Article

The Autocratic Welfare State: Old-Age Pensions, Credible Commitments, and Regime Survival

Comparative Political Studies
2018, Vol. 51(5) 659–695

DOI: 10.1177/0010414017710265
journals.sagepub.com/home/cps
SAGE

Carl Henrik Knutsen[1] and Magnus Rasmussen[2]

Abstract

In this article, we argue that autocratic regimes are no less likely than democracies to adopt old-age pensions, although autocratic programs are less universal in their coverage. Our theoretical argument focuses on the strong incentives that autocratic regimes have for enacting and maintaining such programs to ensure regime survival. Autocratic pension programs can be considered club goods that (a) are targeted to critical supporting groups and (b) solve credible commitment problems on promises of future distribution, thereby mitigating probability of regime breakdown. We test three implications from the argument, drawing on a novel dataset on welfare state programs and including 140 countries with time series from the 1880s. First, we find that autocracies are no less likely than democracies to have old-age pension programs. But, second, autocracies have less universal pension programs than democracies. Third, pension programs effectively reduce the probability of autocratic breakdown.

Keywords

autocratic regimes, welfare state, democratization and regime change, political regimes, public pension systems

[1]University of Oslo, Norway
[2]Institute for Social Research, Oslo, Norway

Corresponding Authors:
Carl Henrik Knutsen, Professor, Department of Political Science, University of Oslo, Moltke Moes vei 31, Oslo 0851, Norway.
Email: c.h.knutsen@stv.uio.no

Magnus Rasmussen, Senior researcher, Institute for social research, Munthes gate 31, 0260 Oslo, Norway.
Email: rasmussen.magnus@gmail.com

Introduction

The welfare state literature has considered social policy programs either as mechanisms for providing individuals with insurance against risks such as job loss or illness, *or* as tools for progressive redistribution (see Moene & Wallerstein, 2001). Hence, different theoretical models are well suited to explain the existence of social policy programs *in democracies*; most voters are risk averse (Chetty, 2006), thus preferring even costly insurance, *and* the (relatively poor) median voter often has incentives to vote for parties promising income redistribution (Meltzer & Richard, 1981). Yet, also autocracies adopt social policy programs. The perhaps most famous examples are the sickness, accident, and old-age pension programs adopted in Bismarck's Germany, but these are not exceptions. Mares and Carnes (2009) count that the vast majority of the 38 "non-industrialized" (p. 97) countries for which they have data adopted their first old-age pension system under autocracy. Analyzing more than 140 countries with time series from the 1880s, we show that autocracies are *no less likely than* democracies to adopt or have old-age pension systems. But, why would autocrats adopt such programs and spend resources on pensions rather than keeping tax revenues for private consumption, or for investment in repressive capacity to ensure regime survival?

We propose that old-age pension programs in autocracies are, indeed, used for political-survival purposes, and our statistical tests estimate a clear and sizable effect of pension programs on regime survival once adjusting for the endogenous adoption of such programs. Social policies, and notably old-age pensions, involve targeted distribution of resources to particular groups, and thus constitute a special form of co-optation. While formalized social policy programs have received comparatively little attention in the literature on co-optation in autocracies, which often focuses on discretionary distribution and patronage networks, this notion is not new. Mares and Carnes (2009), for instance, argue that social policies are used for targeted distribution to critical groups. However, neither Mares and Carnes nor the wider literature have elaborated on a second key feature of such programs, namely, how they allow autocrats to make *credible promises* of future distribution. We thus respond to the theoretical challenge raised by Acemoglu and Robinson (2006); why would any group trust a dictator's promise of future distribution (see also, for example, Svolik, 2012)?

We conceptualize pensions in autocracies as *club goods*, and make a theoretical contribution to the welfare state *and* autocratic politics literature by formulating a novel argument highlighting that such programs combine two characteristics—(a) *intertemporal credibility* and (b) *potential for targeting*—which make them instrumental for autocratic survival. First, in contrast

with discretionary distribution of private goods, old-age pensions are fairly transparent, stable, and predictable, and not easy to reverse without substantial costs for rulers. Hence, distribution through these programs—not unlike particular institutions such as ruling parties or legislatures (Boix & Svolik, 2013; Gehlbach & Keefer, 2011, 2012; Magaloni, 2008; Svolik, 2012)—can mitigate credible commitment problems. Second, in contrast with pure public goods, pensions can be targeted toward identifiable groups. Hence, autocrats can design programs channeling resources (only) to groups they need support from to stay in power ("critical supporters") rather than spending precious resources on irrelevant groups. Combining these insights, we clarify how autocrats' incentives to secure long-term support from critical groups is key to understanding how and why welfare states develop and, conversely, how the adoption and design of pensions contribute to explain why some autocracies endure longer than others. By doing so, we bridge the welfare state literature, which has mainly focused on (developed) democratic contexts, and the rapidly growing literature on autocratic politics.

The argument generates different empirical implications, which we test using our new Social Policies Around the World (SPAW) dataset with global coverage and extensive time series. Leveraging these data, thereby going beyond the usual postwar Organisation for Economic Cooperation and Development (OECD) country focus of most welfare state studies, we make several empirical contributions: First, we provide more stringent and comprehensive tests of relationships between regime type and different features of social policies. Using data from countries across the world from the 1880s, we corroborate the finding on developed Western countries from Kim (2007) that democracies provide more universal pension benefits than autocracies. We also provide more systematic corroborating evidence for the claim by Mares and Carnes (2009) that autocracies are as likely as democracies to implement old-age pensions. Second, we introduce the first explicit large-*n* tests on how social policy programs affect regime survival, thereby informing the autocratic politics literature, which has mainly focused on structural conditions, political institutions, or discretionary policies as determinants of regime survival. Finally, we contribute to the welfare state literature by highlighting the unique role played by old-age pensions, showing that such programs clearly enhance autocratic regime survival, while other transfer schemes do not. The estimated effect of pensions, which holds in models accounting for the implementation of pensions being endogenous to the security threats facing the regime, is robust and substantial in size. This suggests that future research can benefit from studying the historical development and effects of pensions separately from other programs.

After having reviewed relevant literature, we present our argument ("Why Would Autocrats Implement Pension Programs" section). We thereafter describe the relevant variables from the SPAW dataset. In the empirical analysis section, we report evidence that autocracies are as likely as democracies to adopt and have pension systems, although autocracies—as theoretically anticipated, due to autocracies typically having narrower coalitions of critical supporters than democracies (see Bueno de Mesquita, Smith, Siverson, & Morrow, 2003)—have less universal systems. Finally, we report and discuss tests showing that pension systems reduce the probability of autocratic regime breakdown.

Literature

Most welfare state researchers have focused on OECD democracies (Esping-Andersen, 1990; E. Huber & Stephens, 2001; Iversen, 2005). Different theoretical accounts then also suggest that social policies—and the provision of public services such as basic healthcare and primary education (e.g., Harding & Stasavage, 2014; Lake & Baum, 2001; Stasavage, 2005)—should be more widespread, better financed, and have broader coverage in democracies than in autocracies (Acemoglu & Robinson, 2006; Ansell, 2010; Boix, 2003; Haggard & Kaufman, 2008). There is evidence that Western countries expanded welfare spending due to franchise extensions from the 19th century onward (Lindert, 2005), and Ansell and Samuels (2014) identify a positive effect of democracy on various social spending measures, even when controlling for country-fixed effects.

Yet, other empirical studies employing alternative specifications report that democracies are not, on net, associated with higher social spending (e.g., Mulligan, Sala-i-Martin, & Gill, 2003).[1] A historical pioneer in implementing welfare policy programs was Imperial Germany, under the strategic guidance of Chancellor Bismarck (see Rimlinger, 1971). Former Communist dictatorships presided over extensive social policy programs (Milanovic, 1998), but so did autocracies such as Partido Revolucionario Institucional (PRI)-Mexico (Magaloni, 2006). Desai, Olosgård, and Yousef (2009) find that—among autocracies—those with harsher repression provide more welfare spending.

Different extant studies provide two pertinent insights about autocratic politics, which we combine in our argument on why autocratic regimes often implement targeted pension systems: First, policies with distributional consequences are intentionally designed by many autocrats to remain in power. Second, ensuring that promises that are made to the regime's critical supporters are credible is of utmost importance.

Regarding the first insight, some autocrats might be genuinely concerned with the welfare of their citizens (Wintrobe, 1998, pp. 95-96) and provide, for instance, old-age pensions to improve their quality of life. However, the literature often considers policymaking as a strategic tool used mainly for enhancing regime survival, and numerous studies suggest that autocratic regimes rely on particular, critical groups for their political survival, prioritizing economic policies that satisfy these groups. Bueno de Mesquita et al. (2003) theorize that whereas large coalition rulers find it cost-effective to spend resources on public goods, small coalition rulers should allocate private goods directly to their few vital supporters. Democracies, due to larger coalitions, should thus observe more spending on public goods than dictatorships (see also, for example, Lake & Baum, 2001; Stasavage, 2005). Regarding social policies, more specifically, regime survival was widely considered a core motivation for Bismark's reforms (Rimlinger, 1971), and Mares and Carnes (2009) and Haggard and Kaufman (2008) outline how social policies have been targeted to key groups, at least in part, for political-survival reasons in various autocracies.

Regarding the second insight, Acemoglu and Robinson (2006) highlight how autocratic elites often *cannot* credibly commit to future redistribution, even when faced with revolutionary threats; they would expectedly stop redistributing (i.e., "wasting resources") once the threat is removed. The dictator may ensure survival through repression, but this is costly (Wintrobe, 1998), and co-optation is often preferable (see Gandhi, 2008). While autocrats face difficulties in making their promises of future distribution credible, this is not impossible. Different studies (Boix & Svolik, 2013; Gandhi, 2008; Geddes, 1999; Gehlbach & Keefer, 2011, 2012; Magaloni, 2006; North & Weingast, 1989; Svolik, 2012; Wright, 2008 for democracies, see Iversen, 2005) point out that autocrats can reduce credible commitment problems through establishing institutions, such as regime parties or legislatures, that allow for monitoring and constraining the actions of the ruler. We propose that also the *design of distributive policies* may matter; formalized social policy programs—particularly pensions—can reduce credible commitment problems.

Why Would Autocrats Implement Pension Programs?

Democratic governments are widely assumed to have strong incentives to implement social policy programs to please broad groups of voters, either as redistribution to relatively poor voters or as insurance to risk-averse voters against various risks. Despite this, we argue that it remains theoretically indeterminate whether or not democratic governments are more likely (than

autocratic) to adopt certain social policy programs, especially pensions. While previous contributions have highlighted that such indeterminacy may stem from adopting a more realistic understanding of how democracies work (see Ansell & Samuels, 2014), this also follows from a more realistic understanding of autocratic politics.

