Does the abolition of direct payments for consultations reduce emergency room visits? Evidence from a French natural experiment.

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Abstract

We analyze the effects of a 2017 French reform that removed direct payments for all the care consumed by pregnant women after the 6th month of pregnancy. Direct payments create a liquidity constraint on individuals' income that can deter care consumption, especially among low-income individuals. In France, a free complementary health insurance (CHI) managed by the National Health Insurance (NHI) already exists to exempt the care consumption of low-income individuals from direct payments. Therefore, this study investigates whether there are still individuals who are constrained by the price of a medical consultation despite the existence of this free CHI. To do so, we use a sample of the exhaustive database on individuals' care consumption, the SNIIRAM. This database records all out-patient care and hospital care consumed by almost all the French population. Using a triple differences estimator, we estimate the causal impact of the reform on the number of out-patient consultations, and the number of ED visits made by pregnant women. We find that the abolition of direct payments led to a small increase in the number of General Practitioner (GP) consultations of pregnant women (+6.4 consultations by month per 100 women). This result suggests that a small proportion of pregnant women was still subject to liquidity constraints for care, despite the existence of the CMU-C. In addition, we test for the existence of a substitution between out-patient care and emergency care. Since ED visits are free from direct payments, liquidity constrained individuals might prefer to go to the ED instead of a general practice to avoid the payment of a consultation. We find no effect of the reform on the number of ED visits made by pregnant women. Put together, the results provide empirical evidence that the presence of direct payments for out-patient consultations restricts the access to GP care of liquidity constrained pregnant women, and do not encourage the use of EDs for non-urgent conditions. This paper contributes to a very scarce literature on the liquidity sensitivity of health care consumption in high-income countries, but also to the literature on the inappropriate utilization of EDs by suggesting that the absence of direct payments for ED visits is not a determinant of inappropriate use. These findings provide useful information for the public debate about extending the exemption of direct payments for care to the general French population.

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1 Introduction

The World Health Organization (WHO) describes direct payments as the most inequitable form of funding [37]. Since the 1978 Alma Ata conference [47], it shows that the risk of catastrophic health care expenditure (spending more than 40% of income for health care) increases with the share of direct payments in total health care expenditure. However more than 100 million of households faced catastrophic health care expenditure in 2008 worldwide [37]. The presence of direct payments for care products and services increases the probability of care renouncement. And the lower the income, the higher this probability. Following WHO recommendations, many developing countries have gradually removed direct payments for a specific set of services and/or targeted populations [39]. Direct payments can also prevent access to care in developed countries. But evidence on the effect of direct payments' exemption on care consumption is lacking in these countries. Most of the reforms that aim at increasing financial access to primary care in European countries ensured free care by removing copayments. For instance, copayments for GP consultations were removed in Sweden in 2002 for children and adolescents aged 7-19 [31], in Norway in 2010 for adolescents aged 12 to 15, and in Ireland in 2015 for children under 6 years, and individuals aged 70 and older [32, 46].

The effect of direct payments should not be confused with the effect of insurance coverage. In France, direct payment is the main source of payment for out-patient care. The price of a medical consultation (for instance €25 for a consultation with a GP) is fully paid by patients. These payments impose a liquidity constraint on individuals' income that can deter care utilization. Thus, liquidity constraints induced by direct payments are a concern for out-patient consultations. This effect should be distinguished from the price sensitivity of care consumption, i.e., the sensitivity to copayments. In France, a National Health Insurance (NHI) covers 70% of out-patient care and 80% of inpatient care expenditure but unlike out-patient care, direct payments are not required for inpatient care. On top of that, 96% of the French population is covered by a complementary health insurance (CHI) [3] that reimburses the remaining share of expenditure. These two insurances (the French NHI and the private CHI chosen by patients) reimburse afterwards the price of the consultation to patients.

In 2017 a French reform removed direct payments for two specific populations[1]: (1) pregnant women for all the care consumed (on top of the prenatal care recommended by the French NHI guidelines) between the first day of the 6th month of pregnancy and the 12th day following birth delivery, and (2) individuals suffering from long-term diseases (LTD) for all the care services used in relation to their disease. In addition, the reform removed the cost-sharing for the care consumed by pregnant women[2]. Note that pregnant women were already exempt from direct payments and cost-sharing for the prenatal care recommended by NHI guidelines (see appendix A.8). So the reform affected the care consumed by pregnant women *on top* of the recommended

[1] This reform is known as the "*Tiers-Payant*" reform in France.

[2] Care consumed by LTD patients in relation to their disease was already covered at 100% by the French NHI before the reform.

care. Since 2017, the cost of all out-patient care consumed after 6 months of pregnancy is zero. Note that the abolition of cost-sharing did not increase the reimbursements of pregnant women who were covered by a private CHI, like 96% of the French population. For this reason, we think that the main impact of the reform is the abolition of direct payments. This reform was not the first to remove direct payments for specific populations (see appendix A.7 for details on payment exemptions). In particular, direct payments (and cost-sharing) were removed for low-income households in 2000. This was made through the introduction of a free complementary health insurance (CHI) managed by the French NHI called "CMU-C"[3]. This CHI provides full insurance to the beneficiaries and prohibits direct payments for all the care consumed. So the risk to forgo or delay care consumption because of the presence of direct payments is limited by the existence of the CMU-C since 2000. Therefore, it is legitimate to question the need for the 2017 "Tiers-Payant" reform, which extended the exemption of direct payments to pregnant women and LTD patients who were not CMU-C beneficiaries. The annual income to be eligible to the CMU-C should not exceed €8,723 for a single individual in 2017[4], which corresponds to 70% of the poverty line value in France in 2017 [18]. We wonder if some individuals above the CMU-C eligibility threshold are still liquidity constrained by direct payments for care.

Thus, we investigate in this paper the following questions. First, we analyze if the abolition of direct payments resulted in an increase in the care consultations of pregnant women. Second, if any impact is found, we examine the possible spillover effects of the reform on ED visits. Since direct payments are not required for ED visits, the reform could have resulted in a decrease in ED visits if EDs were used as substitutes to GP or specialists consultations before the reform to avoid the payment of a consultation. We investigate the impact of the reform on two outcomes: (i) the care consumed by pregnant women (consultations and ED visits), and (ii) the probability of pre-term births. We propose a triple difference (DDD) estimator to assess the causal impact of the reform on the care consumption of French pregnant women between July 2014 and June 2018.

This paper is organized as follows. The next section 2 details the reform and the context of its implementation. Section 3 discusses the sensitivity of care consumption to price and direct payments. Since the reform led to a change in both the level of direct payments and the level of cost-sharing, this section will discuss the mechanisms through which these changes can affect care consumption. Then, section 4 explains the extent to which the incentive to substitute out-patient care for EDs was removed by the reform. The following section 5 presents the data and section 6 the empirical strategy. Results are reported in section 7. The robustness of the results is tested in section 8. Section 9 concludes this paper.

[3]Couverture Maladie Universelle Complémentaire.
[4]Source: https://www.legifrance.gouv.fr/jorf/id/JORFTEXT000034316282

2 The reform

The objective of the 2017 "Tiers-Payant" reform is to improve access to out-patient care by removing liquidity constraints, i.e., direct payments for care. Originally, all individuals insured by the French NHI should have been affected by a withdrawal of direct payments. But in France, care is covered by two insurances: the French NHI, and private insurance companies that cover the copayments. Organizing a general exemption of direct payments was too complicated to implement in the French system where many private insurance companies reimburse a share of consultation fees. Therefore, this project was abandoned. Instead of a general withdrawal of direct payments, the French government targeted two specific populations: pregnant women and individuals suffering from long-term diseases (LTD)[5]. The 1st of January 2017, direct payments were removed for: (i) all the care consumed by pregnant women on top of the care recommended by the NHI guidelines, between the 6th month of pregnancy and the 12th day after the birth delivery ; (ii) all care and services used by LTD patients, in relation to their disease. In addition, the reform ensured full coverage (balance billings excluded[6]) for the care consumed by pregnant women after 6 month of pregnancy. This change of coverage did not affect pregnant women who were covered by a CHI because their copayments were reimbursed before the reform, as for the 96% of the French population who has a CHI. For this reason, we believe that the main impact of the reform for pregnant women is the abolition of direct payments. LTD patients were not concerned by this change in coverage because they were already exempted from cost-sharing long before the reform for the care used in relation to their disease. All recommended medical examinations carried out during a pregnancy episode (see table A7 in appendix A.8) were already exempt from direct payments before the reform, and covered for 100% by the French NHI. These examinations concern mainly prenatal consultations (with a gynecologist, a midwife or a GP) and childbirth preparation sessions. So the reform removed direct payments for all the care consumed by pregnant women which is not recommended by the NHI guidelines, after 6 months of pregnancy. Putting differently, the reform targeted all the care consumed in addition to the recommended prenatal care.

Before 2017, the absence of direct payments was not a right for pregnant women and LTD patients. The French Ministry of Health estimated that pregnant women advanced an average of €640 during a pregnancy episode and that patients with diabetes (one of the LTD disease

[5]Long before the reform, the French Social Security has established a list of 30 long-term diseases (LTD) for which care expenditure are covered at 100% by the NHI. It concerns for instance patients suffering from cancer, diabetes, or heart failure. The exhaustive list of LTD covered by the French NHI is available from https://www.ameli.fr/assure/droits-demarches/maladie-accident-hospitalisation/affection-longue-duree-ald/affection-longue-duree-ald.

[6]In France, liberal physicians are free to chose between two sectors of practice. Either they affiliate to sector 1. In this case the price of the consultations is regulated and physicians cannot charge patients with balance billings. In return, they benefit from tax reductions [10]. Or they affiliate to sector 2. In this case they are free to set prices and charge patients with balance billings, but they pay more taxes. The French NHI only covers the regulated price of a consultation (€25 for a GP consultation). Balance billings can be covered by a complementary health insurance (CHI). In 2013, 6.8% of GPs and 26% of overall physicians were liberal physicians affiliated to sector 2 [19].

listed by the French NHI) had to pay an average of €1,100 per year for their care through direct payments before the reform [2]. But since the 1st of July 2016, it was possible for physicians to already exempt these patients from direct payment on a voluntary basis. In the following of this paper, we will control the estimations for the increase in the number of physicians removing direct payments before the reform, i.e., from mid-2016. By allowing the possibility to adapt before the reform, the French government objective was to encourage physicians to test and introduce smoothly direct payment exemptions. In fact, physicians were reluctant to this reform as they feared cash flow problems due to long payment delays from the French NHI. To reassure physicians, the French NHI has committed to pay a €1 penalty each time a payment took more than 7 days to be paid. The French NHI paid a total of €640K of penalties to physicians for the last half of 2016, and a total of €150K for the first half of 2017.

To sum up, we are interested in this paper in analyzing the impact of the 2017 "Tiers-Payant" reform on the care consumption of pregnant women. The main shock induced by the reform is the abolition of direct payments for the care used after 6 month of pregnancy (and before day+12 after the birth delivery) in addition to the prenatal care recommended by the NHI guidelines that was already exempt from direct payments before the reform. In the following of this paper, we will refer to pregnancy episodes after 6 months (and before day+12 after the birth delivery) as the "treatment" period. We will also use the terminology "control" period or "pre-treatment" period to refer to pregnancy episodes before 6 months. Timelines of the reform are reported in appendix A.1.

3 The sensitivity of health care consumption to price and liquidity

Care consumption can be influenced by changes in both the level of direct payments and cost-sharing. But the sensitivity of care consumption to the level of insurance coverage and the level of direct payments can differ.

3.1 The price elasticity of care consumption

From an economic perspective, price sensitivity refers to the way the demand is affected by a change in the price of a product. For "normal" goods, demand decreases with price. On the health care market, most prices are not set freely by care providers. In a majority of countries, health expenditure are partially covered by a regulated health insurance system. The system can either rely on private health insurance companies (like in Switzerland), or on a national (public) health insurance (like in France). In all, the prices of medical consultations (resp. medicines) are defined by a contract between the insurance company or the regulator, and the physicians (resp. the pharmaceutical industry). In practice, the "price sensitivity" of care consumption depends on the level of cost-sharing, i.e., the share of the price paid by the patient after reimbursement

by the insurance[7].

Available empirical evidence suggests that health care consumption is price sensitive. Results from the famous RAND experiment show that general practitioners' (GP) utilization decreases with the level of cost-sharing [30]. A study analyzing the effect of a strong reduction in patients' cost-sharing at age 70 in Japan also found evidence that care consumption is price sensitive. Both out-patient care and inpatient care increase with more generous insurance coverage [41]. Similarly, several high-income countries have removed copayments for GP consultations in the past decades, ensuring free care to patients. Results of the evaluations suggest that GP utilization is price sensitive. In 2015, Ireland withdrew a €52.50 copayment for GP consultations of children aged 6 or less. It was followed by a 28.7% increase in the number of consultations [32]. Similar results were found in Sweden where copayments fell from $10 to $0 for GP consultation of adolescents below 19 in 2001. The overall number of GP consultations increased by 9% [31]. In 2010, Norway abolished copayment of €17.50 for a GP consultation for adolescents between 12 and 15 years old. It resulted in an increase in the number of GP consultations of 13.8% for males and 22.1% for females [34]. In a companion study Landsem and Magnussem (2014) exploit the discontinuity induced by the reform at the age of 16. They found a 10% to 15% decrease in GP consultations at the age of 16 when copayments are reintroduced [27].

3.2 The sensitivity of care consumption to liquidity constraints

The life-cycle theory of consumption developed by Modigliani and Brumberg in the 1950s [13] considers that the consumption of individuals is determined by their lifetime expected income. The implication of this theory is that individuals borrow money during their young life, then accumulate savings during their active life and eventually sells their assets and consume through dissavings during retirement[8]. Among others, this theory was criticized on the ground that the introduction of liquidity constraints[9] in the model would imply important changes on consumption behaviors [48]. In particular, liquidity constraints could prevent individuals to borrow money and invest in assets to smooth consumption when income is low [12]. In this case individuals are constrained in their capacity to consume by their available income. But since this constraint decreases with income, it is mostly a concern for low-income individuals. The sensitivity of consumption to liquidity constraints was empirically confirmed by using the unemployment rate as a proxy for the prevalence of liquidity constraints [20]. Now, the liquidity sensitivity of consumption to liquidity constraints is well established in the literature. Results from a recent paper show that low-income consumers purchase more goods on paydays [33]. This result suggests that liquidity constrained individuals delay consumption to a moment when they are less constrained (for instance on payday)[10]. Transposing this mechanism to health care consumption implies that the existence of direct payments for care might lead to the same behaviours. On one

[7]The remaining price paid by the patient is often denoted by the term "out-of-pocket expenditure".

[8]One of the life-cycle theory hypothesis is that individuals do not wish to leave inheritance [5].

[9]The literature also uses the terminology "borrowing constraints"

[10]Liquidity constraints may also lead to forgo consumption, or to purchase less expensive substitutes if competition exists on the market.

hand liquidity constrained individuals may either delay or forgo care consumption in the presence of direct payments. On the other hand they may substitute to a cheaper care accessible without direct payments. For now, the sensitivity of health care consumption to liquidity constraints has received little attention in the literature.

We found one recent study that evaluates the role of liquidity constraints on the care consumption of Medicare beneficiaries [22]. In this paper, the authors use quasi-random variations in the time of the month when pensions are paid by Social Security to its beneficiaries[11]. They find that the number of drug scripts increases by 11 percent on payday. Importantly, results show that "important" medical drug prescriptions (meaning that non-adherence to prescription can lead to severe short-term health consequences, e.g., blood thinners) are also sensitive to direct payments. Also, some Medicare recipients are enrolled in a federal government program that subsidies the purchase of private health insurance, and can partly cover copayments, depending on their income. The authors are able to distinguish between recipients with full coverage (no copayments), partial coverage (subsidized copayments) and no coverage (no-subsidized copayments). They found no increase in drug scripts on payday for individuals facing no copayments. To our knowledge, this study is the first to provide empirical evidence of the sensitivity of health care consumption to direct payments (and so liquidity constraints).

3.3 The sensitivity of care consumption in France

In France, the National Health Insurance (NHI) covers part of the healthcare expenditure of French residents. In 2017, 77.8% of healthcare expenditure in France was covered by the French NHI [3]. Out-patient care (for instance a consultation with a GP) is covered at 70% by the French NHI. The percentage reimbursed for hospital care (including emergency care) is 80%, and direct payments are not required for inpatient care. The remaining share of expenditures is to be paid by patients or their complementary health insurance (CHI) afterwards. In France, 96% of the population is covered by a CHI [3] but level of coverage are of course heterogeneous. Even after the reform, some care doesn't belong to the benefit package, and remain not reimbursed by the French NHI, as for instance ineffective medicines, alternative treatments (like osteopathy), or consultations with a psychologist. Medical consultations of pregnant women are undertaken by a GP, a gynecologist or a midwife, and are covered by both the French NHI and a CHI. So, as we explained in section 2, there is no change in coverage for 96% of patients following the reform. The main impact of the reform, if any, should rely on the liquidity mechanism arising from the removal of direct payments.

