6-2009

Patents and Growth: Empirical Evidence from the States

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PATENTS AND GROWTH: EMPIRICAL EVIDENCE FROM THE STATES*

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In the Uruguay Round, negotiators for the United States persuaded its trading partners to incorporate uniform minimum standards for the protection of intellectual property rights ("IPRs") directly into the General Agreement on Tariffs and Trade. Although individual countries may adopt higher standards for protection, the agreement on Trade Related Aspects of Intellectual Property Rights ("TRIPs") imposed on all countries the fairly high standards of protection then existing in only a relative handful of countries. Proponents of TRIPs argue that the agreement is trade-related and will prove to mutually enhance the welfare of countries that are net exporters of IPR goods, as well as countries that are net-IPR goods importers. Opponents contend that the measure was mere rent-seeking by the United States and will prove beneficial only to countries that are net-IPR exporters. This paper uses the United States as a natural experiment in an attempt to cast some light on this debate. The United States has provided uniform federal patent protection since 1790, yet the patenting activity across states has varied considerably. The residents of some states patent more; the residents of other states patent less. Using a macroeconomic model to explain variations in state economic performance, this paper finds that, within a regime of uniform patent protection, patenting by the residents of other states (or "external patenting") has a positive correlation with a state's per capita income and gross domestic product. Moreover, when the states are broken down into quartiles based upon their levels of

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patenting activity, the correlation becomes more positive for states in the lower patenting quartiles.

INTRODUCTION

In the Uruguay Round of Multilateral Trade Negotiations ("Uruguay Round"), negotiators for the United States persuaded its trading partners to incorporate uniform minimum standards for the protection of intellectual property rights ("IPRs") directly into the General Agreement on Tariffs and Trade ("GATT").[1] Although

1. Agreement on Trade-Related Aspects of Intellectual Property Rights, Apr. 15, 1994, Marrakesh Agreement Establishing the World Trade Organization, Annex 1C, 33 I.L.M. 1197 (1994) [hereinafter TRIPs]. For a summary of the various stories that might account for the adoption of TRIPs, see, for example, Peter K. Yu, *TRIPs and Its Discontents*, 10 MARQ. INTELL. PROP. L. REV. 369, 371–79 (2006) (offering four stories to account for the adoption of TRIPs: (i) the bargain narrative where net-IPR importing countries agreed to TRIPs in return for broader access for their own agriculture products and textiles in the net IPR-exporting countries; (ii) the coercion narrative where net IPR-exporting countries forced the net IPR-importing countries to agree to TRIPs using their superior bargaining power; (iii) the ignorance or paternalism narrative where adopting more extensive protection for IPR goods was in the self-interest of the net IPR-importing countries, but they did not realize it; and (iv) the self-interest narrative, where adopting more extensive IPRs was in the self-interest of the net IPR-importing countries, and they

individual countries may adopt higher standards for protection,[2] the agreement on Trade Related Aspects of Intellectual Property ("TRIPs") imposed on all GATT signatory countries the fairly high standards of protection then existing in only a relative handful of countries.[3] The adoption of TRIPs may prove desirable, from the United States' perspective, for two reasons. First, adopting uniformly high protection for IPRs may increase world economic growth, allowing the United States to share in a larger pie. So far, however, empirical studies have shown little or no direct positive correlation between higher IPR protection and increased growth, and seem to suggest that a lower level of IPR protection may maximize growth for lesser-developed countries.[4] Second and alternatively, TRIPs may prove desirable for the United States even if it reduces (or fails to increase) world economic growth so long as it improves the terms of trade for the United States as a net exporter of IPR products.

In order to evaluate these alternative justifications for TRIPs, this Article examines the correlation between patenting and economic growth for the forty-eight continental U.S. states. Because federal law provides a uniformly high regime of patent rights within the United States,[5] and because patents list the state of residence of their inventors,[6] I can use the United States' experience as a natural

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realized it, but for some reason were unable to accomplish that result domestically without the external push of TRIPs).

2. This aspect of TRIPs has led some to decry the agreement as a "one-way ratchet," allowing member states to increase the level of IPRs from the minimums that TRIPs requires, but not to decrease the level. *See, e.g.*, Rochelle Cooper Dreyfuss, *TRIPs—Round II: Should Users Strike Back?*, 71 U. CHI. L. REV. 21, 22 (2004).

3. Professor Kitch, for example, has described TRIPs as requiring other countries to adopt the level of protection provided for IPRs in the United States. *See* Edmund W. Kitch, *The Patent Policy of Developing Countries*, 13 UCLA PAC. BASIN L.J. 166, 167 (1994) (noting in the introduction that "the GATT agreement requires" signatory countries to adopt IPRs that "resemble[] the American system"). However, the high level of IPRs provided in the United States may not be high enough in every respect. An arbitration panel of the World Trade Organization—the entity responsible for enforcing TRIPs—has found that the exemption from the public performance right provided for small businesses in section 110(5) of the United States Copyright Act, 17 U.S.C. § 110(5) (2006), violates TRIPs and the Berne Convention. *See* Panel Report, *United States—Section 110(5) of the U.S. Copyright Act*, § 7.1, WT/DS160/R (June 15, 2000), *available at* http://www.wto.org/english/tratop_e/dispu_e/cases_e/ds160_e.htm (finding that the public performance exemption for small businesses contained in section 110(5)(b) of the United States Copyright Act violated TRIPs).

4. *See infra* notes 14–15, 34–49 and accompanying text.

5. *See* Patent Act of 1952, Pub. L. No. 593, 66 Stat. 792 (1952) (codified as amended at 35 U.S.C. §§ 1–376 (2006)).

6. *See* 37 C.F.R. § 1.63(c)(1) (2008) (requiring inventors to list their address as part of the oath or declaration that they must file in connection with a nonprovisional patent application, unless the information is provided in the application data sheet).

experiment. Examining the macroeconomic correlations, if any, between state income, state income growth, and state patenting within the United States may suggest who is likely to benefit from a uniformly high IPR regime.

I. THE EXISTING LITERATURE: PATENTS AND GROWTH

In attempting to justify the adoption of TRIPs, proponents have focused on establishing that TRIPs will, in one way or another, promote welfare generally, and that it is not mere rent-seeking that benefits the net exporters of IPR goods alone.[7] Assuming that the central purpose of GATT is to promote free trade, this general welfare focus is essential. If I define free trade as either: (i) reductions in barriers to trade; or (ii) other changes to the legal rules of international trade that, in either case, are *mutually* welfare-enhancing, then, by definition, TRIPs promotes free trade only if it promotes welfare generally. If it is simply a form of rent-seeking on behalf of net-IPR exporters, then TRIPs does not promote free trade and, to that extent, does not belong within the GATT framework.

Proponents of TRIPs have advanced three primary arguments in an attempt to establish that uniformly high IPRs will likely prove mutually welfare-enhancing. First, TRIPs proponents have argued that strong IPRs are an important and direct contributor to a country's economic growth.[8] Building on well-known microeconomic theories concerning free riding, public goods, and the reasons why too little innovation will occur in the absence of IPRs, this argument posits that adopting stronger IPRs will tend to ensure that a country's resources are allocated to the most valuable uses and will thus promote each country's economic growth.[9] Second, TRIPs proponents have argued that strong IPRs can help attract foreign direct investment, or capital or technology transfers, from more technologically advanced countries.[10] With the assurance that their

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7. *See, e.g.*, John H. Barton, *The Economics of TRIPs: International Trade in Information-Intensive Products*, 33 GEO. WASH. INT'L L. REV. 473, 475 (2001) ("Consequently, as the WTO and the world move into this new area of regulation, it is important to examine the extent to which the non-zero-sum mutual benefit assumptions of traditional free trade theory are satisfied for [information-intensive products].").

8. *See, e.g.*, Kitch, *supra* note 3, at 176–77.

9. This argument reflects both the ignorance and self-interest accounts offered by Professor Yu. *See* Yu, *supra* note 1, at 375–79.

10. *See, e.g.*, Carlos A. Braga & Carsten Fink, *How Stronger Protection of Intellectual Property Rights Affects International Trade Flows*, *in* INTELLECTUAL PROPERTY AND DEVELOPMENT 19, 19–34 (Carsten Fink & Keith E. Maskus eds., 2005); Michael J. Ferrantino, *The Effect of Intellectual Property Rights on International Trade and*

intellectual property will be protected, multinational firms can more readily transfer their intellectual property to a country, build manufacturing plants that utilize the intellectual property, and train a native workforce.[11] Third, TRIPs proponents have argued that uniform IPRs will also encourage established Northern[12] research facilities to examine uniquely Southern problems, such as malaria and other tropical diseases.[13] With uniform IPRs, Northern firms can invest in solving Southern problems, knowing that their resulting discoveries will receive IPR protection.

To date, the empirical research attempting to establish any one of these arguments has generally fallen short.[14] Perhaps because

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Investment, 129 WELTWIRTSCHAFTLICHES ARCHIV 300, 327–29 (1993); Keith E. Maskus & Denise Eby Konan, *Trade-Related Intellectual Property Rights: Issues and Exploratory Results*, *in* ANALYTICAL AND NEGOTIATING ISSUES IN THE GLOBAL TRADING SYSTEM 401, 439–40 (Alan V. Deardorff & Robert M. Stern eds., 1994).

11. *See* Jeong-Yeon Lee & Edwin Mansfield, *Intellectual Property Protection and U.S. Foreign Direct Investment*, 78 REV. ECON. & STAT. 181, 185–86 (1996); Pamela J. Smith, *Are Weak Patent Rights a Barrier to U.S. Exports?*, 48 J. INT'L ECON. 151, 170 (1999); Sharmila Vishwasrao, *Intellectual Property Rights and the Mode of Technology Transfer*, 44 J. DEV. ECON. 381, 399–400 (1994).

12. The "North-South" phrasing is common in economic discussions of development and international trade and refers to the divide between the generally well-developed economies of the northern hemisphere and the poorer and far less developed economies of the southern hemisphere. *See* Carlos Alberto Primo Braga, *The North-South Debate on Intellectual Property Rights*, *in* GLOBAL RIVALRY AND INTELLECTUAL PROPERTY 173, 173–79 (Murray G. Smith ed., 1990); Ishac Diwan & Dani Rodrik, *Patents, Appropriate Technology, and North-South Trade*, 30 J. INT'L ECON. 27, 79–47 (1991).

13. *See, e.g.*, Jean O. Lanjouw & Iain Cockburn, *Do Patents Matter?: Empirical Evidence after GATT* 2–3 (Nat'l Bureau of Econ. Research, Working Paper No. 7495, 2000), *available at* http://www.nber.org/papers/w7945.

14. *See* Braga & Fink, *supra* note 10, at 34 (finding a correlation between stronger IPRs and trade generally, but finding no correlation between stronger IPRs and trade in high technology goods); Ferrantino, *supra* note 10, at 327–28 (looking for correlations between a country's decision to join a major international intellectual property treaty and technology transfer to that country, but finding only limited support for such a correlation); Edwin Mansfield, *Unauthorized Use of Intellectual Property: Effects on Investment, Technology Transfer, and Innovation*, *in* GLOBAL DIMENSIONS OF INTELLECTUAL PROPERTY RIGHTS IN SCIENCE AND TECHNOLOGY 107, 114 (Mitchel B. Wallerstein et al. eds., 1993) (using survey data to examine whether a firm's decision on the level and nature of direct investment in a foreign country depends upon the firm's perception of the level of IPRs available in that country and finding that a firm's perception of "a country's intellectual property rights protection will have little effect on the total amount invested by U.S. firms in that country," but may affect the nature of the investment); Maskus & Konan, *supra* note 10, at 439–40 (searching for, but not finding, a correlation between a country's level of IPR and foreign direct investment); Lanjouw & Cockburn, *supra* note 13, at 29–30 (using survey evidence in an attempt to establish that Northern firms began investing more heavily in Southern diseases, such as malaria, in response to TRIPs, but failing to find a clear increase in such investment).

TRIPs is such a recent development,[15] an empirical link between high IPRs and welfare improvements for lesser developed countries and other net IPR importers has eluded researchers. This Article builds on macroeconomic growth theories and the United States' experience with uniformly high IPRs in an attempt to define more clearly the potential winners and losers from a uniformly high IPR regime.

Macroeconomic examination of economic growth over the last forty years has centered around the Solow or neoclassical model and various extensions and responses to it.[16] The neoclassical model posits income as a function of capital, labor, and technology. Assuming a Cobb-Douglas production function:[17]

$$Y = K^{\alpha}(\Phi_t L)^{\beta} \qquad (1)$$

where Y is real income, K is capital, L is labor, and Φ_t is, at least nominally, technology.[18] The coefficients α and β represent the output elasticities of the factor inputs. They indicate the percentage change in output associated with a given percentage change in factor input, with constant returns to scale, $\alpha+\beta=1$.[19] If I assume both constant returns to scale and competitive factor markets,[20] the coefficients also equal the relative share of output paid to labor and capital. The relative shares of national income in the United States have been quite stable for decades, with labor receiving sixty-five percent of the total and capital receiving the other thirty-five percent, and existing empirical studies using the Solow model have generated corresponding estimates of output elasticities for labor and capital.[21]

Solow's work has resulted in two primary lines of empirical inquiry. First, one of the key predictions of the neoclassical model is

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15. TRIPs was adopted as part of the GATT framework in 1994. *See* TRIPs, *supra* note 1. However, developing countries were given a five-year period to adopt domestic intellectual property rights consistent with TRIPs. *See id.* at art. 65(1)–(2).