To summarize, the core of our argument is that autocratic regimes often have strong incentives to implement pensions to please groups of "critical supporters"—even if these groups can be fairly few in numbers. By critical supporters we mean individuals who support the regime *and*, if they were to retract their support, would substantially increase the probability of the regime ending.[2] Institutionalizing the distribution of resources through pensions makes *future* distribution to particular supporting groups more credible than promises of discretionary private-goods distribution. When distributive policy is clearly codified, the autocrat cannot, for reasons we detail below, easily backtrack on promises once the relevant group no longer poses an imminent threat. Implementing generous pension programs does imply unwelcome future costs. Yet, this is something the autocrat willingly accepts if the alternative is losing power; when promises of future distribution are considered credible, the benefiting groups will more likely continue to support the regime. Program benefits in autocracies should, however, be *targeted* toward the specific groups that the regime *needs* support from—be it industrial workers or soldiers—whereas "irrelevant" citizens should receive little. By appeasing groups that might otherwise contest the regime, the introduction of such programs should bolster autocratic regime survival.

Old-age pensions should be a particularly suitable policy for ensuring regime survival. First, the amount of resources channeled through pensions often dwarfs the amount channeled through other programs, such as unemployment benefits (to illustrate, OECD's Social Expenditure data show that old-age pensions expenditures, for 22 OECD countries with data across recent decades, are, on average, more than 5 times larger than unemployment benefits expenditures). Second, pensions can be designed to target a variety of groups; they are co-optation devices that can channel resources to a more eclectic pool of potential critical supporters than, for example, maternity leave. Pensions are not restricted to pregiven groups, such as women of childbearing age, who may or may not be relevant for political survival. Unemployment benefits advantage those with high risks of losing their jobs, and autocracies have typically focused on ensuring the job safety of critical supporting groups through employment protection and other means (e.g., Mares & Carnes, 2009). Disability insurance might come in handy for the autocrat's critical supporters, but the prospect of a disabilitating injury is relatively small compared with that of growing old. Furthermore, old-age

pensions speak directly to the temporal dimension of the credible commitment problem (we elaborate on other features of pensions that are pertinent to this problem below). For most concerned individuals, pensions will be paid out in the not-too-near future—that is, when current coalition members are retired from their positions, and thus perhaps less capable of holding the autocrat accountable. Absent old-age pension schemes, current coalition members may not trust dictators to provide future resources. Below, we explore the argument and its assumptions in more detail.

Pensions and Targeting

First, we leave repression aside and consider co-optation through current and (promised) future distribution of resources as the key type of policy tool that the regime can employ to retain power.[3] Furthermore, we simplify by considering the population in dictatorships—in addition to the dictator and his or her very close collaborators, representing the regime—as divided into two groups, namely the regime's "critical supporters" and "other citizens." We assume that the regime actors are mainly concerned with regime survival, and as the regime only requires the approval of critical supporters to retain power, it will only be concerned with pleasing them. The group of critical supporters may, depending on the context, consist of, for example, soldiers, land owners, civil servants, former army veterans, salaried professionals, business owners, or industrial workers. One key aspect for our purposes, however, is that members of this group are identifiable according to some observable features (e.g., "Participated in war of independence" or "Works in the manufacturing sector").

Critical supporters, we assume, care about present and future consumption, and thus require resources distributed to them today *and* credible promises of resources tomorrow. If not, they may seek to replace the regime with another regime more willing to comply with their demands, or even pursue democratization, which carries some benefits also for economic and other elites (Ansell & Samuels, 2014). The regime can basically co-opt the group of critical supporters through three strategies: (a) lump-sum provision of private goods to each critical supporter, (b) public-goods provision benefiting both critical supporters and other citizens, or (c) provision of club goods to the group of critical supporters. Club goods are nonrivalrous; their consumption has externalities, and club goods thus affect a group of individuals. But, in contrast to public goods, they are excludable. Pension checks are posted to individuals and consumed by them privately—distribution through pensions therefore has some private-good characteristics—but there are good reasons to consider *targeted pension programs as club goods*.

One key aspect of social policies is their insurance function (Moene & Wallerstein, 2001). Individuals cannot perfectly predict whether they will be incapable of working when they turn 70, and many would sacrifice some *current* income to insure against low income levels accompanying old age. Payments should, importantly, be implemented under clearly specified conditions and in a rule-following manner. If the government could simply decide on a case-by-case basis which members should (not) receive payments, this would undermine the program's general credibility and reduce expected utility for all program members. Hence, the *insurance aspects of welfare programs have nonrivalrous properties*. Regarding excludability, this requires the presence of observable, codifiable criteria, such as occupation, for distinguishing program members from nonmembers. The excludability aspect of pensions, combined with the regime's incentives, indicates that regimes will aim to target programs (only) toward their critical supporters. This follows the general logic of Bueno de Mesquita et al. (2003), in that regimes with smaller "winning coalitions" (i.e., fewer critical supporters) prefer funneling resources to these select supporters—leaving them with larger individual rewards and greater potential income loss if the regime breaks down and they find themselves outside the new regime's coalition. This should, in turn, boost the "loyalty" of critical supporters, helping the regime solidify power. Thus, autocracies should have less universal pension programs than (typically larger coalition) democracies, and these targeted programs should increase the longevity of autocratic regimes.[4]

Different historical examples illustrate how targeted policies can be used to co-opt critical supporters. Haggard and Kaufman (2008) describe how social policies in various East European, Asian, and Latin American countries have been targeted to critical groups: In Thailand and South Korea, for example, social insurance was limited "to core constituents in the state itself: the military and civil servants" (p. 140). In Fascist Italy, Mussolini's support base notably included the independent farmers of Northern Italy, specifically in the Po Valley. When entering office, Mussolini immediately extended benefits to his coalition, with self-employed farmers becoming, for the first time, eligible for pensions with special conditions (International Labor Organization, 1922). Argentinean history provides another example: Perón's ascension to power in 1946 was quickly accompanied by social policy actively being used to garner support (Epstein, 1979). Pensions were immediately extended to Perón's core supporters, unionized industrial workers. Later, Perón used special programs to secure support from various strategically important groups that could potentially threaten his position (Mesa-Lago, 1978); at least 12 special programs operated in the early 1950s. When Perón fell in a military coup, so did the foundations for this complex system.

With its power base secure, the Argentine military regime removed existing special pension programs in 1967, instead setting up a unified program. Three notable exceptions were, however, made for the police, civil servants, and the military. These groups kept their special programs, presumably illustrating the importance of tailoring pensions to target groups critical for regime survival.

Pensions and Credible Commitments

Thus, autocratic regimes often have strong incentives to underprovide truly public goods; these are only "cost-effective" in ensuring political survival when the critical supporting groups are very large, which they typically are in democracies but not in autocracies (Bueno de Mesquita et al., 2003). But why would autocratic regimes establish club goods—notably in the form of targeted pensions—for the group of critical supporters collectively rather than pursue discretionary distribution of private goods to such supporters individually? The latter does, indeed, have some benefits from the regime's viewpoint. First, if the regime, at some point, more strongly consolidates power, it can easily reduce the amount of private goods distributed, leaving more resources for, for example, the autocrat's personal consumption. Formalized pension programs are, as discussed below, more difficult to adjust at will. Thus, dictators who consider themselves very safe in power might prioritize private-goods distribution. Second, discretionary distribution of private goods is arguably *easier* to direct exclusively to critical supporters. In practice, pension and other social programs may lead to "leakage" or "waste"; some citizens who are unimportant for the dictator's survival may receive benefits. For instance, former servicemen with little political influence are covered by veteran benefits, or "irrelevant" parts of the manufacturing sector producing in the periphery are covered by pensions for manufacturing workers, even if these are mainly intended for powerful firms and workers in the capital.

Yet, autocratic regimes, particularly when their grip on power is insecure, may prefer programs targeting critical groups over private-goods distribution. One reason is the same stability of such programs that make them monetarily more costly. *The regime wants to avoid making critical supporters uncertain about future distribution*; such uncertainty may induce the latter to attempt replacing the regime. Acemoglu and Robinson (2006), among others, highlight the fundamental problems of time consistency and credibility of promises facing autocrats. They propose that relinquishing decision-making powers through political liberalization is the only viable solution for autocrats facing imminent threats. Still, recent contributions have highlighted how particular *institutions* in autocracies can alleviate this problem (Boix &

Svolik, 2013; Svolik, 2012). We further propose that the way in which distributive *policies* are designed influences whether promises of future distribution are credible. If the future reversal of policies entail high economic or political costs, autocrats can more credibly promise their continuation. Thus, autocrats may want to "tie their hands" when designing policies that channel resources to critical groups. One way of doing this is through codifying pension programs. Iversen (2005) observes that time-inconsistency problems can arise also for social policies, and describes how democracies—especially under PR electoral rules—may resolve them through building programmatic parties and coalition governments. While (many) autocracies do not leverage these options, there are other reasons why autocrats reduce issues of time inconsistency and non-credible promises through pension programs.

Implementing pensions should stabilize distributive policy because of three features: (a) clear rules for payments; (b) frequent payments at regularized intervals, allowing actors to quickly observe rule violations; and, (c) substantial sunk costs with setting up pension programs. All three features suggest that pension programs allow for more credible promises than distribution through discretionary spending, whereas the second and third, to some extent, differentiate pensions from other social programs.

The first two features are decisive for supporters' ability to monitor and identify whether the regime reneges on promises. A comprehensive game-theoretical and applied literature on repeated games and decentralized cooperation has emphasized that clear yardsticks and frequent interaction are key elements in upholding cooperation; noncooperative behavior will be swiftly punished with high probability, increasing costs of noncooperation and thus making promises of future cooperation credible (e.g., Keohane, 1984). Discontinuation of pension payments or reversal of programs constitutes identifiable events that could function as signal or "sparks" (Kuran, 1989), alleviating antiregime collective action. Older supporters will be immediately informed if cooperation is breached after payments are supposed to begin (date of retirement), and discontinued benefits for current pensioners from the same social group are an easily observable signal to younger supporters of the noncredibility of the autocrat's pledges. Compared with other programs, supporters do not have to wait for a specific event (e.g., a large economic crisis bringing about mass unemployment, relevant for unemployment benefits) to observe whether the autocrat is sticking to his or her side of the bargain. Regarding the third feature, pensions are associated with substantial sunk costs, as the regime initially needs to invest in administrative and monitoring capacity, including the hiring and training of officials, buildings, information and management systems for collecting contributions from firm payrolls, and so on. This reduces (the relative) costs of continuing the

program, and ensures more effective distribution of resources once the system is up and running, creating lock-in effects (Pierson, 2000). Furthermore, while words are cheap, spending resources on setting up a targeted pension program provides a more costly, and thus credible, signal that the autocrat will channel resources to benefiting groups also in the future. Such sunk costs are presumably lower for the—in most observed contexts—fiscally smaller programs, such as maternity or work injury programs, and much smaller for discretionary spending.

