The Making of a Good Woman: Extended Parental Leave Entitlements and Mothers' Work Commitment in Germany[1]

Markus Gangl and Andrea Ziefle
Goethe University Frankfurt am Main

The authors investigate the relationship between family policy and women's attachment to the labor market, focusing specifically on policy feedback on women's subjective work commitment. They utilize a quasi-experimental design to identify normative policy effects from changes in mothers' work commitment in conjunction with two policy changes that significantly extended the length of statutory parental leave entitlements in Germany. Using unique survey data from the German Socio-Economic Panel and difference-in-differences, triple-differenced, and instrumental variables estimators for panel data, they obtain consistent empirical evidence that increasing generosity of leave entitlements led to a decline in mothers' work commitment in both East and West Germany. They also probe potential mediating mechanisms and find strong evidence for role exposure and norm setting effects. Finally, they demonstrate that policy-induced shifts in mothers' preferences have contributed to retarding women's labor force participation after childbirth in Germany, especially as far as mothers' return to full-time employment is concerned.

An impressive body of social science research has documented the pivotal role of public policies in facilitating women's labor force participation and in shaping patterns of gender inequality in labor market careers and pay (e.g., Korpi 2000; Gornick and Meyers 2003; Mandel and Semyonov 2005, 2006; Kenworthy 2008; Esping-Andersen 2009; Mandel and Shalev 2009*b*;

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Pettit and Hook 2009; Korpi, Ferrarini, and Englund 2013). Complementing the traditionally recognized decommodification role (Esping-Andersen 1990, 1999; Korpi and Palme 1998), the resulting defamilialization has come to be considered a second key achievement of modern welfare states: by collectivizing the cost of child and elderly care through various mixes of direct cash benefits, family leave entitlements, and subsidized or publicly provided care services, modern welfare states liberate women from their traditional homemaker role, enable their full participation in the labor market, and hence ultimately reduce gender inequality in both the domestic and the economic spheres (Lewis 1992; Sainsbury 1996, 1999; Korpi 2000; Esping-Andersen 2002, 2009; Gornick and Meyers 2009; Pettit and Hook 2009). Naturally, scholars continue to be divided in their assessment of the extent to which welfare states empirically succeed in reaching these ambitious aims (see, e.g., the debate in Gornick, Meyers, and Wright [2009]) and, more recently, of the extent to which beneficial policy impacts might accrue in class-specific ways (Mandel and Shalev 2009*a*; Shalev 2009; Cooke 2011; Korpi et al. 2013). But the basic fact that public policies, or lack thereof, shape how women participate in the labor market is undisputed.

What is much less clear, however, is why public policies are actually as effective as indicated in current research. Across the social sciences, most studies of women's labor force participation implicitly or explicitly adopt the standard microeconomic model of behavior and hence explain policy effects—that is, the impact of taxation systems, child care availability, or parental leave entitlements on women's employment—as women's or households' rational response to economic incentives and constraints set by alternative policies (Korpi 2000; Gornick and Meyers 2009; Blau, Ferber, and Winkler 2010; Korpi et al. 2013). And indeed, microeconomic theory does provide a parsimonious and versatile framework that is undoubtedly consistent with many aspects of gendered labor markets and the role of public policies therein. Yet it is also the case that recent empirical research has also consistently yielded observations about the relationship between family pol-

annual meeting of the American Sociological Association, the 2012 International GSOEP User Conference at the DIW Berlin, the 2014 spring meeting of the ISA RC28 Social Stratification and Mobility, and at seminars at the Center for Demography and Ecology, University of Wisconsin—Madison, the Institutet för Social Forskning Stockholm, the Department of Sociology at the University of Konstanz, and the Department of Economics at the University of Frankfurt. We are especially grateful to the *AJS* reviewers, Tom DiPrete, Walter Korpi, and the participants at the above seminars for many exceptionally useful suggestions and comments. All remaining deficiencies are our own. Direct correspondence to Marcus Gangl, Goethe University Frankfurt am Main, School of Social Sciences, Theodor-W.-Adorno-Platz 6, 60629 Frankfurt am Main, Germany. E-mail: mgangl@soz.uni-frankfurt.de or mgangl@ssc.wisc.edu

icies and women's labor market success that are less easily reconciled with standard labor supply theory—namely, nonlinear relationships between public policies and women's labor force participation, suggesting adverse effects of long parental leave entitlements in particular (Pettit and Hook 2005; Kenworthy 2008; Boeckmann, Misra, and Budig 2015), as well as negative effects of extensive family policy packages on women's representation in management, on the gender pay gap, on women's returns to tertiary education, and on the wage penalty for motherhood (Mandel and Semyonov 2005; 2006; Pettit and Hook 2009; Budig, Misra, and Boeckmann 2012)—and which then suggest that social processes beyond straightforward economic incentives and constraints might be operative in generating the observed relationships between public policies and patterns of women's labor force participation.

One such complementary mechanism is provided by models of statistical discrimination that describe potentially adverse impacts of "women-friendly" (Hernes 1987) public policies via employer expectations, especially in cases in which family policy tends to reconfirm rather than undermine the traditional gender division of labor (e.g., Mandel and Semyonov 2005, 2006; Bergmann 2009; Gangl and Ziefle 2009; Mandel and Shalev 2009*b*; Korpi et al. 2013). Another, perhaps even more fundamentally sociological mechanism is highlighted in a body of recent work that emphasizes welfare states as reflecting but also shaping the moral economy of modern societies, where citizens' belief systems are inherently affected by those norms that are institutionalized in and legitimized by public policy environments (Svallfors 1997, 2007, 2010; Mau 2003, 2004). Similar policy feedback effects on gender identity, gender attitudes, and gender role beliefs have of course been long-standing concerns in the feminist literature that has emphasized the normative connotations of gender-related institutions, the welfare state as regulator of relationships between care and work very much included (e.g., Lewis 1992; Orloff 1996, 2009; Pfau-Effinger 2004, 2008; Kremer 2007; Ferree 2010; see also Cooke and Baxter 2010; Cooke 2011).

The research we report on here strives to theoretically elaborate and empirically document the causal relevance of such normative policy feedback in the specific case of the relationship between family policy, work-family preferences, and women's labor force participation. We do so by conducting a quantitative case study of women's changing work commitment in conjunction with two distinct policy changes within Germany's statutory parental leave program, including the 1992 watershed reform that extended leave duration to 36 months following childbirth and hence created one of the most generous leave entitlements worldwide (e.g., Gauthier 1996; Henderson and White 2004; Pettit and Hook 2009; Ray, Gornick, and Schmitt 2010; Korpi et al. 2013). Focusing on the impact of singular policy events within a given country in fact enables us to implement an informative quasi-

experimental interrupted time-series (ITS) design to evaluate the causal relationship between family policy and women's work-family preferences, which we consider a rare historical opportunity in a field that is inevitably dominated by macrocomparative and static modes of analysis and the at-best tentative causal conclusions they engender. Our design is further enriched by the fact that we are able to draw on survey data from the German Socio-Economic Panel (GSOEP), which not only spans a historical observation window including two relevant policy changes and contains a wealth of sociodemographic data but is internationally unique in providing panel data on respondents' work commitment. These data specifically permit us to observe individual-level changes in respondents' subjective work orientation over time and to apply suitable panel data estimators to support causal inferences about genuine policy effects.

These methodological aspects notwithstanding, we also believe that an in-depth case study on normative policy feedback in the case of extended parental leave entitlements in Germany has considerable substantive merit. For one thing, parental leave programs are probably the most contentious element within the package of "women-friendly" public policies, not the least since the question whether they tend to serve women's integration into the labor force or rather maintain the traditional gender division of labor remains contested on both theoretical and empirical grounds (e.g., Morgan and Zippel 2003; Kremer 2007; Gornick et al. 2009; Cooke 2011; Korpi et al. 2013). The German case is of particular interest in this respect because the duration of statutory parental leave was expanded well beyond the 12 months common in the better-researched Nordic countries during the late 1980s and early 1990s and also because, in contrast to a Scandinavian policy context, long parental leave entitlements clearly are a complementary policy instrument within Germany's broadly conservative institutional regime that has, up until very recently, been primarily geared toward supporting traditional family arrangements (Korpi 2000; Korpi et al. 2013). Both aspects may increase the likelihood that negative effects of parental leave programs on maternal employment outweigh any positive impact on women's labor force participation prior to family formation and may thus be relevant to understanding why Germany seems to be the country case—besides France, another country with extensive parental leave entitlements—mainly responsible for the "paradoxical" results reported in Mandel and Semyonov (2005, 2006) and the nonlinear relationships between parental leave entitlements and women's employment observed in Pettit and Hook (2005), Kenworthy (2008), Budig et al. (2012), or Boeckmann et al. (2015).

Such an interpretation seems warranted indeed in light of several German studies that have consistently concluded that mothers' time off employment substantially increased with extended parental leave entitlements

(e.g., Ondrich, Spiess, and Yang 1996; Ziefle 2009; Grunow, Aisenbrey, and Evertsson 2011; Schönberg and Ludsteck 2014; Ziefle and Gangl 2014). What is more, the results of the research we report here strongly suggest that such policy effects have not been restricted to purely behavioral change in response to changing economic incentives but have also comprised normative feedback effects on mothers' subjective work commitment. As we report below, we obtain consistent empirical evidence that the two policy changes that expanded parental leave entitlements in Germany, and the watershed 1992 reform in particular, had adverse effects on mothers' subjective work orientation, and we are also able to show that these normative feedback effects did contribute to retarding mothers' labor market reentry in the longer run, especially as far as their return to full-time employment is concerned. In what follows, we first develop a theoretical model of normative policy feedback suited to our specific application. We then describe the institutional and labor market environment in Germany, and we detail the relevant changes to Germany's federal parental leave program that are at the heart of the ITS design we adopt in this article. We discuss our data sources, research design, operationalization, and statistical modeling in the article's methodology section and present our empirical results afterward. The final section then discusses our results and some broader implications of our findings for the study of the relationships between public policy, women's employment, and gender inequality.

MOTHERHOOD, WORK COMMITMENT, AND THE WELFARE STATE

The secular changes witnessed in women's demographic behavior, educational attainment, and labor force participation in the United States and other industrialized countries over much of the 20th century are prompting renewed interest in the role of women's preferences as both drivers and consequences of their increasing involvement in the economic sphere (Bielby 1992; Charles 2011). For instance, Goldin (2006) has described long-term planning horizons, increasing work commitment, and career orientation as well as joint decision making in households as elements of a "quiet revolution" that has transformed the lives of American women born since midcentury. Similarly, Hakim (2000, 2002, 2003*a*) has emphasized the role of heterogeneity in women's life course preferences for understanding women's labor market outcomes and women's differential responsiveness to family and labor market policy (see also the exchange between McRae [2003*a*, 2003*b*] and Hakim [2003*b*]). And on a truly global scale, Inglehart and Norris (2003) have documented the shift toward increasingly progressive gender cultures and gender role norms that comes with modernization, increasing wealth, and secularization.

An Exposure Model of Life Course Events and Preference Formation

The coincidence of substantial heterogeneity of individual preferences and values at any point in time and sweeping broad-scale historical changes is of course in perfect accordance with sociology's traditional emphasis on the importance of socialization processes, notably during formative stages in adolescence, whether for economic aspirations (Parsons 1951), political values (Inglehart 1977, 1990), or gender roles and work-family preferences (Bielby and Bielby 1984; Bielby 1992; England 2005; Davis and Greenstein 2009). As a consequence, it is processes of generational replacement that are typically identified as the key driver of cultural change (Mannheim [(1928) 1952], with respect to gender role norms; specifically, Bolzendahl and Myers [2004], Brooks and Bolzendahl [2004], and Pampel [2011]), which then also imply that structural or institutional conditions that shape individual values and identities primarily generate long-term and lagged impacts on preferences, norms, and societal culture (e.g., Svallfors 2007, 2010; Campbell 2012).

Importantly, however, acknowledging the critical role of socialization experiences for preferences and orientations is not equivalent to assuming that preference change would necessarily occur over historical periods only. Instead, adopting a socialization perspective implies an emphasis on the fundamental relevance of learning from experience and information, which, although surely shaped by and contingent on earlier formative experiences during adolescence, is unlikely to discontinue at later stages in the life course (see Parsons [1951] and, more recently, Kohn [1977, 1989], Bolzendahl and Myers [2004], Brooks and Bolzendahl [2004], and Elder and Shanahan [2006]). Of particular importance in this respect might be key life course transitions like family formation that involve changes in daily routines and activities, associated changes of employment status, but also changing roles and role expectations. Effectively, the central tenet of any exposure model of preference formation is that exposure to novel circumstances and conditions will trigger a certain extent of corresponding preference adaptation, so that work-oriented events and life course transitions should in general engender attitudinal change toward more work-oriented preferences, whereas family-oriented transitions should have the opposite effect (Bielby and Bielby 1984, 1989; Bielby 1992). Importantly, this prediction refers to a genuine adaptation of preferences that occurs as an unintended consequence of the (potentially self-chosen) life course event in question—whether resulting from an ex post cognitive rationalization of the original decision made in order to minimize cognitive dissonance (Festinger 1957), as a sour grapes response of mentally discounting the desirability of alternatives that are currently unavailable (Elster 1983), or due to roles as experience goods and the genuine reevaluation of alternatives based on lived experience that any major role transition may bring about—net of any tendency of, for exam-

ple, initially more family-oriented men and women being more likely to have children in the first place. In the current context, we apply this exposure perspective to account for the impact of life course events, notably childbirth, on women's work identities, work-family preferences, and subjective work commitment. In what follows, we see these terms interchangeably as all corresponding to Bielby and Bielby's (1984, p. 235) seminal definition of "work commitment as the centrality of the work role as a source of intrinsic satisfaction relative to other adult roles," which succinctly comprises the crucial distinction between work commitment (and closely related concepts) as representing individuals' *personal satisfaction* derived from alternative life course roles on the one hand and gender or motherhood norms that refer to (shared) attitudes and belief systems concerning the social desirability or moral appropriateness of particular behaviors, notably the socially acceptable balance between care and work activities while raising children, on the other hand (see also Hakim 2003*b*).

