The Effect of Women's Descriptive Representation on Premature Mortality

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Although many studies have found that women legislators pursue policies important to women, we know relatively little concerning the effect of women's descriptive representation (WDR) on women's quality of life. We address this question by examining the relationship between the election of women state legislators and public health. Specifically, we estimate the effect of WDR on premature death rates from 1982 to 2010. We find that WDR serves to improve the well-being of both women and men. However, the effect of WDR on women's health is contingent on several important factors. First, the health-improving effects of WDR are most strongly related to the descriptive representation of Democratic women. Second, the effect of Democratic WDR on women's health, relative to men's health, increases with the scale of the state's Medicaid program. Finally, we estimate the mediating role of nine specific policies, finding that four policies mediate the effect of Democratic WDR on premature mortality. Overall, our results suggest that policy remedies aimed at improving the severe underrepresentation of women in government may help to improve the relatively low ranking of the United States on indicators of public health.

Despite a significant increase in the election of women to political office over the last 50 years, women remain vastly underrepresented at all levels of government. As of 2019, women comprised slightly more than half of the adult population in the United States, yet they occupied only 24 percent of all US House and Senate seats, and a mere 29 percent of all state legislative seats (Center for American Women and Politics [CAWP 2019]).[1]

Over the years, scores of studies have examined the consequences of this disparity in descriptive representation for American politics. One of the most important questions addressed in this literature concerns the impact that women legislators have on public policy. Although there is some debate concerning the answer to this question (e.g., Celis et al. 2008; Celis and Childs 2012), the balance of studies have generally found that compared to men, women are more likely

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to pursue a variety of policies that are important to women, such as women's rights, education, health care, public safety, social welfare, economic equality, the environment, and the well-being of children. The distinctive priorities of women legislators regarding these issues has been observed in democracies across the world (Bolzendahl and Brooks 2007; Park 2017), as well as across various dimensions of legislative behavior in the United States, including bill sponsorship (Saint-Germain 1989; Bratton and Haynie 1999; Bratton 2005), roll call voting (Thomas and Welch 1991; Swers 1998), and representation on legislative committees where such issues are typically assigned (Thomas and Welch 1991; Dolan and Ford 1997).

Clearly, the existing research provides numerous examples of the distinctive impact of women representatives in the policy process, especially concerning policies important to women. Yet, we know relatively little concerning the impact of women's descriptive representation (WDR) on women's lives. Does the election of women to government office ultimately serve to improve the well-being of the women they represent? The answer to this question has enormous implications for debates concerning the importance of WDR and the appropriateness of policy mechanisms, such as gender quotas, to ensure the participation of women in the policymaking process. In this paper, we address this question by examining the relationship between WDR and one of the most important and widely studied indicators of population health and well-being—premature mortality.

Despite the mounting evidence of the connection between politics and health at the US state level (Kim and Jennings 2009; Patton 2014), and the city level (Ronzio, Pamuk, and Squires 2004; Costich and Patton 2012), scholars have only recently begun to turn their attention to the potential health-improving effects of WDR. Indeed, we are aware of only three such studies, each of which has examined this question using cross-national data. Utilizing a sample of developed and developing countries, Westfall and Chantiles (2016) found that the use of gender quotas in national elections was associated with longer life expectancy for women, as well as reductions in maternal and infant mortality. Swiss, Fallon, and Burgos (2012) examined a sample of developing countries and found that WDR was positively related to child immunization rates, as well as infant and child survival rates. And in a study of health outcomes in 30 European countries from 1990 to 2010, Mackenbach and McKee (2015) found that the representation of women in the parliament was significantly related to several indicators of public health.

Our study extends the literature in several important ways. Importantly, our research is the first to examine this question using data for the United States. Specifically, we examine the impact of WDR in US state legislatures on premature death rates over the three-decade period 1982–2010. Our research is also the first to examine the extent to which the potential health benefits of WDR are restricted to women, or if these health-improving effects spill over to the rest of the population. Accordingly, we test two competing hypotheses regarding the effect of WDR. Assuming that women state legislators are likely to pursue policies that have a disproportionate impact on the well-being of

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women, the "targeted benefits" hypothesis predicts that their presence in the legislature will have a greater mortality-reducing effect for women, compared to men. The "universal benefits" hypothesis predicts that WDR will result in better health, and thus lower premature death rates, for both women and men. We test these hypotheses by estimating the effect of the percentage of women in the state legislature on gender-specific premature death rates and infant death rates, utilizing state panel data. Our results provide strong evidence that WDR can have a positive effect on state health outcomes for both women and men, especially when Democratic (rather than Republican) women are in office. Yet, consistent with the targeted benefits hypothesis, the average effect of WDR on women's premature mortality is significantly larger than its effect for men.

In the remainder of the article, we explore two general mechanisms through which WDR might affect state health outcomes. First, we argue that the effect of WDR on health may be mediated through the effects of three types of policies that have important effects on health—social regulatory policies designed to incentivize healthy behavior, government policies and programs that affect material well-being, and government policies that affect healthcare provision. Accordingly, we estimate the mediating role of nine specific policies that are broadly representative of these three policy dimensions. We find that four of these policies—anti-smoking policies, alcohol taxes, graduated licensing laws, and imprisonment rates—serve to mediate the effect of WDR on premature mortality.

Second, we argue that WDR may have important effects on the content of Medicaid policy that disproportionately benefit children and their parents. Therefore, we test the hypothesis that the health-improving effect of WDR on health may increase with the scale of state Medicaid programs. We find strong evidence in support of this argument for all three indicators of premature mortality. Overall, our results support the conclusion that the underrepresentation of women in government in the United States has had important effects for all Americans that go far beyond those already established in the literature. As the government role in healthcare provision continues to increase, our findings suggest that the importance of women in government will only become more significant.

Theorizing the Link Between Women's Descriptive Representation and Public Health

Has the descriptive representation of women had a positive effect on public health? The plausibility of such a relationship would seem to hinge on the existence of a causal chain of relationships that render the ultimate impact of descriptive representation highly contingent (Childs and Krook 2009). First, we must assume that women are more likely than men to initiate and support legislation that either directly or indirectly promotes public health. Second, the distinctive policy leadership of women must result in a greater rate of passage of policies that affect public health. And third, the policies passed and implemented

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by government must have a significant effect on broad measures of health for a large share of the population. We examine the plausibility of each of these causal linkages, focusing on the specific context of state governments.

On balance, the extant literature shows that women state legislators provide substantive representation on policies broadly defined as women's issues—public assistance, education, health, and child welfare, among others. The evidence supporting this conclusion comes from a variety of analyses of elected officials' attitudes and behavior. For example, surveys of the policy preferences of state legislators have repeatedly shown that women are more likely than their men colleagues to support women-friendly policies (Thomas 1991; Reingold 1992). Research has also shown that women are more likely to seek appointments to health, welfare, and education committees in legislatures (Thomas and Welch 1991; Dolan and Ford 1997) and are more likely than men to introduce women-friendly legislation, though some studies find this is dependent on party control of the legislature (Saint-Germain 1989; Bratton and Haynie 1999; Bratton 2005; Osborn 2012). Finally, several studies have found that women are more likely than men to support such legislation when it comes to the floor for a vote (e.g., Saint-Germain 1989; Thomas and Welch 1991).

