Social Policy and Autocracy: Evidence from East German Administrative Data*

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Abstract

This paper studies the role of social policy in shaping citizen-state relations under autocratic rule. I argue that social policies can increase political support especially when they signal recognition—of citizens' needs, burdens and social identities. To examine this argument, I focus on the introduction of extended maternity leave in former East Germany, a policy that extended material benefits but also signaled attentiveness to women's dual roles as workers and caregivers. Using administrative data on civilian bureaucrats, I show that women giving birth after the policy reform saw a surge in membership of the ruling party compared to women giving birth shortly before. I also find suggestive evidence of gendered spillovers: first, affected grandmothers were more likely to become members, but not grandfathers. Second, in present-day surveys, likely affected daughters lean more towards left-wing ideology.

Keywords: Autocracy, Social Policy, Intergenerational Transmission

JEL Codes: H53, P20, N44

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1 Introduction

Global democracy has regressed to levels last seen in the late 1980's. Instead, autocracies now exert control over more than 70% of the world's population (V-Dem 2025). This shift underscores the need to revisit an enduring question: how do autocracies sustain their rule? Reliance on brute force alone is costly and can breed resentment, so autocracies often seek to cultivate a measure of genuine popular acquiescence. One avenue to foster such reciprocal behavior is through effective policymaking. Concretely, autocracies may offer social policies that benefit the populace in exchange for support and willingness to accept costly obligations. Prior work has linked social policy reforms to political support, but largely in democratic settings and with mixed evidence (Campbell, 2012).

Why do some policies succeed while others falter? Scholars have argued, that success often does not solely rely on the provision of direct benefits, but also on whether a policy credibly signals state competence (Voigtländer and Voth, 2021), political priorities (Boas, Hidalgo and Toral, 2021), and enduring commitments (Holland, 2017; Knutsen and Rasmussen, 2017). Citizens infer from policies not only what they receive, but what the state values and whether it can be trusted over time. Building on this insight, I argue that social policies are especially effective when they signal *attentiveness*—that is, whether people feel seen, not just served. Attentiveness reflects the state's recognition of citizens' specific needs, burdens, and social identities. This kind of symbolic recognition can generate deeper political support than material incentives alone. This perspective may help explain why the legacies of autocratic social policy often spread and persist—shaping political attitudes even after direct benefits disappear and regimes transition to democracy (Hong, Park and Yang, 2023).

To test this argument, I study a landmark social policy with a distinct notion of gendered recognition: the extension of maternity leave in the former German Democratic Republic (GDR). In June 1976, the ruling Socialist Unity Party (SED) abruptly expanded paid maternity leave from three to twelve months for women giving birth to a second or subsequent child. Leveraging granular administrative records on civilian bureaucrats, I implement a difference-in-differences design comparing women giving birth just before and just after the reform. In the GDR, where the SED was not just the ruling party but the institutional face of the regime, party membership served as a concrete expression of political support and reciprocity toward the state. I find that the reform

substantially increased women's likelihood of joining the SED, with suggestive but sizable intergenerational spillovers along female family lines. These results underscore how social policies that resonate with citizens' lived experiences can leave enduring political imprints.

The main data source underlying these results is the so-called Central Cadre Database (CCDB). The CCDB is a large administrative register covering 370,000 CVs allowing me to construct a unique individual-level panel data set. The CVs were originally collected to document the career progress of selected individuals in leading positions and a substantial fraction of people working in the GDR administration, i.e. low to high-ranking civilian bureaucrats. Hence, the data allow for a detailed view on life under socialist rule.[1]

I begin the analysis descriptively, by documenting a process of separation between women and the SED parallel to entering parenthood. In a simple event-study framework, I show that once women became mothers, they were significantly less likely to join the sole-governing party than women of the same age who gave birth later in life. Importantly, no such abrupt changes can be seen among men. Yet, women did not exit sociopolitical life altogether; instead, they redirected their engagement toward gender-specific venues by joining the Democratic Women's League (DFD). This shift suggests that becoming a mother reframed how women related to the political system, in particular through institutions that more explicitly acknowledged their gendered roles.

I then turn to the maternity leave extension as a particularly revealing case for studying the political effects of recognition. While the GDR officially championed gender equality, women remained structurally disadvantaged in the workplace. At the same time, they shouldered the vast majority of domestic responsibilities, from childcare to housework, effectively working a "double shift." This dissonance between ideology and lived experience made policies addressing care work especially salient. By easing a core burden of everyday life, the reform did more than provide material support—it offered symbolic affirmation of women's roles.

To estimate the reform's political impact, I implement a difference-in-differences design. Concretely, I compare women likely exposed to the new policy—those who gave birth to a second or subsequent child between July and December 1976—with women who gave birth in the six months prior, before the reform was implemented. Note, that the introduction of extended mater-

[1]While there exist few descriptive studies from the early 2000s (e.g. Best and Hornbostel 2003, Salheiser 2005), the register has been seldom explored. More recently, de Juan, Haass and Pierskalla (2021) have used the data to explore the effects of mandatory military conscription.

nity leave was *de jure* enacted with retrospective eligibility. Women who gave birth in the early half of 1976 could have partially taken one year of leave. However, *de facto* aggregate expenditure on the leave extension program implies strong lags in policy implementation. Back-of-the-envelope calculations using policy spending data suggest that take-up in both groups differed by roughly 40-50%. Moreover, since the policy reform was announced in May 1976 and implemented shortly after in June 1976, selection into treatment could not have been a result of planned parenthood allowing me to identify the causal effect of maternity leave on SED support.

I find that the reform increased membership in the SED by approximately three to four percentage points, with mean membership rates being 23 percent in the baseline pre-period. Notably, I do not observe a comparable response among men. This suggests that heightened party support was not solely attributable to the direct benefits provided to households. Instead, it implies that the policy likely served as a signal to women: the state government recognized the dual burden women faced in terms of household duties and labor market participation. By easing part of this burden, the reform conveyed attentiveness to women's lived realities—which was, in turn, met with increased political support.

Three supporting pieces of evidence align with this interpretation. First, individual-level heterogeneity exercises suggest little variation in women's responses to the reform in different subgroups. Neither highly educated women nor those facing greater time constraints reacted differently. Instead, it seems that women across social and educational groups were affected homogenously. A pattern we would expect because the policy spoke to a widely shared experience of care work across women—regardless of occupation, or status. Second, the only meaningful moderator that emerges is pre-existing daycare infrastructure: women in areas with more extensive childcare provision responded more strongly to the reform. Prior government investment likely increased the salience of the SED's commitment to caregiving and women's wellbeing. Third, post-regime surveys show that reform beneficiaries viewed gender equality in the GDR more positively. This reinforces the idea that the policy shaped how women perceived the state's stance on gender roles.

As a final exercise, I explore whether the effects of the reform extended beyond its immediate targets. If social policy acts not just as material support but as symbolic recognition, then its political effects may diffuse—in particular through shared social identities. Using the CCDB data, I find that indirectly affected grandmothers—but not grandfathers—were significantly more likely to join the SED. The magnitude of this effect is substantial: indirectly treated grandmothers are

2.5%-points more likely to become party members, reflecting roughly 60-70% of the direct effect. I also document suggestive intergenerational spillovers to the children of reform beneficiaries. Daughters born in the second half of 1976, versus those born before, express significantly stronger left-wing ideological preferences in present-day survey data (40% of a standard deviation). Notably, these differences based on birth month do not exist among respondents from West Germany nor other East German birth cohorts that were not subject to the reform. While based on cross-sectional data and thus interpreted with caution, these patterns align with the idea that recognition-based social policy can be effective in shifting political preferences that diffuse beyond those directly targeted.

These findings make several contributions to the existing literature. First, they speak to a key question in the study of social policy: why do some reforms generate lasting political support while others do not? I argue that policies which signal recognition—rather than merely distributing material benefits—can be particularly effective in shaping political behavior. This perspective helps bridge materialist and symbolic accounts of policy feedback, suggesting that recognition may serve as a crucial mechanism for political loyalty, even in the absence of democratic responsiveness. Second, the use of linked individual-level administrative data enables the analysis of revealed rather than survey-based, stated preferences, which has been the focus of many earlier empirical studies on non-democracies. It also allows for the identification of intergenerational spillovers offering new insights into how policy effects spread through familial and gendered lines. Third, the results demonstrate that even relatively light-touch social policy reforms can generate long-lasting political responses that outlive their material benefits.

The remainder of the paper is structured as follows. Section 2 develops the theoretical argument that recognition-based social policies can influence political support in autocracies. Section 3 provides the historical background of the empirical case study: a maternity leave extension in former East Germany. Section 4 describes the main data sources. In Section 5, I show how political engagement evolves alongside the transition to parenthood. Section 6 shows how maternity leave can reshape this pattern, and Section 7 explores intergenerational spillovers. Section 8 concludes.

2 Theoretical Argument

Autocratic rulers have long been understood to rely on a mix of repression and co-optation to maintain power (Wintrobe, 1998). Classic theories of authoritarian politics highlight that sheer coercion is unsustainable on its own—it is too costly and breeds resistance in the long run (Geddes, 1999; Brady, 2009). Thus, dictators seek loyalty as well as obedience. A prominent framework is the "authoritarian bargain", an implicit pact where citizens relinquish political rights in exchange for economic benefits and social welfare (Desai, Olofsgård and Yousef, 2009). In this traditional view, social policy serves a material instrumental purpose: regimes distribute resources to co-opt key constituencies and stave off unrest. However, here I argue that social policies can function as symbolic signals of recognition beyond material benefits—gestures that affirm citizens' status and worth in the political community—thereby cultivating legitimacy and quasi-voluntary support even in the absence of elections.

Early scholarship on authoritarian durability emphasized tangible distribution and patronage. Selectorate theory formalized this logic, arguing that dictatorships survive by allocating private goods to a winning coalition rather than broad public goods (Bueno de Mesquita et al., 2003). Yet, in practice, many autocracies implement fairly broad welfare programs, contrary to the expectation that only democracies do (Mares and Carnes, 2009). Historical examples, from Bismarck's social insurance to late-20th-century oil monarchies, illustrate an autocratic "social contract" trading welfare for political acquiescence. Empirical studies have also shown that these programs can meaningfully bolster regime stability: Knutsen and Rasmussen (2017) link pension programs to higher regime survival; Mohr (2023) shows housing provision increased regime popularity in former East Germany; and Magaloni (2006) documents how Mexico's PRI regime used poverty relief to co-opt popular sectors, sustaining a loyal base of support.

Yet, viewing social policies in autocracies only as payoffs for obedience risks a one-dimensional understanding. Recent studies have pushed beyond this purely instrumental narrative. For example, Lü (2014) identifies a multi-layered response to the abolition of schooling fees in China: while regime trust and legitimacy were boosted, the policy reform likewise raised expectations on the government. Similarly, Borisova, Smyth and Zakharov (2024) show that an urban renewal policy in Moscow, by involving citizens in collective decisions about housing, inadvertently fostered new social capital and some political engagement. These findings hint at a larger point: policies

can reshape state-society relations and citizen attitudes in unexpected ways, sometimes beyond a regime's initial intent.

Policy objectives may also evolve over time. For instance, social assistance in China, initially designed to alleviate poverty, gradually transitioned into a tool for state surveillance, with benefits targeted toward politically sensitive groups to preempt dissent (Pan, 2020). However, such politically selective inclusion can itself carry unintended consequences: when others are excluded from benefits, the perception of unfairness may generate frustration rather than loyalty. Emerging evidence suggests that social benefit extensions may breed resentment among citizens when they perceive themselves as unfairly excluded (Kosec and Mo, 2024; Albertus and Schouela, 2025).

These heterogenous responses relate more broadly to policy feedback theory, which holds that public policies are not only outcomes of political processes, but can also shape political behavior and citizen expectations in return (Skocpol, 1992; Pierson, 1993). Pierson (1993) distinguishes two separate mechanisms through which feedback occurs. First, resource effects of social policy may enable political engagement or change the incentive structure for doing so. Second, interpretive effects suggest that policies function as messages—they can signal information and meaning. Mettler and Soss (2004) advance this idea, arguing that policies can shape perceptions of who is a valued member of the polity. In authoritarian contexts—where formal representation is limited—such interpretive feedback may be especially potent, as policies can substitute for electoral voice by fostering a sense of inclusion and recognition.

