Title: The Indirect Effects of Subsidised Healthcare in Rural Ghana

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# Abstract

Social networks provide a channel through which health policies and programmes can affect those with close social ties to the intended beneficiaries. We provide experimental evidence on the indirect effects of heavily subsidised healthcare. By exploiting data on 2,151 households from a randomised study conducted in a rural district of Ghana in 2005, we estimate the extent to which social networks, defined by religion, influence the uptake of primary care services. We find that people socially connected to households with subsidised care are *less* likely to use primary care services despite the fact that the direct effect of the intervention is *positive*. We extend the empirical analysis to consider the implications of these changes in behaviour for welfare but find no evidence of indirect effects on child health and healthcare spending. In the context of this study, the findings highlight the potential for healthcare subsidies to have unintended consequences.

# Introduction

Rural households in developing countries are located in communities where they are typically embedded in strong social networks. Such kinship relationships give rise to the possibility that policies and programmes affect not only families directly targeted but also those with close social ties to the intended beneficiaries. Indirect effects are important to capture if policymakers are to get a sense of the overall impact of a policy on the entire population. Interest is likely to be particularly acute when the indirect effects are large relative to the direct effects of a policy or operate in the opposing direction.

Indirect effects due to the disease environment have long been recognised in public health. Vaccinations against contagious disease offer protection both to individuals given the vaccine and those without immunity in close proximity. Depending on the disease, herd immunity from epidemiological externalities is estimated to protect unvaccinated individuals when the proportion immunised is as low as 80 percent ([Fine, 1993](#_ENREF_9)), providing one of the key reasons for why governments subsidise the price of vaccines.

Less recognised are the behavioural channels through which social interactions can modify the overall impact of a health intervention. One mechanism is social learning whereby individuals learn via others about the benefits of a health product. A number of recent studies have examined the role of peers in the adoption of health interventions, showing that financial incentives to increase the uptake of disease-specific technologies – insecticide-treated bed nets, HIV testing, and deworming treatment – affect not only the behaviour of the intended beneficiaries but also that of their peers ([Dupas, 2014](#_ENREF_8); [Godlonton & Thornton, 2012](#_ENREF_11); [Kremer & Miguel, 2007](#_ENREF_12)).

This papers examines the indirect effects of subsidised healthcare in rural Ghana. The intervention involved paying the health insurance premium of an existing prepayment scheme, thereby providing free public healthcare for beneficiaries. It provides experimental evidence on the extent to which social networks – defined primarily by religion – influence the uptake of primary care services. The findings are relevant for policy because they can inform decisions on whether and for how long to subsidise health services and, in doing so, speak to the sustainability of government and donor investments in health. The analysis build on two previous papers reporting the direct effect of the intervention ([Ansah, Narh-Bana, Asiamah, Dzordzordzi, Biantey, Dickson et al., 2009](#_ENREF_3); [Powell-Jackson, Hanson, Whitty, & Ansah, 2014](#_ENREF_18)), It also complements a number of studies about social learning ([Adhvaryu, 2014](#_ENREF_1); [Foster & Rosenzweig, 2010](#_ENREF_10); [Munshi & Myaux, 2006](#_ENREF_15)) and those that have used experimental variation in exposure to a health technology induced by price subsidies to identify social effects ([Dupas, 2014](#_ENREF_8); [Godlonton & Thornton, 2012](#_ENREF_11); [Kremer & Miguel, 2007](#_ENREF_12); [Oster & Thornton, 2012](#_ENREF_17)).

Our paper contributes to the literature on indirect effects in health in several ways. First, as countries make efforts to move towards universal health coverage, social insurance schemes are increasingly being rolled out ([World Health Organization, 2010](#_ENREF_26)). This paper provides some of the first evidence on the ripple effects of such a scheme. Second, the subsidy under investigation was applied to a broad package of health services making the findings generalisable beyond the disease-specific health products studied elsewhere. Finally, much of the literature on social effects focuses on the adoption of health technologies and stops short of assessing the implications for welfare. We extend our empirical analysis to consider the indirect effect of subsidised healthcare on child health (as measured by haemoglobin levels) and financial strain (as measured by out-of-pocket healthcare spending).

# Literature

The theoretical literature highlights a number of channels through which healthcare subsidies could influence uptake of a health product or services through a social network. Kremer and Miguel ([2007](#_ENREF_12)) develop a framework in which individuals in a social network receive information about adoption, effectiveness of the technology and how to use the technology. The model allows for indirect effects through the disease environment, a pure imitation effect, social learning in how to use the technology, and social learning about the benefits of the technology. Imitating the behaviour of peers and learning how to use a technology from peers always result in positive indirect effects. By contrast, externalities through the disease environment can generate negative social effects because the protection from disease afforded those in close proximity to adopters of the health technology reduces the need to adopt the technology themselves. The social effect from information on the benefits of the technology can be either positive or negative depending on the difference between prior beliefs and actual private adoption benefits.

