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Gender Differences in Children's Antibiotic Use and Adherence

Christine Blandhol
Anja Sautmann

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Abstract

Using in-home health records for 1,763 children in Mali, this paper examines gender differences in the uptake and duration of treatment with antibiotics. The detailed data provide a window into parents' day-to-day decisions while accounting for symptoms. There are no gender differences in starting treatment, but boys are over 10 percentage points more likely to complete a course of antibiotics than girls. This difference is driven by families with an educated household head. An explanation may be that (male) household heads are less involved in caring for girls, so that benefits from education that lead to better care accrue overproportionally to boys.

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Christine Blandhol† and Anja Sautmann‡

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†Princeton University and Statistics Norway: blandhol@princeton.edu.

‡Development Economics Research Group, World Bank, asautmann@worldbank.org.

1 Introduction

There are more than 1.5 million "missing women" in Sub-Saharan Africa each year, and major contributors to the gender disparity in mortality are curable diseases like malaria and diarrhea (Anderson and Ray, 2010, 2018). In fact, Sub-Saharan Africa is the only region where excess female mortality has increased between 1990 and 2008 (World Bank, 2011). Yet there is comparatively little direct evidence from the region that men and women are receiving systematically different levels of healthcare. By comparison, for South Asia, where mortality differences are similar or even smaller, there is extensive evidence of differential treatment in health care for boys and girls. Parents seek care more often and earlier for sons and provide them with more nutritious diets than daughters (Borooah, 2004; Pandey et al., 2002), boys are breastfed longer and more likely to get vaccinated, and male fetuses receive more pre-natal care (Jayachandran and Kuziemko, 2011; Oster, 2009; Bharadwaj and Lakdawala, 2013).

In this paper, we examine gender differences in the use of antibiotics for children under five years in poor households in Mali. Antibiotics provide a case study for care-seeking behavior and treatment adherence, two important margins for childhood health outcomes. On the one hand, there is a deficit in antibiotics use in Sub-Saharan Africa, where more than 490,000 children annually die from pneumonia alone – an illness often cured by an antibiotic (UNICEF, 2016; World Health Organization, 2005). On the other hand, treatment adherence is especially important for drugs that are at risk of being rendered ineffective through the rise of resistant pathogens and bacteria, as many common antibiotics are (World Health Organization, 2012).

Our analysis exploits unique daily health records collected at home from a sample of 1,763 children in Mali. The data were collected as part of a randomized controlled trial that tested the effect of two common health policies – subsidies and healthworker visits – on the decision to visit a formal care provider conditional on health status, and ultimately health outcomes (Sautmann et al., 2017; Dean et al., 2017). However, the original data collection also included information on the prescribed treatments and medications taken, and these data give us information on antibiotics type, price, and length of treatment. The daily symptom records allow us to control for the types of

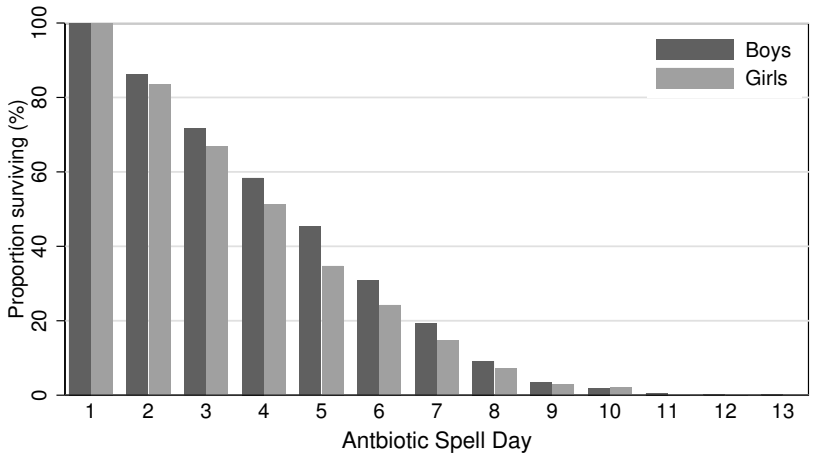
symptoms experienced prior to treatment start, as well as the persistence of symptoms after (which may act as a motivator and reminder to complete the treatment; Cohen et al., 2014). Symptoms are an important determinant of acute healthcare decisions that are often not observed. In addition, we derive the recommended treatment duration for each antibiotics course we observe from W.H.O. guidelines (World Health Organization, 2001).

Our analysis confirms that observed symptoms are an important factor in the decision to begin treatment with an antibiotic. The child's gender, on the other hand, plays no significant role for whether an antibiotic is started; both unconditional and conditional on symptoms, the gender effect on treatment uptake in our sample is small and not significantly different from zero.

For treatment duration, by contrast, we find that gender is an important determinant. Figure 1 shows the survival distribution of the number of days boys and girls are given antibiotic treatments. In this raw data, girls are much more likely to take the antibiotic course for shorter periods. Since antibiotics use is a two-step decision, first the choice of starting treatment and then treatment length, this unconditional difference is suggestive but not conclusive. We need to account for selection at the second stage, because children who actually start treatment can otherwise not be compared directly, and this could also affect observed gender differences. For example, if boys are on average healthier than girls when starting treatment and healthier children take the antibiotic for shorter periods, this will mask gender differences in adherence. Of course, both the absence of a gender effect on treatment uptake, the first stage, and our ability to control for symptoms in detail alleviate some of these selection concerns. Nonetheless, we use Heckman's (1976) procedure to examine and correct for potential selection on unobservables, exploiting exogenous variation in access to treatment from the randomized trial for which the data were originally collected.

As one would expect, one important source of selection is observed illness severity. OLS and Heckman-corrected estimates of binary treatment completion and the daily probability of ending treatment imply that selection into treatment is significant when we control only for demographics: children who are more likely to begin treatment also take the antibiotic for longer. However, controlling for symptoms and the type of illness substantially reduces selection on unobservables,

Figure 1: Survival distribution of antibiotic treatment lengths.



Note: Sample includes 704 uninterrupted treatment spells from antibiotics prescribed at community clinics, private doctors, hospitals, or pharmacies. The figure shows the proportion of spells that have not ended for each day after the start of the treatment, separated into girls and boys.

a finding that suggests that our data successfully capture many of the determinants for starting a medication that also affect completion of the treatment course.

Both with and without illness controls and the Heckman correction, the estimation results show that girls are 10 to 14 percentage points less likely to complete an antibiotics course, and have an almost five percentage point higher daily probability of ending treatment before the recommended treatment duration is reached. A Lasso approach to selecting covariates as well as alternative probit specifications deliver very similar results. This is on top of already low completion rates for boys of around 50 percent.

The stark difference in treatment completion reveals a new channel for gender health disparities that has not been previously explored in depth. It is a surprising channel, since an explicit preference for boys would suggest that parents are less willing to spend money and time on seeking care for daughters, yet we see no gender difference in care-seeking. Instead, we see a large discrepancy in treatment length, even though giving a pill for an extra day is by comparison essentially cost-free. In order to better understand this result, we explore two potential mechanisms. First, we

examine if parents do discriminate on costs, but in less visible ways, namely by purchasing only partial treatment for girls.[1] However, we find no evidence of this phenomenon. Second, we explore if treatment adherence might be explained by subtle differences in how gender determines who is involved in the care of the child at home. We build on the fact that the household head's education plays an important role in ensuring high adherence. This is likely based on the ability to read and understand instructions, understand the pathways of antibiotics effectiveness and antibiotics resistance, and so on; other factors correlated with education may also contribute (for example, households with an educated household head are likely to have a higher income overall). We then ask whether the education of the household head – given Malian traditional family structures, this is in almost all cases the husband – differentially affects treatment adherence for boys and girls. Indeed, we find that the *entire* effect of the child's gender on the one hand, and the household head's education level on the other, is explained by higher adherence rates of boys: the sons of more educated household heads are 16 to 17 percentage points more likely to complete treatment than either girls or sons of uneducated heads of family. An explanation may be that household heads are less involved in the care of girls, so that benefits from education and literacy that lead to better home care accrue overproportionally to boys (note that many individuals in this sample have no schooling and household head's median education level is less than four years).

Our results have several important implications. First, we uncover some of the determinants of antibiotics adherence, and show that adherence rates are overall worryingly low. Poor adherence contributes to fostering resistant pathogens. Moreover, non-adherence for antibiotics can lead to illness relapse, and if girls also have lower adherence rates for malaria treatment, vaccination courses, and other medications, this may contribute to gender differences in an array of health outcomes.

Second, the fact that gender disparities manifest themselves in treatment duration but not in treatment start suggests subconscious favoritism over an explicit son preference. Such small differences in parents' treatment of their children in the privacy of the home may well be pervasive,

[1] Partial treatment could happen if pharmacists or doctors allow parents to buy medications in "installments", and parents fail to return to buy the remaining portion.

but are usually very difficult to detect. Our detailed data give us a unique window into the gender bias in children's day-to-day treatment. While individual instances may be inconsequential, they can in accumulation lead to significant differences in boys' and girls' outcomes. This more subtle form of discrimination is not only harder to detect, but might also be harder to address.

Lastly, our findings suggest that gender differentials in health outcomes could be at least in part related to educational differences between parents (with the caveat that our sample has very low rates of education overall). As part of the push for the Millennium Development Goals, the last two decades saw significant improvements in child and maternal mortality worldwide. Child mortality rates are highest, and efforts to reduce them consequently greatest, in countries with overall low levels of human development, where educational gender disparities also tend to be high. Through this channel, recent successes in fighting child mortality might overproportionally benefit boys and thus contribute to the contemporaneous increase in excess female mortality in Sub-Saharan Africa, at least temporarily.

Section 2 discusses the determinants of antibiotics use and shows how differences in treatment start can lead to biased estimates of adherence behavior if not corrected. Section 3 presents a description of the demographics of our sample, the observed antibiotics treatments, and the effect of the randomized intervention that we use as a source of exogenous variation in treatment uptake for the Heckman procedure. Section 4 estimates the determinants of starting a course of antibiotics and the duration of treatment, and uncovers the role of the household head's education level for the gender bias in antibiotics adherence. Section 5 concludes.

2 Conceptual Framework

Treatment adherence to antibiotics depends on a two-step decision process. First, the child needs to be started on treatment – a doctor or pharmacist must recommend the use of an antibiotic and prescribe or sell it to the parent, the parents must buy it – and second, the parent must decide day to day whether to give the drug to the child, based on doctor recommendations and potentially their

own assessment of the costs and benefits. In this section, we discuss some of the factors that go into both decisions and lay out our estimation approach, including the use of a Heckman correction to control for selection into treatment on unobservables.