Building on this framework, I posit that social policy can serve as a symbolic signal of recognition. In regimes where citizens lack meaningful electoral participation, such signals can substitute for the affirmation that democratic representation might otherwise confer, deepening political support. Indeed, authoritarian welfare measures often have expressive dimensions: they signal who belongs to the regime's favored community and communicate that the state "sees" and values its citizens (or certain segments of them). By extending social protections or benefits, the regime effectively says "You matter to us"—a message that can resonate on an emotional and normative level. A similar logic underpins authoritarian elections. Even when manipulated, elections can signal to the populace that governance is rooted in popular will, projecting an image of responsiveness and inclusion (Gandhi and Lust-Okar, 2009).[2] Beyond elections, autocrats have increasingly turned

[2]Recent empirical work highlights that such uncontested elections may not only signal responsiveness but lead autocratic leaders to address citizen demands in practice (Lueders, 2022).

to participatory technologies—such as call-in shows and feedback platforms—as tools to bolster popular support and cultivate an image of responsiveness (Chapman, 2021). These practices form part of a broader strategy of legitimation, widely recognized as a key pillar of authoritarian durability (Gerschewski, 2013). To this end, regimes invest heavily in narratives that portray their rule as just, effective, or benevolent (Guriev and Treisman, 2020). Social policy is an ideal vehicle to reinforce such narratives: it not only distributes resources but also distributes recognition. In turn, such symbolic recognition can enhance citizens' sense of dignity and status vis-à-vis the state (Mendelberg, 2022). Political sociologists have noted that individuals have a deep desire for recognition (Taylor, 1994); being acknowledged by the government in concrete ways (like receiving a benefit or being targeted by a development program) may satisfy this desire. In turn, such policies can engender a sense of gratitude and reciprocity, fostering what Levi (1988) terms "quasi-voluntary compliance"—a reservoir of goodwill towards the regime.

I further argue, that once a policy engenders such political support in its immediate beneficiaries, that support can diffuse across social networks, particularly within families. For example, mothers that directly benefit from specific social policies, may shape relatives' attitudes recounting personal experiences of state support. Over the kitchen table or in family gatherings, such experiences can become part of a family's political folklore. Over time, children who grow up hearing such anecdotes—or directly observe their parents' gratitude—are also likely to develop a more positive baseline attitude toward the government. This mechanism aligns with longstanding research on political learning and intergenerational transmission (Jennings and Niemi, 1968; Jennings, Stoker and Bowers, 2009; Oskarsson et al., 2022). Yet, these spillovers may resonate more forcefully when policies entail symbolic recognition that speaks to shared identities and social roles.

Importantly, the mechanisms outlined here do not require that authoritarian policymakers intentionally design social policies to generate political loyalty or intergenerational spillovers. In practice, autocrats may implement social policies for a range of strategic or pragmatic reasons—such as managing demographic shifts, increasing labor force participation, or appeasing international audiences. Yet, even when the primary intent lies elsewhere, social policies can still produce powerful political feedback effects. As long as citizens interpret these policies as signals of recognition—particularly of burdens tied to social identity—supportive attitudes can emerge and diffuse across familial lines.

Hypotheses. Based on these arguments, I distill two hypotheses.

Hypothesis 1: Beneficiaries of social policies that signal recognition of their specific social roles, identities, or burdens are more likely to increase political support for the regime than those who do not directly benefit from such policies.

Hypothesis 2: The political effects of recognition-based social policies diffuse across generations, particularly along familial lines that share social identities and roles.

3 Historical Context

To evaluate these theoretical claims, I examine a reform in which the logic of recognition-based social policy was especially salient: a maternity leave extension in the former German Democratic Republic. The GDR provides a compelling setting for this analysis, combining an explicit ideological commitment to gender equality with persistent structural inequalities in women's lived experiences—particularly in the burden of care work. Against this backdrop, the maternity leave extension offered not just material support but a symbolic gesture of recognition that spoke to women's dual roles as workers and caregivers. The remainder of this section provides historical background on women's status in the GDR, the reform itself, and the institutional landscape in which political engagement unfolded.

Women in the GDR. In 1949, when the first Constitution of East Germany was promulgated, gender equality was written into GDR law. Article 7 proclaimed that: "Men and women have equal rights. All laws and regulations which conflict with the equality of women are abolished." This commitment was rooted in both socialist ideology and a desire to distance the regime from fascist family ideals. Yet, practical concerns also played a major role. In the aftermath of World War II, labor shortages made it imperative for the regime to facilitate high female labor force participation—which by 1976 had already surpassed 80% (Wyrwich 2019). Educational attainment followed a similar trend: by 1989, women made up nearly 40% of graduates from higher education institutions (Ross 1999).

Despite rapid advances in education and labor force participation, women's progress in the GDR was far from uniform. Many women were employed in roles for which they were overqualified, and few ascended beyond lower-level managerial positions (Ross 1999). Structural inequality was

even more pronounced in the domestic sphere. Time-use surveys from the 1960s and 1970s show that mothers shouldered around 80-90% of all household labor—amounting to an additional 30-40 hours per week spent on childcare and domestic tasks (Albrecht 1969, Obertreis 1986, p. 309). This sharp imbalance stood in contrast to the regime's ideological commitment to gender equality and likely contributed to the steep fertility decline observed during the 1960s. In practice, the burden of care remained largely invisible to the state's formal narratives—setting the stage for policies that could later function as recognition of that burden.

Reform. At the 9th Party Congress in May 1976, the SED announced a sweeping package of social policy reforms, formally adopted in mid-June. These included higher minimum wages, increased pension payments, and targeted improvements in support for women. A central objective was to address declining fertility: while the total fertility rate had been 2.5 children per woman in the mid-1960s, it had dropped sharply to 1.6 by the early 1970s (Kaminsky, 2017; Conrad, Lechner and Werner, 1995). Among the most consequential measures was a generous expansion of maternity leave—later cited by women as the most important policy influencing fertility decisions (Speigner, 1981). Though embedded in a broader agenda, the policy stood out both in its immediate relevance and its symbolic resonance to women.[3]

The reform took effect in mid-June 1976. It raised post-birth maternity leave from 12 to 20 weeks, with full wage compensation for all mothers. In addition, working women were granted up to one year of leave following the birth of a second or subsequent child, with compensation set at sickness allowance rates—typically between 70% and 90% of prior earnings (Adams, 1989; Braun and Klein, 1995). The duration of paid leave was often a binding constraint: once benefits ended, mothers generally returned to work, and their children entered state-run nurseries (Israel and Kerz-Rühling, 2008). While the policy was initially designed to boost fertility, the regime framed it as a symbol of its attentiveness to women's needs. A quote from the key SED newspaper, *Neues Deutschland* (28.8.1976), read:

> "It is only natural that this support, which is given to mothers and their families, resonates greatly among all workers. Among women themselves, it has bolstered their commitment to making significant contributions to the socialist society."

[3]Likely as a result of the numerous policy interventions, total fertility returned to approx. 1.9 children per woman in the late seventies (Conrad, Lechner and Werner 1995).

Given that the quote originates from a party-controlled newspaper, it should be read less as a reflection of public opinion and more as an articulation of the SED's intended messaging around the reform.[4] Nonetheless, the historical literature has similarly noted the broader shift in improving women's conditions through social policy as a clear attempt to legitimate the SED rule (Hockerts 1994, Schmidt 1999).

Importantly, other policy measures introduced at the same time did not differentially affect second or higher time mothers depending on when their children were born. For instance, the reform also included the roll-out of a 40-hour work week for women with at least two children. However, this applied to all mothers of two or more children, regardless of the particular birth dates.[5]

SED. The main governing party in former East Germany was the SED, which controlled almost all aspects of socioeconomic life. This entailed that membership was often a prerequisite for individual career progress to higher ranks within the system (Gebauer 2003). At its peak, almost every sixth person in the GDR was a member of the SED (Christian, Gieseke and Peters 2019, p. 23). However, entering and staying in the SED was a costly effort. Becoming a member one needed to acquire two recommendations from experienced members, publicly state adherence to the socialist SED statutes, and pay monthly membership fees.[6] Membership fees were determined based on pre-tax income and ranged from 0.5% to 3% of an individual's salary (Schneider 1977). Moreover, membership was typically a time-consuming effort. Besides having to attend local party meetings, it was often tied to secretarial work, campaign assistance, and local community work e.g. as members of housing commissions (Pannen 2018, p. 10). Reportedly individuals did not enter the SED precisely because they feared having to give up regular leisure activities (Christian, Gieseke and Peters 2019, p. 76). Taken together, one can interpret SED membership as a costly act of reciprocity towards the *SED-state*.

Mass Organizations. The second pillar of socio-political life in the GDR were mass organizations. These were large voluntary associations of citizens to pursue different interests such as sports, social and cultural life (Koelges 2001, p. 54). Mass organizations served a dual purpose. On the one hand, they functioned as instruments of regime control, enabling the SED to mobilize the population and reward loyal elites with privileges. On the other hand, they also gave citizens the

[4] In Appendix A, I provide further newspaper impressions on the policy reform.

[5] In 1986, one-year maternity leave was also extended to mothers of first-born children, but the state substantially increased family allowance benefits at the same time (Adams 1989).

[6] Becoming a member of the SED was a two-step process. Individuals were first listed as candidates and confirmed after a year. Membership duties did not differ between the two, candidates were only missing specific voting rights.

possibility to inconspicuously engage in socio-political activity without having to enter the SED (Eppelmann et al., 1996, p. 547).

One of the largest mass organizations was the Democratic Women's League (DFD). Towards the end of the GDR, approximately 1.5 million citizens were members of the DFD, roughly every sixth woman (Jurich 2006, p. 32). Its goals were manifold—among others, the DFD strove for peace, equal rights, and healthy family values. Daily activities constituted the organization of reading groups, art exhibitions, and sports groups as well as the provision of educational services on parenting (Koelges 2001, p.64-74). DFD branches at firms organized meetings, discussion circles, and coordinated female representation at the workplace (von Friesen 1972, p.18; Pritchard 2019). Hence, time commitments were comparable to party work for actively involved members.[7] While the DFD clearly acknowledged SED leadership, its activities were characterized by a fairly apolitical character and distinct from other organizations many of the high-ranking positions were filled with non-SED members (Koelges 2001, p.64-74).

4 Data

To empirically assess how the maternity leave reform shaped political behavior in the GDR, I draw on various data sources. This section describes data records and key variables used in the analysis.

CCDB. The main dataset I leverage, is the so-called Central Cadre Database (CCDB). The CCDB was the result of GDR regime efforts to collect and document information on present and future cadres. Starting in 1972 rich data covering sociodemographics, education, memberships, and many more individual characteristics were collected to regularly produce statistical overviews of the state's labor force (Best and Hornbostel 2003). Concretely, the presidency of the East German Council of Ministers decreed in 1983 that the CCDB should include the universe of employees in central and local bureaucracies, leading personnel in large enterprises, cadres in ministries,[8] firms engaged in foreign trade, and selected individuals from institutions of higher education (Remy 2003).[9] Primary data collection was first conducted at the workplace and subsequently merged

[7] DFD membership fees were, however, negligible. On average, fees were 61 cents, while the mean income in 1975 was approximately 889 GDR-Mark (DDR 1990).

[8] Only the ministries of defense, home affairs, and state security were not subject to data collection.

[9] In practice, approximately half of the individuals can be classified as simple employees, e.g. administrative staff and chauffeurs, whereas the remainder consists of individuals with mid to high ranking positions within the East German system, e.g. CEOs, professors and state secretaries.

into a large digital database. In Appendix Figure A2, I show an annotated excerpt of an original data entry form. During regime transition, much of the data documentation was lost but carefully reconstructed by archivists in the 1990s (Rathje 1996).

While the potential purposes were manifold, the data was leveraged only seldom during the existence of the GDR (Best and Hornbostel 2003). Here, I rely on the last wave of data collected in 1989 before the demise of the GDR. Focusing on the last wave is standard in the existing yet scarce literature and the result of data quality considerations (see e.g. Best and Hornbostel 2003, Salheiser 2005, de Juan, Haass and Pierskalla 2021). Crucially, the data in the last wave contain biographic histories allowing me to reconstruct a yearly panel dataset covering approximately 370,000 individuals. This represents 4.4% of the working population in 1989 who accounted for 7.2% of all childbirths in 1976.[10]

For the main analysis, I focus on the subsample of women who gave birth to a second or higher child in early vs. late 1976, assuming differential treatment intensities in both subsamples. This sample definition reduces the number of individuals to 2,795. The reason I focus on this coarse sample of women is to ensure similarity in life circumstances. Comparing baseline observables in the year prior to the reform suggests similarity along key dimensions (see Table 1 and discussion in Section 6). To capture dynamics over time, I restrict the data to a balanced panel covering eight years before and after policy implementation (1968-1983). The main outcome of interest is a membership dummy in the SED which takes the value of one for all years after party entry.[11] Summary statistics for all individuals in the CCDB as measured in 1989 are reported in Appendix Table A1.

A key limitation of the data is that individuals were surveyed through employing institutions in each wave (Best and Hornbostel 2003). This means that one needed to be affiliated with an employer in 1989 to appear in the CCDB.[12] Analyses in which treatment directly affects the likelihood of being listed with an employer in 1989 may suffer from selection bias. I discuss this concern in greater detail in Section 6.1.

LV. To corroborate the main analysis, I draw on survey data on attitudes collected shortly after

[10] Own calculations based on DDR (1977) and DDR (1990).

[11] Except for one observation membership is an absorbing status.

