Efficiency wages for politicians:

Do better paid elected officials produce better outcomes?

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"A raise is a reward, and on the most important issues of balancing the books, they've failed." Joe Sensenbrenner, mayor of Madison, Wisconsin

"They've cut social programs down to the bone and now they want to give themselves a raise." Kathy Bernier, family counselor from Boston

It's "idiotic to have people in charge of a budget of trillions of dollars and pay them nickel and dime." Dave McNeely, political columnist for the Austin (Texas) American-Statesmen

"The truth is they don't get paid enough, and the danger is that if we don't change the pay system we'll only have people who can afford to take these jobs." Frank Quinn, fire commissioner from San Francisco

Quoted in USA Today, Jan. 10, 1989

Periodically, Congress must pass legislation raising members' salaries. As the quotes above indicate, the issue of raises for elected representatives is a contentious one. Congressmen are put in the position of justifying pay increases. An argument in favor of higher salaries is that without them the best people will have little incentive to run for office. Low wages for elected officials, and for judges and many other positions in the Federal government, would result, the argument implies, in reduced effectiveness of government. This paper is an attempt to assess that argument.

The fundamental argument is one of efficiency wages (Akerloff, 1982; Shapiro and Stiglitz, 1984; Yellen, 1984). The legislators or governors must be paid enough to keep them happy in their current position and to compensate them for foregoing otherwise highly lucrative careers in the private sector and to attract the best and brightest from the private sector into government service. Of course, those arguing for higher salaries neglect the other perquisites of elective office, amenities that private sector jobs would not have. They also seem not to grasp the irony of arguing that current politicos aren't paid enough to attract high quality candidates for

elective office.

That there is a link between salaries and performance of legislators is a common theme in the popular press.[1] For example, Edwin Yoder (1989) argues that one of the best arguments for higher salaries is that higher pay would reduce corruption. He suggested that had congressmen been paid more they would not have been so willing to intervene with Federal regulators on the behalf of Charles Keating of the Lincoln Savings and Loan. While the logic of his argument is suspect, because Keating made campaign contributions over which the recipients have far less discretion than salary, the idea that compensation influences behavior on the job is very much like the efficiency wage story of Shapiro and Stiglitz (1984). Workers paid above their marginal product are less likely to shirk for fear of losing a job better than any other they are likely to get. Indeed, Becker and Stigler (1974) make the connection to law enforcement personnel explicit, writing "trust calls for a salary premium not necessarily because better quality persons are thereby attracted, but because higher salaries impose a cost on violations of trust." McCormick and Tollison (1981) extend the argument to elected representatives, "politicians will be less corrupt if they are paid more, and voters may view legislative determination of pay as a method of controlling malfeasance among politicians."

The Yoder position does not link compensation of elected officials with any specific policy objectives. An editorial (or letter to the editor) from the *St. Louis Post-Dispatch* in 1991 makes such a link. Specifically, the writer proposed an amendment to the U. S. Constitution stating that

[1]Squire (1993) also finds a link between salaries and the public opinion of legislators. Professionalism of legislatures is related to compensation, length of legislative sessions, and whether the individual can make a career out of service. More professional legislatures pay higher salaries but, according to Squire's regression analysis, those legislatures in which professionalism is higher are held in lower regard than their more amateur counterparts.

"The members of the House shall receive an annual salary of $75,000 a year (senators $85,000) with a year-end cumulative bonus of 1 percent for each 1 percent reduction in the outstanding national debt, and a 2 percent bonus for each 1 percent reduction in the annual tax rate." Why the writer thinks the goals of reducing the national debt and the tax rate should take precedence over stable prices, low unemployment and rapid growth is not known.

An editorial in the *Tampa Tribune* (1997) suggested a link between raises for legislators and the rate of growth of household income. At a time when Congress proposed to raise its salary by more than $3000, about a 2.3% increase, the editorial reports that the median income of households rose only 1.2%, about $410. The editorial says, "A less controversial way to raise lawmakers' pay would be to link future increases to changes in the national family median income." The editorial concludes, "Congress should proudly give itself the raise the economy says it deserves - $410. More money will surely be available if lawmakers are *more productive next year*." (Emphasis added.) The editorialist clearly thinks that lawmaker productivity is directly linked to real income growth. It is also clear that the writer believes this link to be without time lag.

Whether a consensus could develop around which desirable outcomes are linked, and in what way, to bonuses or routine salary increases is unknown. What is known is that members of Congress have become quite unwilling to openly and honestly vote themselves a salary increase. Inflation adjustments had been made automatic, but in recent years those automatic increases have been blocked by opponents who attach riders to spending bills. Additionally, many members of Congress have gone public with their refusal to accept pay increases. In late 1997 an attempt to block a scheduled increase failed. The resulting raise amounted to 2.3% of the base salary, or

about $3000. A February 1, 1998, *Milwaukee Journal Sentinel* article reported that 11 of Wisconsin's federal legislators were not going to keep the increase for themselves. Several directed the raise go to the Bureau of Public Debt to reduce the national debt. Others intended to donate all of their raises to charities. Representative Jerry Kleczka suggested that the pay increase was an easy issue for people "to demagogue". He said, "There are those who think we should wear hair shirts also."

With all of this acrimony and debate about the appropriateness of Congressional pay increases, it seems natural to ask whether there is any link between salaries and performance, especially of broad measures of public welfare. Specifically considering the rate of economic growth as the measure of effective government, this paper seeks to explain differences in economic growth across states using salary data on state legislators and governors.[2] If the argument for higher Federal salaries is correct, then one would expect to find a similar relationship at the state level. Support for the argument that higher wages draw higher quality candidates and, presumably, higher quality elected and appointed officials, would be a finding that states that pay higher salaries experience faster growth in real income, all other things constant. In this paper, growth regressions are estimated in a variety of ways to assess the merits of the higher wages for elected officials as a means of fostering better outcomes.

The organization of the paper is as follows. The next section II briefly reviews some of the literature on economic growth. Section III discusses the hypotheses, how they will be tested, and elaborates on the empirical approach. Section IV describes the data and section V presents

[2]One could certainly debate the appropriateness of the rate of growth as the best measure of public welfare. Future work could extend the analysis to more fundamental measures of well-being such as infant mortality rates, drop-out rates, and etc.

the results. A brief conclusion ends the paper.

