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# Is Results-Based Aid More Effective than Conventional Aid? Evidence from the Health Sector in El Salvador*

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**Abstract**

Results-based aid (RBA) models link funds to outcomes, rather than paying for inputs. Despite their theoretical appeal and recent adoption by donors and multilateral development banks, there is limited empirical evidence supporting this form of aid for national governments. We estimate the effects of a RBA model using a natural experiment in El Salvador, where the same community health intervention was implemented in 98 municipalities using one of three financing models. The Salud Mesoamerica Initiative funded fourteen municipalities with a RBA model that partially conditions funds on the attainment of externally measured maternal and child health targets. Fifty-four municipalities funded inputs using conventional aid and thirty had national funds. Using a difference-in-difference approach and national health systems data we find that preventive health services increased by 19.8% in conventional aid municipalities and by 42% in RBA municipalities compared to national funds, suggesting that the results-based conditionality roughly doubled aid effectiveness. Effects are driven by increases in maternal and child preventive services incentivized by the RBA model. Rather than diverting resources from other populations, we find that the expansion of health services under RBA also benefited men and the elderly, not explicitly incentivized by the results model. While data on final health outcomes are not available, our results on proxy measures point to potential improvements in population health. The effects appear to have been driven by a more rapid expansion of health infrastructure and qualified personnel by motivated national authorities.

**Keywords**: Foreign Aid, Results Based Aid, Performance Incentives, Health Services, El Salvador
JEL codes: F35; J13; I18; H51

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## 1. Introduction

According to the OECD, foreign aid totaled $125bn USD in 2015.[1] In countries that depend highly on foreign aid, these transfers can amount to 16.1% of a country's GDP (Djankov, Montalvo and Reynal-Querol 2008, pp. 173). Whether foreign aid fosters wellbeing and the form in which it should be delivered have been extensively debated. Moreover, the empirical evidence of aid's effects on economic growth and improvements in social indicators is mixed.[2] Amongst reasons proposed for the divergent findings are the degree of aid fungibility (Pack and Pack 1993), political incentives of recipient governments (Moss, Pettersson and Van de Walle 2008), and agency problems between donors and recipients (Cordella and Dell'Ariccia 2007).

In recent years, and partially as a response to the lack of consistent evidence about the positive effects of development aid, donor and multilateral organizations have implemented new aid models that link funding more closely to results. For instance, multilateral banks have introduced lending instruments that disburse funds conditional on achieving outputs or outcomes rather than funding inputs upfront under conventional aid.[3] These funding models are referred to in the literature by different names such as *Results Based Aid (RBA)*[4], *Results Based Funding, Results Based Financing, Cash on Delivery,* and *Pay for Performance*.[5] From the agency theory perspective, the potential impact of delivering aid linked to results depends on the characteristics and number of tasks to perform, the type of contract, whether preferences are aligned between donor and agent, and the type of preferences (over actions or outcomes) (Benabou and Tirole

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[1] This statistic comprises "aid statistics on official and private flows in terms of disbursements and commitments and includes aggregate data (no breakdown by recipient) on official development assistance (ODA), other official flows (OOF), private and NGO data by donor, type of aid and flow. The data covers development resource flows (net disbursements, grant disbursements, loan commitments) from all bilateral and multilateral donors. Data are available from 1960 onwards." See https://www.theguardian.com/global-development/2015/apr/08/foreign-aid-spending-2014-least-developed-countries. Accessed 01/12/2017.

[2] Some examples of studies on this topic include Alesina and Dollar (2000), Benham (1962), Bourguignon and Sundberg (2007), Burnside and Dollar (2000, 2004), Collier and Dollar (2001), Doucouliagos and Paldam (2009), Easterly (2003), Easterly (2007, 2008), Easterly et al. (2004), Morrisey (2015), Qian (2015), and World Bank (1998).

[3] Some specific examples are the World Bank Program for Results, the Inter-American Development Bank Results-Based Loans, and the Asian Development Bank Results-Based Lending. See https://www.cgdev.org/publication/introduction-cash-delivery-aid-funders for some examples of programs.

[4] For the purposes of this paper, we will refer to arrangements between a donor and a national government that ties funds (fully or partially) to measurable results (outputs or outcomes) as Results-Based Aid (Perakis and Savedoff 2015). This is to be distinguished from traditional conditionality in aid in which financial support is linked to implementing policy reforms favored by donors or limits how resources are spent. Moreover, we use RBA to distinguish from Results-Based Financing as in Pearson et al (2010), which uses the latter to describe financing schemes for providers (individuals, enterprises, NGOs) rather than national governments.

[5] Conditioning aid on results has been criticized by others who argue that conditionalities might be hard to enforce (Killick 1997) or may induce unnecessary effort investments to meet them (Berg 1997). See also Svensson (2003). See Clist (2016) for a review about pros and cons of RBA.

2003; Holmstrom and Milgrom 1991).[6,7] As such, whether RBA models might work better than conventional aid remains an open question for which more empirical studies are needed.

In this paper we estimate the effects of RBA using a unique natural experiment in El Salvador. Starting in 2009, the country implemented a health reform that expanded services in the 98 poorest municipalities using community health teams composed of physicians, nurses and community health workers assigned to specific catchment areas. To finance this expansion, the national government garnered domestic budget (national funds), multilateral credits, and donor funding, resulting in different funding sources for each municipality. One of the donors was the *Salud Mesoamerica Initiative* (SMI), which funded 14 municipalities. SMI combines donor and national funds (50/50) to improve maternal and child health services for the poorest populations in the region.[8] If a recipient country achieves pre-specified health targets in the intervention areas, a "performance tranche" incentive equal to 25% of the funding package is reimbursed with the only restriction that the government spends the additional resources within the health sector.

We use rich national health systems data and a difference in differences strategy to identify the impacts of SMI's RBA scheme on the delivery of health services, our measure of effectiveness. We distinguish between effects on maternal and child health services explicitly incentivized by the RBA and services for other populations covered under the community health model. To isolate the effects of the RBA, we compare outcomes in RBA funded municipalities and conventional aid municipalities to outcomes in municipalities funded with national funds. The difference between these two provides a plausible estimate of the results based conditionality of RBA.[9] An important characteristic of the health systems data used in the analysis is that it was not used for evaluating the RBA conditions, and as such is not subject to deliberate differential reporting.

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[6] Holmstrom and Milgrom (1991) illustrate the multitasking problem in principal-agents models. The empirical evidence on the shifting on effort is mixed since it depends on the form of the health production function (Miller and Babiarz 2013). An illustration of this can be found in Gertler and Vermeersch (2012) in the setting of the pay-for-performance scheme for health care providers in Rwanda. While they find an effect of increased institutional delivery, they don't find an effect for early prenatal care visits, both of which were incentivized outcomes. They argue that increased institutional delivery came from patients already in contact with the health system and hence less costly to recruit, while early prenatal care required identification of pregnant women not yet in contact with the health system and hence more costly to serve.

[7] Azam and Laffont (2003) and Cordella and Dell'Ariccia (2007) propose an application of this model to the case of international aid. However, even if priorities are aligned but the costs to meet targets are too high in terms of effort and resources, relative to the benefit of obtaining the reward, the incentives will not be effective (Clist 2016). Moreover, when agents are multitasking, incentives may turn inefficient as agents optimize towards activities with the largest net benefit.

[8] The Mesoamerica region for the purposes of SMI is comprised of 8 countries: Mexico (Chiapas), Belize, Guatemala, El Salvador, Honduras, Nicaragua, Costa Rica and Panama.

[9] Conventional input-based aid is different from national funds since they are earmarked to finance the health reform, and as such are less fungible. In addition, since conventional aid resources are attached to monitoring and auditing by donors they have an implicit commitment device that is not necessarily active when national funds are used. If the effect of RBA is the additive effect of conventional input-based aid (A) and conditioning on results (R), then the difference between RBA and A is (A+R)-A = R, the effect of conditioning aid on results.

We find that RBA leads to an increase in the provision of health services, driven by preventive visits for women and children, precisely the services incentivized by the results-based model. Our results are robust to alternative specifications including different formulations of the comparison group, alternative specifications of the treatment variable, and different functional forms. We find evidence of positive spillovers to men and the elderly, which are consistent with the broader objectives of the health reform. Our results show geographical spillovers of aid to bordering non-RBA municipalities, but no differential effects of RBA. Final health outcomes are not available, but proxy measures of health from hospital discharge data provide suggestive evidence of improvements in population health in RBA municipalities that are consistent with large increases in preventive care. While coefficients are estimated imprecisely and are not statistically significant, they point towards potential longer-run impacts.

The main driver of increased production of health services in RBA municipalities appears to be a faster expansion of health facilities and strengthening of the health workforce with higher-skilled health practitioners such as nurses and doctors. Available qualitative evidence suggests that the more rapid expansion of services was motivated by the RBA performance incentives, which national officials viewed as a flexible funding source for additional investments in the health reform nation-wide. Health authorities may have also been motivated by the reputational repercussions of participating in a regional initiative (Regalia et al. 2017).

Our results contribute to an extensive academic debate about aid effectiveness from international donors to developing countries.[10] These studies usually compare cross-country evidence over time to estimate the effects of aid through growth regressions, with mixed results (Clemens et al. 2012; Galiani et al. 2017; Rajan and Subramanian 2008). Our study improves upon previous literature in several ways. First, we have variation in the type of aid delivered within the same country and in the same policy and political environment, avoiding confounding factors such as the context in which aid is delivered or cross-country variation in cultural barriers to aid. Second, we study a relatively short time-horizon, reducing the potential for confounding effects from other policies or programs, which are more likely to affect studies that use long panels of data at the country level. Our results are also related to the literature on the effectiveness of block grants (Musgrave 1997). The results in this paper are most similar to Olken et al. (2014) who show that performance incentives to local governments improve the implementation of educational and preventative health programs. The case we analyze here is different in that performance incentives were provided to a centralized planner, i.e. national government, while

[10] See Easterly (2008) for a review.

the implementation of the health reform was managed at a decentralized level, i.e. municipal governments. Thus, our results should be of interest to policy makers and researchers interested in policy design, implementation, and management at national levels (Prittchet and Woolcock 2004).

The paper is structured as follows. First, we describe the RBA model and its implementation in El Salvador. The next section describes the administrative data sources used in the analysis. Section four discusses the empirical strategy and section five presents the results of RBA on the production of health services and ambulatory care sensitive hospitalizations. Section six performs several robustness checks. Section seven explores potential pathways by analyzing changes in health units and personnel in RBA and comparison municipalities, and section eight discusses the findings and concludes.

## 2. The Salud Mesoamerica Initiative and the Health Reform in El Salvador

The Salud Mesoamerica Initiative (SMI)[11] aims to reduce health inequities by extending coverage and improving the quality of health interventions for the poorest 20% of households in the Mesoamerica region, focusing primarily on maternal and child health. SMI provides grants equal to 50% of the project value while countries contribute the other 50%. SMI then offers governments a results-based incentive payment of 25% of the total value of the funding envelope, conditional on achieving results. Targets are agreed between SMI and governments for a set of key indicators related to inputs and quality of care, utilization of services and health outcomes. If a weighted average of 80% of targets is achieved at the end of each funding period, the government receives the "performance tranche" incentive payment.[12] SMI performance indicators are monitored by an independent third party using household and facility surveys (Mokdad 2015).

In El Salvador, SMI is organized in two closely linked phases.[13] The SMI performance framework included 10 indicators in the first phase and 11 in the second phase, each with pre-specified targets. Most of the indicators for the first phase are related to the supply of inputs at

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[11] SMI is a public-private partnership funded through the Bill and Melinda Gates, the Carlos Slim Foundations and the Government of Spain. It is implemented by the Inter-American Development Bank.

[12] If targets are not met, the performance tranche is not paid for that period and continued funding is conditional on meeting targets in the following period. See www.sm2015.org and Cruz and Martinez (2016) for additional details of the SMI results based model.

[13] Interventions for the first phase include improving the supply chain of essential health care inputs, designing, and updating country norms and protocols, training health personnel, supporting service delivery platforms and establishing the Essential Obstetric and Neonatal Care (EONC) strategy. Each phase lasts between 18 to 24 months and performance incentives are conditioned on payment indicators and targets related to the objectives of each phase. The specific interventions supported by SMI and the associated performance framework are country-specific and agreed jointly with the country government. The original design had two phases, however a third phase was approved in 2017 and is expected to begin in 2018.

the primary level for family planning, prenatal and child preventive care and vaccination storage. The second and final phase focused in improving coverage of family planning, maternal and child services, as well as the timeliness and quality of maternal care. The full performance framework for El Salvador for the first and second phase is presented in Tables 1A and 1B respectively.

Total investment from SMI for the first phase was $8,125,000 including both initiative and country funds. The incentive tranche was $1,625,000, equivalent to around one percent of the 2015 Ministry of Health (MoH) budget for primary level care. The government effectively reached 80% of the performance targets for the first phase, and was paid the performance tranche in June of 2014. The detailed results of the first phase can be found in Table 1A. Given the timing of the intervention and data availability, our analysis concentrates on the effects of the first phase of SMI in El Salvador.

The launch of SMI in El Salvador coincided with the roll out of the country's health reform, which sought to reduce health inequities amongst the extreme poor through expanded access to health services. The health reform implemented a community-based model of health service provision prioritizing the country's poorest municipalities.[14] One of the reform's most important aspects was the reorganization of primary health service provision around community health teams (Ecos F). Ecos F are responsible of serving approximately 600 families in rural areas and 1,800 families in urban areas in well-defined areas of influence using a combination of home visits and in-facility medical services determined by national guidelines and based on individual health risk assessments.[15] The composition of each Ecos F team (rural or urban) in terms of human resources, with a physician, nurse and community health agent, was clearly established in national norms as was the portfolio of services, with a clear focus on preventive care.[16]

During, the first stages of the reform, the government prioritized its implementation in the country's 98 poorest municipalities using existing poverty maps.[17] The Ministry of Health inventoried the needs of those municipalities to fully implement the reform in terms of health units,

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[14] For a complete description of the health reform see Rodriguez, Maria Isabel "La Reforma de Salud en El Salvador", 2009, available at http://www.paho.org/els/ (accessed November 25, 2017).

[15] These assessments or *dispensarización* are a central part of the functioning of Ecos F. They consist of a census of the area of influence conducted by the team, where risks are established based on socio-demographic, family, and environmental profiles for each family. This family census or *fichas familiares* are then used to organize primary care for the community and is the first activity done once the Ecos F is established.

[16] See MINSAL (2011) for a detailed description.

[17] The MoH initially used a municipal poverty map constructed from 2002-2004 household data for targeting social and health programs. The poverty map was later updated by IDB to identify municipalities with extreme poverty using 2007 Census data, the most recent to date. Tejerina (2011) describes and compares these two targeting mechanisms and how they were used for identifying SMI funded municipalities. The country implemented the reform in five stages, with the first two stages targeting the poorest municipalities and those with the highest chronic malnutrition (Rodríguez, 2009). The focus here is on the 98 poorest municipalities only targeted in the first two stages, that occurred in 2010 and 2011 since this criteria was the one used to define SMI municipalities.

personnel and supplies according to the guidelines put forward in the reform. In addition, it established the needs not only of primary (outpatient) care but also those of secondary (inpatient) care and the networks that articulated them. Given that the set of needs from this diagnosis exceeded the national funding capacity, the government identified funds from external partners including the Inter-American Development Bank (IDB), the World Bank, the government of Japan and private funds from SMI. The overarching objective of the government was to fund the reform in its entirety in as many of the poorest municipalities as quickly as possible.[18]

SMI was launched in El Salvador in 2010 during the early planning and implementation of the health reform. The country's national health authorities prioritized the new SMI funds to fill funding gaps for implementation of the health reform in municipalities with incomplete or no funding (see Appendix C for the complete list of municipalities with funding sources). SMI funds were allocated to the poorest municipalities amongst those with remaining funding gaps[19]. First, municipalities were rank ordered according to their poverty level using information from the most recent census. An inventory of funding gaps was conducted for each municipality, starting with the poorest, and SMI funds were allocated to close the gap. This process was continued until the total funding of SMI was exhausted, reaching a total of 14 municipalities.[20] Municipalities with complete funding from the national government, credits or other donors were not included as part of SMI.

