Essays in Political Economy and Development Economics

by

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Duke University

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Erica Field

Dissertation submitted in partial fulfillment of the requirements for the degree of
Doctor of Philosophy in the Department of Economics
in the Graduate School of Duke University
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Abstract

This dissertation explores questions in political economy and in development economics. I ask and answer two research questions.

First, I look at whether peaceful or violent protests are more effective at steering policy change. I study this question in the context of the US Civil Rights Era, and evaluate the effects of protests on legislator votes in the US House. I use a fixed-effects specification, and find that peaceful protests caused a liberal shift and therefore were effective from the point of view of the Civil Rights Movement but violent protests caused a conservative shift and therefore backfired.

Second, I look at whether the structure of social networks in rural Western Kenya is affected by a large development intervention. In joint work with Robert Garlick and Kate Orkin, we evaluate the effects of a large unconditional cash transfer and a psychological intervention. We cross-randomize villages into these two interventions, and measure household interactions in four types of networks: talking about goals, talking about challenges, giving money or goods, and receiving money or goods. We estimate effects on total link counts, measures of homophily, and measures of link intensity.

Acknowledgements

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Contents

List of Tables

List of Figures

1

Introduction

This dissertation explores two research questions. The first is in the political economy of protest movements, focusing on the US Civil Rights Movement of the 1960s. The second is in the economics of social networks in a developing-country context, studying the effects of a large randomized control trial.

In chapter 2, I ask whether peaceful or violent protests more effective at achieving policy change. I study the effect of protests during the Civil Rights Era on legislator votes in the US House. Using a fixed-effects specification, my identifying variation is changes within the congressional district over time. I find that peaceful protests made legislators vote more liberally, consistent with the goals of the Civil Rights Movement. By contrast, violent protests backfired and made legislators vote more conservatively. The effect of peaceful protests was limited to civil rights-related votes. The effect of violent protests extended to welfare-related votes. I explore alternative explanations for these results and show that the results are robust to them. Congressional districts where incumbents were replaced responded more strongly.

Furthermore, congressional districts with a larger population share of whites responded more strongly. This is consistent with a signaling model of protests where protests transmitted new information to white voters but not to black voters.

In chapter 3, in joint work with Robert Garlick and Kate Orkin, I ask if social networks are affected by a large economic development intervention in rural Western Kenya. Social networks are crucial determinants of information flow and economic outcomes in developing countries. We evaluate a randomized control trial, studying a combination of two interventions. The first is a large unconditional cash transfer, amounting to one year's average household income. The second is a psychological intervention designed to promote goal-oriented thinking. We assess the effects on respondent's social networks. We measure links in four types of networks: talking about goals, talking about challenges and bad experiences, giving money or goods, and receiving money or goods. We find that effects are mainly focused in challenges links. Although on most outcomes either neither or only the cash transfer had a significant effect, the combined effect of the two interventions was often significant. The interventions changed the total number of links but had a more muted effect on measures of homophily that we construct. Further work is necessary to speak to the dynamics of link formation and homophily in more detail. These preliminary findings suggest that the assumption of exogenous networks might be appropriate in comparable contexts.

2

Does Hate Drive Out Hate? Representation in Congress and (Non-)Violent Protests in the US Civil Rights Movement

2.1 Introduction

> *Why should we love our enemies? . . . Darkness cannot drive out darkness; only light can do that. . . . Hate multiplies hate, violence multiplies violence, and toughness multiplies toughness in a descending spiral of destruction.*
>
> – Martin Luther King Jr. (1957)

The goal of most protest movements is to change economic outcomes. The March for Life in the United States, the Yellow Vests Movement in France, and the Umbrella Movement in Hong Kong are recent examples, each with claims that have clear economic implications. But protest leaders disagree about whether protests should remain peaceful or use violence to achieve their goals. Today, the Antifa and the Refuse Fascism movements are closely tied in their goals but choose opposite tactics. While the Antifa chooses physical violence against white supremacy,[1] Refuse Fascism commits to remain peaceful unless attacked.[2] Howard Zinn described this fundamental dilemma in 1965 as follows:

> "To insist on perfect tranquility with an absolute rejection of violence may mean surrendering the right to change an unjust social order. On the other hand, to seek justice at any cost may result in bloodshed so great that its evil overshadows everything else and splatters the goal beyond recognition." (Zinn, 2002, p. 223)

Are violent protests more successful than peaceful ones at changing policy? I study this question in the context of the 1960s US Civil Rights Movement.

The Civil Rights Movement offers an ideal context to evaluate the effect of protests for two reasons. First, peaceful and violent protests were both salient parts of the movement. Second, the civil-rights legislation passed by Congress in the 1960s are amongst the most significant policy changes in the United States during the 20th century.[3] The 1964 Civil Rights Act, the 1965

[1] The Antifa operates as a loose network of activist groups, and while their activity has been widely reported on, they avoid contact with the press. In the description of the Anti Defamation League: "[the Antifa's] ideology is rooted in the assumption that the Nazi party would never have been able to come to power in Germany if people had more aggressively fought them in the streets in the 1920s and 30s." (Source: `https://www.adl.org/resources/backgrounders/who-are-the-antifa`, accessed on March 20, 2019.)

[2] In its September 2017 statement, Refuse Fascism wrote: "Refuse Fascism does not initiate violence. We oppose violence against the people and among the people, but we uphold people's fundamental and legal right to self-defense." (Source: `https://refusefascism.org/2017/09/09/a-statement-from-refusefascism-org-responding-to-attacks-on-antifa/`, accessed on March 20, 2019.)

[3] 58 percent of Americans responding to a 1999 Gallup poll listed the 1964 Civil Rights Act as one of the most important events of the 20th century. This makes the CRA

Voting Rights Act, and the 1968 Open Housing Act collectively outlawed racial discrimination in public accommodations, education, the labor market, electoral participation, and the housing market. These acts radically altered both employment norms (Dewey, 1952; Wright, 2013) and the provision of public goods in the region. The re-enfranchisement of African Americans led to congressional representation that was more supportive of civil rights (Schuit and Rogowski, 2017) and to increased local government spending targeted towards African Americans (Cascio and Washington, 2014).

I use within-congressional district over-time variation between 1960 and 1972 to identify the effects of peaceful and violent pro-civil rights protests. As an example to illustrate the identification strategy, if we observe that peaceful protests in Durham were followed by more liberal representation of Durham in the US House, we would infer that peaceful protests were effective. By contrast, if they were followed by more conservative representation, we would infer that they backfired.

I find that peaceful protests shifted representation in the liberal direction, while violent protests caused a backlash and made representation more conservative. The effect of peaceful protests was concentrated on legislator votes on civil-rights issues. The effect of violent protests was broader, including legislator votes on welfare-related issues. The identifying assumption is that, conditional on controls for regional time trends, protest history was uncorrelated with time-varying determinants of conservatism. To assess the validity of this assumption, I test robustness to a number of alternative explanations

second amongst legislative events only to the 1920 ratification of the 19th Amendment, recognizing women's right to vote. (Source: `https://news.gallup.com/poll/3427/most-important-events-century-from-viewpoint-people.aspx`, accessed on March 20, 2019.)

under which this assumption would be violated, and find that these are not driving my results.

To measure conservatism, I build on the NOMINATE methodology (Poole and Rosenthal, 1985, 2007). NOMINATE is widely used in the literature to measure the voting behavior of legislators in Congress (e.g., Autor et al., 2017; Campante and Hojman, 2013). It estimates legislators' policy positions in a two-dimensional space. I follow the recommendation of the literature in mapping the two-dimensional NOMINATE scores onto a single dimension which I call conservatism (McCarty, 2011). How conservatively a legislator votes may vary across issue themes. For example, Southern Democrats tended to vote more conservatively on social issues but more liberally on economic issues. To capture this potential heterogeneity, I construct separate conservatism scores for separate issue themes. In this way, I estimate the effect of protests on conservatism on civil rights-related and other issues separately.

I reconstruct protest history in congressional districts relying on news reports from The New York Times as they were coded in a data set called the Dynamics of Collective Action (DOCA). DOCA uses human coding to record event-level information on protests based on the Times. DOCA finely codes the claims of the protest event, its form, its size, whether violence was reported. This allows me to reconstruct separate protest histories for peaceful and violent pro-civil rights protests as well as anti-civil rights and Vietnam War protests. These latter two are confounders, and controlling for them makes the peaceful protest effect stronger.

I consider and address the effects of reporting bias in the Times on my study. Reporting in the Times may be biased in two ways: (i) by not reporting events that happened, and (ii) by reporting events but inaccurately.

Of these, (ii) is unlikely to be a concern. Although in the 1960s the Times wasn't a nationally circulated newspaper yet (George and Waldfogel, 2006), and didn't have its own reporters on the ground, it was sourcing its news reports from local media bureaus. Therefore information reported in the Times closely matches information reported in the local media. On the other hand, (i) can bias my estimates in two ways. Take the univariate case with a single type of protests as an illustrative example. First, (i) can cause an attenuation bias if some protests happened in the district but zero were reported. Second, (i) can cause an amplification bias if some protests happened but a fewer, non-zero many of them were reported.[4] Importantly, (i) only affects the magnitudes of my coefficient estimates but not their signs. If protests had no local effect on conservatism, reporting bias wouldn't in general make the estimates show a spurious positive or negative effect. Moreover, my evaluation of the effects of protests is unaffected by the bias in the magnitudes. This is because I do not evaluate the coefficient estimates as marginal effects of an additional protest. Instead, I calculate the average effects of peaceful and violent protest histories at the end of my sample. This way, I assess the effects of the Civil Rights Movement as a whole rather than of the marginal protest.

The average effect of peaceful protests thus computed at the end of my sample was a .18-standard-deviation (sd) shift in the liberal direction on civil rights issues. The average effect of violent protests was a .14-sd shift in the conservative direction. To put these in context, the difference between the average North Carolina representative and the average Georgia representative was .17 sd. By comparison, the difference between the average California

[4] See appendix 2.A for mathematical illustrations.

representative and the average Georgia representative was 1.74 sd. The effect of peaceful protests during the Civil Rights Movement can thus be conceptualized as potent enough to make representation in Georgia more like in North Carolina, a state which was known as a more liberal state within the South.[5]

I explore alternative explanations that could spuriously drive my results and show that my results are robust. First, my estimates would be spurious if protests picked up heterogeneous voter response to national events. I test robustness to this explanation by allowing for differential time trends in the demographic composition of districts. Doing this does not change my results. Second, protests may have been correlated with unobserved time-varying shocks that fixed effects don't capture. As a consequence, estimates for violent protest may spuriously indicate a conservative shift. To speak to this explanation, I allow for differential time trends in pre-sample political leanings. My results are robust to this. Third, reporting bias may be confounding my estimates if protests are correlated with local media penetration, and The New York Times is less likely to report events from districts with lower media penetration. To test robustness to this explanation, I allow for differential time trends in newspaper circulation and radio ownership. My results are robust to this also.

I contribute to the literature by showing that the choice of protest tactic matters for policy making. In the context of the Tea Party movement, the literature has found that peaceful protests were effective at changing electoral outcomes and legislation in Congress (Madestam et al., 2013). Minority

[5] This sentiment was echoed by Bob Jones, a prominent segregationist, in a 1965 letter to the KKK: "We want enough Klans people at this Rally for the press never again to use the word liberal when they write about the State of North Carolina" (quoted by Cunningham, 2013, p. 72).

protests overall during an earlier period were also found to have had an effect on legislation in Congress (Gillion, 2012). However, whether peaceful or violent protests had different effects on legislation has not been studied yet. On labor-market and housing-market outcomes, the riots of the 1960s were found to have had a negative effect (Collins and Margo, 2004, 2007), and presidential election outcomes were differently affected by violent than by peaceful protests during the Civil Rights Movement (Wasow, 2017). My paper shows that differential changes in electoral outcomes translated into differential changes in policy making.

The rest of the paper proceeds as follows. In section 2.2, I outline the theoretical framework and illustrate the endogeneity problem. In section 2.3, I discuss the data that I use. In section 2.4, I introduce the empirical specification and discuss the results. In section 2.5, I discuss and show robustness to alternative explanations of my results. In section 2.6, I conclude.

2.2 Theoretical motivation

2.2.1 Protests as signaling to the legislator

In a limited-information environment, if the legislator cares about voters' policy preferences, protests may serve as meaningful signals. If protests incur costs to protesters, they reveal information to the legislator. Lohmann (1993, 1994) provides a signaling framework to guide thinking about the determinants protests.

In Lohmann's models, a finite set of voters make a decision between the status quo and a fixed alternative policy position. Voters' utility from either the status quo or the alternative depends on the state of the world. However, the state of the world is unobserved, and voters only receive a noisy private signal about it. After receiving their private signals, they decide whether or not to engage in costly protest action, and they update their beliefs about the state of the world after observing other voters' protest action decisions.

In this framework, the protest action decision is determined by three factors: (i) the cost of action, (ii) the individual voter's benefit from policy change, and (iii) the probability that the individual voter's action would be decisive. In the context of the Civil Rights Era, the cost of action was higher in Southern states like Mississippi than in liberal states like Washington or New York. Lynchings were one historically endemic way in which costs were imposed on challengers of the status quo (Aldrich and Griffin, 2018, p. 106):

> "Lynchings were a common tactic designed to terrorize blacks and of course eliminate particularly outspoken opponents to Jim Crow. Often these were related more to the economic and especially social aspects of Jim Crowism, but blacks who tried too hard to register and vote or, worse, engaged in politics more fully, were sometimes lynched themselves."

While lynchings were at their peak during the solidifying of Jim Crow, firebombings and murders were common in the 1950s and 1960s. Beyond the assassination of Martin Luther King Jr., the much publicized murders of Emmett Till and Medgar Evens, and the assassination attempt against James Meredith are some high-profile examples from this time period.

However, the benefit that pro-civil rights voters could gain from policy change was also higher in Southern states than in liberal districts elsewhere. In retrospect, dismantling the system of segregation in political participation, in labor markets, and in education proved economically beneficial to

African Americans (Donohue and Heckman, 1991; Wright, 2013; Cascio and Washington, 2014). And indeed, the contemporary expectation was no less than this.[6]

Lastly, the perceived probability of individual action being decisive was possibly lower in deeply conservative and in deeply liberal districts than in more moderate districts. If the pivotal voter was unlikely to change her mind after observing the information revealed by protests, pro-civil rights voters' incentive to protest was weaker.

2.2.2 A simple model of protests

To connect ideas about the emergence of protests with my empirical specification, and to think about the endogeneity problem, consider the following model. To simplify exposition, take the one-district and one-time period case, and ignore the distinction between peaceful and violent protests. Suppose that the legislator's best response Y is linear in protest activity A: $Y \equiv \beta A + \mathbf{Z}'\boldsymbol{\gamma} + U$ where $\mathbf{Z}$ is a vector of political factors observed by both the legislator and the protester, and U is a residual term. Suppose also that $U \perp\!\!\!\perp A, \mathbf{Z}$, so that $f_U(x \mid A, \mathbf{Z}) = f_U(x)$ for every $x \in \mathbb{R}$, and U is homoskedastic. Since Y is the legislator's best response, β is the causal effect of protests. The protester solves

$$\max_{A \in [0,\infty)} \mathbb{E}(u(Y, A) \mid A, \mathbf{Z}),$$

or equivalently, $\max_{A\in[0,\infty)} \mathbb{E}(u(\beta A + \mathbf{Z}'\boldsymbol{\gamma} + U, A) \mid A, \mathbf{Z})$. The expectation is taken over U which is the only stochastic variable from the protester's

[6] Cascio and Washington (2014) quote Martin Luther King Jr. stating about the Voting Rights Act before its passage, "With it the Negro can eventually vote out of office public officials who bar the doorway to decent housing, public safety, jobs and decent integrated education."

perspective. Independence implies that $\mathrm{D}_1 f_U(x \mid A, \mathbf{Z}) = 0$, and therefore $\mathrm{D}_1\mathbb{E}(u(\beta A + \mathbf{Z}'\boldsymbol{\gamma} + U, A) \mid A, \mathbf{Z}) = \mathbb{E}(\mathrm{D}_1 u(\beta A + \mathbf{Z}'\boldsymbol{\gamma} + U, A) \mid A, \mathbf{Z})$ and $\mathrm{D}_2\mathbb{E}(u(\beta A + \mathbf{Z}'\boldsymbol{\gamma} + U, A) \mid A, \mathbf{Z}) = \mathbb{E}(\mathrm{D}_2 u(\beta A + \mathbf{Z}'\boldsymbol{\gamma} + U, A) \mid A, \mathbf{Z})$. Then for an interior solution, the first-order condition is

$$\underbrace{\beta\mathbb{E}(\mathrm{D}_1 u(Y, A) \mid A, \mathbf{Z})}_{\substack{\text{expected} \\ \text{marginal benefit}}} + \underbrace{\mathbb{E}(\mathrm{D}_2 u(Y, A) \mid A, \mathbf{Z})}_{\substack{\text{expected} \\ \text{marginal cost}}} = 0.$$

Let $u(y, a) \equiv -(y - Y^*)^2/2 - \kappa a$ so that the protester's utility is linear in protests, and quadratic single-peaked in ideology, with ideal point at Y^*. Then $\mathrm{D}_1 u(y, a) = -(y - Y^*)$ and $\mathrm{D}_2 u(y, a) = -\kappa$, and the protester's optimal choice is

$$A = \max\left\{0, \frac{\beta(Y^* - \mathbf{Z}'\gamma) - \kappa}{\beta^2}\right\}, \tag{2.1}$$

To think about peaceful protests, suppose protests have a positive causal effect but they impose a direct utility cost on the protester.