II. Literature Review

In the last 15 years a large body of literature has developed on the determinants of economic growth. An early contributor to this literature was Mancur Olson whose book *The Rise and Decline of Nations* (1982) linked slow economic growth to the development of interest groups. These interest groups worked to capture a larger share of the national output, but at the same time contributed to the slowing of the expansion of that output. A variety of researchers have tested the Olson hypothesis but few have found compelling evidence in its favor. (Wallis and Oates, 1988; Nardinelli, Wallace and Warner, 1987; Choi, 1983; Murrell, 1984; Pryor 1983; Gray and Lowery, 1988; McCallum and Blais, 1987)

Nardinelli, Wallace and Warner (1987) estimate equations which include the level of income and the number of years since "statehood" to test Olson's "institutional sclerosis" hypothesis against "catching up" (what Barro and Sala-i-Martin (1995) call absolute convergence). Olson argues that the longer the period of stability, with no disruption in the social institutions, the more interest groups there will be. Over time, the accumulation of these groups leads to their becoming ever more harmful to the economy. Hence, there is a link between time since statehood and growth. Catching up, on the other hand, is the recognition that some poor countries grow rapidly while rich countries grow slowly. The Nardinelli, et al., results provide no evidence in favor of institutional sclerosis but strongly support catching up.

Wallis and Oates (1988) test the Olson hypothesis on a panel of state income data for the period 1902-1982. The original idea was to test whether older states had larger state and local

governments. Using a panel data framework, they find that government size, as measured by state and local revenues as a percentage of per capita income or state and local expenditures as a percentage of per capita income, is smaller in the older states. This is opposite what one would expect given the Olson hypothesis. Wallis and Oates then turn to testing the Olson hypothesis directly with their panel of data. They find that support for the Olson hypothesis is weak, that support for the opposite case is much stronger. Namely, older states tend to grow faster than younger states in their data, at least when the growth is measured in per capita terms. Only if growth is measured on total income and the Confederate states are aged from the Civil War do they find support for the Olson hypothesis.

The literature also began to focus on the importance of fiscal institutions for growth. For example, Canto and Webb (1987) find support for the hypothesis that differences is state and local tax and spending policies influence the growth rate of state per capita personal income. Helms (1985) finds that tax increases that fund transfer payments are harmful to growth while taxes which finance local public services enhance growth. Yu, Wallace, and Nardinelli (1991) assess the relative importance of fiscal policy and "catching up" as explanations for the different rates of growth among the American states. Their analysis is not conclusive, though the evidence does suggest that differences in fiscal policies across the states appear to be less important than catching up. They do find that in some specifications for some time periods the ratio of state taxes to state income at the start of the period is statistically significant. This is especially true of the period 1975-1985, when the share of state taxes is statistically significant but initial income is

not.[3]

Scully (1988) uses data on 115 countries to assess the importance of liberty for economic growth. He finds that societies with closed political systems, with command economies, and little individual freedom grow more slowly, and are less economically efficient, than are those with open political systems, market economies, and extensive civil liberties. Landau (1985) and McCallum and Blais (1987) also provide empirical evidence on the role of government and economic growth in a cross-national setting. McCallum and Blais find some support for the Olson hypothesis but what is more interesting is that they also find evidence that countries with large social safety nets grow more rapidly than others.

More recently, Persson and Tabellini (1994) have argued that maldistribution of wealth has harmful repercussions on economic growth. They contend that distributional conflict results in taxes on investment and other growth promoting activities. These policies slow economic growth. They present empirical evidence supportive of their theoretical model.

Partridge (1997) extends the work of Persson and Tabellini (1994) on the effects of inequality on growth. While Persson and Tabellini examine cross-country differences, Partridge looks to differences across the states of the United States. He finds that state fiscal variables have no effect on differences in growth rates but finds that the Gini coefficient and the share of income going to the middle quintile are each positively related to growth. The former result is opposite

[3] Mofidi and Stone (1990) relate state and local tax and spending variables to employment in manufacturing and to net investment. They find that the nature of the government spending matters for whether taxes inhibit or encourage net investment and manufacturing employment. Carroll and Wasylenko (1994) perform a similar analysis. They find that the fiscal variables have a stronger impact in the 1970s than in the 1980s. Neither of these studies assesses the effects of these variables on growth of income directly.

that found by Persson and Tabellini while the latter result is consistent with Persson and Tabellini. Growth is measured over ten year spans with the explanatory variables taking the values of the first year in the span.

Knack and Keefer (1997) examine the role of "social capital" in determining economic growth in a cross-section of countries over the period 1980-1992. Social capital they operationalize as the tendency for people to trust others and for people to be civic-minded. They find that in those countries where trusting "people" is more prevalent growth is faster. Similarly, in countries with more civic mindedness growth is faster. The paper delves into the mechanism by which trust and civic mindedness influence growth. Knack and Keefer find that both variables are lower the greater is income inequality, suggesting that some of the evidence on the deleterious effects of inequality are the result of low levels of social capital. Moreover, social capital (trust and civic-mindedness) are also found to be higher where secondary education is greater, suggesting a linkage between education and growth that had not been exploited before.

McCormick and Tollison (1981) studied the market for legislators in great detail. One conclusion of their analysis is that the wages of legislators are dramatically larger when the legislature sets its own wage, subject to governor approval, than when the wage is set in the state constitution. In the lower houses, this difference amounted to $11,455 in 1974. The difference in the state senates was even greater, $12,875. McCormick and Tollison argue that this pay differential reflects the effects of monopoly power, when legislators determine their own wage, relative to competitive markets. They assert that under the monopoly power scenario, because the wages of legislators will be higher, that "more able individuals will compete for legislative seats" (p. 66).

III. Empirical approach

The empirical analysis consists of two complementary regression approaches. An implication of the efficiency wage literature is that well-paid workers will work harder. The first empirical model attempts to assess this implication for the state legislators. Taking as a measure of effort of legislators the number of bills before them during the legislative session, on the one hand, and the number of bills enacted, on the other, the efficiency wage model would suggest that well-paid legislators produce more bills and more enactments, all other things held constant.[4] The efficiency wage literature also suggests that well paid workers are more productive. The second empirical approach relates salary to a measurable outcome, specifically the rate of growth of personal income per capita. The rest of this section expands upon these different approaches.

