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Consanguinity and Other Marriage Market Effects of a Wealth Shock in Bangladesh

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Abstract This paper uses a wealth shock from the construction of a flood protection embankment in rural Bangladesh coupled with data on the universe of all 52,000 marriage decisions between 1982 and 1996 to examine changes in marital prospects for households protected by the embankment relative to unprotected households living on the other side of the river. We use difference-in-difference specifications to document that brides from protected households commanded larger dowries, married wealthier households, and became less likely to marry biological relatives. Financial liquidity-constrained households appear to use within-family marriage (in which one can promise ex-post payments) as a form of credit to meet up-front dowry demands, but the resultant wealth shock for households protected by the embankment relaxed this need to marry consanguineously. Our results shed light on the socioeconomic roots of consanguinity, which carries health risks for offspring but can also carry substantial benefits for the families involved.

Keywords Marriage · Embankment · Flood protection · Consanguinity

Introduction

A woman's marital prospects have important implications for her subsequent life outcomes. Conditions of marriage—such as dowries, marrying biological relatives, age at

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marriage, and spousal wealth—affect socioeconomic outcomes for the woman and her children, including the likelihood that she will have to endure domestic violence, her status in her husband's home, her health and scholastic attainment, and her control over reproductive choices (Bloch and Rao 2002; Field and Ambrus 2008; Jahan 1990; Jensen and Thornton 2003; Tiemoko 2001; Wickrama and Lorenz 2002). Marrying a cousin or uncle—a surprisingly common practice around the developing world, accounting for more than 50 % of all unions in some countries—can decrease the amount of dowry required but increases the risk of genetic diseases among offspring. Understanding the determinants of these conditions of marriage is thus important.

In this article, we study the construction of a flood protection embankment in rural Bangladesh coupled with pre- and post-embankment data on 33,000 marriages to examine how a plausibly exogenous change in wealth manifests itself in changes to marriage conditions. This embankment induced a discrete improvement in socioeconomic conditions for families living on the embankment side of the river relative to the opposite bank that remained unprotected. Its major effect was to extend the crop-growing season, thereby increasing wealth for households on the protected side. We investigate differential changes in the conditions of marriage for protected households using panel data on the entire universe of marriages in treatment and control villages across a 14-year period pre- and post-embankment.

Marrying a biological relative[1] increases the likelihood that offspring will receive two copies of a deleterious gene from parents, which results in higher morbidity and mortality rates (Bittles and Makov 1988; Shah et al. 1998). Social scientists have a limited understanding of why so many couples accept these risks. We use difference-in-differences (DD) analysis coupled with the exogenous variation in wealth to test some implications of a model of consanguinity as a response to credit constraints (Do et al. forthcoming). We provide evidence that marrying consanguineously reduces the need for dowry payments. Because a bride's parents often have neither cash on hand nor access to credit to make an up-front dowry payment, they use within-family marriage (where it becomes possible to promise ex-post payments) as a form of credit. Given that protection from flooding increases wealth, the need for consanguineous marriage is reduced for protected households following embankment construction.

Our article thus contributes to a literature that attempts to uncover the economic motivations behind specific marriage practices, such as polygyny (Gould et al. 2008; Jacoby 1995), endogamy (Edlund 1999), dowry (Botticini and Siow 2003), and exchange marriage (Jacoby and Mansuri 2010). Our article also fits in a literature that studies the effects of policy changes and shocks on longer-run intrahousehold marriage and social outcomes (e.g., Ambrus et al. 2010; Deininger et al. 2010). Finally, this article is also related to an emerging empirical literature on the determinants of spousal matching (Banerjee et al. 2009; Hitsch et al. 2010; Wong 2003).

[1] In the mainly Muslim countries of North Africa and West and Central Asia, and in large parts of South Asia, marriages between close relatives account for between 20 % to 50 % of all unions, with an additional 2.8 billion people in countries where 1 % to 10 % of marriages are between biological relatives (http://www.consang.net). Cousin marriage appears to be a social norm in Pakistan, Afghanistan, Iraq, and Yemen, where about 50 % of marriages are between first cousins (Bittles 1994, 2001; Caldwell et al. 1983; New York Times 2003; Rowlatt 2005).

The key identifying assumption underlying our DD empirical strategy is that changes in marriage patterns over time on the unprotected side of the river constitute a valid counterfactual for the changes in marriage patterns on the protected side. We establish the credibility of this strategy by examining baseline differences in outcomes and common pretreatment trends. We check sensitivity of results to household fixed-effects analysis (using the subset of families with multiple marriages before and after embankment construction) as well as on collapsed data (one pre-embankment and one post-embankment observation) that minimizes serial correlation (Bertrand et al. 2004). All our specifications add fixed effects for every year of analysis so that the effects we report are identified from a discrete jump tied to embankment construction, controlling for the longer-term trend.

The next section builds on the literature on marriage and family to develop theoretical expectations of the effects of embankment construction on marriage outcomes more broadly and on consanguinity in particular. We then describe the setting and our data sources. Following that, we present empirical results that test our identification assumptions, showing how the embankment affects first-stage economic outcomes. We then present results on a broad set of marriage outcomes: dowries, age at marriage, and spousal wealth. After we document the changes to consanguinity rates and try to uncover the structural changes that led to the large drop in consanguinity, we conduct sensitivity analyses and offer conclusions.

Theory

Studies of marriage in the developing world continue to be shaped by William J. Goode's theory of modernization and marital change (Goode 1963). Goode argued that the waning power of families in dictating the conditions of marriage would lead to the demise of traditional marriage patterns, including early female age at marriage, large age gaps between spouses, dowry or other wealth transfers at the time of marriage, and marriage between blood relatives. Subsequent research found considerable support for the decline of some traditional marriage patterns, most notably early marriage (Mensch et al. 2005). Yet, the institution of dowry has proven resilient and even resurgent in many parts of the world, particularly in South Asia (Anderson 2007; Caldwell et al. 1983; Rao 1993). Consanguinity has also remained resilient in spite of the adverse biological risks for children (Grant and Bittles 1997).

In part, the resilience of traditional marriage practices derives from the persistent role of families in marital decisions and in the postmarital social and economic life of married couples (Rosenzweig and Stark 1989). Economic theory highlights the marriage matching process based on costs and benefits to marital partners and their families (Becker 1973, 1991). Caldwell et al. (1983) described these dynamics from a demographic and anthropological tradition, highlighting the role of marriage transactions in providing economic security. The literature has since tried to model various conditions of marriage, including dowry (Rao 1993), consanguinity (Givens and Hirschman 1994), spousal wealth and occupation (Fafchamps and Quisumbing 2005; Rosenzweig and Stark 1989), and spousal education (Esteve and McCaa 2008; Han 2010), allowing for asymmetric effects by gender (Amin and Cain 1997; Protik and Kuhn 2006).

Theoretical Model

We develop stylized two-sided matching models to help frame how the wealth and risk mitigation benefits of the embankment affect marriage decisions. Banerjee et al. (2009), Hitsch et al. (2010), and Wong (2003) have used information from online dating sites and newspaper advertisements to demonstrate that such models can simulate actual outcomes quite well. We discuss the basic predictions of the models here, and Online Resource 1 provides details on our two-step modeling approach to describe matches when multiple attributes of embankment protection can affect outcomes:

1. We derive analytical predictions on matching in a marriage market where each person has only two discrete attributes: embankment protection status and wealth status. To generate analytical solutions, we follow the literature (e.g., Siow 1998; Weiss 1997), and assume that there exists a medium of exchange (such as dowry) that can be used to transfer utility from the bride to the groom.
2. We then relax this assumption on transferable utility, endow each person with multiple continuous characteristics, and simulate stable matches in a larger marriage market characterized by search frictions.

These models predict that the protected are likely to secure better matches only along characteristics that are complementary. For example, if the wealth that a man and woman bring to a marriage represents complementary inputs toward generating marital surplus, then those living on the protected side would, in general, choose (and be able) to marry into wealthier households. However, characteristics that are not complementary inputs— such as age at marriage or age gaps—should remain unaffected because the protected are not willing to pay relatively more than the unprotected for this characteristic. Furthermore, if the embankment's primary contribution is to lower flood risk exposure, we should observe negative assortative matching in protection post-embankment (i.e., more cross-river marriages). The unprotected have the largest marginal gain from bonding with a protected family, and are therefore willing to pay the most to secure that match. Search frictions across the river may dampen this effect. A corollary is that protected men should receive larger dowries. These simple matching models are useful for interpreting the basic DD program evaluation results, but they do not attempt to model consanguinity.