The model developed by Kremer and Miguel (2007) is concerned with adoption peer effects arising from increased exposure to a technology. Its relevance to the current study lies in the fact that the direct effect of the free healthcare intervention was to increase use of primary care services ([Ansah, Narh-Bana, Asiamah et al., 2009](#_ENREF_3)). The intervention also substantially reduced health care spending by households ([Powell-Jackson, Hanson, Whitty et al., 2014](#_ENREF_18)), providing an income shock which could generate indirect effects through informal risk-sharing. Angelucci & De Giorgi (2009) show that cash transfers targeting the poor can affect others within the same village when there is informal risk-sharing. In the absence of formal credit and insurance markets, beneficiaries may share part of their income by providing gifts and loans to other families in their social network. In a standard risk-sharing model, households in a village fully insure against idiosyncratic health shocks by pooling resources and consuming a fixed share of total income ([Angelucci & De Giorgi, 2009](#_ENREF_2); [Townsend, 1994](#_ENREF_22)). Household consumption is thus independent of individual income conditional on total resources. The key implication is that if there is an increase in the income of some households in the village (group A), aggregate resources in the village increase, and resources are allocated to other households (group B) in the village through informal mechanisms. How informal risk-sharing affects healthcare utilisation of households in the social network then depends on the nature of the resources transferred, as we discuss in Section 5.

The most rigorous empirical research on social networks in health exploits experimental variation in the exposure to a health technology induced by price subsidies to identify indirect effects. Studies on insecticide-treated bed nets, menstrual cups, and HIV testing have found evidence of positive social effects, whereby adoption of a health product or service by an individual leads others in the same social network to take it up ([Dupas, 2014](#_ENREF_8); [Godlonton & Thornton, 2012](#_ENREF_11); [Oster & Thornton, 2012](#_ENREF_17)). By contrast, a study in Kenya found evidence of negative social effects in the adoption of deworming treatment ([Kremer & Miguel, 2007](#_ENREF_12)). In these studies, the most plausible explanation for the emergence of social effects is social learning. Through social interaction, individuals learn how to use or learn about the benefits of a technology, which in turn affects their own behaviour. In the study of deworming, it is argued negative learning effects were driven by households learning that private costs from the side effects of the drugs (nausea) outweighed private benefits (lower infection rates).

# Methods

# Free Care Experiment

We use data from a randomised trial of removing user fees for health care undertaken in 2005 in Dangme West, a poor rural district in Southern Ghana ([Ansah, Narh-Bana, Asiamah et al., 2009](#_ENREF_3); [Powell-Jackson, Hanson, Whitty et al., 2014](#_ENREF_18)). Malaria was the leading cause of morbidity and mortality in children under five in Ghana at the time of the study, accounting for 45 percent of reported deaths in this age group ([World Health Organization, 2009](#_ENREF_25)). The study provided free health care to households randomly assigned to the intervention group by paying the premium for them to enrol into an existing prepayment health insurance scheme in May 2004. Households in the control group continued to pay a fee-for-service for publicly provided health services in accordance with the national policy at the time. The community prepayment insurance scheme covered the costs of primary care, including diagnostics and drugs with no limit, and a limited set of services provided at the secondary level referral hospital. It covered the costs of health services in the public sector, allowing members to choose from any of the primary health facilities in the district and a referral hospital of their choice when referred.

The study was announced to the public only once the enrolment window for the year was closed, such that all households that were going to self-select into insurance had already done so and were excluded from randomisation. Treatment and control thus comprised households that had chosen not to self-enrol into the insurance scheme. No household was able to change their assigned group at any point during the one year study period because the enrolment process occurred only once a year. The study assisted households with the administrative process of enrolment, informing members of their benefits and ensuring picture identification cards were issued. Ethical approval for the original trial was obtained from the ethical review board of the Ghana Health Service and the London School of Hygiene and Tropical.

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# Sample

Households with at least one child aged 6 to 59 months and not already enrolled in the prepayment health insurance scheme were eligible to participate in the study. The sample frame consisted of approximately 8,700 households with children under five years of age living in the study area. A total of 2,332 households were selected at random using a computer random number generator and then visited in person. No household refused consent but 138 were excluded from the main experimental study because they had already enrolled voluntarily into the prepayment health insurance scheme by the time the registration window had closed.