Assessing legislator work effort is a difficult conceptual and practical problem. One could expect the number of bills before a state legislature to increase with a) the length of the legislative session and b) the size of the state. One might also expect that the number of bills before a legislature, and how many of them are enacted, to depend upon the party in control of the two houses of the state legislature and the governorship. In this case, however, whether the number of bills rises under Democrats or Republicans is not intuitively clear. The analysis also includes year and state specific effects, to control for any effects common across states at a point in time or consistent over time within a given state.

The length of the legislative session may be endogenous, the session getting drawn out so

[4]One could easily object to these measures of legislator effort. Certainly legislators spend time in constituency service, in committee meetings, lobbying other legislators and the governor, and on other activities as routine parts of their job. Meaningful numerical variables that capture these activities over the time span for each of the 50 states of the United States do not, to my knowledge, exist.

that more of the pending bills come up for a vote. One would expect the legislatures that serve year round to contend with more legislation than their counterparts that sit for only a few weeks. In any given year, whether the state legislature sits for the entire year or for only a few weeks or two or three months is not endogenous. In other words, the extent of professionalization (high compensation and full time service) of the legislature is not determined in a given year by the number of pending bills. Consequently, rather than use the length of the legislative session to explain the number of bills or enactments, a dummy variable for full time legislators is included.

Finally, the principal variable of interest in this analysis is the compensation of the legislators. One approach is to simply include the real value of the legislator salary as an explanatory variable. This approach is not entirely satisfactory, however, because the same real salary in one state may be quite small compared to the level of income of the citizen's in the state whereas it could be quite high compared to the average income in some other state. Consequently, an additional approach is to use the legislator's salary relative to the real income per capita. In either case, the hypothesis is that where legislator salaries are higher, more bills come before the legislature and more bills are enacted.

A formal representation of this model is:

$$Legislation_{st} = \acute{a}_0 + \acute{a}_1 W_{st} + \sum_{i=2}^{K} \acute{a}_i \, X_{ist} + \mu_{st}$$

where the á's are parameters to be estimated, μ is a random error, X_{ist} is the ith independent variable in state s at time t, and W_{st} is the real salary of the legislators, either in level or relative to state mean income, in state s at time t. Support for the efficiency wage model is evidence that $\acute{a}_1$

> 0.

The second empirical model for testing the efficiency wage theory uses the growth rate of real per capita personal income as a measure of legislator and governor effectiveness or productivity.[5] To be meaningful, the empirical model of real personal income per capita must take account of the major influences on growth developed formally. Barro and Sala-i-Martin (1995) summarize the determinants of growth from the endogenous-growth literature. Among these are governmental actions such as: "taxation, maintenance of law and order, provision of infrastructure services, protection of intellectual property rights". In addition, those factors which influence the growth rate of the population and the diffusion of new technology are also important.

In the empirical work that follows, many of these factors are assumed to be constant across states. The individual states of the United States are, it is assumed, far more similar than dissimilar along many of these dimensions. The states are certainly more alike than are, say, the United States and Japan, Germany, and Mexico, all countries that are frequently in the typical cross-national studies.

The basic equation estimated in this paper is as follows:

$$g_{st} = \acute{a}_0 + \acute{a}_1 Y_{s(t-1)} + \sum_{i=2}^{k} \acute{a}_i \; X_{is(t-1)} + \sum_{j=1}^{J} \hat{a}_j W_{js(t-1)} + \mu_{st}$$

where g_{st} is the growth rate of personal income in state s in period t, the á's and â's are parameters to be estimated, $Y_{s(t-1)}$ is the level of personal income in state s at the start of the

[5]The nature of the linkage between salaries and economic growth is not specified.

period t, and the $X_{is(t-1)}$ are institutional, regional, state, or time variables, and the $W_{js(t-1)}$ are the legislator and governor salaries in state s at time t-1, or variables which capture idiosyncracies in the computation of the legislator salaries. The parameter $á_1$ is a measure of the speed of conditional convergence, conditional because the $X_{is(t-1)}$ capture differences in the steady state level of personal income across the states.

The fundamental question of this paper is whether salaries of legislators and governors influence the rate of economic growth. The null hypothesis is that these variables both have no effect, that $â_1 = â_2 = 0$. The alternative hypothesis is that at least one of the variables has some effect.

The efficiency wage story described above makes a more specific hypothesis. If that story is true, then one would expect to find $â_1 > 0$, or $â_2 > 0$, or both are greater than zero. Negative or zero coefficients is evidence against the efficiency wage story.

A difficulty with this approach is that the lag with which the legislator and governor salaries influence the health of the economy is unknown. Certainly the contemporaneous salaries are not correct, for they can hardly attract the better qualified this year.[6] Moreover, current year salaries are likely to be jointly determined with current outcomes, in a good year legislators vote themselves larger pay increases than in a bad year. In the analysis that follows, when the time period of analysis is five or more years it is assumed that the salary at the start of the period determines the period's growth. However, for the annual data, salaries are lagged one and two

[6]Current high salaries may induce greater effort, less shirking, on the part of sitting officials with attendant benefits to the state, as discussed above. I regress bills and bills enacted against salaries as a means of assessing this possibility. Results of these regressions are reported in Tables 3 and 4 and discussed below. Whether more bills or more enactments translate into current period growth is an empirical question.

years. The evidence is that it makes little difference, so the reported results are for the two year lags.

The hypothesis is tested in a variety of more specific models related to the time period of the analysis. For example, the period of the analysis could be the entire span of years covered by the data, 1971-1989. If either $\hat{a}_1$ or $\hat{a}_2$ is non-zero in this case, then the 1971 real salary of the elected official or officials in question is correlated with the rate of growth in real personal income per capita from 1971 to 1989. Alternatively, the period of the analysis could be (roughly) ten years, 1971-81 and 1981-89, making a time-series cross-section analysis possible. The period of the analysis can be, and is, further shortened to roughly five year spans and ultimately to one observation per year.

