Can An Economic Crisis Shift Attitudes From Socialism to Capitalism? Evidence From a Quasi Natural Experiment[*]

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Abstract

While many socialist countries suffered from harsh economic crises, studying their impacts on economic and political attitudes is challenging because of the scarcity of reliable data in non-democratic contexts. We study a democratic socialist setting for which we have ample information on such attitudes: the Israeli kibbutzim. Exploiting an economic crisis that hit some kibbutzim more than others, we find that the crisis led to reduced support for leftist political parties. This effect persisted for over 20 years after the crisis had ended. The electoral movement we document was rooted in a rightward shift in economic attitudes, suggesting that economic crises may undermine socialist regimes by silently changing attitudes toward them. Uniquely to our setting, we can also study mechanisms of recovery from the crisis. First, we find that while a sharp debt relief arrangement restored trust in the leadership, it did not reverse the impact of the crisis on economic beliefs. Second, as part of their efforts to recover from the crisis, kibbutzim liberalized their labor markets. Analyzing the staggered shift away from equal sharing to market-based wages, we find that this labor market liberalization led kibbutz members to move further rightward in their political voting and economic attitudes.

A new revolution is possible only as a result of a new crisis - Marx and Engels (1850).

1 Introduction

Economic crises disrupt lives, reshape societies, and alter policies. For this reason, an extensive body of work in the social sciences has studied their political implications, uncovering major influences on political attitudes and behavior (Margalit, 2019; Giuliano and Spilimbergo, 2023a).[1] Most of this research was based

[1]Abundant work has contributed to our understanding of the political implications of economic shock downfalls. To mention a few recent empirical studies, in order of publication: Alesina and Giuliano (2011); Brunner et al. (2011); Stevenson and

on the experience of market economies with limited attention paid to socialist economic systems. Yet, such systems might respond differently to economic crises. Furthermore, the analysis in existing studies centered on the crisis period without evidence of the post-crisis period and recovery mechanisms. In this paper, we contribute to the literature in three directions. First, we study the impact of an economic crisis in a socialist system. Second, our case study allows us to follow the socialist economic system from the beginning to the recovery from the crisis, through the signing of a debt relief agreement and the liberalization of labor markets. We exploit this transition away from socialism to study how institutional transformations feed back into attitudes (Acemoglu and Robinson, 2021; Acemoglu et al., 2021). Finally and without exaggerating external validity, our findings might have implications beyond the Israeli context, adding to the understanding of the shift from socialist regimes to the more market-oriented systems that took place around the world.

Studying economic crises in socialist settings is important, given that many socialist countries have endured harsh economic downfalls. Moreover, our ability to extrapolate findings from capitalism to socialism is limited, given that individuals in socialist systems differ in attitudes, education, and information (Alesina and Fuchs-Schündeln, 2007; Abramitzky and Sin, 2014; Fuchs-Schündeln and Masella, 2016). The scarcity of work in non-market settings may stem from the fact that studying political attitudes in a socialist context is challenging. Because socialist countries are often not democracies, they tend to administer fewer surveys. Furthermore, as respondents might be intimidated by state authorities, their responses are less likely to be truthful. Most importantly, electoral preferences are harder to quantify as elections in socialist countries are often not free. To overcome these challenges, we focus on Israeli kibbutzim (plural of kibbutz), communities considered among the most successful and longest-lived experiments in voluntary and democratic socialism.

Throughout the 20th century, members of kibbutzim owned their assets jointly, and individuals' monthly incomes were set to be equal for all members regardless of education or productivity. However, in the mid-1980s, an unexpected and severe economic crisis hit the kibbutzim, with some more affected than others. We use the fact that different kibbutzim experienced economic crises of varying severity as a quasi-natural experiment to study their effect. This natural experiment, combined with the kibbutzim having democratic and free political institutions alongside an egalitarian economy, allows us to explore the political implications of economic downfalls in a socialist system.

Exploiting Israeli electoral data from 1977 to the present and using difference-in-differences methodology, we find that members of kibbutzim who experienced a more severe crisis shifted rightward in their voting in national elections.[2] The crisis' effect persisted for more than two decades after it was over. This finding is especially striking given that kibbutzim traditionally overwhelmingly supported the leftist parties, with over 90% of the votes cast to these parties before the crisis. The ideology of leftist parties resonated very strongly with the traditional socialist kibbutzim. Economically, the left-wing parties in Israel hold a more socialist ideology and advocate for stronger redistributive policies, while the right-wing parties support a more capitalist market economy. The rightward move of kibbutzim is an indication of a decline in support for the socialist ideology and an increase in support for a more market economy.

We next turn to directly analyze the effect of the crisis on attitudes using surveys administered to thousands of kibbutz members, eliciting their social, political, and economic attitudes.[3] Our challenge here

Wolfers (2011); De Bromhead et al. (2013); Margalit (2013); Guriev and Ananiev (2015); Funke et al. (2016); Algan et al. (2017); Karadja et al. (2017); Martén (2019); Ahlquist et al. (2020); Autor et al. (2020); Guiso et al. (2021); Guriev and Papaioannou (2022); Carreri and Teso (2023); Giuliano and Spilimbergo (2023b). For comprehensive reviews, see: Margalit (2019); Giuliano and Spilimbergo (2023a).

[2]This finding aligns some anecdotal evidence from the kibbutzim. See Tabakoff (2019).

[3]Throughout this work we use the term "attitudes" to capture people's opinions regarding economic and political issues.

is that the surveys were only administered from 1989, after the crisis had begun. To address this concern, we perform various tests that all show no correlation between pre-crisis attitudes and the severity of the crisis. In particular, we show that the intensity of the crisis does not predict pre-crisis voting behavior or affiliation with more leftist ideological movements, nor does it predict the pre-crisis dissolution of the policy of communal sleeping arrangements that became controversial in the 1970s. Hence, if there is a correlation between the crisis severity and the pre-crisis attitudes, it would have to be orthogonal to the variations of all the proxies we examine. As we find this to be unlikely, these results support the interpretation that our findings using the survey suggest a causal effect.

We uncover that members of kibbutzim who experienced more severe economic crises increased support for the liberalization of labor markets and reduced support for socialism. Similar to the evidence on voting patterns, this attitude shift persisted for over two decades, long after the economic crisis was resolved. Moreover, the effect of the crisis persisted to a greater degree in adolescents and young adults, consistent with the "impressionable years hypothesis" (Krosnick and Alwin, 1989; Carreri and Teso, 2023). According to this hypothesis, political perceptions are most amendable to life events after childhood and during young adulthood. Events occurring later in life are expected to have a lesser influence on one's attitudes.

The finding that the economic crisis lowered support for redistribution does not align with the evidence from capitalist societies. Existing research shows that following economic downturns, people tend to lose trust in the governing institutions (Guriev and Ananiev, 2015; Algan et al., 2017), pushing them to vote for more anti-establishment and extreme parties on the right and on the left (Funke et al., 2016; Autor et al., 2020). Still, most of the work that can directly elicit economic preferences shows that in terms of underlying attitudes, people in market-based systems demand more economic security and redistribution following a crisis (Alesina and Giuliano, 2011; Margalit, 2013; Karadja et al., 2017; Margalit, 2019; Martén, 2019; Ahlquist et al., 2020).[4] Even when an electoral rightward shift manifests itself, this is often explained by reasons unrelated to demand for redistribution (Guriev and Papaioannou, 2022; Giuliano and Spilimbergo, 2023a).

The leading explanation for the observed pattern in capitalistic systems is that individuals who become sufficiently poorer due to a negative economic shock will be interested in the state expanding its welfare programs (Meltzer and Richard, 1981). In addition, deep-rooted beliefs, such as the importance of luck in determining economic status, may change due to a crisis (Piketty, 1995; Alesina and La Ferrara, 2005; Alesina and Fuchs-Schündeln, 2007). On the face of it, these mechanisms should also operate in socialist contexts such as the kibbutzim, so why did the effect we document point in the opposite direction?

One possibility is that trust in socialist institutions plummeted following the crisis, resulting in aversion to the ideology affiliated with such institutions. Indeed, the crisis caused a sharp decline in the trust in the kibbutz leadership, consistent with findings from capitalistic contexts (Guriev and Ananiev, 2015; Algan et al., 2017). Yet, once we broaden the scope of our analysis, we find that this cannot be the whole story. In an innovative part of our paper, we study the impact of a recovery plan. We do this by exploiting a staggered signing of debt relief arrangements with the government and banks that paved the road to recovery for kibbutzim joining them. We find that signing the debt relief agreements restored trust in the kibbutz leadership. Consequently, the negative effect on trust was short-lived and completely reversed seven

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These may be driven both by preferences and beliefs. See Giuliano and Spilimbergo (2023a).

[4]To our knowledge the only paper to document an economic shift rightward following a crisis is by Carreri and Teso (2023). However, the authors study political elites, which may differ in important ways from the rest of the population.

years after the crisis ended. Hence, our findings suggest that, at least in the socialist context, policymaking can rehabilitate political trust, an important finding given the economic significance of political trust (Zak and Knack, 2001; Rothstein and Uslaner, 2005; Nunn and Wantchekon, 2011). At the same time, this finding also implies that the increased support for liberalization persisted even after the trust was restored, making trust an unlikely candidate to explain the entire shift in economic preferences. So, what else can explain the rightward shift in economic attitudes?

Given that the crisis had such a long-lasting effect, we bring evidence of a crucial mechanism. Following the crisis, starting from the end of the 1990s, many kibbutzim shifted from socialist economies to more capitalistic ones, substantially reducing income equality and introducing a market-based wage system (Abramitzky, 2008; Abramitzky and Lavy, 2014; Abramitzky, 2018). This labor market reform was implemented years after the debt relief arrangements. So, while the debt relief arrangement merely helped socialist kibbutzim repay their debts, the labor market reforms completely altered the economic structure of the kibbutzim by liberalizing their labor markets.

The shift away from full equality across kibbutzim was sharp and staggered, allowing us to identify the reform's causal impact (Abramitzky and Lavy, 2014). Specifically, we estimate both an event study and dynamic difference-in-differences specifications to capture the dynamics of the deliberations leading up to the reforms and the implementations following them. Exploiting our high-frequency data, we demonstrate that no pre-trends were leading up to the reform, validating our research design. We consider the fact that we can causally identify the political implications of exposure to liberalized markets in a natural setting as a unique contribution of our paper.[5]

We find that labor market liberalization led kibbutz members to move rightward in their voting in national elections and to embrace attitudes supporting liberalized market mechanisms. Support for equality and collectivist holdings of assets plummeted. Yet, strikingly, while following the reform members moved rightward in their attitudes, they also reinforced their support of communal mutual guarantee of social safety nets. In a way, kibbutzim shifted to an ideology of *Capitalism with Compassion*. They moved away from the notion of complete equality to the idea of community support for each individual.

Consistent with the shift in attitudes, kibbutzim members further transitioned away from voting for leftist parties following the reform. To show this, we use an RDD-inspired design, exploiting the fact that some kibbutzim reformed just before the elections and others just after the elections. We validate this design by bringing results of multiple balancing and placebo tests.

Still, while the reform explains part of the crisis' effect, it cannot explain all of it, as we show that members' attitudes and electoral behavior changed directly following the crisis, a decade before the reforms took place. We hypothesize that the crisis directly affected members by pushing them to view the socialist system as less effective. This may have been coupled with increased weight given to efficiency compared to equality following the crisis (Fisman et al., 2015). Consequently, members opted more for a liberalized labor

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[5]By studying the effect of the introduction of liberalized labor markets on attitudes, we complement a few recent studies that causally identify the impact of exposure to free financial markets on various outcomes (Jha and Shayo, 2019; Margalit and Shayo, 2021; Jha and Shayo, 2022). We find that experience with capitalism enforces its support (Roth and Wohlfart, 2018). This finding complements recent work claiming that experience with communism alters attitudes toward markets (Laudenbach et al., 2020). It also aligns with a study by (Enke, 2023), which suggests that exposure to markets had a pivotal impact on the development of morality. Our work is also consistent with the finding that securing property rights increases support for free markets, though we underscore that this is not identical to liberalizing labor markets (Di Tella et al., 2007). More broadly, our work aligns with important recent work that uncovers how institutional arrangements feed into determining preferences (Acemoglu and Robinson, 2021; Acemoglu et al., 2021).

market. Indeed, we demonstrate that following the liberalization of the labor markets, the perception of work ethics in the kibbutz increased substantially, consistent with the notion that people viewed liberalized markets as more efficient after the crisis. Hence, in addition to triggering a reduction in political trust and a positive experience with liberalized markets, the crisis directly contributed to a rightward shift in members' economic attitudes.

While the combination of democratic institutions with complete income equality is unique to kibbutzim, this peculiarity allows us to make a first step in studying the effect of economic crises in a centralized economy rather than a market economy. Our findings indicate that crises do not simply increase support for redistribution, as may be inferred after studying only capitalistic contexts. Rather, our findings are consistent with the idea that economic shocks may cause individuals to question their current economic system, be it a free market economy or a socialist economy. So, while crises increase demand for redistribution in capitalistic settings, under a socialist system, they generate doubt about centralized planning and raise support for liberalized markets.

Moreover, while the relative advantage of our setting is the high internal validity of the estimates, our findings might also have implications beyond the Israeli context. In particular, our work may add to the understanding of the shift from socialist regimes to the more market-oriented systems that took place at the end of the last century, when the fall of the communist regime in Eastern Europe and Russia resulted from an acute economic crisis (Blanchflower and Freeman, 1997; Ravallion and Lokshin, 2000; Fuchs-Schündeln and Schündeln, 2005; Alesina and Fuchs-Schündeln, 2007). We offer unique evidence of how an economic crisis in a socialist context may affect norms and values and raise the demand for introducing liberalized economic ideas (Abramitzky and Sin, 2014).

Though communism was rejected, there is a growing concern about increased income inequality in capitalist countries like the US and Israel. Influential works (Piketty and Saez, 2013; Piketty and Zucman, 2014; Saez and Zucman, 2016) uncovered the rise in income and wealth inequality in the US and worldwide. When analyzing the ideological transition of the kibbutzim, we conclude that although most kibbutzim members support the reform liberalizing labor markets, they still want to maintain the principle of mutual guarantee. It seems that when reflecting on how they want to live and build their society, most members want to live in neither a traditional socialist kibbutz nor a capitalist society. They prefer something in the middle – a market economy within a compassionate society with a comprehensive safety net. In fact, western societies have often strived for just that - to find an institutional structure to exploit markets' efficiency while balancing it with sufficient safety nets and shared prosperity (Johnson and Acemoglu, 2023). The road paved by the reformed kibbutzim provides broad lessons relevant to such societies.

Methodologically, our paper demonstrates the importance of exploring the mechanisms of persistence through a combination of a qualitative understanding of the historical context and a quantitative analysis. The qualitative historical knowledge of the kibbutzim informed our various design strategies. This led to a holistic view that enables us to track kibbutzim members' attitudes over 50 years through the crisis and its aftermath, including the ultimate transition away from socialism.

The paper is organized as follows. In part 2, we briefly summarize the historical background of our context. Part 3 brings details of the data we use. Part 4 makes the argument that the crisis' severity was orthogonal to the pre-crisis attitudes at the kibbutz level. In part 5, we show that the crisis shifted members rightward electorally. In part 6, we study the crisis's impact on attitudes independently of the reform. In part 7, we study the reform's effect on political attitudes and behavior. Part 8 concludes.

2 Historical Background

2.1 Kibbutzim

The Israeli kibbutzim are voluntary communities where members have lived with high-income equality for almost a century. Among the key features of the kibbutzim are that "all assets belong to the kibbutz and members have no private property [...] Each member of a kibbutz received an equal share of the total income regardless of her ability and effort [...]" (Abramitzky, 2008). Most kibbutzim were established in the 1930s and 1940s, and today, there are over 270 kibbutzim located all over Israel. The number of members in these kibbutzim amounted to 120,000 members, or 2.6% of the Jewish population in Israel (Ibid).

Through most of the 20th century, kibbutzim acted as semi-independent economic units, with most members working inside the kibbutz and the kibbutz employing little outside labor force. Still, every kibbutz was affiliated with movements that varied in their level of socialist ideology. These are the Takam (60% of kibbutzim), the Artzi (32%), and the religious (6%) movements. Our analysis focuses only on the first two (secular) groups. In 1999, the Takam and the Artzi movements were united, but their ideological predispositions persisted (Ben-Rafael and Shemer, 2020). Kibbutzim affiliated with Artzi hold the most socialist ideology and are considered more conservative in preserving kibbutz values (Abramitzky, 2008). Even in the more moderate Takam movement, there was a split into a sub-movement Meuchad, which was more socialist than the primary sub-movement Ichud.

From the 1970s, kibbutzim had been borrowing on a large scale, primarily to finance improved housing and other kibbutz facilities. At first, the loans were not linked to the cost-of-living index and were easy to repay in the context of escalating inflation. However, in 1985, the government introduced a dramatic anti-inflation program. The indexation of loans and the artificially high interest rates announced by the government in 1985 left many kibbutzim with high debt levels, resulting in an acute crisis. Crucially, depending on the kibbutzim's financial portfolio, some were severely hit and needed immediate assistance, some were more moderately hit, while others were unaffected.

In Figure A1, we plot the spatial distribution of the kibbutzim on Israel's map. Red dots represent kibbutzim more severely hurt by the crisis, and blue dots represent kibbutzim less hit. In this Figure we do not include kibbutzim for which we do not have an external estimate of their crisis' severity (see section 3.1). We note that two kibbutzim are geographical outliers positioned in the far south of Israel. Our main results regarding the effect of the crisis are robust to their exclusion from the sample.

To handle the kibbutzim crisis, the government, the banks, and the kibbutzim created the Kibbutz Arrangement Board in 1989. The government tried to prevent the collapse of the kibbutzim through a series of loan resettlement agreements. The first arrangement in 1988 failed, and the second, applied starting in 1997, was more successful. 214 kibbutzim gradually joined the latter arrangement, which allowed the banks and government generous loan forgiveness (Rosenthal and Eiges, 2014). We collected data on the dates that each kibbutz signed the debt relief agreement, and section 6.3 studies the effect of the debt settlement on attitudes.

In the 1990s, kibbutzim faced additional external pressures. These include the decline in the world prices of agricultural goods, of which kibbutzim produced a large amount, bad financial management, and a high-tech boom during the mid-1990s that made the outside option for jobs more attractive. In response to these challenges, many kibbutzim shifted away from equal sharing by introducing market-based wages starting at

the end of the 1990s. With information on the exact year when each kibbutz reformed and liberalized its labor market, section 7 studies the effect of the labor market liberalization on attitudes.

In reformed kibbutzim, members could keep their earnings for themselves, creating a link between members' productivity and earnings for the first time. Those who worked outside the kibbutz (approximately one-fourth of all members) kept their earned market wages instead of adding them to the kibbutz's pool. For members who worked inside, market wages were set to reflect wages of non-kibbutz workers with similar occupations, education, skills, and experience.

Shifting away from equal sharing was a major change in lifestyle for kibbutzim members, and it required an overwhelming majority to implement (Manor, 2004). Hence, this reform was preceded by intense debates among kibbutzim members as they discussed if and how to reform. We hand-collected data from the kibbutzim, which indicate that, on average, these deliberations lasted for two years until the reform was passed.

Important in our context is that despite the shift towards a more "capitalistic" model, the language used to describe reformed kibbutzim – "a safety net model" – suggests that even reformed kibbutzim still care for weak members in need. The language reveals that even though liberalizing kibbutzim let go of complete equality, mutual guarantee and social safety nets remained part of the core objective of the kibbutzim's mission. In reformed kibbutzim, a "kibbutz tax" was deducted from members' gross wages to guarantee a safety net for older members and very low-wage earners in the kibbutz.[6]

2.2 Israel's Political System

Since our primary outcome is the voting patterns in the Israeli Parliament, we briefly describe the Israeli national political system. The Israeli governance system is a parliamentary one. Citizens do not vote for the prime minister directly but instead vote for the Knesset (the Israeli Parliament). The voting for the Knesset takes place in a multi-party system. Over a dozen parties contend for the 120 legislative seats every election and usually, more than ten win representation. Thus, the Israeli parties' map changes between elections, as some parties are not reelected, and other new parties appear.