While autocrats *can* roll back pensions or reduce payments over time (e.g., by not adjusting for inflation), pensions should thus provide fairly strong insurances to recipients of future distribution, thereby reducing coalition incentives to remove the regime. Indicatively, Ponticelli and Voth (2011), using European data from 1919 to 2008, find that cuts in public spending generate unrest and antiregime collective action. Our SPAW data also show that pension programs are durable and seldom retracted or scaled down substantially. Only 6% of country-year observations experience changes in terms of expanding or reducing the number of groups covered by pension programs, and in autocracies, only 10% of changes are reductions. Moreover, such reductions in pensions are far less likely in established autocracies than immediately after regime change when autocrats may cut programs benefiting supporters of the old regime (see also Albertus & Menaldo, 2012). The relative frequency of pension reductions is almost 3 times higher in the first 3 years after regime change than in subsequent years. Withdrawing pension benefits from its critical supporters will likely endanger the regime. Anticipating this, critical supporters perceive promises of future resource distribution as credible if linked to pensions, particularly if the regime is expected to be long lived. Hence, critical supporters have incentives to support the regime's hold on power once a program is in place.

Summary and Hypotheses

In sum, pensions are particularly effective credible commitment devices for autocratic regimes because of their sheer size and large sunk costs, and because payments are frequent and come at regularized intervals. Pension schemes are also co-optation devices for which the pool of beneficiaries can be adjusted to cover different types of critical supporters (not only, for example, women of childbearing age or workers facing great risks of unemployment). Hence, resources can be allocated directly to groups exhibiting the financial and organizational resources to challenge the regime, or groups from which the regime needs consent or even active support to fend off coup attempts—the most common cause of autocratic regime death (Svolik, 2012).[5] Another pertinent

feature of old-age pensions is that prospective recipients will be paid out after several years—that is, when current coalition members are retired, and thus less capable of holding the autocrat accountable. The capacity to physically partake in revolts may decline with age, and supporters lose their relevance as economic producers and disengage from collective-action conducive organizations such as unions once they retire. Absent old-age pension schemes, currently influential "coalition members" may not trust dictators to provide them with future resources.

Our argument suggests (at least) three distinct hypotheses: First, even if the literature contains good arguments for why democratic governments should adopt social programs to please voters, autocratic regimes might be as likely as democracies to adopt such programs. While vote-maximizing democratic politicians have strong incentives to provide (various) social policy programs, autocrats should have particularly strong incentives to provide old-age pension programs. Thus, our first hypothesis is as follows:

Hypothesis 1 (H1): Autocracies are no less likely than democracies to adopt/have old-age pensions.

We reiterate that our argument predicts weaker incentives for autocratic regimes to adopt other social policy programs, and we thus consider it more likely that democracy correlates positively with adoption/existence of such programs.[6] Nonetheless, our argument also implies that the pension programs pursued in autocracies should typically be more targeted than in democracies, as autocracies have narrower supporting coalitions. This yields the following hypothesis:

Hypothesis 2 (H2): Old-age pensions are less universal in autocracies than in democracies.

Factors influencing the risk of regime breakdown might incentivize autocrats to initiate (or expand) pensions. However, forward-looking autocrats may implement such programs as safety nets long before the economy crashes or protesters assemble in the streets (when it may be too late to save the situation). Nonetheless, the notion that such programs—once in place—boost autocratic survival prospects is another testable proposition following our argument. Hence, our final hypothesis is as follows:

Hypothesis 3 (H3): Old-age pension systems reduce the risk of autocratic regime breakdown.

The SPAW Dataset

Our SPAW dataset covers all sovereign countries in the world with available sources, and some variables extend back into the 19th century. There are two main categories of variables: *legislative changes* and *program characteristics*. For both categories, SPAW contains information on sickness, maternity, unemployment, disability/work injury, family allowances, and old-age pensions. SPAW draws on various sources, notably the International Labor Organization (ILO) Legislative series (1919-) and U.S. Labor Department Social Security Programs Throughout the World (SSPTW) reports (1937-).[7] Much effort has gone into using multiple sources—both cross-country reports and country-specific sources—to cross-check validity and reliability. While this introduces an element of subjective judgment even for de jure measures, this is preferable to employing a single source, both for increasing coverage and for avoiding specific issues associated with the individual sources. Extant studies of social policies largely rely on aggregated spending measures. While interesting for some purposes, they are often poor proxies for welfare-state characteristics of proposed causal relevance (see, for example, Esping-Andersen, 1990; Scruggs, 2006). We aim to capture whether benefits are targeted to *specific groups*. Overall spending measures are ill suited for this purpose. Measures of the share of the population insured can also be problematic for our purpose, as insurance statistics will not distinguish countries where, for example, only one small group, say industrial workers, is insured, from another where benefits accrue to all workers but a long required contribution period make only some eligible. To capture how autocrats use targeted benefits to placate particular groups, we draw on the following variables from SPAW.

First, we use measures on the first year of *major* reform and presence of (above-minimum threshold) programs. Contrary to previous attempts to measure first year of legislation (e.g., Mares & Carnes, 2009), we distinguish between major and special programs (see Online Appendix for closer discussion). Several countries legislated special *public servant pensions* during the 19th century, way before introducing old-age pension programs in their modern form. In Argentina, the first civil servant pension (originally for the judiciary and treasury) was enacted in 1803, whereas the first law covering industrial workers passed in 1946 (Mesa-Lago, 1978). Previous attempts to measure "first laws" have often used SSPTW reports, but these lack consistent criteria over time (or between countries) on what constitutes a first law—depending on the edition, SSPTW reports switch between using special or major programs. Thus, we have drawn information from the entire archive of SSPTW reports alongside other sources, and our main source is the ILO Legislative series, which we deem as the most reliable.

More specifically, we use dichotomous measures of whether (national-level) pension (and other social policy) programs are adopted/exist or not, and the operational "minimum threshold" is that the program covers ≥ 1 of the following eight major social/occupational groups: agricultural workers, industrial/production workers, small-firm workers, self-employed, students, employers, temporary/casual workers, and family/domestic workers.[8] This categorization does not perfectly capture all groups of theoretical interest. Pertinent groups, such as public servants and military officers, are not explicitly included, and in many contexts only (occupational or geographical) subsets of, for example, "employers" or "industrial/production workers" will be relevant. Unfortunately, collecting such comparable data across countries and time would, at best, be extremely labor intensive. Hence, empirical estimates on links between, for example, autocracy and old-age pension program adoption may be downward biased, as programs targeted only to narrower groups are not counted. Nonetheless, theoretical and historical case studies on social groups and regime change emphasize the roles played by broader groups corresponding well with the SPAW categories, highlighting coalitions of, for example, industrial workers or middle-class groups such as self-employed or business owners (e.g., Ansell & Samuels, 2014; Collier, 1999; Rueschemeyer, Stephens, & Stephens, 1992). Insofar as the categories represent coalitions of actors relevant for regime survival, SPAW provides decent measures for testing our argument.

Second, we draw on *Universalim Indices* from SPAW, capturing what parts of the population are included on equal terms in the same program.[9] The original indices range from 0 to 9: 0 designates when programs are absent and 1 when programs are means tested based on some *property* criteria—income-based exclusions are *not* counted as means-tested programs. Furthermore, contribution- or employment-based programs are scored as 1 + number of groups covered (among the seven groups that we code; if all groups are covered, the score is $1+7=8$). Maximum 9 scores indicate that all residents are automatically entitled to benefits—a fully universal system.[10] When testing H2 on regime types and how widely targeted pension programs are, we omit observations without programs (0) or with only means-tested programs (1) to obtain a cleaner and theoretically more appropriate measure.[11] Online Appendix Figure A1 shows the baseline Universalism Index distributions for the six major programs. Although this index is not of interval scale, we employ easy-to-interpret linear regression models as baselines, and ordered logit/probit models or logit/probit models run on dummy variables (using different cutoffs) for robustness checks.

While the SPAW data allow us to capture theoretically important characteristics for an extensive sample, there are potential reliability and validity issues. First, coding de jure programs allows for an extensive dataset with

comparable measures across time and space, but some programs may be "empty shells." Hence, our data could include pension programs that exist on paper only. While conducting systematic tests on de facto program characteristics would have been ideal, these are very difficult to code in a valid manner. Furthermore, *if* many programs are simply empty shells, they would expectedly not fulfill the above-theorized roles and have a clear causal effect on regime survival. Hence, our strong results below may actually underestimate the "true effect" of pension programs on regime survival. Likewise, de facto dismantling of existing programs—which happens (see Mares & Carnes, 2009, p. 94), despite the presumably strong incentives of leaders to maintain them—should attenuate our estimates. Second, we do not differentiate groups according to size. In principle, one could weight scores by group size, but sufficiently disaggregated information for coding the sizes of groups comparably across space and time is not available. We therefore use non-weighted measures. Third, the Universalism Indices do not make qualitative distinctions between the particular groups covered. Expectedly, some major groups—such as domestic workers—are constituencies that autocrats are unlikely to grant benefits, whereas other—such as industrial workers—are critical supporting groups in many contexts due to superior power resources and organizational capacity. In democracies, leaders may need to cater to both groups to be reelected, and autocracies should thus have less universal systems than democracies. Our results, which indicate higher universalism among democracies, would not reflect our argument well if pension systems were first enacted covering only domestic or family workers (while excluding, for example, industrial workers). This is, however, not the case in practice. During our coding, we found no instances of coverage being extended to domestic/family workers without urban manufacturing/industrial workers also receiving benefits.[12] While imperfect, the measures used thus capture our theoretical concepts fairly well.

Empirical Analysis

We organize the empirical tests in three subsections: First, we test H1 on regimes and adoption of pensions. We then test H2 on how targeted or universal pensions are under different regimes. Finally, we test H3 by investigating how programs affect autocratic regime breakdown.

Does Regime Type Matter for Probability of Adopting Pensions?

We argued that old-age pensions can be effectively targeted toward groups capable of affecting the survival of autocratic regimes. But, as outlined in the introduction, democratic politicians also have strong incentives to adopt

pensions and other social policy programs. Hence, while democracies and autocracies may adopt pensions for different reasons, we do not expect any clear and systematic regime differences in the probabilities of adopting or having such programs.

To test H1, we mainly use the dichotomous Boix, Miller, and Rosato (2013) regime measure (henceforth BMR). BMR distinguishes between democracies and autocracies based on existence of free and fair elections *and* whether more than half of the male population can vote.[13] The dependent variable in Models A1 to A3, Table 1, is a dummy recording whether an old-age pension system (covering ≥ 1 major occupational/social group) exists, whereas Models A4 to A6 have adoption of pension system as dependent variable. BMR and existence of pension system correlate by .26 (9,166 observations from Model A1); democracies more often have pension systems than autocracies, although the upper-left graphic of Figure 1 shows that the difference has dwindled in more recent decades.

