Celebrating Daughters: Evidence from India's Goddess Festival*

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Abstract

Nationwide religious festivals are common in developing countries, yet their short-run behavioral effect on the people inside them is less understood. I measure that effect for a festival that praises daughters. Each autumn, India keeps Navratri, nine nights for the Hindu goddess Durga, whose central rite worships young girls as her living form. Exploiting the timing of survey interviews relative to the festival's lunar dates, I document that the girl-boy gap in medical care-seeking closes when the care decision falls during the festival. The change comes from girls; I detect no change in care for boys. Girls also eat a wider variety of foods in the same nine days. The response is at least as strong in non-Hindu households. It is concentrated where the public celebration is built around a daughter, and largest where the goddess herself is the daughter come home. It is absent at festivals for male gods, for other goddesses, and for the same goddess celebrated without public spectacle. The evidence points to salience. For nine days, the public celebration of a daughter suspends the ordinary tilt against girls.

Keywords: son preference; gender gaps in health; care-seeking; festivals; salience; religion.
JEL codes: I12, I15, J13, J16, Z12.

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1 Introduction

Festivals are among the oldest and most expensive institutions developing countries keep, and the poorest households spend heavily on them (Rao, 2001; Banerjee and Duflo, 2011). While economists have begun to measure what these festivals do to economic outcomes (Campante and Yanagizawa-Drott, 2015; Montero and Yang, 2022), less is understood about what they do, while they run, to the people inside them. The answer may depend on what is celebrated, because no festival is neutral. Each one praises something: wealth, the harvest, the dead, workers. I study a festival that praises daughters and ask whether parental investment in daughters changes while it runs.

Navratri is among the largest celebrations on earth (McDermott, 2011; UNESCO, 2021). In its central rite, Kanya Puja, families receive young girls as the goddess's living form; their feet are washed, they are fed first, and they are bowed to (Rodrigues, 2003; Erndl, 1993). The praise does not stay inside the rite. During the autumn festival of 2019, India's morning papers carried it on their news pages: the Hindustan Times called the rite "a day of glory and joy" for girls from the poorest households, and the Deccan Herald reported a Hindu family worshipping a Muslim girl as the goddess herself.[1]

The festival is kept in India, where a hundred million women are missing. The daughters who are born get less of everything sons get: they are nursed for less time, fed less, given less of their parents' time, and taken for medical care less often when they fall sick.[2] So the festival celebrates the very girls who get the least, and the question is whether its statement changes what those girls receive.

To answer it, I follow the decision a family faces when a child falls sick, whether to seek medical care. I compare the decision for girls and for boys, when it is exposed to the

[1] " 'Kanya Pujan' a day of glory and joy for have-not girls," *Hindustan Times*, Lucknow edition, October 8, 2019; "Family worships Muslim girl as Durga in Kumari Puja," *Deccan Herald* (PTI), October 7, 2019.

[2] The estimate of more than one hundred million missing women is from Sen (1990) and Anderson and Ray (2010). In India, census counts of children aged 0–6 fell from 962 girls per 1,000 boys in 1981 to 919 in 2011, the most recent census (Office of the Registrar General and Census Commissioner, India, 2011). On the role of prenatal sex selection, see Jha et al. (2006); the children in any survey are, by construction, the girls who were born. On the investments daughters receive less of, from the food on their plates and the time they are nursed to medical care when they fall sick, see Barcellos et al. (2014), Jayachandran and Pande (2017), and Dupas and Jain (2024).

festival and when it is not. Exposure is quasi-random: the moon sets the festival's dates, and the survey interviews every day, so families do not control whether the decision falls inside Navratri. Specifically, the illness and the care decision sit somewhere in the two weeks before the interview, and I measure exposure as the share of those fourteen days that the festival covers. The comparison, a difference-in-differences, runs within the same state, interview month, and survey round. To identify the effect, the timing of interviews must be uncorrelated with omitted factors that affect the treatment of girls in particular.

I find that daughters are better treated during these nine days. The evidence comes from the pooled 2015–16 and 2019–21 rounds of the National Family Health Survey, 102,708 sick children in all. Parents seek medical care for 62 percent of them, and for girls they seek it less often. During the festival, this changes. A girl whose whole decision falls inside the festival is 8.2 percentage points more likely to be taken for care, a rise of 13 percent on that baseline. Figure 1 shows the gap closing in the raw data. The change comes from girls; I detect no change for boys. Most of the movement is a change of destination rather than new care. Three-fifths comes from families who would otherwise have stopped at a pharmacy, a shop, or a relative, and the rest comes from families who would have done nothing. A back-of-the-envelope calculation implies that a single Navratri sends roughly 600,000 additional girls to a medical provider, out of the 12 million girls under five who are ill in the festival fortnight.[3]

I support this design in four ways. I check whether girls are reported sick more often during the festival, when the effect appears and fades, whether other dates produce the same effect, and whether other festivals produce it. Girls are no more likely to be reported sick when the festival covers their window, and the illnesses reported for them are no different in type or severity. So the gap closes because sick girls are taken for care, not because different children are counted as sick. Traced week by week, the gap is flat before the festival. It narrows only while the decision window overlaps the festival, and it returns toward its earlier level afterward, with no detectable sign that families reshuf-

[3] Appendix Section C.1 gives the derivation line by line. In brief, 57 million girls under five (UN, 2019), of whom 21 percent are ill in a two-week window; the festival covers 38 percent of the typical ill girl's decision window; and where it covers, it raises her chance of care by the girl-specific estimate.

fled the timing of care. The effect is pinned to the festival's exact days rather than to the season around them. Windows displaced three weeks to either side of the festival, a window fixed at the festival's average autumn position, and another survey year's festival dates all carry nothing. I also place a nine-day window at every other possible point in the calendar. None of these placements produces an estimate as large as the actual one. Other festivals show nothing under the same design. Three large festivals for male gods, Diwali, and the spring festival of the same goddess, kept at home without a public celebration, all leave the gap untouched. Each carries a confidence interval that rules out an effect of Navratri's size.

The festival also changes what young girls are fed. For every child aged 6 to 23 months, the survey records the foods eaten in the twenty-four hours before the interview. On festival days, girls' dietary diversity rises by about eight percent of a standard deviation, enough to move girls from slightly behind boys to slightly ahead. The timing matches the care-seeking result, and nothing moves at placebo festivals. This second outcome matters for two reasons. It is measured for every child rather than only the sick, so it confirms the pattern in a sample where reporting plays no role. And it involves no provider and no payment, so it is decided entirely within the household.

The effect is not the result of direct religious observance. Rather, the festival influences behavior through the surrounding celebration. The public celebration raises the attention paid to daughters through streets, holidays, and shared public life, reaching observant and non-observant households alike, for as long as it lasts. This mechanism makes specific predictions, and the data meet each one. The effect grows with the share of the decision window that the festival covers. It reaches beyond the faithful. Muslim and Christian households, roughly three in ten of my sample, do not worship the goddess but can observe the cultural significance of girls. They respond at least as strongly as Hindu households. It follows the content of what is celebrated. Festivals for male gods and for other goddesses move nothing, and the effect is largest in the east, where the goddess herself is the daughter come home. In spring, the same goddess is worshipped with the same rites but no public celebration. Nothing moves, so the public celebration is necessary. The effect appears on decisions families make in the moment, at free facilities

and at home, not on services the state schedules. And it vanishes when the festival ends.

The alternatives fail against the same evidence. An income channel would place the additional care at paid providers. It is at free ones. A government campaign would travel through the outreach layer. It shows nothing. Social pressure would make watched households respond more. They do not. Regional warmth toward daughters would appear at the same regions' other festivals. It does not. A mechanism running through the household's own worship could not move the Muslim and Christian households that respond as strongly as Hindu ones. In economic terms, nothing about prices, supply, or information changed during those nine days. What changed is how much a daughter's illness weighs on her parents. How the festival changes it has three parts. The celebration helps the group it places at its center. Its public presence decides whom it reaches. And the help lasts only as long as the celebration does.

My first contribution is to the economics of religion. That literature has mostly asked how observance changes the observant; there is much less on what religious events do to the communities around them (Iyer, 2016). Recent work traces the effects of fasting, pilgrimage, and festival timing on output, attitudes, decisions, and development (Campante and Yanagizawa-Drott, 2015; Clingingsmith et al., 2009; Montero and Yang, 2022; Mehmood et al., 2023; Dasgupta and Datta, 2024), and, most recently, on behavior during the days themselves: violence against women rises during Holi (Poblete-Cazenave and Martínez V., 2025). I add a festival's short-run behavioral effect on the people inside it, with effects that reach beyond those who observe it and gains that follow whom it celebrates. Religion appears in the study of son preference as a carrier of the norms. Here, for nine days, it carries the reverse.

My second contribution is to gender economics, on the treatment of daughters. That work has documented the disadvantage across childhood and its consequences for survival (Bharadwaj and Lakdawala, 2013; Das Gupta, 1987; Arnold et al., 1998; Anukriti et al., 2022). It has traced the roots to kinship, old-age support, and the pressure to bear sons (Jayachandran, 2015; Jayachandran and Pande, 2017). What loosens the disadvantage has come from outside the culture, from quotas that curb stereotypes (Beaman et al., 2009), from classrooms that reshape attitudes (Dhar et al., 2022), from screens that shift

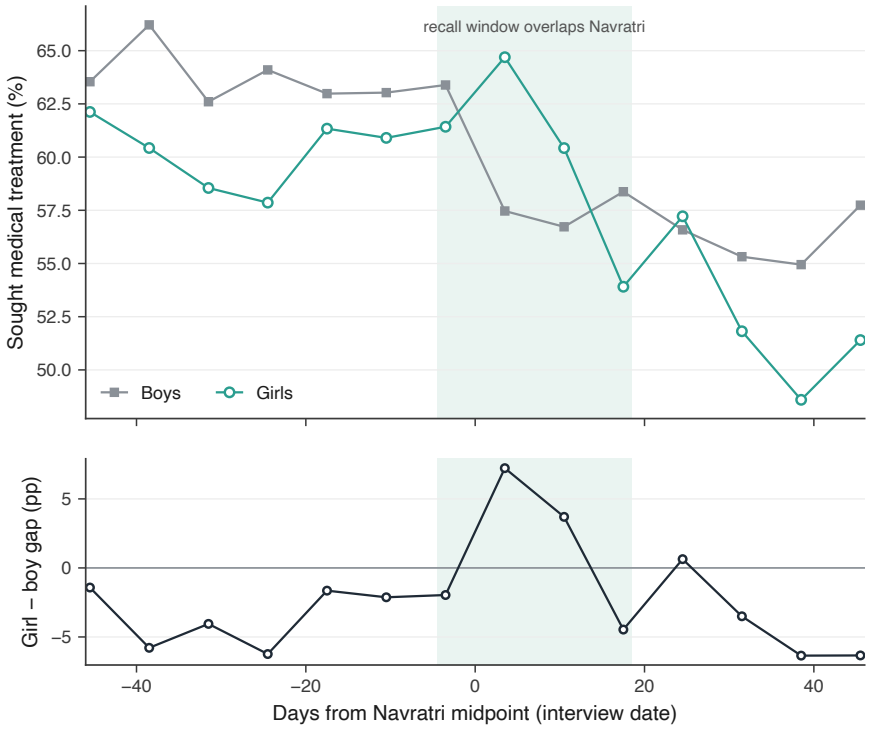
them (Jensen and Oster, 2009; La Ferrara et al., 2012), from earnings that raise women's worth (Qian, 2008), and from campaigns that advertise daughters' value (Dasgupta and Sharma, 2024). Even the fastest of these levers is measured over years, and the disadvantage outlasts free hospital care (Dupas and Jain, 2024). Little is known about the day to day, about how the treatment of a daughter moves decision by decision. The results here supply that margin. Daughters' care responds to a recurring cultural event, at no cost, within days, and from the girls' side, with no detectable change for boys.