Remark 1 (peaceful protests)**.** *Suppose that $\beta, \kappa > 0$. There is a threshold level for the ideology gap, and only above this threshold are there protests: $Y^* - \mathbf{Z}'\boldsymbol{\gamma} > \kappa/\beta$. Below the threshold, the cost of protests outweighs the gains from them. That is, peaceful protests will only occur in sufficiently conservative districts.*

To think about violent protests, suppose the opposite: protests have a negative causal effect but they yield a direct utility *gain* for the protester. This gain represents a psychic utility from releasing frustration when facing a political stalemate. However, releasing frustration is politically counterproductive.

Remark 2 (violent protests)**.** *Suppose that $\beta, \kappa < 0$. We get the same threshold for the ideology gap but in this case protests only occur* below *the threshold: $Y^* - \mathbf{Z}'\boldsymbol{\gamma} < \kappa/\beta$. That is, violent protests won't occur in sufficiently conservative districts.*

Predictions 1 and 2 are driven by the linear cost structure. A model of quadratic cost with a fixed cost component would generate similar predictions.

2.2.3 The endogeneity problem and strategies to address it

Suppose that $\mathbf{Z}$ can be partitioned as $(\mathbf{Z}_1', \mathbf{Z}_2')'$ where $\mathbf{Z}_1$ is observed by the econometrician but $\mathbf{Z}_2$ is not. Endogeneity arises because of $\mathbf{Z}_2$. From the econometrician's perspective, $\mathbf{Z}_2$ enters both protests and the residual term. To make this explicit, rewrite equation (2.1) as

$$A = \max\left\{0, \frac{\beta(Y^* - \mathbf{Z}_1'\gamma_1) - \kappa}{\beta^2} - \frac{\mathbf{Z}_2'\boldsymbol{\gamma}_2}{\beta}\right\}. \tag{2.2}$$

If $\boldsymbol{\gamma}_2 \neq \mathbf{0}$, regressing Y on A and $\mathbf{Z}_1$ does not recover the causal effect, β.

Given the panel structure of my data, unobserved factors that are fixed in the district do not cause bias. Suppose that in the panel setting, rather than only recent protest activity, it is the history of *all* protests that matters. Denote the protest history of congressional district ℓ at the beginning of congressional term t by $H_{\ell t-1}$. The legislator's best response is

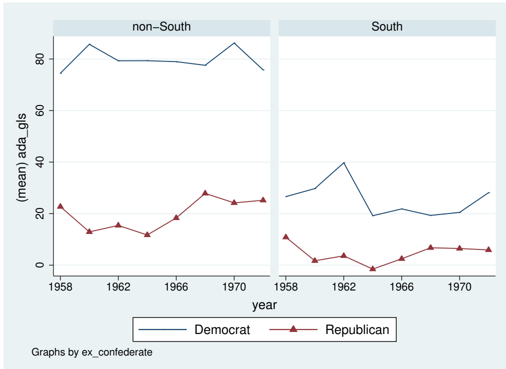
$$Y_{\ell t} = \beta_\ell H_{\ell t-1} + \mathbf{Z}_{1,\ell t-1}'\gamma_1 + \underbrace{\mathbf{Z}_{2,\ell t-1}'\boldsymbol{\gamma}_2}_{\text{unobserved}} + \lambda_\ell + U_{\ell t},$$

where λ_ℓ is a district fixed effect. β_ℓ is the district-specific treatment effect of protests, capturing the idea that the potential policy change in response to information revealed by protests is greater in some districts than in others. λ_ℓ controls for any time-invariant characteristic of the district that affects conservatism, and therefore the protest decision. Within the signaling framework, these are long-run conservatism and the long-run cost environment in the district. Therefore $\mathbf{Z}_{2,\ell t-1}$ is a vector of deviations from the district-level means of conservatism and costs.

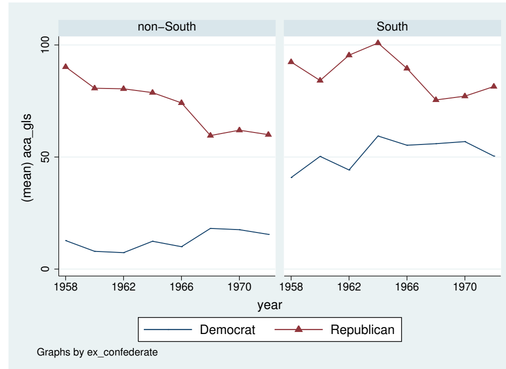
In my analysis, described in section 2.4, district fixed effects control for long-run conservatism and costs, and South/non-South time fixed effects control for congressional term-specific changes to conservatism and the cost environment that differentially affected the South and the rest of the country. I also assess the robustness of my estimates to differential time trends. Differential time trends capture the remaining variation in $\mathbf{Z}_{2,\ell t-1}$ insofar as it is correlated across districts that share certain observed characteristics, e.g., demographic composition, economic conditions, or pre-existing political preferences.

2.3 Data

Legislator ideology in the US House. I use several measures of ideology. Spatial models of roll-call voting in the US House yield one set of my measures. The key idea of spatial models is that the policy space can be described as a one- or two-dimensional Euclidean space, and that legislators have single-peaked preferences over outcomes in this policy space. Each legislator's most preferred outcome is called her *ideal point.* Every roll call is a choice between two points in the policy space (the outcomes if *yea* wins vs. if *nay* wins), and the legislator will choose the point that is closer to her ideal point. The ideal point is interpreted in the literature as the legislator's ideology. Another way



(a) ADA, a liberal interest group



(b) ACA, a conservative interest group

FIGURE 2.1: Average adjusted interest-group ratings in the 11 ex-Confederate states and elsewhere

to understand the ideal point is as a description of who else the legislator tends to vote together with.

A prominent flavor of spatial models is Keith T. Poole and Howard Rosenthal's family of NOMINATE models (see, e.g., Poole and Rosenthal, 1985; 2007). In addition to their use by political scientists, economists have also used them to study political polarization (e.g., Campante and Hojman, 2013; Halberstam and Montagnes, 2015; Autor et al., 2017). For comparability across congresses, the variety called DW-NOMINATE is widely used but is not appropriate for my analysis. This is because in any model, to achieve comparability across time, we need to make assumptions about the evolution of ideal points from one congress to the next. The assumption made by DW-NOMINATE is that each legislator's ideal point can only change following a linear trend during her career in the House. Crucially, this masks abrupt changes which might have happened in response to civil-rights protests.

One alternative that allows for non-linear change over time is a modification of DW-NOMINATE proposed by Nokken and Poole (2004). NOMI-

NATE estimates not only legislators' ideal points but also cutlines between the *yea* and the *nay* alternatives in the policy space for each roll call. Given legislators' ideal points, the cutlines best separate those who voted *yea* from those who voted *nay*. Nokken and Poole's model takes the cutlines from DW-NOMINATE, and then re-estimates legislators' ideal points holding the cutlines fixed. This creates a comparable policy space across congresses, but also allows legislators' ideal points to change more freely.

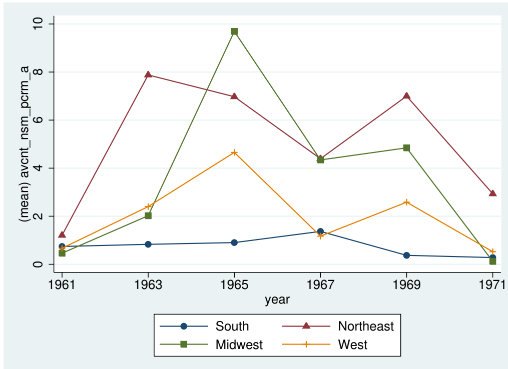
To focus in on specific issue areas, I select roll calls according to three different classification schemes. I use Aage R. Clausen's, David W. Rohde's, and Sam Peltzman's classifications. The Clausen and the Rohde schemes allow for only one classification code for each roll call. The Peltzman scheme allows for two classification codes, which makes it the finest amongst the three. Fineness has implications for measurement error because it affects the accuracy with which I identify civil-rights roll calls under the three schemes.

Ideally, I would re-estimate NOMINATE scores on the subset of roll calls that are classified under civil rights. However, this approach is plagued by measurement error. One weakness of the ideal-points approach is that the estimated ideology scores are noisy when the number of roll calls is small (McCarty, 2011; 2016).

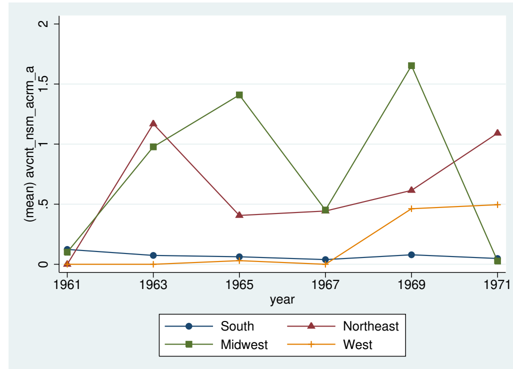
Instead of re-estimating ideal points on area-specific subsets of roll calls, I compute what I call the average signed distance (ASD) for each legislator to the roll-call cutlines that NOMINATE estimates. This way of projecting the two NOMINATE dimensions onto a single dimension follows the suggestion of McCarty (2011). A positive (negative) ASD indicates that the legislator tends to vote on the conservative (liberal) side of that issue area. For example, in civil-rights roll calls according to Rohde's classification, Tennessee representative Robert Everett had an ASD of .299 in the 87th congress, and an ASD of $-.109$ in the 88th. On the other hand, in defense-related roll calls, he had ASD's of $-.312$ and $-.322$ in the same two congresses. This indicates a switch to liberal voting patterns in civil rights roll calls, but no switch in defense-related roll calls.

As an alternative to scores from ideal-points models, I also use interest-group ratings of legislators from 1960 to 1972. Amongst other interest groups, the Americans for Democratic Action (ADA) and the Americans for Constitutional Action (ACA) selected roll-call votes for each congress to construct evaluations of legislators. Each legislator got a score assigned by ADA, and another score by ACA. The scores are calculated as the share of roll calls on which they voted according to ADA's and ACA's positions. For the analysis, to make the scores comparable across congresses, I implement the adjustment proposed by Groseclose, Levitt, and Snyder (1999). Figure 2.1 shows party means of the adjusted ADA and ACA scores for the 11 ex-Confederate states and elsewhere.

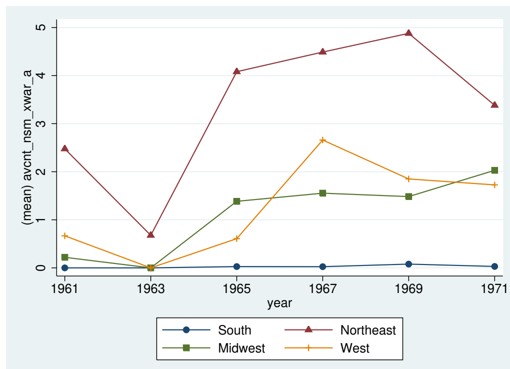
Protest events. To measure protest activity, I use Susan Olzak's *Dynamics of Collective Action* (DOCA) data set. DOCA covers protest events in the United States between 1960 and 1995. The source of all information in DOCA is *The New York Times.* Amongst other characteristics, it codes details on the reported race of participants, the claims that were identified, estimates of the number of protesters, whether the protesters used violence, whether the police used violence, whether anyone was arrested, and whether there was any property damage. DOCA covers not only civil-rights protests but also riots and Vietnam War protests.



(a) Pro-civil-rights protests



(b) Anti-civil-rights protests



(c) War protests

FIGURE 2.2: Regional means of population-weighted average event counts in congressional districts

To construct county-level measures of protest activity, I count events in the DOCA data that are reported to have had at least 100 participants. To aggregate these to the congressional-district level, I proceed as follows. First, I intersect county and district boundaries. Then I calculate the estimated population size in each intersection. Finally, for each district, I compute the population-weighted average of county-level counts of each intersection that belongs to it. I repeat this procedure for the subset of protests that I identify as having involved violence by protesters, and for the subset that I identify as having involved violence against protesters. The former includes protests in the traditional sense as well as race riots. I include race riots in my protest measures because they were responses to institutional racial inequality, and because they shaped public opinion and policy (Button, 1978).

The assumption implicit in this procedure is that protests affect everyone equally within the county and affect no one across county boundaries. The interpretation of the resulting district-level protest measure is as the number of protests that the average citizen of the district was exposed to.

I construct measures of anti-CRM and war protests the same way. Figure 2.2 shows the regional evolution of the resulting protest measures.

Election outcomes. For general elections for US House seats, I compute the winning candidate's margin of victory and the Republican vote share from the *Candidate Name and Constituency Totals, 1788–1990* (ICPSR 2) data set. For primary races, I get candidate counts from Stephen Pettigrew's *US House Primary Elections, 1956–2010,* data set.

District characteristics. I use radio ownership and the share of blacks amongst the population as exposure measures for shift-share-type instruments. I obtain both of these from the 5-percent IPUMS sample of the 1960 census. The smallest observable geographical unit in this sample is what IPUMS calls a "mini-PUMA." A mini-PUMA is a geographical block with a population of at least 50 thousand. Like counties, mini-PUMAs don't perfectly align with congressional districts. To assign radio ownership and black population share to districts, I intersect mini-PUMA boundaries with district boundaries, and whenever a mini-PUMA intersects with multiple districts, I calibrate the weights assigned to these intersections to minimize the discrepancy between the implied population estimate for districts, and actual district population. For this calibration, I get a measure of actual district population from the *Congressional District Data* of Adler Scott at the University of Colorado, Boulder.

2.4 The effect of protests

2.4.1 Estimation strategy and identification

Notation and timing. I denote the outcome variable by $Y_{\ell t}$ and an aggregate measure of protest history by $A_{\ell t-1}$. ℓ indexes congressional district and t indexes a two-year congressional term. I also disaggregate protests by protester use of violence, and denote the history of protests with peaceful protesters by $P_{\ell t-1}$, of protests with violent protesters by $V_{\ell t-1}$.

To understand timing, take the 89th congress as an example. The 89th congress met between January 3, 1965, and January 3, 1967. Elections for this congress were held on November 3, 1964. For measures of legislator ideology, $Y_{\ell 89}$ is constructed from roll call votes during the 89th congress of the legislator who represented ℓ. For election outcomes, e.g., Democratic vote share, $Y_{\ell 89}$ refers to the vote share in the November 3, 1964, elections. For measures of protests, e.g., $A_{\ell t-1}$, $A_{\ell 88}$ refers to the history of protest activity until November 3, 1964.