## 3. Data

To analyze the effect of the RBA, we use administrative health systems data from the MoH in El Salvador, covering all outpatient and inpatient services provided by the MoH in the country. For all datasets, we have access to detailed micro-level data at the individual service level (i.e. outpatient visit or hospital discharge), from the earliest period available in electronic format. Prior to the reform, in 2008, around 96 percent of maternal and child preventive care in rural areas was provided in public facilities of which close to 90 percent was in the MoH.[21]

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[18] This account of the implementation of the reform is based on Tejerina (2011) and conversations with government and IDB officials who participated in the process at the time.

[19] While per-capita expenditures under the health reform model were the same across all municipalities, municipalities with funding gaps tended to have larger populations and thus require more funds in absolute terms. No other criteria except for funding gaps in the poorest municipalities was used to determine placement of SMI funding. Since SMI required 50-50 co-financing from the national government, most SMI funded municipalities were also funded with funds from IDB credits as counterpart funding, meaning they were also subject to conventional aid requirements.

[20] A detailed account of the selection of municipalities for SMI can be found in Tejerina (2011).

[21] Estimates based on the *Encuesta Nacional de Salud Familiar* 2008. Rural areas comprise most of the municipalities prioritized for the reform. The other main public provider is the ISSS (*Instituto Salvadoreño de Seguridad Social*), which has more relevance in urban

*Health visits data:* Data on outpatient visits spans from 2009 to 2015 and contains information on the unit providing the service, basic patient information (sex, age, municipality of residence) and the reason for the visit, which is classified according to preventive and curative, and the main diagnosis ICD-10 codes. Data on preventive care is further classified by first visit and subsequent visits for family planning, prenatal, and infant care. For prenatal and infant care, the data allows us to identify early detection of pregnancy (first health visit in the first trimester of pregnancy) and age of the child at the first infant visit. All data is compiled at the local level by the physician or nurse that performed the consultation (including its classification as preventive or curative) and then reported to the central level for digitalization, coding of ICD-10 codes, review and aggregation.

*Hospital discharges data:* Data on hospital discharges are available for a longer period (the electronic systems were first put into place in secondary and tertiary care units) spanning 2005 to 2015. These data have a similar structure and variables as outpatient care, but the reason for hospitalization only includes the ICD-10 code of the main diagnosis. We use these codes to distinguish between obstetric and non-obstetric hospitalizations.[22]

*Family records data*: a household census performed by community health teams in their area of influence. It was collected from 2010 to 2013. Family records (*fichas familiares*) capture data on household, family and individual characteristics and are used to obtain health risk profiles to guide the provision of primary care services at the community level and are a central part of medical records established by the health reform. These records are one of the first activities of community health teams and as such we use them to measure the onset of the reform.

We aggregate data on outpatient visits and discharges by quarter and municipality of residence of the patient for outcomes related to the provision of services. While ideally these counts would be converted to municipal level rates for purpose of analysis (dividing the number of services by the population of interest), the only population data available is from projections based on the most recent census, which date back to 2007. Given a large degree of measurement error in these estimates, we prefer to control for common time trends, including population changes, as explained in the next section. Finally, it is important to note that the health systems data used in this study was not used to evaluate the RBA performance indicators, as SMI required that these be measured by a third party through medical records reviews, health facility and

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areas. The share of services provided by the MoH was relatively similar in 2014 based on data from UNICEF's MICS, where the share of maternal preventive care provided by public facilities was 97 percent of which the MoH provided 93 percent.

[22] Obstetric hospitalizations are those of Chapter XV of ICD-10, which includes conditions related to pregnancy, childbirth and the puerperium. About half of the discharges with this ICD code are deliveries in our data.

household surveys and audits. A more detailed discussion of the role that reporting could have on our results is presented in Section 6.

## 4. Empirical Strategy

Identification of the effects of RBA requires identifying a credible counterfactual, i.e. what would have happened if funding for the 14 RBA municipalities was obtained through either conventional aid or national funding. As discussed in section 2, SMI provided funding for 14 of the 98 poorest municipalities prioritized for the new health reform model. Other international organizations (e.g., country foundations, IDB, World Bank) financed 53 municipalities using conventional aid mechanisms (fully or partially), while the remaining 31 municipalities were financed exclusively using national budget and were not linked to multilateral credits or other donor funds.

Table 2 presents baseline statistics for the three groups of municipalities. The last columns show the p-values for the differences in means between the three groups. Variables in the first panel are computed at the municipality level. Municipalities in the RBA group are larger in terms of population size but show no significant differences in terms of the percentage of households below the poverty line. The table also shows that RBA municipalities joined the reform slightly later, with a lower share of families enrolled in the health reform during year 2010 as opposed to year 2011. In addition, RBA municipalities produced more outpatient visits as a share of their population, mainly driven by a larger number of curative visits. In terms of health resources per 1,000 people, RBA municipalities had a lower rate of health units available and hence a lower rate of doctors, nurses, or community health workers (CHWs) before the intervention started. The next groups of variables are computed from the family records survey. The results show that the groups of municipalities by funding source are very similar in terms of household level and individual level characteristics. Although our empirical strategy controls for cross sectional differences by including municipality fixed effects, the similarities shown in Table 2 across groups supports the hypothesis that selection into different types of funding is negligible, except for an evident difference in terms of population size.

Our goal in this paper is to isolate the effects of the incentives-component of the RBA model. To do this, we estimate the differences in outcomes between municipalities financed through RBA relative to national funds, and the difference in outcomes between municipalities financed with conventional aid relative to national funds. We argue that the difference between these two effects provides a plausible estimate of the RBA incentives, since it represents the effect of RBA net of the effects of conventional aid.[23]

Our empirical strategy uses the variation in the start of the health reform across municipalities and over time (Figure 1). The map at the top of Figure 1 represents municipalities in the reform according to the calendar quarter (Q), since 2010, in which they enrolled 10% of the eligible population.[24] The bottom three figures present the same map but shading only municipalities by type of funding: Ministry of Health (national funding), conventional aid (AID), and SMI (RBA). The figures show that the first set of municipalities reached this cap in Q3 of 2010, after which enrollment started in most of the other municipalities, with the last set reaching this cap in Q4 of year 2012. There is substantial heterogeneity in the timing of treatment onset across municipalities regardless of the type of funding. This is an important component of the variation used to identify the effect of the RBA model since not only we can use a comparison group formed by non-RBA municipalities, but also use variation in the timing of onset within RBA-municipalities.

We estimate a difference in differences linear regression interacting an indicator for the type of municipality (SMI or conventional aid) with the time of onset as defined before. Formally, we regress:

$$(1)\quad Y_{jt} = \beta_0 + \beta_1 Onset_{jt} + \delta_1 RBA_j \times Onset_{jt} + \delta_2 AID_j \times Onset_{jt} + \phi_j + \phi_t + \varepsilon_{jt}$$

Where $Y_{jt}$ is an outcome of interest, i.e. medical visits, number of outpatient visits, etc.; $Onset_{jt}$ is a binary indicator equal to one when the municipality reaches 10% of enrolment of the eligible population and equal to zero for all periods before the date of this cap; $RBA_j$ is a binary indicator that equals one if the municipality is included in the RBA model and zero otherwise; $AID_j$ is a binary indicator that equals one if the municipality is included in the group of municipalities that received conventional aid and zero otherwise; $\phi_j, \phi_t, \varepsilon_{jt}$, are time fixed effects, municipality fixed effects, and unobservable characteristics that vary within municipality and across time,

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[23] Funds from SMI and other aid organizations come from a similar normative frame. Aid from multilateral organizations were earmarked for the health sectors and did not interact with other sectors (See IDB and WB loan documents). Technical assistance from SMI was applied across all municipalities, producing a lower bound estimate of true effects. In the case of other organizations there is no reason to believe that the portfolios of aid are different in that they were explicitly delivered to finance the implementation of the health reform. In addition, qualitative evidence shows that technical assistance was not different across international organizations, and SMI municipalities may have had lower levels of technical assistance at the beginning of the reform.

[24] We measure enrollment as the percentage of family records (*fichas familiares*) relative to the total family records completed by the reform in a municipality. These records were the first activity that community health teams were required to perform in their area of influence and were completed between 2010 and 2013. The coverage of these assessments was close to 93 percent of the population by 2013 according to population projections.

respectively. The key parameter of interest is $\delta = \delta_1 - \delta_2$, i.e. the differences between having aid plus incentives tied to results and receiving conventional aid without incentives.[25] We estimate (1) by OLS including a binary indicator for each calendar quarter-year and a binary indicator for each municipality in our sample.[26]

Identifying the effects of the reform through (1) requires the assumption that in the absence of the RBA model, treated municipalities with incentives would have had the same trends in potential outcomes as comparison municipalities that received aid and those that were financed with national funding. One important caveat of this approach is the impossibility to test for this assumption, known as the “parallel trends” in potential outcomes assumption. However, we can explore whether there were any significant changes over time across municipalities before the start of the treatment. In other words, if we find that during pre-treatment periods, municipalities in the RBA group have significantly different trends compared to comparison municipalities, then it would be more likely that the assumption of trends in potential outcomes is violated (see Meyer, 1995 and Barham, 2011).

In Figure 2 to Figure 11 we plot time series for different outcomes by funding source. The figures plot the average deviation from the mean for quarters before and after the onset of the health reform. A first look at the figures shows that the trends before onset, at time 0, are similar between groups, and that after onset, RBA municipalities experience a steep increase in health services delivery or related outcomes. For example, Figure 2 shows that the trends of the three groups in total outpatient visits are very similar with a slight downward slope in the periods before the onset of the reform. At the start of the reform the level of outpatient visits in RBA municipalities starts to increase rapidly, which is mimicked by the other two groups only two years after the onset of the reform. A similar pattern is shown by most of the other figures, illustrating that the RBA incentives may have motivated a more rapidly implementation of the reform.

To test for differences in pre-treatment trends, we regress:

$$(2)\ \ Y_{jt} = \alpha + \sum_{t=1}^{S} \beta_t Q_t + \sum_{t=1}^{S} \gamma_{tj} Q_t\, Treat_j + \phi_j + \upsilon_{jt}$$

[25] Alternatively we ran the following regression $Y_{jt} = \beta_0 + \beta_1 Onset_{jt} + \gamma_1 FAID_j \times Onset_{jt} + \gamma_2 FAID_j \times RBA_j \times Onset_{jt} + \phi_j + \phi_t + \varepsilon_{jt}$ where $FAID_j$ equals to one for all municipalities that received foreign aid regardless of the incentives contract. In this regression $\gamma_2$captures directly the difference $\delta_1 - \delta_2$ in (1) and we can compute directly p-values for the difference between RBA and AID municipalities. The main reason for running regression (1) is to illustrate the effect of AID against National Funding municipalities.

[26] Almost all variables we study are counts of medical visits. We estimate coefficients using linear regression since the fixed effects regression proposed transforms the outcomes as deviations of the group mean where each group is a municipality. The empirical distribution of such transformations is very close to a normal distribution, making the OLS assumption plausible in this case.

Where $Y_{jt}$ is an outcome of interest; $Q_t$ are binary indicators for each quarter, for $= 1, \dots, S$ , during the pre-treatment period; $Treat_j$ is defined as before but can take values of one and zero for the different comparisons of interest indexed by *j*, i.e. RBA vs AID, RBA vs National Funding, and AID vs National Funding; $\phi_j, \upsilon_{jt}$, are municipality fixed effects, and unobservable characteristics that vary within municipality across time, respectively. In this regression, the $\gamma_t$ coefficients capture the mean change in $Y_{jt}$ since $t = 1$ to $t = s$, correspond to the quarters before the start of the reform. We implement a test of joint significance for the $\gamma_t$ coefficients. If we fail to reject the hypothesis that they are jointly equal to zero, i.e. that pretrends in outcomes are not related to treatment status, we would be more confident of the assumption of parallel trends in potential outcomes.

In addition to the pre-trends test, we conduct a falsification test in which we regress model (1) but change the date of onset to be one year before the true onset date. We restrict the sample to years prior to the third quarter of 2010 (when the reform starts in the first set of municipalities) and run the same regression on the complete set of outcomes. The absence of effects in this falsification test supports the notion that results are not driven by spurious correlations between RBA status and outcomes. All our main results include this falsification test. With very few exceptions, the results for the falsification tests show no significant differences between any types of comparison: RBA vs National Funds, RBA vs AID, or AID vs National Funds. This result supports the hypothesis that the main findings are not due to a spurious relation between health production and treatment status, lending credibility to the effects estimated previously.

In the first column, we also report the p-values for the F-test for the joint hypothesis of the $\hat{\gamma}_{tj}$ coefficients in model (2). Though this is not a direct test of the assumption of "parallel trends" in potential outcomes, in most cases, the results show that there are no significant differences in trends between RBA and National Funding, RBA and AID, or AID and National Funding municipalities. The only exceptions are the variables associated to hospitalizations where the trends between RBA municipalities and the other two groups differ slightly, showing a steeper upward trend for the group of municipalities under RBA. However, this pre-treatment upward trend implies that estimates for these outcomes may be an underestimate. The main reason the pre-trends differ when we study outcomes based on the hospitalizations data is that the RBA group includes two municipalities that are very large in terms of baseline production compared to the rest. Another reason is that the time series of hospital discharges data is longer, starting in 2005, approximately five years before the implementation of the reform. After dropping the two largest municipalities and focusing only on pre-trends during the previous year (which are more likely affect treatment status), the results show that the pre-trends in all outcomes are very similar across the three groups (see Table A.20 and A.21 in the Appendix).

Finally, our sample is clustered within 98 municipalities. As such, we must account for intra-cluster correlation in the statistical inference. Standard methods of correcting standard errors rely on large sample theory both in the number of observations and in the number of clusters. Given the small number of clusters in each intervention group, we instead use randomization inference methods that are robust to small number of clusters. Specifically, we use the Wild bootstrap to generate *p*-values for the hypothesis of no treatment effects in model (1) (Cameron, Gelbach, and Miller 2008). Our Wild bootstrap procedure assigns symmetric weights and equal probability after re-sampling residuals, and uses 999 replications (Davidson and Flachaire 2008).

## 5. Results

This section presents estimates of equation (1), including a test of statistical significance for the differences between municipalities under RBA with those under conventional aid $(\delta_1 - \delta_2)$ to assess the contribution of the results based conditions. Overall, the results show that municipalities for which aid was conditioned on incentives experienced higher production of medical services, particularly preventive health services promoted by the RBA model. We discuss the effects in detail below, dividing the analysis by type of care and population.

### *a. Outpatient visits*

Table 3 presents the impact of RBA on total outpatient visits (Panel A) and separately for preventive visits (Panel B) and curative visits (Panel C). Distinguishing between the types of visits is relevant since the RBA model was explicitly designed to incentivize preventive care. We first test the effect on all outpatient visits regardless of the population group (column 1) and we separately test effects on outpatient visits for reproductive age women 15-49 years old (column 2) and children under 5 years old (column 3), since the RBA sought to incentivize preventive care for these groups.