My last contribution is to the literature on salience. Attention is scarce (Kahneman, 1973). What is momentarily salient carries disproportionate weight in behavior (Taylor and Fiske, 1978), and salience moves choices from taxation to consumption, in India down to what households eat on religiously salient days (Chetty et al., 2009; Bordalo et al., 2013; Mullainathan and Shafir, 2013; Atkin et al., 2021). Closer to this setting, ambient public events reach into how family members are treated at home (Card and Dahl, 2011), and calendar beliefs steer parents' investments toward particular children (Mocan and Yu, 2020). I add salience at the scale of a society, a naturally occurring attention shock carried by a recurring celebration, and a new margin, whether a sick girl sees a doctor. The festival supplies nothing new. It repeats a message centuries old, behavior responds while the message is delivered, and the effect repeats rather than compounds. The treatment of daughters depends not only on what parents believe but on what they attend to at the moment of a decision. The returns to messages about daughters may depend as much on timing as on content.

2 Background

Navratri is one of the largest festivals in India. Through its rites and its celebration, it is also an annual public statement about what a daughter is worth. In this section, I first describe the decision the paper studies, whether a sick child is taken for medical care. I then describe the festival, its message about daughters, its four public forms, and its reach beyond the households that observe it. Last, I place the festival in the calendar around it, which supplies the comparisons later sections use.

Figure 1: Care-Seeking around Navratri



Notes: Share of sick children taken to a medical provider, by interview date relative to the Navratri midpoint (seven-day bins), for boys (gray) and girls (teal); the lower panel plots the girl-minus-boy gap. The shaded band marks interviews whose two-week recall window overlaps the festival. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

2.1 Sick daughters and sick sons

Indian families have long favored sons. About 108 boys are born for every 100 girls, against a biological norm near 105 (Jha et al., 2006), and the gap reflects choices rather than biology (Sen, 1990). Most of a daughter's disadvantage, however, is not one dramatic act. It accumulates in small, ordinary decisions, in how long she is breastfed, in what she is fed, in whether her fever warrants a trip to the clinic (Jayachandran, 2015). Boys are nursed longer and receive more of their parents' time (Barcellos et al., 2014); the shortfall is steepest for daughters born later into the family (Jayachandran and Pande, 2017); and where the small decisions accumulate unchecked, they add up to the toll Sen (1990) counted in the tens of millions of "missing women."

This paper studies one of those decisions. India's National Family Health Survey asks every mother whether each child under five was ill with fever, a cough, or diarrhea in

the two weeks before the interview; 22 percent were. For each illness, it asks whether treatment was sought from a medical provider. Parents sought treatment for 63 percent of sick boys and 61 percent of sick girls. The gap looks small on paper. It is not small in children. Roughly 12 million girls under five are sick in any two-week span, so two percentage points is a quarter of a million girls every fortnight who would have seen a doctor if they were boys.[4]

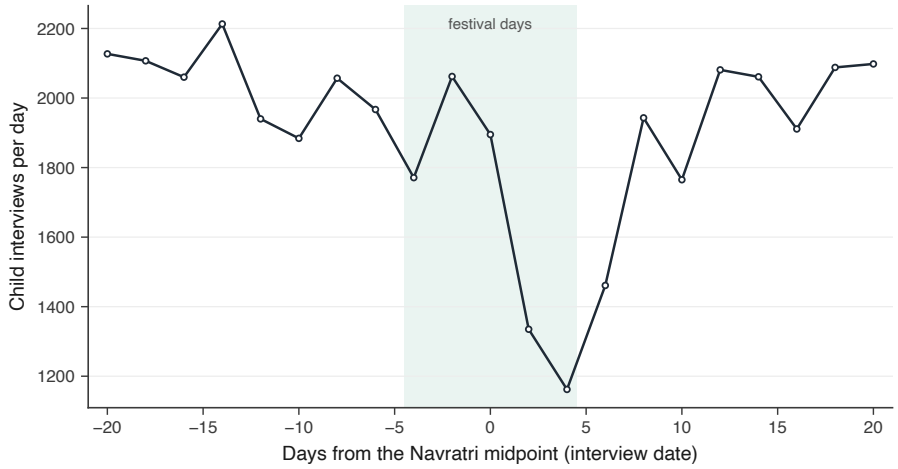
2.2 The festival

Navratri, "nine nights," honors the goddess Durga. It is celebrated twice a year. The spring festival, Chaitra Navratri, is kept quietly at home. The autumn festival, Sharad Navratri, is one of the largest events in Indian public life. The festival's dates are an astronomical calculation. It begins on the first day of the bright half of the lunar month of Ashvin, so its start moves by about eleven days a year and falls anywhere from late September to mid-October across the survey years (Appendix Table 1). No family, and no survey team, chooses when it arrives. Its scale shows in the survey itself. The survey keeps no festival, yet on festival days interviewing runs about 15 percent below the surrounding weeks (Figure 2).

The nine nights are not merely named for the goddess; they are addressed to her, and through her to girls. Observant households fast through the festival and recite its liturgy, the Devi Mahatmya, which tells the goddess that "all women, throughout all the worlds, are your forms" (Devi Mahatmya 11.6). On the festival's eighth or ninth day, households perform Kanya Puja, the worship of the girl (Rodrigues, 2003; Erndl, 1993). Young girls, usually between two and ten, from the family or the neighborhood, are received as living embodiments of the goddess. Their feet are washed, a thread is tied at the wrist, they are fed the festival meal before anyone else, given gifts, and bowed to (Appendix Figure 1). The practice is not a regional curiosity. It is kept in homes from Punjab to Bengal, and the Ramakrishna order has performed it at its Belur Math headquarters since Vivekananda

[4]The United Nations estimates 57 million girls under five in India in 2019; at the survey's 21 percent two-week girl-illness rate, roughly 12 million girls are sick in any fortnight, and two percentage points of that is roughly a quarter of a million.

Figure 2: The Survey Slows for the Festival



Notes: Child interviews per day, by interview date relative to the Sharad Navratri midpoint, pooled across the survey years; the shaded band marks the nine festival days. Interviewing falls by about 15 percent during the festival. The decline is a fall in the number of interviews, not a change in who is interviewed: the sex composition of the sample is unchanged, and the estimate survives dropping every festival-day interview (Section 5). National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

instituted it there in 1901. For these nine days, the festival's own logic makes the girl not a symbol of the goddess but her form. Whether it is this private rite or the public celebration surrounding it that moves families is a question the paper's comparisons are built to answer.

The myth the festival commemorates is a victory. Durga is created by the gods to slay the buffalo demon Mahishasura, the liturgy recounts her battles, and the tenth day is named for the outcome, Vijayadashami, the tenth of victory. The myth is the same everywhere in India; the celebration is not. In the east it enacts a homecoming. Married daughters travel to their parents' homes for the festival, the season's songs of welcome, the *agomoni*, are sung in a mother's voice awaiting her daughter, and Bengal calls the goddess *utsaber meye*, the daughter of the festival. The tenth day sends her, and them, back to their marital homes.

The same nine nights are kept in four ways across India. In the east the homecoming is also the public form. Durga Puja fills streets and squares with community pandals and public images of the goddess for days (Guha-Thakurta 2015; Appendix Figure 2). In the south it is Golu. Households display arrangements of dolls on tiered steps, and families visit one another's displays through the nine nights (Simmons et al., 2018). In the west it

is Garba, nights of organized, ticketed dancing, a social occasion more than a rite. And in the north the public spectacle of the season is the Ramlila, a staged serialization of the epic of the god Rama, ending in Dussehra; the goddess is worshipped there too, but at home, Kanya Puja included. The forms are distinct enough that UNESCO has inscribed three of them separately, each as a heritage in its own right, the Ramlila in 2008, Kolkata's Durga Puja in 2021, and Gujarat's Garba in 2023 (UNESCO, 2008, 2021, 2023). In the east, the figure in public view is the goddess as a daughter come home; in the south it is an arrangement in the home that neighbors come to see; in the west and the north, what fills the streets is a dance floor or a male god's epic. None of this requires a passerby to know any story. What the festival celebrates is visible in who is celebrated. I will use this variation in the festival's public content for the mechanism analysis of Section 6.

Nor does the festival belong only to Hindus, because its reach does not run through belief. Its great days are public holidays. The Reserve Bank's calendar closes the banks, the stock exchanges suspend trading for Dussehra, and schools across the east break for a week or more, whatever the students worship. Historians of the festival describe its Kolkata form as a public art event as much as an act of worship (Guha-Thakurta, 2015), and when UNESCO inscribed that celebration in 2021, its citation recorded that the crowds walking the streets to admire the installations cut across the divides of class, religion, and ethnicity (UNESCO, 2021). The survey's own slowdown during the festival (Figure 2) is the same fact, measured. An institution that keeps no festival bends to one. Seven in ten households in the sample are Hindu; the rest do not observe Navratri, but for nine days they live inside it. The paper returns to this fact.

Navratri also sits in a crowded calendar, and the crowding is useful. A month before it, Ganesh Chaturthi fills streets with public processions for the elephant-headed god, as public as Navratri, as festive, and about someone else. Three weeks after it, Diwali honors Lakshmi, the goddess of wealth, a goddess who is not a daughter come home; in Bengal the same date brings public pandals for the goddess Kali. The spring holds Chaitra Navratri, the same Durga with the same rites and no public celebration, along with Ram Navami for Rama. Each of these festivals shares something with Sharad Navratri and differs in the deity, the season, or the celebration. Later sections use them one at a time.

Three features of this setting drive the paper. First, the festival's dates are set by the moon and the interview's date by the survey's route, so the family controls neither of the two schedules that decide whether its two weeks of recall cover the festival. Second, the festival's content varies across regions in a documented way, from a public celebration of the goddess as a daughter to a commercial dance to a male god's epic. Third, the calendar supplies comparison festivals that isolate, one at a time, the deity, the season, and the celebration. Section 4 builds the design on the first feature; Section 6 uses the second and third, together with the festival's reach beyond the households that observe it.

3 Data

The paper uses two sources of data. The first is the two most recent rounds of India's National Family Health Survey, which record children's illnesses, the care they received, and the exact date of the interview. The second is a calendar of festival dates compiled from Hindu almanacs, which places each child's decision window against the festival.

3.1 The National Family Health Survey

The NFHS is India's arm of the Demographic and Health Surveys (International Institute for Population Sciences (IIPS) and ICF, 2021), a nationally representative household survey covering every state and union territory, in which mothers aged 15 to 49 are interviewed about each of their children under five. NFHS-4 was fielded from January 2015 to December 2016 and covers 259,627 children; NFHS-5 was fielded from June 2019 to May 2021, interrupted by the COVID-19 pandemic from March to November 2020, and covers 232,920 children. Together the rounds hold 492,547 children in about 58,000 sampling clusters.

Using these data, I construct the paper's two outcome variables, whether a sick child was taken to a medical provider and the variety of foods a young child was fed. The first comes from the survey's illness module. For every child under five, the mother is asked

whether the child was ill with fever, a cough, or diarrhea in the two weeks before the interview; the questions cover 471,961 of the children, of whom 102,710 were reported ill. For each illness she is then asked where, if anywhere, she sought advice or treatment, from a list of some two dozen sources. The outcome is the survey's summary indicator for whether treatment was sought from a medical provider, meaning a government hospital, dispensary, health center, or health worker, or a private hospital, doctor, or clinic. A visit to a pharmacy or shop, advice from a relative or traditional healer, and seeking nothing at all count as zero. The source detail matters later. It lets me split care into free public facilities, doorstep outreach, and paid private providers (Section 6). Two records with impossible interview dates are dropped, leaving 102,708 sick children in the estimation sample.

The second comes from the survey's food module. For every child aged six to twenty-three months, sick or well, the mother reports every food the child ate in the twenty-four hours before the interview; the outcome counts the food groups consumed, out of eight, following the World Health Organization's standard (World Health Organization and UNICEF, 2021). Appendix Section B.1 lists the groups and how they are built. The two rounds hold 140,859 children with this module.

A sample conditioned on illness is a sample conditioned on a report. Children enter it only when their mothers say they were sick. The worry this raises is that festival exposure could change who enters the sample rather than how sick girls are treated. The data allow this worry to be checked rather than assumed. Exposed and unexposed sick children look alike on the characteristics that predict care-seeking (Appendix Table 2), and the share of children reported ill does not move with the festival, for girls or for boys (Section 5). And the second outcome is built so the worry cannot arise at all. The diet count is recorded for every child, sick or well, with no screen to pass.