Going backwards in the decision process, we start by modeling treatment adherence. There are two main reasons why the length of treatment may vary. First, doctors may prescribe different amounts of the drug, and second, parents may adhere to this prescription differentially. Arguably, trained doctors and pharmacist will usually prescribe at least the minimum dose of a medication, which is reflected in the standard packaging size of a licensed drug and in official treatment guidelines (WHO guidelines for treatment durations with antibiotics are not gender-specific). However, anecdotally, a doctor or pharmacist at a community clinic may also allow a cash-strapped parent to buy a small amount of the drug right away, with the idea that they will return to get the full treatment course later (which the parent may or may not do). In this manner, doctors' and parents' choices may interact in leading to differential outcomes for boys and girls.

This leads us to the second important margin of non-compliance, the degree to which parents follow medical recommendations, found either on the drug packaging or given by the provider. It is likely that the hurdle to "over-compliance", i.e. taking the drug for longer than recommended, is fairly high, because of packaging sizes.[2] On the other hand, parents can easily end the treatment early (note that the children are five years or younger, and so we assume that any decisions about treatment and adherence are made by the parents).

We expect parents to fail to adhere to the recommended treatment length if the costs of completion are greater than the benefits. The benefits of completing a course of antibiotics depend substantially on the (perceived) risk of not getting better or relapsing when ending treatment prematurely, and the direct disutility associated with the illness – pain and discomfort to the child, fussiness, disrupted sleep, etc. Cohen et al. (2014) also argue that the belief in the efficacy of the medication depends on the illness progression after starting treatment. Therefore, the decision to continue treatment will depend on observed or assumed illness type, illness severity, and

[2]It may be, however, that the doctor gives spare tablets e.g. for children who may vomit up a dose, or that the antibiotic requires more than one dose a day and the parent stretches the drug out over a longer period.

the duration of symptoms. The – perceived or real – risk of not curing the illness may be modulated by factors such as the child's age, because younger children are more susceptible to adverse consequences of untreated illness.

Adherence costs described in the literature include the opportunity cost of using a drug now vs. saving pills for future use, possible real or suspected side effects, and the mental cost of remembering (Cohen et al., 2014). These costs may also be correlated with illness severity; for example, remembering to continue the treatment may be harder if the child has no symptoms anymore. The costs are again likely to vary with other factors. For example, households with fewer resources and more than one child might be more inclined to save pills in order to use the medication another time or for another child. We would also expect that the caretaker's education affects their ability to read and follow package instructions, knowledge about the efficacy of antibiotics, and the importance of following treatment directions.

Lastly, families might also treat children differently despite similar short-term costs for them and similar risks associated with the illness for the child. This may partly be related to (perceived) long-term returns to investing in one child over another. Parents with adopted or fostered children might either prioritize their biological children, or conversely pay more attention to their foster charges, especially if they are compensated for their care by the biological parents (Hamilton et al., 2007). Under severe resource constraints, a family might prioritize their first-born child: several studies have documented birth order effects in areas such as education (Black et al., 2005) and nutrition (Behrman, 1988). Finally, there may be differential treatment by gender: parents may care more about restoring a boy's health, based on pure preferences or because of a boy's higher earning potential, but they might also pay more attention to a girl's discomfort, for example because boys are trained to complain less (Schechter et al., 2003).

We estimate antibiotics treatment length with a linear probability model based on a latent utility model. We estimate two specifications, one where the binary decision is to treat the child for the recommended length according to W.H.O. treatment recommendations (completion model), and one in which the decision is to end treatment on a given day after starting the antibiotics course

(failure model). According to W.H.O. guidelines, most antibiotics in our sample should be taken for a minimum of five days. There are some exceptions for certain antibiotic types and symptoms, described in more detail in Section 3.3. For the completion model, we use two alternative definitions of treatment completion; the first, denoted Model 1, uses the minimal recommended length for each antibiotic type, the second, Model 2, uniformly counts an antibiotic course as completed on day five. The rationale for Model 2 is that doctors, pharmacists, or even parents may choose different antibiotic types for girls and boys.

We denote the net benefit from completing the prescribed treatment by y^*, the characteristics of the illness spell by W, and a vector of observable, child, household, and parent characteristics by X, breaking out the child's gender G. The other child characteristics include whether the child is the first-born, their age, whether they are adopted or fostered, and an indicator for whether the child is an only child. Household characteristics include total assets (log) and whether the household head and mother have any schooling. To control for observable illness characteristics we use information on symptoms that occurred before starting treatment. To avoid concerns about specification searching, we also show results using covariates selected using double lasso from a wider set of candidate control variables (Belloni et al., 2014).[3]

Ongoing symptomaticity is an important determinant for continuing treatment, both because it is a reminder to the parents, and because it is an indicator of the severity of illness. In order to capture symptom persistence, we define indicators S for whether the child presented any symptoms, or the symptom that was named as the reason for care, after treatment start (in Section 3.2, we provide a more in depth description of these variables).

[3]We define the set of candidate controls from the baseline survey as: dummies for experiment treatment groups, age of child, birth order, an indicator for whether the child is adopted, number of siblings, household income and assets, household size, the age, literacy, and education of the mother and household head, distance to the clinic, and an indicator for whether the household head has a salary. In addition to information about demographics from the baseline survey, we also include dummies for the presence of each symptom type in the pre-care days and indicators for whether any symptoms or the named symptom were present for three and five days after the start of the antibiotic course. We also dummy out missing values, recode them to zero. We include squared terms for all of the aforementioned controls.

The latent net utility of completing treatment for a given spell i and child j is then

$$y^*_{ij} = \alpha_1 + \beta_1 G_j + \gamma_1 X_j + \delta_1 W_{ij} + \eta_1 S_{ij} + \varepsilon_{ij}. \tag{1}$$

We observe if the child takes the drug for the recommended length and therefore completes treatment ($y^*_{ij} > 0$). One concern of the completion model in including variables S is that they may be endogenous to treatment adherence, especially if symptomaticity is also correlated with gender. This is partially alleviated by estimating the failure model, which only includes spell days conditional on not having ended treatment yet. Thus, on any given day we are only comparing spells for which treatment adherence is invariant and uninterrupted during the spell.

The latent net utility for child j continuing treatment on day t of spell i is

$$y^*_{ijt} = \alpha_2^{pre} + \alpha_2^{post} + \beta_2 G_j + \gamma_2 X_j + \delta_2 W_{ijt} + \eta_2 S_{ijt} + \varepsilon_{ijt} \tag{2}$$

The set of days t includes all days t in spells of length greater than or equal to t in which failure (non-adherence) has not yet occurred. We observe if the antibiotic course is stopped on a given day ($y^*_{ijt} \leq 0$, failure model). In an ideal world, failure rates would be 0 prior to antibiotics course completion, and 100% on the day after the course is complete (with no observations past that day). More generally, we would expect failure to be discretely lower on days prior to completion of the antibiotic course than afterwards. We therefore allow the constant to differ on pre-completion and post-completion days (dummies α_2^{pre} and α_2^{post}). Moreover, since we are specifically interested whether there are gender differences in completing the spell, in some specifications, we interact the gender indicator with the pre- and post-completion dummies to test whether there are gender differences in failure rates prior to completion.

The choice to seek treatment will be affected by similar factors as the choice to adhere. Illness characteristics before treatment start W, as well as household characteristics like wealth and education X, will play a role. In addition, we assume that the monetary cost of the doctor consultation and treatment substantially affects the treatment choice. Ex ante, parents form expectations about the

cost depending on whether their child receives the randomly assigned healthcare subsidy, denoted by indicator variable FC (for "free care"). Note that the doctor's decision to prescribe treatment may again modulate parents' choices. The antibiotic will only be prescribed if the doctor believes that the child has an illness that is treatable with antibiotics. In turn, conditional on the doctor's recommendation to treat, parents may differentially choose to follow this recommendation. We collapse the chain of decisions of parents seeing a doctor, the provider prescribing treatment, and the parents purchasing the drug, and model the final outcome of these choices as the net benefit v^* from starting treatment:

$$v_{ij}^* = \alpha_3 + \beta_3 G_j + \gamma_3 X_j + \delta_3 W_{ij} + \kappa FC_j + u_{ij}, \tag{3}$$

where u_{ij} is an error term describing unobservable factors entering the treatment choice. We observe treatment start if $v_{ij}^* > 0$.

Our two-stage model of the decision to start treatment and then adhere to it highlights the potential for selection into treatment at the second stage. Different choices for boys and girls at the treatment stage can lead to observed gender differences in completion rates without any difference in adherence behavior. For example, although Anderson and Ray (2010) note that there is little evidence that there is differential incidence and severity of infectious diseases for boys and girls, Wamani et al. (2007) find in a meta-analysis that boys are generally more often stunted in Sub-Saharan Africa. Stunting is associated with increased exposure to illness during childhood (Black et al., 2008). This may indicate that boys and girls differ in how often they contract illnesses or how severe the associated symptoms are. More prominent symptoms can motivate parents to seek treatment for the child in order to reduce their discomfort, but also serve as a reminder to complete the treatment.

Most of the variables described earlier that we believe enter the adherence decision are observed in our data; importantly, our data allow us to control for an array of illness indicators. Nonetheless, there may be factors that affect treatment adherence that we do not observe and that

matter for both decisions, so that ε_{ij} (or ε_{ijt}) and u_{ij} are correlated. Through the selection into treatment, ε_{ij} may be in addition correlated with observable characteristics that affect the choice to treat, including potentially gender. An example are differential perceptions about side effects or efficacy of antibiotics. We would expect these beliefs to both affect whether parents purchase an antibiotic treatment and whether they adhere to treatment recommendations.

If such unobserved factors are important, *and* the unobservables are correlated with gender, it could bias our estimates of the gender effect β_1 or β_2. For example, if boys are less resilient than girls, this may introduce a bias in the treatment adherence of girls relative to boys, because an overall stronger and more robust child may be taken to a doctor less often and also receive a drug for a shorter period.

As we show below, there is no *a priori* gender difference in treatment start, which makes selection on unobservables that are correlated with gender less of a concern. Nonetheless, we carry out a correction using the Heckman method, both because it corresponds with our original analysis plan, and because it allows us to gauge the importance of unobservables in the treatment and adherence decisions. Moreover, although we are limited by the small number of families where two siblings of opposite gender start a course of antibiotics, the gender difference in adherence also seems to persist within families (Appendix Table A.7).

Assuming a normally distributed error term, we use the Heckman procedure in the first decision stage to construct the inverse mills ratio λ and include it in the second stage. This gives us an estimate of the correlation of the error terms in the selection and adherence utilities $corr(u, \varepsilon) = \rho$ in equations (1) to (3). If $\rho = 0$, there is no selection on unobservables and an uncorrected estimate of equation (1) or (2) yields similar results as the Heckman corrected estimate. If $\rho > 0$, then children who are more likely to start a course of antibiotics are also more likely to complete or continue the treatment. On the other hand, if $\rho < 0$, then children who start treatment will have *lower* completion and higher failure rates.