The results show an increase of 765.6 outpatient visits overall in the municipalities under the RBA model compared to municipalities with national funding, a relative increase of 6.7% with respect to the average of 11,367 outpatient visits in the periods before the start of the RBA model. The difference between municipalities under conventional aid and national funding is positive but not significant at conventional levels. Below each coefficient, we report the main p-value computed by a standard cluster-robust adjustment to standard errors and the p-value from the

Wild-bootstrap for each group separately. The next rows, at the end of each panel, show the p-values of a significance test for the differences between RBA and conventional aid. While the magnitude of the RBA effect is more than twice that of conventional aid, the difference in total outpatient visits is only significant at the 15% confidence level. Columns (2) and (3) of Panel A estimate the effects on total outpatient visits for women 15-49 and children under 5 with similar results. RBA again more than doubles the number of outpatient visits relative to conventional aid, but the differences are not significant at conventional levels.

Increasing preventive care was one of the main goals of both the health reform and a key focus of SMI. Panel B and Panel C in Table 3 divide the analysis of outpatient visits into preventive visits and curative visits, respectively.[27] In Panel B, we find that RBA generated a significant increase of 964.2 visits over municipalities with national funding, corresponding to a 42.5% increase relative to the baseline level. Conventional aid municipalities experienced an increase of 210 additional visits over national funding, significant at the 10% level. The most noteworthy finding is the pronounced increase in preventive visits in RBA municipalities relative to conventional aid, a more than fourfold increase which is significant at the 5.6% level using the Wild Bootstrap. Similar results are found when we analyze the effect of the RBA model on preventive visits for women and children, the groups prioritized by SMI for improvements in health service delivery. These results suggest that there are large gains from linking aid to incentives, particularly when the goals between recipient and donor are aligned.

In Panel C we show the same analysis for curative visits, which were not explicitly incentivized by SMI and include treatments to ailments, accidents and other causes that are less susceptible to proactive outreach from health providers[28]. We find no evidence of effects from RBA or conventional aid on the number of curative visits (the estimated coefficient on RBA on total curative visits is negative but not significant). Coupled with results in the previous two panels, this result indicates that the effect of RBA on the production of outpatient visits is driven entirely by preventive visits, precisely the type of service incentivized by the RBA and promoted through the expansion of the health reform.

*b. Maternal and Infant preventive visits*

We next examine a specific subset of health services promoted by the RBA. Table 4 shows the effects of the RBA scheme on preventive care visits for family planning consultations, prenatal

[27] Clinical records are coded by the attending physician according to the type of consultation as being a curative or a preventive visit.
[28] Note that curative visits might be expected to decline over the long run if preventive visits achieve the objective of improving population health.

care, puerperal care, and infant preventive visits. Panel A shows the effects on the number of first visits and Panel B shows effects on the number of follow-up visits. This distinction is relevant since first visits are a proxy for initiation of health care and hence reflect the effects of the reform on the extensive margin, while follow-up visits reflect the intensive margin.

In the first column of Panel A we find that the effect of RBA on the production of new family planning visits is positive but not significant, while conventional aid produced a reduction of 4.2 visits, significant at the 10% level. The difference of approximately 10 family planning visits between RBA and conventional aid is significant at conventional levels. Column (2) shows that the average number of first prenatal care visits increased by 24.4 visits, a 26.9% increase with respect to baseline, in RBA municipalities, and 11.2 visits for conventional aid municipalities, a 26.1% increase with respect to baseline levels. While the differential effects are more than 2-fold between RBA and conventional aid municipalities, the difference is significant at only the 16% level. Column (3) shows the number of preventive visits during the puerperium (40 days following delivery). While the number of visits in RBA municipalities increased by 21 visits (24.2%), the number of puerperal visits in conventional aid municipalities increased by 6.7 (15.8%), and the difference between the two is statistically significant. This result suggests that the RBA was successful a promoting the tracking of patients and provision of services following a birth in the community. This is consistent with the results in column (4) where we observe that the number of preventive infant visits increased by 21.4 (22.6%) for children under 5 years old, doubling the number of services for this population relative to conventional aid (though the difference is not statistically significant).

In Panel B of Table 4 we present results on the number of follow-up visits for the same outcomes excepting puerperal visits. There is no significant effect on the number of subsequent family planning consultations from RBA, conventional aid, or between the two. For prenatal care, the number of follow-up prenatal care visits again doubles in RBA municipalities relative to conventional aid, although the difference is not significant. Lastly, the number of follow up visits for children under 5 years old increased by 45.2 visits (10.3%) in RBA municipalities, while conventional aid municipalities did not experience a significant increase of visits for children. The coefficients between these two groups are significantly different from each other at the 10% level, indicating that incentives were effective at promoting more follow-up visits for children.

*c. Inpatient Visits*

In the previous two sub-sections we found that RBA promoted a large increase in preventive consultations delivered through primary health services, both at the extensive and intensive margin. We now study whether RBA modified utilization of secondary care, related to inpatient visits or hospital discharges. The direction of the effects at the secondary level is theoretically ambiguous. Hospitalizations could be reduced if the RBA has positive effects on health through increased preventive care, as we found in previous results.[29] On the other hand, if increased preventive care led to the detection of previously unknown health cases that required higher level care, the RBA could have resulted in increased hospitalization.

In the first column of Table 5, we report the effects of RBA on total inpatient visits. The results show that there are no significant differences in the number of hospitalizations for any cause for patients from RBA or AID municipalities, or between the two. In column (2) we observe that the number of hospitalizations for women 15-49 years old increased by 10.5 (7.7%) with respect to national funding municipalities, and there are no effects on AID or between AID and RBA. However, separating the type of inpatient visits between obstetric and non-obstetric causes reveals that effects on reproductive age women in the RBA group are driven entirely by obstetric visits, with an increase of 13.3 additional hospitalizations (14%), while the difference between conventional aid municipalities and national funding is not significant. While the difference between RBA and AID is not significant at conventional levels, the magnitude of the difference reveals a threefold increase in the number of obstetric related hospitalizations due to RBA. While we are not able to further disaggregate the data by specific obstetric procedures, we speculate that the likely nature of obstetric hospitalizations, and hence their expansion, are increased institutional deliveries for pregnancies detected through the expanded preventive care as a result of RBA.

*d. Timeliness of care*

Available data sources do not include final health outcomes related to the expansion of health services (i.e. maternal and neonatal mortality), however we are able to construct some intermediate measures of health behavior with available administrative data. In Table 6 we study the effects of RBA on the timing of prenatal care visits and timing of first visits for infants. Timely care during pregnancy and after birth could lead to early detection and treatment of conditions that could worsen over time. Both are linked to improved health outcomes at birth and childhood (see Rozenweig and Schultz 1983, Grossman and Joyce 1990, Carroli, Rooney, and Villar 2001,

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[29] An alternative explanation for possible negative effects on hospitalizations is that municipalities may be shifting effort away from hospitals to primary care in municipalities. However, hospitals do not depend on municipal authorities, as they are managed centrally. While in principle, the central level could have redirected resources from hospital care to primary care, this is inconsistent with the results we show, as they imply a reduction in hospitalizations across the board for all conditions and population groups, which is not the case as shown in Tables 5 and 9.

Evans and Lien 2005) and were part of the performance framework of the first phase of SMI. For prenatal care, we construct a binary indicator for whether the prenatal consultation took place during the first trimester of the pregnancy. For infant visits, we construct a binary indicator equal to one if the infant visit occurs within the first week of birth. In both cases, we construct the percentage of first visits that are classified as early visits.

We find that there are is no effect of RBA on early detection of pregnancies, while conventional aid municipalities experienced a decline with respect to national funding municipalities. As such, relative to conventional AID, RBA produced a significantly higher percentage of early prenatal care visits.  In column (2) we show that RBA increased the percentage of early infant care visits by 11.6 percentage points with respect to national funding, while early capture of infants is negative but not-significant in conventional aid municipalities. The difference between RBA and AID is again significant, indicating that the RBA boosted the provision of early care for pregnant women and newborns.

*e. Spillovers to other population groups*

The populations prioritized by SMI through the RBA were low-income women and children. If the RBA incentives redirected limited resources towards these groups, and away from other populations, then we might expect negative spillovers on the production of health care for youth, men and the elderly. On the other hand, if efforts to increase health service production are driven by the expansion of infrastructure and personnel that increase capacity of the system to broaden services to all, then we might expect positive spillovers to other population groups. To explore spillovers, we analyze outpatient visits by men 15 - 49 years old and elderly individuals over 60 years old.

Panel A of Table 7 shows that RBA increased the number of outpatient visits to men 15 – 49 years old by 84.2 (9.8%) relative to national funding while conventional aid increased the provision of outpatient visits for men by 41.6 (8.7%) visits. While the qualitative difference between RBA and conventional aid is again large, the difference is not statistically significant. Column (2) shows that the effect on total outpatient visits is mainly driven by an increase in preventive visits. Similarly, Panel B shows that the RBA model increased the number of outpatient visits for the elderly by 74.3 (5.7%) visits with respect to national funding, while conventional aid produced no effects. Again, preventive visits drive the effects. The difference between RBA and AID is statistically significant, suggesting that the RBA model not only generated important effects on the intended population of reproductive age women and young children, but also had positive effects of a similar magnitude on the rest of the population.

*f. Spillovers to neighboring municipalities*

While the RBA model was targeted to a subset of 14 municipalities, the national administrative health data allows to explore spillovers to neighboring municipalities, whether it was included in the health reform or not. With an expansion of health service coverage in RBA municipalities documented in the previous sub-sections, we ask whether populations in bordering municipalities may have sought care in RBA municipalities because they could obtain faster or better care. This could attenuate the effects of the RBA model as some of the capacity of municipalities funded with RBA would have been used to treat persons from other municipalities.

To explore geographical spillovers, we analyze whether RBA and AID municipalities received a larger number of visits from patients who reside in adjacent and to non-adjacent municipalities.[30] The first column in Table 8 shows the results found in previous sections hold when we only focus on patients that reside in the targeted municipalities. Outpatient visits from patients who reside in adjacent municipalities increased in both RBA and AID municipalities (Column 2), and the effect is approximately of the same magnitude. This result holds for total outpatient visits, preventive and curative visits. The number of health services provided to residents of non-bordering municipalities in Column (3) is smaller, as might be expected from increased distance, and again the difference between RBA and AID is not significant. These results suggest that AID may have expanded certain services accessible to non-residents, such as consultations in the clinical setting. However, a majority of the additional preventive services caused by RBA appear to be targeted to local residents, consistent with the expansion of the health reform's community based model that delivers services with health teams assigned to specific catchment areas within a municipality.

*g. Effect of RBA on the effectiveness of primary care*

Evidence in the previous sub-sections indicates that the RBA resulted in a substantial increase in the provision of preventive health services for the populations in these municipalities. A key question is whether this increase in health care resulted in improved health outcomes. While the administrative data used in our analysis does not report specific health outcomes, we use the number of ambulatory case sensitive (ACS) hospitalizations as a proxy measure of health.

[30] We define adjacent municipalities as those that share any border. Outpatient consultations are grouped according to the relationship of their municipality of residence and the facility in which they sought care: same municipality, adjacent municipality and other municipality. The first group are those individuals residing in the same municipality as the facility where they sought care, the second consists of consultations of individuals residing in an adjacent municipality to that of the facility where they received care, and the last one consists of those living in non-adjacent municipalities to the facility where they received care. We then aggregate consultations by municipality-quarter according to the municipality where the facility is located and we estimate equation (1) on outpatient consultations classified according to the groups described previously.

ACS hospitalizations are defined as those hospitalizations that could have been avoided if timely and adequate primary care was provided. Examples of these types of conditions are those preventable by vaccination, those related to common ailments such as diarrhea, or respiratory infections and those related to the treatment of chronic conditions such as diabetes or hypertension. There is a large medical literature relating ACS hospitalizations to the adequacy and effectiveness of primary care (Rosano et al 2013) and as such it is used frequently as an indicator of access and quality of primary care in different settings.[31] Moreover, various studies establish a causal link between increased access to health care through insurance expansions and ACS hospitalizations (Dafny and Grueber, 2005; Kaestner et al 2001; Kolstad and Kowalski, 2012). This suggests that ACS is a plausible proxy health indicator that could be sensitive to increased effectiveness of primary level care caused by the RBA.

We use hospital discharge data to construct a measure of ACS conditions using the list provided by Rodriguez et al (2012), which has been used previously in the Latin American context. We perform the same analysis as that presented in Table 5 for hospitalizations, but restrict it only to ACS hospitalizations overall, for women 15 to 49 and for children under five. Results are shown in Table 9. While estimates lack precision and are not statistically significant, the direction and magnitude of the coefficient is consistent with a reduction in ACS hospitalizations in municipalities under RBA. A reduction of 6.5 ACS hospitalizations in RBA municipalities represents about 8% from baseline. The magnitude of the effect for ACS hospitalizations for women 15-49 is even larger in relative terms (15.7% of baseline). The main cause of this type of hospitalizations among women are infections of the genitourinary tract in pregnancy (about 41 percent of the total). The best practice for prenatal care visits is to monitor for urinary infections, since they can be easily treated with antibiotics at the primary level, but if left untreated they could lead to kidney infections and low birth weight. Therefore, reductions in hospitalizations for this cause is consistent with improved and/or more frequent prenatal care.[32] The effect on children is also negative, although of smaller magnitude and larger p-value. While far from conclusive, these results suggest that the increase in preventive consultations from the RBA may have had an important, while imprecisely estimated, effect on health, especially for reproductive age women.

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[31] For instance, in the United States ACS hospitalizations is included as an indicator of prevention quality by the Agency for Healthcare Research and Quality (AHRQ). In Australia the measure is used for measuring health system performance and accountability and it even is directly linked to funding. There is also evidence of the adequacy of the indicator in the Latin American context (Ciapponi et al 2012).

[32] As shown in Table 4, we do have evidence of the latter, as RBA municipalities caused increases of both the extensive and intensive margin of prenatal care, but we do not have currently any data to assess the former.

## 6. Robustness checks and competing explanations

In this section, we conduct robustness tests for different specifications and analyze pre-intervention trends in the periods before the onset of the health reform. Results are presented in tables A.1 to A.21 in the Appendix. The second column in each table and outcome shows the results when we estimate all models specifying the treatment effect as a continuous variable rather than the binary version that equals one in the period when enrollment reaches 10%. In all cases, the results are very close to our main specifications presented in the paper. We also run all the analyses using 1% of enrollment of the population as the threshold that defines the onset of the treatment in each group and find very similar results.

An additional concern is that each observation needs to be adjusted by population size. In other words, we would need to account for the fact that an additional medical visit is not the same in a municipality with 10,000 inhabitants as in a municipality with 1,000 inhabitants. Different realizations of the same variable are being drawn from populations of different size, hence leading to endogenous sampling in our setting. In other words, the realization of one visit is being drawn with different probabilities of selection in municipalities of different size. One way of correcting for this is to weight our results by the population size of each municipality. Ideally, one would have access to the population size of each municipality for each period. However, the only reliable population count available is that of the Census from El Salvador implemented in year 2007. We run a Weighted Least Squares (WLS) regression of model (1) weighting each observation by the population size of the municipality in year 2007. In all cases the WLS results show larger positive effects than our main results.

We also estimate all models excluding the largest municipality in each group according to the average of each outcome variable at baseline. Given the small number of clusters in each group we want to explore if the largest municipality in each group is not driving the results. The results are slightly sensitive to dropping the largest municipality in each group but remain at a similar relative effect size.