Nevertheless, this correlation ignores other factors affecting adoption of pensions. Pensions became increasingly common during the 20th century, although this expansion occurred earlier in some regions than others. As democratization has clustered in temporal waves, differently timed across regions (Huntington, 1991), we always include year dummies and (eight) region dummies. We further include log GDP per capita; richer countries have more resources to spend on pensions, and income correlates with democracy. Online Appendix Table A1 provides specifics on operationalization and sources used for the control variables. Our baselines are logit models with errors clustered on country to account for panel-specific autocorrelation.[14]

In Model A1, our most parsimonious specification accounting for income as well as region- and year-specific effects, the correlation between democracy and pension system vanishes. BMR is positive, but $t = 0.3$. Model A2 adds two potentially relevant demographic controls, an (time-invariant) ethnic fractionalization index—a fractionalized society may make it more difficult, or less desirable, for leaders to implement pensions (see, for example, Alesina, Baquir, & Easterly, 1999)—and log population size. Although the latter control variable is statistically significant—larger countries more often have pensions, possibly due to the increased complexity of running distributive schemes through patronage networks in larger countries, necessitating formal programs—BMR does not change much. Model A3 further adds urbanization, which correlates with regime type and pensions, and two variables capturing available resources for, and alternative strategies of, co-optation (and repression). These are revenues from oil, gas, coal, and metals as percentage of GDP and military personnel as percentage of the population. BMR turns negative in A3, but remains insignificant ($t = -0.03$). These null

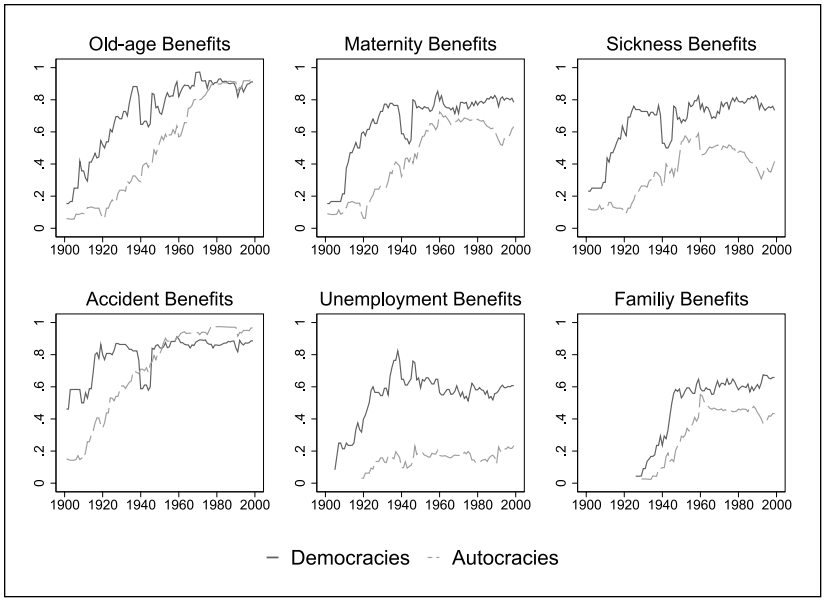

Figure 1. Shares of democracies and autocracies, as operationalized by Boix, Miller, and Rosato (2013), with programs in six social policy areas during the 20th century.

results are robust to adding other potentially relevant controls, such as civil war, British ex-colony status, literacy rates, or dependence on agriculture (Online Appendix Table A3). We also control for trade to account for stronger incentives to adopt social policies in open economies (e.g., Mares, 2005), and results are robust. Results are also retained when lagging all independent variables, substituting BMR with the continuous Polity index, controlling for many autocracies being Communist regimes, and running the models on post-1945 samples only (see Online Appendix).[15]

Models A4 to A6 resemble A1 to A3 but only include countries without pensions in $t-1$ and have introduction of pensions in t as dependent variable. These models show that *autocracies are no less likely to adopt pensions than democracies*; while BMR is always positive, *t* values range from 1.1 to 1.3. Thus, there is quite robust evidence that democracy is not systematically associated with having or adopting old-age pensions. This corroborates H1.

These results do not, however, imply that the typical "Autocratic Welfare State" is identical to the democratic. Indeed, our argument provided indications on how they may differ, for instance, suggesting that autocracies might have relatively weak incentives to adopt other types of social policy

Table 1. Democracies, Autocracies, and Existence or Adoption of Pensions.

	A1	A2	A3	A4	A5	A6
	DV: Existence pension program			DV: Adoption pension program		
	b/(t)	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*
Democracy (BMR)	0.128	0.095	−0.010	0.353	0.437	0.350
	(0.32)	(0.24)	(−0.03)	(1.11)	(1.32)	(1.09)
Ln GDP p.c.	0.498*	0.693***	−0.123	0.098	0.190	−0.178
	(1.75)	(2.59)	(−0.42)	(0.62)	(1.03)	(−0.59)
Ln population		0.445***	0.424**		0.305**	0.317**
		(2.85)	(2.41)		(2.55)	(2.47)
Ethnic fractionalization		−0.615	−1.040		−0.273	−0.842
		(−0.62)	(−1.09)		(−0.46)	(−1.27)
Urbanization			0.063***			0.029***
			(3.94)			(2.70)
Military size			−0.010			0.045
			(−0.05)			(0.51)
Resource dependence			0.001			0.004
			(0.06)			(0.25)
Region dummies	Y	Y	Y	Y	Y	Y
Year dummies	Y	Y	Y	Y	Y	Y
n	9,166	8,330	7,881	1,504	1,344	1,252
Countries	140	137	137	114	111	107

Logit regressions with errors clustered on country. Existence of minimum old-age pension system is the dependent variable in A1–A3 and adoption in A4–A6 (here, countries already having systems are omitted). Year dummies, region dummies, and constant are omitted from the table. Maximum time series extends from late 1880s to 2004. BMR = Boix, Miller, and Rosato; DV = dependent variable.
*$p < .10$. **$p < .05$. ***$p < .01$.

programs than pensions, as these are not equally effective as tools of political survival. Models B1 to B5 in Table 2 resemble A3 from Table 1 but employ, respectively, the existence of (above-minimum threshold) programs for unemployment, maternity leave, family allowance, work injury, and sickness leave benefits as dependent variables. BMR is positive and significant at 5% for unemployment (B1), work injury (B4), and sickness leave benefits (B5). These policies are presumably less easy (than pensions) to use for directing resources toward autocratic winning coalitions. However, democracy is *not* systematically related to maternity leave or family allowance programs. These findings are not predicted by our theoretical argument (but they do not directly contradict it either). We speculate that the results could partly stem from the active pronatalist policies—with the intended purpose of increasing childbirths and expanding the population (for evidence that autocracies have

Table 2. Democracies, Autocracies, and the Existence of Various Welfare Programs.

	B1	B2	B3	B4	B5
	Unemployment	Maternity	Family allowance	Work injury	Sickness
Program	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*
Democracy (BMR)	1.002**	0.272	0.289	1.036***	0.650**
	(2.45)	(0.91)	(0.82)	(2.79)	(2.50)
Ln GDP p.c.	1.184**	−0.209	0.272	−0.446	0.002
	(2.45)	(−0.58)	(0.75)	(−0.91)	(0.00)
Ln population	0.746***	0.279	0.092	0.002	0.560**
	(3.62)	(1.26)	(0.42)	(0.01)	(1.97)
Ethnic fractionalization	−1.676	−1.052	−0.787	−0.396	−2.390**
	(−1.25)	(−1.03)	(−0.74)	(−0.29)	(−2.12)
Urbanization	0.015	0.024**	0.039***	0.004	−0.005
	(0.83)	(2.05)	(2.84)	(0.18)	(−0.41)
Size military	0.260	0.106	−0.303	0.347	0.020
	(1.38)	(0.48)	(−1.42)	(0.78)	(0.11)
Resource dependence	−0.011	−0.004	−0.049***	0.011	0.009
	(−0.33)	(−0.33)	(−2.58)	(0.47)	(0.55)
Region dummies	Y	Y	Y	Y	Y
Year dummies	Y	Y	Y	Y	Y
n	7,039	7,825	6,724	7,765	7,894
Countries	130	136	136	135	136

Logit regressions with errors clustered on country, with existence of program above-minimum standard as dependent variables. BMR = Boix, Miller, and Rosato.
*$p < .10$. **$p < .05$. ***$p < .01$.

systematically higher fertility and population growth rates, see Przeworski, Alvarez, Cheibub, & Limongi, 2000, chapter 5)—pursued in many regimes to prepare for war (see Obinger & Petersen, 2017). Thus, we conjecture that while our argument focusing on "internal security threats" to the regime may explain the pension findings, a logic centering on "external security threats" might account for the maternity leave and family allowances results.

Do Autocracies Adopt Less Universal Pensions Than Democracies?

Despite similar propensities for democracies and autocracies to have pensions, our argument also indicates that pensions should be targeted on fewer

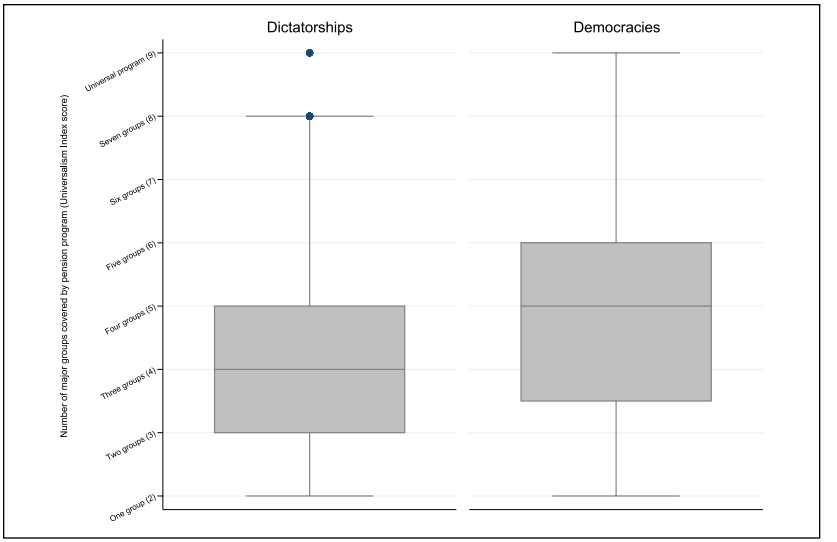

Figure 2. Box plots displaying the distribution of old-age pension programs according to the Universalism Index (omitting observations without programs or with only means-tested programs) for autocratic and democratic regime observations.

groups in autocracies. Indeed, the mean Universalism Index score for pensions is 3.4 for democracies and 1.5 for autocracies. In post-World War II (WWII) samples, the respective numbers are 4.4 and 3.0. As noted, we omit observations without programs (0 score) and with means-tested programs (1 score) from our analysis. Figure 2 shows that also among the remaining observations, democracies typically have more universal programs, with the median democratic program covering four groups and the median autocratic covering three. Yet, income level, or country-specific historical factors, could affect both regime type and pension features. We thus run different models, controlling for year- and country-specific effects to test H2. We use linear models for our baseline specifications, but ordinal logit and probit specifications and logit and probit models using dummies (based on various cutoffs for the Universalism Index) give similar results.