Bushway et al. (2007) highlight the importance of including an instrumental variable in the selection equation that is excluded from the second stage, that is, the variable only affects adherence

through its effect on the probability of starting an antibiotic course.[4] In our case, the randomized price subsidy implemented during the survey period provides exogenous variation in treatment uptake. Researchers have previously speculated that price paid may directly affect utilization, for example due to sunk cost effects or learning about quality. However, Cohen and Dupas (2010) and Ashraf et al. (2010) do not find evidence of a sunk cost effect for mosquito bednets or water disinfection products.[5] The perceived quality of treatment should also not depend on who pays for it, the NGO or the patient. Lastly, since the price subsidy is by design randomized within the population of interest, its assignment is not correlated with any of the factors that influence adherence. In Appendix sections A.1-A.3, we show that the randomization was successful, by providing details on the balance of the sample. Table A.3 also shows that the subsidy did not affect the overall number of illness spells reported, even though it significantly increased treatment rates.[6] For these reasons, we assume that conditional on an illness spell occurring, the price subsidy only affects treatment duration through changing the probability of seeking care, and therefore satisfies the exclusion restriction.

3 Demographics, Illness Incidence, and Antibiotics Use

In this section we provide details about the study design and sample population and the health diary data we use. We show that boys and girls do not differ significantly *a priori* according to observable demographic and household characteristics or illness incidence (all possible sources of selection). We also provide a description of antibiotics treatment rates and the WHO recommendations for each type of antibiotic. Lastly, we confirm that the price subsidy successfully decreased treatment costs and increased treatment rates.

[4] While the model is formally identified without a valid exclusion restriction, the inverse Mills ratio is close to linear in certain parts of its range, so if there are no variables that only enter the selection equation, there is a risk of collinearity of λ with the other explanatory variables.

[5] Note, however, that there is some tentative evidence of sunk cost effects in education (Hidalgo et al. (2013); Ketel et al. (2016)).

[6] Since this was the first rainy season where the subsidy was available, and the share of the treated in the overall population was small, the increase in healthcare use did not reduce illness incidence.

We use data collected during a baseline and endline survey conducted in late 2012 and 2013 for a randomized control trial of price subsidies and community health worker visits in the outskirts of Bamako, Mali. The interventions were rolled out in early 2013. In order to be eligible for the program, families had to pass a proxy-means test for poverty and had to have children under 5 years old. Detailed information on the design of the randomized trial and the sampling frame can be found in Sautmann et al. (2017).

The Malian health care system rests in large part on public so-called community health clinics, which are mainly funded by user fees, but also supported by private NGOs and the government. In collaboration with the local NGO Mali Health, children in the "free care" (FC) treatment group received free consultations from doctors at the local clinic, as well as cost-free treatment and medication for any of the five main causes of child mortality (diarrhea, malaria, vaccine-preventable diseases, respiratory infections, and malnutrition) at the clinic or from the pharmacy attached to the clinic. The treatment groups and the control group of the randomized trial live in Sikoro, which is served by two clinics that collaborate with Mali Health. Mali Health conducts quality checks on their partner clinics based on treatment guidelines that standardize the treatment children receive for common illnesses. This means that all children in the sample receive similar care.

1,732 children were surveyed in 2012 and enrolled in the program in 2013. Of those children, 1,567 were present at the follow-up survey in 2013, corresponding to an attrition rate of 9.5%. Another 201 children were added after the baseline survey (typically newly born children or children of a mother in the program who had been previously absent; no children were added whose household and mother were not included in the baseline survey). In total, 1,768 children from 1,003 households and 645 compounds were included in the final sample. For five children, gender was not recorded and we omit them from the analysis here. Treatment was randomized at the compound level.[7]

[7] A household is defined as all the people who report the same household head. More than one household may live in the same compound. Compounds are a cluster of dwellings that share some facilities, including usually a common courtyard and any sanitation.

3.1 Demographic Characteristics

Demographics were collected at baseline and, where feasible (for immutable characteristics such as gender or age), missing data was corrected in the follow-up survey. Table 1 shows an overview of the child, household, and parent characteristics. The average age of children in the study is three years, about 20% are the firstborn to their mother, and 6% are adopted or fostered, a common practice in Mali. Each child has, on average, about three siblings. A household is defined as all people who report a common household head. The average self-reported value of households' total assets is $6,564. The average income is $67 per week. In our sample, 46% of mothers report some education. Household heads (who are typically male) have higher literacy and education rates than mothers; more than 64% report some education. 50% of our sample receive the randomized healthcare subsidy.

As a first step, we examine pre-existing differences in observable demographic characteristics for boys and girls. Column 4 of Table 1 reports differences in group means of girls and boys from a regression of each of the variables on a male dummy. Overall, all of the gender differences are small and insignificant. In a study of Nigerian women, Milazzo (2014) found that families were less likely to foster a girl if they already had a daughter, while the same did not hold for sons. In our data, however, we find no evidence of any differential rate of adoption or fostering of girls and boys. Moreover, in contrast with evidence from India (Jensen, 2003) and Nigeria (Milazzo, 2014), we find no significant gender difference in the number of siblings each child has, nor in the financial situation of households. Therefore, differences in parents' fertility behavior based on the gender of the first child can be at most very small in our data. In the last row of Table 1, column 4, we also show the difference in the proportion of girls and boys eligible for free care. The difference is small (0.03) and not significant, suggesting that the subsidy was successfully balanced on gender.[8]

[8] In Appendix A.2 we provide additional information about the balance of the sample by treatment group on the covariates included in the analysis.

Table 1: Comparison of child, household, mother, and household head characteristics.

	(1)	(2)	(3)	(4)	(5)
				Boys - Girls	
	Mean	SD	N	Difference	SE
Child					
Male	0.52	(0.50)	1,763		
Age (years)	3.10	(1.60)	1,763	-0.11	(0.07)
First-born	0.19	(0.40)	1,763	-0.01	(0.02)
Adopted or fostered	0.06	(0.24)	1,763	0.00	(0.01)
No. of siblings	2.94	(2.12)	1,743	0.03	(0.11)
Household					
Assets (USD)	6,564	(13,965)	1,685	-360	(686)
Weekly income (USD)	67	(83)	1,615	-2	(5)
Household size	6.8	(3.5)	1,728	-0.0	(0.2)
Mother educated	0.46	(0.50)	1,743	-0.03	(0.02)
Household head educated	0.64	(0.48)	1,753	-0.01	(0.02)
Household head has salary	0.11	(0.32)	1,732	0.01	(0.02)
Treatment group					
Free care	0.50	(0.50)	1,763	0.03	(0.03)

Notes: Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All eligible children in baseline households who were present in 2013 are included. This comprises children who were born to eligible households (with at least one eligible child in 2012) after the baseline survey. Column 4 present differences in group means of boys and girls. Standard errors from a regression of the variable on a male dummy are clustered at the compound level and shown in column 5.

3.2 Health Diary Information

The core element of the data collection is a detailed health calendar for each child, containing daily information on any symptoms the child exhibited, all consultations that occurred with respect to the child's health, and all medications taken. Health calendar data was collected on a weekly basis over the course of nine weeks in 2013 from the child's primary caretaker (usually the mother). Caretakers were given pictorial diaries and asked to mark off any symptoms and health-related events on the day they occurred. The surveyor reconstructed the child's health history with them during the next visit. To reduce Hawthorne effects, the surveyors were carefully instructed never to comment on the child's health or express judgement, and to emphasize that they had no medical qualification.

The caretaker was prompted about a set of symptoms derived from checklists in the World Health Organization Integrated Management of Childhood Illness (IMCI) handbook (World Health Organization, 2014) and selected because they are relatively easy to spot: convulsions or spasms, lethargic or unconscious, unable to drink, vomiting everything, coughing, difficulty breathing, more than three loose stools, blood in stool, sunken eyes, and unusually hot (skin). In addition, five additional symptom groups were created from free-form survey answers about other symptoms experienced: rash or spots or itch, head, neck, or eye ache, cold symptoms (runny nose), wound or injury or burn, and earache.

It can be subjective or difficult to know what *illness* a child has. Moreover, the mother's perception about the health status of the child might be different for boys and girls. The survey questions were therefore designed to use neutrally described and standardized symptoms observations, and the enumerators were carefully trained not to connect symptoms with specific illnesses (e.g. hot skin with malaria) or to comment on the severity, risk level, or meaning of a symptom (e.g. diarrhea as a factor in malnutrition).

Table 2 provides an overview of illness incidence during the survey period (more detail on the data collection and the symptoms and spells recorded can also be found in Sautmann et al., 2017). In total, we observe about 60 days per child. Contiguous days of symptoms were grouped

in to illness spells. The incidence of illness was significant: over the survey period we observe 4,200 illness spells in total that last on average over seven days.[9] Due to the nature of the data, some of the illness spells may be censored. Among theillness spells, 386have the potential to be left-censored (e.g., because they started on the day after a missing data point), while 18 illness spells have the potential to be right-censored, meaning that the record of the spell ended with a day with missing data (e.g., at the end of the survey). We restrict the analysis to uncensored spells in order to ensure that we observe all pre-care days and whether an antibiotic treatment was started. We also exclude the small number of spells with missing baseline child, household, and illness covariates. Our final analysis sample consists of 3,530 illness spells. The average child had more than two illness spells over the time period and presented symptoms for over 18 of the observed days (more than 30%). Most commonly, children exhibited cold symptoms: on average, 19% of the total observed days for each child. In addition, coughing (12%) and unusually hot (9%) were commonly reported symptoms.

Gender differences in illness incidence could be an important source of bias in gender-specific treatment adherence. However, column 3 shows that there are no obvious differences in how often boys and girls become ill. Overall, the number of illness spells per child is not statistically different for boys and girls. The length of the average illness spell for girls and boys is also similar. Similarly, there is no gender difference in the incidence of specific symptoms, with the exception of "difficulty breathing", a symptom which boys, on average, present on a higher proportion of the total observed days than girls, significant at the 5% level. Nonetheless, in our analysis, we control for the presence of these observed symptoms. Note also that there may be differences in incidence that are unobservable in our data. We cannot rule out, for example, that there are gender differences in the severity of a given symptom. This is another motivation for using the Heckman procedure to examine selection on unobservables.

[9]The study was conducted during the rain season, when the incidence of illnesses such as malaria, diarrhea, and respiratory infections is higher than during the rest of the year. The average spell length excludes censored illness spells.

Table 2: Illness incidence and type.