Therefore $Y_{\ell t}$ is always determined after $A_{\ell t-1}$. For legislator ideology, $Y_{\ell t}$ and $A_{\ell t}$ are contemporaneous. For election outcomes, $Y_{\ell t}$ is determined before $A_{\ell t}$.

Specification. The specification that does not discriminate between peaceful and violent protests regresses the outcome, $Y_{\ell t}$, on an aggregate measure of protest history, $A_{\ell t-1}$:

$$Y_{\ell t} = \beta A_{\ell t-1} + \lambda_\ell + \theta_{\text{South}(\ell)\times t} + \mathbf{X}'_{\ell t}\boldsymbol{\gamma} + U_{\ell t}. \tag{2.3}$$

λ_ℓ and $\theta_{\text{South}(\ell)\times t}$ are district and South/non-South time fixed effects. Given the fixed effects, I use variation within district and within term to estimate β. I compare outcomes with higher-than-average protests to outcomes with lower-than-average protests. Therefore $\widehat{\beta} > 0$ if outcomes are above average when protests are above average.

$\mathbf{X}_{\ell t}$ is a vector of controls. For all results in the paper, the controls include distance to New York City with t-varying slope. This controls for potential reporting bias by *The New York Times* that is due to geographic distance. For robustness checks, the controls also include the population share of blacks, average family income, or the population share in metropolitan centers, all with t-varying slopes.

Peaceful and violent protests in the data have the opposite association with outcomes. Because of this, β in (2.3) might be close to zero even if true effects of protests are large. To allow for heterogeneity by the use of violence, I also estimate

$$Y_{\ell t} = \beta_1 P_{\ell t-1} + \beta_2 V_{\ell t-1} + \lambda_\ell + \theta_{\text{South}(\ell)\times t} + \mathbf{X}'_{\ell t}\boldsymbol{\gamma} + W_{\ell t}. \tag{2.4}$$

Constructing protest history. I compute a populated-weighted event count for each district ℓ during each two-year term t, as described in section 2.3. Denote this event count by $T_{\ell t}$. The aggregate measure of protest history, $A_{\ell t-1}$, is constructed as the cumulative sum of district ℓ's past event counts, $\sum_{s\leq t-1} T_{\ell s}$. This formulation incorporates two assumptions: (i) $Y_{\ell t}$ has a memory—that is, it integrates not only events that occurred in the previous period but also earlier events—and (ii) $Y_{\ell t}$ has a constant marginal response to an additional event. In specification checks, I show that both (i) and (ii) are supported by the data.

The disaggregation by the use of violence is as follows. I first compute average counts of events that exhibit markers of violence by protesters. Denote this by $T^V_{\ell t}$. Then I get an event count of peaceful protests as $T^P_{\ell t} \equiv T_{\ell t} - T^V_{\ell t}$. In turn, my measures of protest history are $P_{\ell t-1} \equiv \sum_{s \leq t-1} T^P_{\ell s}$ for events with peaceful protesters, and $V_{\ell t-1} \equiv \sum_{s \leq t-1} T^V_{\ell s}$ for events with protester violence.

Identification. As discussed in subsection 2.2.3, the signaling model implies that the incidence of protests is determined by three factors: (i) the cost of protest action, (ii) the individual voter's benefit from policy change, and (iii) the probability that the individual voter's action would be decisive. Varying conditions across districts make for heterogeneity in these three factors. In particular, (ii) and (iii) manifest as a heterogeneous treatment effect across districts. Estimates of equation (2.4) capture an average of these treatment effects. (i)–(iii) also vary with long-run conservatism and hospitability toward protest activity in the district. These are captured by the district fixed effect in equation (2.4).

Deviations from the district-level mean in (i)–(iii) pose a threat to identification. These are not captured by the district fixed effect. If such deviations in determinants of conservatism were correlated with protests, estimates of the protest effects would be spurious. I discuss robustness checks for the presence of confounding deviations in section 2.5.

Table 2.1: Protests and legislator ideology

	Nokken–Poole		adjusted ADA		adjusted ACA	
	(1)	(2)	(3)	(4)	(5)	(6)
protest history	−0.000		0.001		−0.000	
	(0.0015)		(0.0016)		(0.0019)	
... peaceful		−0.002*		0.002*		−0.002
		(0.0013)		(0.0013)		(0.0016)
... violent		0.020***		−0.016*		0.016*
		(0.0067)		(0.0085)		(0.0086)
N	1806	1806	1806	1806	1806	1806

Outcome variables are standardized to have zero mean and unit variance. Higher values correspond to more conservative ideology, except for the ADA liberalism score which is on a reversed scale. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 2.2: Protests and legislator ideology by issue area

	civil rights		welfare		environment	
	(1)	(2)	(3)	(4)	(5)	(6)
protest history	−0.004***		−0.001		0.003**	
	(0.0012)		(0.0013)		(0.0014)	
... peaceful		−0.006***		−0.002*		0.002
		(0.0012)		(0.0012)		(0.0013)
... violent		0.020***		0.017***		0.012*
		(0.0054)		(0.0066)		(0.0071)
N	1806	1806	1806	1806	1806	1806

Outcome variables are standardized to have zero mean and unit variance. Higher values correspond to more conservative ideology. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

2.4.2 *Mean legislator behavior*

Now I turn to discussing the estimates. To conceptualize the effect size, rather than focusing on the marginal effect associated with one additional observed protest, I evaluate the effect of the whole Civil Rights Movement. To do this, I calculate the average protest history at the last period of my sample conditional on having had any protests, and compare predicted conservatism with this average history with predicted conservatism with a history of no protests.

Violent protests shifted legislators in the conservative direction. Point estimates are similar across the ADA and the ACA interest-group ratings and the Nokken–Poole ideology measure (table 2.1). On the Nokken–Poole measure of roll-call voting behavior across all roll calls, violent protests shifted the legislator .14 standard deviations (sd) in the conservative direction (95% confidence interval: $[.05\,\mathrm{sd}, .23\,\mathrm{sd}]$). The effect of peaceful protests is borderline significant: peaceful protests shifted her .07 sd in the liberal direction ($[-.14\,\mathrm{sd}, .00\,\mathrm{sd}]$),

Effects on interest-group ratings are similar but have larger standard errors. This is not surprising. Interest-group ratings are less precise than the Nokken–Poole ideology measure. This is because ideal-point estimation requires observing a relatively large number of votes (McCarty, 2011, 2016). The ADA and the ACA pick around 30 to 50 roll calls in a two-year term to determine legislators' ratings. By contrast, although the number of roll calls varied substantially over the 1960s, the Nokken–Poole measure relies on average on around 400 roll calls per term.

Peaceful protests had a clear effect on legislator votes on civil-rights roll calls (table 2.2). They shifted the legislator in the liberal direction by .18 sd ($[-.25\,\text{sd}, -.12\,\text{sd}]$). The effect on welfare roll calls is measured with noise but the point estimate also indicates a .07-sd liberal shift ($[-.13\,\text{sd}, .00\,\text{sd}]$). The effect on environmental roll calls is insignificant at the 10% level.

Violent protests had the opposite effect on all three issue categories. The point estimates are similar but the effect is most pronounced on civil rights. Violent protests shifted the legislator in the conservative direction by .14 sd on these roll calls ($[.06\,\text{sd}, .21\,\text{sd}]$). The effect on welfare roll calls is also statistically significant at the 5% level, but not on environmental roll calls.

2.4.3 Legislator polarization

The shifts in mean legislator behavior came with changes in polarization. To investigate this, I construct a polarization score as follows. Let $s_{\ell t}$ denote the ideology score during congress t of the legislator who represents district ℓ. Within each congress t, I compute the average ideology score, $\bar{s}_{\cdot t}$. I construct a raw polarization score as the absolute deviation from the congress-t mean: $p_{\ell t} \equiv |s_{\ell t} - \bar{s}_{\cdot t}|$. To get standardized effect sizes, I studentize $p_{\ell t}$ by subtracting the mean and dividing by the standard deviation. Therefore, as before, the estimated effects are in units of standard deviation.

Table 2.3: Legislator polarization

	Nokken–Poole		civil rights		welfare		environment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
protest history	0.000		0.003		−0.002		−0.006***	
	(0.0018)		(0.0020)		(0.0017)		(0.0019)	
. . . peaceful		0.002		0.006***		−0.001		−0.006**
		(0.0024)		(0.0022)		(0.0020)		(0.0025)
. . . violent		−0.017*		−0.028***		−0.016		−0.001
		(0.0098)		(0.0085)		(0.0100)		(0.0114)
N	1806	1806	1806	1806	1806	1806	1806	1806

Outcome variables are standardized to have zero mean and unit variance. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Peaceful protests increased polarization in civil-rights legislation by .17 standard deviations (sd; [.05 sd, .30 sd]; table 2.3). The estimate for welfare roll calls is centered around zero. The estimate for environmental roll calls indicates a .18 sd reduction in polarization ([−.32 sd, −.04 sd]).

Violent protests reduced polarization on civil-rights roll calls by .20 sd ([−.32 sd, −.08 sd]) and on welfare roll calls by .11 sd ([−25 sd, .03 sd]). The point estimate for environmental roll calls is close to zero.

2.4.4 Distributional shift in legislator behavior

The question remains if the mean and polarization effects of protests came with an increase or decrease of liberal lawmaking. Peaceful protests shifted the mean to the left while increasing polarization. This may have come either by only already liberal legislators voting more liberally, or by some conservative legislators also voting more liberally. Similarly, violent protests shifted the mean to the right while reducing polarization. Despite the conservative shift, violence may still have gained liberal votes from conservative legislators, at the expense of the support of ardent liberals.

Indeed, on civil-rights votes, evidence suggests that peaceful protests induced a liberal shift in moderately conservative districts. Conversely, violent protests reduced extreme liberalism, and slightly increased moderately conservative representation. To see this, I proceed as follows.

I partition the scale of the raw ideology scores into four subsets. The raw ideology scores are projected Nokken–Poole NOMINATE scores. Recall that zero has a distinct meaning on the projected scale. Roughly, zero means that the legislator supports liberal and conservative positions on roll calls equally as often. Therefore the first division I make is at zero, and I label legislators

Table 2.4: The distribution of legislator ideology, civil-rights roll calls

	liberal		conservative		liberal	either
	extreme	moderate	moderate	extreme	either	moderate
	Panel A: together					
protests	−0.106	0.231**	0.039	−0.164	0.125	0.270**
	(0.0770)	(0.1070)	(0.1593)	(0.1084)	(0.0803)	(0.1190)
N	1806	1806	1806	1806	1806	1806
mean outcome	35.991	30.897	19.103	14.009	66.888	50.000
	Panel B: by protester use of violence					
protests						
... peaceful	0.025	0.114	0.043	−0.182	0.139	0.157
	(0.0705)	(0.1084)	(0.1687)	(0.1127)	(0.0878)	(0.1226)
... violent	−1.414***	1.394***	−0.001	0.022	−0.020	1.393**
	(0.5126)	(0.5001)	(0.4095)	(0.2129)	(0.3698)	(0.5557)
N	1806	1806	1806	1806	1806	1806
mean outcome	35.991	30.897	19.103	14.009	66.888	50.000

The estimates are from linear probability models. Outcome values are either 0 or 100. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

with scores below zero as liberal and above zero as conservative.

The second division is separating legislators with moderate and extreme positions. I define moderate positions as raw scores within the median distance from zero. Formally, I find the median distance as

$$m \equiv \arg\min_{m'} \left(\#\{s_{\ell t} : |s_{\ell t}| \leq m'\} - \#\{s_{\ell t} : |s_{\ell t}| > m'\}\right),$$

and obtain the partition as $(-\infty, -m]$ for extreme liberal, $(-m, 0]$ for moderate liberal, $(0, m]$ for moderate conservative, and (m, ∞) for extreme conservative positions.

I estimate equation (2.4) to obtain protest effects on the probability of the district having extreme liberal, moderate liberal, etc., representation.

I present the results in table 2.4. In the notation of the empirical model, the first four columns show estimates for $Y_{\ell t} \equiv \mathbb{1}\left(s_{\ell t} \in (-\infty, -m]\right)$, $Y_{\ell t} \equiv \mathbb{1}\left(s_{\ell t} \in (-m, 0]\right)$, etc. The fifth column shows estimates for $Y_{\ell t} \equiv \mathbb{1}\left(s_{\ell t} \leq 0\right)$, and the sixth column for $Y_{\ell t} \equiv \mathbb{1}\left(s_{\ell t} \in (-m, m]\right)$,

Violent protests brought about a conservative shift by pushing probability mass from extreme liberalism to moderate liberalism. They reduced the probability of extreme liberal representation by 9.87 percentage points (pp) and increased the probability of moderate liberal representation by 9.73 pp ($[-16.93\,\text{pp}, -2.82\,\text{pp}]$ and $[2.85\,\text{pp}, 16.61\,\text{pp}]$). This led to a 9.72-pp increase in moderate representation ($[2.07\,\text{pp}, 17.37\,\text{pp}]$) but left the probability of liberalism unchanged. Overall, these results suggest that violent protests were not successful in increasing support for liberal legislation.

Peaceful protests had no statistically significant effect on these measures of the distribution. The point estimates indicate a 4.00-pp increase in liberalism and a 4.52-pp increase in moderate representation ($[-.98\,\text{pp}, 8.98\,\text{pp}]$ and $[-2.44\,\text{pp}, 11.48\,\text{pp}]$). This was accompanied by a 5.24-pp reduction in extreme conservatism ($[-11.64\,\text{pp}, 1.52\,\text{pp}]$), and a 3.28-pp increase in moderate liberalism ($[-2.87\,\text{pp}, 9.43\,\text{pp}]$), and a 1.24-pp increase in moderate conservatism ($[-8.33\,\text{pp}, 10.81\,\text{pp}]$).

2.4.5 Mechanism of accountability: protests and election outcomes

The results before show that protests did meaningfully change the mean and the distribution of legislator behavior. Did they do so by removing incumbents from their seats, or by making incumbents change their issue positions? It appears the effect manifested as a combination of the two.

Incumbent response. To construct a test for the presence of incumbent response to protests, consider the following simplified model of ideology, campaign platforms, and protests. Let $Y_{\ell t}$ denote the congress-t incumbent's roll-call voting behavior. Suppose that two candidates run for office in the election for congress t, one of whom is the congress-$(t-1)$ incumbent. Suppose also that both the incumbent and her challenger commit to the platform they would implement if elected. Denote the congress-$(t-1)$ incumbent's platform by $Y^*_{\ell t}$ and her challenger's platform by $Y'_{\ell t}$. We can then decompose the observed roll-call voting behavior as $Y_{\ell t} \equiv Y^*_{\ell t} W_{\ell t} + Y'_{\ell t}(1 - W_{\ell t})$ where $W_{\ell t}$ is a binary variable indicating if the congress-$(t-1)$ incumbent got re-elected. Although $Y^*_{\ell t}$ and $Y'_{\ell t}$ are not observed, $Y_{\ell t}$ and $W_{\ell t}$ are.

Ignore the fixed effects, and consider only a single type of protest activity measured by $A_{\ell t-1}$. Suppose the congress-$(t-1)$ incumbent's platform is given by $Y^*_{\ell t} = \alpha^* + \beta^* A_{\ell t-1} + U^*_{\ell t}$ and the challenger's platform is given by $Y'_{\ell t} = \alpha' + \beta' A_{\ell t-1} + U'_{\ell t}$ with $\mathbb{E}(U^*_{\ell t} \mid A_{\ell t-1}, W_{\ell t}) = \mathbb{E}(U'_{\ell t} \mid A_{\ell t-1}, W_{\ell t}) = 0$. Then

$$\mathbb{E}(Y_{\ell t} \mid A_{\ell t-1}, W_{\ell t}) = \delta_0 + \delta_1 W_{\ell t} + \delta_2 A_{\ell t-1} + \delta_3 W_{\ell t} A_{\ell t-1},$$

where $\delta_0 \equiv \alpha'$, $\delta_1 \equiv \alpha^* - \alpha'$, $\delta_2 \equiv \beta'$, and $\delta_3 \equiv \beta^* - \beta'$. If the incumbent does not respond to protests, $\beta^* = 0$, and therefore $\delta_2 + \delta_3 = 0$.

