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Disrupted Routines: Natural Disasters and the Gendered Allocation of Household Work in the United States

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Abstract

A large literature documents the consequences of U.S. disasters for migration, wealth, employment, and health, but has largely treated the household as an undifferentiated unit, leaving open how the work of getting through a disaster is distributed among the people inside it. We address this by linking county-level hazard data from the Spatial Hazard Events and Losses Database for the United States (SHELDUS) to American Time Use Survey (ATUS) time diaries for married women and men, 2003–2021, and comparing daily unpaid household work in counties that experienced a high-fatality disaster with counties that did not. Married women in affected counties report approximately 18.5 fewer daily minutes of unpaid work, concentrated in housework and childcare, while married men's unpaid work is statistically unchanged and their housework rises by about 8 minutes. A pooled interaction model confirms the gender difference is statistically significant. With only 6 of 47 state clusters treated, the women's estimate survives a wild cluster bootstrap and randomization inference; the pooled estimate is less robust. The 68-minute baseline gender gap narrows by roughly 20 minutes, or about 30 percent, and the effect does not persist into the following year. Men also lose substantially more paid work time than women. Disasters therefore reorganize household time in gender-specific ways, with implications for how recovery and relief programs are designed.

Introduction

Natural disasters are a recurring feature of American life, and their frequency and cost are rising. A substantial body of population–environment research has established that these events reshape households in durable ways: they drive migration and residential instability (Elliott, 2015; Elliott & Howell, 2017; Sheldon & Zhan, 2022; Spring, 2025), widen wealth inequality (Howell & Elliott, 2019), depress local employment (Belasen & Polachek, 2008; Groen & Polivka, 2008), damage household finances (Gallagher & Hartley, 2017), and affect health and mortality (Deryugina & Molitor, 2020; Leppold et al., 2022). Recovery from these shocks is unequal, sorting along lines of race, class, and place (Elliott & Pais, 2006; Finch et al., 2010; Fussell et al., 2010; Klinenberg, 2002; Tierney, 2025).

This literature has told us a great deal about what happens *to* households after a disaster. It has told us much less about what happens *inside* them. Disasters generate an enormous volume of unpaid work — clearing and repairing damaged homes, securing food and water under degraded conditions, caring for children whose schools have closed and for adults who are ill or displaced, navigating aid bureaucracies. Almost none of this work appears in official recovery accounting, and the quantitative literature on U.S. disasters, which typically observes households as units, cannot say who performs it. That is a substantive gap rather than a technical one: the distribution of recovery labor determines who bears the burden of adaptation, and it is a dimension of disaster impact that current data infrastructures largely miss.

Feminist disaster scholarship has long argued that this work falls disproportionately on women, and has documented the mechanisms through detailed qualitative and case-study research (Bradshaw, 2013; Enarson, 1998; Enarson & Morrow, 1998; Fothergill, 2004). That work is rich in process and meaning but limited in generalizability. A complementary strand, mostly in low- and middle-income settings, has used household survey data to estimate how weather and disaster shocks shift labor allocation by gender (Afridi et al., 2022; Akter, 2021; Boyd, 2023). What has been missing is nationally representative evidence, for a high-income setting, on how a major disaster changes the daily allocation of unpaid work between women and men.

We provide such evidence by linking county-level hazard data from the Spatial Hazard Events and Losses Database for the United States (hereafter SHELDUS) to time diaries from the American Time Use Survey (hereafter ATUS) for married women and men over 2003–2021. The linkage follows an approach now established in population–environment research, in which SHELDUS county-level hazard exposure is merged to nationally representative household panel or survey data (Spring, 2025). Our contribution is to bring time-diary data to that design, which allows us to observe not merely whether households are affected but how their members reallocate the hours of the day.

Our research question is whether the gender gap in unpaid household work widens or narrows after a major disaster, and through which activities any change operates. We find that it narrows, substantially. Married women residing in counties that experienced a high-fatality disaster report approximately 18.5 fewer minutes of unpaid work per day than otherwise comparable women in unaffected counties, while married men's unpaid work is statistically unchanged and their housework rises by about 8 minutes. Because married women in our sample perform about 68 more minutes of unpaid work per day than married men, this asymmetry narrows the gap by roughly 20 minutes, or about 30 percent. The change is concentrated in housework and childcare, does not persist into the following year, and is accompanied by a much larger loss of paid work time among men.

We are explicit throughout about what these findings do and do not establish. They do not show that disasters are good for gender equality: the gap narrows partly because women lose time they would otherwise have spent caring for their households and homes, and partly because men lose employment. Both are costs. Nor can we observe couples, since the ATUS interviews one respondent per household; we characterize the time use of married women and married men in affected communities, not how particular spouses negotiate with one another. We return to both points at length.

Background

Household consequences of disasters in the United States

Research on U.S. disasters has advanced rapidly over the past two decades, aided by the availability of county-level hazard data. Studies have documented effects on residential mobility and migration (Curtis et al., 2015; Elliott, 2015; Sheldon & Zhan, 2022), on the geography of neighborhood distress (Elliott & Howell, 2017), on wealth and its distribution (Howell & Elliott, 2019), on household finances and tax-reported income (Deryugina et al., 2018; Gallagher & Hartley, 2017), on local labor markets (Belasen & Polachek, 2008; Groen & Polivka, 2008), and on mortality and health (Deryugina & Molitor, 2020; Leppold et al., 2022). Reviews of the economic literature situate these findings within a broader accounting of disaster costs (Botzen et al., 2019; Boustan et al., 2020).

A parallel body of work emphasizes that these consequences are distributed unequally. Race, socioeconomic position, tenure, and place shape who is exposed, who can leave, who returns, and who recovers (Elliott & Pais, 2006; Finch et al., 2010; Fussell et al., 2010; Klinenberg, 2002; Spring, 2025; Tierney, 2025). This is now among the most robust findings in the field.

What this literature does not do is look inside the household. Its outcome measures — migration, income, wealth, employment, mortality — are either household-level aggregates or individual outcomes that do not speak to the division of labor among members. The result is that a large category of disaster-induced work, the unpaid labor of maintaining a household under adverse conditions, remains essentially unmeasured in nationally representative U.S. data. Our aim is to bring that work into view.