Although the evidence strongly suggests that women promote and support women-friendly policies, this does not mean that the presence of women in the state legislature has necessarily resulted in a greater rate of passage of such policies at the institutional level. Indeed, a number of studies have found that women often face barriers to advancing their agenda in the legislature and their policymaking effectiveness may be limited due to a variety of factors, including a backlash from male colleagues (Kathlene 1994; Bratton 2002), constraints imposed by party leaders (Osborn 2012, 2014), and until recent years, a lack of representation on the most powerful committees (Cammissa and Reingold 2004). Finally, there may simply be too few women in most state legislatures to make a significant difference in the type of legislation that is ultimately passed.

A handful of studies have examined the policy impact of women legislators by estimating the relationship between the percentage of women in the state legislature and the adoption of one or more policies assumed to be prioritized by women. This includes studies of policies that directly affect women such as abortion restrictiveness (Berkman and O'Connor 1993; Patton 2007), child support enforcement (Besley and Case 2003), and domestic violence (Mutpfay 1997), among others. Studies have also found the percentage of women state legislators to be related to the adoption of social policies that have a broader impact on society beyond women. This research includes studies examining the effects of women state legislators on criminal justice punitiveness (Yates and Fording 2005), public assistance generosity (Reingold and Smith 2012), and workers' compensation generosity (Besley and Case 2003). Although the literature is not unanimous in its conclusions regarding the effect of WDR (e.g., Tolbert and Steuernagel 2001; Caiazza 2004; Cowell-Myers and Langbein 2009), the balance of the evidence suggests that women state legislators promote policies that support women's interests, broadly defined, and that women have often (if not always) contributed to the passage of such policies at the

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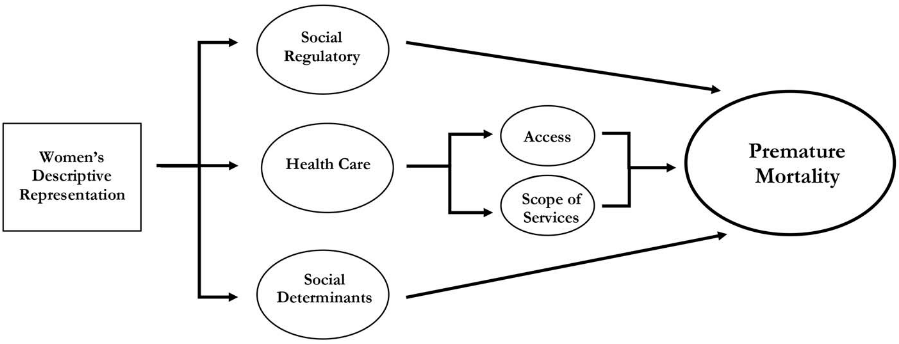
Figure 1. Causal pathways linking WDR to premature mortality.



institutional level. The final causal link to examine, therefore, is the effect that such policies might have on public health. To guide our analysis, we propose a theoretical framework for understanding the link between WDR and health outcomes that highlights three major policy mechanisms. This framework is graphically illustrated in figure 1.

First, state policies may affect public health through social regulatory policies that incentivize healthy behavior and minimize the risk of early death. Although state policies address a wide range of behaviors and conditions that affect health, policies addressing the use of tobacco and alcohol have been found to be among the most effective in promoting public health. For example, most (but not all) states ban smoking in public spaces to reduce the negative effects of cigarette smoking. These laws have resulted in improved air quality and improved health, especially among non-smoking workers who otherwise would experience the negative health effects of second-hand smoke (Schmidt 2007). State policies also regulate alcohol consumption and its negative health effects through alcohol taxes and stiff penalties for driving while intoxicated. Studies have thoroughly demonstrated that by raising the costs of alcohol consumption, these laws have led to a decrease in alcohol use and a reduction in alcohol-related disease and death (Chaloupka Frank, Grossman, and Henry Saffer 2002; Voas, Scott Tippetts, and Fell 2000).

State regulatory policies also seek to minimize the risk of accidental deaths, which have a disproportionate effect on premature death rates due to the fact that such deaths often occur at a young age. Among the most important policies in this category are laws that seek to reduce traffic fatalities, such as seatbelt laws (Cohen and Einav 2003) and motorcycle helmet laws (Derrick and Faucher 2009). Many studies have found that collectively, these policies have had a profound influence on individual behavior leading to positive societal health outcomes (Farrelly, Evans, and Sfekas 1999; Levy, Michael Cummings, and Hyland 2000; Levy, Friend, and Polishchuk 2001; Brownson, Haire-Joshu, and Luke 2006; Derrick and Faucher 2009).

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Second, women state legislators might affect health outcomes through their effects on healthcare policy. One tool that states use is their power to regulate the healthcare industry, including insurance companies. For example, Kun and Muir (1997) found that women legislators played a significant role in the diffusion of laws requiring hospitals to end the practice of early postpartum discharges in the early 1990s, before federal legislation ended so-called "drive-through deliveries." This led to the diffusion of other forms of "length of stay" laws requiring in-hospital care after certain procedures (such as mastectomies). States may also issue health mandates that require insurance companies to cover certain types of health care services. Requirements to cover preventative services that disproportionately affect women, such as breast cancer, cervical cancer, and osteoporosis screening have had significant mortality-reducing effects for women (Tolbert and Steuernagal 2001).

In addition to regulation of private healthcare, state governments play a significant role in the provision of government-funded healthcare programs. Numerous studies have found that state Medicaid programs can have a significant mortality-reducing effect, especially among women, infants, and children (Moss and Carver 1998; Sommers, Baicker, and Epstein 2012; Patton 2014). Yet, states exercise a great deal of discretion in the design of their Medicaid programs. The most obvious policy tool used to limit or expand the Medicaid rolls is eligibility rules. While almost all states set eligibility for children to receive Medicaid or CHIP at 200 percent of the federal poverty line (or higher), there is considerable variation in eligibility rules across states for parents or caretaker relatives of Medicaid-eligible children. States may also limit access to care and program usage by charging premiums, co-pays, and/or co-insurance to select Medicaid recipients. In addition to discretion in granting access to Medicaid, states also have significant discretion in the benefits they provide for their Medicaid-eligible population. The federal government mandates a list of benefits all states much cover, but a large number of benefits are left to the discretion of the states. Examples of such discretionary benefits and services include prescription drug benefits, respiratory care services, diagnostic screening, preventive and rehabilitative services, and health homes for recipients with chronic conditions.

Finally, government policies may also affect health outcomes through their effects on what public health experts refer to as social determinants of health, which are generally defined as non-medical direct or indirect influences on health. Studies have consistently shown that social class is one of the most important social determinants of health (Woolf and Braveman 2011). As a result, public policies that focus on reducing poverty and improving social mobility can contribute to improved health. This includes investments in early childhood programs, which are often focused on lower-income families and help to improve cognitive and non-cognitive development that reduce future risky health behaviors (McGovern, Miller, and Hughes-Cromwidk 2014). But it may also include public assistance programs (Kim and Jennings 2009), such as Aid to Families with Dependent Children/Temporary Assistance for Needy Families (Currie and Cole 1993), and food security programs, such as

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Supplemental Nutrition Assistance Program and Women, Infants, and Children (Gunderson and Ziliak 2015). While the positive health effects of any single policy or program enacted at the state level may be modest, given the fact that women legislators have been found to support a wide range of potential health-improving policies, the cumulative impact across the entire range of state policies could be quite significant.

