). Within the health-economics literature, many authors have highlighted the different incentives in commonly used payment systems. For example, capitation contracts[1] can reduce the utilization of health services (Pauly, 1990), yet can lead to the under-provision of necessary services (Blomqvist, 1991) and to the cream-skimming of patients (Newhouse, 1996).

Empirical evidence suggests that physicians do respond to incentives. Several studies show that fee-for-service physicians (who receive a fee for each service provided) perform more clinical services than do physicians paid a salary or capitation contract; examples include: Gaynor and Gertler (1995); Hillman et al. (1989); Krasnik et al. (1990).[2] Yet, the interpretation of these results is unclear. While the increase in services can be beneficial to patients waiting for treatment, high-powered incentive contracts may cause physicians to see too many patients and to neglect other tasks such as the quality of care and administrative duties.[3] Theoretical work suggests that low-powered incentive contracts can be optimal when some tasks are unobservable or observable only at very high costs (*e.g.*, Holmstrom and Milgrom 1991; Baker 1992). These contracts are often referred to as "mixed-compensation" contracts in the physician compensation literature (Eggleston 2005). Mixed-compensation contracts may encourage the provision of unobserved aspects of services (such as the quality of care) which are valued by the patient but unobservable to them or to the (public or private) insurer (Ma and McGuire 1997). As shown by these authors, the effect of these contracts depends on whether the quality of care and observable services are complements or substitutes. As long as they are substitutes, increasing the upfront payment for the quality of care (*e.g.*, introducing a *per diem*) and a lower marginal payment for the volume of services, will encourage the physician to provide a better quality of care.

To date, empirical work on multi-tasking issues has been limited by data constraints.[4] Most data sets contain only narrow measures of physician behaviour; *e.g.,* the number of patients seen by a physician

[1]Under capitation contracts the physician receives a fixed payment per patient (per period) and must then pay for any services the patient uses.

[2]See Léger (2007) for a recent review of the empirical evidence on physician compensation systems.

[3]Another concern is induced demand which can be exacerbated under high-powered incentive contracts. See, for example, Evans (1974) and Gruber and Owings (1996).

[4]Paarsch and Shearer (2000) treats multi-tasking issue within a manual-labour context.

(Gaynor and Gertler 1995), the hospitalization rate of patients (Hillman et al. 1989) or the rate of antibiotic prescription (Hutchinson and Foley 1999). Yet, evaluating compensation contracts within a multi-tasking context requires measuring physician behaviour along multiple dimensions. This paper presents a step in that direction. We combine panel, survey and administrative data to analyse the multi-dimensional response of physicians to changes in their contracts (and incentives).

Our data come from the province of Quebec, Canada. They provide detailed information on the labour-supply behaviour of specialist physicians working in the province over a five-year period (1996-2000), including the number of services each physician provided, the weeks and hours each physician worked (disaggregated into several activities), individual earnings, as well as personal characteristics. From these data, it is also possible to construct a proxy for the quality of care provided by physicians: the average time per clinical service.[5] Importantly, our sampling period covers a major policy change with respect to specialist physician pay. Quebec specialists were traditionally paid a fee-for-service (FFS) contract. In 1999, the government introduced a mixed compensation system (MC) as an alternative to the FFS system. Physicians who are paid under MC receive a base wage (or *per diem*), independent of the number of services provided, and a reduced (vis-à-vis the FFS contract) fee-for-service. The discount rate on the fee-for-service averaged 40%,[6] substantially changing physician incentives.

We exploit this contractual change to measure the effect of incentives on various aspects of physician labour supply: services provided, hours devoted to seeing patients, hours devoted to teaching and to administrative tasks and income. It is noteworthy that the MC system is optional and applies only to specialist physicians working in health establishments (mainly hospitals). By 2000, 30.03% of specialists were paid according to the MC system.

Attributing changes in observed physicians' behaviour to changes in the compensation policy requires controlling for a competing hypothesis that can explain the observed changes; namely that different types of physicians are attracted to different compensation policies - a phenomenon known as *adverse selection* in the theoretical literature and *self selection* in the empirical literature (Chiappori and Salanié 2003). Thus, one would expect the physicians who have a low productivity in terms of services provided to be more inclined to choose the MC system, *ceteris paribus*. The longitudinal aspect of our data allows us to observe individual physicians under both the FFS and the MC payment systems. We use this information to isolate changes in behaviour resulting from the change in the compensation system from self-selection. More precisely, we estimate fixed effect regression models which can be regarded as a generalization of a difference-in-differences approach. Panel models with censored data are also used when necessary.

Our results suggest that physicians did react to the change in incentives resulting from the switch to MC, reducing the (billable) volume of services provided by 6.15%. There is also evidence that the change had important effects on other dimensions of physician labour supply and addressed, at least

[5]Ma and McGuire (1997) suggests the use of this variable as a proxy for the intensity or quality of treatment provided by the physician. Of course, the measure is imperfect: it does not distinguish between time spent with patients and time spent between patients, nor does it provide any information on its actual effect on the health of patients.

[6]The discount rate is the weighted average fraction of FFS prices paid for clinical services when performed under MC (the weights being the relative importance of each service in 1998).

partially, multi-tasking problems associated with FFS: the time spent per service increased by 3.58% under the MC system and the hours devoted to activities not remunerated under FFS (administrative and teaching) increased by 7.9%. These results are qualitatively robust to various specifications of our empirical model and sample composition, although there is evidence that the reaction to the reform varied across gender and specialty.

The rest of the paper is organized as follows. The next section describes the institutional rules governing physician compensation in Quebec. Section 3 describes the data, the variables, the treatment and control groups used in our empirical analysis. It also presents descriptive statistics. Section 4 explains our empirical strategy, while Section 5 discusses the results. Section 6 provides a series of robustness checks to our empirical specification and the last Section concludes.

2 Institutional Background

Health care falls under the jurisdiction of provincial governments under the Canadian constitution. While the federal government does exert some influence on conformity to national standards through the Canadian Health Act, policies remain largely a provincial responsibility. Within the province of Quebec, the vast majority of specialist physicians have been paid under a FFS compensation system until 1999.[7] The fees paid are service specific, accounting for the difficulty and time intensiveness of the service.[8] They are periodically negotiated between the Quebec Government and the *Fédération des médecins spécialistes du Québec* (FMSQ).

In September 1999, the Quebec Government introduced the MC system for 19 out of 31 specialties. The scheme became accessible to other specialties later in 2000. Adoption of the MC system was optional. In hospitals, specialists work within departments, made up of physicians performing similar tasks. Upon the introduction of the MC system, each department voted on its adoption, switching to the MC system only if the vote passed unanimously. In 2000, about 31,03% of the specialists were paid, at least in part, by the new system.

The MC scheme combines a *per diem*, paid independently of the number of services provided,[9] with a discounted FFS payment. As mentioned above, the discount rate on the fee-for-service averaged 40%, substantially altering the monetary incentives to supply services. Note, however, that the discount varied across services and specialties. At the extreme, some services were deemed *non-billable* under the MC system, receiving no fee-for-service.

A number of additional conditions bear mention. First, the *per diem* only applies to certain activities, principally time spent on administration, teaching and seeing patients, the most notable exclusion being time spent on research. Research activities are typically paid by the hospital where they take place, or through government grants. Second, the MC system is available only for activities completed in health establishments (mainly hospitals). Hours of work and services provided within private clinics continues

[7] FFS compensation represented about 80% of specialists' earnings in 1999.

[8] See Fortin et al. (2006) for a more detailed and formal presentation of specialists' compensation in Quebec.

[9] During the period under study, the half *per diem* was set equal to 300 CAN$ for 3.5 hours of work.

to be paid under the FFS system.[10] Third, services provided within hospital emergency rooms by non emergency-room physicians are excluded from the MC system. Finally, there are restrictions on the number of *per diems* a physician can claim and the time-period during which he/she can claim them. *Per diems* are claimed on a 3.5-hour basis (these are denoted half *per diems*). The maximum number of half *per diems* that a physician can claim during a two-week period is 28 and these can only be claimed Monday to Friday between 7AM and 5PM. Once the maximum number of *per diems* is reached, or when a physician works outside the *per-diem* claimable hours, he/she is paid on the FFS basis.

The actual annual income earned by a physician can differ from the gain he derives from practice due to two further institutional factors: income ceilings and regional-practice compensating differentials. Two types of income ceilings were active during the sampling period. The first is a ceiling on the net income a specialist could earn in a private clinic during a given semester. Net income is defined as 65% of income earned by the physician in private clinics and allows the physician to net out expenses. Second, up until 1999, the government also imposed a ceiling on gross income (independent of where it was earned). In both cases, once the ceiling was attained, payments to physicians were reduced by 75% from their regular levels.[11]

Regionally differentiated compensation rates are designed to induce physicians to practise in remote areas of the province. Physicians who agree to practise in these regions receive extra income. These incentives are strongest for young physicians (those with less than three years of experience) who are also penalized for practising in designated university regions (Montreal, Quebec City and Sherbrooke).[12]

3 Data and Variables

3.1 Data

Our data contains information on the practice patterns and individual characteristics of physicians practising in Quebec between 1996 and 2000.[13] These data come from two sources. First, each year, the *Collège des médecins du Québec* (CMQ) conducts a survey of its members. This survey contains information on individual characteristics such as specialty, age and gender, as well as establishment and geographic characteristics. The survey also contains time-use information, namely, time spent at work, time devoted to seeing patients in establishments or private clinics,[14] time devoted to teaching, administrative duties and research activities (all measured as average hours per week).

