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RESEARCH PAPER

International migration, remittances, and remaining households: evidence from a trade embargo

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Abstract
Identifying the impact of remittances on household members remaining behind is difficult due to selection into migration. In this paper, we exploit an unexpected embargo on Qatar, the second major destination among Nepali migrants. Using longitudinal data on about 1,500 Nepali households with migrants prior to the embargo, we assess how this shock translates into changes in remittances and development outcomes. We find a 56% reduction in remittances for households with a migrant in Qatar. At least in the months immediately after the shock, such a fall in remittances does not seem to translate into recipient household's welfare. However, we cannot exclude that such effect might materialize in the medium run. That is particularly true for poor and credit-constrained households, especially vulnerable to the remittance windfall and lacking the ability to move their migrants or other household members to other destinations.

Keywords: embargo; Nepal; Qatar; remittances

***JEL Classification*:** O15; O12; J61; E20

1. Introduction

Recent decades have witnessed an overall rise in international remittances,[1] which increased from an estimated $ 126 billion in 2000 to $ 689 billion in 2020 (McAuliffe Khadria, 2019). This rise is attributed to the increase in migration between developed and developing countries, partly due to the fall in migration costs due to technological advancements (Meyer Shera, 2017). Remittances represent the

[1]"Remittances are financial or in-kind transfers made by migrants directly to families or communities in their countries of origin" (McAuliffe Khadria, 2019).

highest source of received foreign income in many developing countries (Ratha, 2005). The rise of skilled migration should strengthen these trends and the importance of remittances for migrant origin countries (Bollard et al., 2011). Nepal is a case in point with remittances representing over a quarter of GDP, making it a significant source of the country's foreign exchange earnings (McAuliffe Khadria, 2019). In addition, 56% of Nepali households receive remittances as reported in the Nepal Living Standards Survey (2010/11) (Central Bureau of Statistics, 2011, as cited in Bhandari, 2016). These are particularly important in rural Nepal, where remittances constitute the second most important source of household income (Bhandari, 2016). Therefore, shocks to remittances are likely to have detrimental effects on the country's economy and origin households' welfare. A defining feature of Nepali employment migrants is their reliance on short-term contracts and concentration in Gulf Cooperation Council (GCC, a union that consists of Bahrain, Kuwait, Oman, Qatar, Saudi Arabia, and the United Arab Emirates [UAE]) countries and Malaysia (McAuliffe Khadria, 2019). More specifically, about 90% of migrant workers from Nepal were concentrated in Qatar, Kuwait, UAE, Saudi Arabia, India, and Malaysia. Therefore, migrants are highly vulnerable to shocks in these destinations (McAuliffe Khadria, 2019).

It has been nonetheless difficult to identify the impact of remittances on households' welfare given selection into migration and hence, selection into those receiving remittances (Clemens McKenzie, 2018). In this paper, we exploit the impact of an unexpected shock on the second major destination country among Nepali migrants (Endo Afram, 2011). On June 5th, 2017, Egypt, Saudi Arabia, UAE, and Bahrain cut all diplomatic ties with the State of Qatar. They closed air spaces, land borders, and blocked access to sea ports. The blockade countries' measures generally targeted the entire economy of Qatar without focusing on governmental entities, thus causing substantial financial losses to individuals (Javed, 2018) with migrants being particularly vulnerable (Toppa, 2017). We exploit such a sudden shock to assess its impact on remittances, then how changes in remittances affected consumption and expenditure outcomes in Nepali households. More specifically, we use longitudinal data spanning the years 2016, 2017, and 2018 from the Household Risk and Vulnerability Survey in Nepal. Our results point to a 56% reduction in remittances for households with a migrant in the country exposed to the shock (vs. households with migrants in other international destinations not affected by the shock). Such a remittance windfall does not immediately translate into the households' assets, total income, total expenditures, or food insecurity. Given the fact that we measure the effect 1 year after the embargo, we cannot exclude the possibility that such effects will materialize in the medium term. Our results indicate that it would be particularly the case for poor and credit-constrained households who experience the strongest fall in remittances. We also show that those households have much less ability to adjust to the remittance shock by moving their migrants or other household members to another destination.

A methodological challenge is that households decide to send migrants, so there is a self-selection problem with the expectation that more able members are sent abroad (Karki Nepal, 2016). Another issue is that remitting behavior is not random (Karki Nepal, 2016). In other words, migrants decide how much to send back home and this may be correlated with household characteristics or outcomes of interest, limiting the possibility of identifying a causal effect (Adams, 2011). In our analysis, we address these issues by selecting only households with migrants (in Qatar and other international destinations) prior to the embargo, exploiting an exogenous

shock that affected remittance behavior. The closest paper to ours is the seminal paper written by Yang (2008) who exploits the effect of the Asian financial crisis on remittances received by Filipino households and how this exogenous change in remittances affects households' consumption and investment. First, we follow a similar identification strategy by exploiting panel data and limiting the sample to households with an international migrant prior to the shock (the embargo in our case, the financial crisis in Yang, 2008). Second, to deal with endogeneity in remittance behavior, Yang (2008) exploits the 1997 Asian financial crisis and the exogenous change in remittances due to differential changes in exchange rates across Filipino migrants' destination countries. In our case, one difference is that our remittance shock – the embargo on Qatar – is completely external to recipient households. Another difference is that the embargo we exploit is expected to induce a detrimental effect on remittances; Yang (2008), the opposite through changes in exchange rates. We also follow Yang (2008) in assessing the stability of our coefficient of interest to the addition of pre-embargo characteristics.[2]

Our contribution in this paper is twofold. First, we contribute to the literature seeking to assess the impact of remittances on economic development. Remittances have been argued to improve households' welfare and alleviate poverty, despite having a questionable impact on economic development in recipient countries (Adams, 2011). Studies either examine remittances at the macro or the micro level (Rapoport Docquier, 2006). Macro-level data tend to be of poor quality (Rapoport Docquier, 2006), rendering their results questionable (Gubert, 2017). Although macro data have improved, micro-level household survey data have the advantage of capturing formal and informal channels (Clemens McKenzie, 2018). Moreover, by collecting a wide array of disaggregated data, micro-level data allow for a more accurate examination of remittances (Adams, 2011). Our second contribution is to shed light on the unintended consequences of a trade embargo. A large literature seeks to assess the impact of an embargo on the targeted country (see, for instance, Felbermayr et al., 2019; Crozet Hinz, 2020; Ahn Ludema, 2020; Draca et al., 2021; Chakravarty et al., 2021 among others), but little is known about the indirect consequences for non-targeted countries. One exception is Al-Malk et al. (2022) who exploit the embargo on Qatar to quantify the trade elasticity to distance. In this paper, we examine the consequences of the same embargo on Nepali households with migrants in Qatar at the time of the shock. Therefore, we examine whether the effect of the embargo spills over to other non-targeted countries.