One specificity of the French system is that direct payments are required for the majority of out-patient care consultations. Patients must pay directly (out-of-pocket) 100% of the price of

[11]The day when social security paychecks are paid to recipients is pre-determined by the recipients' date of birth. So depending on their day of birth, two individuals of same age can receive their paychecks on two different weeks.

a medical consultation[12], which is reimbursed afterwards for 70% by the French NHI. The 30% cost-sharing is also reimbursed after the consultation by the patient's CHI if any. In France, the price of a GP consultation is €25, the price of a gynecological consultation is €30 and the price of a consultation with a midwife is €23 (it increased to €25 in 2019)[13]. After 2019, the price of a midwife consultation increased to €25 but this change is out of our period of analysis (2014-2018). These prices include a fixed contribution of €1 that must be paid by patients (or their CHI). In the absence of the reform, patients have to pay the full price of the consultation. Later, the French NHI reimburses the patients for 70% of the consultation price minus the fixed contribution, i.e., €16.8 for a GP consultation, €20.3 for a gynecological consultation and €15.4 for a consultation with a midwife. The copayment (€8.2 for a GP consultation, €9.7 for a gynecological consultation and €7.6 for a consultation with a midwife) might also be reimbursed after the consultation by the patient CHI if he holds one. Ater the 2017 reform (that acts from the 6th month of pregnancy to the 12th day after the birth delivery), physicians are not allowed to claim pregnant women any payment for the consultation. Since the reform also ensures full insurance to pregnant women (although it changed nothing for those who are covered by a CHI), the full price of the consultation is then paid by the French NHI to the physician after the consultation.

To our knowledge, only one study, dating back to 2000, has investigated the sensitivity of health care consumption (and health expenditure) to direct payments in France [17]. It was also the year (2000) when a free CHI "CMU-C" was implemented to remove direct payments and offer full insurance to low-income individuals who are likely to be liquidity constrained. The question we address in this paper is whether the rest of the population, i.e., people who are not eligible to the CMU-C, are exposed to liquidity constraints because of direct payments for care or not. This issue has not been studied yet. Some elements support the hypothesis that there are still individuals who are liquidity constrained by the price of a medical consultation. First, in 2013, the proportion of the French population who reported foregoing care at least once in the past 12 months for financial reasons was estimated between 21% and 33% [28]. Second, the maximum income to be eligible for the CMU-C is very low, far below the poverty level, as mentioned in the introduction of this paper[14]. So there are people with very low income who are not eligible for the CMU-C. These people might be subject to liquidity constraints. This paper contributes to explore the influence of liquidity constraints (i.e., the impact of direct payment exemption) on out-patient care consumption.

[12]This is not the case for hospital care.

[13]If the GP or the gynecologist is affiliated to sector 2, the regulated price can be increased by balance billings that are not covered by the NHI. All the regulated prices for physicians are available on the NHI website at the following address: https://www.ameli.fr/medecin/exercice-liberal/remuneration/consultations-actes/tarifs.

[14]The income eligibility threshold for the CMU-C corresponds to 70% of the poverty line.

4 EDs as substitutes to out-patient care

Price elasticity or the sensitivity to liquidity constraints are generally influenced by the existence of substitute goods and services. If some pregnant women were subject to liquidity constraints, it was possible for them to visit an ED. In fact, EDs are highly accessible facilities (opened 24/7) and no direct payment is claimed after a visit in the vast majority of them. Moreover, every patient presenting to an ED is treated without discrimination. These services are able to deal with situations ranging from the mildest conditions to life-threatening conditions that requires immediate resuscitation. These characteristics make the ED a good candidate to be used as a substitute to out-patient care. Because ED visits are exempt from direct payments in most EDs, a common fallacy among patients is that emergency care is free. In a 2013 survey on ED users conducted by the French Ministry of Health, one of the possible answer to the question "why did you come to the emergency room?" was "because cash advance is not required"[15] [15]. In reality EDs are not free, and patients receive at home a bill for the 20% copayment. The price of any admission to a French ED in 2017 was €25.32. [25]. This price is increased at least by the price of a medical consultation (€25, plus possible extra fees during the night and on week-end) and by the price of medical diagnostic procedures if any. So the lowest price for an ED visit in France is €50[16]. 80% of this price is reimbursed by the NHI. In comparison the regulated price of a GP visit is €25 of which 70% is reimbursed by the NHI. Thus, from the patient point of view, a GP visit is theoretically preferable to an ED visit. However, because 96% of patients are fully covered by the NHI (70%) and a CHI (30%), they are not sensitive to the price of consultations or visits. Conversely, they can be sensitive to the fact that direct payments are not requested in most EDs, contrary to GPs in ambulatory care setting. This can be a motive to substitute a GP or a specialist consultation to an ED visit. Because the cost of an ED visit is much higher for the regulator than the cost of an out-patient consultation, such a substitution would raise an efficiency issue. In fact, the average cost of an ED visit for the French NHI is about €148 [11] while a GP consultation costs 70% of €25. The indirect effect of the 2017 reform was to make this substitution unattractive by removing direct payments for medical consultations.

Empirical evidence of substitution between EDs and out-patient care has been found in several countries. Several papers find a negative correlation between accessibility to primary care services and ED utilization [44], [40], [35], [36]. In England, an experimentation was conducted to ensure the opening of GP practices every day of the week. A study of the experimentation's effects found that it decreased the number of ED visits by 9.9%[16]. But results are mixed regarding the substitution between EDs and GP for financial reasons. Again, the respective effects

[15]This was answered by less than 3% of the patients surveyed [9] but it is likely that patients under-report this reason to come to the ED because of social desirability bias [26].

[16]The provisional report on the social security accounts for September 2021 states that the average copayment for a non-hospitalized ED visit was €20.3 in 2019. Since the copayment is 20% of the total price, it suggests that the price for a non-hospitalized ED visit is €101.5 in average. This report is available from https://www.securite-sociale.fr/files/live/sites/SSFR/files/medias/CCSS/2021/Rapport%20CCSS-Septembre2021.pdf

of the level of cost-sharing and the presence of direct payments must be separated to analyze the results of the literature.

Concerning the effect of insurance coverage, evidence from the 2006 Massachusetts health insurance reform show that the introduction of new or better coverage for out-patient care decreased the number of ED visits per capita by 5.2% to 8.4% [29]. This result support the existence of a substitution between EDs and out-patient care because of the level of cost-sharing for out-patient consultations[17]. Opposite results have been found in the Oregon Health Experiment (which expanded Medicaid eligibility). Evidence of a complementarity between emergency care and out-patient care was found among the population of low-income and uninsured individuals [43]. Finally, one study examined the indirect impact of the abolition of copayments for GP consultations of children under 6 in Ireland in 2015, and found no impact on ED overall utilization [46]. But they found that the proportion of patients coming to the EDs following the referral of a GP increased by more than 2 pp. These results do not support the existence of a substitution between EDs and out-patient care because of the presence of copayments.

To our knowledge, the pure effect of liquidity constraints for out-patient care on ED visits has not been investigated yet. In this paper, we contribute to this literature by using administrative data on French individuals' care consumption to analyze the impact of the 2017 "Tiers-Payant" reform whose main effect was to remove direct payments for out-patient care consultations. Two research questions are addressed in this study. First, we analyze the impact of removing liquidity constraints (through direct payments exemption) on the care consultations of pregnant women. Second, if any impact on care consultation, we examine the possible spillover effects of the reform on ED visits, if EDs are substitutes to out-patient care.

5 Data

We use data hosted by the National Health Data System (SNDS) whose objective is to promote access to French health databases for research, study and evaluation purposes [42]. The SNDS is managed by the French National Health Insurance (NHI), the *Caisse Nationale d'Assurance Maladie des Travailleurs Salariés (CNAMTS)* [4]. The SNDS was created in 2016 [1] and contains two main data sources: data on hospital activity coming from the *Programme de Médicalisation des Systèmes d'Information (PMSI)* and data on out-patient care from the *Système National d'Information Inter-Régimes de l'Assurance Maladie (SNIIRAM)*. The matching of these two databases is performed by the SNDS. Access to these data is limited but possible upon submission of an access request file justified by a research project.

We use data from the "*Echantillon Généraliste de Bénéficiaires Simplifié*" (EGB-S) which consists of a 1/97th sample of the SNIIRAM data. These data record the care consumption of

[17] Putting differently, this result suggest that the respective demands for emergency care and out-patient care have negative cross-price elasticities.

individuals affiliated to the French NHI[18]. The SNIIRAM consists of an exhaustive administrative dataset that aims to track all the reimbursements paid by the NHI to French beneficiaries. As big data requires large storage capacities, the SNIIRAM is only available for 2 years (plus the current year), which offers limited potential for panel data analysis on care pathways. In addition, since the purpose of these data is to record information about health care reimbursements, non-users (i.e., individuals covered by the NHI who do not consume any care during a year) are not included in the data. The EGB-S was created to overcome these limitations. This sample is representative of the age and gender distribution of the beneficiaries. It allows to observe care users and non-users for a period of 20 years from present. For these reasons, the EGB-S is preferred to other databases for longitudinal analyses.

5.1 The identification of pregnancy episodes

We identify pregnancy episodes on the basis of their outcome: the birth delivery. To do so, we use PMSI data as part of the EGB-S database. The PMSI is a comprehensive dataset measuring hospital outputs in a medical approach. It is supplied by a French classification of diseases - the *Groupe Homogene de Malades* (GHM) - inspired by the DRG's classification. We follow an algorithm developed by Blotiere et al. in 2018 [8] to select hospital admissions resulting in a birth delivery. But since the French GHM classification changed in 2012, the algorithm reported in the publication is obsolete for the period we study (2014-2018). Therefore we adapted the algorithm to the new classification based on the methodology proposed by the *Agence Technique d'Information sur l'Hospitalisation (ATIH)*[19]. For sake of comparability we select pregnancy episodes resulting in single live-births. Live births represent 73.9% of total pregnancy episodes in France and twin pregnancies represent 1.7% of live births [8]. Single live-births are identified among birth deliveries with a gestational age greater than 22 weeks of amenorrhea in order to distinguish viable births from stillbirths and abortions (elective, therapeutic or spontaneous). In administrative hospital databases, medical conditions that require hospitalizations are classified through the French GHM classification of diseases and assigned to a Principal Diagnosis, with possible report about Associated Diagnoses. To identify pregnancy episodes we select principal diagnosis code *O.80.0* which designs uncomplicated birth deliveries. We also select associated diagnoses starting with the *Z.37* code which refers to the birth delivery outcome. Since we do not want to miss birth deliveries occurring outside of the hospital, we also select hospitalizations with the associated diagnosis *Z39.00*. Finally, we select hospitalizations that report the performance of a birth delivery medical procedure in order not to miss deliveries that occurred during a hospitalization for an other diagnosis than the ones pre-cited. It also allows us to accurately date the birth delivery by using the date of the performance of the delivery procedure instead of the starting date of hospitalization. Once the date of birth delivery is precisely identified, we use information about the gestational age (delay in days or weeks since the last menstrual period)

[18] Since 2016, these data record the care consumption of 95.6% of the French population.

[19] We thank Pierre-Olivier Blotiere from the CNAMTS for is help in the adaptation of the algorithm to the new classification of diseases.

to calculate the starting date of pregnancy retrospectively. It is essential to know pregnancies' starting dates and birth delivery dates to construct a pre-treatment period (before 6 month) and a treatment period (after 6 month) to identify the time when women are exempted from direct payments. All dates are available at the daily level in the data, which enables us to reconstruct periods of payment exemptions very precisely for each pregnancy episode. Knowing the starting and ending dates of pregnancies, we generate all the days from the beginning of the pregnancy to the 12th day after birth delivery to obtain a daily panel of observations for each pregnancy episode.

We identify 36,696 single live-births deliveries during the 2014-2018 period using the algorithm. We excluded 844 pregnancy episodes with missing gestational age and 66 with a gestational age lower than 6 months. We also dropped 57 hospitalizations with a principal diagnosis and/or an associated diagnosis designing a birth delivery but reporting no delivery procedure. Finally we deleted 10 observations because the delay between two pregnancy episodes of the same woman was lower than 9 months. This selection provides a sample of 35,719 birth deliveries to be linked with data on care consumption. More details on the identification and selection process for pregnancy episodes can be found in appendix A.2.

5.2 Sample design

After having identified pregnancy episodes, we use historical data on individuals' affiliations to the French NHI from the EGB-S sample. We use this information to design a sample of pregnant women whose care consumption is observed continuously during their pregnancy episode taking place between the 1st of July 2014 and the 30th of June 2018.

We exclude 93 pregnancy episodes who leave the EGB-S sample during their pregnancy (for instance because they move to a foreign country, or because they switch to another NHI plan which is not included in the EGB-S). Also, selecting observations between the 1st of July 2014 and the 30th of June 2014 (for balance purpose of the panel between pre and post treatment periods) leads to the exclusion of 3,364 pregnancies[20]. At this stage of the selection process we observe 32,262 pregnancy episodes for 27,861 women between mid-2014 and mid-2018. We attribute out-patient care consultations and ED visits to each pregnancy episodes at the day when the care was consumed. Then we aggregate data at the week-of-pregnancy level for the purpose of an "event-study" analysis. We center weeks of pregnancy on the first week of the treatment period, so that weeks of pregnancies are measured as the distance to the beginning of the treatment, i.e., of the direct payment abolition period (at 6 months of pregnancy) [21]. The

[20]The direct payment abolition reform impacted pregnant women over 6 months of pregnancy until day 12 after the birth delivery (see section 2 for details). Since pregnant women are identified on the basis of the birth delivery, treatment period is over-represented at the beginning of the first year of the panel (2014). Conversely, pre-treatment period is under-represented in the end of the last year of the panel (2018). To avoid this issue, we start the panel on the 1st of July 2014 and ends it on the 30th of June 2018.

[21]In the sample used for the analysis, the average number of weeks of pregnancy observed are 24.5 (standard

advantage of this dataset is the comprehensiveness of the care consumption during pregnancy episodes, that enables to run a panel analysis. However, data contain very few information on socio-demographic characteristics of women. We only observe age, gender, whether women are affiliated to the CMU-C or not, and their place of residence (city and *département*) [22] To compensate for this lack of data, We add data on physician's density at the pregnant women's *département* level. It will control for the provision of care around women's places of residence in further analysis. Unfortunately, information on women's places of residence is missing for a large part of the panel. Deleting observations with missing values results in the exclusion of 12,704 pregnancy episodes. We exclude women whose age is lower than 18 years at the date of birth delivery leading to 168 pregnancy episodes deletions. We also exclude the birth delivery period since out-patient consultations and ED visits occurring during this period are very unlikely to be sensitive to liquidity constraints[23]. This exclusion leads to the loss of 264 pregnancy episodes. Because we want to ensure the robustness of a panel data analysis that introduces individuals' and time fixed effects, we only keep pregnancy episodes observed at least 4 consecutive weeks (317 pregnancy episodes excluded). The final sample used in this study contains 18,809 pregnancy episodes belonging to 16,811 women observed between the 1st of July 2014 and the 30th of June 2018. The total number of observations is 604,070.

6 Empirical strategy and method

In this paper, we examine (i) the existence of liquidity constraints for care consumption in France; (ii) the existence of a substitution between ED visits and out-patient care to avoid the payment of a consultation. To do so, we analyze the effects of the direct payment abolition reform of 2017 ("Tiers-Payant") on (a) the number of out-patient care consultations, and (b) the number of ED visits of pregnant women between 2014 and 2018. If some pregnant women were liquidity constrained by the price of a medical consultation before the reform, we should observe an increase in the number of out-patient care consultations. Also, if these women were using EDs are substitutes to out-patient care, the reform should decrease the number of ED visits. In order to test for these two hypotheses, we implement a difference-in-difference-in-differences strategy, or triple differences (DDD). In what follows, we detail the triple difference estimator and its identifying assumption. This estimator is rather intuitive since it can be computed as the difference between two difference-in-differences (DD) estimators.