Women's Descriptive Representation and State Mortality Rates: An Empirical Analysis

We estimate the relationship between women state legislators and premature mortality utilizing state panel data for the period 1982–2010.[2] We restrict the analysis to years preceding the implementation of the Affordable Care Act due to the profound impact that this legislation had on government-funded health care provision as well as a lack of data to properly control for changes in relevant policy variables.[3] Our primary dependent variable is a measure of premature death, measured separately for women and men. Specifically, we utilize an indicator that public health scholars refer to as "years of potential life lost" (YPLL). This measure is widely used in federal, state, and local health planning, in decision-making regarding allocation of health resources, and as an outcome variable in the public health literature (e.g., Centers for Disease Control and Prevention 2008; Costich and Patton 2012). The advantage of YPLL is that, as an indicator of premature mortality, it represents the total number of years not lived by an individual who died before the expected age of death (often set at age 75). Although YPLL is similar to the mortality rate, as Gardner and Sanborn (1990, 322) note, "mortality rates describe the amount of death in a population, but they fail to quantify the burden of loss resulting from this mortality." Because YPLL assigns greater weight to deaths that occur at a younger age, health researchers generally agree that it provides a better measure of the social and economic consequences of mortality.

Our indicators of YPLL are measured as a rate per 100,000 population, and are age-adjusted to the 2000 US population. We utilize two YPLL measures as primary dependent variables—one for women and one for men. Figure 2 provides descriptive information on variation in YPLL levels and trends across four illustrative states. Specifically, the figure includes trends for two states that are among those with the lowest YPLL rates during our sample period (California and Minnesota) and the two states with the highest YPLL rates as of 2010 (Mississippi and West Virginia). The solid lines represent YPPL rates for women, while the dashed lines represent YPLL rates for men. Two overarching trends are immediately apparent. First, YPLL rates are generally higher for men than for women, reflecting the well-known fact that women generally live longer than men. Second, there has been a more or less steady decline in YPLL rates over the last three decades for both men and women.

The figure also suggests that there can be significant variation in YPLL rates and trends across states. For example, California has witnessed one of the largest

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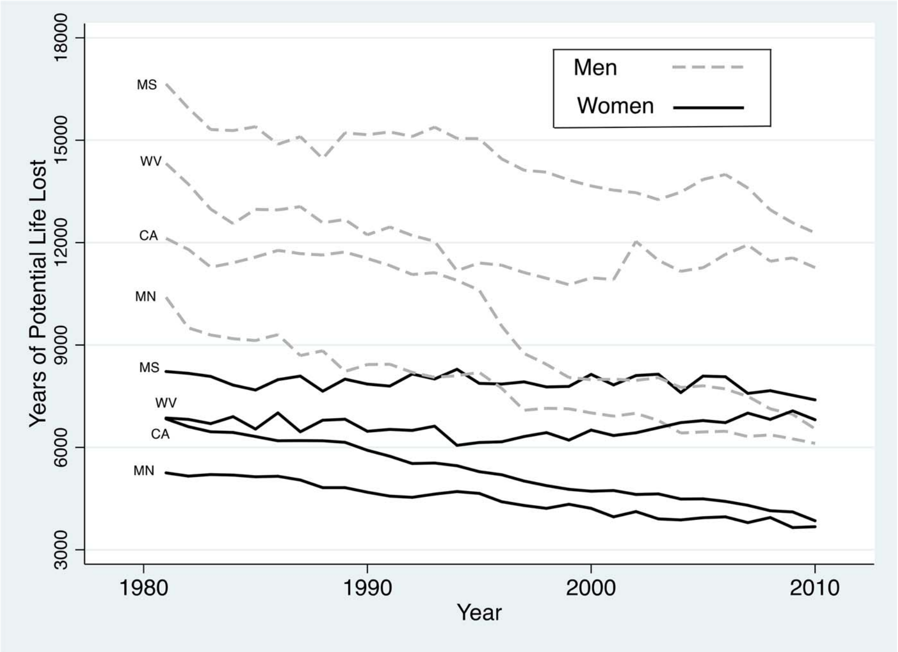
Figure 2. Trends in YPLL for selected states, 1981–2010.



Note: The vertical axis represents YPLL, age-adjusted based on 2000 census data.

decreases in YPLL in recent decades. However, the change in YPLL has been far less dramatic in many other states, especially for women. In the state of West Virginia, for example, women's YPPL has actually increased since 1990. While these examples are merely illustrative, the data for the full sample reflect significant variation in state YPLL rates and trends over the three-decade period of our analysis. This suggests that state-specific factors may indeed play an important role in trends in population health.

In addition to YPLL, we also utilize state infant death rates as an alternative dependent variable. Specifically, the infant death rate is measured as number of deaths before 1 year of age per 1000 live births. Consistent with the literature, we transform this variable by computing the natural log of the infant death rate for estimation purposes. As one might expect, the correlation between the infant death rate and YPLL in our sample is high, at 0.84 for MYPLL and 0.83 for WYPLL. Infant death rates and YPLL thus appear to be tapping the same underlying concept—presumably, population health.[4]

Independent Variables

Studies of WDR generally measure WDR in one of two ways—the percentage of legislators that are women, or the percentage of legislators that are Democratic

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women. We refer to these two indicators as "women legislators" and "Democratic women legislators" below. The case for restricting the indicator of WDR to Democratic women is based on the fact that while Democratic women legislators continue to express more liberal viewpoints than their male copartisans (Frederick 2009), over time Republican women legislators have become more conservative and according to some studies, ideologically indistinguishable from their male copartisans (Epstein, Niemi, and Powell 2005; Osborn 2012). Since we assume that the effect of WDR potentially operates through the adoption and implementation of ideologically salient social policies, it is therefore plausible that Republican women may be more ambivalent in their support of such policies and that it is Democratic women legislators who are driving the effect of WDR on policy. Indeed, several studies of state legislative policymaking have found that the effect of WDR is "party-dependent" in this way (e.g., Berkman and O'Conner 1993; Bratton 2002; Hogan 2008; Reingold, Widner, and Harmon 2019).

In addition to indicators of WDR, we also include several control variables to help guard against bias due to confounding variables. We include three additional political variables—the percentage of state legislators that are African American, state citizen liberalism and state government liberalism (Berry et al. 1998).[5] Given the fact that African Americans, liberals and Democrats are more likely to support government programs related to social welfare and public health (Berry et al. 1998; Bratton and Haynie 1999), it is possible that stronger representation of these interests in state politics will lead to better health outcomes for women, who are more likely than men to benefit from government social welfare programs. However, policies that improve education, the environment, and strengthen public health, for example, should improve the health of all citizens, regardless of gender. All political variables are lagged 1 year to account for the gap between policy adoption and implementation.