[10] In fact, physicians who demonstrate that there is a strong complementarity between private clinic and hospital activities can choose to receive a *per diem* when working in a private clinic. However, in this case, they would not receive any FFS payment for services performed in the clinic. Therefore most physicians choose to be paid on the FFS basis when working in a private clinic.

[11] Investigations with our data showed that the ceiling on gross income imposed real constraints to only a small fraction of physicians and its removal had a negligible effect on physician behaviour. See Section 6 below.

[12] The government altered this system in December 2003, abolishing the penalties to young physicians for practising in university regions as well as changing the bonuses paid for work in remote regions and the division of the regions.

[13] We ignore data from 2001 and 2002 since there was an increase in fees paid to physicians (in the order of 11%, on average) at the beginning of 2001 and we did not want to confound the impact of this increase with that of the reform.

[14] Private clinics are administered in the private sector, yet are subject to the public sector fee schedules.

The second source of data is the *Régie de l'assurance-maladie du Québec* (RAMQ), a public administrative organization which collects information on the income and billing practices of physicians. Income and productivity (the number of services provided) data are available on a quarterly basis for each physician.

The data from the annual surveys and from the RAMQ were matched on the basis of an anonymous billing number attributed to each physician. This also allowed us to keep track of each physician in our sample across time periods.

3.2 Variables

Typically, physicians provide a variety of services, each remunerated at (possibly) different rates. To construct a measure of the volume of services supplied, we aggregated services together to generate an annual quantity index of services (weighted by the basic fees in 2000).[15] Recall that the government designated some services as non-billable under the MC system. Since our data come from the billing records of physicians, these services are unobservable when they are performed under MC. We have therefore decided to exclude them from our measure of clinical services for all physicians over the sampling period. Including these services would have created an upward bias in the impact of the reform on the volume of clinical services. As long as the reform does not induce physicians under MC to substitute across various clinical services, our measure of the effect (in percentage) of the reform on the volume of clinical services will not be affected by their exclusion.

3.3 Definition of Sample, Treatment, and Control Groups

Since we focus on a comparison between the FFS and MC schemes, we dropped from our sample all physicians who received less than 100% of their earned income under these schemes over the sample period. Physicians in specialties, such as geriatrics, psychiatry and public health, who receive an important part of their income from salary were therefore excluded. We also excluded physicians who are never observed under the FFS system. This excludes some young physicians who begin their career during the sample period and are only observed under the MC system.

In order to analyse the effect of the reform, we divided our sample into treatment and control groups. Our definition of these groups centered around the physician's participation in the MC system once it became available in the year 2000. A natural criterion would have been to condition treatment on being remunerated under MC. However, due to the particular setting of the reform (see Section 2), changing from FFS to MC does not exclude practising under FFS (*e.g.*, during particular hours of the day, in emergency rooms or in private clinics). We thus defined the treatment group as those physicians who were compensated *in part* under MC in every quarter of 2000. Similarly, the control group is defined as those physicians being remunerated exclusively by FFS in every quarter of 2000. Physicians

[15] The formula for calculating the volume of services for a physician i at year t is given by $\Sigma_{j=1}^{m} p_j^{2000} S_{ij}^t$, where m is the number of different services, p_j^{2000} is the basic fee for service j in 2000, and S_{ij}^t is the number of services j performed by physician i at time t.

who did not match these criteria were excluded from the sample.[16] Such a restrictive definition of the treatment and control groups is aimed at building as homogeneous groups as possible.[17] This same objective also lead us to exclude some observations from the study. First, the following laboratory specialties were excluded: anatomopathology, medical biochemistry, medical microbiology and nuclear medicine. Moreover, the MC scheme was introduced later in 2000 for cardiac and vascular surgery, internal medicine, otorhinolaryngology, physiatry, radiology and urology.[18] Therefore these specialties were also dropped from our analysis. Finally, ophthalmology was removed from our sample since very few physicians of this specialty chose MC. Their participation rate to MC was only 2% in 2000. We are, therefore, left with the following specialties in our sample : anesthesiology, cardiology, dermatology, endocrinology, gastroenterology, general surgery, gynecology and obstetrics, nephrology, neurosurgery, neurology, orthopaedics, pediatrics, pneumology, radio-oncology, and rheumatology. The final sample contains 2 120 physicians, down from 8 549 specialist physicians in the initial database.

3.4 Descriptive Statistics

Table 1 provides information concerning the number of physicians in each specialty in our sample as well as the participation rates in the MC scheme (the proportion of physicians who switched to MC) in 1999 and 2000. Of the 2 120 physicians in our sample, 423 are in the treatment group (they switched to MC) and 1 697 are in the control group (they remained in FFS). There are 1 655 males and 464 females. Cardiology (260 physicians), General surgery (259 physicians) and Pediatrics (253 physicians) are the largest specialties, while Radiation Oncology (30 physicians), Neurosurgery (31 physicians) and Rheumatology (44 physicians) are the smallest specialties.

The participation rate to MC strongly varies across specialties (from 2.59% for gastroenterology to 83.87% for neurosurgery in 2000). Two reasons may explain this result. First, the average discount rates vary from one specialty to another. For example, in neurosurgery, services are compensated at a relatively high discount rate (including non-billable services) of 53%. Similarly, in radiation oncology where the MC participation rate are 70%, the average discount rate is 54%. Thus, there is a positive correlation between the level of MC participation rate and the MC discount rate. Second, some specialties are known to have a relatively low productivity in terms of volume of services. This is the case for pediatrics since children need special care. The *per diem* may induce them to choose the MC system even though their discount rate is quite small (14.27%). This may partly explain their relatively high MC participation rate of 43.5%.

Table 2 shows summary statistics on practice variables for the treatment and control groups, before and after the reform. We note that before the reform, the control group provided more services on average

[16] To account for changes in practice behaviour in 1999 for the physicians who changed from FFS to MC in the last quarter of 1999, we introduced a dummy variable identifying these treated individuals in the regressions for clinical services and income. Time-use variables do not overlap with the reform in 1999 since the CMQ surveys are conducted in July while the reform was introduced in September.

[17] In Section Section 3.3, we check the robustness of our results with respect to other definitions. We obtain very similar estimates of the impact of the reform.

[18] These specialties' associations reached an agreement with the Quebec Health and Social Services Department only after some delay.

(132 430$ *vs* 112 540$), had a larger income (212,740 *vs* $178,840$) and worked slightly fewer hours per week (54 *vs* 55.28) than the treatment group. These statistics indicate that physicians who chose to remain under FFS were, on average, higher productivity physicians, at least in terms of the volume of services. Consequently, the control-group physicians would have suffered more from the reduction of fees under the MC scheme than the treatment group. This is consistent with the presence of a self-selection problem. Also, before the reform, physicians in the treatment group worked less in private clinics (generally not subject to mixed compensation) and they performed more non-clinical activities such as administrative and teaching tasks (compensated under the MC *per diem*).

Table 3 shows the makeup of the treatment and control groups in terms of the personal characteristics of the physicians in our sample. The Male physicians represent 81% of the control group (before the reform) while representing 72% of the treatment group, indicating that the MC participation rate is smaller for males than for females. Similarly francophone physicians represent 79% of the control group and 86% of the treatment group, which indicates that the MC participation rate is higher for francophone than for non-francophone physicians. Also, the average age of physicians in the treatment group is smaller than that of the control group. This indicates that the MC participation rate decreases with age. Moreover, the MC participation rate is more important in Metropolitan regions (CMA with or without teaching university) than in smaller areas.

The statistics on volume of service, income and time-use variables can be directly used to compute simple difference-in-difference (DD) estimates of the impact of the reform. However, since no control variables are taken into account in such estimates and given the presence of a potential self-selection bias, we will focus on econometric estimates.

4 Estimation Strategy

Our empirical strategy relies on a panel data model approach that can be seen as a generalization of the DD approach. The panel dimension of our data is used to control for sample self-selection.[19] We also account for censoring, when required, by extending this framework to a pooled tobit model.

4.1 A Panel Data Model with Sample Selection

We seek to measure the effect of the introduction of the mixed compensation system on physician behaviour. Let y_{it0} denote the supply of services associated with physician i's practice at time t under the FFS scheme. Similarly, y_{it1} denotes the supply of services by physician i at time t under the MC scheme. We denote, for the present, y as a generic measure of supply. In our empirical work we will consider specifications with the volume of clinical services, hours of work (disaggregated in various dimensions) and earnings as our dependent variable. We specify outcomes as a set of linear regression

[19]While the decision to adopt MR was not individual based, ethical concerns prohibited us from accessing information on the department in which an individual physician was working which would have allowed modeling the voting decision to adopt RM. Instead, our individual-based decision rule should be interpreted as a long-run equilibrium under which individuals choose their department based on similar preferences for RM.

equations:

$$y_{it0} = \theta_t + \mathbf{x}_{it}\beta + c_i + u_{it0} \tag{1}$$

$$y_{it1} = \theta_t + \mathbf{x}_{it}\beta + \alpha + c_i + u_{it1}, \tag{2}$$

where the parameter θ_t denotes a time-varying intercept and $\mathbf{x}_{it}$ denotes a set of observable characteristics (such as age, specialty, gender, region, type of establishment and language). The disturbances are broken down into two components: c_i denotes an individual unobserved effect that does not vary over time while u_{it0} and u_{it1} represent temporary unobservable variables affecting the outcomes under the respective compensation systems. It is assumed that $E[u_{it0}|\theta, \mathbf{x}_i, c_i] = E[u_{it1}|\theta, \mathbf{x}_i, c_i] = 0$. Note that equations (1) and (2) represent the distribution of outcomes in the population. As such, (2) represents the (hypothetically) observed supply if all specialist physicians were to be paid under the MC scheme at $t > k$, where k is the year of introduction of MC. The effect of MC on y is captured by α and is called the average treatment effect (ATE) in the literature (*e.g.*, Vanness and Mullahy 2006). ATE estimates the average variation in the supply of services if all physicians change from FFS to MC as compared with no change at all. In this model, the assumption of a common time-varying intercept under each scheme (*i.e.,* same θ_t) is crucial to identifying the effect of MC on y. This *parallel trend* assumption will be tested for various supply of services equations in the econometric section.

