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Free health care for the poor: a good way to achieve universal health coverage? Evidence from Morocco

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Abstract

Policies and programs aimed at giving access to health care free of charge for some segments of the population are increasingly being put in place by low- and middle income countries, going against the Washington-consensus era recommendation to impose user fees on public health care to insure a better quality of service. Yet, such policies may not be suited for middle-income countries, where user fees are not necessarily be the biggest barriers to health care. We study a nationwide example of such a policy with the generalization of the Medical Assistance Regime or RAMED in the Kingdom of Morocco, a policy aiming at giving free access to hospital care to the poorest quarter of the population. Using nationally representative panel data collected before, during and after the extension of the policy, we show that the removal of user fees did have a positive impact on access to health care, but that this impact comes mostly from poorer, rural households. We also study the impact on health expenditures, and find that it has not led to a decrease of the financial burden, except for the subset of urban households that have recurring health expenditures. Overall, our result show that in a middle income country , user fees, even modest, seem to significantly deter healthcare usage for the rural part of the population.

Keywords: Demand for health care, targeted policies, Morocco

JEL classification: I11, G22, H43

1 Introduction

What is the most efficient and socially equitable way to finance health care in low and middle income countries? Given the prevalence of informal work, social health insurance and other pre-financing systems generally appear unsuitable; on the other hand, administrative capacity constraint and the limited tax base seem to preclude broad-based tax-financed systems (Schieber and Maeda, 1999) In the wake of the Bamako Initiative (1987), a policy consensus was reached recommending the use of cost-recovery in the form of user fees in order to secure the supply of drugs and improve the quality of care in low-income countries (Gilson and Mills, 1995). Yet, despite some early successes (Litvack and Bodart, 1993), the performance of cost-recovery policies inspired by the Bamako Initiative remain controversial (McIntyre et al., 2006; Lagarde and Palmer, 2011).

In the past ten years, increased fiscal space have led several developing countries to make progress towards the objective of universal health care. These advances are often made in a piecemeal fashion and do not follow simple, pre-existing strategies (Lagomarsino et al., 2012). A policy trend that is gaining traction is the removal of user fees and the return to free health care provision. While these policies often have important effects upon being implemented, their continued effectiveness has been questioned due to concerns about the sustainability of their financing (Yates, 2009).

This renewed interest in free primary care parallels a recent trend in the micro-development literature. Randomized control trials have shown that 'free' health interventions may have big effects on demand; even a modest price may deter utilization, beyond what static demand models would predict (Cohen and Dupas, 2010; Powell-Jackson et al., 2014). These studies often conclude to a positive cost-benefit of free health care, once externalities are accounted for. However, this does not ensure that these benefits will materialize when and if the pilot programs or experiments are generalized, due to the prevalence of issues linked to implementation (Lagarde and Palmer, 2011; Lépine et al., 2018).

This paper aims to provide evidence concerning the real-world effect of one such gratuity program, the medical assistance regime (RAMED) put in place in Morocco in 2012. Compared to other free health care initiatives, this policy has several original features. First, health services being already provided free of charge in public health dispensaries, the gratuity concerns tertiary health services at hospitals, which were previously subject to fee-for service. Second, the benefits of the program are reserved to the poor, the program making use of a targeting system through proxy means testing.

We use an original, nationally representative household panel survey to estimate the effect of the RAMED on consultation behavior and on the healthcare expenditures of households. The questions of access and financial burden are relevant because, *a priori*, the effects of such a policy are ambiguous. Following gratuity, we would expect healthcare demand to increase in public hospitals, but this does not necessarily mean that the effect of overall healthcare demand will be positive, if the increased demand comes merely from a substitution of public for private suppliers of care. Moreover, the effect on the financial burden of households is uncertain, as it may cause some households that were previously excluded from health services to consume more, and thus to increase their income share devoted to health care. These questions are all the more relevant that Morocco is a lower middle income country , whereas most of the evidence we have comes from low-income countries; and it belongs to a geographical area, Middle East and North Africa, for which the evidence on health systems performance is scarce.

Our empirical strategy relies on a combination of propensity score matching (PSM) with panel difference-in-differences estimation, aiming at controlling for the selection bias based on observable as well as time-invariant unobservables linked to program participation as well as outcomes. We estimate our model separately for urban and rural areas. Another originality of our paper is that we are able to separate short-term effects (during the ramp-up of the policy) from longer term effects, after the policy has effectively been extended.

Our main results are that the RAMED gratuity program for hospital care has had a positive effect on health care demand for households in rural areas. This effect is 'net', i.e. it does not come from a shift in demand from public to private facilities. There is no discernible effect on health care access in urban areas, compared to the counterfactual. However, for urban households, the policy has led to a reduction in *conditional* health care expenditures, i.e. a reduction of the financial burden for households that have recurring expenses linked to health. Finally, it is linked with a slightly higher budget share devoted to health in rural areas. Overall, our results points towards pre-existing financial barriers to health care for poor, rural population, and militate in favor of a re-orientation of the program towards those households.

The paper is structured as follows. The next section presents the country context of Morocco and gives institutional details on the policy under study. The third section reviews briefly the academic literature relevant to our question. The fourth section presents the data and the empirical strategy used. The fifth section is devoted to commenting some descriptive statistics, and the sixth section presents our results. The last section concludes and discusses the results.

2 Country and policy context

Health and health care in Morocco A lower-middle income country, Morocco has a GDP per capital below the average of the Middle East and North Africa region (MENA). At 5.9% of GDP, global expenditure on health care is in line with the average of middle income countries, but due to a lower development level, this translates into a lower level of health care spending per capita in absolute terms: 446.6 USD PPP in 2014, compared to 712 USD in the MENA region and 581 USD in the average middle income country (cf. Table 1).

Yet, Morocco's overall health indicators are comparable to the regional average. Life expectancy at birth is 74 years, against a regional average of 73 years [1]. At 28 per 1000 live birth, under-5 mortality rate is below the average for middle income countries. The immunization rate against measles for children aged 12 to 23 month reaches 99 percent, above the regional average (86%) as well as the average of Middle Income countries (85%). Health care in Morocco is primarily financed through private expenditure, to a greater extent than in comparable countries. Public expenditure on health care represents less than 34% of total health expenditures, against 50% in the MENA region and 52% in middle income countries. Private expenditures on health care are primarily constituted by out-of-pocket spending (OOP); these represent 58.4% of total expenditure on health, compared to 44.3% in the MENA region and 36.1% in middle-income countries.

The prevalence of prepayment mechanism and health insurance is low. Before 2005, only civil servants had access to comprehensive health insurance. A law implemented in 2005 imposed mandatory health insurance to formal private sector workers, yet as of 2012 coverage remained low, at 20% of the overall population. Coverage rates vary widely between urban and rural areas: in the last case, only 7% of the population has access to health insurance, against 30% in cities.

The country has put in place a network of health centers and free clinics that are open to all free of charge. Yet, health care supply is low in international comparison. In 2009, there were on average 0.62 physicians for 1000 inhabitants, against 1.31 in the region. In 2012, Morocco was one of the 57 countries in the world that were singled out by the WHO as having a number of physicians and nurses below 'critical thresholds' (Chauffour, 2017). Moreover, the supply is unevenly spread over the territory. For instance, there is on average 1.3 physician per 1000 inhabitants in the region of Casablanca

[1] All figures in this paragraph come from the World Bank Health Nutrition and population database, year 2014 unless specified. The regional averages exclude oil-producing countries

Table 1: national indicators on health and health care

	Morocco	**MENA***	**Middle Income**
Life expectancy at birth (years)	74	73	71
Mortality rate, under-5 (per 1,000 live births)	28	23	40
Immunization, measles (% of children ages 12-23 months)	99	86	85
Health expenditure per capita, PPP	446,6	711,7	581,2
Health expenditure, total (% of GDP)	5,9	6,3	5,8
Out-of-pocket health expenditure (% of total expenditure on health)	58,4	44,3	36,1
Health expenditure, public (% of GDP)	2,0	3,2	3,0
Health expenditure, public (% of total health expenditure)	33,9	49,5	52,1
Health expenditure, private (% of GDP)	3,9	3,2	2,8
Health expenditure, private (% of total health expenditure)	66,1	50,5	47,9
Health expenditure per capita (current US$)	190,1	253,5	291,9
Improved sanitation facilities (% of population with access)	76,7	89,6	65,3
Improved water source (% of population with access)	85,4	92,6	92,2
Out-of-pocket health expenditure (% of private expenditure on health)	88,3	87,6	75,3

Source: World Bank WDI, year 2014 unless indicated
* except high-income

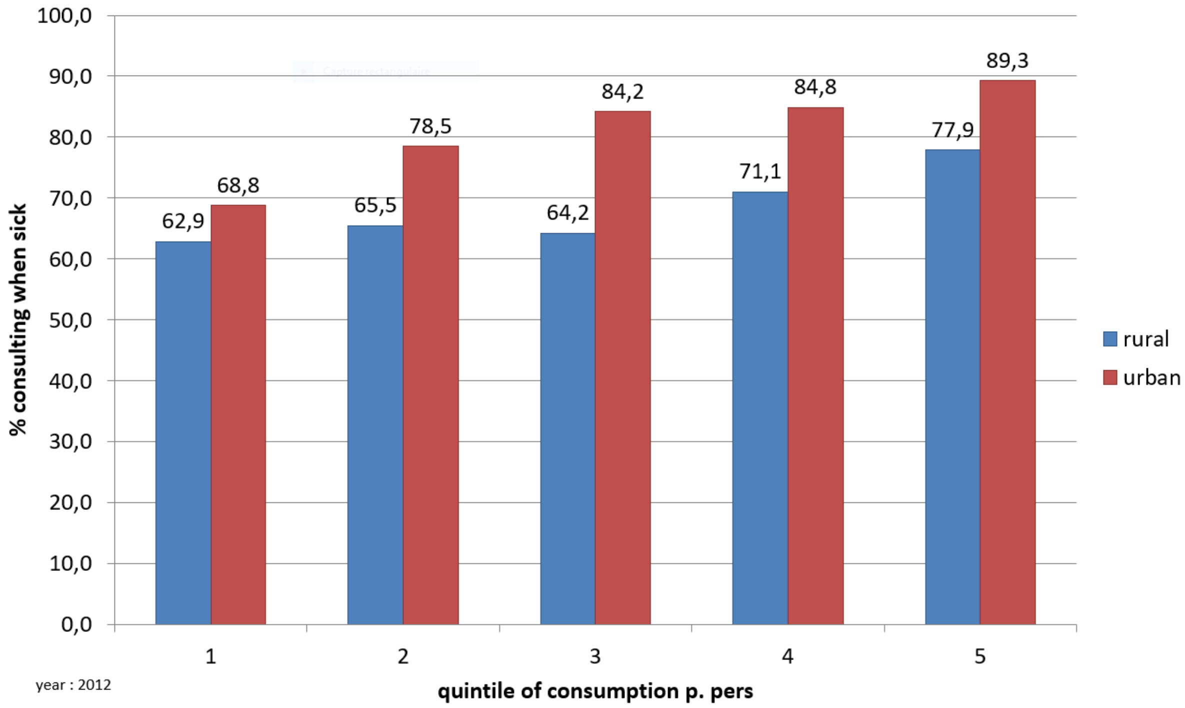
(the economic capital), against 0.25 per 1000 inhabitants in the remote region of Taza-Al Hoceima.