Models C1 to C2, Table 3, report standard fixed effects specifications with errors clustered on country. C1 controls for income level, and the point estimate indicates that going from autocracy to democracy increases the pensions Universalism Index with 0.3 (t = 2.00), a modest increase.

Table 3. Democracies, Autocracies, and Universalism of Pensions.

	C1	C2	C3	C4	C5	C6
	Fixed effects	Fixed effects	ECM	ECM	System GMM	System GMM
	b/(*t*)	*b*/(*t*)	*b*/(*t*)	*b*/(*t*)	*b*/(*t*)	*b*/(*t*)
Democracy (BMR)	0.320**	0.281	0.168**	0.162**	0.259***	0.213**
	(2.00)	(1.59)	(2.52)	(2.21)	(2.67)	(2.43)
ΔDemocracy			0.043	0.044		
			(1.11)	(1.06)		
Ln GDP p.c.	0.203	0.266	0.151**	0.148**	0.379***	0.383**
	(1.02)	(1.33)	(2.01)	(2.01)	(3.03)	(2.37)
ΔLn GDP p.c.			0.009	0.075		
			(0.08)	(0.66)		
Ln population		0.310		−0.058		−0.016
		(0.80)		(−0.39)		(−0.14)
ΔLn population				0.219		
				(0.52)		
Urbanization		0.022		0.008		0.000
		(1.50)		(1.41)		(0.04)
ΔUrbanization				0.000		
				(0.02)		
Size of military		0.087		0.073*		0.035
		(0.91)		(1.85)		(0.67)
ΔSize military				0.014		
				(0.34)		
Resource dependence		−0.006*		−0.001		−0.002
		(−1.77)		(−0.80)		(−0.74)
ΔResource dependence				−0.002		
				(−1.05)		
Lagged dependent variable			−0.349***	−0.352***	0.260***	0.276***
			(−8.58)	(−8.46)	(9.18)	(8.55)
Year dummies	Y	Y	Y	Y	Y	Y
n	5,064	4,678	5,006	4,583	4,910	4,543
Countries	132	131	132	131	132	131

Universalism Index for pensions is a dependent variable (observations with nonexistent program or means-tested program are excluded). (Nondifferenced) independent variables in ECMs are lagged by 1 year. Errors are clustered on country. Maximum time series extends from late 1880 to 2004. ECM = error correction model; GMM = Generalized Method of Moments; BMR = Boix, Miller, and Rosato.
*$p < .10$. **$p < .05$. ***$p < .01$.

Random-effects specifications give similar results (see Online Appendix). However, BMR loses significance in C2 ($t = 1.59$), which adds population, urbanization, military size, and resource dependence as controls. The somewhat weaker result in C2 mostly comes from the lower sample size. Upon rerunning C1 on the 4,678 observations in C2, the t value drops from 2.00 to 1.72. Thus, rejecting H2 based on Model C2 is premature, and we probe further specifications.

To investigate whether democracy predicts not only levels of but also subsequent changes in universalism, we run error correction models (ECMs). In addition to country-fixed effects, these models control for the lagged dependent variable and (1-year lagged) levels as well as first differences (from $t-1$ to t) for all independent variables. Hence, C3 and C4 investigate whether levels and/or changes in regime type affect changes in universalism from $t-1$ to t. Following standard interpretation of ECMs, a significant lagged-level coefficient suggests a long-term effect, and a significant first-difference coefficient suggests a short-term effect. The ECMs show that only the lagged-level coefficient is clearly separable from 0 ($t = 2.52$ for C3; $t = 2.21$ for C4), suggesting a long-term effect. We also test the system GMM estimator from Blundell and Bond (1998), which is particularly suitable for analyzing slow-moving variables such as regime type and pensions. As for ECM, system GMM models include lagged dependent variables. Both the parsimonious (C5; $t = 2.67$) and extensive (C6; $t = 2.43$) GMM models find a positive democracy coefficient.[16]

While not entirely conclusive, the weight of the evidence thus suggests that autocracies target their pensions on fewer social groups than democracies do. The Online Appendix displays results on other social policy areas. We find that democracies have more universal unemployment benefits programs than autocracies but results are mostly insignificant for other programs. In sum, there is evidence that democracies pursue more universal welfare policies but only in the realms of unemployment benefits and—as anticipated by our argument—old-age pensions.

Do Pensions Reduce Risks of Autocratic Regime Breakdown?

We test H3, which states that pension programs should reduce the risk of autocratic regime breakdown, by employing data from Geddes, Wright, and Frantz (2014). They employ a regime definition emphasizing formal and informal rules for selecting policies and leaders, and their measure of regime failure captures changes from autocracy to democracy, between different autocracy types, and even between regimes of similar type (e.g., between the Personalist Mobutu and Kabila regimes in Zaire/Congo). Initially, we employ

a naive assumption and consider pension programs as exogenous—despite the evidence in Table 1 showing that they are endogenous to several factors. We run logit and probit models with regime failure as the dependent variable, controlling for ethnic fractionalization, GDP per capita, population, military size, and resource dependence. We also include the Geddes et al. regime dummies, as autocracy types are widely considered to differ in regime durability (Geddes, 1999; Hadenius & Teorell, 2007). We include log regime duration in some models; older regimes are generally at lower risk of being deposed (e.g., Svolik, 2012). Yet, tenure length may partly stem from existence of pension programs. Hence, including regime duration may induce posttreatment bias, and we also report models without this control.

These models are reported in Online Appendix Table A16. Given the plausible argument on how pension programs bolster autocratic regimes, the lack of a statistically significant association between pension programs and regime failure might, at first glance, be surprising. Is there no effect on autocratic survival, after all? Such a conclusion would, however, not only be premature but incongruent with the argument that H3 is drawn from. We expected autocrats to implement (expensive) pension programs where the regime perceives its future survival is threatened. In such situations, the regime may initiate pensions to shore up coalition support or co-opt new groups. Programs should thus tend to exist in contexts where probability of regime survival is, *initially*, low. Even if pension programs have a positive causal effect on regime survival, pension programs and regime failure may display 0 correlation.[17] Figure 3 shows that the bivariate correlation between pension program adoption and regime breakdown is close to 0 when measured contemporaneously. Yet, the correlation is typically positive when regime failure is measured before, and negative when measured more than 5 years after pensions are introduced. Also logit and probit regressions including controls display a similar pattern. When pensions are measured 3 to 5 years before breakdown, they predict lower chances of breakdown, and results are often (weakly) significant (Online Appendix Table A17). Furthermore, past regime failure—signaling a more unstable political climate—sometimes predicts a higher likelihood of adopting pension programs (Online Appendix Table A18).

Hence, both our theoretical argument and these preliminary findings suggest that we should model the "selection" of pensions and the effect of these programs on regime survival simultaneously. To this end, we employ Instrumental Variables Probit (IVProbit) models. As this model can be fairly sensitive to specification choices, we test different combinations of controls and instruments, and also run (more robust) 2SLS models. To produce consistent estimates of the causal effect, we must identify *instruments* that are

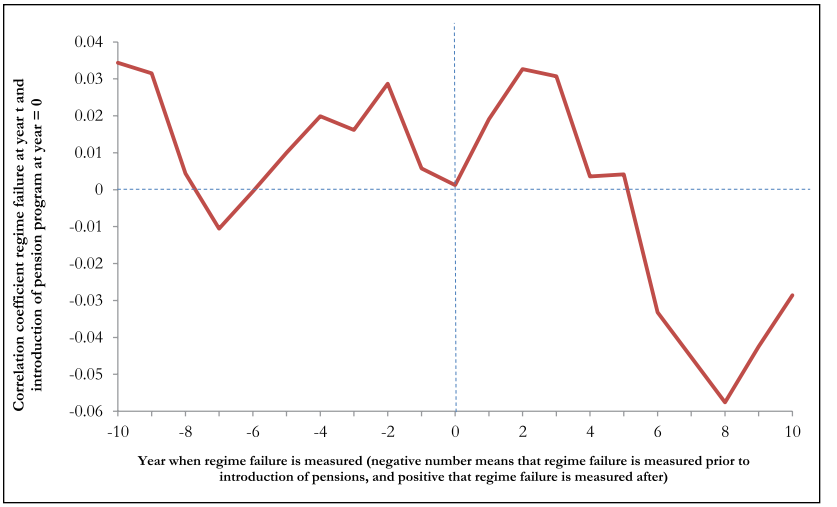

Figure 3. Bivariate correlation coefficients between autocratic regime failure measured at year *t* and introduction of old-age pensions in year 0.
Correlation coefficients are displayed as 3-year moving averages centered on year *t*.

strongly correlated with pensions *and* not directly related to regime failure. Drawing on an insight from the literature on institutions and economic outcomes (Acemoglu, Naidu, Restrepo, & Robinson, 2014; J. D. Huber, Ogorzalek, & Gore, 2012; Knutsen, 2011; Persson & Tabellini, 2003), we construct instruments tapping exogenous regional (and global) variation in the endogenous independent variable.

The underlying notion—backed up by literature on cross-border learning and other diffusion mechanisms for social policy design (e.g., Obinger, Schmitt, & Starke, 2012)—is that regimes residing in neighborhoods where most countries have pensions are more likely to adopt and continue such programs, everything else equal. While some cross-country variation in adoption, as argued, stems from perceived regime threats and strategic calculation by the regime, this is not the only source of variation. We aim at isolating the (exogenous) share of variation not affected by strategic adoption; Weyland (2005, 2007), for instance, highlights how various cognitive biases may make leaders more likely to adopt regionally "common models" of such policies, inducing cross-border "learning." Furthermore, once conditioning on covariates (with one caveat related to neighboring regime breakdowns discussed below), neighboring country pension programs should not directly affect regime failure. The exclusion restriction is conditional on the controls

included, such as time-trend/year-fixed effects, region-/country-fixed effects, and regime age. Hence, our instrument—percentage share of other countries in the region (as defined by Miller, 2015) having pensions in that year—is expectedly strong *and*—while this can never be fully ensured—might satisfy the exclusion restriction. In some models, we add share of (other) countries *globally* with pension programs to pick up exogenous global trends. While the latter instrument turns out much weaker than the former, adding it allows for running standard overidentification tests on the exclusion restriction.

Table 4 reports second-stage results from 12 IV specifications. D1 and D2 (adding log regime duration to D1) only use the "region-share" instrument. This instrument is, quite naturally, highly collinear with not only the region dummies but also the year dummies. D1 and D2 omit these dummies but include a linear time trend. Both models report a clear, negative causal effect (significant 5%) of having pension programs on regime failure. Furthermore, the region-share t value in the first stage (Online Appendix Table A19) is high (> 5), and the Kleibergen–Paap F values are around 15, suggesting a moderately strong instrument.[18]

Yet, including only one instrument disallows overidentification tests for checking the exclusion restriction. Thus, we add the "global-share" instrument in D3 and D4. This instrument turns out much weaker than the regional, and the Kleibergen–Paap F values drop to around 8, suggesting a modest (downward) weak instrument bias.[19] Nonetheless, the effect of pensions is retained, and the Hansen J test p values (around .4) indicate that the exclusion restriction holds.