At present, and not the least because of the acute dearth of longitudinal data on women's work identities and work commitment, empirical evidence on the importance of respective life cycle effects is very limited. In their own seminal study, Bielby and Bielby (1984) in fact did find women's work commitment to be responsive to life course events such as childbirth and marriage, and this result has also been confirmed in a recent study by Evertsson (2013) using representative Swedish panel data. Also, though confined to cross-sectional and trend data analysis, Hakim (2003*a*) reports increasing support for egalitarian role models among younger women in British and Spanish data, which, in the absence of discernible cohort effects, she attributes to learning from actual life course experiences. Further supportive evidence on the role of life course transitions can be derived from the closely related literature on trends in gender norms and gender ideologies, where it is typically reported that observable age effects are fully accounted for by family and employment status covariates (e.g., Bolzendahl and Myers 2004; Brooks and Bolzendahl 2004; Pampel 2011; Kraaykamp 2012). This aggregate evidence also resonates well with findings from the longitudinal studies of Bielby and Bielby (1984, 1989; see also Bielby 1992) on workplace involvement and continuity of careers as reinforcing women's work identity, whereas part-time or intermittent participation, let alone work interruptions, have the opposite effect. In a similar vein, Evertsson (2013) reports that improving career prospects became reflected in women's increasing work commitment in Sweden.

Normative Policy Feedback through Contextualized Role Exposure

If preferences are shaped by and adapt to life course events, the basic exposure model directly suggests one key channel of policy effects on women's

work commitment. In short, policy feedback may be expected to occur in this case because, if, and to the extent that public policies empirically affect the nature of this role transition, most critically of course with respect to caregivers' actual life course trajectories in terms of changes in labor force participation and employment. A statutory parental leave program, especially in the form of quite extended job guarantees supported by accompanying leave benefits as usual in European welfare states, represents a public intervention that is naturally highly salient in this respect. Providing parents with the right to claim parental leave while protecting their jobs and supporting families through parenting benefits in the meantime sets deliberate economic incentives in favor of one or both parents interrupting their careers temporarily on behalf of child welfare (e.g., Gornick et al. 2009; Blau et al. 2010).

In fact, all of the available empirical evidence suggests that women, as prime caregivers, tend to respond strongly to respective policy incentives, so that extensions of parental leave entitlements are usually followed by corresponding increases in the actual duration of employment interruptions (e.g., Ondrich et al. 1996; Rønsen and Sundström 2002; Lalive and Zweimüller 2009; Ziefle 2009; Grunow et al. 2011; Schönberg and Ludsteck 2014; Ziefle and Gangl 2014). As parental leave programs thus directly alter women's actual life course experiences, the degree of women's exposure to the caregiver role varies in response to policy context, and, as described before, standard exposure theory then suggests that more extensive exposure to nonwork environments will tend to weaken work commitment (Bielby and Bielby 1984, 1989; Bielby 1992). In other words, the more generous parental leave entitlements become, the more negative the impact of motherhood on women's subjective work commitment because extended leave entitlements tend to increase mothers' time off work (and vice versa). Importantly, and in contrast to the second channel of influence to be discussed presently, this mechanism describes a microlevel process that strictly depends on the actual life course trajectory of the direct caregiver, for whom policy-induced exposure to nonwork environments is expected to decrease work orientation. By direct implication, this also suggests that negative policy feedback of parental leave on work commitment critically depends on the extent of policy-induced changes to the actual duration of caregivers' employment interruptions.

Normative Policy Feedback from Cultural Diffusion and Norm Setting

Besides microlevel exposure effects just described, welfare state institutions are likely to have important broader contextual effects on preferences that extend beyond the implications of direct behavioral change, but also beyond the direct beneficiaries of any specific policy or program. As Kre-

mer (2007) in particular has argued, family policy embeds and reflects broader social norms of balancing motherhood and employment, and institutions designed accordingly thus promote specific models of care in a behavioral and, more importantly, also in a moral sense (see Lewis 1992; Orloff 1996; Cooke 2011; for the case of Germany, see specifically Pfau-Effinger [2004, 2008]). In other words, as family policy institutions embed consensus solutions for the trade-offs between women's work and family roles, they may also serve as normative anchors in the process of preference formation and change, especially within the context of major life course transitions that involve both exposure to novel situations and receptivity to novel information. One plausible mechanism of normative anchoring is that public policies provide highly legitimate focal point solutions that individual preferences adapt to. The main alternative is that normative anchoring occurs through cultural diffusion where preference adaptation follows from changes of individual role perceptions and expectations that internalize policy-induced and empirically observable changes in other mothers' (typical) care and employment behaviors. In either case, this normative anchoring of care models precisely corresponds to Homans's (1974) famous dictum of "What is is always becoming what ought to be."

To the extent that such normative anchoring of care ideals occurs in practice, family policy instruments like parental leave programs will have a sustained impact on individual preferences due to the social multiplier effects they create, not the least since the latter are not confined to the population of caregivers experiencing actual change in personal employment trajectories and life course roles. Actually, depending on the specific nature of such macrolevel multipliers, these wider policy feedback processes may be relevant either in the short run if policy mainly provides cultural and normative focal points or more in the medium to longer run if changes of preferences result from internalizing observed changes in typical care behavior in caregivers' social environment. At present, there is no prior work available that would provide direct evidence of such norm setting behavior, yet the broader welfare state literature is strongly suggestive of the general mechanism insofar as, across countries, the level of public support for the welfare state clearly correlates with broad institutional features of welfare states (Svallfors 1997, 2006, 2007, 2010; Andress and Heien 2001; with respect to gender roles more specifically, see Lück and Hofäcker [2008]). In fact, the theoretical literature in the area likewise argues that besides redistributing resources and defining political constituencies (Pierson 1993; Campbell 2012), policies critically influence citizens' understanding of their rights and responsibilities in society (Mau 2003, 2004; Svallfors 2006, 2007; with respect to gender inequality, see specifically Lewis [1992], Orloff [1996, 2009], Pfau-Effinger [2004, 2008], and Kremer [2007]). In our present work we extend this argument to processes of, as we term it, normative policy feedback

in which policy may affect citizens' perceptions of the desirability of alternative life course roles in terms of personal satisfaction, that is, relative utility, rather than citizens' moral belief systems or role norms more commonly addressed in current research. Applied to the case of parental leave programs specifically, this once more entails the prediction that extensions of program generosity, notably in terms of the duration of paid leave, will tend to decrease mothers' work commitment (and vice versa). As the channel of influence just described is cultural rather than behavioral, however, respective policy feedback is likely to occur broadly within the population of new mothers rather than being restricted to the subset of mothers whose employment status and broader life course trajectories have actually been changing in conjunction with childbirth.

WOMEN'S EMPLOYMENT AND GENDER INEQUALITY IN THE GERMAN LABOR MARKET

In what follows, we provide a detailed empirical test of normative policy feedback and its generative mechanisms at the micro- and macrolevel through a quasi-experimental quantitative case study of the impact of a major policy reform within Germany's federal parental leave program on women's work commitment. Before describing our research design and the empirical results in greater detail, it seems worthwhile to briefly summarize key aspects of women's labor market position and the institutional environment in Germany, including a detailed description of the specific policy changes in its parental leave program that underpin our empirical study in the next section.

As is well known from the comparative literature (e.g., Lewis 1992; Gauthier 1996; Sainsbury 1996, 1999; Gornick, Meyers, and Ross 1997; Esping-Andersen 1999, 2009; Korpi 2000; Pfau-Effinger 2004; Kenworthy 2008; Pettit and Hook 2009; Cooke 2011), Germany is an interesting country case to study with respect to gender inequality. Institutionally, Germany in many respects is the prototypical example of the conservative continental European welfare regime as far as decommodification is concerned (Esping-Andersen 1990; Korpi and Palme 1998), combined with a system of generous family policies geared toward supporting the traditional gender division of labor (Lewis 1992; Gauthier 1996; Sainsbury 1999; Korpi 2000; Gornick and Meyers 2003; Pfau-Effinger 2004; Rosenfeld, Trappe, and Gornick 2004; Pettit and Hook 2009; Korpi et al. 2013). German family policies have historically exhibited a focus on universal cash transfers to families, notably through universal child benefits and generous tax breaks to single-earner families through its system of joint income taxation (Gustafsson 1992; Dingeldey 2001). Also, Germany's long-established semipublic kindergarten system does provide near-universal coverage of subsidized child care facili-

ties primarily aimed at children ages 3–6, yet prekindergarten child care has, up until very recently, been strongly familialized via both generous parental leave entitlements and a lack of both private and public prekindergarten child care facilities (Gornick et al. 1997; Rosenfeld et al. 2004).

These features of the institutional context are also reflected in patterns of gender inequality in the labor market, especially if Germany is compared to other industrial countries. Most fundamentally, women's labor force participation grew considerably more slowly after the 1960s than in the United States and other continental or northern European countries and has fully caught up only during the 1990s and, especially, the 2000s (Gornick et al. 1997; Rosenfeld et al. 2004; Pettit and Hook 2005, 2009; Mandel and Semyonov 2006; Kenworthy 2008; Hanel and Riphahn 2012). Characteristically, motherhood has continued to be a prime factor in women's careers, with the German labor market featuring large employment penalties for motherhood well above those common in either Scandinavian or Anglo-Saxon environments (Gornick et al. 1997; Korpi 2000; Pettit and Hook 2005, 2009; Mandel and Semyonov 2006; Kenworthy 2008; Gangl and Ziefle 2009). Occupational sex segregation and the gender wage gap are comparable in level to those of the United States or Britain and are, especially as far as the gender pay gap is concerned, clearly above levels observed in the Nordic countries (e.g., Waldfogel 1998; Rosenfeld et al. 2004; Mandel and Semyonov 2005). Paralleling the case of employment, comparatively large wage penalties for motherhood play a significant role for gender wage gaps (Waldfogel 1998; Rosenfeld et al. 2004; Sigle-Rushton and Waldfogel 2007; Gangl and Ziefle 2009), while strong union representation and widespread collective bargaining coverage tend to reduce the impact of occupational segregation for gender wage inequality (Blau and Kahn 2003; Hinz and Gartner 2005).

Finally, German reunification adds an interesting historical twist insofar as it implied, among other things, the unification of two very distinct gender regimes, namely, the former West German male breadwinner model sketched before and the former East German dual-earner model built around high levels of women's labor force participation and extensive child care services characteristic of state socialist economies (Rosenfeld et al. 2004). Despite convergence in occupational and industrial structures as well as the adoption of West German institutions in all policy areas (Rosenfeld et al. 2004), several characteristic differences continue to represent a legacy of the former German Democratic Republic's gender egalitarianism. Institutionally, public child care services for prekindergarten children continue to be more prevalent in East Germany, typically covering about one-third of children under 3 years old as compared to the West German average of about 5% (Dienel 2002). Also, and despite some quite remarkable convergence toward the West German model of the full-time male breadwinner and the (part-time) female secondary earner since the mid-1990s, East German

women's labor force participation has remained above West German levels, in particular among mothers with small children (Rosenfeld et al. 2004; Kreyenfeld, Konietzka, and Böhm 2007; Hanel and Riphahn 2012), and gender norms continue to be significantly more progressive among East German women and, especially, men (Braun, Scott, and Alwin 1994; Lee, Alwin, and Tufiş 2007).

COMPLETING GERMANY'S CONSERVATIVE GENDER REGIME: PHASING IN PARENTAL LEAVE ENTITLEMENTS

While the above and much of the comparative welfare state literature may give the impression of long-run stability in broad welfare and gender regime arrangements, it should not be underestimated that many family policy programs are of relatively recent origin. Quite in contrast to other elements of welfare states, family policy clearly has been an area of welfare state expansion and policy innovation in most if not all industrialized countries since the 1970s (Gauthier 1996; Korpi 2000; Henderson and White 2004; Korpi et al. 2013). In fact, Germany is no exception to this, and Rosenfeld et al. (2004, p. 104) have rightly observed that "the distinct employment and social policies [relevant to gender inequality] in the East and West reached full maturity only in the 1980s." In West Germany, the introduction of a statutory maternity leave entitlement (*Mutterschaftsurlaub*) by the then Social Democrat–led administration in 1979 represented the first measure to respond to women's rising labor force participation and the increasingly pressing issue of balancing work and family roles (see Dienel [2002] for the following). The new maternity leave was built to extend the already existent maternal protection legislation (*Mutterschutz*) that stipulates a 14-week period around expected delivery during which prospective mothers are prohibited from working but continue to receive their full salary as a mandatory employer-provided benefit. The 1979 maternity leave program for the first time provided an entitlement of an additional four months of leave from mothers' current contract plus a maternity leave benefit that replaced earnings up to a benefit cap of DM 750, then about half the average earnings of female full-time workers.

Following the election of a conservative-liberal government in 1982, policy priorities shifted further toward subsidizing familial infant care. By 1986, the Federal Parental Leave Benefit Act (*Bundeserziehungsgeldgesetz*) created a statutory parental leave entitlement (*Erziehungsurlaub*) and an associated parenting benefit (*Erziehungsgeld*) with the stated goals of supporting familial care of infants and of limiting the fiscal costs of family policy relative to extended public child care provision (Bundesminister für Jugend 1989). The Parental Leave Act did not merely amount to a single change of entitlements, but, being contingent on fiscal constraints, actually

comprised a sequence of successive entitlement extensions that were eventually implemented between 1986 and 1992. The initial 1986 reform introduced a statutory parental leave entitlement up until 10 months after birth, including the maternal protection period. Leave could be claimed by either parent, though in practice it was mothers who continued to almost exclusively use the leave entitlement (Bundesminister für Jugend 1989). As under the earlier maternity leave act, employers were prohibited from either renegotiating job contracts or dismissing workers on parental leave. Parental leave taking was furthermore supported by a flat-rate parenting benefit of DM 600 per month that turned into an income-tested benefit after six months. Unlike the earlier maternity leave benefit, however, parenting benefits were also paid to economically inactive parents.