Despite free primary care, financial barriers to health care remain an important issue in Morocco. Consultation rates in case of sickness exhibit a strong socio-economic gradient (Fig. 1): in urban areas they vary between 69% in the first quintile and 89% in the top quintile (in rural areas, the figures are respectively 63% and 78%). Among the people renouncing health care, 60% mention "not being able to pay" as the main reason (ONDH, 2013).

Like numerous low- and middle-income countries, the bulk of the public health care budget is devoted to tertiary care sector, e.g. hospitals. Yet it is among this sector that financial barriers to access are the most salient, as hospital care is subject to user fees for patient without health insurance. Before 2012, the very destitute benefitted from a fee waiver known as the *certificat d'indigence* ("certificate of destitution"). Yet, this old mechanism (it was put in place in 1913 by the French colonial administration) was widely perceived as being insufficient to counter financial barriers to hospital care, due to several limiting features. The certificate was valid for only one person (no coverage of dependents), and for one medical care episode (limited validity in time). Furthermore, the eligibility criteria were never specified formally, and its delivery by local officers was subject of widespread suspicions of corruption.

RAMED, a targeted scheme for waiving hospital fees A reform of the certificate d'indigence was envisioned starting in the mid-90s. The new system, known as RAMED (an acronym for *Régime*

Figure 1: Consultation rates by consumption quintile, 2012 (source: ONDH)



d'assistance médicale, or "medical assistance regime") was supposed to cover all persons without health insurance below a certain monetary poverty threshold, approximately equivalent to 1.8 USD per person and per day, and representing one quarter of the population (8.5 million individuals). The RAMED card was supposed to cover the entire household, and to have a validity of two years. Holders of the cars were to be entitled to free health care in public hospitals.

Initially, the system was to be financed mainly through fiscal revenues, completed by contributions by the communes and beneficiaries themselves. The hospitals were supposed to be directly reimbursed for the treatment of RAMED patients through a third-party buying agency. Ex-ante, the cost of the new scheme was projected at approximately 5,3 Bn of Moroccan dirhams per year (approx. 530 M USD), or 630 DH per beneficiary and per year (63 USD), representing 0.5 of GDP.

Eligibility to the RAMED system is determined through a proxy means test, based on characteristics of households' dwelling (such as the number of rooms, its connection to the electricity and sanitation grid), possession of consumer durables, and the possession of livestock in rural areas. For the purposes of this discussion, two institutional details are worth mentioning:

- As well as discriminating between eligible and non-eligible households, the proxy means tests sorts eligible households in two categories : the "very destitute" on one hand, that receive the

RAMED card free of charge (corresponding to a living standard of less than 0.9 USD per person and per day); and the "vulnerable" on the other hand, that had to pay a contribution of 120 DH (12 USD) per person and per year to obtain the card giving access to free health care;

- The selection and attribution process is supervised by the Interior Ministry, who retains a modicum of flexibility, allowing them to reclassify some non-eligible households according to the proxy means test as eligible to RAMED.

This design was supposed to provide a balance between the impartiality of the mechanism and some adaptability to the households real living conditions

Implementation RAMED was introduced nationwide in 2012, after a four-year pilot program that took place in one poor rural region of the country. The generalization of RAMed system was conducted in a context of political grievances following the Arab Spring, and was largely perceived as a political gesture destined to appease social tensions.

The new regime was widely marketed as 'free health care for the poor'; yet the financing mechanisms to accommodate the influx of health care demand were not put in place until several years later. Only limited amounts of fiscal resource were affected to the special fund, which in turn did not do any disbursement until 2016[2]. The financial burden of caring for RAMED patients fell on the hospital themselves, which had to deal with an influx of new patients with unchanged resources (see figure 2 for an example in one big hospital of the country). This led to massive increases in waiting times, congestion, and shortages of medical material and medicine.

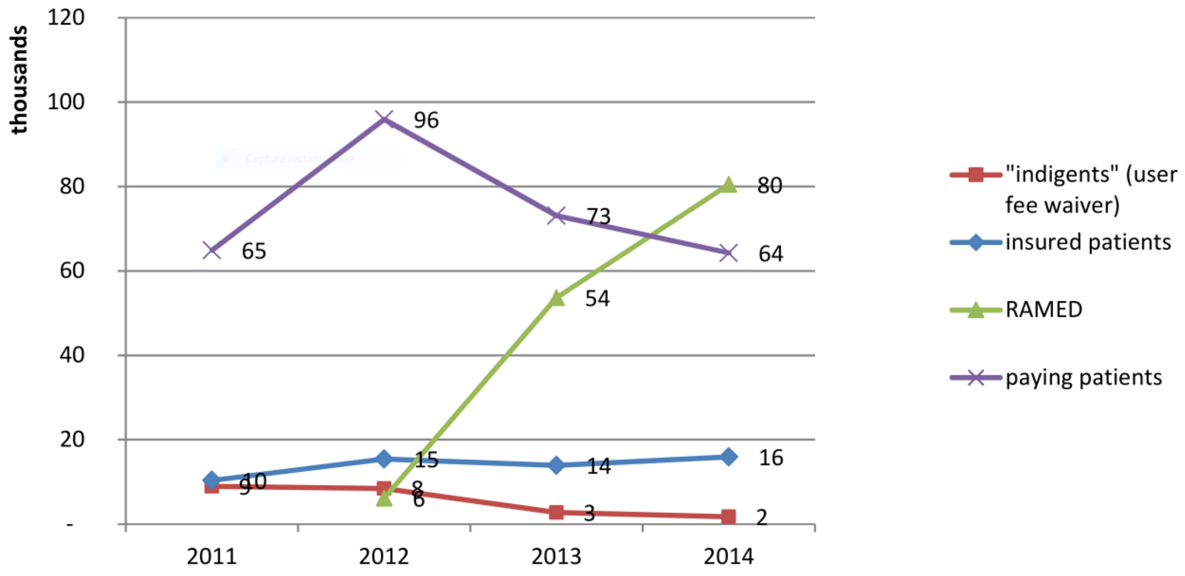
From the point of view of the hospitals, the introduction of an obligation to tend to RAMED patients without being able to charge user fees is akin to a system of 'uncompensated care', where hospitals are mandated to provide care for patients with no capacity to pay[3].

Hospitals are capacity constrained and the prices charged are fixed administratively. An exogenous increase in demand for hospital care is thus likely to result in shortages, materialized through lengthening waiting times for chirurgical interventions, increased congestion and rotation, and lack of equipment and medication. In the literature, these phenomena come under the header "quality of care" (e.g. Alderman and Lavy, 1996; Litvack and Bodart, 1993). Another effect is the worsening of governance, in the form

[2] Details relative to the funding and financial architecture of RAMed can be found in ONDH (2017).

[3] See Currie and Madrian (1999), for a quick overview of the place of uncompensated care in US health care system, and Wagner (2016) for an analysis of this mode of provision at the hospital level.

Figure 2: Fes University Hospital : number of patients by health coverage status, 2011-14



of kickbacks or "mandatory gifts" to doctors, a practice already widespread in Morocco previous to the introduction of RAMED (Transparency International, 2016).

A qualitative study commissioned by the National Observatory of Human Development (ONDH) found that, after the introduction of RAMED, public hospitals experienced higher waiting times and delays, an increased turnover rate for patients, numerous shortages, and frequent breakdowns of technical equipment due to intensive usage (ONDH, 2017). Degradation of the equipment appeared as a by-product of the hospitals diverting their investment and maintenance budgets towards treating patients. Doctors and medical personnel experienced heightened stress and discontentment, leading to a broad opposition to the policy among professionals.

These adverse effects on quality of care and hospital functioning do not preclude an increased access to health care for poor populations, if the introduction of RAMED is accompanied by sufficient demand shifting of better-off patients towards the private sector. There is indeed anecdotal evidence that insured patients have been driven towards care in the private health care sector following the introduction of RAMED and the subsequent degradation in healthcare quality. Thus, the net effect of the introduction of RAMED on access to health care depends on the interactions of two countervailing effects: the size of this crowding-out of solvable patients on the one hand, the demand-inducing effect of the abolition of user fees on poor people.

3 Relevant Literature

The dual relationship between health and economic status has long been recognized as one of the principal factors preventing some households from exiting poverty (Smith, 1999). Poor households face a higher risk of income loss due to ill-health, through foregone earnings on the labor market as well as a consequence of high out-of-pocket expenditures for health care. This relation between health and poverty is mediated through healthcare systems. Beyond some broad similarities (high level of out-of-pocket expenditure, low coverage rates of health insurance, focus on tertiary health care to the detriment of prevention, as described byWhitehead et al. (2001)) there is considerable variation in the design and features of healthcare systems in low- and middle income countries (Lagomarsino et al., 2012). The assessment of the performance of healthcare systems in reaching better levels of health and protection against health risk is thus a major policy issue. Yet, evidence regarding the effect of various health systems on the health status of populations is hard to come by, due to the difficulty of finding exogenous sources of variation in health systems that are not correlated to health status (Levy and Meltzer, 2008; Card et al., 2009; Shigeoka, 2014; Tanaka, 2014). To avoid this difficulty, most studies focus on intermediate outcomes, such as healthcare utilization and out-of-pocket expenditures, and to what extent healthcare systems affect those (Peters et al., 2008).

User fees and Bamako Initiative In the context of developing countries, a debate that has focused the attention of researchers and health care practitioners is the question of whether to finance health care services mainly through general government budget, or whether to increase the financing of these services through user fees imposed at the point of care. "Cost-recovery" policies, consisting in increased user fees to finance the health care system, were promoted in the 1980s by multilateral institutions, in particular the World Bank (Akin et al., 1987). In a similar vein, the "Bamako Initiative" (1987) called for user fees accompanied by targeted exemptions for the poorest in order to increase the availability of essential drugs and increase the quality of care in Sub-Saharan Africa (Mills, 2014).

The empirical background to these policy recommendations were papers on the determinants of health care demand that concluded to a low price elasticity of demand for medical care, and as a consequence a low welfare impact of financing healthcare through direct payments. The robustness of this finding has later been questioned by papers that took better into account selection issues as well as the effect of quality of care and patient heterogeneity (Cissé et al., 2004).

The “Bamako Initiative” is often regarded as a failure, due to the perception that the imposition of user fees has had a strong negative effect on healthcare utilization. Yet the evidence regarding the effect of user fees in low and middle income countries is less clear cut. In a systematic review, (Lagarde et al., 2009) note that the majority of studies on this topic suffer from important methodological weaknesses; moreover, there are some instances where the imposition of user fees has not led to a significant decrease in health care demand (Dzakpasu et al., 2013).

The main objection against policies inspired by the Bamako initiative seems to be the difficulty of bringing about quality improvements following the imposition of user fees. Indeed, when accompanied by measurable quality improvements, there is evidence that user fees do not deter utilization of health facilities, even for poor households (Litvack and Bodart, 1993; Alderman and Lavy, 1996).

Another stumbling block for such policies is the difficulty of guaranteeing fairness through targeted fee exemption mechanisms. For instance, Flores et al. (2013) note that in Cambodia, although fee waivers for poor household existed de jure, they were supposed to be attributed at the point of care. This situation generated conflicting incentives for the hospitals, which had to balance their solidarity objectives with the need of obtaining funding through user fees, and as such led to the hollowing out of this solidarity mechanism.