Gender and disaster

Feminist disaster scholarship supplies both the motivation and the expectation. Women face elevated mortality risk in many disaster contexts, particularly where their social and economic position constrains mobility and access to information (Neumayer & Plümper, 2007). Their caregiving responsibilities typically expand during response and recovery, as they absorb the work of feeding, sheltering, and caring for household members and neighbors under degraded conditions (Bradshaw, 2013; Enarson, 1998; Enarson & Morrow, 1998). Fothergill's (1996, 2004) research on gender, class, and family following flooding in Grand Forks documents in detail how recovery labor is distributed and how it interacts with existing inequalities.

Quantitative work in this vein has largely developed in low- and middle-income settings, where agricultural livelihoods make labor reallocation directly observable. Akter (2021) finds that catastrophic flooding in Pakistan shifts the gender division of labor; Afridi et al. (2022) show that drought-induced production shocks in Indian agriculture generate gendered labor responses; and Boyd (2023), in this journal, links rainfall variability to mothers' time use and child nutrition in rural Uganda. These studies establish that environmental shocks reorganize household time along gendered lines. Whether they do so in a high-income, largely non-agricultural setting, and in which direction, is an open question.

The gendered division of unpaid work

Answering it requires a baseline. In the United States, as across industrialized societies, women perform substantially more unpaid domestic work than men. The gap narrowed considerably between the 1960s and the 1990s but the convergence slowed thereafter, and the remaining asymmetry has proven durable (Altintas & Sullivan, 2016; Bianchi et al., 2000, 2012; England, 2010; Sayer, 2005). Reviews of several decades of research find that women continue to perform the majority of routine domestic work even when employed full time and even when they out-earn their partners (Coltrane, 2000; Lachance-Grzela & Bouchard, 2010).

Three families of explanation compete. *Time availability* accounts hold that domestic work is allocated to whoever has hours to spare. *Relative resources* accounts hold that the higher-earning partner purchases exemption from undesirable tasks. *Gender ideology* accounts hold that the division of domestic labor is itself a site at which gender is produced and displayed, so that housework is performed and avoided in ways that affirm gendered identity rather than in ways that maximize efficiency (Brines, 1994; Greenstein, 2000; West & Zimmerman, 1987).

Household production theory (Becker, 1965, 1981) supplies the analytical apparatus we use — time is a scarce input, and allocations respond to changes in constraints — and its unitary treatment of the household has been relaxed by collective and bargaining models that give spouses distinct preferences (Chiappori, 1992; Lundberg & Pollak, 1993; Manser & Brown, 1980; McElroy & Horney, 1981). Neither tradition, however, explains why the resulting specialization is so consistently gendered, or why it persists where relative earnings would predict otherwise. Brines (1994) and Greenstein (2000) show that wives who out-earn their husbands do not reduce their housework proportionately. That residual is what accounts of gender as a social structure are designed to address (Risman, 2004), and it carries an implication our design can test: if the division of domestic labor is reproduced partly through the stability of daily routine and institutional arrangement, it should be responsive to events that disrupt them.

Competing expectations

Two other literatures inform what we should expect. Research on the COVID-19 pandemic found that domestic and care work rose for both women and men but rose more for women, widening the relative gap (Carlson et al., 2021; Collins et al., 2021; Del Boca et al., 2020). Research on men's job loss finds the opposite direction: wives increase labor supply when husbands become unemployed (Lundberg, 1985), and unemployed men increase housework, though by far less than the time freed from paid work (Gough & Killewald, 2011).

These point in opposite directions, and the difference is instructive. The pandemic sharply *raised* the volume of domestic and care work to be done, primarily through school and childcare closure, and that additional work was allocated along existing lines. Job loss *frees* time without raising domestic demand. A severe natural disaster plausibly does both: it generates recovery work while simultaneously interrupting the routine domestic work of ordinary life, closing schools and workplaces, and displacing paid employment. Which effect dominates is an empirical question, and it is the question we take up. If disasters primarily add caregiving and reconstruction demands allocated along conventional lines, the gender gap should widen. If they primarily interrupt the routines through which the conventional division is enacted, while displacing men from paid work, the gap should narrow.

Data and Methods

Time use data

Time-use data come from the ATUS, conducted annually by the U.S. Bureau of Labor Statistics. The ATUS draws its sample from households completing their final month of the Current Population Survey (CPS). One individual aged 15 or older per household is randomly selected to complete a single 24-hour time diary, in which a trained interviewer guides the respondent sequentially through the previous day, recording each activity, its duration, and its location.

Because activities are reconstructed for a designated day rather than estimated in the abstract, time-diary measures are generally more accurate than stylized survey questions asking about typical weekly hours, which tend to overstate housework and other socially salient activities (Altintas & Sullivan, 2016; Bianchi et al., 2000). Two features nonetheless matter here. First, because each respondent contributes a single diary day, day-to-day idiosyncrasies introduce measurement error in individual time use; being unrelated to a county's disaster status, this should widen standard errors rather than bias estimates. Second, a disaster could in principle change how respondents *report* time rather than how they allocate it. We have no direct test for such differential reporting and flag it as a caveat, noting only that our effects are concentrated in routine, easily recalled activities.

A more fundamental constraint is that the ATUS interviews one respondent per household. We therefore cannot observe couples. Nothing in our data reveals how spouses negotiate or jointly reallocate time, and we make no claims of that kind. What we observe is the time use of married women and married men living in disaster-affected and unaffected communities.

Hazard data

Data on natural hazards come from SHELDUS (CEMHS, 2025), which provides county-level information on eighteen hazard types: avalanche, coastal hazard, drought, earthquake, flood, fog, hail, heat, hurricane and tropical storm,

landslide, lightning, severe thunderstorm, tornado, tsunami and seiche, volcano, wildfire, wind, and winter weather. SHELDUS also reports aggregated property and crop damage, injuries, and fatalities.

We link the two sources by the respondent's county of residence and survey year, assigning each ATUS respondent the hazard experience of their county in the year of interview. Because the ATUS identifies county of residence only for respondents in sufficiently populous counties, the analytic sample is restricted to those for whom county is available; it covers 403 counties. Following thresholds used in prior county-level disaster research (Boustan et al., 2020), we classify a hazard event as *major* if it resulted in 25 or more fatalities in the respondent's county during the year.