We include several other control variables to account for the economic and social determinants of public health. To measure social class, we include measures of per capita income (in 2014 dollars) and the percentage of citizens with a college degree. For each of these variables, we construct separate measures for men and women using data from the Current Population Survey. In addition to these gender-specific indicators, we also include two general indicators of economic well-being in a state—the state poverty rate and the state unemployment rate—as these indicators are likely to be related to the health of all citizens. Based on past research, we expect state poverty rates to be positively related to YPLL. The effect of the unemployment rate is expected to be negative, consistent with other studies that have found a counter-cyclical effect of unemployment on mortality (Sullivan et al. 2009).

We also include the percentage of the state population that is black and Hispanic in each state. We expect that black population size will be positively related to YPLL and infant death rates, based on the literature which finds that blacks are more likely to experience poorer health, even after controlling for socioeconomic conditions (Patton 2014). We expect the opposite effect for

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Hispanic population size, due to the large body of evidence which finds that Hispanics tend to enjoy better health and longer life expectancy compared to white non-Hispanics, leading to what some scholars refer to as the "Hispanic mortality paradox" (Ruiz, Steffen, and Smith 2013).

Finally, we include two indicators of the state health environment. The literature is replete with findings which show that individuals who live in cities and states with higher levels of social capital tend to have better health outcomes. Therefore, we include an indicator of state social capital, which we expect will be negatively related to YPLL (Hawes et al. 2013).[6] We also include the number of primary care physicians per 100,000 state population. We expect that this variable will have a similar health-improving effect given the many studies which have found access to primary care to be an important determinant of a variety of health outcomes.

Due to the panel structure of the data, we estimate all models with fixed effects for states and years. The inclusion of state fixed-effects effectively controls for the full set of state-specific, time-invariant factors that affect health, and which remain relatively stable over time. This feature of the design also restricts the model to explaining within-state variation in state health outcomes, which reduces the possibility of endogeneity due to omitted variable bias. The inclusion of year fixed effects controls for the effects of variables that are common to all states over the time period, and would include the effects of nationally administered social programs such as Medicare and Social Security, as well as major advances in the treatment of diseases.

Estimation and Results

To estimate the effect of WDR on state YPLL, we utilize error correction models (ECMs), estimated with OLS and panel corrected standard errors. ECMs have become increasingly popular for the estimation of time-series cross-section models due to their ability to model complex temporal dynamics that are often present in time series data (Suzanna and Keele 2008). They are also appropriate for our measures of premature mortality as panel unit root tests find that our data are non-stationary. We estimate single-equation ECMs for each measure of premature mortality, the general form of which follow equation 1 below:

$$\Delta \text{Premature Mortality}_{i,t} = \alpha_{i,t} + \beta_0 \text{ Premature Mortality}_{i,t-1} + \beta_1 \Delta \text{WDR}_{i,t} + \beta_2 \text{WDR}_{i,t-1} \quad (1)$$

For each dependent variable, we regress the first difference of premature mortality on the lagged level of premature mortality, the first difference of WDR, the lagged level of WDR, and the first differences and lagged levels of our set of control variables. [7] For each independent variable, there are two types of effects that are estimated by equation 1. For Women Legislators, β_1 captures the immediate, short-term effect of Women Legislators on Y. However, each X in equation 1 may also have a long-term effect on Y that is not experienced immediately, but is distributed over time. For Women Legislators, this long-term

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effect is calculated as β_2/β_0 and is known as the long-run multiplier (LRM). The ECM specification is particularly useful for our analysis as we expect that the effects of many of the independent variables may not be immediately affected by changes in the health environment.

Models 1–4 in Table 1 present regression results for ECM models of WYPLL and MYPLL. Models 5–6 present results for infant death rates. For each of these dependent variables, we report two versions of the model based on our two measures of WDR—women legislators and Democratic women legislators. To facilitate comparison of the effects of the independent variables across the various models, each dependent variable was transformed to standard deviation units (z-scores) prior to estimation. Thus, the coefficient estimates in Table 1 reflect the expected standard deviation change in the dependent variable given a one-unit increase in the independent variable.

The results in Table 1 clearly show that there is no significant effect of WDR on state health outcomes when WDR is measured as all women legislators. However, the indicator based on Democratic women legislators has a negative, statistically significant long-term effect on premature death rates for both men and women and its effect is on the cusp of statistical significance for infant death rates ($p = .07$). Based on the coefficient estimates reported in Table 1, the LRM for a 10-point in increase in Democratic women legislators on women's YPLL is −0.17. That is, a 10-point increase in Democratic women legislators leads to a 0.17 standard deviation decrease in YPLL for women. The LRM for men is slightly smaller at −0.14. Statistical tests indicate that the difference in the two effects is on the threshold of statistical significance. [8] Therefore, we conclude that these results generally support the targeted benefits hypothesis, although the difference in the effects is relatively modest.

Having examined the effects of WDR in relative terms, what do these results mean in substantive terms? Given that the standard deviation of WYPLL in our sample is approximately 1,000, this means that a 10-point increase in WDR is expected to result in a long-term decrease of approximately 170 years in WYPLL. Now consider the fact that the difference in WDR between the state with the largest percentage of women in the state legislature (Nevada, at approximately 40 percent) and the state with the smallest percentage of women in the legislature (Wyoming, at 11 percent) represents a difference of 29 percentage points. Based on our results, this equates to an additional 501 years of life lost per 100,000 residents in Wyoming compared to Nevada, all else equal.

Finally, we turn to the effects of WDR on infant death rates. According to the results, an increase of 10 percentage points in Democratic women legislators is associated with a total decrease of approximately 0.09 standard deviations (approximately 2.5 percent) in the infant death rate, ceteris paribus.[9,10]

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Table 1. Regression Estimates of the Effects of WDR on State Health Outcomes, 1982–2010