Since the mid-nineties, the Israeli Parliament has had three main political camps – the left, the center, and the right – where the two former camps are allied against the latter. The Kibbutz Movement is historically very strongly affiliated with the left camp. However, different kibbutzim support different parties within the left camp. The more ideological Artzi movement favored *Mapam*, a communist party that supported the Soviet Union's early days. Takam, the more moderate movement, supported the historic *Mapai* party that was founded in the 1930s and governed Israeli unchallenged until 1977. Its ideology was Zionist-socialist, though generally, it was a pragmatic movement. *Mapai* advocated for more restrained socialist policies and wished to create strong bonds with the US, unlike its more minor ally (*Mapam* party). The left-wing parties supported the kibbutzim, which formed the core of their electoral base. So, after the rightist Likud party came to power in 1977, the kibbutz lost priority status as the government prioritized the West Bank settlements.

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[6]Kibbutzim's shift away from equal sharing led the government to appoint a public committee, the Ben-Rafael Committee, which extended the official definition of a kibbutz to include both the "renewed kibbutz" and the "collective kibbutz." This committee legitimized the renewed kibbutzim model that still adhered to core kibbutz values and facilitated the transformation of kibbutzim that departed from the traditional collective model (Ben-Rafael and Topel, 2011).

These historic parties still have representation in contemporary Israeli politics. *Mapai* has turned into the Labor Party, and *Mapam*, jointly with the *Ratz* party, created the *Meretz* party in 1992. These two parties are the main leftist parties in Israel. Since 1992, they have consistently won parliamentary seats, and in many elections, they were the only leftist parties contending. However, their relative strength has weakened steadily. In 1992, they had 56 legislative seats, but in the final elections in 2020, they only won 6. Meanwhile, the two other political camps gained popularity. The center parties held no parliamentary seats in 1992 but gained 33 in the 2020 election. In comparison, the right camp grew more moderately, and their parliament members increased from 58 to 65 during the same period.

3   Data and Measurement

3.1   Measuring the Severity of the Crisis

We use two chief measures of the severity of the financial crisis in each kibbutz. The first measure, Economic Strength, was constructed by the Israeli government when it tried to assess the significance of the crisis across different kibbutzim in 1994. The government assigned each kibbutz to one of four groups: the first consisted of kibbutzim hurt most severely by the crisis, while the fourth consisted of kibbutzim not hurt at all. Groups 2 and 3 were intermediately hit. The second measure, Credit Rating, devised in 1995, also divided the kibbutzim into four groups, and it was based on the economic strength measure, the debt per member, the ability to repay debt as reflected by economic forecasts of the kibbutz Arrangement Board, type and diversification of industries, and the kibbutz's land value. These measures indicate how external experts documented the variation in the severity of the crisis across kibbutzim.

Producing a new way to measure the severity of the crisis, we also generate a survey-based measure. We elaborate on these surveys in section 3.3. To measure the intensity of the crisis using the survey, we use the answer to the question: "How would you define the situation of your kibbutz today from an economic perspective?". The respondents were asked to rate on a 5-point Likert scale ranging from 1 (Not Good at All) to 5 (Very Good). We use the average score given at each kibbutz from 1989 to 1996 – the peak years of the economic crisis and before most kibbutzim signed the debt restructuring agreements – as our measure of the degree of the financial crisis.[7] We use this measure in continuous discrete forms. The latter is calculated by splitting the sample to quarties.

As seen in Table A1, all three of our measures are highly correlated. This is consistent with kibbutzim members correctly internalizing the magnitude of the crisis in their locality. But, becaue the survey-based measure is threatened by endogeneity concerns, we largely use it to ensure our results are robust.

Table A2 offers descriptive statistics, stratifying our sample into groups according to the primary measures. The statistics are based on the 1983 Israeli Census, the last available census before the crisis. In most of our variables, the differences across groups are minor. The one variable that seems to vary significantly and monotonically between different groups is the number of people. The bigger the kibbutz was in 1983, the less it was hit by the crisis in 1985. This imbalance should not be a major concern since we include

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[7]When we calculate the explanatory variable for each observation sampled before 1996, we take the average economic rating given to the kibbutz excluding said observation. This procedure guarantees that the same individual never determines the dependent and independent variables in the same row. So, we will not have a mechanical correlation when checking the association between crisis intensity and political preferences, as described above.

kibbutz fixed effects in our benchmark specifications. When we do not, we add it as a control in robustness tests to ensure it does not drive the results.

3.2 Electoral Data

In our main analysis, we examine the effect of the crisis on the electoral voting of kibbutzim members. To this end, we use data published by the Central Elections Committee of Israel for the general public. For every term of the Knesset (the Israeli Parliament), the data includes a locality identifier and election poll identifiers, the number of eligible voters, and the number of votes cast for each political party.

We focus on elections between 1977 and 2019. We start in 1977 because, from this year, Israeli politics changed dramatically, with the right winning a majority for the first time. We stop at 2019, as taking later elections will coincide with the COVID-19 pandemic. In our robustness specifications, we drop elections conducted in 1988 and 1992. We do this because while these years are already after the crisis has started, our measures of the severity of the crisis are calculated for 1994-1996. So, it is unclear if these years should be included in the post-sample or not. We show that our results are not altered by excluding them.

In most cases, all eligible voters in an election poll are from a specific kibbutz, allowing us to measure how kibbutz members voted. In some cases, election polls combine people from a kibbutz or a few kibbutzim with people outside a kibbutz. As a result, we cannot distinguish kibbutz members' votes from non-kibbutz members' in these voting polls. So, we take only kibbutzim with a designated voting poll in every election in our sample. We also focus our analysis on kibbutzim from the Takam and Artzi movements, dropping religious kibbutzim because they were very few and generally did not experience the crisis. This limits our sample to 159 kibbutzim in the benchmark analysis.

As the parties' map constantly changes in Israel, we create a political variable that persists through time. We assign each party to one of three political camps – left, center, and right. Our categories are based on Shaul Shenhav's map of political parties (unpublished, updated by the author to the 2020 elections), and we cross-reference it to the parties' self-proclaimed political affiliation. Some parties disappeared from the sample in some elections (either because they dissolved or did not receive enough votes), while new parties emerged. So, while no party changed its political orientation, the parties composing each camp changed from election to election. Our outcome variables are the percentage of voting in each kibbutz for all parties that are part of each category out of all cast votes. We are also interested in voting turnout, defined as the proportion of voters among those eligible.

We show descriptive statistics of the voting behavior of kibbutzim, stratified to the groups and pre and post-crisis, in Table A3. We combine groups 1 and 2, as well as groups 3 and 4, for presentation purposes. From the descriptive evidence, our main results already emerge: pre-crisis voting behavior is similar between groups, while post-crisis, kibbutzim that were hit to a greater degree lean more to the right.

3.3 Survey Data

The survey we use was implemented in most years since 1989, except in 1990, 2006, 2008, and 2010. Until 1998, the survey was carried out by filling out paper questionnaires; since then, it has been done online. The sample includes about 200 (randomly chosen from the 268) kibbutzim annually and targets individuals randomly selected in each kibbutz. However, since it went online, the sample mainly contains people who

responded. Therefore, different kibbutzim were included each year, leading to a sample of 240 kibbutzim. Since the survey is anonymous, we cannot link individuals' responses over time. Therefore, the data is structured as repeated cross-sections.

We compare the means of demographic variables (age, gender, education, affiliation with one of the two kibbutz movements) of the sample to the means of all kibbutzim populations, as recorded by the Israeli Central Bureau of Statistics, and find that the sample is overall representative. This evidence is presented in the online appendix Table A4.

We use the survey data to construct outcome variables that would allow us to investigate the mechanisms through which the crisis influenced electoral voting. The surveys elicit kibbutz members' attitudes toward free markets and socialism and their perception of the kibbutz and its leadership. In the questions of interest, the respondents are asked to rate on a 5-point Likert scale ranging from 1 (strongly oppose/disagree) to 5 (strongly support/disagree) the extent to which they support/agree with a series of statements.

Our first interest is in questions eliciting support for a liberalized labor market. This set includes the following questions: whether a higher wage should compensate individuals who work more, whether the kibbutz should undergo a privatization process, and finally, whether the kibbutz should pay differential wages. The higher the number, the more the individual supports a liberalized market. We follow Goldin et al. (2006) to construct an index capturing the joint variation in answers to these questions. The summary measure is computed by taking an equal-weighted average of Z-scores of each relevant question. The Z-scores are calculated using the untreated observations' mean and standard deviations from the same survey year (using a year-specific control group for each question). This index allows us to compare the answers to different questions on a unified scale. A higher score in the index suggests higher support for liberalized markets.

We also construct an index to capture trust in leadership. To do that, we use two survey questions: "to what degree do you trust the economic leadership of the kibbutz?", and "to what degree do you trust the social leadership of the kibbutz?". We collapse them to a unified index following the same approach.

Additionally, we use questions regarding socialist ideology. These include questions regarding the support for economic equality and mutual guarantee (support of a broad economic safety net). They also include two questions that capture what we define as collectivism: support of having joint ownership of assets and having joint ownership of means of production. These two questions are also aggregated to a single index. The ideological cluster of questions is included in the surveys only starting from 2001.

Lastly, we make use of the fact that the survey includes some demographic details. In particular, it includes the date of arrival to the kibbutz. This allows us to consider only the people who were in the kibbutz during the crisis, offsetting concerns about migration. As we discuss later, we use the education variable to deal with the issue of emigration. Lastly, we use the age variable to perform heterogeneity tests.

3.4   Auxiliary Data

We gathered data on several additional variables of the kibbutzim. First, a threat to our identification of the effect of the crisis on voting is the establishment of community extensions (*Harchavot*). These are neighborhoods constructed in the kibbutzim and populated by non-kibbutzim members. If establishing a community extension is correlated with the crisis, this may bias our results. Notice this is a threat only

to the analysis of voting because, for the survey data, we can determine the respondents' arrival date. To ensure this does not confound our analysis, we hand-collected information about establishing community extensions in kibbutzim in our sample. In Figure A2, we show the number of kibbutzim with and without an extension for every year. Later, we show that our results also hold when controlling for the existence of an extension.

Second, as mentioned previously, a possible mechanism at work is the introduction of reforms that liberalized the labor market. We rely on information from the Institute for the Research of the Kibbutzim and Cooperative Idea to determine when each reform occurred at different kibbutzim. We plot the distribution of the timing of the reforms in Figure A3. The reforms were preceded by debates, which may have played a role in molding members' preferences. To account for them, we hand-collected information about the timing of the initiation of debates, whenever possible, from 50 kibbutzim in our sample. We discovered that overall intensive debates started, on average, two years before the implementation of the reform.

Third, we also gathered information about the year each kibbutz signed a debt relief agreement with the government. As we will show later, these debt relief arrangements had an important role in mitigating some of the adverse political effects of the crisis. In Figure A3, we plot the distribution of the signing year.

4   The Exogeneity of the Crisis

The economic crisis that hit kibbutzim was unexpected and followed the hyperinflation and stabilization program in Israel as a whole. Still, one can be concerned that kibbutz ideology was correlated with the crisis in a way that would bias our estimates. For example, less ideological kibbutzim (such as those affiliated with the Takam movement) might be more likely to suffer a more severe crisis and then be more likely to change attitudes. Alternatively, more ideological kibbutzim might be more likely to make bad economic choices and undergo crisis. To causally identify the political implications of the crisis, we need to establish that the intensity of the crisis was orthogonal to the pre-crisis attitudes. Our challenge is that we do not have survey data from the pre-crisis period, so we cannot directly check members' attitudes. To deal with this, we use alternative measures to proxy for pre-crisis attitudes.

First, we examine how kibbutzim, affected by the crisis to varying degrees, voted. We plot the means of two of our primary outcomes – support of the right and of the left – for each one of the different groups between 1977 and 1984. We do so for our main measures of the crisis: the Economic Strength and the Credit Rating. As seen in Figure A4, the baseline and the trends are highly similar across groups. All of the differences are much smaller than one standard deviation of the means.

To further reinforce this finding, we also look at voting by party before the crisis. We do this only for the left camp, as voting for other camps before the crisis was limited. We also restrict the sample to Zionist, that is, non-arab parties. This leaves us with the *Havoda* party and the *Ratz* party. In Table A5, we regress voting for each party on the economic measures during the pre-period, controlling for year-fixed effects. As can be seen, the results are perfectly balanced.

In Table A6, we perform additional tests. In column (1), we regress affiliation with Artzi on the economic measures. Affiliation with Artzi indicates the kibbutz is leaning more to the left, and this was determined long before the crisis. We find null results, again consistent with no difference in baseline attitudes.

We also exploit a unique episode to further examine the correlation between the pre-crisis ideological dispositions and the severity of the financial crisis. Within the Takam (less ideological movement) were two sub-ideological movements: Meuchad and Ichud. These sub-movememnts were established in the 1950s due to strife in some kibbutzim following Stalin's death. The Meuchad movement was more socialist and identified with Stalin's policies and the communist Soviet Union, while the Ichud movement was less ideological and rejected Stalin and his policies.

We begin by regressing, within the Takam movement, affiliation with Meuchad on the intensity of the crisis. Similar to the results concerning the affiliation with Artzi, we find null results. Further, the ideological strife in the 50s caused many kibbutzim to split into two or more kibbutzim. The ideological differences were so large and meaningful that kibbutzim members did not want to remain within of the same community anymore. In some kibbutzim where Ichud members were a majority, the more ideological individuals moved to Meuchad kibbutzim, and vice versa. Some kibbutzim even split, creating two kibbutzim with identical names but with an affiliation to a different ideological movement. We create a sample that consists only of kibbutzim that either experienced significant mobility of its members or that split because of ideological strife. We classify them as Ichud (less socialist) and Meuchad (more socialist) and regress the crisis severity measure on a dummy indicator of ideology affiliation. The underlying assumption is that because members originate from the same kibbutzim, they are similar across many dimensions so that we can capture the effect of ideology at the pre-crisis time on the severity of the crisis. We do this with and without fixed effects at the group level. As shown in Table A6, columns (3) and (4), the coefficients are not significantly different from zero. Moreover, they are of small magnitude, indicating no correlation between the pre-crisis attitudes in this sample and the economic situation in the 90s.

Table A6, column (5), presents the final test results, based on another unique characteristic of the kibbutzim that is connected with the strength of their ideology. Initially, in each kibbutz, all children stayed in a shared children's house from birth. They spent most of their time in this house, where they slept. Children spent only a few hours with their parents every afternoon. The idea was to ensure equality among children and endow them with socialist values. However, with time and as the socialist zeal decreased in kibbutzim, parents started demanding to move away from this arrangement and have their children sleep at home. Over time, more and more kibbutzim succumbed to the pressure posed by parents and abolished the common sleeping arrangement. Most kibbutzim adopted this change during the 70s and 80s, with almost all others completing the transition by 1991. We posit that the stronger the socialist ideology was in a kibbutz, the later it transformed into a familial sleeping arrangement. This is because there was less pressure from parents in these kibbutzim, and their leadership was less likely to succumb to such pressures. Under this hypothesis, we see a null effect when we regress the crisis severity on the year of the change in sleep arrangement, indicating a balance in ideology before the crisis.

If there is a correlation between pre-crisis attitudes and the crisis's intensity, it would have to be orthogonal to the correlations that we have examined thus far. We find it highly unlikely that such a correlation existed.

5   The Effect of the Crisis on Voting

To estimate the effect of the crisis on electoral outcomes, we use a simple Difference-in-Differences design. For a causal interpretation using this specification, it is sufficient to ensure similar pre-crisis trends across

different groups of kibbutzim. Figure A4 and Table A5 already show this. To reinforce this finding, in Figure 1, we plot the effect of the crisis on voting behavior by year. To do so, we take our two main measures and set the treatment to be the most severely hit kibbutzim (group 1) and the control to be the least hit kibbutzim (group 4). We regress the support for the left and the right on this dummy variable for each year seperately.
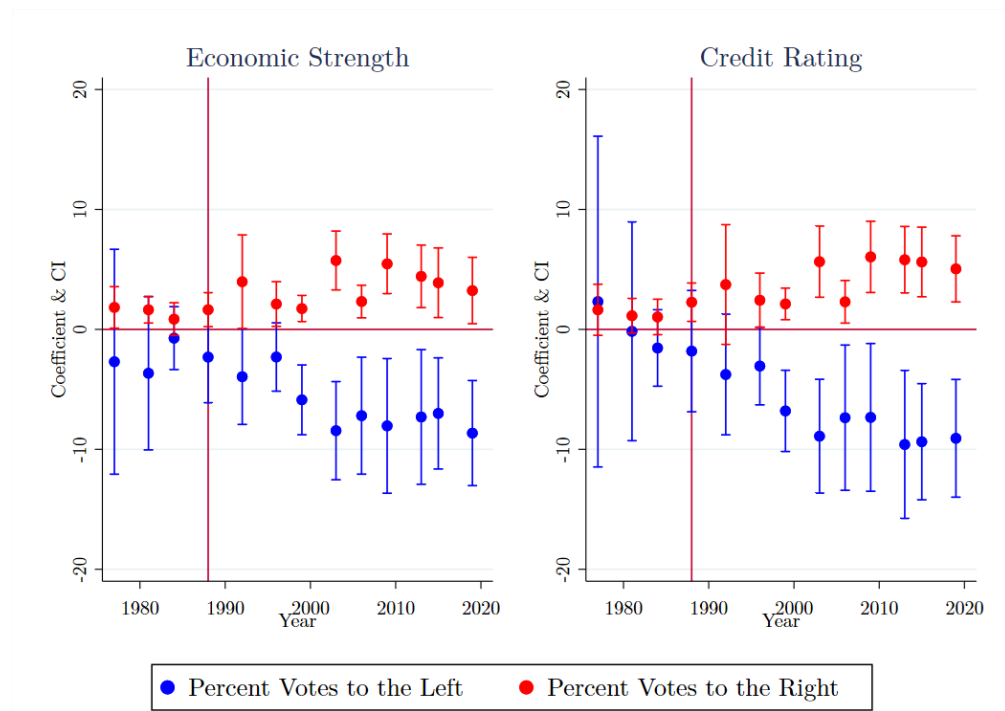

Figure 1: The Effect of The Crisis on Voting By the Two Measures

We get the percent of votes cast to the left and to the right in each kibbutz between the years 1977 and 2019. For each year separately, we regress these outcomes on the crisis' severity, as calculated by varying measures. In Panel A, we use the Economic Strength measure, and in Panel B, the Credit Rating measure. The treatment group is always kibbutzim severely hit (group 1), while the control group includes kibbutzim that were not hit (group 4). In blue, we depict the coefficients and 95% CIs for voting left, and in red, we depict the coefficients and CIs for voting right. The horizontal line exhibits the null, and the vertical line is positioned in 1988, the timing of the first elections after the crisis had begun.

Before the crisis, the coefficients are null and stable. Consistent with Table A3, this indicates no pre-trends or significant differences in the pre-period baseline. What is striking is that after the crisis, the effect accumulates over time, with the final years seeing the greatest effect of the crisis.

To validate our results, we repeat the procedure, this time defining treatment and control differently. In Figure A5, the treatment comprises groups 1 and 2 and the control group comprises groups 3 and 4. While this allows us to enlarge our sample, it comes at a cost of driving the estimated coefficients down, since groups 2 and 3 are more similar than groups 1 and 4. We also show results using the Survey Based measure as another way to verify our findings. In addition, we replicate the results dropping kibbutzim once an extension is established in them in Figure A6. All the tests are consistent with the benchmark results.