6.1 Identification of the reform impact

The "Tiers-Payant" reform was introduced on the 1st January 2017 in France. It removed direct payments for all the care consumed by pregnant women between the 6th month of pregnancy and the 12th day after the birth delivery. Since care recommended by the NHI was already

deviation=4.6) during the control period and 9.9 (standard deviation=2.9) during the treatment period.

[22] The *département* is a French administrative division of the territory smaller than the region.

[23] Excluded period start from the week before the week of the birth delivery.

exempted from direct payments before the reform (see table A7 in appendix A.8), it only affected the care that is consumed on top of the recommended care. For a given pregnancy episode p of a pregnant woman i, two periods are observed: a control period ($T = 0$) before 6 months of pregnancy, and a treatment period ($T = 1$) after 6 months of pregnancy (and until day +12 after birth delivery). There is no treatment before the 1st of January 2017, whatever the stage of pregnancy. Let us introduce a dummy variable $Post_{2017} = 1$ if care is consumed on a year $t \geq 2017$ posterior to the reform, and $Post_{2017} = 0$ otherwise. In addition, the population of pregnant women can be subdivided into two groups of French NHI beneficiaries: a group that benefits from a free public CHI (CMU-C) which ensures no direct payments and no cost-sharing for all care consumed before and after the 2017 reform and at any time during the pregnancy (and not only during the pregnancy), and a group that does not benefit from the free CHI CMU-C. In other words, pregnant women who benefit from the "CMU-C" are always untreated, and remain unaffected by the reform since direct payments and cost-sharing were already removed for all the care they consumed before 2017. Therefore we create a third dummy variable $\mathbb{1}_{g=1}$ to designate the group of women who were affected by the reform after 2017. Thus CMU-C beneficiaries are denoted by $g = 0$ and other pregnant women who do not benefit from the public CHI are denoted by $g = 1$.

Let us use the potential outcomes framework as introduced in the Rubin causal model[24]. Consider Y_{1igt} the potential outcome of a pregnant woman i who belongs to the group g of beneficiaries at time t if treated by the reform. Conversely, consider Y_{0igt} the potential outcome of a same pregnant woman i belonging to the group g of beneficiaries at time t if not treated by the reform. Thus Y_{0igt} is the counterfactual of Y_{1igt} and represents in the context of this paper the care that would have been consumed by a pregnant women i in the absence of the direct abolition payment policy. We call it “potential” outcomes because we cannot observe Y_{1igt} and Y_{0igt} simultaneously. It might seem convenient to propose a difference-in-differences strategy and compare the differences in care consumption during the treatment period and the control period, before and after the reform . But one can reasonably doubt about the consistency of the DD estimator[25]. Estimates would be biased if some structural changes occurred concomitantly with the reform (for instance in the medical guidelines regarding the follow-up of pregnancies at hospital). To eliminate this source of bias, we propose a triple-differences estimator to assess the causal impact of the reform on the following outcomes: (i) Medical consultations (GP, gynecology, midwifery) and (ii) ED visits. We compare the differential - before *vs* after the reform - in the outcomes of (affected) group $g = 1$ and (unaffected) group $g = 0$ during the treatment period to the differential in the outcomes of group $g = 1$ and group $g = 0$ in the control period. Note that using CMU-C beneficiaries (g=0/1) to build a DDD estimator comes down to suppose that CMU-C beneficiaries have access to the same quality of care than other patients, which is

[24]For a modern presentation of this model, see Angrist and Pischke’s book [6].

[25]This estimator relies on the following assumption called the "common trend assumption" which might not hold in practice: $(E[Y_{0it}|T = 1, Post_{2017} = 1] - E[Y_{0it}|T = 1, Post_{2017} = 0]) = (E[Y_{0it}|T = 0, Post_{2017} = 1] - E[Y_{0it}|T = 0, Post_{2017} = 0])$

a reasonable assumption for France.

So we can define δ as the "true" causal average treatment effect on the treated of the reform :

$$\begin{aligned} \delta &= E[Y_{1igt} - Y_{0igt} | T = 1, g = 1, Post = 1] \\ &= \delta_1 - \delta_0 \end{aligned} \tag{1}$$

Where $\delta_1 = E[Y_{1igt} | T = 1, g = 1, Post = 1]$ designs the potential outcome of a treated woman affected by the reform, and $\delta_0 = E[Y_{0igt} | T = 1, g = 1, Post = 1]$ denotes what would have been observed for the same woman in the absence of the reform.

The DDD estimator implemented to assess the causal impact of the reform is:

$$\begin{aligned} \hat{\beta} = \{&([E(Y_{1igt} | T = 1, g = 1, Post = 1)] - [E(Y_{0igt} | T = 1, g = 1, Post = 0)]) \\ &- ([E(Y_{0igt} | T = 0, g = 1, Post = 1)] - [E(Y_{0igt} | T = 0, g = 1, Post = 0)])\} \\ &- \\ \{&([E(Y_{0igt} | T = 1, g = 0, Post = 1)] - [E(Y_{0igt} | T = 1, g = 0, Post = 0)]) \\ &- ([E(Y_{0igt} | T = 0, g = 0, Post = 1)] - [E(Y_{0igt} | T = 0, g = 0, Post = 0)])\} \end{aligned} \tag{2}$$

$\hat{\beta}$ is a consistent estimator of the “true” effect of the reform δ if the following assumption is verified:

$$\begin{aligned} \{&([E(Y_{0igt} | T = 1, g = 1, Post = 1)] - [E(Y_{0igt} | T = 1, g = 1, Post = 0)]) \\ &- ([E(Y_{0igt} | T = 0, g = 1, Post = 1)] - [E(Y_{0igt} | T = 0, g = 1, Post = 0)])\} \\ &= \\ \{&([E(Y_{0igt} | T = 1, g = 0, Post = 1)] - [E(Y_{0igt} | T = 1, g = 0, Post = 0)]) \\ &- ([E(Y_{0igt} | T = 0, g = 0, Post = 1)] - [E(Y_{0igt} | T = 0, g = 0, Post = 0)])\} \end{aligned} \tag{3}$$

The DDD estimator does not rely on the assumption of "parallel trends" in the outcomes of the untreated and the treated in the absence of treatment. The DDD assumption presented in equation (3) supposes that the differential in the outcomes of group $g = 1$ and group $g = 0$ in the treatment period would have evolved similarly to the differential in the outcomes of group $g = 1$ and group $g = 0$ in the control period, in the absence of the reform. In practice, this assumption

Table 1: Number of consultations and ED visits depending on whether pregnant women were affected by the reform or not

	(1) All	(2) Before 2017	(3) Before 2017	(4) After 2017	(5) After 2017
		Before 6 month	After 6 month	Before 6 month	After 6 month
	Sum	Sum	Sum	Sum	Sum
			Public CHI (CMU-C): No		
GP consultations					
Total	49,252	25,310	6,888	13,393	3,661
Out-patient care outside hospital	44,183	23,118	5,849	12,101	3,115
Hospital out-patient care	5,069	2,192	1,039	1,292	5,46
Gynecological consultations					
Total	58,255	24,322	13,975	12,695	7,263
Out-patient care outside hospital	41,643	18,640	8,797	9,693	4,513
Hospital out-patient care	16,612	5,682	5,178	3,002	2,750
Midwife consultations					
Total	41,153	9,939	14,838	7,180	9,196
Out-patient care outside hospital	9,191	3,415	1,745	2,754	1,277
Hospital out-patient care	31,962	6,524	13,093	4,426	7,919
Emergency Department visits					
Total	7,462	3,453	1,217	2,091	701
Hospitalized	914	260	311	146	197
Non-hospitalized	6,548	3,193	906	1,945	504
			Public CHI (CMU-C): Yes		
GP consultations					
Total	18,095	9,272	2,528	4,951	1,344
Out-patient care outside hospital	16,026	8,320	2,100	4,432	1,174
Hospital out-patient care	2,069	952	428	519	170
Gynecological consultations					
Total	12,716	5,086	3,018	2,849	1,763
Out-patient care outside hospital	7,841	3,354	1,607	1,892	988
Hospital out-patient care	4,875	1,732	1,411	957	775
Midwife consultations					
Total	11,871	3,179	3,903	2,078	2,711
Out-patient care outside hospital	2,201	870	337	687	307
Hospital out-patient care	9,670	2,309	3,566	1,391	2,404
Emergency Department visits					
Total	3,304	1,580	549	886	289
Hospitalized	443	130	147	75	91
Non-hospitalized	2,861	1,450	402	811	198
CMU-C: No					
N	475,282	219,957	90,606	116,547	48,172
Pregnancies	14,830	11,015	9,485	5,603	5,123
Women	13,228	10,422	9,125	5,553	5,083
CMU-C: Yes					
N	128,788	59,221	23,118	32,447	14,002
Pregnancies	3,979	2,939	2,455	1,561	1,491
Women	3,583	2,751	2,345	1,542	1,476

Sources: Author's calculations from the EGB-S database.
Notes: Beneficiaries of the CMU-C were unaffected by the reform. Conversely, pregnant women who are not beneficiaries of the CMU-C were affected by the reform. Column 1 reports the total number of consultations and ED visits in the sample for the whole period of observation (mid-2014 to mid-2018). Column 2 (*resp.* 4) reports consultations and visits consumed during the control period (before 6 months of pregnancy) before (*resp.* after) the reform (2017). Column 3 (*resp.* 5) reports consultations and visits consumed during the treatment period (after 6 months of pregnancy) before (*resp.* after) the reform.

cannot be tested since we do not observe the potential outcomes of a woman if treated and if not treated by the reform at the same time. But it is possible to propose a "visual check" of trends in treatment period and control period in groups $g = 0$ and $g = 1$ before the reform in order to assess whether the evolution is similar or not. Also, we will propose an "event-study" analysis where the event is not exactly a period of time but a pregnancy week. It will enable to partially test for the DDD assumption by estimating whether the differential in the outcomes of groups $g = 1$ and $g = 0$ evolved similarly during the control period (before 6 months of pregnancy) after the reform.

Table 2: Differences in means for pregnancy characteristics and care consumption between affected (no CMU-C) and unaffected women (CMU-C).

	(1)	(2)	(3)	(5)	(6)
	All	Before 2017		After 2017	
		Before 6 months	After 6 months	Before 6 months	After 6 months
	diff.	diff.	diff.	diff.	diff.
Pregnancy characteristics					
Mother's age	1.972***	2.026***	2.111***	1.894***	1.696***
Pregnancy duration (in weeks)	0.095***	0.053***	-0.026	0.237***	0.142***
N. of observed pregnancy weeks	-0.097***	0.083***	-0.304***	-0.303***	0.164***
GP consultations					
Total	-0.037***	-0.041***	-0.033***	-0.038***	-0.02***
Out-patient care outside hospital	-0.031***	-0.035***	-0.026***	-0.033***	-0.019***
Sector 1	-0.035***	-0.039***	-0.028***	-0.036***	-0.022***
Sector 2	0.002***	0.002***	0.002***	0.001***	0.002***
Hospital out-patient care	-0.005***	-0.006***	-0.007***	-0.005***	-0.001
Gynecological consultations					
Total	0.024***	0.025***	0.024***	0.021***	0.025***
Out-patient care outside hospital	0.027***	0.028***	0.028***	0.025***	0.023***
Sector 1	0.004***	0.005***	0.002	0.005***	0.001
Sector 2	0.023***	0.023***	0.026***	0.02***	0.022***
Hospital out-patient care	-0.003***	-0.003***	-0.004**	-0.004***	0.002
Midwife consultations					
Total	-0.006***	-0.008***	-0.005	-0.002	-0.003
Out-patient care outside hospital	0.002***	0.001	0.005***	0.002**	0.005***
Hospital out-patient care	-0.008***	-0.009***	-0.01***	-0.005***	-0.007*
Emergency Department visits					
Total	-0.01***	-0.011***	-0.01***	-0.009***	-0.006***
Hospitalized	-0.002***	-0.001***	-0.003***	-0.001***	-0.002***
Non-hospitalized	-0.008***	-0.01***	-0.007***	-0.008***	-0.004***
CMU-C: No					
N	475,282	219,957	90,606	116,547	48,172
Pregnancies	14,830	11,015	9,485	5,603	5,123
Women	13,228	10,422	9,125	5,553	5,083
CMU-C: Yes					
N	128,788	59,221	23,118	32,447	14,002
Pregnancies	3,979	2,939	2,455	1,561	1,491
Women	3,583	2,751	2,345	1,542	1,476

Sources: Author's calculations from the EGB-S database.
Notes: *p<0.1, **p<0.05, ***p<0.01. This table reports the difference in means of care consumption between pregnant women in group $g = 1$ (not beneficiaries of the CMU-C) and pregnant women in group $g = 0$ (beneficiaries of the CMUC-C). The p-value associated with the test of equality in means is reported next to the value of difference through the following legend: *p<0.1, **p<0.05, ***p<0.01. Column (1) reports differences in means for the whole sample. Columns (2) and (3) reports differences in means for the pre-reform period, while columns (5) and (6) reports same statistics for the post-reform period. For each of these periods, differences in means are estimated for the control period of pregnancy (before 6 months) when direct payments are not exempted (columns (2) and (5)), and for the treatment period (after 6 months) when direct payments are exempted (columns (3) and (6)).

Of 18,809 pregnancy episodes experienced by 16,811 women, 3,979 belong to 3,583 women who benefit from the free CHI "CMU-C" during their pregnancy (see table A4). In this paper, we are interested in several outcomes. First, the out-patient care consultations of pregnant women which includes GP consultations, gynecology consultations and midwife consultations. Second, the ED visits of pregnant women. A distinction is made between hospitalized and non-hospitalized ED visits because substitutes to out-patient consultations are more likely to be non-hospitalized ED visits. All outcomes are measured at the week of pregnancy level. Table A5 reports the total

number of consultations and ED visits observed in the sample, by treatment period before and after the reform. Table 1 presents the same statistics depending on the group of beneficiaries (CMU-C or not). One can see that 67,347 GP consultations are observed between mid-2014 and mid-2018 in the sample. Of these consultations, 49,952 were consumed by pregnant women in the affected group $g = 1$, and 18,095 by women in unaffected group $g = 0$ (CMU-C beneficiaries).

The majority of ED visits are non-hospitalized visits because in our sample many hospitalized ED visits correspond to the admission of pregnant women who are about to give birth. Since we excluded the period of the birth delivery, we also excluded the majority of hospitalized ED visits. Interestingly, the majority of hospitalized ED visits take place after 6 months of pregnancy while the majority of non-hospitalized ED visits take place before 6 months of pregnancy. In all, 10,766 ED visits are observed, of which 3,304 are made by CMU-C beneficiaries.

Table A6 show weekly means of care consumption outcomes by treatment period (and the associated difference in means), before and after the reform. The average age of mothers in the sample is 30, with a standard deviation of 5. The average duration of a pregnancy episode is 37 weeks (8 months and a half). In average, it corresponds to the number of weeks we observe in the data. The average number of GP consultations remained stable before and after the reform in both periods. In all, the average number of GP consultations is 0.1 per week, but the number of consultations decreases during the pregnancy (the weekly mean is 0.12 before 6 months and 0.07 after 6 months). The average number of gynecology consultations is 0.11 per week. A 0.01 increase in mean is observed in the treatment period after the reform. Midwife consultations increased in both treatment and control periods after the reform. There were 0.06 consultations per week during the control period and 0.20 consultations per week during the treatment period after 2017. Turning to emergency care, the overall probability to have an ED visit during a week of pregnancy is 2.4%.

Table 2 now reports differences in means of care consumption between pregnant women affected by the reform (non CMU-C) and pregnant women unaffected by the reform (CMU-C). The difference in the average number of consultations per week (and its significance at the 95% confidence interval) is calculated for the whole sample and by treatment periods, before and after the reform. The differences reported show that women who are not covered by the CMU-C (group $g = 0$) are 2 years older in average. They have less GP consultations than CMU-C beneficiaries, except for consultations with a GP who is allowed to charge patients with balance billings (sector 2). In contrast, they use more gynecological consultation than CMU-C beneficiaries. Also, women affiliated to the CMU-C use more emergency care than other women in average. These results are consistent with previous findings showing that CMU-C beneficiaries have higher health expenditure than the rest of the population because they are in poorer health [38, 24].

We present the trends in care consumption of pregnant women by pregnancy weeks and by group of beneficiaries for (i) primary care consultations (figures 5, 6, 7) and (ii) emergency care (figure 8). It shows the comparability of care consumption trends (regardless the outcome that is picked) between pregnant women affiliated to a public CHI (figures on the right-side) and pregnant women who are not (figures on the left-side). The trends before *versus* after the reform in both the control and treatment periods are very similar, except for GP consultations and midwife consultations. A decrease in the number of GP consultations is observed after the reform in the treatment period for beneficiaries of the CMU-C. Also, it seems that the number of consultations with a midwife slightly increased after the reform in both groups. But this increase is mainly observed during the treatment period for CMU-C beneficiaries.