The remainder of this paper is organized as follows: section 2 gives an overview of the blockade and trends on migration and remittances. Section 3 discusses the identification strategy employed and data used. In section 4, we present the results of the impact of the shock on remittances and other development outcomes. We propose different channels to explain these results in section 5. Finally, section 6 concludes.

2. Background

On June 5th, 2017, Egypt, Saudi Arabia, UAE, and Bahrain imposed a major trade embargo on Qatar (Al-Malk et al., 2022), which was not expected. Figure 1 plots the count of the news on the embargo targeting Qatar over the period January 2015 to

[2]Similar to Yang (2008), our paper differs from other studies that exploit different shocks as instrumental variables. McKenzie Yang (2012) point out several shortcomings of utilizing shocks as instruments.

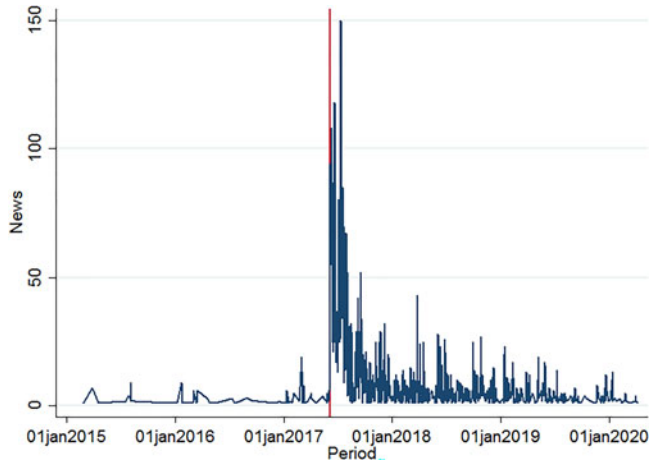

Figure 1. News about the embargo.
Notes: Embargo News about Qatar released in any country.
Source: Al-Malk et al. (2022).

April 2020. There is a spike on the blockade date before which no mention of such an event can be spotted.[3] Also, none of these news recordings come from Nepal. Therefore, we argue that migrants could not anticipate the blockade and change remittance behavior beforehand.

When the embargo was imposed, migrants were affected in different ways. First, the rise in food prices due to the closure of the land border had a negative effect on migrants residing in Qatar (Human Rights Watch, 2017). Therefore, it is expected that migrants who kept their jobs needed to spend more money for their consumption and had less money available to send as remittances. Second, some migrants were laid off from sectors that were affected. For instance, the manufacturing sector suffered due to the loss of raw materials, while the transportation sector was negatively affected due to closure of the land border for trade (Javed, 2018). Moreover, since 50% of tourists to Qatar are GCC nationals, tourism declined following the blockade (Yap et al., 2020), which meant that migrants employed in the hospitality industry were heavily affected. In particular, taxi drivers faced substantial losses as their daily earnings fell by 70% (Javed, 2018). To deal with such adverse effects on its tourism sector, Qatar has facilitated its visa policies and introduced a new visa-free entry for nationals of 80 countries (Javed, 2018). Although such a move contributed to somewhat offsetting the decline in GCC tourists, numbers have not reverted yet to pre-blockade levels (Javed, 2018). Third, some Qataris employ migrants to work in Saudi Arabia. When the blockade was suddenly imposed, those migrants were stranded there with no channel to send them food, water, or money (Human Rights Watch, 2017). They could not send remittances back to their countries of origin either.

Table 1 reports the distribution of Nepali migrants overseas, pre- and post-embargo based on the Household Risk and Vulnerability Survey. India is the largest destination,

[3]This figure is taken from Al-Malk et al. (2022) where we elaborate the unexpected nature of the blockade in more detail. The few news count before the blockade date in Fig. 1 are either an error or refer to a threat of imposing an embargo by other countries.

Table 1. Nepal's migration and remittances

Location of overseas workers pre- and post-embargo					
	Number of overseas workers		% of total		% change in remit
Location	Wave 1 (2016)	Wave 3 (2018)	Wave 1 (2016)	Wave 3 (2018)	Wave 3–Wave 1
India	589	482	33.64	34.28	105.31
Malaysia	359	265	20.50	18.85	3.80
Qatar	244	193	13.93	13.73	28.85
Saudi Arabia	242	187	13.82	13.30	11.25
UAE	100	104	5.71	7.40	12.53
Japan	45	41	2.57	2.92	10.58
South Korea	26	25	1.48	1.78	39.40
United States	18	18	1.03	1.28	36.65
Australia	13	13	0.74	0.92	−32.14
Hong Kong	10	10	0.57	0.71	−64.0
United Kingdom	5	4	0.29	0.28	57.80
Israel	2	1	0.11	0.07	400
Other	98	63	5.60	4.48	−7.05
Total	1,751	1,406	100	100	

Notes: This table shows data for migrants from households with international migrants, excluding mixed households with migrants in Qatar and other international destinations. We report information on overseas migrants pre- and post-embargo.

accounting for more than 30% of migrants in both periods. It is followed by Malaysia, Qatar, and Saudi Arabia, all of which represent more than 10% of the total separately. The remaining migrants are dispersed across other countries, none of which surpasses 8% of the total. Households with relatively more wealth are more likely to be able to send members abroad (Sijapati et al., 2015). But selection may also differ across destinations. India remains the main destination possibly because of geographic proximity, cultural ties, and the open border with Nepal. Since the cost to migrate to India is lower, "the probability of having a migrant in India is higher in households with lower economic standing, while the converse is true for other countries (i.e., countries other than India, Malaysia and the Gulf)" (Sijapati et al., 2015, p. 20). India tends to also attract migrants with the lowest level of education. It is the opposite for the United States, while the level of education is similar for workers going to Malaysia, Qatar, and Saudi Arabia (Sijapati et al., 2015).[4] According to Table 1, the number of migrants to Qatar declined over the period. Generally,

[4]Regmi et al. (2020) suggest that families with educated household heads or with higher land holding area are more likely to select the GCC countries as migration destinations but the external validity of this study is limited to one Nepali district (Chitwan).

migrants' representation of the total did not change much over the two periods, except for Malaysia and UAE where it declined and increased by around 2 percentage points. Over the period, remittances sent by households increased from all destinations except for Australia, Hong Kong, and other destinations. Remittances from Qatar increased but less than those from other destinations like India, South Korea, the United States, or the United Kingdom.