To estimate the pooled effect of the crisis, we regress

$$Y_{kt} = \kappa_k \cdot \text{Kibbutz}_k + \tau_t \cdot \text{Time}_t + \sum_{i=1}^{4} \beta_i \cdot \text{Crisis}_{ik} \cdot \text{Post}_t + \epsilon_{kt} \tag{1}$$

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Where Y_{kt} is a political outcome of interest at the kibbutz-year level. Kibbutz$_k$ is a fixed effect for the kibbutz k, and Time$_t$ is fixed effect for the year t. Crisis$_{ik}$ is an indicator that is 1 if the kibbutz is in group i according to the measure used and 0 otherwise. The omitted group is 4, the group for which the crisis hit the least severely. Post$_t$ is 1 if the year is after 1985 and 0 otherwise. The coefficients of interest are β_1, β_2, and β_3, which capture the effect of having a harder crisis in the post-crisis period.

Our main results are depicted in Table 1. We take each of our four outcome variables of interest and regress them on crisis severity using the specification outlined in equation (1). We always get a null result for voter turnout, indicating the crisis did not influence it. So, the crisis did not change the degree of political engagement in the kibbutzim.

	% Turnout	% Left	% Center	% Right	$\frac{Right+Center}{Left}$
Economic Strength					
Severe Crisis	0.543	-3.745	1.346	2.012	1.975
	(1.188)	(2.901)	(1.336)	(0.862)	(0.991)
Moderate Crisis	0.615	-1.458	0.0198	0.547	0.593
	(0.879)	(1.869)	(0.986)	(0.527)	(0.594)
Mild Crisis	1.001	-0.277	-0.727	-0.276	-0.257
	(1.071)	(2.055)	(1.284)	(0.571)	(0.644)
Observations	1,937	1,937	1,788	1,937	1,788
Credit Rating					
Severe Crisis	0.0752	-6.907	3.168	2.834	3.000
	(1.412)	(4.262)	(2.453)	(0.757)	(0.887)
Moderate Crisis	-0.661	-5.232	2.102	1.093	0.123
	(1.196)	(3.724)	(2.286)	(0.547)	(0.635)
Mild Crisis	-0.259	-4.835	2.208	0.512	0.655
	(1.243)	(3.741)	(2.322)	(0.584)	(0.682)
Observations	1,937	1,937	1,788	1,937	1,788

Table 1: The Crisis Shifted People from the Left to the Center and the Right

We get voter turnout and percent of votes cast to each political camp as outcomes. In the last column, we take the votes cast to the center and the right, divided by the number of votes cast to the left. Each observation is at the elections-kibbutz level, and the sample includes all elections from 1977 to 2019. We keep only kibbutzim that have appeared throughout the years. Using a Two-Way-Fixed-Effect Difference-in-Difference specification, we estimate the effect of the crisis on them. The specification is outlined in equation (1). The coefficients reported are of the degree of the severity of the crisis interacted with an indicator of the year after 1985. The omitted group is the group of kibbutzim that did not go through a crisis, according to every measure. In Panel A, we report estimates using the Economic Strength measure and in Panel B, we use the Credit Rating. We cluster at the kibbutz level.

In all specifications, the coefficients capturing the effect on the support of the left are negative and monotonically increasing. This indicates that the harder the crisis was, the fewer people voted to the left after it took place, though we underscore not all coefficients are statistically significant. On the other hand, the coefficients for the right are positive and decreasing, so the harder the crisis was, the more people voted to the right. The coefficients on the center are always positive, though statistically insignificant. This is consistent with people leaving the left also for the center. People who previously voted for the center possibly opted for the right following the crisis, explaining our weaker overall results on the center. We note that

there are fewer observations in the center column because, in 1992, there was no center party.

In the last column we represent the results when taking as an outcome the ratio of the votes to the center and the right to the votes to the left. This number enables us to capture the electoral shift away from the left more parsimoniously. The results indicate that most of our effect is driven by the kibbutzim that were hit the most.

We ensure our results hold when using the survey-based measure in Table A7. In Panel A and B, we use the full sample of kibbutzim for which we have survey information. In Panels C and D, we limit ourselves only to kibbutzim for which we have external measures. In Panels A and C, we enter the measure discretely, and in Panels B and D, we do so continuously. The results are even stronger when we use this measure, boosting our confidence in them.

In Table A8, we make an identical analysis, omitting the years 1988 and 1992 in the post-period. We do this because they occurred after the crisis started but before its severity was assessed. The results are similar to our benchmark specification and indicate that our results are not sensitive to this decision.

In Table A9, we keep kibbutzim in our sample only if no extension community was ever built in them. In Table A10, we also include kibbutzim that established such communities, but we omit them from our sample once it happens. It is important to take these steps to ensure the changes in voting are not driven by immigration patterns in the kibbutzim. The results remain similar.

Overall, we conclude that the crisis shifted members from voting to the left to voting to the right. Immigration patterns do not drive these results. Further, this effect was long-lasting and incremental, rising over time.

This finding is striking given that kibbutzim members tightly identified themselves with the left throughout their entire history. Their leaders were the left leaders, their institutions were intertwined with the leftist parties, and their ideology was socialist. Only a major event could make members systematically turn away from such bonds. Yet, it is exactly what the crisis was – an impactful episode that could be a belief-twisting event.[8]

One concern about our interpretation of the results is that left and right parties in Israel differ not only in their economic ideology, but in additional dimensions as well. It is unlikely that the economic crisis caused kibbutz members to increase support for right-wing parties because they expected more sympathy from the right. If anything, the left-wing parties were more sympathetic to kibbutzim and more likely to bail them. So it is implausible that members of the kibbutzim who were hit more by the crisis would increase support to the right-wing parties due to narrow economic self-interest. Still, the left and right-wing parties diverge in their attitudes toward the Israeli-Palestinian conflict, so on the face of it, it could be that kibbutzim members changed their attitudes in this dimension. To examine this, we use a survey question eliciting attitudes towards the conflict that was asked in the years 2002-2005. The question asks how likely it is the Israelis and Palestinians will be able to reach an agreement. The right and the left are very starkly divided on this issue, with the left much more optimistic than the right. As shown in Table A12, there is a null correlation between the intensity of the crisis and the answer to this question, indicating that the crisis did

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[8]As a quote that Abramitzky (2018) brings puts it: "After the debt crisis, suddenly, kibbutz members, who had regarded themselves as partners in a more or less flourishing enterprise, discovered that in reality they had nothing. They had no old age pension, no social security, no house, no property of any kind, no rights of bequest—and in most cases not very much to bequeath. Kibbutzniks [kibbutzim members], who had felt themselves to be the most secure individuals on the planet, instead found themselves abandoned, naked, and buffeted by a savage storm. The trauma was extreme; the loss of confidence, crippling."

not alter beliefs about the conflict.

6   Pre-Reform Mechanisms

Through which mechanism did the crisis influence the voting behavior of kibbutzim members, even 23 years after it ended? One possibility is that the effect persisted through the labor market reform that eventually took place in kibbutzim. One possibility is that by encouraging kibbutzim to liberalize their labor markets, the crisis set each kibbutz on a different economic path, influencing political behavior. This might explain why the effect is driven mostly by later years. To examine this, in Table A11, we add to our regression specifications an indicator equal to 1 if the kibbutz has already implemented a reform and 0 otherwise. The coefficient is negative for the left and positive for the center and right camps. However, it is small and not statistically significant for the latter. Though not well identified, this is consistent with people changing their voting post-reform from the left to the center and possibly the right. However, even after controlling for the effect of the reform, the coefficients of the crisis are consistent with the benchmark specification. This suggests that the crisis affected kibbutzim members' electoral behavior even independently of the reform.

In this part, we examine the channels through which the crisis altered electoral behavior independent of the reform, while the next section suggests that the labor market reforms had an additional effect. To this end, in this section, we use the survey data for a cross-sectional analysis. We underscore that our results have a causal interpretation since, in Section 4, we found evidence that pre-crisis attitudes were balanced.

6.1   Economic Attitudes and Trust

The economic crisis may have triggered a shift to the right through two different mechanisms, which we can measure with the survey data. First, it may have altered people's economic attitudes. Second, it may have caused them to lose trust in the left's leadership. To investigate these two competing mechanisms, Figure 2 divides the kibbutzim into four groups according to each measure. Next, we separately calculate the mean of each group's free labor market index. As can be seen, for all three measures, the support for liberalized labor markets increased monotonically with the severity of the crisis.[9] We also performed the same exercise to study the effect of the financial crisis on trust in leadership. We separately calculate each group's mean of an index of trust in leadership. We find that the more severe the crisis, the lower the trust in leadership.[10] So, this figure is consistent with both mechanisms being at play. The crisis increased support for liberalized labor markets and decreased support in the kibbutz leadership that was associated with socialism.

Yet, the raw averages give only suggestive evidence. Table A13 shows the formal results when we regress the indices on the economic measures. In this table, we control for individual, time, and kibbutz level

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[9]this is consistent with what historical analysis reveals about the sentiment spreading in the kibbutzim following the crisis. As one kibbutz member put it in 1997: "We are witnessing the erosion of the kibbutz work ethic and the collapse of the labor system, both of which are taking a toll on kibbutz life. The kibbutz work ethos was as integral to kibbutz ideology as the ethos of cooperation and equality. Lacking the work ethos, the kibbutz community cannot survive." (Mort and Brenner, 2003; Abramitzky, 2018)

[10]"For many years, members had trusted that their kibbutz would take care of them, and kibbutzim had trusted members to work hard even though this was not legally enforced. The financial crisis and corresponding reforms may have inadvertently and irreparably damaged this trust. Kibbutz members became more calculated and suspicious. One member of Kibbutz Gesher Haziv told Mort and Brenner (2003, p. 74): "You felt secure. It wasn't a true life, but people met together in the dining room and elsewhere. Now, it's broken. No one has time. I don't have time myself. I don't feel secure now. I trust my family, not the kibbutz." (Abramitzky, 2018)

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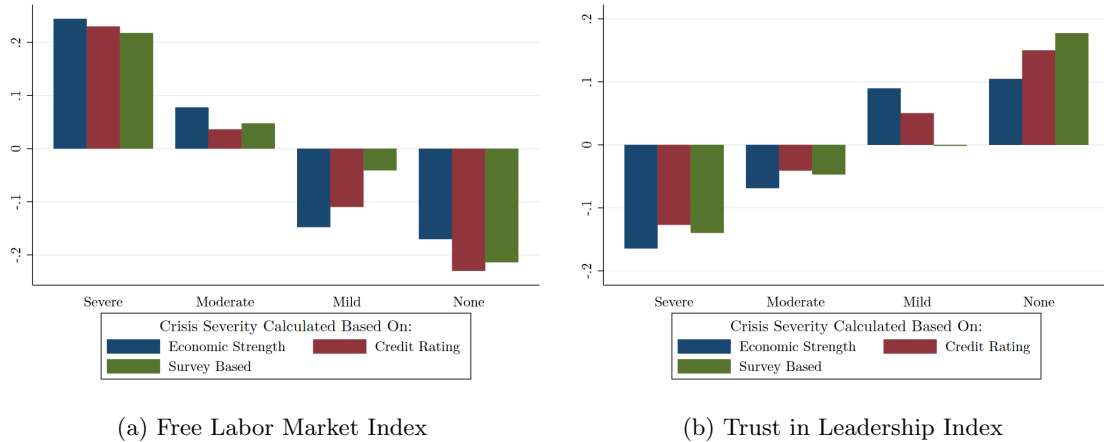(a) Free Labor Market Index   (b) Trust in Leadership Index

Figure 2: The Crisis Increased Support for Free Labor Markets and Decreased Trust in Leadership

We stratify the sample into 4 groups according to all three discrete crisis measures. For each group, we calculate the average labor index and trust index. In the first panel, we plot the free labor market index, and in the send panel, we plot the trust in leadership index. The blue columns depict the results according to the Economic Strength measure, the red columns represent the Credit Rating measure, and the green columns show the results for the Survey Based measure.

controls, including affiliation with the Artzi movement. We also restrict the sample only to individuals who were in the kibbutzim during the crisis and cluster standard errors at the kibbutz level. This demonstrates that the econometric specifications are consistent with the raw results.

In Figure 3, we perform the same excercise, splitting our sample at the median year to short term (until 2001) and long term (from 2002). The differences in trust between groups disappear in the long term, while ideological differences persist. So, while members who experienced the crisis remained more right-wing in the long term, their trust in their leadership was rehabilitated after a few years. In comparison, we saw that the crisis's electoral effect was most pronounced in the long term. Taken together, these findings are at odds with the conjecture that the decrease in trust triggered the electoral movement.

We continue to examine the differences in the short and long term formally. As there are solid theoretical grounds to believe these differences might vary with age, we stratify our sample into different sub-samples according to how old each individual was during the crisis. The age groups are 0-14, 15-29, 30-44, 45-59, and 60+. A subject is assigned to a group when his age is in the relevant range during the crisis (1987-1996). So, for example, the first group (0-14) contains all individuals born between 1973 and 1996. Note that this means that age groups are not mutually exclusive. The age groups are the relevant ages between 1987 and 1996 because this is the peak period of the financial crisis. We also test robustness to defining age groups only according to the individual's age in 1991, ensuring groups are mutually exclusive. The results remain similar.

We also further stratify the sample into short- and long-term sub-samples, as described above. For each age and long- or short-term sub-sample, we estimate the effect of the crisis on the labor and trust indices separately. In Figure 4, we show the estimates based on the Economic Strength measure. The treatment group is the group most hit by the crisis (group 1) and the control is comprised of the kibbutzim that were not hit by the crisis (group 4). In Figure A7, we present robustness results when adding group 2 to the treatment and group 3 to the control.

(a) Labor Index by Economic Strength   (b) Trust Index by Economic Strength

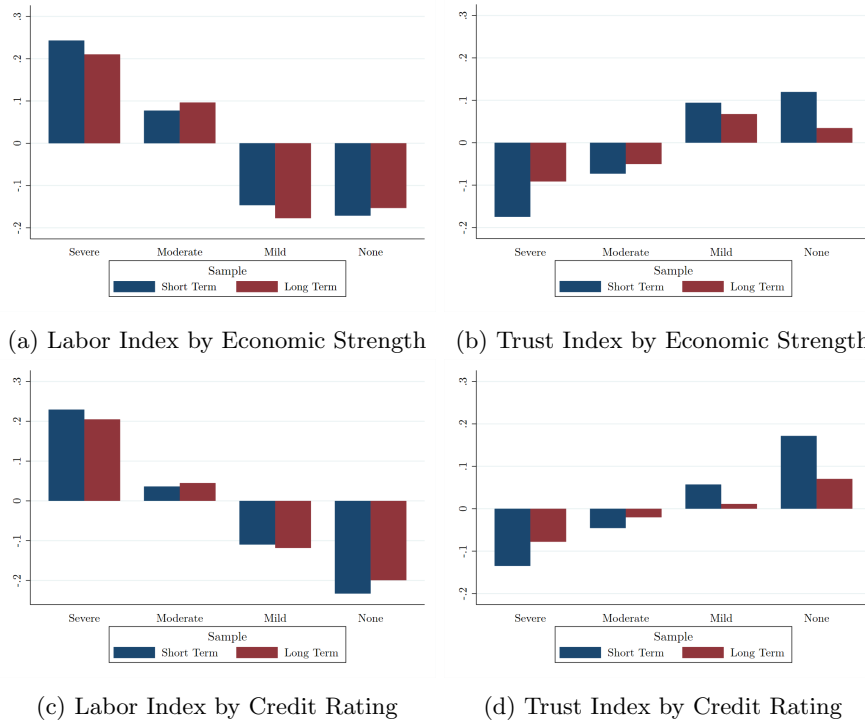(c) Labor Index by Credit Rating   (d) Trust Index by Credit Rating

Figure 3: Support in Free Markets Persists, But Changes in Trust are Temporary

In the two upper Panels, we stratify the sample into 4 groups according to the Economic Strength measure. For each group, we calculate the average free labor market index and trust in leadership index in the years before and after 2001. In the two lower Panels, we do the same according to the redit Rating measure. On the left, we plot the free labor market index, and on the right, the trust in leadership index. The blue columns depict the index's average in the short term, and the red columns represent the average in the long term.

We present each age group's short-term (green) and long-term (red) coefficients. We do so for the free labor market index in Panel A and for the trust in leadership index in Panel B. We add 95% CIs. We control for individual level-controls and for affiliation with the Artzi movment while clustering at the kibbutz level. Consistent with evidence in the literature, the results for the age group 0-14 are null in both the short term and the long term. This suggests that the crisis did not affect young children. We see in Panel A that in all other age groups, the short-term coefficient is positive and significant. However, as for the long-term effect, the effect seems to persist mostly for people aged 15-29, aligning with the impressionable years hypothesis. In addition, there is observable persistence in the age group of 30-44. However, for members who were older than 45 during the crisis, the persistence is null. As for trust, the effect is positive in the short term for all the groups and null in the long term in all groups.

To summarize, the crisis moved people rightward in their economic perceptions and decreased their trust in leadership. The former effect persisted in the long run, chiefly in the young, while the second effect was transitory. Hence, it is likely that the persistent change in political behavior is at least partly rooted in the ideological transformation. In the last section of this chapter, we explain why the effect on trust eroded over time.
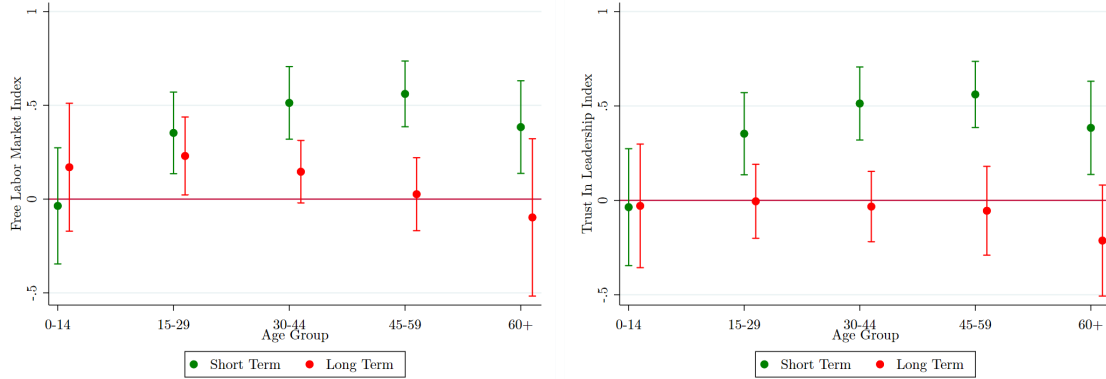
(a) Free Labor Markets Index　　　　(b) Trust in Leadership Index

Figure 4: The Crisis's Effect on Economic Attitudes Persists More in Younger Cohorts

This figure plots the coefficients when regressing the indices on the Economic Strength measure. The treatment group is kibbutzim in the first group (severely hit), and the control group is the fourth group (kibbutzim not hit at all). We also show the respective 95% CIs. The main specification is estimated separately for different age group and within each age group separately for the sample years 1991-2002 (green) and 2003-2018 (Red). Panel A presents the coefficients from regressions in which the free labor market index is the explained variable, while in Panel B, the explained variable is the trust in leadership index. We included individual-level controls, kibbutz-level controls, and year Fixed Effects. We keep only individuals that lived in the kibbutz during the crisis. We cluster at the kibbutz level.

6.2　Robustness

We conduct several analyses to check that our estimates are robust to different specifications. The labor index is the average score of three questions. However, not all questions appeared in the survey in all years. This may raise concerns that the varying composition of the labor index might bias our results. Table A14 reports our results for each question composing the index separately. Our results hold for all questions.