Subsequently, leave entitlements were successively extended without affecting the fundamental character of the program. For all births since January 1988, the duration of parental leave entitlements and benefits was extended to 12 months, to 15 months since June 1989, and to 18 months since June 1990. Effective January 1992, the duration of parental leave was finally extended to 36 months, although the duration of federal parenting benefits was increased to 24 months only.[2] With a slight modification in 2001—when parents were entitled to use up to one year of parental leave (now termed *Elternzeit*) flexibly anytime between the second and eighth birthdays of a child—the system remained unchanged up until January 2007, when an all-new parenting benefit (*Elterngeld*) was taking effect that closely mirrors the corresponding Swedish program by offering generous earnings replacement during parental leave, but by also being limited to only 12 months of paid leave for the primary caregiver while including separate provisions for two additional "daddy months" of paid leave set aside for the secondary caregiver (Bundesministerium für Familie 2008; also see fig. 1 for a concise summary of the time line of the various policy changes). Since its inception, the parental leave program has been one of the most popular social policies in Germany, as upward of 95% of parents, thereof the mother in about 98% of cases, receive parenting benefits for their newborn children, which for lack of better official statistics also suggests a very high take-up rate of parental leave among working women since receipt of parenting benefits presupposes using at least some parental leave entitlement (Bundesminister für Jugend 1989). As its most consistent impact, several recent German studies have found that the parental leave program has successively increased the duration of child-related employment interruptions, not the least since many mothers tend to exhaust statutory

[2] In response, several German states introduced state parenting benefits that continued the federal benefit program during the third year of parental leave entitlements, although sometimes at lower benefit rates.

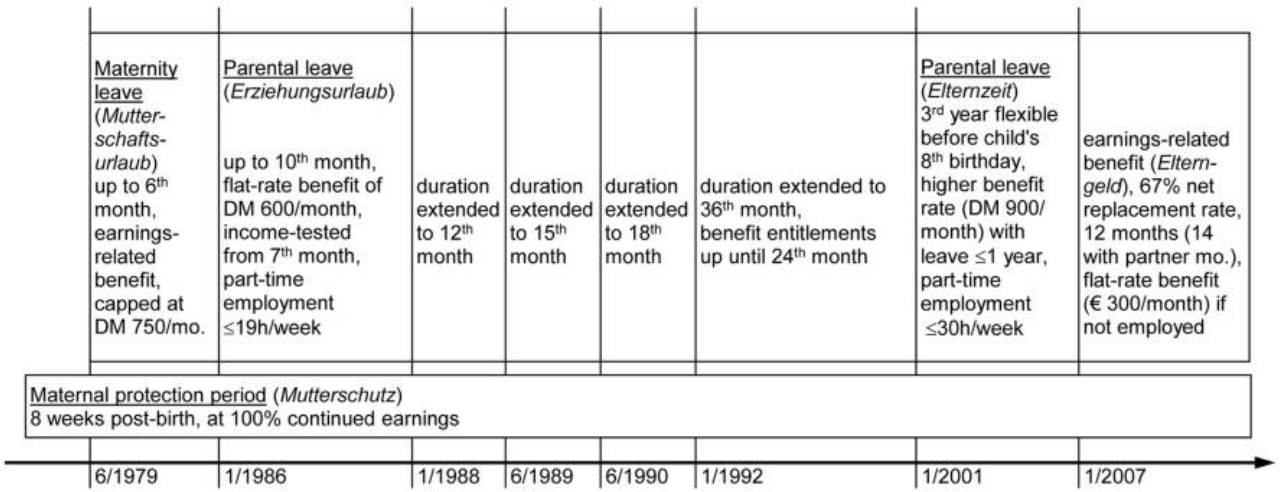

FIG. 1.—Maternity and parental leave entitlements in Germany, late 1970s–2010. Data are from Dienel (2002) and Bundesministerium für Familie, Senioren, Frauen und Jugend (2008).

leave entitlements (Ondrich et al. 1996; Ziefle 2009; Schönberg and Ludsteck 2014; Ziefle and Gangl 2014).

RESEARCH DESIGN, DATA, AND STATISTICAL METHODOLOGY

The present study uses a unique data set to address the empirical relationship between welfare state institutions and women's work commitment. More specifically, we draw on the 1990–2004 waves (survey waves G–U) of the German Socio-Economic Panel (GSOEP), a nationally representative longitudinal study of West and East German households (Wagner, Frick, and Schupp 2007). The GSOEP contains two item batteries that may be used to construct measures of women's subjective work commitment in the sense of Bielby and Bielby's (1984, p. 235) definition of "the centrality of the work role as a source of intrinsic satisfaction relative to other adult roles." As these batteries have been fielded occasionally between 1990 and 2004, the GSOEP provides unique survey data on women's work-family preferences spanning much of the critical policy period in question. And although the GSOEP preference data do not encompass the full period since the introduction of the federal parental leave entitlement in 1986, the available survey data do permit us to trace the impact of policy reforms since 1990, which in particular includes the watershed 1992 reform. As a result, the GSOEP survey data enable us to exploit the occurrence of two policy changes in Germany's parental leave program as a natural experiment on the impact of parental leave entitlements on women's work commitment. The design of the GSOEP as a longitudinal household survey furthermore aids the identification of policy effects in two major ways. First, as a household survey, the GSOEP permits us to include a rich set of control variables, ranging from data on current demographics, employment, occupations, or wages to job histories and biographical information for both respondents and their partners. Second, as a panel survey, the GSOEP is unique in providing longitudinal preference data at the individual level, allowing for unusually effective control of unobserved heterogeneity bias and a focus on actual preference change at the respondent level in our ITS design following on a major policy shift in Germany.

Research Design and Specific Hypotheses

The core question of our study is whether and to what extent women's subjective work commitment is affected by prevailing family policy arrangements, notably parental leave entitlements. The German policy reforms provide a natural experiment to address this issue, as the sequential extension of statutory entitlements creates exogenous variation in the policy environment. In addition, being able to conduct the evaluation of institutional

impacts through an ITS (before-and-after) design within a single country has the advantage of keeping many important factors fixed, notably gender culture, but also labor market regulation, industrial relation systems, and other features of welfare regimes that may be hard to explicitly measure, or the partial impact of which may be hard to effectively isolate and control for in more conventional macrocomparative analyses.

Thanks to the fact that Germany introduced its parental leave program sequentially over a relatively short period of time, our natural experiment actually comprises multiple treatment groups defined by alternative family policy contexts of increasing generosity of parental leave entitlements.[3] As our GSOEP survey data span the 1990–2004 period, we observe women during the 1989–91 period when leave entitlements stood at 18 months after birth, between 1992 and 2000 with 36 months of postbirth leave, and from 2001 onward when the third year of leave entitlements could be flexibly taken anytime between a child's second and eighth birthdays.[4] Furthermore, there are two separate treatment groups within any policy period since parental leave entitlements by definition apply to working mothers only, whereas parenting benefits were paid to employed and nonemployed mothers alike. As a consequence, there are two treatment groups with varying treatment intensity per period: parental leave and flat-rate benefits to mothers employed before birth and flat-rate benefits only to women who had been economically inactive before confinement. To estimate treatment effects from difference-in-differences and triple-differenced estimators (see the section on statistical estimation below), these two treatment groups will be contrasted to two separate control groups in each policy period, namely (young) women without children and (older) mothers with fertility histories completed prior to 1989 for whom changes in subjective work commitment are likewise traced across the observation window. Assignment to all treatment groups is dynamic in principle; that is, group membership changes at the point of an additional birth during a particular policy period, and multiple treatment group membership is possible for women

[3] In addition, while projected in principle, the precise sequence of policy changes was far from being predictable for individual households because extensions were contingent on fiscal constraints; as a consequence, the potential for endogenous treatment assignment as families may otherwise deliberately have wanted to plan fertility in ways to ensure coverage under specific policy regimes is quite limited in principle (see Ondrich et al. 1996), even though our statistical estimators will naturally also incorporate precautions to safeguard our inferences against respective bias (see the section on statistical estimation below).

[4] Strictly speaking, the 1989–91 period could be divided further as statutory leave entitlements were increased from 15 to 18 months effective June 1, 1990. As the first survey wave containing the relevant preference data was fielded in 1990 only, there are very few births observed before this policy change, however. Pragmatically, we refrain from further differentiation within the earliest policy period.

giving multiple births within the observation window. As a result, our empirical parameter estimates identify the marginal effect of treatment group membership on women's work commitment.

The wealth of available contrasts in this design enables us to test our theoretical framework in an unusually detailed and nuanced way. To begin with, the successive extension of entitlements across policy periods implies that any policy effect on women's work commitment should become more pronounced in later policy periods, potentially especially strongly so with the 1992 watershed reform that introduced the 36-month leave entitlement. Consistent with the notion that preferences may be critically shaped by prevailing institutional realities at the point of important life course transitions, we expect policy feedback effects operating through exposure mechanisms to be confined to women who gave birth during the period in question and who thus directly experienced the varying realities of family policy institutions. In addition, the distinction between employed and nonemployed mothers (i.e., births covered by parental leave entitlements or not) enables us to tap into alternative mechanisms that underlie any observed policy feedback on women's work commitment. If microlevel processes of role exposure are the prime transmission channel, policy feedback should be observed only among mothers employed prior to birth, whose actual behavior—that is, the length of work interruptions—is critically affected by increasingly more generous parental leave entitlements. Also, it is evident that the 1992 policy change should have the largest impact in this case since this is where the major extension of leave entitlements occurred.

In contrast, if norm setting plays a significant role, spillover feedback effects to economically inactive mothers should occur as well, whether through changes in their own expectations following from social changes in observed employment behavior or because of financial and moral compensation of the caregiving role that is associated with universal parenting benefits. This norm setting channel should in fact be particularly relevant with respect to the smaller 2001 policy change. As material incentives hardly changed from the 1992 policy period, and if anything became more employment-friendly via the introduction of a higher short-term parenting benefit, exposure-based mechanisms would not predict an additional (marginal) impact of the 2001 policy change on women's work-family preferences. In terms of norm setting, however, the flexibility of being able to spend time on parental leave up until the eighth birthday of a child may signal a significant extension of what may be conceived as the legitimate caregiving period, and also the fact that the policy change was heavily touted as a family-friendly measure in the German federal election of 2002 may have generated respective cultural policy feedback. Finally, we should also note that such cultural norm setting may generate spillovers to young women's work commitment prior to actual family formation, which we hence define as a separate important

control group. In contrast, women with completed fertility histories prior to our observation window serve as an overall control group benchmarking our analysis against secular trends in women's work commitment that may have occurred irrespective of changing parental leave policies. From our theoretical framework of policy-induced preference change working through either direct exposure to the caregiver role or norm setting via changing care behavior in the population of mothers, we believe it is possible to safely rule out any kind of policy effects among women with completed fertility histories, whose individual work commitment should not be affected by policy changes irrelevant to their own life courses. For the reader's convenience, we summarize these various design contrasts and our associated theoretical expectations in table 1.

To respect differences in state-level policies, historical experiences, and gender cultures, but also the unique experience of the transition from state

TABLE 1
Comparison Groups in the DDD Design

Policy Period and Treatment and Control Groups	Hypothesized Effect on Work Commitment			
	Role Exposure		Norm Setting	
	Vs. Control Group (DiD)	Marginal (DDD)	Vs. Control Group (DiD)	Marginal (DDD)
1989–91:				
Employed mothers, birth covered by parental leave 1989–91	(−)	Baseline	(−)	Baseline
Nonemployed mothers, birth 1989–91, not covered by parental leave	0	Baseline	(−)	Baseline
Women without children	Baseline	Baseline	Baseline	Baseline
Mothers with pre-1989 fertility history	0	Baseline	0	Baseline
1992–2000:				
Employed mothers, birth covered by parental leave 1992–2000	−	−	−	−
Nonemployed mothers, birth 1992–2000, not covered by parental leave	0	0	−	−
Women without children	0	0	(−)	(−)
Mothers with pre-1989 fertility history	0	0	0	0
2001–4:				
Employed mothers, birth covered by parental leave 2001–4	−	0	−−	−
Nonemployed mothers, birth 2001–4, not covered by parental leave	0	0	−−	−
Women without children	0	0	−	(−)
Mothers with pre-1989 fertility history	0	0	0	0

Note.—0 = null impact, (−) weakly negative impact, − negative impact, −−strongly negative impact.

socialism and the collapse of the East German economy that may have differentially affected women's work-family preferences in the two parts of Germany, we moreover systematically test all our hypotheses separately for East and West Germany. Consistent with socialization theories of preference formation, we apply a developmental rather than residential sample separation; that is, our East German sample includes all women who grew up in either the former German Democratic Republic or one of the East German states of reunified Germany, irrespective of current state of residence, and vice versa.[5] As one immediate implication, the first treatment group among East German women is defined by births of 1991 only, whereas all pre-reunification births of 1990 have been assigned to the control group of mothers with fertility histories prior to survey observation, that is, fertility histories during the former German Democratic Republic in this case.[6] To gain additional insight into whether the broader gender regime and gender culture may have mattered for the relationship between parental leave policies and women's preferences, we apply our general research design as summarized in table 1 separately to the East and West German samples thus defined in all the subsequent analyses.

Data

The 1990–2004 waves (survey waves G–U) of the GSOEP contain item batteries to construct appropriate measures of women's work commitment, the dependent variable of this study. More specifically, the GSOEP has administered two batteries that record subjective measures of various life domains as sources of personal satisfaction that can be used to determine the relative

[5] Our material results are unaffected by this choice, not the least since East-West migration flows, while significant during the immediate transition after German reunification in historical perspective, affect only a minority of women in our sample, namely, some 10% of East German and about 0.5% of West German women as defined by our socialization-based sample split. Full results for our respective sensitivity analysis are available in table S4 of the online supplement to this article.