Is free health care a credible alternative? The perceived failure of cost-recovery policies has shifted the focus of developing countries towards alternatives policies to increase medical coverage, such as community-based health insurance or health equity funds (Jütting, 2004; Yilma et al., 2015; Flores et al., 2013). One option that has enjoyed much attention is simply the generalization of free health care through the suppression of user fees (Yates, 2009; Meessen et al., 2011).

There appears to be strong micro-level empirical evidence supporting such policies of user fees removal. Cohen and Dupas (2010) show in the context of a randomized controlled trial of malaria prevention policies that even a modest (and subsidized) price for antimalarial insecticide-treated bed nets had strong negative impact on usage, without increasing allocative efficiency, resulting in a very positive cost-benefit calculation in favor of free distribution of bed nets (when taking into account positive externalities). Fafchamps and Minten (2007) analyze the effect of the suspension of user fees for health care services in Madagascar following a disruptive domestic political episode, and find evidence of a significant increase of visits to health centers. Powell-Jackson et al. (2014) conduct a randomized experiment in rural Ghana on the removal of user fees. They find increased utilization, reduced health spending, and manage to detect improved health on children that were suffering from anemia.

These papers rely on experimental or quasi-experimental design, and as such have a high internal validity. Yet, concerns about external validity remain, particularly when trying to evaluate whether the positive effects of removal of user fees would scale up when implemented at a national level. Indeed, the (qualitative) "process evaluation" literature on the introduction of free health care stresses the disruptive effect that such policies have on overall healthcare systems (Ridde et al., 2012) . In contrast to targeted user fee exemptions, broad-based free health care schemes are generally the product highly politicized top-down decisions, presented as a 'gift of the leader to the people', but lacking in financial and technical preparation. Those policies are generally followed by an increase in utilization by the population, but the "free" nature of health care remains partial, as patients frequently have to make up for lacking supply in medicine and consumables with their own funds. Moreover, broad-based free health care schemes are generally accompanied by a degradation in the quality of care and a diminishing trust between patients and health care professionals (Lagarde et al., 2009).

To our knowledge, there is little literature trying to reconcile the micro-evidence from quasi-experimental design with the qualitative observation of a disruptive effect of free health care on health systems. An exception is Lépine et al. (2018), which uses a pooled synthetic control method to study the generalization of a policy of user fee removal in Zambia, and find no evidence of an increase in health care utilization, contrary to what pilot studies suggested.

Benefit incidence of public expenditure on health care Another way to study the effect of free health care policies is to look at them through the lens their effect health equity (Wagstaff and Van Doorslaer, 2000). On the domestic political scene, fee exemptions are often framed as policies aiming to increase the equity of the access to health care, by doing more for the poor.

The theoretical basis for the public provision of private goods in developing countries is that it can achieve distributional when the scope for tax and transfer policies is constrained by limited administrative capacity. Besley and Coate (1991) show that in a setting where a good can be purchased on a private market for a higher quality, the public provision of a lower quality version of this good, financed by a proportional tax, can lead to a redistribution from rich to poor, if quality is a normal good. This argument applies in particular to services such as schooling and medical care, for which a higher quality version is usually available on the private market.

This theoretical argument seems to run contrary to a large literature on benefit incidence, which tends to show that public spending on healthcare and education benefit disproportionately to the most favored classes in low-income countries (Van de Walle, 1998). For the specific case of health care services, national and cross-country studies find that the overall incidence of public spending in poor countries is generally regressive, benefitting more than proportionally to the better-off subpopulations (see Castro-Leal et al. (1999) for a study on six sub-Saharan African countries, and Van de Walle (1994) for a study on Indonesia). This pattern of regressive benefit incidence of public spending in health care is mainly due to lower utilization tertiary services by the poor (hospital or specialty clinics). Thus, one way to make to make the public spending on healthcare more "pro-poor" is to divert demand from better-off categories of the population towards the private sector. Thus, a policy aiming at giving priority access to the public service to the least well-off people may achieve redistribution from rich to poor. The question of knowing whether such a policy is welfare increasing depends on the deadweights losses, if any, generated by such a policy.

4 Data and empirical strategy

4.1 Data

Our data comes from a panel household survey conducted in three waves between 2012 and 2015 by the National Observatory for Human Development (ONDH, *Observatoire National du Développement Humain*). At baseline, the survey is representative of the national, urban and rural population. The sample consists of 7853 households, corresponding to 37444 individuals, for a mean of 4.76 persons per household on average. The panel consists in a multi-purpose LSMS-type survey, of living conditions, with modules on consumption, consumer durables and equipment, characteristics of housing, as well as earnings and debt. Additionally, the questionnaire comprises a module on health and health care, eliciting information on self-declared morbidity and health care usage in the past 4 weeks for all members of the household. Finally, information on schooling and occupation are collected for all individuals, as well as a specific module on social inclusion and subjective poverty.

The baseline survey was conducted in 2012, between the months of april and july. Table 2 presents the

Table 2: Panel structure

	2012	2013	2015
national			
# of households	7854	7370	6977
% of previous wave		93,8%	94,7%
% of baseline			88,8%
rural			
# of households	3119	3022	2945
% of previous wave		96,9%	97,5%
% of baseline			94,4%
urban			
# of households	4735	4348	4032
% of previous wave		91,8%	92,7%
% of baseline			85,2%

structure of the panel data. At the national level, there is a 6.2% attrition rate between the first and the second wave and another 5.3% attrition between the second and the last wave, leading to a total attrition of 11.2% between the baseline and 2015.

The survey was conceived and collected independently from the policy under study. As mentioned previously, the decision of generalizing RAMED was taken by the King, and caught the Health Ministry officials by surprise. As a result, even though the announcement was made in March 2012, the administrative process to collect the applications was not effective before September 2012, and the first cards were delivered at the end of the year. We thus consider the baseline wave of the panel to be a "pre-program" survey, although strictly speaking the program was announced before the data collection. For subsequent waves (2013 and 2015), the questionnaire was amended in order to collect information on the precise affiliation status of the individual with respect to RAMED: whether the individual is affiliated to RAMED or not, whether the affiliation is made in his/her own name or if he/she is covered through another individual in the household, and whether they have applied for the RAMED program but been denied or are still waiting for a reply from the administration.

4.2 Empirical strategy

Our goal is to estimate the effect of the RAMED hospital fee waiver on healthcare usage and on the financial burden of health. To identify the causal effect of RAMED, a suitable counterfactual for the potential outcome of treated individuals has to be constructed.

For each treated individuals, we have two different sources of information for such a counterfactual: first, the pre-treatment variables for the same household; second, the presence of comparable households in terms of observable covariates, who have not been treated (i.e. are not covered by RAMED). Our strategy exploits both sources of information in order to estimate the average effect of the treatment on the treated (ATT).

We follow Nolan (2008) and Yilma et al. (2012) in using a panel difference-in-differences propensity score matching method. First, we estimate the determinants of being effectively affiliated to RAMED using a propensity-score matching method. The goal of this step is to remove the selection bias into RAMED coming from observable households characteristics, such as standard of living, health status of household members at baseline, and theoretical eligibility. To the highest extent possible, we retain only covariates likely to affect both the treatment status and the outcome status (Caliendo and Kopeinig, 2008).

In the next step, we estimate a panel difference-in-difference model between the treatment group and the comparison group, as defined by the propensity score. This step helps controlling for selection bias due to time-invariant unobservable factors such as attitude to risk or latent household head status.

We conduct the analysis at the national level, as well as separately between urban and rural household. In addition, we estimate the panel diff-in-diff model for the whole time period under consideration (2012-2015), as well as over subperiods: 2012-2013 and 2013-2015 . The division in two subperiods is relevant from a policy point of view: there is qualitative evidence of congestion phenomena taking place in public hospitals (ONDH, 2017). To the extent that this is the case, this would be a violation of the stable unit treatment assumption (SUTVA) underlying the potential outcome framework (Angrist and Pischke, 2009).

To construct our propensity score, we use a kernel matching algorithm based on a Gaussian kernel with a 0.2 bandwidth (Leuven and Sianesi, 2003). The covariates used in the matching step are described in table 3 below.

5 Descriptive statistics

Figure 3 depicts the consultation rate at the household level, by year and by coverage status[4]. Households declaring any kind of medical consultation (at hospital, dispensaries, or private practices) in the previous

[4] we exclude from the analysis all households covered by the health insurance of the public sector or of the private formal sector)

Table 3: Covariates, by residency and coverage status

	rural						urban					
	2012		2013		2015		2012		2013		2015	
age of hh head	51.29	(14.15)	52.16	(13.94)	53.81	(13.80)	49.93	(13.39)	51.38	(13.02)	53.49	(12.92)
female hh head	0.10	(0.29)	0.11	(0.31)	0.12	(0.33)	0.21	(0.41)	0.23	(0.42)	0.26	(0.44)
education of hh head												
primary or lower	0.93	(0.25)	0.93	(0.25)	0.93	(0.25)	0.76	(0.43)	0.76	(0.43)	0.77	(0.42)
lower secondary	0.05	(0.22)	0.05	(0.22)	0.05	(0.22)	0.12	(0.33)	0.12	(0.32)	0.11	(0.32)
upper secondary	0.01	(0.11)	0.01	(0.11)	0.01	(0.11)	0.08	(0.27)	0.08	(0.28)	0.08	(0.27)
higher ed.	0.01	(0.09)	0.01	(0.09)	0.01	(0.09)	0.04	(0.20)	0.04	(0.19)	0.04	(0.20)
household structure												
household size	5.42	(2.55)	5.56	(2.60)	5.48	(2.47)	4.42	(2.02)	4.60	(2.07)	4.60	(2.00)
# women	2.68	(1.56)	2.77	(1.57)	2.72	(1.53)	2.24	(1.31)	2.36	(1.32)	2.39	(1.29)
# children (<6 y)	0.88	(1.04)	0.89	(1.04)	0.86	(1.04)	0.61	(0.86)	0.58	(0.85)	0.55	(0.82)
# elderly (>65 y)	0.34	(0.61)	0.35	(0.62)	0.41	(0.65)	0.23	(0.50)	0.26	(0.51)	0.33	(0.57)
employment rate	0.45	(0.26)	0.42	(0.25)	0.46	(0.26)	0.42	(0.29)	0.41	(0.28)	0.41	(0.28)
# independent	0.80	(0.82)	0.72	(0.78)	0.72	(0.78)	0.63	(0.71)	0.57	(0.68)	0.58	(0.69)
# wage workers	0.50	(0.77)	0.53	(0.75)	0.53	(0.78)	0.48	(0.72)	0.54	(0.75)	0.51	(0.75)
eligibility to RAMED												
eligible 'poor'	0.13	(0.34)	0.13	(0.34)	0.13	(0.34)	0.16	(0.37)	0.16	(0.37)	0.16	(0.37)
eligible 'vulnerable'	0.41	(0.49)	0.41	(0.49)	0.40	(0.49)	0.07	(0.25)	0.07	(0.26)	0.07	(0.26)
quintile of wealth index												
1st quintile	0.52	(0.50)	0.51	(0.50)	0.49	(0.50)	0.02	(0.14)	0.02	(0.14)	0.02	(0.14)
2nd quintile	0.37	(0.48)	0.37	(0.48)	0.37	(0.48)	0.13	(0.34)	0.12	(0.33)	0.09	(0.29)
3rd quintile	0.08	(0.28)	0.09	(0.28)	0.10	(0.31)	0.36	(0.48)	0.35	(0.48)	0.32	(0.47)
4th quintile	0.02	(0.13)	0.02	(0.14)	0.03	(0.16)	0.32	(0.47)	0.33	(0.47)	0.34	(0.47)
5th quintile	0.01	(0.11)	0.01	(0.11)	0.01	(0.10)	0.18	(0.38)	0.18	(0.39)	0.23	(0.42)
health status												
# of chronically ill	0.44	(0.71)	0.32	(0.63)	0.32	(0.59)	0.51	(0.74)	0.43	(0.70)	0.37	(0.66)
# temporary ill	0.19	(0.47)	0.17	(0.53)	0.19	(0.48)	0.20	(0.48)	0.15	(0.42)	0.20	(0.51)
mean standardized BMI	23.31	(2.57)	23.22	(2.53)	23.21	(2.45)	23.62	(2.70)	23.70	(2.57)	23.49	(2.54)
overweight	0.95	(0.97)	0.94	(0.95)	1.06	(1.04)	0.97	(0.90)	1.06	(0.93)	1.20	(1.00)
underweight	0.04	(0.21)	0.05	(0.24)	0.05	(0.23)	0.03	(0.21)	0.03	(0.19)	0.05	(0.24)
Observations	2573		2513		2450		2417		2413		2254	

Standard deviations in parentheses.