Measuring a child's exposure to the festival requires the exact date of the interview, and the survey records it. The survey also records the child's sex and age, the household's urban or rural location, wealth quintile, and religion, and the mother's education and age, which serve as controls. Because the NFHS samples in two stages, villages or urban blocks first and households within them, standard errors throughout are clustered

at the sampling cluster. Regressions are unweighted; robustness to the survey's sampling weights is shown with the other specification checks.

3.2 Festival dates

To place each child's decision window against the festival, I compile a calendar of festival dates from Drik Panchang, a standard Hindu almanac. Because festival dates are astronomical calculations rather than reports, almanacs agree on them; the dates used here were verified against independently published calendars for each survey year. Appendix Table 1 lists Sharad Navratri's dates in the survey years. The same sources supply the dates of Chaitra Navratri, Ganesh Chaturthi, Diwali, and the festivals for male gods used as placebos, and press reports confirm the regional festival dates, such as Kali Puja, held on Diwali's date in the Bengal region. Single-day festivals are given nine-day windows, matching Navratri's length, so that every placebo is compared on equal terms.

4 Empirical Strategy

To study the effect of Navratri on how parents treat girls, I need a comparison. The obvious one, families who worship the goddess against families who do not, will not work. Families who worship more differ in many other ways. So I do not compare families. Instead, I use the timing of the festival. I ask whether a parenting decision that happens to fall during Navratri is made differently for a girl than for a boy, compared with the same decision made at other times of the year. This section defines how I measure a decision's exposure to the festival, sets out the estimating equation, states the assumption that makes the estimate causal, and asks what could violate it.

Measuring exposure to Navratri.— I first define each child's exposure to the festival. The surveys record a decision over a recall window. For a sick child, the mother is asked whether, in the last two weeks, the child was ill and whether she sought treatment for it from a medical provider, so the decision sits somewhere in the fourteen days before the interview. The survey records the interview's exact date, and the festival's dates are

known for each year, so each child's window can be placed against the festival precisely.

I measure exposure as the share of the recall window that falls inside the festival:

$$\text{overlap}_i = \frac{\text{number of days of child } i\text{'s recall window that fall within Navratri}}{\text{length of the recall window}}. \quad (1)$$

The window is fourteen days for the care decision and one day for diet, which is asked over the last twenty-four hours. For the care decision, overlap runs from zero, when the window sits away from the festival, to about 0.64, when all nine festival days fall inside the fourteen-day window. To see the measure at work, consider two children in the same state in the autumn of 2016, when the festival ran from 1 to 10 October. A child interviewed on 10 October has a recall window holding all nine festival days, an overlap of 0.64. A child interviewed on 10 September, a month earlier on the survey's route through the same state, has a window that ends before the festival begins, an overlap of zero. Between the two sit children whose windows catch one, four, or seven festival days. If the illness is equally likely on any day of the window, overlap_i is simply the chance that the decision fell during the festival. I use this continuous measure rather than a yes-or-no flag because the decision is spread across the window. A child interviewed on the second day of the festival has a window that is mostly *before* it, while a child interviewed just after it has a window that covers it fully. The fraction weights each child by how much of the decision window the festival actually covers.

Estimating equation.— I estimate the following regression on children who faced the decision, sick children for care-seeking and children aged six to twenty-three months for diet:

$$y_i = \alpha + \beta_1 \,\text{girl}_i + \beta_2 \,\text{overlap}_i + \beta_3 \,(\text{girl}_i \times \text{overlap}_i) + X_i'\gamma + \delta_s + \delta_m + \delta_r + \varepsilon_i. \quad (2)$$

The coefficient of interest is β_3. To see what it captures, consider the three terms it is built on. The term β_1 is the gender gap that is there at all times. The term β_2 picks up anything the festival does to every child at once, such as clinics that close or feast food that all children share. What is left in β_3 is how the festival moves the gap between girls and boys. A positive β_3 means the girl–boy gap narrows when the decision falls during

Navratri. The design is a difference-in-differences. The first difference is between girls and boys, the second between decisions that fall during the festival and decisions that do not. Boys absorb whatever the festival does to every child, the everyday gender gap absorbs itself, and β_3 is what remains. The usual worry in such designs, that the two groups were already drifting apart, can be checked directly here rather than assumed, and Section 5 shows the gap is flat in every week before the festival.

Specifically, the controls X_i include whether the household is urban, the mother's education, the household's wealth, the mother's age, and the child's age. To compare children surveyed at similar times across years when the festival fell on different dates, I include fixed effects for state, interview month, and survey round. Standard errors are clustered at the survey cluster. Finally, I show that the estimate is not sensitive to the exclusion of the controls, of the fixed effects, or of both (Section 5).

Identification.— For β_3 to be causal, a child's exposure to the festival must be unrelated to the child's sex and to how much the family favors sons, once the fixed effects are taken out. Formally,

$$\mathbb{E}[(\text{girl}_i \times \text{overlap}_i)\, \varepsilon_i \mid \delta_s, \delta_m, \delta_r, X_i] = 0. \tag{3}$$

This assumption is plausible because no family can choose to be surveyed on a festival whose date moves with the moon. The interview date comes from the survey's route through the country, and the festival's date comes from the lunar calendar. Their overlap is, in effect, a coincidence of two schedules that the family does not control. The lunar calendar's shifting place in the solar year has identified effects in India before (Iyer and Shrivastava, 2018), and survey fieldwork crossing a religious event has done the same elsewhere (Bassi and Rasul, 2017).

What would it take to violate this assumption? The design leaves a confound little room, because it would need three properties at once. It must affect girls differently from boys. Anything common to both sexes (closed clinics, festival food, a wave of illness) is absorbed by the overlap term. It must sit on the festival's exact days rather than run with the calendar. Patterns fixed in calendar time, even patterns specific to girls, are absorbed by the fixed effects and by the girl-by-month interactions reported in Section 5. And it

must switch on and off at the scale of a two-week window. Little in Indian life switches on and off at that scale except the festivals themselves, and festivals can be tested one at a time. The sections that follow test the season by displacing the festival's window, festivals for male gods against the goddess's, and other goddesses against Durga.

The identifying assumption of equation (3) cannot itself be formally tested. I therefore provide corroborating evidence for it in two main ways. First, I test for balance. Exposed and unexposed sick children do not differ on the characteristics that predict care-seeking, the share of children reported ill does not move with exposure, and the sex composition of the sick sample is stable (Appendix Table 2). Second, I test for pre-trends. The girl–boy gap is flat in every week before the festival and moves only when the decision window overlaps it (Section 5). Two residual worries are named here and tested later. Fieldwork thins during the festival itself (Figure 2); Section 5 shows this is a fall in volume, not a change in whom the survey reaches, and that the estimate survives dropping every interview conducted on a festival day. And because exposure is defined by the interview's own timing, every exposed child is interviewed near the festival by construction. If the festival changed what mothers *say* rather than what they do, timing alone could not separate the two. For this reason the outcome is one that is hard to misreport, a completed visit to a medical provider, and Section 5 probes the reporting margin directly.

5 The Festival and Care for Sick Children

When a child falls ill, the parent decides whether to take the child to a medical provider. The outcome, defined in Section 3, records whether treatment was sought. Parents seek such treatment for 62 percent of sick children, and sick girls are taken less often than sick boys. Does this gap shrink when the decision falls during Navratri? This section shows three results. First, the gap narrows when the care decision overlaps the festival. Second, the narrowing follows the festival week by week, appearing only in the weeks the recall window overlaps it and vanishing after. Third, it grows with exposure, so that the more of the decision window the festival covers, the larger the effect. A fourth fact, that the effect is at least as strong in households that do not observe the festival, opens the mechanism

analysis in Section 6. Section 5.2 then shows the same pattern on a second margin, what a young child is fed, measured for every child rather than only the sick.

The gender gap narrows during the festival.— Figure 1 plots the share of sick children taken for medical treatment by the date of the interview relative to the festival, separately for girls and boys. Away from the festival, the girls' line sits below the boys'. As the decision window moves over the festival, in the shaded band, the girls' line rises toward the boys', and the gap between them closes. Table 1 reports the estimate from equation (2), building from the raw interaction to the full specification. The coefficient on the interaction of girl with festival overlap is 0.128 and is precisely estimated (column 4), and it barely moves as the controls and fixed effects enter, ranging between 0.126 and 0.128 across the four columns, a first sign that exposure to the festival is unrelated to the characteristics that predict care-seeking. Because overlap can reach at most 0.64, nine festival days inside a fourteen-day window, the most exposed case is a girl whose entire recall window fell over the festival. Relative to boys, such a girl is 8.2 percentage points more likely to be taken for medical treatment than she would be away from the festival. This is 13 percent of the mean treatment rate of 62 percent.

Per decision, the effect is 12.8 points, and most of it is a change of destination.— The 8.2-point figure understates what the specification itself asserts. Overlap is the share of the recall window the festival covers. It is the chance, for a child sick at some point in those fourteen days, that the care decision fell on a festival day. Even the most exposed child faces a chance of at most nine in fourteen, so 8.2 points is the per-decision effect diluted by the chance that her illness missed the festival. Undiluted, for a decision that did fall during Navratri, the effect is the coefficient itself, 12.8 points, 21 percent of the base rate. Most of that movement is not care conjured from nothing. A sick child's care ends in one of three places, nowhere, an informal source such as a pharmacy, a shop, or a relative, or a medical provider, and the festival's effects on the three must sum to zero. They split unevenly. Informal-only care falls by 7.9 points, no care at all by 5.0 points (Appendix Table 6, Panel B). Three-fifths of the movement is families who were already treating the illness changing where they treat it, from the corner chemist to a clinic, and the clinic they

Table 1: Care-Seeking for Sick Children and Exposure to Navratri

	(1)	(2)	(3)	(4)
Girl × festival overlap	0.127***	0.126***	0.128***	0.128***
	(0.036)	(0.036)	(0.035)	(0.035)
Effect at maximum overlap (pp)	8.2	8.1	8.2	8.2
Estimated separately: girls				0.109**
				(0.043)
Estimated separately: boys				-0.034
				(0.041)
Household and child controls	No	Yes	No	Yes
State, month, round FE	No	No	Yes	Yes
Outcome mean	0.62	0.62	0.62	0.62
Observations	102,708	102,708	102,708	102,708

Notes: The outcome is whether the parent sought treatment for a sick child from a medical provider (a government or private health facility or qualified provider), as opposed to a shop, a relative, or no treatment. Each column reports the coefficient on the interaction of an indicator for a girl with the share of the child's two-week illness-recall window that falls within Navratri, estimated on the same sample of sick children. Column (1) includes only the girl indicator and the overlap; column (2) adds the household and child controls (urban residence, mother's education, household wealth, mother's age, and child's age); column (3) adds state, interview-month, and survey-round fixed effects without the controls; column (4), the preferred specification, includes both. The effect at maximum overlap evaluates the coefficient at an overlap of 0.64, the largest value the measure takes (the nine festival days inside a fourteen-day window), in percentage points. The two rows below re-estimate the preferred specification on girls and on boys separately, replacing the interaction with the overlap term alone: the movement is on the girls' side. Standard errors clustered at the survey cluster in parentheses. * p<0.10, ** p<0.05, *** p<0.01. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

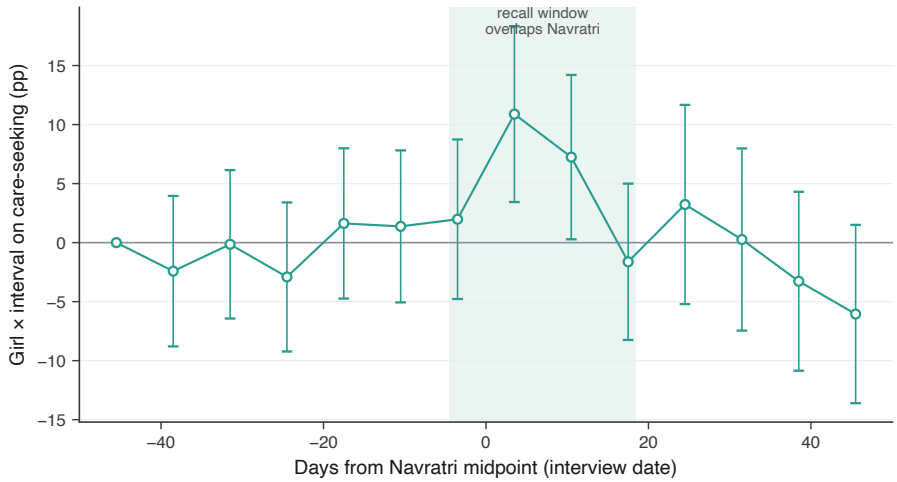
choose is typically a free public facility (Table 4). That is a marginal change in effort, the kind a nine-day change in attention can plausibly tip. The component that is new care, 5.0 points, sits at the modest end of what deliberate programs that insure, pay, or remind families achieve (Dupas, 2011).