Consanguinity

Consanguinity remains a surprisingly common practice in much of the developing world, even though the genetic risks for the offspring of the union of biologically close relatives are well understood in the scientific community (Bittles and Neel 1994; Grant and Bittles 1997; New York Times 2003). Estimates of excess birth defects in first cousin progeny have ranged from 0.7 % to 7.5 % (Zlotogora 2002). Given these health risks, it is important to understand why so many households marry biological relatives.

The socioeconomic benefits associated with consanguineous marriage may help to explain its prevalence despite the documented health risks. The literature speculates about various reasons why marriage to a biological relative may be preferred, including a strong family tradition, lower spousal search and matching costs, strengthening of family ties, closer relationships between the wife and her in-laws, greater autonomy for women, lower risk of divorce, and property retention within a family (Bittles 1994; Sandridge et al. 2010). Caldwell et al. (1983) and Bittles (1994) have suggested that households marry within the family in South Asia to avoid large dowry payments at the time of marriage. Do et al. (forthcoming) modeled a marriage market failure in which families pledge wealth to improve the quality of the match but cannot credibly commit to future transfers. An *ex ante* transfer (dowry) or a consanguineous marriage contract between relatives for whom a long-term contract is enforceable are two substitute solutions to the market failure.[2] Families facing a credit constraint (who find it more expensive to pay dowries) would be more likely to engage in consanguinity as a way to delay payments. The promise to pay over a longer period is more credible when made within the family.

An exogenous shock to wealth that relieves the liquidity/credit constraint (such as that afforded by the embankment) creates a natural experiment that allows us to test the central implication of this model. We do so by constructing a DD estimate of changes in consanguinity in protected and unprotected areas before and after construction of the embankment. Furthermore, because the liquidity constraint binds only the bride's family (because only the bride pays dowry), gender-disaggregated analysis of the embankment's effect provides a triple-difference test of this implication. Although we show evidence in favor of a few specific reduced-form implications of this model, other models of the marriage market may yield the same types of relationships among wealth, consanguinity, and dowries.

Setting and Data

Since 1963, the International Centre for Diarrhoeal Disease Research, Bangladesh (ICDDR,B) has conducted periodic socioeconomic censuses and recorded all vital events of residents in 148 villages in Matlab district. During the study period, residents of this rural area were mostly poor and landless, subsisting off fishing, agricultural labor, and sharecropping.

Using ICDDR,B data, we observe all 33,000 marriages (or 52,000 marriage decisions) of Matlab residents between 1982 and 1996, and we merge census data from 1982 to these marriage files. Note that these are demographic surveillance data (on the basis of visiting all households in the surveillance area on a frequent basis), and we therefore observe all marriages, regardless of where that marriage is registered. When a Matlab resident marries someone outside the district, we observe that marriage but only from one side.[3] A within-Matlab marriage is observed from both

[2] Conversations during our fieldwork in rural Bangladesh in 2006 also indicated that sometimes this long-term contractual solution is mediated by the intergenerational link in inheritance, which makes the contract easier to enforce. A male cousin may agree to marry a cousin who cannot afford to pay dowries on the promise of a larger future bequest by the grandfather.

[3] Consanguinity status is not recorded in such cases. This is potentially an important selection issue, and we verify that there is no statistically significant difference in an indicator for "missing consanguinity information" across protected and unprotected areas either overall (p value = .29), or during the pre-embankment period. This variable is not significantly affected by embankment construction (protected × post) either.

sides. We know the age at marriage of each individual and his or her spouse; any biological relationship between them (i.e., consanguinity status); and their wealth (including landownership), occupation, and location (and, therefore, embankment protection status).

We supplement these data with retrospective dowry information, cropping practices, and land value information reported in the 1996 Matlab Health and Socioeconomic Survey (MHSS; Rahman et al. 1996). The MHSS cross-sectional data set, which covers a random subsample of more than 4,000 households from Matlab, asks all respondents to recall dowries exchanged during past marriage transactions (thus covering marriages both before and after embankment construction). We use the estimated cash value of dowries as reported by women who are interviewed separately from their husbands. Table S1 in Online Resource 1 lists the variables used in this study and their definitions.

The Meghna-Dhonagoda Embankment: First-Order Effects and Validity as a Natural Experiment

The Meghna-Dhonagoda River runs through the middle of the study area, and the Water and Power Development Authority in Bangladesh used external donor funds in 1987 to construct a 65 km embankment along the northwest bank (see Fig. 1) that prevents water overflow and provides systems for pumped drainage and irrigation along the waterway (Strong and Minkin 1992). The embankment was breached during abnormally high floods in 1987 and 1988, after which it was strengthened and resealed in 1989. Consequently, in our empirical specifications, the pre-embankment period is 1982–1986, and the post-embankment period covers 1989–1996. Before establishing the first-order effects of the embankment on wealth, we examine whether the embankment can be treated as an exogenous shock.

What the Embankment Construction Helps Identify

The most important concern is that people residing on the southeast bank of the river are not an appropriate control group for the "treated" households because the placement of the embankment on the northwest bank may itself signal some preexisting differences between the two groups.

During fieldwork interviews we conducted in 2006, Matlab residents indicated that the embankment was placed on the northwest side mainly because drainage was worse in that bank prior to embankment construction. Residents also expressed complaints that the project was coordinated by politicians in conjunction with the largest local landowners who actually live in Dhaka (Briscoe 1998; Kabir 2004). Neither politicians nor large landowners are part of our sample because they do not live in Matlab district. However, other forms of preferential treatment for the protected side of the river may be possible, and we therefore delve into the program evaluation and anthropological literatures to examine these issues.

One possibility is that agriculture practices are systematically different across the two banks because of drainage differences. However, program evaluations of the

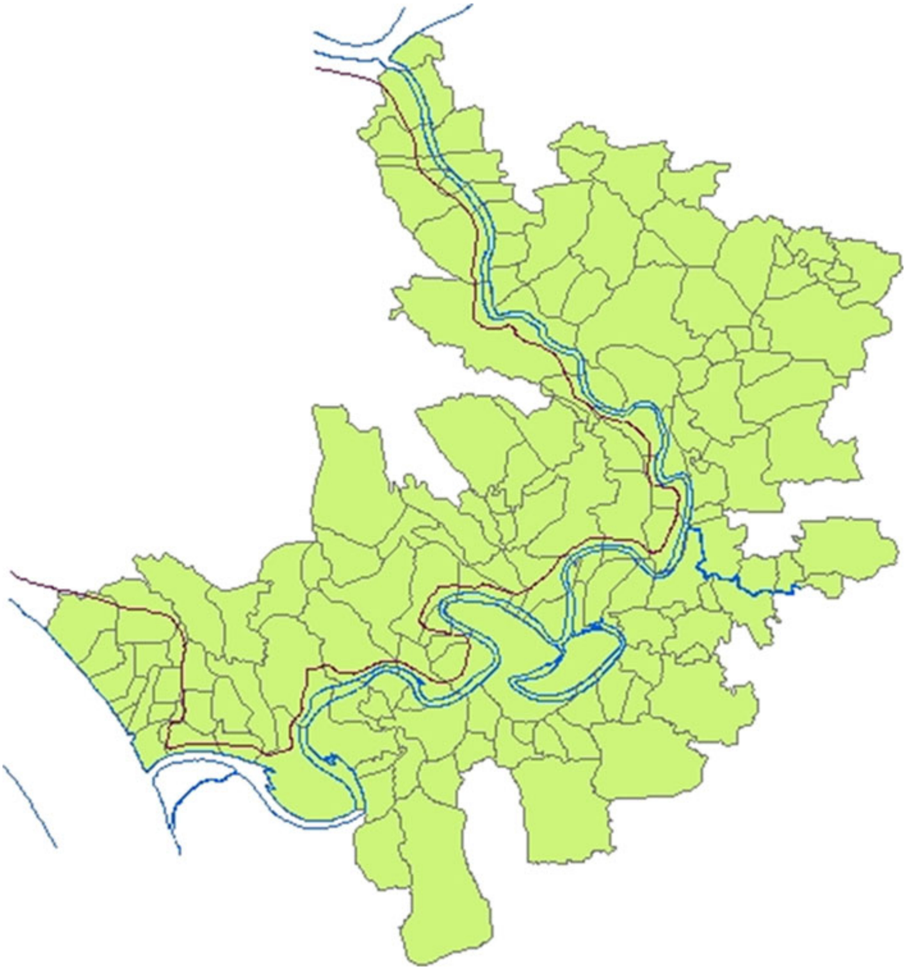

Fig. 1 Matlab Surveillance Area, the river and the embankment. The light gray polygons are villages, the double line is the river, and the thin line is the embankment

embankment project report that the geographic areas experienced similar weather patterns, households grew similar crops and reported similar incomes, and demographic distributions were nearly identical. Furthermore, we find no mention of any other significant differences in technology or other construction, such as bridges, that may have happened to coincide with embankment construction (Briscoe 1998; Strong and Minkin 1992; Thompson and Sultana 1996).[4] Descriptive statistics from our own data show that the protected and unprotected groups are similar prior to construction along most observable dimensions (see Table 1). We present statistical test of