4 weeks receive a value of one. Several stylized facts emerge. First, the consultation rate is higher in cities than in rural areas, which reflects both the higher living standards in urban areas, as well as the higher density of healthcare supply. Second, households covered by RAMED have higher consultation rates than uncovered households (the gap between RAMED and uncovered households goes from 9 percentage points, for urban households in 2015, to 33 percentage points in 2013 for the same areas). Third, there seems to be a general downward trend in consultation rates year after year. Table 4, shows that this is also the case in the entire population, as well as for different types of consultation (private or public, outpatient or hospital). Figure 4 in the appendix shows that it also is the case among households benefitting from health insurance (civil servants or employees of the private formal sector). One possible explanation for this downward trend in the consultation rate is the progressive attrition of older people from the panel due to mortality, these individuals also being those with the highest demand for health care.

Figure 3: Consultation rates by year and coverage type

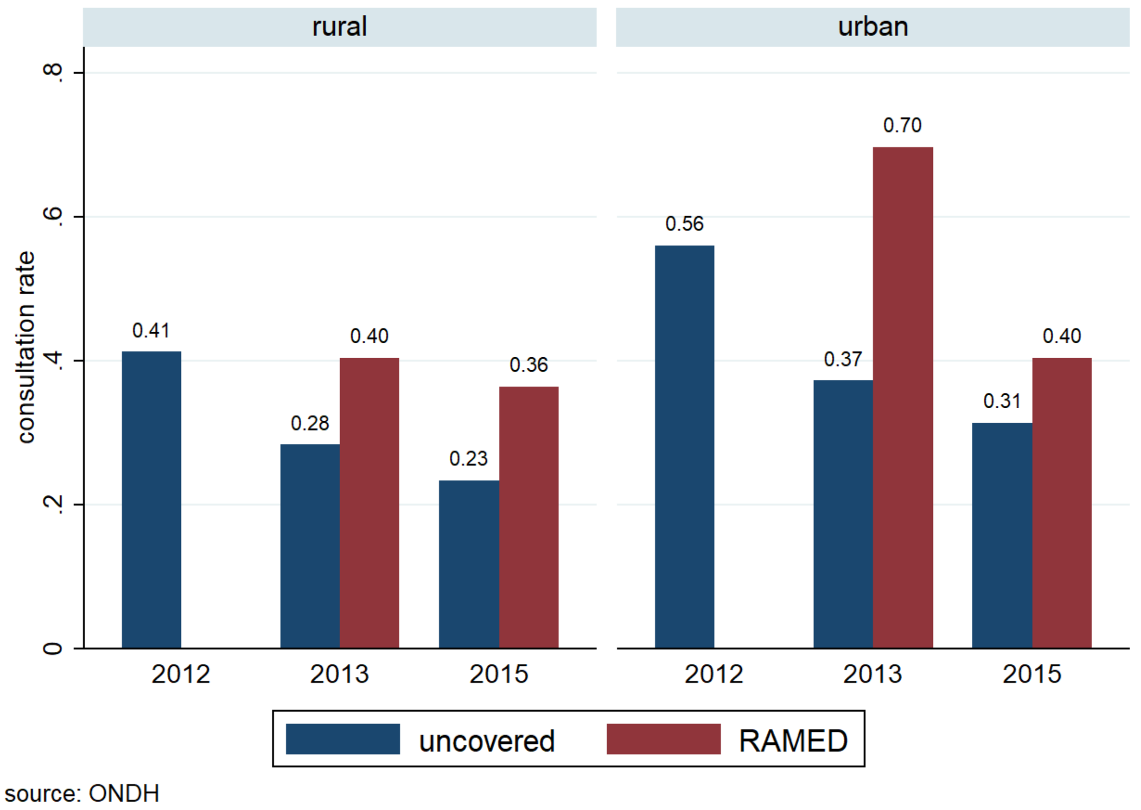


Table 4 gives the average health expenditure per capita, by year and residency, as well as the mean budget share devoted to health. The average health expenditure per capita is lower in the 2013 compared to other years, in urban as well as in rural areas (7% lower compared to 2012 in rural areas; minus 25% in urban areas). This could reflect the effect of RAMED, seasonality, or systematic measurement error.

Table 5 presents descriptive statistics (mean and standard deviation) of the covariates used in our empirical specification, based on the pooling of the 2013 and 2015 waves. In urban areas, households covered by RAMED appear to differ systematically from uncovered ones: their heads are older, they are more likely to be female-headed than their uncovered counterparts, and they comprise more children and females. They also belong to lower wealth quintiles, and have a lower average BMI. In rural areas,RAMED households differ less systematically from uncovered households. This may be a reflection of the higher degree of homogeneity in living conditions prevailing in the countryside. Overall, households covered by RAMED have in common to have a higher rate of self-declared morbidity, as well to be more likely fulfill the theoretical criteria for eligibility to the scheme (60% against 51% in rural areas; 27% against 8% in urban areas).

Table 4: Outcome variables, by year

	rural			urban		
	2012	2013	2015	2012	2013	2015
health exp./pers (DH)	710.7	662.3	767.6	1573.1	1174.1	1535.7
	(2138.7)	(2043.3)	(2513.8)	(4461.1)	(2824.4)	(5249.7)
health budget share (%)	8.3	9.6	9.4	11.2	11.9	11.2
	(8.7)	(8.3)	(8.7)	(10.3)	(8.4)	(9.7)
any consultation (0/1)	0.331	0.246	0.240	0.446	0.334	0.288
consultation in public sector (0/1)	0.154	0.134	0.118	0.211	0.166	0.123
consultation in private sector (0/1)	0.184	0.115	0.118	0.241	0.171	0.161
outpatient consultation (0/1)	0.215	0.148	0.155	0.285	0.201	0.194
hospital consultation (0/1)	0.127	0.100	0.081	0.171	0.137	0.089
Observations	3119	3022	2945	4735	4348	4032

mean of variables, standard errors in parentheses.
monetary values (DH) adjusted for inflation using CPI

6 Results

6.1 Affiliation to RAMED

We first take a look at the estimation of the propensity score for affiliation to RAMED. We use a probit model at the household level to estimate the determinants for "early entrants" (the household who affiliated between 2012 and 2013) and for "followers" (those who affiliated to RAMED between 2013 and 2015). In the appendix, we show the estimate for the entire time interval under consideration (from 2012 to 2015).

The marginal effects for the probit models are shown in tables 6 and 7. We conduct the estimation at the national level (column 1) and by residency (urban or rural). Covariates of our models include characteristics of the household head (age and age squared, education, gender), household structure and activity profile, theoretical elibility to RAMED, quintile of wealth, and various measures of health status at the household level: the number of chronically and temporary ill members, the mean standardized Body Mass Index (BMI) of all members age 20 to 60, and the number of overweight and underweight members in the household, based in individual BMI.

In 2013 (table 6), affiliation to RAMED at the national level is positively linked to the age of the household head (+0.08 p.p.) as well as to his gender: female-headed household have a +0.04 higher probability of being affiliated to RAMED compared to their male-headed counterparts. Affiliation is negatively linked to the education level of the head: households headed by an individual with no or only primary education have a +0.07 probability of being affiliated, compared to an high school-educated

Table 5: Covariates, by residency and coverage status

	rural				urban			
	uncovered	RAMED	diff.	p-val.	uncovered	RAMED	diff.	p-val.
age of hh head	53.68	54.20	-0.52	0.24	52.83	53.84	-1.01	0.02
household size	5.35	5.44	-0.09	0.26	4.44	4.64	-0.20	0.00
# women	2.67	2.72	-0.06	0.24	2.28	2.45	-0.17	0.00
# children (<6 y)	0.84	0.87	-0.03	0.36	0.53	0.58	-0.05	0.06
# elderly (>65 y)	0.43	0.40	0.03	0.13	0.32	0.33	-0.01	0.42
employment rate	0.44	0.43	0.01	0.10	0.41	0.41	0.00	0.60
# independent	0.72	0.66	0.06	0.02	0.55	0.58	-0.03	0.23
# wage workers	0.50	0.54	-0.05	0.04	0.50	0.51	-0.01	0.67
# of chronically ill	0.30	0.41	-0.12	0.00	0.37	0.51	-0.14	0.00
# temporary ill	0.17	0.19	-0.01	0.40	0.16	0.21	-0.05	0.00
mean standardized BMI	23.20	23.18	0.02	0.77	23.70	23.41	0.29	0.00
overweight	0.94	1.00	-0.06	0.04	1.07	1.09	-0.03	0.40
underweight	0.04	0.06	-0.02	0.01	0.03	0.05	-0.01	0.08
female hh head	0.12	0.15	-0.03	0.01	0.24	0.29	-0.05	0.00
primary or lower	0.93	0.94	-0.01	0.14	0.75	0.81	-0.06	0.00
lower secondary	0.05	0.04	0.01	0.25	0.11	0.11	0.00	0.63
upper secondary	0.01	0.01	0.00	0.75	0.09	0.06	0.03	0.00
higher ed.	0.01	0.01	0.00	0.33	0.05	0.02	0.02	0.00
eligible 'poor'	0.12	0.17	-0.05	0.00	0.14	0.18	-0.04	0.00
eligible 'vulnerable'	0.39	0.43	-0.04	0.00	0.06	0.09	-0.02	0.00
1st quintile	0.52	0.48	0.04	0.02	0.02	0.03	-0.01	0.06
2nd quintile	0.36	0.39	-0.03	0.02	0.10	0.12	-0.02	0.05
3rd quintile	0.09	0.10	-0.01	0.31	0.31	0.38	-0.07	0.00
4th quintile	0.02	0.02	0.00	0.65	0.33	0.32	0.01	0.52
5th quintile	0.01	0.01	0.00	0.37	0.23	0.14	0.09	0.00
Observations	5279				4987			

Pooled means for 2013 and 2015 waves. p-val denotes the p-value associated to a t-test (resp. p-test for binary variables) of difference of means.

