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# Economic Crisis and Disillusionment from Socialism: 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 where 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. We document that the electoral movement was rooted in a rightward shift in economic attitudes, suggesting that economic crises may undermine socialist regimes by silently changing attitudes toward them. In our unique setting, we can also study recovery mechanisms 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 attitudes. 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.

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*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 on the experience of market economies with limited attention paid to socialist economic systems. Yet, 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), such systems might respond differently to economic crises. Furthermore, existing studies center chiefly on the crisis period without bringing much evidence of the post-crisis period and recovery mechanisms. In this paper, we contribute to the literature in two directions. First, we study the impact of an economic crisis in a socialist system by exploiting rich data on political attitudes. Second, our case study allows us to follow the socialist system from the beginning to the recovery from the crisis by signing a debt relief agreement and the liberalization of labor markets. We further exploit this transition away from socialism to study how institutional transformations feed back into attitudes (Acemoglu and Robinson, 2021; Acemoglu et al., 2021).

Studying economic crises in socialist settings is important, given that many socialist countries have endured harsh economic downfalls. 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.

[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 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) and Giuliano and Spilimbergo (2023a).

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] Overall, we calculate the ratio between votes cast to non-left parties and votes cast to left parties increased by 18 percentage points in kibbutzim that were more severely hit by the crisis. Furthermore, the crisis' effect persisted for more than two decades after it was over. These findings are especially striking given that kibbutzim traditionally overwhelmingly supported leftist parties, with over 90% of the votes cast to these parties before the crisis. The ideology of leftist parties resonated 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 may indicate a decline in support for the socialist ideology and an increase in support for a more market economy. The shift in voting might directly reflect voting based on changing economic considerations. An alternative interpretation is that it may have allowed kibbutzim members to vote on different dimensions, such as security. A third possibility is that the transition away from leftists parties reflected loss of trust in the national left leaders. Our evidence aligns most closely with the first interpretation, but we cannot completely rule out the other two.

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 empirical challenge here is that the surveys were only administered from 1989 after the crisis had already 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 continued 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 amenable to

[2]This finding aligns with some anecdotal evidence from the kibbutzim. See Tabakoff (2019). We focus on national elections because these are the most important elections in Israel. Unlike the American system, local representatives have limited authority, and often their candidacy is less ideological. Furthermore, data constraints make the analysis of local elections far more challenging for our period of interest.

[3]Throughout this work, we use the term "attitudes" to capture people's opinions regarding economic and political issues. These may be driven both by preferences and beliefs. See Giuliano and Spilimbergo (2023a).

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 that finds that 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] 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 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 trust is an unlikely candidate for explaining the entire rightward shift in economic preferences, given that the effect of the crisis on redistribution persisted for over 20 years.

A second possibility, which we consider the most plausible, is that kibbutzim members became disillusioned with socialism after its failure. In their eyes, the crisis proved that an egalitarian economic system is unsustainable and justifies a transition to a market-based economy. A piece of evidence supporting this interpretation is that exposure to a successful market-based regime reinforced the transition away from socialism.

Following the crisis, starting from the end of the 1990s, many kibbutzim shifted from social-

[4]Other 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). Yet, 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). 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.

ist 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 (Abramitzky and Lavy, 2014), allowing us to identify the reform's causal impact. 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 deliberations and 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. Doing a back on the envelope calculation, we find that the reform explains 11%-16% of the crisis' effect, while the rest is attributed to direct disillusionment from socialism.

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

[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).

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.

The relative advantage of our setting is the high internal validity of the estimates, but our findings might also have implications beyond the Israeli context. While kibbutzim are unique societies that exists only in Israel, attempts to create more just societies have existed throughout history, and our findings might shed light on how economic preferences and political attitudes have evolved in these understudied communities.[6] Furthermore, over the last several decades, countries like Russia and China have shifted away from socialism following economic shocks. Since Russia and China are not democracies and lack transparency, it is hard to determine the channel through which the economic crisis impacted political attitudes. Specifically, it is difficult to distinguish whether socialism declined because of a decline in popular support, a central government decision to stop enforcing it, or an erosion of state capacity that made it impossible for the central government to enforce. The democratic decision-making in kibbutzim, together with the substantial economic shock that varied across kibbutzim and the rich data on a range of political outcomes, allow us to shed light on the popular support mechanism.

We stress that unlike these regimes, kibbutzim were voluntary communities embedded in in a democratic market-society. Members did not only elect their local and national leaders, but could exit the kibbutzim if so desired. While the uniqueness of the kibbutzim makes direct inference to communist regimes harder, we think the effect we document might actually be *smaller* compared to similar events happening in non-voluntary regimes. We postulate that resentment toward the economic system might be greater when it is forced upon the citizens, and that the benefits from a transition to free market might be greater. Regardless, kibbutzim are a rare natural experiment in voluntary socialism that allows us to understand better what citizens choose when they are free to vote and leave rather than coerced to stay. So while any inference must be made with caution, we believe our results are informative for understanding the effect of crises on attitude in socialist systems.

Although 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

[6]Kibbutzim were not the only type of communes that existed in world history. The kibbutzim were inspired by thinking originating in the Soviet Union and Eastern Europe; rural collective communes influenced the kibbutzim income sharing and other features. Additionally, settlements in rural China operated as communes for decades. Like the kibbutzim, they not only shared production and assets but also put less emphasis on familial ties and facilitated shared kitchens (Qian, 2008; Meng et al., 2015) The shift from collectivism in these communes was also triggered by a crisis—the Chinese famine—though it was different. Despite the shift away from collectivism, rural communes still exist today in East Asia, in countries such as China, Japan, and Vietnam (Nathan and Kelkar, 2000) In Europe and North America, religious and socialist communes have always existed, and many of them faced similar economic trade-offs as did the kibbutzim (Abramitzky, 2018).

US and worldwide. When analyzing the ideological transition of the kibbutzim, we conclude that although most kibbutzim members support the reform of liberalizing labor markets, they still want to maintain the principle of mutual guarantee. 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. 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 persistence mechanisms 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 from socialism.

The paper is organized as follows. In part 2, we briefly summarize the historical background of our context. We outline both the institutional structure of the kibbutzim, and describe the Israeli national political system. We refer the reader to Abramitzky (2018) for a detailed discussion of kibbutzim and their history. Part 3 discusses the various datasets we use. Part 4 makes the argument that the crisis' severity was plausibly orthogonal to the pre-crisis attitudes at the kibbutz level. In part 5, we combine a difference-in-differences strategy with high frequency data on voting to show that the crisis induced kibbutz members to shift rightward in their national election voting. In part 6, we study possible mechanisms underlying this transition. In particular, we investigate the crisis' impact on economic and political attitudes, elicited by surveys. We show that the crisis had an effect on rightward shifts in attitudes, and that this effect was above and beyond the effect of a subsequent labor reform. We next move to study the effects on attitudes of the recovery from the financial crisis. We start by examining the role of the debt arrangements between kibbutzim, the major banks, and the Israeli government designed to help kibbutzim repay their debts and recover from the financial crisis. In part 7, we study the effects of a labor reform that liberalized kibbutzim's labor markets on political attitudes and behavior. We show that this institutional change further exacerbated the crisis' impact by further shifting kibbutz members' attitudes to the right. 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, mostly by migrants from Russia and Eastern Europe, who were influenced by the socialist ideology. Today, there are 265 kibbutzim located all over Israel. The number of members in these kibbutzim amounted in 2023 to 200,000 members, or 2% of the Jewish population in Israel (Ibid).

Throughout 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 the severity of the crisis (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, which was applied 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, which started 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 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 the lifestyle of kibbutzim members, and it required an overwhelming majority of the members to implement it (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.[7]

## 2.2 Israel's Political System

Since one of our outcomes 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 directly vote for the prime minister but instead for the Knesset (the Israeli Parliament).[8] The voting for the Knesset takes place in a multi-party system. Over a dozen parties contend for the 120 legislative seats every election; usually, more than ten win representation. Thus, the Israeli parties' map changes between elections, as some parties are not reelected, and other new parties

[7]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).

[8]There were two elections, in 1996 and 1999, in which voters had two tickets, one for a political party and one directly to the prime minister. During this time, Israel examined implementing reforms in its electoral system but overturned its decision once it was evident these were unsatisfactory. In these elections, we only consider voting for political parties to maintain consistency across time.

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 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. To this day, kibbutzim see the right's rise to power as at least partially responsible for their decline.

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.

We emphasize that while some parties on the right are not market oriented (e.g. *Shas*, that rarely received votes from kibbutzim members, who are mostly secular and Ashkenazim), the major rightist party, the *Likud*, has generally been more pro-markets than the major leftist parties – The Labor party and *Ratz* (later becoming *Meretz*). This has been especially true under the Menachem Begin and Benjamin Netanyahu administrations, covering crucial periods before and after the financial crisis. Moreover, we note that we also find electoral transitions among kibbutzim members from left parties to center parties. The latter parties are similar to the left in terms of security and civil rights dimensions, but they are less supportive of the welfare state.

# 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 Israeli government constructed the first measure, Economic Strength, 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.[9] We use this measure in continuous and discrete forms. The latter is calculated by splitting the sample into quartiles.

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, because 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 (CBS, 1972, 1983) . 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 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

[9]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.

identifier and election poll identifiers, the number of eligible voters, and the number of votes cast for each political party (Knesset, 2021).

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 whether these years should be included in the post-sample. 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 149 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.[10] 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, which is 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

[10]To be specific, we categorize the left as the Labor Party (under its different names) and the *Meretz* Party (and its predecessor *Ratz*), the party *Am Ehad* (which split from Labor in 1999 and united with it again after 2003), *Sheli* party, and all Arabs parties. In the center, we include The Third Way, The Center Party, *Shinui*, *Kadima*, Independent Liberals, *Yachad* and *Yesh Atid* (none of which won seats for more than three elections during our period of interest). Finally, on the right, we include the *Likud* party (which won seats every year), *Israel Beiteinu*, *Moledet*, *Tehiya*, *Ometz*, *Kach*, *Morasha*, *Tzomet*, and all strictly religious Jewish parties, including ultra-orthodox parties. We exclude Israel Be'aliya, which won seats in 1996-2003. This party was affiliated with the right, but it merged capitalist and socialist economic ideologies, it was also highly sectorial, and its electorate consisted almost exclusively of immigrants from the Soviet Union. We also exclude the Gil party that won seats in 2006. Gil was an outlier in Israel's politics. Its main agenda was advocating for senior citizens' rights, and the voting for it was later by and large identified as an act of protest. Therefore, deriving political affiliation from voting for Gil is without much merit. We also remove Flatto Sharon's party, which revolved around one individual with no clear ideology, and the Tami party, which mixed leftist and rightist ideas.

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 (IRKCI, n.d.). 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 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 for having joint ownership of assets and 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. Figure A2 shows 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 find 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 the left – for the different groups between 1977 and 1984. We do so for our main measures of the crisis: the Economic Strength and the Credit Rating. For readability, we group kibbutzim with severe or moderate crisis in one group and kibbutzim with mild or no crisis in another group. As seen in Figure A4, the baseline and the trends are highly similar across these two groups. All of the differences are much smaller than one standard deviation of the means.

To reinforce this finding further, we will also look at voting by party before the crisis. We do this only for the Zionist left camp, which leaves us with the Havoda party (Labor party) and the Ratz party. Because there was hardly any vote outside the leftist party before the crisis, looking at the within-left variation is more demanding and meaningful. In Table A5, we regress voting for each party on the economic measures during the pre-period for each year separately. As can be seen, the results are well balanced. The only caveat is that in 1984, there seemed to be less votes for *Havoda* than for the *Ratz* in the mild crisis group. However, since the omitted group is the group that did not experience any crisis, we do not find this problematic. As we report 54 coefficients, we will get some statistically significant results even with perfect balancing.

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 examine further 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-movements 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 regress affiliation with Meuchad on the intensity of the crisis within the Takam movement. As shown in column (2), similar to the results concerning the affiliation with Artzi, we find null coefficients (one coefficient, in the survey measure, is significant at the 10% level). 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 in 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. 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. The exception is group 4 in the credit rating measure. 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 connected with their ideology's strength. 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' intensity, it would have to be orthogonal to the correlations that we have examined thus far. We find it 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 separately regress the support for the left and the right on this dummy variable for each year.

To rule out that the results simply reflect a return towards the national average, we flexibly control for the 1977 baseline voting, interacted with the complete set of year indicators. When estimating the effect of the crisis on voting for the left (right), we control for baseline voting for the left (right).

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, using both its continuous and discrete forms. 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:

$$V_{ke} = \gamma_k + \mu_e + \rho_e \cdot \text{Baseline}_k^V \cdot \text{Election}_e + \sum_{i=l}^{4} \beta_i \cdot \text{Crisis}_{lk} \cdot \text{Post}_e + \epsilon_{ke} \tag{1}$$

Where $V_{ke}$ is a political outcome of interest at the kibbutz $k$ and election year $e$ level. Political outcomes include voter turnout, the percentage of voters casting their votes to the left, center, and right, or the ratio of votes cast for different camps. $\gamma_k$ is a fixed effect for the kibbutz $k$, and $\mu_e$ is fixed effect for the election year $e$. $\text{Crisis}_{lk}$ is an indicator that is 1 if kibbutz $k$ is in group $l$ according to the severity's measure used and, 0 otherwise. The omitted group is 4, the group for which the crisis hit the least severely. $\text{Post}_e$ is 1 if the election is after 1985 and 0 otherwise. The coefficients of interest are $\beta_1$, $\beta_2$, and $\beta_3$, which capture the effect of having a harder crisis in the post-crisis period. We always flexibly control for the kibbutz's Baseline value of outcome $V$ in year 1977, $\text{Baseline}_k^V$, interacted with the set of election year dummies, $\text{Election}_e$.

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.

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 5th column, we represent estimates from a regression in which the outcome variable is the ratio of the sum of the votes to the center and the right to the votes to the left. This ratio enables us to capture concisely the electoral shift away from the left. The results indicate that most of our effect is driven by the most hit kibbutzim. Still, to the extent that part of our effect is driven by people moving from the center to the right, this measure will under-represent the true effect of the crisis. In the 6th column, we replace the dependent variable with the ratio of votes to the right and votes to left.

Focusing on kibbutzim which were hit the most, our benchmark estimate is that compared to the counterfactual, there was a decrease of 4.7-6.0 percentage points in their vote for leftist parties. On the face of it, this seems like a modest change compared to the baseline of 90% of votes for the left. However this high baseline indicates how strong leftist ideology was in the kibbutzim before the reform. In this sense, it is striking that the crisis was forceful enough to shift voting to the right, even in communities with such strong allegiance to the left. To illustrate this point, consider the voting for the right. Our estimates range from 2.5 to 3.0 compared to a baseline of 3.0. This means that support for the right roughly doubled following the crisis.