The effect against the everyday gap.— The everyday gap gives these magnitudes their scale. At ordinary times, sick girls trail sick boys by 2.3 percentage points (s.e. 0.3), the coefficient on girl in the specification of column 4. At the average exposure of treated children, the festival's effect is about five points, enough to close that gap.

The narrowing follows the festival week by week.— Figure 3 makes the lower panel of Figure 1 precise. It estimates the girl–boy gap in seven-day bins of the interview date, now with the controls and fixed effects of equation (2), taking the weeks well before the festival as the reference. The gap is flat in every bin before the festival, rises in the two bins where the recall window overlaps it, by about eleven percentage points in the first week and seven in the second, and returns toward zero after, the later weeks scattering

on both sides of it. These interval-by-interval estimates are noisier than the pooled slope of Table 1; what they establish is the shape, nothing before the festival, a jump during it, nothing after.

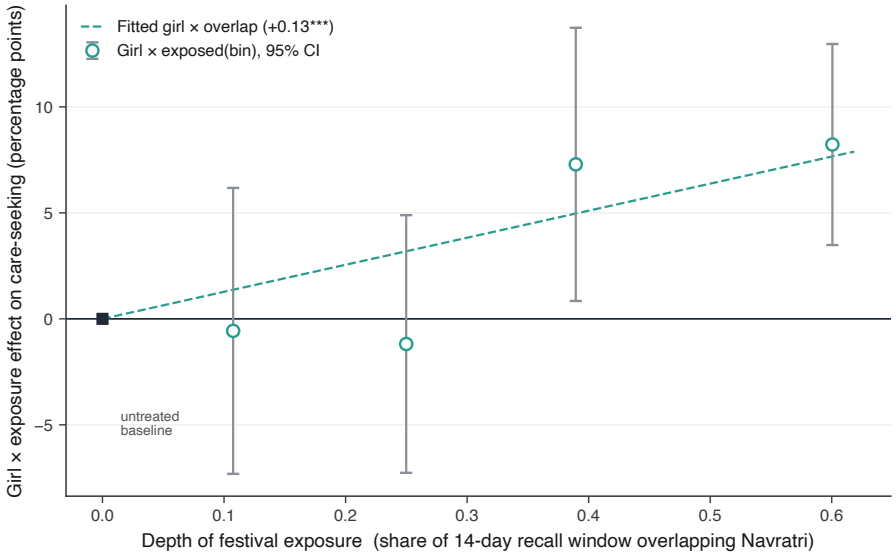
Figure 3: Event Study: The Care-Seeking Gap around Navratri



Notes: Coefficients on girl × seven-day intervals of the interview date relative to the Navratri midpoint, with the controls and fixed effects of equation (2); the omitted reference is the interval furthest before the festival, and the shaded band marks interviews whose recall window overlaps it. Bars are 95% confidence intervals, clustered at the survey cluster. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

It grows with exposure to the festival.— If Navratri is what moves care-seeking, then the more of a child's decision window the festival covers, the larger the effect should be. This is a demanding prediction, because how deeply the window overlaps the festival is set within a state by the interview date alone, so it is unrelated to the state's son preference, its form of the festival, or its region, all of which the fixed effects already hold fixed. Figure 4 shows the effect follows the prediction. I sort the children whose window touches the festival into bins by how much of it the festival covers, and estimate the girl–boy gap in each against children whose window falls outside the festival. The gap is essentially zero for children whose window barely clips the festival; it is flat at shallow exposure and rises where coverage is high, reaching about eight percentage points for children whose window covers the festival in full, the same slope as the single interaction in equation (2). Anything that could mimic this pattern would have to track the festival's coverage of the decision window day by day, within a state.

Figure 4: The Care-Seeking Gap Grows with the Depth of Festival Exposure



Notes: Coefficients on girl × indicators for bins of festival-exposure depth—the share of the two-week recall window inside Navratri—from one regression with the controls and fixed effects of equation (2). Children whose window misses the festival are the omitted group; each point sits at its bin's mean overlap, and the dashed line is the fitted girl × overlap slope. Bars are 95% confidence intervals, clustered at the survey cluster. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

5.1 Validity and Robustness

Several concerns remain about whether the effect is really the festival. I take them in turn, from the most serious to the least.

The sick children near the festival could be a different group.— This is the most basic concern, because the estimate is computed on sick children. If girls were more or less likely to be reported sick near the festival, the sex composition of the sample would move with the festival, and the interaction could pick that up mechanically. It does not. The share of girls among sick children is 0.47 in the festival window and 0.48 in the weeks before it, a difference that is not significant, and a regression of an indicator for illness on girl × overlap, across all children, is a precise zero, as are the same regressions for each type of illness and for its severity markers (Appendix Table 2). Conditional on the fixed effects the design uses, the children interviewed in the festival window are also balanced on their observable characteristics. None of the six covariates in Panel A of Appendix Table 2 differs significantly from the pre-festival window, the largest being mother's edu-

cation at 0.057 (s.e. 0.067). They do not differ in birth order or in the number of brothers in the household either, the two strongest predictors of how a girl is treated in India, when each is regressed on girl $\times$ overlap (Appendix Table 5, Panel C). The thinning of fieldwork during the festival itself (Figure 2) does not change this conclusion. It is a fall in volume, not a change in whom the survey reaches. Sixty-one percent of the exposed children were interviewed on the ordinary working days just after the festival, with the recall window reaching back into it, and the share of girls is the same whether the interview fell on a festival day (0.48), just after it (0.46), or away from the festival altogether (0.46). The result does not, in any case, rest on the children interviewed while fieldwork was thin. Dropping every interview conducted on a festival day, which removes two in five of the exposed children and leaves only ordinary working days whose recall windows still reach back into the festival, the estimate remains positive and significant at 0.09, smaller than the full-sample estimate because the surviving interviews fall mostly in the week after the festival, where the effect has already begun to fade (Appendix Table 3).

The result might be a pre-existing trend, not the festival.— A more subtle worry is that the girl–boy gap was already narrowing on its own, for reasons that have nothing to do with the festival, and that Navratri simply sits on top of that trend. If that were so, the gap would be moving before the festival, not only during it. Figure 3 has already shown it is not. The gap is flat in every bin before the festival and returns to zero after it. The effect is concentrated exactly where the decision could have fallen during the festival, and nowhere else.

The narrowing could be a seasonal pattern specific to girls.— The month fixed effects absorb the average October, but not a pattern in which girls are treated differently in autumn for reasons that have nothing to do with the festival, such as a harvest, the school calendar, or a weather cycle. Two tests answer this. First, the pattern can be absorbed directly. Adding interactions of girl with interview-month dummies, and then with a flexible cubic in the day of the year, leaves the estimate unchanged, if anything slightly larger, near 0.14 (Appendix Table 5, Panel A). What identifies the effect is the festival switching on and off inside the month, day by day, and no smooth seasonal pattern matches that. Second,

the window can be displaced. Moving the nine-day window three weeks earlier or later, pinning it to the festival's average autumn position, or handing each child another survey year's festival dates all yield nothing (Appendix Table 8; Appendix Figure 3 plots the tests). The effect sits on the festival's exact days, not on a band of autumn. Fieldwork crosses the festival in two survey years, 2016 and 2019, whose festivals fell two days apart, so these are within-season tests.

The estimate might reflect who falls in the festival window, not the festival itself.— The families interviewed near the festival may differ from others in ways that also shape care-seeking (they may be richer, more educated, more urban), so that the interaction tracks who happens to land in the window rather than the festival. If that were so, controlling for those differences would move the estimate. It does not. Table 1 already showed the coefficient is essentially the same with no controls at all, with the household and child controls alone, with the fixed effects alone, and with both together. It stays within a narrow band around 0.13. That a rich set of observable characteristics leaves the estimate unchanged makes it less likely that unobserved differences are driving it. The bounding argument of Oster (2019) makes this reasoning precise. The controls and fixed effects raise the R^2 from 0.001 to 0.07 while leaving the coefficient essentially unchanged, near 0.13. Even under the most conservative bound, where the observables and unobservables together would explain all of the variation in care-seeking, the bias-adjusted estimate does not fall toward zero; it is 0.15, if anything slightly above the estimate itself, and only unobserved selection working against the observed differences could overturn it (Appendix Table 4). Adding birth order and the number of brothers in the household, each interacted with girl, leaves the estimate at 0.13 (Appendix Table 5, Panel B).

The festival might change what mothers say, not what they do.— Care-seeking is the mother's own report, so one might worry that the goddess festival changes how a daughter's care is *described*, making attentiveness to girls the admirable thing to report, rather than the care itself. This worry is harder to set aside than the others. Exposure is defined by the interview's timing, so every exposed child is interviewed near the festival. If the festival changed what mothers say rather than what they do, timing alone could not separate

the two. The outcome is the answer. It is the survey's summary measure of whether the child was taken to a *medical provider*, a public facility or a private hospital or doctor, and it counts a pharmacy, a shop, or a relative as no treatment, not a soft claim of having "sought advice" (Appendix Table 6). Taking a sick child to a clinic is a costly, concrete act, harder to invent after the fact than a yes to whether one did anything. The pattern across margins points the same way, though I lean on it only lightly. The interaction is small on the broad measure that counts any source and sizable on actual medical care, the reverse of what a desire to give the admirable answer would produce, but nearly nine in ten mothers already report some source, so that margin could not move much in any case. The weight of the defense is that the effect appears on a completed visit to a medical provider, the hardest of these answers to invent after the fact, and not on the easiest thing to say.

An effect this size might appear by chance at some point on the calendar.— Try enough dates, and some will line up with a narrowing gap by chance. To address this, I re-estimate the interaction at every possible placement of a nine-day window in the calendar away from Navratri, 286 placements in all. Appendix Figure 4 plots the resulting t-statistics. The actual festival ranks first among all 287 possible placements, an exact randomization p of 0.003, and no placement produces a coefficient as large as the actual estimate; the largest placebo values are mid-July windows unrelated to any festival, sitting exactly at the expected extreme of that many draws. Placing the festival anywhere else in the year never reproduces the effect.

Any autumn festival or holiday might produce the same pattern.— It is fair to ask whether any festival, or simply a holiday in the same season, would move care-seeking in the same way. It would not.[5] Appendix Table 7 repeats the exercise festival by festival. Three large festivals for male gods, each a public holiday estimated on at least as many exposed children as Navratri, and Diwali, a grander holiday three weeks later in the same season, all leave the gap untouched. Whatever moves care-seeking is specific to Navratri.

[5]Elsewhere, holidays move care the other way, by delaying it; cardiac mortality peaks at Christmas and New Year partly because care is postponed (Phillips et al., 2004). A holiday account of Navratri would lower care, not raise girls'.

A single influential state might be driving the result.— Son preference is far stronger in some states than others, so one large state with a sharp festival response could carry the national estimate while most of the country shows nothing. It does not. Appendix Figure 5 re-estimates the regression dropping each state in turn, and every one of the resulting estimates falls inside the baseline confidence band.

The estimate might depend on the particular choices behind the model.— The estimate is not fragile (Appendix Table 3). It stays near 0.14 to 0.15 when I restrict the sample to interviews within thirty, forty-five, or sixty days of the festival, so the comparison is local; it is unchanged when every interview from March 2020 onward is dropped; it survives survey weighting and clustering the standard errors at the state level; and it holds when I estimate the model as a logit rather than a linear probability model. The same is true of the fixed effects, which matter here for a specific reason. The survey routes its fieldwork through a state in a temporal block, so a state and the timing of its interviews are linked. Replacing the separate state and month effects with the state $\times$ calendar-month cell leaves the estimate at 0.132; using the state $\times$ fieldwork-month block instead gives 0.133; and allowing a separate girl coefficient in every such block, which leaves only the day-to-day variation in exposure inside one state's single month of fieldwork, gives 0.147. Nor does inference rest on the cluster structure. Allowing errors to be spatially correlated across all survey clusters within two hundred kilometers of one another (Conley, 1999) raises the standard error only from 0.035 to 0.043 ($t = 3.0$).