[4] Because Matlab is part of a demographic surveillance area run by the International Centre for Diarrheal Disease Research (ICDDR,B), the population has been exposed to some development programs. Our results are robust to controlling for exposure to the largest of these programs, the Matlab Maternal and Child Health and Family Planning program and the Safe Motherhood Initiative.

Table 1 Means of pre-embankment (1982–1986)[a] variables

Variable	Protected	Unprotected	Difference[b]
% of Consanguineous Marriages	0.08	0.07	0.01
Female Age at Marriage	19.04	19.00	0.04
Male Age at Marriage	25.92	25.65	0.27
Household Land Owned (1982)	11.12	10.28	0.84
Land Owned by Spouse's Household (1982)	10.82	9.89	0.93*
% of Marriages to Spouse From Outside Matlab	0.48	0.58	−0.10**
% of Marriages to Spouse From Outside Village	0.86	0.88	−0.02
Hindu	0.07	0.15	−0.08**
Farmer	0.74	0.73	0.01
Youngest Child	0.61	0.61	0.00
Oldest Child	0.18	0.16	0.01**
Household Size (1982)	6.67	6.72	−0.06
Average Number of Marriages per Household	1.76	1.76	−0.00
Share of Households Paying Dowry	0.52	0.47	−0.04
Average Size of Dowry (in taka; 1996 = 100)	8,621.253	7,190.5	−1,430.8
Total Marriage Observations	5,226	11,051	
Village Characteristics			
Village population	1,640.13	1,184.66	455.47
Female population	811.91	583.13	228.77
Male population	828.19	601.52	226.67
Population, age <10	0.29	0.29	−0.00
Population, age 10–25	0.34	0.35	−0.00
Population, age 25–50	0.24	0.24	0.00
Population, age >50	0.13	0.12	0.01
Hindu	0.04	0.15	−0.10**
Large market (= 1 if village has a large market)	0.03	0.08	−0.05
Distance to the nearest large market (km)	1.97	1.76	0.21
Travel time to the nearest large market (minutes)	32.69	27.08	5.61
Small market (= 1 if village has a small market)	0.16	0.20	−0.04
Distance to the nearest small market (km)	0.56	0.65	−0.09
Travel time to the nearest small market (minutes)	13.47	14.51	−1.04
Number of villages	32	92	

Notes: For the household size and the average number of marriages per households, households with more than one marriage outcomes are considered. Retrospective data on dowries are from 1996 MHSS data set. The difference in Hindu population is due to high concentration of Hindu households (more than 80 % of the total village population) in eight villages.

[a] The period is 1985–1986 for consanguinity and 1982–1986 for all other variables.

[b] Differences between the means were tested by regressing each variable on the binary variable "protected" (using clustered standard errors).

*$p \leq .05$; **$p \leq .01$

differences prior to embankment construction at the level of variation in the data that we will use for our analysis (clustered by village), finding no significant differences at baseline in terms of the consanguinity rate, male and female ages at marriage, landownership, the propensity to marry outside the village, household size, or number of marriages per household. We also report statistical tests based on a large set of factors from the 1996 MHSS—such as share of households paying dowry; average size of dowry; village demographics; and location, distance from, and access to markets—and find no statistical differences between the protected and unprotected set of villages.

Spousal landownership exhibits a statistically significant difference, and the magnitude of this difference is about 8 % of the average sample value. Because our DD estimation strategy will rely on relative changes in marriage outcomes across the protected and unprotected banks between pre- and post-embankment periods, it is important to examine whether the assumption of "parallel trends" holds for this variable. Table 2 conducts a rigorous version of this test by running regressions of the following form for the two to five years of data prior to embankment construction:

$$Outcome\left(Y_{it}\right) = \gamma_{t-5} + \gamma_{t-4} + \ldots + \gamma_{t-1} + \beta_0\left(Protected\right)_i$$
$$+ \beta_5\left(Protected\right)_i \cdot \gamma_{t-5} + \beta_4\left(Protected\right)_i \cdot \gamma_{t-4} + \ldots + \beta_1\left(Protected\right)_i \cdot \gamma_{t-1} + e.$$

The set of interaction terms between the indicator for a household living on the side of the river where the embankment *will be* built $(Protected)_i$ with a dummy variable for each pre-embankment year is the most flexible way to look for differences in the trends in preconstruction characteristics across the two sides of the river. In four of the five pre-embankment years, there is no statistical difference in spousal landownership between the two banks, and the point estimates go in both directions, depending on the year. The pre-embankment difference in spousal landownership from Table 1 is entirely driven by just one unusual year of data. We also conduct a more simple linear trends test and verify that there is no statistically significant difference in the linear trend across the two banks of the river during the pre-embankment period.

Table 2 also shows that protected and unprotected trends in the consanguinity rate prior to embankment construction are not statistically different, and the magnitudes of the differences are very small when compared with the actual embankment effect estimates we present later.[5] Any relative changes in trends post-embankment that our DD estimates uncover are therefore not merely a continuation of preexisting differences in trends. There is also no clear trend in dowries during the pre-embankment period.

[5] Fig. S3 in Online Resource 1 graphs the pre- and post-embankment consanguinity rates. Consanguinity falls on both sides of the river in the year prior to embankment construction, but there is no differential decrease on the protected side. Moreover, the preexisting trends are also not statistically different when broken down by gender (results available upon request).

Table 2 Preexisting differences in trends in marriage outcomes by protection status

	1 Consanguineous Marriage	2 Spouse Above Average Land	3 Spouse Different Village	4 Size of the Dowry Payment (in taka)
Protected × Year 1		−0.023	−0.066†	464.891
		(0.037)	(0.034)	(1,098.759)
Protected × Year 2		0.012	−0.015	1,616.739
		(0.030)	(0.023)	(1,105.236)
Protected × Year 3		0.003	0.022	1,889.607†
		(0.028)	(0.019)	(998.048)
Protected × Year 4	0.011	0.086**	−0.013	−821.899
	(0.013)	(0.028)	(0.022)	(731.030)
Protected × Year 5	0.015	0.010	−0.008	299.003
	(0.013)	(0.026)	(0.021)	(1,124.718)
Above Average Amount of Land		0.051**		
		(0.017)		
Land Owned (in decimals)	−0.000		0.001**	
	(0.000)		(0.000)	
Observations	7,130	6,199	14,938	10,092

Notes: All models, except for 4, are estimated using probit model in which the dependent variables are binary variables indicating whether a marriage was consanguineous (column 1); whether a spouse owns above average amount of land (column 2); and whether spouse is from a different village (column 3). In column 4, the dependent variable is the size of the dowry payment (including 0 for if no dowry was paid), reported in taka; the model is estimated using OLS regression. Shown are marginal effects, estimated at the means. Year dummy variables (1982–1986) are included but not shown. The Year 1–Year 5 variables in the table refer to 1982–1986. Data on consanguinity are available only from 1985 onward. Robust standard errors are shown in parentheses.