	Women YPLL			Male YPLL		Infant Death Rate
Independent Variables	(1)	(2)	(3)	(4)	(5)	(6)
ΔWomen Legislators $_t$	0.00501 [0.00344]	—	0.00107 [0.00240]	—	0.00830 [0.00523]	—
Women Legislators$_{t-1}$	−0.00388 [0.00245]	—	−0.00212 [0.00149]	—	−0.000536 [0.00330]	—
ΔDem. Women Legislators$_{t-1}$	—	0.00188 [0.00380]	—	−0.00128 [0.00272]	—	0.00670 [0.00568]
Dem. Women Legislators$_{t-2}$	—	−0.00693*[0.00284]	—	−0.00359*[0.00166]	—	−0.00635*** [0.00352]
Black Legislators$_{t-2}$	−0.00194 [0.00327]	−0.00138 [0.00330]	−0.00612*[0.00249]	−0.00590*[0.00251]	0.00842*[0.00413]	0.00903*[0.00407]
ΔCitizen Liberalism$_{t-1}$	−0.00464** [0.00142]	−0.00476** [0.00142]	0.000759 [0.000923]	0.000694 [0.000923]	−0.00259 [0.00190]	−0.00264 [0.00190]
Citizen Liberalism$_{t-2}$	−0.00288*[0.00135]	−0.00303*[0.00136]	0.000377 [0.000895]	0.000298 [0.000900]	−0.000470 [0.00188]	−0.000530 [0.00188]
ΔUnemployment Rate$_t$	−0.0306** [0.0110]	−0.0319** [0.0109]	−0.0218** [0.00783]	−0.0222** [0.00781]	−0.0110 [0.0155]	−0.0127 [0.0155]
Unemployment Rate$_{t-1}$	−0.0129 [0.00723]	−0.0134 [0.00717]	−0.0120*[0.00516]	−0.0121*[0.00514]	−0.0192*[0.00949]	−0.0202*[0.00951]
ΔCollege Education$_t$	−0.0120*[0.00482]	−0.0117*[0.00480]	−0.00454 [0.00272]	−0.00444 [0.00273]	−0.00556 [0.00677]	−0.00497 [0.00675]
College Education$_{t-1}$	−0.0233** [0.00522]	−0.0230** [0.00517]	−0.00582 [0.00315]	−0.00567 [0.00315]	−0.0202** [0.00683]	−0.0193** [0.00677]
ΔBlack Population$_t$	0.0967*[0.0464]	0.101*[0.0465]	0.0704 [0.0367]	0.0724*[0.0367]	−0.0189 [0.0597]	−0.0169 [0.0593]
Black Population$_{t-1}$	−0.00697 [0.0118]	−0.00816 [0.0118]	−0.00370 [0.00780]	−0.00429 [0.00761]	0.0472** [0.0151]	0.0497** [0.0149]
Hispanic Population$_{t-1}$	−0.0277** [0.00487]	−0.0268** [0.00475]	−0.0198** [0.00409]	−0.0193** [0.00409]	−0.0205** [0.00677]	−0.0170*[0.00663]
Social Capital$_{t-1}$	−0.0156 [0.0122]	−0.0175 [0.0125]	−0.0253** [0.00860]	−0.0264** [0.00872]	—	—
Physicians Per Capita$_{t-1}$	−0.000966*[0.000480]	−0.000845 [0.000484]	−0.000577 [0.000350]	−0.000512 [0.000351]	−0.000469 [0.000685]	−0.000384 [0.000686]
Lagged Level of Dependent Variable	−0.401** [0.0414]	−0.408** [0.0419]	−0.257** [0.0350]	−0.261** [0.0349]	−0.703** [0.0519]	−0.706** [0.0518]
R^2	0.304	0.305	0.366	0.367	0.394	0.366
N	1392	1392	1392	1392	1392	1392

Note: Cell entries are slope coefficients and (t-values) for all independent variables that exhibit at least one statistically significant coefficient for models 1–6. The full table of results, including estimates for the remaining control variables, are reported in our online appendix Table A-3 (Supplementary material). Significance tests are based on panel-corrected standard errors. The dependent variable for columns 1–4 is age-adjusted YPLL per 100,000 women (models 1 and 2) and men (models 3 and 4), transformed to standard deviation units. The dependent variable for columns 5–6 is the natural log of the infant death rate. All models are estimated with state and year fixed effects.

$*p < .05$; $^{**}p < .01$; $^{***}\ p < .10$

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How Do Women Legislators Matter? An Analysis of Policy Mechanisms

Our results thus far suggest that WDR leads to a significant reduction in state mortality rates for women, men, and infants. But how do women legislators matter? In the remainder of the article, we explore the mechanisms that connect WDR to premature mortality by applying the theoretical framework depicted in figure 1. We begin with an analysis of the potential mediating effects of several state policies that are broadly representative of the three policy mechanisms represented in figure 1—social regulatory policies, healthcare policies, and policies that affect social and economic well-being.

First, we examine the possible mediating effects of social regulatory policies that seek to incentivize healthy behavior and prevent accidental deaths. All of the policy indicators in this category are taken from the State Health Policy Research Dataset, which is publicly available (Silver and Macinko 2014). We include indicators of two types of laws regulating alcohol use—beer tax rates and an index of DUI law stringency (0–4). We also include an index of state anti-smoking law stringency (0–4) and three state policies which seek to reduce traffic fatalities—seatbelt law stringency (0–2), motorcycle helmet law stringency (0–2), and an index of graduated licensing laws (0–3), which place restrictions on the driving privileges of teen drivers.

Government healthcare programs can directly affect premature mortality by providing or subsidizing access to quality healthcare. The most important healthcare program over which state legislators exercise policymaking authority is Medicaid. Due to the mortality-reducing effects of state Medicaid programs (Sommers et al. 2012), WDR may serve to reduce mortality rates at least partly through the effects of women legislators on the design and funding of state Medicaid policies. To measure state Medicaid policy, we use a measure of state Medicaid spending per capita (in 2014 dollars).

Finally, many studies have found that government programs may indirectly affect public health through their effect on social determinants of health such as poverty, income, and education. We therefore examine the potential mediating effect of two categories of state social policies—welfare and criminal justice. Welfare programs, variously defined, have been found to have a positive effect on population health both in the United States and abroad (Conley and Springer 2001; Kim and Jennings 2009; Gunderson and Ziliak 2015). We measure social welfare policy by using a measure of public welfare spending (per poor person). We also examine the effects of punitive criminal justice policies on state health outcomes. Specifically, we examine the mediating effect of the state imprisonment rate, which scholars have found to be an important predictor of premature mortality for women and infants, presumably due to the financial and emotional stress experienced by partners of incarcerated fathers (Wildeman 2012).

To estimate the mediating effects of these state policies, we utilize the causal steps approach popularized by Baron and Kenny (1986), supplemented by a

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Sobel test of the significance of the indirect effects (Sobel 1982). The analysis proceeds in two steps. First, we estimate the effect of WDR on the nine state policies hypothesized to serve as mediators of the relationship between WDR and premature mortality. For each of the nine policies, we utilize state panel data for the period 1982–2010 and regress each policy on WDR, a lag of the dependent variable, state and year fixed effects, and a set of control variables commonly used in the literature on state policy adoption. [11] As we did for the analyses presented in Table 1, for each policy, we estimate two regressions—one in which the indicator of WDR includes all women legislators and a second model that utilizes our indicator of Democratic women legislators.

The results of these analyses are presented in Table 2. For each indicator of WDR, the table presents the marginal effect of WDR on each of the nine state policies. As with the analyses of state mortality rates presented in Table 1, the version of WDR based on Democratic women legislators has a somewhat stronger effect on these policies than the indicator based on all women legislators. Of the nine policies, Democratic WDR has a statistically significant effect on four policies—state anti-smoking laws, state beer tax rates, graduated licensing laws, and state imprisonment rates. These four variables thus serve as potential mediators of the relationship between WDR and state mortality rates presented earlier in Table 1.

The next step for our analysis is to re-estimate the effect of WDR on state premature mortality, adding these state policy variables to the set of independent variables included in the original models represented in Table 1. If the four state policies that were significantly related to WDR are also found to be related to state premature mortality rates, this provides evidence of mediation. Because the state policies added to the analysis are likely to affect some types of deaths but not others, we utilize two versions of YPLL. First, we utilize a measure of YPLL based solely on deaths due to what the CDC classifies as "unintentional injuries." This includes deaths due to motor vehicle accidents, poisoning, drowning, unintentional falls, and suffocations, among others. The state policies most likely to affect YPLL based on this definition are the laws that address traffic safety (seatbelt laws, helmet laws, graduated licensing laws) and laws regulating alcohol use (DUI laws, beer tax rates).