We flag the temporal resolution of this linkage as the most serious measurement limitation of the study. Exposure is measured at the county-year level, whereas time use is measured for a single diary day. A respondent interviewed early in the year is assigned a disaster that may have occurred later that year, and a respondent in a county struck late in the previous year is coded as unexposed. Some treated diaries therefore precede the event and some control diaries follow a recent one. Both forms of misclassification dilute the contrast between treated and control observations and attenuate estimates toward zero, so our estimates should be read as conservative lower bounds on the within-year response. Defining exposure at finer temporal resolution using the event dates recorded in the SHELDUS event files, and adding post-event lags, would sharpen the timing of treatment and permit estimation of adjustment dynamics. We regard this as the most important extension of the present analysis.

Measures

We define *paid work* as income-generating activity and *unpaid work* as non-income-generating productive activity, most of which occurs within the household and some in the neighborhood and wider community. Following established practice in time-use research, we exclude leisure, socializing, and personal care from unpaid work (Bianchi et al., 2000; Sayer, 2005).

Unpaid work is measured as total daily minutes across four components, constructed from the ATUS activity lexicon. *Household activities* comprise the whole of ATUS major activity category 02: housework; food and drink preparation, presentation, and clean-up; interior and exterior maintenance, repair, and decoration; lawn, garden, and houseplant care; animal and pet care; vehicle maintenance and repair; appliance, tool, and toy maintenance; and household management. *Childcare* comprises caring for and assisting household children and activities related to their education and health (codes 0301–0303). *Adult care* comprises caring for and helping household adults (codes 0304–0305). *Community service* comprises caring for and assisting non-household children and adults, including activities related to their education and health (major category 04). This aggregation follows the scheme used in the established time-use literature on the domestic division of labor (Bianchi et al., 2000; Hofferth et al., 2020; Sayer, 2005). Table 9 reports the full crosswalk between ATUS activity codes and our categories.

Table 9
Crosswalk between ATUS activity codes and analytic categories

Analytic category	ATUS activity codes	Contents
Household activities	02 (all two-digit subcategories 0201–0299)	Housework; food and drink preparation, presentation, and clean-up; interior maintenance, repair, and decoration; exterior maintenance, repair, and decoration; lawn, garden, and houseplant care; animal and pet care; vehicle maintenance and repair; appliance, tool, and toy maintenance; household management
Childcare	0301, 0302, 0303	Caring for and assisting household children; activities related to household children's education; activities related to household children's health
Adult care	0304, 0305	Caring for household adults; helping household adults
Community service	04 (all two-digit subcategories 0401–0499)	Caring for and helping non-household children and adults, including activities related to their education and health
Total unpaid work	Sum of the four categories above	—
Paid work	0501, 0502	Working; work-related activities
Sports and exercise	13	Sports, exercise, and recreation
Religious activities	14	Religious and spiritual activities
Volunteering	15	Volunteer activities
Socializing and leisure	12	Socializing, relaxing, and leisure

For analyses of other margins, *paid work* comprises working and work-related activities (codes 0501–0502); *sports and exercise* is major category 13; *religious and spiritual activities* is category 14; *volunteering* is category 15; and *socializing and leisure* is category 12.

Analytic sample

The analytic sample comprises 78,555 diary days, 44,039 from women and 34,516 from men. We restrict the sample to respondents aged 18–64 who are married with a spouse aged 18–64 present in the household, and exclude diary days falling on public holidays.

The sample runs from 2003, the first year of the ATUS, through 2021. The endpoint reflects the coverage of the linked county-level hazard extract available at the time of analysis rather than a substantive choice. The window includes the first two pandemic years, during which both time use and ATUS field procedures were unusual.

Table 1, Panel C reports the distribution of disaster exposure, and the concentration is important enough to state plainly. Of the 78,555 diary days in the analytic sample, 833 (1.1%) are coded as treated — 457 of women's observations and 376 of men's. These are drawn from 9 counties and 16 county-years, located in 6 of the 47 states represented in the sample.

Table 1
Mean daily minutes of unpaid work, by gender

Panel A: All respondents	**Women and men**	**Women**	**Men**	**Gender gap**
Total unpaid work	156.92	186.72	118.89	67.83
Household activities	118.92	141.31	90.35	50.95
Childcare	28.11	34.90	19.45	15.44
Adult care	1.66	1.75	1.55	0.20
Community service	8.23	8.77	7.54	1.23
N	78,555	44,039	34,516	
Panel B: Respondents with children under 18				
Total unpaid work	216.22	268.67	155.66	113.01
Household activities	131.95	166.33	92.26	74.06
Childcare	78.55	96.49	57.84	38.65
Adult care	1.29	1.23	1.37	−0.13
Community service	4.42	4.62	4.19	0.43
N	20,798	11,146	9,652	
Panel C: Disaster exposure				
Treated diary days	833	457	376	
Share of diary days treated (%)	1.06	1.04	1.09	
Treated counties (of 403)	9	—	—	
Treated county-years	16	—	—	
States with ≥ 1 treated county-year (of 47)	6	—	—	
Share of treated days in 3 largest treated counties (%)	93.5	—	—	

The composition of that treated group bears directly on interpretation. Three metropolitan counties account for 93.5% of treated diary days: Clark County, Nevada (364), Maricopa County, Arizona (253), and Philadelphia County, Pennsylvania (162). In each, the qualifying county-years are high-mortality extreme-heat episodes with modest property losses; median property damage across those observations is roughly $4.7 million. The remaining 6.5% of treated diary days come from the destructive events more usually associated with the word disaster — Hurricane Katrina in Orleans and Jefferson Parishes in 2005, Hurricane Sandy in Richmond County in 2012, and the Camp and Carr wildfires in Butte and Shasta Counties in 2018 — where median property damage is roughly $2.6 billion, three orders of magnitude larger, but where the ATUS contributes only a handful of diary days each. A fatality threshold selects events that killed many people, and in the United States over this period those are disproportionately urban heat waves rather than events that destroy housing stock.

We therefore describe what follows as estimates of how household time allocation differs in county-years of high disaster mortality, and we return in the discussion to what this composition implies for interpretation and for external validity.