16. Robert M. Solow, *A Contribution to the Theory of Economic Growth*, 70 Q.J. ECON. 65 (1956).

17. *See id.* at 76.

18. As Mankiw, Romer, and Weil have explained, Φ_t actually reflects "not just technology but resource endowments, climate, institutions, and so on." N. Gregory Mankiw, David Romer & David N. Weil, *A Contribution to the Empirics of Economic Growth*, 107 Q.J. ECON. 407, 410–11 (1992).

19. With constant returns to scale, a proportional increase in each factor increases aggregate output by the same proportion.

20. In competitive factor markets, each factor receives as its price its marginal productivity. *See, e.g.*, Solow, *supra* note 16, at 79.

21. *See* Alicia H. Munnell & Leah Cook, *How Does Public Infrastructure Affect Regional Economic Performance?*, *in* IS THERE A SHORTFALL IN PUBLIC CAPITAL INVESTMENT? 69, 77–80 (Alicia H. Munnell ed., 1990).

convergence.[22] By assuming that returns to capital are diminishing,[23] the neoclassical model suggests that initially capital-poor economies will offer higher returns to capital, attract capital from capital rich economies, and hence grow more quickly. A corollary of this convergence hypothesis is that, given the same technology, all economies with the same investment and labor growth rates will converge to an identical level of output per worker, regardless of the initial conditions. A number of empirical studies have tested this convergence hypothesis. Baumol, for example, tested for convergence among sixteen industrialized countries over the period 1870–1979.[24] To test for convergence, Baumol estimated the equation:

$$\ln\left(Y/N\right)_{i,1979} - \ln\left(Y/N\right)_{i,1870} = a + b\ln\left(Y/N\right)_{i,1870} + \varepsilon_i \qquad (2)$$

where N is each country's population in either 1870 or 1979. Despite the very simple form of the regression, the convergence coefficient b was significantly negative, indicating a correlation between higher initial per capita income and slower growth.[25] As De Long has noted, however, a serious flaw with Baumol's work lies in its sample selection.[26] By selecting sixteen countries with comparable per capita incomes as of 1979, a finding of convergence was virtually inevitable.

Moreover, although other studies have supported the convergence hypothesis, at least for economies expected to share the same balanced growth path,[27] disparity between North and South

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22. *See, e.g.*, Paul M. Romer, *Increasing Returns and Long-Run Growth*, 94 J. POL. ECON. 1002, 1002 (1986) (noting that, in the neoclassical growth model, "[t]he rate of return on investment and the rate of growth of per capita output are expected to be decreasing functions of the level of the per capita capital stock. Over time, wage rates and capital-labor ratios across different countries are expected to converge.").

23. This means that for any given level of labor, increasing the investment of capital will increase output, but the increase in output will not be as much for higher levels of capital stock. As a simple example, consider this: giving a worker a shovel to dig holes will result in a significant increase in the output of holes compared to the worker digging with his hands or a stick; giving him a second shovel will not increase productivity as much.

24. William J. Baumol, *Productivity Growth, Convergence, and Welfare: What the Long-Run Data Show*, 76 AM. ECON. REV. 1072, 1074 (1986).

25. *See id.* at 1076.

26. J. Bradford DeLong, *Productivity Growth, Convergence, and Welfare: Comment*, 78 AM. ECON. REV. 1138, 1138–39 (1988).

27. *See* Robert J. Barro & Xavier Sala-i-Matin, *Convergence Across States and Regions*, 1 BROOKINGS PAPERS ECON. ACTIVITY 107, 153 (1992); Douglas Holtz-Eakin, *Solow and the States: Capital Accumulation, Productivity, and Economic Growth*, 46 NAT'L TAX J. 425, 429–32, 437 (1993); *see also* Romer, *supra* note 22, at 1012–13 (summarizing some of the evidence that supports a convergence hypothesis).

growth rates persists. Rather than narrow, as the neoclassical model predicts, the gap in per capita income between North and South has grown.[28] To explain this continuing disparity, Lucas and Romer developed endogenous[29] growth models, in which differences in growth rates could persist.[30] In particular, Lucas and Romer focused on variations in the growth of human capital as a possible explanation for the continuing disparity in North-South economic growth.[31] Their endogenous growth models posited that a more highly educated work force would prove more productive.[32] As Mankiw, Romer, and Weil

28. As Barro has explained:

> In neoclassical models with diminishing returns, such as Solow (1956), Cass (1965) and Koopmans (1965), a country's per capita growth rate tends to be inversely related to its starting level of income per person. Therefore, in the absence of shocks, poor and rich countries would tend to converge in terms of levels of per capita income. However, this convergence hypothesis seems to be inconsistent with the cross-country evidence, which indicates that per capita growth rates are uncorrelated with the starting level of per capita product.

Robert J. Barro, *Economic Growth in a Cross Section of Countries*, 106 Q.J. ECON. 407, 407 (1991).

29. "Endogenous" means "arising from within," or internal, and is the opposite of exogenous or external shocks. A hurricane, such as Katrina, is an exogenous shock that may have a substantial impact on a geographic region's economic output; the decision of individuals to invest in the New Orleans recovery, on the other hand, would be an endogenous variable in a model that attempts to explain such investment decisions. *See* 2 NEW PALGRAVE DICTIONARY OF ECONOMICS 134 (John Eatwell et al. eds., 1987).

30. Robert E. Lucas, Jr., *The Mechanics of Economic Development*, 22 J. MONETARY ECON. 3, 27 (1988); Paul M. Romer, *Endogenous Technological Change*, 98 J. POL. ECONOMY S71, S94 (1990).

31. As Paul Romer explained in his conclusion:

> The most interesting positive implication of the model is that an economy with a larger total stock of human capital will experience faster growth. This finding suggests that free international trade can act to speed up growth. It also suggests a way to understand what it is about developed economies in the twentieth century that permitted rates of growth of income per capita that are unprecedented in human history. The model also suggests that low levels of human capital may help explain why growth is not observed in underdeveloped economies that are closed and why a less developed economy with a very large population can still benefit from economic integration with the rest of the world.

Romer, *supra* note 30, at S99.

32. *See* Lucas, *supra* note 30, at 38; Romer, *supra* note 30, at S73.

> The growth rate is increasing in the stock of human capital, but it does not depend on the total size of the labor force or the population. In a limiting case that may be relevant for historical analysis and for the poorest countries today, if the stock of human capital is too low, growth may not take place at all.

Id.

have shown,[33] with the addition of human capital, the production function can be written as:

$$Y = K^{\alpha} H^{\beta} (\Phi_t L)^{\gamma} \tag{3}$$

where H is human capital.

Testing the model empirically, Mankiw, Romer, and Weil found that convergence was conditional.[34] Because economic growth also depended upon growth in human capital, convergence between rich and poor countries would occur only if human capital grew at similar rates between rich and poor countries.[35] So long as human capital continued to grow more quickly in rich countries, their work suggested that the disparity in growth rates between rich and poor countries would continue.[36]

Second, having demonstrated that factors aside from labor and capital might influence economic growth, the notion of conditional convergence opened the door to the possibility that other factors might contribute to or influence economic growth. Typically, for each additional input factor proposed as relevant, researchers simply added another term to the Cobb-Douglas production function. To test empirically whether the factor contributed to or influenced economic growth, economists take the natural log of the Cobb-Douglas production function to translate it into a linear production function that can be estimated. Thus, to explore whether public capital, as well as private capital, played a role in output, Munnell used data from the forty-eight states to estimate[37]:

$$\ln Q = \ln MFP + a \ln K + b \ln L + c \ln G \tag{4}$$

where Q was state output; MFP was the level of technology; K was the stock of private capital; L was the stock of labor; and G was the stock of public capital.

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33. Mankiw et al., *supra* note 18, at 416.

34. *See id.* at 421.

35. *See* Barro, *supra* note 28, at 409.

> Moreover, given the human-capital variables, subsequent growth is substantially negatively related to the initial level of per capita GDP. Thus, in this modified sense, the data support the convergence hypothesis of neoclassical growth models. A poor country tends to grow faster than a rich country, but only for a given quantity of human capital; that is, only if the poor country's human capital exceeds the amount that typically accompanies the low level of per capita income.

Id.; see also Mankiw et al., *supra* note 18, at 416 n.7 (summarizing some of the previous work that emphasized the importance of human capital in economic growth).

36. *See* Barro, *supra* note 28, at 409.

37. *See* Munnell & Cook, *supra* note 21, at 74–81.

Using this approach, two papers have extended the neoclassical growth model to expressly include a proxy for the strength of a country's IPRs.[38] Gould and Gruben, for example, regressed for ninety-five countries the average annual real per capita gross domestic product ("GDP") growth rate between 1960 and 1988 against: (1) the log of real GDP per capita in 1960; (2) physical capital savings, defined as the log of the share of investment in gross domestic product; (3) a proxy for human capital savings, defined as the log of secondary-school enrollment rates in 1960; and (4) a proxy for the strength of IPRs, defined as the log of the index of patent protection developed by Rapp and Rozek.[39] The Rapp and Rozek index ranks countries by an integer from one to six[40] based on their patent laws' conformity with the standards set forth in the *Guidelines for Standards for the Protection and Enforcement of Patents* of the U.S. Chamber of Commerce Intellectual Property Task Force,[41] where one represents no patent protection and six represents full conformity. When Gould and Gruben included the natural log of the Rapp and Rozak index for each country in their regression, they found the coefficient on the variable was statistically insignificant, suggesting that there was no correlation between a country's increased level of IPRs and economic growth.[42]

Park and Ginarte performed a similar regression.[43] They used their own index, with values ranging (continuously) from zero to five, to reflect the increasing strength of IPR protection in a country.[44] There are some notable differences between their index and the Rapp and Rozek index. First, Park and Ginarte used continuous values, rather than integer values.[45] Second, rather than compare coverage to the U.S. Chamber of Commerce Guidelines, they established their own standard that measures a country's patent protection in five categories: (1) subject matter coverage; (2) membership in

38. David M. Gould & William C. Gruben, *The Role of Intellectual Property Rights in Economic Growth*, 48 J. DEV. ECON. 323, 328–38 (1996); Walter G. Park & Carlos Ginarte, *Intellectual Property Rights and Economic Growth*, 15 CONTEMP. ECON. POL'Y 51, 54–56 (1997).

39. Richard T. Rapp & Richard P. Rozek, *Benefits and Costs of Intellectual Property Protection in Developing Countries*, J. WORLD TRADE, Oct. 1990, at 75, 79.

40. Actually, the Rapp and Rozak index ranks countries from 0 to 5, but Gould and Gruben add 1 to the index values in order to take the natural log of the index values. *Id.*

41. UNITED STATES CHAMBER OF COMMERCE, GUIDELINES FOR STANDARDS FOR THE PROTECTION AND ENFORCEMENT OF PATENTS (1987).

42. Gould & Gruben, *supra* note 38, at 330.

43. Park & Ginarte, *supra* note 38, at 54–55.

44. *Id.* at 52–54.

45. *Id.* at 53 tbl.1 (reporting values for IPR strength in each country).

international agreements; (3) circumstances where protection is lost; (4) enforcement; and (5) duration.[46] Despite the different standards, the indices generate similar proxy values for most countries, as shown in Table 1.

Table 1: Indices Reflecting Strength of Patent Protection Country-by-Country[47]

Country	Park & Ginarte (1997)	Rapp & Rozek (1990)	Country	Park & Ginarte (1997)	Rapp & Rozek (1990)
Algeria	3.24	3	Kenya	2.49	5
Argentina	2.06	2	Korea	3.00	4
Australia	2.84	5	Mauritius	2.37	5
Austria	3.53	5	Mexico	1.30	3
Belgium	3.48	6	Netherlands	3.70	6
Bolivia	1.48	2	New Zealand	2.98	5
Brazil	1.52	2	Nicaragua	0.94	3
Cameroon	2.04	3	Norway	2.92	5
Canada	2.67	5	Pakistan	1.70	4
Central Afr. Rep.	2.04	3	Panama	2.15	3
Chile	1.96	3	Paraguay	1.29	2
Colombia	1.13	3	Peru	0.65	2
Congo	2.04	3	Philippines	2.52	5
Costa Rica	1.84	4	Portugal	1.82	4
Denmark	3.11	6	Rwanda	2.43	5
Ecuador	1.60	2	Senegal	1.99	3
Finland	2.39	5	Singapore	2.16	5
France	3.48	6	South Africa	3.45	6
Germany	3.29	6	Spain	3.53	5
Greece	2.01	5	Sri Lanka	2.76	5
Guatemala	1.15	4	Sweden	2.99	6
India	1.39	2	Switzerland	3.23	6
Ireland	2.46	5	Trinidad/Tobago	2.73	5
Israel	3.53	6	Turkey	1.29	2
Italy	3.50	6	U.K.	3.26	6
Jamaica	2.44	4	U.S.A.	3.52	6
Japan	3.48	5	Uruguay	1.63	4
Jordan	1.52	5	Venezuela	0.75	3

Having created their own index, Park and Ginarte used it to perform a growth regression similar to that of Gould and Gruben, regressing the difference between the log of GDP per adult worker in 1990 and in 1960 for sixty countries against: (1) the log of real GDP per worker in 1960; (2) the log of the capital savings rate; (3) the log of the human capital savings rate; (4) the log of research and

46. *See* Park & Ginarte, *supra* note 38, at 52–54.