5.2 A Second Margin: What Girls Are Fed

Every estimate so far is computed on children who fell ill, and the checks above answer the worries that conditioning raises one at a time. The survey's food module offers a margin that requires no conditioning at all. For every child aged six to twenty-three months, sick or well, the mother lists the foods the child ate in the twenty-four hours before the interview, and I count the food groups consumed, out of eight, following the World Health Organization's standard (Section 3). Feeding is also the archetype of the small, ordinary decision this paper began with. It is made inside the household, several times a day, with

no provider to reach, no travel, and no fee. Because the recall is a single day, exposure here is simply whether the interview fell during the festival, so equation (2) applies with the overlap replaced by an indicator for being interviewed during Navratri.

The pattern repeats. Table 2 builds the estimate exactly as Table 1 did. The interaction of girl with the festival is 0.151 food groups in the full specification and moves little as the controls and fixed effects enter. The magnitude is modest against the outcome's scale, about five percent of the mean of 3.07 food groups, or eight percent of a standard deviation. It is nonetheless enough to change the ordering. Girls trail boys slightly in ordinary weeks and move ahead of them on festival days. As with care-seeking, the movement is on the girls' side. Their diversity rises by 0.136 ($p = 0.05$) while boys, if anything, drift down and insignificantly. Feast food shared by all children, or the festival's fasts, cannot be what it picks up. Figure 5 traces the interaction in nine-day intervals around the festival, flat for the month before, a jump on the festival days themselves, and back to zero after.

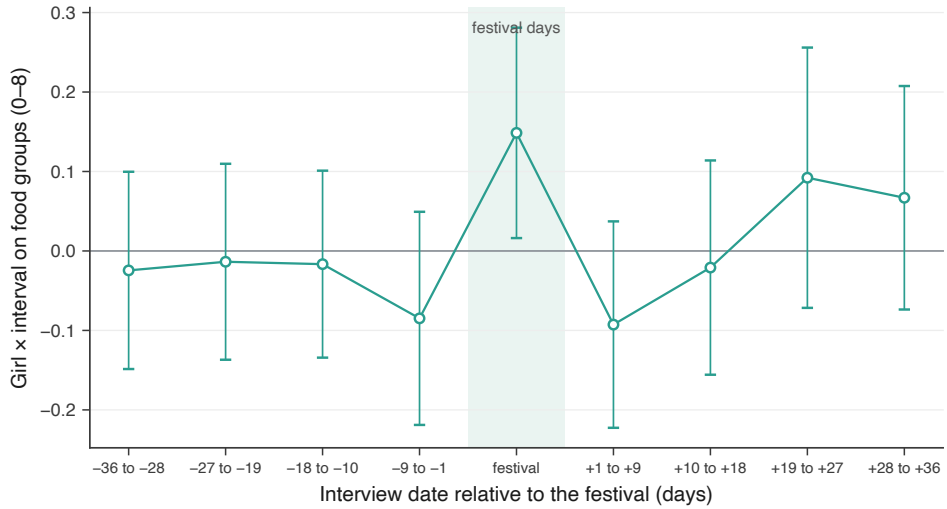
Table 2: What Girls Are Fed: Dietary Diversity and the Festival

	(1)	(2)	(3)	(4)
Girl × interviewed during festival	0.165*	0.181**	0.136*	0.151*
	(0.086)	(0.083)	(0.083)	(0.080)
Household and child controls	No	Yes	No	Yes
State, month, round FE	No	No	Yes	Yes
Outcome mean	3.07	3.07	3.07	3.07
Observations	140,859	140,859	140,859	140,859

Notes: The outcome is the number of food groups, out of eight, the child consumed in the twenty-four hours before the interview, following the World Health Organization's diversity standard for children aged six to twenty-three months; the sample is every child of that age with the food module, not only sick children. Each column reports the coefficient on the interaction of an indicator for a girl with an indicator for being interviewed during Navratri, so the recalled day falls within the festival. Column (1) includes only the girl and festival indicators; column (2) adds the controls (urban residence, mother's education, household wealth, mother's age, and child's age in months); column (3) adds state, interview-month, and survey-round fixed effects without the controls; column (4), the preferred specification, includes both. Standard errors clustered at the survey cluster in parentheses. * p<0.10, ** p<0.05, *** p<0.01. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

The result also passes the same discipline. Nothing happens at Ganesh Chaturthi (-0.05, $p = 0.49$), and nothing at the spring Navratri ($+0.02$, $p = 0.72$), estimated on twice as many festival-day interviews. No single food carries it. The largest single-group movement is in staple grains ($+0.040$, $p = 0.03$), followed by continued breastfeeding

Figure 5: What Girls Are Fed, around the Festival



Notes: Coefficients on girl × interval from a single regression of the number of food groups consumed in the past twenty-four hours (children 6–23 months) on girl, interval indicators, and their interactions, with interviews more than 36 days from the festival as the reference and the controls and fixed effects of equation (2). The shaded interval is the festival's own days; whiskers are 90% confidence intervals, clustered at the survey cluster. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

(+0.026, $p = 0.05$), itself a documented margin of daughters' disadvantage (Barcellos et al., 2014), with most other groups positive and small. And the response is absent in the richest forty percent of households (−0.05, not significant), whose children's diets have the least room to move, and present below the top.

Two limits are worth the same plainness as before. The estimate is marginal in precision, significant at the ten percent level with the girls' side at five, so this margin supports the care-seeking result rather than standing on its own. An exact randomization test of the kind used for care-seeking, re-estimating the interaction at every feasible placement of a nine-day window in the calendar, gives $p = 0.09$, in line with the estimate's analytic precision. And because the diet window is a single day, festival-day interviews are too few to split by religion with precision; the non-Hindu estimate is positive, +0.08 with a standard error of 0.14. The non-observer test rests with care-seeking. What it adds is different, a margin with no illness conditioning, no provider, and no reporting of a past event, only what the child ate yesterday, moving for girls in the same nine days.

6 Why the Gap Narrows

Why does the festival narrow the gap? The evidence points to salience. The surrounding celebration raises the attention households pay to their daughters, for as long as it lasts. The ingredients of this mechanism are established. Psychology treats attention as a scarce resource that decisions compete for (Kahneman, 1973), and shows that whatever is momentarily salient carries disproportionate weight in judgment and behavior (Taylor and Fiske, 1978). Economics finds the same forces in the field. Salient attributes distort consumer and tax decisions (Chetty et al., 2009; Bordalo et al., 2013; Mullainathan and Shafir, 2013). Ambient public events reach into how family members are treated at home (Card and Dahl, 2011), and calendar beliefs steer parents' investments toward particular children (Mocan and Yu, 2020). What this paper adds is a naturally occurring version at societal scale, a nine-day period in which public life itself is the salient stimulus, and what it makes salient is daughters. The effect does everything salience requires. It reaches households that do not observe the festival, follows the content of what is celebrated, requires the public celebration rather than the private rite, grows with exposure, ends when the exposure ends, appears on decisions where attention can act, and concentrates where the celebration is built around a daughter. Each was a way the claim could fail. This section shows them one at a time, then rules out the alternatives.

One feature of the results matters throughout. The gap narrows because care for girls rises, not because care for boys falls (the exposure effect is $+0.109$ for girls and -0.034, not significant, for boys). Whatever the festival does, it acts on daughters in particular.

The effect reaches households that do not observe the festival.— Muslim and Christian households keep no fast and worship no goddess, but they can observe the cultural significance of girls. They show the narrowing at least as strongly as Hindu households, $+0.152$ against $+0.095$, a difference of $+0.064$ (s.e. 0.070) that is statistically indistinguishable from zero (Table 3; each group separately in Appendix Figure 6). Within these households the result carries every mark of the original (Table 3). It is specific to the festival's window, absent at placebo festivals, and concentrated in free care. Their response also follows the same regional map as everyone else's, $+0.190$ in the east, where the celebration is

the daughter's homecoming, against +0.10, not significant, in the other three forms. For these families the festival is only an environment, so the environment is the only thing their response can reflect.

Table 3: The Same Effect in Households That Do Not Observe the Festival

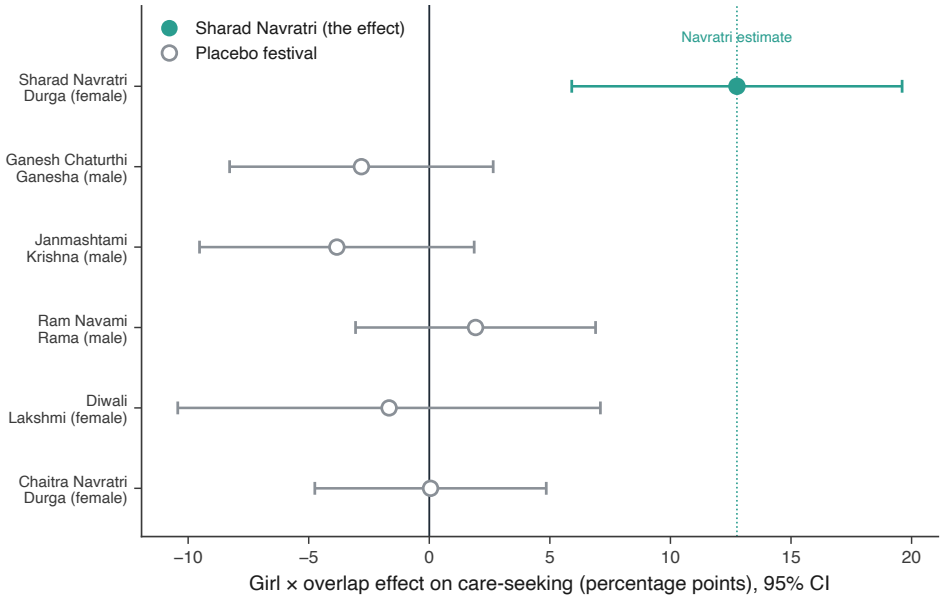
	Hindu	(s.e.)	Non-Hindu	(s.e.)
Girl × Navratri overlap	+0.095**	(0.047)	+0.152***	(0.052)
Difference (non-Hindu - Hindu)			+0.064	(0.070)
A. Placebo festivals and holidays				
Ganesh Chaturthi (male deity)	-0.039	(0.036)	-0.007	(0.044)
Republic Day	-0.029	(0.028)	+0.013	(0.053)
Independence Day	-0.053	(0.045)	-0.038	(0.052)
B. By care layer				
Public facility (free)	+0.080**	(0.037)	+0.098**	(0.050)
Outreach layer	+0.002	(0.016)	+0.029	(0.025)
Private provider (paid)	+0.027	(0.044)	+0.044	(0.047)
C. Non-Hindu households, by the region's festival form				
East (Durga Puja)			+0.190***	(0.061)
The other three forms			+0.096	(0.098)

Notes: Every estimate re-run separately for Hindu and non-Hindu (Muslim, Christian, Sikh, Buddhist, other) households. The headline row is equation (2); Panel A estimates the overlap effect separately for girls and boys; Panel B replaces the Navratri overlap with placebo windows (nine days each): Ganesh Chaturthi, and the two national holidays, which for households that do not observe Hindu festivals are the shared days off; nothing moves on them. Panel C replaces the outcome with care at each layer of the health system. Hindu N = 73,557, non-Hindu N = 29,151 sick children. * p<0.10, ** p<0.05, *** p<0.01. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

The effect follows what the festival celebrates.— India's calendar supplies festivals with different content, celebrated by the same households in the same states. The effect appears only where the content is a daughter. Three large festivals for male gods leave the gap unchanged, each estimated on at least as many exposed children as Navratri, and Diwali, a larger festival of a different goddess, leaves it unchanged as well (Appendix Table 7, Figure 6).

The effect requires the public celebration.— The spring Navratri is the same goddess with the same rites, including Kanya Puja, kept at home without the public celebration. It produces nothing (+0.05 percentage points, against +12.8 in the autumn), even among the Hindu families who observe both (Appendix Table 7). The worship is present in the spring; the celebration around it is not; the effect follows the celebration.