	(1)	(2)	(3)	(4)
			Boys - Girls	
	Mean	SD	Difference	SE
Panel A. Overall incidence				
Average spell length (days)	7.17	(7.52)	-0.15	(0.29)
Illness spells per child	2.64	(1.43)	-0.03	(0.07)
Observed days per child	59.8	(9.7)	-0.04	(0.44)
Total days ill per child	18.5	(15.8)	0.32	(0.77)
Panel B. Proportion of all observed days				
Any symptom present	0.310	(0.261)	0.008	(0.013)
Convulsions, spasms	0.001	(0.016)	-0.001	(0.001)
Lethargic or unconscious	0.015	(0.057)	0.001	(0.002)
Unable to drink	0.004	(0.025)	-0.000	(0.001)
Vomiting everything	0.013	(0.033)	0.001	(0.002)
Coughing	0.119	(0.173)	-0.004	(0.009)
Difficulty breathing	0.017	(0.056)	0.006**	(0.003)
> 3 loose stools	0.027	(0.074)	0.004	(0.004)
Blood in stool	0.002	(0.026)	0.001	(0.001)
Sunken eyes	0.007	(0.030)	0.001	(0.001)
Unusually hot	0.092	(0.131)	0.001	(0.006)
Other	0.007	(0.033)	-0.002	(0.002)
Wound, injury, or burn	0.011	(0.046)	0.001	(0.002)
Rash, spots or itch	0.009	(0.045)	-0.001	(0.002)
Head, neck or eye ache	0.004	(0.024)	0.002	(0.001)
Cold symptoms	0.187	(0.227)	0.017	(0.011)
Stomach ache	0.003	(0.019)	-0.000	(0.001)
Ear ache	0.003	(0.022)	0.001	(0.001)

Notes: Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1. Average spell length does not include censored spells. Symptom incidence is the number of days the symptom was observed as a fraction of the total number of observed days for each child. Column 3 shows the difference in means between boys and girls. Standard errors from a regression of the variable on a male dummy are clustered at the compound level and shown in column 4.

Table 3: Antibiotic types in the sample, recommended treatment length, and frequency.

Drug	Use	Treatment length	Exceptions	N	Percent
Amoxicillin	Tonsillitis, bronchitis, pneumonia, infections of ear, nose, throat or skin, UTIs, tooth abscess or infection; penicillin tablet.	5 days or more	3 days for tooth infections only.	179	25%
Ampicillin	Mastoiditis, chronic typhoid or paratyphoid fever, meningitis, other infections, e.g heart or gallbladder; penicillin injection.	7 days or more		2	0.3%
Ceftriaxone	Mastoiditis, pneumonia, salmonella infections, meningitis, croup, lyme disease; intramuscular injection.	5 days or more for most uses	4-6 days in some cases.	69	9.7%
Co-Trimoxazole	Otitis (ear), bronchitis, pneumonia, typhoid and para-typhoid fever, salmonella, UTI, skin and other infections; oral administration or intravenous in severe cases.	5 days or more	3 days for typhoid and paratyphoid fever, enteritis due to salmonella or E-coli	234	33%
Erythromycin	Respiratory tract infections, pneumonia, mouth and tooth infections; tablet or powder.	5 days or more		8	1.1%
Gentamicin	Pneumonia, meningitis, heart and other infections; intramuscular injection.	7 days or more	5-10 days for soft tissue injuries.	22	3.1%
Metronidazole	Pneumonia, tooth and mouth infections/abscesses, brain abscess, amoebiasis and giardiasis and other infections; oral.	5 days or more	3 days for giardiasis.	116	16.3%
Other or unspecified				82	11.5%
Total				712	100%

Source: Drug information: World Health Organization. *Notes:* Number and percentage of each antibiotic as reported in our sample. "Other" contain antibiotics given infrequently, like Ciprofloxacin, often obsolete due to widespread resistance. "Unspecified" drugs were reported as antibiotics but without a specific brand or generic name.

3.3 Antibiotic Treatments

At each surveyor visit, mothers were asked if they discussed their child's health with anyone in the preceding week, and the surveyors recorded details of the visit as well as treatments received and costs incurred. In addition, they were asked to report any medications purchased and taken at home. In total, the children in our sample received 712 courses of antibiotics from a formal healthcare provider such as a doctor, clinic, or pharmacy. Table 3 shows the different types of antibiotics given, and their frequency in our sample. Most common are Co-Trimoxazole (33%), Amoxicillin (25%), and Metronidazole (16%).

Some children were treated with drugs from informal sources such as market stalls or traditional healers that parents reported to be antibiotics. However, in our analysis we focus on antibiotic treatments from formal sources including public and private clinics, private doctors, hospitals, and pharmacies. Some drugs sold as antibiotics at informal sellers are unmarked tablets of unknown origin and content, or counterfeit; medications from these sources may also be stored inappropriately or may have expired. There is little information whether and how the drugs were prescribed, or what instructions parents were given, if any. Antibiotics from informal sources are in any case not the norm; the majority of antibiotics treatments in our sample come from formal sources.

We do not observe directly what directions parents received about treatment length. Prescribed treatment length for an antibiotic can depend on the severity of illness. The handbook *WHO Model Prescribing Information: Drugs Used in Bacterial Infections* provides prescription guidelines for most antibiotic types in the sample (World Health Organization, 2001). The recommended treatment length is at least five days in most circumstances. There are some exceptions; for example, in the case of tooth abscesses (Amoxicillin), for typhoid or paratyphoid fever, salmonella, or E-coli (Co-Trimoxazole), and for giardiasis, a parasite found in contaminated water (Metronidazole), the treatment recommendation is three days. In our main specification (Model 1), we define minimum treatment completion for each drug separately according to Table 3. We code an antibiotic course as completed with a treatment length of seven days for Ampicillin, four days for Ceftriaxone, three days for Co-Trimoxazole when accompanied by a fever or diarrhea symptoms, three days for Metronidazole when accompanied by diarrhea symptoms, and five days otherwise.

In a second specification (Model 2), we define treatment completion uniformly as taking the antibiotic for at least five consecutive days. This number was chosen because the overwhelming majority of recommendations (across drugs and across illnesses for the same drug) require at least five days of treatment.[10] This alternative specification accounts for the possibility that the doctor's decision of which antibiotic type to prescribe might also depend on the child's gender, if it for

[10]Tooth pain was not reported in our sample, and the incidence of giardiasis has been found to be low in Mali (Berger, 2015).

example affects costs (but note that our results on the cost of the antibiotic below do not support this type of differential prescription behavior). However, both specifications find a very similar gender effect on adherence.

Because our data record only whether a medication was taken at all or not on a given day, we cannot say if children received the correct dosage. It is possible that parents of non-adherent children actually accelerated the treatment by giving higher doses for fewer days. We define "failure", or treatment end, as the first day on which the child is not given the antibiotic. Thus, if the treatment recommendation is five days, but failure occurs on or before the fifth day after treatment was started, this treatment course falls in the "not completed" category. Interrupted treatments (where the same antibiotic is taken again within a few days) are therefore included as non-adherence; there are only four such cases.

A small number of antibiotic spells are censored, for example because they occur at the end of the survey (and last beyond the symptom spell). We code the completion variable for these spells as missing. For Model 1, minimum completion, we define this threshold according to the minimum length the antibiotic should have been taken. For Model 2, five-day, we define it as five days before the end of the survey period. As a consequence, 14 (12) antibiotic spells are coded as missing according to Model 1 (2). In the failure model, we can estimate continuation probability on every day of the antibiotic spell, even when the spell is not observed to the end, so these estimates are not affected by right-censoring.

In order to identify unobserved selection effects later on, we use exogenous variation in access to treatment from the randomized enrollment of families into the healthcare subsidy treatment described above, the research project for which the data were originally collected. Appendix Table A.3, panel A, shows that the price subsidy was successful in reducing both the cost of a consultation and a course of antibiotics. Panel B of Table A.3 further shows that the price subsidy increased both overall treatment rates and the proportion of illness spells treated with an antibiotic.

4 Results

In this section we first document the low overall rates of adherence and show that there is a sizable gender difference in raw adherence rates. This motivates the formal analysis of selection and adherence behavior that follows.

Table 4 starts with some treatment summary statistics. Panel A shows that 20% of spells are treated at a formal source, and 13% are prescribed an antibiotic course. Within those, 4% of illness spells are treated with more than one type of antibiotics.

Before summarizing antibiotic completion rates, we make two sample restrictions. First, we focus on the first course of antibiotics taken within an illness spell. Second, for the small number of illness spells where multiple antibiotics were started on the same day, we choose the antibiotic which was taken for the longest time. Courses of antibiotics are prescribed and started, on average, on the sixth day of an illness spell and parents give the course of antibiotics to the child for four days on average. Only about 46% of antibiotic courses are given for the minimum recommended treatment length according to Table 3, and 40% of antibiotic courses are given for at least five days.

The table shows that there is no average raw gender difference in treatment start or cost, the time to treatment, or overall treatment length. By contrast, there is a large, statistically significant difference between boys and girls in the proportion of antibiotic courses that are completed, according to either definition (taken for the minimum treatment length or for five days). We already saw this difference in figure 1 in the introduction.

Note first that differences in adherence may not translate into large differences in average treatment length because there is censoring at the top: the number of tablets contained in the drug packaging automatically limits the length of time the antibiotic can be taken (only children who receive more than five tablets can take the antibiotic for more than five days). Second, while these aggregate raw figures are indicative, they are not conclusive: for example, it is possible that gender differences in treatment start are explained or masked by differential illness incidence for boys and girls.

Table 4: Treatment choice, length, and cost.

	(1)	(2)	(3)	(4)
			Boys - Girls	
	Mean	N	Difference	SE
Panel A. Treatment Choice				
Proportion of illness spells:				
Received consultation	0.20	3,526	-0.008	(0.015)
Treated with antibiotic course	0.13	3,526	-0.012	(0.012)
Treated with more than one course of antibiotics	0.04	3,526	0.004	(0.007)
Panel B. Treatment Length				
Time to treatment start (days)	6.02	470	-0.37	(0.68)
Treatment length (days)	4.10	498	0.64***	(0.22)
Completed antibiotic treatment?				
Minimum completion	0.46	503	0.14***	(0.05)
5 days or more	0.40	505	0.16***	(0.05)
Panel C. Treatment Cost				
Consultation where antibiotic prescribed (CFA)	280	519	-133	(98)
Antibiotic course (CFA)	709	518	85	(126)

Notes: Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1. Treatment refers to a course of antibiotics. Only consultations and antibiotics from formal sources are included. Formal sources includes community clinic, private doctor, hospital, or pharmacy. Right-censored antibiotic spells are excluded from treatment length in days. Minimum completion length including exceptions is defined in Table 3. The difference in means between boys and girls is shown in column 3. Standard errors from a regression of the variable on a male dummy, clustered at the compound level, are reported in column 4.