47. Both indices range from 1 to 6, with 1 meaning no patent protection and 6 indicating maximum protection. The indices have a correlation of 0.794301, n=58, $p < 0.0001$, which indicates a high level of correlation between the two indices.

development expenditures; (5) the log of the population growth rate; (6) the log of their IPR index variable; and (7) the log of a variable representing the degree of market freedom present in the country.[48] Their regression yielded a negative IPR coefficient, but as with Gould and Gruben, the coefficient was statistically insignificant even at the ten percent level.[49] Again, this suggests no correlation between increasing the level of IPR protection in a country and economic growth.

Although neither paper found a direct correlation between IPRs and growth, both papers performed further regressions and found that increasing IPR was statistically correlated with increased investments in physical and research capital, *ceteris paribus* (that is, holding all other variables constant).[50] Because increased capital investments correlate with increased growth rates, Park and Ginarte suggested that increased IPRs may increase growth indirectly.[51]

The existing empirical studies thus provide only limited support for a relationship between IPRs and growth. Based upon these studies, even the most optimistic proponent of stronger IPRs can assert only that "the statistical correlation between IPRs and economic growth is positive under some circumstances."[52] Further, in the existing literature, there has also been no attempt to incorporate the role that cross-country variations in technological growth may play in explaining variations in economic growth. To the contrary, the usual assumption has been that technological growth does not vary across countries or across time. Mankiw, Romer, and Weil, for example, assume that growth in technology, which they denominate g, is "constant across countries."[53] In their view, "g reflects primarily the advancement of knowledge, which is not country-specific."[54] They further assume that technology grows at a constant rate of two percent annually over the entire period of their study, 1960 to 1985.[55]

48. *Id.* at 54–56.

49. *Id.*

50. *See* Gould & Gruben, *supra* note 38, at 336–38; Park & Ginarte, *supra* note 38, at 59. Gould and Gruben do not perform such regressions directly, as Park and Ginarte do; rather, they select a set of instrumental variables for their IPR index and find a correlation between the estimated IPR index values from their instrumental variables and growth.

51. *See* Park & Ginarte, *supra* note 38, at 59.

52. Keith E. Maskus, *Lessons from Studying the International Economics of Intellectual Property Rights*, 53 VAND. L. REV. 2219, 2235 (2000).

53. *See* Mankiw et al., *supra* note 18, at 410.

54. *Id.*

55. *Id.* at 412–13 & n.6.

The purposes of this Article are therefore two-fold. First, by incorporating variables that reflect variations between states and over time in patenting activity, this Article attempts to examine directly the role that technological innovation, as reflected by patenting activity, plays in explaining economic growth. Second, by incorporating variables that reflect: (i) patenting by a state's own residents ("internal" patenting) and (ii) patenting by the residents of other states ("external" patenting), this Article attempts to examine the relative contributions of "domestic" and "foreign" innovative activity to a state's economic growth. Although differences exist between how uniformly high IPRs may affect economic growth within a legally and culturally unified entity such as the United States and how they may affect growth internationally, a consideration of the winners and losers within the United States from a uniform patent regime may cast some light on the likely winners and losers internationally from TRIPs.[56]

II. PATENTS AND GROWTH: EVIDENCE FROM THE UNITED STATES

Because a federal statute defines the scope of patent protection within the United States, patent protection has been—to a large extent—uniform across the United States.[57] As a result, using data on

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56. One of the most significant differences is that most patents issued by the United States, and almost all valuable patents, are owned by publicly traded corporations. As a result, if these patents generate economic rents for their owners, an argument can be made that these rents will, in turn, be passed along to the corporations' shareholders located throughout the United States, and the patents' benefits will not therefore be isolated to the particular state where the inventive activities were undertaken. As a practical matter, this rent redistribution mechanism will not be as uniformly present internationally. Like most of the other differences, including a common market and legal system, uniform language, and shared culture within the United States, this difference between the effects of uniformly high patent protection within the United States and the effects of uniformly high patent protection internationally should cut in favor of the free trade justification. That is, if patenting by the residents of other states does not contribute to economic growth within the continental United States, it is that much more unlikely that foreign patents contribute to growth internationally.

57. Before the creation of the Federal Circuit in 1982, appeals from district court cases involving patent infringement were heard by the various circuit courts around the country. Empirical studies have demonstrated some disparity in the percent of patents found valid and infringed between the circuits. *See* GLORIA K. KOENIG, PATENT INVALIDITY: A STATISTICAL AND SUBSTANTIVE ANALYSIS 4–22 to –23 (1st ed. 1980) (finding only about 35% of litigated patents held valid for period from 1954 to 1978); Lawrence Baum, *The Federal Courts and Patent Validity: An Analysis of the Record*, 56 J. PAT. OFF. SOC'Y 758, 760 (1974) (noting that between 1921 and 1973 the circuit courts found nearly two-thirds of adjudicated patents invalid); P.J. Federico, *Adjudicated Patents: 1948–54*, 38 J. PAT. OFF. SOC'Y 233, 236 (1956) (finding that courts upheld the validity of patents in only 30% to 40% of the cases in which validity was an issue); Simone A. Rose,

patenting and growth from the states and an augmented growth model, I can explore the relationship between both internal and external patenting on a given state's per capita income, output, and economic growth. For the purposes of this analysis, "internal" patenting refers to patents for which the lead inventor resides in the state—the state's "own" patents. Internal patents can serve both to improve the productivity of a state's other factor inputs and as a source of rents, derived from both licensing fees from, and product sales to, residents of other states. "External" patenting refers to patents for which the lead inventor resides in another state. External patents might also improve a state's productivity, either as a direct source of technical innovation[58] or through access to products or services incorporating the externally patented innovation. In either case, access to external patents may entail a license fee or rent transfer to the patent-holding state.[59] Where a state cannot afford such access or the market for access otherwise fails, the existence of an external patent may foreclose a state from pursuing certain avenues of technological innovation. However, because patents are, in theory, only available for novel and nonobvious technological advances,[60] a new patent should not preclude anyone from continuing to use the preexisting technology.

Given the potential roles of internal and external patents, the question is whether I can find any economically and statistically significant correlation between a state's internal patenting, external patenting, and various measures of a state's economic performance within a regime of uniformly high IPRs. I begin by identifying the data used for the analysis.

Patent "Monopolyphobia": A Means of Extinguishing the Fountainhead, 49 CASE W. RES. L. REV. 509, 561–62 (1999) (presenting data reflecting validity rate of 21.63% to 53.57% from 1944 through 1982).

58. Because all patents are published and by law (35 U.S.C. § 112 para. 1 (2001)) must include an enabling description of the innovation, residents of one state can use, subject to the ability to obtain and afford a license, the technical innovations disclosed in the patents of other states' residents.

59. This is not inevitable. The rents associated with a patent innovation might flow to the state(s) where the innovation is being practiced, or they might flow to the state(s) where the owners of the patent reside, which, in the case of a patent owned by a publicly traded corporation, might be all of the states. If either of these is true, then we should expect no statistically significant correlation between the place of patenting and growth.

60. 35 U.S.C. §§ 102–03 (2006) (detailing the requirements that preclude an individual from obtaining a valid patent on preexisting technology).

A. The Data

A state's inventiveness is measured by the number of patents issued to the state, and the data is taken from a United States Patent and Trademark Office ("USPTO") publication that lists the number of patents issued by state for each year since 1883.[61] Although a patent may be assigned to a corporation, the actual inventor(s) must file for the patent in his, her, or their name(s) and list his, her, or their state of residence.[62] The USPTO report uses the state of residence of the first named inventor as the state of invention.[63] As the first named inventor on a patent application is usually the lead inventor, there is likely a strong correlation between the residence of the first named inventor and the place where the invention occurred.

The Bureau of Economic Analysis ("BEA") provides annual data on personal income and gross state product for each state.[64] The Department of the Census provides data on state population.[65] I converted all dollar values to year 2000 dollars using the consumer price index from the Bureau of Labor Statistics.[66] As a proxy for human capital, the analysis used the percentage of adults twenty-five years of age or older who had received at least a bachelor's degree.[67] I obtained the educational levels of state populations from the Statistical Abstract of the United States, but data on educational

61. Patenting data by state since 1963 is available online at http://www.uspto.gov/web/offices/ac/ido/oeip/taf/cst_utlh.htm (last visited Apr. 24, 2009). For earlier years, see U.S. PAT. & TRADEMARK OFF., U.S. DEP'T OF COMMERCE, REP. NO. 7, TECHNOLOGY ASSESSMENT & FORECAST 187–95 tbl.A-2 (1977).

62. Under the Patent Act of 1952, an assignee may apply for a patent on behalf of the inventor only if the "inventor refuses to execute an application for patent, or cannot be found or reached after diligent effort." 35 U.S.C. § 118 (2006). Even in such a case, the assignee must still make application for the patent not in its own name, but "on behalf of and as agent for the inventor." *Id.*

63. U.S. PAT. & TRADEMARK OFF., EXTENDED YEAR SET—HISTORIC PATENT COUNTS BY COUNTRY, STATE, AND YEAR 1 (2007), http://www.uspto.gov/web/offices/ac/ido/oeip/taf/cst_utlh.htm ("The origin of a patent is determined by the residence of the first-named inventor.").

64. Bureau of Economic Analysis, Regional Economic Accounts, http://www.bea.gov/regional/index.htm#gsp (last visited Apr. 24, 2009) (providing state-by-state data).

65. U.S. Census Bureau, Resident Population—States 1980–2007, tbl.12, http://www.census.gov/compendia/statab/tables/09s0012.pdf (last visited Apr. 24, 2009) (providing annual state population data since 1981).

66. U.S. Dept. of Labor, Bureau of Labor Statistics, Consumer Price Index, ftp://ftp.bls.gov/ pub/special.requests/cpi/cpiai.txt (last visited Apr. 24, 2009).

67. For 1960, the data reported is for adults twenty-five years of age or older who have completed at least four years of college. Bureau of Economic Analysis, Regional Economic Accounts, http://www.bea.gov/regional/index.htm#gsp (last visited Apr. 24, 2009).

attainment was available only at ten-year intervals.[68] I obtained private and public capital stocks by state directly from Alicia H. Munnell, but her data covers only the period 1969 through 1985.[69] The methodology used to determine public and private capital stocks on a state-by-state level is described in Munnell.[70]

B. *Summary of the Regression Analyses Performed*

Given the data available, I performed regressions on three different sets of data. The first set used data averaged over the fifty-year period from 1951–2000. The second set used annual data, including gross state product, private capital and public capital, labor, and patenting from 1969 through 1986. The third set focused on longer term economic relationships and therefore used averages for per capita income, human capital, and patenting over six ten-year periods, 1946–1955, 1956–1965, 1966–1975, 1976–1985, 1986–1995, and 1996–2005.

Moreover, for each set of data, I performed two different types of regressions. The first type pools data for all forty-eight continental states and implicitly assumes that the growth relationship between capital, human capital, labor, and patenting and economic output is constant across the states. The second type divides the forty-eight states into patenting quartiles and thereby allows the relationship between patenting and economic output to vary between those states with high levels of patenting and those with lower levels of patenting.

III. PATENTS AND GROWTH: POOLED REGRESSIONS

A. *A Preliminary Look with Fifty-Year Averages: Patents and the Role of Convergence*

Having described the data, I begin with a preliminary look at the relationship between a state's patenting and its economic growth over the last half of the twentieth century, from 1951 through 2000. Over that period, the states have exhibited sharply varying levels of patenting, ranging from the State of Mississippi with an average of 3.22 patents issued annually per one-hundred-thousand population ("PPK") to the State of Delaware with an average of 72.63 PPK

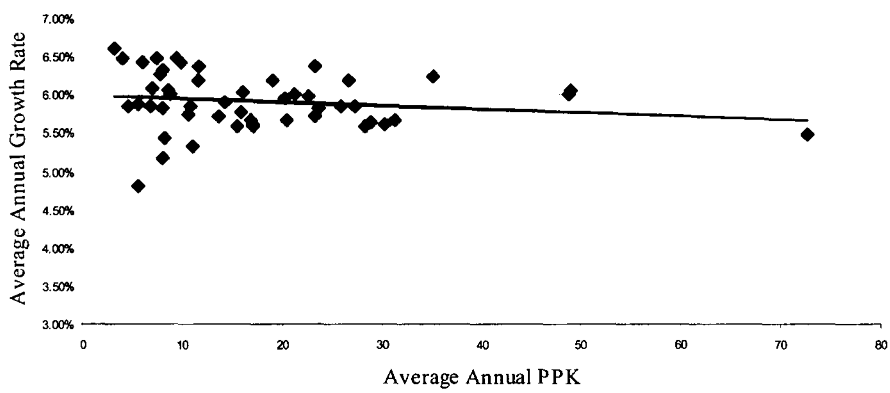
68. U.S. CENSUS BUREAU, STATISTICAL ABSTRACT OF THE U.S., TABLE 225—EDUCATIONAL ATTAINMENT BY STATE: 1990 TO 2006 (2009), http://www.census.gov/compendia/statab/tables/09s0225.pdf (last visited Apr. 24, 2009).

69. *See* Munnell & Cook, *supra* note 21, at 73 tbl.4.

70. *Id.*

annually. If I regress average annual economic growth rate[71] against average annual patenting over the period 1951–2000, pooling the forty-eight continental states, and without accounting for other factors, the coefficient on patenting is negative and statistically significant, b=-0.000065 (p=0.078), as Figure 1 reflects.