However, one could still, on theoretical grounds, expect the exclusion restriction to be violated *if* pensions regionally and globally correlate with incidences of regime failure regionally and globally, *and* regime failures abroad directly affect the risk of regime failure domestically. This could, for example, stem from the following causal pathway:

$$\text{Pensions regionally} \rightarrow \text{Regional instability} \rightarrow$$
$$\text{Domestic instability} \rightarrow \text{Domestic pensions.}$$

This pathway assumes that the regime-stabilizing effect of pensions operates in neighboring countries, and that there are spillover effects on domestic regime stability from regime collapses in neighboring countries, for example, because opposition actors are spurred to collective action or learn effective strategies from watching events in neighboring countries. If so, the exclusion restriction is violated in the previously discussed models, as there is a direct link between the instrument and dependent variable. To account for this causal pathway potentially affecting results, we control for shares of (other) countries

Table 4. Second-Stage IV Regressions With Autocratic Regime Failure as Dependent Variable and Existence of Old-Age Pensions as Endogenous Independent Variable.

	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10	D11	D12
	IVProbit	IVProbit	IVProbit	IVProbit	IVProbit	IVProbit	IVProbit	IVProbit	IVProbit	IVProbit	FE2SLS	FE2SLS
Model	*b*/(*t*)	*b*/(*t*)	*b*/(*t*)	*b*/(*t*)	*b*/(*t*)	*b*/(*t*)	*b*/(*t*)	*b*/(*t*)	*b*/(*t*)	*b*/(*t*)	*b*/(*t*)	*b*/(*t*)
Pension program	−0.770**	−0.867**	−0.735**	−0.831**	−0.720**	−0.809**	−2.481***	−2.551***	−3.119***	−3.141***	−0.196*	−0.207*
	(−2.32)	(−2.33)	(−2.18)	(−2.19)	(−2.11)	(−2.13)	(−4.50)	(−4.74)	(−14.37)	(−16.21)	(−1.81)	(−1.79)
Military regime	0.643***	0.724***	0.650***	0.731***	0.656***	0.730***	0.182	0.251	−0.190	−0.147	0.077**	0.101***
	(5.90)	(5.66)	(6.10)	(5.82)	(6.24)	(5.94)	(0.68)	(0.84)	(−0.63)	(−0.44)	(2.27)	(2.81)
Monarchy	−0.288	−0.374*	−0.275	−0.360*	−0.256	−0.335	−0.427	−0.501*	−0.475*	−0.520**	0.075**	0.032
	(−1.43)	(−1.71)	(−1.39)	(−1.67)	(−1.30)	(−1.57)	(−1.64)	(−1.90)	(−1.94)	(−2.11)	(2.37)	(0.81)
Personalist regime	0.397***	0.444***	0.398***	0.445***	0.411***	0.454***	0.148	0.185	0.004	0.025	0.032	0.043
	(4.43)	(4.39)	(4.46)	(4.42)	(4.55)	(4.47)	(0.80)	(0.91)	(0.02)	(0.11)	(1.19)	(1.38)
Other autocracy	0.474*	0.389	0.479*	0.395	0.468*	0.393	0.130	0.057	−0.147	−0.203	0.132	0.136
	(1.74)	(1.25)	(1.75)	(1.27)	(1.72)	(1.28)	(0.47)	(0.20)	(−0.50)	(−0.70)	(0.73)	(0.73)
Ln GDP p.c.	−0.077	−0.093	−0.079	−0.094	−0.083	−0.097	−0.055	−0.065	−0.000	−0.005	−0.036*	−0.044**
	(−1.04)	(−1.16)	(−1.08)	(−1.20)	(−1.12)	(−1.22)	(−0.49)	(−0.55)	(−0.00)	(−0.04)	(−1.89)	(−2.09)
Ln population	−0.034	−0.044	−0.035	−0.045	−0.028	−0.037	0.152**	0.145*	0.222***	0.216***	−0.051	−0.041
	(−1.03)	(−1.25)	(−1.08)	(−1.30)	(−0.85)	(−1.07)	(1.98)	(1.86)	(3.31)	(3.19)	(−0.93)	(−0.69)
Ethnic fractionalization	−0.086	−0.069	−0.090	−0.073	−0.096	−0.080	0.169	0.181	0.190	0.194		
	(−0.53)	(−0.40)	(−0.56)	(−0.42)	(−0.60)	(−0.47)	(0.58)	(0.60)	(0.55)	(0.56)		
Urbanization	0.007*	0.008*	0.007*	0.008*	0.007*	0.008*	0.010*	0.010*	0.013**	0.013**	0.002**	0.002**
	(1.85)	(1.91)	(1.80)	(1.86)	(1.75)	(1.80)	(1.78)	(1.81)	(2.25)	(2.25)	(2.30)	(2.24)
Size of military	−0.133*	−0.145*	−0.134*	−0.145*	−0.113	−0.124	−0.011	−0.020	0.008	0.003	−0.026***	−0.028***
	(−1.78)	(−1.86)	(−1.78)	(−1.86)	(−1.50)	(−1.59)	(−0.13)	(−0.24)	(0.08)	(0.03)	(−2.79)	(−2.61)
Resource dependence	−0.001	−0.000	−0.001	−0.000	−0.001	−0.001	0.002	0.002	0.001	0.001	0.001	0.001
	(−0.12)	(−0.06)	(−0.14)	(−0.08)	(−0.19)	(−0.13)	(0.55)	(0.55)	(0.22)	(0.26)	(1.44)	(1.15)

(continued)

Table 4. (continued)

	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10	D11	D12
	IVProbit	IVProbit	IVProbit	IVProbit	IVProbit	IVProbit	IVProbit	IVProbit	IVProbit	IVProbit	FE2SLS	FE2SLS
Model	*b*/(t)	*b*/(t)	*b*/(t)	*b*/(t)	*b*/(t)	*b*/(t)	*b*/(t)	*b*/(t)	*b*/(t)	*b*/(t)	*b*/(t)	*b*/(t)
Ln regime duration		0.095**		0.094**		0.087**		0.080		0.054		0.028***
		(2.15)		(2.12)		(2.00)		(1.45)		(0.85)		(3.83)
Global regime failures					2.758	2.644	1.750	1.566			0.380*	0.360*
					(1.43)	(1.38)	(1.02)	(0.93)			(1.72)	(1.66)
Region regime failures					1.226**	1.191*	0.655	0.618			0.127	0.130
					(2.00)	(1.96)	(1.18)	(1.14)			(1.37)	(1.39)
Time trend	0.002	0.000	0.002	0.000	0.003	0.001	0.017***	0.015***			0.003***	0.002*
	(0.46)	(0.08)	(0.41)	(0.04)	(0.69)	(0.33)	(3.44)	(2.98)			(3.27)	(1.78)
Year dummies									Y	Y		
Region dummies							Y	Y	Y	Y		
Country dummies											Y	Y
n	3,248	3,248	3,248	3,248	3,247	3,247	3,247	3,247	3,120	3,120	3,283	3,283
Countries	98	98	98	98	98	98	98	98	98	98	97	97
Instruments	R	R	R + G	R + G	R + G	R + G	R +G	R + G	R	R	R + G	R + G
Hansen *J* test *p* value			.39	.43	.56	.58	.56	.58			.50	.51
Kleibergen–Paap *F* stat	15.4	15.2	7.7	7.6	7.9	7.8	7.9	7.8	8.2	7.9	7.9	7.8

All diagnostic tests for IVProbit models are conducted on structurally equivalent (fixed effects) 2SLS models. First-stage regressions are reported in Online Appendix Table A19. Errors are clustered on country in all models. Maximum time series are 1946 to 2004. R = share of other countries in region having pensions that year; G = share of other countries globally having pension program that year.
*$p < .10$. **$p < .05$. ***$p < .01$.

regionally and globally experiencing regime failure (in given year) in D5 and D6. This does not alter the estimated effect of pensions on regime survival, but, as expected, the Hansen *J* test *p* values increase even further, increasing our confidence that the exclusion restriction holds for these models.

D7 and D8 include region-fixed effects. While this clearly weakens the first-stage *t* value of our regional instrument (to, respectively, 1.1 and 2.1) due to collinearity, the absolute *t* values for pensions on survival increase to, respectively, 4.5 and 4.7. We tried substituting the time trend in D7 and D8 with year dummies, but these models did not converge. After omitting the regional and global regime failure variables and using only the regional instrument, these IVprobit regressions (D9 and D10) run. Still, the dummies lead to exceedingly high collinearity with the instrument in the first stage, and we thus put less trust on the results. But, *if* we are to believe them, there is a strong effect of pensions on regime breakdown ($t < -14$). Finally, D11 and D12 are fixed effects 2SLS specifications using both the regional and global instruments. These are conservative models, controlling for country-specific effects, and they report a causal effect of pension programs significant at 10%. Although results are slightly weakened (see Online Appendix) when lagging all independent variables 1 additional year, the finding is fairly robust. Ideally, we would test whether withdrawing and introducing pension programs have different effects on regime survival. Our argument indicated that withdrawing a program is particularly dangerous for autocrats. This may well explain why very few autocracies have abolished major pension programs, with the side-effect that we cannot estimate separate models on implementing and abolishing pensions. With this caveat in mind, the results above suggest that pensions guard against regime breakdown in autocracies, supporting H3.[20]

Our models not only indicate a statistically significant but also a *substantively* large effect, although confidence intervals around the point predictions are wide (see Figure 4). For illustration, Model D1 predicts that the Mussolini regime had about 1.7% chance of breakdown in Italy-1925. Had the regime *not* posited a pension program, however, the best estimate is 8.8%. The model indicates that the personalist Perón regime was less consolidated, with a 7.3% breakdown probability in Argentina-1952. However, our best guess of this probability is 24.8% if Perón had removed the Argentine pension system. Figure 4 reports the predicted probability in a (nonpension) observation that actually observed a breakdown of its military regime, Thailand-1973. Student protests in Bangkok, following rising middle-class discontent with the economic situation, resulted in an intervention by the Thai King and subsequent regime change. *Perhaps*, if the regime had instituted pensions covering the discontent middle-class groups of Bangkok, they *might* have been less willing