[6] German reunification occurred on October 3, 1990. The 1990 East German wave of the GSOEP was fielded during the summer of 1990, i.e., was completed before the directly impending institutional and economic transformation. As a consequence of reunification, the role of the 1991 treatment groups also differs between East and West Germany. Quite apart from the relatively small number of births in this single-year treatment group that inevitably reduces the statistical power of our data to isolate any meaningful treatment effect among East German women, the economic turbulence of the immediate transition period following reunification must leave considerable doubts about whether looking at changes in East German women's work commitment in 1991 should be seen as a useful natural experiment to isolate the impact of changing parental leave entitlements at all. For substantive as well as practical purposes, we will hence consider the 1991 East German observations largely as another baseline control group from which to assess any changes in work commitment following from the two subsequent policy changes.

salience of work and family roles as a source of intrinsic satisfaction. The first of these batteries (referred to as battery 1 in the following) is worded as follows: "Various things can be important for various people. Are the following things currently (1) Very important, (2) Important, (3) Less important or (4) Not at all important to you?" and includes items (*a*) "Independently maintain a high standard of living [*sich etwas leisten können*]," (*b*) "To realize one's potential [*sich selbst verwirklichen*]," (*c*) "Be successful in one's career [*Erfolg im Beruf haben*]," (*d*) "Have a happy marriage/relationship [*eine glückliche Ehe/Partnerschaft haben*]," and (*e*) "Have children [*Kinder haben*]," among others. The second battery (battery 2 henceforth) is worded as follows: "With respect to well-being and satisfaction with life, which of the following aspects are (1) Very important, (2) Important, (3) Less important or (4) Not at all important to you?" and has items (*a*) "Work [*die Arbeit*]," (*b*) "Family [*die Familie*]," (*c*) "Income [*das Einkommen*]," and (*d*) "a successful career [*der Erfolg im Beruf*]," among others.[7] Both item batteries have been administered repeatedly yet do not belong to the core GSOEP questionnaire that is annually repeated. For the purposes of this article, it is extremely fortunate that both batteries have been administered in temporal intervals that enable us to exploit the natural experiment of policy change in Germany precisely as described before.

More specifically, battery 1 was administered in 1990, 1992, 1995, and 2004 (waves G, I, L, and U) in the West German samples of the GSOEP, thus generating preference observations during each of the three 1989–91, 1992–2000, and 2001–7 policy periods. As this permits a consistent evaluation of change in women's work-family preferences over the whole period in question, all West German analyses will be based on battery 1 exclusively. With respect to East German respondents, data options unfortunately are slightly more restricted. Battery 1 was administered in 1992, 1995, and 2004 in the East German GSOEP sample only, which thus allows for an evaluation of preference change following the 2001 reform, that is, between the second and third policy periods only. Fortunately for this article, the GSOEP administered battery 2 among East German respondents in 1990, 1991, 1994, 1998, and 1999 (waves G, H, K, O, and P) so that a separate assessment of preference change between policy periods 1 and 2, that is, the impact of the watershed 1992 reform, is feasible for East Germany as well. Our analyses for West Germany will not draw on battery 2 at all, since it was administered only in 1994, 1998, and 1999, that is, exclusively within the second policy period, in the West German sample.

[7] Items omitted for the current analysis include "Be there for others," "Own a house," "Be politically and/or socially involved," and "See the world and/or travel extensively" (all battery 1) and "Friends," "Leisure," "Home," "Political influence," "Health," "Conservation of natural environment," "Faith/religion," "Neighborhood," and "Mobility" (all battery 2), respectively.

We utilize these batteries to construct measures of subjective relative work orientation, that is, the subjective relative importance of work over family as a source of well-being and satisfaction, aiming to operationalize Bielby and Bielby's (1984) classic definition of work commitment. More specifically, we construct a difference score

$$RW_{it} = \hat{f}_{it,\text{work}} - \hat{f}_{it,\text{family}} \quad (1)$$

separately for each battery using the predicted factor scores for family and work orientation from a principal components analysis of the above items. Empirically, we obtain clearly distinct principal components for both batteries, with items *a*–*c* loading exclusively on one (work) component and items *d* and *e* on a second (family) component in the case of battery 1, and item *b* versus items *a* and *d* forming the family-work contrast in battery 2.[8] Besides proper adherence to conceptual foundations, forming a difference score also implies that interpersonal heterogeneity in placing the location of the scales' verbal stimuli, usually a major concern in analyses of attitude data (see King et al. 2004), is accounted for in our analysis. In addition, our difference scores constructed from the predicted factor scores correlate very highly with difference scores based on the raw data for the directly relevant items. In the case of battery 1, the correlation between the factor difference score and the difference of raw items *e* and *c* is .84; for battery 2, the correlation between the factor difference score and the difference of raw items *b* and *e* is even .91. As a consequence, our substantive conclusions do not materially differ between analyses that use, as we do here, the factor difference scores as their dependent variable and analyses based on differences of raw scores.[9]

We base our subsequent analysis on all GSOEP women respondents of working age, that is, between ages 16 and 64, at the time of the survey interview. Depending on the item battery used, we retain samples of about 5,000–5,500 person-year observations of complete preference and covariate data for about 2,500–3,000 East German GSOEP respondents and close to 15,000 complete-data person-year observations for some 8,000 West German women. All covariates used in our analyses are described in more de-

[8] From battery 2, item *c*, income, is not clearly associated with either the family or the work factor but loads about equally on both ($\lambda = .29$ and $\lambda = .38$, respectively). For the retained items, relevant item-factor correlations are in the range of $\rho = .591$–$.794$ for the items loading on the work factor and $\rho = .820$–$.822$ for the items loading on the family factor from battery 1; respective correlations are $\rho = .853$–$.871$ on the work factor and $\rho = .927$ on the family factor from battery 2.

[9] We have in fact conducted the entire empirical analysis using six alternative measures for the dependent variable, including analyses of difference scores of the relevant raw items and analyses using direct (i.e., nondifferenced) factor scores or raw items, without material changes in our substantive conclusions. Detailed results from these supplementary analyses are available in tables S1–S3 of the online supplement to this article.

tail in the next section, and the accompanying appendix table A1 provides essential descriptive statistics on our estimation samples.

Statistical Estimation

As with any interrupted time-series design, difference-in-differences (DiD) estimators are the primary vehicle to estimate the average impact of treatment on outcomes (cf. Moffitt 2005; Morgan and Winship 2007; Gangl 2010; Wooldridge 2010), in our case the average impact of policy change on women's work commitment.[10] The simplest estimator is to use the ordinary least squares (OLS) regression

$$RW_{it} = \alpha_t + \Delta \mathbf{D}_{it} + \boldsymbol{\beta} \mathbf{X}_{it} + \gamma_{ct} + \varepsilon_{it}, \quad (2)$$

which gives the group-level DiD estimator. The OLS regression (2) estimates the vector of treatment effects Δ of belonging to one of the six treatment groups identified in table 1 above on women's relative work orientation RW_{it} relative to period effects γ_{ct} that describe baseline changes in the two control groups by survey wave t.[11] Equation (2) furthermore controls for a set of covariates $\mathbf{X}_{it}$ in order to adjust for both baseline differences in observable covariates and observable sources of concomitant group-specific change that is unrelated to changing parental leave entitlements. On the basis of extensive specification searches, we arrived at a parsimonious model specification that includes age and its square, education, number of children, age of the youngest child and its square, partner log earnings, and state female unemployment rates as empirically relevant controls.

It is widely known that the group-level DiD estimator (2) may be biased if observed covariates provide insufficient control of either unobserved heterogeneity between comparison groups or group-specific changes unrelated to the treatment of interest (see Cook and Campbell 1979). The availability of unique panel data on women's work-family preferences from the GSOEP affords the opportunity to safeguard our analyses against many of these biases, however. More specifically, our panel data on women's work commit-

[10] Strictly speaking, we are interested in the average treatment effect on the treated that describes the average response to treatment in a historically given context. However, since we can also plausibly negate the assumption of a means-end relationship between family formation and subsequent preference change, the average treatment effect on the treated and the average treatment effect coincide, allowing us to simplify terminology.

[11] Strictly speaking, women with completed fertility histories prior to our observation period form the reference group in all analyses. Since our empirical estimates do not indicate any significant differences in preference trends between young women prior to first birth and women with completed fertility histories, however, for simplicity we refer to both groups as the control groups of our analysis against which the marginal effects of belonging to one of the six treatment groups are being contrasted.

ment permit us to estimate person-level DiD estimators via either the fixed-effects (FE) estimator

$$RW_{it} = \alpha_t + \Delta\mathbf{D}_{it} + \boldsymbol{\beta}\mathbf{X}_{it} + \gamma_{ct} + \mu_i + \varepsilon_{it}, \quad (3)$$

the lagged dependent variable (LDV) estimator

$$RW_{it} = \beta_0 RW_{it-1} + \alpha_t + \Delta\mathbf{D}_{it} + \boldsymbol{\beta}\mathbf{X}_{it} + \gamma_{ct} + \varepsilon_{it}, \quad (4)$$

or the Arellano-Bond (AB) estimator

$$RW_{it} = \beta_0 RW_{it-1} + \alpha_t + \Delta\mathbf{D}_{it} + \boldsymbol{\beta}\mathbf{X}_{it} + \gamma_{ct} + \mu_i + \varepsilon_{it} \quad (5)$$

(see Halaby 2004; Morgan and Winship 2007; Gangl 2010; Wooldridge 2010). These panel data estimators enable us to control for unobserved heterogeneity between respondents arising from the impact of fixed effects μ_i of temporally invariant unobserved characteristics of respondents (FE), individual past work orientation RW_{it-1} (LDV), or both (AB). From the perspective of both exposure theories of preference formation and standard life course analysis that conceives of life courses as "endogenous causal chains" (Mayer and Müller 1986), it is the LDV model that is theoretically most appealing in the present context, and we will mostly focus on respective estimates when discussing results. To allow for greater theoretical eclecticism, but also as a sensitivity analysis on the impact of differences in substantive assumptions implied by each model, we will systematically provide readers with estimates from all four DiD estimators below.[12] Also, in addition to the standard covariate specification described before, the two dynamic panel (LDV and AB) estimators enable us to incorporate the lagged effects of marital and employment status, own log earnings, and labor force experience, a parsimonious specification for the vector of dynamic control variables again achieved after extensive specification searches.

Because our data cover multiple policy periods, our parameter estimates from any of equations (2)–(5) actually imply the triple-differenced (DDD) estimate of the impact of policy change on women's work commitment across periods p of

[12] As Angrist and Pischke (2009) note, the FE and LDV estimators can usefully be considered as bracketing estimates of the true parameter under the alternative substantive assumption about the role of unobservables. The more flexible AB estimator takes the middle ground position in this respect yet also requires at least one additional wave of data—i.e., a minimum of three for DiD estimation, a minimum of four for triple-differenced (DDD) estimation—because of the need to construct lagged instrumental variables. In consequence, we are able to apply the AB estimator in the case of our analyses for West Germany only and can only use this to form a DDD estimate of the policy change only between 1992 and 2001, i.e., the second and third policy periods we distinguish.

$$\begin{aligned}\hat{\Delta}_{\mathrm{DDD}} = E(RW_{i,p=2} - RW_{i,p=1}) - \{\beta_0(E(RW_{it-1,p=2}) - E(RW_{it-1,p=1}))\\ + \boldsymbol{\beta}(E(\mathbf{X}_{i,p=2}) - E(\mathbf{X}_{i,p=1})) + (\gamma_{c,p=2} - \gamma_{c,p=1})\\ + [E(\mu_{i,p=2}) - E(\mu_{i,p=1})]\} - [E(\varepsilon_{i,p=2}) - E(\varepsilon_{i,p=1})],\end{aligned} \tag{6}$$

where the terms in braces describe the different components that our effect estimates are adjusted for, in this case given for the most encompassing AB estimator, and idiosyncratic errors assumed to have zero means, that is, $\mathrm{E}(\varepsilon_{i,p=2}) = \mathrm{E}(\varepsilon_{i,p=1}) = 0$. Relative to the most encompassing adjustment described in equation (6), the FE DDD estimator will contain potential bias due to residual differences in past work commitment between mothers across policy periods (i.e., the first term in braces will become part of the error terms, e.g., if, net of observed covariates, the relationship between unobserved past levels of work commitment and fertility choices changes across policy periods in otherwise unspecified ways), the LDV DDD estimator will potentially be biased because of differences in regression to the preference mean across policy periods (i.e., if processes of preference change between interviews change across policy periods in unspecified ways, as captured by the last of the terms in braces), and the OLS DDD estimate will potentially be biased because of the omission of either factor. Stated in more substantive terms, by comparing adjusted preferences of mothers (i.e., treatment group members) across policy periods only, the DDD estimator of equation (6) effectively controls for the potential endogeneity of motherhood status in general, and the panel data (FE/LDV/AB) estimators in particular correspond to alternative regression specifications that address potential endogenous change in couples' fertility decisions across policy periods. Seen this way, any systematic discrepancies in parameter estimates from the OLS estimator on the one hand and the FE, LDV, and AB estimators on the other hand will be indicative of the presence of endogenous change in couples' fertility decisions across policy periods in Germany, and any further inconsistencies among panel data estimators will indicate more specifically the type of respective change in fertility decisions involved.

In what follows, we present empirical estimates of the effect of changes in parental leave policies on women's work commitment in West and East Germany during the 1990s and early 2000s. Our focus will be on estimating and reporting average treatment effects of policy change throughout, not the least since the available sample size prevents us from disaggregating our analysis further while retaining sufficient power to isolate effects at conventional levels of statistical precision. In addition to providing effect estimates as such, we will also explore role exposure and norm setting as potential mechanisms that generate the observed impact of policy change on women's work commitment, and we will illustrate the behavioral

implications of policy-induced changes in work commitment for mothers' labor force participation. We will provide more specifics on these supplementary analyses in the course of presenting our empirical analysis below.

EMPIRICAL RESULTS

Trends in Women's Work Commitment in Germany

Before presenting our core estimates of the role of parental leave policies in shaping women's work commitment, we first briefly describe observable trends in women's work commitment in Germany more broadly. To that end, figure 2 provides the data on women's average level of work commitment by survey year and separately for East and West Germany, using our measure of relative work orientation constructed from differencing factor scores based on the two item batteries as described before. Part *A* describes the trend in work-family preferences among all women of working age, and part *B* provides the same information for mothers of small children under age 3 only.