We ensure our results hold when using the survey-based measure in Table A7. In panels 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. We enter the measure discretely in Panels A and C, and in Panels B and D, we do so continuously. The results are consistent 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 has ever been built in them. While in some specifications we lose some statistical power due to a major reduction in the sample size, the results are consistent and remain significant. 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.[11]

One concern about our interpretation of the results is that left and right parties in Israel differ not only in their economic ideology but also in additional dimensions. 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 it could be that kibbutzim members changed their attitudes in this dimension. To examine this, we use a survey question eliciting attitudes toward the conflict that was asked in 2002-2005. The question asks how likely the Israelis and Palestinians will be able to reach an agreement. The right and the left are starkly divided on this issue, with the left much more optimistic than the right. As shown in Table A11, there is a null correlation between the crisis intensity and the average answer to this question at the kibbutz level, indicating that the crisis did not alter beliefs about the conflict.

One interpretation for the rightward shift in voting is that kibbutz members started to lose faith is socialist values. Alternatively, it is possible that as kibbutz members' support towards socialist values declined (as we show in survey data), they became more likely to vote for the parties that best represented their preferences on other topics. For example, some members might have been more skeptical about the peace process and would have voted for a right-wing party, but they did not because they prioritized their support for socialist values. Once support for socialist values

[11] As Abramitzky (2018) quotes Daniel Givron: "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."

declined, members started to vote to the right based on their other priories.

To examine this idea, we replicated the analysis of the effect of the crisis on voting in Table 1, splitting the sample to kibbutzim above and below the median in their optimism regarding the peace process. We emphasize that while the crisis occurred in the late 1980s, the question we use has been asked in the early 2000s. For this reason, the interpretation of this exercise can merely be suggestive due to the endogeneity of the variable by which we perform the heterogeneity test. The evidence is mixed. When using one of our measures of crisis ("economic strength"), we find that the effect of the crisis on voting is stronger, although not statistically significantly so, for kibbutzim that were less optimistic about the peace process. However, the effects when using other crisis measures are not different by pessimism towards peace process. Since our findings are inconclusive, more research is needed to understand the exact mechanisms behind the rightward shift in voting following the crisis.

Overall, according to our benchmark specification, experiencing a severe crisis increases the share of right votes by 2.5 pp, compared to a baseline of 3.2 pp. Hence support of the right in kibbutzim almost doubled. Yet, this calculation might overestimate the magnitude of the effect because there was a general rightward shift over time. The mean of the percent votes of the kibbutzim to the right across the entire period we study is 5.2, indicating a marked increased post-crisis. When this is taken as the benchmark, the severe crisis roughly increased the support for the right by 50%.

To give that comparison of the magnitude of this effect, consider the national transition from leftist coalition to rightist coalition in Israel following the 1973 Yom Kippur War. In 1973, the Leftist parties had 57 seats in the Knesset (the Israeli Parliament). In 1977, the left camp had only 39 seats. In contrast the right had 54 seats in 1973, and won 62 seats in 1977. Overall, taking the ratio of right to left, the change in ratio was from 0.94 to 1.59, which is roughly equivalent to the conservative calculation of the magnitude of the effect we document.

## 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 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 A12, we add to our 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 reform's effect, the crisis coefficients 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.[12] The difference between severely hit kibbutzim and kibbutzim with no crisis is almost 0.5 SDs. 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.[13] The difference between severely hit kibbutzim and kibbutzim with no crisis is almost 0.3 SDs. 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 controls, including affiliation with the Artzi movement. We also restricted the sample to individuals in the kibbutzim during the crisis and clustered 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 exercise, 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

[12] This is consistent historical analysis 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)

[13] "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)

few years. In comparison, we saw that the crisis' 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. There is solid theoretical grounds to believe these differences might vary with age. So, 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 their age is in the relevant range during the crisis (1987-1996). 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 tested robustness by 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. Figure 4 shows 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.

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 movement while clustering at the kibbutz level. Consistent with evidence in the literature, the age group 0-14 results are null in both the short and long term. This suggests that the crisis did not affect young children. Panel A shows that the short- term coefficient is positive and significant in all other age groups. However, the long-term 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, persistence was null for members older than 45 during the crisis. 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. In the next subsection, we explain why the effect on trust eroded over time. Overall, we conclude that it is likely that the persistent change in political behavior is at least partly rooted in the ideological transformation. As suggestive evidence reinforcing this interpretation, we present in Figure A8 a scatter diagram of the average voting to left parties after 1990, and the average kibbutz-level Free Labor Market Index. A strong negative correlation (Pearson is equal roughly to -0.4) emerges.

One possibility is that changes in trust could explain changes in voting. Kibbutzim leadership and the leadership of left-wing parties could have been perceived as intertwined, in which case

members may have stopped voting to the left because they lost trust in the leadership of the left. However, while we showed the effect of the crisis on trust in leadership is short-lived, the effect on voting is long-lasting. Therefore, we do not think that changes in trust toward the kibbutzim leadership can explain the long-lasting changes in the voting behavior. Still, we cannot completely rule out the possibility that the financial crisis in kibbutzim was perceived as (at least partly) the left parties' failure and respondents' attitudes turned against them.

### 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 excluding observations during the crisis. In practice, we drop the observations from the 1989-1996 surveys. We do this because our measures were calculated up until 1994-1996, so we want to take only observations after them. We present the results based on this sample in the online appendix Table A15, columns (3)-(4). We lose statistical significance for the trust in leadership index, but the coefficient for the free labor market index remains significant, though it is smaller in magnitude. The direction of the coefficients is unchanged. This is consistent with the crisis' effect on trust being merely transitory, while being persistent for economic attitudes.

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. We underscore that emigration will bias our results towards zero if once a kibbutz is hit by the crisis, people most opposing socialism will leave it.

Still, to examine whether this is a concern, we rely on the result of Abramitzky (2008), 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 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 way 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 at the individual level:

$$Y_{ikt} = \gamma_k + \tau_t + \mathbf{X}_i\theta + \phi \cdot \text{Reformed}_{kt} + \sum_{j=-6}^{j=6} \pi_j \cdot \text{Period}_{ktj} + \epsilon_{ikt} \tag{2}$$

Where $Y_{ikt}$ is a survey outcome (Free Labor Market Index or Trust In Leadership Index) of individual $i$ in kibbutz $k$ at time $t$. $\gamma_k$ and $\tau_t$ are fixed effects for kibbutz $k$ and year $t$. $Period_{ktj}$ is a dummy variable that is equal to 1 if the year $t$ minus the debt settlement year of kibbutz $k$ 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. $\mathbf{X}_i$ is a vector of controls at the individual $i$ level, including sex, survey year and birth year. $\text{Reformed}_{kt}$ is a indicator of whether kibbutz $k$ has already been reformed in year $t$.

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. The effect accumulates to about 0.2 SDs. 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 A9.

The results reported in Figures 5 and A9 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 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 estimation to causally identify the effect of the debt agreement.

Formally, we regress:

$$Y_{ikt} = \gamma_k + \tau_t + \mathbf{X}_i\theta + \phi \cdot \text{Reformed}_{kt} + \xi \cdot \text{Signed}_{kt} + \epsilon_{ikt} \tag{3}$$

Where $Y_{ikt}$ is a survey outcome (Free Labor Market Index or Trust In Leadership Index) of individual $i$ in kibbutz $k$ at time $t$. Again, $\gamma_k$ and $\tau_t$ are fixed effects for kibbutz $k$ and year $t$. $\mathbf{X}_i$ is set of controls at the individual level and $\text{Reformed}_{kt}$ controls for the passage of the reform at kibbutz $k$ in time $t$. The only new variable is $\text{Signed}_{kt}$, which is an indicator if the kibbutz $k$ signed a debt arrangement before time $t$. As can be seen in Table 2, the results from this estimation align with the results from the Event Studies.

## 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 severely 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 in Equation 2, only changing the horizons and considering periods compared to the *reform*, rather than the debt arrangement. To be precise we estimate:

$$Y_{ikt} = \gamma_k + \tau_t + \mathbf{X}_i\theta + \sum_{j=-5}^{j=7} \pi_j \cdot \text{Period}_{ktj} + \epsilon_{ikt} \tag{4}$$

Where $Y_{ikt}$ is a survey outcome of individual $i$ in kibbutz $k$ at time $t$. Again, $\gamma_k$ and $\tau_t$ are fixed effects for kibbutz $k$ and year $t$. $Period_{ktj}$ is a dummy variable that is equal to 1 if the year $t$ minus the year of the of the start of the *deliberation year* of kibbutz $k$ is equal to $j$, and 0 otherwise. $\mathbf{X}_i$ is a vector of controls at the individual $i$ level, including sex, survey year and birth year.

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 not include 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 effect of the deliberation from the implementation's effect on this outcome.

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.[14] 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.[15]

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.[16] 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 A10, we replicate our results, omitting kibbutzim that reformed before 2001. This ensures kibbutzim contribute to pre-treatment observations. In Figure A11 we replicate our results using the approach of Borusyak et al. (2021). Importantly, when applying the correction, we still do not observe any pre-trends in the event studies. Moreover, the results are identical qualitatively,

[14] 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".

[15] 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)

[16] Abramitzky (2018) 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."

though at times, we lose some accuracy. We underscore that the design of Borusyak et al. (2021) is conservative in 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 inference, are statistically significant and similar to the TWFE estimations.

Table 3 presents 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 that is both for deliberation and implementation. Formally:

$$Y_{ikt} = \gamma_k + \tau_t + \delta \cdot \text{Deliberation}_{kt} + \phi \cdot \text{Reformed}_{kt} + \mathbf{X}_i\theta + \epsilon_{ikt} \tag{5}$$

Where $Y_{ikt}$ is a survey outcome of individual $i$ in kibbutz $k$ at time $t$. Again, $\gamma_k$ and $\tau_t$ are fixed effects for kibbutz $k$ and year $t$, and $\mathbf{X}_i$ is a vector of controls at the individual $i$ level, including sex, survey year, and birth year. $\text{Deliberation}_{kt}$ is an indicator for the kibbutz $k$ being in the deliberation process during year $t$, and $\text{Reformed}_{kt}$ is an indicator that the kibbutz is after implementing the reform.

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 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.

### 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? Table A16, provides 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 84 kibbutzim that reformed precisely a year before or after one of the elections between 1996 and 2013 and also voted in 1977. We classify the 43 kibbutzim that reformed a year before an election as treated kibbutzim and the 41 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.

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 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:

$$V_{ke} = \alpha \cdot \text{Artzi}_k + \mu_e + \rho \cdot \text{Baseline}_k^V + \psi \cdot \text{Treatment}_{ke} + \epsilon_{ke} \tag{6}$$

Where $V_{ke}$ is some electoral outcome at the kibbutz $k$ and election year $e$ level; percent cast to a political camp or the voter turnout. $\text{Artzi}_k$ is an indicator if the kibbutz is part of the Artzi movement, $\mu_t$ is a series of election dummies, and the variable of interest $\text{Treatment}_{et}$ is equal to 1 if kibbutz $k$ reformed (or deliberated) just before election year $e$, and 0 if it reformed (or deliberated) just after election year $e$. We always flexibly control for the kibbutz's Baseline value of outcome $V$ in year 1977, $\text{Baseline}_k^V$.

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 6 estimation results. Panel A depicts the coefficient associated with treatment when treatment is defined as deliberation. Panel B does the same for implemen-

tation 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. Panels C and D replicate the results while omitting control for the Artzi variable.

In Table A18 we further investigate the effect of the implementation of the treatment. The tests in this table serve as robustness checks and as preparation for the placebo analysis, which is introduced later. The regression estimates in this table are based on the benchmark specifications with some sample modifications. In Panel A, we omit the kibbutzim 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.

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 results.

Overall, we estimate that the effect of having a severe crisis compared to no crisis is a reduction

of between 4.8-5.9 percent points in voting to the Left. We also estimate that such a crisis increases the chance of reforming by 14.5-17 percent points. Lastly, the estimated effect of the reform on voting left is -4.6 percent points. The effect through the reform explains between 0.667 and 0.78 pp of the crisis' effect, which amounts to 11.3%-16.25% of the overall effect of the crisis. We conclude that over 80% of the crisis' effect was driven by disillusion with socialism rather than experimentation with capitalistic policy.

# 8 Conclusions

Since the financial crisis of 2008, there has been great interest in uncovering the political and economic consequences of economic crises. However, the existing 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. We assembled and analyzed reliable micro-level data on political voting and economic attitudes in this context.

Using the fact that Israeli kibbutzim experienced economic crises of varying severity as a quasi-natural experiment, we study the 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 likely 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 setting, we also study the recovery from the crisis. 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 amenable to this development. The end of the crisis did not restore belief in socialism.

Our findings contribute to a generalized understanding of economic crises, suggesting that instead of universally pushing people leftward, they ignite opposition to the prevailing system. Our study also brings a unique perspective on developing socialist systems in the face of crises and market liberalization. As obtaining reliable political data in socialist regimes is difficult, our study

offers a unique glimpse into the underlying mechanisms linking economic downturns to shifts from socialism to capitalism. We present evidence that suggests socialism (at least when combined with voluntary participation) erodes in response to a decrease in public support and financial crises.