3. Empirical analysis

3.1. Data

We use two waves out of the three waves of panel data from the Household Risk and Vulnerability Survey in Nepal (Walker Jacoby, 2020). The survey was carried out by the World Bank for three consecutive years from 2016 to 2018. It covers 6,000 households in non-metropolitan areas in Nepal.[5] The retention rate of households across all three survey waves is 94%, giving a total of 5,654 households that are available in all waves (Walker et al., 2019).[6] The longitudinal nature of the dataset allows us to track households overtime and effectively deal with potential omitted variable bias (Andreß, 2017). In addition, it gives us data pre- and post-embargo, allowing us to carry a first-difference estimation to identify the causal effect of the embargo on different outcomes. The survey dates are as follows: wave 1 was conducted between June 5th, 2016 and August 21st, 2016; wave 2 between June 12th, 2017 and August 14th, 2017, and wave 3 between June 10th, 2018 and August 22nd, 2018. For our main analysis, we omit wave 2 because our main outcome variable, remittances, is asked retrospectively (remittances received over the past 12 months) but use it to explore the common trend assumption.

3.2. Identification strategy

Estimating the effect of the embargo on Nepali households is challenging since it is subject to self-selection and omitted variable biases. Households select whether and which member to send out of the country and migrants choose whether to remit or not as well as the remittances amount. Since an individual's choice to migrate is endogenous, the likelihood to receive remittances is likely to be also driven by factors that potentially differentiate these households from others with no migrants. In short, comparing households with migrants to others without migrants is likely to capture unobserved differences between them.[7] To deal with this key identification challenge of selection into migration, we only focus on households with international migrants in the pre-embargo period, i.e., wave 1 of our survey, similar to Yang

[5]Stratified sampling was used in this survey. In order to obtain the sample, the country was divided into 11 strata following the NLS-III survey, except that three urban strata were excluded. A total of 50 out of the 70 districts of Nepal were chosen with probability proportional to size in order to increase households' concentration, with the size measured in number of households. Then primary sampling units (PSUs) were selected from all administrative wards in those 50 selected districts, for each strata at a time. Finally, 15 households were randomly chosen from the entire list of households at each PSU. A clarifying table and map from Walker et al. (2019) are provided in Fig. A.1 in the online Appendix.

[6]Despite the full sample attrition being 6%, all households with international migrants prior to the embargo in our sample have been followed in wave 3, featuring no attrition.

[7]Table A.1 in the online Appendix shows that there is a statistically significant difference between these households for most characteristics.

(2008).[8] Our sample of households with international migrants pre-embargo is observed in wave 3, so we have a balanced panel. This allows us to deal with unobserved heterogeneity between households. We then compare households with migrants in Qatar, potentially exposed to the embargo, with other households with migrants in other international destinations. This gives us a final sample of 1,508 households. Specifically, we estimate the following first-difference model:

$$\Delta Y_{jt} = \phi_0 + \beta(QatMig_j) + \epsilon_{jt} \tag{1}$$

Y_{jt} denotes different outcome variables of household j at time t. Our main outcome variable is the sum of remittances received over the last 12 months. We transform such a variable into logarithmic form (adding the value 1 to deal with zero values to ease interpretation).[9] Alternative outcomes are defined progressively. $QatMig_j$ is an indicator variable that equals one if the household has at least one migrant in Qatar before the embargo took place.[10] In a first-differencing model, this variable measures how the embargo affects the change in outcomes of households with at least one migrant in Qatar. The constant captures the average change in outcomes across all households of our sample. Finally, ϵ_{jt} denotes the error term.

Our identification rests on the parallel-trend assumption that households with migrants in Qatar prior to the trade embargo would follow a similar trajectory compared to those with migrants in other international destinations. We explore the plausibility of the identifying assumption in three ways. First, we follow Yang (2008) in including pre-embargo characteristics in our regression equation to check for potential contamination of our main coefficient. Specifically, this allows us to test for the possibility that differences in pre-embargo characteristics between treated and control households are the drivers of differential outcomes that we attribute to the embargo. Therefore, we augment our main model in equation (1) with pre-embargo characteristics X_{jt-1}. These controls include at the household level: the number of members including overseas, the household's per capita income (in log), an indicator for whether the household is in the 2nd, 3rd, or top quartiles of the sample distribution of household's per capita income; at the household head level: the age, the marital status being single, various indicators related to the highest level of education completed by the household head, and the head's occupation; at the migrant level: the age of migrants and the number of months away. All these controls aim to capture omitted variables that are likely to be correlated with destination choice and remittances or household welfare and are thus reported at

[8]If we were to define our sampled households based on the presence of migrants in the post-embargo period, we may capture the effect of the embargo itself such as the return of migrants who were working in Qatar due to job loss. In fact, there was a reduction of 25% in the number of Nepali migrants in Qatar in July following the blockade (Toppa, 2017).

[9]As a robustness check, we report results using inverse hyperbolic sine transformation in online Appendix D. We should note that sampling weights are not useful in our setting since our selected sample has not the ambition to be representative at the country level. However, sampling weights could be used to test whether the model is misspecified in the presence of heterogeneous effects (Solon et al., 2015). Although less precisely estimated, we found similar results using sampling weights (these are available upon request).

[10]We drop mixed households (i.e., those with migrants in Qatar and other international destinations) to rule out any crowding-out effect. Our results – available upon request – are robust to the inclusion of these households.

pre-embargo level (i.e., wave 1).[11] As a result, our main coefficient β assesses the effect of the embargo on outcomes of the households followed overtime, conditional on changes in outcomes related to household's pre-crisis characteristics.