Now let y_{it} denote the supply of services by physician i at time t under the *observed* payment scheme, *i.e.*, the scheme chosen by physician i at time t. Then, following Heckman et al. (1999), the observed outcomes are linked to the above equations through:

$$y_{it} = D_{it}y_{it1} + (1 - D_{it})y_{it0} \tag{3}$$

where D_{it} is the indicator variable of the specialist i's decision to select MC at time t. Thus in this model, participation in the treatment group ($D_{it} = 1$) or to the control group ($D_{it} = 0$) is endogenous. Substituting (1) and (2) into (3) gives:

$$y_{it} = \theta_t + \mathbf{x}_{it}\beta + D_{it}\alpha + \epsilon_{it}, \tag{4}$$

where

$$\epsilon_{it} = c_i + u_{it0} + D_{it}(u_{it1} - u_{it0}), \tag{5}$$

which comprises the unobserved productivity effects under fee-for-service as well as the unobserved gain to individual i of selecting the MC system.[20] As is well known (Heckman et al. 1997; Angrist and Imbens 1999) standard regression methods applied to (4) generally fail to provide a consistent estimate of α due to a non-zero covariance between D_{it} and ϵ_{it}. The three components of the error term ϵ_{it} in (5) allow identification of the reasons for this difficulty. First, if physicians differ with respect to unobservable

[20] In practice some physicians may work under both systems at the same time, complicating the selection model. For simplicity, we present the binary choice model, in which physicians are assumed to be observed in one or the other regime.

permanent elements (as represented by c_i) that affect their productivity, and if, for instance, low productivity physicians prefer the MC system, then self-selection leaves high productivity physicians under FFS and low productivity physicians under the MC system. A comparison of productivity levels will therefore confound the effects of the compensation system with the differences in unobserved productivity. Consequently, individuals may be more suited to one regime or the other – self-selection thus implies that $E[c_i|\theta, \mathbf{x}_i, D_{it} = 0] \neq 0$ and $E[c_i|\theta, \mathbf{x}_i, D_{it} = 1] \neq 0$.

A second problem occurs when physicians who decide to switch to MC experience a temporary change in the supply of services just prior to the change in the compensation system, something often referred to as *Ashenfelter's Dip*. In this case, $E[u_{it0} \mid \theta, \mathbf{x}_i, c_i, D_{it} = 1] \neq 0$. Given our panel is over five years however, we should be able to control for any temporary changes in supply. Therefore, we will assume there is no *Ashenfelter's Dip* in the sequel.

A third problem occurs when the effects of the reform are not homogeneous, that is, when $E[u_{it1} - u_{it0}|\theta, \mathbf{x}_i, c_i, D_{it} = 1] \neq 0$. This expression represents the average unobservable change in the supply of services for the doctors who change from FFS to MC over the average treatment effect. Conditional on c_i and the explanatory variables, the OLS coefficient associated with D_{it} will therefore estimate $\tilde{\alpha} = \alpha + E[u_{it1} - u_{it0}|\theta, \mathbf{x}_i, c_i, D_{it} = 1]$. The parameter $\tilde{\alpha}$ is called the treatment effect on the treated (TT) in the literature (*e.g.*, Heckman and Vytlacil, 2001). TT is generally different from ATE (given here by α), the common impact of the reform, since it estimates the average impact of the reform only for those who actually choose to change to MC. Only when the population of physicians is homogeneous, TT will be equal to ATE. In this paper, we focus on the TT's since the ATE's are not identified without imposing additional structure to the model.

We are thus left with a self-selection problem.[21]A number of approaches have been developed for controlling for this endogeneity, each of which requires different assumptions in order to be valid. In the health literature, the approach which has been most widely used is the instrumental variable method (*e.g.*,Earle et al. 2001; Hadley et al. 2003; Basu et al. 2007). However, given the longitudinal nature of our data, it is natural to use an alternative approach based on panel data regressions. Thus, we observe each physician in the selected sample before and after the introduction of the MC system. We can therefore exploit information on the supply of services prior to the introduction of the MC system to eliminate the individual effect c_i which is the source of the self-selection problem. More precisely, using (4) and (5) and the definition of $\tilde{\alpha}$, the model to be estimated becomes:

$$y_{it} = \theta_t + \mathbf{x}_{it}\beta + D_{it}\tilde{\alpha} + c_i + \eta_{it}, \tag{6}$$

where $\eta_{it} = u_{it0} + D_{it}(u_{it1} - u_{it0}) - D_{it}E[u_{it1} - u_{it0}|\theta, \mathbf{x}_i, c_i, D_{it} = 1]$, with $E(\eta_{it}|\theta, \mathbf{x}_i, c_i, D_{it}) = 0$. Our model assumes strict exogeneity of $\{\theta_t, \mathbf{x}_{it}, D_{it}\}$ for all t, conditional on the individual effect. Note that the model *does not* impose the absence of correlation between the individual effect and the explanatory variables. Eq. (6) will be estimated using a fixed effects (or within) transformation to eliminate the

[21]Our model ignores the possibility of self-selection of patients (not physicians) into different provider types. In that case, we would expect the case-mix of physicians to change with the reform and our estimates could be biased. However, this problem is not likely to be important since the information concerning whether a physician is FFS or MC is confidential and cannot be released to the public by the RAMQ.

individual-specific effects. Also, the absence of self-selection will be tested using a Hausman test comparing the coefficients of the fixed effects (FE) model with those of the corresponding random effects model. The latter model is more restrictive since it *does* imposes zero correlation between the observed explanatory variables and the individual effect.

When applying the FE model, the presence of heteroskedasticity of the error terms and serial correlation in η_{it} may give an improper covariance matrix estimator.[22] We provide robust standard error estimates that are valid in the presence of arbitrary heteroskedasticity and serial correlation in η_{it} when the length of the panel (here, equal to 5) is small (see Arellano 1987).

4.2 Extension to Censored Data

Some of the practice variables of interest in our study are left-censored in the sample (this is the case, *e.g.*, for teaching hours in 46 % of the sample and for research hours in 69 % of the sample). For those variables, the FE effects model estimated by OLS will be inconsistent. Moreover, a standard FE tobit will also be inconsistent due to the so-called *incidental parameters problem*; *i.e.*, for a fixed length of the panel, the number of parameters to be estimated increases with the number of observations.

To accommodate this, we estimate the model using a *pooled tobit*. Also, we add dummy variables to account for some observable time-invariant individual variables (*e.g.*,Treated=1 for physicians who chose MC, sex, language, specialty (14 dummies)). Note that, being time-invariant, these variables are not identified under the FE model. This yields a new vector of explanatory variables, $\tilde{x}_{it}$. It is assumed that conditional on $\tilde{x}_{it}$, the error term of the corresponding latent model is distributed $N(0, \sigma^2)$. A useful feature of the pooled tobit is that the estimated parameters will be consistent and asymptotically normal even if the error terms are arbitrarily serially correlated. We provide estimates of the standard errors of coefficients robust to serial correlation of unrestricted form.[23] The impact of the reform is measured by the average over the doctors who changed from FFS to MC of the estimate of $E(y_{it}|\theta, \tilde{\mathbf{x}}, D_{it} = 1) - E(y_{it}|\theta, \tilde{\mathbf{x}}, D_{it} = 0)$ at $t = 2000$ (period post-reform).

5 Results

Results of fixed effects OLS and pooled tobit are presented in Tables 4 to 11. Table 4 provides results for all specialties in the sample, with the control group given by specialists who remained in the FFS scheme in 2000. For all models considered, tests of parallel trend do not reject the null hypothesis that, prior to the reform, time effects (θ) were the same in both control and treatment groups. This test is crucial for the identification of the impacts of the reform. Moreover, the Hausman test rejects the random effects model for all OLS models.

[22] See Bertrand et al. (2004) for a discussion of the serial correlation problem when using a DD approach.

[23] An alternative to the pooled tobit is the random effects tobit. However, this model imposes strong restrictions on the variance matrix. Since this model imposes that the error terms are equicorrelated, the variance matrix will have common diagonal entries and common off-diagonal entries. In any case, econometric investigations showed that estimated coefficients from these two models do not differ very much.