Our second version of YPLL is based on all other causes of death (other than unintentional injury deaths). These deaths are far more likely to be caused by illness and disease. For models utilizing this version of YPLL, we therefore include Medicaid spending, state welfare spending, state imprisonment rates, anti-smoking laws, and state alcohol laws. We utilize this same set of policy variables for our model of infant death rates as well. Although not all of these state policy variables were significantly related to WDR in Table 2, we nevertheless include them in these analyses to help provide a more complete model specification and minimize the possibility of omitted variable bias.

We test our mediation hypotheses for each measure of premature death by estimating equation 2 below. Equation 2 estimates the effect of WDR, the

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Table 2. OLS Regression Estimates of the Effects of WDR on State Policy Variables, 1983–2010

Policy Variables	All Women Legislators	Democratic Women Legislators
Medicaid Spending	0.492	0.890
	[0.755]	[0.966]
Anti-Smoking Laws	**0.00800***	**0.0121****
	[0.00351]	**[0.00409]**
Beer Tax Rate	**0.000507***	**0.000604***
	[0.000231]	**[0.000238]**
DUI Laws	−0.00348	−0.00304
	[0.00192]	[0.00237]
Seatbelt Law	−0.00598	−0.00445
	[0.00327]	[0.00320]
Helmet Law	0.000601	0.00120
	[0.00106]	[0.00139]
Graduated Licensing Law	0.00237	**0.00512*****
	[0.00205]	**[0.00267]**
Imprisonment Rate	**−0.452****	**−0.499****
	[0.128]	**[0.152]**
Welfare Spending	−5.293	−15.75
	[9.960]	[11.06]

Note: Cell entries are slope coefficients for the effect of WDR (columns) on state policies hypothesized to affect state mortality (rows). All estimates are based on state panel data regressions of each state policy on its lag, WDR, and the following control variables: state citizen and government ideology, the percentage of state legislators that are black, per capita income, the poverty rate, the unemployment rate, the number of physicians per capita (for all but welfare spending and imprisonment rate), and a full set of state and year fixed effects. Panel corrected standard errors in parentheses. The full results are reported in our online appendix (Supplementary material).
$^{*}p < .05$; $^{**}p < .01$; $^{***}\ p < .10$

relevant state policy variables and the full set of control variables utilized in equation 1 on premature mortality.

$$\Delta \text{Premature Mortality}_{i,t} = \alpha_{i,t} + \beta_0 \text{Premature Mortality}_{i,t-1} + \beta_1 \Delta \text{WDR}_{i,t} + \beta_2 \text{WDR}_{i,t-1} \quad (2)$$

The results of this analysis are presented in Table 3. The table presents the predicted effect of a one-standard deviation increase in each policy variable on premature mortality. As can be seen, each of the nine state policies has a statistically significant effect on premature mortality for at least one of our mortality indicators. These findings are consistent with the public health literature and thus provide us with greater confidence in the performance of our models. Sobel tests confirm that the indirect effects of WDR through these four paths are statistically significant. Interestingly, the effect of WDR is reduced but remains

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Table 3. Regression Estimates of the Effects of State Policies on Premature Mortality Rates, 1983–2010

Policy Variables	YPLL for All Causes (~Unintentional Injury)		YPLL (Unintentional Injury)		Infant Death Rates
	Women	Men	Women	Men	
Medicaid Spending	**−0.0528^ [0.0277]**	**−0.0794** [0.0266]**	—	—	0.0250 [0.0344]
Anti-Smoking Laws	**−0.0334** [0.0117]**	−0.00697 [0.00856]	—	—	0.00454 [0.0171]
Beer Tax Rate	***−0.522* [0.241]***	0.243 [0.152]	0.538 [0.304]	0.265 [0.204]	**−0.861** [0.282]**
DUI Laws	**−0.0307* [0.0124]**	**−0.0228* [0.0101]**	−0.0209 [0.0255]	−0.0115 [0.0184]	−0.0296 [0.0225]
Seatbelt Law	—	—	**−0.118* [0.0558]**	***−0.150* [0.0590]***	—
Helmet Law	—	—	**−0.153* [0.0665]**	**−0.179** [0.0444]**	—
Graduated Licensing Law	—	—	**−0.0792** [0.0213]**	−0.0163 [0.0125]	—
Imprisonment Rate	**0.000503** [0.000127]**	0.000128 [0.000084]	—	—	**0.000601** [0.000139]**
Welfare Spending	−0.00241 [0.00499]	−0.00128 [0.00336]	—	—	**−0.0181* [0.00714]**
Democratic Women Legislators	**−0.0355* [0.0179]**	**−0.0170*** [0.00989]**	−0.00622 [0.0279]	−0.00153 [0.0165]	−0.0320 [0.0223]

Note: Cell entries are the effect of state policies on YPLL, based on a one standard deviation increase in the policy variable. Bolded entries are statistically significant at the 0.05 level. All non-italicized estimates represent long-term effects of the independent variable. Italicized entries indicate short term effects (when the long-term effect was not statistically significant). All estimates are based on state panel data regressions of each state policy on its lag, WDR, and the following control variables: state citizen and government ideology, the percentage of state legislators that are black, per capita income, the poverty rate, the unemployment rate, the number of physicians per capita, and a full set of state and year fixed effects. Panel corrected standard errors in parentheses. The full results are reported in our online appendix (Supplementary material).
* $p < .05$; ** $p < .01$, *** $p < .10$

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statistically significant. This suggests that there may be other unmeasured paths through which WDR affects premature mortality. Nevertheless, given that we are able to link the effect of WDR to specific state policies known to have significant mortality-reducing effects, these results provide greater support for a causal interpretation of our initial results.

Revisiting the Effect of State Medicaid Programs

Our mediation analysis uncovered relatively little evidence of a mediating effect of state Medicaid programs. Although Medicaid spending was found to have a significant negative effect on YPLL for men and women, Medicaid spending was found to be unrelated to WDR. However, the relationship between Medicaid spending, WDR and mortality may be more complicated than we have thus far considered. Rather than serving as a simple mediator, WDR and Medicaid spending may instead interact to affect mortality. On the one hand, we might expect the effect of Medicaid on mortality to be conditional on the level of WDR due to the effect of WDR on the content of state Medicaid policy. If women legislators place greater priority on the well-being of women, families and children as the literature suggests, then it is possible that WDR may affect the design of state Medicaid programs in a way that results in a greater emphasis on services benefitting these target groups. Indeed, in our online appendix (Supplementary material), we provide empirical evidence supporting this possibility by showing that just prior to the passage of the ACA, there was a significant relationship between WDR and the generosity of state Medicaid eligibility policies for pregnant women, children, and their parents. As a result, the mortality-reducing effect of Medicaid spending may therefore be greater in the presence of relatively high levels of WDR due to the fact that the prevention of mortality among younger people (i.e., children and their parents) contributes more years of life lost to our measures of premature mortality than do the deaths of older citizens.