head; college-educated household heads have a -0.05 probability of being affiliated (the latter coefficient being significant at the 10% level only). The variables reflecting household demographic structure are generally not significant, except for the number of children, which is associated with a +0.01 probability of being affiliated. The proportion of the household in employment is negatively associated to the probability of being in affiliated (-0.05); the number of household members working as independents (as opposed to the omitted category of wage workers) is positively related to affiliation (+0.03). The theoretical eligibility of households to RAMED, as reflected by the thresholds of the "proxy means test", is significantly linked to effective affiliation three years later. Households who are classified as "poor" (i.e. household who are entitled to the RAMED card for free) have a +.027 probability of being affiliated to RAMED in 2015; household who are categorized as 'vulnerable' in 2012 (those who have to pay a yearly premium of DH150 - approx. USD 15), have a +0.025 probability of being in the scheme in 2015. Both coefficients are significant at the 5% level. Wealth is negatively related to affiliation, in the sense that the top 2 quintiles have respectively a -0.04 and -0.12 probability of being affiliated, compared to the 3rd quintile. Finally, variables reflecting the health status and the risk of ill health at the household

Table 6: probit model of RAMED affiliation in 2013, by residency

	(1) national		(2) rural		(3) urban	
urban	0.011	(0.013)	.		.	
age of hh head	0.007***	(0.002)	0.005	(0.004)	0.009***	(0.003)
square age of hh head	-0.000***	(0.000)	-0.000	(0.000)	-0.000***	(0.000)
female hh head	0.034***	(0.012)	0.028	(0.025)	0.031**	(0.012)
education of hh head						
primary or lower	0.074***	(0.020)	0.070	(0.055)	0.066***	(0.018)
lower secondary	0.038	(0.024)	0.014	(0.064)	0.044**	(0.022)
higher ed.	-0.053*	(0.032)	-0.071	(0.096)	-0.033	(0.029)
household structure						
household size	-0.004	(0.003)	-0.001	(0.005)	-0.003	(0.004)
# women	-0.003	(0.005)	-0.011	(0.008)	0.003	(0.006)
# children (<6 y)	0.014***	(0.006)	0.008	(0.009)	0.020***	(0.007)
# elderly (>65 y)	0.015	(0.010)	-0.009	(0.017)	0.034***	(0.011)
employment rate	-0.052***	(0.017)	-0.085***	(0.032)	-0.032	(0.020)
# independent	0.025***	(0.006)	0.019**	(0.010)	0.032***	(0.007)
eligibility to RAMED						
eligible 'poor'	0.027**	(0.013)	0.077***	(0.023)	-0.021	(0.016)
eligible 'vulnerable'	0.025**	(0.011)	0.035**	(0.016)	0.025	(0.018)
quintile of wealth index						
1st quintile	0.017	(0.015)	0.004	(0.025)	0.058*	(0.034)
2nd quintile	0.002	(0.014)	-0.009	(0.026)	0.013	(0.016)
4th quintile	-0.041***	(0.013)	-0.171**	(0.069)	-0.034***	(0.012)
5th quintile	-0.124***	(0.017)	-0.038	(0.061)	-0.116***	(0.016)
health status						
# of chronically ill	0.019***	(0.005)	0.022**	(0.010)	0.017***	(0.006)
# temporary ill	0.011	(0.008)	-0.005	(0.015)	0.018**	(0.009)
mean standardized BMI	-0.004**	(0.002)	-0.001	(0.003)	-0.005***	(0.002)
overweight	0.001	(0.005)	-0.005	(0.009)	0.002	(0.006)
underweight	-0.026	(0.022)	-0.053	(0.041)	-0.013	(0.023)
Observations	6861		2846		4015	

Standard errors in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Marginal effects from a probit model of affiliation to RAMED in 2013 as a function of 2012 covariates.

level are associated with an increased probability of RAMED affiliation: the probability of affiliation increases by 0.02 for each chronically ill household member (temprary illness is not significant at the national level). It is also negatively associated with mean standardized BMI of adult members, which reflects the living standards of households (McLaren, 2007; Wittenberg, 2013). Overall, RAMED affiliation seems to be related to three broad families of factors: negatively to ability to pay (as reflected by education and female head, employment rate and wealth quintile), positively with need for health care (as reflected by illness), and positively to statutory elibility (although this last factor also reflects poor living standards).

Compared to the national level, some differences emerge when considering the determinants by residency (column 2 and 3). Education of the household head is not significant in rural areas; this presumably reflects the fact that the overall level of education is lower in the countryside. Conversely, the

Table 7: probit model of new RAMED affiliation in 2015, by residency

	(1) national		(2) rural		(3) urban	
urban	0.007	(0.017)	.		.	
age of hh head	0.002	(0.003)	-0.001	(0.005)	0.006*	(0.003)
square age of hh head	-0.000	(0.000)	0.000	(0.000)	-0.000**	(0.000)
female hh head	0.071***	(0.014)	0.083***	(0.029)	0.060***	(0.015)
education of hh head						
primary or lower	0.111***	(0.023)	0.132*	(0.070)	0.106***	(0.022)
lower secondary	0.100***	(0.027)	0.143*	(0.078)	0.086***	(0.026)
higher ed.	-0.051	(0.034)	0.117	(0.097)	-0.070**	(0.034)
household structure						
household size	-0.003	(0.004)	0.001	(0.007)	-0.005	(0.005)
# women	-0.001	(0.006)	-0.008	(0.010)	0.003	(0.007)
# children (<6 y)	0.011	(0.007)	0.015	(0.011)	0.006	(0.009)
# elderly (>65 y)	0.021*	(0.012)	0.014	(0.020)	0.030*	(0.016)
employment rate	-0.054***	(0.021)	-0.098**	(0.039)	-0.035	(0.024)
# independent	0.046***	(0.008)	0.041***	(0.013)	0.052***	(0.010)
eligibility to RAMED						
eligible 'poor'	0.090***	(0.016)	0.122***	(0.029)	0.065***	(0.020)
eligible 'vulnerable'	0.063***	(0.014)	0.083***	(0.020)	0.043*	(0.025)
quintile of wealth index						
1st quintile	-0.044**	(0.020)	-0.078**	(0.030)	-0.023	(0.052)
2nd quintile	-0.015	(0.018)	-0.060**	(0.030)	0.021	(0.021)
4th quintile	-0.045***	(0.016)	-0.064	(0.059)	-0.035**	(0.015)
5th quintile	-0.146***	(0.019)	-0.156**	(0.076)	-0.123***	(0.018)
health status						
# of chronically ill	0.011	(0.007)	0.024*	(0.014)	0.007	(0.009)
# temporary ill	0.003	(0.011)	0.011	(0.016)	-0.008	(0.014)
mean standardized BMI	-0.004*	(0.002)	-0.009**	(0.004)	-0.002	(0.003)
overweight	0.007	(0.006)	0.017	(0.011)	-0.000	(0.008)
underweight	-0.029	(0.025)	-0.054	(0.042)	-0.009	(0.031)
Observations	5976		2341		3635	

Standard errors in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Marginal effects from a probit model of affiliation to RAMED in 2015 as a function of 2013 covariates.

strength of the association between statutory eligibility status and effective affiliation seems to be higher in rural areas, as reflected by the coefficients on 'eligible poor' and 'eligible vulnerable' (respectively, 0.8 and 0.4). One interpretation for this is that the range of living conditions is narrower in rural areas. As a consequence, formal eligibility criteria weight more heavily on the decision by the authorities to grant access to RAMED.

The determinants of gaining affiliation between 2013 and 2015, shown in table 7, are similar to those for gaining affiliation between 2012 and 2013. At the national level, the main difference is that the number of chronically ill household members is no longer significant; neither is the number of children or the age of the household head. In contrast, the number of elderly in the household, which was insignificant in table 6, is now positive and significant at the 10% level. At the sub-national level (columns 2 and 3), the main difference with the previous table is that the statutory eligibility to RAMED is now significant in urban areas (although the effect of being classified as ‘vulnerable’ is only significant at the 10% level). One interpretation of these results would be that the “early adopters” are characterized by an acute need for hospital care, as reflected by the significance of the “chronic illness” variable. This could be the result of self-selection, as well as a “top of the pile” effect (officials treating in priority the files for which the need for health care is greatest).

Results for the whole period under consideration (i.e. studying the determinants of affiliating at any time between 2012 and 2015) are shown in table 15 in the appendix, and are mostly consistent with both sets of determinants, and reflect need as well as theoretical eligibility and ability to pay.

6.2 Consultation

Our first outcome under consideration is access to health care, as measured by consultation behaviour at the household level. We start by considering all types of consultation, measured by a dummy variable at the household level. The estimates of our panel difference-in-difference propensity score matching specification are displayed in table 8, for the two sub-periods (2012-2013 and 2013-2015). As usual, we present estimates at the national, urban and rural level. The matching is done through gaussian kernel with a bandwidth of 0.2.

For the first entrants (columns 1 to 3), the diff-in-diff coefficient is positive and significant. The magnitude, is important, corresponding to a +0.11 probability of having any type of consultation at the household level. The effect is stronger in cities (+0.17) than in rural areas (+0.09). However, the high magnitude of the diff-in-diff coefficient comes from the degradation of the outcome for the comparison group between 2012 and 2013: for this group, the probability of having any consultation goes down by 12 percentage points nationally (-0.12 in rural areas and -0.16 in urban areas).

For the households gaining access to RAMED in 2013-2015 (left panel of table 8), the size of the difference-in-difference is lower than in 2012-2013, but the coefficients are still significant at the national level (+0.05) as well as for rural areas (+0.07). In urban areas the diff-in-diff coefficient is positive, but no longer statistically significant. Compared to the previous time period, the time effect is still negative, but of a lower magnitude, and does not reach significance in rural areas.

Table 8: Matching Diff-in-Diff : consultation (0/1), by residency and time period

	2012-2013			2013-2015		
	(1) national	(2) rural	(3) urban	(4) national	(5) rural	(6) urban
year 2013	-0.120***	-0.117***	-0.164***			
	(0.00921)	(0.0112)	(0.0122)			
RAMED	0.0307*	0.00642	0.0145			
	(0.0168)	(0.0190)	(0.0211)			
year 2015				-0.0255***	-0.0112	-0.0309**
				(0.00886)	(0.0152)	(0.0136)
RAMED				0.00535	0.0264	0.000931
				(0.0136)	(0.0214)	(0.0185)
Diff-in-Diff	0.112***	0.0945***	0.172***	0.0501***	0.0724***	0.0218
	(0.0225)	(0.0247)	(0.0331)	(0.0167)	(0.0279)	(0.0269)
constant	0.393***	0.344***	0.490***	0.274***	0.220***	0.309***
	(0.00674)	(0.00908)	(0.0107)	(0.00545)	(0.0106)	(0.00863)
Observations	13826	5714	8086	11788	4710	7067

Standard errors in parentheses
* p<0.1, ** p<0.05, *** p<0.01
bootstrapped standard errors (50 replications)

Public vs. private The RAMED card allows its holder to be treated for free at public facilities. Any increased demand might thus come from two distinct channels (Hangoma et al., 2018): an 'uptake effect', corresponding to an increased overall use of health care, and a 'switching' effect coming from a shift in healthcare demand from private to public facilities. To discriminate between these two possible mechanisms, we run our regression separately for private and public facilities. A negative sign on the latter would be consistent with patients switching from private to public facilities.