Both parts of figure 2 first of all confirm that well-known differences between East and West Germany in terms of women's labor market involvement, but also in terms of East-West convergence in women's labor market behavior over time (Rosenfeld et al. 2004; Hanel and Riphahn 2012), are also reflected in our measure of women's subjective work-family preferences. Measured by either item battery, work orientation has consistently been lower among West German women, yet there has also been considerable convergence of women's preferences in East and West Germany during the 1990s and up to the mid-2000s. Actually, convergence occurred as a result of change in both parts of Germany: first, work commitment declined among working-age women in East Germany between reunification and the mid-1990s and has remained largely stable since. Then, at least in item battery 1, which is available over the full 1990–2004 period, work commitment among West German women increased between the mid-1990s and 2004. We have no ready explanation for the discrepant West German trend based on our battery 2 measure, yet as battery 2 data are not spanning different policy periods for West Germany, it is also worth noting that all estimates of policy impact on West German women will exclusively rest on battery 1 data in any case.

That said, part *B* of figure 2 provides initial evidence that women's work commitment varies across the life course. Work orientation among mothers of small children is well below that of the average working-age woman in both East and West Germany (see also related evidence in Bielby and Bielby [1984] and Evertsson [2013]). At the same time, part *B* also shows that trends in work commitment have not been homogeneous

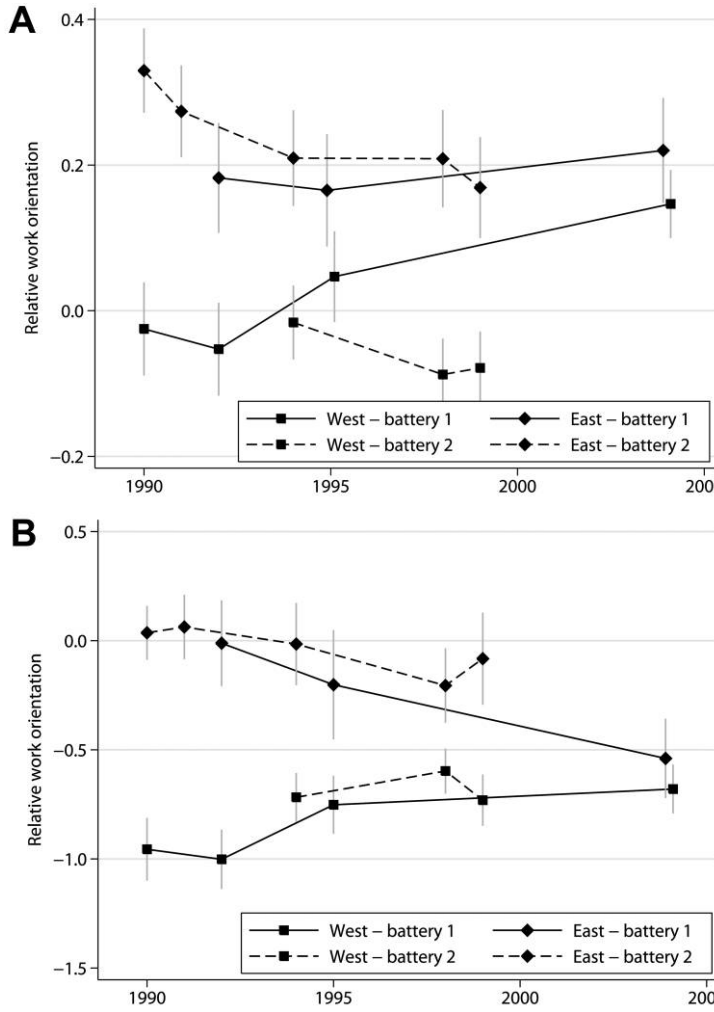

FIG. 2.—Aggregate trends in women's work-family preferences, Germany 1990–2004. Part A: all women ages 16–64; part B: mothers with children under age 3. Whiskers indicate 95% confidence intervals of the estimated means. See the discussion of data in the text for a description of item wording in batteries 1 and 2. Data are from the GSOEP, waves G–U, weighted data.

among German women during the 1990s and early 2000s. To begin with, it is true among mothers of young children, too, that work orientation has consistently been higher in East Germany but also that preferences have converged considerably between East and West Germany over time. Relative to the overall picture of part *A*, East-West differences in work

commitment among mothers of small children had been much more pronounced in the early 1990s and subsequent convergence of preferences thus considerably more dramatic.

Shortly after German reunification, the average work orientation among mothers of small children differed by a full point on our measures between East and West Germany, as compared to about 0.2 among working-age women in general.[13] Within less than 15 years since reunification, this glaring gap in mothers' work orientation has been almost fully eradicated. In 2004, we obtain an East-West difference of merely 0.14 in the average work commitment score among mothers that is not even statistically significant at conventional levels any longer; by the same token, overall East-West differences in average work commitment have declined from 0.2 to 0.07, which just barely remains statistically significant in a one-sided *t*-test of group differences and also represents a smaller relative decline of the gap between East and West Germany. Besides the sheer amount of change, it is also interesting to note obvious differences in the sources of convergence among mothers relative to women more generally. Among women in general (fig. 2, part *A*), change in preference occurred at about equal rates in East and West Germany; yet change was much more clearly confined to the immediate transition period following reunification in the East followed by more secular changes in the West starting by about the mid-1990s. With respect to mothers of small children (fig. 2, part *B*), however, it is evident that it is mainly East German mothers who have converged to the West German average. Among West German mothers, average work orientation increases from score values of −1.0 in 1992 to −0.75 in 1995 and remains at about that level also in 2004, which parallels developments among women more generally up until the mid-1990s but sees mothers falling behind the general trend of rising work orientation among women in West Germany afterward. Among East German mothers, average work orientation steadily declines from score values of −0.01 in 1992 to −0.20 by 1995 and −0.54 by 2004 on item battery 1. Moreover, to the extent that both batteries overlap, trends in battery 2 measures are entirely consistent with the findings based on the battery 1 measure.

Motherhood, Parental Leave Entitlements, and Work Commitment

The descriptive findings of figure 2 speak to significant change in women's work-family preferences during the 1990s and early 2000s in Germany.

[13] Recall that factor scores are standardized to mean zero and unity standard deviation by construction. A one-point difference on the resulting difference score can thus be interpreted as a 1-SD difference in terms of either component of the score, which in practice happens to be essentially equivalent to a one-category difference in responses to one of the (family- or work-related) raw items.

Also, the clear increase in family orientation among East German mothers provides face validity to our theoretical claim that adoption of West German family policies may have had an impact on women's preferences, over and on top of its better-documented impacts on women's labor force behavior. By the same token, however, the increasing work orientation of West German mothers at first glance runs counter to our expectations concerning the effects of increasingly generous parental leave policies. Yet it is also obvious that net trends in work commitment will result from the interplay of multiple processes, so that any adverse impact of changing parental leave policies may be masked by counteracting contemporary trends that have increased work commitment, most importantly, women's rising educational attainment, women's increasing economic independence, and the postponement of family formation. As a consequence, multivariate analysis is required in order to isolate policy effects proper.

Tables 2 and 3 contain the core regression estimates of our study. Table 2 provides key DiD parameter estimates for models with work commitment as measured by item battery 1 as their dependent variable; table 3 has complementary estimates for battery 2 as the dependent variable. Since battery 2 is useful to assess the impact of the (1992) policy change in East Germany only, no estimates for the West German sample are included. As a robustness check of the results, we systematically include OLS, FE, and LDV parameter estimates for all models and also the AB parameter estimates in the case of item battery 1 in the West German sample, the sole case in which the minimum four data points are available to construct the required instrumental variables for the AB estimator. In both tables, we omit all results for control variables to simplify the presentation, but note that most of the covariate effects show the expected sign.[14] Most importantly, German women's work orientation is found to decrease with number of children but increase nonlinearly with both children's and mothers' age. Partner earnings, marriage, and, at least among West German women, part-time employment tend to decrease work commitment, while work commitment tends to increase with labor force experience and education, in particular among West German women. No major impact is found for aggregate labor market conditions as captured by the overall female unemployment rate in respondents' state of residence. To complete the presentation, figures 3 and 4 visualize our preferred LDV DiD estimates of the changing impact of motherhood and the resulting LDV DDD estimates of the impact of policy change on women's work commitment, the main results of this study.

[14] Full estimates for control variables in our main regression specifications are available in appendix table A2.

TABLE 2

WORK COMMITMENT AND EXTENDED PARENTAL LEAVE ENTITLEMENTS, ITEM BATTERY 1: SUBJECTIVE IMPORTANCE OF WORK VERSUS FAMILY

	EAST GERMANY			WEST GERMANY			
	OLS	FE	LDV	OLS	FE	LDV	Arellano-Bond
Birth before 1989–91[a]	−.57**	NA	−.20	−.71**	NA	−.28**	NA
	(.13)		(.14)	(.09)		(.09)	
Covered birth:							
1989–91[b]	−.67**	NA	NA	−.59**	−.17	−.22**	NA
	(.21)			(.09)	(.17)	(.07)	
1992–2000	−.72**	−.66**	−.49**	−.70**	−.73**,‡	−.67**,‡	−.74
	(.11)	(.15)	(.11)	(.06)	(.09)	(.07)	(.10)**
2001–4	−1.14**,‡	−1.31**,‡	−1.22**,‡	−1.18**,‡	−1.24**,‡	−1.19**,‡	−1.18**,†
	(.16)	(.28)	(.23)	(.10)	(.20)	(.16)	(.23)
Noncovered birth:							
1989–91[b]	−.75**	NA	NA	−.49**	−.04	−.14	NA
	(.21)			(.08)	(.16)	(.09)	
1992–2000	−.66**	−.32	−.42**	−.82**,‡	−.66**,‡	−.43**,‡	−.63**
	(.10)	(.19)	(.13)	(.05)	(.13)	(.09)	(.13)
2001–4	−1.18**,‡	−1.29**,‡	−.88**	−.97**	−.78**	−.92**,†	−.96*
	(.15)	(.36)	(.26)	(.10)	(.27)	(.23)	(.38)
Time trends:							
1992	NA	NA	NA	−.08	−.09	NA	NA
				(.06)	(.06)		
1995	.13	−.16	NA	−.10	−.03	.10	.16
	(.10)	(.11)		(.34)	(.68)	(.12)	(.10)
2004	−.22	−.93**	−.39	−.29	−.27	−.04	.33
	(.10)	(.17)	(1.59)	(.44)	(.87)	(.19)	(.23)
1992 × birth before 1989[a]	NA	NA	NA	.12	.09	NA	NA
				(.07)	(.07)		
1995 × birth before 1989[a]	−.05	.22	NA	.16**	.24**	.03	−.13
	(.10)	(.12)		(.07)	(.09)	(.09)	(.08)
2004 × birth before 1989[a]	−.01	.89**	.06	−.04	.47**	−.14	−.01
	(.11)	(.20)	(.04)	(.08)	(.15)	(.10)	(.18)
Constant	.61	−.78	1.49	.04	.39	−1.08**	NA
	(.86)	(.93)	(27.4)	(.52)	(.99)	(.33)	
N	5,658	5,658	2,388	14,707	14,707	6,402	3,283

NOTE.—Cluster-corrected SEs are in parentheses. Controls: age, age squared, education, partner log earnings, number of children, age of youngest child, age of youngest child squared, state unemployment rate; additional controls in LDV/Arellano-Bond specifications: marital status, employment status, own log earnings, labor force experience (all lagged). FE specifications omit age and age squared because of multicollinearity with trend controls. Data are from the GSOEP, waves G–U.

[a] East German respondents before 1991; West German respondents before 1989.

[b] East German respondents births in 1991.

* $P < .05$ (two-sided).

** $P < .01$ (two-sided).

† Statistical significance for equality of coefficients across consecutive policy periods $P < .10$ (two-sided).

‡ Statistical significance for equality of coefficients across consecutive policy periods $P < .05$ (two-sided).

TABLE 3
WORK COMMITMENT AND EXTENDED PARENTAL LEAVE ENTITLEMENTS, ITEM BATTERY 2: WORK VERSUS FAMILY AS A SOURCE OF SATISFACTION WITH LIFE

	EAST GERMANY		
	OLS	FE	LDV
Birth before 1991	−.56** (.10)	NA	−.23 (.12)
Covered birth:			
1991	−.65** (.23)	.65 (.47)	−.14 (.23)
1992–2000	−.56** (.11)	−.33*‡ (.15)	−.45** (.11)
Noncovered birth:			
1991	−.38 (.21)	−.22 (.43)	−.09 (.25)
1992–2000	−.60** (.13)	−.40 (.26)	−.38* (.15)
Time trends:			
1994	.39 (.42)	−.09 (.47)	NA
1998	.31 (.35)	−.27 (.40)	.12 (.15)
1994 × birth before 1991	.04 (.09)	.28** (.11)	NA
1998 × birth before 1991	.01 (.09)	.48** (.18)	−.02 (.11)
Constant	.06 (.71)	−.44 (.10)	.53 (2.14)
N	5,136	5,136	2,702

NOTE.—Cluster-corrected SEs are in parentheses. Controls: age, age squared, education, partner log earnings, number of children, age of youngest child, age of youngest child squared, state unemployment rate; additional controls in LDV model: marital status, employment status, own log earnings, labor force experience (all lagged). FE specification omits age and age squared because of multicollinearity with trend controls. Data are from the GSOEP, waves H–O.

* $P < .05$ (two-sided).

** $P < .01$ (two-sided).

† Statistical significance for equality of coefficients across consecutive policy periods $P < .10$ (two-sided).

‡ Statistical significance for equality of coefficients across consecutive policy periods $P < .05$ (two-sided).