We might also expect the effect of WDR to be conditional on the scale of a state's Medicaid program. Specifically, we should expect WDR to have the strongest effect on health outcomes in states with healthcare programs that reflect generous eligibility policies and a broader range of healthcare services, as this provides women legislators with the necessary policy tools to affect health outcomes among children and their parents. Conversely, in states where the role of state government in healthcare provision is more limited, there may simply be fewer opportunities for women state legislators to implement policies favoring women and children due to the competition for resources from other target populations, such as the elderly and disabled.

We can easily test this hypothesis by estimating an interaction effect between WDR and our measure of Medicaid spending. We test this hypothesis for each of our three measures of premature mortality using the ECM specification below in equation 3. If WDR and Medicaid spending interact as we hypothesize, then

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we should expect the coefficient for the interaction term (β_5) to be negative.

$$\begin{aligned}\Delta\text{Premature Mortality}_{i,\text{t}} &= \alpha_{i,\text{t}} + \beta_0\text{Premature Mortality}_{i,t-1} + \beta_1\Delta\text{WDR}_{\text{i,t}} \\ &+\beta_2\text{WDR}_{i,t-1} + \beta_3\delta\text{Medicaid Spending}_{i,t} + \beta_4\text{Medicaid Spending}_{i,t-1} \\ &+\beta_5(\text{AWDR}_{i,t} * \delta\text{MedicaidSpending}_{i,t}) + \beta_3(\text{WDR}_{i,t} * \text{MedicaidSpending}_{i,t}) \\ &+\beta_7\delta\text{StatePolicies}_{i,t} + \beta_8\delta\text{StatePolicies}_{i,t-1} + \beta_9\delta\text{Controls}_{i,t} \qquad (3)\end{aligned}$$

To save space, we simply summarise the results for the interaction effects and leave the detailed results for our online appendix (Supplementary material). As was the case in the additive analyses reported in Table 1, the short-term effects of WDR continue to be insignificant. However, the coefficient for the interaction between the lagged level of Democratic women legislators and Medicaid spending is negative and statistically significant for each model of premature mortality—WYPLL ($p < .01$), MYPLL ($p < .05$), and infant death rates ($p < .01$). Figure 3 graphs the predicted long-term effect of WDR on each measure of premature mortality, across the range of values for our indicator of Medicaid spending. The figure reveals three important insights. First, we can see that the slopes of the lines in each plot are negative. That is, the mortality-reducing effect of women legislators increases in strength as Medicaid generosity increases. Second, for each measure of premature mortality, the effect of women state legislators does not reach statistical significance until some point between \$1,500 and \$2,000 of Medicaid spending per capita. For the last decade of our analysis period (2001–2010), the mean Medicaid spending value was \$2,136. This suggests that in recent years, for close to half the states the effects of WDR through Medicaid were not sizable enough to lead to a significant reduction in premature mortality. We cannot determine precisely why this is the case, but it could be due to the fact that there simply are not enough resources devoted to Medicaid in these states for WDR to have a measurable impact on such broad measures of premature mortality as used in our analysis. However, for states in which the Medicaid program is relatively large, this has provided women legislators with greater opportunity to have a positive effect on their state's health.

Third, and perhaps most significantly, the slope of the interaction term for WYPLL is greater than that for MYPLL. This is evidenced by the difference in the slopes of the lines in the figure for WYPLL and MYPLL and implies that the effect of WDR on women's health, relative to men's health, continues to grow as the scale of the state's Medicaid program increases. At the upper end of the Medicaid spending distribution the total effect of WDR on WYPLL is predicted to exceed the effect for MYPLL by approximately 40 percent—a difference that is statistically significant. However, at the lowest end of the Medicaid spending scale, the effects of WDR on WYPLL are statistically indistinguishable from the effects on MYPLL. We suspect that this result is likely due to the fact that significantly more women are covered by Medicaid compared to men. As such, this suggests that the validity of the targeted benefits hypothesis, which received

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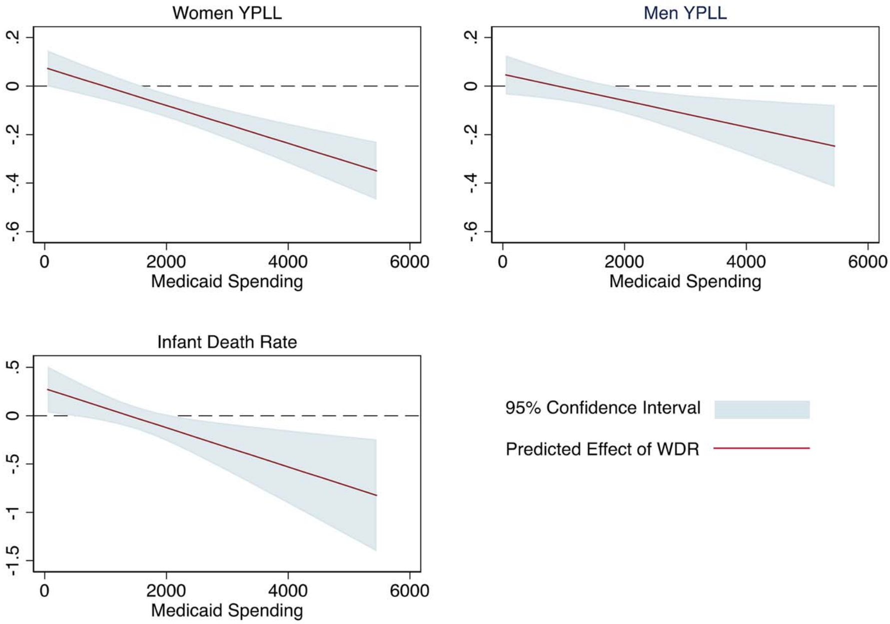
Figure 3. The conditional effect of WDR on state YPLL and infant.



Note: The figures present the predicted marginal effect of WDR on YPLL for men and women and the infant death rate. The full set of results for the underlying regression models are presented in our online appendix (Supplementary material).

equivocal support in our results reported in Table 1, is also conditional on the scale of the state's Medicaid program. It is only in states where the government role in healthcare is relatively larger that WDR seems to provide greater benefits for women, compared to men.

Conclusion

This research represents several significant advances to the literature on the impact of WDR. Most importantly, our analysis adds to a growing body of evidence which finds that the effects of WDR are felt far beyond the legislative chamber and can have important implications for population health and well-being. While previous studies have found this to be true at the cross-national level, our research is the first to provide evidence of this relationship in the United States. Although our conclusions are limited to WDR in state government, given the fact that WDR has been found to influence the policymaking process at the federal and local levels, our results suggest similar effects might also be found for other levels of government. Although we have limited our focus to WDR, it is also possible that the descriptive representation of other under-represented groups, such as African Americans and Latinxs, has had significant consequences

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for the health of these groups. Our findings suggest that this may be an important direction for future research.