The results of our diff-in-diff PSM specification are presented in table 9 and 10. For the 'early adopters' (2012-13), the effect of RAMED is positive and significant for both public and private facilities (+8.3 p.p. for public and +3.2 p.p. for private) at the national level. When segmenting by residency, the diff-in-diff coefficient for public facilities remains positive and significant for both areas; regarding private facilities, the significance persists only in cities. This counterintuitive result may be explained by the fact that, *de jure*, patients wishing to access inpatient care at specialist hospitals need to be referred by first-line facilities (*dispensaires*) or by a GP. In this regard, private consultation is a complement, more than a substitute, to consultation at public facilities.

The results for the second wave of RAMED adherents are presented in table 10. The effect of RAMED on consultations in private facilities is still positive, but no longer significant. The effect on consultations in the public sector is positive everywhere, but significant only at the national level (at the 1% threshold)

Table 9: Matching Diff-in-Diff : consultation by sector, 2012-2013

	national		rural		urban	
	(1) public	(2) private	(3) public	(4) private	(5) public	(6) private
year 2013	-0.0496***	-0.0764***	-0.0364***	-0.0697***	-0.0592***	-0.0818***
	(0.00742)	(0.00720)	(0.00950)	(0.00900)	(0.00998)	(0.00934)
RAMED	0.0579***	-0.0256*	0.0233	0.00116	0.108***	-0.0412**
	(0.0146)	(0.0132)	(0.0187)	(0.0205)	(0.0249)	(0.0176)
Diff-in-Diff	0.0832***	0.0324**	0.0823***	-0.00293	0.0796**	0.0691**
	(0.0219)	(0.0137)	(0.0271)	(0.0268)	(0.0371)	(0.0276)
constant	0.183***	0.219***	0.150***	0.184***	0.208***	0.243***
	(0.00574)	(0.00480)	(0.00768)	(0.00665)	(0.00711)	(0.00644)
Observations	13826	13826	5714	5714	8086	8086

bootstrapped standard errors in parentheses (50 replications)
* p<0.1, ** p<0.05, *** p<0.01

Table 10: Matching Diff-in-Diff : consultation by sector, 2013-2015

	national		rural		urban	
	(1) public	(2) private	(3) public	(4) private	(5) public	(6) private
year 2015	-0.0429***	-0.0102*	-0.0132	-0.00570	-0.0390***	-0.000676
	(0.00822)	(0.00608)	(0.00882)	(0.0110)	(0.00914)	(0.0106)
RAMED	0.0271**	-0.0246**	0.0458***	-0.0111	0.0448**	-0.0422***
	(0.0126)	(0.0106)	(0.0167)	(0.0139)	(0.0182)	(0.0144)
Diff-in-Diff	0.0524***	0.0197	0.0397*	0.0289	0.0329	-0.00238
	(0.0168)	(0.0149)	(0.0212)	(0.0226)	(0.0259)	(0.0201)
constant	0.141***	0.142***	0.103***	0.119***	0.141***	0.169***
	(0.00651)	(0.00515)	(0.00833)	(0.00662)	(0.00697)	(0.00672)
Observations	11788	11788	4710	4710	7067	7067

bootstrapped standard errors in parentheses (50 replications)
* p<0.1, ** p<0.05, *** p<0.01

as well as in rural areas, albeit at the 10% threshold. Estimates over the entire period (2012-2015), presented in the table 17 in the appendix, are consistent with these results: the entire effect of RAMED comes from rural areas (+6.1 p.p), the diff-in-diff effect in urban areas being small and insignificant. There is no strong evidence for 'switching' from private to public facilities, which can be taken as an indicator of the relevance of the policy: in rural areas, demand for health care was indeed being held back by user fees in public hospitals.

Overall, these results are consistent with a gradual congestion of hospital facilities with the scaling up of RAMED. The lack of any significant effect of a removal of user fees on healthcare demand can be explained by supply-side constraints; the fact that we observe an effect only in the first period, during the scaling up of the program, when the number of affiliated households was still small, backs this hypothesis up.

Ambulatory vs. hospital care The RAMED card allows households and individuals covered to access hospital care for free. Basic health care was already provided for free at community health centers and dispensaries. However, their spread over the territory is uneven and the quality of care is heterogeneous (Ministère de la Santé, 2013; Chauffour, 2017). Moreover, at least in cities, patients have the option to visit private practitioners, either in private practices or clinics. As a way of checking that the effects that our models capture is really the effect of the removal of user fees, we examine the differential effect of accession to RAMED on outpatient care vs. hospital care. Outpatient care included consultations at private practices, clinics, community health centers and dispensaries. Due to data limitations, we are not able to distinguish between outpatient and inpatient care at the hospital level.

Table 11: Matching Diff-in-Diff : consultation by type, 2012-2013

	national		rural		urban	
	(1) outpatient	(2) hospital	(3) outpatient	(4) hospital	(5) outpatient	(6) hospital
year 2013	0.000579	-0.0383***	0.00126	-0.0331***	0.000233	-0.0425***
	(0.00115)	(0.00561)	(0.000937)	(0.00982)	(0.00191)	(0.00808)
RAMED	-0.00865**	0.0537***	-0.00636	0.0317*	-0.0119**	0.0865***
	(0.00379)	(0.0142)	(0.00460)	(0.0171)	(0.00573)	(0.0189)
Diff-in-Diff	-0.000579	0.0425**	0.00277	0.0268	-0.00467	0.0584**
	(0.00472)	(0.0170)	(0.00462)	(0.0272)	(0.00922)	(0.0275)
constant	0.996***	0.147***	0.998***	0.121***	0.994***	0.166***
	(0.000834)	(0.00483)	(0.000876)	(0.00802)	(0.00140)	(0.00582)
Observations	13826	13826	5714	5714	8086	8086

bootstrapped standard errors in parentheses (50 replications)
* p<0.1, ** p<0.05, *** p<0.01

The results of the diff-in-diff PSM for the first period (2012-2013) are displayed in table 11. At the national level, the diff-in-diff coefficient is positive and significant for hospital care; it is negative and insignificant for outpatient care. By residency, the diff-in-diff coefficient for outpatient care is positive in urban and rural areas, but significant only in urban areas. There is no evidence of a substitution of hospital care for outpatient care. Notice that the RAMED households were already more likely to consult at hospitals before the scheme was put in place, as evidenced by the coefficient of the 'RAMED' variable, which captures pre-treatment differences between treated and controlled.

The results for the second period (2013-2015) are summarized in table 12. At the national level, the diff-in-diff coefficient is not significant for either type of consultation. At the subnational level, the diff-in-diff is not significant for either type of consultation in urban areas; only in rural is there a positive impact on consultation at public hospitals (+3.8 p.p.), the latter being only significant at the 10% level.

Over the entire period (2012-2015) (cf. table 18 in appendix), getting access to a RAMED card does not have any significant effect on the probability of consulting at a hospital facility at the national level. In rural areas, access to RAMED is significantly linked with a higher probability of having any hospital consultation at the household level (+4.1 p.p.). In cities, the coefficient for hospital care is negative over the whole period (-3.4 p.p.), significant at the 10% level.

Table 12: Matching Diff-in-Diff : consultation by type, 2013-2015

	national		rural		urban	
	(1) outpatient	(2) hospital	(3) outpatient	(4) hospital	(5) outpatient	(6) hospital
year 2015	0.000368	-0.0304***	-0.0000228	-0.0216**	0.000455	-0.0348***
	(0.000908)	(0.00593)	(0.000770)	(0.00906)	(0.00199)	(0.00812)
RAMED	-0.000715	0.0215**	0.000597	0.0285**	-0.00267	0.0215
	(0.00177)	(0.0104)	(0.000579)	(0.0145)	(0.00421)	(0.0149)
Diff-in-Diff	-0.000368	0.0161	-0.00329	0.0381*	0.00260	-0.00791
	(0.00247)	(0.0156)	(0.00216)	(0.0209)	(0.00544)	(0.0146)
constant	0.997***	0.106***	0.999***	0.0824***	0.995***	0.120***
	(0.000705)	(0.00418)	(0.000579)	(0.00638)	(0.00145)	(0.00585)
Observations	11788	11788	4710	4710	7067	7067

bootstrapped standard errors in parentheses (50 replications)
* p<0.1, ** p<0.05, *** p<0.01

Overall, the results presented in this section point towards a positive, albeit modest, effect of RAMED on access to hospital care (and to health care in general) for populations that previously didn't have access to any form of coverage. The effect is greatest in the time period immediately after the inauguration of the scheme, when the numbers of adherents was still relatively modest; over the whole period, it is significantly different from zerop only for rural residents. Our results imply that user fees were a real barriers to health care access for rural residents. This is all the more likely to be the case, that the density of medical services is much lower in rural areas compared to cities. The contrast between the strong effect of RAMED on its adopters in cities in 2012-13 and its insignificance afterwards may reflect congestion effects due to capacity constraints.

6.3 Health expenditures

The objective of the universal health care (UHC) agenda is to provide halth care to all without causing financial hardship. In this section, we examine the effect of gaining acces to 'free' health care on health expenditures at the household level. In theory, the effect of a removal of user fees is ambiguous, if there were some households that were liquidity constrained pre-program. Having access to hospital care free of charge may diminish the financial burden of households due to health care; but on the other hand, the overall spending on health care may increase if some households had previously zero expenditure on health care due to lack of access. This is especially likely to be the case if there are ancillary costs linked to hospital care (for example transport costs or the opportunity costs linked with having one person accompany the sick for time of his treatment), if the case if gratuity is incomplete or if there are informal payments to medical practitioners (Ensor, 2004; Wagstaff and Lindelow, 2008) . Such mechanisms may be at play in the case of Morocco, especially after RAMED was put in place: interviews with medical professionals in the public sector have reported hospitals have faced increased inventory shortages, especially on consumables and medical supplies, after the introduction of the fee waiver, forcing them in some cases to ask the patients to purchase themselves some of the supplies on the private market (ONDH, 2017).

Table 13: Matching Diff-in-Diff : Health expenditures, national

	2012-2013				2013-2015			
	(1) any exp (0/1)	(2) p. cap. (qrt)	(3) p. cap. (ln)	(4) budg share (%)	(5) any exp (0/1)	(6) p. cap. (qrt)	(7) p. cap. (ln)	(8) budg share (%)
year 2013	0.126***	0.314***	-0.270***	0.864***				
	(0.00832)	(0.0526)	(0.0281)	(0.137)				
RAMED	0.0299	0.0732	-0.0983	1.206***				
	(0.0183)	(0.108)	(0.0605)	(0.356)				
year 2015					-0.192***	-0.562***	0.598***	-0.479***
					(0.00822)	(0.0536)	(0.0392)	(0.176)
RAMED					0.00547	-0.0127	-0.0402	0.819***
					(0.0148)	(0.0799)	(0.0572)	(0.294)
Diff-in-Diff	0.0296	0.264*	0.0341	0.764	0.0272	0.107	-0.163*	0.733
	(0.0240)	(0.135)	(0.0893)	(0.514)	(0.0238)	(0.0989)	(0.0958)	(0.471)
constant	0.620***	3.306***	6.439***	9.746***	0.741***	3.600***	6.071***	10.42***
	(0.00669)	(0.0390)	(0.0248)	(0.119)	(0.00586)	(0.0388)	(0.0282)	(0.123)
Observations	13826	13826	8762	13826	11788	11788	7157	11788

bootstrapped standard errors in parentheses (50 replications)
* p<0.1, ** p<0.05, *** p<0.01

We use various indicators of the financial burden of health to estimate the effect of RAMED on health expenditures at the household level. First, we build a dummy equal to one if the household has had any health expenditure over the reference period; second, we take the quartic root of total per capita health expenditure as an indicator of unconditional expenditure on health (the quartic root transformation is used to deal with the right-skewed nature of health expenditures); third, we use the natural logarithm of total per capita health expenditure at as an indicator of conditional health expenditures (i.e. conditional on the household having positive health expenditures), as the transformation eliminates all households with zero expenditure on health; finally, we use the budget share of health in total household consumption, which is an indicator of the financial burden supported by households due to health. As usual, we run our estimations by sub-periods and for rural and urban households separately.