Two exclusions warrant comment. We restrict attention to married respondents with a spouse present because our question concerns the division of domestic labor within couple households; consequently our findings do not speak to single, cohabiting, or separated adults, for whom the constraints imposed by a disaster may differ considerably. We also exclude same-sex married couples, reflecting the small number of such couples in the ATUS over much of the study period rather than any assumption that domestic allocation is a heterosexual phenomenon. Both are real limitations.

A measurement point about our central variable: the ATUS records respondent sex as a binary administrative category inherited from the CPS. We use the terms *women* and *men* throughout and analyze differences between these categories as evidence about gendered social organization, but our data cannot distinguish sex assigned at birth from gender identity and cannot represent nonbinary respondents.

Analytic strategy

We estimate the association between major disaster exposure and daily minutes of unpaid work using ordinary least squares:

$$Y_{it} = \beta_0 + \beta_1 \, \text{Disaster}_{ct} + \mathbf{X}_i'\gamma + \delta_{d(t)} + \eta_{m(t)} + \tau_{y(t)} + \theta_{s(i)} + \varepsilon_{it} \quad (1)$$

where Y_{it} is the time individual i spends on unpaid work on diary day t, and Disaster_{ct} is a binary indicator equal to one if a major disaster occurred in respondent i's county of residence c during the year containing diary day t. The coefficient β_1 compares time use of individuals in disaster-affected counties with those in unaffected counties in the same year. The vector $\mathbf{X}_i$ collects individual controls, and δ, η, τ, and θ denote day-of-week, month, year, and state fixed effects.

In the most parsimonious specification, $\mathbf{X}_i$ includes age, age squared, sex (in the pooled sample), race and ethnicity indicators, and five birth-cohort-by-decade indicators. Our preferred specification adds education, number of children, and marital duration. Fixed effects absorb day-of-week differences in time use, seasonality in domestic time allocation, national macroeconomic shocks, and time-invariant differences in state labor and social policy.

We estimate Eq. 1 separately for women, for men, and for the pooled sample, and also estimate a pooled model interacting the disaster indicator with a female indicator. This interaction provides a direct significance test for the gender difference, which comparing two separately estimated coefficients does not.

Two choices require justification. First, our baseline includes state rather than county fixed effects. Because the disaster indicator varies at the county-year level, county fixed effects absorb a large share of the variation the design exploits; we report them as a robustness check and note in advance that the substantive conclusion is unchanged. Second, our baseline estimates are unweighted, and we report estimates weighted by ATUS final sampling weights, rescaled to mean one within survey year, as a robustness check; the weighted women's estimate is slightly larger in magnitude and remains significant at the 1% level.

Following Bertrand et al. (2004), who show that serial correlation can severely understate standard errors when treatment varies at a more aggregate level than the unit of observation, we cluster standard errors at the state level, and also report county-level and two-way (county-by-year) clustering.

Clustering addresses within-group correlation but not the separate problem created by a small number of *treated* groups. Because a 25-fatality county threshold is restrictive, only 6 of the 47 state clusters contain a treated county-year, and asymptotic cluster-robust inference is known to over-reject severely in that setting regardless of the total number of clusters (Conley & Taber, 2011; MacKinnon & Webb, 2017). We therefore report the number of treated clusters alongside the total throughout, and we supplement conventional cluster-robust p-values with two procedures designed for this situation. The first is a wild cluster restricted bootstrap-t with Rademacher weights, imposing the null and resampling by state cluster over 9,999 replications (Cameron et al., 2008). The second is randomization inference: we reassign treatment at the county-year level, drawing 16 treated county-years at random from the roughly 4,200 in the sample, and compare the observed t-statistic with the resulting reference distribution over 4,999 draws. We treat these, rather than the analytic p-values, as the basis for judging precision.

Equation 1 identifies an association between residing in a disaster-affected county and daily time use, net of the controls and fixed effects described. Counties that experience high-fatality disasters may differ from those that do not in ways our controls do not capture, and we do not claim a causal estimate.

Results

The baseline gender gap

Before examining disasters we establish the baseline. Table 1 reports mean daily minutes of unpaid work by gender, for the full sample and for respondents with children under 18.

Married women in our sample spend approximately 68 more minutes per day on unpaid work than married men. Most of this reflects household activities, where women spend 51 minutes more, and childcare, where women spend 15 minutes more. Among respondents with children under 18 the gap is considerably larger: 113 minutes, comprising 74 minutes in household activities and 39 minutes in childcare. Adult care and community service contribute negligibly.

Figures 1 and 2 trace the gap over the study period. In 2003 women spent 73 more minutes per day on unpaid work than men; the gap declined to 64 minutes by 2011 and has since fluctuated between 64 and 68 minutes. The long-run decline is driven by housework and childcare, while the gaps in adult care and community service show no clear trend. The pattern matches the broader literature: substantial historical convergence, followed by stalling at a still-large residual (Bianchi et al., 2012; England, 2010).

Disasters and unpaid work

Table 2 presents estimates of Eq. 1. Panels report the pooled sample, women only, and men only; column 1 gives the parsimonious specification and column 2 our preferred specification with the full control set. Estimates are stable across the two.

Table 2
Major disasters and unpaid work, by gender

Major disaster	All (1)	All (2)	Women (3)	Women (4)	Men (5)	Men (6)
	−8.61* **	**−8.79*** **	**−17.85*** **	**−18.48*** **	**1.75**	**1.82**
	(2.76)	(2.76)	(3.97)	(3.90)	(4.29)	(4.24)
Observations	78,555	78,555	44,039	44,039	34,516	34,516
Treated observations	833	833	457	457	376	376
State clusters (total / treated)	47 / 6	47 / 6	47 / 6	47 / 6	47 / 6	47 / 6
Wild cluster bootstrap *p*	—	.062	—	.024	—	.706
Randomization inference *p*	—	.023	—	.003	—	.715
95% bootstrap-t interval	—	−17.9, 0.4	—	−35.8, − 1.2	—	−9.0, 12.6
Additional controls	No	Yes	No	Yes	No	Yes
Outcome mean	156.9	156.9	186.7	186.7	118.9	118.9

In the pooled sample, residents of counties affected by a major disaster report 8.79 fewer minutes of unpaid work per day than residents of unaffected counties ($p < .01$). This aggregate conceals a pronounced gender asymmetry. Among women the reduction is more than twice as large, at 18.48 minutes per day ($p < .01$). Among men the estimate is small, positive, and statistically indistinguishable from zero (1.82 minutes). The aggregate effect is thus driven almost entirely by women.