The results reported in tables 2 and 3 in fact provide consistent support for our basic hypothesis that extended parental leave entitlements have lowered women's work commitment in Germany. As evident from figure 3, relative work orientation has steadily declined among mothers across successive policy periods characterized by increasingly generous parental leave entitlements. By the early 1990s, work commitment among mothers did not strongly differ from either comparison group of (younger) women without children and (older) women with completed pre-1989 fertility history; yet increasing divergence is evident over the two subsequent policy periods: the 1992 watershed reform that introduced the three-year leave entitlement and the 2001 reform that introduced the flexibility to use parental leave up until a child's eighth birthday. This finding consistently applies to both West and East German women, and even the quantitative magnitude of change is remarkably similar despite the necessity of using two different item batteries in the East German analysis. Also, it is remarkable that strong and consis-

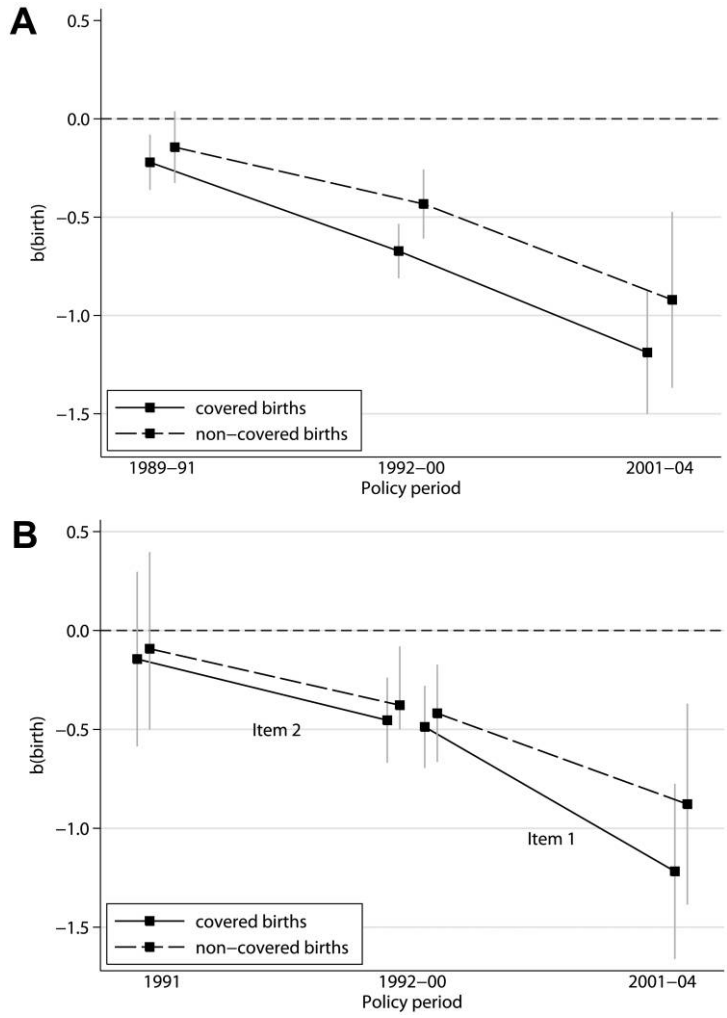

FIG. 3.—DiD estimates of the effect of motherhood on women's work commitment by policy period, LDV specification, Germany 1990–2004. Part A: West Germany; part B: East Germany. Whiskers represent 95% confidence intervals of the estimated coefficients. Results for West Germany refer to item battery 1; results for East Germany to item batteries 1 and 2 (see the discussion of data in the text for a description of item wording in batteries 1 and 2). See tables 2 and 3 for detailed regression results.

tent trends are visible among both employed mothers who are actually covered by statutory parental leave entitlements and economically inactive mothers who obviously have no entitlement to parental leave but receive flat-rate parenting benefits only. Evidently, a clear divergence in work commitment occurred during the 1990s and the early 2000s, that is, in line

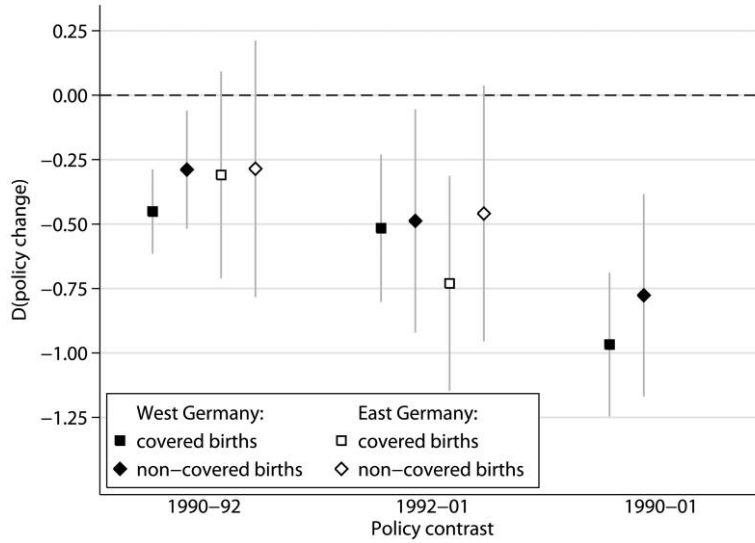

FIG. 4.—DDD estimates of the effect of parental leave entitlement changes on women's work commitment, LDV specification, Germany 1990–2004. Whiskers represent 90% confidence intervals of the estimated coefficients. Results for West Germany refer to item battery 1; results for East Germany to item batteries 1 (contrast 1992–2001) and 2 (contrast 1990–92). See tables 2 and 3 for detailed regression results.

with the timing of changes in the German parental leave program, between mothers on the one hand among whom—and in both East and West Germany, and whether being employed or economically inactive prior to childbirth—subjective work orientation decreased and both (younger) childless women and (older) mothers with completed fertility history on the other hand among whom there are no visible systematic changes in work commitment in the period in question.

Differencing the LDV DiD estimates of figure 3 across policy periods (or the corresponding parameter estimates in tables 2 and 3 for alternative estimators, of course) yields the LDV DDD estimates of the impact of policy change on women's work commitment in Germany. As has been implicit in the previous paragraph, our estimates imply that extended parental leave entitlements have led to a significant decline of work commitment among German mothers, in both East and West Germany, and among both employed and economically inactive mothers. In fact, both the 1992 and the 2001 reforms appear to have had considerable empirical impact. Our LDV estimates suggest that the 1992 reform has lowered work orientation by about 0.5 points among West German mothers covered by parental leave and still by about 0.25 points among economically inactive mothers in West Germany and both employed and

economically inactive mothers in East Germany. The 2001 reform may have had an even larger impact than that, with negative effect estimates on the order of 0.5 points among all groups of mothers except employed mothers in East Germany, among whom the 2001 reform impact is estimated to be as high as −0.75 points.[15] The magnitude of the estimated effects is indeed sufficiently large to be statistically significant even on the basis of the relatively small sample size available in the GSOEP for a differentiated analysis like ours, although confidence intervals around our estimates for the East German sample tend to be very wide, and, as evident in figure 3 for the first policy change, some estimates hence fail to reach conventional levels of statistical significance.[16] Also, our results are qualitatively and quantitatively robust across all four alternative estimators, notably if compared between the three panel data estimators (FE, LDV, and AB). OLS estimates largely agree with the latter but do not find an impact of the 1992 reform on employed mothers in either East or West Germany. As this discrepancy in parameter estimates suggests a changing relationship between unobservables and employed women's fertility decisions—specifically, higher fertility among more work-oriented women after the 1992 reform—it is evident that the panel data estimators are indeed superior to OLS in capturing the effect of the watershed 1992 reform on women's work commitment because of their ability to address endogenous change in couples' fertility decisions. Among the panel data estimators, LDV tends to yield slightly more conservative estimates of the 1992 reform than FE, whereas roles are reversed with respect to the evaluation of the 2001 policy change.

A Look into the Black Box of Policy Feedback on Work Commitment: Role Exposure or Norm Setting?

As our data suggest strong impacts of changing parental leave entitlements on mothers' work commitment in Germany, we now seek to further probe the empirical relevance of the two generative mechanisms sketched above—changes in role exposure at the microlevel and changed norm setting at the macrolevel—in the next step of our analysis. To that end, we present the results from several supplementary analyses that extend our

[15] As the confidence intervals suggest, the difference in the point estimates of treatment effects across reform periods is not statistically significant at conventional levels, however.

[16] We explicitly provide results of two-sided tests of statistical significance of parameter differences across policy periods at the level of $P < .10$ and $P < .05$ in tables 2 and 3. As our hypotheses are directed, one-sided tests of parameter differences are statistically appropriate, corresponding (at $P < .05$) to the results of the two-sided tests at a level of $P < .10$. As almost all relevant parameter differences are significant at a level of $P < .05$ in two-sided tests, our interpretation is not materially affected by the choice of threshold except where noted.

core model by incorporating specific operationalizations for the two mediating processes of role exposure and norm setting. Since both analyses imply further restrictions in terms of available samples, observation window, or required substantive assumptions, the following mediation analyses are explicitly not to be conceived as a formal decomposition of the total policy impact into its constituent sources. Rather, and more modestly, our aim is to collect supplementary evidence on the empirical relevance of the hypothesized mediating processes, which may not the least also indicate fruitful directions for further work probing the relevance of normative policy feedback in Germany and elsewhere.

Parental leave, exposure to the caregiver role, and work commitment.—To explicitly test for the relevance of role exposure, we first add the actual duration of employment interruptions as an additional covariate. As argued before, the hypothesis is that work commitment should be negatively affected by women's direct exposure to the caregiver role, and such exposure is expected as a consequence of extended parental leave entitlements as actually utilized by mothers. Table 4 provides our respective estimates for the empirical impact of the duration of employment interruptions, defined as the cumulative number of months of economic inactivity related to the care of an under-6-year-old, on women's work commitment from alternative model specifications.

More specifically, we present estimates from four types of models. First, we present estimates from overall models that fit a common duration effect in the sample, but we then also fitted a status-specific model that provides separate estimates for the duration effect among previously employed mothers covered by parental leave versus economically inactive mothers without leave entitlements. To the extent that role exposure is occurring empirically, we expect a negative duration effect on commitment in the overall model and stronger effects of the duration of employment interruptions among previously employed mothers for whom parental leave will imply the more pronounced life course transition, including novel role exposure to a nonwork environment. Second, we present both standard regression specifications that incorporate the duration of employment interruptions as a regular covariate and instrumental variable (IV) estimates that use the DDD treatment groups defined by policy period and coverage status as instruments for the duration variable. The latter specifications thus estimate the so-called *local average treatment effect* (LATE; see Angrist, Imbens, and Rubin 1996) that specifically describes the average effect of duration on work commitment among those mothers who increased the duration of work interruptions because of the change in policy. Arguably, these LATE estimates are of significant interest in our case; yet we also want to alert the reader to the fact that their interpretation requires acceptance of the exclusion restriction inherent in IV estimation, that is, the assumption that the impact of policy changes on

TABLE 4

THE IMPACT OF THE DURATION OF EMPLOYMENT INTERRUPTION ON WORK COMMITMENT

	FE	LDV	FE-IV (LATE)	LDV-IV (LATE)
Overall specification:				
West Germany, 1990–2004	−.06** (.02)	−.03** (.01)	−.36** (.05)	−.33** (.06)
			$F = 198$	$F = 14.1$
			($P < .01$)	($P < .01$)
East Germany, 1992–2004 (item battery 1)	−.10** (.05)	−.05** (.02)	−.58** (.12)	−.37** (.06)
			$F = 106$	$F = 42.5$
			($P < .01$)	($P < .01$)
East Germany, 1991–98 (item battery 2)	.01 (.07)	$7.1E^{-5}$ (.02)	−.21 (.15)	−.23* (.12)
			$F = 113$	$F = 13.6$
			($P < .01$)	($P < .001$)
Status-specific specification:				
West Germany, 1990–2004:				
Covered births	−.09** (.02)	−.05** (.01)	−.16** (.04)	−.16** (.03)
			$F = 454$	$F = 108$
			($P < .01$)	($P < .01$)
Noncovered births	.01 (.02)	−.01 (.01)	−.02 (.03)	.03 (.02)
			$F = 632$	$F = 112$
			($P < .01$)	($P < .01$)
East Germany, 1992–2004 (item battery 1):				
Covered births	−.15* (.04)	−.12** (.03)	−.25** (.07)	−.23** (.05)
			$F = 378$	$F = 32.2$
			($P < .01$)	($P < .01$)
Noncovered births	.05 (.04)	−.02 (.02)	.05 (.06)	−.08 (.05)
			$F = 380$	$F = 60.6$
			($P < .01$)	($P < .01$)
East Germany, 1991–98 (item battery 2):				
Covered births	−.01 (.05)	−.01 (.03)	−.07 (.09)	−.09 (.07)
			$F = 239$	$F = 21.3$
			($P < .01$)	($P < .01$)
Noncovered births	.02 (.05)	$2.4E^{-3}$ (.02)	−.01 (.09)	−.06 (.09)
			$F = 223$	$F = 21.2$
			($P < .01$)	($P < .01$)

NOTE.—Parameter estimates for duration of child care–related work interruption in years; cluster-corrected SEs are in parentheses. Controls: age, age squared, education, partner log earnings, number of children, age of youngest child, age of youngest child squared, state unemployment rate; additional controls in LDV model: marital status, employment status, own log earnings, labor force experience (all lagged). FE specification omits age and age squared because of multicollinearity with trend controls. IV specifications use births by policy period and parental leave entitlement status as instruments; Staiger-Stock F-statistics indicate joint significance of instruments in first-stage regressions on the instrumented variables. Data are from the GSOEP, waves G–U.

* $P < .05$ (two-sided).

** $P < .01$ (two-sided).

work commitment operates solely through the mechanism of increasing the duration of work interruptions. Strictly speaking, the exclusion restriction thus violates our hypothesis that complementary processes of norm setting may (also) be playing a role. In light of our evidence from the main analysis, we provide parameter estimates from both FE and LDV panel data models but omit standard OLS estimates.