In another robustness check, we control for the kibbutz population in 1983, which we found to be imbalanced. These results are shown in the online appendix Table A15 columns (1)-(2). Adding these controls does not change the results.

In addition, we run the benchmark specifications while restricting the sample not to include observations during the crisis. In practice, we drop the observations from the 1989-1996 surveys. We present the results based on this sample in the online appendix Table A15, columns (3)-(4). We do this as our measures were calculated up until in 1994-1996, so we want to take only observations after them.

Another potential concern might be that the financial crisis caused a demographic shift in kibbutzim. Our setting enables us to account for any immigration patterns to the kibbutzim, as we focus only on individuals that were in the kibbutz during the crisis. However, our specifications might be affected by emigration patterns from the kibbutzim. To examine whether this is a concern, we rely on Abramitzky (2008)'s result, who finds that less educated individuals are less likely to leave the kibbutz. So, we keep in our sample only respondents with less than a BA degree. This sample is less susceptible to demographic changes. We report the results in Table A15, columns (5)-(6). Results are highly similar to the results obtained from the full sample. Hence, it does not seem that emigration patterns are affecting our results.

6.3 The Debt Settlement

In 1989, the kibbutzim signed a debt relief agreement. However, this agreement did not succeed in relieving the kibbutzim's debts. So, between 1997 and 2012, the kibbutzim hit by the crisis gradually reached an additional and more effective settlement with the banks and the government. This arrangement was more successful and it paved the road for recovery for the kibbutzim.

Most of the kibbutzim signed the agreement as soon as possible (in 1997). Yet, the timing of the signing varied across kibbutzim, and some joined the arrangement later. This happened for various reasons, one being the severity of the crisis. Indeed, when we regress the timing of the signing on the Economic Strength measure, we find that the third and the fourth groups signed the agreement on average 2-12 years after the first group.

This leads to a concern that the kibbutzim that signed the agreement later were on a different trend than the kibbutzim that signed earlier, hampering our ability to identify the causal effects of the debt arrangement. We conduct an event study to test for that directly, taking the labor and trust indices as outcomes. This means we run the following specification:

$$Y_{kt} = \kappa_k \cdot \text{Kibbutz}_k + \tau_t \cdot \text{Time}_t + \sum_{j=-6}^{j=6} \pi_j \cdot \text{Period}_{itj} + \epsilon_{kt} \tag{2}$$

Where the only new variable is Period_{itj}. This is just a series of dummy variables equal to 1 if the year minus the debt settlement year is equal to j, and 0 otherwise. We take six periods from either side because the first signing year was 1997, and we have annual data from 1991.

As can be seen in Figure 5, pre-trends are completely balanced despite our concerns. Following the debt arrangement, trust increases quickly and significantly, which explains why differences in this outcome disappear in the long term. In contrast, the debt has no effect on the labor index.

Recently, some problems have been raised regarding the Two-Way-Fixed-Effect specification we have outlined in Equation 2, in a setting where the treatment is staggered (Borusyak et al., 2021; Callaway and Sant'Anna, 2021; Roth et al., 2023). To address these concerns, we replicate our Event Study estimation using the method developed by Borusyak et al. (2021). The results are shown in Figure A8.

Together, the results reported in Figures 5 and A8 indicate that while the effect on trust is robust, large, and significant, the labor index results indicate a null effect. This suggests that the debt arrangement did not substantially alter economic attitudes. So, while an effective policy by the kibbutzim's leaders restored trust in them, it did not turn members back to advocating for socialism. Sound policy can rehabilitate public trust but not make people converge to their prior views. This finding also explains why the effects on long-term trust and labor attitudes diverge. As the debt did not restore socialist economic attitudes, differences caused by the crisis persisted in the long term.

To validate our results further, we perform an analysis in which we keep only kibbutzim in the first and second groups of the economic measures. We hypothesize that since all the kibbutzim in the first two groups were hit quite severely by the crisis, they would all have endeavored to sign the agreement quickly. If so, then the variation in signing timing will not result from the severity of the crisis but rather from the ability of the kibbutzim to settle with the banks and the government quickly enough. Indeed, in the two

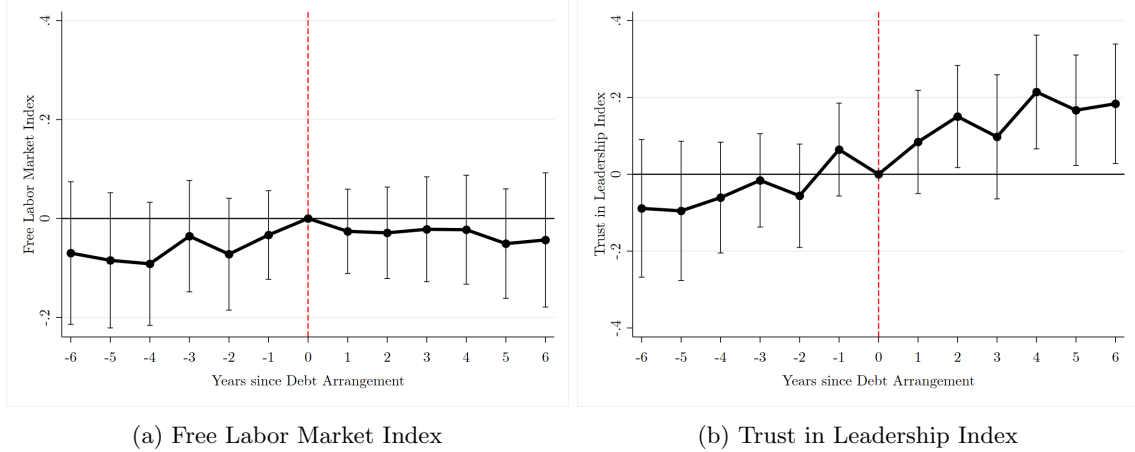(a) Free Labor Market Index   (b) Trust in Leadership Index

Figure 5: The Debt Arrangement Restored Trust But Did Not Reverse Economic Attitudes

We take the labor (left panel) and trust (right panel) indices as outcomes. We estimate the effect of the debt arrangement on them using a Two-Way-Fixed-Effect Event Study specification, as shown in Equation 2. The dashed red line represents the time of the signing of the arrangement. We focus on a window of 6 years before and after the signing. We bring each period's coefficient and 95% CI. We cluster and the kibbutz level. Our sample includes only people who were in the kibbutz during the crisis and are older than 20.

chief measures, we find a null difference between the timing of the signing agreement between the groups, indicating that the timing of signing is not endogenous within this group. Hence, we can perform a simple Difference-in-Differences specification to causally identify the debt agreement's effect.

Formally, we regress:

$$Y_{kt} = \kappa_k \cdot \text{Kibbutz}_k + \tau_t \cdot \text{Time}_t + \gamma \cdot \text{Signed}_{kt} + \epsilon_{kt} \tag{3}$$

Where the only new variable is Signed_{kt}, an indicator if the kibbutz k signed a debt arrangement before time t. As can be seen in Table 2, the results from this specification align with the results from the Event Studies.

Severity of the crisis	**Free Labor Market Index**		**Trust in Leadership Index**	
measured by:	Two-Way-Fixed-Effect	Borusyak et al.	Two-Way-Fixed-Effect	Borusyak et al.
Economic Strength				
Debt Arrangement	-0.030	-0.031	0.147	0.123
	(0.047)	(0.059)	(0.058)	(0.063)
Observations	8,928	8,892	7,819	7,786
Credit Rating				
Debt Arrangement	-0.031	-0.024	0.148	0.113
	(0.046)	(0.058)	(0.054)	(0.057)
Observations	9,547	9,511	8,375	8,342

Table 2: The Effect of the Debt Arrangement Within Severely Hit Kibbutzim

We include in our sample only kibbutzim that are in the first or second groups; that is the kibbutzim that were hit the hardest. In Panel A, we do it according to the Economic Strength measure and in Panel B, according to the Credit Rating measure. Within this sample, we estimate the effect of the debt arrangement on the labor and trust indices. In the columns (1) and (3), we do so using Two-Way-Fixed-Effect specification, as presented in Equation 3. In columns (2) and (4) we use the approach of Borusyak et al. (2021). We cluster at the kibbutz level and include individual-level controls.

7 The Labor Market Reforms as a Mechanism

As mentioned above, another mechanism through which the crisis may have influenced voting patterns is by encouraging labor market liberalization. Abramitzky (2008) shows that more severey hit kibbutzim tended to liberalize to a greater extent. If the experience with free labor markets impacts attitudes, which translates to a shift in political behavior, this establishes a causal chain from the crisis to electoral voting. Moreover, as liberalization occurred only a few years after the crisis ended, this channel might explain why the effect accumulated over time. We plot the number of kibbutzim that reformed each year in Figure A3. 32 kibbutzim never reformed.

7.1 The Effect of the Reform on Attitudes

We begin by estimating the effect of the reform on attitudes. The challenge in this analysis is to show that there were no pre-trends. As the timing of the reform may have depended on the intensity of the crisis, kibbutzim may have been on different time trends before joining the arrangement. We face an additional challenge: kibbutzim started discussing the reform on average two years before implementing it. During these deliberations, members engaged in persuasion efforts to convince members to vote for the reform. These deliberations may have impacted our outcomes of interest even before the implementation of the reform took place.

To deal with both challenges, we exploit our high frequency data to use, again, an Event Study specification. This approach allows us both to check directly if there are pre-trends and to disentangle the effect of the deliberation compared to that of the implementation of the labor market reform. We use the same regression as in Equation 2, only that the periods now relate to the timing of when a kibbutz implemented the labor market reform.

More elaborately, the baseline, normalized to 0, is the year preceding the start of the deliberation ($t = -1$). We focus on the five years before the beginning of the deliberation ($-5 \leq t \leq -1$), the two years of the deliberation ($t = 0$, $t = 1$), the year of the implementation ($t = 2$) and the five subsequent years ($3 \leq t \leq 7$). Dashed red lines depict the beginning of the deliberation and the implementation. Formally, we use exactly the same specification as when studying the debt arrangement, only changing the horizons, and considering periods compared to the reform, rather than the debt arrangement:

$$Y_{kt} = \kappa_k \cdot \text{Kibbutz}_k + \tau_t \cdot \text{Time}_t + \sum_{j=-5}^{j=7} \xi_j \cdot \text{Period}_{itj} + \epsilon_{kt} \tag{4}$$

In this section, we use data from the 2001-2011 surveys, which includes all of the questions of interest. Surveys questionnaires from earlier years do no include the ideology-related questions. This sample also moves us away from the end of the crisis, thus avoiding confounding pre-reform trends related to the crisis and debt settlement. We do not include survey data beyond 2011 because very few kibbutzim reformed afterward.

As seen in Figure 6, there is no pre-trend in attitudes supporting liberalization before the deliberation about the reform started. This finding alleviates the concern of reverse causality, namely, that the support for reform rose, leading to the start of the deliberation period. Instead, the timeline of events was that

kibbutz members' engagement in debate and persuasion on the reform led to greater support for it. This increase in support persists throughout the deliberation and implementation, with the peak reaching a 0.6 standard deviation increase above the benchmark. The conclusion is that liberalizing the labor market, which exposed kibbutz members to the free labor market system, further changed attitudes. However, we cannot fully disentangle the deliberation's effect from the implementation's effect on this outcome.

(a) Free Labor Market Index   (b) Trust in Leadership Index

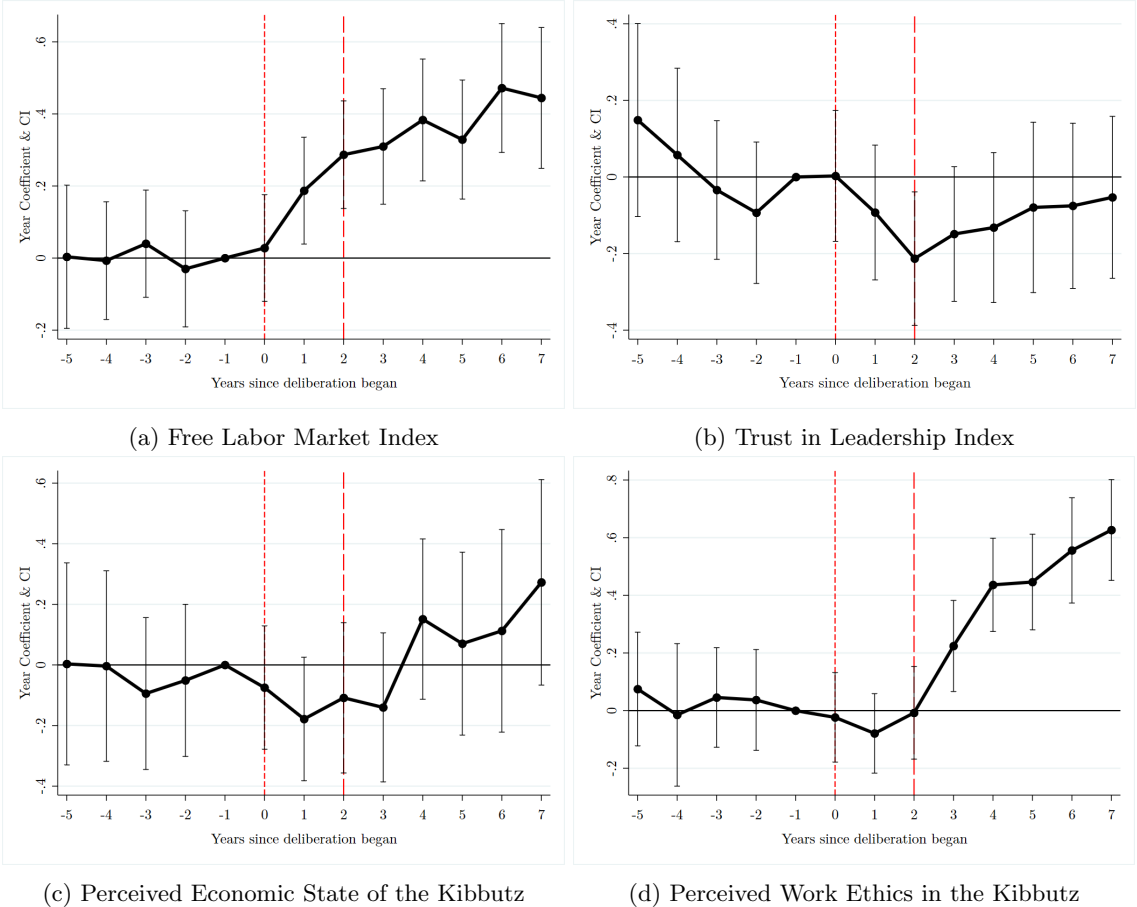(c) Perceived Economic State of the Kibbutz   (d) Perceived Work Ethics in the Kibbutz

Figure 6: The Reform Pushed People's Economic Attitudes Rightward

We estimate the effect of the deliberations and the implementation of reforms on various outcomes using a Two-Way-Fixed-Effect Event Study specification, as shown in Equation 4. Time $t = 0$ depicts the start of the deliberation, and implementation happens at $t = 2$. We include individual-level controls and cluster and the kibbutz level. We bring the 95% CIs. In Panel A, the outcome is the labor index; in Panel B, it is the trust index; In Panel C, it is the perceived economic state of the kibbutz; and in Panel D, the outcome is the perceived work ethics in the kibbutz. We focus only on the sample spanning 2001-2011.

In Figure 6, we plot the coefficients for trust, which show that trust in the leadership slightly decreased during the deliberations. This is perhaps an expected result given the challenges the kibbutz faced then and the fierce disagreements. However, there was a break in trend following the reform, and trust in leadership stagnated.

We inspect what might drive this attitude change in the bottom panels of Figure 6. We examine the assessment by kibbutz members of the kibbutz's economic state and the perceived work ethics. We find that both improved following the implementation of the reform. The rise in the perceived work ethics is especially striking. This sharp and steep rise may explain the underlying move away from socialism following the crisis.

We conjecture that the socialist system was blamed for the crisis, as it did not set the right incentives for kibbutzim members, and free riding was pervasive.[11] Once the kibbutz shifted away from equal sharing (socialism) to wages set at the value of the marginal product (capitalism), perceived work ethics improved, marginalizing the critique of the kibbutzim economy.[12]

In Figure 7, we look more closely at the structure of the ideology of the kibbutzim members following the reform. We examine kibbutzim members' support for equality, mutual guarantee, and collectivism. The latter captures the degree to which kibbutzim members support the joint holding of assets. Support for collectivism and equality fell after the implementation, demonstrating a further shift from socialism following the crisis.[13] However, there was an increase in the support of mutual guarantee. So, following the reform, the tide towards laissez-faire included a wave of increased support for a safety net. This development can be coined as *Capitalism with Compassion*.

In Figure A9, we replicate our results, omitting kibbutzim that reformed before 2001. This ensures kibbutzim contribute pre-treatment observations. In Figure A10 we replicate our results using the approach of Borusyak et al. (2021). Importantly, also when applying the correction we still do not observe any pre-trends in the event studies. Moreover, The results are identical qualitatively, though at times, we lose some accuracy. We underscore that the design of Borusyak et al. (2021) is conservative in terms of inference, and that the specification we use is demanding as we include multiple controls and stratify the effect by period. Still, the pooled DiD results we soon show, which are less demanding in terms of inference, are statistically significant and similiar to the TWFE estimations.

In Table 3, we present the results from the Difference-in-Differences specification on all seven outcomes of interest. In Panel A we use the Two-Ways-Fixed-Effects design with a treatment both for deliberation and for implementation. Formally:

$$Y_{kt} = \kappa_k \cdot \text{Kibbutz}_k + \tau_t \cdot \text{Time}_t + \delta \cdot \text{Deliberation}_{kt} + \phi \cdot \text{Implementation}_{kt} + \epsilon_{kt} \tag{5}$$

For each outcome, we present the coefficient for being during the deliberation, which is an indicator if the observation is at $t = 0$ or $t = 1$. We also present the coefficient for the implementation, that is an indicator if the observation is at $t \geq 2$. The baseline for both coefficients is the pre-deliberation period ($t \leq -1$). Hence, the Implementation coefficients should be interpreted as the difference between the average outcome after the implementation of the reform and the average outcome before the deliberation, after parsing out the controls. Focusing on the implementation effect: We obtain similar patterns to our event studies in almost all specifications. In Panel B, we drop kibbutzim that reformed before 2001, preserving the TWFE specification. In Panel C, we replicate the same approach using the specification of Borusyak et al. (2021). Our results are highly stable across the different panels, and the coefficients are similar in magnitude. For example, the estimated effect on the labor index in column 1 is identical in all three models.

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[11]This aligns with the historical assessment of Abramitzky (2018). He writes: "Kibbutzim also discovered the economic principle of comparative advantage: 'A lawyer who was also a skilled cowman could be replaced relatively cheaply, and his monetary value to the kibbutz was much greater as a lawyer than as an agricultural worker' (Near 1997, p. 353). Kibbutzim began encouraging members to seek high-paying jobs outside the kibbutz and to establish small businesses within the kibbutz".

[12]As a kibbutz member put it after his kibbutz was reformed: "Was Hasolelim [his kibbutz] more of a kibbutz when each member thought he was doing all the work and the other members were living on his back? I ask myself: Was it more of a kibbutz when we were forced to stop calling volunteer work days because no one turned up?" (Gavron, 2000).

[13]Abramitzky shares an anecdote capturing exactly that: "A member of Kibbutz Gesher Haziv told Mort and Brenner (2003, p. 76): "You mean Pete [the factory manager] makes a lot more money than we do? Okay. That doesn't bother me. It bothers a lot of people, but not me. So some are adding on to their houses and others aren't. Okay."