Another potential concern is that results are in part driven by improved reporting of data on health services by RBA and or AID municipalities rather than pure effects of performance incentives. If this were the case then our results could be biased as a result of changes in reporting rather than actual changes in services. We believe this is not a major concern in this context for several reasons. First, the data that we use was not directly tied to the RBA performance evaluation of the country. All of the performance indicators of the RBA model were limited to the 14 targeted municipalities and measured externally or independently verified by a third party through medical records reviews, health facility surveys or audits. Qualitative interviews of government officials confirm that data sources used by SMI were independent and not subject to manipulation (El Bcheraoui et al, 2017). Second, the administrative data used for the analysis have been recorded for years before the reform started so that municipalities had substantial experience in gathering and reporting these data before the start of the reform. Furthermore, RBA incentives were not tied to any results linked to service delivery for men or elderly, yet, we find large effects on both groups, supporting the hypothesis that service provision was not over-reported for the incentivized populations of women and children. Finally, we also find effects on hospitalizations, which are recorded in an independent system by a different set of providers, and there were no performance incentives associated to outcomes at the secondary level.

## 7. Pathways: Effects of RBA on the allocation of resources for primary care

In this section we explore pathways that led to an increase in preventive care as a result of the RBA model. We analyze changes over time in the allocation health units and the subsequent reallocation of the health sector workforce in the different municipalities.[33] The analysis relies on personnel and health care unit data for MoH units in operation between 2009 and 2015. Thus, we have two data points for each municipality, one prior to the introduction of the health reform and one after (See Table 10 panels A and B).[34] We restrict the analysis to primary care units for two reasons. First, the health reform focused primarily on primary care and a majority of preventive consultations occur at this level. Second, we can only attribute changes in resources at the primary care level, since higher levels of care (hospitals and centralized administrative personnel) serve both RBA and comparison municipalities.

We implement a difference-in-difference approach akin to equation (1) but with one pre-treatment and one post-treatment observation for each municipality. The results presented on Panel C of Table 10, although imprecisely estimated, have several key takeaways when analyzing their magnitude and direction. First, there is an increase in average primary care units (effect significant at conventional levels) and primary care units per capita (effect not significant at conventional levels) in RBA municipalities relative to both AID and national funding municipalities

[33] Human capital represents about 80 percent of the annual budget in primary care from the MoH in El Salvador. Primary care units on the other hand, are complements of health care personnel particularly when serving rural and dispersed populations.

[34] Unfortunately, there is not reliable data at the municipality level in between those two periods, since while the reform was taking place, personnel was allocated in the human resources database to regions rather than municipalities. Therefore, we cannot distinguish in which health unit a MoH employee was assigned. While employees were assigned to specific units, this is not reflected in the data we have, except for the years we include in the analysis.

(columns 1 and 2). This is supporting evidence that the RBA successfully and rapidly increased the number of health units.

Columns (3) to (7) look at the number of personnel per capita, first by looking at total personnel and then dividing by type of workforce by medics, nurses, community health workers (CHWs), and administrative staff. Results show that, while there are no significant differences in total personnel, there are changes in the composition of the health workforce. The point estimates, though not significant, suggest that the number of medics and nurses per capita increased more in RBA municipalities than in the other two groups. This increase came at the expense of the number of CHWs and administrative staff (see columns 6 and 7).

For each primary care team (Ecos F) in the reform, physicians were the ones in charge of providing consultations with the aid of nurses. CHWs on the other hand were mostly in charge of performing community outreach and educational activities, but were not allowed to provide consultations. The results in Table 10 suggest that the response of the national government to the RBA was to increase the supply of inputs most directly related to the provision of primary care consultations such as doctors and nurses and to improve the access of these services by increasing the number of primary care units. In fact, the increase in primary care units and physicians is on the order of a 50 to 40% over baseline, a large and economically significant change. These results could very well explain how the increase in preventive visits occurred over all population groups, including men and the elderly who not targeted directly by the RBA.

Another key pathway that could explain our results is increased productivity by health personnel. Unfortunately, our ability to measure effort levels by personnel in terms of hours worked or quality of the care is limited by data availability. Instead we construct a raw measure of productivity: the number of primary care visits per medical staff available in 2009 and 2015. The results shown in column (8) to column (10) in Table 10 show that there was a decrease in the number of primary care visits per health practitioner. This is mostly driven by a decrease in the number of curative visits in RBA municipalities. These results suggest that the effects of the RBA on service provision were likely caused by an expansion of units and personnel, rather than increased productivity as measured by the number of consultations per health care worker. On the other hand, it is possible that a lower workload for providers allowed for higher quality interactions and more time per patient. While we don't have data to analyze these outcomes, the direction of RBA effects on avoidable hospitalizations suggest that this could have been the case.

An important related question is whether the effects of RBA occurred because of a higher volume of resources and investment in these municipalities. That is, if RBA municipalities had a higher level of resources than comparison municipalities to produce health services, then we could be overestimating the effect of the incentive component of the RBA. Available evidence shows that this was not the case. First, as explained earlier, the implementation of the reform had a similar cost structure established by national guidelines. Each Ecos F was designed to serve a pre-specified population of 600 families in rural areas and 1800 families in urban areas with an established number of health workers and a portfolio of services.[35] Second, when we compare the level of units and health workers per capita between all groups in 2015 (Panel B of Table 10), that is, after the reform was implemented, we can see that RBA municipalities had either the same or even lower level of resources per capita in terms of units and all personnel categories than municipalities funded through national sources or conventional aid. Moreover, the level of spending in medical inputs such as medicines and lab tests in Ecos F in 2015 was also lower in RBA municipalities ($4593 USD per 1000 inhabitants) than those funded with Aid ($5215 USD per 1000 inhabitants) and those financed with national funds ($5454 USD per 1000 inhabitants).[36] Taken together, this suggests that RBA municipalities did not have more resources per capita for primary care relative to comparison municipalities. Instead, we speculate that the RBA stimulated a degree of "catch up" in the implementation of the reform.

To further our understanding of the channels leading to increased provision of preventive care shown in section 5, we verify whether services were delivered at the primary care level rather than the secondary care level. This is an indicator of effective implementation of the reform, since the reform promoted access to primary care. It is also an additional indicator of overall efficiency gains of the health sector since preventive visits should occur in primary care settings, which are less costly, potentially freeing resources from hospitals (e.g., eliminating waiting lines, saving nurse-hours). Outpatient consultations in hospitals can be up to five times costlier than those taking place at the primary level (Gallo and Raigoza, 2013). Table 11 shows that the effect on total preventive visits found before is driven entirely by an increase of these visits in primary care facilities rather than hospitals. While there is no reduction in preventive visits occurring in hospitals, the results show that any additional demand for preventive health services in these municipalities is being absorbed entirely by primary care facilities.

A natural extension is to consider the mechanisms by which the RBA model, with performance incentives to the national government linked to performance in local health areas

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[35] Each rural Ecos F consists of seven members: a physician, a professional nurse, an auxiliary nurse, three community health workers and a multipurpose health worker. Each urban Ecos F is similar to a rural one, except that it has an additional six community health workers. By design each community health worker was required to serve 200 families. The detailed portfolio of services provided by Ecos F can be found in MINSAL (2011).

[36] Unfortunately, we have no data on spending in medical inputs prior to the implementation of the reform to perform a similar difference-in-difference estimate as that presented in Table 10.

(municipalities), increased service delivery. Our results point to an expansion of services on the extensive margin, accelerating procurement processes and cutting through red tape to build health centers and hire human resources, rather than expansion on the intensive margin. This effect is consistent with the actions under direct control of agents responding to incentives at the national level, rather than more motivated health personnel in the field. We also rule out the diversion of resources from non-RBA municipalities as a likely explanation. Descriptive data on health service provision (such as that provided in Figure 1) show that services in comparison municipalities were flat or increasing during the study period, suggesting that resources were not diverted. This is consistent with anecdotal reports from national health authorities, who maintain that additional efforts were made in RBA municipalities with the objective of meeting targets by established deadlines, but that the first order objective of the Ministry of Health at that time was the full implementation of the health reform in all 98 municipalities. Furthermore, SMI provided centralized technical support for procurement and monitoring systems that were applied system wide, thus benefiting all municipalities. As such, if anything, we might expect positive externalities from the centralized nature of the RBA contract, which would tend to attenuate the true impacts of the model.

## 8. Discussion

RBA has gained increasing attention as a policy tool to increase aid effectiveness (Perakis and Savedoff 2015). Previous research has examined pay-for-performance schemes at the provider level, such as teachers (Hanushek and Woessmann 2010) or health workers (Miller and Babiarz 2013), but much less is known about aid linked to results for governments (Birdsall et al. 2012, Perakis & Savedoff, 2015). Two notable exceptions examine performance incentives to sub-national governments, a village level field experiment in Indonesia by Olken et al. (2014) and results-based funding for health in Argentina (Gertler et al, 2014). Olken et al (2014) find that financial incentives on top of aid to local governments led to a faster rate of improvement in preventive health indicators, but had no effect on health or education outcomes. Gertler et al (2014) find that results based funding to provincial governments coupled with fee-for service at the provider level led to improved neo-natal health outcomes. To our knowledge, our study is the first to examine the causal effects of incentives directed to a national government.

There are two main reasons for the dearth of causal evidence on RBA. First, most studies on the subject lack a clear counterfactual and hence can at most establish a correlation but not causation (Pearson et al. 2010; Perrin 2013). Second, by definition, RBA models bundle financial incentives with funding to achieve a common objective, making it difficult to isolate the differential effects of the aid component from the results-based conditions (Janus 2014). Yet identifying the effects of the results-based component of RBA separate and apart from the effects of funding is both conceptually appealing and policy relevant, since RBA requires additional efforts and resources, for example to monitor results and enforce conditions.

Our study exploits a unique natural experiment in the context of El Salvador's health reform, which was implemented with multiple funding sources, including RBA, conventional aid and national funds. The RBA incentives were negotiated with and directed to the national government, conditional on achieving results in RBA funded municipalities. Compared to areas funded only with national resources, we find that RBA leads to a 42% increase in preventive care services. In contrast, municipalities with conventional aid achieved an increase of 19% in preventive visits, that is, about half of what was achieved for those under the RBA model. This result suggests that the RBA incentives roughly doubled the effectiveness of conventional aid as measured by the delivery of preventive health services.

The additional cost of the RBA performance bonus in El Salvador amounted to $1,625,000 USD for the first phase of the study that we evaluate here. While a complete cost-effectiveness analysis is outside the scope of this paper, a back of the envelope calculation, with an average of 754 additional outpatient visits[37] produced per quarter over a period of 8 treatment quarters in the municipalities under RBA, implies an additional 84,448 services, or a cost of approximately $19 USD per service, for the periods we analyze. The cost per additional visits under RBA is thus substantially lower than the average cost of providing an outpatient visit using traditional funding sources ($30 USD)[38].

Our analysis of investments in infrastructure and personnel suggests that the increase in services was driven at least in part by an accelerated investment in health units and staffing of physicians. Anecdotal evidence suggests that this may have been accomplished through a more focused implementation of the community health model in RBA municipalities by national health authorities, motivated by the urgency of gaining the incentive tranche which could then be invested in further expansion of the health reform model, as well as potential reputational effects of meeting the RBA targets ("Misleading means", 2014; Regalia et al, 2017). SMI used a similar RBA scheme for several countries in the region, and countries could compare their performance with others. While we don't know of any empirical evidence of reputational effects on national

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[37] 754 is the difference in preventive visits under RBA and conventional aid in Table 3, Panel B.
[38] Estimate based on data from Gallo and Raigoza (2013) including the cost of all of the main activities provided at the primary level.

governments,[39] there is some evidence among physicians that these could drive large responses (Kolstad, 2013). [40]

According to the analysis of in-depth interviews with key MoH informants, the "results-based" part of the RBA model had two other effects: increased accountability and management by results (El Bcheraoui et al 2016; Iriarte et al 2017). The external verification of results by SMI mobilized better communication between central and local levels of government, and improved accountability at all levels, including increased responsiveness of the national level to local demands. Moreover, the focus on results led to management by results. In the case of El Salvador, this was reflected in detailed implementation plans at the national and local levels, and increased monitoring and evaluation of those plans.

Foreign aid has been a controversial topic in development economics for decades (Easterly 2008). In an effort to improve aid effectiveness, recent literature advocates linking aid to results, with the underlying objective of better aligning donors and recipient's interests so that the goals for which aid is delivered are met (Birdsall et al. 2012). Yet the empirical evidence supporting results based aid, particularly when directed to central levels of government, is scarce. Our study helps fill this evidence gap using a unique natural experiment in El Salvador's health sector, suggesting that results based aid may in fact deliver on its promise as a policy tool for promoting more effective development.

[39] These effects are related to the literature in economics to the role of reference-based utility in behavior (Heffetz and Frank, 2008).
[40] In fact, Kolstad (2013) finds that with the introduction of quality reports cards among cardiologists in Pennsylvania, intrinsic motivation, modeled as the ability to perform well relative to a reference group had larger effects than profit incentives.

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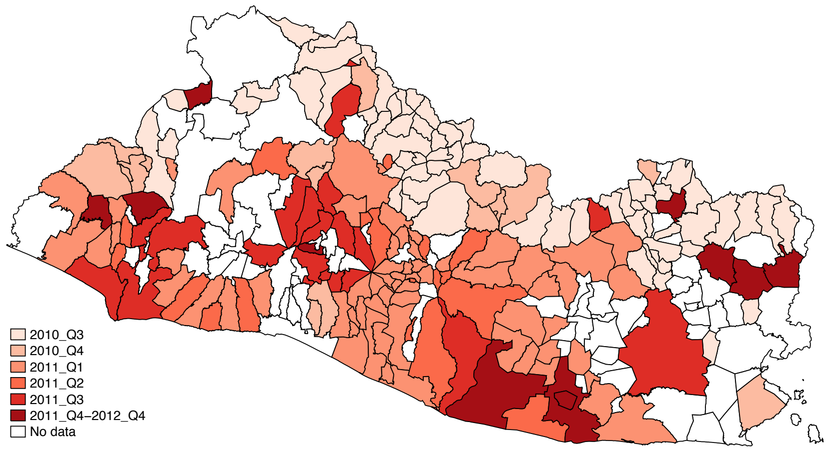
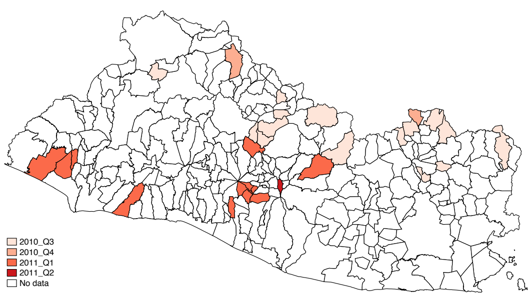
## Figures