Figure 1. Average Annual Growth Rate Versus Average Annual Patents Per One-Hundred-Thousand Population (PPK): 1951–2000



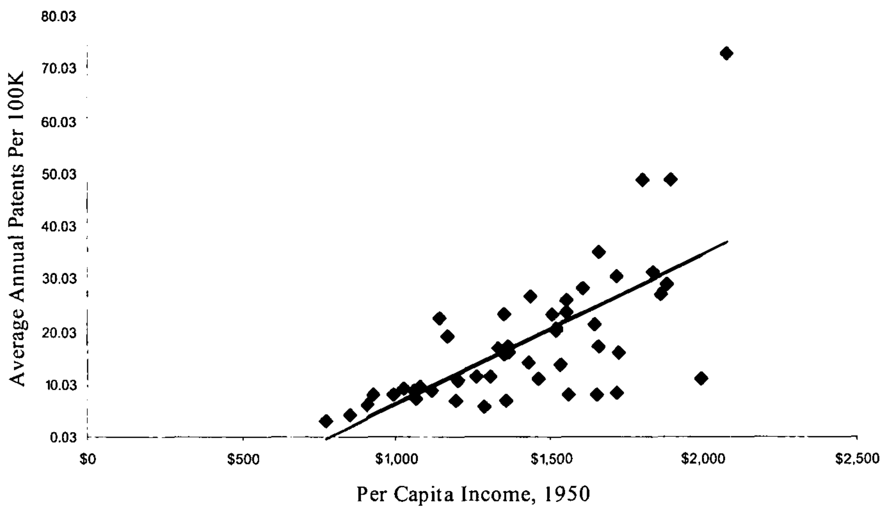
My initial check of the partial correlation between increased patenting and growth thus suggests a negative correlation between increased patenting and economic growth.[72] However, as we have seen, a state's growth will undoubtedly depend on factors other than the amount of patenting alone. Thus, the question therefore arises whether some other factor is causing the seemingly negative correlation.

When I look at which states are patenting, I find that generally those states with higher per capita income in 1950 patented more frequently, as Figure 2 reflects.

71. Economic growth is defined here as the average annual growth in nominal per capita income from 1951–2000, using nominal per capita income data by state from the BEA.

72. If the top and bottom five patenting states over the period are omitted from the regression, the coefficient becomes statistically indistinguishable from zero, b=-0.000095 (p=0.215).

Figure 2. Average Annual PPK: 1951–2000 Versus Per Capita Income in 1950



Thus, the wealthiest states patented more often. While I expect patenting to be positively correlated with economic growth, I expect states with higher initial per capita income to grow more slowly, reflecting convergence. To isolate the respective roles of patenting and convergence in economic growth, I begin with Baumol's 1986 model augmented by human capital and a state's per capita level of patenting activity. I therefore estimate:

$$\overset{*}{y} = a + b\ln(y_i) + c\overset{*}{k}_h + d\bar{t} + \varepsilon \qquad (5)$$

where:

$\overset{*}{y}$ is a state's average annual growth in real per capita income from 1951 through 2000;

y_i is the state's initial per capita income in 1951 (in year 2000 dollars) and represents the convergence criteria;

$\overset{*}{k}_h$ is the state's average annual growth rate in human capital from 1950 through 2000, where human capital is defined as the percentage of state residents over the age of twenty-five years who have obtained at least a bachelor's degree;

and $\bar{t}$ is the average annual number of per capita patents issued to a lead inventor resident in the state from 1951 through 2000.[73]

In addition, to get some sense for the respective roles of internal and external patenting, an additional regression was performed which included the variable $\bar{t}_{us}$. This variable, $\bar{t}_{us}$, is defined as the average number of patents issued annually to residents of the United States, other than the state at issue, divided by the relevant state's population over the period from 1951 through 2000.

The existing theoretical and empirical work on growth suggests that the convergence coefficient, *b*, should be negative, and that the human capital coefficient, *c*, should be positive. In predicting the sign on the coefficient for internal patenting, two possibilities exist. If, as Mankiw, Romer, and Weil assume,[74] the knowledge reflected in patented technological advances remains fundamentally nonexcludable, then a state's own patenting should not explain why that state grew more or less quickly than other states. If, despite the patent system, knowledge remains nonexcludable, then the patented technological advances of one state are immediately and freely available to all. I would therefore expect the coefficient on internal patenting, *d*, to be statistically insignificant. On the other hand, if I assume that the patent system works as intended and does secure to the patent holder some degree of exclusive control over the knowledge a patent reflects, then I would expect the coefficient on internal patenting to be positive.

With respect to external patenting, there is again the question of whether the patent system works as intended and grants a patentee some degree of exclusive control over the patented knowledge. If it does not, and patenting activity is simply a proxy for the (necessarily) non-state specific advancement of knowledge, then the coefficient on external patenting should be positive. On the other hand, if the coefficient on internal patenting is positive, suggesting that the patent system does provide the excludability it intends, then a free-trade view of TRIPs would suggest that the external patenting coefficient should also be positive. Within the structure of uniformly high patent rights that federal law creates, technological advances should flow

73. In contrast to the human capital data, patenting is already a rate variable—the number of patents issued annually reflect the additions to the technology stock in a given year. As a result, the regressions use the average annual patenting rather than attempt to concoct a growth variable from the annual patenting data.

74. *See supra* text accompanying note 53.

readily from one state to another, allowing one state to benefit from the technological advances of the others.

In the light of these expectations, four equations were estimated using pooled income, human capital, and patenting data for the forty-eight continental states. The first equation estimated contains the convergence criteria and human capital growth only. To this basic regression, the second adds internal patenting only, the third adds internal and external patenting, and the fourth adds external patenting only. Table 2 summarizes the results, with p values[75] in parentheses.[76]

Table 2: Growth Regression with Convergence Criteria, Human Capital, and Patenting

Dependent Variable: Average Annual Growth in Real Per Capita Income, 1951–2000

Regressor	(1)	(2)	(3)	(4)
Intercept	0.08246 (< 0.0001)	0.11607 (< 0.0001)	0.11505 (< 0.0001)	0.08904 (< 0.0001)
$ln\ y_i$	0.00828 (< 0.0001)	0.01149 (< 0.0001)	0.0112 (< 0.0001)	-0.00829 (< 0.0001)
$\overset{*}{k}_h$	0.33618 (0.0002)	0.30527 (0.0002)	0.27075 (0.0011)	0.28922 (0.0011)
$\bar{t}$	--	7.92409 (0.004)	7.1970 (0.0079)	--
$\bar{t}_{us}$	--	--	-0.01679 (0.0871)	-0.02111 (0.0443)
Adj. R^2	0.652	0.705	0.719	0.675

75. A p value indicates the coefficient's level of statistical significance directly. Thus, if p=0.1, the coefficient is statistically significant at the ten percent level—that is, a coefficient whose actual value was zero would take on the calculated value due to random statistical variation only ten percent of the time. Similarly, if p=0.01, the coefficient is statistically significant at the one percent level and would arise due to random variation only one percent of the time.

76. In Table 2, $ln\ y_i$ is the natural log of real per capita income in 1950; $\overset{*}{k}_h$ is the state's average annual growth rate in human capital from 1950 through 2000; $\bar{t}$ is the average annual number of per capita patents issued to a lead inventor resident in the state from 1951 through 2000; t_{us} is the average number of patents issued annually to residents of the United States, other than the state at issue, divided by the population of the state at issue; p values are in parentheses. The adjusted R^2 value reported at the bottom of the Table identifies the percentage of variation in the states' average annual growth in real per capita income accounted for by the variables included in the regression. Thus, the four variables included in the full regression in Column 3 account for 71.9% of the variation in the forty-eight states' average annual growth in real per capita income over the period from 1951 to 2000.

As Table 2 reflects, all coefficients are statistically significant at the one percent level, except for the coefficient on $\bar{t}_{us}$, which is statistically significant at the ten percent level in the full regression (Column 3) and at the five percent level when internal patenting is omitted from the regression (Column 4). Multicollinearity does not appear to be a problem.[77] Consistent with the existing literature on convergence, the convergence coefficient is significant and negative. In the full regression (Column 3), a one percent increase in initial per capita income was associated with 1.12 percentage point reduction in average growth, *ceteris paribus*. Also consistent with the existing literature, the coefficient on human capital growth is significant and positive. In the full regression (Column 3), a one percentage point increase in the human capital growth rate is associated with a 0.27 percentage point increase in the growth rate of real per capita income, *ceteris paribus*.

The first patent coefficient, internal patenting, $\bar{t}$, is positively correlated with a state's economic growth rate. In the full regression (Column 3), an increase of one in a state's average annual patenting per capita is associated with a 719.7 percentage point increase in the state's growth, or to put the scale in more attainable terms, an increase of one in a state's average patenting per one hundred thousand population is associated with a 0.007197 percentage point increase in the state's growth, *ceteris paribus*. This result tends to suggest that the knowledge reflected in a patent can remain state-specific and tends to reject the assumption of Mankiw, Romer, and Weil that knowledge is not country-, or in this case, state-specific, at least in the presence of a uniform patent regime.

A question remains as to why the partial correlation shown in Figure 1, regressing average growth against average annual patents per one-hundred-thousand population, was negative. The regression results in Table 2 suggest that the negative partial correlation arises

77. Multicollinearity refers to a high level of correlation between two or more of the regressors. It occurs when two variables essentially capture the same data. I checked for multicollinearity both by examining the correlation between the regressors and using the auxiliary regression technique. The absolute value of the correlation: (i) between average annual growth in human capital and average external patents per capita was 0.30; (ii) between average annual growth in human capital and average internal patents per capita was 0.189; and (iii) between average annual internal and external patents per capita was 0.058. I ran three auxiliary regressions, regressing average annual growth in human capital, internal patenting, and external patenting individually, each against the remaining right-hand side variables. The adjusted R^2s for these auxiliary regressions were 0.205, 0.402, and 0.053 respectively. These R^2 values for the auxiliary regressions are much smaller than the R^2 values for our main regressions, suggesting that multicollinearity is not a problem. Also, the estimated coefficients are robust across model specifications.

because: (1) initially wealthier states patent more often; (2) convergence causes initially wealthier states to grow more slowly; and (3) for the data set studied, the negative convergence effects outweighed the positive effects from higher levels of internal patenting. To illustrate, consider the roles of convergence and patenting on the growth rates of the five states with the highest level of annual patenting and the five states with the lowest level of annual patenting over the period 1951–2000. The five states with the highest levels of annual patenting were Delaware, Connecticut, New Jersey, Massachusetts, and Illinois, with an average annual patent per capita of 0.000473. The five states with the lowest levels of annual patenting were North Dakota, Alabama, South Dakota, Arkansas, and Mississippi, with an average annual patent per capita of 0.0000515. However, the top five patenting states had a much higher per capita income in 1951 than the bottom patenting states: $13,452.79 versus $7,674.82 (in year 2000 dollars). If all else were constant between these states, I would expect, using the full regression results (Column 3), per capita income for the top five patenting states to grow 0.6288 percentage points slower than the bottom five due to convergence alone.[78] However, holding all else constant, I would also expect the top five patenting states to grow 0.303 percentage points faster than the bottom five due to increased patenting.[79] Internal patenting is thus associated with increased growth, but it does not outweigh the slower growth associated with convergence.

The association between internal patenting and growth also appears to be economically significant, comparable in magnitude to the association between growth rates in per capita income and human capital. Using the same "top-five-versus-bottom-five" approach, the five states with the fastest growth in human capital from 1951 to 2000 were Arkansas, Alabama, Minnesota, Tennessee, and Kentucky, with an average annual growth rate in human capital of 3.49 percent. The five states with the slowest growth in human capital were Nevada, Wyoming, Indiana, Delaware, and Arizona, with an average annual growth rate in human capital of 2.26 percent. If all else were constant between these states, I would expect, using the full regression results (Column 3), per capita income for the top five states in human capital

78. The difference in growth was obtained by multiplying the difference in the natural logs of the initial per capita income by the convergence coefficient from Column 3 of Table 2: -0.0112*[ln(13,452.79)-ln(7,674.82)]=-0.6286.

79. The difference in growth was obtained by multiplying the difference in patenting levels for the top and bottom patenting states by the internal patenting coefficient from Column 3 of Table 2: (0.000473-0.0000515)*7.197=0.00303.

growth to grow 0.333 percentage points faster than the bottom five states due to their higher growth rate in human capital.[80] Relative to the bottom five states in each category, the increase in per capita income growth for the top five states in human capital growth was, at 0.333 percentage points, comparable to the 0.303 percentage points increase for the top five states in patenting. This suggests that the correlation between patenting and growth is not merely statistically significant, but also economically significant.

In contrast to the positive correlation between internal patenting and growth, external patenting is negatively correlated with a state's economic growth. In the full regression (Column 3), an increase by one in the average number of patents issued annually to residents of the United States, other than the state at issue, is associated with a 1.679 percentage point reduction in growth, all else constant. This suggests that although every state has, at least in theory, access to the new technology other states have patented, patenting by other states does not increase, but rather reduces, a state's growth rate in per capita income.

It is not self-evident why the coefficient on external patenting would be negative. The simplest explanation is that licensing or purchasing products that incorporate the externally patented innovation entails a rent transfer out of state, and thus serves as a drag on a state's economic growth. However, a firm's decision to license, rather than invent around, an external patent presumably indicates a rational, self-interested decision. To the extent that the decision to license increases the firm's utility, the decision should promote, rather than reduce, the state's economic well-being. Two possibilities, nevertheless, remain for the negative coefficient: (1) foreclosure and (2) dependence.