Table 2.5 takes this test to the data. There is evidence of incumbent response on civil-rights roll calls both for peaceful protests and for violent protests ($p \approx .000$ and $p \approx .007$; column (2)). Incumbent response was also statistically significant for peaceful protests on environmental roll calls ($p \approx .044$; column (4)). However, it was indistinguishable from zero for for both protest types on welfare roll calls ($p > .10$; column (3)) and for violent protests on environmental roll calls ($p \approx .637$; column (4)),

Table 2.5: Incumbent response to protests

	Nokken–Poole (1)	civil rights (2)	welfare (3)	environment (4)
incumbent won[a]	0.258**	0.208**	0.282***	0.221**
	(0.1075)	(0.0895)	(0.1020)	(0.0938)
protest history				
. . . peaceful	−0.002*	−0.006***	−0.003**	0.002
	(0.0013)	(0.0013)	(0.0013)	(0.0014)
. . . violent	0.024***	0.022***	0.021***	0.018**
	(0.0079)	(0.0068)	(0.0078)	(0.0085)
protest history × inc. won				
. . . peaceful	0.001	0.001	0.001**	0.001
	(0.0006)	(0.0006)	(0.0005)	(0.0006)
. . . violent	−0.011	−0.006	−0.012*	−0.014**
	(0.0069)	(0.0073)	(0.0067)	(0.0071)
p for no incumbent response[b]				
. . . peaceful	0.134	0.000	0.249	0.044
. . . violent	0.050	0.007	0.129	0.637
N	1806	1806	1806	1806

[a]Indicates if the candidate who was elected served in the 86th congress.
[b]p-value for the null hypothesis that the coefficients for "protests" and "protests × incumbent won," for the corresponding protest type, sum to zero.
Outcome variables are standardized to have zero mean and unit variance. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

These results suggest two conclusions. First, incumbents did respond to peaceful protests on roll calls that were relevant to the goals of the protests more narrowly. Second, part of the legislative response to protests was brought about by challengers who replaced incumbents, particularly for violent protests.

Table 2.6: Voter behavior

	turnout		Dem. votes (% of pop.)		Rep. votes (% of pop.)		margin of victory	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
protest history								
. . . peaceful	0.045**		−0.004		0.048***		−0.099	
	(0.0202)		(0.0099)		(0.0119)		(0.0717)	
. . . violent	−0.013		−0.051		−0.081*		0.176	
	(0.0821)		(0.0446)		(0.0466)		(0.1775)	
recent protests[a]								
. . . peaceful		0.050		−0.004		0.043**		−0.059
		(0.0349)		(0.0157)		(0.0193)		(0.0707)
. . . violent		−0.088		−0.079*		−0.041		−0.104
		(0.0630)		(0.0425)		(0.0551)		(0.1855)
N	1806	1806	1806	1806	1806	1806	1806	1806
mean outcome	51.851	51.851	17.202	17.202	15.618	15.618	32.539	32.539

[a]Recent protests are measured as $\Delta P_{\ell t-1} = P_{\ell t-1} - P_{\ell t-2}$ for peaceful protests and $\Delta V_{\ell t-1} = V_{\ell t-1} - V_{\ell t-2}$ for violent protests, where $P_{\ell t-1}$ and $V_{\ell t-1}$ denote the peaceful and violent protest histories.

Outcome values are scaled to fall between 0 and 100. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress.

Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Voter behavior. Protests also changed equilibrium voter behavior. A history of peaceful protests increased turnout by 1.30 percentage points (pp; table 2.6, column (1)). The change in turnout was smaller in response to only *recent* protests (.39 pp; column (2)), indicating that past protests mattered for turnout as well. This was accompanied by a small reduction of the population share of Democratic voters (columns (3) and (4)) and a statistically significant increase of the population share of Republican voters (columns (5) and (6)). The margin of victory did not change statistically significantly (columns (7) and (8)).

The point estimates of the peaceful protest history effect were large relative to the mean outcome for Republican vote share and the margin of victory. The increase in Republican vote share was 8.81 percent of the mean, while the reduction in the margin of victory was 8.78 percent of the mean. The increase in turnout was 2.5 percent of the mean.

Violent protests had no statistically significant effect on these outcomes.

Table 2.7: Incumbency

	incumbent during 86th congress				incumbent		incumbent party	
	ran	won	ran	won	ran	won	vote share	won
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
protest history								
. . . peaceful	−0.080	−0.118	−1.060**	−1.022***				
	(0.0923)	(0.1012)	(0.4360)	(0.3844)				
. . . violent	−0.233	0.089	0.755	0.765				
	(0.4645)	(0.3469)	(0.8223)	(0.6758)				
recent protests[a]								
. . . peaceful					−0.233	−0.065	0.065	−0.118
					(0.4836)	(0.5090)	(0.0857)	(0.4315)
. . . violent					−0.379	−0.503	−0.055	−0.017
					(0.6915)	(0.8736)	(0.1737)	(0.7407)
excl. redistricted			YES	YES	YES	YES	YES	YES
N	1806	1806	914	914	914	914	914	914
mean outcome	49.225	45.792	47.921	44.967	85.339	79.431	64.686	89.934

[a]Recent protests are measured as $\Delta P_{\ell t-1} = P_{\ell t-1} - P_{\ell t-2}$ for peaceful protests and $\Delta V_{\ell t-1} = V_{\ell t-1} - V_{\ell t-2}$ for violent protests, where $P_{\ell t-1}$ and $V_{\ell t-1}$ denote the peaceful and violent protest histories.
Outcome values are scaled to fall between 0 and 100. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

In their bids for re-election, incumbents benefited from the redrawing of district boundaries sparked by the US Supreme Court's 1962 decision in the Baker v. Carr case and subsequent cases. Columns (1) through (4) of table 2.7 show estimates from regressing an indicator variable for whether a politician who was an incumbent in the 86th congress ran or won in the district. The 86th congress directly precedes my sample. Thus these estimates reflect the cumulative effects of protests on incumbency. While these effects were not statistically significant in the full sample, peaceful protests are associated with a 30 percentage point reduction in the probability that an 86th-congress incumbent runs or wins.

Columns (5) through (8) define the incumbent as the politician who represented the district in the previous time period. These specifications can only be estimated in districts whose boundaries were not redrawn. The effects on these indicators, as well as on the incumbent party's vote share and on whether the incumbent party's candidate won, were not statistically significant.

2.5 Alternative explanations

The protest effects estimated in section 2.4 would be spurious if they were driven by confounders that peaceful or violent protests picked up. In this section, I consider and test robustness to alternative explanations.

Heterogeneous voter response to national events. Instead of responding to local protest activity, voters may be responding to national events. If different voter types responded differently, and the incidence of protests was correlated with voter composition, the local protest effects I measure would be spurious.

If the relevant dimension of heterogeneity was race, we would expect that districts where a higher share of the population was white followed different trends in the outcome. Moreover, controlling for heterogeneous trends by race should absorb the variation in protests that is correlated with the outcome, rendering the protest effect estimates zero. The idea is similar if the relevant dimension of heterogeneity was another demographic measure.

This motivates the robustness check of adding interactions of the time fixed effect with demographic measures of the district to the vector of controls. Formally, this means controlling for $Z_\ell \times \mathbb{1}\,(t = s)$ for $s \in \{87, \ldots, 92\}$ where Z_ℓ is a fixed district characteristic measured before the 87th congress.

Differential time trends. $\theta_{\text{South}(\ell)\times t}$ in equation (2.4) controls for time trends of arbitrary form that are specific to the South or the rest of the country. It is possible that districts within these regions follow heterogeneous time trends in the outcome. If this heterogeneity is correlated with the incidence of protests, the estimated local protest effects would again be spurious.

In a classical difference-in-differences setup, confirming parallel pre-treatment trends in the outcome would be reassuring evidence that differential time trends are not driving the estimates. In my context, because congressional districts are not stable geographical units in time, I cannot test for violations of parallel pre-treatment trends. Instead, I directly test the robustness of the local protest effects to adding controls for differential time trends.

Reporting bias and media penetration. If reporting bias is driven by a factor other than distance to New York City, this may cause bias in the estimates. Media penetration may be one such factor. Taking two districts that are equally far from New York City, the one that has a less active local media market might be less able to relay news about locally salient events to the national media. If the incidence of protests was correlated with local media market conditions, this could be driving the estimates. To test if this is the case, I add time-interacted controls for newspaper circulation and the population share of those who owned a radio.

2.6 Conclusion

Protest movements recently as well as in the past have relied on both peaceful and violent action to promote their cause and achieve policy change. Whether violence is more effective than peaceful action, or whether it backfires, has not been established yet. I use a fixed-effects strategy to evaluate this question.

Using within-district variation, I find that peaceful protests cause a liberal shift in representation in the US House, while violent protests hurt the goals of the Civil Rights Movement and cause a conservative shift. Evidence is suggestive of the information channel as the key behind these effects. Districts where a larger share of the population was white responded more to both peaceful and violent protests. This is consistent with protests exposing white voters to information that they were shielded from due to segregation. Overall, the results illustrate the role of information asymmetries in the political process, and the adverse effect that violence has in such an environment.

Appendix

2.A Attenuation and amplification bias from misreporting in The New York Times

Consider the simple linear data-generating process $Y_i = \alpha + \beta X_i + U_i$ with $X_i \perp\!\!\!\perp U_i$. Suppose that X_i is measured with error. In particular, instead of X_i we only observe $\widetilde{X}_i \equiv B_i X_i$ where B_i is a multiplicative reporting bias term.

Example 1 (attenuation bias)**.** *Suppose $B_i \sim \text{Bernoulli}(p)$ with $B_i \perp\!\!\!\perp X_i, U_i$. Then with probability $(1-p)$, $\widetilde{X}_i = 0$ when X_i may be non-zero. The probability limit of the OLS estimator is*

$$\widehat{\beta} \xrightarrow{p} \frac{\text{Cov}\left(Y_i, \widetilde{X}_i\right)}{\text{Var}\left(\widetilde{X}_i\right)} = \beta \frac{\text{Cov}\left(X_i, B_i X_i\right)}{\text{Var}\left(B_i X_i\right)}.$$

Here,

$$\text{Cov}(X_i, B_i X_i) = \mathbb{E}(B_i)\text{Var}(X_i) = p\text{Var}(X_i),$$

and

$$\text{Var}(B_i X_i) = \mathbb{E}\left(B_i^2\right)\mathbb{E}\left(X_i^2\right) - \mathbb{E}(B_i)^2\mathbb{E}(X_i)^2 = p\mathbb{E}\left(X_i^2\right) - p^2\mathbb{E}(X_i)^2.$$

Plugging back,

$$\widehat{\beta} \xrightarrow{p} \beta \frac{p\text{Var}(X_i)}{p\mathbb{E}\left(X_i^2\right) - p^2\mathbb{E}(X_i)^2} = \beta \underbrace{\frac{\text{Var}(X_i)}{\text{Var}(X_i) + (1-p)\mathbb{E}(X_i)^2}}_{<1\ \forall p<1\ and\ \mathbb{E}(X_i)\neq 0},$$

therefore unless $p = 1$ *or* $\mathbb{E}(X_i) = 0$, $\widehat{\beta}$ *underestimates the magnitude of* β*.*

Example 2 (amplification bias)**.** *Suppose* $B_i \equiv \bar{B}$ *is constant with* $\bar{B} \in (0, 1)$ *so that when* X_i *is non-zero, so is* $\widetilde{X}_i$*. The probability limit of the OLS estimator is*

$$\widehat{\beta} \xrightarrow{p} \frac{\mathrm{Cov}\left(Y_i, \widetilde{X}_i\right)}{\mathrm{Var}\left(\widetilde{X}_i\right)} = \beta \frac{\mathrm{Cov}(X_i, \bar{B}X_i)}{\mathrm{Var}(\bar{B}X_i)} = \beta \frac{\bar{B}\mathrm{Var}(X_i)}{\bar{B}^2\mathrm{Var}(X_i)} = \beta \frac{1}{\bar{B}}.$$

Therefore $\widehat{\beta}$ *overestimates the magnitude of* β*.*

3

Evaluating Multidimensional Programs in the Presence of Endogenous, Multidimensional Networks

joint project with Robert Garlick and Kate Orkin

3.1 Introduction

Social networks affect economic outcomes in developing economies in a variety of ways. In developing countries, access to formal insurance against economic shocks tends to be limited. By turn, informal insurance does not guarantee full insurance, and households' position in the social network determines their ability to smooth their consumption (Townsend, 1994; Udry, 1994; Ambrus, Mobius, and Szeidl, 2014; Wang, 2015). The literature has also demonstrated ways in which the local structure of networks affects informal borrowing relationships and favor exchange (Karlan, Mobius, Rosenblat, and Szeidl, 2009; Jackson, Rodriguez-Barraquer, and Tan, 2012), and beyond the realm of economic transactions, networks modulate the diffusion of information and beliefs (Conley and Udry, 2010; Banerjee, Chandrasekhar, Duflo, and Jackson, 2013; BenYishay and Mobarak, 2018; Beaman, BenYishay, Magruder, and Mobarak, 2018).

Recognizing that networks play a salient role in economic phenomena, the literature has offered theories that describe how networks form. Homophily and assortative matching, i.e., that people tend to have links with similar others, are a key theme (Currarini, Jackson, and Pin, 2009), and recent literature shows that networks respond to exogenous shocks (e.g., Comola and Prina, 2017). However, we still know relatively little about how development interventions affect network structures.

In this paper, we study households' network formation in response to a large positive economic shock and a psychological intervention in rural Western Kenya. In this setting, as the economy is reliant on agriculture, households are poor, and financial markets are incomplete, households are often set back by negative economic and health shocks. Social networks are crucial for informal risk-sharing arrangements to contain these shocks.

In our study, we cross-randomize villages into two interventions: a large unconditional cash transfer worth approximately US$1,000, and a psychological intervention. The cash transfer is substantial, equivalent to one year's average household income.[1] The psychological intervention is a light-touch intervention designed to promote goal-oriented thinking. We measure household interactions in four types of networks: (i) talking about goals, (ii) talking about challenges and bad experiences, (iii) giving money or goods, and (iv) receiving money or goods. These four network types capture interactions in

[1] The short-term effects of unconditional cash transfers on non-network outcomes in Western Kenya were evaluated by Haushofer and Shapiro (2016).

the psychosocial and the economic domain. We evaluate effects of the interventions on link counts, reported measures of homophily, and interaction intensity.[2] Although we collect identifying information on every link that respondents list, we do not yet parse this information. Therefore, we do not look at link dynamics, nor do we match links to other households in our data.

We find that only the challenges network type responded to the interventions. The cash transfer reduced the number of reported challenges links, and it changed how frequently respondents talked with the people whom they listed as challenges links. The psychological intervention generally had a statistically weak effect but often pointing in the same direction as the effect of the cash transfer. Because of this, the combined effect of the two interventions was often statistically significant. We find little effect on reported measures of homophily, and we find no effect on either the number or the intensity of giving and receiving links.

Taken together, these results offer evidence that in contexts where households are highly vulnerable to economic shocks, social networks are relatively persistent. This implies that exogeneity may be a plausible assumption in comparable contexts when modelling interactions on networks. The fact that we find effects but that these are localized in the challenges network type supports this reading. The fact that for many outcomes we estimate relatively narrow confidence intervals, ruling out large effects in either direction, lends

[2] For both economic shocks and psychological interventions, the empirical literature suggests a role for networks. Cash transfers and other economic shocks can, but do not always, have spillover effects on non-recipients' consumption, human capital investment, and psychological outcomes (Angelucci and De Giorgi, 2009; Bobonis and Finan, 2009; Angelucci, De Giorgi, Rangel, and Rasul, 2010; Baird, de Hoop, and Özler, 2013; Haushofer, Reisinger, and Shapiro, 2015). A smaller literature also shows that psychological interventions can have spillover effects on non-recipients living nearby (Bernard, Dercon, Orkin, and Taffesse, 2014).

further confidence. We explore whether outliers are causing the non-result in the economic domain and we find no evidence for this.