The remaining 2,194 households were randomly assigned to treatment and control groups. A public lottery that involved pulling out “yes” and “no” pieces of paper from a rotating barrel was used to assign households. Individual households were therefore well informed as to the treatment assignment of their neighbours. At the baseline household survey in May 2004 a total of 2,151 households were found and interviewed (1,053 households in the intervention arm and 1,098 in the control arm). In the final household survey, carried out at the end of the malaria transmission season between December 2004 and February 2005, 969 households (92 percent) in the intervention arm and 1,012 households (92 percent) in the control arm were successfully followed up. The sampling methods are described in more detail elsewhere ([Ansah, Narh-Bana, Asiamah et al., 2009](#_ENREF_3)).

# Outcomes

Our main outcome of interest is the number of primary care visits per person each year. Data on healthcare seeking behaviour were collected using pictorial diaries that were supplied to households over a six month follow-up period and collected by fieldworkers on a monthly basis so as to limit problems of recall ([Ansah & Powell-Jackson, 2013](#_ENREF_4)). The diaries were designed specifically for a situation in which the majority of child carers – the primary respondents in our study – were not literate. They recorded the type of illness the child suffered from during the period as well as the type of health provider visited, with the possible options including primary health clinic, hospital, private pharmacy, and traditional healer. We refer to the first two as formal health care providers and the remaining two choices as informal providers.

We go beyond much of the literature to examine the impact of social networks on individual welfare. The first outcome is the level of haemoglobin which underpins the measurement of anaemia, a multi-factorial, broad-based measure of health status, particularly appropriate in a country where malaria is the leading cause of morbidity and mortality amongst children under five years of age. It is a commonly used objective outcome of community interventions on malaria morbidity and its causes include malaria, inadequate dietary intake of iron and intestinal worm infection, all of which are entirely treatable. Haemoglobin concentration was measured just before and almost one year after the introduction of the free care intervention during a household survey that took finger-prick blood samples from children aged between 6 and 59 months.

The second welfare measure is out-of-pocket spending on health care. Data on health spending were collected during the household survey at both baseline and endline, using a recall period of four weeks. We trim the sample at the 99.5th percentile owing to a small number of observations with implausibly high expenditure values. The exchange rate at the time of the study was $US 1 = 10,600 cedis. Expenditure data relate to the costs of medical care and other costs such as those associated with transport to and from the health care provider. Finally, additional data on characteristics of the family were collected through the household survey. Baseline descriptive statistics of the outcomes and covariates are presented in Table 1 (Panel A and Panel B).

# Social Network Measures

We identify households in a social network using cohort-based measures that are defined along the lines of religion, ethnicity and occupation. The data do not contain information on any other household characteristics that could provide the basis to construct a measure of social links. Other commonly used measures in the literature are based on the geographical proximity of individuals or information on respondents’ closest friends and relatives. Cohort-based networks rest on the idea that social interaction is greater between individuals of certain traits. They capture the extent of potential as opposed to actual social ties ([Bandiera & Rasul, 2006](#_ENREF_7)).

Each of our three definitions is potentially relevant in the context of this study, although we regard religion as our primary means of defining a social network. Individuals identify themselves with a particular religion – a social institution widely recognised as being important in Ghana. Religion brings people together, most obviously but not exclusively through a common place of worship. Ethnic groups are well defined in Ghana and are considered central to a person’s identity. Ethnicity tends to signify a common language and set of cultural norms that encourage social interactions. Finally, occupation is relevant because individuals of the same profession within a village are likely to spend more of the working day together, whether it be farming, fishing and so on.

In the baseline household survey, parents were asked about their religion, ethnicity and occupation. We make use of this information to compute for each household in the sample: 1) the number of neighbours in the same reference group who were given free healthcare; 2) the number of neighbours in the same reference group; and 3) the share of neighbours in the same reference group with free care, where *neighbours* is used throughout as short-hand for other sampled households residing in the same village. By construction, because the variables are defined at the village level, they also capture households living nearby to each other.

We have data on households resident in 158 villages (communities) with an average number of 33 households (Table 1, Panel C). Each household has on average 25 other households of the same religion in its village, of whom 13 were given subsidised healthcare. The share of neighbours in the same reference group ranges from 45% to 47% depending on the measure of social ties. As we can see by the standard deviations, there is considerable variation in these measures.

# Empirical Strategy

There are well-known methodological challenges in the estimation of social effects ([Manski, 1993](#_ENREF_13)). The problem arises when trying to infer whether the average behaviour in a group influences the behaviour of the individuals that comprise the group when in fact the former might simply reflect the latter. Put another way, it can be difficult to separate whether individuals who are socially connected behave in a similar manner because they influence each other or because they have similar (unobserved) characteristics.