In fact, across all differences in details of the specification, our estimates agree that the duration of work interruptions does indeed lower women's subjective work orientation. In addition, the LATE estimates obtained from either FE or LDV specifications are consistently larger than the standard estimates, often by an order of magnitude; in the case of item battery 2 used in the analyses of the East German sample for the 1990s, it is in fact only in the IV specifications that we obtain the expected positive effect, and more clearly so in the LDV specification than in the FE model. In substantive terms, this discrepancy between standard and LATE estimates suggests that it may indeed be the case that policy-driven extensions of employment interruptions are affecting women's preferences in particularly pronounced ways. In the same line of reasoning, it is telling that all our results in table 4—except for battery 2 results that do not show evidence of any duration effect at all—indicate that, as expected, role exposure effects are particularly pronounced among mothers who had been employed prior to childbirth.[17] The negative effects of work interruptions thus primarily occur among mothers covered by parental leave, for whom (extended) entitlements explicitly create (extended) exposure to the (novel) caregiver role, while there actually is no evidence of respective direct exposure effects related to caregiving activity among homemaking mothers.[18]

Policy feedback on work commitment via norm setting.—While the mechanism of role exposure relies on changes in individual life course trajectories, we argued before that preference adaptation may also follow from macrolevel processes of cultural diffusion, normative anchoring, or norm

[17] As an additional institutional twist, it should be noted that the German parental leave program creates incentives for parents to claim maximum leave length versus their employer, since only parental leave use claimed prior to entering the maternal protection period will be independent of employer discretion. While subsequent changes in leave arrangements may be negotiated individually between parents and employers, in practice this creates an additional exogeneity aspect to the present analysis since leave duration thus tends to be set (by parents) prior to the occurrence of any observable post-birth shift in preferences that we focus on here.

[18] One direct implication of this pattern of results is that our theoretically grounded reservations about the validity of the LATE estimates may have been somewhat overcautious. Despite having sketched two potential mechanisms for policy effects, our evidence suggests that role exposure operates exclusively among mothers working prior to birth. Hence, the required exclusion condition might hold at the level of the subgroups of mothers working and not working prior to giving birth, thus also justifying a reliance on the LATE parameter.

setting. In contrast to direct role exposure, norm setting involves responses to cultural signals but also learning about and adjustment to observable changes in typical behavior of relevant others, which generates social spillover and multiplier effects beyond direct role exposure. In fact, our main analysis already contained a wealth of evidence to suggest that norm setting has been an important part of policy feedback on women's work commitment in the German case. Most importantly, simple role exposure is quite inadequate to explain why similar policy impacts were consistently observed for both employed and economically inactive mothers despite significant differences in entitlements and despite the fact that direct role exposure turns out to have been empirically irrelevant among homemaking mothers. Besides, the unexpectedly strong impacts of the 2001 reform may also point to the importance of norm setting processes as the actual institutional change in terms of entitlement generosity had been rather limited.

To push the case for the presence of norm setting processes further, table 5 provides a set of estimates from a series of additional FE and LDV models that incorporate an interaction between the magnitude of policy impact and years since policy change. Here the idea is that normative policy feedback is likely to involve temporally lagged effects if social learning and multiplier effects are important, especially if norm setting occurs from observing how other mothers actually change work and care behaviors in response to new policy incentives. In that vein, the key hypothesis is that norm setting processes imply normative policy feedback to grow with increasing time since policy change, that is, the expectation of a negative interaction between the effect of a policy change on work commitment and the time since policy enactment. Unfortunately, we lack the multiple waves of data required for this type of analysis for all policy periods except the 1992–2000 period and hence are restricted to this single policy period for this particular analysis. Interestingly, however, following the 1992 watershed reform that introduced the three-year leave entitlement, we do find evidence of the expected negative interaction between time since reform and the size of treatment effects among West German mothers, whether employed or economically inactive, in table 5. Among East German mothers, too, there is at least tentative evidence of some lagged effects among economically inactive mothers; yet it seems equally clear that our relatively small samples do not provide sufficient statistical power for a more definitive answer in this case.

Importantly, however, this supplementary evidence on the importance of lagged treatment effects due to cultural processes of norm setting also helps to better understand the surprisingly large effects associated with the 2001 policy change in the main analysis. Since the main analysis reported time-constant policy effects only, it abstracted from any systematic change

TABLE 5
Extended Parental Leave Entitlements and Lagged Change in Work Commitment

	West Germany, 1990–2004 (Item Battery 1)		East Germany, 1991–98 (Item Battery 2)	
	FE	LDV	FE	LDV
Covered birth:				
1989–91[a]	−.18 (.17)	−.25** (.07)	.25 (.50)	.24 (.21)
1992–2000	−.55** (.13)	−.37** (.10)	−.26 (.34)	−.40* (.17)
1992–2000 × time since 1992 policy change	−.04 (.03)	−.08** (.02)	.02 (.07)	−.00 (.06)
2001–4	−1.25** (.20)	−1.19** (.16)	NA	NA
Noncovered birth:				
1989–91[a]	−.10 (.16)	−.16 (.09)	.56 (.53)	.42 (.34)
1992–2000	−.34* (.17)	−.25* (.13)	.44 (.39)	.24 (.37)
1992–2000 ×time since 1992 policy change	−.08* (.04)	−.04 (.03)	−.26 (.23)	−.44** (.11)
2001–4	−.65* (.28)	−.78** (.24)	NA	NA

Note.—Cluster-corrected SEs are in parentheses. Controls: age, age squared, education, partner log earnings, number of children, age of youngest child, age of youngest child squared, state unemployment rate; additional controls in LDV model: marital status, employment status, own log earnings, labor force experience (all lagged). FE specification omits age and age squared because of multicollinearity with trend controls. Data are from the GSOEP, waves G–U.

[a] East German respondents before 1991; West German respondents before 1989.

* $P < .05$ (two-sided).

** $P < .01$ (two-sided).

in the strength of normative policy feedback over time. If, however, we take the evidence of table 5 at face value, our estimates suggest that the normative impact of the 1992 watershed reform probably increased significantly over time; and if we take this lag in policy feedback into account, the treatment effect for the 2001 reform is actually no longer statistically significantly different from predicted work commitment toward the end of the pre-2001 policy period, in particular among mothers employed prior to childbirth.[19] In other words, the seemingly large effects for the small 2001 policy change in the main analysis are actually quite well explained by noting that they in all likelihood correspond to null effects once the temporal lag in the unfolding of the full effects of the 1992 reform has been adequately considered; the policy change that had empirically important

[19] More specifically, including the interaction term, the estimated treatment effect stands at some −0.75 units by the year 2000 among West German mothers with a childbirth covered by parental leave and even well exceeds the estimate from the corresponding main effect model for East German women. The only group for whom some visible difference remains are homemaking mothers in West Germany, among whom the 2001 policy change may eventually have resulted in a further decline of work commitment on the order of some −0.3 points.

feedback effects on work commitment among German mothers had in fact been the 1992 watershed reform.

Does It Matter? Parental Leave, Work Commitment, and Women's Subsequent Employment

Finally, any analysis like ours that tracks changes in individual preferences and subjective orientations is bound to incur the question whether what we observe reflects mainly shifts in individual perception and emphasis or whether the reported changes in the normative and motivational domain then also correspond to verifiable changes in respondents' actual behavior. To address such concerns, and also to underscore the substantive relevance of our findings on normative policy feedback effects associated with extended parental leave entitlements, we report on a final analysis that aims to estimate the behavioral implications of policy-induced changes in women's work commitment for women's labor force participation and employment. In the present context we should like to stress that we are not concerned with the question whether, besides normative policy feedback demonstrated before, mothers in Germany have also responded behaviorally to changes in the parental leave program; there is wide consensus in current research that they have and that the parental leave extensions of the late 1980s and the 1992 watershed reform have clearly increased mothers' time off work (see Ondrich et al. 1996; Ziefle 2009; Grunow et al. 2011; Schönberg and Ludsteck 2014; Ziefle and Gangl 2014). Rather, we are concerned specifically with the question whether we can marshal novel evidence on the issue whether that behavioral response may in part have been a consequence of preference change as described in the current analysis or whether the increasing duration of work interruptions among German mothers has to be conceived of as a purely rational response to changing economic incentives.

To that end, table 6 reports empirical estimates from a series of regression models predicting women's labor force participation, employment, and full-time employment two years after the present survey interview. We utilize future employment as our dependent variable in order to ensure the proper temporal order between cause and effect, that is, to avoid endogeneity bias due to concomitant changes in preferences and behavior, and we again resort to FE and LDV panel data specifications in order to estimate the impact of over-time preference changes on employment and labor force participation, controlling as before for a wide range of observed controls as well as unobserved time-invariant person-specific factors. Also, we again provide estimates from both standard FE

$$\mathrm{LFP}_{it+2} = \alpha_t + \Delta RW_{it} + \boldsymbol{\beta}\mathbf{X}_{it} + \gamma_{ct} + \mu_i + \varepsilon_{it} \quad (7)$$

TABLE 6
THE IMPACT OF WORK COMMITMENT ON WOMEN'S SUBSEQUENT EMPLOYMENT

	FE	LDV	FE-IV (LATE)	LDV-IV (LATE)
Labor force participation, $T + 2$:				
West Germany, 1990–2004	.01* (.00)	.02** (.00)	.17** (.04) $F = 13.7$ ($P < .001$)	.04** (.01) $F = 47.4$ ($P < .001$)
East Germany, 1992–2004 (item battery 1)	.02** (.01)	.02** (.00)	.02 (.04) $F = 9.3$ ($P < .001$)	.05* (.03) $F = 23.2$ ($P < .001$)
East Germany, 1991–98 (item battery 2)	−.00 (.01)	.01** (.01)	−.00 (.11) $F = 2.6$ ($P < .05$)	−.00 (.11) $F = 2.6$ ($P < .05$)
Employment, $T + 2$:				
West Germany, 1990–2004	.01** (.00)	.02** (.00)	.19** (.04)	.04** (.02)
East Germany, 1992–2004 (item battery 1)	.02** (.01)	.01** (.00)	.08 (.05)	.02 (.03)
East Germany, 1991–98 (item battery 2)	−.00 (.01)	.01 (.01)	.01 (.01)	.05 (.14)
Full-time employment, $T + 2$:				
West Germany, 1990–2004	.01** (.00)	.03** (.00)	.03** (.00)	.19** (.02)
East Germany, 1992–04 (item battery 1)	.01** (.01)	.02** (.00)	.23** (.06)	.13** (.04)
East Germany, 1991–98 (item battery 2)	−.00 (.01)	.02** (.01)	.01 (.01)	.21 (.16)
Full-time employment share, $T + 2$ (employed women only):				
West Germany, 1990–2004	.01 (.01)	.03** (.00)	.32** (.07) $F = 7.2$ ($P < .001$)	.24** (.03) $F = 30.4$ ($P < .001$)
East Germany, 1992–2004 (item battery 1)	−.00 (.01)	.02** (.01)	.15** (.06) $F = 7.5$ ($P < .001$)	.18** (.05) $F = 14.5$ ($P < .001$)
East Germany, 1991–98 (item battery 2)	−.00 (.01)	.01* (.01)	.01 (.01) $F = 113$ ($P < .001$)	.13 (.18) $F = 1.5$ ($P > .10$)

NOTE.—Cluster-corrected SEs are in parentheses. Controls: age, age squared, education, marital status, employment status, log earnings, labor force experience, partner log earnings, number of children, age of youngest child, age of youngest child squared, state unemployment rate. FE specification omits age and age squared because of multicollinearity with trend controls. IV specifications use births by policy period and parental leave entitlement status as instruments; Staiger-Stock F-statistics indicate joint significance of instruments in first-stage regressions on work commitment; first-stage F-tests for outcomes employment, $T + 2$, and full-time employment, $T + 2$, are equivalent to those for labor force participation at $T + 2$. Data are from the GSOEP, waves G–W.

* $P < .05$ (two-sided).

** $P < .01$ (two-sided).

and LDV estimators

$$\mathrm{LFP}_{it+2} = \beta_0 \mathrm{LFP}_{it} + \alpha_t + \Delta RW_{it} + \boldsymbol{\beta}\mathbf{X}_{it} + \gamma_{ct} + \varepsilon_{it} \tag{8}$$

and their IV extensions, since the former provide an overall estimate of the effect of work commitment on women's labor force behavior in our sample, whereas the IV specifications using the DDD treatment and control groups as instrumental variables once more result in LATE estimates of the specific employment impact of those policy-induced preference changes that occurred in the wake of the sequence of changes to the German parental leave program under study here.

Irrespective of these particulars, however, the main result from this final analysis is that we obtain consistent evidence for a clearly positive impact of women's subjective work commitment on their subsequent labor force and employment behavior, net of a broad array of unobserved and observed sociodemographic controls. Virtually all of the parameter estimates reported in table 6 have the expected positive sign, so that any policy-induced weakening of women's subjective work commitment can be predicted to also lower their labor force participation and employment rates in the medium and possibly longer run. The effect estimates are consistently largest for full-time employment rates as the dependent variable, where our results imply that a one-point difference in work commitment scores—about the magnitude of the policy feedback from the 1992 reform including lagged effects—may lower women's full-time employment rates by up to three percentage points. In general, and as before, our LATE parameter estimates tend to be consistently larger than the corresponding estimates from standard panel specifications, which suggests that the employment effects that follow specifically from normative policy feedback induced by changes to the parental leave program may have actually been rather large, potentially because mothers' labor force attachment is particularly responsive to changes in work commitment. Because of our relatively small samples, our respective LATE parameter estimates are again not very precisely estimated but nevertheless suggest that a one-unit change in the work commitment score—that is, about the total effect of the 1992 reform—might reduce labor force, employment, and full-time employment rates among women by anywhere between approximately five and 20 percentage points. Also, it should be noted that our respective evidence is somewhat less persuasive for the East German sample, probably not the least again because of its smaller sample size.

DISCUSSION AND CONCLUSIONS

The empirical evidence compiled here strongly implies that preferences indeed follow policy. Utilizing difference-in-differences estimators and unique

longitudinal survey data that offer both rich controls and repeated measures of women's work-family preferences, our various analyses have consistently implied that mothers' subjective work commitment has in fact changed in response to successive extensions of parental leave entitlements in Germany. This applies to the watershed 1992 reform in particular, which increased parental leave entitlements to three years following childbirth, and which we estimate to have led to a decline of mothers' work commitment by about one-half of a standard deviation on our indicator in the short run, and possibly up to a full standard deviation because of lagged diffusion effects within the first decade afterward. As we could also demonstrate that preference changes associated with parental leave reforms were clearly predictive of changes in women's actual labor force participation, it is probably appropriate to conclude that extended parental leave entitlements have had a significant impact on mothers' labor market involvement in Germany. Thus, the adoption of the West German system of parental leave entitlements probably contributed to East-West convergence in both levels of mothers' work commitment and, derivative to the former, mothers' labor force participation and employment. Among West German mothers, too, extended parental leave entitlements have at least contributed to considerably moderating the secular trend of mothers' increasing labor market attachment and employment. According to our results, the entitlement extension has clearly resulted in more strongly family-oriented preferences among mothers relative to both own past preferences and preferences of childless women, so that motherhood has arguably come to constitute an even more distinctive event in women's family and economic lives than before. Likewise, we could show by comparison to (older) women with completed fertility histories prior to the policy change that the changes we observe were occurring within the clearly circumscribed sample of new mothers and were not part of any more generally regressive trend of women's work commitment in Germany.