$^{\dagger}p \leq .10$; $^{**}p \leq .01$

Table 1 does, however, show a statistically and quantitatively significant level difference in the propensity to marry outside the subdistrict (Matlab) across the two banks prior to embankment construction. For this variable again, we confirm that this is just a level difference: there is no statistical difference in the pre-embankment trends in the propensity to marry outside the subdistrict across the two banks of the river. Nevertheless, we refrain from using this as an outcome variable in our analysis of the effects of the embankment. The protected bank also has a smaller Hindu population. We either control for religion or study the subsample of Muslims in some specification checks in order to show that the results we report are robust.

Thus, although the embankment construction is far from a manipulated experiment, the search for possible threats to empirical identification indicates that it is a plausibly exogenous source of variation in a DD setting.

First-Order Effects of the Embankment on Income, Wealth, and Risk

Before turning to marriage outcomes, we examine the first-order effects of embankment construction on economic outcomes using regressions of the form

$$Y_{it} = \alpha + \beta_1(Protected)_i + \beta_2(Post)_t + \beta_3(Post \times Protected)_{it} + e_{it}$$

whenever we have panel data, or

$$Y_{it} = \alpha + \beta_1(Protected)_i + e_i$$

for any outcome for which we have only cross-sectional data.

Frequent flooding in Matlab destroys crops and induces volatility in household income, and the embankment provides security by extending growing seasons and increasing farm incomes for protected agricultural households. During fieldwork we conducted in December 2006, Matlab residents often reported that the primary effect of the embankment was to increase the number of crop cycles from only one per calendar year to two or three because the monsoon swelling of rivers would encroach on the agricultural land as long as three to five months per year prior to embankment construction. Consistent with these informal interviews, Matlab data from 1996 indicate that protected rice farmers enjoyed almost one extra growing season per calendar year compared with farmers on the other side of the river (*t* test significant at the 1 % level). Thompson and Sultana (1996) mentioned that the largest effects of flood protection projects should be on monsoon crops, and accordingly, our data show that protected farmers grew 2–3 times more Aman and Aus paddy (the two varieties grown during the monsoon) per decimal of cultivated land compared with unprotected farmers (*t* tests significant at the 5 % level). Meanwhile, the embankment had no discernible effect on yields for the dry season Boro paddy.

Protected farmers should become wealthier from these large increases in rice yields. We measure this effect directly by using principal components analysis to construct asset indices for household wealth in 1982 and 1996 (see Table 3).[6] Compared with unprotected households, protected households experienced a greater increase (significant at the 5 % level) in asset ownership between 1982 (pre-embankment) and 1996 (post-embankment). Moreover, this change was driven almost entirely by farmers.[7] A hedonic regression of the value of land using cross-sectional 1996 data reveals that post-embankment, the unit price of land was more than 3,000 taka higher per decimal on the

[6] The index measures household ownership of the following assets: a radio, a watch or clock, a bicycle, cows, and a hurricane lamp. Data are taken from Matlab DSS 1982 and 1996 household censuses. Occupation of household head is kept as reported in 1982.

[7] Both landowners and tenant farmers benefited in significant ways, which indicates that the extended growing season on the protected side probably increased both the productive capacity of land as well as the demand for agricultural labor. From Table 3, it may appear that the effect on tenants was larger than that on landowners, but this is because the index is capped when an individual reports owning all such items. Wealthier landlords attained the maximum value even pre-embankment, and the change in their index value was therefore smaller.

Table 3 Effects of the embankment on wealth using census data

		Growth in the Asset Index 1982–1996		
		Protected	Unprotected	Difference Across Protected/ Unprotected
All Occupations	Mean	0.39	0.3	0.085*
	SE	(0.02)	(0.01)	(0.04)
	Observations	5,460	11,656	17,116
Non-Farm Occupations	Mean	0.26	0.26	−0.0004
	SE	(0.03)	(0.02)	(0.07)
	Observations	1,530	3,207	4,737
Farmers (Landowners)	Mean	0.43	0.34	0.092†
	SE	(0.02)	(0.02)	(0.048)
	Observations	3,378	7,270	10,648
Farmers (Tenant)	Mean	0.5	0.23	0.278**
	SE	(0.05)	(0.03)	(0.01)
	Observations	552	1,179	1,731

Notes: The asset index is constructed through principal components factor analysis and measures household ownership of any combination of the following assets: radio, watch or clock, bicycle, cows, and a hurricane lamp. Data are taken from Matlab DSS 1982 and 1996 household censuses. The test of differences across protected/unprotected sides of the river clusters standard errors by village.

†$p \leq .10$; *$p \leq .05$; **$p \leq .01$

protected side.[8] The variance of assets across households within a village (which would be linked to changes in risk exposure) did not differentially change across the protected or unprotected banks (see Table S2 in Online Resource 1). Consistent with the fieldwork findings, the mean wealth effect of the embankment thus appears to dominate changes in variance.

In summary, we find that gains in wealth were likely the most salient change and the dominant embankment effect for protected Matlab residents. Thus, we focus on identifying the effects of wealth changes in our empirical analysis. Photographs in Online Resource 1 (Fig. A2) show that the embankment is not a soaring barrier that can protect residents from the gushing floodwaters that are an enduring risk to life and property periodically faced by rural Bangladeshis. Rather, it is a more modest barrier designed to protect agricultural fields from seasonal variation in water levels that render those fields inarable during the monsoons. The data on cropping cycles, agricultural yields, land values, and wealth by occupation all indicate that the embankment performs this limited function well, bestowing a positive wealth shock on protected households and particularly on farming households.

[8] These results are available upon request. Control variables for this regression include total land area under irrigation, distance to nearest market, travel time to nearest market, whether the village has a credit institution, whether the village participates in the MCHFP program, and controls for area under cultivation and cost of cultivation by crop type.

Effects of the Embankment on Marriage Outcomes

Estimation Strategy

Our DD setup compares the marriage market outcomes for protected households following embankment construction to their pre-embankment outcomes, after differencing out the corresponding change in unprotected household outcomes. Our general estimation equation thus appears as follows:

$$Y_{it} = \alpha_h + \gamma_t + \beta_1 \left(Protected \right)_i + \beta_2 \left(Post \right)_t + \beta_3 \left(Embankment \right)_{it} + \delta \mathbf{X}_{it} + e_{it}.$$

Y_{it} represents the marriage outcome of individual i who marries in year t. The DD estimate—labeled "embankment"—is the interaction between "protected" (a time-invariant indicator for whether i's household (at the time of marriage) is on the protected side of the river) and "post" (an indicator for whether i's marriage took place before or after embankment construction). All specifications include year (or year of marriage) dummy variables, γ_t. For binary outcomes, we replace the linear specification with a probit. $\mathbf{X}_{it}$ is a vector of demographic control variables, which may vary across equations for different marriage outcomes.[9] We include household fixed effects (α_h) where possible, which controls for household-specific unobservable preferences, such as heterogeneous attitudes toward risk. The sample for household fixed-effects regressions is restricted to only those households experiencing at least one marriage before and at least one marriage after embankment construction (e.g., for two children). All specifications report standard errors clustered by village.

Effects of Embankment on Spousal Wealth, Age at Marriage, and Dowry

We now examine the embankment's effect on other marriage market outcomes to help us understand changes in the market associated with the wealth shock. The model in Online Resource 1 predicts that because protected, newly wealthy households present a more desirable profile, these households will assortatively match to spousal characteristics that are complementary to their own in producing marital surplus. We use this insight to test whether socioeconomic status (SES) is complementary across spouses. Land is the primary asset for Matlab households, and we measure a spouse's SES according to the amount of land owned by the head of the household in 1982.[10]

Table 4 shows that protected households were 3 percentage points more likely to marry into wealthier households (in terms of landownership) after embankment construction relative to the unprotected (a 10 % increase from their pre-

[9] When the outcome is spousal landownership, land owned by the individual's household is included as a control. When the outcome is spouse age at marriage or spousal age gap, individual age at marriage is included as a control. For the fixed-effects model, controls for gender and birth order of the individual and number of siblings are included.

[10] Our data observe the amount of land owned by a household only if it lies within the surveillance area, so this specification cannot include any household marrying outside Matlab. In DD specifications, we do not see any evidence of differential post-embankment marriage migration rates across protected and unprotected households, so a sample excluding these migrants should not yield biased estimates of the embankment effect.