Following previous studies ([Dupas, 2014](#_ENREF_8); [Godlonton & Thornton, 2012](#_ENREF_11); [Kremer & Miguel, 2007](#_ENREF_12); [Oster & Thornton, 2012](#_ENREF_17)), we exploit the randomised study design to estimate the indirect effects of free healthcare through social networks. Randomisation of free healthcare was at the individual household level. Hence, not only is individual assignment to the free healthcare intervention random, but who and how many people within an individual’s social network get free healthcare is also random. Table A1 in the Appendix provides evidence in support of the integrity of the experimental design. Moreover, it shows that there is no association between the social network measures and the receipt of free healthcare. The basic idea behind the analysis is to compare primary care utilisation across individuals who have the same total number of social contacts but, by chance, have a different number of social contacts with free healthcare. In practice, our variable of interest – generating the exogenous variation in peer behaviour – is the share of neighbours of the same religion with free healthcare.

The analysis of social effects is conducted at the household level, although results are similar if the unit of observation is a child (results available on request). In families with more than one child, we take the average rate of primary care use across all children. Our main specification is of the form:

$y\_{ij}=β\_{0}+β\_{1}FreeCare\_{ij}+β\_{2}ShareFreeCare\_{ij}+X\_{ij}β\_{3}+ε\_{ij}$ (1)

where $y\_{ij}$ is primary health care visits per year of household $i$ in village j, $FreeCare\_{ij}$is a dummy equal to 1 if the household was given free healthcare, $X\_{ij}$ is a vector of household characteristics, and $ε\_{ij}$ is the disturbance term. Our variable of interest is $ShareFreeCare\_{ij}$, the share of neighbours of the same religion given free care. We impute this share to be zero if there are no neighbours of the same religion. In the explanatory variables, $X\_{ij}$, we include the total number of neighbours of the same religion. We also include in $X\_{ij}$ a set of demographic controls that include years of education of the mother, the number of children in the household, an asset index, and dummies for different categories of distance to the nearest health clinic, religion and ethnicity. The demographic controls were all measured at baseline. We run regressions of a similar form to generate results for other outcomes and when using different definitions of social networks. Standard errors are clustered at the village level in all regressions.

To identify social effects we could have characterised our use of free care in the empirical strategy as a mechanism of convenience – ie. an intervention that provides exogenous variation in exposure to primary health services. A natural extension then would be to pursue an instrumental variable approach, using random assignment to instrument take-up of primary care of socially connected families, as in Godlonton and Thornton ([2012](#_ENREF_11)). Such a strategy would identify the effect of others’ health seeking behaviour on that of family $i$. However, the exclusion restriction for the instrument requires that free care generated social effects only through its influence on health seeking behaviour, an assumption we believe is difficult to maintain given the nature of the intervention. As already discussed in Section 2, the exclusion restriction will likely be violated if subsidised healthcare stimulates greater informal risk sharing between households within a social network.

# Results

# Religion-Based Networks

We begin by defining social networks in terms of religious affiliation. Table 2 provides estimates of social effects on the number of primary health care visits per year. The direct effect of removing user fees on health care use is positive and statistically significant at the 5 percent level (Table 2, column 1). Free care increases utilisation by 0.33 clinic visits per year. This variation in health seeking behaviour gives rise to the possibility that any social effects we identify may be generated through increased exposure to health services.

The social effect findings suggest that a family’s religious network has a negative influence on health seeking behaviour (Table 2, column 1). The coefficient of interest shows that increasing the proportion of neighbours of the same religion with free care by 100 percentage points reduces the household’s own utilisation by 0.78 clinic visits per year. This implies that households are almost 30 percent less likely to use primary care if all of their sampled neighbours with the same religion received free care. The finding remains robust to the inclusion of the total number of sample households in the same village (Table 2, column 2). In unreported results, when we test for heterogeneity in the social effect according to own-free care status by running a specification in which we interact $FreeCare\_{ij}$ with $ShareFreeCare\_{ij}$ we find the coefficient on the interaction is positive but insignificant.

Recall that our variable of interest, the share of neighbours of the same religion with free care, is measured at the village level and, by construction, captures households living nearby. It may therefore be acting as a proxy for geographical proximity. To explore whether the results are explained by the geographic proximity of families, irrespective of the social ties between them, we control for share of neighbours of other religions with free care. Religious ties remain significant and of the same magnitude, while the share of neighbours of other religions with free care is not associated with primary care use (Table 2, column 3). This finding suggests that geographical proximity is not driving the result and our measure of religious connections has empirical content. In an additional robustness check we include the number rather than the share of neighbours of the same religion with free care. The results remain qualitatively unchanged (Table 2, column 4). Finally, in Table A2 of the Appendix, we show the results remain similar when we use a Poisson regression.