[12] This does not necessarily mean an active employment status, but includes employees on temporary leave (e.g. parental leave, military service, higher educational studies). Note that information on temporary leaves only exists for 1989, not prior years, and are not included in the anonymized data made available by the Bundesarchiv.

reunification. The survey called Lebensverlaufsstudie (LV) was run in 1991/1992 by the Max-Planck-Institut für Bildungsforschung in Berlin (Mayer 1995). Individuals were sampled from five different birth cohorts and drawn from the citizen registry of the GDR. To capture changes in attitudes, I focus on a survey question in which individuals had to evaluate gender equality in the GDR. In particular, I construct a sample consisting of all individuals who had their second child born five years prior to extended leave introduction versus five years after.

The LV data, broadly representative of the GDR population in 1989, can also help characterize how selected the CCDB sample is. In Appendix Table A2, I provide summary statistics for harmonized variables including education, sociopolitical background, and demographics. CCDB members were more likely to have completed ten years (or more) of schooling (68% versus 47%) and to have attained a higher educational degree (26% versus 14%). They were also more likely SED members (48% versus 21%) and so were their parents (for fathers 27% versus 21%, and mothers 12% versus 8%). Hence, individuals in the CCDB were generally positively selected on socioeconomic background and government support. Estimates should be interpreted against this backdrop.

ALLBUS. Finally, to investigate intergenerational effects I use ALLBUS survey data from present-day Germany (GESIS 2024).[13] ALLBUS is a large-scale repeated cross-sectional survey. To study ideological transmission, I focus on respondents living in former East German states born between 1975 to 1976 in Germany, where the main outcome variable is ideological positioning on a scale from one (left) to ten (right).[14] While the data is cross-sectional and I cannot control for individual fixed effects, I use individuals living in West Germany and other, untreated, East German birth cohorts for placebo exercises.

5 Women and Socio-political Participation

To set the stage for the causal analysis I begin with a descriptive exercise. In particular, I probe the idea that once women enter motherhood, government support declines, potentially due to the differential increase in household burden. To explore this, I implement event-study models in the spirit of Kleven, Landais and Søgaard (2019), where the event of interest is the birth of a first child.

[13] I supplement the 1980-2021 waves (GESIS 2024) with the 2023 wave (GESIS 2025).

[14] For sample size reasons, I include children born in 1975 in the control group. I also only include individuals born within present-day German borders.

More specifically, I estimate the following equation

$$Y_{ist}^{g} = \sum_{j \neq -1} \alpha_j^g \cdot \mathbb{1}[j = t] + \sum_k \beta_k^g \cdot \mathbb{1}[k = age_{is}] + \sum_y \gamma_y^g \cdot \mathbb{1}[y = s] + v_{ist}^g \tag{1}$$

where Y_{ist}^{g} is the outcome of interest for individual i of gender g in year s at event time t. The α's capture the dynamics of interest while the β's and γ's capture age and year fixed effects, respectively.

Results. Figure 1 Panel (a) shows a clear gap in SED membership for women after giving birth to their first child relative to those who become mothers later in life. This decline is not short-lived, in fact, the gap grows over the first ten years and persists well beyond. In contrast, men exhibit a very different pattern—their probability of becoming an SED member continuously grows, even in the year in which men enter parenthood.

Can this gender gap be explained simply by time constraints introduced by child-rearing? To answer this question, I conduct the same descriptive exercise, but with membership in the Women's League (DFD), the main mass organization for women in the GDR, as the outcome. If women are simply not left with enough time to participate in socio-political activities, we would expect similar patterns for disengagement in this organization. Figure 1 Panel (b), however, shows the opposite. Once women become mothers, they start joining the Women's League at sizable rates. Taken together, these patterns suggest a gendered divergence in political behavior around the transition to parenthood. Rather than signaling general withdrawal from socio-political life, the decline in SED membership among new mothers appears to reflect a shift away from the ruling party specifically—a form of political distancing tied to the experience of motherhood.

6 Maternity Leave and SED Membership

Motherhood is associated with a decline in SED membership among women. In this section, I show that targeted social policy can mitigate this decline by alleviating the burdens of parenthood and signaling state recognition of women's roles.

6.1 Empirical Approach

To estimate the effect of maternity leave on SED membership, I employ a difference-in-differences strategy. Because of the sudden announcement and introduction of extended maternity leave in June 1976, I focus on women who gave birth to a second or higher child in that same year. The first difference I rely on is the month of birth in 1976. Specifically, I define women who gave birth for the second (or higher) time from January to June as control and those women giving birth for the second (or higher) time from July to December as treated.[15] The idea is that women in the control group were less likely to experience extended maternity leave compared to the treatment group as a result of lags in the policy implementation (imperfect retrospective enactment). The second difference I use for identification is time. That is, I compare women's party membership before 1976 versus afterward. The identifying assumption is that in the absence of the policy reform women in both subsamples would have followed parallel trends in party memberships. More formally, I run regressions of the following type:

$$Y_{it} = \beta \text{Post76}_t \cdot \text{Treat}_i + \theta_i + \gamma_t + \varepsilon_{it} \tag{2}$$

where the main outcome is a membership dummy in the SED in year t of individual i. Post76_t equals unity for 1976 and all subsequent years. Treat_i equals unity for all women who gave birth to a second, third, or higher child between July to December 1976. The β-coefficient in this setting captures the causal effect of higher maternity leave exposure on SED membership. Since I do not observe actual treatment take-up, that is, whether an individual takes extended maternity leave, we can think of the coefficient β as the intention-to-treat effect in an IV-framework. The remainder of Greek letters capture individual and year fixed effects as well as the error term. In addition to this static specification, I also estimate dynamic difference-in-differences models:

$$Y_{it} = \sum_{j \neq -1} \beta_j \cdot \text{Treat}_i \cdot \mathbb{1}[j = t] + \theta_i + \gamma_t + \varepsilon_{it}. \tag{3}$$

Here, the individual estimates for the β_j's allow me to identify trends in the years before the reform.

Although the CCDB does not represent a random sample of the GDR population, concerns about

[15] In case a woman gave birth in both periods, she is defined as treated.

selection bias are mitigated as I exploit within-individual variation over time. Still, one potential issue is that the data was collected through employers in 1989. If treatment directly affects the likelihood of being listed with an employer in 1989, the estimation may suffer from selection bias. Historical and empirical evidence, however, suggests this is unlikely. First, the historical record does not support the notion that maternity leave significantly affected women's long-term employment outcomes (Merkel 1994).[16] Second, in Appendix Table A3, I show that women who gave birth to their second child after the reform were, if anything, more likely to be employed in 1989 than their pre-reform counterparts. Here, the data are drawn from the *Lebensverlaufsstudie*, which sampled individuals directly from the GDR citizen registry. Yet, in line with the leave extension, treated women have on average accumulated half a year less of employment experience over their life cycle. Finally, in Appendix Table A4, I also find no evidence of a greater probability of switching sectors within the CCDB database.[17]

Treatment. For identification, I assume differential treatment intensities in the defined control and treatment groups. This is consistent with existing evidence documenting the importance of information diffusion for the take-up of social benefits: knowledge about new policies spreads over time.[18] To quantify the difference in treatment intensities, I draw on aggregate expenditure data. Recall, that maternity leave was extended for all mothers of newborns by eight weeks (from 12 to 20 weeks), yet second or higher time mothers saw an extension to a full year (i.e. an additional *seven months* besides the eight weeks). The addition of these seven months was separately introduced as a new spending item in government expenditure data (DDR 1981): while in 1976 a total of 44.8 mio East German mark was spent on the extension, this amount grew to 154.8 mio mark in 1977—an increase of 246%.

To assess whether the 246% spending increase reflects differential treatment take-up, I construct a simple counterfactual under the assumption of *perfect* retrospective implementation. First, assume for simplicity constant birth rates across months in 1975-1977. For each birth month, calculate the total number of eligible leave months separated by the years in which the payout happened.[19] The mother of a child born e.g. on the first of February 1976 could have received a total of one-year maternity leave. The first 20 weeks would have entered the usual maternity leave expense item, the next six months until the end of the year would have entered the new spending item for 1976

[16] In 1989, 92.4% of all working-age women (16-60 years old) were employed (Kaminsky 2017, p. 97).
[17] Note, that this cannot account for job switches to non-CCDB employers.
[18] For a recent survey, see Ko and Moffitt (2024).
[19] Illustrated in Appendix B.

and the expenses for January the following year would have been registered in 1977. I proceed by calculating all maternity leave months for each birth month cohort and assigning them to 1976 and 1977 accordingly. Under the assumption of perfect retrospective treatment take-up and constant births, we would then expect a relative increase in the new expenditure item by only 100%. Hence, I conclude that retrospective enactment was not perfectly implemented.

In a counterfactual scenario, assume a constant partial treatment take-up rate for all mothers giving birth before July 1976 and full treatment take-up for all mothers giving birth later. To match the spending increase of 246% from 1976 to 1977, the implied partial treatment rate must be 52%. Relaxing the constant childbirth assumption accounting for the annual number of second (or higher) time births, the matched partial treatment rate is approximately 59%. In either case, the back-of-the-envelope calculations suggest a substantive difference in treatment take-up. However, to remain cautious, I also probe sensitivity with respect to the control group. In particular, I provide results in which I define women who gave birth in early 1975 as the control group. These women gave birth sufficiently far in advance such that eligibility for maternity leave expired by the time the reform was implemented. The key reason I focus on women giving birth only in 1976 for the main exercise is to ensure greater similarity in observed and unobserved covariates between the control and treatment groups.

6.2 Main Finding

To elicit the influence of maternity leave on SED membership I begin with a simple graphical analysis comparing mean party membership rates among women more and less likely to have been affected by the reform. Figure 2 Panel (a) shows that prior to the reform, the two groups followed parallel trends, even for women giving birth in late '75 and early '77. If anything, women who gave birth before the reform, the control group, are slightly more likely to be SED members. However, this pattern reverses with the introduction of maternity leave in 1976. Women who gave birth once leave was enacted are suddenly markedly more likely to enter the SED.

To support a causal interpretation of this baseline pattern, I show in Table 1 that women in the main control and treatment groups are similar along key dimensions in the year prior to the reform. Among twenty variables only two appear to be, both, quantitatively and significantly different between the two samples. First, women in the control group (those who gave birth in the

early half of 1976) are on average slightly older than those giving birth later. Yet, this difference occurs by construction. Comparing the exact age at the birth of the second child yields no significant difference. Second, women in the control group are ex-ante more likely to be members of the mass women's organization. It is not entirely clear how these differences may correlate with the treatment and outcome, yet to remain cautious I include a saturated set of controls capturing year-by-birth-year and year-by-covariates[20] fixed effects and run a formal event study regression.

The estimated effects are presented in Figure 2 Panel (b) and confirm that the uptake in SED membership relative to the control group is robust to the inclusion of an extensive set of controls. In Table 2, I show estimates in the static difference-in-differences setup. Across different specifications, I estimate an effect of leave on SED membership of around three to four percentage points with mean membership being 23% in the baseline pre-period 1975. These findings provide causal evidence that social policy—when it visibly eases burdens tied to social identity, such as care work—can enhance political support in authoritarian regimes.

6.3 Robustness

To substantiate the main finding, I conduct a series of robustness checks.

Placebo. First, in a falsification exercise, I impose placebo policy reforms in each of the five years prior to 1976 and five years after. For each placebo reform, I focus on all women giving birth for the second time in a given year. Akin to the main specification I define the treatment group as those mothers who gave birth in the second half of a given year.[21] Each sample period is restricted to eight years pre- and eight years post-birth for consistency with the main analysis. The placebo treatment period is defined as the later eight years. In Appendix Figure A3 Panel (a) I show estimates for second-time mothers and Panel (b) depicts estimates for first-time mothers. Among second-time mothers, placebo policies across all years are insignificant and close to zero. Among first-time mothers, the placebo estimates are more volatile. This is consistent with more profound changes in life circumstances surrounding first childbirth. Yet, the overall pattern suggests that the estimated effect in 1976 for second-time mothers is a clear outlier and not a mechanical result.

[20] I use covariates measured for 1975 including fully interacted dummies for occupational and higher education, pre-recorded membership in the women's organization, and social background (worker, intelligenz, service). The intelligenz/intelligentsia is broadly defined as the intellectual and academic class.

[21] To avoid women appearing in multiple placebo samples, I only use second-time mothers and not third, fourth etc.

Control Group Sensitivity. Second, I show robustness to an alternative definition of the control group. In the baseline design, I compare women giving birth in early 1976 versus late 1976. I focus on this coarse set of individuals to ensure similarity along key dimensions. However, since some women giving birth in early 1976 were partially treated one may worry about a direct SUTVA violation. To address this concern, I exchange the control group (early 1976 mothers) with women giving birth for the second or higher time in early 1975. These women gave birth sufficiently far in advance such that eligibility expired by the time extended leave was implemented. The results of this alternative specification are presented in Appendix Table A5 and Figure A4. The estimates suggest an effect of leave on SED membership in the ballpark of 4-6%-points. These somewhat larger estimates are in line with the expectation that the difference between fully treated and partially treated women should be smaller than full treatment versus no treatment. Nonetheless, this exercise does not rule out other types of treatment spillovers.