6.2 Estimation of the reform impact

To estimate the impact of the direct payment reform abolition, we first implement an analysis similar to an event-study analysis where the event is not a time period but a pregnancy week. An event-study analysis allows to estimate the (possible) heterogeneity of the reform impact over time, which here corresponds to the impact of the direct payment abolition reform by weeks of pregnancy, for the group of affected women (no CMU-C), in comparison with the group of unaffected women (CMU-C). This specification has several advantages.

First, it enables us to control for the evolution of the pregnancy episode by looking at the (possible) heterogeneous effects of the reform over weeks of pregnancy. Second, it also allows to track the care consumption of pregnant women in affected and unaffected groups of beneficiaries (CMU-C or not) over weeks of pregnancy and to compare the differential effects. Third, it gives a test of the identifying assumption of the triple difference estimator by comparing the significance of the pre-reform trends between affected and unaffected pregnancy episodes. The model estimates the impact of the reform on care consumption across 38 weeks of pregnancy and two groups of pregnant women.

We denote Y_{ipgwq} the care consumption (primary care or emergency care) of a pregnant woman i belonging to group g observed each week w of a quarter q during a pregnancy episode p. We assign to each week of pregnancy an event-time w ranging from -26 to 11 (38 weeks of pregnancy maximum), with week -2 before the abolition of direct payments as reference pregnancy-week event. We assigned week -2 since the identification of starting date of pregnancy - and thus of the 6th month of pregnancy - can be subject to small variations because of unobserved variations in the date of conception. By choosing week -2 as reference pregnancy-week event, we ensure the reference to be in the control period.

We estimate the following regression by Ordinary Least Square (OLS):

$$
\begin{aligned}
Y_{ipgwq} = & \left\{ \sum_{w=-26}^{-3} \delta^{0}_{w,post,cmu:no}(\mathbb{1}_{g=1} \cdot Post_{2017}) + \sum_{w=-1}^{11} \delta^{1}_{w,post,cmu:no}(\mathbb{1}_{g=1} \cdot Post_{2017}) \right\} \\
& + \sum_{w=-26}^{-3} \delta^{0}_{w,cmu:no}(\mathbb{1}_{g=1}) + \sum_{w=-1}^{11} \delta^{1}_{w,cmu:no}(\mathbb{1}_{g=1}) + \sum_{w=-26}^{-3} \delta^{0}_{w,post}(Post_{2017}) \\
& + \sum_{w=-1}^{11} \delta^{1}_{w,post}(Post_{2017}) + \delta_w + bW^{'}_{ipgwq} + \alpha_i + \gamma_q + e_{ipgwq}
\end{aligned} \tag{4}
$$

Here, $\mathbb{1}_{g=1}$ is a binary indicator equal to one if the woman is not covered by the CMU-C, and $Post_{2017}$ is a binary variable equal to one after 2017, 0 otherwise. So the interaction between these two variables ($\mathbb{1}_{g=1} \cdot Post_{2017}$) is equal to one for pregnant women who are not beneficiaries of the CMU-C after the reform.

Equation (4) specifies an event-study analysis, so the impact of being affected by the reform is estimated by week of pregnancy. We introduce pregnancy week dummies δ_w which control for the trend in care consumption of unaffected women (CMU-C) before the reform. The coefficients of interest in this event-study are the $\delta^{1}_{w,post,cmu:no}$ since they estimate, by week of pregnancy, the impact of the reform on affected pregnant women (non-beneficiaries of the CMU-C) in the treatment period (after 6 month of pregnancy). Coefficients $\delta^{0}_{w,post,cmu:no}$ estimate the differential in care consumption between non CMU-C and CMU-C beneficiaries during the control period, after 2017, by pregnancy week. Then, variable $\mathbb{1}_{g=1}$ interacted with pregnancy week dummies controls for the differential trend in outcomes between non CMU-C and CMU-C beneficiaries before the reform, and variable $Post_{2017}$ interacted with pregnancy week dummies control for the post-reform trend in the outcomes of CMU-C beneficiaries.

$W^{'}_{ipgwq}$ control for a set of covariates including the age of pregnant women, medical density in women's *département* of residence and the number of observed weeks of the pregnancy episode. Individual's fixed effects α_i are introduced, as well as control for quarterly macroeconomic shocks γ_q. Standard errors are clustered at the pregnant woman level to correct for heteroskedasticity between two pregnancies.

We finally estimate an average treatment effect on women affected by the reform:

$$
\begin{aligned}
Y_{ipgwq} = & \beta(T_w \cdot \mathbb{1}_{g=1} \cdot Post_{2017}) + b_1 Post_{2017} + b_2 T_w + b_3 \mathbb{1}_{g=1} \\
& + b_4(Post_{2017} \cdot T_w) + b_5(T_w \cdot \mathbb{1}_{g=1}) + b_6(Post_{2017} \cdot \mathbb{1}_{g=1}) \\
& + b_7 Post_{2016} + b_8(Post_{2016} \cdot \mathbb{1}_{g=1}) + b_9(T_w \cdot \mathbb{1}_{g=1} \cdot Post_{2016}) \\
& + b_{10} W^{'}_{ipgwq} + \alpha_i + \delta_w + \gamma_q + e_{ipgwq}
\end{aligned} \tag{5}
$$

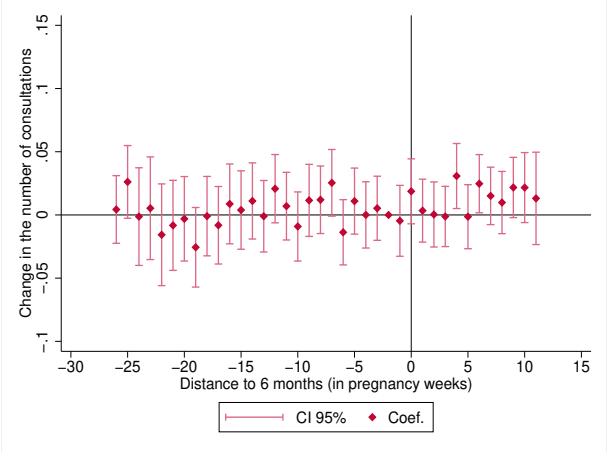
In the above equation, β is the parameter of interest. The triple interaction of variables T (the treatment period, i.e., after the 6th month of pregnancy), $\mathbb{1}_{g=1}$ (affected women who are not beneficiaries of the CMU-C) and $Post_{2017}$ (dummy for the post-reform period) corresponds to the triple difference estimator. The associated coefficient β measures the estimated average effect of the reform on the number of medical consultations and ED visits of a pregnant woman affected by the reform. The model introduces all the interactions terms that form the DDD estimator. In addition, we control for pre-reform confounding factors that could arise from the possibility allowed by the French government to abolish direct payments for pregnant women and LTD patients since the 1st of July 2016 on a voluntary basis. The same controls as for the event-study analysis are introduced in the variable W'_{ipgwq}.

7 Results

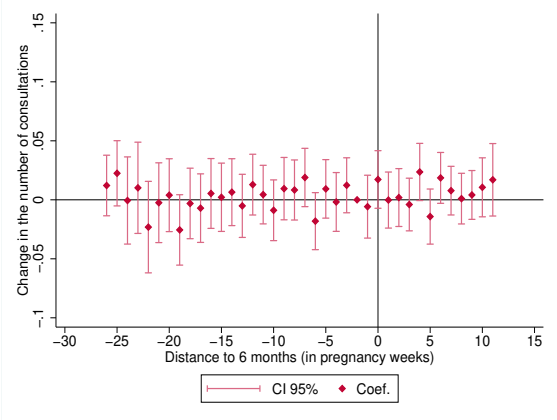
7.1 On primary care consultations

Results of the estimation of equation (4) by OLS on the number of out-patient care consultations are reported in several graphs. Figures 1 to 3 report the results of the event-study analysis. It provides a visualisation of the impact of removing direct payments on the care consumed by pregnant women who are not beneficiaries of the CMU-C (i.e., affected by the reform), by weeks of pregnancy. The event-study estimates as many impacts of the reform as the number of weeks (38). The average number of out-patient consultation by weeks of pregnancy is 0.1 (see table A6). In these graphs, weeks of pregnancy are displayed on the x-axis. They are expressed as the delay since the first week of the 6th month of pregnancy, when direct payments are removed. So negative values of the x-axis represent pregnancy weeks in the control period (before 6 months) and positive values represent pregnancy weeks in the treatment period (after 6 months). Since the reform impacted the care consumed after 6 months of pregnancy, the coefficients of interest are those on the right of the vertical bar (i.e., coefficients $\delta^1_{w,post,cmu:no}$ in equation (4)). They are displayed with their associated confidence intervals so that one can easily assess if a coefficient is significant or not. A significant coefficient is interpreted as a significant difference in the number of consultations between non CMU-C and CMU-C beneficiaries after the reform, in comparison with the difference in the number of consultations between those two groups before the reform, for a given week of pregnancy. So if the reform had an impact, a significant coefficient should be observed during the treatment period, when consultations are exempt from direct payments.

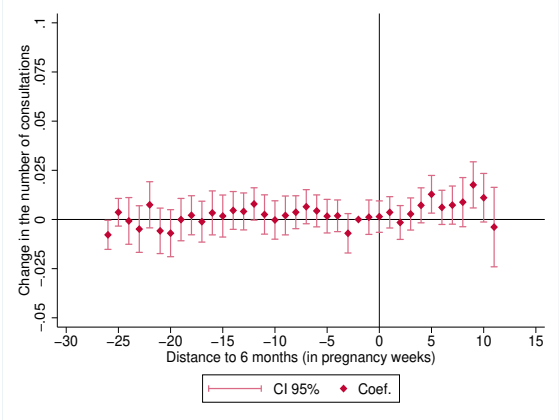
Figure 1: Event-study analysis of the reform impact on the number of GP consultations by weeks of pregnancy



(a) Total GP consultations



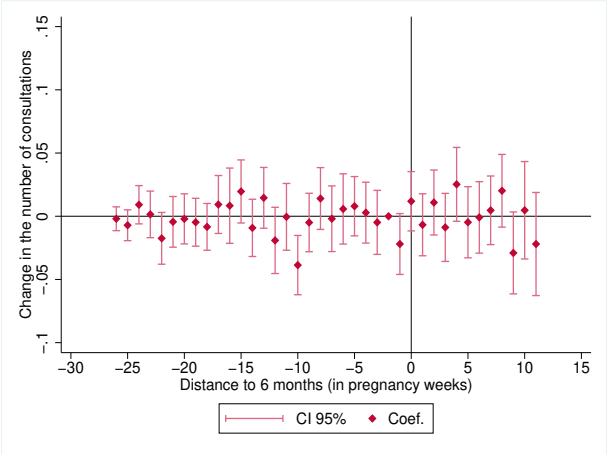
(b) Outpatient GP consultations



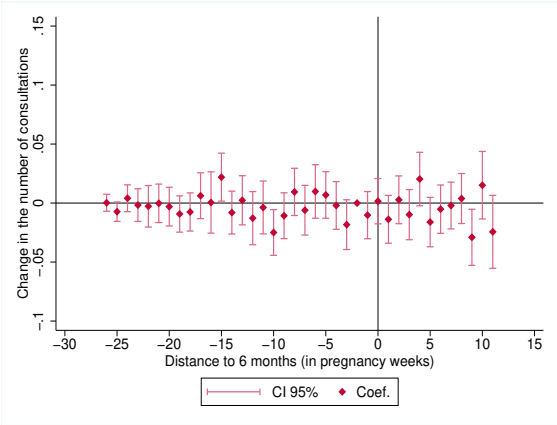
(c) Hospital outpatient GP consultations

Sources: Author's calculation from EGB-S.
Notes: Coefficients of the estimation of the 2017 reform impact on affected pregnant women (non beneficiaries of the CMU-C) by weeks of pregnancy, and their associated confidence intervals (CI) are reported on this graph. Results are obtained from the estimate of equation (4) by OLS. Standard errors are clustered at the individual (pregnant woman) level. Weeks of pregnancy are displayed on the x-axis as a distance (in weeks) from the first week of the 6th month of pregnancy (week=0).

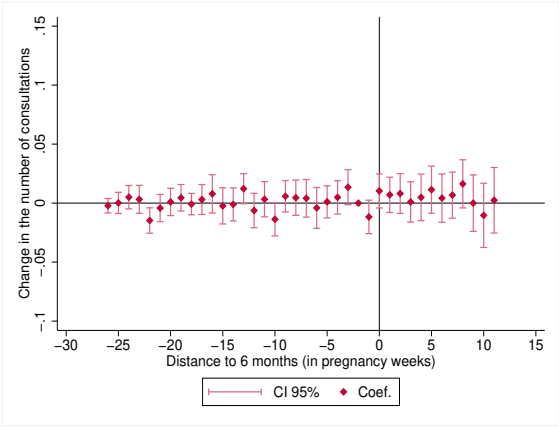
Figure 2: Event-study analysis of the reform impact on the number of gynecological consultations by weeks of pregnancy



(a) Total gynecological consultations



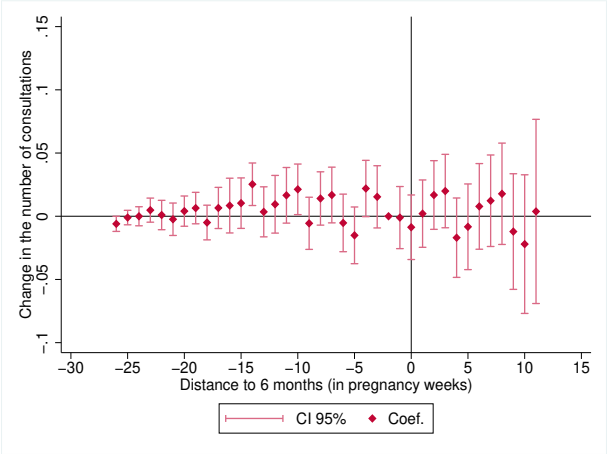
(b) Outpatient gynecological consultations



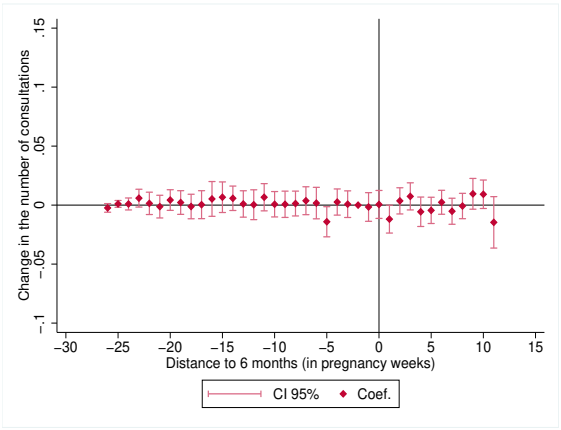
(c) Hospital outpatient gynecological consultations

Sources: Author's calculation from EGB-S.
Notes: Coefficients of the estimation of the 2017 reform impact on affected pregnant women (non beneficiaries of the CMU-C) by weeks of pregnancy, and their associated confidence intervals (CI) are reported on this graph. Results are obtained from the estimate of equation (4) by OLS. Standard errors are clustered at the individual (pregnant woman) level. Weeks of pregnancy are displayed on the x-axis as a distance (in weeks) from the first week of the 6th month of pregnancy (week=0).

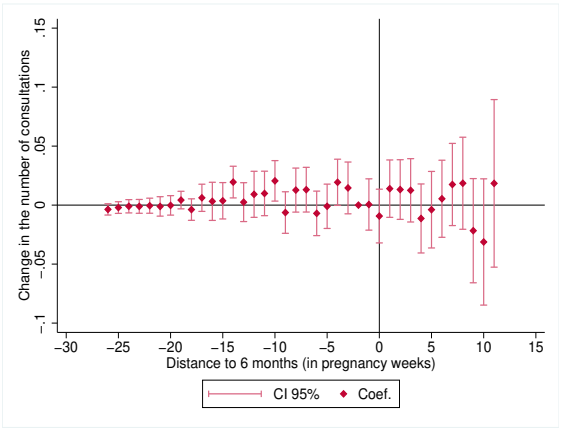
Figure 3: Event-study analysis of the reform impact on the number of midwife consultations by weeks of pregnancy



(a) Total midwife consultations



(b) Outpatient midwife consultations



(c) Hospital outpatient midwife consultations

Sources: Author's calculation from EGB-S.
Notes: Coefficients of the estimation of the 2017 reform impact on affected pregnant women (non beneficiaries of the CMU-C) by weeks of pregnancy, and their associated confidence intervals (CI) are reported on this graph. Results are obtained from the estimate of equation (4) by OLS. Standard errors are clustered at the individual (pregnant woman) level. Weeks of pregnancy are displayed on the x-axis as a distance (in weeks) from the first week of the 6th month of pregnancy (week=0).