In what follows, we therefore extend the evidence by controlling for observable covariates and for possible selection on unobservables in treatment adherence. First, we estimate the gender difference in starting a course of antibiotics, conditional on demographic and illness characteristics. This analysis uncovers any cumulative gender differences in the decision sequence of prescription, purchase, and administration of the antibiotic. Next, we estimate the gender effect on treatment adherence (completion model) and the daily probability of discontinuing treatment (failure model). This allows us to control additionally for variation due to daily symptom incidence, and uncovers any significant gender difference in the daily probability of ending treatment before the completion

of an antibiotics course. As discussed above, we also correct the simple OLS estimates with a Heckman selection model, using the exogenous variation in access to treatment from the price subsidy, both to examine the potential role of unobservables in general and to rule out any potential for biased estimates due to selection and a correlation of gender with unobservable factors. Our results confirm that there is no gender effect on treatment start but a significant gender difference in adherence, driven by higher failure rates before treatment course completion for girls; given the lack of gender differences in selection into treatment, it is no surprise that the Heckman corrected results are very similar to the OLS results. As a last step, we examine heterogeneity and find that the household head's education level seems to play a significant role in the adherence gender effect.

4.1 Treatment Choice

Table 5 presents OLS estimates for the probability of starting a course of antibiotics obtained from a formal source of care. Column 1 includes only the gender of the child and price subsidy eligibility as explanatory variables, while column 2 controls for other demographic and household characteristics. These include the age of the child, first-born and adoption status, and an indicator for being an only child. The household variables include schooling of the child's mother and the household head, and household assets.

Column 3 additionally controls for illness characteristics, with sixteen dummies that indicate if the child ever presented a specific symptom type during the illness spell prior to seeking care. Cold symptoms (runny nose) are the omitted category. Symptoms are likely to change depending on what care the child received. Therefore, we only use whether a symptom was ever present in the period before starting treatment. Appendix Figure A.1 shows which symptoms are associated with antibiotic use. In column 4, we show the gender effect controlling for covariates chosen using double lasso. Of the candidate controls, only five symptom indicators were included by the procedure.

The effect of gender on the probability of starting treatment is reported in the first row of the table. The coefficient is a closely estimated zero in all specifications. This is consistent with the

Table 5: Estimates of the probability of starting a course of antibiotics for an illness spell.

	(1)	(2)	(3)	(4)
Male child	-0.012	-0.014	-0.009	-0.010
	(0.012)	(0.012)	(0.007)	(0.007)
Free care (FC)	0.106***	0.104***	0.044***	0.046***
	(0.013)	(0.013)	(0.008)	(0.008)
Additional controls				
Demographics		x	x	
Symptoms			x	
Double-lasso				x
Mean (control)	0.07	0.07	0.07	0.07
Nr of clusters	572	572	572	572
N	3,530	3,530	3,530	3,530

Notes: Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the compound level in parentheses. Each observation is an illness spell. The dependent variable is whether a course of antibiotics was prescribed and purchased at a formal source for an illness spell. Cold symptoms is the omitted symptom category.

descriptive evidence above, which showed similar treatment rates for boys and girls.

Eligibility for the price subsidy increases the probability of starting an antibiotic course by between 4.4 and 10.6 percentage points, significant at the 1% level. In comparison to the baseline treatment rate (7%), this effect constitutes a near doubling of the probability of receiving an antibiotic (consistent with the findings in Sautmann et al. (2017) on the overall probability of seeking formal health care as a result of the subsidy).

To summarize, we find no evidence that parents differentiate between their sons and daughters when deciding to seek medical advice. If parents do seek care for sons more often, doctors are exactly "undoing" this choice with their treatment decision, by prescribing antibiotics to girls more frequently, conditional on seeing them.

4.2 Treatment Adherence

4.2.1 Completion Model

Next, we examine differences in treatment adherence by gender. Table 6 begins by showing simple OLS estimates of the probability of completing a course of antibiotics and then, in order to control for any selection into treatment based on unobservables, uses a Heckman correction for selection bias. In Model 1, we define treatment completion according to the minimum treatment lengths by antibiotic type described in Section 3.3. In Model 2, treatment completion is defined as taking the antibiotic for five days or more.

In the first two columns, we only include demographic covariates. Next, we additionally control for illness characteristics. This is done, first, by including dummies for the symptoms present during the pre-care days of the illness spell, as in the regressions above. Second, we include two indicator variables that equal one if the child (a) exhibited any symptoms, and (b) exhibited the symptom the parents named as the reason for starting treatment, for at least three days from the first day of treatment onwards. For Model 2 in Table 6 we define these two symptom indicators using five days from the first day of treatment, instead of three days. The last two columns present results using covariates chosen by the double lasso procedure.

We find that boys are between 9.7 and 13.1 percentage points more likely to complete the prescribed treatment in Model 1, and between 12.2 and 15.7 percentage points in Model 2, and this effect is significantly different from zero at the 5% level or less in all specifications. The magnitude of the gender effect does not change substantially when controlling for illness characteristics.

Perhaps as expected given the lack of gender differences at the first stage, the gender effect also does not vary when correcting for selection bias using the Heckman procedure. In the second column with demographic controls only, the estimated selection effect ρ is large and positive, and in Model 2 it is significant at the 1% level, indicating that unobservables that make a child more likely to start treatment also lead to higher treatment completion rates. Interestingly, the selection effect is much smaller and not significant in the fourth column, where we control for symptoms.

This suggests that a lot of unobserved selection on illness severity can be controlled for with our basic symptom records.

Table 6 shows in fact that one of the most important factors in treatment completion is symptomaticity. Whether the child presented symptoms for three days consecutively after the start of treatment increases the probability of completion in Model 1 by 45 percentage points, significant at the 1% level. In Model 2, exhibiting symptoms for five days or more increased the probability of five day completion by 20 percentage points, also significant at the 1% level. The presence of symptoms is also the only explanatory variable included by the double lasso procedure in columns 5 and 6. By comparison, the additional dummies that focus on the symptom that was named as the reason for treatment do not affect completion probabilities significantly.[11]

4.2.2 Failure Model

Table 7 uses a more flexible specification and estimates the effect of gender and other characteristics on the daily probability of *failing to continue* treatment on a given day. As discussed above, this approach also alleviates concerns about the endogeneity of symptomaticity and treatment adherence. The first three columns again only include demographic controls while columns 4 and 5 add indicators for each symptom observed in the pre-care period. In addition to the pre-care symptom indicators, we also control for the presence of any symptoms on the day in question, as well as the presence of the symptom that was named as the main reason for seeking care.

As discussed in Section 2, "optimal" drop-out behavior would entail zero probability of failure prior to treatment completion. Some antibiotics may be prescribed for longer than the minimal recommended length, so the doctor-recommended drop-out rate after the minimal completion date is likely less than zero, but above one. In columns 2 - 7, we include indicator variables that allow the drop-out rate to differ discretely between days before and after completion. In Model 1, pre-completion days are those within the minimal length of time the antibiotic must be taken, in Model 2 these are days one through five. We also include two interaction terms between gender and days

[11] Other combinations of 3-day and 5-day indicators do not significantly affect the results on gender.

Table 6: Estimates of the probability of completing the course of antibiotics.

	(1) OLS	(2) Corrected	(3) OLS	(4) Corrected	(5) OLS	(6) Corrected
Panel A. Model 1: Minimum completion						
Male child	0.131***	0.125***	0.099**	0.097**	0.116***	0.117***
	(0.046)	(0.046)	(0.043)	(0.041)	(0.043)	(0.043)
Named symptoms present (3 days)			0.062	0.061		
			(0.067)	(0.066)		
Symptoms present (3 days)			0.450***	0.450***	0.456***	0.455***
			(0.056)	(0.054)	(0.044)	(0.044)
Selection effect ρ		0.324		0.055		0.058
		(0.240)		(0.225)		(0.264)
Mean (girls)	0.38	0.38	0.38	0.38	0.38	0.38
Nr of clusters	246	246	246	246	246	246
N	460	460	460	460	460	460
Additional Controls						
Demographics	x	x	x	x		
Symptom indicators			x	x		
Double-lasso					x	x
Panel B. Model 2: Completed five days or more						
Male child	0.157***	0.143***	0.135***	0.122***	0.137***	0.141***
	(0.045)	(0.047)	(0.046)	(0.046)	(0.043)	(0.043)
Named symptoms present (5 days)			0.019	0.019		
			(0.068)	(0.065)		
Symptoms present (5 days)			0.201***	0.204***		
			(0.055)	(0.054)		
Symptoms present (3 days)					0.404***	0.397***
					(0.042)	(0.042)
Selection effect ρ		0.586***		0.420		0.363
		(0.230)		(0.379)		(0.253)
δ			0.40			
Mean (girls)	0.31	0.31	0.31	0.31	0.31	0.31
Nr of clusters	245	245	245	245	245	245
N	462	462	462	462	462	462
Additional Controls						
Demographics	x	x	x	x		
Symptom indicators			x	x		
Double-lasso					x	x

Notes: Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the compound level in parentheses. Cold symptoms is the omitted symptom category. The parameter ρ is the estimated correlation between the error terms of the selection and adherence equations. The parameter δ is the estimated ratio of the covariance between the treatment and the unobserved controls and the treatment and observed controls, which would produce a gender effect of zero. In Columns 5 and 6, both the variables indicating symptoms present and named symptoms present for three and five days were included as candidate controls in the double-lasso procedure, but only the symptoms present for three days variable was chosen as a control by the procedure.

before and after completion (e.g. "male × pre-completion day" and "male × post-completion day" in Table 7). This allows us to focus on gender differences in drop out before the antibiotics course is completed.

In the first column, when we only include demographic controls, we find that boys are 2.7 and 3.8 percentage points less likely to stop treatment on any day, significant at the 1% level. Columns 2 and 3 show that this is driven by non-adherence: boys are between 3.5 and 4.3 percentage points less likely to stop treatment on the pre-completion days of the treatment. After completion, boys are equally likely to stop the treatment relative to the (selected) girls who are still receiving treatment at that point. The results are similar when controlling for illness characteristics. The presence of any symptom reduces the daily probability of stopping the treatment by between 22 and 26 percentage points. The presence of the symptom that parents named as the reason for starting the treatment additionally decreases the probability of failure by between 9 and 11 percentage points, significant at the 1% level. As before, there is some selection, although the estimated selection effect is now negative, since children who are more likely to start treatment are *less* likely to stop treatment on any given day. As before, however, once we control for symptoms in columns 5 and 7, the selection effect becomes smaller and not significant.