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# 9 Figures

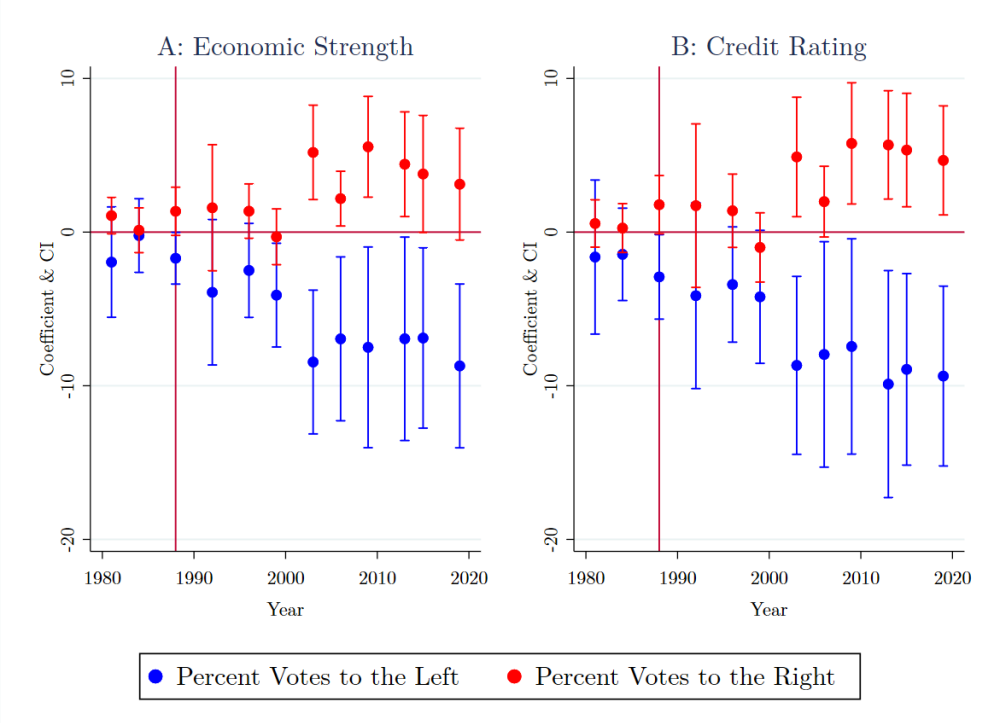
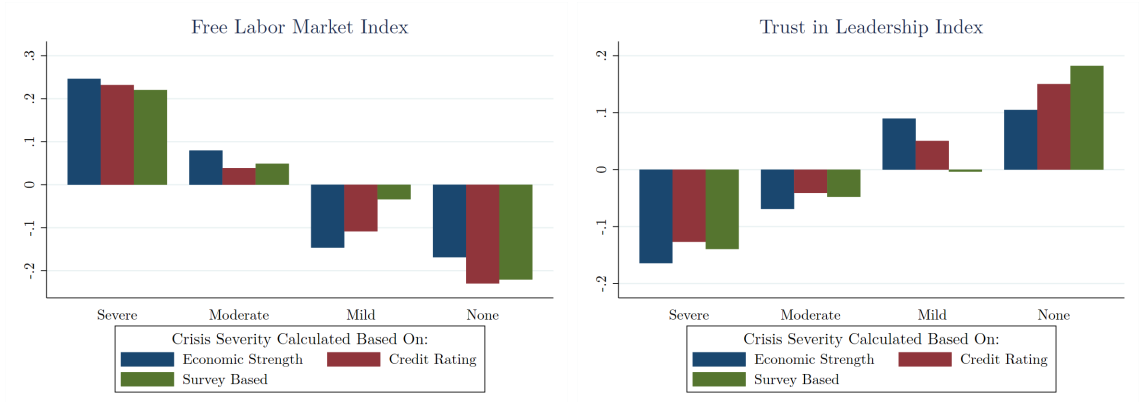



Figure 1: The Effect of The Crisis on Voting By the Two Measures

Notes: We estimate the crisis' impact on the percent of votes cast to the left and right in each kibbutz between 1981 and 2019. For each year separately, we regress these outcomes on the crisis' severity. To determine the crisis' severity, we use the Economic Strength measure in Panel A, and the Credit Rating measure in Panel B. The treatment group is kibbutzim severely hit (group 1), while the control group includes kibbutzim that were not hit (group 4). We always include as a control the percentage of the voting to the respective camp in 1977. In blue, we depict the coefficients and 95% CIs for voting left; 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.

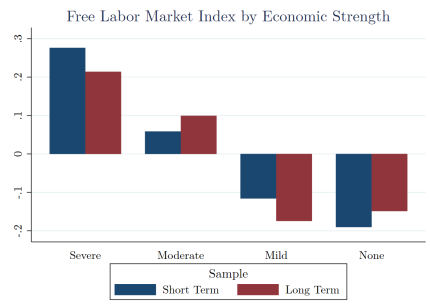



(a) Free Labor Market Index

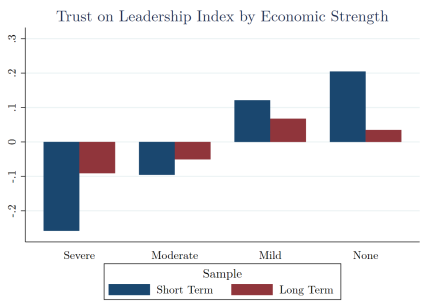
(b) Trust in Leadership Index

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

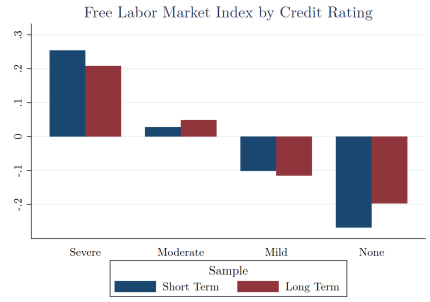
Notes: We stratify the sample into four groups according to all three discrete crisis measures. For each group, we calculate the average free labor market index and trust in leadership index. In the first panel, we plot the free labor market index by the values of the crisis' measures. In the second panel, we do the same for trust in leadership index. The blue columns depict the results according to the Economic Strength measure, the red for the Credit Rating measure, and the green columns show for the Survey Based measure.



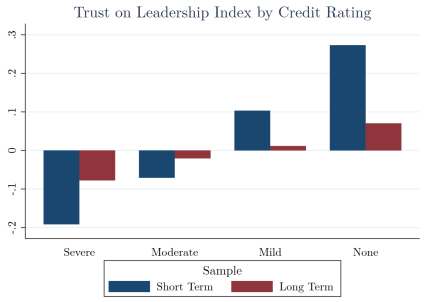

(a) Free Labor Market Index by Economic Strength




(b) Trust in Leadership Index by Economic Strength



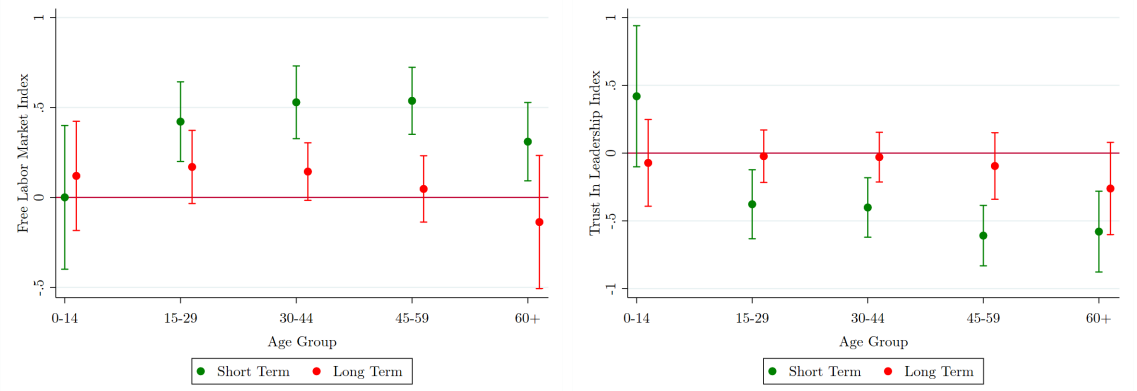

(c) Free Labor Market Index by Credit Rating




(d) Trust in Leadership Index by Credit Rating

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

Notes: In the two upper Panels, we stratify the sample into four groups according to the Economic Strength measure. For each group, we calculate the average free labor market and trust in leadership indices before and after 2001. In the two lower Panels, we do the same according to the credit 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.

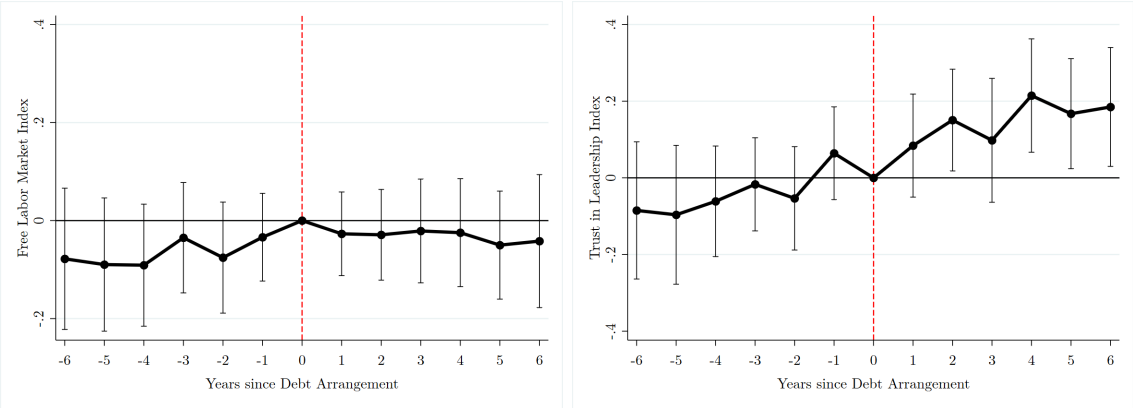



(a) Free Labor Markets Index

(b) Trust in Leadership Index

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

Notes: 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 groups and within each age group separately for 1991-2001 (green) and 2002-2018 (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 only keep individuals who lived in the kibbutz during the crisis. We cluster at the kibbutz level.




(a) Free Labor Market Index

(b) Trust in Leadership Index

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

Notes: We take the free labor market (left panel) and trust in leadership (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 depicts 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 at the kibbutz level. We include controls at the kibbutz and individual level. Our sample includes only people in the kibbutz during the crisis and older than 20 when surveyed.


(a) Free Labor Market Index

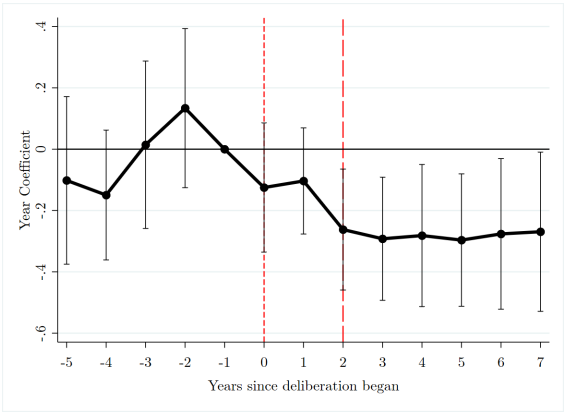
(b) Trust in Leadership Index

(c) Perceived Economic State of the Kibbutz

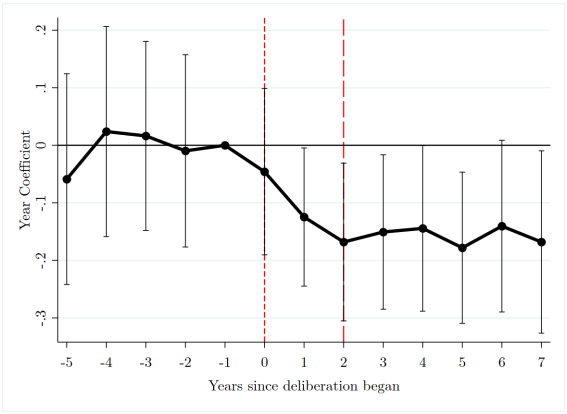
(d) Perceived Work Ethics in the Kibbutz

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

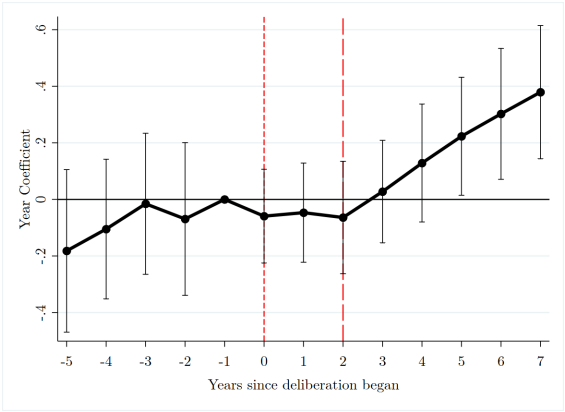
Notes: We estimate the effect of the deliberations and the implementation of reforms on various outcomes using a Two-Way-Fixed-Effect Event Study specification, as shown in Equation 4. Time $t = 0$ depicts the start of the deliberation, and implementation happens at $t = 2$. We include individual-level controls and cluster and the kibbutz level. We bring the 95% CIs. In Panel A, the outcome is the free labor market index; in Panel B, it is the trust in leadership 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.




(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

Nots: 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.

# 10 Tables

| | %Turnout | %Left | %Center | %Right | $\frac{Center+Right}{Left}$ | $\frac{Right}{Left}$ |
|---|---|---|---|---|---|---|
| **Economic Strength** | | | | | | |
| Severe Crisis | -0.633 | -4.892*** | 1.630* | 2.541*** | 0.179*** | 0.0709*** |
| | (1.146) | (1.790) | (0.905) | (0.851) | (0.0646) | (0.0264) |
| Moderate Crisis | -0.0743 | -1.746 | -0.0253 | 0.823 | 0.0382 | 0.0168 |
| | (0.947) | (1.277) | (0.684) | (0.530) | (0.0412) | (0.0139) |
| Mild Crisis | 0.624 | -0.508 | -0.593 | -0.202 | -0.00748 | -0.00476 |
| | (1.046) | (1.362) | (0.837) | (0.577) | (0.0432) | (0.0144) |
| Observations | 1,911 | 1,911 | 1,764 | 1,911 | 1,764 | 1,911 |
| **Credit Rating** | | | | | | |
| Severe Crisis | -1.153 | -5.970*** | 2.303** | 3.067*** | 0.210*** | 0.0836*** |
| | (1.524) | (1.775) | (1.093) | (0.741) | (0.0534) | (0.0195) |
| Moderate Crisis | -1.189 | -2.836* | 0.234 | 1.152** | 0.0554 | 0.0294*** |
| | (1.412) | (1.443) | (0.998) | (0.529) | (0.0397) | (0.00957) |
| Mild Crisis | -0.345 | -1.966 | 0.285 | 0.308 | 0.0299 | 0.0132 |
| | (1.418) | (1.513) | (1.066) | (0.573) | (0.0424) | (0.0113) |
| Observations | 1,911 | 1,911 | 1,764 | 1,911 | 1,764 | 1,911 |

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

Notes: We take as outcome variables the voter turnout and percent of votes cast to each political camp. In the two last columns, we construct kibbutz-election level ratios. Each observation is at the elections-kibbutz level, and the sample includes all elections from 1977 to 2019. We only keep 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 the severity of the crisis interacted with an indicator for the years 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; in Panel B, we use the Credit Rating. We always flexibly control for the baselevels on the examined outcome in 1977, interacting it with a full set of year dummies. We cluster standard errors at the kibbutz level.

| Severity of the crisis measured by: | **Free Labor Market Index** | | **Trust in Leadership Index** | |
|---|---|---|---|---|
| | Two-Way-Fixed-Effect | Borusyak et al. | Two-Way-Fixed-Effect | Borusyak et al. |
| **Economic Strength** | | | | |
| Debt Arrangement | -0.032 | -0.04 | 0.153*** | 0.134** |
| | (0.0471) | (0.06) | (0.0574) | (0.0621) |
| Observations | 8,906 | 8,852 | 7,797 | 7,746 |
| | | | | |
| **Credit Rating** | | | | |
| Debt Arrangement | -0.034 | -0.033 | 0.153*** | 0.122** |
| | (0.0465) | (0.0587) | (0.0538) | (0.0558) |
| Observations | 9,525 | 9,489 | 8,353 | 8,320 |