On average physicians who changed to MC reduced their volume of billable clinical services by 8 300CAN$,[24] which represents a decrease of 6.15%. This result confirms studies such as Hillman et al. (1989), Krasnik et al. (1990), Hutchinson and Foley (1999), Gruber and Owings (1996), and Barro and Beaulieu (2003), that show that financial incentives have an impact on the number of services provided by physicians. However, note that we might underestimate the impact (in percentage) of the MC system since it is possible that MC physicians substitute billable services for non-billable (thus less paid) services. The decrease in the volume of services is accompanied by a reduction of 1.14 in weekly clinical hours.[25] The reduction in the number of clinical hours is mainly due to the reduction of 0.81 in hours worked per week in private clinics.[26] This is the case since the reform had no significant impact on clinical time spent in hospitals.

There is also evidence that the reform addressed some of the multi-tasking problems associated with the FFS contract. First, note that the time spent per billable service increased by 3.58%.[27] This result is therefore consistent with MC physicians substituting quality for quantity in their supplies of clinical services.[28] Second, switching to the MC system caused physicians to spend more time (0.53 more hours per week) on non-clinical activities; *i.e.,* administrative tasks and teaching. While these activities are important in insuring the quality of health care (both in the present and future), they are not remunerated under the FFS system and are likely to be neglected. The measured increase, in the order of 7.92 %, suggests that the reform had positive effects in this direction as well. Notice, however, that we are less successful in decomposing the total increase in non-clinical activities into constituent parts; the effect on administrative activities is not significantly different from zero.

We also note that the reform was not cheap – according to our results, physicians who chose the MC system benefited from an increase in annual income of 17 290 CAN$; *i.e.,* an increase of 8.05%. What is more, the impact of the reform caused a reduction of 1.09 total hours of work per week performed by MC physicians. As such, the reduction in the fees and the introduction of the *per diem* induced MC physicians to consume more leisure. Other studies (*e.g.*, Croxson et al. 2001), have also shown that changes in the payments received by doctors affect their number of working hours. Finally, we note that time spent on research activities decreased by 14.7%.

6 Robustness Checks

To validate our results, we performed a number of robustness tests.

[24]All results in dollars are in constant (2000) CAN$.

[25]The results for weeks of work are not reported because they are insignificant in all specifications.

[26]The estimated coefficients on hospital hours and hours worked in private clinics do not add up to the coefficient on total clinical hours because the latter was estimated by OLS and the former were estimated by pooled tobits.

[27]This is due to the fact that the reduction (in %) in volume of billable services is larger than the reduction (in %) in clinical hours worked.

[28]Some caution should be exercised in interpreting this result since we are not able to differentiate between time actually spent with patients and time spent between patients. We return to this point in our conclusion.

6.1 Placebo Reform

First, we analyze a placebo reform, suppressing data from 1999 and 2000 and using 1998 as the post-reform period. The results are presented in Table 5. The coefficients of the variable (= 1 if treated in 1998) in the table should not be significant since no true reform has been introduced during this year. If they are significant, then it raises the possibility that the results of Table 4 are also significant by mere chance. Results from the table indicate that no coefficients are significant, which is quite encouraging for the validity of our estimates of the impacts of the reform.

6.2 Heterogeneous Response

The next results in Tables 6 and 7 provide an analysis of the reform by gender. They show that females benefit more from MC than males. First, females who changed from FFS to MC increased their income by 20 290 CAN$ in comparison with 15 480 CAN$ for males. Second, the reduction in billable clinical services is more important for females than for males. MC females reduce their volume of services by 8.22% as compared with 5.54% in the case of men (10 080 CAN$ *vs* 7 620 CAN$). Women seem to be financially penalized by the FFS system. They bill fewer services than men not only because they work fewer hours than men,[29] but also because, on average, they spend more time with their patients.[30] The higher increase in income for females thus partly reveals that the MC system has a stronger effect on their income because their "productivity", as measured by their number of services per clinical hour, is lower than that of males. The *per diem* benefits more to physicians with lower productivity and therefore induces a larger increase in females income.

Part of the results for women can also be explained by the behaviour of MC pediatricians since approximately one-half of the physicians in the female treatment group are pediatricians. The results for this specialty are presented in Table 8. This decrease of 12 990 CAN$ (= 12.8%) in their volume of services is larger than that for all specialties (= 8 300 CAN$). This decrease is mostly explained by the reduction in time spent in private clinics (2.92 hours per week), which is almost equal to the reduction of 2.88 in total clinical hours worked. Prior to the reform, 53% of the volume of billable pediatric services were performed in private clinics. By reducing time spent in these clinics, the reform has also reduced the number of services provided in them. We also observe that the MC pediatricians did not increase the number of services provided in hospitals. Therefore, the fall in volume may only represent a reduction in the number of services billed in private clinics. The MC pediatricians also increased by 1.56 hours per week their time spent on non-clinical activities (administrative tasks and teaching) and reduced by 2.01 hours a week their time spent on research. All in all, they reduced their total hours of work by 1.87 hours per week.

Table 9 shows results for general surgery, another specialty which reacted strongly to the reform. MC general surgeons decreased their volume of services by 15 320$ (= 10.7%) while increasing their level of income by 18 370 $ (= 8.14%). However, we cannot say much more about the effect of the reform on the use of their time since none of the coefficients measuring the impact of the reform is significant at

[29] In our sample, the average hours of work per week was 51.03 for females and 52.6 for males in 2000.

[30] In our sample, the average volume of services per clinical hour was 2.76 for women and 3.14 for men in 2000.

the 5% level.

Table 10 shows the results for all specialties in the sample, excluding pediatrics and general surgery. After removing these two specialties, the reduction in the volume of services performed by MC specialists is less important, but still evident. It is 4 610 CAN$ instead of 8 300 CAN$ (or 3.32% as compared with 6.15%). On the other hand, its impact on the income of MC specialists remains very close to the previous estimates, corresponding to an increase of 17 950 CAN$ as compared with 17 290$ for all specialties. Moreover, it seems that the reduction in total clinical hours and in private clinic hours is mainly due to the behaviour of MC pediatricians since none of the coefficients associated with these two variables are significant at the 5% level for the other specialties. A similar result is obtained for research hours. In short, the reform had a more important impact on the allocation of time for pediatrics and general surgery than for other specialties. This result could partly be explained by the specific characteristics of these specialties.

6.3 Changing the Control Group

Following Song and Manchester (2007), we consider how robust our results are to using a different control group: general practitioners who were paid exclusively on FFS contracts. As long as this control group is affected by the same economic shocks through time as are specialists who chose to be paid on a FFS basis (the basic control group), one should expect the estimated effect of the reform to be similar to the original one. Alternatively, the general-practitioner control group may be less susceptible to confounding forces such as general-equilibrium effects or the removal of income ceilings (see below). Table 11 presents the results. Generalists do not perform the same types of services as MC specialists. Therefore, for this control group, instead of using billable services on MC to construct a measure of volume of services, we used all services billed by generalists.

Our tests of parallel trend are rejected in this case for most time-use variables (except time spent on private clinic and on administrative duties). Thus, strictly speaking, our identification criterion fails for this sample. This may reflect the fact that general practitioners do not represent a natural control group for MC specialists. In spite of this, it is reassuring to see that the results obtained when using generalists as the control group are *qualitatively* similar to those obtained when using FFS specialists. Quantitatively there are some important differences though. Thus, using the alternative control group, our results show that the reform reduced the volume of clinical services performed by MC specialists by 5 700 CAN$ (or 3.9%) as compared with 8 300 CAN$ (or 6.15%) when using FSS specialists as the control group. Also, the impact of the reform on total clinical hours per week of MC specialists is now 1.64, which is larger than the estimate obtained when FSS specialists are used as the control group(= 1.14). However, contrary to our results with FSS specialists as the control group, MC physicians do not seem to spend more time per service. Indeed, in percentage, the reductions in the volume (3.90%) and in clinical hours (3.88%) are quite the same. Again, the reduction in clinical hours is mainly due to the decrease of 1.47 hour per week in private clinics. However, contrary to the previous estimate, the number of hours in hospitals increases now by 1.18 hour per week, indicating the possibility of a substitution between private clinic and hospital hours. Note however that the statistic on the test of parallel trend is very large for hospital hours which might indicate a serious problem when computing the marginal effects of the reform.

Non-clinical hours per week increases by 1.12 hours which is far more important than the growth in hours spent teaching (0.50 hours a week). Again, the large value of the parallel trend test statistic suggests a problem with these results. Note however that the latter estimate is quite similar to the one obtained when using MC specialists as the control group (= 0.47).

Finally, there is a difference of 6 090 CAN$ in the two estimates of the impact of the reform on the income of MC specialists. This gap might be attributable to an increase in the income of specialists in 2000 which the general practitioners did not benefit.

In summary, tests of parallel trends suggest that FFS specialists are a better control group for MC specialists than are general practitioners. However, results using general practitioners as a control group qualitatively validate our previous results in spite of some quantitative differences: estimates of the effect of the reform increase for all variables except the volume of services. Moreover, the increase of hospital hours is now significant.

6.4 General-Equilibrium Effects

As an additional check, we considered the possible presence of general-equilibrium effects (*e.g.*, Heckman et al. 1998). These would occur if, for example, the reduction in the volume of services on the part of the treatment group led to an offsetting increase in the volume of services among control-group physicians, perhaps in reaction to increased waiting periods on the part of patients. The fact that the average volume of services is higher among FFS physicians after the introduction of the reform is consistent with this explanation which would place doubts on the validity of our control group and our measures of the effect of the reform.