Second, we cannot be certain that our pre-crisis household's controls capture the changes in outcomes related to all initial heterogeneities between households with migrants in Qatar and those with migrants in other destinations. In the absence of multiple household data prior to June 5th, 2017, we cannot directly test the parallel pre-trend assumption. However, it is likely migrants may not stop or reduce remitting immediately when the embargo is imposed. It may take a few weeks for them to adjust their consumption and asset patterns. We exploit the timing of wave 2 to make an educated guess about the validity of the common trend assumption.[12] In the second wave, households are interviewed right after the embargo, between June 12th, 2017 and August 14th, 2017. In the absence of pre-existing trends, we should expect the negative impact on remittances to materialize only after a few weeks. Based on a similar first-difference estimation (equation (1)) using only waves 1 and 2 on our sample of households with international migrants, we can show that the effect on remittances only kicks in 7 weeks after the embargo. The key to this exercise is the gradual addition of 2 weeks from wave 2. The idea is that since the embargo took place in June 5th, the closer the interview to that date, the more unlikely the interviewed household's remittances were affected. However, as we extend the duration, we are including more post-embargo periods to see whether an effect starts to show.[13] Figure 2 shows that our coefficient of the effect of having a migrant in Qatar post-embargo is close to zero and not statistically significant in the first 7 weeks post-embargo (6 weeks after interview). However, although not statistically significant, we see a negative coefficient in week 8 after the embargo (7 weeks after interview). Thus, it gives us some suggestive evidence that further into the future, an effect is likely to be present following the embargo but was not present in the immediate weeks after June 5th, 2017.[14]

Third, the validity of the common trend assumption also rests on the fact that the embargo does not directly affect households with migrants in other destinations. That might be a concern for households with migrants in blockading countries.[15]

[11]In case of multiple migrants, all of migrant controls are averaged at the household level. The vector of pre-crisis characteristics differs from that of Yang (2008) since we do not have information on migrant's highest education level completed, migrant's occupational indicator, migrant's marital status being single, and the relationship of the migrant to the household's head. Table A.3 in the online Appendix shows no difference between households with migrants in Qatar vs. other international destinations in the means of most variables at the household's and head's levels. Exceptions are that households with migrants in Qatar are more likely to be Muslim and have a single head. However, the differences are more pronounced if we look at the characteristics of the migrants. For instance, migrants to Qatar are slightly older, spend less time away and are more likely to return compared to international migrants sent to other destinations.

[12]We do not use wave 2 in the main analysis because of retrospective remittances data. This wave reports data on remittances pre- and post-embargo and therefore is likely to bias our estimates.

[13]Given interview starts on June 12th, 2 weeks: is on or before June 27th, 4 weeks: is on or before July 11th, 6 weeks: is on or before July 25th and 8 weeks: is on or before August 1st.

[14]The analysis is restricted to up to 7 weeks of the second round, because after that period the number of respondents declines substantially as shown in Fig. B.2 in online Appendix B.

[15]Based on a few aggregated data from the Nepal Labour Migration Report 2020, Fig. B.1a in the online Appendix shows that the blockading countries seem to also have experienced a change in trend following the embargo. Although based on imperfect aggregates, Figs B.1a and B.1b are instructive for two reasons.

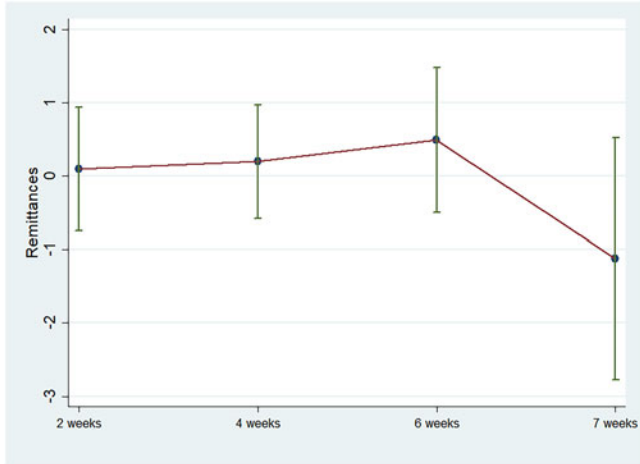

Figure 2. Test pre-embargo trends.
Source: Authors' computations using waves 1 and 2 of the Household Risk and Vulnerability Survey in Nepal.

We will therefore test the sensitivity of our results to dropping households with migrants in blockading countries.

4. Results

4.1. The impact of the embargo on remittances

Main results. In Table 2, we present the results regarding the impact of the embargo on remittances for households with a migrant in Qatar, considering households with at least one international not-in-Qatar migrant as the control group. Our sample covers households observed in both waves 1 and 3, and includes households with at least one international migrant (excluding mixed-migrant households). Every cell in Table 2 reports the coefficient estimate β of the $QatMig_j$ variable from a different specification, with robust standard errors in parentheses. Column 1 in Table 2 presents the coefficient estimates from equation ((1)), not augmented with any pre-embargo control variables. Standard errors clustered at the PSU level are shown in column 3 of Table C.1 in the online Appendix. Cameron Miller (2015) point out that clustering of standard errors is routinely used at the PSU in complex survey designs.[16]

First, they provide a visual confirmation that remittances from Qatar and non-blockading countries were on similar trends prior to the embargo. Second, they clarify our identification assumption that in the absence of treatment, remittances from Qatar would have followed a similar increasing trend compared to remittances from non-blockading countries (Fig. B.1a) or from the rest of the world (Fig. B.1b).

[16]The suggestions in clustering of standard errors is either at the treatment level (Cameron Miller, 2015) or the sampling design level (Abadie et al., 2017). Yang (2008) follows the former and clusters at country of destination, but in his case the treatment varies across different countries of destination, whereas our treatment only takes place in Qatar vs. other destinations. Thus, we cluster standard errors following the sampling design approach. It means we cluster standard errors at the PSU level (371 clusters).