Figure 6: Which Festivals Move the Gap



Notes: Coefficients on girl × festival overlap, in percentage points, from separate care-seeking regressions of the form of equation (2), with 95% confidence intervals clustered at the survey cluster. Only the autumn festival of Durga (teal) narrows the gap. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

The effect grows with exposure.— Exposure depth varies within a state through the interview date alone. Two children in the same state face the same festival, but one recall window may cover a single festival day and another may cover nine. The gap closes in proportion. The effect rises from near zero at shallow exposure to about eight percentage points at full coverage (Figure 4).

The effect lasts only as long as the exposure.— A change in what parents believe or want would outlast its cause. The effect does not. On both outcomes the gap is flat before the festival, moves while the decision window overlaps it, and returns toward its earlier level after, with no detectable offsetting decline in later weeks (Figures 3 and 5). Attention passes with the celebration that produced it, and returns with the next one; repetition renews the effect without compounding it.

The effect appears where attention can act.— A shift in attention can move a decision that is open at that moment; it cannot move a service that runs on a schedule. The additional care appears at fixed government facilities, where a family must decide to go (+0.086),

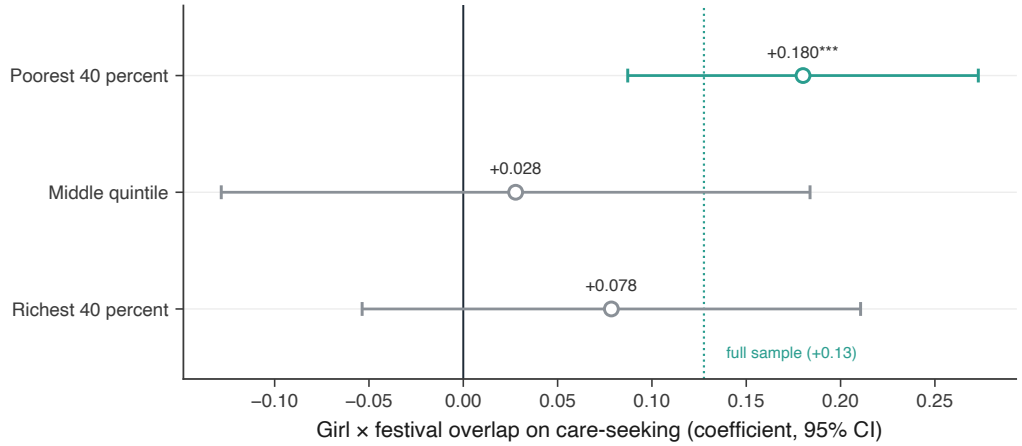
and not at the outreach layer, where the state brings care on its own timetable (+0.014; Table 4). It appears in what girls are fed at home, a decision with no provider and no fee (Section 5.2). And it is concentrated among the poorest forty percent of households (+0.180; Figure 7), where a decision of this kind is most likely to hang in the balance.

Table 4: Free Care, at Facilities the Family Must Reach: The Effect by Care Layer

Care sought at	Girl × overlap	(s.e.)
Any medical provider	+0.128***	(0.035)
Outreach layer (anganwadi, camp, mobile, sub-center)	+0.014	(0.014)
Public facility (govt hospital, dispensary, PHC, CHC)	+0.086***	(0.030)
Private provider (paid)	+0.043	(0.033)

Notes: Each row re-estimates equation (2) with the dependent variable equal to one if treatment for the sick child was sought at the listed layer of the health system. Sources are classified from the survey's provider codes: the outreach layer is care brought to or near the home (anganwadi/ICDS center, health camp, mobile clinic, sub-center/ANM); public facilities are fixed government providers (hospitals, dispensaries, urban health centers, PHCs, CHCs); private providers are paid. Pharmacies, shops, traditional healers, and relatives are not counted as medical care. Controls and fixed effects as in equation (2); standard errors clustered at the survey cluster. N = 102,708 sick children. * p<0.10, ** p<0.05, *** p<0.01. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Figure 7: The Effect Is Concentrated Where Care Is Least Assured



Notes: Coefficients on girl × festival overlap from equation (2), estimated separately by household wealth, with 95% confidence intervals clustered at the survey cluster; the dotted line marks the full-sample estimate. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

The effect is concentrated where the celebration is built around a daughter.— The same nine nights are kept in four forms across India, and the gap narrows where the public celebration is built around a daughter, not where the nights are a dance gathering or a play

about a male god (Table 5). The effect is largest in the east, where the celebration is most explicitly a daughter's. Durga is the daughter come home to her parents, and the streets fill with her image for days. Comparing the east with all three other forms, the contrast is 0.14 (s.e. 0.07), significant at the ten percent level. The east keeps other large public festivals, Ganesh Chaturthi and Diwali, with more exposed children than Navratri itself. At those festivals the interaction is -0.02 (Table 5). Eastern households do not favor daughters whenever a festival arrives; they do it at this one. What the regions contribute is the form of the celebration, and where that form is built around a daughter, daughters' care rises.

Table 5: The Gap Narrows Where the Celebration Is Built around a Daughter

Region and form	How it is celebrated	Girl × overlap	*N*
East *Durga Puja*	Public pandals fill the streets; Durga is the daughter come home to her parents.	+0.203*** (0.056)	21,935
South *Golu*	Households display arrangements of dolls on tiered steps and visit one another's displays.	+0.134 (0.083)	11,743
West *Garba*	Ticketed all-night dance gatherings; a social occasion more than a rite.	-0.023 (0.100)	7,716
North *Ramlila*	The public spectacle is the play of the god Rama; the goddess is worshipped at home.	+0.042 (0.062)	61,087
East vs. all other forms		+0.136* (0.072)	

Notes: Each row estimates equation (2) on the sick children of one regional form of Sharad Navratri, with the paper's controls, state, interview-month and survey-round fixed effects, and standard errors clustered at the survey cluster in parentheses. The bottom row reports the contrast between the east, whose celebration is built around a daughter, and all three other forms, estimated as a triple interaction. A regional taste for daughters at festivals in general cannot account for the pattern: at the same regions' other large public festivals (Ganesh Chaturthi and Diwali) the same interaction is -0.02 in the east and -0.03 in the south, on more exposed children than Navratri itself. States are grouped by dominant public form: Durga Puja (West Bengal, Assam, Odisha, the northeast), Golu (the southern states), Garba (Gujarat, Maharashtra), and Ramlila (the Hindi-belt north). * $p<0.10$, ** $p<0.05$, *** $p<0.01$. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Alternatives.— An income channel would place the additional care at paid providers. It is at free ones. An income channel would also feed daughters at other feasts. At Ganesh Chaturthi, which supplies more festival-day interviews than Navratri itself, girls' diets do not move (Section 5.2). A government campaign would travel through the outreach layer of the health system, which shows nothing. Social pressure would make watched households respond more; households under the eyes of in-laws or long-settled neighbors re-

spond no more than others, and the feeding margin is invisible to neighbors altogether (Appendix Table 9). A taste for daughters cannot explain who responds. Families that already have a son respond no differently from families that do not (-0.02, not significant), and the effect is if anything larger among families with only daughters who still hope for a son, $+0.245$ against $+0.078$, a difference of 0.17 with a standard error of 0.08 (Appendix Table 10). The households with the most revealed preference for a son respond the most. Festival travel does not carry it. Mothers interviewed away from home are three percent of the sample, no more common during the festival, and among mothers at home the effect is intact ($+0.135$; Appendix Table 3). Grandeur is not the answer. Diwali is grander than Navratri and does nothing, and Garba is the west's grandest Navratri and does nothing.

7 Conclusion

This paper asked what a celebration's statement does to the people it honors. The answer came from nine days each autumn when India's public life says the opposite of what its everyday decisions say. A society whose ordinary choices favor sons keeps, once a year, a great festival built around a daughter, whose central rite worships young girls. For those nine days, the culture changes sides.

The decisions listen. When a sick child's care falls inside those nine days, the gap between daughters and sons closes, because care for girls rises. The same days raise the variety of what girls are fed. The response comes with no money attached. It is not delivered by the state, does not outlast the festival, and does not need an audience. It does not require the household to observe the festival at all; Muslim and Christian families respond as strongly as Hindu ones. Across India it concentrates where the public celebration is built around a daughter, and is largest where the goddess herself is the daughter come home. It is absent where the same nine nights are kept as a dance gathering or a play about a male god. The explanation that survives this pattern is salience. The surrounding celebration turns households' attention toward their daughters. While it lasts, the ordinary tilt in small decisions is suspended; at the festival's height, the estimates

suggest it runs the other way.

The effect is brief by construction; attention passes with the festival. Nothing in this paper suggests that a festival can substitute for the economic forces behind son preference. What the episode shows is harder to see with standard instruments. The tilt against daughters is not a fixed feature of the households that hold it. It moves, within the same families and the same season, when the world outside says something different about girls. And it moves in households the message was never addressed to. Festivals are among the oldest instruments a society has for declaring whom it honors. These results suggest the declarations are not merely expressive. What a festival celebrates reaches into households and changes, for a time, what children receive. The direction follows the content. On Holi, a festival of license, violence against women rises (Poblete-Cazenave and Martínez V., 2025); on Navratri, care for daughters does. Whether other celebrations carry other reversals, for other groups and other places, is a question this design can now ask. A message that closes a gap for nine days is not a policy. But it identifies a margin, what daughters are worth in the public eye, that moves faster and reaches further than the forces usually asked to explain it.

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Online Appendix

Celebrating Daughters:
Evidence from India's Goddess Festival

Josué Djossou

A Appendix Figures and Tables

A.1 Appendix Figures

Appendix Figure 1: Kanya Puja: The Worship of the Girl during Navratri



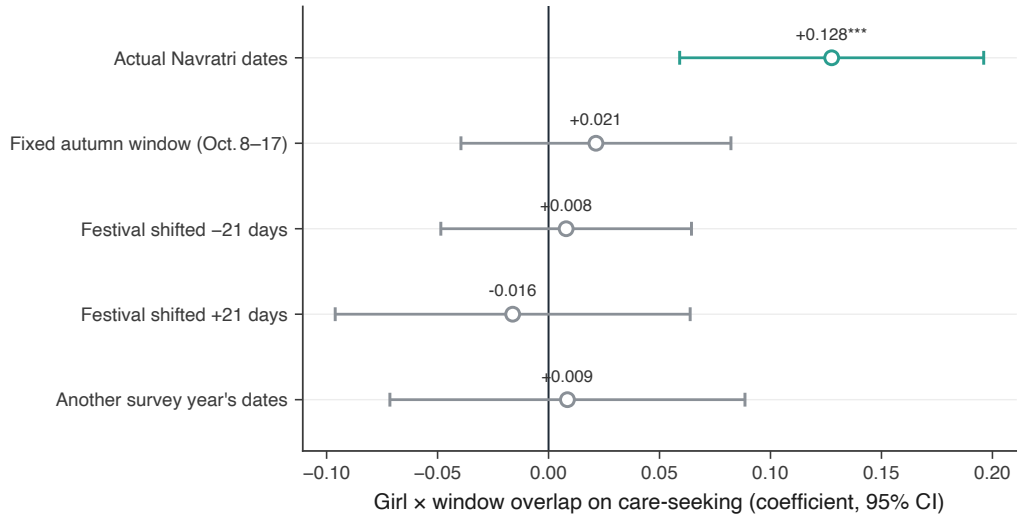
Notes: Young girls received for Kanya Pujan during the festival: veiled, marked with the goddess's vermilion, and fed the festival meal before anyone else. Photograph by Nvstudio989, Wikimedia Commons, CC BY-SA 4.0.