### Figure 1: Calendar quarter in which the municipality reached a 10% of enrollment of its target population

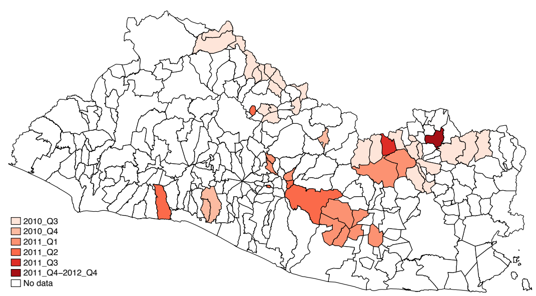
*All municipalities in El Salvador*




*Financed by National Funds*



*Financed by Conventional aid*



*Financed by SMI's -RBA*

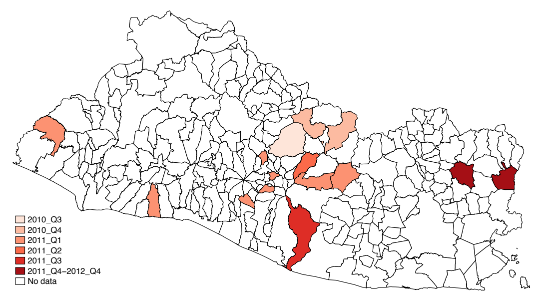


**Figure 2: Trends in total outpatient visits of RBA, conventional aid, and national funding municipalities**

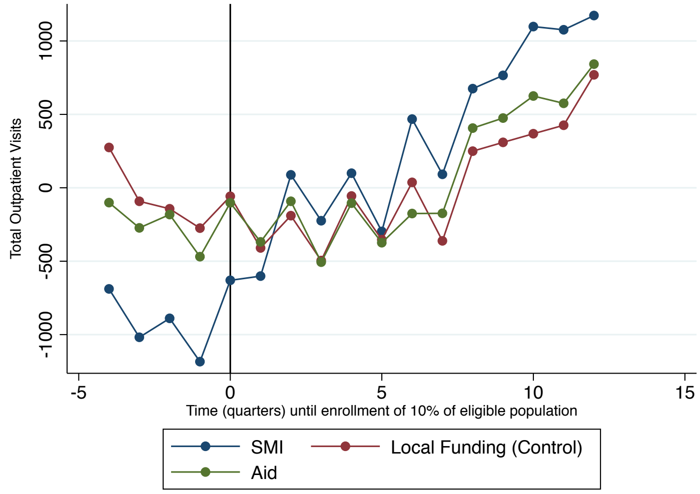



**Figure 3: Trends in total preventive visits of RBA, conventional aid, and national funding municipalities**

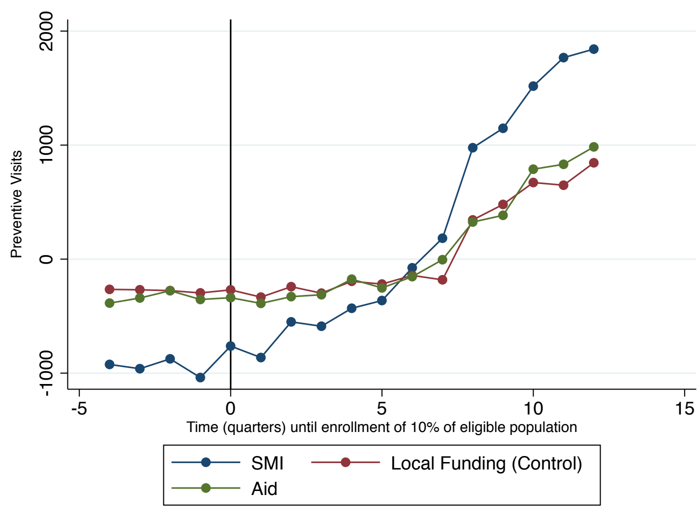


**Figure 4: Trends in total curative visits of RBA, conventional aid, and national funding municipalities**

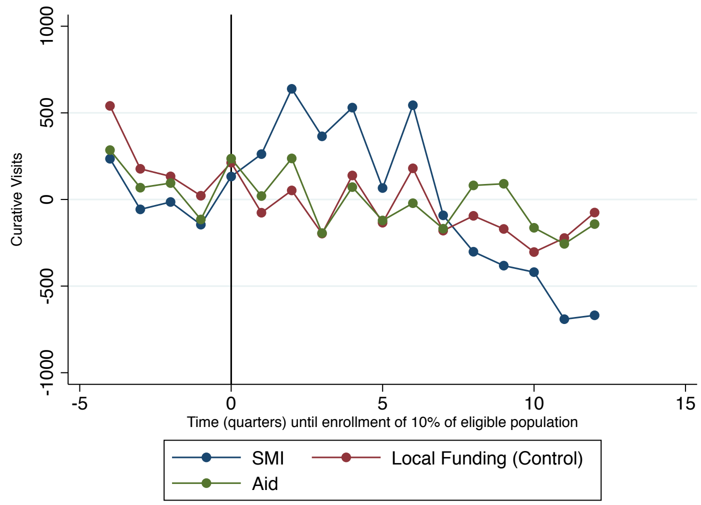



**Figure 5: Trends in total prenatal care initial visits of RBA, conventional aid, and national funding municipalities**

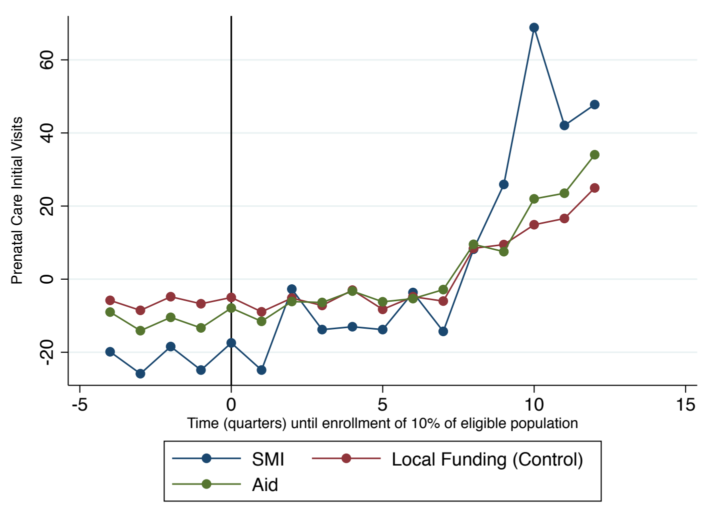


**Figure 6: Trends in total puerperal care visits of RBA, conventional aid, and national funding municipalities**

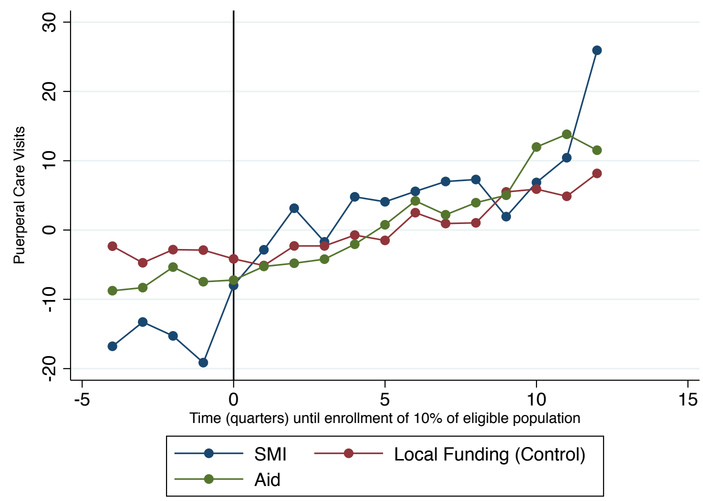



**Figure 7: Trends in total discharges of RBA, conventional aid, and national funding municipalities**

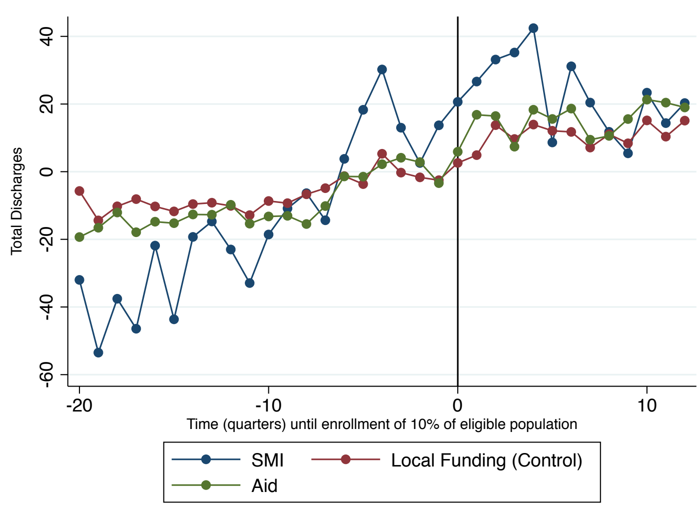


**Figure 8: Trends in total discharges for women 15 to 49 years old of RBA, conventional aid, and national funding municipalities**

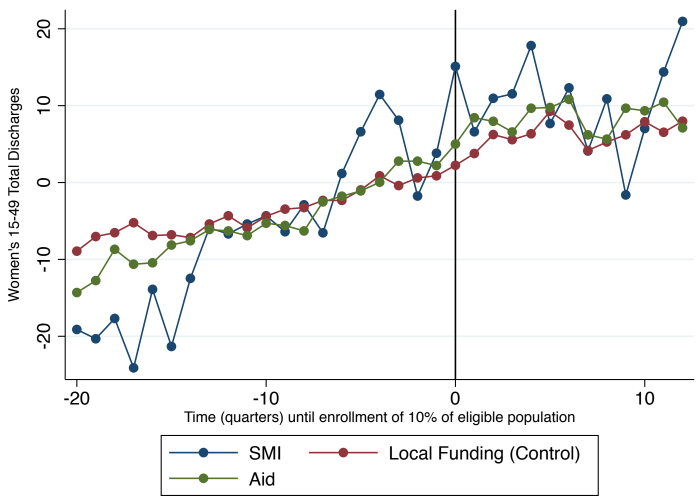



**Figure 9: Trends in total discharges for obstetric visits women 15 to 49 years old of RBA, conventional aid, and national funding municipalities**

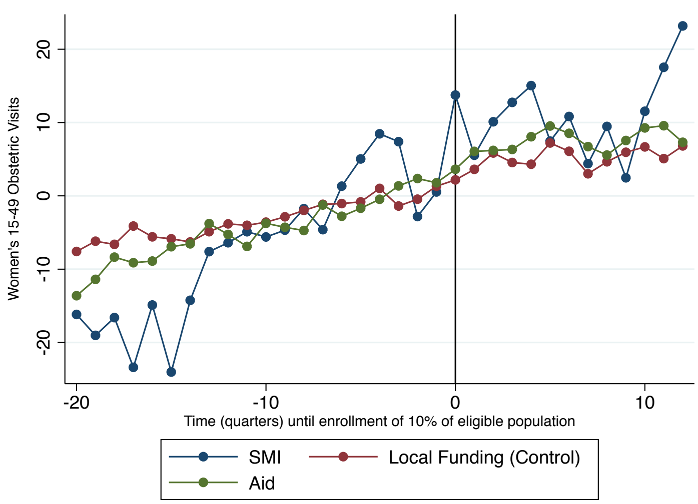


**Figure 10: Trends in total preventive visits by men in RBA, conventional aid, and national funding municipalities**

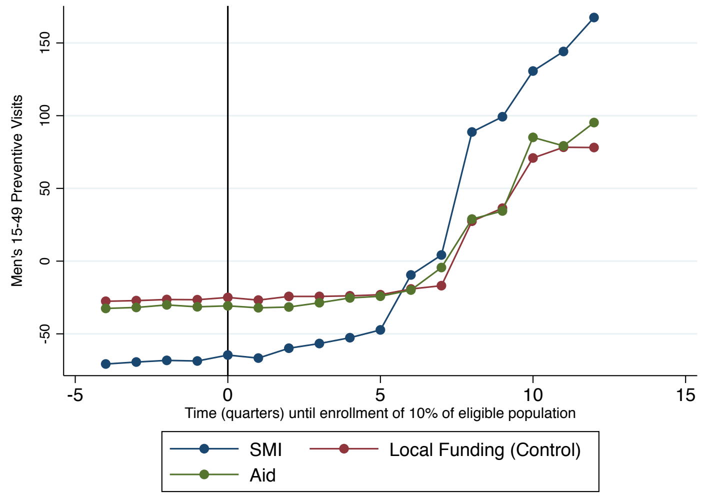



**Figure 11: Trends in total elderly preventive visits in RBA, conventional aid, and national funding municipalities**

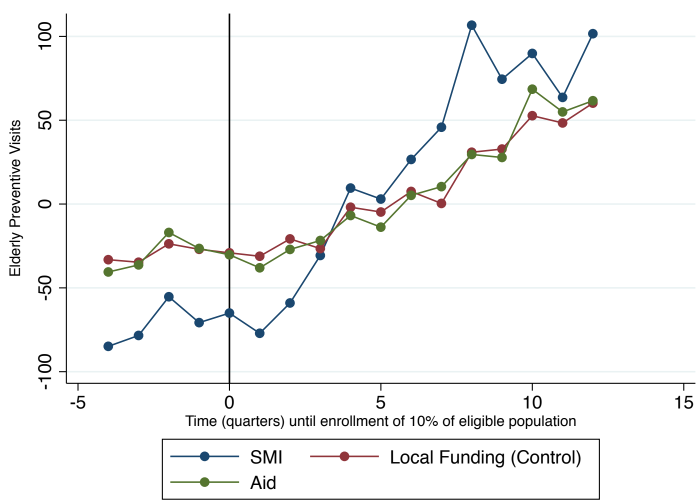


**Table 1A. Performance framework for the first phase of Salud Mesoamerica Initiative in El Salvador**

| Type | Indicator Description[1] | Units | Baseline | Target | 1st Phase Follow-up | Source of Verification |
|---|---|---|---|---|---|---|
| Reform Rollout | Number of Family and Specialized Community Teams (ECOS) created[2] | ECOS | 37 | 68 | 59 [49-67] | External Surveys in Health Facilities |
| Reform Rollout | Number of Families ascribed to Family Community Teams (ECOS)[3] | Families | 14,681 | 38,661 | 59,495* | Independent review of records |
| Inputs for Care | Number of community health units with the necessary inputs for prenatal care[4] | UCSF[5] | 35 | 68 | 75 * [69-77] | External Surveys in Health Facilities |
| Inputs for Care | Number of community health units with stocks of four family planning methods (injection, barrier, orals, IUD) | UCSF | 11 | 65 | 71* [62-75] | External Surveys in Health Facilities |
| Inputs for Care | Number of community health units with the necessary inputs for infant care[6] | UCSF | 24 | 58 | 71* [62-75] | External Surveys in Health Facilities |
| Inputs for Care | Number of community family health units with refrigerator or cool box to care for vaccines adequately | UCSF | 43 | 65 | 60* [50-68] | External Surveys in Health Facilities |
| Guidelines & Norms | Review of the national policy for the disbursement of micronutrients in powder for children 6 to 23 months[7] | NA | No | Yes | Yes* | Norms and protocols |
| Guidelines & Norms | Inclusion in the norm of the adequate dose of therapeutic zinc for treatment of diarrhea in children younger than 5 years old (20 mg of zinc during 10-14 days in each episode of diarrhea) | NA | No | Yes | Yes* | Norms and protocols |
| Timeliness of Care | Percentage of pregnant women recorded in the prenatal registry that had prenatal birth control performed by a doctor or nurse before 12 weeks. | % | 67 | 77 | 64.9 [58.8-70.6] | Administrative data verified through audits (random sample of registries) |
| Timeliness of Care | Percentage of children younger than one year registered in the system that were recorded in less than eight days | % | 51 | 61 | 90.1* [86.6-92.9] | Administrative data verified through audits (random sample of registries) |

Notes: The table includes the full performance framework and targets for the first phase of *Salud Mesoamerica Initiative*. If a country met, 80 percent of all targets in the performance framework it received the performance tranche equivalent to 50 percent of the country's investment in the project during that phase. An independent third party evaluated whether the country met the targets in each indicator according to the source of verification detailed in the table. For numeric targets, the criteria to determine whether it was met was that the target fell within a 95 percent confidence interval estimated by the third party considering the design of the survey. The 1st phase follow-up column includes the results of the evaluation performed by the third party. NA=Not available.