Placing Table 2 alongside Table 1 gives the implication. Because disaster exposure is associated with 18.48 fewer minutes of women's unpaid work and 1.82 more minutes of men's, the implied change in the woman-minus-man gap is roughly 20 minutes per day — a narrowing of about 30% of the 68-minute baseline gap. In the aftermath of a major disaster, married women and men in affected communities look meaningfully more alike in their domestic time than married women and men elsewhere.

Which activities change

Table 3 disaggregates unpaid work into its four components. For household activities the pooled estimate is small and insignificant (− 2.70 minutes), masking offsetting movements by gender. Women in affected counties reduce household activities by 12.06 minutes per day, equivalent to 8.5% of their sample mean ($p < .01$), while men *increase* household activities by 8.15 minutes ($p < .10$). Women reduce and men increase precisely the category — cooking, cleaning, laundry, household maintenance — most consistently found to be allocated by gender rather than by availability or resources. The adjustment is substantial but far from eliminating a gap that stands at roughly 51 minutes at baseline.

Table 3
Major disasters and components of unpaid work

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	HH act.	HH act.	Childcare	Childcare	Adult care	Adult care	Comm. serv.	Comm. serv.
Panel A: All respondents								
Major disaster	−2.50	−2.70	−6.00***	−5.90***	−0.30	−0.30	0.20	0.10
	(2.68)	(2.70)	(0.91)	(0.90)	(0.63)	(0.63)	(0.78)	(0.79)
Observations	78,555	78,555	78,555	78,555	78,555	78,555	78,555	78,555
Add. controls	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	118.9	118.9	28.11	28.11	1.66	1.66	8.23	8.23
Panel B: Women								
Major disaster	−11.70***	−12.06***	−7.09***	−7.22***	0.18	0.17	0.77	0.64
	(1.76)	(1.75)	(1.55)	(1.54)	(1.07)	(1.18)	(1.18)	(1.20)
Observations	44,039	44,039	44,039	44,039	44,039	44,039	44,039	44,039
Add. controls	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	141.3	141.3	34.90	34.90	1.75	1.75	8.77	8.77
Panel C: Men								
Major disaster	8.24*	8.15*	−4.99***	−4.79***	−0.80***	−0.80***	−0.69	−0.73
	(4.75)	(4.81)	(0.87)	(0.84)	(0.20)	(0.20)	(1.65)	(1.65)
Observations	34,516	34,516	34,516	34,516	34,516	34,516	34,516	34,516
Add. controls	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	90.35	90.35	19.45	19.45	1.55	1.55	7.54	7.54

For childcare, both women and men reduce time, with women reducing more: 7.22 minutes per day for women, about 21% of their mean, against 4.79 minutes for men (both $p < .01$). Because the reduction is common to both genders and larger for women, childcare contributes to the narrowing of the gap through a decline in the total rather than through reallocation between women and men.

For adult care and community service we find no significant effects for women or for the pooled sample. Men reduce adult care by 0.80 minutes per day, which is large relative to men's small adult-care mean of 1.55 minutes but negligible in absolute terms. We build no interpretation on these two margins.

Testing the gender difference directly

Comparing coefficients estimated in separate samples does not establish that they differ. Table 4 reports a pooled model interacting disaster exposure with a female indicator. The woman-minus-man differential response is − 13.31 minutes for total unpaid work (p = .007), − 9.15 minutes for household activities (p = .008), and − 3.22 minutes for childcare (p = .013). The gender asymmetry driving the narrowing is statistically significant, not merely a descriptive contrast. The interaction for paid work is + 22.75 minutes (p = .010), reflecting men's much larger reduction in employment hours.

Table 4
Direct test of the gender difference (disaster × female interaction)

Outcome	Men (baseline)	Women − Men (interaction)	p (difference)	Women (implied)
Unpaid work	−1.50	−13.31***	.007	−14.81
	(2.39)	(4.72)		
Household activities	2.29	−9.15***	.008	−6.86
	(2.57)	(3.30)		
Childcare	−4.14***	−3.22**	.013	−7.36
	(0.64)	(1.25)		
Paid work	−26.44**	22.75***	.010	−3.69
	(12.71)	(8.51)		

Robustness

Our results depend on the fatality threshold defining a major disaster. Because that choice is consequential, we re-estimate across thresholds from zero to 150 county fatalities (Table 5), and we read the pattern more cautiously than a simple robustness claim would allow.

Table 5
Alternative fatality thresholds

Threshold	Treated county-years	All (1)	Women (2)	Men (3)
0 fatalities	3,164	2.92**	5.03***	0.30
		(1.221)	(1.735)	(1.715)
10 fatalities	47	−10.43***	−11.23***	−9.76**
		(2.747)	(2.959)	(3.652)
25 fatalities	16	−8.80***	−18.48***	1.82
		(2.756)	(3.988)	(4.238)
50 fatalities	5	1.20	−5.28	11.44
		(4.810)	(4.429)	(7.601)
75 fatalities	3	−10.33***	−8.62**	−10.99
		(3.637)	(3.889)	(7.884)
100 fatalities	2	−7.84*	−12.10*	−2.06
		(3.914)	(7.135)	(3.899)
150 fatalities	2	−7.84*	−12.10*	−2.06
		(3.914)	(7.135)	(3.899)

The effect does not scale monotonically with the threshold. At the lowest threshold — any event with at least zero recorded fatalities — the pooled estimate is a small positive 2.19 minutes, driven by a 5.03-minute increase among women. At our preferred threshold of 25 the estimates turn sharply negative: −8.70 pooled, − 18.40 for women, and an insignificant + 1.82 for men. At a threshold of 50 the women's estimate falls to − 5.28 and loses significance, before returning to − 8.62 ($p < .05$) at 75 and − 12.10 ($p < .10$) at 100.