(a) Support of Equality

(b) Support of Collectivism
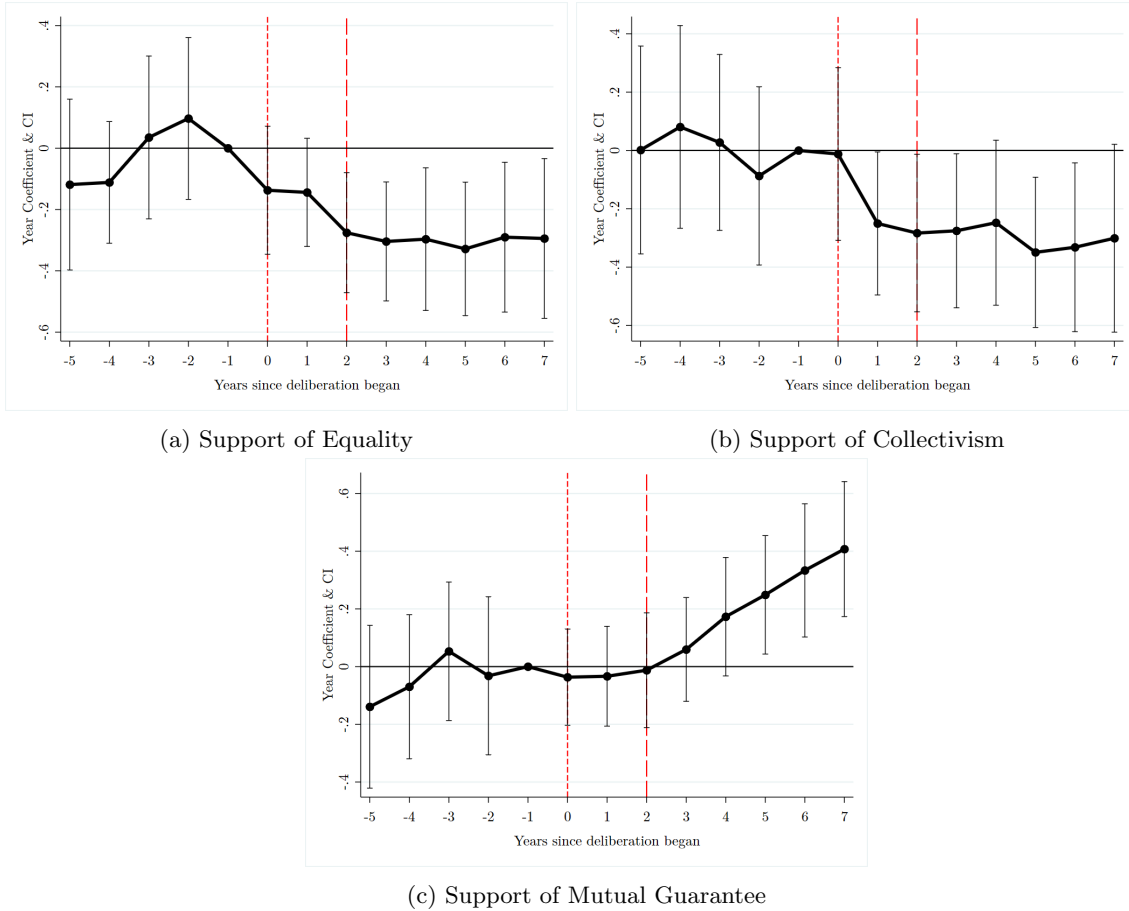
(c) Support of Mutual Guarantee

Figure 7: The Reform Triggered a Shift from Equality and Collectivism to Mutual Guarantee

We estimate the effect of the deliberations and the implementation of reforms on various outcomes using a Two-Way-Fixed-Effect Event Study specification, as shown in Equation 4. Time $t = 0$ depicts the start of the deliberation and implementation happens at $t = 2$. We include individual-level controls and cluster and the kibbutz level. We bring the 95% CIs. In Panel A, the outcome is the support of equality; in Panel B, it is support of collectivism; and In Panel C, it is the support of mutual guarantee. We focus only on the sample spanning 2001-2011.

7.2 The Effect of the Reform on Voting

So far, we have demonstrated that the reform influenced ideological attitudes in the kibbutzim. Did this translate into a shift in electoral outcomes? In Table A16, we provide some descriptive statistics consistent with reformed kibbutzim shifting from the left to the center and the right. We compare the support of each camp in reformed and unreformed kibbutzim each year.

However, to assess the reform's impact on voting patterns more formally, we exploit the fact that Kibbutzim reformed in different years. To isolate the reform's effect on electoral patterns, our analysis focuses only on the 120 kibbutzim that reformed precisely a year before or after one of the elections between 1996 and 2013. We classify the 61 kibbutzim that reformed a year before an election as treated kibbutzim and the 59 kibbutzim that reformed a year after an election as control kibbutzim. We observe each kibbutz only once at the elections adjacent to its reform timing. So, if a kibbutz is reformed in 1995, it will be observed only at the elections of 1996, and it will be considered treated. Likewise, a kibbutz that was reformed in 2004 will only be observed at the election polls in 2003 and will be classified as control.

	Free Labor Market Index	Trust in Leadership Index	Support of Equality	Support of Mutual Guarantee	Collectivism Index	Kibbutz's Economic Condition	Work Ethics in Kibbutz
Two Way Fixed Effect							
Deliberation	0.066	-0.092	-0.107	-0.111	-0.110	-0.180	-0.176
	(0.051)	(0.069)	(0.071)	(0.062)	(0.010)	(0.091)	(0.060)
Implementation	0.235	-0.155	-0.286	0.013	-0.243	-0.080	0.139
	(0.059)	(0.073)	(0.079)	(0.074)	(0.102)	(0.111)	(0.072)
Observations	7,466	7,458	7,075	7,186	7,295	7,511	7,396
Reformed After 2001 (TWFE)							
Deliberation	0.059	-0.093	-0.107	-0.062	-0.111	-0.157	-0.149
	(0.053)	(0.070)	(0.073)	(0.063)	(0.102)	(0.096)	(0.063)
Implementation	0.229	-0.159	-0.300	0.150	-0.248	-0.035	0.203
	(0.067)	(0.077)	(0.087)	(0.085)	(0.114)	(0.129)	(0.080)
Observations	5,083	5,078	4,831	4,890	4,966	5,115	5,075
Borusyak et al.							
Deliberation	0.061	-0.091	-0.119	0.059	-0.062	-0.105	-0.118
	(0.053)	(0.076)	(0.079)	(0.081)	(0.125)	(0.086)	(0.063)
Implementation	0.228	-0.082	-0.224	0.442	-0.124	0.230	0.379
	(0.082)	(0.093)	(0.111)	(0.125)	(0.169)	(0.123)	(0.087)
Observations	4,796	4,796	4,555	4,623	4,696	4,827	4,793

Table 3: The Effect of Reforms on Attitudes: DiD

In Panel A, we regress each outcome separately on an indicator if the kibbutz is during deliberations ($t = 0$ or $t = 1$), and an indicator if the kibbutz is after implementation. We included kibbutz FEs, year FEs, and individual-level controls. Hence, this is a TWFE specification with two treatments, as shown in Equation 5. We report the coefficient of both treatments. In Panel B we replicate the excercise, dropping kibbutzim that reformed before 2001. In Panel C we reproduce the results using the design of Borusyak et al. (2021).

Our identification assumption is that the coincidence between a kibbutz's reform date and the general election's year is random within the sample. Therefore, there will be no systematic differences between the control and treated kibbutzim at baseline. This assumption is plausible chiefly because elections are often not anticipated in the Israeli system, but rather occur due to dynamic political circumstances. In our sample, all six elections took place earlier than required by law. Thus, it is unlikely that kibbutzim managed to plan their reform (or deliberation process) to occur just before or after an election.

To support our identification assumption that belonging to the control or treatment group is random, we present balancing tests using our survey data. First, we took a series of questions in the survey until 1996 and regressed the answer to these questions on the treatment indicator, including year-fixed effects. The results are presented in Table A17, where in columns (1)-(3), the treatment is the deliberation, while in columns (4)-(6), the treatment is of the implementation. As can be seen, before 1996, the kibbutzim did not differ in any of the observed variables, regardless of their treatment status in subsequent years.

Thus, based on our identification assumption, any difference we detect in voting behavior is caused by the reform. Therefore, to identify the reform's influence on voting patterns, we estimate the following regression model:

$$V_{kt} = \alpha \cdot \text{Artzi}_k + \tau_t \cdot \text{Time}_t + \theta \cdot \text{Treatment}_{kt} + \epsilon_{kt} \tag{6}$$

Where V_{kt} is some electoral outcome; percent cast to a political camp or voter turnout. Artzi_k is an indicator if the kibbutz is part of the Artzi movement, Time_t is a series of time dummies, and the variable of interest Treatment_{kt} is equal to 1 if kibbutz k reformed (or deliberated) just before t, and 0 if it reformed (or deliberated) just after t.

While the specification mentioned above captures the implementation treatment effect, we also want to estimate the effect of the deliberation treatment. To do that, we follow an identical procedure, only we define a kibbutz as treated if it started deliberating one year before the elections (that is, if it reformed one year after the elections) and control if it started deliberation one year after the elections (that is if it reformed three years after the elections). Notice that in this procedure, what was before a control group becomes the treated group, and the control group is a distinct new subsample of kibbutzim.

In Table 4, we present equation (4) estimation results. Panel A depicts the coefficient associated with treatment when treatment is defined as deliberation. Panel B does the same for implementation treatment. As can be seen, while the effect of starting the deliberation is null, reforming before the election substantially affects electoral patterns. Following a reform, members vote in lower numbers to the left and instead shift to the center and the right. Neither treatment influences voting turnout.

In Table 4 we further investigate the effect of the implementation treatment. The tests in this table serve as robustness checks and as preparation for the placebo analysis, which introduce later. The regression estimates in this table are based on the benchmark specifications with some sample modifications. In Panel A, we omit the kibbutzim that were reformed a year before or after the elections of 1996 (that is, in 1995 and 1997). In Panel B, we omit the kibbutzim that reformed near the 1999 elections (in 1998 and 2000). In Panel C, we omit the kibbutzim that reformed near the 2013 elections (in 2012 and 2014). Lastly, we also investigate what happens when we broaden our definition of treatment and control groups while moving away from the cutoff date. In Panel D, we take kibbutzim that reformed two years before or after an election. This sample includes almost all the kibbutzim in Israel that ever reformed. We define the kibbutz as a control if it reformed up to two years after the elections and treated if it reformed up to two years before it. Some kibbutzim appear twice in this specification: once as control and once as treated. For example, the kibbutzim reformed in 2004 will be part of the control group in 2003 and the treated group in 2006. In all specifications, we control for the indicator of affiliation with the more ideological Artzi movement. The main results we obtained hold in all specifications.

	% Turnout	%Left	% Center	% Right
Deliberation				
Treatment	-0.874	0.381	-0.256	0.383
	(1.581)	(1.296)	(0.623)	(0.738)
Observations	99	99	99	99
Implementation				
Treatment	1.629	-5.785	2.084	2.023
	(1.583)	(1.632)	(0.834)	(0.829)
Observations	121	121	121	121

Table 4: The Reform Pushed Members Rightwards Electorally

In panel A we define treatment to be kibbutzim that started deliberating just before elections, and control to be kibbutzim that started deliberating just after elections. We regress on treatment, voting turnout, and percent of votes cast to the left, center and the right, as shown in Equation 6. In Panel B we do the same, only treated kibbutzim are kibbutzim that reformed one year before, and control are kibbutzim that reformed one year after. We always control for affiliation with Artzi and a full set of year dummies.

To strengthen the credibility of the interpretation we employ two placebo exercises. First, we remove from our primary sample kibbutzim that reformed around the 1996 elections and set each kibbutz's treatment status one election backward. Therefore, a kibbutz that was reformed in 1998, just before the elections

of 1999, is considered in this placebo setup as treated for the 1996 elections. In reality, however, it was treated for the 1999 elections. Similarly, a kibbutz reformed in 2000 will be considered a control for the 1996 elections. We also do a similar analysis and set the treatment or control year one year forward. So, for example, the kibbutz reformed in 1995 will be treated for the 1999 elections. Table A19, Panels A and B present these placebo estimation results.

In Panels C, D, and E, in the same table, we present results for our second set of placebo tests. In Panel C, we divide the sample that reformed after 1997 into treatment and control based on their actual treatment/control status. However, we run the regression using only voting results in the 1996 election – before any kibbutzim had reformed. Similarly, in Panel D, we take kibbutzim that reformed after 2000 and examine their voting patterns in the 1996 and 1999 elections. Finally, we also implement a placebo estimation based on omitting kibbutzim that reformed after 2011 and using the remaining kibbutzim as outcomes in the regressions of the 2013 election results. Namely, a round of voting after all the kibbutzim in the sample have already reformed. As seen in all placebo estimations, the point estimates show null effects, consistent with our interpretation of the original results.

8 Conclusions

Since the financial crisis of 2008, there has been great interest in uncovering the political and economic consequences of economic crises. However, this research has focused on the experience of market economies. Because socialist countries are often not democratic, it is difficult to elicit people's attitudes reliably. In this paper, we contribute to the literature by studying the evolution of political and economic attitudes in a society that is socialist yet democratic. In this context, we assembled and analyzed reliable micro-level data on political voting and economic attitudes.

Using the fact that Israeli kibbutzim experienced economic crises of varying severity as a quasi-natural experiment, we study the political and economic implications of economic crises in a socialist and democratic system. We find that people in kibbutzim who experienced a more severe crisis shifted rightward in their voting in national elections, and that this effect persisted for more than two decades after the crisis was over.

This electoral transition was rooted in a move away from socialist ideology toward supporting the free-market system. Using surveys that elicited the economic and political attitudes of thousands of kibbutz members, we find that members of kibbutzim that experienced more severe economic crises increased support for the liberalization of labor markets and reduced support for socialism, and that this shift persisted long after the economic crisis was resolved. The crisis had an especially long lasting effect on adolescents and young adults. Taken together, our results are consistent with economic downfalls triggering resentment toward the prevailing economic system, which persist in the long run, especially among the young. Exploiting our unique settings, we also study the recovery from the crisis. First, while the crisis changed trust and economic attitudes, a debt relief arrangement partly reversed the effect. Following the signing of the arrangement, trust in leadership was restored, indicating that sound policy can help leaders regain trust. However, economic perceptions were less amendable to this development. The end of the crisis did not restore belief in socialism.

Second, we find that after kibbutzim liberalized their labor market and moved away from equal sharing to a market-based wages, members' attitudes shifted further away from socialism to endorsing capitalism. This explains at least partially how the crisis altered attitudes even 25 years after it ended. Still, kibbutz

members increased their support of mutual guarantee and a safety net for weak members, suggesting that their preferred model is a hybrid between market-based wages and community support, a model that we call "Capitalism with Compassion." .

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Online Appendices

9   Online Appendix: Figures
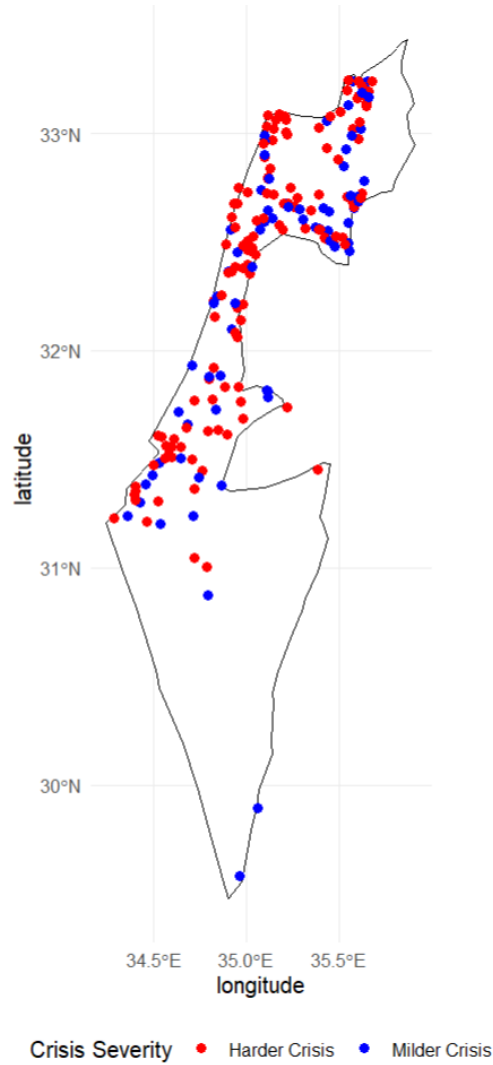
Figure A1: Map of Kibbutzim by Crisis Severity

This map demonstrates the location of the kibbutzim for which we know their Economic Strength in 1994. Red points represent kibbutzim more hit by the crisis. Blue points represent kibbutzim that experienced a milder crisis.
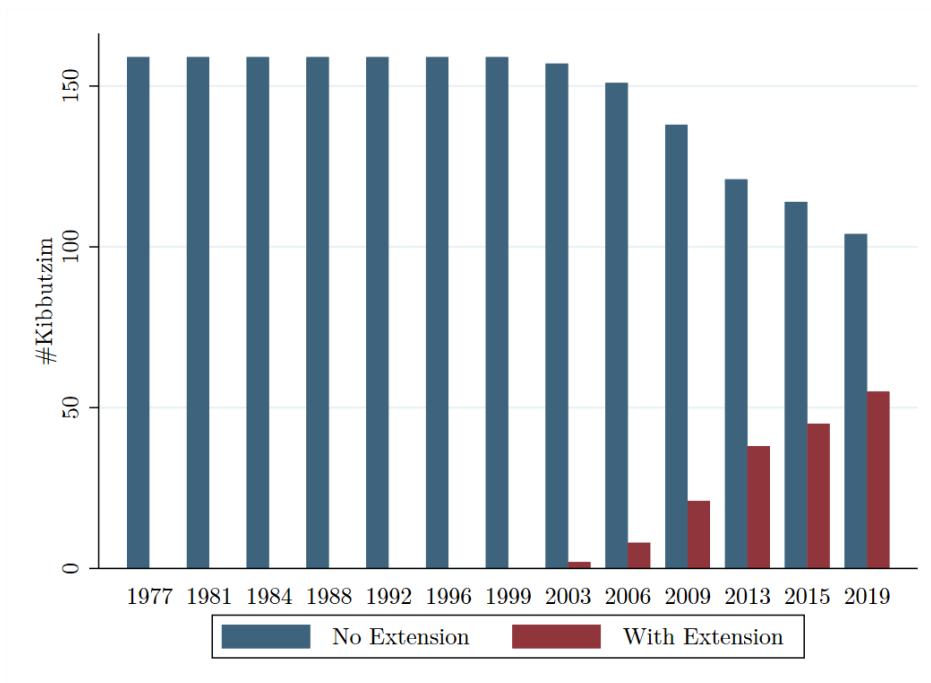
Figure A2: Numbers of Kibbutzim by Year and Existence of an Extension

For each election year between 1977-2019 we plot in blue the number of kibbutzim that do not have an extension (*Harchava*) in blue, and the number of kibbutzim with one in red.