Birth-specific age profile. Finally, one may be concerned about the fact that the control group gave birth earlier in 1976. Hence, there was less time for these women to join the SED before child-bearing in 1976. This concern should be mitigated by the fact, that the average difference in SED membership between women giving birth for the first time and a year before first child-bearing is quantitatively very small (half a percentage point, see Appendix Table A6). Moreover, since SED membership is an absorbing state I test for differences in the survival probability across all years in the post-period. Appendix Figure A5 confirms that treated women are more likely to have entered the SED at each analysis period and the differences in survival probability are statistically significant.

6.4 Mechanism

The extension of maternity leave is associated with a measurable increase in SED membership among women. I argue that this effect is driven not just by material benefit, but by the policy's symbolic value: it signaled the party's attentiveness to women's specific needs and burdens. This recognition, in turn, fostered a greater willingness among beneficiaries to cooperate with and support the state. If this mechanism holds, we should expect treated women to hold more favorable views about gender equality in the GDR—reflecting a perception that the regime was responsive to their lived realities.

Survey Evidence. In line with this argument, I begin by providing suggestive evidence based on survey data (LV) collected after the demise of the GDR. In particular, I focus on a survey item that asked respondents about their perceptions of "equal rights for men and women." I construct a dummy equaling unity for all those survey participants indicating that equal rights were "better in the former GDR." The remainder either responded with indifference or perceived equality as better in the new federal republic. To test whether exposure to the maternity leave reform shaped retrospective views on the state's stance on gender equality, I compare individuals who had a second child before versus after July 1976, allowing for differential effects by gender. The following equation summarizes the estimation strategy:

$$Y_i = \alpha \text{Female}_i + \beta \text{BirthPost76}_i + \gamma \text{Female}_i \cdot \text{BirthPost76}_i + \varepsilon_i \tag{4}$$

where Y_i represents individual i's survey response. Female_i is a simple gender dummy equaling one for women and BirthPost76_i is an indicator equal to one if individual i had their child born after July 1976. The total sample consists of all individuals having a second child in the five years prior and five years post-reform implementation. ε_i denotes the error term.

Roughly two-thirds of survey respondents in the sample agree that gender equality was better in the GDR. At first glance, maternity leave exposure does not appear to significantly shift these views: the average difference between pre- and post-reform respondents is negligible (column 1 in Table 3). However, this masks important heterogeneity by gender. Column 3 shows that women who gave birth to a second child after the introduction of extended maternity leave are significantly more likely to report that gender equality was better in the GDR, compared to those who gave birth before. The estimated correlation is quantitatively meaningful—equivalent to approximately one-quarter of the baseline mean—and remains robust after including covariate controls such as cohort, education and sector fixed effects (Columns 4 and 5). In contrast, men show no comparable response; if anything, their views are slightly more negative, though not significantly so.

These results suggest that the maternity leave reform influenced how women perceived the GDR's stance on gender equality by easing the disproportionate burden of childcare. Notably, this divergence in attitudes appears among women who otherwise shared a common political and social environment under the GDR. The only meaningful difference between them is the timing of their second child—and thus whether they were exposed to the reform. That such timing is associ-

ated with differing views on gender equality, even after the regime's demise, points to the lasting influence of recognition-based social policies.[22]

Alternative explanations. While the findings, thus far, are consistent with a recognition-based interpretation, several plausible alternative explanations merit consideration. One concern is that women may have joined the party to secure particularistic benefits, such as career advancement—long associated with SED membership. However, this incentive was present for both the treatment and control groups, and thus cannot by itself explain the differential response. Still, career-related motives might interact with treatment in subtler ways. For instance, it is possible that both groups wished to join the SED, but only treated women had sufficient time—thanks to extended leave—to follow through. Alternatively, women may have perceived a risk of professional penalty for taking leave and joined the party to preempt or offset that risk. In what follows, I present empirical evidence that speaks against both of these time- and career-based explanations.

Heterogeneity. If time constraints were the primary barrier to SED entry, we would expect the effect of maternity leave to vary with the intensity of childcare responsibilities. Specifically, women with more children, who face greater time demands, should exhibit stronger responses to the leave extension.[23] However, Table 4, Column 2, shows no evidence of such moderation. While women with more children are generally less likely to join the SED, the effect of the reform does not vary meaningfully with family size. This suggests that time availability alone is unlikely to explain the observed treatment effect.

Next, I test whether career incentives drive the results. If women joined the SED primarily to enhance professional advancement, we would expect women at different career stages to react heterogeneously to leave. To explore this, I leverage career information derived from the GDR's nomenclatura system, a hierarchical classification of leadership positions in state and economic institutions (Schneider, 1994, p. 64-65). I construct a binary indicator for whether individuals were officially listed in the nomenclatura by 1975.[24] As shown in Column 3, the estimated treatment effect is somewhat higher for nomenclatura-listed women, but the effect for non-listed women—3.5 percentage points—remains nearly identical to the baseline estimate of 3.9 percentage points.

[22] These results also echo qualitative work. Finzel (2003) concludes based on in-depth interviews "that after unification conditions to combine motherhood and paid work had become more difficult because of the considerably shorter fully paid maternity leave and the lack of secure employment".

[23] In the late 1960s, mothers of two children reported spending 11.6 hours per week on childcare while mothers of four kids spent 14.2 hours (Albrecht 1969).

[24] Roughly 15% of the women in the sample were classified as such (see Table 1).

I also examine heterogeneity by education. If career ambition correlates with educational attainment, one might expect stronger treatment effects among more educated women. Yet Column 4 reveals no significant difference in responsiveness between lower- and higher-educated women, further undermining a career-based explanation.

Finally, I examine whether the political response to extended maternity leave varied by pre-existing daycare infrastructure for infants and toddlers. Theoretically, the direction of this heterogeneity is ambiguous. On the one hand, the reform might be most impactful in low-daycare-density regions, where leave provided essential relief in the absence of other state support. On the other hand, the signal of attentiveness to women's needs may have carried higher credibility in high-daycare-density regions—areas where the state had already demonstrated a commitment to supporting working mothers. Rather than providing isolated relief, the leave extension could be interpreted as part of a broader, credible commitment to gender equality.

To test these hypotheses, I calculate the number of daycare facilities per birth at the district level in 1976,[25] and match this information to individuals based on their last recorded place of employment in the CCDB.[26] I then test for differential effects of leave on SED membership across high- and low-density districts. The results support the recognition hypothesis: in high-daycare-density regions, the leave extension increases SED membership by 6.5 percentage points, while in low-density regions, the effect is a small, statistically insignificant 1.9 percentage points. This suggests that the political impact of maternity leave was strongest where it reinforced existing state commitment to supporting women. Importantly, this pattern also indicates that the reform was not primarily valued for its material benefit. If the policy had been mainly attractive for this reason, we would have expected stronger responses in regions with less daycare coverage, where the leave extension filled a more acute gap in support. Instead, the stronger effects in high-coverage areas are more consistent with the symbolic and expressive dimension of the reform.

Other outcomes. To further assess the time constraint hypothesis, I test whether extended maternity leave affected women's participation in other socio-political activities, specifically mass organizations. If lack of time were the primary barrier to political engagement, we would expect leave to increase involvement in these organizations. Figure 3 presents dynamic difference-in-differences estimates. Panel (a) shows that leave has no significant impact on the number of mass

[25] District-level data ($n = 15$) comes from DDR (1977).

[26] Unfortunately, the CCDB does not provide previous location histories. I assume that maternity leave exposure did not affect in which district an individual was working in 1989.

organization memberships, while Panel (b) indicates no meaningful change in DFD membership. If anything, treated women show a slight, insignificant decline in participation.

I also examine whether leave exposure influenced career progression. In Panel (c), I test for effects on nomenclatura enlistment—a marker of higher-status positions—and find no significant impact. If anything, leave slightly reduces the probability of such advancement. Together, these results provide further evidence against time constraints and career benefits as the primary mechanisms driving SED membership. Instead, they align more closely with the recognition-based interpretation.

Fathers. To further probe the mechanisms underlying the political effects of maternity leave, I turn to an analysis of men who became fathers for the second or higher time in 1976. Studying fathers serves two important purposes. First, it provides a direct test of whether the political response to the reform was driven by household-level factors, such as income stabilization or a broader sense of household support. If the main driver of increased SED membership among women was simply an improvement in household welfare, we would expect to see a comparable effect among fathers. Second, fathers offer a useful comparison to test the recognition mechanism. If the effect on women primarily reflects a sense of being seen and valued by the state in their caregiving role, this recognition effect should be far weaker among men whose social identity was less directly tied to domestic responsibilities.

Following the same empirical approach used for mothers, I construct the sample using fathers who had their second or higher child in 1976. Figure 4 presents the dynamic difference-in-differences estimates, while static results are shown in Appendix Table A7. Across all specifications, the results are clear: fathers do not exhibit any significant change in SED membership as a result of the reform. The estimated effects are consistently small, statistically insignificant, and vary in sign depending on the model specification.

This null result is theoretically informative. First, it challenges the idea that the political impact of maternity leave was simply a matter of household welfare. If financial support were the primary driver, we would expect fathers—who also benefit from household income stability—to exhibit similar increases in SED membership. Second, the absence of a response among fathers weakens an alternative interpretation that the reform served as a broader signal of the state's commitment to expanding social benefits. If mothers joined the SED because they expected greater social protections going forward, the same logic should apply to fathers. Instead, the null effect among

fathers strengthens the recognition-based interpretation: the reform's political impact was driven by its specific resonance with women's social roles and burdens.

7 Intergenerational Transmission of Ideology

The maternity leave reform appears to have significantly shaped women's political attitudes and perceptions of the state, but did these effects extend beyond the immediate beneficiaries? In this section, I explore the idea of intergenerational transmission of ideology—whether the attitudinal shifts observed among mothers also affected those close to them.

7.1 Effects on Grandparents

Grandparents provide a compelling test case for the diffusion of political attitudes because they were not direct beneficiaries of the maternity leave reform, yet they were closely connected to those who were. To quantify the effects on this older generation, I use data from the CCDB. The database includes entries for SED membership of individuals' parents as recorded in 1989. This allows me to run simple cross-sectional regressions where the outcome is a binary indicator of whether grandparents were SED members. Akin to the main analysis, I define the control and treatment groups based on the month of birth of the second or higher child in 1976—those born in the first half of the year (January to June) serve as the control group, while those born in the second half (July to December) are the treatment group.

Results. The results of this exercise are presented in Table 5 Panel (a). I find that grandmothers in the treatment group are significantly more likely to be SED members in 1989, with an increase of approximately 2.5%-points. The intergenerational effect is of sizable magnitude, that is, about 60-70% of the direct effect. However, I find no comparable increase in SED membership among grandfathers.

These results are consistent with the theoretical argument. In the GDR, both parents and grandparents were comparatively young, meaning grandparents were typically still in the active labor force when they became grandparents. As a result, they were relatively less involved in sharing the childcare burden (Zwiener 1994, p.73). It is unlikely that the increased support for the SED was directly due to experiencing the childcare benefits of the reform. Instead, the most plausible expla-

nation is that grandmothers developed more favorable attitudes toward the SED through social interactions, conversations, and shared stories with their daughters. Importantly, this diffusion was gendered. The reform spoke directly to the shared social identity of mothers and grandmothers, both of whom were familiar with the dual burden of work and caregiving.

To further validate these results, I conducted placebo exercises imposing reforms in other years than 1976. The static estimates for this exercise are depicted in Appendix Figure A6 Panel (a). Here, each placebo reform sample consists of the women giving birth to their second child in a given year.[27] Across five placebo reforms both before and five after 1976, the same baseline specification yields non-significant effects close to zero.

7.2 Effects on Children

Finally, I investigate whether the impact of the maternity leave reform extended to the next generation, the children directly exposed to it. To answer this question, I draw on survey data collected after the demise of the GDR. Specifically, I investigate whether individuals living in East German states born in July to December 1976 differ in their ideological positioning from those who were born in the three preceding semesters.[28] The outcome variable is constructed from a survey item that asked individuals to rank their ideological position on a scale from one (left) to ten (right). I invert the measure such that higher values reflect more left-wing ideology and standardize the outcome for ease of interpretation.

Results. The results of this analysis are presented in Table 5 Panel (b). I find that individuals who were more likely to have been exposed to maternity leave report more left-wing ideological positions, but insignificantly so (Column 1). Yet, restricting the sample to daughters only reveals pronounced and marginally significant shifts in ideology. The effect size is relatively large matching approx. 40% of a standard deviation (Column 2 and 3). This suggests that growing up in an environment shaped by a recognition-based social policy can leave lasting ideological imprints. One plausible explanation for the gendered response is that mothers who directly benefited from the reform may have internalized a positive view of government support, especially toward their daughters. These mothers may have communicated this perspective to their daughters more

[27] This differs slightly from the baseline (second *or higher time* mothers), such that women do not enter multiple samples.
[28] The reason I focus on two years instead of one is the very limited sample size. However, this allows me to control for seasonality, i.e. being born in the first versus second half of a year.

strongly, emphasizing the value of state recognition and support for women's roles. This echoes recent work by Farré et al. (2023), who find that the introduction of only 13 days of paternity leave significantly (0.26 SD) increased gender-egalitarian attitudes among affected children in Spain.