Table 2. Main results and shock on development outcomes

	No controls		All controls without BC
(a) Remittances			
Remittance receipts	−0.834*	−0.760*	−0.879*
	(0.451)	(0.454)	(0.471)
Extensive margin	−0.070*	−0.063*	−0.070*
	(0.037)	(0.038)	(0.039)
(b) Income and consumption			
Total income	0.192	0.204	0.452
	(0.458)	(0.357)	(0.370)
Agricultural income	0.183	0.118	0.194
	(0.313)	(0.305)	(0.316)
Non-agricultural income	0.222	0.268	0.487
	(0.481)	(0.412)	(0.426)
Sell land	0.002	0.001	0.004
	(0.007)	(0.007)	(0.007)
Total expenditure	0.061	0.042	0.041
	(0.070)	(0.070)	(0.073)
Food expenditure	0.040	0.040	0.021
	(0.035)	(0.035)	(0.036)
Frequent non-food exp	0.075*	0.056	0.061
	(0.043)	(0.043)	(0.044)
Infrequent non-food exp	0.031	0.009	−0.005
	(0.101)	(0.104)	(0.110)
(c) Non-consumption disbursements			
Educational expenditure	0.657**	0.646**	0.503*
	(0.290)	(0.293)	(0.303)
Health expenditure	−0.128	−0.219	−0.209
	(0.350)	(0.353)	(0.366)
Buy land	−0.026**	−0.025**	−0.020
	(0.012)	(0.012)	(0.013)
Has loans	−0.023	−0.024	−0.029
	(0.028)	(0.028)	(0.029)
Repayments of loans	−0.400	−0.385	−0.568
	(0.396)	(0.404)	(0.415)

(*Continued*)

Table 2. (*Continued.*)

	No controls		All controls without BC
Loan amount	−0.302	−0.314	−0.396
	(0.327)	(0.331)	(0.342)
(d) Other outcomes			
Asset index	−0.106	−0.119	−0.096
	(0.088)	(0.088)	(0.090)
Food insecurity	0.004	0.007	0.003
	(0.017)	(0.017)	(0.017)
Observations	1,508		1,184

Robust standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$.
Notes: Coefficient estimates from equation (1) are reported for the $QatMig_j$ variable, a dummy of having a migrant in Qatar present in a household pre-embargo. All of the control variables are fixed at pre-embargo period value (i.e., wave 1). Columns 2 and 3 include pre-embargo characteristics: these controls include at the household level: number of members including overseas, logarithm of household's per capita income, an indicator for whether the household is in the 2nd, 3rd, and top quartile of the sample distribution of household's per capita income; at the household head level: age, marital status being single, various indicators related to the highest level of education completed by the household head and head's occupation, at the migrant level: age of migrants, and number of months away. In case of multiple migrants, all of migrant controls are averaged at the household level. Column 3 *without BC* refers to the sample without blockading countries.

The estimates in panel (a) in Table 2 show that the embargo had a negative and statistically significant effect on remittances received. Specifically, a household with a migrant in Qatar is associated with a 56% reduction in remittances compared with households with international migrants in other destinations.[17] Since Bollard et al. (2011) stressed the importance to distinguish the intensive from the extensive margins, we also replace the dependent variable for the fact to remit or not. Our results show that households with at least one international migrant in Qatar in wave 1 are less likely to remit due to the embargo vs. those households with at least one international migrant in another international destination. In column 2, we augment our specification with controls for household size, the logarithm of household's income per capita, an indicator for whether the household is in the 2nd, 3rd, or top distribution of household's per capita income, household's head age and marital status being single, head's highest level of education completed and head's occupation, migrant's age and months the migrant is away (all migrants controls are averaged at the household's level in case of multiple migrants). The estimates remain negative and statistically significant throughout the different columns, though they slightly differ in magnitude. Detailed results for coefficient estimates of the control variables are presented in Table C.2 in the online Appendix. In column 3, we also control for pre-embargo characteristics but drop blockading countries since they are likely to have been affected by the embargo,

[17]To interpret the coefficient's estimate, we follow Halvorsen Palmquist (1980) correction for the case of log-linear form with dummy variables, $(100^*(e^{\beta} - 1))$.

but the estimated coefficient of our main variable remains statistically significant and has a similar magnitude.[18]

4.2. The impact of the embargo on household welfare

Remittances and shocks, in part through remittances, may play a role not only reducing poverty and improving well-being of recipient households but are also likely to have an effect on the economic development of recipient countries. This would be the case if remittances constitute an alternative mean to finance human capital and entrepreneurial investments in migrants' origin households. Also, remittances can be used to increase savings and participation in the income and health scheme via pension and social security system contributions (Cuadros-Meñaca, 2020).

Household income, consumption, and other outcomes. In Table 2, we estimate the main regression of the first-difference model with and without all set of control variables and investigate different outcomes across rows. Panel b of Table 2 includes total income defined as the sum of proceeds from agricultural and non-agricultural sources in addition to proceeds of members in long-term employment. Total expenditure is measured as the sum of expenditure on food, frequent non-food items and infrequent non-food items. Panel c shows non-consumption disbursements such as educational expenditure, health expenditure, whether the household buys land, has loans, the amount of loans, and loan repayments. Panel d explores the effect of the shock on an asset index and food insecurity. The asset index is calculated following Walker et al. (2019). In addition to a long list of household characteristics and durables, the asset index also includes ownership of livestock and household head's age and education as a measure of human capital. Specifically, we include: number of storeys of the house, ownership of dwelling, wall material, roof material, type of toilet, foundation of the house, source of drinking water, type of fuel used for cooking, type of stove used for cooking, ownership of facilities: (telephone, mobile phone, cable TV, email/internet), ownership of durable goods: (radio/cassette/CD player, camera (still/movie), bicycle, motorcycle/scooter, motor car, etc., refrigerator or freezer, washing machine, fans, heaters, television/VCR/VCD player, pressure lamps/petromax, telephone sets cordless/mobile, sewing machine, furniture, rugs, clocks, jewelry (including watches), computer/printer). We measure food insecurity following Coates et al. (2007) by using the questions on the households' food consumption patterns from the survey. Yang (2008) finds that exchange rate shocks, which manifest themselves in part via changes in remittances, have negligible effects on household consumption but large effects on various types of household investments such as child schooling, child labor, and entrepreneurial activity. Karki Nepal (2016) finds an effect on expenditure in child education although this is not translated into improved educational and child labor outcomes. Alcaraz et al. (2012) find that remittance recipient households seem to be credit constrained since they face the negative shock on remittances by sending their children to work. Our estimates do not provide robust evidence that the shock had deteriorated the households' welfare. At best, households are less likely to buy land. By contrast, we do find a surprising positive effect on educational expenditures. Such an effect could

[18]We also obtain similar results when the dependent variable is transformed into inverse hyperbolic sine (Table D.1 in the online Appendix) or when we use propensity score matching combined with a difference-in-difference approach (Caliendo Kopeinig, 2008, online Appendix D.2.)

echo some of the findings showing an increase in schooling as a result of a negative income shock in case of strong substitution effects (Carrillo, 2020; Kruger, 2007; Shah Steinberg, 2017). Also, girls seem to spend less time working. However, we do not find any evidence that the embargo shock has decreased school attendance for children aged 10–17, either for boys or girls. We provide detailed results in Appendix Table D.5 in the online Appendix.[19]

5. Why is the fall in remittances not translating into welfare deterioration?

Given the substantial decline in remittances, it may seem odd at first that we do not find evidence that this reduction is passed on to other outcomes (with the exception of educational expenditures). We explore three main mechanisms, namely the confounding effect of returned migration, the distributional effect of the embargo shocks, and the ability of the most affected households to adopt coping strategies.