First, external patents may serve to foreclose certain avenues of innovation or economic development. Foreclosure may occur in either a strong or weak form. In its strong form, foreclosure may reflect instances of bargaining difficulties or other instances of market failure that simply preclude licensing of an external innovation. Or, alternatively, it may represent instances where a foreign firm holds a patent simply to block development of competing products, without any intent to exploit or allow another to exploit the patented innovation. If such instances of strong foreclosure are sufficiently

80. The difference in growth was obtained by multiplying the difference in human growth rates for the top and bottom states in human capital growth by the human capital coefficient from Column 3 of Table 2: 0.27075*(0.0349*0.0226)=0.00333.

common, they could explain the negative coefficient on external patenting. Although a possible contributor in some instances to the negative coefficient on external patenting, the strong foreclosure hypothesis is difficult to reconcile with the positive coefficient on internal patenting. The positive coefficient on internal patenting suggests that patents contribute to the home state's growth and are thus actively exploited, at least on average. Perhaps internal patents contribute to a state's growth by serving as blocking devices that insulate a domestic firm from the development of foreign competition, but that explanation is not entirely satisfactory. While patents are sometimes used merely to block others from developing a given technology, it seems unlikely that such use or market failure in the licensing market more generally is sufficiently common to explain the negative coefficient on external patenting.

Rather than strong foreclosure, external patenting might also reflect a weak foreclosure. If I think of the competitive process as a race, where the winner obtains a patent and hence the right to obtain the rents associated with a given product innovation, external patenting may reflect instances where a domestic firm has lost the race. In this case, foreclosure does not refer to an inability to obtain access to the innovation but the lost opportunity to collect the rents associated with the externally patented innovation. Here, the sign on external patenting is negative because resources are invested in an attempt to win the race, but upon losing the race, those resources become unproductive. In either its strong or weak form, the foreclosure theory is consistent with the negative coefficient on external patenting.

Second, and alternatively, licensing of foreign technology, while utility-maximizing for the particular firm at issue, may involve a negative externality for the state as a whole. If private capital or internal patenting has increasing returns to scale—the returns for which are external to the individual actor, but internal to the state as a whole—then a firm's decision to license an external patent, while welfare-maximizing for the firm, may not be for the state as a whole. For example, three domestic firms may each be considering whether to license foreign technology or invent around the foreign patent. For each of the firms individually, it is less expensive to license than to invent around. Thus, taking a license is individually rational. However, if the three firms shared the cost, inventing around would be less expensive than licensing. The individual decisions to license thus entail a negative externality for the state as a whole. If the negative externality reflected in this example, or a similar negative externality, represents a common consequence of licensing external patents, then the practice of licensing external patents may entail not only these externality costs associated with discrete licenses, but may breed a cycle of dependence on foreign patents. If I assume that a state's ability to undertake future innovation or the cost of such innovation depends upon a state's active role in past innovation in the field, then licensing external patents may impair the state's ability to undertake future innovation in the field. In making the decision to license an external patent, rather than invent around, a domestic firm will bear only its own costs of licensing and not the similar cost to other domestic firms. As a result, any one domestic firm is unlikely to bear the full costs of the state's dependence on external patenting, suggesting that licensing of external patents will often entail a negative externality. In any event, whatever its precise nature, such a dependence or negative externality hypothesis is consistent with the long-term, statistically significant negative correlation between external patents and growth.

Before placing too much reliance on the results of this initial set of regressions, however, two difficulties must be acknowledged. First, in an open economy, such as the continental United States, there is likely to be considerable factor mobility.[81] An endogeneity problem therefore arises in the first regression. Higher per capita patenting or more rapid human capital growth may be due to factor movement toward states with higher growth rates in per capita income. This endogeneity problem creates a problem for causal inferences—I cannot say whether higher per capita patenting leads to higher growth rates or whether higher growth rates lead to higher per capita patenting—and also biases our regression results. Second, equation (5) omits any variation between the states in private capital accumulation. This omission, given the likely importance private capital accumulation plays in explaining cross-state variation in per capita income growth rates, creates a risk of omitted variable bias in the parameter estimates. To address these concerns, I performed two additional sets of regressions.

B. A Second Look with Annual Data: Patenting and Capital

To avoid the problems of omitted variable bias and endogeneity, the first additional set of regressions expressly adds capital to the

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81. Factor mobility refers to the physical and legal ability of moving capital or labor from one state to another.

regression and uses lagged values[82] for most of the independent variables. To exploit the information in Munnell's data on state-by-state private and public capital, I can augment her basic equation with internal and external patenting. I therefore estimate:

$$\ln(Q_t) = a + b\ln K_{t-1} + c\ln L_t + d\ln G_{t-1} + e\ln T_{t-1} + f\ln T_{us,t-1} + g\ln T_{reg,t-1} + \varepsilon \quad (6)$$

where:

Q_t is the state's real economic output, measured as gross state product, in each year from 1970 through 1986;

K_{t-1} is the state's private capital stock with a one-year lag;

L_t is the state's labor supply, measured as total employment on nonagricultural payrolls from the Bureau of Labor;

G_{t-1} is the state's public capital stock with a one-year lag;

T_{t-1} is the state's internal patenting with a one-year lag;

$T_{us,t-1}$ is the number of patents issued to residents of the United States, other than the state at issue, with a one-year lag; and

$T_{reg,t-1}$ is the number of patents issued to residents of a state's geographic region, other than the state at issue, with a one-year lag.

In addition to the external patenting by residents of the United States as a whole, I also included a variable reflecting external patenting by the residents of states in the same economic region, $T_{reg,t-1}$.[83] I included this variable to examine whether geographic proximity changes the role of external patenting. Beginning with the work of Jaffe, Henderson, and Trajtenberg, a literature has developed using patent citation data to trace information flows.[84] One of the key findings from these studies is a high degree of geographic

82. In my first regression, the endogeneity problem arises because both the dependent variable (growth in real per capita income) and the independent regressors (growth in human capital and per capita internal and external patenting) cover the same fifty-year time period, 1951–2000. Economists typically use lagged variables to address this problem. Thus, rather than look at whether there is a correlation between a state's economic output and patenting in the same year, I examine whether patenting in one year is correlated with the economic output in the next year. This technique solves both the dual causality problem, following the intuition that past events influence the future rather than the other way around, and the statistical problem of biased coefficients by eliminating the correlation between the lagged regressor and the error term associated with the dependent variable.

83. In identifying states in the same geographic regions for purposes of this external regional patenting variable, I follow the BEA's definition of geographic regions. *See* Bureau of Economic Analysis, Regional Economic Accounts, BEA Regions, http://www.bea.gov/regional/docs/regions.cfm (last visited Apr. 24, 2009).

84. Adam B. Jaffe et al., *Geographic Localization of Knowledge Spillovers as Evidenced by Patent Citations*, 108 Q.J. ECON. 577, 577–98 (1993).

localization.[85] Patents tend to cite prior art patents from their own geographic area, suggesting that knowledge remains localized.[86] However, Alcácer and Gittelman have questioned whether these results are reliable.[87] As they show, many of the citations found in patents are inserted by the patent examiner or by the inventor's attorney, rather than by the inventor.[88] For that reason, aggregate citation data may not reflect knowledge flows so much as they reflect the written and unwritten rules governing patent practice. Because the regional patenting variable in this study is not susceptible to such bias, including it in the regression may help address the question of geographic spillovers.

The equation is estimated using pooled annual data for the forty-eight continental states from 1970 through 1986. Following Munnell, one year lags are used on private and public capital and the patenting data in order to address the endogeneity problem that might otherwise arise in an open economy. Three additional regressions were performed as robustness checks, the first omitting $T_{reg,t-1}$, reported as Column 3 in Table 3, the second omitting $T_{us,t-1}$,[89] and the third omitting both external patenting variables, reported as Column 2 in Table 3. A regression of private capital and employment alone was also performed in order to compare the output elasticities of these factors to the historic norms. Table 3 summarizes the results.[90]

85. Giovanni Peri, *Determinants of Knowledge Flows and Their Effects on Innovation*, 87 REV. ECON. & STAT. 308, 320 (2005) (using patent citation data to trace the flow of information and finding that "only 20% of the knowledge generated in the average region flows out of it").

86. *See* Jaffe et al., *supra* note 84, at 591 ("Before moving on, the results on the extent of localization can be summarized as follows. For citations observed by 1989 of 1980 patents, there is a clear pattern of localization at the country, state, and SMSA levels.").

87. Juan Alcácer & Michelle Gittelman, *Patent Citations as a Measure of Knowledge Flows: The Influence of Examiner Citations*, 88 REV. ECON. & STATS. 774, 774 (2006).

88. *See id.* (finding that examiners account for sixty-three percent of the citation in the average patent).

89. As this was merely a robustness check, I omitted the results from this regression from Table 3. There were no statistically significant differences in the coefficient results from this regression.

90. For Table 3, Q_t is the state's real economic output, measured as gross state product, in each year from 1970 through 1986; K_{t-1} is the state's private capital stock with a one-year lag; L_t is the state's labor supply, measured as total employment on nonagricultural payrolls from the Bureau of Labor; G_{t-1} is the state's public capital stock with a one-year lag; T_{t-1} is the state's internal patenting with a one-year lag; $T_{us,t-1}$ is the number of patents issued to residents of the United States, other than the state at issue, with a one-year lag; and $T_{reg,t-1}$ is the number of patents issued to residents of a state's geographic region, other than the state at issue, with a one-year lag; and *p* values are in parentheses.

Table 3: State Output as a Function of Public and Private Capital, Labor, and Patenting

Dependent Variable: Log of Real Annual State Output, 1970–1986

Regressor	(1)	(2)	(3)	(4)
Intercept	1.945 (< 0.0001)	1.731 (< 0.0001)	2.798 (< 0.0001)	2.839 (< 0.0001)
$ln\ K_{t-1}$	0.351 (< 0.0001)	0.368 (< 0.0001)	0.367 (< 0.0001)	0.357 (< 0.0001)
$ln\ L_t$	0.696 (< 0.0001)	0.506 (< 0.0001)	0.488 (< 0.0001)	0.494 (< 0.0001)
$ln\ G_{t-1}$	--	0.0957 (< 0.0001)	0.100 (< 0.0001)	0.103 (< 0.0001)
$ln\ T_{t-1}$	--	0.0726 (< 0.0001)	0.0819 (< 0.0001)	0.0841 (< 0.0001)
$ln\ T_{us,t-1}$	--	--	-0.0967 (< 0.0001)	-0.0830 (< 0.0001)
$ln\ T_{reg,t-1}$	--	--	--	-0.0212 (< 0.0001)
Adj. R^2	0.9917	0.9940	0.9942	0.9944
No. Obs.	816	816	816	816

As Table 3 reflects, all coefficients were statistically significant at the one percent level.

In the first and simplest regression, I included only private capital and labor as explanatory variables, and as the results reported in Column 1 reflect, this regression generates output elasticities for these factors close to the historic 35-65 labor-capital split of national income. The two coefficients also sum to 1.046, which is statistically indistinguishable from the common assumption of constant returns to scale.[91]

When I add the patenting variables, my regression results for the annual data confirm the results obtained in my initial regression for the fifty-year averaged data. Increased internal patenting is correlated with increased output; external patenting is associated with reduced output. Here, the coefficients reflect the output elasticities of these factors. In other words, using the full regression results reported in Column 4, a 1% increase in a state's level of internal

91. A separate regression was performed that included a constant returns to scale restriction. For the restriction, $F[1, 813]=176.69$, with a p value of < 0.0001.

patenting is associated with a 0.0841% increase in state output, *ceteris paribus*. Similarly, holding all else constant, a 1% increase in external United States patenting, or in external regional patenting, is associated with a 0.083%, or a 0.0212%, respectively, decrease in a given state's output.

When I consider the regressions which include patenting, the output elasticities of the factor inputs sum to 1.043 with internal patenting only (Column 2, sum of coefficients on labor, public and private capital, and internal patenting), and is again statistically indistinguishable from constant returns to scale.[92] In contrast, when external patenting is included, the coefficients sum to 0.941 with $T_{us,t-1}$ alone (Column 3, sum of coefficients), and to 0.936 with both external patenting variables (Column 4, sum of coefficients), but these results are again statistically indistinguishable from constant returns to scale.[93] Moreover, it is also interesting to note that the magnitude of the positive output elasticity of internal patenting (0.0841, from Column 4) is just slightly more than the absolute value of the negative output elasticity of external U.S. patenting (-0.0830, from Column 4). This rough equivalence suggests that a state's output will fall if external patenting increases and the state's internal patenting fails to keep pace. The rough equivalence, but opposite signs, of internal and external patenting also reinforces the dependence hypothesis. Where firms within a state license external patents, rather than invent around them, a state risks falling further behind in its economic output.

1. Fixed Effects Model

Again, however, statistical problems exist with this regression of the annual data. The first is that even adding in variables for public and private capital, other state-specific variations likely remain, such as differences in a state's natural resources or proximity to international markets, that may affect a state's economic output. If these omitted variables are material and correlate with one of our included regressors, this omission creates a risk of omitted variable bias. Rather than attempt to add additional variables for each factor that might influence a state's economic output, I estimated a fixed

92. A separate regression was performed that included a constant-returns-to-scale restriction. For the restriction, $F[1, 811]=180.25$, with a p value of < 0.0001.