The longer time spans are, obviously, more like the long-run. Just as obviously, growth is a long run phenomenon. However, this limits the analysis by allowing the researcher to use few variables, because of small degrees of freedom. More importantly, if the explanatory variables take on the values at the start of a long time period, one biases the results away from finding any effect. In the current context, it seems highly unlikely that the rate of growth in the latter part of the 1980s was significantly determined by the fraction of a state's population that was, say, a college graduate in 1971. That is not to say that no effect will be found, for that percentage may well be persistent over time. But if an effect is found one cannot be sure if it is because the value for the start of the period was truly instrumental in the growth or whether it merely was a good instrument for the true variable. Additionally, if the time periods are long the researcher implicitly assumes that the process linking growth and the explanatory variables has been stable over that long period. A stable relationship over five years seems more plausible than a stable relationship

over twenty years or more.

The shorter the time frame, the more observations available, but the more tenuous the link to long run growth. However, with shorter time periods one can account for more of the variation in the cross-sectional units via more explanatory variables, including year and state specific fixed effects. It may be that the individual states are comparatively more alike than the different countries of the world, but that does not mean that the states are not sufficiently different in their institutions, histories, cultures, major agricultural and other products, that some of their growth experience is unique.[7] The year specific effects capture any shocks to growth that are common across states in a given year, reducing the concern that year to year changes cannot address long run growth.

One advantage of the analysis of the 10 year periods over that of the full 19 years is that the 1970s and 1980s may differ substantially in an important way related to state politicians. One difference may be that legislators became more "professional", especially over the 1970s, where professional legislatures are those that "meet in unlimited sessions, pay their members well and provide superior staff and facilities" (Squire, 1992). For example, Morris Fiorina (1994) reports that the number of state legislatures with annual sessions jumped from about 40% of 31 nonsouthern states in 1970 to about 75% in 1972, and over 80% by 1980.[8] On the other hand,

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[7]Fixed effects in a dynamic panel data model are biased. Monte Carlo evidence suggests that this bias is limited to the coefficient on the lagged dependent variable used as a regressor and that this bias falls as the number of time periods rises. Additionally, the coefficient on the lagged dependent variable is biased down. (Judson and Owen, 1997; Kiviet, 1995)

[8]The states omitted from Fiorina's data resulting in 31 non-southern states include Nebraska, Minnesota, Alaska and Hawaii, as well as the southern states. Nebraska and Minnesota are omitted because they held non-partisan elections throughout much of Fiorina's period. Alaska and Hawaii are omitted because they did not hold state elections prior to becoming states in 1958.

real biennial compensation, averaged over these 31 states, was stagnant during much of the 1970s, but rose rapidly from 1982 til 1988, faster than real per capita income judging from his Figure A-3. More is said about legislator and governor salaries in the data section that follows.

IV. Data

The data set for this paper brings together information from "The Book of the States" and "The World Almanac" to construct a time series of salaries for state governors and state legislators. The data set covers the period 1971 to 1989. Table 1 provides descriptive statistics and definitions of the variables. Illinois is omitted from some of the analysis because legislator salary data are missing for some years. In order to use lagged values of some variables as explanatory variables data for 1971 is sometimes omitted. The full data set then consists of 882 data points, 18 for each of 49 states.

The growth rate of real personal income per capita (ratepc) is the difference in the natural logarithms of current and previous (or starting) period income.

The legislator salary data is especially varied. Several states pay an annual salary while other states pay a set rate per day for each day the legislature is in session. Some states also pay an additional per diem on top of this salary and still others pay a flat fee combined with a daily rate. Salary data for the legislators of each state are computed as the sum of any fixed payments plus any per diem multiplied by the number of days either the legislature was in session or the number of days from the start to the end of the legislative session, depending upon the institutions for the individual state. A complete accounting of these institutional details is available upon request. Salary and personal income variables are deflated by the CPI.

Table 2 shows for legislator and governor salaries, and the number of days the legislature is in session, both by the calendar and by actual days open, how many states had upward or downward trends, or no trend from 1971 til 1989. In most cases there is no trend in legislator salaries, though among those with a trend real salaries are about twice as likely to decline over time. Real gubernatorial salaries decline over the period in 60% of the states while they increase in only 3 of the 49 states in the sample.

Looking only at the 1971-1979 period, real legislator salaries rose in just 1 state and fell in 6 based on p-values for the trend variable less than .05. In 1 additional state the p-value is less than .10 (and greater than .05) and the trend positive; one state had a negative trend with a p-value between .05 and .10. For governor's salaries, 3 states had positive trends at the .05 level of significance and 2 more were significant at the .10 level. In 20 states the governor's salary declined over the decade, the trends significant at the .05 level, with 2 additional states having negative trends significant at the .10 level.

By contrast, during the 1980-1989 period, real legislator salaries rose in 12 states and fell in 5 based on p-values for the trend variable less than .05. In 4 additional states the p-value is less than .10 (and greater than .05) and the trend positive; one state had a negative trend with a p-value between .05 and .10. For governor's salaries, 11 states had positive trends at the .05 level of significance and 4 more were significant at the .10 level. Four states had negative and significant trends (at the .05 level).

Finally, checking for a difference between decades by state one finds that in ten states the legislator salary trend is negative and discernibly different between the 70s and 80s, that 8 had the same positive trend in each decade and 27 exhibited no trend in either decade. Two each had a

more negative trend in the 80s and a more positive trend in the 70s. Turning to governor's salaries, 24 had the same negative trend in each decade, 4 had a stronger negative trend in the 70s and 1 a stronger negative trend in the 80s. In six states the positive trend was constant between decades and in 3 the positive trend was greater in the 1970s. In one state the trend was positive in the 1980s but negative in the 70s. Ten states exhibited no trend.[9]

Comparing the results for legislator and governor salaries the contrast is clear. Throughout the period of the sample, real governor's salaries have tended to fall in more states than have real legislator salaries, and have tended to rise in fewer. Put differently, the variation in real legislator salaries across time is somewhat limited, finding discernible trends in only about half the states. Governor's salaries, on the other hand, vary over time in upwards of 60% of the states, with the trends being predominantly negative.

Additional variables in the analysis include separate dummy variables for whether the state has an income tax, a sales tax, a corporate income tax, severance taxes, or a lottery. Also, dummy variables indicate whether the state governor has veto authority, whether the governor is a Democrat, and if the state legislature is controlled entirely by the Democrats. There are also regional, state and year specific dummy variables. The regional dummies correspond to the census regions.