Three mechanisms plausibly generate this non-monotonicity, and we think the first is the most important. As the threshold rises the treated group shrinks rapidly, so each estimate rests on a small and changing set of county-years; at the higher thresholds the treated group is small enough that a single event's composition can move the coefficient, and the 50-fatality estimate is best read as imprecise rather than as evidence of a genuine reversal. Table 5 reports these counts: the treated group falls from 47 county-years at a 10-fatality threshold to 16 at 25, 5 at 50, and 3 at 75. The 50-fatality estimate rests on five county-years in three counties, and we read it as uninformative rather than as evidence of a reversal. Second, and mechanically, as the threshold rises the counties that experienced destructive but lower-fatality events are reclassified from treated to control, contaminating the comparison group; this contamination is minimal at low thresholds and grows as the threshold rises. Third, and substantively, low-fatality events are common and mostly minor, so pooling them with severe events dilutes average treatment intensity, and the relationship between severity and household adjustment need not be proportional to a fatality count.

We do not claim our estimates are invariant to the threshold; they are not, and the 50-fatality row in particular interrupts the pattern. What holds is that the women's effect is negative and significant at most thresholds that isolate severe events, and that the men's effect is consistently small and insignificant. We read the evidence as

suggesting that the gender-asymmetric narrowing is a feature of severe disasters specifically rather than of disaster incidence as such, while acknowledging that a fatality cutoff is a coarse proxy for severity and that the sensitivity is a genuine limitation. A continuous intensity measure, or exposure defined on event dates rather than county-years, would address it more cleanly than any single threshold.

Table 6 collects further checks. Replacing state with county fixed effects leaves a significant reduction for women (− 12.84 minutes, $p < .01$) and sharpens men's response into a significant increase (+ 6.18 minutes, $p < .01$), reinforcing the equalizing pattern. Adding state-by-year fixed effects preserves a large negative effect for women (− 21.42 minutes, $p < .01$); under this most demanding specification men's unpaid work also falls (− 15.13 minutes, $p < .05$), so the gap still narrows but men's estimate proves more sensitive to state-year shocks. County-level and two-way clustering leave the women's effect significant at the 1% level. Weighting by ATUS final weights rescaled to mean one within survey year leaves the women's effect essentially unchanged (− 19.76 minutes, $p < .01$).

Table 6
Robustness of the effect of major disasters on unpaid work

(1) Baseline (state FE, state-clustered)	**All**	**Women**	**Men**
	−8.80* **	**−18.48*** **	**1.82**
	(2.76)	(3.99)	(4.24)
(2) County-clustered standard errors	−8.80***	−18.48***	1.82
	(2.57)	(4.21)	(3.68)
(3) Two-way (county × year) standard errors	−8.80**	−18.48***	1.82
	(3.93)	(5.79)	(4.15)
(4) County fixed effects	−3.81	−12.84***	6.18***
	(3.48)	(4.83)	(2.28)
(5) State × year fixed effects	−16.07***	−21.42***	−15.13**
	(3.15)	(4.39)	(6.65)
(6) Weighted (year-normalized ATUS weights)	−8.10	−19.76***	3.31
	(6.22)	(6.41)	(8.24)
Observations	78,555	44,039	34,516
Treated observations	833	457	376

Inference deserves separate comment, because the conventional p-values overstate precision. Under the wild cluster restricted bootstrap the women's effect remains significant at conventional levels ($p = .024$), though far less emphatically than the analytic p-value below .001 suggests, and the 95% bootstrap-t interval is wide, running from − 35.8 to − 1.2 minutes. Randomization inference gives $p = .003$ for women. The pooled estimate is more fragile: its analytic p of .003 becomes .062 under the bootstrap and .023 under randomization inference, so whether the aggregate effect clears a 5% threshold depends on which procedure one adopts. The men's estimate is insignificant under all three ($p = .67$, .71, and .71). We therefore rest our conclusions on the women's estimate and on the gender difference, and treat the pooled effect as suggestive.

Two further checks bear on interpretation. Replacing the fatality-based indicator with a continuous measure of property damage (the inverse hyperbolic sine of real damages) yields no significant effect for any group. Given the composition of the treated group described above, this is what one would expect: the county-years that clear the fatality threshold are largely heat episodes with small property losses, so mortality-based and damage-based measures of severity select almost disjoint sets of events. It also means our results speak to high-mortality exposure specifically and not to destructive exposure. And a dynamic specification adding the prior year's county disaster status shows the effect concentrated in the disaster year: the contemporaneous women's effect remains − 17.0 minutes ($p < .01$) while the one-year lag is small and insignificant. The reallocation we document is short-run.

Paid work and other activities

Table 7 reports estimates for paid work and Table 8 for four non-work categories. In the pooled sample, disaster exposure is associated with about 14 fewer minutes of paid work per day, imprecisely estimated. Among women the estimate is small, positive, and insignificant. Among men it is − 35.26 minutes ($p < .05$). Men in disaster-affected counties lose substantially more employment time than women, consistent with evidence that hurricane-related labor market disruption concentrates in male-dominated sectors such as construction, transportation, and extraction (Belasen & Polachek, 2008; Groen & Polivka, 2008). The roughly 8 additional minutes men devote to household activities plausibly comes out of this much larger loss of paid work time — a conversion rate well under a quarter, closely echoing Gough and Killewald's (2011) finding that unemployed men convert only a fraction of freed time into housework.

Table 7
Major disasters and paid work, by gender

Major disaster	**All (1)**	**All (2)**	**Women (3)**	**Women (4)**	**Men (5)**	**Men (6)**
	−14.40	**−13.97**	**4.27**	**5.16**	**−35.27****	**−35.26****
	(8.61)	(8.39)	(8.68)	(8.68)	(15.25)	(15.44)
Observations	78,555	78,555	44,039	44,039	34,516	34,516
Additional controls	No	Yes	No	Yes	No	Yes
Outcome mean	157.5	157.5	129.9	129.9	192.5	192.5