* Target was met according to the evaluation criteria, that is fell within the 95 percent confidence interval estimated by the independent evaluator.

1/ Detailed measurement criteria and computation formulas for each indicator are described in the Operations Manual of El Salvador.

2/ Human resources hired according to number and function to constitute an ECOS as established in the countries national guidelines.

3/ Files and family records collected (*fichas familiares*) as part of the implementation of the health reform.

4/ Lamp for pelvic exam (lantern or light for exams is acceptable), devise to take blood pressure, stethoscope, fetal stethoscope (pinard), weight for adults, vaginal speculum (small), vaginal speculum (medium), vaginal speculum (large).

5/ UCSF. Community Health Family Units.

6/ Weight for babies, weight for children, thermometer, oral rehydration salts, zinc, anthelmintics.

7/ Review of evidence, develop acceptability studies, modify the norm according to results.

**Table 1B. Performance framework for the second phase of Salud Mesoamerica Initiative in El Salvador**

| Type | Indicator[1] | Units | Baseline | Target | Second Phase Follow-up | Source of Verification |
|---|---|---|---|---|---|---|
| Timeliness | Percentage of women in reproductive age (15-49) that received their first prenatal control by doctor or nurse before 12 gestation weeks in their most recent pregnancy in the last two years | % | 64.9 | 74.9 | N.A. | External Surveys in Facilities (Medical Record Review) |
| Timeliness | Percentage of women in reproductive age (15-49) that in their most recent pregnancy had a visit by health personnel, including medical personnel and promoters, a week after birth | % | 81.6 | 91.6 | N.A. | External Surveys in Households |
| Quality | Percentage of women in reproductive age (15-49) that receive four prenatal attentions according to best practices by doctor or nurse in their most recent pregnancy in the last two years[2] | % | 47.5 | 62.5 | N.A. | External Surveys in Facilities (Medical Record Review) |
| Quality | Percentage of pregnant women with institutional attention to birth referred by the Community Teams (ECOS) as part of the activities of the birth plan | % | - | 70 | N.A. | External surveys of community health units |
| Coverage/ Utilization | Percentage of women in reproductive age (15-49)[3] that currently use (or whose partner use) a modern method for family planning | % | 53.5 | 60.5 | N.A. | External Surveys in Households |
| Coverage/ Utilization | Percentage of children 12 to 24 months of age with vaccinations for measles, mumps and rubella (MMR) | % | 66.6 | 73.6 | N.A. | External Surveys in Households |
| Coverage/ Utilization | Percentage of children 12 to 59 months that received two doses of anti-parasitic treatment in the last year | % | 35.4 | 56.4 | N.A. | External Surveys in Households |
| Coverage/ Utilization | Percentage of women that gave their children 0 to 59 years Oral Rehydration Salts and zinc in the last episode of diarrhea | % | 4.4 | 24.4 | N.A. | External Surveys in Households |
| Coverage/ Utilization | Women of reproductive age (15-49) whose most recent birth was attended by a skilled attendant in an institutional setting in the last two years | % | 86.2 | 94.2 | N.A. | External Surveys in Households |
| Health | Percentage of children 6 to 23 months of age that have an hemoglobin value < 110 g/L[4] | % | 46.5 | 36.5 | N.A. | External Surveys in Households |
| Expenditure | Expenditure of the Ministry of Health in the First Level of Care | US$ | 6,291,814 | 8,020,000 | N.A. | MINSAL Administrative data National Accounts |

Notes: The table includes the full performance framework and targets for the second phase of *Salud Mesoamerica Initiative*. If a country meets, 80 percent of all targets in the performance framework it received the performance tranche equivalent to 50 percent of the country's investment in the project during that phase. An independent third party evaluates whether the country meets the targets in each indicator according to the source of verification detailed in the table. For numeric targets, the criteria to determine whether it is met was that the target falls within a 95 percent confidence interval estimated by the third party considering the design of the survey. The 2nd phase follow-up column is not available since the evaluation was not done at the time of this paper, but should be concluded by the end of 2017. NA=Not available.

1/ Detailed measurement criteria and computation formulas for each indicator are described in the Operations Manual of El Salvador.

2/ Measurement of blood pressure, weight gain, fundal height, fetal heart rate, and blood and urine tests according to country national guidelines.

3/ Sexually active women that did not seek pregnancy, women with menopause, hysterectomy, virgin, pregnant or that want to get pregnant, are excluded.

4/ Anemia prevalence in children 6 to 23 months.

## Table 2: Cross-Sectional differences at baseline between RBA Municipalities and non-RBA Municipalities

| | RBA Municipalities | | | | Traditional Aid Municipalities | | | | National Funding Municipalities | | | | T test comparing group means | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Municipality Level Variables** | **Mean** | **Std. Dev.** | **Obs.** | **M** | **Mean** | **Std. Dev.** | **Obs.** | **M.** | **Mean** | **Std. Dev.** | **Obs.** | **M.** | **RBA vs AID** | **RBA vs NF** | **AID vs NF** |
| Population size Y07 | 17,502.3 | 16,484.5 | n.a. | 14 | 9,741.0 | 10,380.3 | n.a. | 54 | 6,307.9 | 5,746.5 | n.a. | 30 | 0.087 | 0.012 | 0.051 |
| Poverty rate Census Y07 | 0.244 | 0.037 | n.a. | 14 | 0.224 | 0.048 | n.a. | 54 | 0.227 | 0.057 | n.a. | 30 | 0.090 | 0.254 | 0.771 |
| Enrolled during Y10 | 0.143 | 0.363 | n.a. | 14 | 0.667 | 0.476 | n.a. | 54 | 0.567 | 0.504 | n.a. | 30 | 0.000 | 0.001 | 0.373 |
| Outpatients visits per quarter Y09 | 11,233.9 | 11,442.8 | n.a. | 14 | 6,262.7 | 6,745.7 | n.a. | 54 | 3,955.7 | 2,346.6 | n.a. | 30 | 0.112 | 0.017 | 0.023 |
| Preventive visits per quarter Y09 | 2,250.2 | 2,225.6 | n.a. | 14 | 1,022.6 | 901.4 | n.a. | 54 | 830.1 | 707.5 | n.a. | 30 | 0.039 | 0.018 | 0.279 |
| Curative visits per quarter Y09 | 8,983.7 | 9,490.8 | n.a. | 14 | 5,240.4 | 5,974.7 | n.a. | 54 | 3,125.7 | 1,669.9 | n.a. | 30 | 0.152 | 0.020 | 0.015 |
| Inpatient visits per quarter Y09 | 359.1 | 438.7 | n.a. | 14 | 178.9 | 256.7 | n.a. | 54 | 88.5 | 68.0 | n.a. | 30 | 0.133 | 0.020 | 0.015 |
| Out. visits per 1,000 people Y09 | 0.617 | 0.128 | n.a. | 14 | 0.776 | 0.367 | n.a. | 54 | 0.782 | 0.299 | n.a. | 30 | 0.008 | 0.010 | 0.930 |
| Prev. visits per 1,000 people Y09 | 0.127 | 0.029 | n.a. | 14 | 0.119 | 0.038 | n.a. | 54 | 0.136 | 0.028 | n.a. | 30 | 0.377 | 0.336 | 0.019 |
| Curat. visits per 1,000 people Y09 | 0.490 | 0.142 | n.a. | 14 | 0.657 | 0.350 | n.a. | 54 | 0.647 | 0.290 | n.a. | 30 | 0.006 | 0.016 | 0.882 |
| In. visits per 1,000 people Y09 | 0.018 | 0.006 | n.a. | 14 | 0.018 | 0.006 | n.a. | 54 | 0.015 | 0.005 | n.a. | 30 | 0.945 | 0.170 | 0.024 |
| Units per 1,000 people Y09 | 0.107 | 0.055 | n.a. | 14 | 0.247 | 0.292 | n.a. | 54 | 0.240 | 0.172 | n.a. | 30 | 0.001 | 0.000 | 0.892 |
| Medics per 1,000 people Y09 | 0.383 | 0.147 | n.a. | 14 | 0.566 | 0.607 | n.a. | 54 | 0.550 | 0.511 | n.a. | 30 | 0.046 | 0.100 | 0.901 |
| Nurses per 1,000 people Y09 | 0.259 | 0.097 | n.a. | 14 | 0.521 | 0.497 | n.a. | 54 | 0.402 | 0.262 | n.a. | 30 | 0.000 | 0.009 | 0.153 |
| CHWs per 1,000 people Y09 | 0.832 | 0.226 | n.a. | 14 | 0.686 | 0.425 | n.a. | 54 | 0.816 | 0.449 | n.a. | 30 | 0.077 | 0.869 | 0.194 |
| **Household level variables** | **Mean** | **Std. Dev.** | **Obs.** | **M** | **Mean** | **Std. Dev.** | **Obs.** | **M** | **Mean** | **Std. Dev.** | **Obs.** | **M** | **RBA vs AID** | **RBA vs NF** | **AID vs NF** |
| Wall: Solid (=1), Other (=0) | 0.571 | 0.495 | 59,357 | 14 | 0.471 | 0.499 | 85,432 | 44 | 0.462 | 0.499 | 46,861 | 29 | 0.034 | 0.031 | 0.833 |
| Floor: Dirt (=1), Other (=0) | 0.326 | 0.469 | 59,357 | 14 | 0.431 | 0.495 | 85,432 | 44 | 0.416 | 0.493 | 46,861 | 29 | 0.004 | 0.070 | 0.741 |
| Environmental risk exposure (=1) | 0.218 | 0.413 | 59,357 | 14 | 0.258 | 0.437 | 85,432 | 44 | 0.28 | 0.449 | 46,861 | 29 | 0.252 | 0.149 | 0.590 |
| Roof: Solid (=1), Other (=0) | 0.555 | 0.497 | 59,357 | 14 | 0.489 | 0.5 | 85,432 | 44 | 0.548 | 0.498 | 46,861 | 29 | 0.505 | 0.946 | 0.511 |
| Fuel: Wood (=1), Other (=0) | 0.565 | 0.496 | 59,361 | 14 | 0.666 | 0.471 | 85,437 | 44 | 0.711 | 0.453 | 46,861 | 29 | 0.053 | 0.004 | 0.303 |
| Electricity available (=1) | 0.832 | 0.374 | 59,357 | 14 | 0.804 | 0.397 | 85,432 | 44 | 0.754 | 0.43 | 46,861 | 29 | 0.332 | 0.035 | 0.092 |
| Phone at home (=1) | 0.717 | 0.45 | 59,357 | 14 | 0.732 | 0.443 | 85,432 | 44 | 0.736 | 0.441 | 46,861 | 29 | 0.825 | 0.780 | 0.911 |
| Water: Piped water (=1), Other (=0) | 0.869 | 0.337 | 59,357 | 14 | 0.836 | 0.37 | 85,432 | 44 | 0.8 | 0.4 | 46,858 | 29 | 0.346 | 0.024 | 0.341 |
| Toilet: Flush (=1), Other (=0) | 0.28 | 0.449 | 59,357 | 14 | 0.258 | 0.438 | 85,432 | 44 | 0.195 | 0.396 | 46,861 | 29 | 0.669 | 0.071 | 0.064 |
| Garbage: Other (=1), Private (=0) | 0.757 | 0.429 | 59,357 | 14 | 0.743 | 0.437 | 85,432 | 44 | 0.764 | 0.424 | 46,861 | 29 | 0.764 | 0.880 | 0.575 |
| Receives Cash Transfer (=1) | 0.283 | 0.451 | 59,345 | 14 | 0.289 | 0.453 | 83,231 | 44 | 0.401 | 0.49 | 46,856 | 29 | 0.944 | 0.122 | 0.012 |
| **Individual level variables** | **Mean** | **Std. Dev.** | **Obs.** | **M** | **Mean** | **Std. Dev.** | **Obs.** | **M** | **Mean** | **Std. Dev.** | **Obs.** | **M** | **RBA vs AID** | **RBA vs NF** | **AID vs NF** |
| Female (=1) | 0.518 | 0.5 | 257,416 | 14 | 0.517 | 0.5 | 344,509 | 44 | 0.512 | 0.5 | 190,942 | 29 | 0.890 | 0.171 | 0.075 |
| Age in years | 27.04 | 21.034 | 257,394 | 14 | 27.074 | 21.077 | 344,494 | 44 | 27.132 | 20.987 | 190,935 | 29 | 0.948 | 0.856 | 0.886 |
| Age: 5 - 10 years | 0.117 | 0.321 | 257,394 | 14 | 0.12 | 0.325 | 344,494 | 44 | 0.12 | 0.324 | 190,935 | 29 | 0.480 | 0.574 | 0.796 |
| Age: 10 - 15 years | 0.139 | 0.346 | 257,394 | 14 | 0.14 | 0.347 | 344,494 | 44 | 0.14 | 0.347 | 190,935 | 29 | 0.579 | 0.604 | 0.989 |
| Age: 15 - 25 years | 0.206 | 0.404 | 257,394 | 14 | 0.204 | 0.403 | 344,494 | 44 | 0.205 | 0.404 | 190,935 | 29 | 0.467 | 0.837 | 0.564 |
| Age: 25 - 35 years | 0.123 | 0.328 | 257,394 | 14 | 0.122 | 0.328 | 344,494 | 44 | 0.123 | 0.328 | 190,935 | 29 | 0.915 | 0.985 | 0.867 |
| Age: 35 - 45 years | 0.097 | 0.296 | 257,394 | 14 | 0.095 | 0.293 | 344,494 | 44 | 0.098 | 0.297 | 190,935 | 29 | 0.180 | 0.942 | 0.218 |
| Age: 45 - 55 years | 0.072 | 0.259 | 257,394 | 14 | 0.073 | 0.26 | 344,494 | 44 | 0.073 | 0.259 | 190,935 | 29 | 0.795 | 0.952 | 0.825 |
| Age: 55 - 65 years | 0.056 | 0.23 | 257,394 | 14 | 0.058 | 0.233 | 344,494 | 44 | 0.057 | 0.232 | 190,935 | 29 | 0.529 | 0.699 | 0.756 |