93. Separate regressions were performed that included a constant-returns-to-scale restriction. For the restriction with external U.S. patenting, $F[1, 810]=10.31$, with $p=0.014$. For the restriction with both external U.S. and external regional patenting, $F[1, 809]=12.57$, with $p=0.0004$.

effects model. A fixed effects model adds a constant for each year, or for each year and each state, to the regression in an attempt to account for year-specific or state-specific variables that are otherwise omitted. Thus, if, for example, Texas has greater natural resources, such as oil or natural gas than Delaware, and that difference plays some role in explaining the two states' respective economic growth, then adding a dummy variable that takes the value of one for Texas and zero for Delaware can account for that difference in two states' starting conditions. Similarly, if an external shock, such as the 1973 Arab oil embargo, caused economic output for all of the states to vary in 1973, as compared to the other years, then adding a dummy variable which takes the value of one for the year 1973, and zero for all other years, can help isolate that effect and thereby improve our ability to identify accurately the relationship between a state's economic output on the one hand and a state's capital, labor, and patenting on the other.

After running these fixed effect regressions, I tested whether the fixed effects model improved our ability to isolate the relationships between output, capital, labor, and patenting, using an F test. An F test compares the overall fit of the basic model with the overall fit of the fixed effect models. In this case, the F statistics for testing the joint significance of year-specific effects (which compares the overall fit of the basic model with the overall fit of a fixed effect model that includes a dummy variable for each year), and for state- and year-specific effects (which compares the overall fit of the basic model with the overall fit of a fixed effect model that includes dummy variables for each state and each year), are $F[15, 795]=6.679$ and $F[46, 749]=63.035$, respectively. The F statistics are above the value for statistical significance at the one percent level.[94] This means that there are omitted state-specific and year-specific variables that are material. I should therefore rely on the coefficients from the full fixed effects model to establish the correlation between state-level patenting and output. Table 4 summarizes the results from the fixed effects models.[95]

94. For $F[15,795]$ and $F[46,749]$, the one percent critical values are 2.14 and 1.68, respectively. With 1970 as the omitted year, the year-effect coefficients for 1973 and 1986 were statistically significant at the ten percent level: the 1973 coefficient was 0.03344 (p=0.0410), and the 1986 coefficient was 0.2206 (p=0.0931). With Alabama as the omitted state, thirty-six of the state-effect coefficients were statistically significant at the ten percent level.

95. In Table 4, Q_t is the state's real economic output, measured as gross state product, in each year from 1970 through 1986; K_{t-1} is the state's private capital stock with a one-year lag; L_t is the state's labor supply, measured as total employment on nonagricultural

Table 4: State Output: Fixed Effects Model

Dependent Variable: Log of Real Annual State Output, 1970–1986

Regressor	Year Effects	Both State and Year Effects
Intercept	11.730 (< 0.0001)	-1.8082 (0.767)
$\ln K_{t-1}$	0.356 (< 0.0001)	0.196 (< 0.0001)
$\ln L_t$	0.501 (< 0.0001)	0.777 (< 0.0001)
$\ln G_{t-1}$	0.0939 (< 0.0001)	-0.1031 (< 0.0001)
$\ln T_{t-1}$	0.0720 (< 0.0001)	0.0625 (< 0.0001)
$\ln T_{us,t-1}$	-0.8952 (< 0.0001)	0.518 (0.351)
$\ln T_{reg,t-1}$	-0.01971 (< 0.0001)	-0.0333 (0.0674)
Adj. R^2	0.9949	0.9989
No. Obs.	816	816

With year-specific effects, the results remain essentially unchanged across the board. However, when the model incorporates dummies for both state-specific and year-specific effects, a number of the coefficients change significantly. In the full fixed effects model, internal patenting remains significantly positive at the one percent level, and external regional patenting remains significantly negative, at least at the ten percent level. External U.S. patenting switches, however, from significantly negative in the year-effects regression to not statistically different from zero in the full fixed effect model. This switch likely reflects omitted variable bias in the basic regression. When I add the dummy variables for year- and state-specific effect and remove that bias, I find that external U.S. patenting is not negatively associated with a state's economic growth.

In determining whether to accord much weight to full fixed effects model and the nominal sign change on the coefficient for

payrolls from the Bureau of Labor; G_{t-1} is the state's public capital stock with a one-year lag; T_{t-1} is the state's internal patenting with a one-year lag; $T_{us,t-1}$ is the number of patents issued to residents of the United States, other than the state at issue, with a one-year lag; $T_{reg,t-1}$ is the number of patents issued to residents of a state's geographic region, other than the state at issue, with a one-year lag; and *p* values are in parentheses.

external U.S. patenting, two additional points deserve emphasis. First, the coefficients on private capital and labor in the full fixed effects model differ substantially from the historical 35-65 division of national income (0.196, with $p < 0.0001$, for capital, and 0.777, with $p < 0.0001$, for labor). Second, the coefficient on public capital becomes negative and statistically significant in the full fixed effects model (-0.1031, $p < 0.0001$). These results are inconsistent with my theoretical expectations and raise some questions regarding the reliability of my results.

2. Patenting Versus R&D Expenditures

In order to distinguish patenting from research efforts, I performed an additional set of regressions, adding state-level research and development ("R&D") expenditures as reported by the National Science Foundation ("NSF"). In the literature, R&D spending is usually taken as a measure of the economic resources devoted to creating new products and services, while patenting is usually taken as a measure of the success of such activities.[96] Including R&D expenditures in my analysis may therefore cast some light on the respective roles that research efforts and research success play in increasing economic output. Because the NSF R&D data set is not complete,[97] the regressions follow the approach of Hall and Ziedonis and include a dummy variable, *NTREP*, equal to one when the state's R&D expenditures are not reported in a particular year.[98]

For this analysis, equation (6) becomes:

$$\ln(Q_t) = a + b\ln K_{t-1} + c\ln L_t + d\ln G_{t-1} + e\ln T_{t-1} + fNTREP + g\ln RDEXP + \varepsilon \quad (6a)$$

where: *NTREP* is a dummy variable set to one where data for the state's R&D expenditures are not reported for that year, and to zero otherwise; and *RDEXP* is the state's reported R&D expenditures with a one-year lag.

Four regressions were performed. The first included the state-level R&D variables but omitted all patenting terms (as is reported in

96. *See, e.g.*, Iain Cockburn & Zvi Griliches, *Industry Effects and Appropriability Measures in the Stock Market's Valuation of R&D and Patents*, 78 AM. ECON. REV. 419, 422 (1988) ("Data on R&D expenditures, where available, are stronger measures of input to the process by which firms produce technical innovation than patents are of its 'output.' ").

97. Of the 816 possible observations of a state's annual R&D expenditures, 371 observations (or 45.5%) were missing.

98. Bronwyn H. Hall & Rosemarie Ham Ziedonis, *The Patent Paradox Revisited: An Empirical Study of Patenting in the U.S. Semiconductor Industry, 1979–1995*, 32 RAND J. ECON. 101, 116 (2001).

Column 1); the second added internal patenting (and is reported in Column 2); the third added external U.S. patenting (and is reported in Column 3); and the fourth added external regional patenting to the regression (and is reported in Column 4). Table 5 summarizes the results.[99]

Table 5: State Output as a Function of Public and Private Capital, Labor, and Patenting

Dependent Variable: Log of Real Annual State Output, 1970–1986

Regressor	(1)	(2)	(3)	(4)
Intercept	1.677 (< 0.0001)	1.733 (< 0.0001)	2.745 (< 0.0001)	2.785 (< 0.0001)
$ln\ K_{t-1}$	0.315 (< 0.0001)	0.368 (< 0.0001)	0.368 (< 0.0001)	0.357 (< 0.0001)
$ln\ L_t$	0.578 (< 0.0001)	0.504 (< 0.0001)	0.486 (< 0.0001)	0.496 (< 0.0001)
$ln\ G_{t-1}$	0.147 (< 0.0001)	0.0963 (< 0.0001)	0.101 (< 0.0001)	0.103 (< 0.0001)
NTREP	0.0270 (0.001)	0.0230 (0.0019)	0.007 (0.376)	0.0059 (0.450)
ln RDEXP	0.010 (< 0.0001)	0.0041 (0.0118)	0.00253 (0.1231)	0.00152 (0.348)
$ln\ T_{t-1}$	--	0.0707 (< 0.0001)	0.0793 (< 0.0001)	0.0825 (< 0.0001)
$ln\ T_{us,t-1}$	--	--	-0.0914 (< 0.0001)	-0.0782 (< 0.0001)
$ln\ T_{reg,t-1}$	--	--	--	-0.0208 (< 0.0001)
Adj. R^2	0.9926	0.9940	0.9942	0.9944
No. Obs.	816	816	816	816

99. In Table 5, the dependent variable, Q_t, is the state's real economic output, measured as gross state product, in each year from 1970 through 1986; K_{t-1} is the state's private capital stock with a one-year lag; L_t is the state's labor supply, measured as total employment on nonagricultural payrolls from the Bureau of Labor; G_{t-1} is the state's public capital stock with a one-year lag; T_{t-1} is the state's internal patenting with a one-year lag; $T_{us,t-1}$ is the number of patents issued to residents of the United States, other than the state at issue, with a one-year lag; $T_{reg,t-1}$ is the number of patents issued to residents of a state's geographic region, other than the state at issue, with a one-year lag; *NTREP* is a dummy variable set to one where data for the state's R&D expenditures are not reported for that year, and to zero otherwise; *RDEXP* are the state's reported R&D expenditures with a one-year lag; and *p* values are in parentheses.

As Table 5 reflects, the sign of the state-level R&D expenditure coefficients are positive, as expected. However, as additional patenting variables are added to the regression, the state-level R&D expenditures coefficient becomes statistically insignificant and also falls from 0.010 in Column 1 to 0.00152 in the full regression (Column 4). This is likely due to some degree of multicollinearity between R&D expenditures and patenting activity. The correlation between the natural logs of internal patenting and state-level R&D expenditures for the data set was 0.70 ($p < 0.0001$). However, using the auxiliary regression technique, the natural log of reported R&D expenses was regressed against the remaining right-hand side variables in equation (6a), and the adjusted R^2 was 0.689. This is less than the adjusted R^2 for the basic model; I should not therefore expect multicollinearity to prevent precise parameter estimates.

Interestingly, this macroeconomic relationship is precisely the opposite of what Cockburn and Griliches find in their analysis of the contribution of a firm's inventiveness to its market value.[100] In their analysis, the patent variables become insignificant when R&D variables are added to their regression, leading them to conclude that R&D expenditures are better measures of input to the innovative function of firms than patents are of their output. For economic growth at the macroeconomic rather than firm level, Table 5 suggests that patenting activity is a better measure of innovation's contribution to growth than R&D expenditures. Along the same lines, for each of the regression results that incorporate internal patenting, the elasticity with respect to internal patenting is significantly higher than the elasticity with respect to R&D expenditures.

3. Model Specification: Levels or Differences

So far I have performed all of my regressions with this annual data using the level of capital, labor, or patenting in a given year as my regressors. The use of levels in these regressions necessarily assumes that the error terms are serially independent and that the variables are stationary. In response to Alicia Munnell's analysis showing a positive correlation between public capital and state economic output,[101] along with a similar analysis reaching similar results by Aschauer,[102] a number of economists pointed out the risk of

100. *See* Cockburn & Griliches, *supra* note 96, at 420–21.

101. *See* Munnell & Cook, *supra* note 21, at 93–95.

102. *See* David Alan Aschauer, *Is Public Expenditure Productive?*, 23 J. MONETARY ECON. 177, 177-200 (1989).

performing these macroeconomic regressions in levels and suggested that the regressions should be performed using first differences[103] rather than levels to avoid the risk of spurious correlation.[104] To test my data for these issues, I can use a test that Bhargava, Franzini, and Narendranathan provided.[105] Calculating their test statistic for the residuals from the full fixed effects model presented in Table 4 yields a modified Durbin-Watson test statistic, d_p=0.4314. The null hypothesis of serial independence in the error term is therefore rejected at the five percent level of significance. Rejecting the null hypothesis suggests in turn that the error terms of my regressors may be serially correlated and that running the regression in levels creates a risk of spurious correlation—that the model will identify a statistically significant correlation between one of my regressors, such as internal patenting, and a state's economic output when such a correlation does not in fact exist. To avoid the risk of spurious correlations, I should use a regression in differences rather than levels.

To estimate equation (6) in differences, I start with equation (6) itself, for the year *t*:

$$\ln(Q_t) = a + b\ln K_{t-1} + c\ln L_t + d\ln G_{t-1} + e\ln T_{t-1} + f\ln T_{us,t-1} + g\ln T_{reg,t-1} + \varepsilon_t \qquad (6)$$

I can also write equation (6) for the previous year, *t-1*:

$$\ln(Q_{t-1}) = a + b\ln K_{t-2} + c\ln L_{t-1} + d\ln G_{t-2} + e\ln T_{t-2} + f\ln T_{us,t-2} + g\ln T_{reg,t-2} + \varepsilon_{t-1} \qquad (6')$$

To perform the regression in differences, I simply subtract equation (6′) from equation (6). Grouping similar terms, I can rewrite the difference between equation (6′) and equation (6) as:

$$\Delta\ln(Q_t) = a + b\Delta\ln K_{t-1} + c\Delta\ln L_t + d\Delta\ln G_{t-1} + e\Delta\ln T_{t-1} + f\Delta\ln T_{us,t-1} + g\Delta\ln T_{reg,t-1} + \varepsilon \qquad (6b)$$

where:

$\Delta ln(Q_t)=ln(Q_t)-ln(Q_{t-1})$ and Q_t is the state's real economic

103. A regression in levels looks for a correlation between the level of patenting in a given year and the state's level of economic output in the following year. A regression in differences looks at how the change in patenting from one year to the next affects the change in state economic output from one year to the next. For an explanation of nonstationary time series econometric modeling, see generally GEORGE E.P. BOX ET AL., TIME SERIES ANALYSIS: FORECASTING AND CONTROL 93–136 (4th ed. 2008).