Following Lewis (1963), we exploit differences in the participation rate across specialties to investigate general-equilibrium effects. If these effects are important then specialties in which the participation rate is highest should exhibit the largest reaction among FFS physicians (the control group). What is more, the effect of the participation rate should be of opposite sign to the measured effect of the reform. We therefore regressed the different practice variables on the participation rate, restricting the sample to FFS physicians. In general, there is little evidence to support the presence of general-equilibrium effects.[31] The participation rate has no significant effect on the volume of services provided among FFS physicians. While it has a significant effect on hours spent seeing patients and on hours spent on administrative duties, in both cases the sign is inconsistent with general-equilibrium effects. In both cases the coefficient on participation rate has the same sign as the estimated effect of the reform. Given these results we conclude that general-equilibrium effects are not a major confounding element in our data.

[31] The full results are available on request.

6.5 Income Ceilings

Finally, we considered the effect of the removal of the gross income ceiling on physician behaviour. Recall, the government removed this income ceiling in January 1999. If this affected the treatment and control groups differently, then it would be a possible confounding effect in our estimates of the compensation system reform. However, there is little evidence to suggest that these ceilings (or their removal) had an important effect on observed physician behaviour. First, only a small number of specialists were actually constrained by the ceiling prior to its removal. In the last semester of 1998 only 5.5% of specialists had an income greater than or equal to the income ceiling. Second, there was no substantive change in our results when we eliminated physicians whose income were close to the ceiling (defined as having an income greater than or equal to the ceiling minus $10,000) from our sample.[32] Given this, we are confident that any effect of the removal of the income ceilings on observed behaviour was minimal.

7 Conclusion

In this paper, we have investigated the labour-supply response of physicians to changes in their contracts. We have combined time-use surveys with administrative payroll information to construct a panel of labour-supply data on Quebec physicians. The comprehensive nature of these data allow us to analyse labour-supply response along many dimensions and address multi-tasking issues in response to a particular reform: the introduction of the mixed compensation system. Our results show that, on average, the reform induced physicians who changed from FFS to MC to reduce their volume of (billable) services by 6.15%. However, they increased their time spent per service by 3.58% and their time spent on administrative and teaching tasks (activities not remunerated under FFS) by 7.92%.

These results underline the importance of accounting for multitasking when evaluating compensation systems; they also point to directions for future research. The increase in time spent per service is suggestive of a quality-quantity substitution in the treatment of patients. This, coupled with the increase in time spent on teaching and administrative duties suggests there were positive effects of the reform, offsetting the reduction in services provided.[33] Of course, time spent per service is an imperfect measure of quality – physicians may simply be taking longer breaks between services, or spending more time with patients without affecting health outcomes. Further study and more extensive data will be needed to fully settle these issues. Comparative information on the health of patients treated by MC and FFS physicians would be useful in this regard. What is more, a complete welfare analysis would weigh the increased time spent per patient (or improvement in health status of the patient) against extra time spent waiting to see a physician. Matching data on physician services to waiting times would be an important step in this direction.

Finally we note that our analysis is limited to reporting the effects of a particular reform – the introduction of the MC system. Yet, policy makers may want to be able to predict the effects of

[32] The results are available from the authors on request.

[33] Another possible benefit of the reform is the reduction in induced demand. Yet, waiting lists to see specialists are very long in Quebec. Under these circumstances, it is difficult to believe that physicians feel it necessary to induce demand to meet income targets.

other reforms, not observed within the current data. For example, the current reform was voluntary, allowing us to identify the effect of treatment on the treated (those that chose the MC system). Yet, the government may want to predict the effect making MC mandatory for all physicians, that is, the average treatment effect. Alternatively, the government may wish to predict the effect of alternative contracts. The fact that the income of physicians who switched to MC increased by 8% suggests that the reform might have been accomplished at a lower *per diem*. Yet, changes in the contract would, undoubtedly, affect participation, services provided and the effect of the reform. One approach to answering these questions would use our data to estimate structural parameters governing labour-supply decisions. These parameters could then be used to solve for the optimal physician behaviour under alternative policies. We leave this task for future research.

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Table 1: Mixed Compensation System characteristics in 1999 and 2000

	MC participation rate		Number of	MC Discount rate	
	1999	2000	physicians	Excluding NBS	Including NBS
Anesthesiology	23.12	32.75	229	31.01	30.90
Cardiology	2.20	6.92	260	44.32	34.45
General Surgery	14.29	18.15	259	48.10	35.33
Orthopedic Surgery	6.96	9.24	184	45.87	38.84
Dermatology	16.39	17.99	139	53.53	32.98
Gastroenterology	1.82	2.59	116	75.00	31.24
Obstetrics and Gynecology	4.08	6.39	219	40.04	37.14
Pulmonology	5.19	5.95	84	79.00	18.60
Neurosurgery	75.86	83.87	31	70.66	52.99
Neurology	9.09	10.83	120	28.60	24.01
Pediatrics	27.88	43.48	253	30.00	14.27
Radiation Oncology	47.62	70.00	30	79.00	54.00
Nephrology	4.41	5.19	77	34.00	21.43
Endocrinology	22.73	24.00	75	28.00	20.30
Rheumatology	41.46	61.36	44	45.23	36.12
Total	14.04	19.95	2120	41.19	30.80

Note: For each specialty in our sample, the first two columns report the proportion (in %) of physicians who chose MC in 1999 and 2000. The number of physicians for each specialty is reported in the third column. The last two columns provide the average discount rate (in %) associated with MC – namely the weighted average fraction of FFS prices paid for clinical services when performed under MC (the weights being the relative importance of each service in 1998)– calculated either over only those services for which the discount is positive, hence excluding non-billable Services (*fourth column*) over all services (*last column*), whether they are billable or not.

Table 2: Descriptive statistics : Practice variables by group[a]

	Control group				Treatment group			
	Before		After		Before		After	
	Mean	St. Error	Mean	St. Error	Mean	St. Error	Mean	St. Error
Volume[b]	132.43	69.08	136.27	74.82	112.54	52.50	109.86	54.49
Income[b]	212.74	78.83	214.50	84.93	178.84	68.12	200.76	63.64
Hours at work[c]	54.00	14.04	52.29	14.04	55.28	12.65	52.38	12.82
Clinical hours[c]	44.70	15.41	44.43	15.44	42.23	14.54	41.45	13.85
______ Hospital[c]	31.02	18.79	30.59	19.48	32.53	17.87	32.83	17.37
______ Private clinic[c]	13.68	15.05	13.83	15.29	9.70	14.24	8.62	13.36
Non Clinical hours[c] [d]	6.20	7.92	4.88	7.65	8.30	8.83	7.34	8.50
___ Administration[c]	3.60	6.51	2.06	6.16	4.19	6.66	2.57	5.99
______ Teaching[c]	2.59	4.04	2.82	4.26	4.11	5.20	4.78	5.72
Research hours[c]	3.10	8.60	2.98	8.06	4.74	9.26	3.58	7.55

Notes: The cells display the average level of the practice variable in *row* and its st. deviation, for the control (*left-hand side columns*) and treatment (*right-hand side columns*) groups, before (*first sub-columns*) and after (*second sub-columns*) reform.

[a] There are 1 697 physicians in the control group and 423 in the treatment group. The period of observation is 1996-2000. N=9 238.

[b] In thousands (2000) CAN$. The volume is limited to clinical services that became billable under the MC scheme. The Before Period is 1996-1999 (the annual CMQ surveys are conducted in July while the reform was introduced in September 1999) and the After Period is 2000.

[c] All hours of work variables are measured on a weekly basis.

[d] Non-clinical hours exclude research hours since the latter are not included in the MC *per diem.*

[e] Age : There are 10 groups of age ranked from 1 to 10. The value 1 means 30 years old and less and the value 10 means 70 years old and more. The age interval among each group is five years.

[f] Census Metropolitan Areas.