The results at the national level are displayed in table 13. Over the 2012-2013 interval (left panel), the effect of RAMED is positive but generally not statistically significant, except for unconditional expenditures where the effect is significant at the 10% threshold. The coefficient of 0.26 corresponds to a difference in health expenditure per person of around 33 DH, or approximately 3 USD. Over the next interval (right panel), the effect of accessing RAMED is also generally not significant, except for the conditional expenditures, where it is negative and significant at the 10% threshold.

The results by residency over the whole time frame (2012-2015) are displayed in table 14. In rural areas (left panel), the effects are not significantly different from zero for all dependent variables. In urban areas (right panel), the diff-in-diff coefficient is significantly different from zero only for conditional health expenditures (column 7): for the households who have had health expenditures in both survey waves, accession to RAMED is associated with a sizable reduction in total health expenditures (approximately -20% compared to previous wave). Detailed results by subperiods, shown in tables 19 and 20 in the appendix, show that this comes mainly from the latter period.

To summarize, the effect of RAMED on the household's financial burden linked to health appear to be modest, not to say mostly nonexistent. However, these results are consistent with the picture painted by the previous section on access to health care, which implied that user fees were a binding constraint to health care access in rural areas only, which is consistent with the fact that we do not witness a reduction in health care expenditure in those parts of the country. On the contrary, in urban areas, were the effect of RAMED on access was weaker, those households that had a great need for health care may have seen their financial burden lightened, as witnessed by the negative coefficient on conditional health expenditures.

Table 14: Matching Diff-in-Diff : Health expenditures, 2012-2015, by residency

	rural				urban			
	(1) any exp (0/1)	(2) p.cap.(qrt)	(3) p.cap.(ln)	(4) budg share (%)	(5) any exp (0/1)	(6) p.cap.(qrt)	(7) p.cap.(ln)	(8) budg share (%)
year 2015	-0.0175	-0.0614	0.107	0.940***	-0.123***	-0.477***	0.416***	-0.126
	(0.0153)	(0.0883)	(0.0676)	(0.293)	(0.0138)	(0.0832)	(0.0574)	(0.247)
RAMED	0.0440**	0.271**	0.0617	1.524***	0.0105	-0.0474	-0.107	0.961***
	(0.0188)	(0.113)	(0.0800)	(0.378)	(0.0167)	(0.112)	(0.0669)	(0.316)
Diff-in-Diff	0.0381	0.241	-0.0148	0.449	0.00890	0.000321	-0.204**	0.158
	(0.0242)	(0.164)	(0.104)	(0.513)	(0.0232)	(0.185)	(0.0975)	(0.516)
constant	0.561***	2.690***	6.037***	7.686***	0.660***	3.715***	6.649***	11.18***
	(0.0110)	(0.0629)	(0.0431)	(0.177)	(0.0102)	(0.0455)	(0.0315)	(0.219)
Observations	5634	5634	2810	5634	7589	7589	4121	7589

bootstrapped standard errors in parentheses (50 replications)
* p<0.1, ** p<0.05, *** p<0.01

7 Conclusion and discussion

In the past two decades, an opposition has crystallized in policy circles between the "safety nets" agenda, mainly promoted by the IMF and the World Bank, and the advocacy of a broader "social rights" approach, supported by institutions such as the ILO. Broadly speaking, "safety nets" designate programs that are non-contributory, and that aim at protecting the poorest household from the negative consequences of shocks (Grosh et al., 2008). Example of such programs are cash and in-kind transfers (conditional or not), price subsidies, workfare programs, and fee waivers on essential services. The two criteria that distinguish social safety nets from the broader concept of social protection is their non-contributory nature (hence, social insurance schemes are excluded), and, in practice if not in theory, the presence of some sort of targeting to minimize the financial costs of attaining a given social target. By contrast, the broader "rights" agenda puts the emphasis on social protection, universality of benefits, and overall distributional consequences of social programs: inequality reduction is at the forefront of the "rights" agenda, while the foremost concern for the "social safety nets" agenda is concerned mainly with poverty reduction.

One of the policies that has gained traction in the previous decade is the removal of user fees for health services. Several African countries have reversed the policies of cost recovery that had been implemented in the 1990s as part of the structural adjustment programs, due to internal political dynamics as well as the perceived adverse social consequences of those policies (Meessen et al., 2011).

In this article, we study one original example of such a policy: a targeted user fee exemption for hospital care, put in place in Morocco in 2012. This policy, called RAMED (*Régime d'Assistance Médicale*), is original in that the gratuity concerns hospital care, primary care being already free in a network of dispensaries located across the country. Another distinctive characteristic is that RAMED makes use of a proxy means test to target the poorest quartile in the country.

This policy gives us a unique opportunity to test the merits of the social safety net approach in an original, and understudied context. A lower middle-income country, Morocco is characterized with good overall health indicators. It is thus not a foregone conclusion that removing user fees will lead to higher overall health care demand . Indeed, compared to other constraints such as a limited supply of good quality care, user fees may not be the biggest constraint faced by poor households, which would explain a low overall effect. Another issue is that an observed increase in consultations following the introduction of the policy may simply reflect a demand shift from private to public health care, or from first line facilities (community health centers and dispensaries) to public hospitals.

Summary of the results This paper makes use of a nationally representative household panel data sets that covers the period just before the introduction of the policy, during its ramp-up, and after the roll-out. We combine a propensity score matching and a panel difference-in-differences specification to construct a suitable counterfactual for the households that have accessed RAMED, in order to neutralize selection bias coming from observed and time-invariant unobserved characteristics.

Regarding selection, our main results are that effective affiliation to RAMED reflects not only the theoretical eligibility to the scheme, but also health care need and the ability to pay of households. This is consistent with self-selection by households, but it is also points towards an active role of public officials in giving access to some households that may not strictly comply with (outdated and overly sensitive) eligibility criteria. On this matter, the situation differs between urban and rural areas: as criteria differ between these two areas, and are more stringent for rural areas, the theoretical eligibility appears to be more binding for inhabitants of the countryside. By contrast, theoretical eligibility appears to be less binding in cities, with other covariates related to living standards such as the number of children, female-headed households, and education appearing as important determinants of affiliation to RAMED. This is consistent with the literature on decentralized targeting of public benefits that shows that local officials sometimes act on the knowledge they have of the constituents' real living conditions (Alderman, 2002; Mansuri and Rao, 2004).

On the issue of health care demand, our result differ strongly according to the time interval under consideration (i.e. during the ramp-up or after the extension), and according to residency (rural or urban). In the time period just after the inauguration, the result of our estimations point toward a very strong effect of the fee waiver on health care demand, especially in urban areas. For the subsequent time period (as well as over the entire time interval under consideration), the effect of RAMED is significant only for rural areas. We interpret this as evidence that user fees were, in fact, a binding constraint for health care demand for rural households. Moreover, we do not find any evidence of any shift in demand between private and public providers, nor between first-line facilities and hospitals. For rural households at least, free health care seems to be a step in the direction of universal health coverage.

The results are more subdued regarding the financial burden of health care. To the extent that there is an effect of RAMED hospital fee waiver, it is limited to the conditional health expenditures of urban households: those who had a regular health care demand, for instance due to chronic disease, have experienced a reduction in their health expenditure. In rural areas, there are few significant effects of RAMED on financial indicators, one exception being a positive coefficient of small magnitude on the budget share devoted to health in the latter time period (2013-2015). This result is consistent with the previous conclusion that it is mainly poor rural households that faced financial barriers to health care.

Discussion The results presented here suffer from several weaknesses. As with any difference-in-difference study, the interpretation of the results as the average effect of the treatment on the treated (ATT) rests on an untestable parallel trend assumption. In our case, there is some doubt as to suitability of this assumption in the case of early adopters (those who have acceded to RAMED in the first months of the program, in 2012-2013). Firstly, the time coefficient in our model is negative and very large; this is what actually drives the result for the first time period. While we have documented in section 5 that there is a general downward trend in consultation rate (even for categories of households who are *a priori* not subject to any of the effects under study here, namely those who benefit from a proper health insurance), there is still reason to doubt. The large effect displayed in the first time period may also be biased, due to the possibility of "pent-up demand". This refers to the fact that, for some type of illnesses, the individual has some degree of choice as to the timing of the medical intervention, should he need one. Hence, some of the healthcare that occurs just after gaining access to an insurance policy may reflect delayed interventions, rather than the causal effect of the policy. This phenomenon has been extensively studied in other contexts (cf. Card et al., 2009; Franc et al., 2016), but without more detail on individual health conditions, we are not able to control for this phenomenon. For this reason, we put more trust in the estimates covering longer time intervals (i.e. the estimates between 2012 and 2015 or between 2013 and 2015).

Another issue that threatens the causal interpretation of our results is the possibility of congestion effects. Qualitative studies as well informal discussions with members of the medical community reflect the perception that RAMED has led to a saturation of hospital infrastructure from the increased demand, and a flight to the private sector of solvent patients (ONDH, 2017). This is a problem for our estimation, because it amounts to a violation of the "stable unit treatment value assumption" (SUTVA), which requires that there is no interference and no contamination of nontreated units through the treatment itself (Angrist and Pischke, 2009). Such an phenomenon would threaten the internal as well as the external validity of our results. One silver lining is that, should this 'negative contamination bias' be present here, it would go in the opposite direction to the bias mentioned in the previous paragraph.

Finally, to the extent that our results conclude to an increased health care demand, how much of it is due to previously unfulfilled needs, and how much of it represent moral hazard? For anybody having visited a public hospital in Morocco, the idea individuals would "over-consume" medical care just because it is free may appear doubtful. Yet the issue cannot be shrugged off, and is of particular interest to the policymaker which has to decide on the use of scarce resources to finance such a program (the total theoretical financial cost of RAMED is estimated at 0.5% of GNI, although as of date the program remains underfunded). One theoretical argument against this interpretation is the model by Nyman (2008) showing that in the case of health insurance, some of the moral hazard is actually welfare-increasing, due to inherent income transfer between the healthy and the sick that insurance program entails. But ideally, one would like to make the case against moral hazard on empirical arguments rather than on theory alone; and for this, data is lacking as well.