Table 4 Estimates of spouse owning above average amount of land

	1 Probit Model Both Genders		2 Probit Model Exclude Two Years Pre-embankment		3 OLS Model Both Genders (collapsed two periods)	
	(1982–1996)	(1982–1993)	(1982–1984, 1987–1996)	(1982–1984, 1987–1993)	(1982–1996)	(1982–1993)
Protected	0.013	0.013	−0.003	−0.003	0.009	−0.005
	(0.017)	(0.017)	(0.021)	(0.021)	(0.020)	(0.022)
Post	−0.034	0.004	−0.023	−0.002	−0.010	−0.013
	(0.027)	(0.026)	(0.036)	(0.034)	(0.025)	(0.026)
Embankment	0.029	0.038†	0.044†	0.053*	0.035	0.057*
	(0.018)	(0.020)	(0.023)	(0.024)	(0.024)	(0.028)
Above Average Land	0.036*	0.008	0.049**	0.015	0.023	−0.004
	(0.014)	(0.015)	(0.017)	(0.018)	(0.018)	(0.020)
Land Owned (in decimals)	0.001**	0.001	0.001**	0.001	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Oldest Child	−0.002	0.003	−0.010	−0.008	0.002	0.013
	(0.009)	(0.011)	(0.011)	(0.012)	(0.018)	(0.022)
Youngest Child	−0.009	−0.008	−0.008	−0.007	−0.009	−0.016
	(0.008)	(0.010)	(0.009)	(0.011)	(0.014)	(0.016)
Hindu	−0.091**	−0.092**	−0.093**	−0.094**	−0.094**	−0.092**
	(0.016)	(0.017)	(0.018)	(0.019)	(0.017)	(0.019)
MCHP	−0.026	−0.027	−0.029	−0.030	−0.044*	−0.054*
	(0.018)	(0.019)	(0.023)	(0.023)	(0.021)	(0.024)
MCHP × Post	0.016	0.001	0.019	0.004	0.015	0.020
	(0.017)	(0.017)	(0.022)	(0.023)	(0.024)	(0.027)
Constant					0.333**	0.353**
					(0.021)	(0.022)
Observations	12,995	10,287	10,635	7,927	6,453	5,264

Notes: Column 1 presents probit estimates for which the dependent variable is a binary variable equal to 1 if the spouse owns an above average amount of land, and 0 otherwise; marginal effects are reported (estimated at the means). Standard errors, shown in parentheses, are clustered at the village level. In column 2, data exclude two years prior to embankment: 1985 and 1986. In column 3, data are collapsed at the household level by two periods: pre- and post-embankment; the dependent variable is equal to the share of marriages in the household (pre- and post-embankment) for which the spouse owned an above average amount of land. Standard errors, shown in parentheses, are clustered at the village level. Columns 4 and 5 present probit estimates for which the dependent variable is a binary variable equal to 1 if the spouse owns above average amount of land, and 0 otherwise; marginal effects are reported (estimated at the means). Standard errors, shown in parentheses, are clustered at the village level. Year fixed effects are included but not shown.

†$p \leq .10$; *$p \leq .05$; **$p \leq .01$

embankment likelihood of 32 %). The results are statistically and quantitatively stronger when we exclude the two years prior to embankment construction, or the last three years of the sample, which are less likely to be associated with the wealth shock.

Because the embankment is a constructed infrastructure that persists, the embankment variable is serially correlated. This can generate inconsistent standard errors that understate the standard deviation of our DD estimator. To correct for this problem, we collapse the data to only two time-series observations per household, such that there is only one "pre-embankment" and one "post-embankment" observation. This is the preferred method of serial correlation correction suggested by Bertrand et al. (2004), especially when the number of cross-sectional units is small. Table 4 shows that the spousal wealth results remain unchanged with this correction. Further triple difference analysis (not shown) indicates that the spousal wealth results are stronger for farming households (who experience the embankment-related wealth gain) than for nonfarming households.

Next, we examine age effects in Table 5, but we find no robust evidence that the embankment changes age at marriage, age of spouse, or the spousal age gap. If each spouse's age at marriage is an independent (i.e., not complementary or substitute) characteristic in the marriage market, then this finding is consistent with the matching model. The age effects are both small in magnitude and statistically indistinguishable from zero.

Table 6 reports the effect of the embankment on dowry using our standard DD specification but using the MHSS 1996 survey. MHSS retrospectively inquires about the dowry paid at the time of marriage for each couple living in the household. We create a retrospective panel and use the dowry amount reported by the wife as our dependent variable. Because a large fraction of families reported not paying any dowry, we report Tobit models.

Husbands from protected households commanded larger dowries after embankment construction, a result predicted by the matching models in Online Resource 1. This shows that in a patrilocal marriage market, husbands capture a larger share of the environmental improvement through the marriage market transfer. The bride's family pays more for her to locate on the protected side.

The Embankment's Effects on Consanguinity

Having studied how the embankment changed the conditions of marriage, we now examine economic motivations (if any) underlying the practice of intrafamily marriage (consanguinity). We begin by describing attitudes toward consanguinity based on a survey of 300 consanguineous and 300 nonconsanguineous households conducted in 2005 in Teknaf, Bangladesh, which is in a different district but in the same (Chittagong) division as Matlab (Mobarak et al. 2012). The attitudinal survey responses indicate that both husbands and wives in rural Bangladesh view consanguinity as conferring some socioeconomic benefits (see Figure S4 in Online Resource 1). Women reported that cousin marriage improves their relationships with their in-laws and reduces dowry payments; men reported that cousin marriage improves the spousal relationship, allows his wife to stay closer to her family, and avoids the splitting of inherited property. The vast majority of respondents who married a cousin did so based on their parents' wishes, which suggests that the benefits of consanguinity mostly accrue to parents rather than the new couple.

Table 5 Estimates of the average age at marriage, spouse's age, and the difference in age between the spouses

	Female Age		Male Age		Husband's Age		Wife's Age		Big Age Difference	
	1	2	3	4	5	6	7	8	9	10
	Collapsed Two Periods	FE	Collapsed Two Periods	FE	Collapsed Two Periods	FE	Collapsed Two Periods	FE	Collapsed Two Periods	FE
Protected	0.170		0.225		−0.117		0.099		0.011	
	(0.123)		(0.252)		(0.220)		(0.140)		(0.014)	
Post	1.674**	1.747**	2.645**	3.542**	0.368†	1.737**	1.016**	2.195**	0.003	−0.096*
	(0.130)	(0.347)	(0.205)	(0.439)	(0.190)	(0.592)	(0.145)	(0.433)	(0.016)	(0.044)
Embankment	−0.038	−0.165	0.105	0.130	0.062	−0.549	−0.208	−0.240	0.013	0.017
	(0.160)	(0.248)	(0.239)	(0.358)	(0.279)	(0.424)	(0.160)	(0.353)	(0.021)	(0.032)
Land Owned (in decimals)	0.002		0.015**		0.007†	−0.579*	−0.003	−0.389	0.000	
	(0.002)		(0.004)		(0.004)	(0.278)	(0.002)	(0.245)	(0.000)	
Oldest Child	−0.676**	−1.385**	−0.679**	−1.351**	0.176	0.703**	0.055	0.888**	0.004	0.093**
	(0.119)	(0.163)	(0.177)	(0.248)	(0.159)	(0.257)	(0.095)	(0.183)	(0.013)	(0.021)
Youngest Child	0.774**	1.222**	2.185**	2.927**	0.326**	−0.037	0.099	0.394	−0.008	−0.064**
	(0.094)	(0.151)	(0.159)	(0.186)	(0.103)	(0.427)	(0.091)	(2.341)	(0.013)	(0.019)
Hindu	−0.017		0.520		−0.067	−0.168	−0.258	−0.469	0.027	
	(0.194)		(0.326)		(0.146)	(0.426)	(0.168)	(0.332)	(0.018)	
MCHP	0.027	0.073	0.076	5.083*	−0.144		0.147		0.005	0.008
	(0.097)	(0.250)	(0.210)	(2.375)	(0.210)		(0.130)		(0.013)	(0.032)
MCHP × Post	−0.087	0.025	−0.010	−0.248	0.394		−0.222		−0.002	−0.026
	(0.133)	(0.250)	(0.210)	(0.337)	(0.250)		(0.174)		(0.020)	(0.032)

Table 5 (continued)