To further validate these findings, I test the same specification on individuals living in non-East German states, who weren't subject to the GDR's maternity leave reform. I find small and insignificant differences, partly with opposite signs (Columns 4-6). As before, I also investigate placebo reforms in five pre- and five post-1976 periods using the baseline specification (Column 1). Results are shown in Appendix Figure A6 Panel (b). Again, I fail to uncover systematic patterns of differential ideology in response to hypothetical reforms in other years.

Taken together, these results provide suggestive evidence that the 1976 maternity leave reform had long-lasting ideological effects, not only on the immediate beneficiaries but also on the next generation.

8 Conclusion

Social policies are a popular tool for cultivating political support in autocracies. Existing research has shown that such policies can strengthen regime legitimacy, but this paper advances our understanding by highlighting a critical mechanism: social policies are particularly effective when they signal state recognition of citizens' specific needs, burdens, and social identities. Such recognition can substitute for the sense of affirmation and inclusion that democratic elections typically provide, fostering political support even in the absence of genuine electoral choice.

I test this argument using the case of a 1976 maternity leave reform in the former German Democratic Republic (GDR). The findings support this perspective: the reform, which directly addressed the dual burden of work and caregiving experienced by women, significantly increased their likelihood of joining the ruling SED. In contrast, men, who did not face the same caregiving responsibilities, showed no comparable political response. This gendered response points to the reform's symbolic value: it was not merely a material benefit but a recognition of women's specific burdens and contributions, signaling that the state saw and valued their roles both at home and in the workforce.

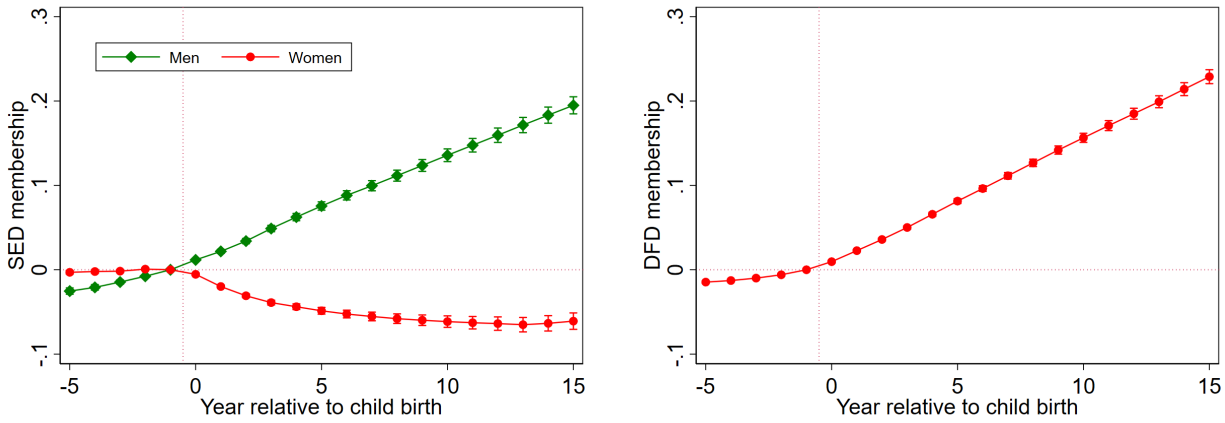
Additional evidence speaks against alternative explanations. The increased political support among

women cannot be attributed to general household welfare improvements, reduced time constraints, or enhanced career incentives. Women did not substantially increase their participation in other mass organizations, nor did they experience notable career advancements as a result of the reform. Instead, the effect was strongest in regions with well-established daycare infrastructure, where the state's commitment to supporting women's dual roles was most credible.

I also find supporting evidence of gendered spillovers across family networks. Grandmothers of women who directly benefited from the policy were significantly more likely to join the SED, suggesting that the recognition effect diffused through shared social identities. Similarly, daughters of affected mothers were more likely to adopt left-wing ideological positions in adulthood. These findings offer a novel, micro-level perspective on the long-lasting legacy of autocratic social policies, demonstrating how recognition-based policies can generate political support that spreads across generations.

Arguably, paid leave is a highly successful policy—only seven countries in the world remain without nationally paid maternity leave (Miller 2021). The results here provide a plausible reason for its wide adoption: citizens reward leaders who implement it with increased political support. From the setting at hand, we learn, that not only large-scale reforms induce such reciprocity, but also more incremental social policy amendments.

Figure 1: Birth of the first child and socio-political participation.

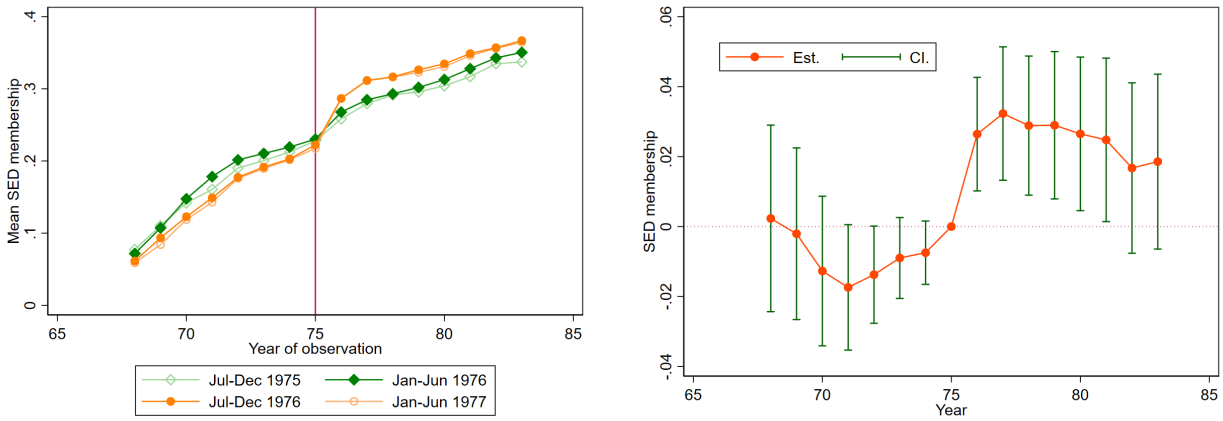


(a) Event study estimates: SED

(b) Event study estimates: DFD

Notes: Both panels report event study estimates from Equation (1). The sample consists of all mothers and fathers in the CCDB in Panel (a) and all mothers in Panel (b). Sample period is 1962-1989. In Panel (a) the outcome variable is SED membership and in Panel (b) the outcome is membership in the mass women's organization (DFD). Standard errors clustered on individual level. Bars represent 95 percent confidence intervals.

Figure 2: Dynamic results—Maternity leave and SED membership.

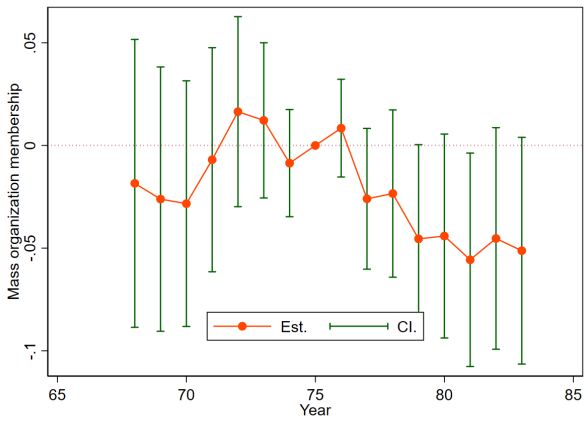


(a) Raw means

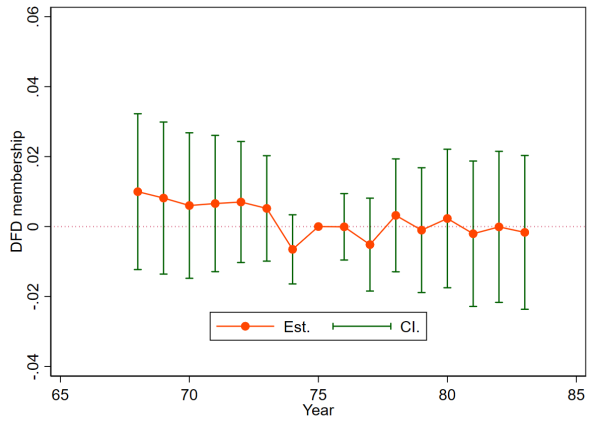
(b) Event study estimates

Notes: Panel (a) reports raw means of SED membership among all women who gave birth to a second or higher child from July 1975 to June 1977 by birth semester. Panel (b) reports event study estimates corresponding to column 5 in Table 2 for women giving birth to second or higher children in 1976. This extends Equation (3) by year-by-birth-year fixed effects of mothers and year-by-covariates fixed effects. The latter includes fully interacted sets of fixed effects among dummies for: occupational education '75, higher education '75, membership in the mass women's organization '75, worker, intelligenz and service sector background. Data from CCDB. Standard errors clustered on individual level. Bars represent 95 percent confidence intervals.

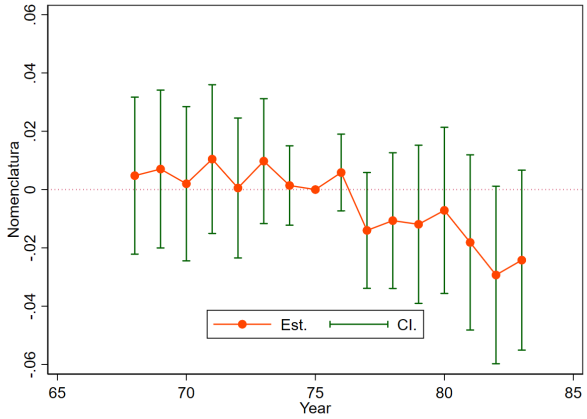
Figure 3: Dynamic results—Maternity leave and other outcomes.



(a) Event study: Mass Organizations



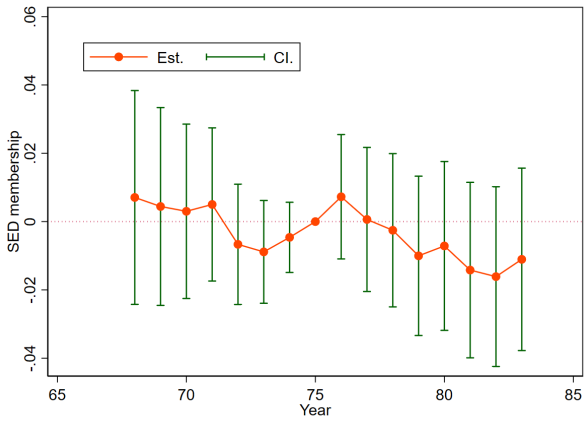
(b) Event study: DFD



(c) Event study: Nomenclatura

Notes: All panels report event study estimates from Equation (3) extended by year-by-birth-year fixed effects of mothers and year-by-covariates fixed effects. The latter includes fully interacted sets of fixed effects among dummies for: occupational education '75, higher education '75, worker, intelligenz and service sector background. Sample consists of all women who gave birth to a second or higher child in 1976. Panel (a) uses number of memberships in mass organizations as outcome variable. Panel (b) uses a DFD membership dummy as outcome variable. Panel (c) uses a the nomenclatura dummy as outcome variable. Data from CCDB. Standard errors clustered on individual level. Bars represent 95 percent confidence intervals.

Figure 4: Dynamic results—Fathers, maternity leave and SED membership.



Notes: Figure reports the effect of leave on second or higher time fathers. Effects are estimated using Equation (3) extended by year-by-birth-year fixed effects and year-by-covariates fixed effects. The latter includes fully interacted sets of fixed effects among dummies for: occupational education '75, higher education '75, worker, intelligenz and service sector background. Data from CCDB. Standard errors clustered on individual level. Bars represent 95 percent confidence intervals.

Table 1: Summary statistics 1975.