In order to facilitate comparison with published work, I have also combined the salary data with the data of Mark Partridge (1997) for the 1970-1980, and 1980-1990 periods. For example, the legislator salary in 1971, the earliest year I have, is paired with his data for the

[9]Performing a similar analysis on the ratio of the officials' salaries to real per capita personal income increases the number of negative trends in both the legislative and gubernatorial salaries.

decade of the 1970s; salary data for 1980 is paired with his data for the decade of the eighties. Table 7 provides descriptive statistics for the Partridge data.

V. Results

The analysis begins by examining the relationship between legislator salary and the number of bills before the body and the number of bills enacted. If high salaries induce greater work effort then one would expect to find higher salaried legislators writing more bills, and possibly enacting more legislation. Tables 3 and 4 report results of this analysis.

Each of Tables 3 and 4 is estimated as a cross section time-series model with state-specific effects.[10] Table 3 shows that there is no statistically significant effect of the legislator salary on the number of bills. Additionally, there is no difference between Democratically controlled and other legislatures. However, the length of the session is significant, both in the number of days the legislature is at work (excluding holidays, etc.) and the number of days from the start til the end of the session. The reported results are from a random effects model, because a Hausman test could not reject random effects.

Table 4 tells a different story. The number of bills enacted increases with the real salary of legislators. Statistical and practical significance, obviously, need not simultaneously occur. In the

[10]One could argue that the intercept should be constant across the states but that the models suffer from heteroskedastic errors. When the model is estimated this way legislator salary becomes a significant, and positive, determinant of the number of bills before the legislature. Democratic control of the legislature and governorship become statistically significant positive influences both on the number of bills and the number of enactments. The number of days the legislature is open for business remains positive and significant, but the number of days from start to finish is negative and significant in the bills equation and insignificant in the enactments equation. These results are available on request.

current analysis three hundred extra dollars of salary for the legislators results in about one additional piece of legislation enacted. There remains no difference between Democratic and non-Democratic control of the legislature or the governorship. The length of the session is positively related to the number of bills enacted, an additional day of work for the representatives generating about 2 additional enactments. Interestingly, one cannot reject random effects in favor of fixed effects for the number of enactments.

Recognizing that state size may influence the number of bills and enactments, that legislator salaries relative to the average level of income may better capture the incentive effects implied by the theory, and the possible endogeneity of the length of the session variables, both the models were re-estimated. The results are different essentially only by the salary variable becoming significant in the bills equation. Finally, the salary variable is interacted with the dummy variables indicating full time legislators. In this case the salary variable is still significant and positive, but the interaction variable is significant and negative. This is true in both the bills and the enactments equations.[11]

To summarize, the evidence from the legislator effort equations suggests that higher paid legislators both write and pass more bills. This is weak evidence in favor of the efficiency wage theory of legislator behavior.

Turning to the growth equations, the analysis is broken down into different time periods for study. First, the growth rate over the entire period, 1971-1989, is explained using the value of salaries in 1971, the value of income in 1971, and regional and institutional dummies. Second, the analysis splits the period in two, 1971 to 1981 and 1982 to 1989. The explanatory variables are

[11]These results are available upon request.

those of the first approach. Next, the period is broken down still further. Finally, the data for the annual growth rates are explained.

The first model relates the growth rate of real personal income per capita (Table 5) over the period 1971-1989 to regional dummy variables, the value of real personal income (total or per capita) in 1971, the real salary of state legislators in 1971, the real salary of the state governor in 1971, the number of days the legislature was in session in 1971, and dummy variables indicating whether the state had a lottery, an income tax, a corporate income tax, a sales tax, severance taxes, or if the governor had veto power. Three regional dummies are individually significant at better than the 5% level, but neither salary variable, or any other variable, is close to significant at conventional levels. This is evidence against the efficiency wage theory.

Subsequently, I split the sample in two, computing growth rates for the period 1971-1981 and 1981-1989 (Table 6). The relationship between growth and legislator and governor salaries was reestimated for this data. As before, the explanatory variables take the values for the first year over which the rate of growth is computed. In this case, legislator salary is insignificant while governor salary is. Moreover, governor salary enters with a negative sign, indicating that a state that pays its governor more experienced slower growth in per capita personal income.[12]

If the state had a lottery at the start of the period then growth of personal income per capita is faster. No regional dummy is significant.

Partridge (1997) estimated the growth rate of states over the period 1960-1990 to assess the importance of income inequality on growth. His data for the period 1970-1990 are combined

[12]The model was also estimated including the starting value of the government's share of total employment. While this variable was statistically significant, its inclusion had little effect on the other coefficients.

with the legislator and governor salary data and the institutional variables from above and his models reestimated. Descriptive statistics and variable definitions for his data are in Table 7, estimation results in Tables 8 and 9.

Table 8 reports the results of replicating Partridge's base model except that the data for the decade of the 1960s has been removed, which required dropping the dummy variable for the 1980s. The 1960s were dropped for this analysis because the salary data begins in 1971. Additionally, Partridge's analysis uses only the 48 contiguous states, so Alaska and Hawaii are not included in the analysis reported in Tables 8 and 9 though they are present in the analysis reported in earlier tables. Note that the proportion of the population that are college graduates, the level of real per capita income, the 1970s dummy, the construction and mining shares, and the west and midwest dummies are each individually statistically significant in Table 8. These results are fully consistent with those reported by Partridge in his Table 1.

The gini coefficient here is not statistically significant, though it is in Partridge's reported results. This indicates that Partridge's results are sensitive to inclusion of the 1960s. Stated differently, Partridge's conclusion that income inequality influences the rate of growth may only be true for the states over the 1960-1990 period but not true for other periods. The finance, insurance and real estate variable is also significant in Partridge's analysis but not here.

Table 9 presents the results when the salary variables are included as regressors and institutional variables are added to the equation. Governors salary has a negative coefficient and the presence of a state lottery at the start of the period a positive one. Both are statistically significant. Moreover, an F-test of Partridge's model versus the model including legislator and governor salary, and dummy variables for the presence of a state lottery and a gubernatorial veto

power rejects the restricted model. The F-statistic is 3.55 and the critical value at the 95% level is 2.50. In other words, the salary variables cannot be omitted from the equation.

The introduction of these salary and institutional variables results in neither the midwest nor the west dummy variables being individually significant. Additionally, the mining share variable is now significant only at the 10% level whereas in the restricted model it was significant at the 5% level.