Our paper contributes to recent literature that shows that networks respond to the introduction of formal financial services. Access to a savings account in Nepal and access to microcredit in India both changed household networks (Comola and Prina, 2017; Binzel, Field, and Pande, 2015; Banerjee, Chandrasekhar, Duflo, and Jackson, 2018). By contrast, being offered a large unconditional cash transfer in our setting did not change economic networks. We also contribute to an empirical literature that shows that deliberate network-creation policies are viable (Feigenberg, Field, and Pande, 2013; Fafchamps and Quinn, 2016; Cai and Szeidl, 2018). While this literature shows that concerted attempts to foster interaction can succeed, we show that interventions that do not make such attempts but substantially alter the economic status of households does not have such collateral effects.

The paper proceeds as follows. Section 3.2 outlines the conceptual framework, section 3.3 describes the economic environment, and section 3.4 describes the data that we collect. Section 3.5 specifies our estimating equation. Section 3.6 presents and section 3.7 discusses the results. Finally, section 3.8 concludes.

3.2 Conceptual framework

Our conceptual framework draws on two literatures in economics. First, we build on a literature demonstrating that cash transfers and other economic shocks can but do not always have spillover effects on non-recipients' consumption, human capital investments, and psychological outcomes (Angelucci and De Giorgi, 2009; Angelucci et al., 2010; Baird et al., 2013; Bobonis and Finan, 2009; Haushofer et al., 2015; **?**). A smaller related literature shows that psychological interventions can have spillovers on non-recipients living nearby (Bernard et al., 2014).

Second, we build on a literature studying endogenous network formation. Theoretical models often model link formation as the outcome of utility-maximizing decisions (e.g., Bramoullé et al., 2016). Recent empirical work has documented that policy interventions can change networks (Banerjee et al., 2014a, 2014b; Binzel et al., 2015; Comola and Prina, 2017) and that deliberate network-creation policies are viable (Cai and Szeidl, 2018; Fafchamps and Quinn, 2016; Feigenberg et al., 2013).

We propose a simple conceptual framework incorporating ideas from both literatures. We offer a high-level sketch of the conceptual framework that nests several specific economic models. This conceptual framework guides the experimental design, questionnaire design, and reduced-form analytical strategy.

We assume each pair of agents $(i, j) \in \mathcal{I} \times \mathcal{I}$ form a network link of type $t \in \mathcal{T}$ if the cost of forming the link is smaller than the benefit for both agents. The benefits and costs may depend on i and j's individual attributes, the existence of links of other types between i and j, and i and j's links of all types to other agents.

We assume the cash transfer and psychological intervention occur after agents have already formed links. Either intervention may change the economic and psychological attributes of treated agents in $\mathcal{I}$. This may change the benefit of each potential network link of each type and hence change the realized set of links.

This simple framework suggests that we should estimate the effect of each intervention on networks of all types for both treatment-eligible and -ineligible agents. We might informally expect that the cash transfers will have larger effects on economic links and the psychological intervention will have larger effects on psychosocial links. We might also informally expect that the interventions will have larger effects on the networks of treatment-eligible than -ineligible agents. However, these informal expectations will not hold in all models of network formation. We view these as predictions to be tested using reduced-form methods, rather than structural assumptions to be imposed *ex ante*.

This broad framework nests a range of specific economic models. We do not describe all such models in this pre-analysis plan. However, we provide one simple example to fix ideas and illustrate the welfare consequences of treatment-induced changes in networks. Our example has a similar flavour to models of informal risk-sharing such as Ambrus et al. (2014) and Wang (2015).

Assume each agent i has two attributes: initial wealth W_i and belief $B_i(\cdot)$. Both attributes vary across the population. Each agent i is risk-averse. The model has two periods. In period 1, agent i divides her wealth between consumption and investment. In period 2, agent i earns a stochastic rate of return on her investment. $B_i(\cdot)$ is her belief about the probability distribution from which the rate of return is sampled. She earns utility from consumption in each period. For simplicity, assume agents have identical preferences and differ only in initial wealth, beliefs, and the realization of their second period wealth conditional on first period investment. In period 1, agent i can form links with all other agents. These links correspond to fully enforceable contracts over transfers in each realized period-2 state of the world. Equilibrium in the model is a set of pairwise contracts between all agents such that no individual contract revision is Pareto-improving, given all other contracts. Equilibrium depends on specific assumptions about the distribution of wealth and beliefs in the population, the distribution governing stochastic investment returns, and the structure of preferences.

The cash transfer increases W for a subset of agents and the psychological intervention changes features of the distribution $B(\cdot)$ for a subset of agents.[3] This directly changes the set of contracts treated agents are willing to accept. This indirectly changes the set of contracts untreated agents are able to access. These shifts in network structure may change treatment effects on treated agents and generate treatment effects on untreated agents. For example, the psychological intervention might make agents believe that their outcomes are less influenced by factors outside their control, which would enter the model as a reduction in the variance of the distribution $B(\cdot)$. Some risk-sharing contracts that were previously *ex ante* utility-enhancing will no longer be utility-enhancing. This will narrow the set of risk-sharing contracts untreated agents can access, reducing their *ex ante* utility.

In the context of this example model, our measures of "network connections" described in Section 3.4 are measures of the set of contracts that yield non-zero transfers in the realized state of the world (active links) and some of the contracts that yield non-zero transfers in other possible states of the world (latent links).

[3] The psychological intervention is designed to increase $B(\cdot)$ but this design may fail. Companion work analyzes measures such as self-efficacy and locus of control to $B(\cdot)$ to assess the treatment effects of the psychological intervention.

3.3 Economic environment

3.3.1 Context

Economic geography. Our study area is in Homa Bay and Siaya Counties in Western Kenya, situated on the northeastern coast of Lake Victoria. The overwhelming majority of Homa Bay and Siaya is Luo. The Luo tribe is the fourth largest of the country, at a population share of 11 percent. The coastal region is generally poor and rural. Wage employment is low, and is at only around 11 percent amongst male household heads in our study sample (table 3.1, panel C). While 70 percent of study households own a phone and 57 percent own a radio, only 7 percent own a television set, and 0.2 percent own a refrigerator (table 3.1, panel D).

The local economy is heavily reliant on agriculture, livestock, fishing, and gold mining. Homa Bay and Siaya has a variety of micro-climates which affect economic activity. Fishing is significant in villages close to the lake, and gold mining is significant in places where the land is arid and cannot support agriculture.

Household composition. The heads of 39 percent of study households are widows and about 60 percent are in a monogamous marriage (table 3.1, panel A).[4] The average household has 4.4 members in total. On average, 1.2 members are under 5 years of age, 1.7 members are primary-school age (6 to 13 years of age), and 0.9 members are secondary-school age (14 to 18 years of age).

Household heads in the study sample are young. While 43 percent of

[4] While polygamy is practiced by 14 percent of all households in our census, we exclude these households from the study sample as they are generally wealthier and are not eligible for GiveDirectly's cash transfer.

Table 3.1: Household characteristics

	mean	s.d.	percentile 10th	percentile 90th	N
Panel A: Demographic characteristics					
married, monogamous	0.596	0.491			8299
widowed	0.390	0.488			8299
household members	5.340	5.113	1	9	8299
household members under 5	1.076	1.541	0	3	8299
household members, 6–13	1.518	1.746	0	3	8299
household members, 14–18	0.784	1.524	0	2	8299
mainly speaks Luo, female	0.992	0.091			5098
mainly speaks Luo, male	0.994	0.078			3242
Panel B: Education					
female: no education	0.137	0.344			8263
female: 8th grade or more	0.433	0.495			8263
male: no education	0.026	0.158			4585
male: 8th grade or more	0.650	0.477			4585
Panel C: Economic characteristics					
wage employment, female	0.045	0.208			7775
wage employment, male	0.109	0.311			4409
contributed to financial group	0.521	0.500			7464
Panel D: Assets					
phone	0.705	0.456			7840
radio	0.573	0.495			7840
television set	0.072	0.258			7840
refrigerator	0.002	0.039			7840
Panel E: Home construction materials					
floor: mud	0.847	0.360			7772
floor: cement	0.147	0.354			7772
roof: iron	0.888	0.316			7781
roof: grass	0.109	0.311			7781
walls: mud	0.879	0.326			7781

This table shows the mean, standard deviation, and sample size for certain household characteristics in our study sample. For non-binary variables, it also shows the 10th and the 90th percentiles.

Table 3.2: Village characteristics

	mean	s.d.	percentile 10th	percentile 90th	N
Panel A: Village size and sample coverage					
number of households in village	73.634	43.529	25	128	415
number of study households in village	27.896	15.322	12	46	415
Panel B: Presence of infrastructure					
has electricity	0.625	0.485			395
has primary school	0.582	0.494			395
has clinic	0.154	0.362			395
distance to paved road (minutes)	37.883	40.425	5	105	395

This table shows the mean, standard deviation, and sample size for characteristics of villages in our sample.

female and 65 percent of male study household heads have completed primary school, almost 14 percent of female and 2.6 percent of male study household heads completed no education at all (table 3.1, panel B).

Economic challenges. Households face significant challenges on a regular basis due to negative economic and health shocks. The average study household has 2.6 school-age children. While public education is nominally free, parents have to pay school fees for their children to attend. School fees, especially at the secondary-school level, put significant strain on households' liquidity, and meeting payments is one of the most often reported challenges in focus groups.

Despite economic hardship, many households make investments into agriculture, livestock, and other productive assets. Some of these investments—e.g., a cow, a motorcycle, a fishing boat—are lumpy in that they require a significant amount of cash to make. Building up savings to make these investments is difficult due to frequent claims made by relatives and friends (called the "kin tax" by Jakiela and Ozier, 2015). In this environment, a popular saving technology to relax liquidity constraints is participation in rotating savings and credit associations (ROSCA's). In a ROSCA, participants make contributions at every meeting, and the sum of their contributions is given to a randomly chosen member. In our study sample, 52 percent of households made contributions to a ROSCA or a similar financial group (table 3.1, panel C). The unconditional cash transfer that households in some of our treatment arms receive is designed as an alternative to relax their liquidity constraints.

Household networks. In village economies in developing countries, social networks are important media of information transmission and economic interactions. On average, households in our study sample report somewhat more psychosocial links than economic links: the average total link count was 0.83 across goals and challenges, and 0.73 across receiving and giving (table 3.A.1, panel A). A large share of these links was with households who live in the same village: 0.65 or 78 percent across goals and challenges, and 0.59 or 81 percent across receiving and giving (panel E).

Psychosocial links also exhibit a substantial amount of homophily. 0.33 or 42 percent of goals links are with people who share the same goals, and 0.30 or 33 percent of challenges links are with people who share the same challenges (panel B). Kin relationships are about similarly important. 0.33 or 40 percent of links across goals and challenges, and 0.24 or 33 percent across receiving and giving are with family members (panel E).

Sharing membership of a financial group or of a religious congregation is prevalent but the least so amongst our measured characteristics of homophily (panel C). 0.16 or 20 percent of links across goals and challenges, and 0.11 or 15 percent across receiving and giving are with members of the same financial group. 0.22 or 27 percent across goals and challenges, and 0.14 or 20 percent across receiving and giving are with members of the same church.

As for perceived economic well-being of other households, both goals and challenges links are more strongly associated with heterophily than with homophily (panel D). Only 25 percent of goals links and 16 percent of challenges links are reported to be with households who are equally as well off as the respondent.

3.3.2 Treatment arms

We cross-randomize villages into two interventions, resulting in four treatment arms:

1. **Cash intervention only** (2,070 households in 104 villages, 85% compliance [2% cash only, 14% placebo only, 69% both]). Treated households received a total of US$1,100 (2016 dollars). GiveDirectly estimates that this is equivalent to one year's average household expenditure.[5] The payment was made in three transfers. The first transfer was a token payment of US$100. The second and the third transfers were payments of US$500 each. The payments were made through *M-Pesa*, the most popular mobile money network in Kenya. Households who did not own a cellphone were provided one, and the cost was deducted from their third transfer. Households in this treatment arm also received a placebo variant of the psychological intervention.

[5] See the question "How much do recipients get?" in GiveDirectly's FAQ: `https://www.givedirectly.org/faq#How%20much%20do%20recipients%20get?`.

2. **Psychological intervention only** (2,055 households in 104 villages, 84% compliance). The treatment was designed to target particular psychological constructs of the treated household member: the growth mindset (Dweck, 2012), self-efficacy (Bandura, 1990, 1997), and the locus of control (Lefcourt, 1991). To this end, we engaged treated individuals for 60-to-90 minutes to watch a video and complete an exercise about their future goals. The video features characters who act as role models. These characters model certain psychological approaches to pursuing goals. They imagine their best possible selves (King, 2001) and contrast it with their present conditions. They promote a growth mindset. They identify and prioritize smaller short-run goals that align with their long-term goals. And finally, they describe how their experiences transformed them to develop self-efficacy. In the exercise that followed, participants were reminded of the video. They were asked to imagine their lives in five years, "after everything has gone as well as it possibly could," and having "worked hard and succeeded at accomplishing everything" they wanted. They were asked to draw their imagined future life. They then ranked their goals, and listed concrete steps to take to reach them. They were also asked to consider and discuss strategies that they can use to overcome possible challenges. At the end of the intervention, we gave participants a single-page calendar depicting the role models from the video, and offered them stickers from which they chose the ones that reminded them of their goals. We encouraged participants to put these stickers on their calendars.

3. **Both cash and psychological intervention** (2,136 households in 103 villages, 89% compliance [2% cash only, 15% psychological only,

71% both]). Treated households were randomized into either first receiving the cash intervention or first receiving the psychological intervention.

4. **Control** (2,007 households in 104 villages, 84% compliance). Households in this treatment arm received a placebo variant of the psychological intervention.

3.3.3 Timing and eligibility

The sample covers villages in two counties of Luoland: Homa Bay and Siaya. Data collection was timed as follows:

1. **Household census** (32,964 households):
   - January 13, 2016 – May 31, 2016 in Homa Bay (households);
   - August 16, 2016 – February 3, 2017 in Siaya (households).
2. **Baseline** (8,313 households):
   - April 22, 2016 – August 2, 2016 in Homa Bay (households);
   - October 13, 2016 – March 31, 2017 in Siaya (households).
3. **Intervention** (7,067 households):
   - November 1, 2016 – April 28, 2017 in Homa Bay;
   - April 4, 2017 – July 10, 2017 in Siaya.
4. **Endline** (10,076 households):
   - May 7, 2018 – September 28, 2018 in Homa Bay;
   - July 27, 2018 – in Siaya.

In every village, households were determined to be eligible for the interventions based on a set of poverty indicators. The cut-offs for eligibility varied from village to village. However, in every village, only monogamous households and households with a widowed primary female member were selected.

3.4 Measurement

3.4.1 Questionnaire structure

We surveyed households at three stages during our study: we conducted a census of all households in our study villages, and a baseline and an endline of study households. At each of these three stages, we asked households about their social networks.

We elicited information on four types of links. The four types were: (a) goals, (b) challenges, (c) giving/lending money/goods, and (d) receiving/borrowing money/goods. (a)–(b) constitute the psychosocial network domain. Questions on this domain were designed to capture information flow and networks of psychological support. (c)–(d) constitute the economic network domain. Questions on this domain were designed to capture the flow of money and commodities. This includes the flow of both gifts and loans. We pooled giving with lending and receiving with borrowing in the questionnaire and the analysis. Financial transactions may have ambiguous or tacit expectations of repayment or reciprocation that make it difficult for respondents to clearly differentiate between giving and lending and between receiving and borrowing.

Household census. Before we determined household eligibility for the interventions, we collected basic demographic information during an initial household census. In the census questionnaire, we asked about the most important links in each of the four network types. The household census covers every household in our study villages who agreed to talk to our enumerators, and therefore provides a truncated snapshot of the complete village social network.