In fact, these results from our quantitative case study are certainly consistent with findings of adverse effects of long parental leave entitlements on women's employment, as, for example, reported in the cross-nationally comparative studies of Pettit and Hook (2005), Kenworthy (2008), Budig et al. (2012), and Boeckmann et al. (2015), and largely driven by the cases of Germany and other (mostly) continental European countries. Our results are hence suggestive of a potentially complex interplay between standard economic mechanisms of behavioral responses to incentives and constraints and more sociological mechanisms of preference and belief system formation in generating the overall empirical impact of family policies on women's labor force behavior. The latter interpretation seems further corroborated by our evidence on the mediating processes that underlie preference change among German mothers. The fact that we observe fairly similar declines in

work commitment among recent mothers irrespective of prebirth employment status (and, hence, actual entitlement status) and relative to both women without children and (older) women with completed fertility histories speaks to the importance of processes of norm setting that extend beyond the more circumscribed population of working women for whom leave entitlements were actually changing. Similarly, findings of a temporal lag until the full impact of the 1992 reform had been realized in our view underscore the conclusion that extended parental leave entitlements have triggered a widespread impact through norm setting among mothers in Germany during the 1990s and early 2000s. At the same time, our results also indicate that actual exposure to the caregiver role has been an important complementary mechanism of preference change at the individual level. Consistent with the behavioral foundations implied, we find that work commitment clearly declined with the duration of employment interruptions among mothers working prior to childbirth. Exposure effects have hence been sharply restricted to the population of mothers for whom actual entitlements and, in response, actual patterns of labor force participation and caregiving have changed over time. In either case, the observation that norm setting and role exposure have likely been important suggests quite pervasive and potentially also quite persistent effects of extended parental leave entitlements on mothers' work-family preferences.

With this key finding of significant preference change following the policy change in Germany, the current analysis should also serve to emphasize one of the core insights in the sociology of the welfare state, namely, that specific public policy configurations are conducive to distinct welfare regimes, that is, constitute alternative equilibrium solutions to common allocative and distributional trade-offs that characterize the division of (paid and unpaid) labor in modern societies (see also Cooke and Baxter 2010). In that sense, it should not come as a surprise that differences of or changes in core institutions are likely to result in equilibrium shifts, which involves interrelated changes in the behaviors of employers, families, and, in our specific case, women in their capacities as partners, caregivers, and workers (see also Mandel and Semyonov 2006). And even as we are far from intending any assessment of the relative role of supply- and demand-side policy impacts or from denying the likely pervasive importance of employer responses to family policy arrangements for women's employment prospects, we consider the present study as demonstrating the weight of this consideration empirically and as acknowledging the complementary impact of family policy on women's own preferences and derivative behavior. The evidence from our study to some extent hence undermines exclusively demand-side accounts of adverse effects of family policy on gender inequality but underscores that, not the least owing to the salience and universalism of public policies in modern societies, the patterns of gender inequality in the labor

market are an equilibrium outcome of both employers' and women's and their families' responses to prevailing institutional environments.

With this focus, our study is in line with other recent research that examines broader policy impacts beyond strictly economic and labor market behaviors (e.g., Hook 2010; Cooke 2011). What is distinctive about the present research, however, is that we have been able to address the relationship between welfare states and social stratification not merely at the level of employer or worker behavior but at the fundamental level of agents' preferences. Here, we have been able to demonstrate with longitudinal survey data, for the particular case of Germany, and for the specific institution of parental leave entitlements that mothers' preferences have been adapting at the individual level and in predictable ways in response to a major change in German family policy. In fact, we consider the empirical evidence from the present study a supreme vindication of a whole generation of feminist research that characterized welfare regimes as incorporating, embedding, and reinforcing models of care and gender relations (see Lewis 1992; Orloff 1996; Sainsbury 1996, 1999; Pfau-Effinger 2004; Kremer 2007; Cooke 2011). Moreover, while having been limited to preference effects among women in the present analysis, our theoretical framework is sufficiently general in order to inform future studies of respective normative effects among men, too, who are beginning to assume a more important caregiving role in several European countries, not the least through the adoption of dedicated "daddy months" within public parental leave programs in Scandinavia but also, more recently, Germany. In a broader sense, the evidence from our present study may thus contribute to the discipline's research program on the relationships between welfare states and the moral economy of modern societies (Mau 2003, 2004; Svallfors 2006, 2007, 2010).

Nevertheless, the strength and immediacy of policy effects visible in our study may still come as a surprise as current theory and empirical research, feminist and mainstream alike, have focused on the convergence of individual attitudes, norms and preferences, and welfare institutions in equilibrium, that is, when compared across entrenched welfare systems that have coalesced over decades if not generations (Svallfors 1997, 2006, 2007, 2010; Andress and Heien 2001; Mau 2003, 2004). In contrast, we have been able to demonstrate that extended parental leave entitlements did create significant and swift responses in mothers' work commitment in Germany during the 1990s and early 2000s. To some extent, these striking findings may naturally reflect particular features of the specific case. Extended parental leave entitlements had been an extremely popular policy at the time, parental leave institutionally complemented existing family policies that supported traditional male breadwinner arrangements, and the watershed 1992 reform effectively doubled prior entitlements by introducing a three-year parental leave, all of which may have resulted in

families being particularly responsive to this specific policy change. Still, it seems important to emphasize the empirical observation that this policy change did not merely trigger behavioral responses in terms of women's labor force participation but also involved changes at the more fundamental level of individual preferences, in this case, a decline in mothers' subjective work commitment. This evidence of strong and fairly sudden preference shifts in response to policy change thus provides support for a broader recognition of the welfare state as a normative anchor, but also for the importance of social multiplier effects, framing, and norm setting for any observable policy impact, whether behavioral or attitudinal.

APPENDIX

TABLE A1
DESCRIPTIVE STATISTICS ON THE ESTIMATION SAMPLES, BY REGION AND TYPE OF ESTIMATOR

	East Germany				West Germany	
	Battery 1		Battery 2		Battery 1	
	OLS/FE	LDV	OLS/FE	LDV	OLS/FE	LDV
Dependent variable:						
Work orientation (differenced factor score)	.15	−.03	.23	.18	−.10	−.19
	(1.55)	(1.47)	(1.30)	(1.27)	(1.73)	(1.69)
Treatment groups:						
Birth before 1989–91[a]	.68	.76	.72	.77	.53	.59
Covered birth:						
1989–91[b]	.01	. . .	.01	.01	.03	.05
1992–2000	.04	.06	.03	.04	.06	.09
2001–4	.02	.02	. . .	. . .	.02	.01
Noncovered birth:						
1989–91[b]	.01	. . .	.01	.01	.03	.04
1992–2000	.05	.05	.03	.03	.10	.06
2001–4	.01	.01	. . .	. . .	.02	.01
Women without children	.24	.18	.24	.18	.33	.28
Current covariates:						
No. of children:						
2 children	.36	.40	.36	.39	.30	.33
3 or more children	.14	.15	.14	.15	.16	.17
Age of youngest child	12.23	13.99	11.63	13.20	10.26	11.09
	(11.53)	(11.23)	(11.13)	(11.07)	(11.42)	(11.47)
Age	39.62	42.38	39.13	41.07	39.95	41.22
	(12.64)	(11.24)	(12.40)	(11.46)	(12.72)	(11.89)
Level of education (years)	12.18	12.35	12.03	12.25	11.54	11.46
	(2.27)	(2.23)	(2.21)	(2.20)	(2.52)	(2.36)
Partnered	.757	.813	.781	.814	.743	.764
Partner annual earnings (log)	3.69	3.74	6.10	6.43	3.54	4.51
	(4.72)	(4.78)	(4.68)	(4.67)	(4.87)	(5.12)

TABLE A1 (*Continued*)

	East Germany				West Germany	
	Battery 1		Battery 2		Battery 1	
	OLS/FE	LDV	OLS/FE	LDV	OLS/FE	LDV
Partner annual earnings (log, positive earnings cases only)	9.67	9.79	9.64	9.77	10.21	10.29
	(.65)	(.71)	(.67)	(.71)	(.63)	(.61)
State unemployment rate	24.63	24.12	22.81	26.28	9.63	9.42
	(4.47)	(4.00)	(5.79)	(4.16)	(2.83)	(2.66)
Regional mobility[c]	.10	.09	.06	.061	.004	.000
Lagged covariates:						
Employed	. . .	.63	. . .	.66	. . .	.59
Labor force experience	. . .	16.07	. . .	16.71	. . .	11.97
		(10.79)		(11.14)		(9.68)
Annual earnings (log)	. . .	7.30	. . .	7.52	. . .	6.48
		(3.82)		(3.55)		(4.29)
Married	. . .	.70	. . .	.72	. . .	.65
Partnered but unmarried	. . .	.10	. . .	.09	. . .	.09
Partner annual earnings (log)	. . .	6.88	. . .	6.86	. . .	6.57
		(4.42)		(4.34)		(4.92)
State unemployment rate	. . .	25.00	. . .	21.88	. . .	7.49
		(4.28)		(6.11)		(2.06)
Survey year:						
1990	. . .	. . .	. . .	. . .	.19	. . .
1991	. . .	. . .	.34	. . .	. . .	. . .
1992	.31	. . .	. . .	. . .	.19	.38
1994	. . .	. . .	.33	.51	. . .	. . .
1995	.30	.59	. . .	. . .	.21	.36
1998	. . .	. . .	.33	.49	. . .	. . .
2004	.39	.42	. . .	. . .	.40	.26
N observations	5,658	2,388	5,136	2,702	14,707	6,402
N individuals	3,276	1,576	2,408	1,620	8,220	3,119

NOTES.—SDs of metric covariates are in parentheses. Data are from the GSOEP, waves G–U.

[a] East German respondents: before 1991; West German respondents: before 1989.

[b] East German respondents: births in 1991.

[c] Region of current residence differing from region of origin (current residence in West Germany for the East German sample and vice versa).

TABLE A2

PARAMETER ESTIMATES FOR THE EFFECTS OF CONTROL VARIABLES ON RESPONDENTS' WORK-FAMILY PREFERENCES IN GERMANY, MAIN ANALYSIS REGRESSION SPECIFICATIONS (TABLE 2)

	EAST GERMANY			WEST GERMANY		
	OLS	FE	LDV	OLS	FE	LDV
Lagged dependent variable:			.39**			.45**
			(.03)			(.02)
× Wave 1995			...			.04
						(.03)
× Wave 2004			.06			.12**
			(.04)			(.03)
Current covariates:						
No. of children:						
2 children	−.23**	.19	−.12	−.27**	.28**	−.04
	(.06)	(.20)	(.07)	(.04)	(.11)	(.05)
3 or more children	−.18*	.48	−.19*	−.40**	.85**	−.08
	(.08)	(.37)	(.09)	(.06)	(.18)	(.06)
Age of youngest child	.01	.03	−.00	−.03**	.03*	−.02**
	(.01)	(.02)	(.013)	(.01)	(.01)	(.01)
Age of youngest child squared	$8.7E^{-5}$	.02**	$2.8E^{-5}$	.00**	−.00**	$4.1E^{-4}$*
	$(2.7E^{-4})$	$(3.0E^{-4})$	$(3.3E^{-4})$	$(1.7E^{-4})$	$(2.3E^{-4})$	$(2.0E^{-4})$
Respondent age	.02	...	.09**	.05**	...	.09**
	(.02)		(.03)	(.01)		(.02)
Respondent age squared	−.00**	...	−.00**	−.00**	...	−.00**
	$(2.0E^{-4})$		$(3.3E^{-4})$	$(1.3E^{-4})$		$(1.8E^{-4})$
Level of education (years)	−.00	.07*	.00	.02**	−.01	.01
	(.01)	(.03)	(.01)	(.01)	(.02)	(.01)
Partner annual earnings (log)	−.06**	−.03**	−.03**	−.07**	−.03**	−.04**
	(.01)	(.01)	(.01)	(.00)	(.01)	(.01)
Regional mobility	.48	.14	−1.01	−.44	.53	...
	(.46)	(.47)	(15.60)	(1.19)	(2.30)	
State unemployment rate	.03	.02	−.07	.02	−.03	−.02
	(.03)	(.03)	(1.11)	(.08)	(.15)	(.02)
Lagged covariates:						
Own employment	...	...	−.04	...	...	−.13*
			(.09)			(.06)
Labor force experience	...	...	.00	...	...	.01**
			(.01)			(.00)
Own annual earnings (log)	...	...	−.00	...	...	.02**
			(.01)			(.01)
Married	...	...	−.31*	...	...	−.44**
			(.13)			(.07)

TABLE A2 (*Continued*)

	East Germany			West Germany		
	OLS	FE	LDV	OLS	FE	LDV
Partnered but unmarried	...	...	−.09	...	...	−.22**
			(.14)			(.08)
Partner annual earnings (log)	...	...	−.00	...	...	.01
			(.01)			(.01)
State unemployment rate	...	...	−.02	...	...	.01
			(.01)			(.02)
N observations	5,658	5,658	2,388	14,707	14,707	6,402
N individuals	3,276	3,276	1,576	8,220	8,220	3,119

NOTE.—The dependent variable is women's work commitment measured by item battery 1 (subjective importance of work vs. family). Cluster-corrected SEs are in parentheses. FE specifications omit age and age squared because of multicollinearity with period trend indicator variables. See table 2 for full parameter estimates for treatment and control group indicators. Data are from the GSOEP, waves G–U.

* $P < .05$ (two-sided).

** $P < .01$ (two-sided).

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