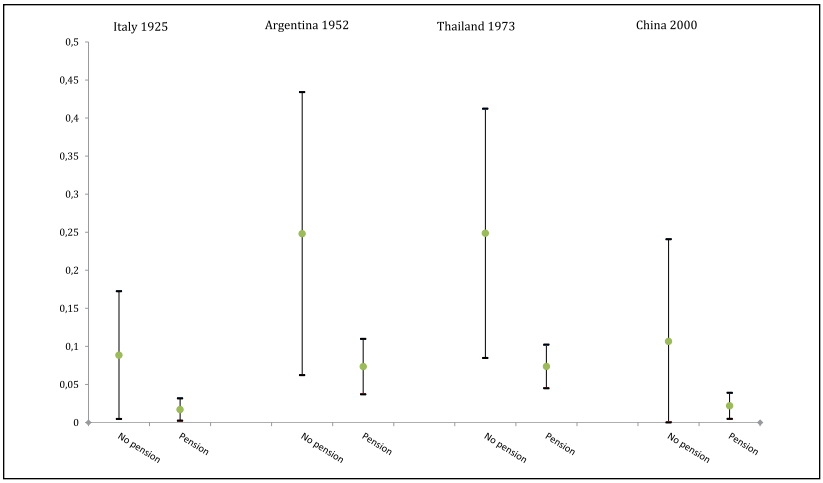

Figure 4. Predicted probabilities of regime breakdown (from Model D1) with 90% CIs.
All covariates set at actual values, except hypotheticals on pensions (Thailand-1973 did not have pension system, but the other three did), and Italy-1925 (not coded by GWF = Geddes, Wright and Frantz) is coded as dominant-party regime. CIs = confidence intervals.

to support the students' cause against the regime, leading to a different outcome. Model D1 indeed predicts a 24.9% chance of breakdown for Thailand-1973, which changes to 7.3% with a pension program in place. Finally, China-2000 had about 2% chance of regime failure, but this would have been above 10% without a Chinese pension system, according to Model D1.[21] Although these point estimates are very uncertain, there is seemingly a large effect of pension programs on regime survival.

One might still worry that the correspondence between social programs—here pensions—and regime durability is driven by some unobserved confounder (although some models above do control for country-specific effects). Perhaps more stable regimes, for some reason, adopt social programs more frequently. To probe this, we run equivalents to the models in Table 4 for maternity leave rather than pension programs. As for pensions, our prior tests showed no systematic difference in likelihood of adopting or having maternity programs between democracies and dictatorships (which we speculate could stem from many autocracies being concerned with keeping fertility rates high). But we do not clearly expect that providing benefits to working mothers increases regime survival. Maternity leave cannot be selectively targeted to *any* critical supporter. Furthermore, these fiscally smaller programs

are associated with lower sunk costs, and payments are made contingent on the event of childbirth, thus lacking features that make old-age pensions particularly suitable as credible commitment devices. If IV regressions with maternity leave program as endogenous independent variable—and regional and global frequency of such programs as instruments—show similar effects on regime breakdown, we should worry that the pension results are spurious. However, Online Appendix Table A21 shows that there is no effect of maternity programs on autocratic survival, whatsoever (most coefficients are actually positively signed). What is more, we find no evidence that work injury, sickness, or unemployment benefits affect regime breakdown either. Thus, the typically financially smaller programs that provide benefits contingent on realization of events such as injury or unemployment do not serve the same survival function for autocratic regimes as old-age pensions. Some models suggest a regime-stabilizing effect of family allowances. Yet, this result is not robust, with the coefficient flipping signs when controlling for region- or year-fixed effects. Thus, the evidence supports the notion that pensions constitute a unique and effective survival tool for autocracies. Indeed, Online Appendix Table A26 shows that the clear effect of pensions as shown in Table 4 holds up when controlling for existence of all other program types.

Conclusion

Major pension programs are not restricted to democracies. In this article, we have presented a novel argument for why autocratic regimes provide pension programs: Autocrats use pensions to direct resources to critical supporting groups. Targeted pensions are often favored over private-goods distribution, because they enable autocrats to credibly commit to future distribution, thereby mitigating the incentives of critical groups to overthrow the regime.

By leveraging the new SPAW dataset, we have systematically tested different implications from this argument, leveraging information from almost 140 countries with time series back to the late 1880s. We find clear patterns in the data that corroborate quite different implications from our argument. First, while autocracies are less likely than democracies to operate other social programs less suitable for directing resources to autocratic coalitions, democracies and autocracies do not systematically differ in the probabilities of adopting or having pensions. However, democracies operate more universal programs than autocracies. Perhaps most notably, we find robust evidence that pensions have a positive effect on autocratic regime survival, suggesting that implementing pensions constitutes an effective survival strategy for autocratic regimes.

Acknowledgments

The authors are grateful to Øyvind Skorge, Andrej Kokkonen, Carsten Jensen, Michael J. Donnelly, Jørgen Møller, Kalle Moene, Johannes Lindvall, Thomas Brambor, Jonas Pontusson, Sirianne Dahlum, Tore Wig, Merete Bech Seeberg, three anonymous reviewers, the editors of *Comparative Political Studies* (*CPS*), participants at the 2014 American Political Science Association (APSA) Annual Meeting in Washington, D.C., and participants at workshops and seminars at the Department of Political Science, Lund University, at the Department of Political Science, Gothenburg University, at Centre for the Study of Equality, Social Organization and Performance (ESOP), Department of Economics, University of Oslo, at the Department of Political Science, University of Oslo, and at the Department of Political Science and Government, Aarhus University for their valuable comments and suggestions.

Declaration of Conflicting Interests

The authors declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The authors disclosed receipt of the following financial support for the research, authorship, and/or publication of this article: The research was partly funded by Knutsen's Research Council Norway, "Young Research Talent" grant, pnr 240505. The authors are also grateful for the support from ESOP, the Department of Economics, University of Oslo, in partly financing data collection efforts related to this article.

Notes

1. There are likely systematic differences in social policy design and spending also within the broader regime categories, among both democracies and autocracies. For instance, democracies with proportional representation (PR) electoral systems may enhance social spending and induce more universal programs than plural-majoritarian (e.g., Iversen & Soskice, 2009; Persson & Tabellini, 2003). In this article, however, we focus on differences between the more general regime categories.
2. Following Geddes (1999), we define a regime as the set of formal and/or informal rules essential for choosing political leaders and maintaining them in power. Furthermore, we conceptualize autocratic regimes broadly as "non-democracies," implying that these regimes lack truly competitive elections and/or extensive franchise rights. In autocracies, the dictator and a close circle of collaborators are thus often the most relevant policymaking actors.
3. Yet, repressive strategies are likely endogenous to autocrats' co-optation strategies (Desai, Olosgård, & Yousef, 2009; Frantz & Kendall-Taylor, 2014).
4. Although our account closely follows "selectorate theory" (Bueno de Mesquita, Smith, Siverson, & Morrow, 2003), we only invoke "critical supporters" (corresponding to "winning coalition") and "other citizens," and not the notions of

"selectorate" or the resulting "loyalty norm" (winning coalition size/selectorate size). Yet, extending our theory could lead to implications resembling those related to the "loyalty norm": Keeping selectorate size constant, narrowing the number of critical supporters should increase rewards (through receiving more individual pension benefits) and potential losses following regime breakdown. This should, in turn, increase the incentives of critical supporters to fight for upholding the current regime.

5. A large theoretical and comparative case-study literature on autocratic breakdown (and democratization, in particular) suggests that industrial workers, business owners, salaried professionals, and other urban middle-class groups are often key groups, having requisite positions of influence on the economy and the ability to effectively organize (e.g., Ansell & Samuels, 2014; Collier, 1999; Rueschemeyer, Stephens, & Stephens, 1992).
6. To reiterate, this follows, for example, from pensions being easier to target to all relevant groups of supporters and from the fact that pensions are typically associated with larger sunk costs than other programs. Yet, this is a matter of degree as also programs such as family allowance benefits may be targeted to different groups and involve sunk investments in administrative capacity. Thus, we remain theoretically open to the possibility of other transfer programs being used for regime survival purposes and having regime-stabilizing effects, though we much more clearly expect a sizable effect for pensions.
7. For details, see Online Appendix and Rasmussen (2016).
8. But, laws (generally) prescribing broad coverage might exclude, for example, ethnic minorities (or majorities, as in Apartheid South Africa) or certain occupational groups. SPAW includes a measure capturing number of explicitly excluded groups. Second, although covered in general systems, groups might receive special benefits from additional programs. In SPAW, the (country-year) average is three special programs, with a maximum of 63 special programs (Greece in 1983). However, the coverage on excluded groups and special programs is thin for our global sample.
9. Below, we do not distinguish voluntary from compulsory coverage, or social insurance from private schemes (provident funds).
10. If different pension programs exist, all are coded and we average over these programs.
11. People with little or no property is not counted as a separate group for other programs, raising dimensionality and measurement level issues. Furthermore, they often presumably lack in economic leverage and resources for organizing collective action, and scoring these programs lower than, for example, employment-based programs for industrial workers does thus not reflect our argument on autocrats targeting programs to critical supporters well.
12. Benefits were, historically, often first extended to urban employees such as industrial, commercial workers, or salaried employees, with or without restrictions on firm size. Where agricultural workers were also included, a distinction was often made between workers using and not using electric power, ensuring that only workers close to urban centers were covered.

13. The latter requirement means that for the early part of our time series, fairly competitive systems with restricted franchise are counted as autocracies. Nonetheless, we conduct tests on alternative democracy measures (e.g., Polity, which often code such cases as fairly democratic) and on restricted time series (post-1945) where few such "competitive oligarchies" exist, and results are robust.
14. We also tested Cox survival models and logit models using a dummy variable BCSTS specification (Beck, Katz, & Tucker, 1998) or cubic splines (Carter & Signorino, 2010) to further account for temporal dependence. Our baseline results are fairly robust (see Online Appendix). Ideally, we would control for country-specific effects in our logit regressions, but this is infeasible. We include country dummies below when investigating degree of universalism, which provides far more within-country variation.
15. However, there are indications that democracies more often have pension systems once using the DD measure from Cheibub, Gandhi, and Vreeland (2010). This partly reflects the shorter sample (post-1945)—*t* values are higher (though still insignificant) for BMR post-1945. We also tested various Cox duration model specifications. Here, the regime coefficient was sensitive to minor specification changes, with many models showing no effect and some showing a positive effect of democracy.
16. The results are fairly robust to employing alternative lag-lengths for the independent variables. We also tested system GMM models instrumenting for levels of the endogenous independent variable (regime type) by using lagged differences and for differences in regime type by using lagged levels (see Roodman, 2009), and results are similar.
17. While autocrats cannot perfectly anticipate regime breakdown, the hypothesized pattern only requires that autocrats have consistent expectations in the sense that perceptions of risks of breakdown correlate with actual risk of breakdown.
18. Critical test values for Kleibergen–Paap *F* statistic are not readily calculable but Cragg–Donald *F* values (which are much higher), assuming classical errors, by far supersede all conventional critical test thresholds.
19. IV-estimates are biased toward the single-equation estimates, which in our case are closer to 0, proportionally to the instruments' weakness.
20. In the Online Appendix, we discuss similar results when only considering breakdowns followed by democratic transitions, finding evidence that pensions mitigate prospects for democratization.
21. The Chinese state pension system of 1951/1958 instituted coverage for the extensive state sector, with higher benefits for trade union members. The system thus covers not only industrial workers but also the growing salaried middle classes, placating two decisive groups with strong collective-action potential.