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

Notes: We include in the sample only kibbutzim that were in the first or second groups according to the economic measures (kibbutzim that were hit the hardest). In Panel A, we do it according to the Economic Strength measure, and in Panel B, we do it according to the Credit Rating measure. Within these samples, we estimate the effect of the debt arrangement on the free labor market and trust in leadership indices. In columns (1) and (3), we do so using the 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 standard errors at the kibbutz level and include individual-level controls.

| | 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 | 0.0755 | -0.0952 | -0.105 | -0.107* | -0.0778 | -0.183** | -0.183*** |
| | (0.0507) | (0.0731) | (0.0718) | (0.0621) | (0.0528) | (0.0913) | (0.0603) |
| Implementation | 0.253*** | -0.158** | -0.289*** | 0.00927 | -0.134** | -0.0733 | 0.138* |
| | (0.0590) | (0.0782) | (0.0790) | (0.0749) | (0.0531) | (0.112) | (0.0726) |
| Observations | 7,365 | 7,357 | 6,977 | 7,086 | 7,195 | 7,410 | 7,302 |
| **Reformed After 2001 (TWFE)** | | | | | | | |
| Deliberation | 0.0731 | -0.0975 | -0.104 | -0.0564 | -0.0844 | -0.162* | -0.155** |
| | (0.0521) | (0.0738) | (0.0738) | (0.0632) | (0.0539) | (0.0961) | (0.0627) |
| Implementation | 0.259*** | -0.167** | -0.301*** | 0.147* | -0.151** | -0.0342 | 0.202** |
| | (0.0661) | (0.0814) | (0.0870) | (0.0849) | (0.0588) | (0.129) | (0.0803) |
| Observations | 5,025 | 5,019 | 4,773 | 4,832 | 4,908 | 5,056 | 5,019 |
| **Borusyak et al.** | | | | | | | |
| Deliberation | 0.0814 | -0.097 | -0.1136 | 0.0601 | -0.0886 | -0.1065 | -0.1213* |
| | (0.0528) | (0.0797) | (0.0786) | (0.0806) | (0.0641) | (0.0863) | (0.0632) |
| Implementation | 0.270*** | -0.0884 | -0.2194** | 0.4325*** | -0.1245 | 0.2378* | 0.3799*** |
| | (0.0806) | (0.0982) | (0.111) | (0.1247) | (0.0841) | (0.124) | (0.0867) |
| Observations | 4,743 | 4,742 | 4,502 | 4,570 | 4,643 | 4,773 | 4,742 |

Table 3: Diff-in-Diff Estimation of the Reform's Effect on Attitudes

Notes: 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 exercise, dropping kibbutzim that reformed before 2001. In Panel C we reproduce the results using the design of Borusyak et al. (2021).

| | % Turnout | %Left | % Center | % Right |
|---|---|---|---|---|
| **Deliberation** | | | | |
| Treatment | 0.702 | 0.262 | 0.111 | -0.175 |
| | (1.153) | (1.280) | (0.610) | (0.776) |
| Observations | 85 | 85 | 85 | 85 |
| **Implementation** | | | | |
| Treatment | 1.818 | -4.598*** | 1.386 | 1.900** |
| | (1.461) | (1.524) | (0.922) | (0.841) |
| Observations | 84 | 84 | 84 | 84 |
| **Deliberation: No Artzi Control** | | | | |
| Treatment | 0.697 | 0.417 | 0.0352 | -0.154 |
| | (1.153) | (1.327) | (0.644) | (0.766) |
| Observations | 85 | 85 | 85 | 85 |
| **Implementation: No Artzi Control** | | | | |
| Treatment | 2.682* | -5.314*** | 1.735* | 2.108** |
| | (1.603) | (1.636) | (0.947) | (0.893) |
| Observations | 84 | 84 | 84 | 84 |

Table 4: The Reform Pushed Members Rightwards Electorally

Notes: We regress the voting turnout and the percent of votes cast to the left, center, and the right on our treatment (either deliberation or implementation) as shown in Equation 6. In panel A, we define treatment to be kibbutzim that deliberated about the reform just before elections, and control to be kibbutzim that started deliberating just after elections. We always control for affiliation with Artzi and for base levels of outcome in 1977. In Panel B, treated kibbutzim are kibbutzim that reformed one year before, and controls are kibbutzim that reformed one year after. Results in Panels C and D follow the same structure while omitting the control for affiliation with the Artzi movement.

# Online Appendices

## 11 Online Appendix: Figures

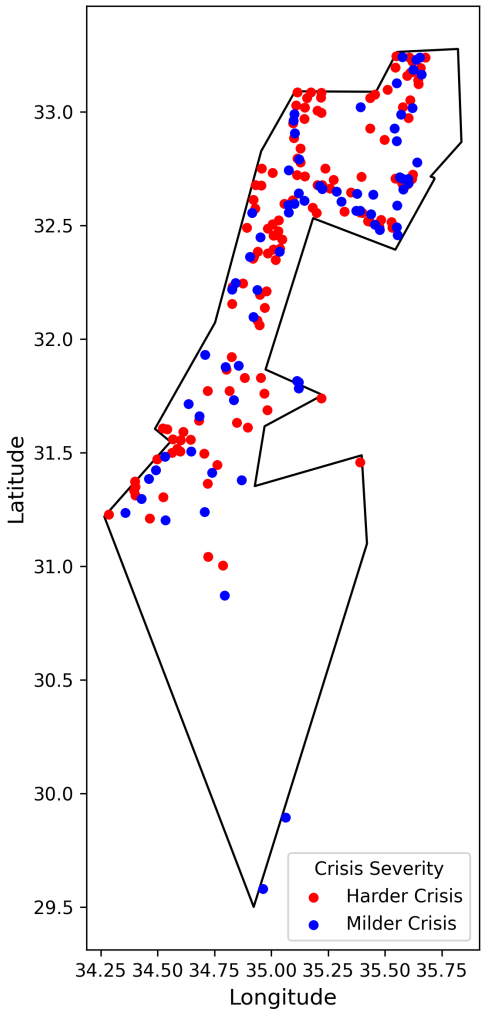



Figure A1: Map of Kibbutzim by Crisis Severity

Notes: 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 (groups 1 and 2). Blue points represent kibbutzim that experienced a milder crisis (groups 3 and 4).

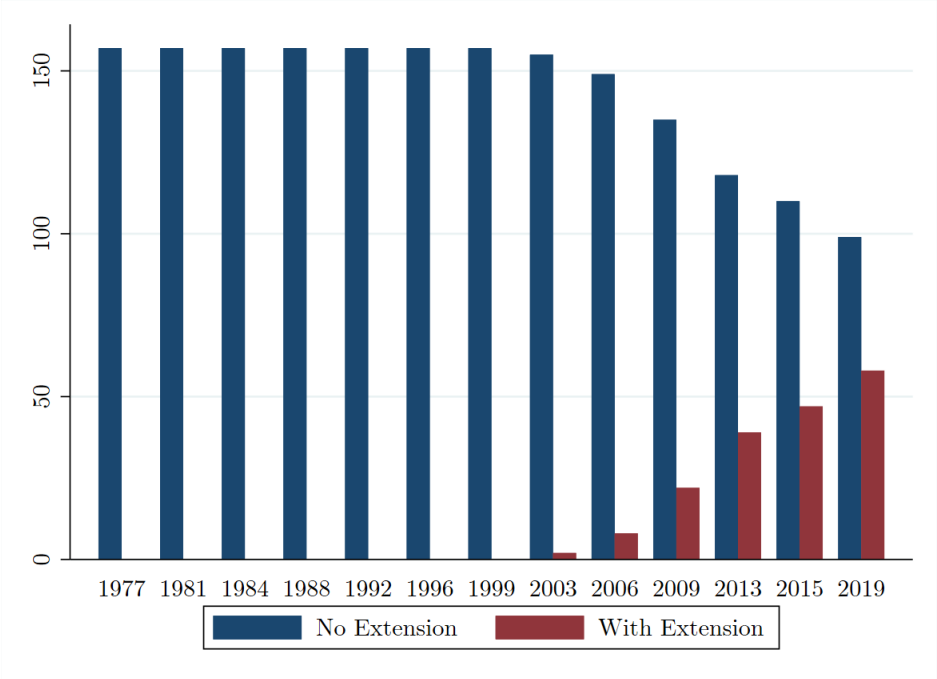
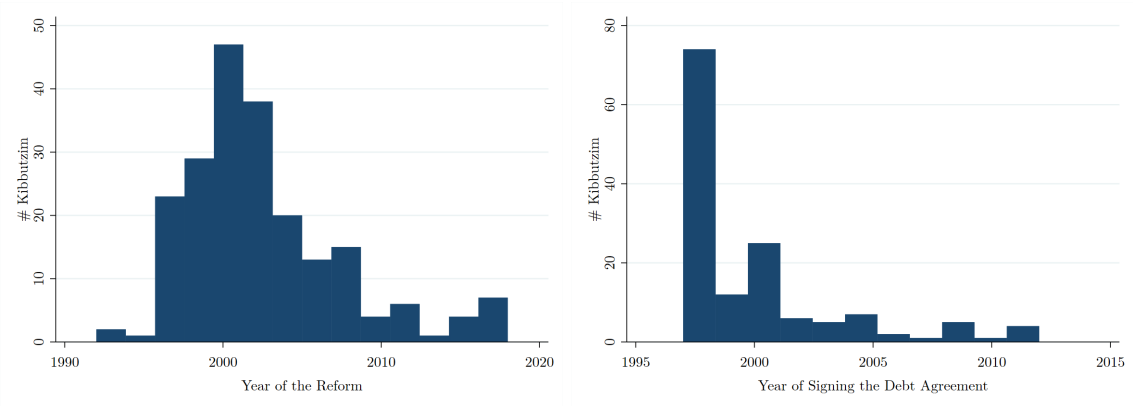



Figure A2: Numbers of Kibbutzim by Year and Existence of an Extension

Notes: 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 Timing

(b) Debt Arrangement Timing

Figure A3: Timing of Reforms in the Kibbutzim

Notes: We plot the distribution of the year of reform on the left and the year of the signing of the debt arrangement on the right.

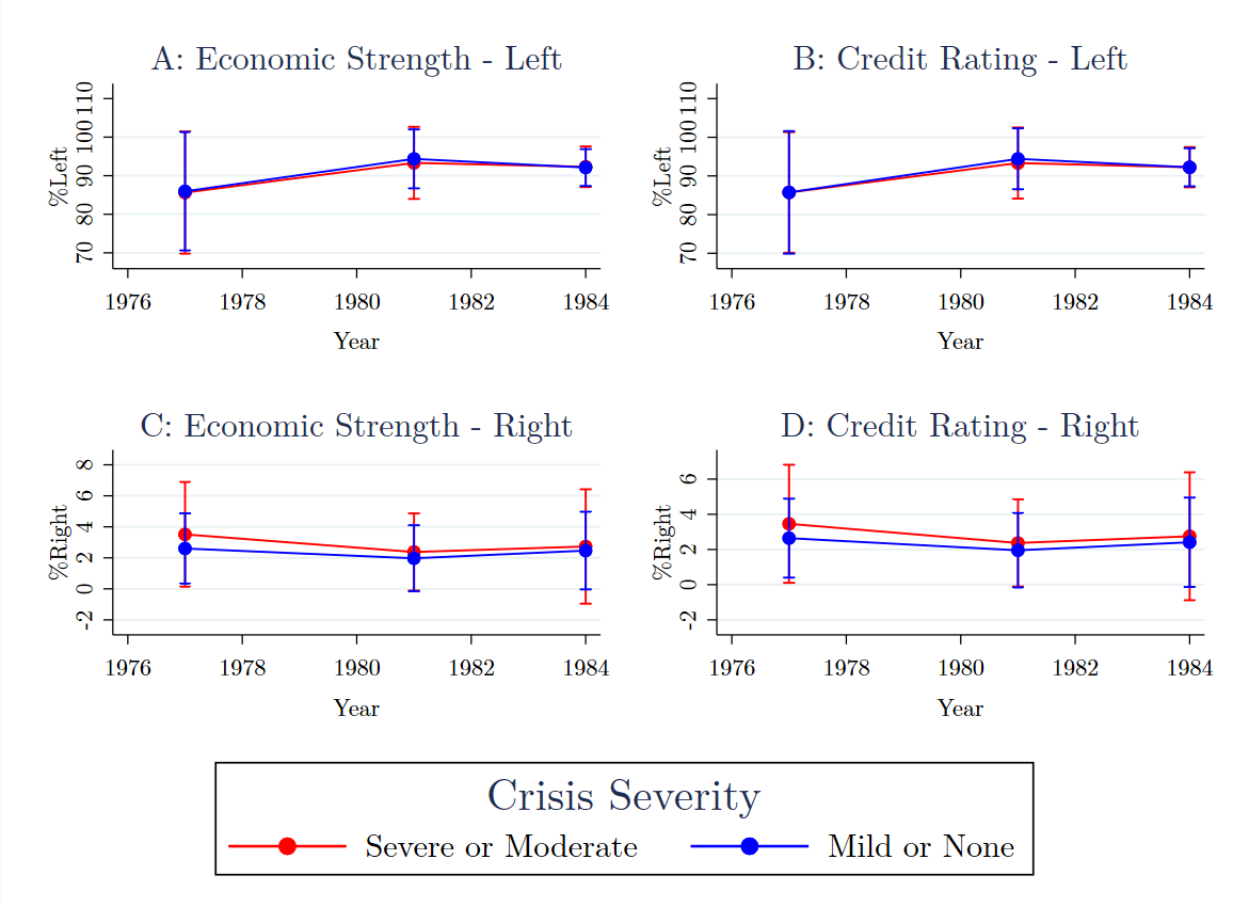



Figure A4: Voting Patterns Before the Crisis are Balanced

Notes: For the measures of Economic Strength and Credit Rating, we divide the sample into two groups by the severity of the measure. Group one includes severe or moderate crisis, and group two includes no crisis and mild crisis. 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, and 1984).