Table 8
Major disasters and other daily activities

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Sports	Sports	Volunteer	Volunteer	Religious	Religious	Social	Social
Panel A: All respondents								
Major disaster	−4.68	−3.52	−0.97	−0.49	3.94*	4.06*	7.03	3.24
	(2.87)	(2.96)	(5.09)	(5.01)	(2.10)	(2.03)	(11.32)	(12.11)
Observations	78,555	78,555	78,555	78,555	78,555	78,555	78,555	78,555
Add. controls	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	19.80	19.80	9.14	9.14	12.06	12.06	295.0	295.0
Panel B: Women								
Major disaster	−6.27*	−4.76*	4.68	5.68	10.41**	10.74**	18.39*	14.80
	(3.43)	(2.83)	(7.12)	(6.78)	(4.71)	(4.57)	(9.77)	(12.29)
Observations	44,039	44,039	44,039	44,039	44,039	44,039	44,039	44,039
Add. controls	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	14.62	14.62	9.78	9.78	14.23	14.23	279.2	279.2
Panel C: Men								
Major disaster	−3.30	−2.61	−5.32*	−5.15*	−1.75	−1.75	2.29	−0.88
	(5.93)	(6.05)	(2.85)	(2.95)	(1.51)	(1.55)	(11.79)	(10.96)
Observations	34,516	34,516	34,516	34,516	34,516	34,516	34,516	34,516
Add. controls	No	Yes	No	Yes	No	Yes	No	Yes
Outcome mean	26.42	26.42	8.32	8.32	9.28	9.28	315.1	315.1

Note: Outcomes are daily minutes in each activity category: sports, exercise, and recreation; volunteer activities; religious and spiritual activities; and socializing, relaxing, and leisure. Day-of-week, month, year, and state fixed effects are included in all specifications. Estimates are unweighted. Robust standard errors clustered at the state level in parentheses. *** p < .01, ** p < .05, * p < .10.

On non-work margins, women in affected counties increase time on religious activities by 10.74 minutes per day ($p < .05$), more than twice the pooled effect, with no significant effect for men. Women's time on social activities rises by about 15 minutes, but this is significant only in the parsimonious specification and imprecisely estimated with the full control set, so we treat it as suggestive at best. Women reduce time on sports and exercise by about 5 minutes. Men reduce volunteering by more than 5 minutes. Seven of the eight estimates for men in these

categories are negative, consistent with men's additional housework being drawn from discretionary time as well as from lost employment.

Taken together the margins suggest two complementary processes. Men are more severely displaced from paid employment and redirect a modest fraction of the freed time to household work — a community-level analogue of the added-worker dynamic documented for individual job loss. Women relinquish some routine domestic work while increasing time on religious activity. We are cautious about the latter: it is consistent with a turn to congregational sources of support after a disaster, but we cannot distinguish that from other explanations, and the community service and adult care estimates that would corroborate a broader turn outward are not significant.

Time diaries let us observe substitution across activities — how time taken on in one domain coincides with time given up in another. They do not let us observe the household processes governing the reallocation: bargaining, negotiation, shifts in relative productivity, or change in the gender ideologies that structure who does what. With one respondent per household we cannot see couples at all. The mechanisms we discuss are readings consistent with the observed patterns, not mechanisms we identify.

Discussion

We asked whether the gender gap in unpaid household work widens or narrows after a major disaster. It narrows, and substantially: married women in affected counties report about 18.5 fewer daily minutes of unpaid work than comparable women elsewhere, married men's unpaid work is statistically unchanged and their housework rises, and the 68-minute gap narrows by roughly 30%. The change is concentrated in housework and childcare, is confirmed by a direct interaction test, and does not persist into the following year.

The primary contribution is to the population–environment literature on disaster impacts. That literature has established, convincingly, that disasters reshape household migration, wealth, employment, and health, and that they do so unequally across race and class. We add a dimension that has been largely invisible in nationally representative U.S. data: the allocation of unpaid work within affected households. The finding that this allocation moves — and moves in the most strongly gender-typed tasks — indicates that time is a margin of household disaster adjustment worth measuring alongside the financial and residential margins that have received more attention. It also suggests that the household-level aggregates on which much of this literature relies conceal substantial within-household variation in who absorbs the shock.

The results also qualify a conclusion that might be drawn too quickly from the COVID-19 literature, which established that the pandemic widened the domestic gender gap in many settings (Carlson et al., 2021; Collins et al., 2021; Del Boca et al., 2020). We find the opposite direction for a different kind of shock. We are careful about the scope of this contrast: we compare our estimates with findings from specific pandemic studies in specific settings, and we do not claim a general law about how crises affect domestic labor. What the comparison establishes is that the pandemic result should not be generalized either. A crisis that sharply raises the volume of care work to be done is not the same as one that interrupts routines and displaces paid employment, and there is no reason to expect the two to move the domestic gender gap in the same direction. Distinguishing environmental shocks by whether they add domestic demand or disrupt domestic routine seems to us a productive line for future research, and one that the growing literature on gendered labor responses to weather shocks in other settings (Afridi et al., 2022; Akter, 2021; Boyd, 2023) is well placed to pursue comparatively.

Several alternative explanations warrant consideration. The narrowing could reflect selective displacement rather than behavioral change: if disasters induce out-migration that differs by gender or household type, the composition of the remaining sample would shift. Given that disaster-induced migration is well documented and socially patterned (Elliott, 2015; Sheldon & Zhan, 2022; Spring, 2025), this deserves emphasis. Our fixed effects cannot rule it out, though the concentration of effects in the disaster year and their disappearance the following year fits a short-run behavioral adjustment better than a compositional one. The narrowing could also reflect differential reporting rather than differential behavior. Finally, the reduction in women's domestic time could reflect a *contraction of the domestic sphere itself* — displaced households, damaged homes, and children out of school may simply present less housework to do — rather than reallocation between women and men. The increase in men's housework argues against this being the whole story, but it may well be part of it, and disentangling the two would require measurement of household conditions we do not have.

Three limitations bear most heavily. First, and most importantly, exposure is measured at the county-year level while time use is measured for a single day, attenuating our estimates toward zero. Finer temporal resolution using SHELDUS event dates, with post-event lags, would sharpen the timing of treatment and reveal the dynamics of adjustment; we regard this as the essential next step. Second, the ATUS observes one respondent per household, so we characterize the time use of married women and married men, not negotiation between spouses. Third, our estimates are sensitive to the fatality threshold, and a continuous intensity measure would address this more cleanly. Related to both, the restrictiveness of the threshold leaves 833 treated diary days in 9 counties and 16 county-years, with only 6 treated state clusters. This limits precision, makes conventional cluster-robust inference fragile, and means our estimates should not be read as characterizing the average U.S. disaster.