Figure 1 presents the results on the number of GP consultations. Figure 2 shows the impact of the reform on the number of gynecological consultations. Figure 3 reports estimates of the reform impact on the number of midwife consultations. Among them, the sub-figures report the impact of the reform for out-patient consultations taking place outside a hospital (sub-figures (b)) and out-patient consultations taking place within a hospital (sub-figures (c)). Sub-figures (a) presents the impact of the triple difference estimation by weeks of pregnancy on the total number of out-patient consultations. Results show no significant impact of the direct payment abolition reform on the total number of GP consultations, except for weeks 4 and 6. But there is no clear increasing trend. Also, we find no effect of the reform on the number of GP consultations delivered outside hospital in general practices (sub-figure 1b). However, we find an increase in the number of GP consultations taking place at hospital (sub-figure 1c) of affected pregnant women after 6 months of pregnancy, following the reform. We find no impact of the reform on

the number of gynecological consultations, by weeks of pregnancy (figure 2a). We obtain similar results for midwife consultations (figure 3a).

In order to estimate an average effect of the reform, we differentiate only two periods: the control period (before 6 months of pregnancy) and the treatment period (after 6 months). This model is estimated by equation 5. Results of the triple difference estimation reported in table 3 reveal a positive impact of the abolition of direct payments on the number of GP consultations. In other words, the number of GP consultations of affected pregnant women that took place at hospital increased after the reform. In all, the exemption of direct payments for GP consultations led to an increase of 0.016 consultations per week by pregnant woman. Putting differently, this result suggests an increase of 6.4 GP visits per month of pregnancy for 100 pregnant women. The effect is small but significant (it corresponds to 1/20 standard deviation of the total number of GP consultations per pregnancy week, (see table A6)). It suggests that it concerns only a small proportion of pregnant women. It is not surprising since only a part of the population is subject to liquidity constraints.

Table 3: Triple difference estimates of the reform impact on the number of medical consultations of pregnant women

	(1) Total consultations	(2) Outpatient consultations outside hospital	(3) Hospital outpatient consultations
Outcome A: General Practitioner (GP) consultations			
Average Treatment Effect on the Treated	0.016436**	0.008596	0.00784***
	(0.00824)	(0.00775)	(0.00277)
R-Square	0.1075	0.1095	0.0807
Outcome B: Gynecological consultations			
Average Treatment Effect on the Treated	-0.0071	-0.00454	-0.00256
	(0.00816)	(0.00677)	(0.00471)
R-Square	0.087	0.1095	0.0952
Outcome C: Midwife consultations			
Average Treatment Effect on the Treated	-0.02059***	-0.00541	-0.01518**
	(0.00735)	(0.00339)	(0.00657)
R-Square	0.1466	0.1247	0.1536
Number of observations	604,070	604,070	604,070
Number of pregnancies	18,809	18,809	18,809
Number of women	16,811	16,811	16,811

Sources: Author's calculations from the EGB-S database.
Notes: *p<0.1, **p<0.05, ***p<0.01. This table reports the coefficient (standard errors in parentheses) of the triple difference estimator, measuring the impact of the direct payment abolition reform on the number of out-patient consultations of pregnant women affected by the reform (who are not beneficiaries of the CMU-C). An average treatment effect on the treated (ATET) is thus estimated. Standard errors are clustered at the individual level to account for possible heterogeneity in the unobserved characteristics of pregnant women between pregnancy episodes. Fixed effects models are estimated by OLS regressions. Regressions control for individual (pregnant woman) fixed effects as well as for specific effects by pregnancy weeks.

If we assume that 5% of the French population is liquidity constrained, it means that the reform would have resulted in an increase of 1.28 GP consultations per month. On the contrary, the abolition of direct payments for care did not significantly change the number of gynecological

consultations of pregnant women. The fact that mandatory prenatal consultations were already exempt from direct payments before the reform (see appendix A.8) might explain the absence of effect on gynecological consultations. Surprisingly, a decrease in the number of out-patient consultations with a midwife at the hospital is found for affected pregnant women following the reform. This decrease is not estimated for out-patient consultations taking place outside of the hospital.

In all, we observe an increase in the number of GP consultations, and a decrease in the number of midwife consultations of pregnant women following the direct payment exemption reform. Both of these results are found for consultations taking place at hospital. It suggests that pregnant women substituted midwife consultations to GP consultations before the reform since their price was lower (€23 for a midwife consultation *versus* €25 for a consultation with a GP). After the reform, the incentive to substitute was removed since direct payments were abolished for both types of consultations.

7.2 On emergency care

Results of the estimation of the reform impact by weeks of pregnancy on ED visits are reported in figure 4. We found no evidence of a change in the trend of ED visits of affected pregnant women after the reform during the treatment period. Also, the differential trends in ED visits between affected (no CMU-C) and unaffected (CMU-C beneficiaries) pregnant women is not significant which suggests no impact of the DDD estimator.

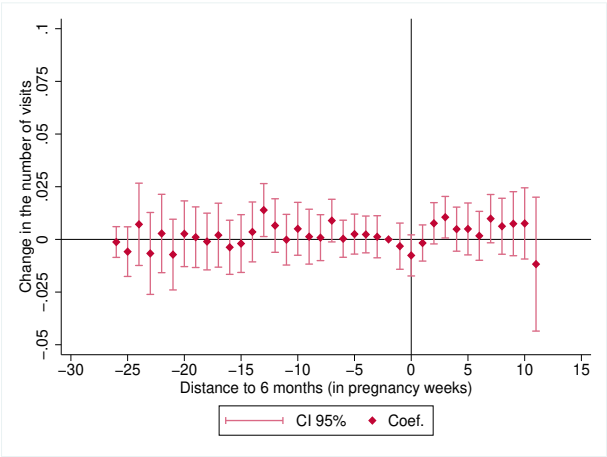
Table 4: Triple difference estimates of the reform impact on the number of ED visits of pregnant women

	(1) Total	(2) Hospitalized	(3) Non-hospitalized
Average Treatment Effect on the Treated	0.000023 (0.00352)	0.001033 (0.00121)	-0.00101 (0.00329)
R-Square	0.071	0.0427	0.0686
Number of observations	604,070	604,070	604,070
Number of pregnancies	18,809	18,809	18,809
Number of women	16,811	16,811	16,811

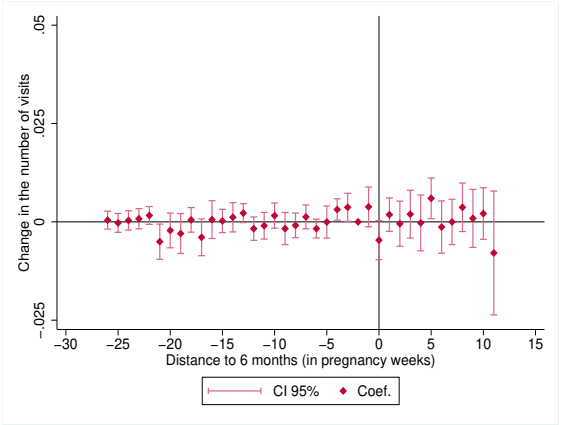
Sources: Author's calculations from the EGB-S database.
Notes: *p<0.1, **p<0.05, ***p<0.01. This table reports the coefficient (standard errors in parentheses) of the triple difference estimator, measuring the impact of the direct payment abolition reform on the number of ED visits of pregnant women affected by the reform (who are not beneficiaries of the free CHI CMU-C). An average treatment effect on the treated (ATET) is thus estimated. Standard errors are clustered at the individual level to account for possible heterogeneity in the unobserved characteristics of pregnant women between pregnancy episodes. Fixed effects models are estimated by OLS regressions. Regressions control for individual (mother) fixed effects as well as for time (weeks of pregnancy) fixed effects.

Results of the estimation of the average effect of the reform are reported in table 4. We find no significant impact of the reform on the number of ED visits, neither on hospitalized visits, nor on non-hospitalized. This result suggests that EDs were not used as substitutes to out-patient care consultations by liquidity constrained pregnant women to avoid the payment of a consultation before the reform.

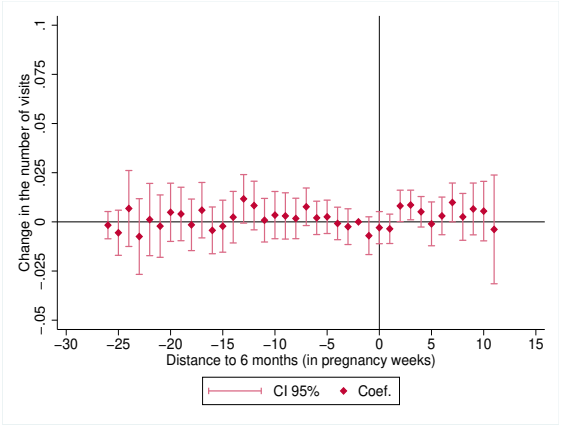
Figure 4: Event-study analysis of the reform impact on the number of ED visits by weeks of pregnancy



(a) Total ED visits



(b) Hospitalized ED visits



(c) Non-hospitalized ED visits

Sources: Author's calculation from EGB-S.
Notes: Coefficients of the estimation of the 2017 reform impact on affected pregnant women (non beneficiaries of the CMU-C) by weeks of pregnancy, and their associated confidence intervals (CI) are reported on this graph. Results are obtained from the estimate of equation (4) by OLS. Standard errors are clustered at the individual (pregnant woman) level. Weeks of pregnancy are displayed on the x-axis as a distance (in weeks) from the first week of the 6th month of pregnancy (week=0).

7.3 On pre-term births

As an additional result, we estimate the impact of the direct payment abolition reform on the probability of pre-term births. A premature birth delivery is an undesirable event because the life of the new born child is threatened by the fact that the organs did not have enough time to

develop. This can lead to long-term consequences for the child's health involving disabilities. In France, in average, there are 50,000 pre-term births each year [23]. A multiple of mechanisms are involved in a pre-term birth, including medical factors for which a treatment can be implemented [21]. It has been established that care utilization during the pregnancy was associated with a lower probability of pre-term births [7, 14, 45]. Therefore, it is of interest to study the potential effect of the "Tiers-Payant" reform on the probability of pre-term births occurrence. One could expect a decrease in this probability through two mechanisms. First, we found that the reform increased access to GPs at hospital, and decreased the use of midwife consultations. These changes in GP and midwife utilization can affect pre-term births. Second, the abolition of direct payments could have an indirect impact on pre-term births through an increase in the disposable income of pregnant women. This paper considers an impact of the reform on three types of out-patient care (GP, gynecological and midwifery). But the additional disposable income ensured by the reform can be used to consume other types of care, or other goods. For instance, the nutritional status of pregnant women is associated with premature births [21], suggesting that improving food quality could influence pre-term births. Therefore, an indirect influence of the reform on women's consumption could lead to a decrease in the risk of premature births.

In order to test these hypotheses, we keep only one observation by pregnancy episode. Thus, the sample used for this analysis contains 18,809 pregnancy episodes and 16,811 women. For each pregnancy episode, we define a binary indicator of a pre-term birth equals to one if the pregnancy episode duration is lower than 240 days (8 months), 0 otherwise. There are 749 pre-term births identified among 18,809 birth deliveries (4.0% of the sample[26] (see table A8 in appendix A.9)). We set the value of variables that varies during the pregnancy at the date of birth delivery, and we sum the number of out-patient consultations used during the pregnancy.

We estimate the impact of the reform on the probability that a pregnancy episode results in a pre-term birth using a Difference-in-Differences (DD) estimator. It consists in estimating whether the difference in the number of pre-term births between affected women (not CMU-C) and unaffected women (beneficiaries of the CMU-C) after the reform is significantly different from the difference in the number of pre-term births between affected and unaffected women before the reform. This is implemented by the estimation of equation (6) by OLS:

$$Y_{ipq} = b_1(\mathbb{1}_{g=1} \cdot Post_{2017}) + b_2\mathbb{1}_{g=1} + b_3 Post_{2017} + b_4 X'_{ipq} + \alpha_i + \mu_q + \epsilon_{ipq} \tag{6}$$

Where Y_{ipq} denotes the outcome of a mother i, during a pregnancy episode p that took place on a quarter q. We estimate this regression on two outcomes: the indicator of a pre-term birth and the duration of a pregnancy episode p in days. $\mathbb{1}_{g=1}$ is a binary indicator that equals to one if a pregnant woman is not a beneficiary of the CMU-C, 0 otherwise. Indicator $Post_{2017}$ equals to one after the reform, 0 before. Controls in variable X_{ipq} include the age of women, physicians'

[26] The rate of premature births is around 6-7% in the French population[23].

Table 5: Impact of the reform on pre-term births and pregnancy duration

	Pre-term birth		Pregnancy duration	
	(1)	(2)	(3)	(4)
Average Treatment Effect on the Treated	-0.00065	-0.00726	-1.0832	-0.6216
	(0.0309)	(0.0309)	(1.7823)	(1.7748)
Effects of the number of consultations				
GP	-	-0.00194	-	0.16731*
		(0.00155)		(0.0891)
Gynecology	-	-0.00716***	-	0.27320**
		(0.00189)		(0.1085)
Midwife	-	-0.00381**	-	0.40550***
		(0.00175)		(0.1009)
R-Square	0.9153	0.9161	0.9652	0.9657
Number of consultations	No	Yes	No	Yes
Number of pregnancies	18,809	18,809	18,809	18,809
Number of women	16,811	16,811	16,811	16,811

Sources: Author's calculations from the EGB-S database.
Notes: *p<0.1, **p<0.05, ***p<0.01. This table reports the coefficient (standard errors in parentheses) of the DD estimator. Results of the impact of the direct payment abolition reform on the probability of pre-term births (columns (1) and (2)) and the pregnancy duration in days (columns (3) and (4)) are reported. The DD estimator measures an Average Treatment Effect on the Treated (women who are not beneficiaries of the CMU-C). Standard errors are clustered at the individual level to account for possible heterogeneity in the unobserved characteristics of pregnant women between pregnancy episodes. Fixed effects models are estimated by OLS regressions. Regressions control for individual (mother) fixed effects.

densities at the *département* level and the number of out-patient consultations with a GP, a gynecologist and a midwife. As for the principal analysis, we control for the implementation of the reform for volunteer physicians in the last semester of 2016. The estimation includes individuals' fixed effects and quarterly dummies. Table 5 reports the results of the estimation of equation (6) with (column 1 and 3) and without (column 2 and 4) the number of out-patient consultations in control.

We find no evidence of a significant impact of the reform on (i) the probability of pre-term births and (ii) the duration of a pregnancy episode. Interestingly, the number of consultations with a gynecologist or a midwife prevents the risk of pre-term birth. Results show that an additional consultation with a gynecologist or a midwife is associated with a lower probability that a pregnancy results in a pre-term birth, and a longer pregnancy duration.

8 Robustness

In the main analysis of the reform impact, the triple differences' estimation relies on a control group that corresponds to the period before the 6th month of pregnancy. To ensure the robustness of the results, we propose to analyze the impact of the reform without using the shock of

the transition to the 6th month of pregnancy as an identification strategy. In fact, this strategy might suffer from two limitations. First, women in the control period (before 6 months) and women in the treatment period (after 6 months) are the same. Second, it results in the possibility for those women to anticipate the transition to the 6th month of pregnancy. So women could delay care consumption to the moment when they are exempted from direct payments. To account for this possibility, we implement a Difference-in-Differences (DD) analysis on a sample of pregnancy episodes observed exclusively during the treatment period[27] (after 6 months of pregnancy). This estimation compares the differential in care consumption of pregnant women between CMU-C (control group) and non CMU-C (treatment group) beneficiaries, before and after the reform. We estimate (i) an "event-study" to check for a difference in the pre-reform trends in outcomes between women in treatment and control groups; (ii) an average effect of the reform on the treated (women who are not beneficiaries of the CMU-C). For the purpose of the event-study analysis, we aggregate the observations by months. The event-study analysis estimates the effect of being in the treatment group (no CMU-C) by month from mid-2014 to mid-2018. There are 29 months observed before the reform (January 2017) and 17 months observed after. The sample used for the robustness is composed of 53,044 observations belonging to 17,189 pregnancy episodes and 15,539 pregnant women.