5.1. Returned migration

We interpret our findings as a change in the amount of remittances received by households with members residing in Qatar. Another interpretation might be that the trade embargo in Qatar increases the migrant's decision to return. That could also explain the lack of detrimental consequences on the households' welfare. We present here some suggestive evidence that downplay such alternative interpretation. First, we follow Yang (2008) in measuring migrant return rate as the total of returned migrants per household post-shock divided by the number of migrants pre-shock. Panel A in Table D.6 in the online Appendix does not indicate that households with migrants in Qatar have higher returned migration rates after the shock. Second, our main remittances results are confirmed when the sample is restricted to households without returned migrants. Finally, panel B of Table D.6 in the online Appendix shows as a robustness check remittances as well as all our development outcomes divided by household size. By doing so, we want to check if the lack of impact on development outcomes were not due to a large sample size as a result of returned migration. It does not seem to be the case.

5.2. The distributional effect of the embargo shock

Results presented in Table 2 may hide large heterogeneity among the affected households. Based on the dataset we use in this paper, Walker et al. (2019) find that effects of rainfall-based shocks are more severe for poorer households who are more exposed to shocks and less likely to cope with them. Similarly, we will assess whether the effect of the embargo shock on remittances differed for poorer vs. richer households. A simple way to look at relative poverty of households is to divide households into quantiles (Fry et al., 2014). Using a wealth index for the classification is better than income which has some problems of accuracy and measurement (Fry et al., 2014). In addition, using a single asset variable does not give us enough information to determine households' status (Fry et al., 2014). A wealth index is particularly useful in the context of low-income countries, since income is likely to come from diverse sources and vary over seasons while expenditure poses difficulties due to both the high price differences over time and

[19]The extensive margin on child labor cannot be estimated since there is no time variation of that status at the child level.

across areas and individuals' unwillingness to disclose expenditure levels (Howe et al., 2008). We use a principal component analysis (PCA) to construct the wealth index (Filmer Pritchett, 2001).[20]

In Table 3, we estimate the main regression of the first-difference model with the entire set of control variables. We split households into quartiles of wealth index in columns 2–5. The coefficient of the effect of the shock on remittances is much higher in magnitude and statistically significant at the 1% level for poorer households, its magnitude also decreases gradually throughout the quartiles. Therefore, it seems that poorer households were more affected, with their remittances being reduced as a result of the embargo. This is also true when we look at migrants' likelihood to remit due to the embargo. Our results in the last three panels show that households with at least one international migrant in Qatar in wave 1 are less likely to remit due to the embargo vs. those households with at least one international migrant in another international destination. The magnitude of the effect is again larger for poorer households and we do find a gradient in the effect as we move through the different quartiles of wealth index. We nonetheless do not find strong evidence of distributional effects on other outcomes such as the asset index, severe food insecurity, total income, and total expenditures. The same is true when income and expenditures are split into subcategories (agricultural and non-agricultural income; on food and non-food expenditures, health and educational expenditures).[21]

These distributional effects do not seem to be driven by confounded shocks. We indeed know that different shocks occurred during the years in which the survey was undertaken and impacted households' well-being severely (Walker et al., 2019). The more severe and widespread shocks were in 2015 and 2016, whereas fewer and less spread shocks took place in 2017–2018 (Walker et al., 2019). Although it did not happen simultaneously at the time of the embargo, we examine whether its aftermath confounds our main coefficient estimates.[22] Following Walker et al. (2019), we categorize all reported shocks into four categories to deal with the very few observations in some of them: "natural disasters (the earthquake, floods, landslide, drought, fire, hail, lightning); agricultural shocks (pests, post-harvest loss, livestock loss); economic shocks (the blockade, price hikes, personal economic shocks); and health shocks (disease, injury, death)" (Walker et al., 2019, p. 54). We report our estimated results of the first-difference model with all control variables augmented with all shocks in online Appendix Table D.8. Our coefficient estimates are negative and statistically significant both on average and for the poorest quartile. This confirms that our main results are not capturing the effect of other shocks.

[20]The variables from pre-embargo period we include following Filmer Pritchett (2001), when available, are: a household's ownership of consumer durables (clock/watch, bicycle, radio, TV, sewing machine, refrigerator, car, motorcycle), a household's dwelling characteristics (toilet facilities (flush toilet, pit toilet/latrine, none/other)), drinking water sources (pump/well, open source, other source), rooms in dwelling (number of rooms, kitchen as a separate room), building materials including roof's and outside wall's material, availability of lightning, type of fuel used for cooking, and ownership of greater-than-6-Acres land. One limitation of this approach is that weights on individual indicators are not theoretically grounded. See Filmer Pritchett (2001) for more details on limitations, technical details, and assumptions of constructing the wealth index using PCA.

[21]See Table D.7 in the online Appendix.

[22]Since we run a first-difference model, the variables of other shocks are equivalent to including pre-shock value interacted with post.