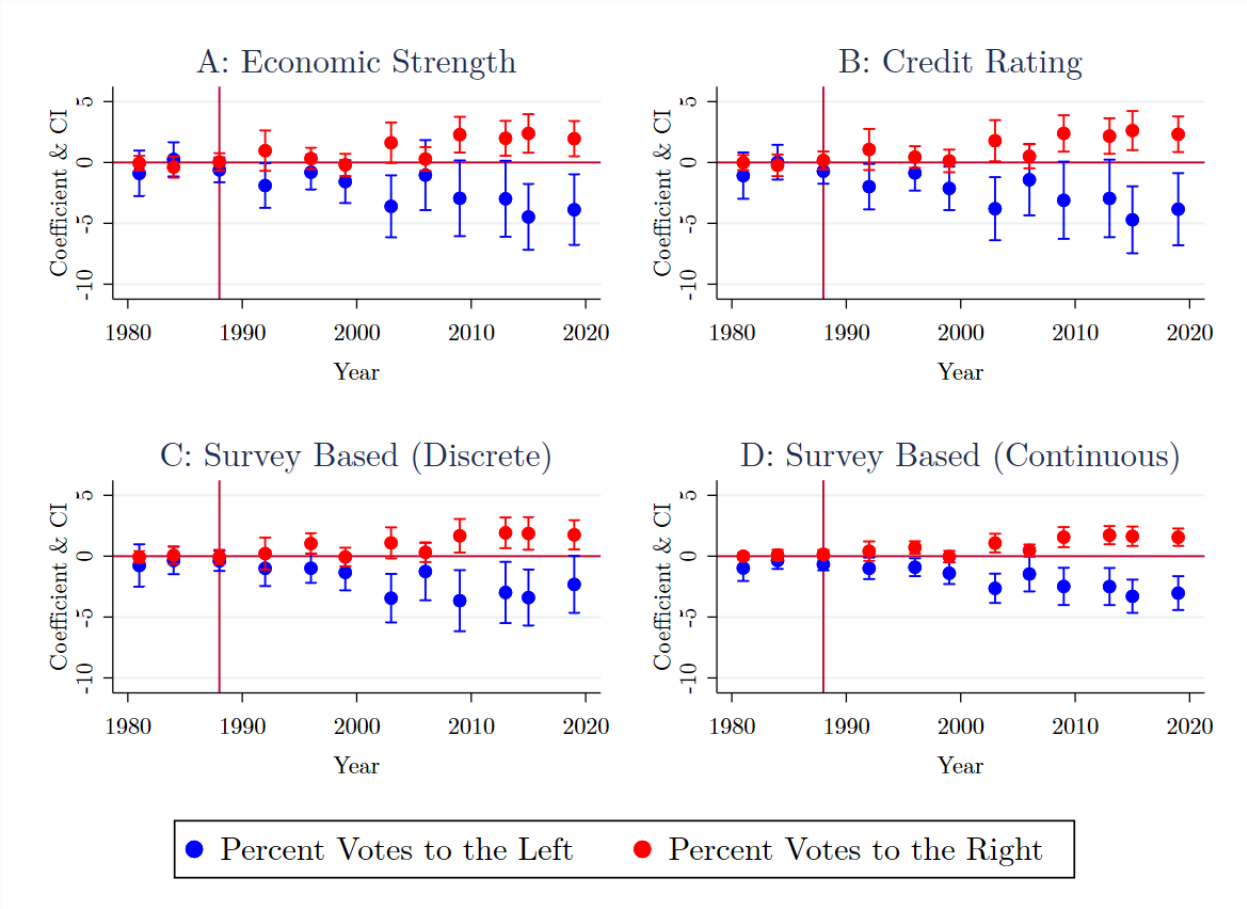



Figure A5: The Effect of The Crisis on Voting: By the 3 Measures

Notes: We take the percent of votes cast to the left and to the right in each kibbutz between the years 1978-2019. 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 the continuous survey-based measures as treatment. We always control for the baseline value of the corresponding outcome in 1977. 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.

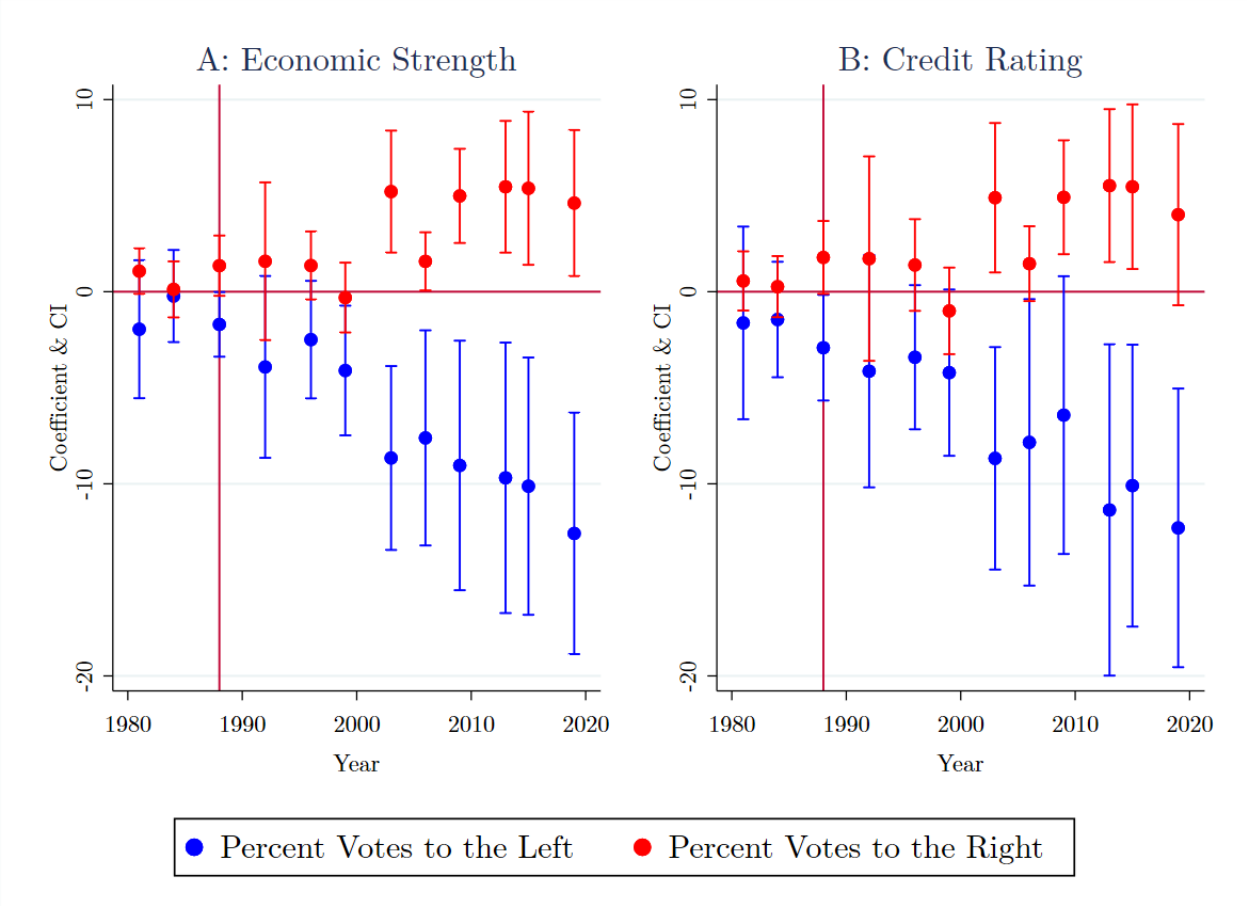
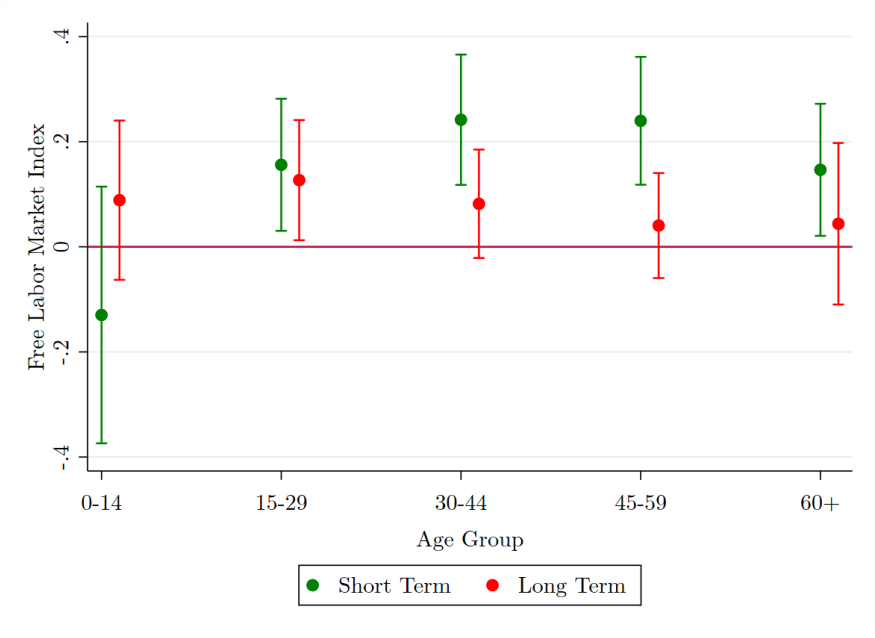
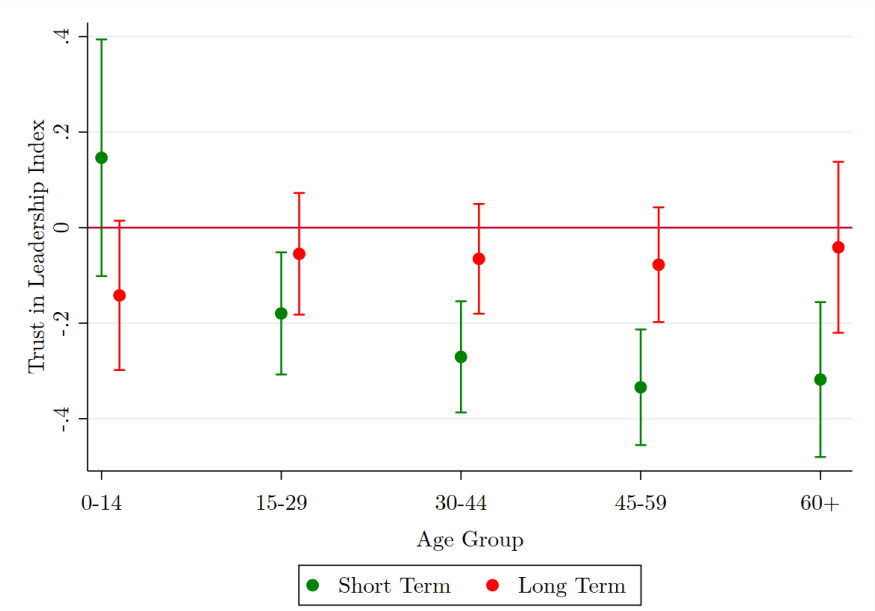



Figure A6: The Effect of The Crisis on Voting: No Extensions

Notes: We take the percent of votes cast to the left and to the right in each kibbutz between the years 1981 and 2019 as outcome variables. For each year separately, we regress these outcomes on the crisis' severity, as calculated by varying measures. We drop 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). We always control for the baseline values in 1977 of the corresponding outcome. 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)

Notes: 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 groups 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.

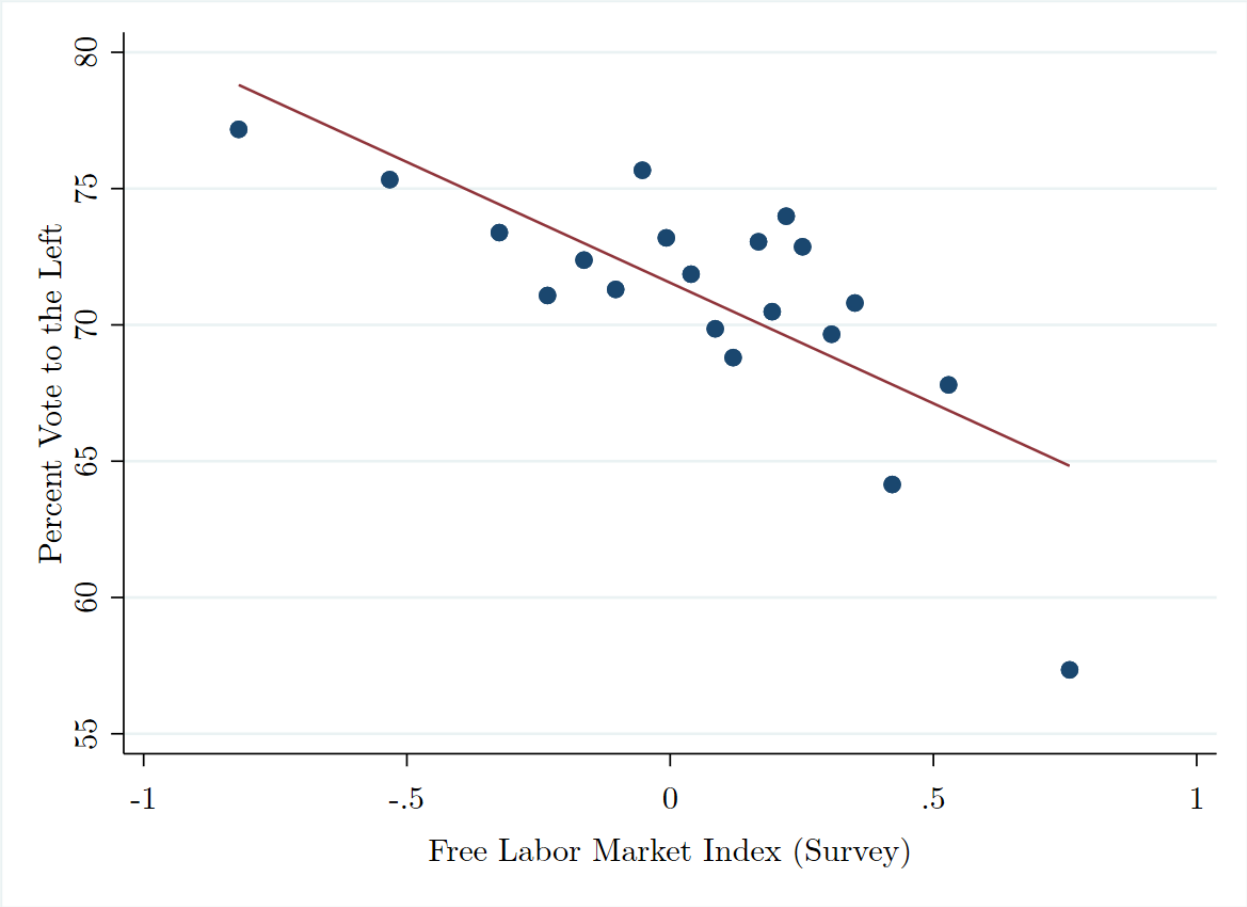
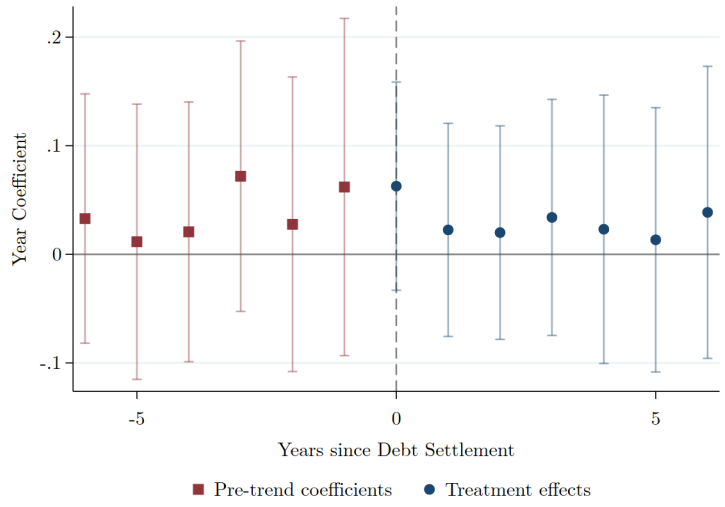
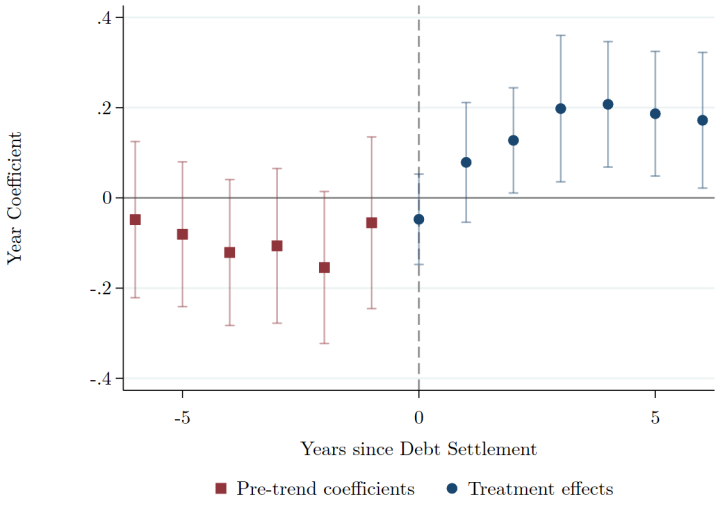