Using the five year growth rates the results are little different (Table 10). Governor salaries are statistically significant and negative, but legislator salaries are not different from zero. This is true whether the model includes regional dummies or state specific intercepts. It is also true with or without the inclusion of the government's share of total employment as a regressor.

Finally, the analysis turns to annual data. The year to year growth rate of real per capita personal income is explained using year and state effects, lagged legislator and governor salaries, the natural logarithm of lagged real per capita personal income, and a variety of other explanatory variables including government's share of employment, the length of the legislative session, and dummy variables indicating Democratic control of the state legislature, the governorship, the presence of a lottery, of a gubernatorial veto, and other fiscal institutions.

Table 11 presents typical results from this analysis. Note that the coefficient on the lagged value of the log of real personal income per capita is statistically significant and negative, as would be predicted by conditional convergence. The rate of convergence is substantially higher than that found by Barro and Sala-I-Martin (1991), however, as it has been in virtually every model. The year dummies are jointly significant and many are individually significant. The fiscal institution variables are not statistically significant.

The parameters of interest are the coefficients on rlegs2 and rgsal2. Recall that these are the salaries of legislators and governors two years prior to the year in which growth is measured. The intent is to capture the effects higher salaries have on attracting better qualified candidates, who also produce better outcomes. The legislator salary is not significant. However, the governor's salary is significant and negative, just as in many of the previous cases. The coefficient is small, and possibly economically meaningful. If governor salary increases by $1000, the growth rate in real per capita income for the year falls by 3 tenths of 1 per percent. The significance of this coefficient is consistent across specifications, including government employment share, making the salaries relative to the mean of the lagged real personal income in the state, adding more variables on the length of the session, Democratic control of the house and governorship and so on.[13]

Discussion and Conclusions

The purpose of this paper is to evaluate the argument that higher pay for elected officials will result in better social outcomes. Two distinct approaches were taken. First, it was found that higher salaries for state legislators results in both more bills for consideration and more bills enacted. These results were robust to changes in the specification to include details on the length of legislative session and the professionalization of the legislature. This was taken as weak evidence that legislators "work harder" the more they are paid, all other things constant.

Second, the analysis examined the relationship between legislator and executive salaries

[13]Results for these other cases are available upon request.

and the rate of growth of per capita personal income. The evidence, from a variety of time periods, is that legislator salaries are not statistically significant determinants of growth. However, the evidence is fairly strong that governor's salaries are significant determinants of growth. Paradoxically, the higher are those salaries the slower is economic growth.

These results beg the question as to the source of this effect. One possibility is spurious correlation. The evidence on governors' salaries is that in a large number of states these salaries have a strong downward trend throughout the period of the analysis. It could simply be that this downward trend happens to coincide with relatively rapid growth in the 1980s. This hypothesis can be tested by extending the period of the analysis to include the time from 1980 to the present.

Alternatively, it might be that some dramatic outliers in governor salaries are driving the results. To assess this possibility, data was examined for such outliers. Texas salaries, both gubernatorial and legislative, stood out as extreme outliers compared to the other states. Interacting the salary variable with a dummy variable for Texas produced results nearly identical to those reported above, though the interaction terms were strongly significant and positive. I extended this approach to include other outliers, New York and North Carolina on the high end of governor's salary, and Arkansas on the low end. These interactions were not significant. The analysis indicates that outliers are not the source of these results.

This paper finds little support for the belief that paying legislators more will result in faster or slower economic growth, though it will likely produce more legislation. That does not mean that paying legislators more is not good public policy or even the right thing to do. It also does not mean that paying them more has no effects on other desirable public outcomes such as infant mortality, literacy, crime rates, and so on, though my personal expectation is that it will not.

But the paper also finds an intriguing puzzle. Why is it that the evidence consistently shows that governors who are paid relatively more reside in states whose growth experience is relatively poor? Is there an economic mechanism behind this result, or is it simply an artifact of the time period? Only further work research on this issue can resolve the question.

Table 1: Variable definitions and descriptive statistics

Variable	Definition	Mean	Standard Dev.
LOTT	1 if state has a lottery, 0 else	0.620	0.490
VETO	1 if governor has veto power, 0 else	0.840	0.370
YTX	1 if state has a personal income tax, 0 else	0.880	0.328
COTX	1 if state has a corporate income tax, 0 else	0.920	0.274
STX	1 if state has a sales tax, 0 else	0.900	0.303
SEVTX	1 if state has a severance tax, 0 else	0.660	0.479
LNRINC	natural log of real total personal income	17.523	1.069
LNRPCPI	natural log of real personal income per capita	9.503	0.162
LAGRINC	lagged value of LNRINC	17.022	1.091
LAGRPCPI	lagged value of LNRPCPI	9.202	0.157
LEGDAYD	1 if days the legislature is in session is missing, 0 else	0.420	0.499
RATE	growth rate of total personal income from 1971 to 1989	0.501	0.176
RATEPC	growth rate of personal income per capita from 1971 to 1989	0.301	0.072
LAGLSAL	lagged real value of legislator salary	8718.120	5866.360
LAGGSAL	lagged real value of governor salary	33.256	11.604
LAGLEGD	lagged number of days the legislature was in session	67.740	90.055
BILL	number of bills before the legislature during the session	1896.002	2440.445
ENACT	number of bills enacted by the legislature during the session	442.288	386.531
RTOTPI	real total personal income in $	70393266.610	84557614.950
RPCPI	real personal income per capita in $	13572.270	2255.770
RLEGSAL	real legislator salary in $	20421.740	13756.510
RGOVSAL	real governor salary in thousands of $	61.937	12.927

Table 2 : Trends in salaries and length of session					
	positive	negative	zero	weak positive	weak negative
Legislator salaries (0,wn)	4 (4)	9 (12)	31 (30)	4 (1)	1 (2)
Governor salaries (-)	3	30	11	3	1
Legislative sessions (legday) 29	5	1	21	1	1
Legislative sessions (calday) 17	2	0	13	1	1

Note: Illinois had missing values for legislator salary and is not included in the data.
Numbers in parentheses are for the model with legday, calday, legdayd, and caldayd variables.
(0,wn) indicates that when all the states are pooled there is no trend in the legislator salary without controls for length of session and a weakly negative relationship when length of session is controlled.