Appendix Figure 2: A Durga Puja Pandal: The Festival's Public Face



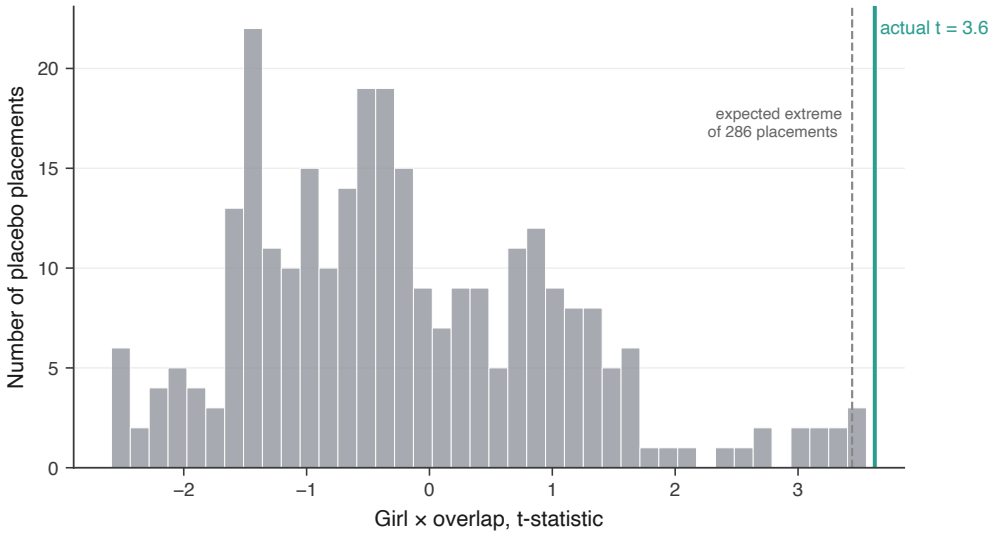
Notes: A pandal—the temporary structure built to house the goddess for the festival's days—lit for Durga Puja in West Bengal, with the evening crowd before it. The celebration takes place in shared streets and grounds, not inside temples. Photograph by Dew kodu pagli, Wikimedia Commons, CC BY-SA 4.0.

Appendix Figure 3: Festival or Season: The Displaced-Window Tests, Plotted



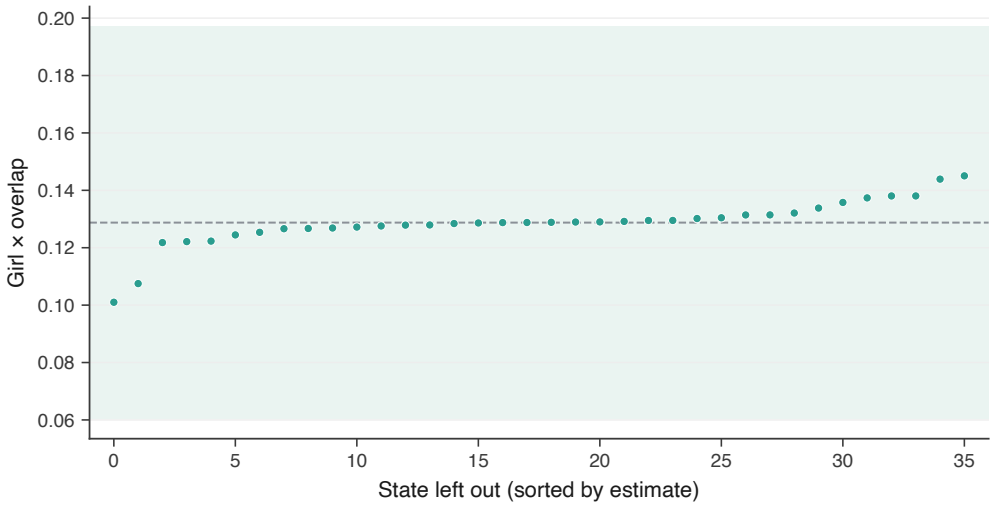
Notes: Coefficients on girl × window overlap from equation (2), each window entered alone (the estimates behind Appendix Table 8), with 95% confidence intervals clustered at the survey cluster. Only the festival's actual dates carry the effect; every displaced window is null. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Appendix Figure 4: Placebo in Time: Actual Estimate versus Random Festival Dates



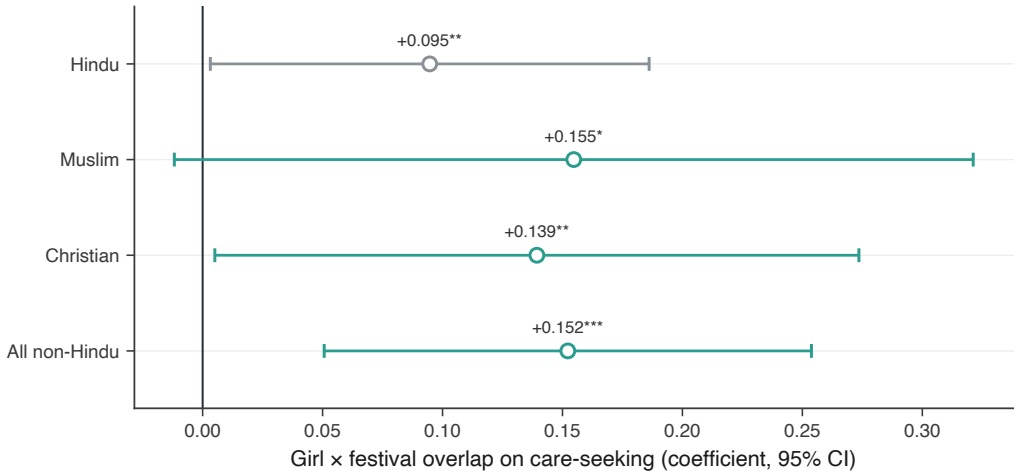
Notes: Each bar counts placements of a nine-day placebo window at one of the 286 feasible days of the year away from Navratri; for each placement the care-seeking regression of equation (2) is re-estimated and the girl × overlap t-statistic recorded. Because the placements exhaust the calendar, the test is exact rather than simulated. The teal line marks the actual festival ($t = 3.6$), the most extreme of all 287 possible placements, an exact randomization p of 0.003; no placement yields a coefficient as large as the actual estimate (0.110 at most, against 0.129, and the largest rivals are negative). The dashed line is the expected extreme of 286 draws given the placebo spread: the near-miss placebos, mid-July windows unrelated to any festival, sit at the noise frontier. The placebo t-statistics are more dispersed than a standard normal, which is why this randomization test accompanies the paper's analytic standard errors. It complements the displaced-window tests (Appendix Table 8), which isolate the festival from fixed seasonal factors within its own season. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Appendix Figure 5: Leave-One-State-Out Estimates



Notes: Each point is the girl × overlap estimate from equation (2), re-estimated dropping one state at a time and sorted by value. The dashed line is the baseline estimate; the shaded band is its 95% confidence interval. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Appendix Figure 6: The Effect by Religion



Notes: Coefficients on girl × festival overlap from equation (2), estimated separately by the household head's religion, with 95% confidence intervals clustered at the survey cluster; the last row pools all non-Hindu households. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

A.2 Appendix Tables

Appendix Table 1: Sharad Navratri in the Survey Years

Year	Festival begins	Vijayadashami (ends)
2015	October 13	October 22
2016	October 1	October 10
2019	September 29	October 8
2020	October 17	October 26
2021	October 6	October 15

Notes: Dates of Sharad Navratri (first day of the nine nights through Vijayadashami) in each year covered by NFHS-4 (2015–16) and NFHS-5 (2019–21) fieldwork, from the Hindu lunisolar calendar. Because the festival is lunar, its start moves across the Gregorian calendar from late September to mid-October. Fieldwork crossed the festival in 2016 and 2019. The 2020 festival fell within the survey's pandemic suspension of fieldwork, and the 2021 festival after fieldwork ended in May 2021, so those years contribute almost no exposed children; the dates are listed because the overlap construction spans every interview year. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Appendix Table 2: Balance and Selection, Care-Seeking Sample

	Festival window	Pre-festival	Adjusted	(s.e.)
A. Covariate balance: festival window vs. pre-festival window				
Girl	0.468	0.480	+0.001	(0.035)
Urban	0.164	0.168	-0.028	(0.032)
Mother's education (0–3)	1.562	1.536	+0.057	(0.067)
Wealth quintile (1–5)	2.469	2.356	+0.060	(0.089)
Mother's age (years)	27.399	26.701	-0.056	(0.459)
Child's age (years)	1.898	1.844	-0.053	(0.088)
B. Selection into the sick sample: incidence, type, and severity				
Illness incidence: girl × overlap			+0.0004	(0.0134)
Fever or cough incidence			+0.0013	(0.0126)
Diarrhea incidence			+0.0054	(0.0078)
Blood in stool (diarrhea cases)			+0.0004	(0.0442)
Chest symptoms (fever/cough cases)			+0.0113	(0.0353)

Notes: Panel A compares sick children whose two-week recall window overlaps Navratri (festival window) with those interviewed in the 28 days before the festival (pre-festival window). The first two columns report group means. Because the survey rotates through states over the calendar, the two windows sample different places, so "Adjusted" – the coefficient on a festival-window indicator with state, interview-month, and survey-round fixed effects – is the comparison the estimator in equation (2) makes. None of the six differs significantly from zero, the largest being mother's education at 0.057 (s.e. 0.067). Means are reported for all variables in this table; mother's education (0–3), household wealth quintile (1–5), and child's age (0–4) enter equation (2) as indicator dummies rather than as continuous variables. Panel B reports the coefficient on girl × overlap in a regression of an indicator for being sick on all children, with the full controls and fixed effects of equation (2); a zero means the pool of sick children is not differentially selected by sex near the festival; the four rows below repeat the test for the type of illness (incidence of fever or cough, and of diarrhea, on all children) and its severity (blood in stool among diarrhea cases; chest symptoms among fever or cough cases, the survey's markers of a more serious episode, asked of the relevant sick children). Neither the mix of illnesses nor their severity moves with exposure. Standard errors clustered at the survey cluster. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Appendix Table 3: Robustness of the Care-Seeking Estimate

	Girl × overlap	(s.e.)	p	N
Baseline	0.129***	(0.035)	0.000	102,708
Sample around the festival				
Interviews within ±60 days	0.151***	(0.037)	0.000	23,767
Interviews within ±45 days	0.156***	(0.038)	0.000	18,409
Interviews within ±30 days	0.144***	(0.040)	0.000	12,471
Dropping all festival-day interviews	0.089**	(0.041)	0.027	100,913
Usual residents only (visitors excluded)	0.135***	(0.035)	0.000	99,437
Interviews before March 2020 only	0.131***	(0.035)	0.000	90,950
Fixed-effects structure				
State × interview-month	0.132***	(0.035)	0.000	102,708
State × fieldwork-month	0.133***	(0.035)	0.000	102,708
State × fieldwork-month, gap free in each	0.147***	(0.055)	0.008	102,708
Alternative weighting, inference, and functional form				
Survey-weighted	0.124**	(0.059)	0.037	102,708
Clustered at state level	0.129***	(0.043)	0.003	102,708
Logit (log-odds)	0.577***	(0.156)	0.000	102,708

Notes: Each row re-estimates the girl × festival-overlap interaction of equation (2). Bandwidth rows restrict to interviews within the stated number of days of the festival. Because fieldwork thins during the festival itself (Figure 2), the next row drops every interview conducted on a festival day, keeping only the ordinary working days after it, whose recall windows still reach back into the festival; this removes 39 percent of the exposed children. The residents row drops mothers interviewed away from home, so the estimate cannot be carried by festival travel. The pre-COVID row keeps only interviews before March 2020; nearly all exposure predates the pandemic, so the estimate is unchanged. The estimate is smaller on this sample because it is drawn mostly from the second week of the event study, where the effect has already begun to decay, and the difference between the two groups is not significant (triple interaction 0.185, s.e. 0.125). The fixed-effects rows address the fact that the survey routes fieldwork through a state in a temporal block, so that a state and the timing of its interviews are linked: they replace the baseline's separate state and month effects with the state × calendar-month cell, then with the state × fieldwork-month block, and finally allow a separate girl coefficient in every such block, so that the interaction is identified only from day-to-day variation in exposure within one state's single month of fieldwork. The design's variation is within these blocks rather than across them: 99 percent of exposed children sit in blocks that also contain unexposed children. The last three rows add survey weights, cluster standard errors at the state level, and re-estimate the model as a logit (the reported coefficient is a log-odds, not a probability). All rows include the controls and fixed effects of the baseline. Standard errors clustered at the survey cluster unless noted, in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Appendix Table 4: Test of Selection on Unobservables (Oster 2019)