	Female Age		Male Age		Husband's Age		Wife's Age		Big Age Difference	
	1	2	3	4	5	6	7	8	9	10
	Collapsed Two Periods	FE	Collapsed Two Periods	FE	Collapsed Two Periods	FE	Collapsed Two Periods	FE	Collapsed Two Periods	FE
Age at Marriage					0.548**		0.200**		0.045**	0.056**
					(0.022)		(0.011)		(0.002)	(0.002)
Constant	18.148**	18.514**	22.362**	18.910**	15.867**	25.342**	13.393**	17.425**	−0.951**	−1.202**
	(0.095)	(0.234)	(0.229)	(1.250)	(0.401)	(0.400)	(0.282)	(1.232)	(0.037)	(0.049)
Observations	8,687	6,156	5,581	4,679	8,678	6,156	5,581	4,679	5,581	6,156

Notes: In columns 1, 3, 5, 7, and 9, data are collapsed at the household level by two periods: pre- and post-embankment. In columns 1 and 2, the dependent variable is the average age of female family members at the time of marriage (if the spouse's age is nonmissing); in columns 3 and 4, the dependent variable is the average age of male family members at the time of marriage (if the spouse's age is nonmissing); in columns 5 and 6, the dependent variable is the average husband's age at marriage (for a female household member); and in columns 7 and 8, the dependent variable is the average wife's age at marriage (for a male household member). Finally, in columns 9 and 10, the dependent variable is a binary variable equal to 1 if the difference between the average age of a female family member at the time of her marriage and her husband's age is greater than or equal to 10 years; and 0 otherwise. Estimates on the collapsed data are difference-in-differences estimates using OLS; standard errors, shown in parentheses, are clustered at the village level. In columns 2, 4, 6, 8, and 10, data are collapsed at the household-year level; fixed-effects models are estimated using OLS; year dummy variables are included but not shown; fixed effects are household-level fixed effects; the sample is restricted to households with nonmissing *consanguinity rate*, *post*, and *protected*. The difference in the sample size between collapsed sample and household-year panel is due to households reporting several marriages in years prior (or post-) embankment, while in the collapsed data these observations would reduce to one observation before and after embankment. Estimates in column 9 are marginal effects from a probit model; standard errors, shown in parentheses, are clustered at the village level.

$^{\dagger}p \leq .10$; $^{*}p \leq .05$; $^{**}p \leq .01$

Table 6 Tobit estimates of dowries

	Nominal	Real
Protected	−1,477**	−13,083*
	(185.2)	(5,958)
Post	29,145**	149,781**
	(225.6)	(41,989)
Embankment	762.8**	10,865†
	(239.9)	(6,322)
Age at Time of Marriage	−480.3**	92,879**
	(12.46)	(930.8)
Constant	−66,973**	−80,074**
	(222.7)	(33,315)
Observations	5,674	4,031

Notes: The sample is a retrospective panel of marriages created from the cross-sectional Matlab Health and Socio-economic Survey (MHSS 1996). Tobit estimates are reported, with standard errors clustered at the village level. Year fixed effects are included but not shown. In the second specification, a GDP deflator (1960–2004) from the Bangladesh Bureau of Statistics is used to convert the dowry values to real terms and deflate all values to 1996 taka.

†$p \leq .10$; *$p \leq .05$; **$p \leq .01$

The reports on lower dowries and centrality of parents' preferences are suggestive of a relationship between financial constraints and consanguinity. We now begin to explore this in our Matlab data by examining how the exogenous shift in wealth associated with embankment construction affects the propensity to marry a cousin.

Effect of the Embankment on Consanguinity

We employ the same DD identification strategy outlined earlier. Results in Table 7 show that although the rates of consanguinity fell over time on both sides of the river, the drop was significantly larger among protected households after the embankment was built. Protected households showed a 1.7 percentage point greater decrease in the likelihood of marrying a biological relative (i.e., second cousin or closer) after embankment construction, over and above the change among the unprotected. The next specification restricts the sample for the post-period to five years beyond embankment construction (until 1993, eliminating the last three years of data), and the results remain virtually identical.

Next we run linear household fixed-effects regressions, restricting the sample to those households with multiple marriages on either side of embankment construction. We find that the same family was 28 % (about 2.2 percentage points) less likely to marry a younger child to a biological relative after embankment than it was to marry an older child who married prior to the embankment construction.[11] We control for gender and birth order effects to ensure that the household fixed-effects result does not merely reflect a cultural preference to marry younger children to their cousins. All regressions control for year (or year-of-marriage) fixed effects.

[11] Fixed-effects logit models produce similar results.

Table 7 Difference-in-differences (DD) and household fixed-effects (FE) regressions of consanguinity

	Dependent Variable = Consanguineous Marriage				Dependent Variable = Share of Consanguineous Marriages (collapsed two periods)			
	1		2		3		4	
	Probit Model		Household FE Model		DD Model		Household FE Model	
	(1985–1996)	(1985–1993)	(1985–1996)	(1985–1993)	(1985–1996)	(1985–1993)	(1985–1996)	(1985–1993)
Protected	0.006	0.007	—	—	0.016	0.012	—	—
	(0.007)	(0.007)			(0.011)	(0.012)		
Post	−0.028**	−0.007	−0.035*	−0.031*	−0.025*	−0.025*	−0.025*	−0.027*
	(0.011)	(0.010)	(0.015)	(0.014)	(0.010)	(0.011)	(0.010)	(0.011)
Embankment	−0.017*	−0.018*	−0.022†	−0.019	−0.027*	−0.022†	−0.025*	−0.019
	(0.007)	(0.007)	(0.011)	(0.013)	(0.012)	(0.013)	(0.012)	(0.013)
Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant			0.099**	0.083**	0.072**	0.074**	0.075**	0.082**
			(0.010)	(0.011)	(0.011)	(0.012)	(0.009)	(0.010)
Observations	27,206	20,153	12,166	9,204	10,744	8,749	10,744	8,749

Notes: In column 1, difference-in-differences results are estimated using probit model in which the dependent variable is a binary variable equal to 1 if a marriage is consanguineous, and 0 otherwise; marginal effects are reported (estimated at the means). The sample is restricted to marriages for which *consanguinity rate*, *post*, and *protected* are nonmissing. Fixed-effects models (column 2) are estimated using OLS in which the dependent variable is a binary variable equal to 1 if a marriage is consanguineous, and 0 otherwise; fixed effects are household-level fixed effects. The sample is restricted to marriages for which *consanguinity rate*, *post*, *protected*, and the rest of explanatory variables are nonmissing and to households with more than one marriage over the study period. Year dummy variables are included but not shown. Controls in all specifications include land owned (in decimals) in 1982; controls for birth order; religion (= 1 if Hindu, and 0 otherwise); and controls for Matlab Maternal and Child Health and Family Planning program (MCHFP) and an interaction term, MCHFP × Post. Control variables not changing over time for a given family (religion and land owned) are not included in the household fixed-effects model. Data in 3 and 4 are collapsed at the household level by two periods: pre- and post-embankment; the dependent variable is the share of consanguineous marriages in the household. Difference-in-differences results in columns 3 and 4 are estimated using OLS; standard errors, shown in parentheses, are clustered at the village level. The sample is restricted to households with nonmissing *consanguinity rate*, *post*, and *protected*. In column 4, a fixed-effects model is estimated; fixed effects are household-level fixed effects. The sample is restricted to households with nonmissing observations for *post* and *embankment*. Control variables not changing over time for a given family (religion and land owned) are not included in the household fixed-effects model. Standard errors, shown in parentheses, are clustered at the village level in probit models.

$^{\dagger}p \leq .10$; $^{*}p \leq .05$; $^{**}p \leq .01$

To address concerns about serial correlation in the embankment indicator, we collapse the data to a pre-embankment and a post-embankment period. Because the data are now aggregated across marriages, the dependent variable becomes the share of consanguineous marriages in the household, and some individual-level controls (such as birth order) are no longer well defined. Both the magnitude and the statistical significance of the main consanguinity results remain unchanged in these specifications, in both DD and household fixed-effects models. In summary, we find robust evidence that a positive wealth shock resulted in a drop in consanguinity.

Is Consanguinity Related to Credit Constraints and Dowries?