Figure A8: Attitudes and Voting Behavior are Correlated

Notes: We collapse to the kibbutz level the mean percent of vote to the left cast after 1990, and the mean free labor market index. We binscatter the relationship, splitting the sample to 20 bins.



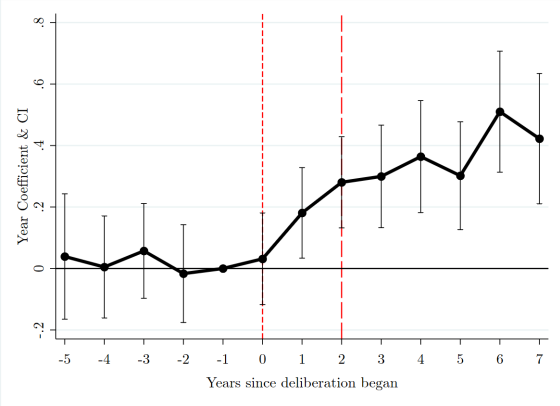

(a) Free Labor Market Index



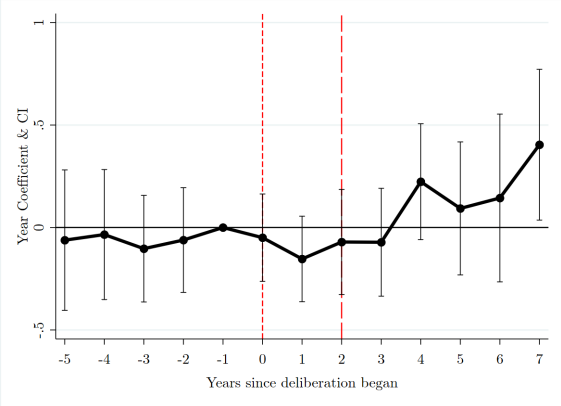

(b) Trust in Leadership Index

Figure A9: Debt Arrangement Restores Trust: Borusyak el al. (2021)

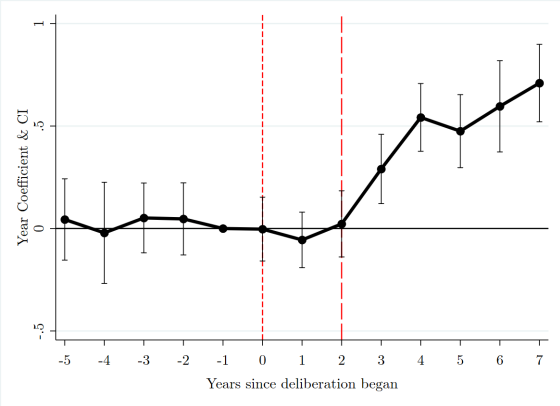
Notes: Using an Event Study approach, we estimate the effect of the debt relief arrangement on the labor and trust indices, using the method developed by Borusyak et al. (2021). We show CIs of 95% level, we cluster at the kibbutz level, and we add individual-level controls. Our sample includes only people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



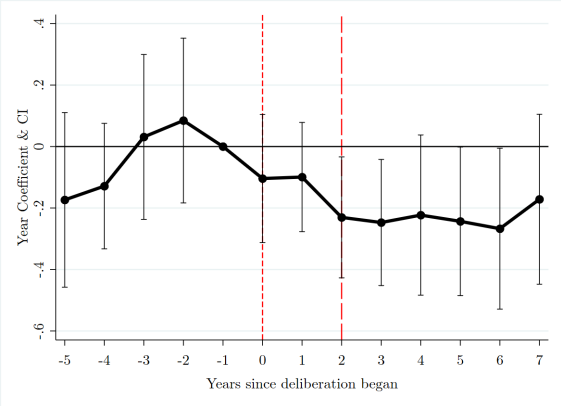

(a) Free Labor Market Index



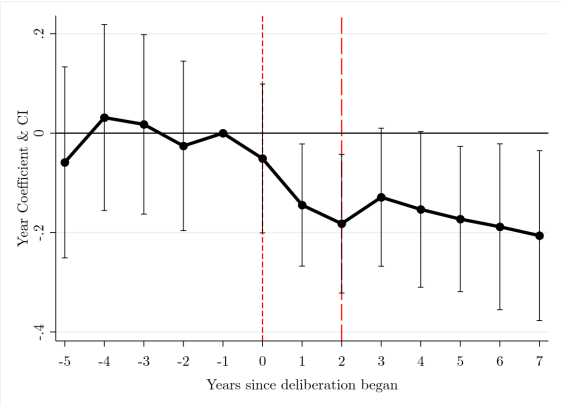

(b) Perceived Economic State in the Kibbutz



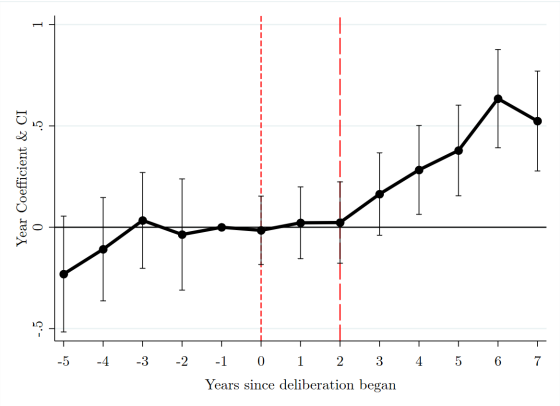

(c) Perceived Work Ethics in the Kibbutz




(d) Support of Equality



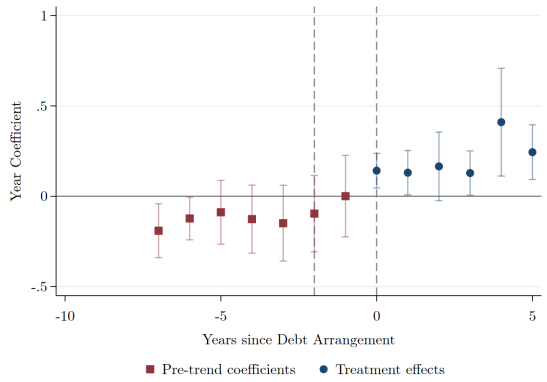

(e) Support of Collectivism



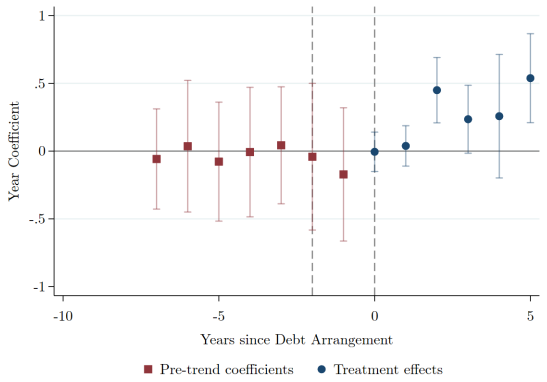

(f) Support of Mutual Guarantee

Figure A10: Reform Event Studies: Only Kibbutzim Reformed After 2001

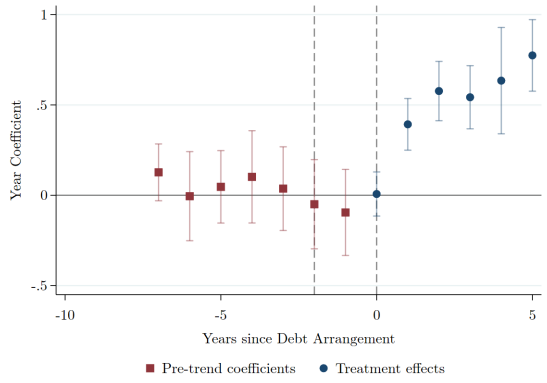
Notes: We estimate the effect of the deliberations and the implementation of reforms on various outcomes using a Two-Way-Fixed-Effect Event Study specification, as shown in Equation 4. Time $t = 0$ depicts the start of the deliberation, and implementation happens at $t = 2$. We include individual-level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



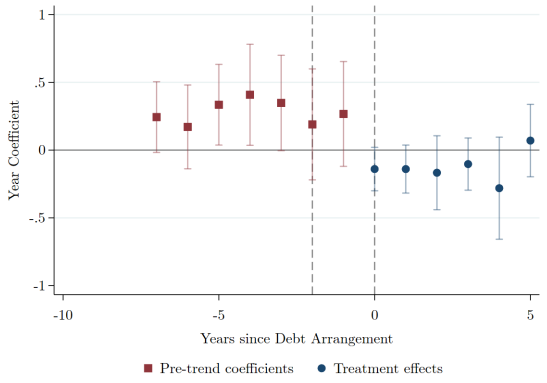

(a) Free Labor Market Index



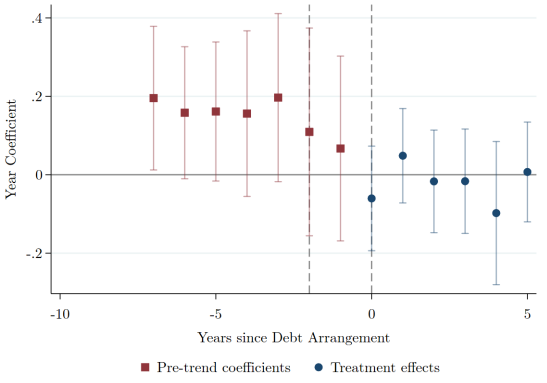

(b) Perceived Economic State in the Kibbutz



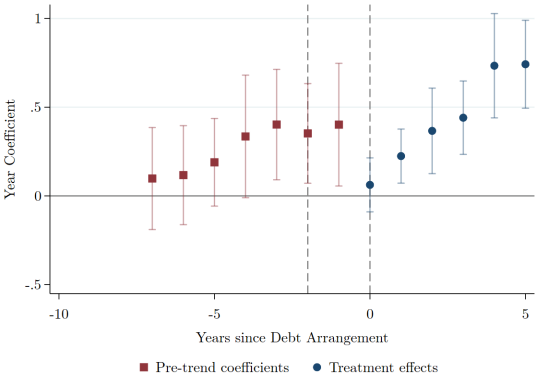

(c) Perceived Work Ethics in the Kibbutz




(d) Support of Equality




(e) Support of Collectivism




(f) Support of Mutual Guarantee

Figure A11: Reform Event Studies: Borusyak et 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.

# 12 Online Appendix: Tables

| | Economic Strength | Credit Rating |
|---|---|---|
| **Survey-Based Continuous** | 0.766 | 0.764 |
| **Survey-Based Discrete** | 0.744 | 0.736 |
| **Economic Strength** | 1.000 | 0.890 |

Table A1: The Three Economic Measures Are Highly Correlated

Notes: 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 | 2.6 | 80.9 | 8.4 | 49.2 | 42 | 63.9 |
| Moderate Crisis | 99 | 486 | 49.2 | 42.7 | 2.6 | 83.1 | 8.3 | 49 | 43.2 | 68.6 |
| Mild Crisis | 41 | 568.4 | 47.6 | 44 | 2.7 | 82.4 | 9.3 | 48.8 | 42.9 | 70 |
| No Crisis | 30 | 568.5 | 49.1 | 43.4 | 2.8 | 81.9 | 9 | 48.3 | 43.3 | 69 |
| **Credit Rating** | | | | | | | | | | |
| Severe Crisis | 35 | 398.4 | 48.5 | 41.3 | 2.6 | 81.8 | 8 | 49.1 | 42.8 | 65.7 |
| Moderate Crisis | 95 | 504.4 | 49.3 | 43.2 | 2.7 | 83.3 | 8.7 | 49 | 43.1 | 68.7 |
| Mild Crisis | 51 | 563.4 | 48.4 | 43.3 | 2.7 | 82 | 8.4 | 48.8 | 43.2 | 70.4 |
| No Crisis | 14 | 583.9 | 48.2 | 45.8 | 2.7 | 80.8 | 10 | 47.9 | 42.6 | 65.5 |

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

Notes: We obtain information on each kibbutz from the 1983 census. 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 | 84.4 | 90.4 | 4.3 | 2.9 |
| | (Severe Crisis) | Post | 768 | 73.4 | 67.4 | 20.4 | 7.1 |
| | Groups 3 & 4 | Pre | 153 | 86.6 | 90.8 | 4.7 | 2.3 |
| | (Mild Crisis) | Post | 408 | 75.2 | 70.1 | 19.8 | 5.4 |
| Credit Rating | Groups 1 & 2 | Pre | 300 | 84.4 | 90.4 | 4.3 | 2.9 |
| | (Severe Crisis) | Post | 800 | 73.3 | 67.4 | 20.4 | 7.1 |
| | Groups 3 & 4 | Pre | 141 | 86.7 | 90.8 | 4.7 | 2.3 |
| | (Mild Crisis) | Post | 376 | 75.6 | 70.3 | 19.9 | 5.3 |
| Survey Based | Groups 1 & 2 | Pre | 231 | 85.1 | 90.5 | 4.2 | 3 |
| | (Severe Crisis) | Post | 616 | 73.6 | 67.1 | 20.4 | 7.4 |
| | Groups 3 & 4 | Pre | 228 | 85.2 | 90.5 | 4.7 | 2.4 |
| | (Mild Crisis) | Post | 608 | 74.5 | 69.4 | 20.1 | 5.9 |