104. *See* Teresa Garcia-Milà et al., *The Effect of Public Capital in State-Level Production Functions Reconsidered*, 78 REV. ECON. & STAT. 177 (1996); Douglas Holtz-Eakin, *Public-Sector Capital and the Productivity Puzzle*, 76 REV. ECON. & STAT. 12 (1994).

105. A. Bhargrava, L. Franzini & W. Narendranathan, *Serial Correlation and the Fixed Effects Model*, 49 REV. ECON. STUD. 533, 534–36 (1982).

output, measured as gross state product, in each year from 1970 through 1986;

$\Delta ln(K_{t-1})=ln(K_{t-1})-ln(K_{t-2})$ and K_{t-1} is the state's private capital stock with a one-year lag;

$\Delta ln(L_t)=ln(L_t)-ln(L_{t-1})$ and L_t is the state's labor supply, measured as total employment on nonagricultural payrolls from the Bureau of Labor;

$\Delta ln(G_{t-1})=ln(G_{t-1})-ln(G_{t-2})$ and G_{t-1} is the state's public capital stock with a one-year lag;

$\Delta ln(T_{t-1})=ln(T_{t-1})-ln(T_{t-2})$ and T_{t-1} is the state's internal patenting with a one-year lag;

$\Delta ln(T_{us,t-1})=ln(T_{us,t-1})-ln(T_{us,t-2})$ and $T_{us,t-1}$ is the number of patents issued to residents of the United States, other than the state at issue, with a one-year lag; and

Δln(Treg,t-1)=ln(Treg,t-1)-ln(Treg,t-2) and Treg,t-1 is the number of patents issued to residents of a state's geographic region, other than the state at issue, with a one-year lag.

When I first difference the equation, the dummy variables for each state and year found in our fixed effects model run in levels fall out. Despite that mathematical result, dropping the dummy variables would nevertheless be inappropriate. I use the dummy variable or fixed effects model to minimize the risk of omitted variable bias. Specifically, if state-specific or year-specific considerations (other than labor, capital, and patenting) that play a role in a state's economic performance are omitted, yet correlate with patenting, then our coefficients will be biased. Ideally, to account for all of the potential omitted variables—both those that are constant, such as the natural resources or geographic proximity of the state to export markets, and those that may vary from year-to-year, such as the amount of natural resources produced or the volume of imports/exports passing through—I should include dummy variables for each state for each year. If I did so, however, I would have more coefficients to solve for than observations, and I could not solve my model. For that reason, as is typical for macroeconomic studies, the fixed effects model included only one dummy for each state and one for each year, rather than one for each state, each year.

While it is true that first differencing the equation eliminates, as a mathematical matter, the dummy variables I was able to include, first differencing does not eliminate the risk of omitted variable bias that the dummy variables were intended to address. While in a first differenced equation, state-specific factors that remain constant from year-to-year fall out, there may nonetheless remain state-specific considerations that both vary year-to-year and affect the state's economic performance. If these considerations are omitted and correlate with one of my regressors, they create the same type of risk of omitted variable bias in my first differenced regression that justified the use of dummies for each state and each year in my base regression. As a result, equation (6b) was estimated: (i) without effects, (ii) with year-specific effects, and (iii) with both year- and state-specific effects. The *F* statistic for testing the joint significance for state- and year-specific effects for the difference regression is $F[61, 699]=6.64$—again above the value for statistical significance at the one percent level.[106] This suggests that, even in differences, the fixed effects model should be used. Table 6 presents the results for both the without effects model and the full fixed effects model.[107]

106. For $F[61, 699]$, the one percent critical value is 1.59. With 1971 as the omitted year, the year-effect coefficients for 1974, 1977, and 1978 were statistically significant at the ten percent level: the 1974 coefficient was -0.06765 ($p=0.0265$), the 1977 coefficient was -0.01819 ($p=0.0175$), and the 1978 coefficient was -0.01485 ($p=0.0008$). With Alabama as the omitted state, eight state-effect coefficients were statistically significant at the ten percent level.

107. In Table 6, $\Delta ln(Q_t)=ln(Q_t)-ln(Q_{t-1})$ and Q_t is the state's real economic output, measured as gross state product, in each year from 1970 through 1986; $\Delta ln(K_{t-1})=ln(K_{t-1})-ln(K_{t-2})$ and K_{t-1} is the state's private capital stock with a one-year lag; $\Delta ln(L_t)=ln(L_t)-ln(L_{t-1})$ and L_t is the state's labor supply, measured as total employment on nonagricultural payrolls from the Bureau of Labor; $\Delta ln(G_{t-1})=ln(G_{t-1})-ln(G_{t-2})$ and G_{t-1} is the state's public capital stock with a one-year lag; $\Delta ln(T_{t-1})=ln(T_{t-1})-ln(T_{t-2})$ and T_{t-1} is the state's internal patenting with a one-year lag; $\Delta ln(T_{us,t-1})=ln(T_{us,t-1})-ln(T_{us,t-2})$ and $T_{us,t-1}$ is the number of patents issued to residents of the United States, other than the state at issue, with a one-year lag; $\Delta ln(T_{reg,t-1})=ln(T_{reg,t-1})-ln(T_{reg,t-2})$ and $T_{reg,t-1}$ is the number of patents issued to residents of a state's geographic region, other than the state at issue, with a one-year lag; and *p* values are in parentheses.

Table 6: State Output as a Function of Labor, Private and Public Capital, and Patenting—First Differences

Dependent Variable: First Difference Log of Real Annual State Output, 1971–1986

Regressor	No Effects	Both State and Year Effects
Intercept	0.00777 (< 0.0001)	0.0399 (0.192)
$\Delta ln\ K_{t-1}$	-0.0588 (0.0130)	-0.0107 (0.604)
$\Delta ln\ L_t$	1.0377 (< 0.0001)	1.028 (< 0.0001)
$\Delta ln\ G_{t-1}$	-0.1120 (0.0153)	0.00852 (0.885)
$\Delta ln\ T_{t-1}$	0.00298 (0.6165)	0.0114 (0.0344)
$\Delta ln\ T_{us,t-1}$	0.00068 (0.9757)	0.1861 (0.666)
$\Delta ln\ T_{reg,t-1}$	0.01416 (0.4736)	0.1034 (< 0.0001)
Adj. R^2	0.6683	0.7830
No. Obs.	768	768

Running the regression in differences changes the results with respect to external regional patenting significantly. Instead of being negative and statistically significant, as it was in the levels model, the external regional patenting coefficient in the first difference fixed effects model is positive and statistically significant. The internal patenting coefficient remains positive and statistically significant, while the external U.S. patenting coefficient remains statistically indistinguishable from zero. This means that a state's own patenting in a given year, as well as patenting activity by the residents of states in the same economic region, are both associated with increased economic output for the state in the following year. In contrast, patenting activity in a given year by the residents of states outside of a state's own economic region is not associated with either an increase or a decrease in that state's economic output in the following year.

That the external regional patenting coefficient is positive and statistically significant provides some support for the free trade view of TRIPs. In a regime of uniform high patent protection, patenting by one state is associated with increased economic output by its neighboring states. However, as Munnell has noted in response to her critics,[108] switching to differences also generates coefficients for private capital and labor that are inconsistent with the theoretical predictions. This inconsistency raises questions regarding the reliability of my regression results.

Moreover, the use of annual data implicitly assumes that the full effects of capital investments and technological innovation are felt in, and restricted to, the next year's economic output. If I assume that a state's capital investment or its patenting decreases sharply in a given year, whether and how that change affects economic performance is likely to depend on whether the decrease is part of a continuing trend of such decreases or a blip in otherwise uniform increases in capital or patenting. Because both capital investments and technological innovation are likely to influence state economic performance over more than an annual period, an annual model may not accurately or fully capture the role of patenting and capital investments in economic growth.

C. *A Third Look with Ten-Year Averages: Patenting and Human Capital*

To explore a longer timeframe for capital and technological effects, and to examine more carefully the relationship between patenting and human capital, I performed a third set of regressions. I have data for human capital and patenting since 1950,[109] and I used this data in my initial regression to estimate the relationships between growth rates, human capital growth, and internal and external patenting per capita over the last half of the twentieth century. Yet, I did not use all of the data available. Given that I have data points for human capital every ten years—i.e. 1950, 1960, 1970, 1980, and 1990—I can average a state's real per capita income, as well as internal and external per capita patenting, for ten-year periods centered on the dates of the human capital data points. Although I do not have private and public capital accumulations for this period, I can use a log of per capita income as a proxy for these terms and estimate:

$$\ln(\bar{y}_t) = a + b\ln(\bar{y}_{t-1}) + c\ln H_{t-1} + d\ln(\bar{t}_{t-1}) + e\ln(\bar{t}_{us,t-1}) + f\ln(\bar{t}_{reg,t-1}) + \varepsilon \qquad (7)$$

108. Alicia H. Munnell, *Policy Watch: Infrastructure Investment and Economic Growth*, J. ECON. PERSP., Fall 1992, at 189, 192–93.

109. For a description of the available data, see *supra* text accompanying notes 61–70.

where:

$\bar{y}_t$ is the state's average per capita real income over three ten-year periods from 1966–1975, 1976–1985, 1986–1995, 1996–2005;

$\bar{y}_{t-1}$ is the proxy for per capita capital and represents the state's average per capita real income over each ten-year period, lagged by one ten-year period, and thus beginning with 1956–1965;

H_{t-1} is the state's human capital at the mid-point of the ten-year periods, lagged by one ten-year period;

$\bar{t}_{t-1}$ is the state's average annual per capita internal patenting, defined as patents issued to residents of the state at issue, over each ten-year period, lagged by one ten-year period;

$\bar{t}_{us,t-1}$ is the average annual per capita external patenting, defined as patents issued to residents of the United States other than residents of the relevant state, over each ten-year period, lagged by one ten-year period; and

$\bar{t}_{reg,t-1}$ is the average annual per capita external regional patenting, defined as patents issued to residents of the state's economic region other than state's own residents, over each ten-year period, lagged by one ten-year period.

I cannot estimate equation (7) directly with ordinary least squares, however. Because a lagged value of the dependent variable is included on the right-hand side, the error term, ε is correlated with a dependent variable. Given this correlation, ordinary least squares estimation is inconsistent. Following Anderson and Hsiao,[110] I therefore first difference[111] equation (7) as follows:

$$[\ln(\bar{y}_t)-\ln(\bar{y}_{t-1})]=a+b[\ln(\bar{y}_{t-1})-\ln(\bar{y}_{t-2})]+c(\ln H_{t-1}-\ln H_{t-2})+d[\ln(\bar{t}_{t-1})-\ln(\bar{t}_{t-2})]$$
$$+e[\ln(\bar{t}_{us,t-1})-\ln(\bar{t}_{us,t-2})]+f[\ln(\bar{t}_{reg,t-1})-\ln(\bar{t}_{reg,t-2})+(\varepsilon_{i,t}-\varepsilon_{i,t-1})$$

I cannot estimate this equation directly either, however, because $[\ln(\bar{y}_{t-1})-\ln(\bar{y}_{t-2})]$ is correlated with the errors $(\varepsilon_{i,t}-\varepsilon_{i,t-1})$. I therefore instrument for $[\ln(\bar{y}_{t-1})-\ln(\bar{y}_{t-2})]$ using $\ln(\bar{y}_{t-2})$ and estimate:

$$\Delta\ln(\bar{y}_t)=a+b\ln(\bar{y}_{t-2})+c\Delta(\ln H_{t-1})+d\Delta\ln(\bar{t}_{t-1})+e\Delta\ln(\bar{t}_{us,t-1})+f\Delta\ln(\bar{t}_{reg,t-1})+\varepsilon \quad (7a)$$

110. T.W. Anderson & Cheng Hsiao, *Estimation of Dynamic Models with Error Components*, 76 J. AM. STAT. ASSOC. 598, 598–606 (1981).

111. We do not need to first difference the data in order to render it stationary. The Bhargrava, Franzini, and Narendranathan study (1982) modified Durbin-Watson test statistic is 1.663, rejecting the random walk null hypothesis. *See* Bhargrava et al., *supra* note 105, at 544.

As previously discussed, as a mathematical proposition, first differencing eliminates the dummy variables for state- and decade-specific fixed effects. Nonetheless, as with the annual data, the risk of omitted variable bias, which the fixed effects model attempts to address, persists. As a result, I regress equation (7a): (i) without fixed effects; (ii) with decade-specific effects; and (iii) with state- and decade-specific effects. Because the coefficients on the dummy variables for two of the decades and eight of the states were statistically significant at the five percent level, I should rely on the full fixed effects model. Results for the model without fixed effects and for the full fixed effects model for equation (7a) are presented in Table 7.[112]

112. In Table 7, the dependent variable, $\Delta[\ln(\bar{y}_t)-\ln(\bar{y}_{t-1})]$, is the difference in the natural logs of the state's average real per capita real income over four ten-year periods from 1966–1975, 1976–1985, 1986–1995, and 1996–2005, and the natural log of the state's average real per capita real income over the preceding ten-year period; $\ln(\bar{y}_{t-2})$ is the natural log of the state's average per capita income over the ten-year period, lagged by two ten-year periods, and thus beginning with 1946–1955, and is the instrument for $\Delta[\ln(\bar{y}_{t-1})-\ln(\bar{y}_{t-2})]$; $\Delta\ln H_{t-1}$ is the difference between the natural logs of state's human capital at the mid-point of the ten-year periods, lagged by one ten-year period; $\Delta\ln \bar{t}_{t-1}$ is the difference in the natural logs of the state's average annual per capita internal patenting, defined as patents issued to residents of the state at issue, over each ten-year period, lagged by one ten-year period; $\Delta\ln \bar{t}_{us,t-1}$ is the difference in the natural logs of the average annual per capita external patenting, defined as patents issued to residents of the United States other than residents of the relevant state, over each ten-year period, lagged by one ten-year period; $\Delta\ln \bar{t}_{reg,t-1}$ is the difference in the natural logs of the number of per capita patents issued to residents of a state's geographic region, other than the state at issue, lagged by one ten-year period; and p values are in parentheses.