Table 3: Explaining the number of bills, annual data 1971-1989

bill	Coef.	Std. Err.	zP>|z|	
dcg	-102.30	112.24	-0.91	0.36
dgv	-90.46	74.62	-1.21	0.23
rlegsal	0.02	0.01	2.84	0.01
y73	759.93	180.27	4.22	0.00
y74	140.47	179.06	0.78	0.43
y75	789.07	183.57	4.30	0.00
y76	356.15	180.77	1.97	0.05
y77	814.71	181.99	4.48	0.00
y78	375.14	180.58	2.08	0.04
y79	544.51	181.57	3.00	0.00
y80	156.62	179.82	0.87	0.38
y81	633.34	182.35	3.47	0.00
y82	262.70	179.80	1.46	0.14
y83	550.85	181.58	3.03	0.00
y84	323.90	181.13	1.79	0.07
y85	618.39	180.20	3.43	0.00
y86	159.32	179.45	0.89	0.38
y87	660.85	183.66	3.60	0.00
y88	505.67	179.81	2.81	0.01
y89	770.30	182.41	4.22	0.00
legday	6.83	0.93	7.35	0.00
calday	2.70	0.96	2.81	0.01
_cons	813.32	330.40	2.46	0.01
R squared	.13			

Note: Hausman test rejects fixed effects in favor of random effects model.

Table 4: Explaining bills enacted, annual date from 1971-1989

enact	Coef.	Std. Err.	tP>|t|	
dcg	-0.33	34.12	-0.01	0.99
dgv	-18.07	22.44	-0.81	0.42
rlegsal	0.00	0.00	1.15	0.25
y73	105.20	54.10	1.94	0.05
y74	-0.43	53.73	-0.01	0.99
y75	91.64	55.14	1.66	0.10
y76	-40.41	54.27	-0.75	0.46
y77	104.41	54.64	1.91	0.06
y78	2.93	54.20	0.05	0.96
y79	59.74	54.52	1.10	0.27
y80	-25.21	53.98	-0.47	0.64
y81	93.29	54.76	1.70	0.09
y82	-36.09	53.97	-0.67	0.50
y83	199.84	54.52	3.67	0.00
y84	-10.10	54.09	-0.19	0.85
y85	70.78	54.08	1.31	0.19
y86	13.35	53.85	0.25	0.80
y87	75.41	55.14	1.37	0.17
y88	7.30	53.97	0.14	0.89
y89	79.78	54.76	1.46	0.15
legday	2.06	0.28	7.29	0.00
calday	1.91	0.30	6.33	0.00
_cons	195.24	53.83	3.63	0.00
R squared	.08			

Note: A Hausman test rejects random effects in favor of fixed effects.

Table 5: Growth of real per capita personal income, 1971-1989

	PARAMETER	STD. ERR.	T-STAT	P-VALUE
INTERCEPT	0.611	0.814	0.750	0.459
LAGRPCPI	-0.041	0.088	-0.469	0.642
LAGLSAL	-7.560E-07	2.860E-06	-0.265	0.793
LAGGSAL	-0.001	0.001	-0.787	0.437
NEWE	0.162	0.045	3.599	0.001
MATL	0.102	0.036	2.859	0.008
SATL	0.124	0.041	3.022	0.005
ESCR	0.062	0.045	1.364	0.183
WSCR	0.050	0.043	1.156	0.256
ENCR	0.012	0.034	0.366	0.717
WNCR	0.026	0.039	0.675	0.505
MOUT	-0.035	0.035	-0.997	0.326
VETO	0.027	0.024	1.126	0.269
LOTT	0.021	0.018	1.151	0.258
SEVTX	0.013	0.024	0.556	0.582
YTX	0.029	0.042	0.684	0.499
COTX	-0.022	0.053	-0.402	0.690
STX	-0.012	0.027	-0.433	0.668
LAGLEGD	1.370E-04	1.301E-04	1.056	0.299

Note: Including a dummy variable for missing values of LAGLEGD has little effect on the estimates. Inclusion of a variable on the proportion of the population with a college degree or with a high school or greater education also has little effect on the results, though those variables are highly significant.

Table 6: Growth in real personal income per capita, 1971-1981, 1981-1989

	PARAMETER	STD. ERR.	T-STAT	P-VALUE
INTERCEPT	1.797	0.585	3.074	0.003
LAGRPCPI	-0.177	0.062	-2.861	0.005
LAGLSAL	3.980E-07	9.100E-07	0.437	0.664
LAGGSAL	-0.001	0.000	-2.124	0.037
LAGVETO	0.021	0.024	0.856	0.395
LAGLOTT	0.115	0.025	4.566	0.000
LAGLDAY	6.581E-05	1.163E-04	0.566	0.573
LAGYTX	0.003	0.042	0.080	0.937
LAGCOTX	-0.013	0.049	-0.269	0.789
LAGSTX	0.014	0.027	0.511	0.611
LAGSEV	0.023	0.018	1.250	0.215
NEWE	0.020	0.040	0.494	0.622
MATL	0.009	0.037	0.236	0.814
SATL	0.034	0.036	0.936	0.352
ESCR	-0.023	0.040	-0.581	0.563
WSCR	-0.021	0.039	-0.536	0.594
ENCR	-0.049	0.034	-1.420	0.159
WNCR	-0.003	0.035	-0.087	0.931
MOUT	-0.047	0.033	-1.406	0.164