	Control				Treatment				Difference	
	Min	Mean	Max	SD	Min	Mean	Max	SD	b	SE
Sociopolitical										
SED membership	0.00	0.23	1.00	0.42	0.00	0.22	1.00	0.42	-0.008	0.016
Union membership	0.00	0.90	1.00	0.30	0.00	0.92	1.00	0.27	0.017	0.011
Mass Women's Organization	0.00	0.14	1.00	0.35	0.00	0.11	1.00	0.31	-0.031	0.012**
All Mass Organizations	0.00	2.18	6.00	1.06	0.00	2.22	6.00	1.01	0.033	0.039
Parents communists pre '45	0.00	0.02	1.00	0.12	0.00	0.01	1.00	0.11	-0.002	0.004
Cumulative distinctions	0.00	0.22	5.00	0.55	0.00	0.22	5.00	0.57	0.001	0.021
Cumulative political education visits	0.00	0.07	2.00	0.28	0.00	0.07	2.00	0.28	-0.007	0.011
Nomenclatura enlistment	0.00	0.15	1.00	0.36	0.00	0.14	1.00	0.35	-0.014	0.013
Education and skills										
8th grade (highest)	0.00	0.17	1.00	0.38	0.00	0.16	1.00	0.37	-0.014	0.014
10th grade (highest)	0.00	0.55	1.00	0.50	0.00	0.58	1.00	0.49	0.030	0.019
High school (highest)	0.00	0.26	1.00	0.44	0.00	0.25	1.00	0.43	-0.008	0.017
Some occupational education	0.00	0.75	1.00	0.43	0.00	0.76	1.00	0.43	0.009	0.016
Higher education	0.00	0.14	1.00	0.34	0.00	0.12	1.00	0.33	-0.014	0.013
Foreign language skills	0.00	0.30	4.00	0.70	0.00	0.31	5.00	0.68	0.011	0.026
Employment and social background										
Employer listed	0.00	0.76	1.00	0.43	0.00	0.73	1.00	0.45	-0.030	0.017*
Worker	0.00	0.56	1.00	0.50	0.00	0.54	1.00	0.50	-0.017	0.019
Intelligenz	0.00	0.07	1.00	0.26	0.00	0.08	1.00	0.27	0.007	0.010
Service	0.00	0.15	1.00	0.36	0.00	0.18	1.00	0.38	0.023	0.014
Demographic										
Number of kids	0.00	1.15	8.00	0.60	0.00	1.14	6.00	0.55	-0.007	0.022
Age	17.00	26.13	41.00	4.31	17.00	25.53	41.00	4.16	-0.599	0.160***
Observations	1462				1333				2795	

Notes: Summary statistics for women in the main estimation sample in the year before policy implementation. The control group consists of women giving birth to a second or higher child from January to June 1976 (and no other child in 1976). The treatment group consists of women giving birth to a second or higher child from July to December 1976. Data from CCDB. Robust standard errors.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Static results—Maternity leave and SED membership.

	SED membership				
	(1)	(2)	(3)	(4)	(5)
Post76	0.140***				
	[0.007]				
Post76 × Treat	0.039***	0.039***	0.040***	0.032***	0.033***
	[0.011]	[0.011]	[0.011]	[0.011]	[0.011]
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	No
Year × Covariates '75 FE	No	No	Yes	No	Yes
Year × Cohort FE	No	No	No	Yes	Yes
R^2	0.758	0.767	0.773	0.775	0.782
Mean dep var	0.241	0.241	0.241	0.241	0.241
Observations	44720	44720	44720	44720	44720

Notes: OLS estimates of Equation (2). Dependent variable is SED membership. The sample is restricted to women giving birth to second or higher child in 1976. Year × Covariates '75 FE include fully interacted sets of fixed effects among dummies for: occupational education '75, higher education '75, membership in the mass women's organization '75, worker, intelligenz and service sector background. Year × Cohort FE are year-by-birth-year FE. Data from CCDB. Standard errors clustered on individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Changes in attitudes.

	Gender equality better in GDR				
	(1)	(2)	(3)	(4)	(5)
Birth post 76	-0.002		-0.079	-0.095	-0.109
	[0.049]		[0.073]	[0.085]	[0.085]
Female		0.189***	0.082	0.063	0.039
		[0.046]	[0.080]	[0.088]	[0.090]
Birth post 76 × Female			0.167*	0.192*	0.193*
			[0.098]	[0.104]	[0.104]
Cohort FE	No	No	No	Yes	Yes
Education FE	No	No	No	No	Yes
Sector FE	No	No	No	No	Yes
R^2	0.000	0.042	0.049	0.054	0.072
Mean dep var	0.687	0.687	0.687	0.687	0.687
Observations	390	390	390	390	390

Notes: OLS estimates of Equation (4). Dependent variable is a dummy equal to one if respondent indicated that gender equality was better in the GDR. Birth post 76 equals one if individual had second child post July 1976. The control group consists of all individuals having their second child from 1971 to June 1976. The full sample consists of all survey respondents who had their second child from 1971-1981. Female indicates respondents gender. Data from LV. Robust standard errors.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Heterogeneity analysis.

	SED membership				
	(1)	(2)	(3)	(4)	(5)
Post76 × Treat	0.039***	0.030	0.035***	0.040***	0.019
	[0.011]	[0.023]	[0.012]	[0.012]	[0.014]
Post76 × No. children '75		-0.047***			
		[0.009]			
Post76 × Treat × No. children '75		0.007			
		[0.015]			
Post76 × Nomenclatura '75			0.084***		
			[0.022]		
Post76 × Treat × Nomenclatura '75			0.038		
			[0.037]		
Post76 × Higher education '75				0.092***	
				[0.025]	
Post76 × Treat × Higher education '75				-0.003	
				[0.040]	
Post76 × Daycare Infra.					0.016
					[0.015]
Post76 × Treat × Daycare Infra.					0.046**
					[0.023]
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
R^2	0.766	0.767	0.768	0.767	0.767
Mean dep var	0.238	0.238	0.238	0.238	0.238
Observations	43968	43968	43968	43968	43968

Notes: OLS estimates of Equation (2) with expanded interactions. Dependent variable is SED membership. The sample is restricted to women giving birth to second or higher child in 1976. Data from CCDB. Standard errors clustered on individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Intergenerational effects.

Panel (a): Parents of treated women.					
	Grandmother SED				Grandfather SED
	(1)	(2)	(3)	(4)	(5)
Treat	0.028**	0.029**	0.024*	0.023*	0.004
	[0.013]	[0.013]	[0.013]	[0.013]	[0.017]
Education FE	No	Yes	No	Yes	Yes
Social Background FE	No	No	Yes	Yes	Yes
Birth Year FE	No	No	No	Yes	Yes
R^2	0.002	0.019	0.034	0.055	0.113
Mean dep var	0.136	0.136	0.136	0.136	0.310
Observations	2795	2795	2795	2795	2795

Panel (b): Children of treated women.						
	Former East Germany			Former West Germany		
	All	Daughters		All	Daughters	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.247	0.401*	0.399*	-0.060	0.052	0.077
	[0.158]	[0.213]	[0.232]	[0.101]	[0.154]	[0.182]
Survey Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Interviewer FE	No	No	Yes	No	No	Yes
Seasonality	No	No	Yes	No	No	Yes
R^2	0.055	0.057	0.105	0.028	0.063	0.074
Mean dep var	-0.073	0.073	0.073	0.065	0.128	0.128
Observations	310	146	146	547	271	271

Notes: In Panel (a): Cross-sectional OLS estimates where treatment is one if an individual gave birth to a second or higher child in Jul-Dec 1976. Dependent variable is SED membership of an individual's parents in 1989. The control sample are women giving birth to second or higher child in Jan-Jun 1976. Data from CCDB. Robust standard errors. In Panel (b): Cross-sectional OLS estimates where treatment is one if an individual was born in Jul-Dec 1976. Dependent variable is a standardized ideology scale (the higher, the more left-wing). The control sample are individuals born Jan 1975-Jun 1976. The sample is split by respondents' current residence. Columns (1) and (4) are all respondents, whereas the remainder are daughters only. Interviewer controls include gender and education FE. Data from ALLBUS. Robust standard errors.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Appendices

A SED Newspaper Impressions

The following list provides quotes from a key SED newspaper.

- "Nun erhalte ich 26 statt bisher 18 Wochen Schwangerschafts- und Wochenurlaub. [...] Das ist Arbeiterpolitik zum Wohle des Volkes. So kommt doch die ganze Fürsorge, Wertschätzung und hohe Achtung gegenüber der Frau und Mutter in unserem Staate zum Ausdruck."

  Neues Deutschland, 31.5.1976

  - Translation: Now I will receive 26 weeks of maternity and parental leave instead of the previous 18 weeks. This is labor policy for the benefit of the people. This truly expresses the care, appreciation, and high respect towards women and mothers in our state.[29]

- "Ich freue mich, daß besonders viele Vorschläge eingeflossen sind, die wir Frauen [...] unterbreiteten und die das Leben der berufstätigen Mütter [...] wesentlich erleichtern. Das beweist doch, daß unser Wort etwas gilt, daß die Partei- und Staatsführung wissen, wo die "Hebel" anzusetzen sind."

  Neues Deutschland, 1.6.1976

  - Translation: I am pleased that a significant number of suggestions have been incorporated, which we, as women, proposed, and that greatly facilitate the lives of working mothers. This proves that our words matter, and that the party and state leadership know which "levers" to apply.

- "Es ist nur zu natürlich, daß diese Unterstützung, die den Müttern und ihren Familien zuteil wird, bei allen Werktätigen eine große Resonanz findet. Bei den Frauen selbst hat sie den Willen zu hohen Leistungen für die sozialistische Gellschaft gestärkt"

  Neues Deutschland, 28.8.1976

  - Translation: It is only natural that this support, which is given to mothers and their families, resonates greatly among all workers. Among women themselves, it has bolstered their commitment to making significant contributions to the socialist society.

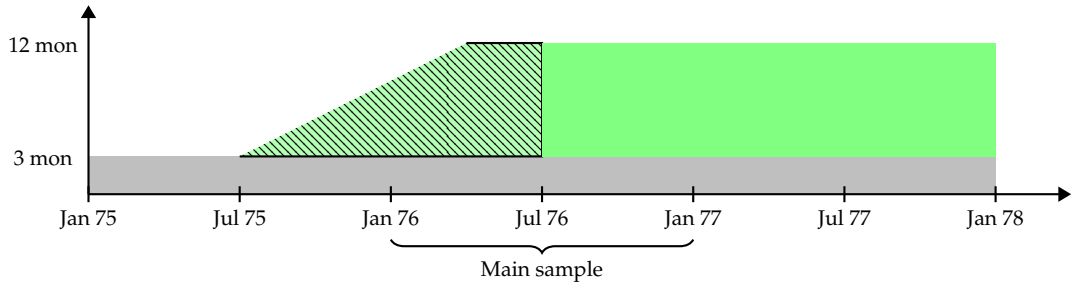
[29] The reader summed maternity leave and six weeks of paid pregnancy leave prior to the expected due date.

B Imperfect Retrospective Enactment

The leave policy was introduced with retrospective enactment based on newborns' birthdays. If a woman gave birth in January 1976, she was eligible for three months of leave under the pre-existing policy arrangement. Afterwards, she would have returned to work. Yet, from mid-June onward, she would have been eligible for leave again until her child's first birthday. Following this logic, Figure A1 Panel (a) depicts the total months of leave a mother was eligible for under perfect retrospective enactment.

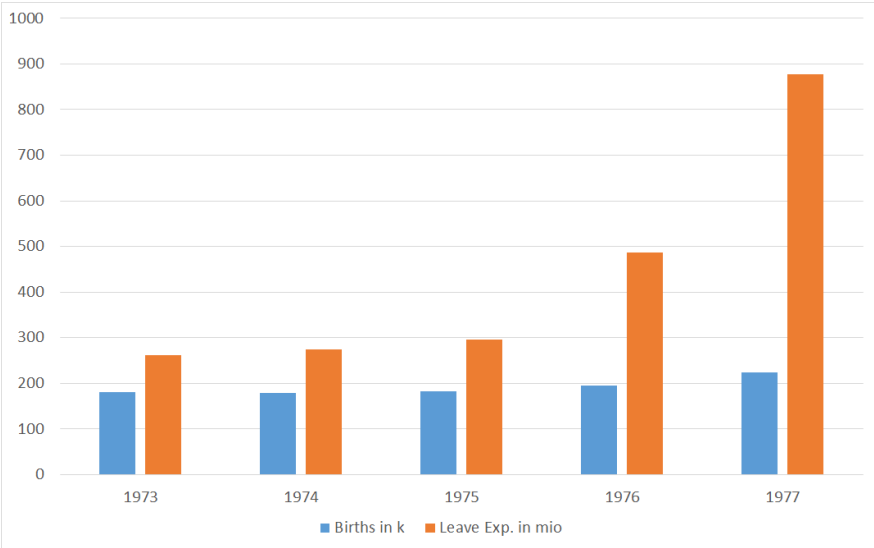
Unfortunately, the CCDB does not include information on leave in 1976. Hence, for identification, I assume that treatment intensities differed among women giving birth in the first versus the second half of 1976. To motivate this assumption I quantify the likely difference in treatment intensities by turning to aggregate expenditure data on maternity leave (DDR 1981). Figure A1 Panel (b) shows the total number of children born versus aggregate expenditure on parental leave. The number of newborns is roughly constant across years, yet expenditure grows remarkably from 1975 to 1976 and again from 1976 to 1977. Expenditures on the main extension for second or higher time mothers are detailed in the main body of the text.

Figure A1: Reform eligibility and spending.



(a) Maximum leave length eligibility

Notes: The x-axis equals time of birth of second (or higher) child. The y-axis represents maximum maternity leave period in months. Gray indicates the baseline leave policy. Green indicates the new policy, where shades denote retrospective policy enactment.