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

Notes: 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 | 52.13 | 48.24 | 51.11 |
| % With No Academic Degree | 57.55 | 60.19 | 57.71 | 59.98 |
| % With BA Degree | 27.97 | 26.51 | 27.67 | 25.24 |
| % In an Artzi Kibbutz | 35.06 | 36.18 | 35.2 | 37.26 |

Table A4: The Survey Is Representative

Notes: 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, during which both the CBS and the IRK survey data are available and within the study period.

| | 1977 | | 1981 | | 1984 | |
|---|---|---|---|---|---|---|
| | % *Havoda* | % *Ratz* | % *Havoda* | % *Ratz* | % *Havoda* | % *Ratz* |
| **Economic Strength** | | | | | | |
| Severe Crisis | -2.543 | -0.413 | -3.342 | -0.280 | 1.333 | -1.969 |
| | (5.687) | (0.462) | (4.046) | (0.297) | (2.109) | (1.504) |
| Moderate Crisis | -1.280 | -0.183 | 1.026 | -0.101 | 1.961 | -1.725 |
| | (3.937) | (0.435) | (2.532) | (0.268) | (1.798) | (1.137) |
| Mild Crisis | 0.312 | 0.365 | 1.810 | -0.224 | 2.766 | -3.156** |
| | (4.442) | (0.542) | (2.545) | (0.299) | (2.037) | (1.299) |
| Observations | 147 | 147 | 147 | 147 | 147 | 147 |
| **Credit Rating** | | | | | | |
| Severe Crisis | 2.662 | -0.773 | 0.719 | -0.332 | 1.845 | -2.979 |
| | (9.229) | (0.782) | (5.669) | (0.270) | (2.963) | (1.803) |
| Moderate Crisis | 6.293 | -0.483 | 5.447 | -0.0333 | 2.807 | -3.110* |
| | (8.492) | (0.774) | (4.910) | (0.256) | (2.878) | (1.612) |
| Mild Crisis | 9.260 | -0.283 | 6.740 | -0.324 | 3.286 | -4.079** |
| | (8.510) | (0.795) | (4.904) | (0.276) | (2.938) | (1.650) |
| Observations | 147 | 147 | 147 | 147 | 147 | 147 |
| **Survey Based** | | | | | | |
| Severe Crisis | -1.764 | -0.667** | -1.281 | -0.0202 | 0.534 | -1.398 |
| | (4.079) | (0.284) | (2.554) | (0.210) | (1.823) | (1.195) |
| Moderate Crisis | 0.400 | -0.382 | 1.233 | -0.243 | -0.520 | 0.392 |
| | (3.159) | (0.316) | (1.603) | (0.241) | (1.639) | (1.189) |
| Mild Crisis | -1.200 | 0.0875 | -0.317 | 0.0873 | 0.289 | -1.167 |
| | (3.667) | (0.401) | (2.034) | (0.235) | (1.659) | (1.102) |
| Observations | 144 | 144 | 144 | 144 | 144 | 144 |

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

Notes: 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. We do so for every election year separately. In Panel A, we show the coefficients 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. Every year, we use the sample of the kibbutzim, for which we have full voting data.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | **Artzi** | **Meuchad** | **Meuchad - No Group FEs** | **Meuchad - Group FEs** | **Familial Sleep Year** |
| **Economic Strength** | | | | | |
| Severe Crisis | -0.251* | 0.100 | -0.400 | -0.513 | -3.396 |
| | (0.150) | (0.207) | (0.412) | (0.592) | (2.452) |
| Moderate Crisis | -0.118 | 0.0682 | -0.615 | -0.719 | -3.185 |
| | (0.121) | (0.178) | (0.374) | (0.535) | (1.994) |
| Mild Crisis | -0.157 | 0.0263 | -0.308 | -0.330 | -1.265 |
| | (0.140) | (0.198) | (0.374) | (0.567) | (2.301) |
| Observations | 147 | 88 | 33 | 33 | 141 |
| **Credit Rating** | | | | | |
| Severe Crisis | -0.293 | 0.0364 | 0.571 | 1.389* | -2.889 |
| | (0.178) | (0.251) | (0.523) | (0.795) | (2.982) |
| Moderate Crisis | -0.0634 | -0.0750 | 0.417 | 1 | -2.638 |
| | (0.164) | (0.240) | (0.509) | (0.732) | (2.741) |
| Mild Crisis | -0.0676 | -0.0762 | 0.769 | 1.716** | -0.137 |
| | (0.173) | (0.252) | (0.508) | (0.786) | (2.904) |
| Observations | 147 | 88 | 33 | 33 | 141 |
| **Survey Based** | | | | | |
| Severe Crisis | 0.0113 | 0.250 | -0.336 | -0.385 | -2.598 |
| | (0.115) | (0.153) | (0.213) | (0.265) | (1.852) |
| Moderate Crisis | 0.0188 | 0.0789 | 0.114 | 0.00552 | -2.546 |
| | (0.118) | (0.155) | (0.285) | (0.436) | (1.908) |
| Mild Crisis | -0.0896 | -0.0769 | 0.114 | 0.494 | -2.371 |
| | (0.114) | (0.143) | (0.227) | (0.350) | (1.865) |
| Observations | 144 | 87 | 33 | 33 | 138 |

Table A6: Various Pre-Crisis Balancing Tests

Notes: We regress various pre-crisis outcomes on the external measures of the crisis. In Panel A, we show the coefficients 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 for which we have full voting data.

| | % Turnout | % Left | % Center | % Right | $\frac{Center+Right}{Left}$ | $\frac{Right}{Left}$ |
|---|---|---|---|---|---|---|
| **A: Full Sample** | | | | | | |
| Severe Crisis | -0.682 | -3.380*** | 1.073 | 2.351*** | 0.155*** | 0.0669*** |
| | (0.969) | (1.276) | (0.716) | (0.626) | (0.0497) | (0.0191) |
| Moderate Crisis | -1.070 | -2.310** | 0.398 | 1.481*** | 0.0769** | 0.0366*** |
| | (0.866) | (1.103) | (0.640) | (0.499) | (0.0362) | (0.0122) |
| Mild Crisis | -0.321 | -2.296** | 0.560 | 1.869*** | 0.0768*** | 0.0377*** |
| | (0.904) | (0.945) | (0.627) | (0.407) | (0.0270) | (0.00819) |
| Observations | 1,989 | 1,989 | 1,836 | 1,989 | 1,836 | 1,989 |
| **B: Full Sample – Continuous** | | | | | | |
| Continuous Likert Scale | -0.603 | -1.495*** | 0.565* | 0.899*** | 0.0656*** | 0.0271*** |
| | (0.384) | (0.501) | (0.322) | (0.248) | (0.0200) | (0.00768) |
| Observations | 1,989 | 1,989 | 1,836 | 1,989 | 1,836 | 1,989 |
| **C: Partial Sample** | | | | | | |
| Severe Crisis | -0.838 | -2.972** | 0.882 | 1.730*** | 0.117** | 0.0492*** |
| | (0.982) | (1.318) | (0.751) | (0.489) | (0.0461) | (0.0156) |
| Moderate Crisis | -1.110 | -2.133* | 0.232 | 1.458*** | 0.0692* | 0.0353*** |
| | (0.934) | (1.130) | (0.661) | (0.553) | (0.0358) | (0.0131) |
| Mild Crisis | -0.247 | -2.263** | 0.537 | 1.817*** | 0.0767*** | 0.0368*** |
| | (0.915) | (0.957) | (0.635) | (0.408) | (0.0274) | (0.00823) |
| Observations | 1,872 | 1,872 | 1,728 | 1,872 | 1,728 | 1,872 |
| **D: Partial Sample – Continuous** | | | | | | |
| Likert Scale | -0.702* | -1.330** | 0.484 | 0.655*** | 0.0504*** | 0.0198*** |
| | (0.383) | (0.513) | (0.336) | (0.193) | (0.0182) | (0.00603) |
| Observations | 1,872 | 1,872 | 1,728 | 1,872 | 1,728 | 1,872 |

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

Notes: We use 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 all the years. Using a Two-Way-Fixed-Effect Difference-in-Difference specification, we regress voting outcomes on the severity of the crisis as outlined in Equation 1. The coefficients reported are the degree of the severity of the crisis interacted with an indicator for the years after 1985. We always control for the baseline in 1977 of the corresponding outcome, flexibly interacted with a full set of year dummies. 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 standard errors at the kibbutz level.

| | % Turnout | % Left | % Center | % Right | $\frac{Right+Center}{Left}$ | $\frac{Right}{Left}$ |
|---|---|---|---|---|---|---|
| **Economic Strength** | | | | | | |
| Severe Crisis | -0.224 | -5.592*** | 1.755* | 2.754*** | 0.201*** | 0.0799*** |
| | (1.293) | (2.084) | (1.031) | (0.997) | (0.0725) | (0.0313) |
| Moderate Crisis | 0.172 | -1.913 | 0.0490 | 0.952 | 0.0442 | 0.0193 |
| | (1.072) | (1.513) | (0.785) | (0.640) | (0.0462) | (0.0171) |
| Mild Crisis | 0.608 | -0.521 | -0.505 | -0.233 | -0.00659 | -0.00608 |
| | (1.181) | (1.618) | (0.936) | (0.700) | (0.0484) | (0.0178) |
| Observations | 1,617 | 1,617 | 1,617 | 1,617 | 1,617 | 1,617 |
| **Credit Rating** | | | | | | |
| Severe Crisis | -0.855 | -6.819*** | 2.459** | 3.358*** | 0.234*** | 0.0958*** |
| | (1.746) | (2.062) | (1.165) | (0.864) | (0.0597) | (0.0229) |
| Moderate Crisis | -1.163 | -3.057* | 0.313 | 1.240** | 0.0630 | 0.0335*** |
| | (1.642) | (1.696) | (1.065) | (0.615) | (0.0443) | (0.0112) |
| Mild Crisis | -0.499 | -2.096 | 0.378 | 0.256 | 0.0340 | 0.0147 |
| | (1.640) | (1.789) | (1.142) | (0.683) | (0.0473) | (0.0136) |
| Observations | 1,617 | 1,617 | 1,617 | 1,617 | 1,617 | 1,617 |

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

Notes: 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 for 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 regress voting outcomes on the severity of the crisis. The specification is outlined in Equation 1. The coefficients reported are of the degree of the severity of the crisis interacted with an indicator for the years after 1985. We always control for the baseline in 1977 of the corresponding outcome, flexibly interacted with a full set of year dummies. 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 standard errors at the kibbutz level.

| | % Turnout | % Left | % Center | % Right | $\frac{Right+Center}{Left}$ | $\frac{Right}{Left}$ |
|---|---|---|---|---|---|---|
| **Economic Strength** | | | | | | |
| Severe Crisis | -1.318 | -8.700* | 2.171 | 4.499*** | 0.315** | 0.174** |
| | (2.861) | (4.380) | (1.571) | (1.443) | (0.156) | (0.0711) |
| Moderate Crisis | 0.486 | -2.029 | 0.113 | 1.606*** | 0.0703 | 0.0293** |
| | (1.211) | (1.828) | (0.798) | (0.530) | (0.0434) | (0.0139) |
| Mild Crisis | -0.187 | -1.686 | 0.593 | 0.273 | 0.0504 | 0.0119 |
| | (1.688) | (2.202) | (1.100) | (0.605) | (0.0564) | (0.0175) |
| Observations | 676 | 676 | 624 | 676 | 624 | 676 |
| **Credit Rating** | | | | | | |
| Severe Crisis | 2.413 | -8.401*** | 2.636** | 3.913*** | 0.290*** | 0.126*** |
| | (2.279) | (2.942) | (1.020) | (0.964) | (0.0851) | (0.0396) |
| Moderate Crisis | 0.828 | -1.792 | 0.0365 | 0.686 | 0.0284 | 0.0167 |
| | (1.375) | (2.312) | (0.870) | (0.690) | (0.0508) | (0.0171) |
| Mild Crisis | 2.504* | -1.622 | 0.732 | -0.718 | 0.00812 | -0.00473 |
| | (1.481) | (2.415) | (1.024) | (0.694) | (0.0497) | (0.0161) |
| Observations | 676 | 676 | 624 | 676 | 624 | 676 |

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

Notes: 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 all the years. We drop kibbutzim that ever extended. Using a Two-Way-Fixed-Effect Difference-in-Difference specification, we regress voting outcomes on the severity of the crisis The specification is outlined in Equation 1. The coefficients reported are of the degree of the severity of the crisis interacted with an indicator forthe years after 1985. We always control for the baseline in 1977 of the corresponding outcome, flexibly interacted with a full set of year dummies. 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 standard errorsat the kibbutz level.

| | % Turnout | % Left | % Center | % Right | $\frac{Right+Center}{Left}$ | $\frac{Right}{Left}$ |
|---|---|---|---|---|---|---|
| **Economic Strength** | | | | | | |
| Severe Crisis | -1.036 | -5.330*** | 2.021*** | 2.478*** | 0.176*** | 0.0664*** |
| | (1.129) | (1.627) | (0.722) | (0.660) | (0.0482) | (0.0207) |
| Moderate Crisis | -0.359 | -2.491** | 0.289 | 1.154*** | 0.0711*** | 0.0274*** |
| | (0.922) | (1.064) | (0.514) | (0.357) | (0.0251) | (0.00714) |
| Mild Crisis | 0.482 | -1.302 | -0.317 | 0.318 | 0.0363 | 0.0115 |
| | (1.029) | (1.177) | (0.704) | (0.424) | (0.0294) | (0.00825) |
| Observations | 1,724 | 1,724 | 1,577 | 1,724 | 1,577 | 1,724 |
| **Credit Rating** | | | | | | |
| Severe Crisis | -1.252 | -5.728*** | 2.538*** | 2.482*** | 0.181*** | 0.0649*** |
| | (1.460) | (1.722) | (0.971) | (0.699) | (0.0433) | (0.0178) |
| Moderate Crisis | -1.160 | -2.921** | 0.330 | 0.969* | 0.0549* | 0.0245** |
| | (1.324) | (1.370) | (0.881) | (0.537) | (0.0310) | (0.00977) |
| Mild Crisis | -0.0403 | -1.595 | 0.187 | -0.0278 | 0.0165 | 0.00395 |
| | (1.339) | (1.413) | (0.942) | (0.540) | (0.0314) | (0.00939) |
| Observations | 1,724 | 1,724 | 1,577 | 1,724 | 1,577 | 1,724 |