# Alternative Cohort-Based Networks

We next consider other types of cohort-based social networks, namely those defined according to the ethnicity and occupation of the household head. Table 3 presents social effect estimates for these alternative social networks. When we define connections in terms of ethnicity, the association between our social network measure and use of primary care is negative but not statistically significant (Table 3, column 1). The equivalent result for social connections defined in terms of occupation is similar in magnitude and significant at the 10 percent level (Table 3, column 2).

Social connections to households with free care in each of the three networks that we define are positively correlated with each other and it may be the case that different types of networks overlap. If so, each of type of network may not provide an independent forum for social interaction. To disentangle the impact of different networks, we include various combinations of cohort-based social links to families with free care as explanatory variables. In column 3 of Table 3, we consider both the share of neighbours of the same religion and the same ethnicity with free care. In column 4, we consider both religion-based and occupation-based social networks. Observe that the coefficient on the share of neighbours of the same religion with free care remains negative, reasonably stable, and statistically significant, albeit at the 10 percent level. Meanwhile, the social effect estimates for ethnicity and occupation-based networks become much smaller and are not statistically significant.

# Other Outcomes

In the final analysis, we examine indirect effects on other outcomes. Free care has no direct effect on the number of hospital visits per year, nor are there any indirect effects (Table 4, column 1). The direct effect of free care on pharmacy care visits is negative, consistent with the change in the relative price of the various health seeking options and a shift towards the public sector, but there are no indirect effects (Table 4, column 2). There is no direct or indirect effects on visits to traditional healers (Table 4, column 3). To assess whether there are any welfare implications of the negative social effects on the uptake of primary health care, we examine the effect of religious networks on the health of children and out-of-pocket health care spending. Estimates show that free care had no direct effect and no indirect effect on the haemoglobin level (Table 4, column 4). We next investigate the effect on out-of-pocket health care spending in the four weeks prior to interview. The removal of user fees reduced health care spending by a large amount but again we find no evidence of social effects (Table 4, column 5).

# Discussion

There is growing interest in the indirect effects of policies in developing countries but much of the evidence pertains to cash transfer programmes and subsidies for specific healthcare products. In this paper we study the indirect effects of subsidies for healthcare that are becoming increasingly widespread as efforts are made to reach universal coverage ([World Health Organization, 2010](#_ENREF_26)). To capture the influence of social interactions on the impact of the free care intervention, we exploited data from a randomised experiment, using cohort-based measures of social networks defined by religion, ethnicity and occupation.

Our main results show that children in households given free care increased their utilisation of primary care clinics. There are, however, negative social effects associated with the subsidies. Children in households with greater exposure to neighbours of the same religion with free care are less likely to use primary health care. Religion appears to be the social network that matters; it dominates other social networks defined in terms of ethnicity and occupation. We find no evidence of social effects on child health or healthcare spending, suggesting that the implications for welfare are negligible. In the context of this study, the findings highlight the potential for healthcare subsidies to have unintended consequences.

The evidence presented in the paper is consistent with our reading of the literature on the importance of religion, in particular Christianity, as a social institution in Ghana. The country has a long history of mission Churches, to the extent that Christianity in the southern parts of Ghana “reigns supreme” ([Meyer, 1995](#_ENREF_14)). Alongside the traditional churches, pentecostal or so-called spiritual churches have become popular ([Assimeng, 1986](#_ENREF_5)), especially amongst women who are able to enhance their public status otherwise denied to them ([Soothill, 2007](#_ENREF_21)). Religion in Ghana is integral to an individual’s identity and provides a forum through which individuals of the same religion can regularly and frequently interact – eg. Sunday worship in the case of Christianity.

To explain the findings we discuss the channels through which the negative social effects may have arisen. Theory points to several potential mechanisms at play. First, the findings may be explained by a specific type of informal risk-sharing. In a standard model of informal risk sharing, an increase in the income of some households in a village will increase informal transfers in the form of loans or donations to other socially connected households in the village. A rise in income can be expected to increase utilisation of health services assuming that the income elasticity is positive. But if, instead, transfers take the form of drugs obtained from neighbours who have free care and use public clinics more often, the indirect effects from the intervention will be negative as families turn to self-medication.