Fourth, and most consequentially for interpretation, the treated group is dominated by urban extreme-heat mortality rather than by destructive events. Because 93.5% of treated diary days come from Clark, Maricopa, and Philadelphia Counties in high-heat-mortality years, our estimates are identified largely off multi-day heat episodes in three large metropolitan areas. A heat wave and a hurricane are not the same shock to household time: heat kills without destroying homes or displacing households, and the mechanisms most plausibly at work — staying indoors, curtailing outdoor and physically demanding tasks, altered childcare arrangements when schools and outdoor activity close down — differ from the reconstruction labor that motivates much of the gender-and-disaster literature. Readers should not generalize these estimates to disasters that destroy housing stock, and the effects we document may be better understood as a response to extreme heat than to disaster in the broad sense. Establishing that distinction requires hazard-type identifiers we do not have in the present extract, and we regard it as a priority alongside finer temporal resolution.

Two further limitations concern reach. Our sample is restricted to married, different-sex couple households aged 18–64, so we cannot speak to households in which the conventional gendered allocation is not the starting point. And while our models control for race, ethnicity, and education, we do not estimate how the disaster response varies across these positions. There is strong reason to expect that it does: exposure, recovery resources, and the organization of domestic labor all vary substantially by race and class (Elliott & Pais, 2006; Fothergill, 2004; Fussell et al., 2010; Howell & Elliott, 2019; Spring, 2025). Estimating that heterogeneity is the obvious extension, and we flag its absence as a real gap in the present analysis.

Conclusion

We want to be unambiguous about what our results do not imply. The narrowing of the gender gap in domestic labor after a disaster is not a benefit of disasters. These events destroy homes and livelihoods and impose large welfare losses, and the equalization we document arises partly because women lose time they would have spent caring for their households and partly because men lose paid employment. Both are costs. Nothing in our analysis weighs against investment in preparedness and mitigation.

Read carefully, however, the findings carry two constructive implications for recovery policy. The first concerns timing. If the domestic division of labor is more malleable when routines are disrupted, and if short-run deviations can influence longer-run expectations, the post-disaster period may be a window in which policies supporting shared domestic responsibility — emergency childcare, flexible and family leave provision, caregiving assistance — are unusually likely to take hold. The second concerns program design. Because disasters reshape household time in gender-specific ways, recovery programs that ignore these dynamics risk returning households to pre-disaster patterns by default. Relief and recovery assessments that count property damage and income loss but not the redistribution of unpaid work will systematically misstate who bears the burden of adaptation. As disasters grow more frequent, the case for measuring that burden — and designing recovery with it in view — becomes stronger, not weaker.

Declarations

Author Contribution

Veeshan Rayamajhee: data analysis and data collectionRaymond March: minor data analysis, manuscript preparationMehadi Shawon: data collection and minor data analysis

Data Availability

We use data on our analysis and this data is avialable upon request.

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Figures

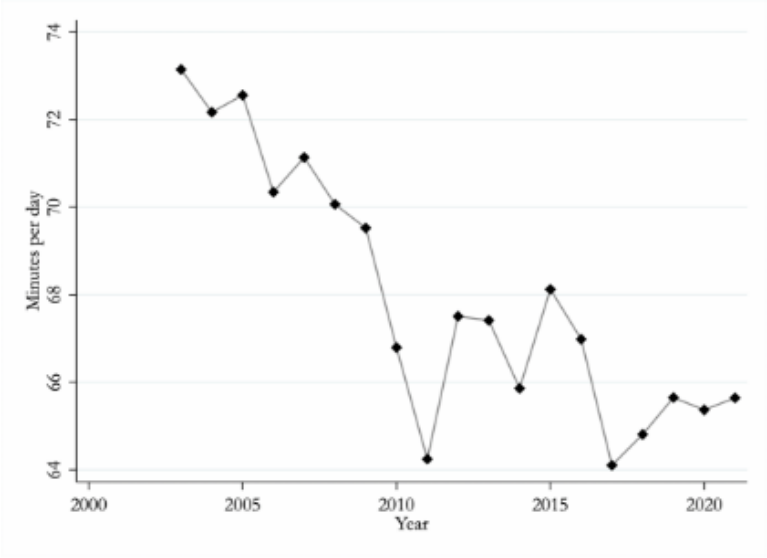


Figure 1

Gender gap in unpaid work, 2003–2021

Note: The gender gap is total daily minutes of unpaid work reported by women minus that reported by men. Each data point is the difference in unweighted means for the analytic sample in that survey year.

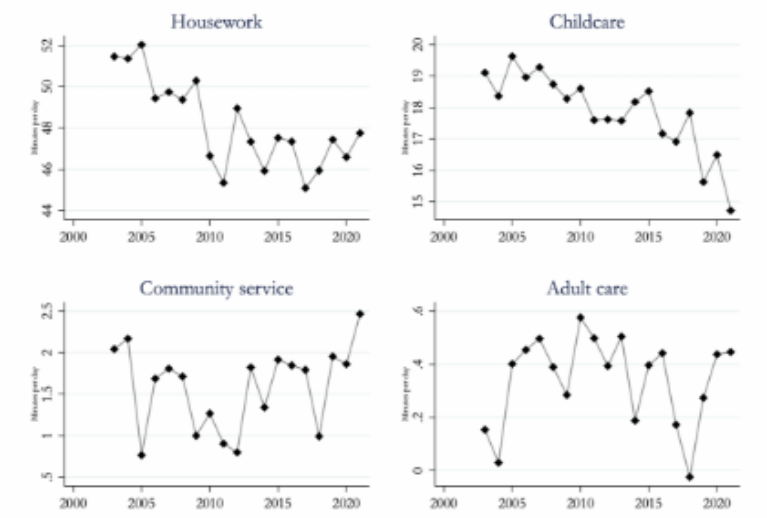


Figure 2

Gender gap in unpaid work, by component, 2003–2021

Note: The gender gap is total daily minutes in each component reported by women minus that reported by men. Panels correspond to household activities (top left, labeled “Housework” in the figure), childcare (top right), community service (bottom left), and adult care (bottom right). Each data point is the difference in unweighted means for the analytic sample in that survey year.