As a last robustness check, we can carry out the test based on Altonji et al. (2005) and Oster (2019) to examine how important omitted unobservables would have to be to make the gender difference in adherence equal to zero. In our preferred specifications of column 3 in Table 6 and column 4 in Table 7 (with demographics and symptoms included), the δ values are 0.40 and 0.88 for a hypothetical R^2 of 1. This means that there would have to be explanatory variables that we did not control for – or, more precisely, independent components of such variables – that are 0.40 and 0.88 times as important as the observables included, respectively. Even though they are below the threshold of 1 suggested by Oster (2019), we consider these values acceptable because our estimates already control for a rich set of covariates, including symptoms. It is also reassuring that the value of δ is closer to 1 in the failure model reported in Table 7, supporting the conclusion that estimating the daily probability of exit, controlling for detailed symptoms and the course of the

illness, is more robust to the omission of unobservables.

In summary, we find that there is a large and statistically significant gender difference in adherence, conditional on demographic and illness covariates and controlling for selection. Our main outcome variables are all binary; in Appendix Tables A.5 and A.6 we show that our main results are not sensitive to using probit instead. As discussed in section 2, whether symptoms are still present has previously been shown to be an important factor in treatment continuation, see for example Cohen et al. (2014). Our findings confirm this; whether the child still presents any symptoms is the most important predictor of adherence in our sample. This may be because the symptom serves as a reminder for treatment, or because parents have incorrect beliefs about the need to continue treatment beyond the child's apparent recovery. Nonetheless, girls are 10 percentage points less likely to complete an antibiotics course, reducing completion rates from 48% to 38%.

4.3 Mechanisms

Our results on the effect of gender on treatment choices and adherence are surprising: if parents have a preference for sons, we would expect this difference to be particularly visible in those actions that require larger investments of time and money. Getting treatment, even with the subsidy, requires significant effort and time transporting the child to the clinic and back and waiting for the consultation. By contrast, completing a course of antibiotics that was previously purchased has a much lower cost, and a priori we might therefore expect that it would be less subject to differential decisions based on gender.

There are two possible mechanisms for the gender difference at the adherence stage that we can partially test in our data. The first mechanism is purchasing and sales behavior. It is anecdotally known that doctors and pharmacists in Mali sometimes give poor families the option to buy or take home only a portion of a prescribed drug if they are unable to pay the full price, so that they can start treatment right away and return later with more cash to purchase the remainder of the drug. It is therefore possible that parents of girls only purchase a share of the prescribed dosage and then do not return to buy the rest.

Table 7: Estimates of the daily probability of not continuing the course of antibiotics.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Model 1: Minimum completion							
Male child	-0.027***						
	(0.010)						
Pre-completion	0.213***	0.217***	0.260***	0.448***	0.472***	0.403***	0.405***
	(0.021)	(0.022)	(0.025)	(0.037)	(0.044)	(0.029)	(0.039)
Post-completion	0.253***	0.245***	0.287***	0.401***	0.425***	0.357***	0.359***
	(0.022)	(0.025)	(0.029)	(0.036)	(0.043)	(0.027)	(0.041)
Pre-completion x Male		-0.035**	-0.034**	-0.048***	-0.048***	-0.047***	-0.047***
		(0.014)	(0.015)	(0.018)	(0.018)	(0.018)	(0.018)
Post-completion x Male		-0.014	-0.013	0.012	0.013	0.007	0.007
		(0.021)	(0.022)	(0.027)	(0.026)	(0.026)	(0.026)
Named symptom present				-0.108***	-0.108***	-0.104***	-0.104***
				(0.017)	(0.017)	(0.017)	(0.017)
Any symptom?				-0.259***	-0.260***	-0.256***	-0.256***
				(0.027)	(0.027)	(0.026)	(0.027)
Additional controls							
Demographics	x	x	x	x	x		
Symptoms				x	x		
Doube-lasso						x	x
Selection effect ρ			-0.095***		-0.052		-0.005
			(0.037)		(0.041)		(0.057)
Mean (girls)	0.22	0.22	0.22	0.22	0.22	0.22	0.22
Nr of clusters	255	255	255	254	254	254	254
N	2,438	2,438	2,438	2,411	2,411	2,411	2,411
Panel B. Model 2: Completed five days or more							
Male child	-0.038***						
	(0.013)						
Pre-completion	0.188***	0.191***	0.244***	0.391***	0.415***	0.347***	0.349***
	(0.026)	(0.026)	(0.031)	(0.038)	(0.048)	(0.029)	(0.044)
Post-completion	0.451***	0.439***	0.492***	0.540***	0.563***	0.488***	0.491***
	(0.029)	(0.033)	(0.040)	(0.042)	(0.050)	(0.035)	(0.050)
Pre-completion x Male		-0.043***	-0.042***	-0.041**	-0.041**	-0.042***	-0.042***
		(0.014)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)
Post-completion x Male		-0.017	-0.016	-0.011	-0.009	-0.011	-0.011
		(0.037)	(0.036)	(0.040)	(0.040)	(0.042)	(0.042)
Named symptom present				-0.086***	-0.086***	-0.083***	-0.083***
				(0.018)	(0.018)	(0.018)	(0.018)
Any symptom?				-0.228***	-0.229***	-0.227***	-0.227***
				(0.029)	(0.028)	(0.028)	(0.028)
Additional controls							
Demographics	x	x	x	x	x		
Symptoms				x	x		
Doube-lasso						x	x
Selection effect ρ			-0.122***		-0.054		-0.005
			(0.047)		(0.050)		(0.0690)
δ				0.88			
Mean (girls)	0.22	0.22	0.22	0.22	0.22	0.22	0.22
Nr of clusters	255	255	255	254	254	254	254
N	2,438	2,438	2,438	2,411	2,411	2,411	2,411

Notes: Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the compound level in parentheses. The dependent variable is 1 if it is the first day the prescribed antibiotic course is not taken. Pre-completion is an indicator for the days before the minimum recommended treatment length respectively, described in table 3. The parameter ρ is the estimated correlation between the error terms of the selection and adherence equations. The parameter δ is the estimated ratio of the covariance between the treatment and the unobserved controls and the treatment and observed controls, which would produce a gender effect of zero.

To test this we examine the dependence of the purchase price of the antibiotic on gender. If doctors prescribe a different antibiotic or a smaller dosage to girls, or parents buy fewer pills for girls at the pharmacy, we would expect to see a gender difference in the cost of the antibiotic. Table 8 shows various estimates of this gender effect on the total price of the antibiotics course purchased.[12] We first estimate the overall gender difference in cost, and then the difference conditional on the type of antibiotic (the first might be affected by the choice of antibiotic type by gender; the second captures if parents purchase a smaller quantity of a given antibiotic for girls). In the last two columns we conduct the estimation for the control group only which did not receive the subsidy. The gender effect on cost is small (about 34 CFA or less than $0.1) and not significant in any of the specifications.

The second mechanism is driven by education, and, we conjecture, based on differences in who is primarily responsible for the care of a child. Appendix Table A.4 shows the effects of all the demographic covariates on treatment completion in the specifications in Table 6. Whether the household head is educated has a large positive effect on the probability of completion. Children with an educated household head are 7 to 9 percentage points more likely to complete the course of antibiotics. Education could be associated with more leisure time, a greater belief in the efficacy of antibiotic treatments, or knowledge about antibiotic resistance. It could also indicate a better understanding of and memory for treatment instructions given by the doctor. All of these suggest that education has positive adherence effects, as we observe in our sample.

Household heads in our sample are 85% male and generally more educated than their wives. We examine differences in the education effect on adherence depending on the child's gender, on the hypothesis that the illness of a boy might lead to greater involvement of the male household head, while the care of a girl is left to the mother, who even in a household with an educated head is typically herself not educated, and may have for example more difficulty reading or remembering treatment instructions.We therefore test whether the education effect on adherence benefits boys

[12]The reference group in column 1 is a course of antibiotics prescribed at the clinic, in column 2 it is Amoxicillin prescribed at the clinic.

and girls differentially by interacting the gender of the child with the household head's education.[13]

In Table 9 we repeat the regressions from the previous section, but interact all relevant gender dummies with the education level of the household head. The results reveal that the entire gender effect on adherence is explained by families in which the household head is educated, and at the same time, the entire education effect is explained by the higher adherence rates of boys (rather than all children). Pre-completion daily failure rates for boys in families with an educated household head are between 6.6 and 7.1 percentage points lower than those for girls. The completion models suggest that boys are 16 to 17 percentage points more likely to complete the antibiotics course among families with an educated household head. By contrast, there is no significant gender effect in other families.

Figure 2 also plots the estimated gender effect by the education level of the household. While we lose some power by looking at more narrowly defined education categories, we see a monotonic relationship between education and differential adherence, which supports the overall take-away from the regression results of the positive relationship between the gender effect and education. These results are noteworthy because they suggest that gender differences in treatment adherence emerge when families are *better* educated. This is not because girls are treated worse in a household with an educated head than in other households; it is because the only group of children who adhere to treatment recommendations are the sons of educated men.

[13]To avoid specification searching, we also used the double-lasso procedure to investigate which interactions with gender should be included. We included all the candidate child and household controls in the original double-lasso procedure and their interactions with gender. The only control selected was the interaction between the indicator for whether the household head is educated and male child.

Table 8: Antibiotic cost.

				Control group only	
	(1)	(2)	(3)	(4)	(5)
Male	100	34	111	58	69
	(113)	(98)	(151)	(217)	(340)
Free care (FC)	-560***	-523***	-538***	0	0
	(148)	(117)	(118)	(.)	(.)
Source					
Other doctor / hospital visit	1559***	1273***	1286***	1042**	962**
	(372)	(306)	(306)	(407)	(374)
Pharmacist	327**	252*	260*	-142	-181
	(138)	(148)	(151)	(231)	(250)
Antibiotic type					
Ampicilline		-45	128		
		(150)	(186)		
Ceftriaxone		1247***	1490**	1909**	3141***
		(395)	(662)	(733)	(805)
Cotrimoxazol		-209***	-172	-367**	-358
		(79)	(111)	(181)	(234)
Erythromycin		-590**	-287	-706*	-453
		(295)	(238)	(365)	(404)
Gentamicine		0	54	-298	-428
		(155)	(267)	(200)	(266)
Metronidazole		-193**	-218*	-530**	-513
		(97)	(131)	(236)	(314)
Other		1293***	1023	2044***	1472**
		(475)	(656)	(667)	(722)
Unspecified		209	609	375	803***
		(206)	(381)	(331)	(293)
Interaction terms					
Male ×Ampicilline			-351		
			(213)		
Male ×Ceftriaxone			-480		-1854
			(795)		(1444)
Male ×Cotrimoxazol			-78		-41
			(164)		(418)
Male ×Erythromycin			-566		-419
			(525)		(649)
Male ×Gentamicine			-113		221
			(307)		(429)
Male ×Metronidazole			65		-72
			(202)		(449)
Male ×Other			334		878
			(893)		(1177)
Male ×Unspecified			-723*		-575
			(418)		(412)
Constant	811***	775***	750***	855***	878***
	(134)	(125)	(143)	(218)	(240)
Nr of clusters	245	245	245	87	87
Nr of observations	462	462	462	135	135

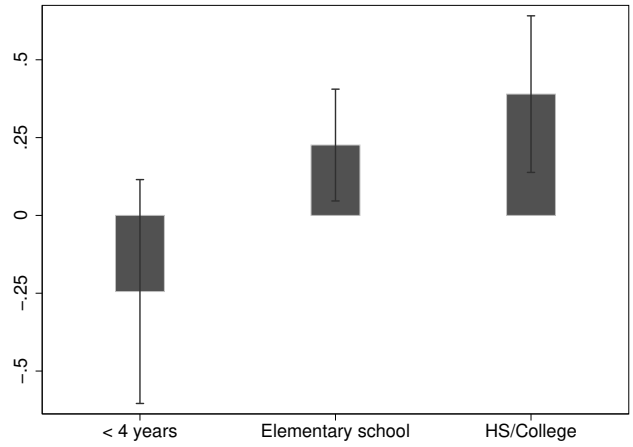
Notes: Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the compound level in parentheses. The reference group in column (1) is "any antibiotic prescribed at the clinic", in (2) "Amoxicillin prescribed at the clinic", in (3) "Amoxicillin prescribed to girls at the clinic". In columns (4) and (5), only children in the control group are included. 476 CFA equaled 1 USD in 2013.