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Appendix

Figure 4: Consultation rates by year and coverage type

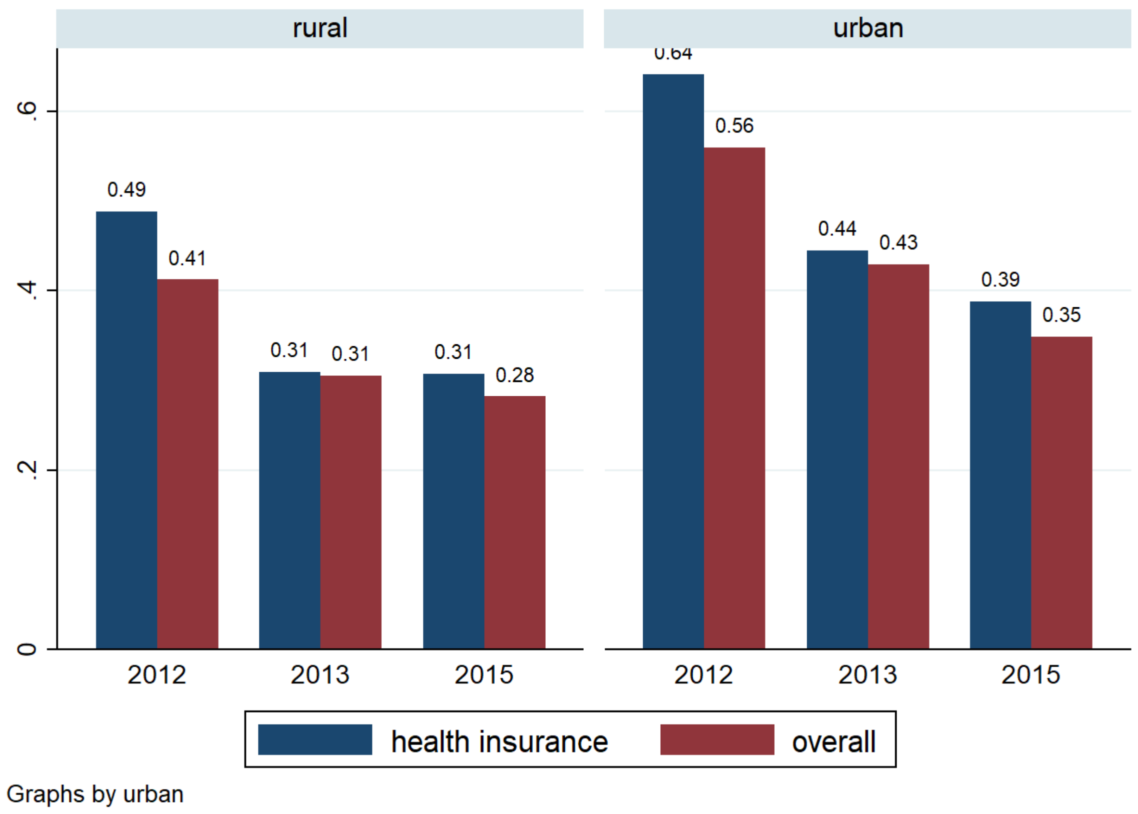

Table 15: probit model of RAMED affiliation in 2015, by residency

	(1) national		(2) rural		(3) urban	
urban	0.009	(0.018)	.		.	
age of hh head	0.006**	(0.003)	0.003	(0.005)	0.009**	(0.004)
square age of hh head	-0.000**	(0.000)	-0.000	(0.000)	-0.000***	(0.000)
female hh head	0.091***	(0.016)	0.110***	(0.031)	0.078***	(0.017)
education of hh head						
primary or lower	0.140***	(0.025)	0.121*	(0.068)	0.139***	(0.025)
lower secondary	0.123***	(0.029)	0.083	(0.078)	0.127***	(0.029)
higher ed.	-0.045	(0.037)	0.037	(0.103)	-0.041	(0.037)
household structure						
household size	-0.009**	(0.004)	-0.005	(0.007)	-0.012*	(0.006)
# women	0.002	(0.006)	-0.001	(0.009)	0.006	(0.008)
# children (<6 y)	0.013*	(0.007)	0.011	(0.011)	0.012	(0.010)
# elderly (>65 y)	0.009	(0.013)	-0.017	(0.021)	0.025	(0.017)
employment rate	-0.019	(0.023)	0.030	(0.040)	-0.050*	(0.027)
# independent	0.058***	(0.008)	0.029**	(0.012)	0.085***	(0.010)
eligibility to RAMED						
eligible 'poor'	0.122***	(0.017)	0.195***	(0.029)	0.064***	(0.022)
eligible 'vulnerable'	0.076***	(0.015)	0.114***	(0.020)	0.033	(0.026)
quintile of wealth index						
1st quintile	-0.021	(0.021)	-0.069**	(0.032)	0.059	(0.052)
2nd quintile	-0.006	(0.019)	-0.056*	(0.032)	0.036	(0.023)
4th quintile	-0.056***	(0.017)	-0.114*	(0.065)	-0.046***	(0.016)
5th quintile	-0.190***	(0.021)	-0.142*	(0.075)	-0.172***	(0.020)
health status						
# of chronically ill	0.025***	(0.007)	0.026**	(0.013)	0.027***	(0.009)
# temporary ill	0.011	(0.011)	0.021	(0.019)	0.005	(0.013)
mean standardized BMI	-0.007***	(0.002)	-0.010**	(0.004)	-0.006**	(0.003)
overweight	0.005	(0.007)	0.014	(0.011)	-0.000	(0.008)
underweight	-0.012	(0.026)	-0.015	(0.043)	-0.012	(0.030)
Observations	6510		2781		3729	

Standard errors in parentheses
* p<0.1, ** p<0.05, *** p<0.01

Marginal effects from a probit model of affiliation to RAMED in 2015 as a function of 2012 covariates.

Table 16: Diff-in-diff matching: consultation (0/1), 2012-15

	(1) national	(2) rural	(3) urban
year 2015	-0.148***	-0.112***	-0.179***
	(0.00983)	(0.0117)	(0.0125)
RAMED	0.0371**	0.0487***	0.0403**
	(0.0148)	(0.0184)	(0.0166)
diff-in-diff	0.0286	0.0494**	0.000359
	(0.0201)	(0.0220)	(0.0217)
constant	0.389***	0.316***	0.449***
	(0.00692)	(0.00988)	(0.00937)
Observations	13242	5634	7589

Standard errors in parentheses
bootstrapped standard errors (50 replications)
* p<0.1, ** p<0.05, *** p<0.01

Table 17: Matching Diff-in-Diff : consultation by sector, 2012-2015

	national		rural		urban	
	(1) public	(2) private	(3) public	(4) private	(5) public	(6) private
year 2015	-0.0831***	-0.0802***	-0.0565***	-0.0667***	-0.105***	-0.0926***
	(0.00671)	(0.00681)	(0.0114)	(0.0119)	(0.00878)	(0.0110)
RAMED	0.0563***	-0.0199*	0.0295*	0.0233	0.0905***	-0.0553***
	(0.0119)	(0.0119)	(0.0156)	(0.0169)	(0.0182)	(0.0159)
Diff-in-Diff	0.0329**	0.00148	0.0614***	-0.0109	-0.00185	0.0127
	(0.0132)	(0.0130)	(0.0209)	(0.0215)	(0.0234)	(0.0196)
constant	0.180***	0.219***	0.145***	0.177***	0.210***	0.252***
	(0.00562)	(0.00497)	(0.00834)	(0.00914)	(0.00639)	(0.00800)
Observations	13242	13242	5634	5634	7589	7589

bootstrapped standard errors in parentheses (50 replications)
* p<0.1, ** p<0.05, *** p<0.01

Table 18: Matching Diff-in-Diff : consultation by type, 2012-2015

	national		rural		urban	
	(1) outpatient	(2) hospital	(3) outpatient	(4) hospital	(5) outpatient	(6) hospital
year 2015	0.000880	-0.0715***	0.000557	-0.0591***	0.00125	-0.0818***
	(0.00106)	(0.00576)	(0.000882)	(0.00861)	(0.00195)	(0.00769)
RAMED	-0.00708***	0.0381***	-0.00200	0.0242*	-0.0126***	0.0575***
	(0.00217)	(0.00916)	(0.00200)	(0.0142)	(0.00477)	(0.0157)
Diff-in-Diff	0.00387	0.00494	-0.000551	0.0410**	0.00835	-0.0344*
	(0.00250)	(0.0110)	(0.00284)	(0.0178)	(0.00593)	(0.0183)
constant	0.997***	0.146***	0.999***	0.119***	0.996***	0.168***
	(0.000819)	(0.00472)	(0.000755)	(0.00781)	(0.00145)	(0.00764)
Observations	13242	13242	5634	5634	7589	7589

bootstrapped standard errors in parentheses (50 replications)
* p<0.1, ** p<0.05, *** p<0.01

Table 19: Matching Diff-in-Diff : Health expenditures, 2012-2013, by residency

	rural				urban			
	(1) any exp (0/1)	(2) p.cap. (qrt)	(3) p.cap. (ln)	(4) budg share (%)	(5) any exp (0/1)	(6) p.cap. (qrt)	(7) p.cap. (ln)	(8) budg share (%)
year 2013	0.139***	0.397***	-0.290***	1.331***	0.116***	0.259***	-0.254***	0.551**
	(0.0133)	(0.0663)	(0.0522)	(0.226)	(0.0100)	(0.0648)	(0.0443)	(0.237)
RAMED	0.0469*	0.368***	0.161	2.170***	0.0306	-0.0283	-0.209**	0.895*
	(0.0283)	(0.131)	(0.113)	(0.458)	(0.0231)	(0.134)	(0.0814)	(0.481)
Diff-in-Diff	0.0361	0.258	0.0348	0.152	0.0173	0.237	0.0488	1.240*
	(0.0357)	(0.176)	(0.157)	(0.749)	(0.0257)	(0.162)	(0.104)	(0.656)
constant	0.567***	2.722***	6.043***	7.835***	0.658***	3.724***	6.684***	11.09***
	(0.0117)	(0.0529)	(0.0388)	(0.157)	(0.00848)	(0.0490)	(0.0323)	(0.183)
Observations	5714	5714	3374	5714	8086	8086	5385	8086

bootstrapped standard errors in parentheses (50 replications)
* p<0.1, ** p<0.05, *** p<0.01

Table 20: Matching Diff-in-Diff : Health expenditures, 2013-2015, by residency

	rural				urban			
	(1) any exp (0/1)	(2) p.cap. (qrt)	(3) p.cap. (ln)	(4) budg share (%)	(5) any exp (0/1)	(6) p.cap. (qrt)	(7) p.cap. (ln)	(8) budg share (%)
year 2015	-0.146***	-0.407***	0.438***	-0.311	-0.221***	-0.642***	0.731***	-0.525**
	(0.0149)	(0.0856)	(0.0690)	(0.241)	(0.0134)	(0.0707)	(0.0452)	(0.228)
RAMED	0.0255	0.219	0.108	1.168***	-0.00175	-0.0897	-0.0706	0.885**
	(0.0216)	(0.137)	(0.0832)	(0.380)	(0.0158)	(0.105)	(0.0640)	(0.386)
Diff-in-Diff	0.0467	0.272	-0.0437	1.227**	-0.00294	-0.107	-0.208**	0.169
	(0.0301)	(0.177)	(0.129)	(0.587)	(0.0261)	(0.157)	(0.0950)	(0.570)
constant	0.698***	3.064***	5.684***	8.903***	0.770***	3.958***	6.309***	11.43***
	(0.0104)	(0.0593)	(0.0432)	(0.165)	(0.00622)	(0.0468)	(0.0261)	(0.156)
Observations	4710	4710	2755	4710	7067	7067	4399	7067

bootstrapped standard errors in parentheses (50 replications)
* p<0.1, ** p<0.05, *** p<0.01