Results of the DD estimation are reported in tables A9 and A10 in appendix A.10. They are consistent with the results obtained with the triple differences estimator. Concerning the effect of the reform on out-patient consultations, the DD and the DDD estimators both estimate a very close effect on the number of GP consultations. The DD estimator finds that the reform led to an increase of 6.8 GP consultations per 100 pregnant women (table A9). In comparison, the DDD estimator found a 6.4 increase in the number of GP consultations per 100 pregnant women. However, the effect is only significant at the 10% level in the DD estimation, and the coefficient measuring the effect of the reform on the number of GP consultations at hospital is not significant in the DD estimation. Again, no impact of the reform is found on the number of gynecological consultations. Finally, consistent with the results of the DDD estimation, a significant decrease in the number of midwife consultations is found. This effect is observed for midwife consultations taking place at hospital.

Results of the event-study analysis reported in figure 9 enables to test for the reliability of the identifying assumption of the DD estimator, i.e., the "common trend" assumption. In fact, the DD estimator corresponds to the "true" causal effect of the reform only under the hypothesis that the trends in the outcomes of affected (not CMU-C) and unaffected (CMU-C) women would have evolved similarly in the absence of the reform. This assumption cannot be formally tested since we cannot observe at the same time the outcome of a woman treated by the reform, and the outcome for the same woman in the absence of the reform. But we can observe if the trends in the outcomes of affected and unaffected women evolved similarly before the reform, or not. This test

[27] This led to the exclusion of 1,620 pregnancy episodes and 1,272 women.

is given by the coefficients on the left of the vertical bar in figure 9, which estimate the difference in the number of consultations between not CMU-C and CMU-C beneficiaries by month before the reform, in comparison with the difference observed in December 2016. If coefficients before the reform are significant, it means that the common trend assumption is violated.

Results displayed in figure 9 suggest that this assumption appears to hold for midwife consultations, gynecologist consultations and ED visits (except for one or two specific months in the beginning of the period). However, the common trend assumption is less reliable for GP consultations. Of the 27 months observed before the reform (January 2017), there are 5 months for which the difference in the number of consultations between affected (not CMU-C) and unaffected (CMU-C) women is significantly lower than the difference observed just before the reform (December 2016). It might explain that an average effect of the reform on the number of GP consultations is not detected, especially for consultations taking place at hospital. But it is not a concern for the results of the DDD estimation since this estimator does not rely on the "common trend" assumption to obtain consistent estimates (see section 6).

Concerning the spillover effects of the reform, results reported in table A10 show no impact on the number of ED visits. This result is consistent with the results of the triple differences estimation. It confirms that there is no evidence of a substitution between emergency care and out-patient care to avoid the payment of a consultation in the population of pregnant women. More precisely, the use of EDs as substitutes for out-patient care is not observed among pregnant women whose liquidity constraints have been removed by the reform. Our results do not rule out the existence of a possible substitution between EDs and outpatient care for reasons other than liquidity constraints.

9 Discussion and concluding remarks

This paper assesses the impact of the French 2017 direct payment abolition reform on the out-patient care and the emergency care consumed by pregnant women. The reform removed direct payments for the care consumed by pregnant women after 6 months of pregnancy on top of the recommended prenatal care, until day 12 after the birth delivery. The presence of direct payments for out-patient care can prevent utilization because it imposes a liquidity constraint on patients' income.

Estimating the effect of this reform allows to test the validity of two hypotheses. First, it tests if liquidity constraints have an influence on the consumption of out-patient care. Second, it tests the existence of a substitution between out-patient care and emergency care to avoid the payment of a consultation. The influence of direct payments (which impose a liquidity constraint) on care consumption in high-income countries has received little attention in the literature. Also, to the best of our knowledge, the spillover effects of removing direct payments for out-patient care have not been studied yet.

In France, only a part of the French population might be subject to liquidity constraints, since a free CHI called "CMU-C" ensures no direct payments and full coverage to individuals

with very low income. Analyzing the effect of the 2017 "Tiers-Payant" reform offers an opportunity to test for the presence of liquidity constraints in France, which has never been done by previous studies. It is somewhat a challenge since the reform only affected a small part of the population, i.e., pregnant women who are not beneficiaries of the CMU-C. But since the income threshold to be eligible to the CMU-C is very low (70% of the poverty line), it is possible that some individuals remain subject to liquidity constraints.

Using a triple differences estimator, we find a small but positive impact of the "Tiers-Payant" reform on the number of GP consultations at hospital. In all, the reform led to an increase of 6.4 GP consultations per 100 pregnant women. In addition, we find a decrease in the number of midwife consultations taking place at the hospital for affected women (not CMU-C) following the reform. Taking together, these results suggest that liquidity constrained women substituted GP consultations to midwife consultations since the price of the latter was lower before the reform, when direct payments were required. Such evidence supports the existence of liquidity constraints for a proportion of pregnant women who are not covered by the CMU-C.

This result is remarkable since, as mentioned in section 2, pregnant women were already exempt from direct payments for the prenatal care recommended by the NHI guidelines before the reform. So the impact of the reform estimated in this study is probably underrated since it concerns a small part of the population that was already exempt from direct payment for a specific care package. For this reason, we should find a stronger effect of the reform in general population.

Turning to the second hypothesis, we find no impact of the reform on the number of ED visits. This result implies that EDs are not used as substitutes for out-patient care by pregnant women who were subject to liquidity constraints before the reform. This result is important for the literature on inappropriate ED visits. It provides empirical evidence that the presence of direct payments for out-patient consultations does not encourage liquidity constrained patients (here, pregnant women) to use EDs for non-urgent conditions. However, it does not prevent the existence of a substitution between ED visits and out-patient consultations for other reasons than liquidity constraints.

The findings of this paper are robust to the use of an alternative specification. A difference-in-differences (DD) model is estimated to account for the possibility that pregnant women can delay care consumption to the period when direct payments are exempted. Estimations are consistent with results from the triple differences model, though the estimated effect of the reform on GP consultations taking place at the hospital is less precise with the DD estimation.

This study has several limits. First, the main caveat is that our findings are estimated on the very specific population of pregnant women. This population has very specific care needs, and a pregnancy is not comparable with a disease. A majority of pregnancies are planed, and so the burden of direct payments can be anticipated. Plus, recommended prenatal care was already exempt from direct payment before the reform. All these reasons make the external validity of

the results questionable. Therefore, an experimentation consisting in removing direct payments for out-patient care should be conducted in the general population. Given the characteristics of the 2017 reform and the population targeted, one should expect to find a stronger influence of liquidity constraints in the general population. Such an experimentation would be useful for the discussion about the extension of the direct payment abolition reform to the whole French population.

A second caveat is that since administrative databases are used, information related to women's socio-demographic characteristics is very limited. To compensate this lack, data on physician's density around women's place of residence was imported as mentioned in section 5.

Last, results could be biased if women applied for the CMU-C complementary insurance in the expectation of a pregnancy. But in this case, results would be downward biased to zero.

To conclude, this paper provides empirical evidence that (i) there are still liquidity constraints for care in France, and (ii) the presence of direct payments for out-patient care does not encourage the utilization of EDs. Our results suggest that liquidity constraints restrict access to medical consultations, and thus support a general withdrawal of direct payments for out-patient care. But it should be confirmed by an experimentation conducted in the general population since our results were estimated on the specific population of pregnant women.

This paper is the very first to estimate a causal effect of the abolition of direct payments for out-patient consultations in France. To our knowledge, it is also the first to provide evidence of the influence of liquidity constraints on access to care consultations. It contributes to a rare literature on the effect of direct payments on health care consumption.

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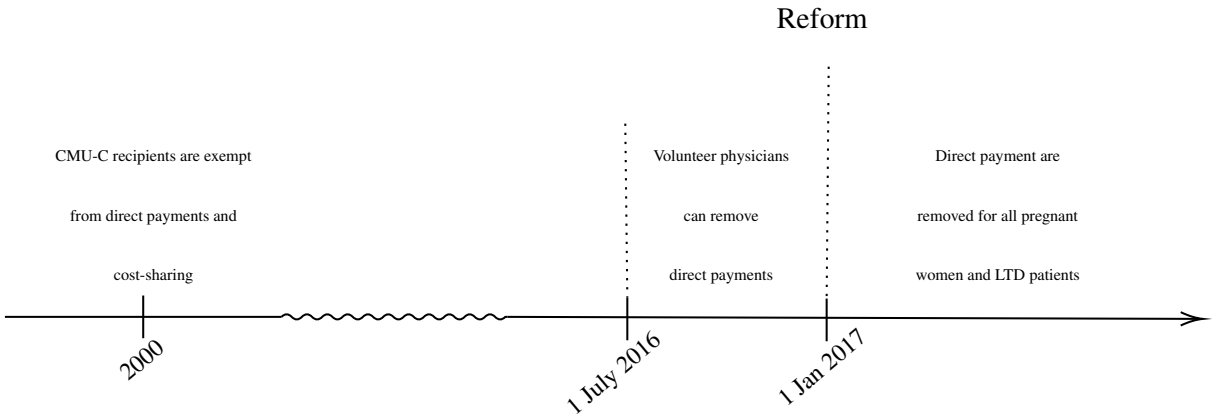
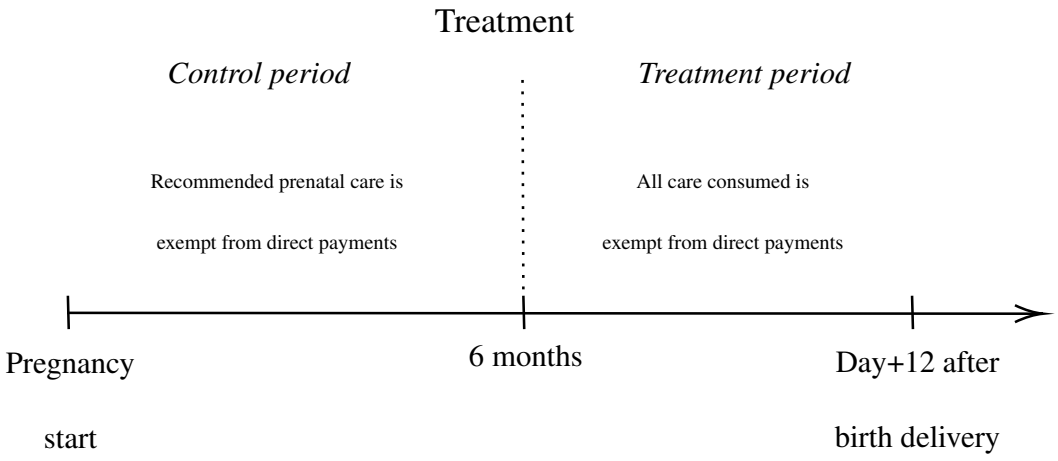
Appendices

A.1 Timelines of the reform





A.2 Algorithm for the identification of pregnancy episodes

We present below the principal diagnoses, the associated diagnoses and the birth delivery medical procedures to select in order to identify hospital admissions resulting in single living births. This algorithm applies only for requests in the French national administrative database related to hospital admissions, the PMSI *(Programme de Médicalisation des Systèmes d'Information)*.

- Principal diagnosis: O.80.0
- Associated diagnosis: Z37.0
- Birth delivery procedures: JQGD001, JQGD003, JQGD004, JQGD005, JQGD008, JQGD010, JQGD012, JQGD013, JQGA002, JQGA003, JQGA004, JQGA005.

Table A1: Number of birth deliveries identified in the data

	2014	2015	2016	2017	2018	All
Birth deliveries identified through the algorithm	7,399	7,424	7,490	7,237	7,146	36,696
Hospitalizations for birth delivery (Z37) but no delivery act	10	13	17	8	9	57
Gestational age missing	142	180	184	158	180	844
Gestational age lower than 6 months (182 days)	11	13	15	12	15	66
Delay between two pregnancy episodes lower than 9 months	2	0	1	6	1	10
Final sample of birth deliveries	7,234	7,218	7,273	7,053	6,941	35,719
Total approximation (×97)	701,698	700,146	705,481	684,141	673,277	3,464,743

Sources: Author's calculations from the EGB-S database.
Notes: 35,719 pregnancy episodes are identified through the algorithm for the 2014-2018 period. Since the EGB-S database is a 1/97th sample of individuals affiliated to the French NHI, we multiply the number of birth deliveries by 97 to approximate the number of birth deliveries that would have been selected in the comprehensive database (the SNIIRAM data).

Table 3 reports the number of birth deliveries identified through the use of the algorithm in the EGB-S database. After selecting relevant birth deliveries, the sample is composed of 35,719 birth deliveries observed during the 2014-2018 period. Since the EGB-S database is a 1/97th sample of individuals affiliated to the French NHI, we approximate the number of birth deliveries that would have been identified by the algorithm if applied to comprehensive data. Approximation shows that 3,464,743 would be identified. For seek of comparison, we present in table A2

the number of birth deliveries registered in national statistics data from INSEE. When multiple births are excluded, we count 3,862,472 birth deliveries between 2014 and 2018 in France. This allows us to compare the approximate number of single live births that would have been identified in non-sampled data (the SNIIRAM comprehensive database) to the number of single live births observed in national registers. Table A3 shows that the number of birth deliveries identified in the EGB-S database through the algorithm is around 10% lower than the number of birth deliveries in national registers.

Table A2: National statistics on birth deliveries in France

	2014	2015	2016	2017	2018	All
Total (1)	818,565	798,948	783,640	769,553	758,590	3,929,296
Twins (2)	13,825	13,539	13,189	12,822	12,505	65,880
Triplets or more (3)	191	190	206	182	175	944
Single live births (4)						
(4) = (1) - (2) - (3)	804,549	785,219	770,245	756,549	745,910	3,862,472

Sources: National administrative data from the National Institute of Statistics and Economic Studies (INSEE). Notes: Between 2014-2018, 3,862,472 single live births were registered in France. Last row reports estimates of the number of single live birth in France by year to compare with the approximate number of birth deliveries identified in the EDB-S database (last row of table A1).

Table A3: Total number of full-term single live birth deliveries in France

	2014	2015	2016	2017	2018	All
Estimates from national registers	804,549	785,219	770,245	756,549	745,910	3,862,472
Approximation from the EGB-S sample	701,698	700,146	705,481	684,141	673,277	3,464,743
Difference	103,151	85,073	64,764	72,408	72,633	397,729
%	12.82%	10.83%	8.41%	9.57%	9.74%	10.30%

Sources: Author's calculations from EGB-S database ; National administrative data from the National Institute of Statistics and Economic Studies (INSEE).
Notes: The number of full-term single live birth deliveries is 10% lower in the approximation made from the EGB-S database in comparison with national registers.

A.3 Sample frequencies

Table A4: Distribution of pregnancy episodes by quarter and treatment status

Year	Quarter	Number of pregnancies				Number of observations			
		Before month 6	After month 6	CMU-C: Yes	All	Before month 6	After month 6	CMU-C: Yes	All
2014	Q3	3,139	1,686	695	3,743	28,440	11,903	7,509	40,343
	Q4	3,085	1,781	787	3,942	27,272	10,684	7,547	37,956
2015	Q1	3,180	1,752	818	4,004	27,855	10,489	7,758	38,344
	Q2	3,240	1,884	846	4,088	28,879	11,360	8,382	40,239
	Q3	3,136	1,962	858	4,064	27,851	12,005	8,330	39,856
	Q4	3,223	1,892	877	4,124	28,266	11,705	8,580	39,971
2016	Q1	3,163	1,795	864	4,004	28,993	10,641	8,431	39,634
	Q2	3,131	1,939	870	4,002	27,361	12,068	8,352	39,429
	Q3	2,947	1,886	877	3,955	26,231	11,560	8,381	37,791
	Q4	3,085	1,706	891	3,857	27,099	10,597	8,747	37,696
2017	Q1	3,059	1,711	883	3,857	27,523	10,236	8,688	37,759
	Q2	2,975	1,857	864	3,819	26,549	11,401	8,671	37,950
	Q3	2,836	1,853	856	3,759	24,722	11,456	8,173	36,178
	Q4	2,864	1,625	796	3,637	25,053	9,996	7,751	35,049
2018	Q1	2,887	1,557	757	3,599	25,792	9,377	7,369	35,169
	Q2	2,060	1,706	576	2,830	20,286	10,420	6,119	30,706
All		18,148	17,189	3,979	18,809	428,172	175,898	128,788	604,070
Women		16,322	15,539	3,583	16,811				

Sources: Author's calculations from the EGB-S database.