Several characteristics of the study setting suggest that drug sharing is at least a potential mechanism. First, there is a need for informal risk-sharing – health shocks are common, formal insurance institutions are missing, and credit constraints are severe. For example, in our data parents reported at baseline that almost 95 percent of children under five were ill in the past year. More objectively, 38 percent of children had anaemia (Hb<10g/dl). Families face credit constraints as shown by the fact that 38 percent of families who had an ill child in the past four weeks but did not go to a health facility report the primary reason as “too expensive or could not afford it”. Second, informal risk-sharing appears widespread, as has been documented in poor rural communities in other countries ([Angelucci & De Giorgi, 2009](#_ENREF_2); [Rosenzweig, 1988a](#_ENREF_19), [1988b](#_ENREF_20); [Townsend, 1995](#_ENREF_23); [Udry, 1994](#_ENREF_24)). When asked how poor households survive in this area, our data show over 70 percent cited assistance (in the form of borrowing or transfers) from family, friends and neighbours as the primary means. Third, the majority of childhood illnesses in the study area are treatable with appropriate drugs and there is a strong demand for private pharmacies ([Ansah, Narh-Bana, Asiamah et al., 2009](#_ENREF_3)) suggesting that drugs rather than the expertise of qualified health professionals are often what patients want.

A second possibility is that households may have learnt via others’ experience that healthcare in the public sector is of a lower quality or worth less than they had previously perceived. Subsidised healthcare encouraged beneficiary households to increase utilisation of primary health services, they shared their experiences with other households in their social network, and this new information deterred these households from using primary care in the public sector. One plausible scenario is that the increase in primary care utilisation led to more overcrowding at facilities and reports of long waiting times that dissuaded others to use services. Although exit interviews and focus group discussions suggest that perceptions of quality in the public sector were high it remains possible that prior beliefs were more optimistic ([Ansah, Narh-Bana, Asiamah et al., 2009](#_ENREF_3)).

When interpreting the findings, we note a number of limitations. First, the advantage of using cohort-based networks is that the boundaries are sharply defined, making measurement straight forward. However, it also means that respondents do not specify their social links themselves when friends and family are arguably the more relevant reference group. For this reason, we expect the social ties between families of the same cohort to be weaker, and our estimates of social effects smaller, than what would be obtained if using information on friends and family to define social links in a village. Second, while we recognise that social effects may have emerged through various channels, the fact that we can only identify the net effect of different behavioural factors does not rule out multiple channels operating simultaneously.

Although we are unable to establish the channel through which the indirect effects emerged, it seems clear that any (positive) influences of social learning must have been weak. It thus seems unlikely that one-off or temporary subsidies for routine health services can permanently shift society to a higher equilibrium level of health care utilisation. There may be no alternative but to continue to subsidise or provide additional incentives for routine health services if take up is to be sustained, as suggested by Kremer and Miguel ([2007](#_ENREF_12)), albeit in the context of de-worming. Unlike de-worming drugs, however, improvements to the quality of services can be made which may trigger positive learning effects as favourable experiences from using services in the public sector trickles to others. This could reverse the sign of the coefficient on the social exposure variable.

Our findings complement other studies in demonstrating the need to consider indirect effects in the evaluation of health policies or programmes. Understanding how such effects operate is critical for informing policy on how to increase the adoption of life-saving health interventions. However, capturing such ripple effects can be challenging. There needs to be a shift in thinking to one that considers indirect effects as a question of interest rather than contamination and a threat to internal validity. More ambitious study are required that use, for example, two-stage randomisation ([Baird, McIntosh, & Ozler, 2011](#_ENREF_6); [Muralidharan & Sundararaman, 2013](#_ENREF_16)) and collect detailed information on the structure of social networks.

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### Table 1. Baseline characteristics of households

|  |  |  |  |
| --- | --- | --- | --- |
|  | Mean | Standard deviation | Observations |
| Panel A: Outcomes at baseline and free care intervention  |
| Assigned to free healthcare | 0.49 | 0.50 | 2149 |
| Any use of formal care in past one month | 0.28 | 0.44 | 2149 |
| Haemoglobin concentration (g/dl) | 10.3 | 1.5 | 2149 |
| Total health care expenditure | 17,335 | 35,641 | 2136 |
|  |  |  |  |
| Panel B: Characteristics at baseline |
| Mother’s education (years) | 5.3 | 4.4 | 2149 |
| Children in household | 1.3 | 0.6 | 2149 |
| Distance from health centre 5 ≥ 10km | 0.20 | 0.40 | 2149 |
| Distance from health centre >10km | 0.16 | 0.36 | 2149 |
| Wealth asset score | 0.002 | 1.8 | 2149 |
| Christian religion | 0.88 | 0.33 | 2149 |
| Muslim religion | 0.06 | 0.24 | 2149 |
| African religion | 0.02 | 0.15 | 2149 |
| Dangme ethnicity | 0.64 | 0.48 | 2149 |
| Ga ethnicity | 0.03 | 0.17 | 2149 |
| Akan ethnicity | 0.07 | 0.25 | 2149 |
| Ewe ethnicity | 0.19 | 0.39 | 2149 |
| Krobo ethnicity | 0.01 | 0.12 | 2149 |
| Northern/Upper ethnicity | 0.05 | 0.21 | 2149 |
|  |  |  |  |
| Panel C: Social networks |
| Number of sample households in same village | 32.4 | 25.2 | 2149 |
| Number of neighbours of same religion with free care  | 12.5 | 11.9 | 2149 |
| Number of neighbours of same religion | 25.3 | 22.8 | 2149 |
| Number of neighbours of other religion | 6.2 | 11.9 | 2149 |
| Proportion of neighbours of same religion with free care | 0.47 | 0.19 | 2149 |
| Proportion of neighbours of same ethnicity with free care | 0.46 | 0.25 | 2149 |
| Proportion of neighbours of same occupation with free care | 0.45 | 0.27 | 2149 |
| Notes: Descriptive statistics are based on the household sample. Outcomes measures at baseline take the mean if households have more than one child. Data on healthcare utilisation are from the baseline household survey but there are no baseline data on the number of health care visits per person each year because the pictorial diaries were only implemented after random assignment and the start of the intervention. The proportion of religious links with free care is coded zero if there are no religious links in the same community. Neighbours are defined as other sampled households residing in the same village. There are 158 villages in the dataset. |