Table 3. Shock on remittances by quartile of wealth index

	Average	Q1	Q2	Q3	Q4
Intensive margin					
Panel A					
No Controls	−0.834*	−3.603***	−1.846	−1.233	0.170
	(0.451)	(1.088)	(1.511)	(1.161)	(0.756)
Observations	1,508	315	314	314	314
Mean	8.312	8.248	7.351	8.428	9.018
Panel B					
All Controls	−0.760*	−3.287***	−1.351	−1.299	0.190
	(0.454)	(1.092)	(1.649)	(1.162)	(0.756)
Observations	1,508	315	314	314	314
Mean	8.312	8.248	7.351	8.428	9.018
Panel C					
All controls	−0.879*	−3.463***	−1.737	−1.090	0.096
Dropping blockading countries	(0.471)	(1.073)	(1.655)	(1.210)	(0.869)
Observations	1,184	276	245	230	241
Mean	8.089	8.242	7.124	7.845	8.886
Extensive margin					
Panel D					
No Controls	−0.070*	−0.306***	−0.166	−0.103	0.016
	(0.037)	(0.090)	(0.127)	(0.096)	(0.061)
Observations	1,508	315	314	314	314
Mean	0.698	0.708	0.621	0.704	0.736
Panel E					
All Controls	−0.063*	−0.281***	−0.122	−0.109	0.016
	(0.038)	(0.090)	(0.140)	(0.097)	(0.061)
Observations	1,508	315	314	314	314
Mean	0.698	0.708	0.621	0.704	0.736
Panel F					
All Controls	−0.070*	−0.297***	−0.151	−0.086	0.010
Dropping blockading countries	(0.039)	(0.088)	(0.142)	(0.101)	(0.071)
Observations	1,184	276	245	230	241
Mean	0.682	0.710	0.608	0.657	0.726

Robust standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Notes: Coefficient estimates from equation (1) are reported for the $QatMig_j$ variable, a dummy of having a migrant in Qatar present in a household pre-embargo. All controls are as defined in Table 2. Results are reported on average and by quartiles of wealth index across columns.

5.3. How do households cope with the remittance shocks?

We explore different channels that could explain why the remittances effect seems to be driven by poor households. We start by controlling for access to credit to see if the effect of the shock on remittances differs whether or not the household has access to credit. We control for access to credit using ownership of bank account pre-embargo in panel A of Table 4. We control for a dummy on whether the household had a bank account in wave 1, and an interaction of this dummy with the shock variable. The effect is only observed for poorer households, and the ownership of the bank account reduces the severity of the shock (−4.799 + 4.472). In panel B, we use the asset index – as a proxy for the ability to use a collateral – interacted with the treatment variable. Those who own some assets are also better able to cope with the shock but only partially. Overall, initial credit constraints seem to matter in the ability to deal with the fall in remittances. In panel C, we implement the same exercise using the information related to whether there is any member of the household which belongs to a community group. We aim at capturing some (informal) insurance mechanism. We do not find strong evidence for such a mitigating effect. In Table D.7 in the online Appendix, we use sale of land as an outcome variable to examine whether households cope by selling their land. As the coefficient estimate shows, there is no evidence that this is the case. Overall, our results suggest that poor households who had a bank account pre-embargo suffered less than their counterparts without one. That being said, we should acknowledge that our data are too limited to explore further the possible role of (formal or informal) insurance mechanisms either at the community or family levels.

5.4. Migratory responses

Another possible explanation is that households may respond differently in their ability to move their migrants or other household members. In panel A of Table 5, we look at migratory responses after the shock by quartile of wealth index. This allows us to understand whether migrant returns differed between poor and rich households. Similar to Yang (2008), we use a measure of migrant return rate as a total of returned migrants per household post-shock divided by number of migrants pre-shock. The results show that returned migration from Qatar increased for poor households but declined for richer households after the shock. In panels B and C of Table 5, we look at the change in the number of migrants to Qatar and other international destinations due to the embargo, respectively. Intuitively, the number of migrants to Qatar decreased and it increased to other international destinations. Although this effect is observed on average and for all quartiles, the magnitudes differ between the rich and the poor. Particularly, the coefficient estimates suggest that although all households exposed to the shock decreased their migrants in Qatar, the magnitude is lower for richer households. Similarly, the increase in migrants to other destinations was higher for richer households. This again points out that the poor are more vulnerable and face greater difficulties in re-allocating their migrants or other household members to other destinations.[23]

[23]Due to small sample size, we cannot identify statistically who (existing migrants in Qatar or household members) is most likely to move as a reaction to the embargo shock. From a descriptive point of view, we know in our sample that the number of Nepali migrants in Qatar decreased from 244 to 193 individuals

Table 4. Coping strategies interacted with shock

Dependent variable: D.lremit	Average	Q1	Q2	Q3	Q4
Panel A					
$QatMig_j$	−1.021	−4.799***	−2.904	−0.734	1.526
	(0.666)	(1.348)	(2.081)	(1.728)	(1.181)
w1 bank acct	−0.472	−2.249***	−0.217	0.937	0.394
	(0.370)	(0.844)	(0.943)	(0.820)	(0.853)
bank*$QatMig_j$	0.592	4.472**	5.052*	−1.119	−2.233
	(0.903)	(2.157)	(2.942)	(2.357)	(1.532)
Panel B					
$QatMig_j$	−1.017**	7.217	−0.113	−1.490	−0.002
	(0.476)	(4.890)	(4.868)	(1.188)	(1.678)
w1 asset index	−0.054	−1.430**	−0.666	0.703*	0.136
	(0.074)	(0.667)	(0.582)	(0.384)	(0.310)
assetindex*$QatMig_j$	0.534***	3.762**	0.536	−0.210	0.051
	(0.173)	(1.845)	(2.043)	(1.023)	(0.489)
Panel C					
$QatMig_j$	−0.235	−3.730**	−3.817	−2.082	0.903
	(0.518)	(1.634)	(2.609)	(1.542)	(0.790)
w1 network	−0.016	−0.543	−0.669	0.372	0.405
	(0.373)	(0.786)	(0.799)	(0.805)	(0.982)
network*$QatMig_j$	−1.523	0.744	3.489	2.030	−4.624*
	(1.012)	(2.208)	(3.334)	(2.373)	(2.458)
Observations	1,508	315	314	314	314

Robust standard errors in parentheses.
***$p < 0.01$, ** $p < 0.05$, *$p < 0.1$.
Notes: Coefficient estimates from equation (1) are reported for the $QatMig_j$ variable, a dummy of having a migrant in Qatar present in a household pre-embargo in addition to its interaction with potential coping strategy variables across different panels. Results are reported on average and by quartiles of wealth index across columns. All regressions include all control variables, as defined in Table 2.

between 2016 (wave 1) and 2018 (wave 3). Among these 244 migrants, about 19% returned to Nepal while about 6% moved somewhere else (Table D.5 in the online Appendix). Compared to other top major destinations (India, Malaysia, Saudi Arabia), the rate of return is not higher for those in Qatar compared to those in other top destinations like India (18%), Malaysia (26%), and Saudi Arabia (23%) but the share of migrants moving to another location is higher (1.5%, 3.3%, and 4.1%). It suggests that for some households, the relocation of the migrant to another destination might have been a source of adjustment. But the majority of households seem to adjust to the shock by sending other household members abroad. Among the household members that stay in Nepal (Table D.4 in the online Appendix), 37% moved out by wave 3. It is a much higher proportion than similar figures of other top destinations such as India (34%), Malaysia (21%), and Saudi Arabia (22%).