Table 3: Descriptive statistics : Personal Characteristics by group

	Control group				Treatment group			
	Before		After		Before		After	
	Mean	St. Error	Mean	St. Error	Mean	St. Error	Mean	St. Error
Sex (Male=1)	0.81	0.39	0.80	0.40	0.72	0.45	0.69	0.46
Language (French=1)	0.79	0.41	0.79	0.41	0.86	0.35	0.87	0.34
Age [a]	5.19	2.09	5.63	2.11	4.66	1.96	5.01	1.94
CMAs with teaching university[b]	0.73	0.45	0.72	0.45	0.78	0.41	0.77	0.42
CMAs without teaching university[b]	0.06	0.25	0.07	0.25	0.08	0.27	0.09	0.29
Census Agglomeration	0.13	0.33	0.11	0.31	0.11	0.31	0.11	0.31
Other areas	0.08	0.28	0.10	0.31	0.03	0.18	0.03	0.17
Regional Hospital	0.15	0.36	0.14	0.35	0.11	0.31	0.10	0.31
Supra Regional Hospital	0.24	0.43	0.24	0.43	0.15	0.35	0.15	0.36
Local and regional Hospital	0.50	0.50	0.50	0.50	0.72	0.45	0.71	0.46
Other Hospital	0.10	0.31	0.12	0.33	0.03	0.16	0.10	0.31
Anesthesiology	0.09	0.28	0.09	0.29	0.15	0.36	0.18	0.38
Cardiology	0.14	0.35	0.14	0.35	0.05	0.21	0.04	0.20
Dermatology	0.07	0.25	0.07	0.25	0.07	0.25	0.06	0.24
Endocrinology	0.03	0.18	0.03	0.18	0.05	0.22	0.04	0.20
Gastroenterology	0.07	0.26	0.07	0.25	0.00	0.07	0.01	0.08
General surgery	0.12	0.33	0.12	0.33	0.11	0.32	0.11	0.31
Nephrology	0.04	0.20	0.04	0.20	0.01	0.10	0.01	0.10
Neurology	0.06	0.25	0.06	0.24	0.03	0.18	0.03	0.17
Neurosurgery	0.00	0.06	0.00	0.05	0.07	0.25	0.06	0.24
Obstetrics and gynecology	0.12	0.33	0.12	0.33	0.03	0.18	0.03	0.18
Orthopaedic surgery	0.09	0.29	0.10	0.30	0.05	0.21	0.04	0.20
Pediatrics	0.09	0.28	0.08	0.28	0.25	0.43	0.26	0.44
Pneumology	0.05	0.21	0.05	0.21	0.01	0.09	0.01	0.11
Radiation oncology	0.01	0.08	0.01	0.07	0.05	0.21	0.05	0.22
Rheumatology	0.01	0.10	0.01	0.10	0.07	0.26	0.06	0.24

Notes: Sample distribution over individual characteristics inside control (*left-hand side column*) and treatment (*right-hand side*) groups, before (*first sub-column*) and after (*second*) the reform.
[a] Age : There are 10 groups of age ranked from 1 to 10. The value 1 means 30 years old and less and the value 10 means 70 years old and more. The age interval among each group is five years.
[b] Census Metropolitan Areas.

Table 4: Impact of mixed compensation on practice variable : all specialties in the sample[a]

(Control group: specialists paid only under the FFS scheme after reform)

	Fixed effects OLS			Pooled tobit						
	Volume[b] / year	Income[b] / year	Hours / week	Clinical hours/ week: Tot.	Clinical hours/ week: Hosp.	Clinical hours/ week: Priv. Cl.	Non-Clinical hours/ week[c]: Tot.	Non-Clinical hours/ week[c]: Admin.	Non-Clinical hours/ week[c]: Teach.	Research hours/ week
Coefficient (= 1 if treated in 2000)	-8.30***	17.29***	-1.09*	-1.14**	0.11	-1.52**	1.23***	1.01	0.60*	-2.43***
St. Error[d]	1.55	2.48	0.58	0.57	0.60	0.74	0.45	0.67	0.34	0.68
Effect of the reform[e]	-8.30***	17.29***	-1.09*	-1.14**	0.15	-0.81**	0.53*	-0.16	0.47**	-0.77***
St. Error[d]	1.55	2.48	0.58	0.57	0.57	0.37	0.32	0.28	0.22	0.25
% effect of the reform	-6.15	8.05	-2.05	-2.57	0.45	-9.07	7.92	-7.89	10.74	-14.72
Log likelihood	-	-	-	-	-35 685	-25 151	-26 066	-19 402	-18 040	-14 353
Hausman test[f]	94.30	93.81	68.13	58.10	-	-	-	-	-	-
Test of parallel trend[g]	1.00	1.19	0.78	0.36	2.35	1.34	4.71	1.53	4.44	4.24

Notes: Regression results for the whole sample of physicians.

[a] Significance levels: * 10%, ** 5%, *** 1%. Period of observation: 1996-2000. $N = 9\ 238$. There are 1 697 physicians in the control group and 423 in the treatment group. In the fixed effects OLS models, we add dummies for years (4), regions (3), establishments (3 : supraregional, "local and regional", and others) and age (8). Also, the variable (= 1 if also treated in 1999) has been introduced in the volume and income regressions. In the pooled tobit models, we also add dummies for group (Treated= 1), sex (Male= 1), language (Francophone= 1) and specialties (14). (These variables are unidentified in the fixed effects OLS models since they are time-invariant).

[b]In thousands (2000) CAN$. The volume is limited to clinical services that became billable under the MC scheme.

[c]Non-clinical hours exclude research hours since the latter are not included in the MC *per diem*.

[d]In the fixed effects OLS models, the standard errors of estimated coefficients are robust to heteroscedasticity and serial correlation of unrestricted form at the individual level. In the pooled tobit models, they are robust to serial correlation of unrestricted form at the individual level.

[e]Measured by the mean of the estimated effect of switching from FFS to MC for each physician who chose the MC scheme after the reform.

[f]The statistic is $\chi^2(20)$ in the cases of volume and income and $\chi^2(19)$ in the cases of hours and clinical hours a week. In cases in which the difference in the variance matrices is not positive definite, a Moore-Penrose generalized inverse is used.

[g] The statistic allows to test the assumption that, before the reform, time changes have the same effect on both control and treatment groups. Years 1996 to 1998 (resp., 1996 to 1999) are used in the estimations of volume and income (resp., all other variables). In the fixed effects OLS models, the statistic is $F(2, 2\ 119)$ for volume and income and $F(3, 2\ 119)$ for all other variables. It is $\chi^2(3)$ (Wald test) in the pooled tobit models.

Table 5: Impact of mixed compensation on practice variables[a] : placebo effect (1996-1998)

(Control group: specialists paid only FFS after reform)

	Fixed effects OLS				Pooled tobit					
	Volume[b] / year	Income[b] / year	Hours / week	Clinical hours/ week Tot.	Clinical hours/ week Hosp.	Clinical hours/ week Priv.	Non-Clinical hours/ week[c] Tot.	Non-Clinical hours/ week[c] Admin.	Non-Clinical hours/ week[c] Teach.	Research hours/ week
Coefficient (= 1 if treated in 1998)	1.03	0.45	0.37	0.41	0.69	0.19	0.25	0.61	-0.20	-1.49
SE[d]	1.35	2.03	0.60	0.62	0.74	0.87	0.52	0.54	0.32	0.89
Effect of the reform[e]	1.03	0.45	0.37	0.41	0.68	-0.01	0.18	0.37	-0.14	-0.51
SE[d]	1.35	2.03	0.60	0.62	0.79	0.44	0.40	0.34	0.20	0.33
% effect of the reform	0.80	0.22	0.69	0.94	2.12	-0.09	2.09	7.57	-3.57	-8.97
Log likelihood	-	-	-	-	-20 500	-14 280	-15 786	-13 703	-10 113	-8 213
Hausman test[f]	99.77	135.49	34.58	37.61	-	-	-	-	-	-

Notes: Regression results from the placebo setting specifying 1998 as the reform year.
[a] Significance levels: * 10%, ** 5%, *** 1%. N=5 281. Physicians in the treatment group are under the MC scheme each quarter during the year after reform (2000). The period of observation is 1996-1998. There are 1697 physicians in the control group and 423 in the treatment group. In the fixed effects OLS models, we add dummies for years (4), regions (3), establishments (3 : supraregional, "local and regional", and others) and age (8). In the pooled tobit models, we also add dummies for group (Treated= 1), sex (Male= 1), language (Francophone= 1) and specialties (12).
[b] In thousands (2000) CAN$. The volume is limited to clinical services that became billable under the system of mixed compensation.
[c] Non-clinical hours exclude research hours since the latter are nor included in the MC *per diem.*
[d] In the fixed effects OLS models, the standard errors of estimated coefficients are robust to heteroscedasticity and serial correlation of unrestricted form at the individual level. In the pooled tobit models, they are robust to serial correlation of unrestricted form at the individual level.
[e] Measured by the mean of the estimated effect of switching from FFS to MC for each physician who chose the MC scheme after the reform.
[f] The statistic is $\chi^2(20)$ in the cases of volume and income and $\chi^2(19)$ in the cases of hours and clinical hours a week. In cases in which the difference in the variance matrices is not positive definite, a Moore-Penrose generalized inverse is used.