### Table 2. Social effect estimates using religion-based networks on clinic visits

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | Basic  | Village population | Other religions | Number of religious neighbours |
| (1) | (2) | (3) | (4) |
| Free care | 0.33\*\* | 0.33\*\* | 0.33\*\* | 0.33\*\* |
|  | (0.15) | (0.16) | (0.16) | (0.15) |
| Proportion of neighbours of same religion with free care | -0.78\*\* | -0.78\*\* | -0.77\*\* |  |
|  | (0.37) | (0.37) | (0.37) |  |
| Number of sample households in same village |  | -0.001 |  |  |
|  |  | (0.005) |  |  |
| Proportion of neighbours of other religion with free care |  |  | -0.05 |  |
|  |  |  | (0.20) |  |
| Number of neighbours of same religion with free care |  |  |  | -0.07\*\*\* |
|  |  |  |  | (0.02) |
| Mean of dependent variable | 2.7 | 2.7 | 2.7 | 2.7 |
| Number of observations | 1973 | 1973 | 1973 | 1973 |
| Notes: \*\*\* denotes significance at 1%, \*\* at 5%, and \* at 10% level. Standard errors, corrected for clustering at the community level, are reported in parentheses. Regressions are estimated by OLS and include demographics that control for mother’s education, number of children in household, household wealth, number of neighbours in the reference group and dummies for categories of distance from the nearest health centre, religion, and ethnicity. Neighbours are defined as other sampled households residing in the same village.  |

### Table 3. Social effect estimates using other cohort-based networks on clinic visits

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | Ethnicity | Occupation | Religion & ethnicity | Religion & occupation |
| (1) | (2) | (3) | (4) |
| Free care | 0.32\*\* | 0.33\*\* | 0.33\*\* | 0.33\*\* |
|  | (0.15) | (0.15) | (0.16) | (0.16) |
| Proportion of neighbours of same ethnicity with free care | -0.41 |  | -0.12 |  |
|  | (0.28) |  | (0.30) |  |
| Proportion of neighbours of same occupation with free care |  | -0.40\* |  | -0.22 |
|  |  | (0.22) |  | (0.22) |
| Proportion of neighbours of same religion with free care |  |  | -0.70\* | -0.65\* |
|  |  |  | (0.39) | (0.37) |
| Mean of dependent variable | 2.7 | 2.7 | 2.7 | 2.7 |
| Number of observations | 1973 | 1973 | 1973 | 1973 |
| Notes: \*\*\* denotes significance at 1%, \*\* at 5%, and \* at 10% level. Standard errors, corrected for clustering at the community level, are reported in parentheses. Regressions are estimated by OLS and include demographics that control for mother’s education, number of children in household, household wealth, number of neighbours in the reference group and dummies for categories of distance from the nearest health centre, religion, and ethnicity. Neighbours are defined as other sampled households residing in the same village.  |