Notes: Panel starts on 2014-07-01 and ends on 2018-06-30 (4 consecutive years). Q1 corresponds to the first quarter of a year and includes the months of January, February and March. Q2 corresponds to the second quarter of a year, etc. Pregnancy episodes are identified on the basis of their outcome, birth delivery. As a result the starting date of pregnancy is calculated retrospectively thanks to the delay from the last period (see section 5 for details). Choice was therefore made to start and to end the panel at the middle of a calendar year in order to have a balanced number of observations before and after the 6th month of pregnancy in the first and the last quarters of the panel. The number of women is lower than the number of pregnancy episodes because some women of the panel have multiple pregnancies during the period of observation.

A.4 Summary statistics by treatment status

Table A5: Number of medical consultations and ED visits

	(1) All	(2) Before 2017	(3) Before 2017	(4) After 2017	(5) After 2017
		Before 6 month	After 6 month	Before 6 month	After 6 month
	Sum	Sum	Sum	Sum	Sum
GP consultations					
Total	67,347	34,582	9,416	18,344	5,005
Out-patient care outside hospital	60,209	31,438	7,949	16,533	4,289
Hospital outpatient care	7,138	3,144	1,467	1,811	716
Gynecological consultations					
Total	70,971	29,408	16,993	15,544	9,026
Out-patient care outside hospital	49,484	21,994	10,404	11,585	5,501
Hospital outpatient care	21,487	7,414	6,589	3,959	3,525
Midwife consultations					
Total	53,024	13,118	18,741	9,258	11,907
Out-patient care outside hospital	11,392	4,285	2,082	3,441	1,584
Hospital out-patient care	41,632	8,833	16,659	5,817	10,323
Emergency Department visits					
Total	10,766	5,033	1,766	2,977	990
Hospitalized	1,357	390	458	221	288
Non-hospitalized	9,409	4,643	1,308	2,756	702
N	604,070	279,178	113,724	148,994	62,174
Pregnancies	18,809	13,954	11,940	7,164	6,614
Women	16811	13,069	11,404	7,086	6,553

Sources: Author's calculations from the EGB-S database.

Notes: Column 1 reports the total number of consultations and ED visits in the sample for the whole period of observation (mid-2014 to mid-2018). Column 2 (*resp.* 4) reports consultations and visits consumed during the control period (before 6 months of pregnancy) before (*resp.* after) the reform (2017). Column 3 (*resp.* 5) reports consultations and visits consumed during the treatment period (after 6 months of pregnancy) before (*resp.* after) the reform.

Table A6: Weekly means of pregnancy characteristics and care consumption

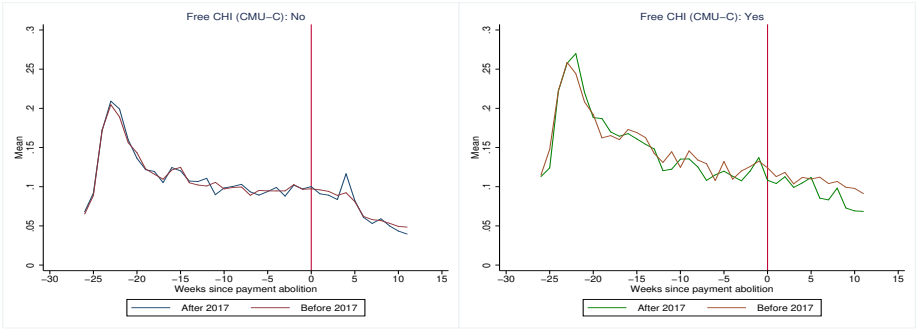
	(1) All		(2) Before 2017		(3) Before 2017		(4) Difference	(5) After 2017		(6) After 2017		(7) Difference
			Before 6 month		After 6 month		(2)-(3)	Before 6 month		After 6 month		(5)-(6)
	Mean	St.Dev	Mean	St.Dev	Mean	St.Dev	p-value	Mean	St.Dev	Mean	St.Dev	p-value
Pregnancy characteristics												
Mother's age	29.923	5.519	29.748	5.494	30.105	5.478	0.000	29.911	5.582	30.219	5.542	0.000
Pregnancy duration (in weeks)	37.395	1.213	37.359	1.222	37.446	1.207	0.000	37.361	1.207	37.492	1.194	0.000
N. of observed pregnancy weeks	37.206	6.640	38.381	4.854	35.611	8.842	0.000	35.574	7.458	39.304	2.904	0.000
GP consultations												
Total	0.107	0.331	0.124	0.354	0.077	0.282	0.000	0.123	0.354	0.076	0.278	0.000
Out-patient care outside hospital	0.096	0.311	0.113	0.336	0.065	0.257	0.000	0.111	0.335	0.064	0.257	0.000
Sector 1	0.095	0.311	0.112	0.335	0.063	0.254	0.000	0.112	0.336	0.064	0.255	0.000
Sector 2	0.004	0.064	0.005	0.070	0.003	0.059	0.000	0.003	0.060	0.003	0.056	0.050
Hospital out-patient care	0.012	0.111	0.011	0.109	0.012	0.114	0.035	0.012	0.113	0.011	0.108	0.099
Gynecological consultations												
Total	0.113	0.326	0.105	0.314	0.125	0.344	0.000	0.104	0.311	0.134	0.355	0.000
Out-patient care outside hospital	0.078	0.272	0.079	0.272	0.076	0.269	0.000	0.078	0.270	0.081	0.278	0.012
Sector 1	0.032	0.180	0.035	0.187	0.030	0.175	0.000	0.032	0.177	0.028	0.170	0.000
Sector 2	0.049	0.219	0.047	0.213	0.050	0.224	0.000	0.048	0.215	0.056	0.238	0.000
Hospital out-patient care	0.035	0.190	0.026	0.166	0.050	0.227	0.000	0.027	0.164	0.054	0.234	0.000
Midwife consultations												
Total	0.101	0.337	0.047	0.218	0.186	0.458	0.000	0.062	0.249	0.206	0.474	0.000
Out-patient care outside hospital	0.027	0.178	0.016	0.127	0.049	0.251	0.000	0.023	0.152	0.035	0.204	0.000
Hospital out-patient care	0.074	0.291	0.031	0.178	0.138	0.397	0.000	0.039	0.200	0.170	0.437	0.000
Emergency Department visits												
Total	0.024	0.165	0.017	0.139	0.034	0.194	0.000	0.020	0.148	0.041	0.213	0.000
Not related with birth delivery	0.017	0.138	0.017	0.139	0.014	0.128	0.000	0.020	0.148	0.015	0.131	0.000
Hospitalized	0.009	0.095	0.001	0.035	0.021	0.146	0.000	0.001	0.037	0.027	0.164	0.000
Not related with birth delivery	0.002	0.044	0.001	0.035	0.003	0.054	0.000	0.001	0.037	0.003	0.058	0.000
Non-hospitalized	0.016	0.132	0.016	0.134	0.013	0.122	0.000	0.018	0.142	0.014	0.125	0.000
Not related with birth delivery	0.015	0.130	0.016	0.134	0.012	0.116	0.000	0.018	0.142	0.012	0.116	0.000
N	627,532		266,425		146,242			142,219		72,646		
Pregnancies	18,809		13,294		15,988			6,836		6,527		
Women	16,811		12,489		14,527			6,768		6,466		

Sources: Author's calculations from the EGB-S database.

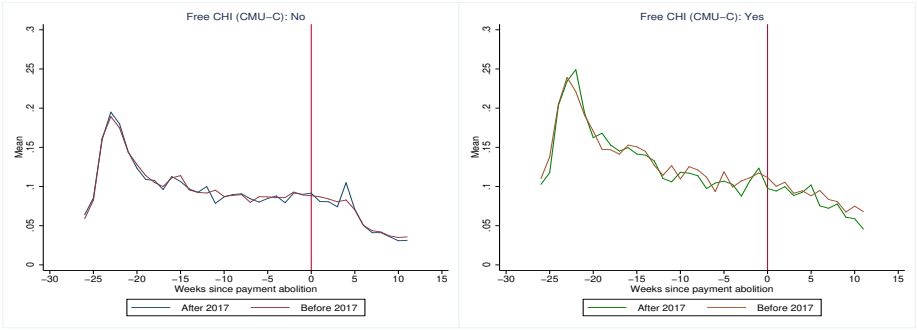
Notes: Column 1 reports weekly means (and standard deviations) of the sample. Column 2 (*resp.* 5) reports the same statistics for pregnancy episodes that were below 6 months of pregnancy before (*resp.* after) the reform (2017). Column 3 (*resp.* 6) reports statistics for pregnancy episodes that were above 6 months of pregnancy before (*resp.* after) the reform (and thus could have been (*resp.* were) exempt from direct payments. Columns 4 and 7 report p-values for tests of equality of means.

A.5 Trends in out-patient consultations

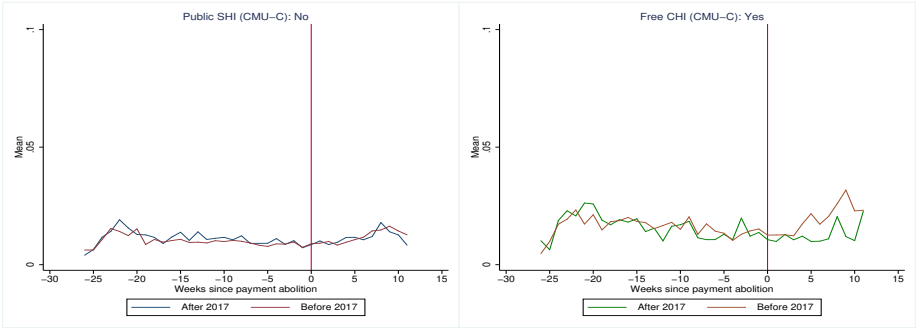
Figure 5: Trends in GP consultations



(a) Total GP consultations



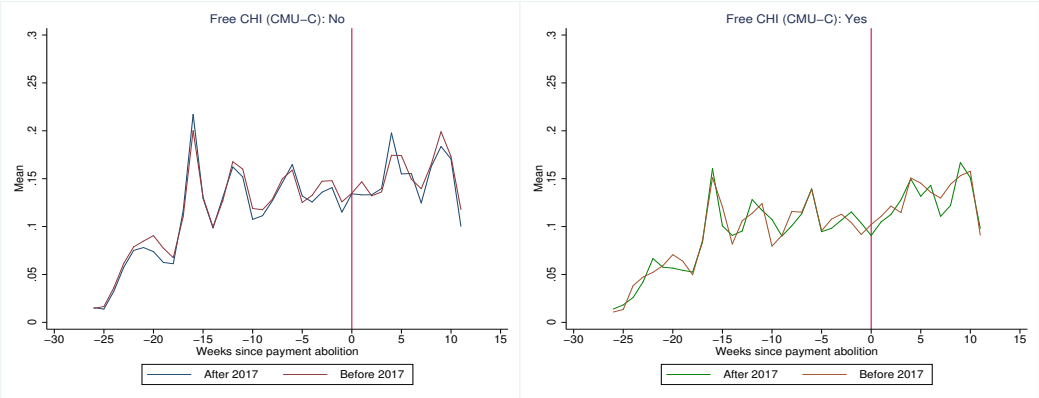
(b) Outpatient GP consultations



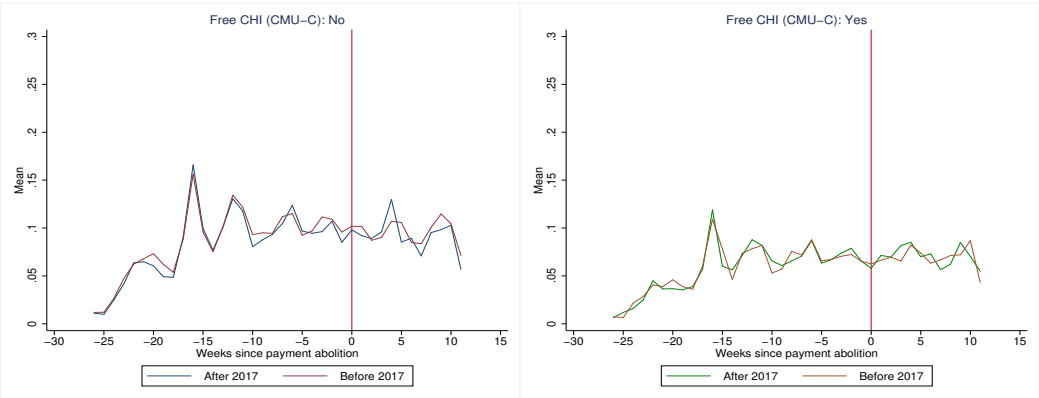
(c) Hospital outpatient GP consultations

Sources: Author's calculation from EGB-S. *Notes:* Trends in means of GP consultations by weeks of pregnancy (reported on the x-axis as a distance in weeks from the first week of the 6th month of pregnancy (week=0)) are represented before and after the reform (2017). Graphs on the left side concern women affected by the reform. Graphs on the right side concern women remaining unaffected by the reform (beneficiaries of the free CHI "CMU-C").

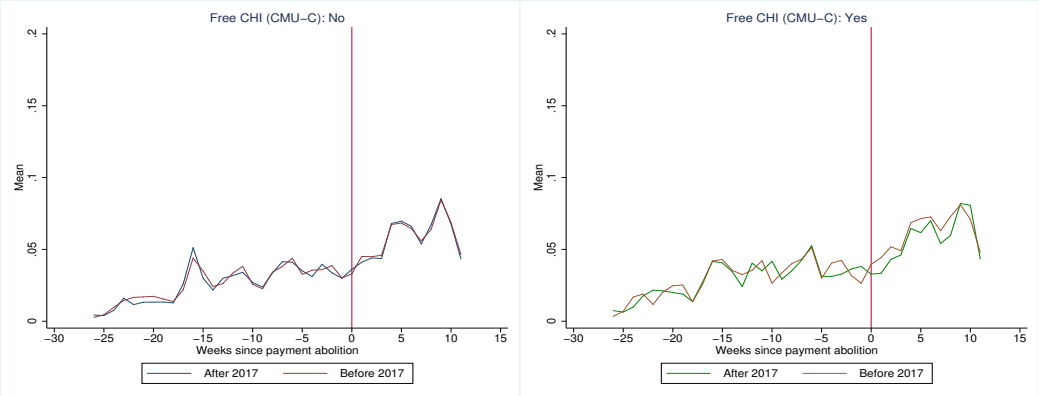
Figure 6: Trends in gynecological consultations



(a) Total gynecological consultations



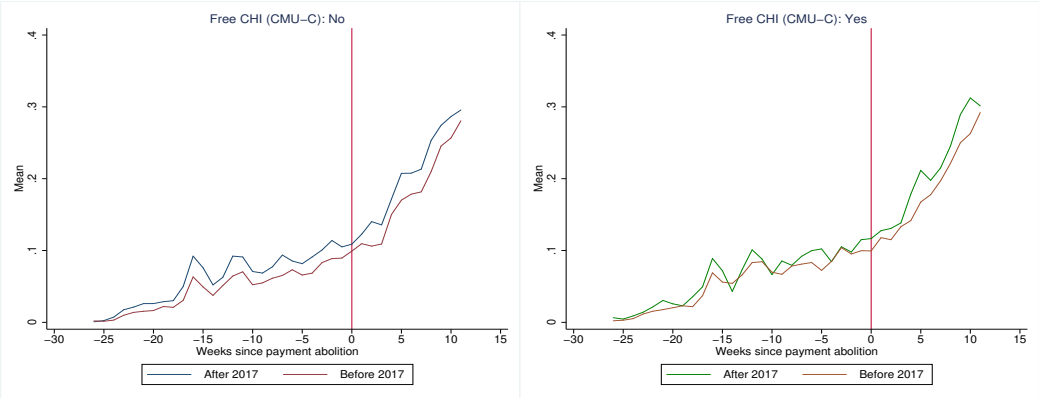
(b) Outpatient gynecological consultations



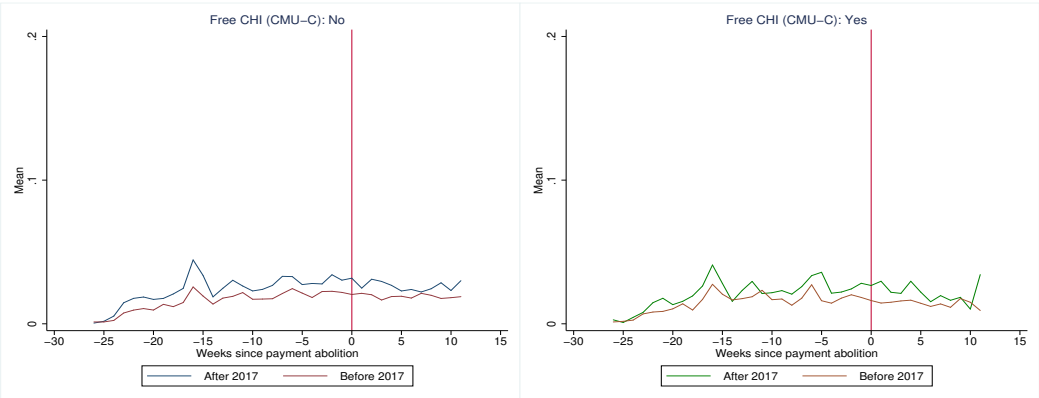
(c) Hospital outpatient gynecological consultations

Sources: Author's calculation from EGB-S.
Notes: Trends in means of gynecological consultations by weeks of pregnancy (reported on the x-axis as a distance in weeks from the first week of the 6th month of pregnancy (week=0)) are represented before and after the reform (2017). Graphs on the left side concern women affected by the reform. Graphs on the right side concern women remaining unaffected by the reform (beneficiaries of the free CHI "CMU-C").