Baseline. The baseline questionnaire was structured broadly similarly to the endline questionnaire. The household networks module elicited links in the four network types, (a)–(d). Differently from the endline questionnaire, the household networks module only asked about actual links, and did not ask about latent links.

Endline. The endline questionnaire was structured into 17 main modules, in the following order: (i) a roster of all household members, (ii) household education, (iii) assets and land, (iv) crops and livestock, (v) technology adoption, (vi) non-farm enterprises, (vii) labor supplied and demanded, (viii) savings and loans, (ix) group membership, (x) consumption, (xi) remittances, (xii) psychological constructs (generalized self-efficacy, locus of control, growth mindset, depression), (xiii) income and asset goals, (xiv) risk preferences, (xv) political participation, (xvi) household networks, and (xvii) anthropometry for children under 5.

In the household networks module, we collected information on links in the four network types. Some of this information was designed to capture homophily: whether they were relatives, whether they were members of the same financial group and the same church, whether they shared the same goals and the same challenges, and what their economic well-being was relative to the respondent. All network questions were collected from the perspective of the respondent, typically the female household head in the sampled household. These may be measures of the individual respondents' networks or measures of their households' networks. We acknowledge that individual and household networks are conceptually distinct measures but our data are not designed to explore this distinction.

The household networks module contained five submodules on links. The first four submodules elicited active and latent links in the four network types, (a)–(d), listed above. The fifth submodule examined links listed in the census but not listed in the first four submodules of the endline survey. It was designed to test if these links were genuinely broken or respondents had simply underreported their links. If the links were broken, we asked the respondent why.

3.4.2 Measures of household networks

We define the following families of network measures:

1. **Degree:**

    (a) *Total number of active links.*

    (b) *Total number of latent links.*

2. **Economic homophily and heterophily:**[6]

[6] All of these measures are based on respondents' self-reported assessment of their economic position relative to the other people in their network. We do not construct an index for this family because the three categories are mutually exclusive. Instead, we use the number of links with people who have roughly the same economic status as the respondent as a summary measure of economic homophily.

(a) *Number of links with people who are worse off than the respondent.*

(b) *Number of links with people who are better off than the respondent.*

(c) *Number of links with people who have roughly the same economic status as the respondent.*

3. **Psychosocial homophily:**[7]

(a) *Number of links with people who share the respondent's goals.*

(b) *Number of links with people who share the respondent's challenges.*

4. **Demographic homophily:**

(a) *Number of links in the same village.*

(b) *Number of links with relatives.*

(c) *Number of links with people who have the same gender as the respondent.*

(d) *Number of links with people who are of similar age as the respondent.*[8]

5. **Group homophily:**

(a) *Number of links with members of the respondent's ROSCA or any other type of savings group.*

(b) *Number of links with members of the respondent's church.*

6. **Economic intensity:**[9]

[7] All of these measures are based on respondents' self-reported assessment of their goals and challenges relative to the other people in their network.

[8] We explore how sensitive this measure is to different definitions of "similar age" and report the results of this sensitivity analysis.

[9] We ask respondents the total value and total number of all loans and gifts given in the 30 days prior to the survey. This is shorter than the time period for the existence

(a) *Total value given as a loan or gift in Kenyan shillings.*

(b) *Total value received as a loan or gift in Kenyan shillings.*

(c) *Number of loans or gifts given.*

(d) *Number of loans or gifts received.*

7. **Psychosocial intensity:**

   (a) *Average frequency of talking about goals.*

   (b) *Average frequency of talking about challenges.*

Link shares and link counts. We define "link share" measures in addition to the "link count" measures defined above. For example, we define both the "number of links with relatives" (measure 4b) and the "share of links that are with relatives" (measure 4b divided by measure 1a). We focus primarily on link counts rather than link shares because link shares are missing for respondents with zero total links. Link shares measures therefore capture changes in links only at the intensive margin for a selected sample. Link count measures don't impose this restriction. They capture a combination of changes on the extensive and the intensive margin for the full sample.

Network types. Some of the above measures are specific to network type. We can only construct measures of psychosocial homophily (3a–b) for goals and

of the economic link (12 months) because we expect more measurement error for recall over 12 months than 30 days. The questionnaire allows enumerators to perform in-the-field calculations to derive the total value from answers like "I loaned KSH5,000 to my cousin and each week I give my sister KSH300." The total value and total number of all loans and gifts received are measured in the same way. We construct an index using all four measures, even though two measures are of financial inflows and two measures are of financial outflows. The index is designed to measure respondents' financial integration into a network in both directions. We are also interested in financial outflows and financial inflows separately, which we measure using the four individual outcomes.

challenges. We can only construct measures of transaction amounts (6a–d) for giving and receiving, and measures of the frequency of information flow (7a–b) for goals and challenges.

For all other measures, we define them for each of the four network types.

Intensity of homophilous and heterophilous links. We explore whether treatment effects on the intensity of links are different for homophilous and heterophilous networks. To do so, we estimate treatment effects on measures 6a–d and 7a–b separately for the following categories of links:

- with people who are worse off than, better off than, versus of roughly the same economic status as the respondent;
- with people who share versus do not share the respondent's goals;
- with people who share versus do not share the respondent's challenges;
- with people in the same village versus different villages;
- with people of the same gender as versus different gender to the respondent;
- with people of a similar age as versus different age to the respondent;
- with members versus non-members of the respondent's ROSCA or any other type of savings group; and
- with members versus non-members of the respondent's church.

We then test if treatment effects on intensity are different for homophilous versus heterophilous networks for each measure of intensity.

Table 3.3: Balance on link counts at baseline

	p-value	N
Panel A: Total link count		
goals	0.770	8301
challenges	0.547	8301
receiving	0.654	8301
giving	0.605	8301
Panel B: Psychosocial homophily		
goals: same goals	0.472	8301
challenges: same challenges	0.797	8301
Panel C: Group homophily		
same financial group: goals	0.586	8301
same financial group: challenges	0.439	8301
same financial group: receiving	0.223	8301
same financial group: giving	0.425	8301
same church: goals	0.352	8301
same church: challenges	0.370	8301
same church: receiving and borrowing	0.123	8301
same church: giving and lending	0.744	8301
Panel D: Economic homophily		
goals: less well off	0.494	8301
goals: equally as well off	0.560	8301
goals: better off	0.887	8301
challenges: less well off	0.119	8301
challenges: equally as well off	0.280	8301
challenges: better off	0.212	8301
Panel E: Demographic homophily		
with kin: goals	0.426	8301
with kin: challenges	0.006	8301
with kin: receiving	0.032	8301
with kin: giving	0.235	8301

This table demonstrates balance on network outcomes measured at baseline. We regress each listed outcome by estimating $Y_{0iv} = \beta_C \text{Cash}_v + \beta_P \text{Psych}_v + \beta_{CP} \text{Cash}_v \times \text{Psych}_v + \mathbf{X_{iv}}'\mathbf{\Gamma} + \varepsilon_{iv}$. $\mathbf{X_{iv}}$ is a vector of controls, containing stratum fixed effects. To assess balance, we conduct the joint test $H_0 : \beta_C = \beta_P = \beta_{CP} = 0$, and report the p-value in the second column. Standard errors are clustered at the village level.

3.5 Estimation

3.5.1 Balance on baseline measures

To assess balance, we test the joint hypothesis that $\beta_C = \beta_P = \beta_{CP} = 0$ in the specification

$$Y_{0iv} = \beta_C \text{Cash}_v + \beta_P \text{Psych}_v + \beta_{CP} \text{Cash}_v \times \text{Psych}_v + \mathbf{X_{iv}}'\mathbf{\Gamma} + \varepsilon_{iv}. \quad (3.1)$$

Y_{0iv} is an outcome of household i in village v measured at baseline, and Cash_v and Psych_v are binary indicators of village treatment assignment. $\mathbf{X_{iv}}$ is a vector of controls, comprised of randomization stratum fixed effects and baseline enumerator fixed effects.

3.5.2 Attrition

3.5.3 Estimating equation

We estimate specifications of the form

$$Y_{iv} = \beta_C \text{Cash}_v + \beta_P \text{Psych}_v + \beta_{CP} \text{Cash}_v \times \text{Psych}_v + \mathbf{X_{iv}}'\mathbf{\Gamma} + \varepsilon_{iv}, \quad (3.2)$$

where i and v index individuals and villages, Y_{iv} denotes the outcome of interest measured in the follow-up, Cash_v and Psych_v are indicator variables equal to one for villages assigned to receive, respectively, the cash and the psychological interventions, and $\mathbf{X_{iv}}$ is a vector of baseline conditioning variables.

We estimate equation (3.2) with two different sets of conditioning variables in $\mathbf{X_{iv}}$. The first set of conditioning variables is the baseline value of Y_{iv} and a vector of randomization stratum fixed effects. The second set of conditioning variables is selected by the double LASSO method (Belloni et al., 2014). We allow the LASSO to select from the baseline value of the outcome, randomization stratum fixed effects, education, assets, home construction materials, endline enumerator fixed effects, village size, household size, respondent age, respondent marital status, and number of baseline out-links. All conditioning variables are measured prior to treatment except endline enumerator assignments. We allow the LASSO to select a different set of conditioning variables for each outcome.

3.6 Results

3.6.1 Effects on total link count

The endline survey elicited link counts for both active and latent (hypothetical) links. This allows us to estimate the effects of the cash transfer and the psychological intervention on only active links, on only hypothetical links, and on both.

Active links. The cash transfer led to a weakly statistically significant decrease in the number of reported active challenges and receiving links (table 3.5). The cash transfer and the psychological intervention had compounding effects. Their combined effect was significant at the 5% level ($p \approx .008$ and .032).

Latent links. Neither the cash transfer, nor the psychological intervention had a significant effect on hypothetical link counts on its own (table 3.6). However, their combined effect was significant at the 5% level on challenges and giving, leading to a reduction in reported latent links ($p \approx .042$ and .019).

Table 3.4: Link counts at endline

	mean	s.d.	percentile 10th	percentile 90th	N
Panel A: Total link count					
goals	1.607	1.284	0	3	6982
challenges	1.786	1.196	0	3	6982
receiving	1.883	1.381	0	4	6982
giving	2.129	1.475	0	4	6982
Panel B: Psychosocial homophily					
goals: same goals	0.752	1.049	0	2	6982
challenges: same challenges	0.715	0.979	0	2	6982
Panel C: Group homophily					
same financial group: goals	0.411	0.751	0	1	6982
same financial group: challenges	0.422	0.741	0	1	6982
same financial group: receiving	0.399	0.748	0	1	6982
same financial group: giving	0.480	0.825	0	2	6982
same church: goals	0.545	0.800	0	2	6982
same church: challenges	0.587	0.806	0	2	6982
same church: receiving and borrowing	0.565	0.827	0	2	6982
same church: giving and lending	0.659	0.885	0	2	6982
Panel D: Economic homophily					
goals: less well off	0.152	0.441	0	1	6982
goals: equally as well off	0.532	0.798	0	2	6982
goals: better off	0.908	1.011	0	2	6982
challenges: less well off	0.176	0.467	0	1	6982
challenges: equally as well off	0.572	0.804	0	2	6982
challenges: better off	1.011	1.007	0	2	6982
Panel E: Demographic homophily					
with kin: goals	1.069	1.145	0	3	6982
with kin: challenges	1.209	1.134	0	3	6982
with kin: receiving	1.278	1.241	0	3	6982
with kin: giving	1.319	1.270	0	3	6982

This table shows the mean, standard deviation, and the 10th and 90th percentiles of network outcomes measured at baseline.

Table 3.5: Total link count

	(1) goals	(2) challenges	(3) giving	(4) receiving
cash	−0.010	−0.082*	−0.040	−0.099*
	(0.0522)	(0.0464)	(0.0595)	(0.0600)
psych	0.006	−0.006	−0.033	−0.043
	(0.0515)	(0.0464)	(0.0653)	(0.0597)
cash and psych	−0.056	−0.030	−0.002	0.020
	(0.0721)	(0.0645)	(0.0869)	(0.0808)
goals links at baseline	0.165***			
	(0.0212)			
challenges links at baseline		0.184***		
		(0.0228)		
giving links at baseline			0.190***	
			(0.0221)	
receiving links at baseline				0.143***
				(0.0259)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.197	0.008	0.242	0.032
outcome mean in control group	1.629	1.840	2.161	1.953
outcome std. dev. in control group	1.2793	1.2162	1.5036	1.4391
clusters	413	413	413	413
sample size	6956	6956	6956	6956

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Active and latent links together. Adding up active and latent link counts, the cash transfer significantly reduced the number of challenges links and weakly significantly reduced the number of receiving links (table 3.7). Although the point estimate for the effect of the cash transfer is more negative on receiving links than on challenges links, the former is only significant at the 10% level.

The two interventions combined significantly reduced the number of challenges, giving, and receiving links ($p \approx .001, .040$, and $.020$).

Table 3.6: Hypothetical link count

	(1) goals	(2) challenges	(3) giving	(4) receiving
cash	−0.018	−0.019	−0.047	−0.041
	(0.0281)	(0.0199)	(0.0317)	(0.0442)
psych	−0.009	−0.005	−0.009	−0.015
	(0.0227)	(0.0201)	(0.0342)	(0.0450)
cash and psych	−0.002	−0.012	−0.013	−0.003
	(0.0344)	(0.0259)	(0.0418)	(0.0547)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.235	0.042	0.019	0.151
outcome mean in control group	0.210	0.161	0.249	0.237
outcome std. dev. in control group	0.5967	0.5242	1.2021	1.3736
clusters	413	413	413	413
sample size	6957	6957	6957	6957

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.7: Total link count, including hypothetical

	(1) goals	(2) challenges	(3) giving	(4) receiving
cash	−0.029	−0.103**	−0.083	−0.140*
	(0.0590)	(0.0517)	(0.0704)	(0.0825)
psych	0.000	−0.009	−0.036	−0.057
	(0.0557)	(0.0525)	(0.0743)	(0.0817)
cash and psych	−0.060	−0.050	−0.026	0.013
	(0.0778)	(0.0711)	(0.0980)	(0.1062)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.091	0.001	0.040	0.020
outcome mean in control group	1.839	2.001	2.410	2.190
outcome std. dev. in control group	1.3606	1.3069	1.9157	2.0861
clusters	413	413	413	413
sample size	6957	6957	6957	6957

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.8: Links with people of better, same, or worse economic status (goals)

	(1) better	(2) same	(3) worse	(4) % better	(5) % same	(6) % worse
cash	−0.067*	0.046	0.009	−0.025	0.011	0.014
	(0.0396)	(0.0323)	(0.0166)	(0.0176)	(0.0170)	(0.0107)
psych	−0.037	0.026	0.018	−0.013	−0.006	0.018*
	(0.0369)	(0.0266)	(0.0185)	(0.0171)	(0.0167)	(0.0105)
cash and psych	0.019	−0.056	−0.011	0.024	0.007	−0.026
	(0.0537)	(0.0409)	(0.0248)	(0.0255)	(0.0239)	(0.0160)
better status at baseline	0.157***					
	(0.0210)					
same status at baseline		0.138***				
		(0.0238)				
worse status at baseline			0.090***			
			(0.0325)			
% better status at baseline				0.097***		
				(0.0141)		
% same status at baseline					0.083***	
					(0.0167)	
% worse status at baseline						0.069***
						(0.0243)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.027	0.549	0.339	0.438	0.493	0.592
outcome mean in control group	0.962	0.512	0.140	0.577	0.337	0.080
outcome std. dev. in control group	1.0392	0.7711	0.4246	0.4046	0.3882	0.2144
clusters	413	413	413	392	392	392
sample size	6956	6956	6956	3612	3612	3612

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

3.6.2 Effects on homophily

Looking only at total link counts masks any changes that may be happening to the *composition* of respondents' links. So next, we turn to effects on homophily.