(a) Reform Titming

(b) Debt Arrangement Timing
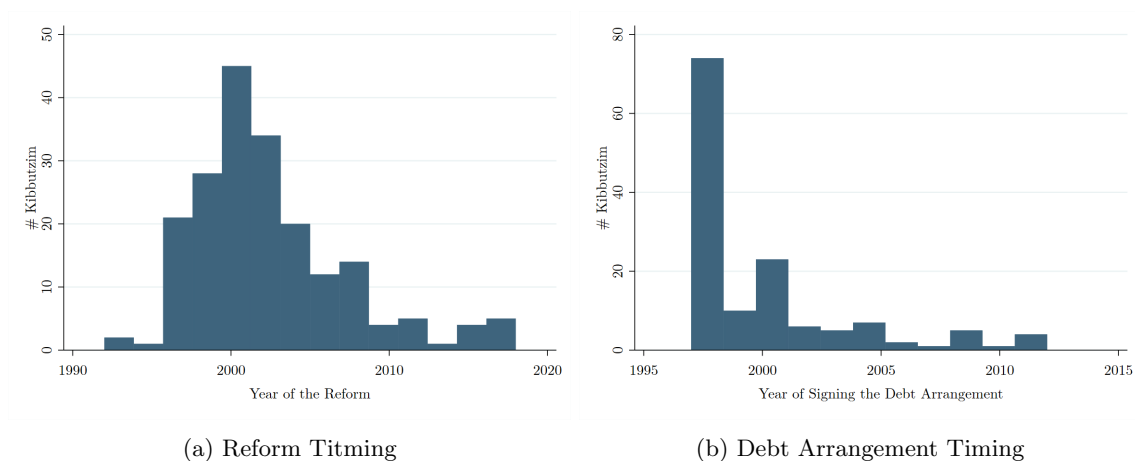
Figure A3: Timing of Reforms in the Kibbuzim

We plot the distribution of the year of reform on the left, and the year of the signing on the debt arrangement on the right
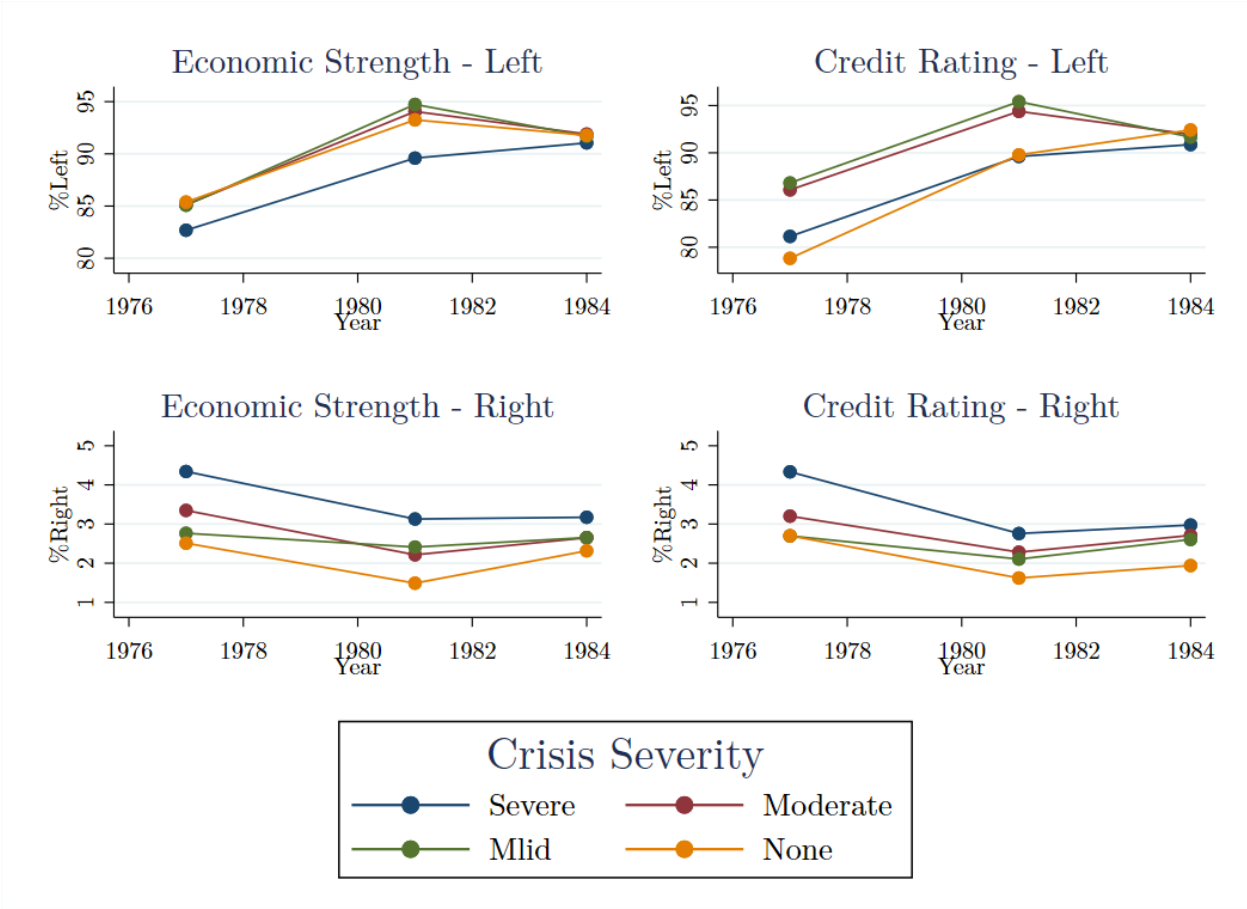
Figure A4: Voting Patterns Before the Crisis are Balanced

For the measures Economic Strength and Credit Rating we divide the sample according to the four groups. We then plot for each group the average percentage of support of the left and the right in each of the pre-crisis elections (1977, 1981, 1984).
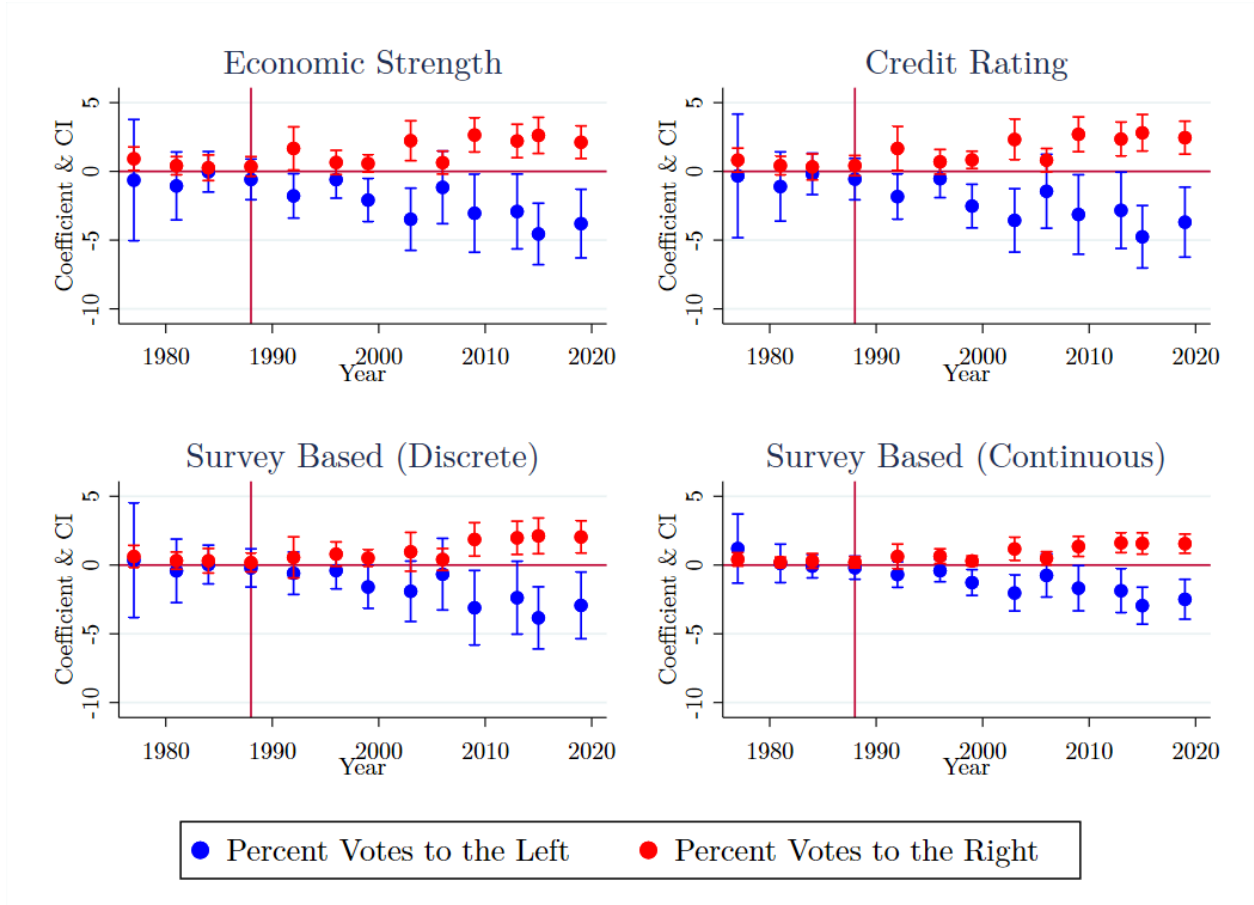
Figure A5: The Effect of The Crisis on Voting: By the 3 Measures

We take the percent of votes cast to the left and to the right in each kibbutz between the years 1977-2019. For each year separately, we regress these outcomes on the crisis' severity. In Panels A, B and C, we use the discrete measures, where the treatment belongs to groups 1-2 (hit severely) and the control belongs to groups 3-4 (hit mildly). In Panel D, we use continuous survey-based measures as treatment. In blue, we depict the coefficients and 95% CIs for voting left, and in red, we depict voting right. The horizontal line exhibits the null, and the vertical line is positioned in 1988, the timing of the first elections after the crisis had begun.
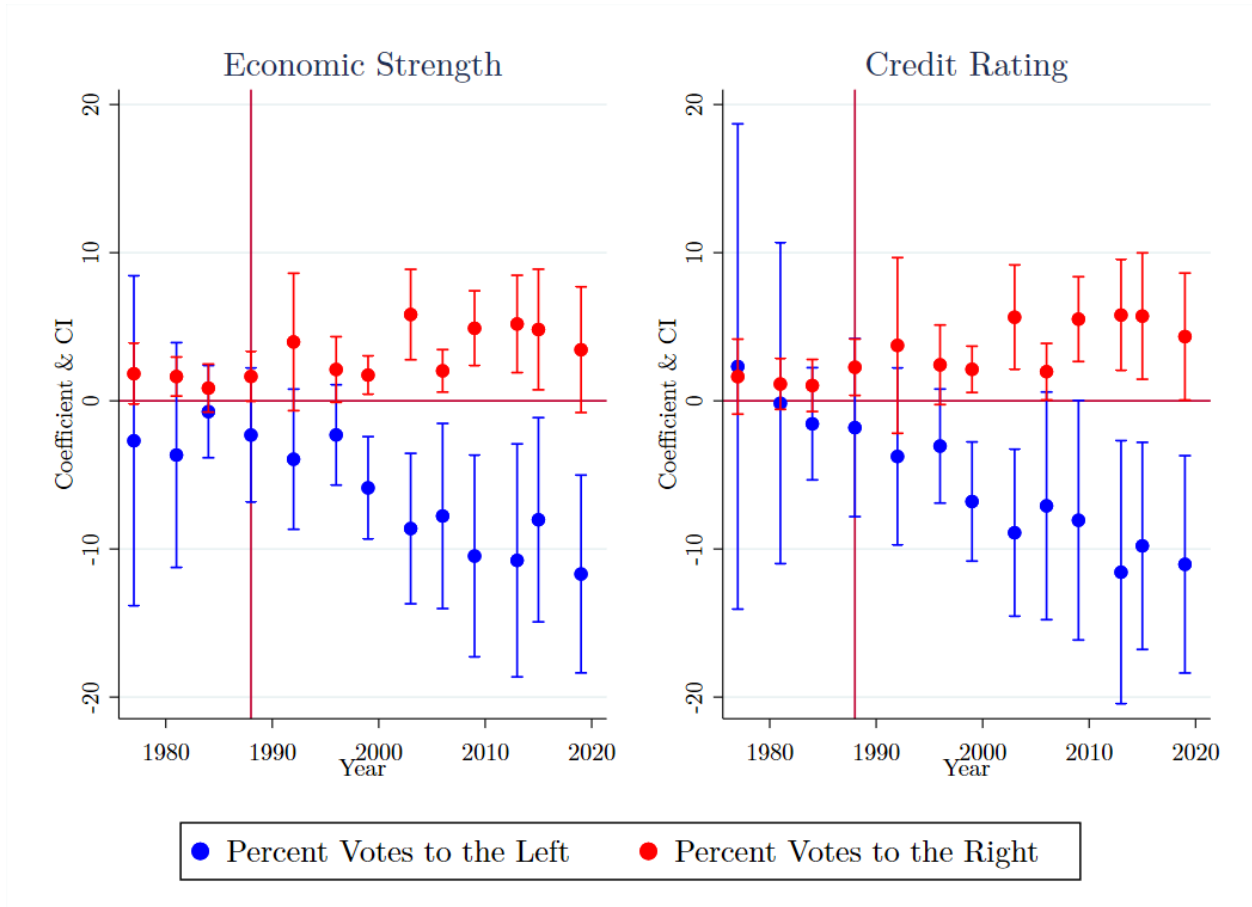
Figure A6: The Effect of The Crisis on Voting: No Extensions

We get the percent of votes cast to the left and to the right in each kibbutz between the years 1977 and 2019. For each year separately, we regress these outcomes on the crisis' severity, as calculated by varying measures. We regress from our sample kibbutzim once they are extended. In Panel A, we use the Economic Strength measure, and in Panel B, the Credit Rating measure. The treatment group is always kibbutzim severely hit (group 1), while the control group includes kibbutzim that were not hit (group 4). In blue, we depict the coefficients and 95% CIs for voting left, and in red, we depict the coefficients and CIs for voting right. The horizontal line exhibits the null, and the vertical line is positioned in 1988, the timing of the first elections after the crisis had begun.
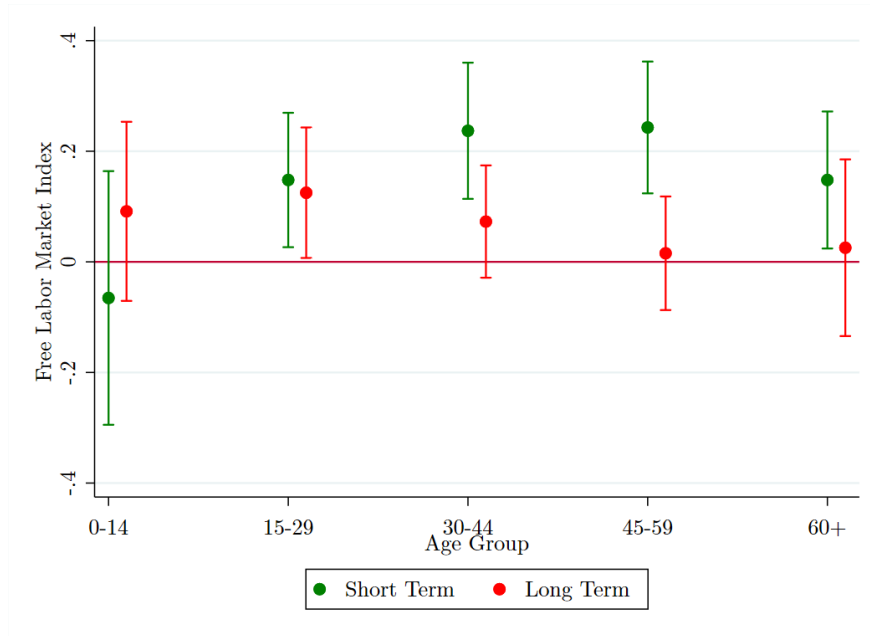
(a) Free Labor Markets Index
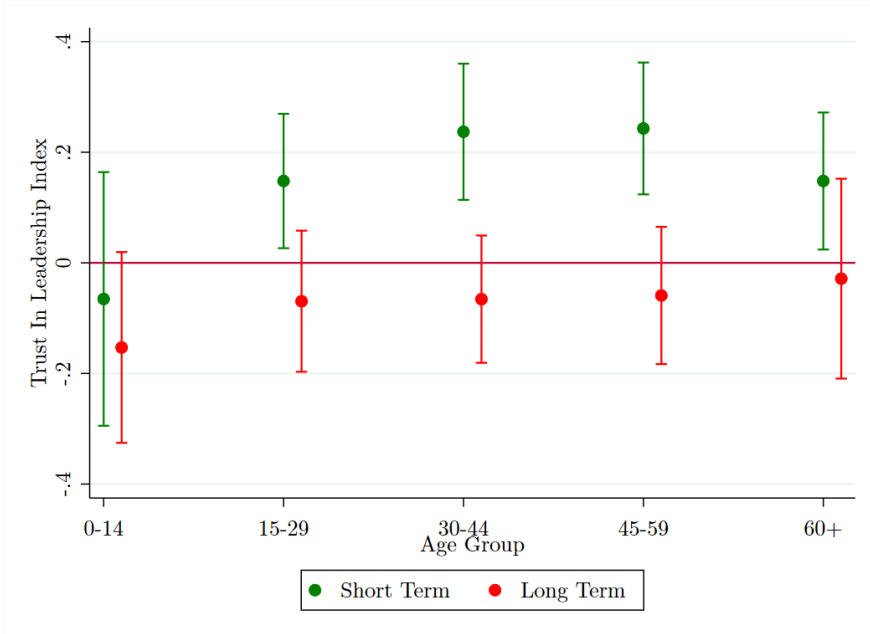
(b) Trust in Leadership Index

Figure A7: The Crisis's Effect on Economic Attitudes Persists More in Younger Cohorts: Robustness

This figure plots the coefficients when regressing the indices on the Economic Strength measure. The treatment group is kibbutzim in the first and second groups (severely and moderately hit), and the control group is the third and fourth groups (kibbutzim not hit at all or mildly hit). We also show the respective 95% CIs. The main specification is estimated separately for different age group and within each age group separately for the sample years 1991-2002 (green) and 2003-2018 (Red). Panel A presents the coefficients from regressions in which the free labor market index is the explained variable, while in Panel B, the explained variable is the trust in leadership index. We included individual-level controls, kibbutz-level controls, and year Fixed Effects. We keep only individuals that lived in the kibbutz during the crisis. We cluster at the kibbutz level.
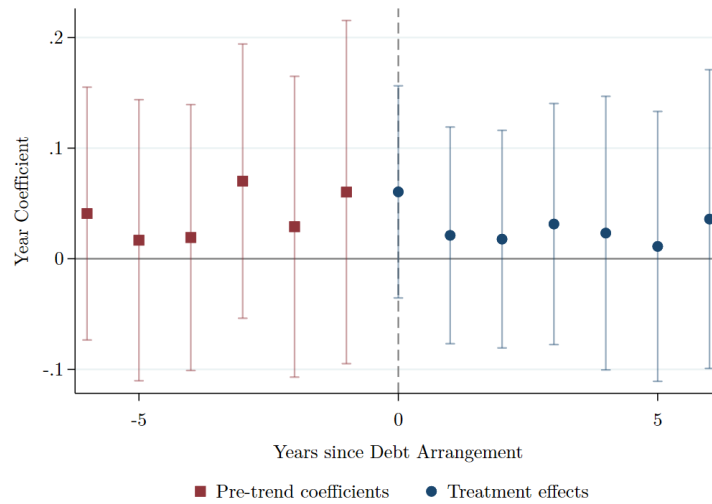
(a) Free Labor Market Index
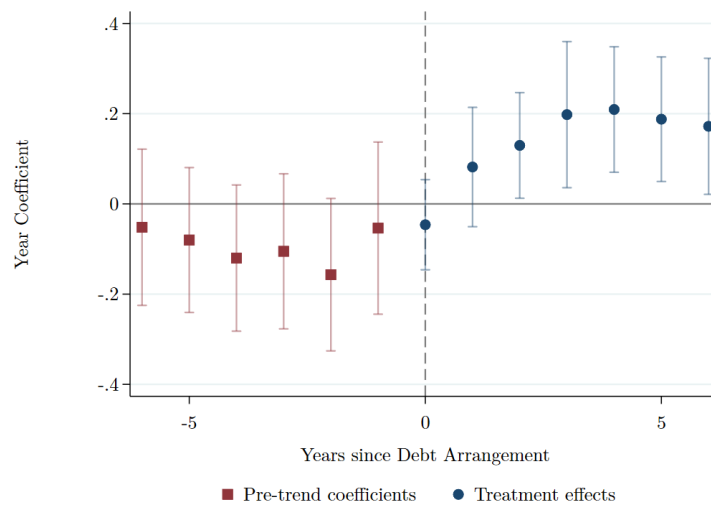
(b) Trust in Leadership Index

Figure A8: Debt Arrangement Restores Trust: Borusyak el al. (2021)

Using an Event Study approach, we estimate the effect of the debt relief arrangement on the labor and trust indices, using the method developed by Borusyak et al. (2021). We show CIs of 95% level, we cluster at the kibbutz level, and we add individual-level controls. Our sample includes only people who were in the kibbutz during the crisis and are older than 20.