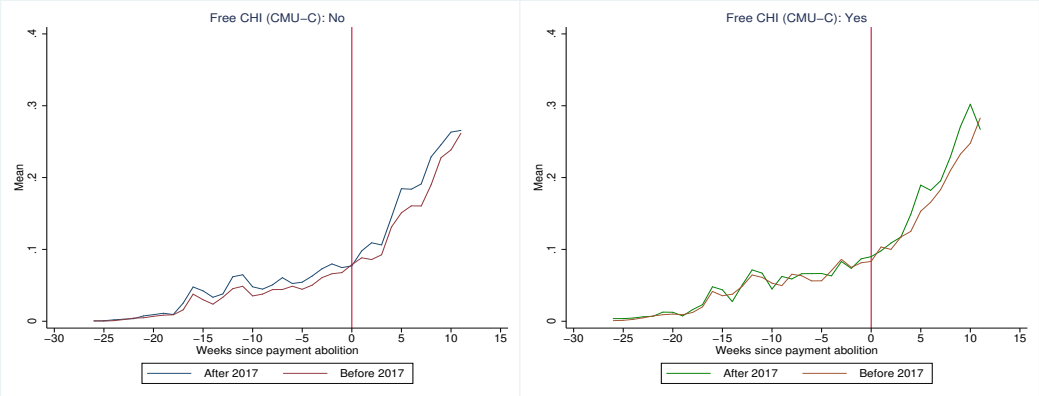
Figure 7: Trends in midwife consultations



(a) Total midwife consultations



(b) Outpatient midwife consultations

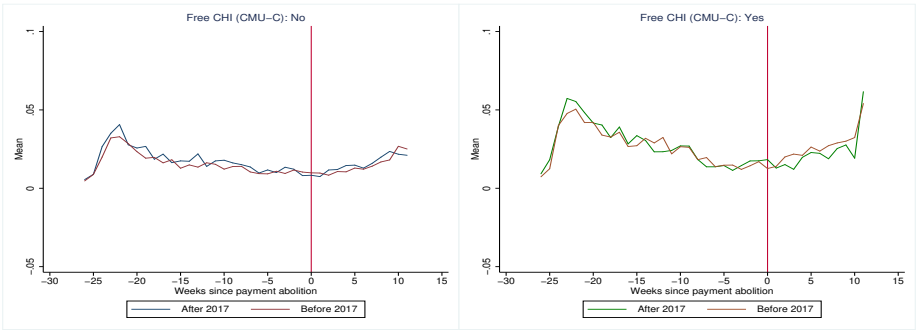


(c) Hospital outpatient midwife consultations

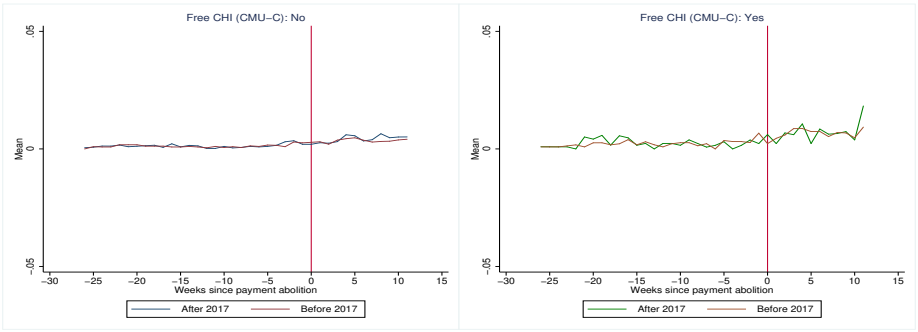
Sources: Author's calculation from EGB-S.
Notes: Trends in means of midwife consultations by weeks of pregnancy (reported on the x-axis as a distance in weeks from the first week of the 6th month of pregnancy (week=0)) are represented before and after the reform (2017). Graphs on the left side concern women affected by the reform. Graphs on the right side concern women remaining unaffected by the reform (beneficiaries of the free CHI "CMU-C").

A.6 Trends in ED visits

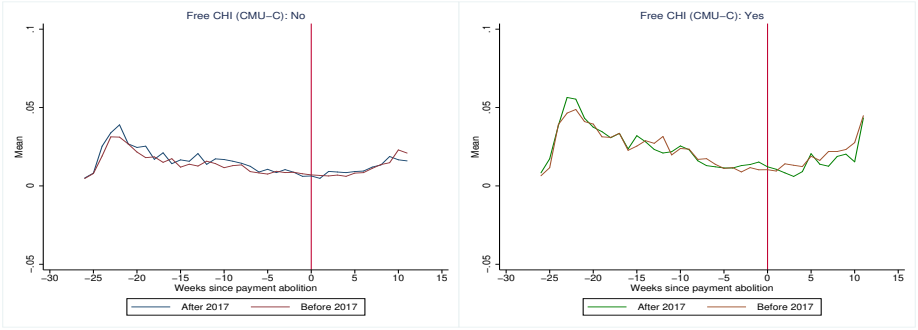
Figure 8: Trends in ED visits



(a) Total ED visits



(b) Hospitalized ED visits



(c) Non-hospitalized ED visits

Sources: Author's calculation from EGB-S. *Notes:* Trends in means of ED visits by weeks of pregnancy (reported on the x-axis as a distance in weeks from the first week of the 6th month of pregnancy (week=0)) are represented before and after the reform (2017). Graphs on the left side concern women affected by the reform. Graphs on the right side concern women remaining unaffected by the reform (beneficiaries of the free CHI "CMU-C").

A.7 Individuals unaffected by the reform

Prior to the 1st of January 2017, direct payments were already removed for the following specific populations and/or under the following circumstances:

(a) Beneficiaries of a free complementary health insurance (CHI) called CMU-C and managed by the French NHI. It prohibits direct payments for care and ensures full coverage. Attribution depends on household's income which must not exceed a certain threshold. This threshold is rather low and corresponds to 70% of the poverty line in France.

(b) Beneficiaries of the "Aide à la Complémentaire Santé" (ACS) program which consists in providing a check to patients paid by the social security, and which can only be used to purchase a private CHI. ACS program was designed to avoid the threshold effect generated by the CMU-C insurance program by subsidizing the purchase of a CHI for individuals with income just above the eligibility threshold.

(c) Beneficiaries of the "State Medical Aid" (AME). This program ensures full coverage and no direct payments for all the care consumed by low-income foreigners in an irregular situation, and living in France for at least 3 months.

(d) Victims of a work-related accident or an occupational disease for all the care received related to the accident or the occupational disease.

(e) Care provided to participants of a screening campaign (e.g. a mammogram performed as part of the breast cancer screening campaign)

(f) Inpatient care delivered in a hospital which contracted with the French NHI.

(g) Contraception consultations for minors over 15.

A.8 Mandatory examinations during pregnancy

Before the 2017 "Tiers-Payant" reform, direct payments (and cost-sharing) were already removed for a set of medical examinations recommended by NHI guidelines to pregnant women. Since the 1st of January 2017, direct payments (and cost-sharing) are removed for the care consumed on top of these medical examinations, from the first day of the 6th month of pregnancy until the 12th day after the birth delivery.

The following table details the recommended prenatal care examinations which were already exempt from direct payment before the reform:

Table A7: Summary of recommended care during pregnancy

Pregnancy month	**0 -3**	**4**	**5**	**6**	**7**	**8**	**9**	**NHI coverage**	**Direct Payments**
Prenatal consultation	✓	✓	✓	✓	✓	✓	✓	100%	No
Ultrasound	✓	×	✓	×	×	✓	×	70% before 6th month	Possible before the 6th month
								100% after	
Childbirth preparation sessions	×	✓	×	×	7 sessions			100%	No
Biological tests	✓	×	×	✓	×	✓	×	100%	No
Anesthesia consultation	×	×	×	×	×	✓	×	100%	No

Note: A free dental check-up is also proposed during the 4th and 5th month of pregnancy.Care recommended by the French NHI guidelines is denoted by a checkmark. On the contrary, a cross is displayed when care is not recommended.
Sources: https://www.service-public.fr/particuliers/vosdroits/F963 ; https://www.ameli.fr/assure/sante/themes/grossesse/consultation-suivi-mensuel ; https://www.service-public.fr/particuliers/vosdroits/F164 ; https://www.ameli.fr/medecin/exercice-liberal/presciption-prise-charge/situation-patient-maternite/situation-patient-maternite

A.9 Number of pre-term births

Table A8: Number of pre-term births by group of beneficiaries

	(1)	(2)	(3)	(4)	(5)
	All	Before 2017		After 2017	
		CMU-C: No	CMU-C: Yes	CMU-C: No	CMU-C: Yes
Full-term births	18,060	2,416	9,570	1,368	4,706
Pre-term births	749	123	377	72	177
Total	18,809	2,539	9,947	1,440	4,883
Women	16,811	2,401	9,470	1,425	4,854

Sources: Author's calculations from the EGB-S database.
Notes: This table reports the number of pre-term births by group of beneficiaries of the CMU-C. A birth delivery is considered premature if the pregnancy duration is lower than 240 days (8 months).

A.10 Robustness of the results

A.10.1 Average effect of the reform: DD analysis

Table A9: Difference-in-differences estimates of the reform impact on consultations

	(1) Total consultations	(2) Outpatient consultations outside hospital	(3) Hospital outpatient consultations
Outcome A: General Practitioner (GP) consultations			
Average Treatment Effect on the Treated	0.068435*	0.046077	0.022358
	(0.0396)	(0.0363)	(0.0148)
R-Square	0.4201	0.4125	0.4609
Outcome B: Gynecological consultations			
Average Treatment Effect on the Treated	-0.05743	-0.03537	-0.02206
	(0.0437)	(0.0327)	(0.0298)
R-Square	0.4293	0.5342	0.4681
Outcome C: Midwife consultations			
Average Treatment Effect on the Treated	-0.15994***	-0.00588	-0.15406***
	0.0619	0.0203	0.0587
R-Square	0.493	0.4711	0.5018
Number of observations	53,044	53,044	53,044
Number of women	15,539	15,539	15,539

Sources: Author's calculations from the EGB-S database.
Notes: *p<0.1, **p<0.05, ***p<0.01. This table reports the coefficient (standard errors in parentheses) of the difference-in-difference estimator, measuring the impact of the direct payment abolition reform on medical consultations of pregnant women affected by the reform (who are not beneficiaries of the free CHI CMU-C). An average treatment effect on the treated (ATET) is thus estimated. Fixed effects models are estimated by OLS regressions. Regressions control for individual (mother) fixed effects as well as for time (months) fixed effects.

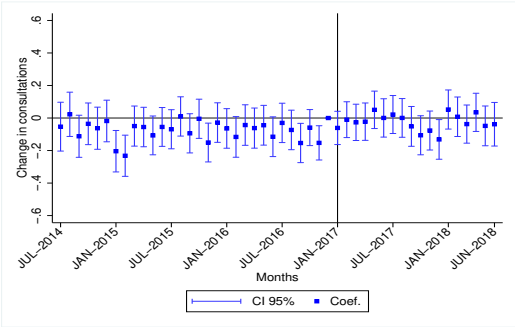
Table A10: Difference-in-differences estimates of the reform impact on ED visits

	(1) Total	(2) Hospitalized	(3) Non-hospitalized
Average Treatment Effect on the Treated	0.013241 (0.0178)	0.004997 (0.00858)	0.008244 (0.0154)
R-Square	0.4447	0.3646	0.4491
Number of observations	53,044	53,044	53,044
Number of women	15,539	15,539	15,539

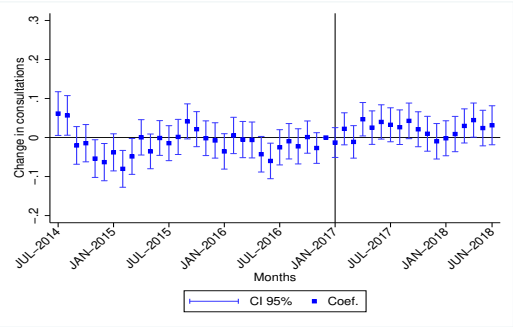
Sources: Author's calculations from the EGB-S database.
Notes: *p<0.1, **p<0.05, ***p<0.01. This table reports the coefficient (standard errors in parentheses) of the difference-in-differences estimator, measuring the impact of the direct payment abolition reform on the ED visits of pregnant women affected by the reform (who are not beneficiaries of the free CHI CMU-C). An average treatment effect on the treated (ATET) is thus estimated. Fixed effects models are estimated by OLS regressions. Regressions control for individual (mother) fixed effects as well as for time (months) fixed effects.

A.10.2 Event-study analysis

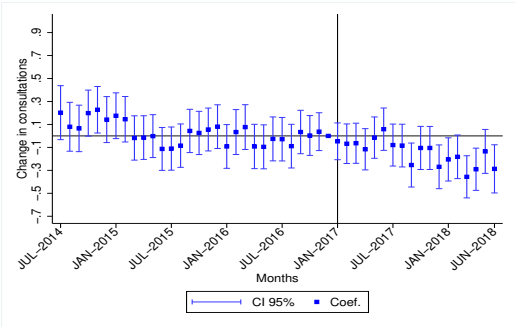
Figure 9: Event-study analysis of the reform impact by months - DD estimation



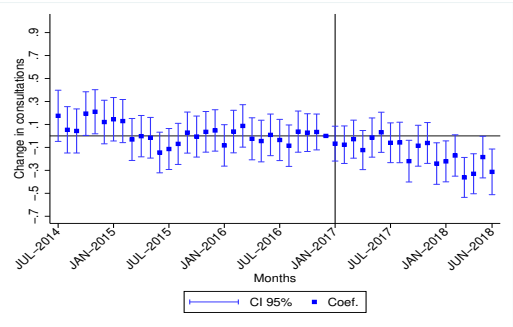
(a) Total GP



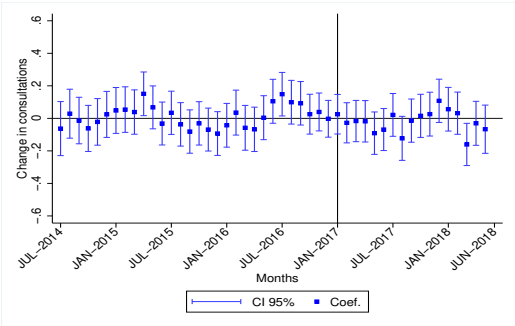
(b) GP consultations at hospital



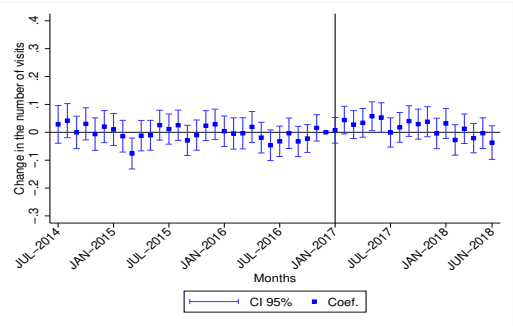
(c) Total midwife



(d) Midwife consultations at hospital



(e) Total gynecologist



(f) Total ED visits

Sources: Author's calculation from EGB-S. *Notes:* Results of an event-study analysis are reported. This analysis estimates the impact of being affected by the reform (i.e. not beneficiary of the CMU-C) by months, from mid-2014 to mid-2018. Coefficients of the interaction between a binary indicator for being in the treatment group (not beneficiary of the CMU-C) and month dummies are reported with their associated confidence intervals (CI). Regressions are estimated by OLS. Standard errors are clustered at the individual (pregnant women) level. Individuals' fixed effects are included in the regression.