	R-max	$\beta^*(\delta=1)$	δ for $\beta^*=0$
Inputs			
Coefficient on girl × overlap, no controls	0.127		
Coefficient on girl × overlap, full controls	0.129		
R^2 , no controls	0.001		
R^2 , full controls	0.069		
Oster bound (most conservative)			
R-max = 1 (all variation)	1.000	0.154	-5.1

Notes: The no-controls regression includes the girl and overlap main effects and their interaction; the full-controls regression adds the household and child controls and the state, interview-month, and survey-round fixed effects of equation (2). $\beta^*(\delta=1)$ is the bias-adjusted interaction coefficient when selection on unobservables is exactly as strong as selection on the observables; δ is the relative strength of selection on unobservables that would be required to drive the coefficient to zero. R-max bounds the R^2 from a hypothetical regression on observables and unobservables together; the table uses the most conservative value, R-max = 1, which treats all of the residual variation in care-seeking as potential confounding. Because adding the observed controls leaves the coefficient essentially unchanged while the R^2 rises, δ is large in magnitude: driving the effect to zero requires selection on unobservables several times stronger than selection on the full set of observables, and of the opposite sign: the observed controls move the coefficient slightly away from zero, not toward it, so δ is negative. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Appendix Table 5: Gender-Specific Seasonality and Son-Preference Robustness

	Girl × overlap	(s.e.)	p	N
A. Flexible gender-specific seasonal controls				
Baseline	0.129***	(0.035)	0.000	102,708
+ girl × interview-month	0.151***	(0.054)	0.005	102,708
+ girl × day-of-year (cubic)	0.140***	(0.037)	0.000	102,708
B. Birth order and sibling sex composition				
+ birth order, girl × birth order	0.130***	(0.035)	0.000	102,708
+ no. of brothers, girl × brothers	0.128***	(0.035)	0.000	102,708
+ both sets and their girl interactions	0.128***	(0.035)	0.000	102,708
C. Balance: son-preference variable on girl × overlap				
Birth order (dep. var.)	-0.113	(0.091)	0.213	102,708
No. of brothers (dep. var.)	-0.047	(0.066)	0.477	102,708

Notes: All rows start from the baseline care-seeking specification of equation (2) and add the stated terms. Panel A adds a girl-specific seasonal pattern: interactions of girl with interview-month dummies, then with a cubic in the day of the year; the festival overlap remains identified because the festival switches on and off inside the month, at a daily scale no smooth seasonal pattern matches. Panel B adds birth order and the number of brothers in the household, each on its own and interacted with girl. Panel C regresses these son-preference variables on the full design and reports the coefficient on girl × overlap, which should be zero if the treatment does not shift family composition. Standard errors clustered at the survey cluster in parentheses. * p<0.10, ** p<0.05, *** p<0.01. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Appendix Table 6: Care-Seeking by Margin: Reporting Hardness and the Destinations of Care

	Mean	Girl × overlap	(s.e.)	p	N
A. The reporting ladder: from easy to hard to misreport					
Sought any source (incl. pharmacy, shop)	0.892	0.050*	(0.028)	0.075	102,708
Taken to a medical provider (baseline)	0.623	0.129***	(0.035)	0.000	102,708
B. The three destinations of care: where the movement comes from					
No care at all	0.108	-0.050*	(0.028)	0.075	102,708
Informal source only	0.269	-0.079***	(0.029)	0.006	102,708

Notes: Each row re-estimates the girl × festival-overlap interaction of equation (2), with the same controls and fixed effects, on a different definition of the outcome, ordered from the easiest to the hardest to misreport, on the full sample of sick children. The first row counts a positive answer if the mother reports seeking care from any source at all, including the pharmacy, a shop, or a relative. The second row is the paper's baseline outcome, the DHS summary measure of whether the child was taken to a medical provider, a public facility or a private hospital or doctor, which already excludes the pharmacy and informal sources; taking a child to a facility is a costly, concrete act rather than a verbal claim of having sought advice. "Mean" is the outcome mean. A pure reporting story would weaken as the outcome becomes more concrete; the interaction does not. Panel B splits care into its three mutually exclusive destinations: no care at all, an informal source only, and a medical provider (the baseline row of Panel A). The three girl × overlap responses sum to zero by construction, so the two rows show where the additional medical care comes from: about three-fifths from families leaving informal-only care and the rest from families who would have sought nothing. Consistent with this, among families who sought care from some source (89 percent of sick children), the probability that the source was a medical provider rises by 0.101 (s.e. 0.034). Standard errors clustered at the survey cluster in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Appendix Table 7: Which Festivals Move the Gap: Deity, Celebration, and Content

Festival	Deity (sex)	Girl × overlap (pp)	Exposed children
A. The effect: the autumn goddess festival			
Sharad Navratri	Durga (female)	+12.76***	4,127
		(3.49)	
B. Large public festivals for male gods			
Ganesh Chaturthi	Ganesha (male)	-2.82	5,630
		(2.79)	
Janmashtami	Krishna (male)	-3.83	5,387
		(2.91)	
Ram Navami	Rama (male)	+1.92	7,769
		(2.54)	
C. Other goddesses, and the goddess without her public celebration			
Diwali	Lakshmi (female)	-1.67	2,800
		(4.47)	
Chaitra Navratri	Durga (female)	+0.05	8,045
		(2.45)	

Notes: Each pair of rows is the coefficient on girl × festival overlap (in percentage points) from a separate care-seeking regression of the form of equation (2), with the same controls and state, interview-month, and survey-round fixed effects; standard errors clustered at the survey cluster in parentheses. Every regression runs on the same 102,708 sick children; the last column reports how many of them had decision windows touching each festival. Single-day festivals are given nine-day windows (the date plus or minus four days). Panel B: three large public festivals for male gods, each estimated on as many or more treated children than Navratri, with confidence intervals that exclude the Navratri estimate. Panel C: Diwali (the goddess of wealth) and the spring Navratri (the same Durga, worshipped at home without the public celebration). Only the autumn festival of Durga, whose public celebration is built around a daughter, narrows the gap. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Appendix Table 8: Festival or Season? Displacing the Festival Window

	Care-seeking
Each nine-day window entered alone (girl × overlap)	
Actual Navratri dates (baseline)	+0.1276***
	(0.0349)
Fixed autumn window (grand-mean dates)	+0.0214
	(0.0310)
Placebo: festival −21 days	+0.0079
	(0.0288)
Placebo: festival +21 days	-0.0162
	(0.0408)
Placebo: another survey year's dates	+0.0085
	(0.0408)
Observations	102,708

Notes: Each cell is the coefficient on girl × window overlap from a care-seeking regression (linear probability; controls and state, interview-month and survey-round fixed effects; standard errors clustered at the survey cluster in parentheses), with the listed nine-day window entered alone. The baseline uses each year's actual Sharad Navratri dates. The remaining rows displace the window within the season: to the festival's grand-mean calendar position (about October 8–17, applied to every year), three weeks before and after the actual festival, and to another survey year's festival dates placed in the child's own year. Only the actual festival days carry the effect; every displaced window is null. These are within-season placement tests: fieldwork crosses the festival in two survey years, 2016 and 2019, whose festivals fell two days apart, so the tests pin the effect to the festival's exact days. They complement the exhaustive placebo-in-time exercise (Appendix Figure 4). * $p<0.10$, ** $p<0.05$, *** $p<0.01$. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Appendix Table 9: Not Social Pressure: Watched and Unwatched Households Respond Alike

	Girl × overlap	(s.e.)	N
A. Household surveillance			
Daughter-in-law of the household head	+0.135**	(0.067)	37,260
Wife of the household head	+0.133***	(0.050)	47,355
Difference (girl × overlap × daughter-in-law)	+0.001	(0.083)	84,615
B. Embeddedness in the village			
Arrived within two years	+0.212**	(0.094)	15,666
Long-time residents	+0.119***	(0.039)	83,771
Difference (girl × overlap × recent arrival)	+0.093	(0.104)	99,437

Notes: Each row estimates equation (2) on the listed subsample; the difference rows report the triple interaction. If the festival worked through neighbors' or in-laws' eyes, the effect should be larger where the household is watched more closely: for a daughter-in-law under her mother-in-law's roof, and for long-settled families embedded in the village. It is not. Residence duration is the mother's years in the current place (visitors excluded). Standard errors clustered at the survey cluster. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

Appendix Table 10: Revealed Son Preference Does Not Gate the Effect

	Girl × overlap	(s.e.)	N
Household already has a son	+0.116**	(0.049)	45,819
Only daughters so far	+0.138***	(0.050)	56,889
Difference (girl × overlap × has a son)	-0.023	(0.070)	102,708
Only daughters and wants another child	+0.245***	(0.062)	34,513
All other households	+0.078*	(0.043)	68,195
Difference (girl × overlap × wants a son)	+0.168**	(0.076)	102,708

Notes: Sons and daughters are counted from the mother's full birth history (children at home, elsewhere, and deceased), excluding the index child, so the groupings are fixed by births that predate the festival. Equation (2) estimated on each subsample; the difference row reports the triple interaction. Households that already have a son respond as much as households with only daughters, and the effect is largest in households with only daughters that report wanting another child—those under the most pressure for a son, whose daughters' care is least assured. Standard errors clustered at the survey cluster. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. National Family Health Survey (NFHS) rounds 4 and 5, Kids' Recode.

B Data Construction

B.1 Dietary Diversity

I construct the dietary-diversity count from the survey's twenty-four-hour food recall, following the World Health Organization's standard for children aged six to twenty-three months (World Health Organization and UNICEF, 2021). For each item on a list of foods, the mother reports whether the child ate it in the day and night before the interview. I group these items into the seven solid-food groups the standard defines: grains, roots, and tubers; pulses, nuts, and seeds; dairy; flesh foods; eggs; vitamin-A-rich fruits and vegetables; and other fruits and vegetables. A group is coded one if the mother reports any item within it, so the count records the variety of a day's diet rather than its quantity.

Several survey items map to a single group. Flesh foods pool poultry, other meat, organ meats, and fish; dairy pools fresh or powdered milk, infant formula, cheese, and yogurt; grains include fortified baby food alongside bread, noodles, and tubers; and the vitamin-A-rich group pools dark green leafy vegetables, orange-fleshed vegetables such as carrots and pumpkin, and fruits such as mango and papaya. The eighth group is breast milk, coded one if the child is still nursing, so a nursing child counts one group higher than the same child weaned onto the same solids. Children whose mothers were asked none of the food questions are dropped. The resulting sample is 140,859 children aged six to twenty-three months across the two rounds.

C Additional Analyses

C.1 Back-of-the-Envelope Computation

The introduction reports that a single Navratri moves roughly 600,000 sick girls to a medical provider who would otherwise not have been taken. I obtain this figure by multiplying three quantities: the number of girls ill when the festival arrives, the share of a girl's decision that the festival reaches, and the effect the festival has where it reaches.

Girls ill during the festival. India had about 57 million girls under five in 2019 (United Nations World Population Prospects). In the survey, 21 percent of girls are reported ill with fever, cough, or diarrhea in a two-week window—an illness that puts a care decision in front of the family. So the number of girls facing such a decision around the festival is about 57 million $\times$ 0.21 $\approx$ 12 million.

How much of the decision the festival reaches. A girl's illness is recorded over the fourteen days before her interview, and the festival lasts nine, so the festival rarely covers a girl's whole decision window; it overlaps part of it. Among the girls the festival reaches at all, it covers 38 percent of the window on average (the mean overlap among exposed children).

The effect where the festival reaches. I use the girl-specific estimate from equation (2), the survey-weighted coefficient on girl $\times$ overlap, 0.124. This is the effect on girls net of anything the festival does to all children at once, so the count reflects girls moved by the festival's attention to daughters rather than by any general festival-time change in care. Because the same coefficient enters as a per-window effect, it is already scaled to the fourteen-day window that the coverage term measures.

Multiplying, 12 million $\times$ 0.38 $\times$ 0.124 $\approx$ 570,000, which I report as roughly 600,000. The computation is deliberately coarse: it treats the national population with the survey's rates, ignores variation in illness and coverage across regions, and rounds at each step. It is meant to convey the order of magnitude—that the festival moves girls to care in the hundreds of thousands over its nine days—not a precise count.