### Table 4. Social effect estimates using religion-based networks on other outcomes

|  |  |
| --- | --- |
|  | Dependent variable: |
|  | Hospital visits | Pharmacy visits | Traditional healer visits | Haemoglobin level | Out of pocket health expenditure |
| (1) | (2) | (3) | (4) | (4) |
| Free care | -0.021 | -0.37\*\*\* | 0.021 | 0.069 | -2821.7\*\* |
|  | (0.054) | (0.12) | (0.026) | (0.061) | (1105.6) |
| Proportion of neighbours of same religion with free care | 0.021 | 0.005 | 0.10 | -0.29 | -1856.4 |
|  | (0.14) | (0.39) | (0.084) | (0.19) | (2801.0) |
| Mean of dependent variable | 0.46 | 2.9 | 0.12 | 11.0 | 9497 |
| Number of observations | 1973 | 1973 | 1973 | 1972 | 1962 |
| Notes: \*\*\* denotes significance at 1%, \*\* at 5%, and \* at 10% level. Standard errors, corrected for clustering at the community level, are reported in parentheses. Regressions are estimated by OLS and include demographics that control for mother’s education, number of children in household, household wealth, number of neighbours in the reference group and dummies for categories of distance from the nearest health centre, religion, and ethnicity. Neighbours are defined as other sampled households residing in the same village.  |

### Table A1. Baseline characteristics by intervention group

|  |  |  |  |
| --- | --- | --- | --- |
|  | No free care (control) |  | Free care (intervention) |
|  | Mean | Standard deviation |  | Mean | Standard deviation |
| (1) | (2) |  | (3) | (4) |
| Mother’s education (years) | 5.19 | 4.39 |  | 5.34 | 4.32 |
| Children in household | 1.31 | 0.56 |  | 1.32 | 0.56 |
| Distance from health centre < 5km | 0.64 | 0.48 |  | 0.63 | 0.48 |
| Distance from health centre 5 ≥ 10km | 0.20 | 0.4 |  | 0.21 | 0.41 |
| Distance from health centre >10km | 0.16 | 0.36 |  | 0.16 | 0.36 |
| Wealth asset score | 0.021 | 1.83 |  | -0.017 | 1.73 |
| Christian religion | 0.87 | 0.33 |  | 0.88 | 0.32 |
| Muslim religion | 0.067 | 0.25 |  | 0.061 | 0.24 |
| African religion | 0.023 | 0.15 |  | 0.021 | 0.14 |
| Dangme ethnicity | 0.037 | 0.19 |  | 0.036 | 0.19 |
| Ga ethnicity | 0.62 | 0.48 |  | 0.66 | 0.47 |
| Akan ethnicity | 0.036 | 0.19 |  | 0.027 | 0.16 |
| Ewe ethnicity | 0.059 | 0.24 |  | 0.074 | 0.26 |
| Krobo ethnicity | 0.21 | 0.41 |  | 0.17 | 0.38 |
| Northern/Upper ethnicity | 0.015 | 0.12 |  | 0.013 | 0.11 |
| Krobo ethnicity | 0.049 | 0.22 |  | 0.041 | 0.20 |
| Number of neighbours of same religion with free care  | 12.3 | 11.8 |  | 12.6 | 12.0 |
| Number of neighbours of same religion | 25.1 | 22.4 |  | 25.5 | 23.1 |
| Proportion of neighbours of same religion with free care | 0.46 | 0.20 |  | 0.47 | 0.18 |
| Proportion of neighbours of same ethnicity with free care | 0.44 | 0.25 |  | 0.47 | 0.24 |
| Proportion of neighbours of same occupation with free care | 0.43 | 0.27 |  | 0.45 | 0.27 |
| Notes: Neighbours are defined as other sampled households residing in the same village. |

### Table A2. Poisson estimates using religion-based networks on clinic visits

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | Basic  | Village population | Other religions | Number of religious neighbours |
| (1) | (2) | (3) | (4) |
| Free care | 1.13\*\* | 1.13\*\* | 1.13\*\* | 1.13\*\* |
|  | (0.065) | (0.065) | (0.065) | (0.065) |
| Proportion of neighbours of same religion with free care | 0.74\*\* | 0.74\*\* | 0.74\*\* |  |
|  | (0.10) | (0.11) | (0.11) |  |
| Number of sample households in same village |  | 1.00 |  |  |
|  |  | (0.0021) |  |  |
| Proportion of neighbours of other religion with free care |  |  | 0.98 |  |
|  |  |  | (0.073) |  |
| Number of neighbours of same religion with free care |  |  |  | 0.97\*\*\* |
|  |  |  |  | (0.0084) |
| Mean of dependent variable | 2.7 | 2.7 | 2.7 | 2.7 |
| Number of observations | 1973 | 1973 | 1973 | 1973 |
| Notes: \*\*\* denotes significance at 1%, \*\* at 5%, and \* at 10% level. Standard errors, corrected for clustering at the community level, are reported in parentheses. Poisson regressions are estimated and include demographics that control for mother’s education, number of children in household, household wealth, number of neighbours in the reference group and dummies for categories of distance from the nearest health centre, religion, and ethnicity. Relative risk estimates are reported. Neighbours are defined as other sampled households residing in the same village.  |