Table 9: Gender effect on antibiotics completion by household head's education level.

	Completion		Failure	
	(1) Min. Completion	(2) >5 Days	(3) Min. Completion	(4) >5 Days
Male child	-0.029	0.016		
	(0.078)	(0.094)		
Male x Head educated	0.174*	0.163		
	(0.091)	(0.106)		
Head educated	-0.010	-0.008		
	(0.064)	(0.074)		
Pre-completion			0.423***	0.363***
			(0.038)	(0.038)
Male x Pre-completion			0.002	0.007
			(0.026)	(0.026)
Post-completion			0.375***	0.511***
			(0.036)	(0.043)
Male x Post-completion			0.072	0.159**
			(0.048)	(0.074)
Male x Pre-completion x Head educated			-0.071**	-0.066**
			(0.028)	(0.027)
Male x Post-completion x Head educated			-0.075	-0.204***
			(0.048)	(0.074)
Additional controls				
Demographics	x	x	x	x
Symptoms	x	x	x	x
Mean (girls)	0.38	0.31	0.22	0.22
Nr of clusters	246	245	254	254
N	460	462	2,411	2,411

Notes: Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the compound level in parentheses. All columns control for observed illness type and symptoms. In column (1) the dependent variable is whether the prescribed antibiotic course is taken for the minimum recommended treatment length as described in Table 3. In column (2) it is whether the course is taken for five days or more. In (3) and (4) the dependent variable is 1 if it is the first day the prescribed antibiotic course is not taken. In column (3) pre-completion and post-completion is defined according to the minimum prescribed treatment length for each antibiotic in Table 3. In (4) they are indicators for (day ≤ 5) and (day > 5) respectively. The constant is suppressed in columns 3 - 4.

Figure 2: Gender effect on antibiotics completion by household head's education level.



Notes: 90% confidence intervals constructed using standard errors clustered at the compound level. The figure shows the estimated coefficients from a regression of treatment adherence (minimum completion) on a gender dummy interacted with indicators for each education level. The reference group is "no education". All specifications control for demographics and symptoms. Children where the head is educated at an islamic school are excluded.

5 Conclusion

We use detailed health data to study gender differences in treatment choice and duration for antibiotics in Mali. We find no gender difference in treatment uptake, but uncover a large and significant gender difference in the probability of continuing the course of antibiotics until completion.

As a result of these differences, boys are over 10 percentage points more likely to complete the prescribed and purchased treatment, controlling for differential symptoms and illness type and other factors unrelated to discrimination. Moreover, we find that the *entire* effect of the child's gender on the one hand, and the household head's education level on the other, is explained by higher adherence rates of boys: the sons of more educated household heads are 16 to 17 percentage points more likely to complete treatment than either girls or sons of uneducated heads of family. An explanation may be that household heads are less involved in the care of girls, so that benefits from education and literacy that lead to better home care accrue overproportionally to boys

These results reveal that subtle mechanisms may be generating treatment differences between

boys and girls. If parents had an explicit preference for sons, we would expect them to apply a lower threshold for seeking care and purchasing an antibiotic for boys than for girls, actions that require investments of time and money. By contrast, completing a course of antibiotics that was previously purchased has an overall much lower cost. Ex ante, this decision should therefore be less influenced by differential willingness to invest in the care of children of different gender. Our result suggests that the persistent excess mortality for women and girls in Africa may be partly caused by implicit biases in care giving. It may be more difficult to address and eradicate these biases than more explicit discrimination in access to treatment at the clinic.

In addition to gender, our data allow us to also examine other determinants of antibiotic treatment uptake and adherence. We confirm that symptoms are important determinants of care-seeking behavior and demonstrate that selection into treatment is largely explained by observed illness heterogeneity among children. This indicates that information about a fairly straightforward set of health indicators of the child can substantially improve power and correct selection biases when examining determinants of the intensive margin of health care use.

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Appendix

A Supplemental Tables

A.1 Attrition

Table A.1 shows the number of children, households, and compounds present during the baseline survey in 2012 and follow-up survey in 2013. The price subsidy was implemented in 2013. In total, we include 1,768 children, 1,003 households, and 645 compounds in our sample. We find no significant differences in attrition between the treatment groups.

Table A.1: Attrition by experiment treatment groups.

	(1)	(2)	(3)	(4)
	Treatment Group			
	Control	Healthworker	Free care	HW & FC
Eligible children (census)	474	446	458	426
At baseline:				
Not found/absent/moved	21	22	17	7
Had died	2	1	0	0
Refusal	0	0	2	0
Enrolled children 2012	451	423	439	419
Present at follow-up 2013	402	387	394	384
Added after baseline	54	43	45	59
Final sample 2013	456	430	439	443

Notes: 1732 children in households found at baseline survey were enrolled and assigned to treatment. 1567 originally enrolled children were surveyed at the follow-up after receiving treatment. In addition, 201 children were born or newly enrolled in existing study households. A Pearson chi-square test that attrition post baseline is independent of the treatment group is not rejected (p = 0.497).

A.2 Balance

Table A.2 compares baseline characteristics of children, households, and parents in the two treatment groups. Overall there are few differences in the covariates. An exception is birth order, where the proportion of children eligible for free care that are first born is lower than in the control group.

Table A.2: Covariate balance by treatment group.

	(1) Control Mean	(2) Free Care Coefficient	(3) Std. Error	(4) N
A. Child characteristics				
Male	0.51	0.03	(0.03)	1,763
Age (years)	3.11	-0.02	(0.07)	1,763
First born	0.21	-0.04	(0.02)	1,763
Adopted	0.06	-0.01	(0.01)	1,763
Number of siblings	2.87	0.14	(0.14)	1,743
B. Household characteristics				
Assets	6170.88	783.01	(1122.77)	1,685
Income	65.91	2.76	(7.06)	1,615
Household size	6.65	0.26	(0.27)	1,728
Mother educated	0.45	0.01	(0.03)	1,743
Head educated	0.62	0.03	(0.04)	1,753
First authority salaried	0.12	-0.01	(0.02)	1,732

Notes: Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Standard errors from a regression of each variable on a price subsidy eligibility dummy, clustered at the compound level in parentheses.

A.3 Impact of subsidy on treatment rates and cost

Table A.3, panel A, shows that the price subsidy was successful in reducing both the cost of a consultation and a course of antibiotics. For visits at which an antibiotic was prescribed, being eligible for the price subsidy is associated with a reduction of the average consultation fee by 389 CFA, on average, significant at the 1% level. The price subsidy also reduced the average cost of a course of antibiotics by 785 CFA, again significant at the 1% level. Note that children in the "free care" group were eligible for free consultations at the local clinic and free medications for the five main causes of childhood mortality. However, not all consultations and purchases of medications occurred at the subsidized clinics, and not all treatment options are covered by the subsidy. The average costs in the "free care" group are therefore substantially lower, but not equal to zero.

Panel B of Table A.3 further shows that the price subsidy increased both overall treatment rates and the proportion of illness spells treated with an antibiotic. The proportion of spells treated at

Table A.3: Comparison of treatment rates and cost. Subsidy group (FC) versus control group.

	(1) Control Group	(2) Effect of Subsidy	(3) N
Panel A. Cost (CFA)			
Consultation where antibiotic was prescribed	548	-389***	712
	(82)	(93)	
Antibiotic course	1,261	-785***	711
	(146)	(158)	
Panel B. Proportion of spells			
Received consultation	0.15	0.11***	3,530
	(0.01)	(0.02)	
Treated with a course of antibiotics	0.07	0.11***	3,530
	(0.01)	(0.01)	
Number of illness spells	1,739	52	3,530

Notes: Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1. Standard errors clustered at the compound level are reported in the parentheses. Panel A includes all antibiotics observed during the survey period. Panel B restricts to uncensored illness spells. All consultations and antibiotic courses from either the clinic, private doctor, hospital, or pharmacy are included. The mean in the control group is in column 1, and the difference between the free care and control group in column 2.

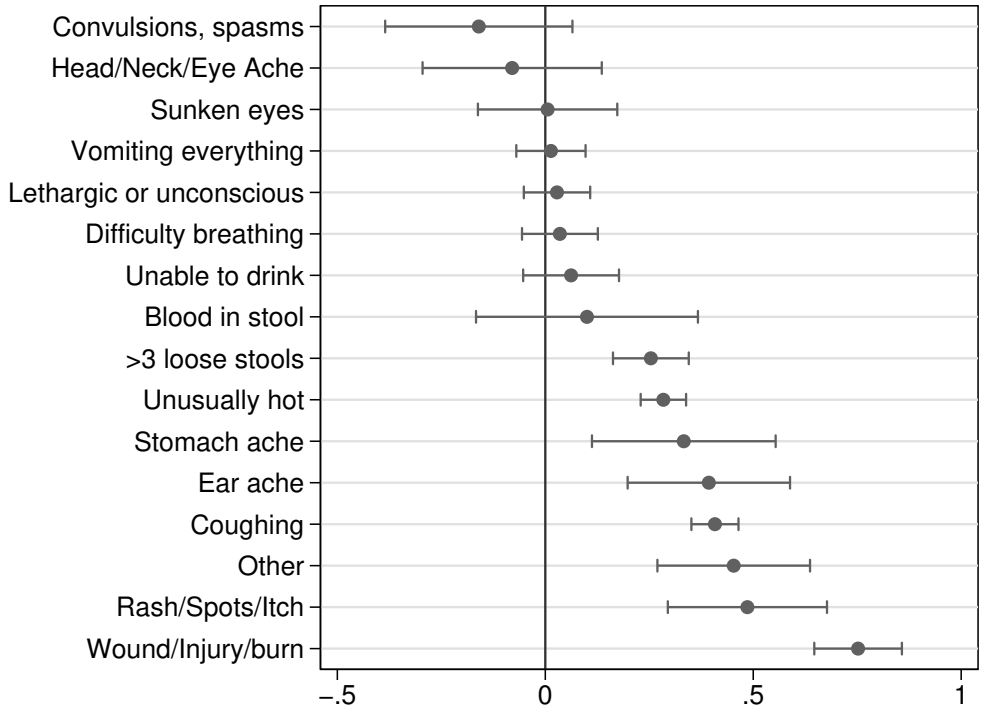
a formal source is 11 percentage points higher for children who are eligible for the price subsidy, significant at the 1% level. Similarly, the proportion of spells that are treated with an antibiotic is almost twice as high in the free care group relative to the control group. The last row reports the number of illness spells in the two groups. It is reassuring that the incidence of illness spells was not altered by the subsidy.