(a) Free Labor Market Index

(b) Perceived Economic State in the Kibbutz

(c) Perceived Work Ethics in the Kibbutz

(d) Support of Equality

(e) Support of Collectivism

(f) Support of Mutual Guarantee
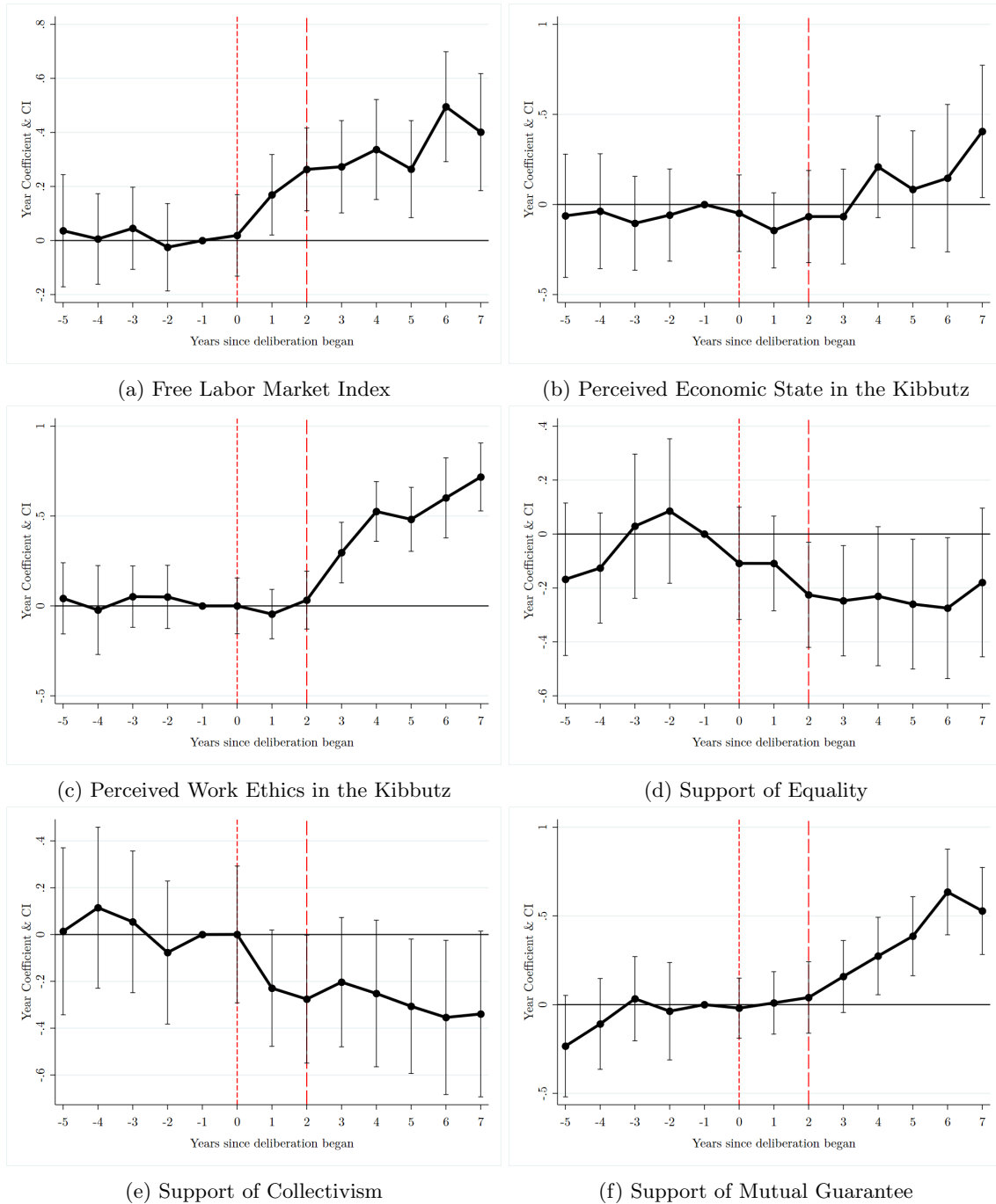
Figure A9: Reform Event Studies: Only Kibbutzim Reformed After 2001

We estimate the effect of the deliberations and the implementation of reforms on various outcomes using a Two-Way-Fixed-Effect Event Study specification, as shown in Equation 4. Time $t = 0$ depicts the start of the deliberation, and implementation happens at $t = 2$. We include individual-level controls and cluster and the kibbutz level. We bring the 95% CIs. We focus only on the sample spanning 2001-2011. We drop kibbutzim reformed before 2001.
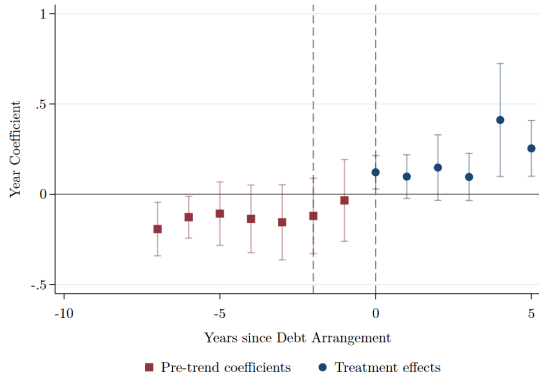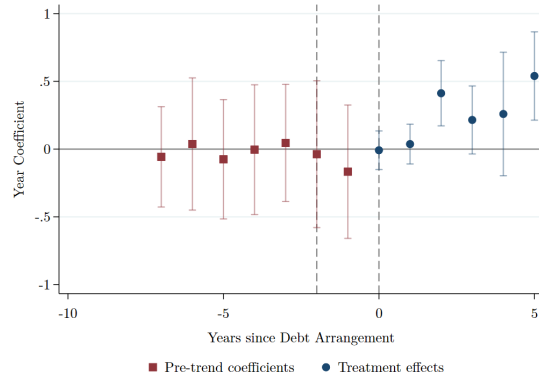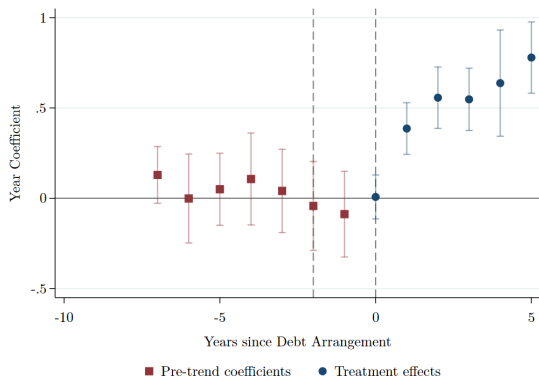
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(a) Free Labor Market Index

(b) Perceived Economic State in the Kibbutz

(c) Perceived Work Ethics in the Kibbutz
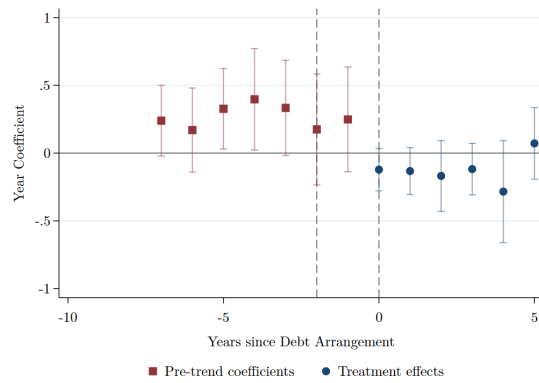
(d) Support of Equality
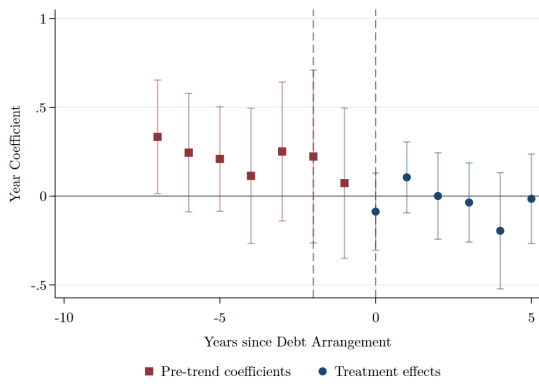
(e) Support of Collectivism
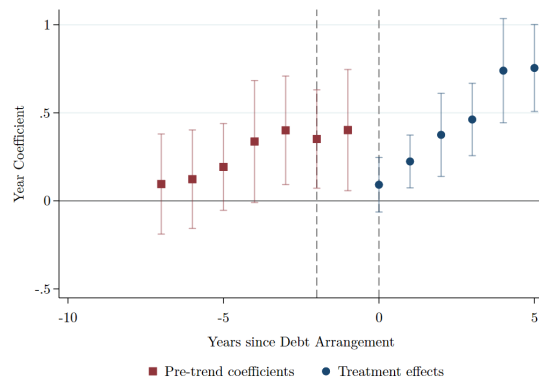
(f) Support of Mutual Guarantee

Figure A10: Reform Event Studies: Borusyak el al. (2021)

Using an Event Study approach, we estimate the effect of the implementation and deliberation of the reform on various outcomes, using the method developed by Borusyak et al. (2021). We focus only on the sample spanning 2001-2011. We show CIs of 95% level, we cluster at the kibbutz level, and we add individual-level controls.

10   Online Appendix: Tables

	Economic Strength	Credit Rating
Survey-Based Continuous	0.740	0.764
Survey-Based Discrete	0.766	0.732
Economic Strength	1.000	0.890

Table A1: The Three Economic Measures Are Highly Correlated

This table presents the correlation between the average survey-based economic and the other crisis severity measures. In the first row, we use the continuous survey-based economic measure. In the second row, we use its transformation into a discrete variable, divided into quartiles. The last row presents the correlation between the two expert-based measures.

	Obs	#People	%Female	%Married	#Children	%High-School	%College	Work Weeks	Work Hours	% Israel Born
Economic Strength										
Severe Crisis	25	411.8	49.2	42.4	2.582	80.9	8.38	49.20	42.01	63.9
Moderate Crisis	99	486.0	49.2	42.7	2.648	83.1	8.29	49.04	43.24	68.6
Mild Crisis	41	568.4	47.6	44.0	2.681	82.4	9.25	48.85	42.86	70.0
No Crisis	30	568.5	49.1	43.4	2.783	81.9	8.99	48.34	43.29	69.0
Credit Rating										
Severe Crisis	35	398.4	48.5	41.3	2.583	81.8	8.04	49.13	42.80	65.7
Moderate Crisis	95	420.6	49.3	43.2	2.654	83.3	8.74	49.02	43.06	68.7
Mild Crisis	51	563.4	48.4	43.3	2.741	82.0	8.38	48.84	43.17	70.4
No Crisis	14	583.9	48.2	45.8	2.704	80.8	10.0	47.93	42.56	65.5

Table A2: Descriptive Statistics by Crisis Severity Measures, Kibbutzim Level Sample

We obtain information on each kibbutz from the 1983 census. In particular, we take the percentage of women, the percentage of married people, the average number of children per household, the percentage of people who finished high school, the percentage of people who have some academic education, the average annual workweek, the average weekly work hours, and percent of people born in Israel. We then stratify the sample according to the 4 groups of each economic measure and depict the average outcomes per group. In Panel A, we do so according to the Economic Strength Measure and in Panel B according to the Credit Rating measure.

	Severity	Period	Obs	% Turnout	% Left	% Center	% Right
Economic Strength	Groups 1 & 2	Pre	288	84.73	89.77	4.38	2.92
	(Severe Crisis)	Post	768	73.07	67.17	20.27	6.94
	Groups 3 & 4	Pre	159	86.92	90.35	4.64	2.39
	(Mild Crisis)	Post	424	74.89	69.88	19.77	3.99
Credit Rating	Groups 1 & 2	Pre	300	84.76	89.80	4.37	2.91
	(Severe Crisis)	Post	800	72.98	67.21	20.22	6.95
	Groups 3 & 4	Pre	147	87.03	90.34	4.69	2.38
	(Mild Crisis)	Post	392	75.22	70.02	19.83	5.08
Survey Based	Groups 1 & 2	Pre	273	85.24	89.99	4.36	2.91
	(Severe Crisis)	Post	728	73.09	67.21	20.22	6.91
	Groups 3 & 4	Pre	192	86.03	90.01	4.69	2.50
	(Mild Crisis)	Post	512	74.70	69.31	19.98	5.57

Table A3: Descriptive Statistics of Electoral Voting, By Crisis Severity and Period

We divide the sample into observations before and after the crisis, and according to kibbutzim hit more (groups 1 and 2) and hit less (groups 3 and 4). In Panel A, we do it according to the Economic Strength measure; in Panel B, according to the Credit Rating measure; and in Panel C, according to the survey-based measure. For each cell we bring the number of observations, the turnout, and the percentage support of each political camp.

	2009 CBS	2009 Survey	2011 CBS	2011 Survey
% Female	48.09	52.27	48.24	51.04
% With No Academic Degree	57.6	60.8	57.6	60.2
% With BA Degree	27.97	27.02	27.67	25.3
% With Advanced Degree	14.48	12.1	14.61	14.39
% In an Artzi Kibbutz	35.06	36.57	35.2	37.51
% In a Takam Kibbutz	64.94	63.43	64.8	62.49

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Table A4: The Survey Is Representative

This table presents descriptive statistics of kibbutz members for 2009 and 2011 from the Israeli Central Bureau of Statistics (CBS) and from IRK surveys, which we use in the paper. The CBS is based on the all kibbutz population. The table presents statistics for two years where both the CBS and the IRK survey data are available and are within the period of the study.

	% Havoda	% Ratz
Economic Strength		
Severe Crisis	1.511	-1.074
	(4.226)	(0.817)
Moderate Crisis	2.474	-0.367
	(3.173)	(0.725)
Mild Crisis	4.654	-0.943
	(3.296)	(0.802)
Observations	368	368
Credit Rating		
Severe Crisis	4.045	-1.804
	(7.024)	(1.249)
Moderate Crisis	6.070	-1.296
	(6.612)	(1.201)
Mild Crisis	7.874	-2.060
	(6.626)	(1.211)
Observations	368	368
Survey Based		
Severe Crisis	0.306	-0.381
	(3.138)	(0.710)
Moderate Crisis	0.692	-0.416
	(2.962)	(0.656)
Mild Crisis	-0.133	0.308
	(2.917)	(0.738)
Observations	388	388

Table A5: Pre-Crisis Voting to Leftist Parties is Balanced

We take as outcomes voting to each one of the two non-arab leftist parties in 1977-1984; *Havoda* and *Ratz*. We regress these outcomes on the measures of crisis severity, controlling for year Fixed Effects and clustering at the kibbutz level. In Panel A we show the coefficients we using as the treatment variable the Economic Strength measure; in Panel B, we use the Credit Rating measure as the treatment; and in Panel C we use the Survey Based measure. The omitted group is the 4th group, which did not experience a crisis. We use the sample of the kibbutzim which we have full voting data on.

	(1)	(2)	(3)	(4)	(5)
	Artzi	**Meuchad**	**Meuchad - No Group FEs**	**Meuchad - Group FEs**	**Familial Sleep Year**
Economic Strength					
Severe Crisis	0.125	-0.104	0.050	-0.016	-0.725
	(0.144)	(0.177)	(0.340)	(0.519)	(2.742)
Moderate Crisis	0.150	-0.024	-0.333	-0.315	-1.498
	(0.120)	(0.143)	(0.293)	(0.585)	(2.327)
Mild Crisis	0.190	-0.056	-0.306	-0.315	-0.718
	(0.140)	(0.172)	(0.305)	(0.585)	(2.678)
Observations	152	105	30	30	143
Credit Rating					
Severe Crisis	-0.073	-0.037	0.714	1.000	-3.048
	(0.180)	(0.222)	(0.550)	(0.770)	(3.677)
Moderate Crisis	0.118	-0.062	0.455	1.191	-3.647
	(0.171)	(0.213)	(0.537)	(0.901)	(3.497)
Mild Crisis	0.044	0.000	0.545	1.176	-3.081
	(0.180)	(0.225)	(0.537)	(0.884)	(3.677)
Observations	152	105	30	30	143
Survey Based					
Severe Crisis	0.083	-0.002	0.083	0.207	-0.507
	(0.118)	(0.147)	(0.295)	(0.442)	(2.236)
Moderate Crisis	0.110	-0.054	0.500	1.402	-0.760
	(0.120)	(0.151)	(0.347)	(0.670)	(2.284)
Mild Crisis	-0.024	-0.172	0.442	0.598	-0.192
	(0.122)	(0.149)	(0.281)	(0.391)	(2.309)
Observations	162	114	30	30	151

Table A6: Various Pre-Crisis Balancing Tests

We regress various pre-crisis outcomes on the external measures of the crisis. In Panel A we show the coefficients we using as the treatment variable the Economic Strength measure; in Panel B, we use the Credit Rating measure as the treatment; and in Panel C we use the Survey Based measure. In column (1) we use as an outcome affiliation with Artzi, and in column (2) within the Takam movement affiliation with Meuchad as an outcome variable. In columns (3) and (4) we concentrate on the groups generated by the crisis in the 50s. We regress within this sample affiliation with Meuchad, with and without controlling for group Fixed Effects. In column (5) we take the year of the shift to family sleeping arrangement as the outcome variable. The omitted group is always the 4th group, which did not experience a crisis. We use the sample of the kibbutzim which we have full voting data on.

	% Turnout	% Left	% Center	% Right	$\frac{Center+Right}{Left}$
Full Sample					
Severe Crisis	-1.158	-3.745	1.606	1.577	1.672
	(0.951)	(1.957)	(1.162)	(0.475)	(0.526)
Moderate Crisis	-0.982	-3.294	0.925	1.734	1.921
	(0.917)	(1.828)	(1.162)	(0.440)	(0.487)
Mild Crisis	-0.268	-3.326	1.342	1.756	1.864
	(0.997)	(1.736)	(1.196)	(0.483)	(0.523)
Observations	2,015	2,015	1,860	2,015	1,860
Full Sample: Continuous					
Continuous Likert Scale	-0.615	-1.848	0.890	0.650	0.719
	(0.385)	(0.777)	(0.524)	(0.209)	(0.234)
Observations	2,015	2,015	1,860	2,015	1,860
Partial Sample					
Severe Crisis	-1.049	-3.327	1.436	1.329	1.386
	(0.983)	(2.015)	(1.202)	(0.465)	(0.513)
Moderate Crisis	-0.863	-3.072	0.740	1.690	1.869
	(0.971)	(1.940)	(1.227)	(0.478)	(0.530)
Mild Crisis	-0.200	-3.267*	1.302	1.696	1.803
	(1.012)	(1.780)	(1.225)	(0.482)	(0.524)
Observations	1,911	1,911	1,764	1,911	1,764
Partial Sample: Continuous					
Likert Scale	-0.570	-1.672	0.817	0.548	0.600
	(0.400)	(0.805)	(0.544)	(0.207)	(0.232)
Observations	1,911	1,911	1,764	1,911	1,764

Table A7: The Effect of the Crisis on Voting: Survey-Based Measure

We get voter turnout and percent cast to each political camp as outcomes. In the last column, we take the votes cast to the center and the right, divided by the number of votes cast to the left. Each observation is at the elections-kibbutz level, and the sample includes all elections from 1977 to 2019. We keep only kibbutzim that have appeared throughout the years. Using a Two-Way-Fixed-Effect Difference-in-Difference specification, we estimate the effect of the crisis on them. The specification is outlined in equation (1). The coefficients reported are of the degree of the severity of the crisis interacted with an indicator of the year after 1985. In Panels A and C, we report estimates using the discrete Survey Based measure, with the omitted group being group 4 (not hit). In Panels B and D, we enter the Survey Based measure continuously. In Panels A and B, we use the full sample, and in Panels C and D, we use only kibbutzim for which we have external measures. We cluster at the kibbutz level.