Table 5. Shock on returned migration and number of migrants

	Average	Q1	Q2	Q3	Q4
A. Share of returned migration					
$QatMig_j$	0.007	0.235***	0.126	0.072	−0.099***
	(0.031)	(0.084)	(0.168)	(0.071)	(0.037)
Observations	1,316	279	268	276	274
Mean	0.171	0.148	0.229	0.165	0.129
B. Δ in number of migrants to Qatar					
$QatMig_j$	−0.361***	−0.594***	−0.578***	−0.439***	−0.122**
	(0.035)	(0.086)	(0.090)	(0.078)	(0.057)
Observations	1,508	315	314	314	314
Mean	0.139	0.086	0.073	0.121	0.245
C. Δ in number of migrants to Other					
$QatMig_j$	0.438***	0.368***	0.502***	0.476***	0.364***
	(0.039)	(0.114)	(0.096)	(0.101)	(0.060)
Observations	1,508	315	314	314	314
Mean	0.843	1.102	0.838	0.838	0.713
D. Log remittances from Qatar					
$QatMig_j$	−3.590***	−5.460***	−4.666***	−4.304***	−2.107***
	(0.428)	(1.033)	(1.387)	(1.060)	(0.657)
Observations	1,508	315	314	314	314
Mean	1.484	0.849	0.799	1.233	2.581
E. Log remittances from Other					
$QatMig_j$	3.248***	2.197***	3.779***	3.458***	2.883***
	(0.298)	(0.746)	(0.807)	(0.711)	(0.605)
Observations	1,508	315	314	314	314
Mean	6.746	7.295	6.390	7.086	6.498

Robust standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Notes: Coefficient estimates are reported for the $QatMig_j$ variable, a dummy of having a migrant in Qatar present in a household pre-embargo. The dependent variables are different proxies for returned migration across panels. All controls are as defined in Table 2. Results are reported on average and by quartiles of wealth index across columns.

Finally, panel D of Table 5 shows how remittances from Qatar fall due to the embargo for households with at least one migrant in Qatar in wave 1 vs. households with at least one migrant in another international destination. This effect is much larger for poorer households and sample means allow us to observe a declining gradient (that the magnitude of the effect declines gradually) as we move throughout the different quartiles. Panel E of Table 5 shows that households with migrants in Qatar – in particular richer households – receive more remittances from other countries, suggesting some compensation from the fall in remittances from Qatar.

In our estimated sample we have 1,285 households with one international migrant (1,045 HH with an international migrant somewhere else and 240 HH with one international migrant in Qatar in wave 1) and 223 households with more than one migrant in an international destination. Given that there are no households in our sample with migrants in Qatar and somewhere else prior to the embargo, that compensating effect is better explained by migrant reallocation rather than other ex-ante diversification strategy in terms of migrants' destinations. Descriptive statistics show that after the embargo, 80.23% of households return their migrants back to Nepal and 17.44% of households move their migrants from Qatar to another international destination. The remaining 2.33% of households that used to have a migrant in Qatar have returned the migrant home and have moved another family member somewhere else abroad.

6. Conclusion

International remittances are transfers of money migrants send to their households in their country of origin. They often consist of very small magnitudes for which they pay a large fee and are done at relatively high frequencies. Remittances are likely to help recipient households overcome credit and liquidity constraints and reduce their risk of falling into poverty. They also have the potential to have an effect on economic development. Remittances in Nepal represent over a quarter of GDP and are the highest source of received foreign income. This paper examined the effect of the unexpected embargo on Qatar in 2017 on remittances received by households in Nepal. Qatar is the second largest destination for Nepali migrants, giving us a suitable context to shed light on how shocks to migrants in host countries affect their origin households.

Using the Household Risk and Vulnerability Survey in Nepal, we follow Yang (2008) and estimate a first-difference model augmented with pre-shock characteristics. By limiting our sample to households with international migrants before the embargo, we deal with the selection into migration problem. In addition, the shock affected migrants in Qatar exclusively compared to migrants in other international destinations, giving us a clean control group. We argue that these allow us to identify the causal effect of the embargo on remittances. Our estimates show that the shock resulted in a 56% fall in remittances for households with a migrant in Qatar. Moreover, we find that poor households suffered the most from the decline in remittances. This result aligns with Walker et al. (2019) who also find that poor households are more vulnerable to shocks and are less likely to cope.

Contrary to Yang (2008), the change in remittances does not seem to translate into major disruptions in the household's source of livelihood. Against alternative explanations, we find that households with less credit constraints in the pre-embargo period could somehow mitigate the shock. Furthermore, since migrants in Qatar from poorer households were more likely to return, this suggests that the impact of poverty spills over to migrants even if they work in rich destinations. Our results are also informative about migration outflows. Nepali migrants seem to shift their location from Qatar, a country which experiences an embargo, to other international destinations. It is also interesting to find out the type of households that are better able to adjust their overseas destinations after the embargo. Poorer households seem less able to adjust by sending their migrants or other household members to other international destinations.

Given the high dependencies of some low- and middle-income countries like Nepal on remittances, helping households to deal with such an external shock by extending access to credit or facilitating other migratory adjustments could be a way forward. However, we acknowledge the limitations of our study given the short nature of the analysis. Further research could shed light on the long-term consequences of the embargo if new data become available. We cannot exclude that in the long run, a reduction of remittances could affect investments in human capital and other assets and deteriorate the country's economic development. We cannot also exclude the development consequences from a negative shock – like in our case – differ from those from a positive change like in Yang (2008). Additional case studies would help us to assess the external validity of the existing research.

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