Table 6: Impact of mixed compensation on practice variables : male[a]

(Control group: male specialists paid only under the FFS scheme after reform)

	Fixed effects OLS				Pooled tobit					
	Volume[b] / year	Income[b] / year	Hours / week	Clinical hours/ week Tot.	Hosp.	Priv. Cl.	Non-Clinical hours/ week[c] Tot.	Admin.	Teach.	Research hours/ week
Coefficient (= 1 if treated in 2000)	-7.62***	15.48***	-1.19	-1.19*	-0.41	-0.91	1.22**	1.13	0.54	-2.16**
St. Error[d]	1.84	2.93	0.74	0.71	0.70	0.89	0.55	0.80	0.40	0.76
Effect of the reform[e]	- 7.62***	15.48***	-1.19	-1.19*	-0.35	-0.44	0.54	-0.06	0.43	-0.74**
St. Error[d]	1.84	2.93	0.74	0.71	0.67	0.41	0.40	0.35	0.27	0.30
% effect of the reform	-5.54	7.04	-2.22	-2.70	-1.03	-5.66	7.53	-2.65	9.42	-12.40
Log likelihood	-	-	-	-	-28 205	-19 867	-20 909	-15 851	-14 329	-11 934
Hausman test[f]	108.28	113.94	50.43	55.67	-	-	-	-	-	-
Test of parallel trend[g]	0.55	0.88	0.84	0.08	0.62	0.96	2.26	1.14	1.74	1.52

Notes: Regression results from the sub-sample of male physicians.
[a]Significance levels: * 10%, ** 5%, *** 1%. N=7 280. There are 1 363 physicians in the control group and 292 in the treatment group. In the fixed effects OLS models, we add dummies for years (4), regions (3), establishments (3 : supraregional, "local and regional", and others) and age (8). Also, the variable (= 1 if also treated in 1999) has been introduced in the volume and income regressions. In the pooled tobit models, we also add dummies for group (Treated= 1), language (Francophone= 1) and specialties (14). (These variables are unidentified in the fixed effects OLS models since they are time-invariant).
[b]In thousands (2000) CAN$. The volume is limited to clinical services that became billable under the MC scheme.
[c]Non-clinical hours exclude research hours since the latter are not included in the MC *per diem.*
[d]In the fixed effects OLS models, the standard errors of estimated coefficients are robust to heteroscedasticity and serial correlation of unrestricted form at the individual level. In the pooled tobit models, they are robust to serial correlation of unrestricted form at the individual level.
[e]Measured by the mean of the estimated effect of switching from FFS to MC for each physician who chose the MC scheme after the reform.
[f]The statistic is $\chi^2(20)$ in the cases of volume and income and $\chi^2(19)$ in the cases of hours and clinical hours a week. In cases in which the difference in the variance matrices is not positive definite, a Moore-Penrose generalized inverse is used.
[g] The statistic allows to test the assumption that, before the reform, time changes have the same effect on both control and treatment groups. Years 1996 to 1998 (resp., 1996 to 1999) are used in the estimations of volume and income (resp., all other variables). In the fixed effects OLS models, the statistic is $F(2, 1\ 654)$ for volume and income and $F(3, 1\ 654)$ for all other variables. It is $\chi^2(3)$ (Wald test) in the pooled tobit models.

Table 7: Impact of mixed compensation on practice variables : female[a]

(Control group: female specialists paid only under the FFS scheme after reform)

	Fixed effects OLS				Pooled tobit					
	Volume[b] / year	Income[b] / year	Hours / week	Clinical hours/ week			Non-Clinical hours/ week[c]			Research hours/ week
				Tot.	Hosp.	Priv. Cl.	Tot.	Admin.	Teach.	
Coefficient (= 1 if treated in 2000)	-10.08***	20.29***	-0.79	-0.83	1.30	-3.37**	1.31	1.22	0.71	-2.62*
St. Error[d]	27.96	4.62	0.96	0.98	1.18	1.35	0.83	1.24	0.67	1.50
Effect of the reform[e]	-10.08***	20.29***	-0.79	-0.83	1.26	2.12 ***	0.65	-0.04	0.55	-0.75*
St. Error[d]	27.96	4.62	0.96	0.98	1.10	0.82	0.55	0.50	0.40	0.44
% effect of the reform	-8.22	10.68	-1.54	-1.83	4.16	-17.71	11.94	-3.23	13.97	-21.40
Log likelihood	-	-	-	-	-7 410	-5 223	-5 094	-3 497	-3 658	-2 350
Hausman test[f] ($\chi^2(16)$)	23.54	29.55	35.71	22.32	-	-	-	-	-	-
Test of parallel trend[g]	0.55	0.88	0.89	1.36	4.12	4.75	6.53	3.61	8.36	14.16

Notes: Regression results from the sub-sample of female physicians.

[a]Significance levels: * 10%, ** 5%, *** 1%. $N = 1\ 958$. There are 334 physicians in the control group and 131 in the treatment group. In the fixed effects OLS models, we add dummies for years (4), regions (3), establishments (3 : supraregional, "local and regional", and others) and age (8). Also, the variable (= 1 if also treated in 1999) has been introduced in the volume and income regressions. In the pooled tobit models, we also add dummies for group (Treated= 1), language (Francophone= 1) and specialties (14). (These variables are unidentified in the fixed effects OLS models since they are time-invariant).

[b]In thousands (2000) CAN$. The volume is limited to clinical services that became billable under the MC scheme.

[c]Non-clinical hours exclude research hours since the latter are not included in the MC *per diem.*

[d]In the fixed effects OLS models, the standard errors of estimated coefficients are robust to heteroscedasticity and serial correlation of unrestricted form at the individual level. In the pooled tobit models, they are robust to serial correlation of unrestricted form at the individual level.

[e]Measured by the mean of the estimated effect of switching from FFS to MC for each physician who chose the MC scheme after the reform.

[f]The statistic is $\chi^2(20)$ in the cases of volume and income and $\chi^2(19)$ in the cases of hours and clinical hours a week. In cases in which the difference in the variance matrices is not positive definite, a Moore-Penrose generalized inverse is used.

[g] The statistic allows to test the assumption that, before the reform, time changes have the same effect on both control and treatment groups. Years 1996 to 1998 (resp., 1996 to 1999) are used in the estimations of volume and income (resp., all other variables). In the fixed effects OLS models, the statistic is $F(2, 464)$ for volume and income and $F(3, 464)$ for all other variables. It is $\chi^2(3)$ (Wald test) in the pooled tobit models.

Table 8: Impact of mixed compensation on practice variables: Pediatrics[a]

(Control group: pediatricians paid only under the FFS scheme after reform)

	Fixed effects OLS			Pooled tobit						
	Volume[b]	Income[b]	Hours	Clinical hours/ week			Non-Clinical hours/ week[c]			Research
	/ year	/ year	/ week	Tot.	Hosp.	Priv. Cl.	Tot.	Admin.	Teach.	hours/ week
Coefficient (= 1 if treated in 2000)	-12.99***	15.83***	-1.87*	-2.88**	0.34	-3.46*	3.92***	3.27*	0.48	-9.56**
St. Error[d]	3.72	6.62	1.12	1.14	1.33	1.83	1.40	1.85	1.19	4.03
Effect of the reform[e]	-12.99***	15.83***	-1.87*	-2.88**	0.49	-2.92**	1.56*	0.48	0.61	-2.01*
St. Error[d]	3.72	6.62	1.12	1.14	1.20	1.48	0.80	0.68	0.56	1.06
% effect of the reform	-12.81	8.69	-3.75	-6.55	2.20	-15.88	27.89	25.14	13.88	-38.20
Log likelihood	-	-	-	-	-3 423	-3 757	-2 678	-2 078	-1 653	-1 147
Hausman test[f]	53.86	37.68	41.10	44.97	-	-	-	-	-	-
Test of parallel trend[g]	0.92	0.48	0.28	1.22	2.27	7.60	9.03	2.55	7.99	9.05

Notes: Regression results from the sub-sample of pediatricians.

[a]Significance levels: * 10%, ** 5%, *** 1%. $N = 1\ 072$. There are 143 physicians in the control group and 110 in the treatment group. In the fixed effects OLS models, we add dummies for years (4), regions (3), establishments (3 : supraregional, "local and regional", and others) and age (8). Also, the variable (= 1 if also treated in 1999) has been introduced in the volume and income regressions. In the pooled tobit models, we also add dummies for group (Treated= 1), sex (Male= 1) and language (Francophone= 1). (These variables are unidentified in the fixed effects OLS models since they are time-invariant.).

[b]In thousands (2000) CAN$. The volume is limited to clinical services that became billable under the MC scheme.

[c]Non-clinical hours exclude research hours since the latter are not included in the MC *per diem.*

[d]In the fixed effects OLS models, the standard errors of estimated coefficients are robust to heteroscedasticity and serial correlation of unrestricted form at the individual level. In the pooled tobit models, they are robust to serial correlation of unrestricted form at the individual level.

[e]Measured by the mean of the estimated effect of switching from FFS to MC for each physician who chose the MC scheme after the reform.

[f]The statistic is $\chi^2(20)$ in the cases of volume and income and $\chi^2(19)$ in the cases of hours and clinical hours a week. In cases in which the difference in the variance matrices is not positive definite, a Moore-Penrose generalized inverse is used. However, it should be noted that the variance matrix of β is not positive definite.

[g] The statistic allows to test the assumption that, before the reform, time changes have the same effect on both control and treatment groups. Years 1996 to 1998 (resp., 1996 to 1999) are used in the estimations of volume and income (resp., all other variables). In the fixed effects OLS models, the statistic is $F(2, 253)$ for volume and income and $F(3, 253)$ for all other variables. It is $\chi^2(3)$ (Wald test) in the pooled tobit models.