(b) Maternity leave expenditure

Notes: Data from DDR (1981); births are number children born in thousands and leave expenditure is the total government expenditure on parental leave in mio East German mark.

C Data construction

C.1 CCDB

The CCDB data were first collected manually. Figure A2 shows an annotated excerpt of the original forms. The data were then entered in a digital register. Below I describe the variable construction from the digital files.

SED. SED membership is coded from the CCDB entries "06.01 Partei", "06.02 von Jahr" and "06.03 bis Jahr" and is a dummy for all years in which the SED is listed as party.

Other memberships. Membership in the other organizations, nameley the main union (FDGB), Mass Women's group (DFD) and the sum of all memberships in non-party organizations is coded from entries "08.01 Organisation", "08.02 von Jahr" and "08.03 bis Jahr" and either a dummy or a count variable.

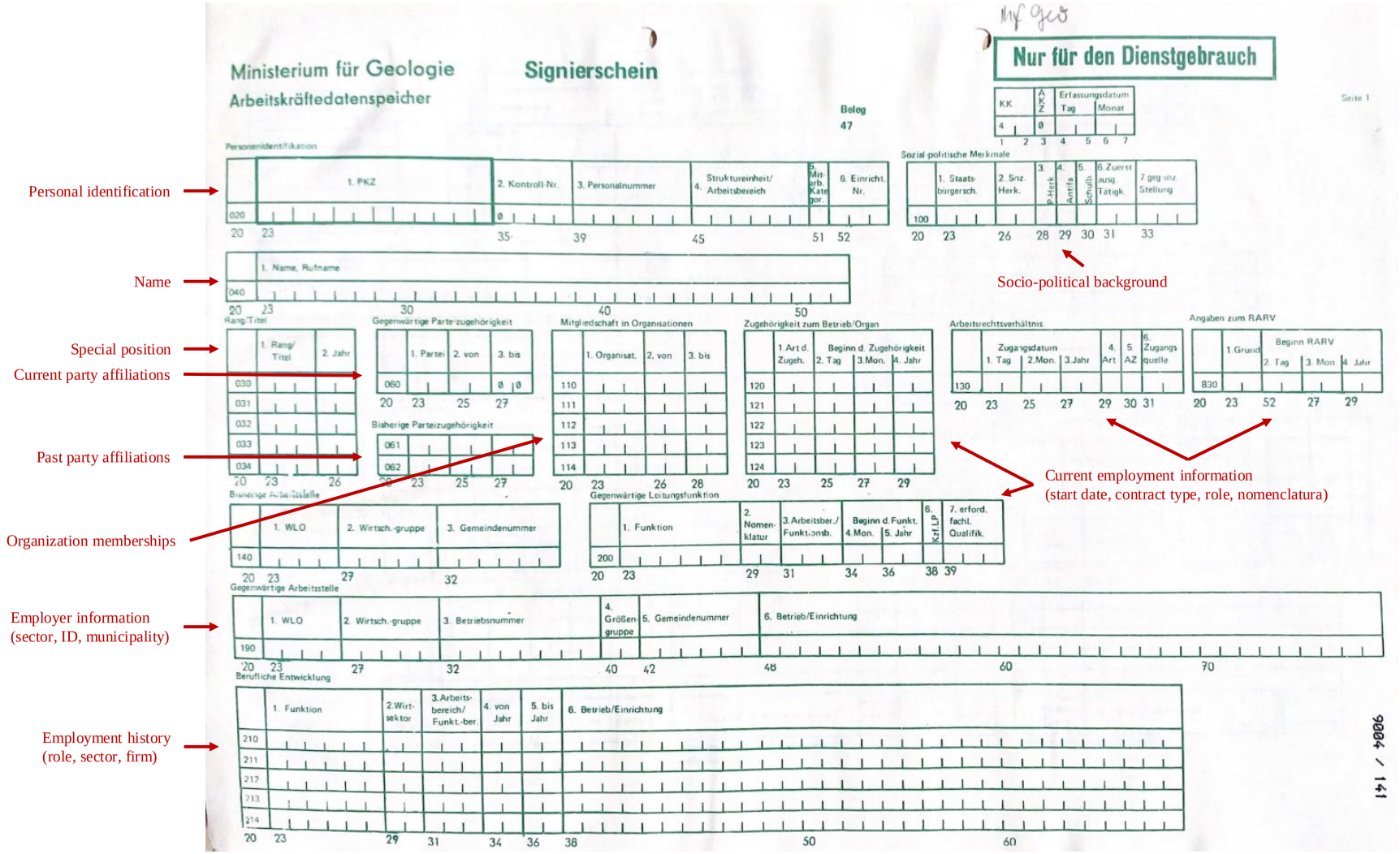
Other political variables. Parental party membership prior to '45 is coded from "27.02 Vater Partei vor 1945" and "27.03 Mutter Partei vor 1945". Political distinctions (e.g. for good citizenship) are coded from "25. Auszeichnungen" and "25.02 Jahr", which I accumulate over an individuals lifecycle. The same applies to political education visits/ party school visits which are coded from "18. Sonstiger politischer Schulbesuch" and "18.02 Abschlussjahr". Finally, I code a dummy for whether an individual is enlisted in the official nomenclatura using "15.03 Nomenklatur", "15.04 von Jahr" and "15.05 bis Jahr".

Education and skills. I code highest schooling degree from entry "07.05 Schulbildung" as dummies for having finished 8th grade, 10th grade or A levels. Occupational education is coded from "17.05 Akademischer Grad" (no academic degree attained), "17.02 Studienform" (Lehrgang/Lehre), "17.06 Abschlussjahr" and "17.01 Bildungseinrichtung" (Einrichtung der Berufsausildung/ Erwachsenenqualif.). The same variables are used to construct a dummy for higher education attainment. Number of foreign languages is a count variable constructed from "22.01 Sprache" and "22.03 Jahr des Erwerbs".

Social background. The listing of an employer is a dummy coded from "13. Gegenwärtige Funktion" and "13.05 seit Jahr" (most recent employment) as well as "14. Berufliche Entwicklung", "14.04 von Jahr" and "14.05 bis Jahr" (past employment). The dummies Worker, Intelligenz and Service denote social class/background and are taken from "07.02 Soziale Herkunft".

Remainder. The variable female is a dummy coded from "30.20 Geschlecht". The number of kids

Figure A2: Data entry form.



Notes: Data entry form from the Ministry of Geology. The text fields were typically filled with a numbered coding scheme (e.g. each mass organization had a unique three-digit key).
Source: Bundesarchiv DC 20/9004.

is a cumulative count variable constructed from ”29. Kinder” and ”29.02 Geburtsdatum Jahr”. The age variable is constructed from ”30.17 Geburtsjahr”.

C.2 LV

Key variable construction is described in the main text. I keep all individuals who have their second child in between 1971-1981. Cohort FEs are dummies for the four different birth cohorts (born in 29-31, 39-41, 51-53, 59-61). Education FEs are dummies for the highest occupational education degree listed in the LV data (no degree, occupational degree, advanced occupational degree, college and university degrees). Sector FEs include dummies based on the first occupation spell (service, worker, agrarian).

For comparison to the CCDB data, I harmonized a set of other variables (Table A2). For the sociopolitical background (worker, intelligenz, service) I used information on paternal occupation classifications. Here, it is important to note that many party members were characterized as working class even if it was not necessarily plausible. The reason was that the SED attempted to maintain the appearance of a workers' and farmers' party. Concerning education, note, that in the GDR there existed specialized education tracks in which upon graduation and three years of work experience individuals attained the equivalent of a college degree (FH). These did not require formal completion of a high school. This explains why the share of higher education graduates is higher than those who completed high school.

C.3 ALLBUS

ALLBUS source raw file is cited in the main text and reference list. I keep all individuals born in either 1975 or 1976 with German citizenship and non-missing interviewer data (gender and education). Ideology is measured from a self-reported left-right scale (inverted such that 10 is left) and standardized. Survey year FEs are dummies for each survey wave (every second year in between 1994-2018). Interviewer FEs include a gender dummy and education dummies. Seasonality equals one if an individual was born between July and December and zero if otherwise.

D Summary Statistics

Table A1: Summary statistics 1989

	Full CCDB			
	min	mean	max	sd
SED membership	0.00	0.46	1.00	0.50
Union membership	0.00	0.95	1.00	0.22
Mass Women's Organization	0.00	0.12	1.00	0.32
All Mass Organizations	0.00	2.48	10.00	0.97
Parents communists pre '45	0.00	0.02	1.00	0.13
Own mother in SED	0.00	0.11	1.00	0.32
Own father in SED	0.00	0.26	1.00	0.44
Cumulative distinctions	0.00	1.59	10.00	2.00
Cumulative political education visits	0.00	0.37	7.00	0.61
Nomenclatura enlistment	0.00	0.28	1.00	0.45
8th grade (highest)	0.00	0.28	1.00	0.45
10th grade (highest)	0.00	0.42	1.00	0.49
High school (highest)	0.00	0.28	1.00	0.45
Some occupational education	0.00	0.70	1.00	0.46
Higher education	0.00	0.24	1.00	0.43
Foreign language skills	0.00	0.36	5.00	0.75
Worker	0.00	0.55	1.00	0.50
Intelligenz	0.00	0.08	1.00	0.27
Service	0.00	0.17	1.00	0.37
Number of kids	0.00	1.36	10.00	1.11
Age	14.00	43.21	89.00	12.01
Observations	371689			

Notes: Unit of observation is an individual in 1989. All data are from the CCDB.

E Representativeness

Table A2: Comparing CCDB and LV.

	CCDB	LV
Sociopolitical		
SED membership	0.48	.21
Father in SED	0.27	.21
Mother in SED	0.12	.08
Worker	0.56	.32
Intelligenz	0.08	.01
Service	0.17	.18
Education and skills		
8th grade (highest)	0.30	.42
10th grade (highest)	0.39	.36
High school (highest)	0.29	.11
Some occupational education	0.69	.77
Higher education	0.26	.14
Demographic		
Female	0.54	.51
Number of kids	1.44	1.93
Age in 1989	43.72	43.62
Observations	114862	2331

Notes: Unit of observation are individuals in 1989. LV data include all entries. CCDB include corresponding weighted birth cohorts.

Table A3: Maternity leave and long-run employment of women.

	Employed in 1989			Employment duration		
	(1)	(2)	(3)	(4)	(5)	(6)
Birth post 76	0.045	0.075	0.064	-3.522***	-0.537	-0.548
	[0.062]	[0.071]	[0.067]	[1.031]	[0.876]	[0.836]
Cohort FE	No	Yes	Yes	No	Yes	Yes
R^2	0.003	0.026	0.026	0.075	0.421	0.533
Mean dep var	0.800	0.800	0.785	17.054	17.054	16.313
Sample	'71-'81	'71-'81	'66-'86	'71-'81	'71-'81	'66-'86
Observations	190	190	405	190	190	405

Notes: Difference between women giving birth to their second child after introduction of maternity leave (post '76) versus women giving birth to their second child before (pre '76). Sample consists of all women having their second child either inbetween 71-81 or 66-86. The outcome variable in columns 1-3 is a simple dummy for whether an individual was employed in 1989. The outcome variable in columns 4-6 is the sum of all employment spells in years until survey collection (1991/1992). The Cohort FE capture birth cohort effects. Data from LV. Robust standard errors.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Maternity leave and switching sectors.

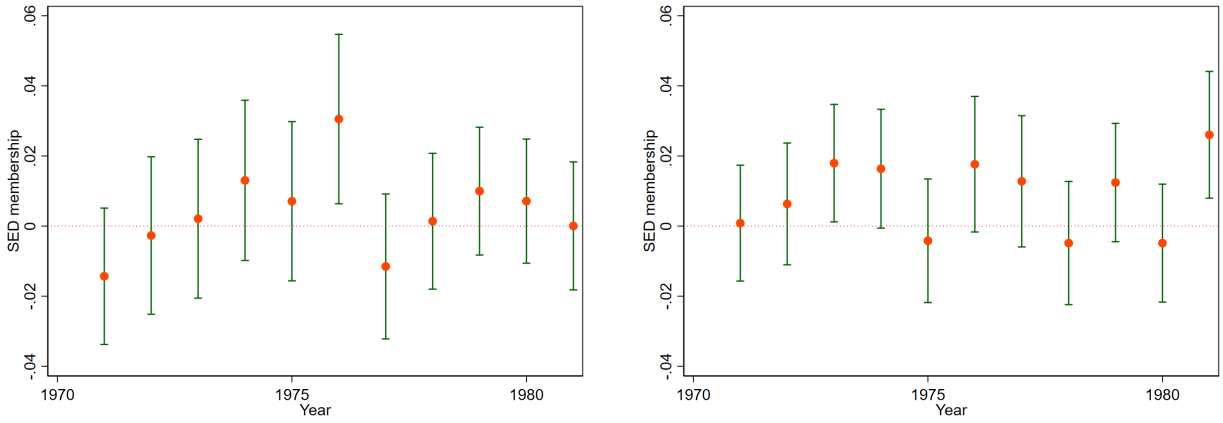
	Switching Sectors				
	(1)	(2)	(3)	(4)	(5)
Post76	-0.151***				
	[0.003]				
Post76 × Treat	0.006	0.006	0.006	0.006	0.006
	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	No
Year × Covariates '75 FE	No	No	Yes	No	Yes
Year × Cohort FE	No	No	No	Yes	Yes
R^2	0.103	0.433	0.440	0.441	0.448
Mean dep var	0.153	0.153	0.153	0.153	0.153
Observations	44720	44720	44720	44720	44720

Notes: OLS estimates of Equation (2). Dependent variable is a dummy for switching employment sectors. The sample is restricted to women giving birth to second or higher child in 1976. Year × Covariates '75 FE include fully interacted sets of fixed effects among dummies for: occupational education '75, higher education '75, membership in the mass women's organization '75, worker, intelligenz and service sector background. Year × Cohort FE are year-by-birth-year FE. Data from CCDB. Standard errors clustered on individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

F Robustness

F.1 Placebo

Figure A3: Static results—Placebo policy introduction in different years.