	% Turnout	% Left	% Center	% Right	$\frac{Right+Center}{Left}$
Economic Strength					
Severe Crisis	1.071	-4.490	1.440	2.174	2.208
	(1.358)	(3.149)	(1.513)	(1.012)	(1.072)
Moderate Crisis	0.919	-1.682	0.0854	0.683	0.731
	(0.994)	(2.024)	(1.103)	(0.626)	(0.649)
Mild Crisis	1.022	-0.346	-0.662	-0.272	-0.252
	(1.197)	(2.215)	(1.382)	(0.686)	(0.709)
Observations	1,639	1,639	1,639	1,639	1,639
Credit Rating					
Severe Crisis	0.491	-7.888	3.355	3.110	3.247
	(1.593)	(4.487)	(2.619)	(0.870)	(0.952)
Moderate Crisis	-0.572	-5.581	2.261	1.205	1.356
	(1.368)	(3.892)	(2.419)	(0.612)	(0.679)
Mild Crisis	-0.390	-5.098	2.385	0.502	0.646
	(1.413)	(3.917)	(2.459)	(0.677)	(0.738)
Observations	1,639	1,639	1,639	1,639	1,639

Table A8: The Effect of the Crisis on Voting Patterns - Without the 88 and 92 Elections

We get voter turnout and percent cast to each political camp as outcomes. In the last column, we take the votes cast to the center and the right, divided by the number of votes cast to the left. Each observation is at the elections-kibbutz level, and the sample includes all elections from 1977 to 2019, except the years 1988 and 1992. We keep only kibbutzim that have appeared throughout the years. Using a Two-Way-Fixed-Effect Difference-in-Difference specification, we estimate the effect of the crisis on them. The specification is outlined in equation (1). The coefficients reported are of the degree of the severity of the crisis interacted with an indicator of the year after 1985. The omitted group is the group of kibbutzim that did not go through a crisis, according to every measure. In Panel A, we report estimates using the Economic Strength measure and in Panel B, we use the Credit Rating. We cluster at the kibbutz level.

	% Turnout	% Left	% Center	% Right	$\frac{Right+Center}{Left}$
Economic Strength					
Severe Crisis	1.992	-6.444	1.780	4.547	3.834
	(2.963)	(9.983)	(3.781)	(1.608)	(2.229)
Moderate Crisis	0.817	-3.727	0.608	1.521	1.789
	(1.141)	(5.921)	(2.902)	(0.589)	(0.753)
Mild Crisis	1.723	-3.717	1.284	-0.237	-0.0455
	(2.141)	(5.758)	(2.841)	(0.638)	(0.766)
Observations	481	481	444	481	444
Credit Rating					
Severe Crisis	2.582	-6.623	1.952	4.412	4.062
	(2.202)	(10.83)	(4.713)	(1.310)	(1.835)
Moderate Crisis	1.578	-7.469	2.675	0.982	1.239
	(1.227)	(8.087)	(3.833)	(0.688)	(0.939)
Mild Crisis	2.503	-7.230	3.408	-0.454	-0.286
	(1.715)	(8.056)	(3.818)	(0.722)	(0.954)
Observations	481	481	444	481	444

Table A9: The Effect of the Crisis on Voting Patterns - No Extensions

We get voter turnout and percent cast to each political camp as outcomes. In the last column, we take the votes cast to the center and the right, divided by the number of votes cast to the left. Each observation is at the elections-kibbutz level, and the sample includes all elections from 1977 to 2019. We keep only kibbutzim that have appeared throughout the years. We drop kibbutzim that ever extended. Using a Two-Way-Fixed-Effect Difference-in-Difference specification, we estimate the effect of the crisis on them. The specification is outlined in equation (1). The coefficients reported are of the degree of the severity of the crisis interacted with an indicator of the year after 1985. The omitted group is the group of kibbutzim that did not go through a crisis, according to every measure. In Panel A, we report estimates using the Economic Strength measure and in Panel B, we use the Credit Rating. We cluster at the kibbutz level.

	% Turnout	% Left	% Center	% Right	$\frac{Right+Center}{Left}$
Economic Strength					
Severe Crisis	0.318	-3.605	1.654	1.673	1.596
	(1.179)	(2.886)	(1.290)	(0.695)	(0.800)
Moderate Crisis	0.466	-1.978	0.380	0.701	0.787
	(0.875)	(1.831)	(0.950)	(0.407)	(0.458)
Mild Crisis	1.037	-0.714	-0.488	-0.0151	0.0510
	(1.060)	(2.008)	(1.246)	(0.471)	(0.532)
Observations	1,760	1,760	1,611	1,760	1,611
Credit Rating					
Severe Crisis	0.112	-6.440	3.373	2.250	2.360
	(1.353)	(4.336)	(2.504)	(0.696)	(0.820)
Moderate Crisis	-0.593	-5.379	2.299	0.985	1.128
	(1.127)	(3.803)	(2.342)	(0.531)	(0.626)
Mild Crisis	0.0938	-4.548	2.130	0.304	0.431
	(1.178)	(3.802)	(2.365)	(0.542)	(0.643)
Observations	1,760	1,760	1,611	1,760	1,611

Table A10: The Effect of the Crisis on Voting Patterns - Drop Once Extended

We get voter turnout and percent cast to each political camp as outcomes. In the last column, we take the votes cast to the center and the right, divided by the number of votes cast to the left. Each observation is at the elections-kibbutz level, and the sample includes all elections from 1977 to 2019. We keep only kibbutzim that have appeared throughout the years. We drop kibutzim once they extend. Using a Two-Way-Fixed-Effect Difference-in-Difference specification, we estimate the effect of the crisis on them. The specification is outlined in equation (1). The coefficients reported are of the degree of the severity of the crisis interacted with an indicator of the year after 1985. The omitted group is the group of kibbutzim that did not go through a crisis, according to every measure. In Panel A, we report estimates using the Economic Strength measure and in Panel B, we use the Credit Rating. We cluster at the kibbutz level.

	% Turnout	% Left	% Center	% Right
Economic Strength				
Severe Crisis	0.559	-3.198	0.989	1.958
	(1.183)	(2.882)	(1.320)	(0.859)
Moderate Crisis	0.624	-1.146	-0.184	0.516
	(0.879)	(1.850)	(0.965)	(0.532)
Mild Crisis	0.993	-0.564	-0.540	-0.247
	(1.070)	(1.977)	(1.217)	(0.558)
Reform	-0.0910	-3.177	1.864	0.312
	(0.543)	(0.726)	(0.512)	(0.388)
Observations	1,937	1,937	1,788	1,937
Credit Rating				
Severe Crisis	0.0951	-6.356	2.804	2.782
	(1.410)	(4.215)	(2.396)	(0.742)
Moderate Crisis	-0.651	-4.964	1.926	1.068
	(1.199)	(3.686)	(2.233)	(0.541)
Mild Crisis	-0.264	-4.979	2.303	0.526
	(1.241)	(3.692)	(2.265)	(0.577)
Reform	-0.116	-3.210	1.904	0.303
	(0.539)	(0.744)	(0.525)	(0.388)
Observations	1,937	1,937	1,788	1,937

Table A11: The Effect of the Crisis on Voting Patterns - Control For Reform

We get voter turnout and percent cast to each political camp as outcomes. In the last column, we take the votes cast to the center and the right, divided by the number of votes cast to the left. Each observation is at the elections-kibbutz level, and the sample includes all elections from 1977 to 2019. We keep only kibbutzim that have appeared throughout the years. Using a Two-Way-Fixed-Effect Difference-in-Difference specification, we estimate the effect of the crisis on them. The specification is outlined in equation (1). We also control for the passage of the reform The coefficients reported are of the degree of the severity of the crisis interacted with an indicator of the year after 1985. The omitted group is the group of kibbutzim that did not go through a crisis, according to every measure. We also report the coefficient of the reform. In Panel A, we report estimates using the Economic Strength measure and in Panel B, we use the Credit Rating. We cluster at the kibbutz level.

	Economic Strength	Credit Rating	Survey Based
Severe Crisis	0.002	0.059	0.007
	(0.076)	(0.064)	(0.05)
Moderate Crisis	0.012	0.082	-0.036
	(0.047)	(0.046)	(0.047)
Mild Crisis	0.005	0.082	-0.015
	(0.057)	-0.055	(0.047)
Observations	2,963	2,963	3,156

Table A12: The Crisis's Intensity is Uncorrelated With Attitudes Towards the Conflict

As an outcome we take a question asked in 2002-2005, asking how likely it is that Israel and Palestine will be able to resolve their conflict. The answers range between 1-4; the higher the number, the more likely peace is feasible. We regress the answer to this question on the three measures of the crisis. The results for each measure is demonstrated in a different column. We control for individual-level controls, year Fixed Effects, and passage of the reform. We cluster at the kibbutz level. Our sample includes only people who were in the kibbutz during the crisis and are older than 20.

	Economic Strength		Credit Rating		Survey Based	
	Free Labor Market Index	Trust in Leadership Index	Free Labor Market Index	Trust in Leadership Index	Free Labor Market Index	Trust in Leadership Index
Severe Crisis	0.287	-0.230	0.290	-0.225	0.299	-0.306
	(0.0661)	(0.0739)	(0.0910)	(0.0832)	(0.0532)	(0.0559)
Moderate Crisis	0.143	-0.152	0.155	-0.161	0.157	-0.224
	(0.0563)	(0.0598)	(0.0838)	(0.0767)	(0.0596)	(0.0539)
Mild Crisis	0.0285	-0.0129	0.0837	-0.0896	0.101	-0.164
	(0.0727)	(0.0715)	(0.0886)	(0.0763)	(0.0634)	(0.0584)
Observations	16,045	14,152	16,045	14,152	17,194	15,170

Table A13: The Effect of Crisis on Indices - Formal Analysis

We regress the Labor and Trust indices on the measures of the crisis. We control for individual-level controls, year Fixed Effects, and passage of the reform. We cluster at the kibbutz level. Our sample includes only people who were in the kibbutz during the crisis and are older than 20.

	Extra Pay	Full Liberalization	Differential Salary
Panel A			
Economic Strength			
Severe Crisis	0.581	0.326	0.396
	(0.116)	(0.136)	(0.148)
Moderate Crisis	0.372	0.0924	0.202
	(0.116)	(0.0788)	(0.127)
Mild Crisis	0.0845	-0.0119	-0.0610
	(0.154)	(0.0964)	(0.148)
Panel B:			
Credit Rating			
Severe Crisis	0.617	0.297	0.381
	(0.206)	(0.140)	(0.199)
Moderate Crisis	0.432	0.102	0.201
	(0.202)	(0.104)	(0.188)
Mild Crisis	0.211	0.0323	0.0761
	(0.209)	(0.112)	(0.190)
Observations	11,436	12,949	12,284

Table A14: The Effect of the Crisis on Economic Attitudes - By Question

We regress the answer to each of the questions making the labor index on the economic measures. We control for individual-level controls, year Fixed Effects, and passage of the reform. We cluster at the kibbutz level. Our sample includes only people who were in the kibbutz during the crisis and are older than 20.

	(1) Free Labor Market Index	(2) Trust in Leadership Index	(3) Free Labor Market Index	(4) Trust in Leadership Index	(5) Free Labor Market Index	(6) Trust in Leadership Index
Panel A: Economic Strength						
Severe Crisis	0.296***	-0.260***	0.191**	-0.118	0.336***	-0.289***
	(0.066)	(0.074)	(0.075)	(0.079)	(0.073)	(0.087)
Moderate Crisis	0.149***	-0.170***	0.103*	-0.080	0.167***	-0.184***
	(0.056)	(0.061)	(0.058)	(0.064)	(0.060)	(0.070)
Mild Crisis	0.031	-0.022	-0.025	0.028	0.038	-0.047
	(0.073)	(0.074)	(0.076)	(0.077)	(0.076)	(0.080)
Observations	16,030	14,138	12,238	11,264	9,265	8,653
Panel B: Credit Rating						
Severe Crisis	0.301***	-0.256***	0.208**	-0.122	0.303***	-0.292***
	(0.094)	(0.089)	(0.093)	(0.090)	(0.092)	(0.089)
Moderate Crisis	0.160*	-0.174**	0.128	-0.088	0.184**	-0.202**
	(0.085)	(0.081)	(0.083)	(0.082)	(0.081)	(0.078)
Mild Crisis	0.087	-0.098	0.056	-0.060	0.078	-0.128
	(0.088)	(0.077)	(0.090)	(0.083)	(0.086)	(0.079)
Observations	16,030	14,138	12,238	11,264	9,265	8,653
Panel C: Survey Based						
Severe Crisis	0.305***	-0.324***	0.268***	-0.212	0.332***	-0.342***
	(0.053)	(0.055)	(0.057)	(0.063)	(0.056)	(0.061)
Moderate Crisis	0.163***	-0.240***	0.120*	-0.149**	0.197***	-0.276***
	(0.059)	(0.054)	(0.064)	(0.061)	(0.061)	(0.059)
Mild Crisis	0.107*	-0.180***	0.105	-0.130**	0.129*	-0.208***
	(0.063)	(0.059)	(0.066)	(0.064)	(0.068)	(0.063)
Observations	17,166	15,145	13,079	12,056	9,837	9,198

Table A15: Various Robustness Checks for the Effect of the Crisis on the Indices

We regress the Labor and Trust indices on the measures of the crisis. We control for individual-level controls, year Fixed Effects, and passage of the reform. We cluster at the kibbutz level. In columns (1)-(2) we control for the kibbutz's size at 1983. In columns (3)-(4) we keep only observations after 1996. In column (5)-(6) we focus only on people with no academic degree. Our sample includes only people who were in the kibbutz during the crisis and are older than 20.

Election Year	# Kibbutzim	# Voters	% Turnout	% Left	% Center	% Right
Reformed						
1996	3	232	79.06	79.91	9.97	9.15
1999	40	307	74.96	76.41	8.94	5.26
2003	124	373	70.94	71.92	11.37	10.52
2006	163	401	66.03	61.51	20.99	5.27
2009	179	431	67.426	50.13	34.00	11.02
2013	188	474	70.90	57.78	26.85	9.54
Unreformed						
1996	229	399	80.54	89.26	4.31	4.90
1999	192	422	75.68	86.27	5.35	3.05
2003	108	457	71.73	77.49	8.63	7.85
2006	69	490	67.28	67.77	18.34	4.12
2009	53	500	67.36	52.93	33.15	8.97
2013	44	538	68.60	64.29	22.42	7.92

Table A16: Descriptive Electoral Statistics for Reformed and Unreformed, By Voting Year

This table presents statistics of the sample by voting year. The sample includes all Takam and Artzi Kibbutzim that had voting polls in each on the 6 elections between 1996-2013. Kibbutzim are considered Reformed starting from the year after the reform (year since reform, =1). Other then the "Kibbutzim" column, which depicts for every year how many Kibbutzim were in each sample, all other statistics describe average figure per Kibbutz in each sample.

	Deliberation Balance			**Implementation Balance**		
	Coeff	SE	Obs	Coeff	SE	Obs
Artzi	-0.116	(0.130)	2,433	-0.019	(0.117)	2,825
Age	0.647	(0.99)	2,433	-1.168	(0.945)	2,825
Gender	-0.024	(0.034)	2,432	-0.008	(0.032)	2,821
Schooling	-0.033	(0.027)	2,433	0.036	(0.026)	2,825
Kibbutz's Economic Status	0.051	(0.164)	2,421	0.076	(0.165)	2,808
Paying for Overtime	0.038	(0.140)	2,162	0.133	(0.130)	2,499
Differential wages	0.034	(0.141)	1,634	0.063	(0.135)	1,854
Reduce Pay for Underworking	0.105	(0.139)	1,320	-0.025	(0.135)	1,507
Trust Social Leadership	0.042	(0.098)	1,631	-0.031	(0.095)	1,853
Trust Economic Leadership	-0.047	(0.118)	1,638	0.098	(0.115)	1,856

Table A17: Deliberation and Implementation Treatments are Balanced

We regress each one of the variables mentioned in the rows on the Treatment variable for the survey years until 1996. In columns (1)-(3) treated kibbutzim started deliberations just before an election, and control kibbutzim started deliberation just after elections. In columns (4)-(6) treated kibbutzim are those reformed just before elections, while control kibbutzim reformed just after elections.

	% Turnout	% Left	% Center	% Right
Without 96 Elections				
Treatment	1.790	-5.940	2.381	1.845
	(1.638)	(1.688)	(0.813)	(0.845)
Observations	107	107	107	107
Without 96-99 Elections				
Treatment	2.647	-4.428	1.749	1.853
	(1.886)	(1.905)	(1.081)	(1.058)
Observations	69	69	69	69
Without 2013 Elections				
Treatment	1.629	-5.785	2.084	2.023
	(1.577)	(1.626)	(0.831)	(0.826)
Observations	118	118	118	118
2 Year Window				
Treatment	-0.832	-3.383	1.185	1.030
	(0.895)	(1.152)	(0.602)	(0.599)
Observations	242	242	242	242

Table A18: Effect of Treatment on Voting, Additional Tests

We define treatment to be kibbutzim that reformed just before elections, and control to be kibbutzim that reformed just after elections. We regress on treatment, voting turnout, percent of cast to the left, center and the right. In panel A we included all kibbutzim to reform a year before or after an election. In panel B we drop kibbutzim that reformed in 1995 and 1997. In panel C we also drop kibbutzim that reformed in 1998 and 2000. In panel D we include all kibbutzim, but those reformed at 2012 and 2014. In Panel E we broaden the window around elections, and take all kibbutzim that reformed up to two years before or after an election. We always control for affiliation with Artzi, and a full set of year dummies.

	% Turnout	% Left	% Center	% Right
Backwards Placebo				
Treatment	-0.0318	-1.340	0.428	0.938
	(1.586)	(1.259)	(0.601)	(0.682)
Observations	107	107	107	107
Forward Placebo				
Treatment	1.431	-2.250	0.310	1.563
	(1.545)	(1.995)	(1.030)	(1.139)
	118	118	118	118
Placebo at 1996				
Treatment	1.037	-0.243	0.143	0.376
	(1.465)	(0.958)	(0.552)	(0.680)
Observations	107	107	107	107
Placebo at 1996-1999				
Treatment	2.540	0.264	-0.201	0.453
	(1.821)	(1.327)	(0.697)	(0.537)
Observations	136	136	136	136
Placebo at 2013				
Treatment	1.296	0.264	-0.276	-0.0668
	(1.285)	(1.691)	(1.160)	(1.026)
Observations	118	118	118	118

Table A19: Reform on Voting: Placebo Examinations

In this table we perform several placebo excercises for the first estimation strategy. In panel A define treatment as kibbutzim that reform just before an election, and control as kibbutzim that reformed a year after elections, but then we sample them one elections before the actual election they reformed in proximation to. In panel B we do a similiar excercise, only sampling kibbuzim one election after the true election. In panel C we take kibbutzim that reformed starting from 1998, and regress them only on 1996 elections. In panel D take kibbutzim that reformed starting from 2002 and regress them only on 1996-1999 elections. Finally, in panel E we take kibbutzim that reformed until 2010, and regress them on 2013 elections. We always control for affiliation with Artzi, and a full set of year dummies.