Table 9: Impact of mixed compensation on practice variables : General Surgery[a]

(Control group: general surgeons paid only under the FFS scheme after reform)

	Fixed effects OLS				Poooled tobit					
	Volume[b] / year	Income[b] / year	Hours / week	Clinical hours/ week			Non-Clinical hours/ week[c]			Research hours/ week
				Tot.	Hosp.	Priv. Cl.	Tot.	Admin.	Teach.	
Coefficient (= 1 if treated in 2000)	-15.32***	18.37***	-2.70	-1.83	0.04	-4.68**	0.62	0.27	0.53	-1.36
St. Error[d]	3.81	4.28	2.48	2.37	2.54	2.00	1.27	1.64	1.04	1.59
Effect of the reform[e]	-15.32***	18.38***	-2.70	-1.83	0.06	-1.02	0.07	-0.83	0.38	-1.07
St. Error[d]	3.81	4.38	2.48	2.37	2.52	0.83	0.95	0.75	0.66	0.77
% effect of the reform	-10.72	8.14	-4.81	-3.77	0.14	-31.47	0.81	-23.05	8.37	-21.53
Log likelihood	-	-	-	-	-4 509	-3 014	-3 093	-2 459	-1 916	-1 235
Hausman test[f]	39.41	8.07	169.10	26.50	-	-	-	-	-	-
Test of parallel trend[g]	1.46	0.98	1.13	0.71	4.81	0.96	1.19	2.08	2.49	2.08

Notes: Regression results from the subsample of general surgeons.
[a]Significance levels: * 10%, ** 5%, *** 1%. $N = 1\ 109$. There are 212 physicians in the control group and 47 in the treatment group. In the fixed effects OLS models, we add dummies for years (4), regions (3), establishments (3 : supraregional, "local and regional", and others) and age (8). Also, the variable (= 1 if also treated in 1999) has been introduced in the volume and income regressions. In the pooled tobit models, we also add dummies for group (Treated= 1), sex (Male= 1) and language (Francophone= 1). (These variables are unidentified in the fixed effects OLS models since they are time-invariant.).
[b]In thousands (2000) CAN$. The volume is limited to clinical services that became billable under the MC scheme.
[c]Non-clinical hours exclude research hours since the latter are not included in the MC *per diem.*
[d]In the fixed effects OLS models, the standard errors of estimated coefficients are robust to heteroscedasticity and serial correlation of unrestricted form at the individual level. In the pooled tobit models, they are robust to serial correlation of unrestricted form at the individual level.
[e]Measured by the mean of the estimated effect of switching from FFS to MC for each physician who chose the MC scheme after the reform.
[f]The statistic is $\chi^2(20)$ in the cases of volume and income and $\chi^2(19)$ in the cases of hours and clinical hours a week. In cases in which the difference in the variance matrices is not positive definite, a Moore-Penrose generalized inverse is used.
[g] The statistic allows to test the assumption that, before the reform, time changes have the same effect on both control and treatment groups. Years 1996 to 1998 (resp., 1996 to 1999) are used in the estimations of volume and income (resp., all other variables). In the fixed effects OLS models, the statistic is$F(2, 258)$ for volume and income and $F(3, 258)$ for all other variables. It is $\chi^2(3)$ (Wald test) in the pooled tobit models.

Table 10: Impact of mixed remuneration on practice variables : specialties other than pediatrics and General Surgery[a]

(Control group: specialists other than pediatricians and general surgeons paid only under the FFS scheme after reform)

	Fixed effects OLS				Pooled tobit					
	Volume[b] / year	Income[b] / year	Hours / week	Clinical hours/ week: Tot.	Clinical hours/ week: Hosp.	Clinical hours/ week: Priv. Cl.	Non-Clinical hours/ week[c]: Tot.	Non-Clinical hours/ week[c]: Admin.	Non-Clinical hours/ week[c]: Teach.	Research hours/ week
Coefficient (= 1 if treated in 2000)	-4.61**	17.95	-0.71	-0.61	0.08	-0.32	0.85*	0.36*	0.55*	-0.89
St. Error[d]	1.90	3.00	0.67	0.67	0.64	0.73	0.47	0.64	0.31	0.63
Effect of the reform[e]	-4.61**	17.95***	-0.71	-0.61	0.08	-0.35	0.47	-0.12	0.44**	-0.45
St. Error[d]	1.90	3.00	0.67	0.67	0.61	0.41	0.36	0.32	0.22	0.33
% effect of the reform	-3.32	8.19	-1.34	-1.38	0.23	-3.76	6.31	-4.32	7.92	-6.97
Log likelihood	-	-	-	-	-25 710	-16 304	-18 997	-14 117	-13 344	-10 615
Hausman test[f] ($\chi^2(19)$)	74.54	95.03	65.27	66.57	-	-	-	-	-	-
Test of parallel trend[g]	0.45	0.67	1.19	0.66	2.47	8.14	0.53	0.74	0.73	3.49

Notes: Regression results from the subsample including all specialties in our sample except pediatrics and surgery.

[a]Significance levels: $\star$ 10%, $\star\star$ 5%, $\star\star\star$ 1%. $N = 7\ 057$. There are 1 697 physicians in the control group and 423 in the treatment group. In the fixed effects OLS models, we add dummies for years (4), regions (3), establishments (3 : supraregional, "local and regional", and others) and age (8). Also, the variable (= 1 if also treated in 1999) has been introduced in the volume and income regressions. In the tobit random effects model, we also add dummies for group (Treated= 1), sex (Male= 1), language (Francophone= 1) and specialties (11). (These variables are unidentified in the fixed effects OLS model).

[b]In thousands (2000) CAN$. The volume is limited to clinical services that became billable under the MC scheme.

[c]Non-clinical hours exclude research hours since the latter are not included in the MC *per diem.*

[d]In the fixed effects OLS models, the standard errors of estimated coefficients are robust to heteroscedasticity and serial correlation of unrestricted form at the individual level. In the pooled tobit models, they are robust to serial correlation of unrestricted form at the individual level.

[e]Measured by the mean of the estimated effect of switching from FFS to MC for each physician who chose the MC scheme after the reform.

[f]The statistic is $\chi^2(20)$ in the cases of volume and income and $\chi^2(19)$ in the cases of hours and clinical hours a week. In cases in which the difference in the variance matrices is not positive definite, a Moore-Penrose generalized inverse is used.

[g] The statistic allows to test the assumption that, before the reform, time changes have the same effect on both control and treatment groups. Years 1996 to 1998 (resp., 1996 to 1999) are used in the estimations of volume and income (resp., all other variables). In the fixed effects OLS models, the statistic is $F(2, 1\ 606)$ for volume and income and $F(3, 1\ 606)$ for all other variables. It is $\chi^2(3)$ (Wald test) in the random effects tobit models.

Table 11: Impact of mixed compensation on practice variables: all specialties in the sample[a]

(Control group: general practitioners paid only under the FFS scheme)

	Fixed effects OLS				Pooled tobit					
	Volume[b] / year	Income[b] / year	Hours / week	Clinical hours/ week: Tot.	Clinical hours/ week: Hosp.	Clinical hours/ week: Priv. Cl.	Non-Clinical hours/ week[c]: Tot.	Non-Clinical hours/ week[c]: Admin.	Non-Clinical hours/ week[c]: Teach.	Research hours/ week
Coefficient (= 1 if treated in 2000)	-5.70***	23.38***	-1.96***	-1.64***	0.98	-2.51***	3.38***	0.65	0.27	-4.14***
St. Error[d]	1.65	2.32	0.56	0.54	0.63	0.66	0.52	0.80	0.37	0.83
Effect of the reform[e]	-5.70***	23.38***	-1.96***	-1.64***	1.18**	-1.47***	1.12***	-0.41	0.50**	-1.09***
St. Error[d]	1.65	2.32	0.56	0.54	0.60	0.40	0.33	0.30	0.22	0.28
% effect of the reform	-3.90	14.80	-4.24	-3.88	3.68	-16.58	18.06	-16.26	10.68	-18.59
Log likelihood	-	-	-	-	-33 207	-48 201	-29 124	-25 202	-10 689	-7 157
Hausman test[f]	165.58	92.61	46.68	140.47	-	-	-	-	-	-
Test of parallel trend[g]	3.77	0.73	12.86	9.41	24.04	2.08	70.15	0.78	8.70	9.80

Notes: Regression results from the whole sample, using general practitioners as the control group.
[a] Significance levels: * 10%, ** 5%, *** 1%. Period of observation: 1996-2000. $N = 13\ 124$. There are 2 662 physicians in the control group and 423 in the treatment group. In the fixed effects OLS models, we add dummies for years (4), regions (3), establishments (3 : supraregional, "local and regional", and others) and age (8). Also, the variable (= 1 if also treated in 1999) has been introduced in the volume and income regressions. In the pooled tobit models, we also add dummies for group (Treated= 1), sex (Male= 1) and language (Francophone= 1). (These variables are unidentified in the fixed effects OLS models since they are time-invariant.).
[b] In thousands (2000) CAN$. The volume is limited to clinical services that became billable under the MC scheme.
[c] Non-clinical hours exclude research hours since the latter are not included in the MC *per diem*.
[d] In the fixed effects OLS models, the standard errors of estimated coefficients are robust to heteroscedasticity and serial correlation of unrestricted form at the individual level. In the pooled tobit models, they are robust to serial correlation of unrestricted form at the individual level.
[e] Measured by the mean of the estimated effect of switching from FFS to MC for each physician who chose the MC scheme after the reform.
[f] The statistic is $\chi^2(20)$ in the cases of volume and income and $\chi^2(19)$ in the cases of hours and clinical hours a week. In cases in which the difference in the variance matrices is not positive definite, a Moore-Penrose generalized inverse is used.
[g] The statistic allows to test the assumption that, before the reform, time changes have the same effect on both control and treatment groups. Years 1996 to 1998 (resp., 1996 to 1999) are used in the estimations of volume and income (resp., all other variables). In the fixed effects OLS models, the statistic is $F(2, 3\ 084)$ for volume and income and $F(3, 3\ 084)$ for all other variables. It is $\chi^2(3)$ (Wald test) in the pooled tobit models.