References

Acemoglu, D., Naidu, S., Restrepo, P., & Robinson, J. A. (2014). *Democracy does cause growth* (NBER Working Paper No. 20004). Cambridge, MA: National Bureau of Economic Research.

Acemoglu, D., & Robinson, J. A. (2006). *Economic origins of dictatorship and democracy*. New York, NY: Cambridge University Press.

Albertus, M., & Menaldo, V. (2012). If you're against them you're with us: The effect of expropriation on autocratic survival. *Comparative Political Studies*, *45*, 973-1003.

Alesina, A., Baquir, R., & Easterly, W. (1999). Public goods and ethnic divisions. *The Quarterly Journal of Economics*, *114*, 1243-1284.

Ansell, B. W. (2010). *From the ballot to the blackboard: The redistributive political economy of education*. Cambridge, UK: Cambridge University Press.

Ansell, B. W., & Samuels, D. J. (2014). *Inequality and democratization: An elite-competition approach*. Cambridge, UK: Cambridge University Press.

Beck, N., Katz, J. N., & Tucker, R. (1998). Taking time seriously: Time-series-cross-section analysis with a binary dependent variable. *American Journal of Political Science*, *42*, 1260-1288.

Blundell, R., & Bond, S. (1998). Initial conditions and moment restrictions in dynamic panel data models. *Journal of Econometrics*, *87*, 115-143.

Boix, C. (2003). *Democracy and redistribution*. Cambridge, UK: Cambridge University Press.

Boix, C., Miller, M. K., & Rosato, S. (2013). A complete data set of political regimes, 1800-2007. *Comparative Political Studies*, *46*, 1523-1554.

Boix, C., & Svolik, M. W. (2013). The foundations of limited authoritarian government: Institutions, commitment, and power-sharing in dictatorships. *Journal of Politics*, *75*, 300-316.

Bueno de Mesquita, B., Smith, A., Siverson, R. M., & Morrow, J. D. (2003). *The logic of political survival*. Cambridge, MA: MIT Press.

Carter, D. B., & Signorino, C. S. (2010). Back to the future: Modeling time dependence in binary data. *Political Analysis*, *18*, 271-292.

Cheibub, J. A., Gandhi, J., & Vreeland, J. (2010). Democracy and dictatorship revisited. *Public Choice*, *143*, 67-101.

Chetty, R. (2006). A new method of estimating risk aversion. *American Economic Review*, *96*, 1821-1834.

Collier, R. B. (1999). *Paths towards democracy*. Cambridge, UK: Cambridge University Press.

Desai, R. M., Olosgård, A., & Yousef, T. M. (2009). The logic of authoritarian bargains. *Economics & Politics*, *21*, 93-125.

Epstein, E. C. (1979). Control and co-optation of the argentine labor movement. *Economic Development and Cultural Change*, *27*, 445-465.

Esping-Andersen, G. (1990). *The three worlds of welfare capitalism*. Princeton, NJ: Princeton University Press.

Frantz, E., & Kendall-Taylor, A. (2014). A dictator's toolkit: Understanding how co-optation affects repression in autocracies. *Journal of Peace Research*, *51*, 332-346.

Gandhi, J. (2008). *Political institutions under dictatorship*. Cambridge, UK: Cambridge University Press.

Geddes, B. (1999). What do we know about democratization after twenty years? *Annual Review of Political Science*, *2*, 115-144.

Geddes, B., Wright, J., & Frantz, E. (2014). Autocratic breakdown and regime transitions: A new data set. *Perspectives on Politics*, *12*, 313-331.

Gehlbach, S., & Keefer, P. (2011). Investment without democracy: Ruling-party institutionalization and credible commitment in autocracies. *Journal of Comparative Economics*, *39*, 123-139.

Gehlbach, S., & Keefer, P. (2012). Private investment and the institutionalization of collective action in autocracies: Ruling parties and legislatures. *Journal of Politics*, *74*, 621-635.

Haber, S., & Menaldo, V. (2011). Do natural resources fuel authoritarianism? A reappraisal of the resource curse. *American Political Science Review*, *105*, 1-26.

Hadenius, A., & Teorell, J. (2007). Pathways from authoritarianism. *Journal of Democracy*, *18*, 143-156.

Haggard, S., & Kaufman, R. R. (2008). *Development, democracy, and welfare states: Latin America, East Asia, and Eastern Europe*. Princeton, NJ: Princeton University Press.

Harding, R., & Stasavage, D. (2014). What democracy does (and doesnât do) for basic services: School fees, school inputs, and African elections. *Journal of Politics*, 76, 229-245.

Huber, E., & Stephens, J. D. (2001). *Development and crisis of the welfare state: Parties and policies in global markets*. Chicago, IL: University of Chicago Press.

Huber, J. D., Ogorzalek, T. K., & Gore, R. (2012). *Democracy, targeted redistribution and ethnic inequality* (Working paper). New York, NY: Columbia University.

Huntington, S. P. (1991). *The third wave: Democratization in the late twentieth century*. Norman: University of Oklahoma Press.

International Labor Organization. (1922). *Legislative series*. Geneva, Switzerland: Author.

Iversen, T. (2005). *Capitalism, democracy, and welfare*. Cambridge, UK: Cambridge University Press.

Iversen, T., & Soskice, D. (2009). Distribution and redistribution: The shadow of the nineteenth century. *World Politics*, *61*, 438-486.

Keohane, R. O. (1984). *After hegemony: Cooperation and discord in the world political economy*. Princeton, NJ: Princeton University Press.

Kim, W. (2007). Social insurance expansion and political regime dynamics in Europe, 1880-1945. *Social Science Quarterly*, *88*, 494-514.

Knutsen, C. H. (2011). Democracy, dictatorship and protection of property rights. *Journal of Development Studies*, *47*, 164-182.

Kuran, T. (1989). Sparks and prairie fires: A theory of unanticipated political revolution. *Public Choice*, *61*, 41-74.

Lake, D. A., & Baum, M. A. (2001). The invisible hand of democracy: Political control and the provision of public services. *Comparative Political Studies*, *34*, 587-621.

Lindert, P. H. (2005). *Growing public: Social spending and economic growth since the eighteenth century* (Vol. 1). Cambridge, UK: Cambridge University Press.

Magaloni, B. (2006). *Voting for autocracy: Hegemonic party survival and its demise in Mexico*. New York, NY: Cambridge University Press.

Magaloni, B. (2008). Credible power-sharing and the longevity of authoritarian rule. *Comparative Political Studies*, *41*, 715-741.

Mares, I. (2005). Social protection around the world: External insecurity, state capacity, and domestic political cleavages. *Comparative Political Studies*, *38*, 623-651.

Mares, I., & Carnes, M. E. (2009). Social policy in developing countries. *Annual Review of Political Science*, *12*, 93-113.

Meltzer, A., & Richard, S. (1981). A rational theory of the size of government. *Journal of Political Economy*, *89*, 914-927.

Mesa-Lago, C. (1978). *Social security in Latin America: Pressure groups, stratification, and inequality*. Pittsburgh, PA: University of Pittsburgh Press.

Milanovic, B. (1998). *Income, inequality, and poverty during the transition from planned to market economy*. Washington, DC: The World Bank.

Miller, M. K. (2015). Democratic pieces: Autocratic elections and democratic development since 1815. *British Journal of Political Science*, *45*, 501-530.

Moene, K., & Wallerstein, M. (2001). Inequality, social insurance, and redistribution. *American Political Science Review*, *95*, 859-874.

Mulligan, C. B., Sala-i-Martin, X., & Gill, R. (2003). Do democracies have different public policies than nondemocracies? *Journal of Economic Perspectives*, *18*, 51-74.

North, D. C., & Weingast, B. R. (1989). Constitutions and commitment: The institutions governing public choice in seventeenth century England. *Journal of Economic History*, *44*, 803-832.

Obinger, H., & Petersen, K. (2017). Mass warfare and the welfare state â causal mechanisms and effects. *British Journal of Political Science*, *47*, 203-227.

Obinger, H., Schmitt, C., & Starke, P. (2012). Policy diffusion and policy transfer in comparative welfare state research. *Social Policy & Administration*, *47*, 111-129.

Persson, T., & Tabellini, G. (2003). *The economic effect of constitutions*. Cambridge, MA: MIT Press.

Pierson, P. (2000). Increasing returns, path dependence, and the study of politics. *American Political Science Review*, *94*, 251-267.

Ponticelli, J., & Voth, H.-J. (2011). *Austerity and anarchy: Budget cuts and social unrest in Europe, 1919-2008* (Working paper) Centre for Economic Policy Research 77 Bastwick Street, London EC1V 3PZ, UK.

Przeworski, A., Alvarez, M. E., Cheibub, J. A., & Limongi, F. (2000). *Democracy and development*. Cambridge, UK: Cambridge University Press.

Rasmussen, M. B. (2016). *The Social Policy around the World (SPaW) Database: Codebook*. Retrieved from https://www.researchgate.net/publication/299445433

Rimlinger, G. V. (1971). *Welfare policy and industrialization in Europe, America and Russia*. New York, NY: John Wiley.

Roodman, D. (2009). How to do xtabond2: An introduction to difference and system GMM in Stata. *STATA Journal*, *9*, 86-136.

Rueschemeyer, D., Stephens, E. H., & Stephens, J. D. (1992). *Capitalist development and democracy*. Chicago, IL: University of Chicago Press.
Scruggs, L. (2006). The generosity of social insurance, 1971-2002. *Oxford Review of Economic Policy*, *22*, 349-364.
Stasavage, D. (2005). Democracy and educational spending in Africa. *American Journal of Political Science*, *49*, 343-358.
Svolik, M. W. (2012). *The politics of authoritarian rule*. Cambridge, UK: Cambridge University Press.
Weyland, K. (2005). Theories of policy diffusion: Lessons from Latin American pension reform. *World Politics*, *57*, 262-295.
Weyland, K. (2007). *Bounded rationality and policy diffusion: Social sector reform in Latin America*. Princeton, NJ: Princeton University Press.
Wintrobe, R. (1998). *The political economy of dictatorship*. Cambridge, UK: Cambridge University Press.
Wright, J. (2008). Do authoritarian institutions constrain? How legislatures affect economic growth and investment. *American Journal of Political Science*, *52*, 322-343.

Author Biographies

Carl Henrik Knutsen is professor of Political Science at the University of Oslo. His research focuses on the economic effects of political institutions and on the determinants of regime survival and regime change.

Magnus Rasmussen is senior researcher at the Institute for Social Research, Oslo. He is currently involved in research projects on the historical origins of distributive norms, and the effect of electoral rules on industrial conflict and collective bargaining using new historical Norwegian geo-coded data.