A.4 Symptom correlates with antibiotic start

Figure A.1 gives us some information which symptoms are associated with antibiotic use. Children who have a cough, fever ("unusually hot"), or diarrhea ("more than three loose stools") are all more likely to start a course of antibiotics relative to children with a cold, and children with a wound, injury or burn even more so. By contrast, children with head, neck, or eye ache are less likely to start an antibiotic. Some of these effects are likely explained by diagnostic differ-

ences. For example, a wound, diarrhea, or coughing could be an indicator of a bacterial infection, whereas vomiting everything, lethargy or unconsciousness are signs of (complications of) malaria. Difficulty breathing can be a sign of pneumonia, but is also caused by asthma.

Figure A.1: Symptom correlates with antibiotic treatment start (omitted category: cold symptoms).



Notes: 90% confidence intervals constructed using standard errors clustered at the compound level. The figure shows the estimated coefficients for each of the symptom indicators from the regression of antibiotic treatment start on child gender, price subsidy eligibility, and the full set of household controls. The omitted symptom category is cold symptoms.

A.5 Extended Table 6

Table A.4: Regression coefficients for demographic controls in Table 6.

	Model 1: Min. completion		Model 2: Five days	
	(1)	(2)	(3)	(4)
Male child	0.131***	0.099**	0.157***	0.135***
	(0.046)	(0.043)	(0.045)	(0.046)
Age (years)	0.015	0.014	0.019	0.025
	(0.017)	(0.016)	(0.016)	(0.017)
First born	-0.222**	-0.121	-0.250***	-0.264***
	(0.098)	(0.089)	(0.075)	(0.079)
Adopted or fostered	-0.033	-0.019	-0.045	-0.054
	(0.100)	(0.082)	(0.107)	(0.096)
Only child	0.238*	0.107	0.277**	0.275**
	(0.128)	(0.116)	(0.111)	(0.118)
Assets (log)	0.001	-0.004	0.003	0.004
	(0.010)	(0.009)	(0.010)	(0.010)
Mother educated	0.013	-0.012	0.022	0.009
	(0.050)	(0.045)	(0.048)	(0.048)
Head educated	0.087	0.073	0.076	0.070
	(0.060)	(0.051)	(0.057)	(0.056)
Named symptoms present (3 days)		0.058		
		(0.068)		
Any symptoms present (3 days)		0.452***		
		(0.056)		
Named symptoms present (5 days)				0.019
				(0.068)
Any symptoms present (5 days)				0.201***
				(0.055)
Additional controls				
Symptom indicators		x		x
Observations	460	460	462	462

Notes: Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the compound level in parentheses. This table reports the coefficients for the child and household covariates included in Column 3 of Table 6.

B Robustness

B.1 Non-linear estimation

Table A.5 presents marginal probit estimates of the effect of gender on the probability of starting a course of antibiotics. The point estimates are extremely close to the OLS estimates presented in Table 5. Similarly, we present marginal probit estimates of the probability of completing a course of antibiotics in Table A.6. In columns 1 - 4 the outcome variable is defined as completing a course of antibiotics. As in Table 6, boys are about 10 percentage points more likely to complete the antibiotic course in all specifications. In columns 5 - 8 the outcome variables is defined as failing to continue treatment on a given day. As in Table 7, boys are about four percentage points less likely to stop the antibiotic in the days before the recommended minimum treatment length and no less likely in the days after.

Table A.5: Marginal probit estimates of the probability of starting a course of antibiotics.

	(1)	(2)	(3)
Male child	-0.013	-0.015	-0.007
	(0.011)	(0.011)	(0.007)
Free care (FC)	0.106***	0.103***	0.044***
	(0.013)	(0.013)	(0.008)
Additional controls			
Demographics		x	x
Symptoms			x
Mean (control)	0.07	0.07	0.07
Nr of clusters	572	572	572
N	3,530	3,530	3,530

Notes: Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the compound level in parentheses. Each observation is an illness spell. The dependent variable is whether a course of antibiotics was prescribed and purchased at a formal source for an illness spell. Cold symptoms is the omitted symptom category.

Table A.6: Marginal probit estimates of the effect of gender on treatment adherence.

	Completion				Failure			
	Minimum completion		>5 Days		Minimum completion		>5 Days	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male child	0.123**	0.123**	0.146***	0.090				
	(0.052)	(0.052)	(0.047)	(0.068)				
Male × Pre-completion day					-0.048***	-0.042**	-0.049***	-0.043**
					(0.017)	(0.017)	(0.017)	(0.017)
Male × Post-completion day					0.007	0.007	-0.006	-0.004
					(0.025)	(0.021)	(0.028)	(0.025)
Additional controls								
Demographics	x	x	x	x	x	x	x	x
Symptoms	x	x	x	x	x	x	x	x
Pre and post-completion day					x	x	x	x
Selection effect ρ		0.010		0.409		-0.087		-0.101
Mean (girls)	0.38	0.38	0.31	0.31	0.21	0.21	0.21	0.21
Nr of clusters	246	246	245	245	254	254	254	254
N	460	460	462	462	2,411	2,411	2,411	2,411

Notes: Standard errors clustered at the compound level. Demographic characteristics include whether the child is the first-born, their age, whether they are adopted or fostered, whether they are an only child, as well as total assets (log) and whether the household head and mother have any schooling. Symptom controls include indicators for each of the symptom categories in the pre-care period. In columns 1 - 4, symptom controls also include an indicator for whether any symptoms were present for three (or five) days, while in columns 5 - 8, symptom controls include an indicator for whether any symptoms and the named symptoms were present on each day.

B.2 Family fixed effects

In our sample, there are 103 families where at least two siblings of opposite gender start a course of antibiotics at some point during the sample period. To control for unobservable household characteristics, we can compare antibiotic adherence rates of siblings. Table A.7 presents our main specifications including family fixed effects. The point estimates suggest that there is still a gender difference in treatment adherence even within families, but due to the small sample size the estimates are quite imprecise and we cannot draw any strong conclusions.

Table A.7: Fixed effects estimates of the effect of gender on treatment adherence

	Completion				Failure			
	Minimum completion		>5 Days		Minimum completion		>5 Days	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male child	0.068	0.121	0.058	-0.004				
	(0.089)	(0.095)	(0.081)	(0.061)				
Pre-completion day $\times$ male					-0.008	-0.063*	-0.017	-0.056*
					(0.031)	(0.033)	(0.030)	(0.033)
Post-completion day $\times$ male					-0.045	-0.048	-0.014	-0.053
					(0.052)	(0.053)	(0.051)	(0.054)
Additional controls								
Demographics	x	x	x	x	x	x	x	x
Symptoms		x		x		x		x
Pre and post-completion day indicators					x	x	x	x
Observations	245	245	241	241	2435	2409	2435	2409

Notes: Standard errors are clustered at the compound level. Demographic characteristics include whether the child is the first-born sibling, their age, and whether they are adopted or fostered. The other controls are as described in Tables 6 and 7.

B.3 Cost of antibiotic treatment

In Section 4.3, Table 8 we found that there was no gender difference in the cost of the antibiotic. In Table A.8 we also control for child and household characteristics as well as indicators for observed symptoms in the pre-care period. Similar to Table 8 there is no significant gender difference in the cost of any of the antibiotic types.

Table A.8: Gender difference in antibiotic cost, including household and symptom controls.

				Control group only	
	(1)	(2)	(3)	(4)	(5)
Male	86	-11	-12	-59	-161
	(119)	(104)	(146)	(289)	(418)
Free care (FC)	-537***	-514***	-531***	0	0
	(143)	(116)	(118)	(.)	(.)
Source					
Other doctor / hospital visit	1573***	1284***	1301***	1294***	1283***
	(393)	(328)	(328)	(470)	(463)
Pharmacist	314*	215	209	199	54
	(166)	(164)	(170)	(332)	(366)
Antibiotic type					
Ampicilline		-430	-118		
		(318)	(360)		
Ceftriaxone		1276***	1480**	2206***	3872***
		(408)	(693)	(770)	(919)
Cotrimoxazol		-195**	-188	-354	-347
		(84)	(119)	(234)	(302)
Erythromycin		-570*	-212	-410	-479
		(307)	(268)	(527)	(619)
Gentamicine		121	90	-607	-526
		(179)	(306)	(498)	(607)
Metronidazole		-92	-177	-386	-347
		(121)	(166)	(379)	(528)
Other		1353***	903	2032***	724
		(476)	(657)	(598)	(634)
Unspecified		209	695*	226	1621***
		(254)	(394)	(578)	(548)
Interaction terms					
Male ×Ampicilline			-683		
			(483)		
Male ×Ceftriaxone			-357		-2185
			(803)		(1480)
Male ×Cotrimoxazol			9		62
			(167)		(493)
Male ×Erythromycin			-672		-344
			(542)		(959)
Male ×Gentamicine			74		384
			(355)		(631)
Male ×Metronidazole			252		316
			(213)		(662)
Male ×Other			615		1757
			(884)		(1155)
Male ×Unspecified			-810*		-1584*
			(444)		(914)
Constant	675***	660***	614***	981*	925
	(253)	(203)	(211)	(532)	(604)
Nr of clusters	245	245	245	87	87
Nr of observations	462	462	462	135	135

Notes: Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the compound level in parentheses. Reference group in column 1 is antibiotic prescribed at the clinic, in 2 Amoxicillin prescribed at the clinic, in column 3 Amoxicillin prescribed to girls at the clinic. In columns 4 and 5 only children in the control group are included. 476 CFA equaled 1 USD in 2013. All columns include controls for household characteristics as well as indicators for observed symptoms in the pre-care period.