Economic homophily. Our measure of economic homophily is a proxy for whether a link is with a person who is economically more or less well-off than the respondent. This is reported by the respondent. In future work, we will construct another measure of economic homophily, after identifying

Table 3.9: Links with people of better, same, or worse economic status (challenges)

	(1) better	(2) same	(3) worse	(4) % better	(5) % same	(6) % worse
cash	−0.134***	0.044	0.014	−0.036**	0.021	0.013
	(0.0392)	(0.0296)	(0.0178)	(0.0179)	(0.0165)	(0.0106)
psych	−0.055	0.029	0.022	−0.015	0.000	0.013
	(0.0372)	(0.0274)	(0.0190)	(0.0170)	(0.0159)	(0.0108)
cash and psych	0.074	−0.076*	−0.033	0.035	−0.007	−0.025*
	(0.0544)	(0.0394)	(0.0250)	(0.0250)	(0.0233)	(0.0149)
better status at baseline	0.161***					
	(0.0218)					
same status at baseline		0.133***				
		(0.0285)				
worse status at baseline			0.100***			
			(0.0320)			
% better status at baseline				0.065***		
				(0.0126)		
% same status at baseline					0.065***	
					(0.0145)	
% worse status at baseline						0.058***
						(0.0202)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.003	0.942	0.880	0.396	0.401	0.912
outcome mean in control group	1.095	0.554	0.166	0.587	0.316	0.092
outcome std. dev. in control group	1.0381	0.7897	0.4416	0.4021	0.3830	0.2345
clusters	413	413	413	404	404	404
sample size	6956	6956	6956	4727	4727	4727

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

which households respondents' reported links correspond to in our sample.

In isolation, the cash transfer statistically weakly reduced the number of goals links with people whom the respondent considers to be economically more well-off than herself (table 3.8). The combined effect of the two interventions was statistically significant at the 5% level.

The effect of the cash transfer was stronger on the number of challenges links (table 3.9). Both the isolated effect of cash and the combined effect were significant at the 5% level.

On giving link counts, neither of the interventions had either an isolated

Table 3.10: Links with people of better, same, or worse economic status (giving)

	(1) better	(2) same	(3) worse	(4) % better	(5) % same	(6) % worse
cash	−0.058	0.023	0.033	−0.034**	0.014	0.023*
	(0.0404)	(0.0377)	(0.0264)	(0.0160)	(0.0142)	(0.0120)
psych	−0.035	−0.022	0.046	−0.005	−0.016	0.023*
	(0.0369)	(0.0382)	(0.0280)	(0.0143)	(0.0128)	(0.0125)
cash and psych	0.044	−0.021	−0.065*	0.022	0.007	−0.030*
	(0.0546)	(0.0517)	(0.0380)	(0.0216)	(0.0190)	(0.0164)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.216	0.582	0.599	0.291	0.704	0.132
outcome mean in control group	0.916	0.787	0.363	0.444	0.367	0.159
outcome std. dev. in control group	1.0203	0.9558	0.7210	0.3982	0.3761	0.2814
clusters	413	413	413	411	411	411
sample size	6957	6957	6957	5997	5997	5997

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.11: Links with people of better, same, or worse economic status (receiving)

	(1) better	(2) same	(3) worse	(4) % better	(5) % same	(6) % worse
cash	−0.118**	0.019	0.004	−0.035**	0.022	0.010
	(0.0473)	(0.0350)	(0.0164)	(0.0155)	(0.0140)	(0.0086)
psych	−0.055	0.007	0.014	−0.011	0.004	0.007
	(0.0423)	(0.0316)	(0.0171)	(0.0134)	(0.0122)	(0.0081)
cash and psych	0.063	−0.048	−0.017	0.030	−0.018	−0.014
	(0.0623)	(0.0464)	(0.0231)	(0.0210)	(0.0197)	(0.0116)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.014	0.444	0.946	0.284	0.560	0.652
outcome mean in control group	1.199	0.542	0.160	0.623	0.277	0.078
outcome std. dev. in control group	1.1672	0.8351	0.4615	0.3938	0.3596	0.2084
clusters	413	413	413	409	409	409
sample size	6957	6957	6957	5808	5808	5808

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

or a combined effect (table 3.10). On receiving link counts, the cash transfer had a statistically significant effect on economic homophily (table 3.11). The combined effect was also significant at the 5% level.

We also look at homophily in terms of the *share* of links that are similar to the respondent, rather than the *count*. Link shares are computed by dividing the link count by the total number of links. This excludes those respondents

Table 3.12: Links with people sharing same goals or challenges

	(1) same-goals	(2) % same-goals	(3) same-challenges	(4) % same-challenges
cash	0.056	0.047*	0.026	0.021
	(0.0421)	(0.0259)	(0.0399)	(0.0218)
psych	0.046	0.015	0.008	0.009
	(0.0443)	(0.0236)	(0.0389)	(0.0209)
cash and psych	−0.091	−0.022	−0.065	−0.021
	(0.0596)	(0.0350)	(0.0580)	(0.0314)
same-goals links at baseline	0.097***			
	(0.0232)			
% same-goals links at baseline		0.023		
		(0.0162)		
same-challenges links at baseline			0.053**	
			(0.0235)	
% same-challenges links at baseline				0.034**
				(0.0140)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.771	0.093	0.435	0.707
outcome mean in control group	0.731	0.435	0.718	0.387
outcome std. dev. in control group	1.0363	0.4458	0.9696	0.4286
clusters	413	392	413	404
sample size	6956	3612	6956	4727

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

from the analysis who reported no links at all, and this type of selection affects the interpretation that we can give to these results. In general, the effects on link shares are similar but statistically weaker than on link counts.

Psychosocial homophily. On goals and on challenges links, we asked the respondent whether the person they listed had the same goals or the same challenges as herself. We use these questions as our measures of psychosocial homophily.

On link counts, neither of the interventions had either an isolated or a combined an effect (table 3.12). The interventions also had no strong effects on link shares.

Table 3.13: Link counts with family members

	(1) goals	(2) challenges	(3) giving	(4) receiving
cash	−0.022	−0.068	0.013	−0.067
	(0.0497)	(0.0461)	(0.0544)	(0.0533)
psych	−0.053	−0.067	−0.022	−0.078
	(0.0480)	(0.0483)	(0.0532)	(0.0519)
cash and psych	0.018	0.022	−0.016	0.017
	(0.0682)	(0.0662)	(0.0756)	(0.0721)
goals in-family at baseline	0.155***			
	(0.0229)			
challenges in-family at baseline		0.197***		
		(0.0239)		
giving in-family at baseline			0.210***	
			(0.0282)	
receiving in-family at baseline				0.192***
				(0.0347)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.219	0.014	0.644	0.016
outcome mean in control group	1.115	1.286	1.340	1.359
outcome std. dev. in control group	1.1536	1.1577	1.2708	1.3249
clusters	413	413	413	413
sample size	6956	6956	6956	6956

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Demographic homophily. We asked the respondent to specify their relationship with each person whom they listed. Using this, we construct an indicator of whether that person was a family member.

In isolation, neither of the interventions had a significant effect on the number of links with family members (table 3.13). Looking at link shares, amongst respondents who reported at least one link, the psychological intervention statistically significantly reduced the share of challenges links that were with family members (table 3.A.3). This effect was similar but statis-

Table 3.14: Link counts with members of same ROSCA

	(1) goals	(2) challenges	(3) giving	(4) receiving
cash	0.001	0.017	0.015	−0.002
	(0.0276)	(0.0269)	(0.0353)	(0.0298)
psych	0.025	0.062**	0.015	0.005
	(0.0278)	(0.0266)	(0.0323)	(0.0281)
cash and psych	0.014	−0.054	−0.024	−0.023
	(0.0385)	(0.0369)	(0.0465)	(0.0396)
same-ROSCA goals links at baseline	0.226***			
	(0.0236)			
same-ROSCA challenges links at baseline		0.249***		
		(0.0296)		
same-ROSCA giving links at baseline			0.201***	
			(0.0268)	
same-ROSCA receiving links at baseline				0.224***
				(0.0390)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.120	0.275	0.852	0.415
outcome mean in control group	0.398	0.398	0.470	0.404
outcome std. dev. in control group	0.7388	0.7104	0.8121	0.7576
clusters	413	413	413	413
sample size	6956	6956	6956	6956

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

tically weaker on goals and receiving links. The combined effects were not significant.

Group homophily. We asked respondents whether the person whom they listed was a member of their ROSCA (or other financial group) and a member of their church. We use these as our measures of group homophily.

Broadly, neither of the two interventions had an effect on group homophily. On the number of links within the respondent's ROSCA, only the psychological intervention had a significant positive effect, and only on

Table 3.15: Link counts with members of same church

	(1) goals	(2) challenges	(3) giving	(4) receiving
cash	−0.017	−0.022	0.021	−0.025
	(0.0291)	(0.0291)	(0.0338)	(0.0315)
psych	−0.012	0.001	0.000	0.005
	(0.0300)	(0.0298)	(0.0316)	(0.0310)
cash and psych	0.027	0.004	−0.034	0.014
	(0.0403)	(0.0416)	(0.0469)	(0.0452)
same-church goals links at baseline	0.212***			
	(0.0219)			
same-church challenges links at baseline		0.172***		
		(0.0230)		
same-church giving links at baseline			0.197***	
			(0.0277)	
same-church receiving links at baseline				0.243***
				(0.0354)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.933	0.564	0.706	0.842
outcome mean in control group	0.554	0.600	0.656	0.579
outcome std. dev. in control group	0.8051	0.8344	0.8814	0.8464
clusters	413	413	413	413
sample size	6956	6956	6956	6956

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

challenges links (table 3.14). On the number of links within the respondent's church, we measure no effects (table 3.15).

Looking at link shares, amongst respondents who reported at least one link, the combined effect was significant on goals and on challenges links within the respondent's ROSCA (table 3.A.4). The effect of the psychological intervention was similar on challenges link shares as it was on link counts. We measure no effects on the shares of links within the respondent's church (table 3.A.5).

Table 3.16: Frequency of talking with goals links

	(1) < 1/week	(2) 1–3/week	(3) ≥ 4/week	(4) % < 1/week	(5) % 1–3/week	(6) % ≥ 4/week
cash	−0.027	−0.019	0.036	−0.015	−0.006	0.023
	(0.0187)	(0.0425)	(0.0437)	(0.0097)	(0.0213)	(0.0205)
psych	−0.027	0.020	0.007	−0.011	0.012	−0.008
	(0.0183)	(0.0436)	(0.0410)	(0.0101)	(0.0213)	(0.0208)
cash and psych	0.034	−0.009	−0.080	0.026*	0.008	−0.032
	(0.0246)	(0.0573)	(0.0576)	(0.0136)	(0.0288)	(0.0280)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.263	0.849	0.318	0.970	0.497	0.361
outcome mean in control group	0.172	0.614	0.828	0.098	0.366	0.527
outcome std. dev. in control group	0.5036	0.9132	0.9656	0.2452	0.4049	0.4265
clusters	413	413	413	408	408	408
sample size	6957	6957	6957	5386	5386	5386

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Summary indices of psychosocial and group homophily. To test for the overall presence of treatment effects on our homophily measures, we construct standardized summary indices for psychosocial and for group homophily (Anderson, 2008). These indices re-weight the constituting variables by the inverse of their variance–covariance matrix, and subtract the control-group mean and divide by the control-group standard deviation to aide interpretation.

We find no overall effects on either psychosocial or on group homophily (tables 3.A.6 and 3.A.7). This mirrors our earlier findings.

We do not construct a summary index for economic homophily as our outcome variables in this family are mutually exclusive. We also do not construct a summary index for demographic homophily as we only have one outcome variable in this family.

3.6.3 Effects on the intensity of interactions

Changes in the presence of links do not fully characterize changes in the nature of interactions. We assess the effects of the interventions on the intensity of interactions in both the psychosocial (goals and challenges) and the the economic (giving and receiving) domains.

Table 3.17: Frequency of talking with challenges links

	(1) < 1/week	(2) 1–3/week	(3) ≥ 4/week	(4) % < 1/week	(5) % 1–3/week	(6) % ≥ 4/week
cash	-0.056***	-0.071	0.050	-0.019*	-0.026	0.048**
	(0.0193)	(0.0456)	(0.0426)	(0.0095)	(0.0191)	(0.0207)
psych	-0.035*	0.026	-0.009	-0.017	0.017	-0.008
	(0.0206)	(0.0485)	(0.0416)	(0.0103)	(0.0200)	(0.0213)
cash and psych	0.060**	0.023	-0.115**	0.027*	0.017	-0.043
	(0.0262)	(0.0610)	(0.0587)	(0.0141)	(0.0273)	(0.0287)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.107	0.614	0.048	0.411	0.671	0.875
outcome mean in control group	0.216	0.694	0.903	0.115	0.371	0.503
outcome std. dev. in control group	0.5503	0.9384	0.9921	0.2693	0.4113	0.4301
clusters	413	413	413	410	410	410
sample size	6957	6957	6957	6045	6045	6045

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Frequency of talking in the psychosocial domain. We asked respondents how frequently they talked in the past 12 months with the people whom they list as goals and challenges links. These serve as our intensity measures for these two network types.

We see no effects of either intervention on goals links (table 3.16). However, we see significant effects on challenges links (table 3.17). The cash transfer strongly significantly reduced the number of challenges links that the respondent had the least frequent interactions with (i.e., talked with less than once a week). The combined effect of the cash transfer and the psychological intervention on this category was smaller and statistically insignificant ($p \approx .107$). Conversely, the effects of the interventions in isolation were not significant on the links with the most frequent interactions (i.e., talked with at least four times a week). However, the combined effect was negative and statistically significant ($p \approx .048$).

Frequency and size of transfers in the economic domain. We asked respondents how many times in the past 30 days they transacted with the people whom they listed as giving or receiving links. We also asked them what the total

Table 3.18: Frequency and value of given transfers

	(1) tot. transf.	(2) avg. transf.	(3) tot. value	(4) avg. value
cash	−2.473	−1.304	32.590	25.465
	(2.5890)	(1.6846)	(65.2283)	(32.0823)
psych	0.796	−0.634	−29.577	−14.379
	(4.1319)	(2.1134)	(68.5233)	(29.5273)
cash and psych	0.348	1.424	36.045	26.842
	(3.9853)	(2.3142)	(114.7032)	(48.5289)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.596	0.777	0.655	0.261
outcome mean in control group	7.154	4.027	955.091	442.726
outcome std. dev. in control group	73.1016	54.9798	1694.6608	697.6140
clusters	413	411	413	411
sample size	6957	5997	6957	5997

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.19: Frequency and value of received transfers

	(1) tot. transf.	(2) avg. transf.	(3) tot. value	(4) avg. value
cash	−0.485	0.037	1.264	30.622
	(0.5998)	(0.5976)	(94.2189)	(53.7313)
psych	0.247	0.576	−75.647	−53.606
	(0.8606)	(0.9092)	(90.9164)	(48.5742)
cash and psych	2.973	2.616	63.033	66.867
	(2.2800)	(2.5400)	(152.2600)	(82.7204)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.183	0.159	0.918	0.486
outcome mean in control group	4.190	2.207	1321.605	725.106
outcome std. dev. in control group	13.5716	5.3222	2838.9652	1488.8195
clusters	413	409	413	409
sample size	6957	5808	6957	5808

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

value of their transactions with each person was. We use this information to assess effects on economic link intensity.

We see neither isolated nor combined effects on either the frequency or the value of given transfers (table 3.18). Similarly, we see no effects on received transfers (table 3.19). Notably, the standard errors on our estimates are large, and the confidence intervals are wide and admit considerable effect sizes in either direction. To investigate whether outliers are introducing noise, masking the treatment effects, we construct two types of outlier-robust alternative measures. First, we winsorize the outcomes at the 1st and the 99th percentiles (tables 3.A.9 and 3.A.10). Second, we take the inverse hyperbolic sine of the outcomes (tables 3.A.11 and 3.A.12). With neither of these alternatives do we get different results.

Summary indices of economic link intensity. Finally, we also construct summary indices to assess the overall effects of the two interventions. We also see no isolated or combined effects on these summary indices (table 3.A.8).