Our conclusions regarding the potential impact of WDR comes with an important caveat. Our analyses consistently find stronger effects for the policymaking impact of Democratic women legislators and in many analyses, our indicator of WDR based on all women legislators is statistically insignificant. As the Republican party has grown more conservative and legislators from both parties have become more polarized along partisan lines, it may be difficult in the contemporary political environment for Republican women legislators to form bipartisan coalitions with their Democratic colleagues, regardless of gender and even on issues like healthcare. If this is the case, then this also raises the possibility that there may be similar limitations to the effects of descriptive representation of other minority groups whose members are elected as representatives of both political parties. This would especially be the case for Latinx elected officials.

Our study is also the first to examine the impact of WDR on gender-specific outcomes, allowing us to test more nuanced theories of the relationship between descriptive and substantive representation. We find that the health-improving effects of WDR are not restricted to women, and that men may also benefit from the presence of women in state government. However, our point estimates of the WDR effect are consistently larger for women's health (compared to men's health), and in some cases these differences in the WDR effect are statistically significant. This result supports theories of descriptive representation, which claim that women legislators are not only more likely to promote different types of policies than men, but they also place greater priority on policies that benefit women (Reingold 1992; Swers 1998; Poggione 2004).

We find that the effects of WDR are mediated by a variety of state policies, including state regulatory policies specifically designed to improve health outcomes, as well as state redistributive policies that indirectly affect health outcomes. Our analyses also find that state Medicaid programs can play an important role in the translation of WDR into positive health outcomes. However, the mortality-reducing effect of WDR is only evident in our statewide measures of premature mortality when the size of the Medicaid program is large enough to have a broad impact on the state population. This suggests that in some policy areas, minority legislators may be naturally constrained in their efforts to provide substantive representation due to the limited power of government to effect change.

Given the substantive importance of our results, we hope that our study will reinvigorate debates over policies designed to remedy the severe underrepresentation of women in government in the United States. According to the Inter-Parliamentary Union, which has been collecting data on the representation of women in government for over two decades, the United States ranked 104th out of 190 countries in 2017.[12] Although a record number of women were elected to the US Congress in 2018, it is likely that the United States will continue to lag behind other industrialized countries in the level of WDR. The United States also lags well behind other developed countries with respect to premature death rates.[13] Is it merely a coincidence that the

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US ranks so similarly on these two indicators? Our results suggest that this is no coincidence and in fact, the underrepresentation of women may play a significant role in the health gap between the United States and the other western nations.

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Notes

1. "Women in Elective Office," Center for American Women and Politics. Accessed January 18, 2020. https://cawp.rutgers.edu/women-elective-office-2020.
2. We exclude Alaska and Hawaii due to the unusual characteristics of the social and physical environments, as well as a lack of data for our measure of state social capital (Hawes, Rocha, and Meier 2013).
3. Specifically, we rely on the State Health Policy Research Dataset for many of our control variables. These data are available from 1980 to 2010.
4. The infant mortality rate has long been considered an important indicator of population health (Anderson 1973; Judge, Mulligan, and Benzeval 1998; Patton, Costich, and Lidsrömer 2017; Murphy et al. 2018). Infant mortality rate is also highly correlated with other health indicators such as disability adjusted life expectancy, but its short-term nature (birth to 1 year) makes it "an important indicator of 'here and now' effects" (Reidpath and Allotey 2003, 346) such as policy changes that would affect women's health during pregnancy and subsequent effects on infant health.
5. As an alternative to the measure of state government ideology we also estimate our models with an alternative measure of the general ideological orientation of the state government—the percentage of all legislators belonging to the Democratic party. The results for the effects of WDR on premature

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mortality are substantively similar in these alternative models. For details, see our online appendix (Supplementary material).

6. Although many studies have documented the effect of social capital on mortality rates among the general population, the evidence suggests that this relationship does not hold for infant mortality rates (Muntaner et al. 2002; Yang, Teng, and Haran 2009). Therefore, we do not include social capital in our infant mortality rate models.
7. All political variables, including all measures of WDR, are lagged 1 year prior to being included in the ECM model.
8. To conduct this test, we first estimated our two YPLL models simultaneously, as a seemingly unrelated regression model. We then conducted a statistical test of the equality of the coefficients. The *p*-value for this test was .053.
9. Because we estimate our infant death rate models using the natural log of infant death rates, the coefficient estimates represent the proportional change in the dependent variable given a one unit increase in the independent variable (rescaled to standard deviation units).
10. In addition to these results estimated by OLS, we also estimated the models in Table 1 using two-stage least squares to address the potential endogeneity of WDR. Although our diagnostics indicated that our instrument for Democratic WDR (the percentage of state legislators elected from single-member districts) performed well, based on Hausman tests of endogeneity we could not reject the null hypothesis of exogeneity for any of our dependent variables. Therefore, we continue to rely on OLS for the remainder of our analyses. A detailed description of the 2SLS analysis, including a defense of our choice of instrument, is provided in our online appendix (Supplementary material).
11. These control variables include state citizen and government ideology, the percentage of state legislators that are black, the unemployment rate, the poverty rate, the percentage of the state population that is black and Hispanic, and the number of physicians per 1000 residents. We utilize OLS for all regressions and ECM's for all dependent variables determined to be non-stationary. The full results are presented in our online appendix (Supplementary material).
12. "Women in National Parliaments," Inter-Parliamentary Union. Accessed May 1, 2019. http://archive.ipu.org/wmn-e/arc/classif010117.htm.
13. Bradley Sawyer and Daniel McDermott, "How do mortality rates in the U.S. compare to other countries?" Kaiser Family Foundation. Accessed May 1, 2019. https://www.healthsystemtracker.org/chart-collection/mortality-rates-u-s-compare-countries/#item-start.

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Supplementary material

Supplementary material is available at *Social Forces* online, http://sf.oxfordjournals.org/.

Appendix

Table A1. OLS Regression Estimates of the Effect of WDR on Medicaid Eligibility Policies, 2008

Independent Variables	Children and Parents		Elderly and Disabled	
	(1)	(2)	(3)	(4)
Democratic Women Legislators	0.0635**	0.0374*	0.0018	−0.0017
[0.0141]	[0.0177]	[0.0027]	[0.0037]	
Government Liberalism	—	−0.0012	—	−0.0001
		[0.0049]		[0.0010]
Citizen Liberalism	—	0.0212*	—	0.0026
		[0.0084]		[0.0018]
Per Capita Income	—	0.0200	—	−0.0045
		[0.0207]		[0.00435]
Poverty Rate	—	0.0073	—	−0.0135
		[0.0491]		[0.0103]
Percent Black	—	0.0087	—	0.0001
		[0.0106]		[0.0022]
Percent Hispanic	—	0.0012	—	0.0023
		[0.0115]		[0.0024]
N	50	50	50	50
Adj. *R*-squared	0.28	0.37	−0.01	−0.04

Note: For models 1 and 2, the dependent variable is a Medicaid eligibility policy index, constructed based on factor analysis of the maximum income eligibility limits for: pregnant women, children 6–18, and adult parents of eligible children. For models 3 and 4, the dependent variable is the income eligibility limit for elderly and disabled recipients, as a proportion of the federal poverty line. Cell entries are unstandardized slope coefficients.

* $p < .05$, ** $p < .01$

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