(a) Second-time mothers

(b) First-time mothers

Notes: Both panels show placebo estimates corresponding to column 5 in Table 2. Each estimate corresponds to a static difference-in-differences regression as in Equation (2), but the samples consist of women giving birth in other years than 1976. The sample periods are centered around each such year with eight years of pre- and eight years of post-period. The latter is defined as the placebo treatment period. Data from CCDB. Panel (a) shows placebo estimates for second-time mothers. Panel (b) shows placebo estimates for first-time mothers. Standard errors clustered on individual level. Bars represent 95 percent confidence intervals.

F.2 Alternative control group definition

F.2.1 Static

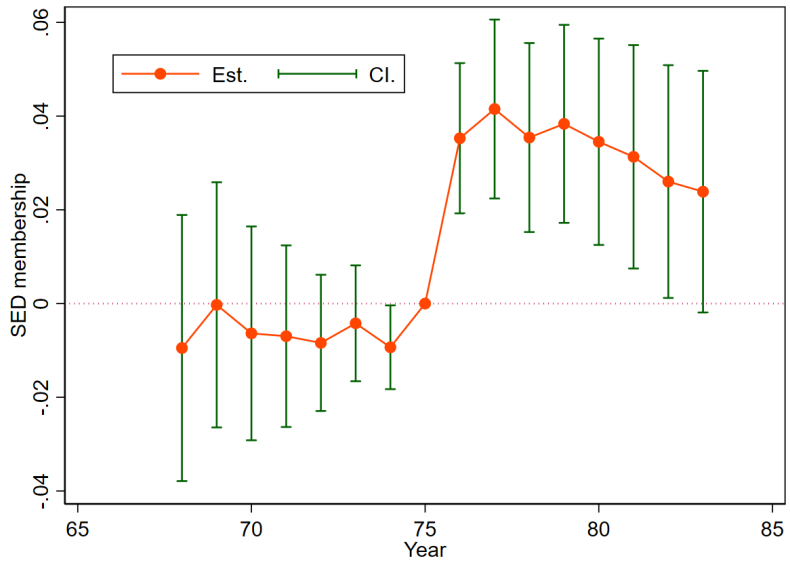
Table A5: Different control group definition—Maternity leave and SED membership.

	SED membership				
	(1)	(2)	(3)	(4)	(5)
Post76	0.120***				
	[0.007]				
Post76 × Treat	0.059***	0.059***	0.057***	0.040***	0.037***
	[0.011]	[0.011]	[0.011]	[0.011]	[0.011]
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	No
Year × Covariates '75 FE	No	No	Yes	No	Yes
Year × Cohort FE	No	No	No	Yes	Yes
R^2	0.766	0.775	0.779	0.785	0.788
Mean dep var	0.238	0.238	0.238	0.238	0.238
Observations	41760	41760	41760	41760	41760

Notes: OLS estimates of Equation (2). Dependent variable is SED membership. The sample is restricted to women giving birth to second or higher child in January to June 1975 and July to December 1976. Women giving birth July to December 1976 remain the treatment group. Year × Covariates '75 FE include fully interacted sets of fixed effects among dummies for: occupational education '75, higher education '75, membership in the mass women's organization '75, worker, intelligenz and service sector background. Year × Cohort FE are year-by-birth-year FE. Data from CCDB. Standard errors clustered on individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

F.2.2 Dynamic

Figure A4: Dynamic results—Different control group definition.



Notes: Figure reports event study estimates from Equation (3) extended by year-by-birth-year fixed effects of mothers and year-by-covariates fixed effects. The latter includes fully interacted sets of fixed effects among dummies for: occupational education '75, higher education '75, worker, intelligenz and service sector background. Dependent variable is SED membership. The sample is restricted to women giving birth to second or higher child in January to June 1975 and July to December 1976. Women giving birth July to December 1976 remain the treatment group. Data from CCDB. Standard errors clustered on individual level. Bars represent 95 percent confidence intervals.

F.3 Event study estimates

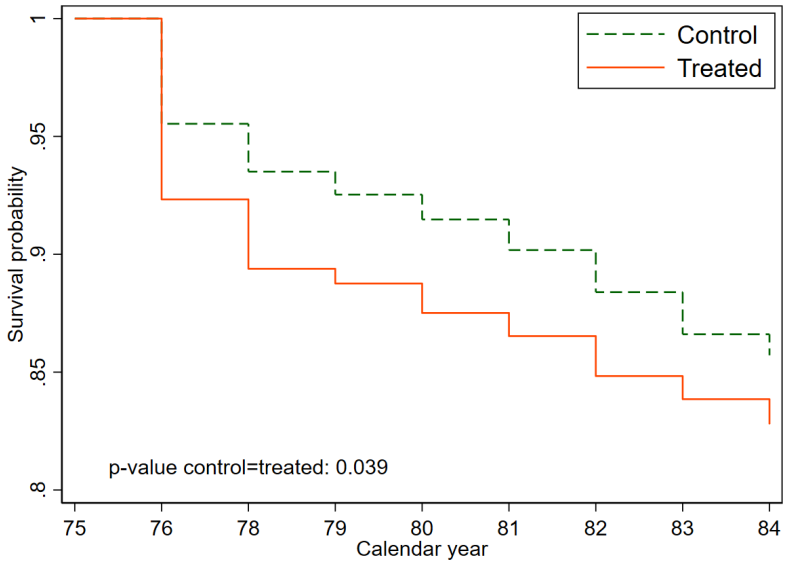
Table A6: Event study—Parenthood and SED membership.

	SED membership	
	Mothers	Fathers
	(1)	(2)
$\text{Birth}_{t=-5}$	-0.003**	-0.025***
	[0.001]	[0.002]
$\text{Birth}_{t=-4}$	-0.002*	-0.021***
	[0.001]	[0.002]
$\text{Birth}_{t=-3}$	-0.002*	-0.015***
	[0.001]	[0.001]
$\text{Birth}_{t=-2}$	0.001	-0.008***
	[0.001]	[0.001]
$\text{Birth}_{t=0}$	-0.005***	0.012***
	[0.001]	[0.001]
$\text{Birth}_{t=+1}$	-0.020***	0.022***
	[0.001]	[0.001]
$\text{Birth}_{t=+2}$	-0.031***	0.034***
	[0.001]	[0.002]
$\text{Birth}_{t=+3}$	-0.039***	0.049***
	[0.001]	[0.002]
$\text{Birth}_{t=+4}$	-0.044***	0.062***
	[0.002]	[0.002]
$\text{Birth}_{t=+5}$	-0.049***	0.076***
	[0.002]	[0.003]
$\text{Birth}_{t=+6}$	-0.052***	0.088***
	[0.002]	[0.003]
$\text{Birth}_{t=+7}$	-0.055***	0.100***
	[0.003]	[0.003]
$\text{Birth}_{t=+8}$	-0.058***	0.112***
	[0.003]	[0.003]
$\text{Birth}_{t=+9}$	-0.060***	0.124***
	[0.003]	[0.004]
$\text{Birth}_{t=+10}$	-0.061***	0.136***
	[0.003]	[0.004]
$\text{Birth}_{t=+11}$	-0.063***	0.148***
	[0.004]	[0.004]
$\text{Birth}_{t=+12}$	-0.064***	0.159***
	[0.004]	[0.004]
$\text{Birth}_{t=+13}$	-0.065***	0.171***
	[0.004]	[0.005]
$\text{Birth}_{t=+14}$	-0.064***	0.183***
	[0.005]	[0.005]
$\text{Birth}_{t=+15}$	-0.061***	0.195***
	[0.005]	[0.005]
Year FE	Yes	Yes
Age FE	Yes	Yes
R^2	0.061	0.104
Mean dep var	0.237	0.541
Observations	2351678	1903995

Notes: OLS estimates of Equation (1). Dependent variable is SED membership. The sample is restricted to individuals having their first child between 1962 to 1989 and the event periods (five years before and fifteen years after first child birth). Left column reports estimates for mothers only and the right column reports estimates for fathers only. Data from CCDB. Standard errors clustered on individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

F.4 Survival Analysis

Figure A5: Survival estimates—Maternity leave and SED membership.



Notes: Kaplan-Meier survival estimates. Absorbing state is entering the SED. The sample is restricted to individuals that survived until 1976. Graph indicates the associated p-value for the log-rank test of equality of survivor functions. Data from CCDB.

G Further results

G.1 Fathers

Table A7: Static results—Fathers, maternity leave and SED membership.

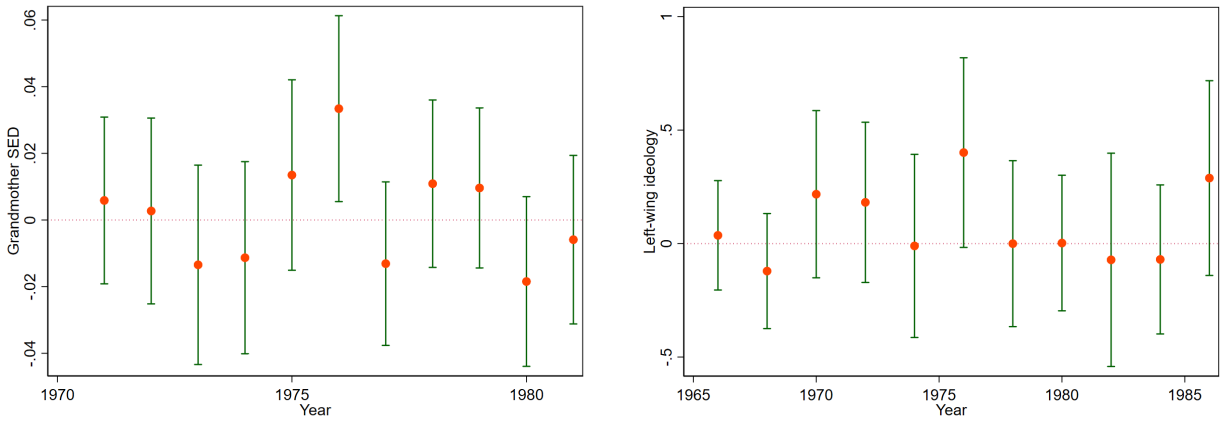
	SED membership				
	(1)	(2)	(3)	(4)	(5)
Post76	0.209***				
	[0.009]				
Post76 × Treat	0.011	0.011	0.010	-0.006	-0.007
	[0.013]	[0.013]	[0.013]	[0.012]	[0.012]
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	No
Year × Covariates '75 FE	No	No	Yes	No	Yes
Year × Cohort FE	No	No	No	Yes	Yes
R^2	0.763	0.778	0.781	0.799	0.801
Mean dep var	0.521	0.521	0.521	0.521	0.521
Observations	44768	44768	44768	44768	44768

Notes: OLS estimates of Equation (2). Dependent variable is SED membership. The sample is restricted to fathers having their second or higher child in 1976. Year × Covariates '75 FE include fully interacted sets of fixed effects among dummies for: occupational education '75, higher education '75, worker, intelligenz and service sector background. Year × Cohort FE are year-by-birth-year FE. Data from CCDB. Standard errors clustered on individual level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

H Intergenerational Effects

H.1 Placebo

Figure A6: Intergenerational Effects—Placebo policy introduction in different years.



(a) Placebo: Grandmothers

(b) Placebo: Daughters

Notes: Both panels show placebo estimates corresponding to column 1 in Table 5. Each estimate corresponds to pre-post comparisons for placebo years. In Panel (a) each estimation sample consists of mothers giving birth to their second child in each year from 1971 to 1981 and the outcome is SED membership of the grandmother. The coefficient represents the mean difference in membership between childbirth in the early versus late half of a year. Data from CCDB. In Panel (b) each estimation sample consists of daughters born in two year sample periods from 1966 to 1986 and the outcome is self-reported left-wing ideology. The coefficient represents the mean difference in ideology between those born in the last semester (e.g. July to December 1976) versus three preceeding semesters (e.g. January 1975 to June 1976). Data from ALLBUS. Bars represent 95 percent confidence intervals.