The main DD result we have reported is consistent with the Do et al. (forthcoming) hypothesis that dowries and consanguinity act as substitutes in the marriage market. The embankment, by increasing wealth on the protected side, relaxed the liquidity constraint (in the sense that these now-wealthier households had more dowry to offer at the time of marriage), taking away this important motivation for marrying within the family. In this section, we examine further evidence on a few other implications of this model.

First, we examine the direct correlation between consanguinity and dowries using the retrospective data on dowries from the MHSS 1996 survey. These data indicate that the dowry transfer at the time of marriage is much smaller in consanguineous unions. The unadjusted bivariate correlation suggests that dowry payments are 25 % lower in consanguineous marriage. When we control for year fixed effects and other covariates such as protection status, the conditional correlation suggests that dowries are 65 % lower at the mean for consanguineous unions. To understand the quantitative significance (monetary magnitude) of this correlation, we use a GDP deflator from the Bangladesh Bureau of Statistics to convert the dowry values to real terms and deflate all values to 1996 taka. We find that consanguinity offers a large quantitatively important savings in dowry payment (2,200 taka in nominal terms, or US$276 in 1996 dollars).

The large savings of dowry that consanguinity permits in turn raises the question whether the wealth shock associated with the embankment was large enough to enable protected households to substitute away from consanguinity so quickly. The 1996 MHSS data indicate that the average farmer on the protected side gained additional agricultural profits of 4,300 taka in 1996 because of the extra growing season, and this had a deflated value of 2,700 in 1987 takas. The wealth effect therefore does seem large enough to allow the newly protected farmers to use the extra agricultural profits to substitute away from consanguinity. In the MHSS data, an extra 1,300 taka was transferred over in dowries in nonconsanguineous marriages that were contracted during the period 1987–1989. Six months of agricultural profits for the average protected farmer would cover this excess dowry, and the liquidity constraint preventing households from paying dowries up front thus does get significantly relaxed within the first year.

Because only brides have to pay the dowry, the embankment relaxes a relevant liquidity constraint only for the bride's family. Table 8 reports the embankment-consanguinity relationship separately for males and females. As predicted by this theory, we observe that the consanguinity effect is stronger in the female sample. Females were 2.0–2.9 percentage points less likely to marry consanguineously, an effect that is statistically significant in every case; the corresponding effect for males was 1.0–1.8 percentage points, which is sometimes not significantly different from zero.

The direction of the male-female difference is actually powerful evidence in favor of the liquidity constraint story for consanguinity, which helps us distinguish this explanation from some other competing models because the leading alternative hypothesis would predict exactly the opposite. We saw in our earlier analysis of dowries that men captured a larger share of the benefits of embankment construction because the marriage market in

Table 8 Consanguinity rates in protected households, difference-in-differences estimates

	(1982–1996)				(1982–1993)			
	Male		Female		Male		Female	
Protected	0.006	0.002	0.014	0.009	0.006	0.003	0.014	0.010
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Post	−0.030*	−0.023	−0.017	−0.036*	−0.026*	−0.017	0.011	−0.037**
	(0.014)	(0.015)	(0.010)	(0.015)	(0.012)	(0.015)	(0.008)	(0.013)
Embankment	−0.018†	−0.014	−0.027**	−0.020*	−0.013	−0.010	−0.029**	−0.023**
	(0.010)	(0.010)	(0.008)	(0.008)	(0.011)	(0.012)	(0.008)	(0.008)
Land Owned (in Decimals)		−0.000*		−0.000		−0.000†		−0.000
		(0.000)		(0.000)		(0.000)		(0.000)
Oldest Child		−0.005		0.012**		−0.008		0.005
		(0.005)		(0.004)		(0.006)		(0.005)
Youngest Child		−0.012**		0.013**		−0.015**		0.012*
		(0.004)		(0.004)		(0.005)		(0.005)
Hindu		−0.048**		−0.043**		−0.044**		−0.040**
		(0.005)		(0.005)		(0.005)		(0.006)
MCHP		0.002		−0.005		0.002		−0.005
		(0.009)		(0.008)		(0.009)		(0.009)
MCHP × Post		0.010		0.026**		0.011		0.025*
		(0.011)		(0.010)		(0.012)		(0.011)
Observations	11,032	11,032	16,174	16,174	8,093	8,093	12,060	12,060

Notes: Difference-in-differences results are estimated using a probit model; marginal effects (estimated at the mean) are reported. The sample is split by gender and is restricted to marriages for which *consanguinity rate*, *post*, *protected*, and the rest of explanatory variables are nonmissing and to marriages from households with more than one marriage during the study period. Year dummy variables are included but not shown. Standard errors, shown in parentheses, are clustered at the village level.

†$p \leq .10$; *$p \leq .05$; **$p \leq .01$

rural Bangladesh is patrilocal. Under a simpler story of consanguinity as an inferior marriage outcome, grooms' families—not brides'—would experience the larger drop. Furthermore, the symmetry in consanguinity—namely, if a female marries her cousin, then her husband is also marrying his cousin—biases us against finding a significant difference by gender, which implies that the difference we report is informative.

Consideration of Alternative Hypotheses Linking Consanguinity to the Wealth Shock

We have shown that several patterns in the Matlab data are consistent with a theory linking consanguinity to credit constraints, but other models may also predict some of these same patterns. Ruling out all other theories with full confidence would require us to specify and estimate a model of the marriage market in which all relevant attributes that are potentially correlated with consanguinity are controlled for, but this is beyond the scope of these data. In this section, we look for evidence that speaks to a few specific alternative theories.

There are several other possible conceptual links between the embankment and rates of consanguinity. First, if consanguinity is a desirable marriage outcome based on cultural or religious preferences, then protected households experiencing a positive wealth shock from the embankment may become more able to attract (or pay for) such marriages. This theory is not consistent with our basic DD and fixed-effects results showing differential decreases in consanguinity following embankment construction rather than increases.

Conversely, if consanguinity is an inferior marriage outcome, protected households—with the additional attractive characteristic they offer on the marriage market—may be more likely to avoid this outcome. However, in that case, we would expect a stronger consanguinity effect in the male sample because the patrilocal nature of the marriage market means that males would experience a differentially greater benefit from embankment construction.

A third possibility is that families marry consanguineously to keep wealth and assets within the extended family. Our data indicate that cross-sectionally poorer families were more likely to engage in consanguinity (results available on request), and that following embankment construction, newly wealthy households moved away from the practice. Both observations directly contradict this hypothesis.

A fourth possibility is that the wealth shock allows protected households to broaden their search distance over a wider geographical area, which would mechanically lower consanguinity if relatives are spatially clustered. To examine this hypothesis, we directly test whether the embankment construction changed the search distance for protected households. Table 9 shows that the embankment had negligible effects on the households' propensity to marry and relocate farther away. We see no significant effect in either the male or the female sample on marrying into a different village, marrying outside the district, marrying across the river, or the distance to the spouse's village. Furthermore, the triple difference result by gender (that the consanguinity effect is larger for females) also makes this hypothesis unlikely. Boys' families initiate the search process in South Asian marriage markets (Vogl 2011), which implies that the consanguinity-search hypothesis would result in larger embankment effects in the male sample, unlike what we find.

A fifth theoretical possibility is that consanguinity is a response to risk exposure. Unprotected households may seek mutual insurance by practicing consanguinity as a way to form robust intergenerational bonds with another household in the extended family. However, the discussion and results presented earlier suggest that the embankment primarily acted as a wealth shock rather than mitigating risk. Moreover, the results in Table 9 suggest that other marriage outcomes associated with risk are unchanged following embankment construction. For example, the model in Online Resource 1 suggests that if the embankment mitigates flood risk, we should observe negative assortative matching with respect to protection status. The coefficient on Protected in Specification 3 of Table 9 shows that households are, in general, much more likely to marry others who are located closer to them—an indication of search frictions in the marriage market—but this propensity to marry close does not differentially change after embankment construction.[12]

[12] This highlights the possibility that empirical results on assortative matching based on cross-sectional data may be uninformative because separating search frictions from true assortative matching in cross-sectional data is difficult. Our panel data, which allow us to control for both "protected" and "post × protected" (labeled "embankment") helps resolve the issue.