## Table 2: Cross-Sectional differences at baseline between RBA Municipalities and non-RBA Municipalities (Contd.).

| | RBA Municipalities | | | | Traditional Aid Municipalities | | | | National Funding Municipalities | | | | T test comparing group means | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Individual level variables** | **Mean** | **Std. Dev.** | **Obs.** | **M** | **Mean** | **Std. Dev.** | **Obs.** | **M** | **Mean** | **Std. Dev.** | **Obs.** | **M** | **RBA vs AID** | **RBA vs NF** | **AID vs NF** |
| Age 65 or more | 0.069 | 0.253 | 257,394 | 14 | 0.07 | 0.255 | 344,494 | 44 | 0.069 | 0.254 | 190,935 | 29 | 0.848 | 0.949 | 0.891 |
| Literate (=1) | 0.206 | 0.405 | 233,212 | 14 | 0.241 | 0.428 | 309,995 | 44 | 0.222 | 0.416 | 172,832 | 29 | 0.008 | 0.263 | 0.136 |
| Schooling: No schooling | 0.228 | 0.42 | 199,450 | 14 | 0.267 | 0.442 | 270,139 | 44 | 0.244 | 0.429 | 150,235 | 29 | 0.014 | 0.358 | 0.087 |
| Schooling: Less than primary | 0.568 | 0.495 | 199,450 | 14 | 0.543 | 0.498 | 270,139 | 44 | 0.575 | 0.494 | 150,235 | 29 | 0.007 | 0.481 | 0.000 |
| Schooling: Less than secondary | 0.133 | 0.34 | 199,450 | 14 | 0.12 | 0.325 | 270,139 | 44 | 0.122 | 0.327 | 150,235 | 29 | 0.175 | 0.246 | 0.807 |
| Schooling: High School | 0.044 | 0.206 | 199,450 | 14 | 0.046 | 0.21 | 270,139 | 44 | 0.04 | 0.196 | 150,235 | 29 | 0.888 | 0.747 | 0.395 |
| Schooling: College | 0.027 | 0.162 | 199,450 | 14 | 0.025 | 0.156 | 270,139 | 44 | 0.019 | 0.137 | 150,235 | 29 | 0.754 | 0.164 | 0.159 |
| Unemployed | 0.567 | 0.495 | 188,978 | 14 | 0.551 | 0.497 | 254,976 | 44 | 0.485 | 0.5 | 134,832 | 29 | 0.811 | 0.250 | 0.061 |
| Formally employed | 0.066 | 0.247 | 188,978 | 14 | 0.056 | 0.23 | 254,976 | 44 | 0.054 | 0.226 | 134,832 | 29 | 0.493 | 0.402 | 0.817 |
| Informally employed | 0.367 | 0.482 | 188,978 | 14 | 0.393 | 0.488 | 254,976 | 44 | 0.461 | 0.498 | 134,832 | 29 | 0.742 | 0.247 | 0.094 |
| Malnourished child (=1) | 0.022 | 0.147 | 11,493 | 14 | 0.034 | 0.18 | 14,422 | 44 | 0.036 | 0.187 | 8,888 | 29 | 0.025 | 0.001 | 0.665 |
| Self perception of health: Healthy (=1) | 0.734 | 0.442 | 256,917 | 14 | 0.652 | 0.476 | 343,720 | 44 | 0.741 | 0.438 | 190,623 | 29 | 0.090 | 0.865 | 0.037 |
| Pregnant women (=1) | 0.03 | 0.17 | 83,044 | 14 | 0.032 | 0.175 | 112,810 | 44 | 0.03 | 0.171 | 61,528 | 29 | 0.449 | 0.987 | 0.401 |
| Family planning utilization rate | 0.128 | 0.334 | 146,499 | 14 | 0.14 | 0.347 | 187,039 | 44 | 0.142 | 0.349 | 110,325 | 29 | 0.585 | 0.517 | 0.855 |

Notes: This table shows the mean, standard deviation, observations and number of municipalities included in the RBA model and non-RBA municipalities. The last column shows a t-test for the differences in means between these municipalities. The source of these data is the “Fichas Familiares” information that records information on every household targeted by the health reform in each municipality. Families in 10 out of 54 municipalities in the group that received aid and 1 out of the 30 municipalities that received national funding were not surveyed, hence we have missing values for all variables.

## Table 3: Effect of SMI RBA Model on Outpatient Consultations

| Population Group | Overall<br>(1) | Women 15-49<br>(2) | Children less than 5<br>(3) |
|---|---|---|---|
| ***Panel A. Outpatient visits*** | | | |
| **RBA x Onset** | 765.626** | 265.319* | 149.029** |
| | (330.838) | (142.783) | (70.970) |
| Large Sample p-value | 0.023 | 0.066 | 0.038 |
| Wild Bootstrap p-value | 0.038 | 0.094 | 0.066 |
| **Aid x Onset** | 267.957 | 117.494* | 63.524 |
| | (175.017) | (70.184) | (40.371) |
| Large Sample p-value | 0.129 | 0.097 | 0.119 |
| Wild Bootstrap p-value | 0.124 | 0.102 | 0.098 |
| Large Sample p-value of RBA vs AID | 0.132 | 0.290 | 0.244 |
| Wild Bst. p-value of RBA vs AID | 0.150 | 0.286 | 0.280 |
| Average of RBA at baseline | 11367.143 | 3729.560 | 2792.131 |
| Average of AID at baseline | 6408.031 | 2104.871 | 1470.682 |
| ***Panel B. Preventive visits*** | | | |
| **RBA x Onset** | 964.212*** | 274.649** | 148.005** |
| | (351.307) | (111.046) | (65.300) |
| Large Sample p-value | 0.007 | 0.015 | 0.026 |
| Wild Bootstrap p-value | 0.000 | 0.018 | 0.004 |
| **Aid x Onset** | 210.039* | 90.189* | 19.981 |
| | (119.702) | (46.615) | (25.027) |
| Large Sample p-value | 0.082 | 0.056 | 0.427 |
| Wild Bootstrap p-value | 0.066 | 0.066 | 0.444 |
| Large Sample p-value of RBA vs AID | 0.035 | 0.102 | 0.063 |
| Wild Bst. p-value of RBA vs AID | 0.056 | 0.112 | 0.034 |
| Average of RBA at baseline | 2266.393 | 588.476 | 1344.893 |
| Average of AID at baseline | 1059.814 | 271.755 | 635.874 |
| ***Panel C. Curative visits*** | | | |
| **RBA x Onset** | -198.587 | -9.330 | 1.024 |
| | (282.232) | (100.025) | (58.579) |
| Large Sample p-value | 0.483 | 0.926 | 0.986 |
| Wild Bootstrap p-value | 0.520 | 0.956 | 0.972 |
| **Aid x Onset** | 57.918 | 27.306 | 43.543 |
| | (180.354) | (56.209) | (34.815) |
| Wild Bootstrap p-value | 0.752 | 0.650 | 0.206 |
| Large Sample p-value of RBA vs AID | 0.378 | 0.721 | 0.469 |
| Wild Bst. p-value of RBA vs AID | 0.424 | 0.692 | 0.538 |
| Average of RBA at baseline | 9100.750 | 3141.083 | 1447.238 |
| Average of AID at baseline | 5348.217 | 1833.116 | 834.808 |
| N | 2,744 | 2,744 | 2,744 |

*Notes:* This table reports the estimates of the treatment effect estimated from an OLS regressions of the dependent variable on the interaction of: (1) an indicator variable for RBA status with a binary indicator that equals one for the quarter in which the municipality enrolled 10% of its eligible population and after; equals zero for quarters before; and, (2) an indicator variable for Aid status, which works in the same manner as the previous variable. We include municipality and time fixed effects in the regressions. The p-values are for tests of the null that the difference is equal to zero. We present a Wild bootstrapped p-value that is robust in samples with small numbers of clusters (Cameron et al. 2008). Our Wild bootstrap procedure assigns symmetric weights and equal probability after re-sampling residuals (Davidson and Flachaire 2008) and uses 999 replications. Each column reports the effect on overall visits for a different sample. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

**Table 4: Effect of SMI RBA Model on Maternal and Infant Preventive Visits**

| Type of Visit | Family Planning (1) | Prenatal (2) | Puerperal (3) | Infant (4) |
|---|---|---|---|---|
| ***Panel A. First Visit*** | | | | |
| **RBA x Onset** | 6.578 | 24.443*** | 20.956*** | 21.405** |
| | (4.392) | (9.127) | (5.231) | (8.928) |
| Large Sample p-value | 0.137 | 0.009 | 0.00 | 0.018 |
| Wild Bootstrap p-value | 0.178 | 0.016 | 0.002 | 0.020 |
| **Aid x Onset** | -4.272* | 11.212** | 6.744*** | 10.402** |
| | (2.301) | (5.146) | (2.514) | (4.413) |
| Large Sample p-value | 0.066 | 0.032 | 0.009 | 0.020 |
| Wild Bootstrap p-value | 0.064 | 0.032 | 0.006 | 0.022 |
| Large Sample p-value of RBA vs AID | 0.017 | 0.164 | 0.012 | 0.244 |
| Wild Bst. p-value of RBA vs AID | 0.036 | 0.160 | 0.030 | 0.252 |
| Average of RBA at baseline | 51.964 | 90.643 | 86.262 | 93.643 |
| Average of AID at baseline | 29.035 | 42.997 | 42.374 | 43.544 |
| ***Panel B. Total visits (excluding the first)*** | | | | |
| **RBA x Onset** | 7.998 | 68.919** | | 45.241** |
| | (19.886) | (31.443) | | (19.575) |
| Large Sample p-value | 0.688 | 0.031 | | 0.023 |
| Wild Bootstrap p-value | 0.692 | 0.038 | | 0.022 |
| **Aid x Onset** | -15.389 | 32.259** | | 7.601 |
| | (9.404) | (14.311) | | (8.075) |
| Large Sample p-value | 0.105 | 0.026 | | 0.349 |
| Wild Bootstrap p-value | 0.090 | 0.032 | | 0.346 |
| Large Sample p-value of RBA vs AID | 0.324 | 0.241 | | 0.065 |
| Wild Bst. p-value of RBA vs AID | 0.277 | 0.268 | | 0.070 |
| Average of RBA at baseline | 135.940 | 412.988 | | 439.452 |
| Average of AID at baseline | 86.987 | 183.223 | | 211.381 |
| N | 2744 | 2744 | 2744 | 2744 |

*Notes:* This table reports the estimates of the treatment effect estimated from an OLS regressions of the dependent variable on the interaction of: (1) an indicator variable for RBA status with a binary indicator that equals one for the quarter in which the municipality enrolled 10% of its eligible population and after; equals zero for quarters before; and, (2) an indicator variable for Aid status, which works in the same manner as the previous variable. We include municipality and time fixed effects in the regressions. The p-values are for tests of the null that the difference is equal to zero. We present a Wild bootstrapped p-value that is robust in samples with small numbers of clusters (Cameron et al. 2008). Our Wild bootstrap procedure assigns symmetric weights and equal probability after re-sampling residuals (Davidson and Flachaire 2008) and uses 999 replications. Each column reports the effect on overall visits for a different sample. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

**Table 5: Effect of SMI RBA Model on Inpatient Visits**

| Type of Visit | Total | Women 15-49 | Women 15-49 Obstetric | Women 15-49 Non-Obstetric |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **RBA x Onset** | 18.246 | 10.511* | 13.327** | -2.816 |
| | (13.187) | (6.077) | (6.188) | (1.960) |
| Large Sample p-value | 0.170 | 0.087 | 0.034 | 0.154 |
| Wild Bootstrap p-value | 0.218 | 0.102 | 0.038 | 0.182 |
| **Aid x Onset** | 8.503 | 4.054 | 4.013 | 0.041 |
| | (7.243) | (3.994) | (3.193) | (1.235) |
| Large Sample p-value | 0.243 | 0.313 | 0.212 | 0.974 |
| Wild Bootstrap p-value | 0.248 | 0.302 | 0.2 | 0.936 |
| Large Sample p-value of RBA vs AID | 0.476 | 0.313 | 0.139 | 0.208 |
| Wild Bst. p-value of RBA vs AID | 0.596 | 0.342 | 0.182 | 0.246 |
| Average of RBA at baseline | 324.192 | 133.873 | 96.666 | 37.208 |
| Average of AID at baseline | 171.405 | 70.545 | 50.792 | 19.754 |
| N | 4312 | 4312 | 4312 | 4312 |

*Notes:* This table reports the estimates of the treatment effect estimated from an OLS regressions of the dependent variable on the interaction of an indicator variable for RBA status with a binary indicator that equals one for the quarter in which the municipality enrolled 10% of its eligible population and after; equals zero for quarters before; and, (2) an indicator variable for Aid status, which works in the same manner as the previous variable. We include municipality and time fixed effects in the regressions. The p-values are for tests of the null that the difference is equal to zero. We present a Wild bootstrapped p-value that is robust in samples with small numbers of clusters (Cameron et al. 2008). Our Wild bootstrap procedure assigns symmetric weights and equal probability after re-sampling residuals (Davidson and Flachaire 2008) and uses 999 replications. Each column reports the effect on overall visits for a different sample. Standard errors are in parentheses.
* p < 0.10, ** p < 0.05, *** p < 0.01.

**Table 6: Effect of SMI RBA Model on timing of care**

| Type of Visit | Prenatal in first trimester of pregnancy | Infant in first week after birth |
|---|---|---|
| | (1) | (2) |
| **RBA x Onset** | -0.001 | 0.116*** |
| | (0.028) | (0.042) |
| Large Sample p-value | 0.979 | 0.007 |
| Wild bootstrap p-value | 0.940 | 0.004 |
| **Aid x Onset** | -0.058** | -0.045 |
| | (0.022) | (0.030) |
| Large Sample p-value | 0.010 | 0.138 |
| Wild bootstrap p-value | 0.014 | 0.156 |
| Large Sample p-value of RBA vs AID | 0.022 | 0.000 |
| Wild Bst. p-value of RBA vs AID | 0.030 | 0.000 |
| Average of RBA at baseline | 0.640 | 0.511 |
| Average of AID at baseline | 0.714 | 0.649 |
| N | 2740 | 2744 |

Notes: This table reports the estimates of the treatment effect estimated from an OLS regressions of the dependent variable on the interaction of an indicator variable for RBA status with a binary indicator that equals one for the quarter in which the municipality enrolled 10% of its eligible population and after; equals zero for quarters before; and, (2) an indicator variable for Aid status, which works in the same manner as the previous variable. We include municipality and time fixed effects in the regressions. The p-values are for tests of the null that the difference is equal to zero. We present a Wild bootstrapped p-value that is robust in samples with small numbers of clusters (Cameron et al. 2008). Our Wild bootstrap procedure assigns symmetric weights and equal probability after re-sampling residuals (Davidson and Flachaire 2008) and uses 999 replications. Each column reports the effect on overall visits for a different sample. Standard errors are in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

## Table 7: Effect of SMI RBA on other population groups

| Type of Visit | Total (1) | Preventive (2) | Curative (3) |
|---|---|---|---|
| | ***A. Men 15-49*** | | |
| **RBA x Onset** | 84.227** | 62.452** | 21.775 |
| | (33.181) | (28.607) | (17.330) |
| Large Sample p-value | 0.013 | 0.031 | 0.212 |
| Wild Bootstrap p-value | 0.018 | 0.052 | 0.226 |
| **Aid x Onset** | 41.592** | 16.800 | 24.792 |
| | (16.540) | (10.841) | (15.875) |
| Large Sample p-value | 0.014 | 0.124 | 0.122 |
| Wild Bootstrap p-value | 0.006 | 0.140 | 0.100 |
| Large Sample p-value of RBA vs AID | 0.211 | 0.122 | 0.866 |
| Wild Bst. p-value of RBA vs AID | 0.250 | 0.156 | 0.878 |
| Average of RBA at baseline | 854.214 | 2.929 | 851.286 |
| Average of AID at baseline | 478.182 | 2.406 | 475.777 |
| | ***B. Elderly*** | | |
| **RBA x Onset** | 74.324** | 80.004*** | -5.680 |
| | (37.142) | (24.822) | (33.787) |
| Large Sample p-value | 0.048 | 0.002 | 0.867 |
| Wild Bootstrap p-value | 0.054 | 0.002 | 0.890 |
| **Aid x Onset** | 7.289 | 10.752 | -3.464 |
| | (22.762) | (10.429) | (20.959) |
| Large Sample p-value | 0.750 | 0.305 | 0.869 |
| Wild Bootstrap p-value | 0.718 | 0.334 | 0.878 |
| Large Sample p-value of RBA vs AID | 0.088 | 0.005 | 0.950 |
| Wild Bst. p-value of RBA vs AID | 0.090 | 0.008 | 0.944 |
| Average of RBA at baseline | 1300.321 | 17.905 | 1282.417 |
| Average of AID at baseline | 765.085 | 9.912 | 755.173 |
| N | 2744 | 2744 | 2744 |