Table 7: Descriptive statistics for Partridge and 1971-1981, 1981-1989

	Definition	Mean	Std. Dev.	Min.	Max.
laglsal	as in Table 1	19831.76	14643.35	0	74222.22
laggsal	as in Table 1	71.99	24.46	24.69	209.88
lagveto	lagged value of veto	0.84	0.36	0	1
laglott	lagged value of lott	0.17	0.37	0	1
laglegd	as in Table 1	64.72	79.41	0	362
lagdayd	as in Table 1	0.46	0.50	0	1
d70	1 if 1970s, 0 else	0.50	0.50	0	1
gini	family income gini coefficient	0.36	0.02	0.31	0.42
middle20	family income share of the middle quintile	0.18	0.00	0.16	0.19
hsplus	percent high school grad but not college grad	46.68	7.60	28.83	61.05
collgrad	percent college grad	13.09	3.76	6.66	23.00
welperc	percent of personal income accounted for by public welfare	1.71	0.61	0.72	3.33
manushar	percent of employment in manufacturing	23.12	8.96	4.23	40.42
farmshar	percent of population on farms	2.53	2.24	0.09	9.91
rpci	real per capita income	10413.26	1790.93	6636.60	14699.00
south	1 if southern, 0 else	0.33	0.47	0	1
midwest	1 if midwestern, 0 else	0.25	0.44	0	1
west	1 if western, 0 else	0.23	0.42	0	1
govtsh	percent of employment by government	19.49	3.85	12.85	30.49
transh	percent of employment by transportation	6.01	1.17	3.26	9.79
firesh	percent of employment in finance, insurance or real estate	4.88	1.05	3.04	8.66
minesh	share of employment in mining	1.69	2.75	0	17.23
constsh	share of employment in construction	5.20	1.23	2.76	9.71
gpercap	Partridge's growth rate of income	20.21	8.69	-9.47	51.04
rate	as in Table 1	0.25	0.14	-0.17	0.66
ratepc	as in Table 1	0.15	0.08	-0.13	0.32
lagrinc	as in Table 1	17.44	1.05	15.49	19.92
lagrpcpi	as in Table 1	9.40	0.16	9.04	9.87
N		96			

Table 8: Partridge's model and data, 1970-80, 1980-1990

	Coef.	Std. Err.	T-statistic	P>|t|	
gini	-10.77978	63.8768	-0.169		0.866
hsplus	-0.0413678	0.2876684	-0.144		0.886
collgrad	1.692339	0.5362334	3.156		0.002
rpci	-0.0024105	0.001043	-2.311		0.023
d70	7.978564	3.66927	2.174		0.033
south	0.9590663	3.504407	0.274		0.785
midwest	-6.260151	2.901052	-2.158		0.034
west	-9.345855	3.246047	-2.879		0.005
minesh	-0.8785378	0.4239126	-2.072		0.041
constsh	-3.012094	0.9627701	-3.129		0.002
manushar	-0.3276504	0.242171	-1.353		0.180
transh	1.32565	1.053585	1.258		0.212
firesh	-1.848329	1.207613	-1.531		0.130
govtsh	-0.120356	0.4470952	-0.269		0.788
farmshar	0.0710104	0.5481605	0.130		0.897
Constant	56.2796	42.52263	1.324		0.189
Adj. R squared	.32				

Table 9: Partridge's model and data with salaries, 1970-80, 1980-90

	Coef.	Std. Err.	t	P>|t|	
gini	-15.19001	64.74829	-0.235	0.815	
hsplus	-0.0878733	0.2742242	-0.320	0.750	
collgrad	1.31471	0.51946	2.531	0.013	
rpci	-0.0024159	0.0010314	-2.342	0.022	
d70	8.829416	3.640183	2.426	0.018	
south	2.885219	3.554382	0.812	0.419	
midwest	-4.676171	3.141739	-1.488	0.141	
west	-5.17417	3.450928	-1.499	0.138	
minesh	-0.7252182	0.4254997	-1.704	0.092	
constsh	-2.675523	0.931308	-2.873	0.005	
manushar	-0.2597609	0.2445964	-1.062	0.292	
transh	1.700898	1.033482	1.646	0.104	
firesh	-1.049381	1.193296	-0.879	0.382	
govtsh	-0.1502373	0.4368123	-0.344	0.732	
farmshar	0.0494108	0.5477885	0.090	0.928	
laglsal	0.0000621	0.0000717	0.866	0.389	
laggsal	-0.1047035	0.0437678	-2.392	0.019	
lagveto	0.0408426	2.799784	0.015	0.988	
laglott	7.767964	2.693531	2.884	0.005	
constant	58.48299	43.95043	1.331	0.187	
Adj. R squared	.40				

Table 10: Growth rate of real personal income per capita, 1971-1976, 1976-1981, 1981-1986, 1986-1989

	PARAMETER	STD. ERR.	T-STAT	P-VALUE
INTERCEPT	6.090	0.881	6.910	0.000
LAGRPCPI	-0.621	0.095	-6.551	0.000
LAGLSAL	-5.220E-07	6.900E-07	-0.757	0.451
LAGGSAL	-0.001	3.544E-04	-1.958	0.052
LAGVETO	0.007	0.053	0.137	0.891
LAGLOTT	0.027	0.017	1.630	0.105
LAGLDAY	-2.127E-05	1.071E-04	-0.199	0.843
LAGYTX	-0.154	0.090	-1.711	0.089
LAGCOTX	0.078	0.061	1.282	0.202
LAGSEV	0.027	0.026	1.047	0.297

Note: Included 49 state specific dummy variables and 3 year specific dummy variables. Results for these variables are available upon request.

Table 11 : Explaining annual growth rates of personal income per capita, 1972-1989

	Coef.	Std. Err.	tP>|t|	
lagrpcpi	-0.11	0.02	-5.86	0.00
rlegs2	-0.00	0.00	-0.27	0.79
rgsal2	-0.00	0.00	-4.00	0.00
y73	0.01	0.01	1.85	0.06
y74	-0.06	0.01	-11.72	0.00
y75	-0.06	0.01	-10.92	0.00
y76	-0.02	0.01	-3.24	0.00
y77	-0.02	0.01	-3.64	0.00
y78	-0.00	0.01	-0.48	0.64
y79	-0.04	0.01	-6.76	0.00
y80	-0.08	0.01	-12.17	0.00
y81	-0.04	0.01	-7.00	0.00
y82	-0.06	0.01	-9.96	0.00
y83	-0.03	0.01	-5.33	0.00
y84	-0.01	0.01	-2.25	0.03
y85	-0.02	0.01	-3.59	0.00
y86	-0.01	0.01	-1.52	0.13
y87	-0.02	0.01	-2.78	0.01
y88	-0.01	0.01	-1.45	0.15
y89	-0.01	0.01	-0.92	0.36
lott	0.00	0.00	0.32	0.75
veto	0.01	0.01	1.07	0.28
sevtx	0.01	0.01	1.01	0.32
_cons	1.12	0.18	6.27	0.00
R squared	.34			

Note: The model also included state specific fixed effects.

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