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

Notes: 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 once they extend. Using a Two-Way-Fixed-Effect Difference-in-Difference specification, we regress voting outcomes on the severity of the crisis. 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. We always control for the baseline in 1977 of the corresponding outcome, flexibly interacted with a full set of year dummies. 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 standard errors at the kibbutz level.

| | Optimism Regarding Peace Process | | |
|---|---|---|---|
| Crisis' Measure: | **Economic Strength** | **Credit Rating** | **Survey Based** |
| Severe Crisis | 0.00443 | 0.0548 | -0.0143 |
| | (0.0821) | (0.0847) | (0.0789) |
| Moderate Crisis | 0.0229 | 0.0627 | 0.0719 |
| | (0.0619) | (0.0786) | (0.0894) |
| Mild Crisis | -0.0278 | 0.0210 | -0.00965 |
| | (0.0934) | (0.0929) | (0.0801) |
| Observations | 146 | 146 | 143 |

Table A11: The Crisis's Intensity is Uncorrelated With Attitudes Towards the Conflict

Notes: As an outcome, we take the kibbutz-level mean answer to 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 mean answer to this question on the three measures of the crisis. The results for each measure are presented in a different column. We cluster standard errors at the kibbutz level.

| | % Turnout | % Left | % Center | % Right | $\frac{Right+Center}{Left}$ | $\frac{Right}{Left}$ |
|---|---|---|---|---|---|---|
| **Economic Strength** | | | | | | |
| Severe Crisis | -0.651 | -4.384** | 1.296 | 2.494*** | 0.172*** | 0.0694*** |
| | (1.153) | (1.719) | (0.849) | (0.847) | (0.0636) | (0.0262) |
| Moderate Crisis | -0.0853 | -1.430 | -0.233 | 0.794 | 0.0341 | 0.0159 |
| | (0.955) | (1.208) | (0.620) | (0.531) | (0.0401) | (0.0139) |
| Mild Crisis | 0.631 | -0.723 | -0.448 | -0.182 | -0.00457 | -0.00414 |
| | (1.044) | (1.274) | (0.754) | (0.567) | (0.0418) | (0.0142) |
| Reform | 0.0872 | -2.727*** | 1.638*** | 0.257 | 0.0327 | 0.00783 |
| | (0.552) | (0.715) | (0.485) | (0.376) | (0.0264) | (0.00853) |
| Observations | 1,911 | 1,911 | 1,764 | 1,911 | 1,764 | 1,911 |
| **Credit Rating** | | | | | | |
| Severe Crisis | -1.170 | -5.489*** | 1.988* | 3.022*** | 0.203*** | 0.0821*** |
| | (1.531) | (1.722) | (1.016) | (0.729) | (0.0522) | (0.0192) |
| Moderate Crisis | -1.198 | -2.589* | 0.0757 | 1.126** | 0.0519 | 0.0286*** |
| | (1.420) | (1.393) | (0.921) | (0.522) | (0.0383) | (0.00932) |
| Mild Crisis | -0.342 | -2.101 | 0.378 | 0.318 | 0.0314 | 0.0135 |
| | (1.417) | (1.435) | (0.972) | (0.565) | (0.0405) | (0.0110) |
| Reform | 0.0813 | -2.721*** | 1.630*** | 0.251 | 0.0321 | 0.00777 |
| | (0.549) | (0.718) | (0.486) | (0.374) | (0.0264) | (0.00855) |
| Observations | 1,911 | 1,911 | 1,764 | 1,911 | 1,764 | 1,911 |

Table A12: The Effect of the Crisis on Voting Patterns - Control For Reform

Notes: 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 regress voting outcomes on the severity of the crisis. 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 forthe years after 1985. We always control for the baseline in 1977 of the corresponding outcome, flexibly interacted with a full set of year dummies. 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 standard errors at the kibbutz level.

| | **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 | 0.288*** | -0.229*** | 0.291*** | -0.225*** | 0.305*** | -0.311*** |
| | (0.0658) | (0.0739) | (0.0911) | (0.0831) | (0.0532) | (0.0559) |
| Moderate Crisis | 0.142** | -0.152** | 0.156* | -0.161** | 0.160*** | -0.230*** |
| | (0.0562) | (0.0598) | (0.0839) | (0.0765) | (0.0597) | (0.0540) |
| Mild Crisis | 0.0276 | -0.0131 | 0.0837 | -0.0899 | 0.109* | -0.172*** |
| | (0.0726) | (0.0714) | (0.0888) | (0.0761) | (0.0628) | (0.0582) |
| Observations | 16,020 | 14,127 | 16,020 | 14,127 | 17,167 | 15,144 |

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

Notes: We regress the free labor market and trust in leadership indices on the measures of the crisis. We control for individual-level controls, year Fixed Effects, an affiliation with the Artzi movement, and passage of the reform. We cluster standard errors 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 | 0.572*** | 0.327** | 0.397*** |
| | (0.114) | (0.136) | (0.148) |
| Moderate Crisis | 0.363*** | 0.0931 | 0.201 |
| | (0.114) | (0.0788) | (0.127) |
| Mild Crisis | 0.0779 | -0.0123 | -0.0629 |
| | (0.151) | (0.0964) | (0.148) |
| **Panel B:** | | | |
| **Credit Rating** | | | |
| Severe Crisis | 0.606*** | 0.296** | 0.378* |
| | (0.202) | (0.140) | (0.199) |
| Moderate Crisis | 0.421** | 0.103 | 0.200 |
| | (0.198) | (0.104) | (0.187) |
| Mild Crisis | 0.202 | 0.0319 | 0.0734 |
| | (0.205) | (0.112) | (0.190) |
| Observations | 11,681 | 12,935 | 12,270 |

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

Notes: We regress the answer to each of the questions making the free labor market index on the economic measures. We control for individual-level controls, year Fixed Effects, and passage of the reform. We cluster standard errors 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 | 0.298*** | -0.259*** | 0.192** | -0.118 | 0.337*** | -0.288*** |
| | (0.0663) | (0.0743) | (0.0741) | (0.0791) | (0.0723) | (0.0875) |
| Moderate Crisis | 0.148*** | -0.170*** | 0.103* | -0.0805 | 0.167*** | -0.183*** |
| | (0.0560) | (0.0607) | (0.0580) | (0.0646) | (0.0604) | (0.0696) |
| Mild Crisis | 0.0307 | -0.0218 | -0.0257 | 0.0278 | 0.0382 | -0.0469 |
| | (0.0725) | (0.0735) | (0.0761) | (0.0770) | (0.0763) | (0.0801) |
| Observations | 16,005 | 14,113 | 12,226 | 11,252 | 9,246 | 8,634 |
| **Panel B: Credit Rating** | | | | | | |
| Severe Crisis | 0.303*** | -0.256*** | 0.210** | -0.122 | 0.304*** | -0.292*** |
| | (0.0937) | (0.0889) | (0.0932) | (0.0902) | (0.0920) | (0.0886) |
| Moderate Crisis | 0.161* | -0.174** | 0.130 | -0.0891 | 0.185** | -0.201** |
| | (0.0847) | (0.0804) | (0.0836) | (0.0818) | (0.0815) | (0.0781) |
| Mild Crisis | 0.0867 | -0.0984 | 0.0569 | -0.0598 | 0.0785 | -0.128 |
| | (0.0879) | (0.0768) | (0.0901) | (0.0830) | (0.0869) | (0.0787) |
| Observations | 16,005 | 14,113 | 12,226 | 11,252 | 9,246 | 8,634 |
| **Panel C: Survey Based** | | | | | | |
| Severe Crisis | 0.312*** | -0.331*** | 0.278*** | -0.218*** | 0.338*** | -0.350*** |
| | (0.0526) | (0.0551) | (0.0568) | (0.0631) | (0.0564) | (0.0611) |
| Moderate Crisis | 0.167*** | -0.247*** | 0.128** | -0.156** | 0.202*** | -0.285*** |
| | (0.0593) | (0.0536) | (0.0639) | (0.0610) | (0.0606) | (0.0591) |
| Mild Crisis | 0.116* | -0.189*** | 0.117* | -0.140** | 0.137** | -0.221*** |
| | (0.0619) | (0.0589) | (0.0659) | (0.0646) | (0.0669) | (0.0623) |
| Observations | 17,139 | 15,119 | 13,068 | 12,044 | 9,818 | 9,178 |

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

Notes: We regress the free labor market and trust in leadership indices on the measures of the crisis. We control for individual-level controls, year Fixed Effects, and passage of the reform. We cluster standard errors 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 | 1 | 356 | 77.53 | 83.33 | 1.81 | 11.96 |
| 1999 | 18 | 318 | 77.68 | 77.77 | 8.12 | 6.26 |
| 2003 | 80 | 412.36 | 72.24 | 75.36 | 9.69 | 8.97 |
| 2006 | 106 | 425.76 | 67.74 | 64.12 | 20.63 | 4.44 |
| 2009 | 119 | 453.99 | 68.59 | 52.48 | 33.84 | 9.47 |
| 2013 | 127 | 500.05 | 72.6 | 59.89 | 26.68 | 8.18 |
| **Unreformed** | | | | | | |
| 1996 | 156 | 425.11 | 80.86 | 91.52 | 2.81 | 4.19 |
| 1999 | 139 | 444.22 | 81.23 | 86.02 | 5.29 | 4.16 |
| 2003 | 77 | 472.51 | 72.86 | 79.82 | 7.86 | 6.76 |
| 2006 | 51 | 520.96 | 68.84 | 69.52 | 18.35 | 3.61 |
| 2009 | 38 | 549.29 | 69.58 | 55.89 | 32.67 | 7.65 |
| 2013 | 30 | 608.13 | 70.59 | 65.07 | 22.04 | 7.65 |

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

Notes: This table presents statistics of the sample by voting year. The sample includes all Takam and Artzi kibbutzim that had voting polls in each of the 13 elections between 1977-2013. Kibbutzim are considered reformed starting from the year after the reform (year since reform = 1). Other than the "Kibbutzim" column, which depicts for every year how many kibbutzim were in each sample, all other statistics describe average figures per kibbutz in each sample.

| | **Deliberation Balance** | | | **Implementation Balance** | | |
|---|---|---|---|---|---|---|
| | Coeff | SE | Obs | Coeff | SE | Obs |
| Artzi | -0.043 | (0.141) | 1818 | -0.026 | (0.139) | 1829 |
| Age | 1.331 | (0.986) | 1818 | -1.702* | (0.991) | 1829 |
| Gender | -0.006 | (0.039) | 1808 | -0.000 | (0.038) | 1821 |
| Schooling | -0.027 | (0.032) | 1818 | 0.048 | (0.032) | 1829 |
| Kibbutz's Economic Status | 0.004 | (0.170) | 1818 | -0.013 | (0.181) | 1829 |
| Paying for Overtime | 0.039 | (0.157) | 1588 | 0.055 | (0.153) | 1600 |
| Differential Wages | -0.000 | (0.156) | 1138 | 0.128 | (0.162) | 1131 |
| Reduce Pay for Underworking | 0.056 | (0.157) | 865 | -0.038 | (0.165) | 863 |
| Trust Social Leadership | -0.015 | (0.110) | 1137 | 0.059 | (0.113) | 1133 |
| Trust Economic Leadership | -0.095 | (0.122) | 1142 | 0.099 | (0.130) | 1134 |

Table A17: Deliberation and Implementation Treatments are Balanced

Notes: 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 that reformed just before elections, while control kibbutzim reformed just after elections.

| | % Turnout | % Left | % Center | % Right |
|---|---|---|---|---|
| **Without 96 Elections** | | | | |
| Treatment | 1.662 | -4.578*** | 1.650* | 1.589* |
| | (1.515) | (1.615) | (0.946) | (0.839) |
| Observations | 76 | 76 | 76 | 76 |
| **Without 96-99 Elections** | | | | |
| Treatment | 1.979 | -4.178** | 1.304 | 1.979* |
| | (1.983) | (1.963) | (1.215) | (1.004) |
| Observations | 52 | 52 | 52 | 52 |
| **Without 2013 Elections** | | | | |
| Treatment | 1.822 | -4.594*** | 1.385 | 1.898** |
| | (1.454) | (1.518) | (0.918) | (0.837) |
| Observations | 81 | 81 | 81 | 81 |
| **2 Year Window** | | | | |
| Treatment | -2.026** | -2.898*** | 0.993* | 0.817 |
| | (0.889) | (1.020) | (0.586) | (0.576) |
| Observations | 155 | 155 | 155 | 155 |

Table A18: Effect of Treatment on Voting, Additional Tests

Notes: 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 votes cast to the left, center, and the right. In Panel A, we include all kibbutzim that reformed 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 except those that reformed in 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, a full set of year dummies, and the baseline in 1977 of the corresponding outcome.

| | % Turnout | % Left | % Center | % Right |
|---|---|---|---|---|
| **Backwards Placebo** | | | | |
| Treatment | -0.729 | -0.879 | 0.420 | 0.457 |
| | (1.415) | (1.394) | (0.586) | (0.830) |
| Observations | 76 | 76 | 76 | 76 |
| **Forward Placebo** | | | | |
| Treatment | 1.621 | -0.152 | -0.652 | -0.0528 |
| | (1.471) | (1.509) | (1.098) | (0.703) |
| Observations | 81 | 81 | 81 | 81 |
| **Placebo at 1996** | | | | |
| Treatment | 0.364 | 0.941 | 0.200 | -0.857 |
| | (1.160) | (0.852) | (0.317) | (0.688) |
| Observations | 76 | 76 | 76 | 76 |
| **Placebo at 1996-1999** | | | | |
| Treatment | 0.136 | 0.320 | 0.299 | -0.116 |
| | (1.388) | (0.873) | (0.481) | (0.457) |
| Observations | 104 | 104 | 104 | 104 |
| **Placebo at 2013** | | | | |
| Treatment | 1.744 | 2.297 | -0.224 | -1.308 |
| | (1.104) | (2.001) | (1.450) | (1.072) |
| Observations | 81 | 81 | 81 | 81 |

Table A19: Reform on Voting: Placebo Examinations

Notes: In this table, we perform several placebo exercises for the first estimation strategy. In Panel A, we define treated as kibbutzim that reform just before an election, and control as kibbutzim that reformed a year after elections, but we use the election before the actual election they reformed in proximity to. In Panel B, we do a similar exercise that uses the election after the true reform election. In Panel C, we take kibbutzim that reformed starting from 1998 and regress them only on the 1996 elections. In Panel D, we take kibbutzim that reformed starting from 2002 and regress them only on the 1996-1999 elections. Finally, in Panel E, we take kibbutzim that reformed until 2010 and regress them on the 2013 elections. We always control for affiliation with Artzi, a full set of year dummies, and the baseline in 1977 of the corresponding outcome.