Table 9 Probit estimates of spouse search hypothesis

	1		2		3		4	
	Spouse From a Different Village		Spouse From Outside of Matlab		Marriage Across the River		Distance to Spouse's Village (OLS)	
	Male	Female	Male	Female	Male	Female	Male	Female
Protected	−0.013	−0.010	−0.151**	−0.053	0.183**	0.108	0.089	−0.129
	(0.015)	(0.013)	(0.024)	(0.033)	(0.036)	(0.093)	(0.189)	(0.278)
Post	0.029	0.000	0.033	−0.001	0.024	0.009	0.572*	0.060
	(0.020)	(0.013)	(0.029)	(0.022)	(0.033)	(0.036)	(0.248)	(0.233)
Embankment	−0.002	−0.002	0.034	−0.009	−0.023	0.020	−0.250	−0.032
	(0.011)	(0.011)	(0.022)	(0.015)	(0.023)	(0.021)	(0.162)	(0.161)
Land Owned (in decimals)	0.000	0.000**	0.001*	0.001**	0.000	0.001	0.004†	0.006*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)	(0.003)
Oldest Child	0.004	−0.023**	−0.007	−0.011	0.007	−0.011	−0.075	−0.283**
	(0.006)	(0.006)	(0.013)	(0.008)	(0.013)	(0.011)	(0.094)	(0.089)
Youngest Child	0.039**	−0.010*	0.019*	−0.001	0.014	−0.023*	0.352**	−0.103
	(0.007)	(0.005)	(0.009)	(0.008)	(0.012)	(0.011)	(0.078)	(0.069)
Hindu	0.014	0.002	0.019	−0.043†	0.058†	0.028	0.864*	1.112**
	(0.028)	(0.023)	(0.020)	(0.022)	(0.031)	(0.031)	(0.351)	(0.353)
MCHP	−0.014	0.000	0.007	0.033	−0.078*	−0.095*	−0.193	−0.291
	(0.015)	(0.011)	(0.023)	(0.022)	(0.032)	(0.040)	(0.206)	(0.213)
MCHP × Post	0.012	0.004	0.004	−0.000	−0.008	−0.016	−0.217	−0.068
	(0.010)	(0.010)	(0.020)	(0.016)	(0.023)	(0.022)	(0.163)	(0.161)
Constant							2.470**	3.182**
							(0.230)	(0.245)
Observations	12,924	20,023	12,924	20,023	6,161	7,170	6,497	7,628

Notes: Estimates from the probit models are presented in columns 1–3, where the dependent variable is a binary variable equal to 1 if a spouse is from a different village, and 0 otherwise (column 1); a binary variable equal to 1 if a spouse is from outside of Matlab, and 0 otherwise (column 2); a binary variable equal to 1 if a marriage is across the embankment, and 0 otherwise (column 3); a binary variable equal to 1 if a marriage is consanguineous and at least one spouse is not from outside Matlab, and 0 otherwise (column 3). Marginal effects are reported (estimated at the means); the sample is split by gender. In 4, estimates are from the OLS model in which the dependent variable is the distance (in km) between the spouses' villages within Matlab. In columns 3 and 4 the sample is restricted to marriages within Matlab (where consanguinity status is nonmissing). Year dummy variables are included but not shown. Standard errors, shown in parentheses, are clustered at the village level.

†$p \leq .10$; *$p \leq .05$; **$p \leq .01$

For risk-averse households, embankment protection may lower their demand for mitigating risk through other channels (e.g., marrying daughters into geographically distant households that are not subject to the same weather patterns or planting different crops, *à la* Rosenzweig and Stark 1989). We would then expect to see changes in female migration patterns for marriage following embankment construction. Table 9 shows no evidence of such behavior: neither girls nor boys were more likely to marry farther away. Increasing wealth may also alter household tastes for risk, but we do not find much evidence either from our fieldwork or in the data that the embankment changed households' risk exposure or their propensity to diversify and hedge against risk through marriage.

Although we report a congruent set of results that jointly amount to strong suggestive evidence that dowries and credit constraints are linked to the practice of consanguinity, there are other plausible explanations for some of our results that are difficult to rule out. The joint distribution of consanguinity with other characteristics that matter in the marriage market may lead to incidental correlations among consanguinity, wealth, and gender. For example, if grooms care about an unmeasured characteristic (such as beauty) but brides care about status, and status is similar within consanguineous groups but not beauty, then the wealth shock may lead to a drop in consanguinity among brides but not grooms. Absent data on all relevant characteristics in the marriage market (some of which, like beauty, are not easily measurable), it is impossible to confidently rule out these alternative hypotheses.

Additional Sensitivity Analysis

Table S3 in Online Resource 1 reports a battery of sensitivity checks on our main results. One concern with our results is that the unprotected side is twice as large in area and population than the protected side. This means that unprotected households are, on average, farther from the river and therefore, on average, are not comparable with protected households. We omit the households farthest from the river either only on the unprotected side or on both sides, and show that the main consanguinity effect that we report in this article is unchanged.

Next, we check that our results are not generated by the endogenous sorting of households around the embankment after construction. Five percent of our sample migrate to another area in Matlab during the post-embankment period for a reason other than marriage, and reestimating the models without these migrants does not qualitatively change the results.[13] In addition, our results are robust to the exclusion of non-Muslims, who follow different marriage customs and face a narrower market. The main results also hold if we control for religion, which is important because the fraction of the population that is Hindu was different at baseline across the two banks of the river.

We also conduct falsification exercises on our DD estimates (modeled after Aghion et al. 2008), which replace the indicator for the actual year of embankment construction with every other possible false embankment year of the sample. In other tests, we replace the variable for protection status (indicating which side of the embankment a household is on) with false embankment locations of northern versus southern villages and the treatment and control groups of the Matlab Maternal and Child Health and Family Planning (MCHFP) Program, an experimental program present in the area. These tests for false times and locations all show that statistical impacts are absent in cases where we should not observe them (e.g., we see no statistical differences in behavior across any two subperiods other than that of embankment construction) and that the actual

[13] Strong and Minkin (1992) also analyzed migration data before and after construction and concluded that there are no real changes in out-migration rates for either the protected or the unprotected areas.

embankment effect typically trumps the false embankment effects.[14] The main consanguinity effect also survives when a direct control is added for the MCHFP.

Conclusion

With the high prevalence of consanguinity in South Asia, the Middle East, and North Africa, it is important to understand the underlying socioeconomic drivers of this practice. Data from rural Bangladesh are consistent with a hypothesis that poor households engage in consanguinity partly in response to their inability to pay dowries up front at the time of marriage. This helps to explain why a large population genetics literature has found that people of lower SES are most likely to engage in consanguinity. If the credit-constraint explanation is correct, then the high child morbidity and mortality effects of consanguinity reported in the literature imply that liquidity constraints and lack of access to credit impose yet another costly burden on poor households in developing countries through their marriage market choices.

This article also documents further changes in marriage markets following wealth gains that accrue to a subset of Matlab residents. Despite a long literature on the consequences of assortative mating for inequality in developed countries, fewer studies have documented these patterns in developing countries (Esteve and McCaa 2008; Mare 1991). The marriage market is increasingly segregated in terms of spousal wealth. Members of farming households who benefit from the wealth shock are differentially more likely to marry into wealthy households, and nonfarmers living on the protected side of the river find it increasingly difficult to marry into the now-wealthier farming households. Men from these protected (wealthier) households start commanding larger dowries. However, norms regarding age at marriage appear much more inelastic compared with the quicker changes in spousal SES that we document.

Finally, our article documents the general equilibrium changes associated with an infrastructure project in disaster mitigation. Evaluations of such projects typically focus on direct impacts on ecosystem equilibrium, agricultural practices and incomes, and health (e.g., Haque and Zaman 1993; Myaux et al. 1997; Paul 1995; Thompson and Sultana 1996), and we show that indirect general equilibrium changes can be quite substantial and need to be taken into account in program evaluation.

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[14] The t statistic for the actual program coefficient is greater than the t statistic for any false program in 100 % of specifications for consanguinity and 92 % of specifications for marrying a wealthy spouse (i.e., one owning an above average amount of land). When the actual and false program groups are both included in the same regression, the actual program coefficient is significant at the 10 % level in 100 % of specifications for consanguinity and in 54 % of specifications for marrying a wealthy spouse. The false program coefficient is significant in only 20 % of consanguinity specifications and is never significant for marrying a wealthy spouse.

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