3.7 Discussion

Effects. Overall, the cash transfer shows a stronger effect on network outcomes than the psychological intervention. Moreover, the main effect that we measure is on interactions about the respondent's challenges and bad experiences. This is consistent with the cash transfer helping recipients cope with negative shocks without changing much about recipients' social interactions.

Although we do measure an effect of the psychological intervention on challenges links within the respondent's ROSCA, we are cautious about interpreting this estimate. One possibility is that the psychological intervention changed how respondents approached coping with their challenges. It might have changed what type of advice they sought and who they sought it from. An alternative possibility is that the psychological intervention made respondents, and those of their friends who were also treated, more goal-focused. Being more goal-focused, they were more likely to join a ROSCA, and became more likely to be in the same one as people whom they had already had links with. It is left to future work to speak to these explanations in detail.

The combined effects of the cash transfer and the psychological intervention were stronger. For some of the outcome measures, the coefficients on the interaction terms indicate that the two interventions are substitutes. However, their compound effect is often still statistically more significant than the individual effects in isolation.

We measure that the interventions combined led to a reduction in the number of challenges links with family members. Furthermore, they led to a reduction in the number of challenges links that the respondent frequently talked with.

Indeed, as many of the effects on homophily and intensity were concentrated in the challenges network type, we see no overall effects on the summary indices that we construct.

Although many of our estimates are statistically indistinguishable from zero, most of our confidence intervals are relatively tight. For example, the effect of the psychological intervention on the number of active links (table 3.5) was a reduction no greater than 0.095 in goals, 0.097 in challenges, 0.161 in giving, and 0.160 in receiving. These correspond to fewer than 1 in every 6 respondent losing one giving (or one receiving) link, or equivalently a loss of 8 percent of links at the control mean, at the lower end of our 95%

confidence interval.

Importantly, our results so far have not substantively exploited the panel nature of our data, namely that we observe respondents' links both at baseline and at endline. Using this information will allow us to assess not only whether aggregate features such as link counts changed in response to the interventions, but also whether existing links were severed and new links were formed.

Measurement. We might be concerned that respondents who received the cash transfer were more willing to report their links in the survey. This might be out of increased trust or due to positive reciprocity. In particular, we might be concerned that the entire effect that we estimate is driven by differential trust or reciprocity. However, our patterns of results indicate otherwise. First, we estimate *lower* link counts amongst respondents who received the cash transfer. Second, the effects of the cash transfer are not even across the four network types. They are the most pronounced on challenges links, and have little effect on goals and giving links. This is consistent with the cash transfer reducing the number of challenging life events, and changing the situations in which the respondent needs financial assistance.

In addition to these patterns, there are features of the survey that we will exploit to test for the role of survey fatigue that might be differentially affecting respondents who received an intervention.

3.8 Conclusion

Social networks crucially affect information flow and economic outcomes in rural developing-country contexts. However, we still know little about the effect of economic interventions on households' social networks. In this paper, we study a combination of a large unconditional cash transfer and a psychological intervention in Western Kenya. We assess the effects of the interventions on households' links in both economic and psychosocial networks. We find that the effects are concentrated in received transfers and in interactions about challenges and bad experiences, and that in these network types the interventions reduced link counts. We find little effect in homophily and in interaction intensity outside of challenges.

Our results suggest that even an unconditional cash transfer that amounts to one year's household income changes little in the structure of households' social and economic interactions. Although conclusively making this finding requires more detailed analysis, it would provide partial reassurance about the assumption of exogenous networks, made in much of the literature.

In further work, we will explore three avenues. First, we will investigate heterogeneous treatment effects. Second, we will incorporate information on the identity of links and use this to study the severing of existing links and the formation of new links. Third, we will follow links in our data of surveyed households. This will allow us to study both direct spillover effects, and homophily in a more detailed way.

Appendix

3.A Additional tables

Table 3.A.1: Link counts at baseline

	mean	s.d.	percentile 10th	percentile 90th	N
Panel A: Total link count					
goals	0.779	0.755	0	2	8313
challenges	0.891	0.675	0	2	8313
receiving	0.717	0.697	0	2	8313
giving	0.746	0.806	0	2	8313
Panel B: Psychosocial homophily					
goals: same goals	0.330	0.570	0	1	8313
challenges: same challenges	0.297	0.524	0	1	8313
Panel C: Group homophily					
same financial group: goals	0.201	0.487	0	1	8313
same financial group: challenges	0.127	0.357	0	1	8313
same financial group: receiving	0.069	0.275	0	0	8313
same financial group: giving	0.145	0.427	0	1	8313
same church: goals	0.238	0.506	0	1	8313
same church: challenges	0.204	0.446	0	1	8313
same church: receiving and borrowing	0.109	0.337	0	1	8313
same church: giving and lending	0.179	0.452	0	1	8313
Panel D: Economic homophily					
goals: less well off	0.036	0.197	0	0	8313
goals: equally as well off	0.195	0.459	0	1	8313
goals: better off	0.335	0.592	0	1	8313
challenges: less well off	0.046	0.217	0	0	8313
challenges: equally as well off	0.144	0.376	0	1	8313
challenges: better off	0.327	0.558	0	1	8313
Panel E: Demographic homophily					
with kin: goals	0.333	0.621	0	1	8313
with kin: challenges	0.335	0.567	0	1	8313
with kin: receiving	0.183	0.443	0	1	8313
with kin: giving	0.307	0.599	0	1	8313
same village: goals	0.624	0.758	0	2	8313
same village: challenges	0.667	0.676	0	1	8313
same village: receiving	0.556	0.666	0	1	8313
same village: giving	0.603	0.771	0	2	8313

This table shows the mean, standard deviation, and the 10th and 90th percentiles of network outcomes measured at baseline.

Table 3.A.2: Hypothetical link share

	(1) goals	(2) challenges	(3) giving	(4) receiving
cash	−0.018	−0.001	−0.009	−0.007
	(0.0117)	(0.0078)	(0.0082)	(0.0091)
psych	−0.007	0.001	0.002	0.003
	(0.0108)	(0.0080)	(0.0093)	(0.0095)
cash and psych	0.016	−0.009	−0.005	−0.004
	(0.0155)	(0.0110)	(0.0119)	(0.0129)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.397	0.215	0.099	0.251
outcome mean in control group	0.111	0.067	0.073	0.076
outcome std. dev. in control group	0.2759	0.2015	0.2086	0.2147
clusters	410	411	411	409
sample size	5746	6224	6138	5984

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.A.3: Link shares with family members

	(1) goals	(2) challenges	(3) giving	(4) receiving
cash	−0.003	−0.003	0.016	−0.002
	(0.0229)	(0.0186)	(0.0184)	(0.0190)
psych	−0.039*	−0.039**	−0.014	−0.035*
	(0.0226)	(0.0180)	(0.0193)	(0.0181)
cash and psych	0.030	0.019	0.003	0.014
	(0.0317)	(0.0272)	(0.0294)	(0.0272)
goals in-family at baseline	0.141***			
	(0.0134)			
challenges in-family at baseline		0.094***		
		(0.0122)		
giving in-family at baseline			0.118***	
			(0.0139)	
receiving in-family at baseline				0.084***
				(0.0154)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.597	0.233	0.825	0.241
outcome mean in control group	0.670	0.693	0.617	0.690
outcome std. dev. in control group	0.4066	0.3955	0.3951	0.3895
clusters	392	404	392	389
sample size	3612	4727	3699	3711

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.A.4: Link shares with members of same ROSCA

	(1) goals	(2) challenges	(3) giving	(4) receiving
cash	−0.004	0.021	−0.006	0.006
	(0.0191)	(0.0174)	(0.0183)	(0.0180)
psych	0.012	0.034**	0.004	0.004
	(0.0188)	(0.0163)	(0.0184)	(0.0169)
cash and psych	0.025	−0.008	0.003	−0.008
	(0.0256)	(0.0234)	(0.0251)	(0.0235)
same-ROSCA goals links at baseline	0.152***			
	(0.0151)			
same-ROSCA challenges links at baseline		0.133***		
		(0.0175)		
same-ROSCA giving links at baseline			0.118***	
			(0.0154)	
same-ROSCA receiving links at baseline				0.084***
				(0.0202)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.047	0.004	0.931	0.900
outcome mean in control group	0.262	0.222	0.233	0.219
outcome std. dev. in control group	0.3711	0.3515	0.3446	0.3444
clusters	392	404	392	389
sample size	3612	4727	3699	3711

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.A.5: Link shares with members of same church

	(1) goals	(2) challenges	(3) giving	(4) receiving
cash	0.000	0.019	−0.003	0.005
	(0.0198)	(0.0168)	(0.0168)	(0.0177)
psych	−0.027	−0.001	−0.009	−0.000
	(0.0196)	(0.0163)	(0.0165)	(0.0160)
cash and psych	0.033	0.002	0.005	−0.000
	(0.0276)	(0.0243)	(0.0237)	(0.0245)
same-church goals links at baseline	0.160***			
	(0.0154)			
same-church challenges links at baseline		0.113***		
		(0.0143)		
same-church giving links at baseline			0.116***	
			(0.0140)	
same-church receiving links at baseline				0.129***
				(0.0179)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.719	0.234	0.691	0.801
outcome mean in control group	0.349	0.329	0.323	0.301
outcome std. dev. in control group	0.3952	0.3898	0.3665	0.3697
clusters	392	404	392	389
sample size	3612	4727	3699	3711

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.A.6: Summary index of psychosocial homophily

	(1) link counts	(2) link shares
cash	0.045 (0.0421)	0.080 (0.0630)
psych	0.029 (0.0422)	0.006 (0.0554)
cash and psych	−0.086 (0.0603)	−0.048 (0.0868)
psychosocial homophily at baseline	0.067*** (0.0162)	
relative psych. homophily at baseline		0.044** (0.0209)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.789	0.555
outcome mean in control group	0.000	-0.006
outcome std. dev. in control group	1.0000	1.0037
clusters	413	377
sample size	6956	3100

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.A.7: Summary index of group homophily, based on link counts

	(1) goals	(2) challenges	(3) giving	(4) receiving
cash	−0.013	−0.005	0.027	−0.020
	(0.0368)	(0.0368)	(0.0435)	(0.0397)
psych	0.011	0.051	0.011	0.007
	(0.0383)	(0.0365)	(0.0404)	(0.0386)
cash and psych	0.032	−0.039	−0.043	−0.009
	(0.0514)	(0.0519)	(0.0582)	(0.0549)
group homophily in goals at baseline	0.159***			
	(0.0170)			
group homophily in challenges at baseline		0.139***		
		(0.0161)		
group homophily in giving at baseline			0.133***	
			(0.0178)	
group homophily in receiving at baseline				0.124***
				(0.0168)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.383	0.828	0.901	0.556
outcome mean in control group	-0.000	0.000	-0.000	0.000
outcome std. dev. in control group	1.0000	1.0000	1.0000	1.0000
clusters	413	413	413	413
sample size	6956	6956	6956	6956

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.A.8: Summary index of economic link intensity

	total		average	
	(1) giving	(2) receiving	(3) giving	(4) receiving
cash	−0.014	−0.002	−0.006	0.021
	(0.0362)	(0.0333)	(0.0336)	(0.0362)
psych	−0.002	−0.025	−0.019	−0.035
	(0.0505)	(0.0323)	(0.0388)	(0.0326)
cash and psych	0.016	0.038	0.040	0.050
	(0.0594)	(0.0545)	(0.0481)	(0.0556)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	1.000	0.783	0.689	0.393
outcome mean in control group	-0.000	-0.000	0.000	0.000
outcome std. dev. in control group	1.0000	1.0000	1.0000	1.0000
clusters	413	413	411	409
sample size	6957	6957	5997	5808

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.A.9: Frequency and value of given transfers (winsorized)

	(1) tot. transf.	(2) avg. transf.	(3) tot. value	(4) avg. value
cash	−0.233	−0.078	6.479	16.526
	(0.1900)	(0.0707)	(54.2260)	(26.9805)
psych	−0.087	−0.054	−42.525	−27.923
	(0.2003)	(0.0671)	(59.3593)	(23.4606)
cash and psych	0.110	0.078	62.826	38.530
	(0.2736)	(0.0908)	(95.9947)	(40.0855)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.280	0.412	0.715	0.341
outcome mean in control group	4.017	1.855	926.562	429.050
outcome std. dev. in control group	5.1046	1.8441	1507.7692	593.3777
clusters	413	411	413	411
sample size	6957	5997	6957	5997

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.A.10: Frequency and value of received transfers (winsorized)

	(1) tot. transf.	(2) avg. transf.	(3) tot. value	(4) avg. value
cash	−0.287	−0.007	−14.946	25.008
	(0.1782)	(0.0741)	(69.9661)	(38.3558)
psych	−0.224	−0.091	−32.131	−26.796
	(0.1842)	(0.0736)	(69.7028)	(35.7401)
cash and psych	0.187	0.049	22.549	20.091
	(0.2516)	(0.1090)	(114.8729)	(59.7318)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.078	0.533	0.776	0.684
outcome mean in control group	3.792	1.987	1231.095	674.991
outcome std. dev. in control group	5.0244	2.0733	2017.3493	965.2371
clusters	413	409	413	409
sample size	6957	5808	6957	5808

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.A.11: Frequency and value of given transfers (inverse hyperbolic sine)

	(1) tot. transf.	(2) avg. transf.	(3) tot. value	(4) avg. value
cash	−0.020	−0.017	0.105	0.133
	(0.0449)	(0.0308)	(0.1196)	(0.0922)
psych	−0.012	−0.014	−0.010	0.002
	(0.0483)	(0.0301)	(0.1244)	(0.0823)
cash and psych	0.015	0.028	−0.007	−0.038
	(0.0632)	(0.0400)	(0.1710)	(0.1256)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.702	0.936	0.474	0.279
outcome mean in control group	1.570	1.175	5.436	5.580
outcome std. dev. in control group	1.1454	0.7626	3.2246	2.2795
clusters	413	411	413	411
sample size	6957	5997	6957	5997

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3.A.12: Frequency and value of received transfers (inverse hyperbolic sine)

	(1) tot. transf.	(2) avg. transf.	(3) tot. value	(4) avg. value
cash	−0.032	−0.002	0.013	0.060
	(0.0410)	(0.0277)	(0.1195)	(0.0881)
psych	−0.043	−0.030	−0.151	−0.107
	(0.0451)	(0.0282)	(0.1294)	(0.0886)
cash and psych	0.020	0.022	0.018	−0.014
	(0.0608)	(0.0410)	(0.1764)	(0.1294)
p-value for $H_0 : \beta_C + \beta_P + \beta_{CP} = 0$	0.203	0.718	0.321	0.513
outcome mean in control group	1.518	1.220	5.681	6.118
outcome std. dev. in control group	1.0899	0.7039	3.3149	2.1590
clusters	413	409	413	409
sample size	6957	5808	6957	5808

Standard errors are clustered at the village level.
Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

4

Conclusion

Using both historical and primary survey data, this dissertation has shed light on the economics and the political economy of unequal societies. In chapter 2, I have combined data on protest events during the 1960s US Civil Rights Movement with measures of legislator behavior in the US House. I have showed that protests can be effective at changing legislator behavior but the choice of protest tactic is crucial. Namely, peaceful protests helped the cause of the Civil Rights Movement, but violent protests damaged it.

In chapter 3, I have looked at the social networks of poor households in Western Kenya. Due to the reliance of the local economy on agriculture, the profound economic poverty of households, and incomplete financial markets to contain risk, households' links in social networks are crucial for their ability to cope with negative economic and health shocks. In a project joint with Robert Garlick and Kate Orkin, we studied the effects of two development interventions on social networks. We cross-randomized villages into a large unconditional cash transfer and a psychological intervention. We assessed effects of these interventions on links in psychosocial network types as well as in economic network types. We found that in our context, despite the substantial positive economic shock that the cash transfer constituted, the interventions had no substantial effects on the number of links households had. We also found no substantial effects on measures of homophily and measures of link intensity. These results provide evidence on the persistence of social networks, and lay the foundations for future work in the study of social networks.

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Biography

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