*Notes:* This table reports the estimates of the treatment effect estimated from an OLS regressions of the dependent variable on the interaction of an indicator variable for RBA status with a binary indicator that equals one for the quarter in which the municipality enrolled 10% of its eligible population and after; equals zero for quarters before; and, (2) an indicator variable for Aid status, which works in the same manner as the previous variable. We include municipality and time fixed effects in the regressions. The p-values are for tests of the null that the difference is equal to zero. We present a Wild bootstrapped p-value that is robust in samples with small numbers of clusters (Cameron et al. 2008). Our Wild bootstrap procedure assigns symmetric weights and equal probability after re-sampling residuals (Davidson and Flachaire 2008) and uses 999 replications. Each column reports the effect on overall visits for a different type of visit. Standard errors are in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

## Table 8: Effect of SMI RBA Model on Consultations by Origin

| Population Group | Same Municipality (1) | Adjacent Municipalities (2) | Other Municipalities (3) |
|---|---|---|---|
| ***Panel A. Outpatient visits*** | | | |
| **RBA x Onset** | 859.307** | 186.216 | 26.406 |
| | (370.414) | (136.028) | (28.641) |
| Large Sample p-value | 0.022 | 0.174 | 0.359 |
| Wild Bootstrap p-value | 0.046 | 0.186 | 0.350 |
| **Aid x Onset** | 368.276** | 194.728** | 61.328 |
| | (169.261) | (92.645) | (49.905) |
| Large Sample p-value | 0.032 | 0.038 | 0.222 |
| Wild Bootstrap p-value | 0.038 | 0.026 | 0.260 |
| Large Sample p-value of RBA vs AID | 0.181 | 0.955 | 0.522 |
| Wild Bst. p-value of RBA vs AID | 0.214 | 0.988 | 0.664 |
| Average of RBA at baseline | 9466.024 | 822.024 | 142.940 |
| Average of AID at baseline | 5203.846 | 390.333 | 177.755 |
| ***Panel B. Preventive visits*** | | | |
| **RBA x Onset** | 939.877*** | 81.762** | 4.107 |
| | (341.839) | (40.073) | (4.774) |
| Large Sample p-value | 0.007 | 0.044 | 0.392 |
| Wild Bootstrap p-value | 0.008 | 0.034 | 0.426 |
| **Aid x Onset** | 185.248 | 55.840*** | 9.813* |
| | (117.136) | (20.048) | (5.226) |
| Large Sample p-value | 0.117 | 0.006 | 0.063 |
| Wild Bootstrap p-value | 0.114 | 0.008 | 0.038 |
| Large Sample p-value of RBA vs AID | 0.029 | 0.540 | 0.356 |
| Wild Bst. p-value of RBA vs AID | 0.046 | 0.614 | 0.414 |
| Average of RBA at baseline | 2110.143 | 98.000 | 11.845 |
| Average of AID at baseline | 991.742 | 43.613 | 11.604 |
| ***Panel C. Curative visits*** | | | |
| **RBA x Onset** | -80.570 | 104.454 | 22.299 |
| | (297.601) | (112.945) | (25.887) |
| Large Sample p-value | 0.787 | 0.357 | 0.391 |
| Wild Bootstrap p-value | 0.840 | 0.368 | 0.406 |
| **Aid x Onset** | 183.028 | 138.888* | 51.515 |
| | (146.801) | (77.678) | (45.727) |
| Wild Bootstrap p-value | 0.224 | 0.068 | 0.334 |
| Large Sample p-value of RBA vs AID | 0.358 | 0.780 | 0.557 |
| Wild Bst. p-value of RBA vs AID | 0.404 | 0.826 | 0.702 |
| Average of RBA at baseline | 7355.881 | 724.024 | 131.095 |
| Average of AID at baseline | 4212.104 | 346.720 | 166.151 |
| N | 2,744 | 2,744 | 2,744 |

*Notes:* This table reports the estimates of the treatment effect estimated from an OLS regressions of the dependent variable on the interaction of: (1) an indicator variable for RBA status with a binary indicator that equals one for the quarter in which the municipality enrolled 10% of its eligible population and after; equals zero for quarters before; and, (2) an indicator variable for Aid status, which works in the same manner as the previous variable. We include municipality and time fixed effects in the regressions. The p-values are for tests of the null that the difference is equal to zero. We present a Wild bootstrapped p-value that is robust in samples with small numbers of clusters (Cameron et al. 2008). Our Wild bootstrap procedure assigns symmetric weights and equal probability after re-sampling residuals (Davidson and Flachaire 2008) and uses 999 replications. Each column reports the effect on overall visits for a different sample. Standard errors are in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

## Table 9: Effect of SMI RBA on ambulatory care sensitive hospitalizations

| Type of Visit | ACS Hospitalizations (1) | Women 15-49 ACS Hospitalizations (2) | Child < 5 ACS Hospitalizations (3) | Elderly ACS Hospitalizations (4) |
|---|---|---|---|---|
| **RBA x Onset** | -6.520 | -2.132 | -0.569 | -0.415 |
| | (7.301) | (1.688) | (2.604) | (1.055) |
| Large Sample p-value | 0.374 | 0.210 | 0.827 | 0.695 |
| Wild bootstrap p-value | 0.450 | 0.278 | 0.830 | 0.830 |
| **Aid x Onset** | 0.636 | 0.365 | 0.004 | 0.567 |
| | (1.553) | (0.573) | (0.991) | (0.419) |
| Large Sample p-value | 0.683 | 0.526 | 0.997 | 0.179 |
| Wild bootstrap p-value | 0.688 | 0.604 | 0.986 | 0.192 |
| Large Sample p-value of RBA vs AID | 0.324 | 0.154 | 0.828 | 0.367 |
| Wild Bst. p-value of RBA vs AID | 0.426 | 0.176 | 0.840 | 0.454 |
| Average of RBA at baseline | 74.614 | 13.471 | 24.877 | 17.006 |
| Average of AID at baseline | 40.123 | 7.735 | 12.702 | 9.324 |
| N | 4312 | 4312 | 4312 | 4312 |

*Notes:* ACS are ambulatory care sensitive hospitalizations as defined in Rodriguez et al (2012) using the ICD-10 code for primary diagnosis in hospital discharges. Women and children ACS hospitalizations are defined as those discharges from the sex and age-group that are ACS according to their primary diagnosis and the broad list used in Rodriguez et al (2012). This table reports the estimates of the treatment effect estimated from an OLS regressions of the dependent variable on the interaction of an indicator variable for RBA status with a binary indicator that equals one for the quarter in which the municipality enrolled 10% of its eligible population and after; equals zero for quarters before; and, (2) an indicator variable for Aid status, which works in the same manner as the previous variable. We include municipality and time fixed effects in the regressions. The p-values are for tests of the null that the difference is equal to zero. We present a Wild bootstrapped p-value that is robust in samples with small numbers of clusters (Cameron et al. 2008). Our Wild bootstrap procedure assigns symmetric weights and equal probability after re-sampling residuals (Davidson and Flachaire 2008) and uses 999 replications. Each column reports the effect on overall visits for a different sample. Standard errors are in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

**Table 10: Effect of SMI RBA on primary units and primary care personnel**

| | Health Units | | Primary Health Care Personnel per 1000 inhabitants by type | | | | | Primary Health Care Services per Personnel | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | *Total primary Units* | *Primary Units per capita* | *Overall* | *Medical staff* | *Nurses* | *CHWs* | *Admin. Staff* | *Outpatient visits* | *Preventive visits* | *Curative Visits* |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10 |
| *Panel A: Mean pre-treatment (2009) key primary care inputs* | | | | | | | | | | |
| RBA | 1.36 | 0.11 | 1.85 | 0.40 | 0.27 | 0.87 | 0.25 | 3428.131 | 794.0962 | 2634.035 |
| National Funds | 1.13 | 0.25 | 2.43 | 0.56 | 0.43 | 0.85 | 0.53 | 2793.556 | 578.1442 | 2215.412 |
| Aid | 1.11 | 0.25 | 2.40 | 0.58 | 0.53 | 0.71 | 0.53 | 3289.363 | 616.7792 | 2672.584 |
| *Panel B: Mean post-treatment (2015) key primary care inputs* | | | | | | | | | | |
| RBA | 5.43 | 0.35 | 3.35 | 0.57 | 0.76 | 1.22 | 0.65 | 2077.278 | 959.6043 | 1117.673 |
| National Funds | 2.42 | 0.43 | 3.97 | 0.55 | 0.81 | 1.46 | 1.04 | 2411.054 | 966.5317 | 1444.522 |
| Aid | 2.66 | 0.43 | 4.12 | 0.65 | 0.93 | 1.51 | 0.89 | 2308.589 | 860.3403 | 1448.249 |
| *Panel C: Difference-in-difference in level of resources* | | | | | | | | | | |
| **RBA Vs. National Funds** | 2.781** | 0.061 | -0.046 | 0.171 | 0.106 | -0.266** | -0.110 | -968.351* | -222.879 | -745.472* |
| | (1.339) | (0.063) | (0.298) | (0.144) | (0.089) | (0.130) | (0.079) | (577.921) | (194.698) | (434.526) |
| Main p-value | 0.330 | 0.912 | 0.876 | 0.235 | 0.236 | 0.040 | 0.166 | 0.094 | 0.252 | 0.086 |
| Wild bootstrap p-value | 0.009 | 0.204 | 0.886 | 0.254 | 0.261 | 0.038 | 0.157 | 0.018 | 0.107 | 0.018 |
| **Aid Vs National Funds** | 0.257 | -0.004 | 0.172 | 0.079 | 0.025 | 0.192 | -0.149* | -598.272 | -144.826 | -453.445 |
| | (0.592) | (0.069) | (0.283) | (0.144) | (0.087) | (0.147) | (0.085) | (505.516) | (166.481) | (376.321) |
| Main p-value | 0.664 | 0.954 | 0.544 | 0.581 | 0.771 | 0.191 | 0.079 | 0.237 | 0.384 | 0.228 |
| Wild bootstrap p-value | 0.568 | 0.933 | 0.523 | 0.575 | 0.782 | 0.226 | 0.083 | 0.086 | 0.222 | 0.087 |
| Large Sample p-value of RBA vs AID | 0.056 | 0.152 | 0.364 | 0.407 | 0.259 | 0.000 | 0.567 | 0.520 | 0.686 | 0.499 |
| Wild Bst. p-value of RBA vs AID | 0.008 | 0.046 | 0.376 | 0.410 | 0.263 | 0.000 | 0.573 | 0.370 | 0.585 | 0.341 |
| Observations | 196 | 196 | 196 | 196 | 196 | 196 | 196 | 188 | 188 | 188 |

*Notes*: Panel C of this table, RBA VS. National Funds reports the estimates of the treatment effect estimated from an OLS regressions of the dependent variable on the interaction of an indicator variable for RBA status (1=RBA, 0=National Funds) with a binary indicator that equals one for the post-treatment period, that is 2015 and zero for the pre-treatment period available in the data (2009).AID Vs. National Funds report a similar estimate but the treatment variable is changed so that 1=AID and 0=National Funds. Only two time periods are included in the data 2009 and 2015, since we cannot assign personnel to periods in between to specific municipalities in the data provided by the MoH. This is because in the transition of the reform (2010-2014), personnel were allocated to the region they worked on in the data and not the specific primary unit. The p-values are for tests of the null that the difference is equal to zero. We present both the p-value computed for large samples and a Wild bootstrapped p-value that is robust in samples with small numbers of clusters (Cameron et al. 2008). Our Wild bootstrap procedure assigns symmetric weights and equal probability after re-sampling residuals (Davidson and Flachaire 2008) and uses 999 replications. Each column reports the effect on a different outcome of interest. All outcomes from column (2) to (7) represent rates of resources per 1000 inhabitants in the municipality of interest using data from population projections. Outcomes from columns (8) to (10) represent consultations by medics and nurses. The number of observations in columns (8) to (10) is slightly smaller as a few municipalities had no doctors or nurses in 2009. Standard errors are in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01. ‘

## Table 11: Effect of SMI RBA on preventive care according to place of occurrence

| Type of Facility | Primary Care Facilities<br>(1) | Hospitals<br>(2) |
|---|---|---|
| ***Panel A. Total Primary Care Visits*** | | |
| RBA x Onset | 872.364* | -106.739 |
| | (440.852) | (227.146) |
| Large Sample p-value | 0.051 | 0.639 |
| Wild Bootstrap p-value | 0.072 | 0.680 |
| Aid x Onset | 281.595 | -13.638 |
| | (216.792) | (93.321) |
| Large Sample p-value | 0.197 | 0.884 |
| Wild Bootstrap p-value | 0.234 | 0.942 |
| Large Sample p-value of RBA vs AID | 0.199 | 0.704 |
| Wild Bst. p-value of RBA vs AID | 0.226 | 0.742 |
| Average of RBA at baseline | 8851.631 | 2515.512 |
| Average of AID at baseline | 5284.075 | 1123.956 |
| ***Panel B. Preventive Primary Care Visits*** | | |
| RBA x Onset | 960.055*** | 4.157 |
| | (346.107) | (20.529) |
| Large Sample p-value | 0.007 | 0.840 |
| Wild Bootstrap p-value | 0.010 | 0.866 |
| Aid x Onset | 226.668* | -16.629 |
| | (126.305) | (15.557) |
| Large Sample p-value | 0.076 | 0.288 |
| Wild Bootstrap p-value | 0.068 | 0.356 |
| Large Sample p-value of RBA vs AID | 0.039 | 0.404 |
| Wild Bst. p-value of RBA vs AID | 0.060 | 0.394 |
| Average of RBA at baseline | 2176.560 | 89.833 |
| Average of AID at baseline | 1006.865 | 52.95 |
| ***Panel C. Curative Primary Care Visits*** | | |
| RBA x Onset | -87.691 | -106.739 |
| | (313.982) | (227.146) |
| Large Sample p-value | 0.781 | 0.639 |
| Wild Bootstrap p-value | 0.714 | 0.680 |
| Aid x Onset | 54.927 | -13.638 |
| | (251.091) | (93.321) |
| Wild Bootstrap p-value | 0.890 | 0.964 |
| Large Sample p-value of RBA vs AID | 0.692 | 0.661 |
| Wild Bst. p-value of RBA vs AID | 0.738 | 0.996 |
| Average of RBA at baseline | 6675.071 | 2425.679 |
| Average of AID at baseline | 4277.211 | 1071.006 |
| N | 2744 | 2744 |

*Notes:* This table reports the estimates of the treatment effect estimated from an OLS regressions of the dependent variable on the interaction of: (1) an indicator variable for RBA status with a binary indicator that equals one for the quarter in which the municipality enrolled 10% of its eligible population and after; equals zero for quarters before; and, (2) an indicator variable for Aid status, which works in the same manner as the previous variable. We include municipality and time fixed effects in the regressions. The p-values are for tests of the null that the difference is equal to zero. We present a Wild bootstrapped p-value that is robust in samples with small numbers of clusters (Cameron et al. 2008). Our Wild bootstrap procedure assigns symmetric weights and equal probability after re-sampling residuals (Davidson and Flachaire 2008) and uses 999 replications. Each column reports the effect on all three outcomes for a different care unit (primary or hospital). Standard errors are in parentheses. *p < 0.10, ** p < 0.05, *** p < 0.01.