Table 7: Regression Results
Ten-Year Averages, 48 States, 1966–2005

Dependent Variable: Ten-Year Averages of Real Per Capita Income

Regressor	Without Fixed Effects	With Full Effects
Intercept	2.2738 (< 0.0001)	0.9481 (0.1725)
$ln\ \bar{y}_{t-2}$	-0.21259 (< 0.0001)	-0.08841 (0.1947)
$\Delta ln\ H_{t-1}$	-0.11485 (0.0119)	-0.02758 (0.5949)
$\Delta ln\ \bar{t}_{t-1}$	0.07846 (< 0.0001)	0.04712 (0.0195)
$\Delta ln\ \bar{t}_{us,t-1}$	0.1455 (< 0.0001)	0.40654 (< 0.0001)
$\Delta ln\ \bar{t}_{reg,t-1}$	-0.1137 (< 0.0001)	0.025653 (0.5255)
Adj. R^2	0.6324	0.8523
No. Obs.	192	192

In the full fixed effects regression, only the coefficients on internal and external U.S. patenting are statistically significant. Both coefficients are output elasticities, indicating that a one percent increase in internal patenting or external U.S. patenting is associated with a 0.04%, or a 0.41%, respectively, increase in state real per capita income, all else constant.

In contrast, the coefficients on the human capital and the external regional patenting variables become statistically indistinguishable from zero. That these coefficients are not significant is surprising. First, given the endogenous growth theoretical models of Lucas and Romer,[113] and the cross-country empirical work of Mankiw, Romer, and Weil,[114] I should expect a positive correlation between human capital and per capita income. Second, and similarly, given the usual conclusion that innovation spillovers are geographically localized based upon patent citation data,[115] as well as my results for the annual data regressions,[116] I would

113. *See* Lucas, *supra* note 30; Romer, *supra* note 30.

114. *See* Mankiw et al., *supra* note 18.

115. *See* Jaffe et al., *supra* note 84, at 595 ("Despite the invisibility of knowledge spillovers, they do leave a paper trail in the form of citations. We find evidence that these trails, at least, are geographically localized. The results, particularly for the 1980 cohort, suggest that these effects are quite large and quite significant statistically."); Peri, *supra*

expect the regional external patenting coefficient to be positive and at least as significant as the external patenting coefficient for the United States as a whole. Yet, neither coefficient is statistically significant for the ten-year averaged data.

The lack of a positive correlation between growth in human capital and economic output raises questions regarding the reliability of my results. In contrast, the switch in the external regional patenting coefficient from statistically significant and positive in the annual data to not statistically different from zero in the ten-year averaged data may reflect the time required for knowledge diffusion.[117] On an annual basis, only patenting by neighboring states is associated with a state's economic output. In contrast, over a ten-year period, patenting not just by the residents of neighboring states, but by residents of other states, generally is associated with a state's economic output.

IV. EXAMINING WHETHER THE RELATIONSHIP BETWEEN PATENTING AND GROWTH VARIES FOR HIGH AND LOW PATENTING STATES

So far my results consistently suggest that internal patenting is positively correlated with a state's per capita income, output, and economic growth. Although more mixed, my results also suggest that external patenting—regional external patenting for the annual data and U.S. external patenting for the ten-year data—is also positively correlated with a state's per capita income, output, and economic growth. Yet, these results are based upon regressions that pool data for the forty-eight states, and are thus "on average" results. They implicitly assume that the relationship between internal and external patenting and economic output or growth is the same for both high and low patenting states. To check whether high and low patenting states respond similarly to internal and external patenting, I repeated

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note 85, at 320 (using patent citation data and finding that knowledge can remain geographically localized).

116. As reflected in Table 6, the external regional patenting coefficient for the annual data was positive and statistically significant. *See supra* text pp. 1504–05.

117. This corresponds with findings, made in studies tracing patent citations, that geographic localization of knowledge weakens over time. *See* Jaffe et al., *supra* note 84, at 596 ("We also find evidence that geographic localization fades over time. The 1980 citations, which have shorter average citation lags, are systematically more localized than the 1975 citations."). *But see* Peri, *supra* note 85, at 314 ("Estimates across the specifications (I through VI) in Table 2 are remarkably stable. Whether 2, 6, or 10 years elapse, the degree of relative geographic localization of knowledge remains rather stable.").

each of the three growth regressions, separating the data for high and low patenting states. To undertake this reexamination, I divided the forty-eight states into quartiles according to their average annual patenting per capita over the relevant time period. Beginning with the initial 1951-2000 regression set forth in equation (5), I re-estimated each of the regressions for the quartiles separately.

Table 8 reports the results for the regressions using the fifty-year averaged data.[118]

Table 8: Growth Regression
Internal and External Patenting Coefficients by Patenting Quartile

Dependent Variable: Average Annual Growth in Real Per Capita Income, 1951–2000

Regressor	Patenting Quartile			
	1st	2nd	3rd	4th
Intercept	0.08119 (0.5096)	0.1683 (0.0077)	0.06538 (0.09186)	0.1531 (0.0036)
$ln\ y_i$	-0.00815 (0.5112)	-0.01653 (0.01190)	-0.00751 (0.06263)	-0.01530 (0.0006)
$\dot{k}_h$	0.4534 (0.2055)	0.06189 (0.7200)	0.7038 (0.0064)	0.1422 (0.2763)
$\bar{t}$	8.0610 (0.2706)	34.9696 (0.415)	17.4 (0.337)	2.8106 (0.487)
$\bar{t}_{us}$	-0.02690 (0.2403)	-0.10338 (0.0578)	0.02782 (0.197)	0.03615 (0.1001)
Adj. R^2	0.325	0.779	0.687	0.901
No. Obs.	12	12	12	12

As Table 8 reflects, relatively few of the coefficients are statistically significant (possibly because of the limited number of observations for each data set). Yet, the signs for the convergence criteria, human capital, and internal patenting are, as expected, positive for each of the four quartiles. Although none of the internal patenting coefficients are statistically significant individually, the

118. For Table 8, the convergence criteria, $ln\ y_i$, is the natural log of per capita income in 1950; $\dot{k}_h$ is the state's average annual growth rate in human capital from 1950 through 2000; $\bar{t}$ is the average annual number of per capita patents issued to a lead inventor resident in the state from 1951 through 2000; t_{us} is defined as the average number of patents issued annually to residents of the United States, other than the state at issue; and p values are listed in parentheses.

likelihood that all four would be positive through random chance alone is only one in sixteen or p=0.06125. Only one of the four external patenting coefficients is statistically significant, and it is negative: -0.10338 (2nd Patenting Quartile, p=0.0578). However, in contrast to the consistently positive sign on the internal patenting coefficients, the signs on the external patenting coefficients switch from negative for the first and second quartiles to positive for the third and fourth quartiles. Thus, the fifty-year average data suggests that external patenting may have a different relationship with economic growth depending on a state's own level of inventiveness.

Before analyzing these results, the first task is to test their robustness. I therefore re-estimated equation (6b), for the annual data from 1970–1986, and equation (7a), for the ten-year averaged data covering 1966–2005, for the four patenting quartiles. I regressed the annual data in differences to avoid the risk of spurious correlation, and for both the annual and ten-year averaged data, I included both year- (or decade-) and state-specific fixed effects.[119] Table 9 reports the results for the annual data set,[120] and Table 10 reports the results for the ten-year average data set.[121]

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119. Statistical tests indicate that both effects were statistically significant for all four quartiles for both regressions.

120. For Table 9, $\Delta ln(Q_t)=ln(Q_t)-ln(Q_{t-1})$ and Q_t is the state's real economic output, measured as gross state product, in each year from 1970 through 1986; $\Delta ln(K_{t-1})=ln(K_{t-1})-ln(K_{t-2})$ and K_{t-1} is the state's private capital stock with a one-year lag; $\Delta ln(L_t)=ln(L_t)-ln(L_{t-1})$ and L_t is the state's labor supply, measured as total employment on nonagricultural payrolls from the Bureau of Labor; $\Delta ln(G_{t-1})=ln(G_{t-1})-ln(G_{t-2})$ and G_{t-1} is the state's public capital stock with a one-year lag; $\Delta ln(T_{t-1})=ln(T_{t-1})-ln(T_{t-2})$ and T_{t-1} is the state's internal patenting with a one-year lag; $\Delta ln(T_{us,t-1})=ln(T_{us,t-1})-ln(T_{us,t-2})$ and $T_{us,t-2}$ is the number of patents issued to residents of the United States, other than the state at issue, with a one-year lag; $\Delta ln(T_{reg,t-1})=ln(T_{reg,t-1})-ln(T_{reg,t-2})$ and $T_{reg,t-1}$ is the number of patents issued to residents of a state's geographic region, other than the state at issue, with a one-year lag; and p values are in parentheses.

121. For Table 10, the dependent variable, $\Delta[\ln(\bar{y}_t)-\ln(\bar{y}_{t-1})]$, is the difference between the state's average per capita real income over four ten-year periods from 1966–1975, 1976–1985, 1986–1995, and 1996–2005, and the average per capita real income over the preceding ten-year period; $\ln(\bar{y}_{t-2})$ is the natural log of the state's average per capita income over the ten-year period, lagged by two ten-year periods, and thus beginning with 1946–1955, and is the instrument for $\Delta[\ln(\bar{y}_{t-1})-\ln(\bar{y}_{t-2})]$; $\Delta\ln H_{t-1}$ is the difference between the natural logs of state's human capital at the mid-point of the ten-year periods, lagged by one ten-year period; $\Delta\ln \bar{t}_{t-1}$ is the difference in the natural logs of the state's average annual per capita internal patenting, defined as patents issued to residents of the state at issue, over each ten-year period, lagged by one ten-year period; $\Delta\ln \bar{t}_{us,t-1}$ is the difference in the natural logs of the average annual per capita external patenting, defined as patents issued to residents of the United States other than residents of the relevant state, over each ten-year period, lagged by one ten-year period; $\Delta\ln \bar{t}_{reg,t-1}$ is the difference in the natural logs of the number of per capita patents issued to residents of a state's

Table 9: Analysis of Annual Gross State Product Data By Patenting Quartile, First Differences, With Year and State Effects

Dependent Variable: First Difference Log of Real Annual State Output, 1971–1986

Regressor	Patenting Quartile			
	1st	2nd	3rd	4th
Intercept	0.0991 (0.0034)	0.05059 (0.5146)	0.1372 (0.2783)	-0.0508 (0.8979)
$\Delta ln\ K_{t-1}$	-0.02437 (0.4603)	-0.01546 (0.5919)	-0.04878 (0.2589)	0.0834 (0.1780)
$\Delta ln\ L_t$	1.2332 (< 0.0001)	1.0597 (< 0.0001)	0.9094 (< 0.0001)	0.9771 (< 0.0001)
$\Delta ln\ G_{t-1}$	0.1458 (0.2031)	-0.1584 (0.0238)	-0.1330 (0.2918)	0.0167 (0.8644)
$\Delta \ln\ T_{t-1}$	0.0832 (0.0001)	0.03005 (0.0075)	0.0127 (0.3344)	0.00544 (0.6883)
$\Delta \ln\ T_{us,t-1}$	1.0707 (0.0170)	0.45903 (0.6817)	1.5290 (0.4007)	-1.1637 (0.8395)
$\Delta \ln\ T_{reg,t-1}$	0.0428 (0.088)	0.01091 (0.6229)	0.1140 (0.0050)	0.2371 (< 0.0001)
No. Obs.	192	192	192	192

geographic region, other than the state at issue, lagged by one ten-year period; and *p* values are in parentheses.

Table 10: Analysis of Ten-Year Average Data By Patenting Quartile, With State- and Decade-Specific Effects

Dependent Variable: Ten-Year Averages of Real Per Capita Income, 1965–1995

Regressor	Patenting Quartile			
	1st	2nd	3rd	4th
Intercept	-0.1856 (0.9238)	-1.3156 (0.5759)	0.9480 (0.5037)	0.69448 (0.6854)
$\ln \bar{y}_{t-2}$	0.0232 (0.9041)	0.1391 (0.5514)	-0.08868 (0.5373)	-0.06553 (0.6957)
$\Delta\ln H_{t-1}$	0.0330 (0.6973)	0.0403 (0.799)	-0.1459 (0.2669)	0.1498 (0.1871)
$\Delta \ln \bar{t}_{t-1}$	0.0390 (0.3125)	0.06677 (0.1130)	0.02881 (0.5692)	0.03799 (0.5234)
$\Delta \ln \bar{t}_{us,t-1}$	0.0390 (0.0623)	0.06677 (0.1862)	0.02881 (0.5449)	0.03799 (0.0064)
$\Delta\ln \bar{t}_{reg,t-1}$	-0.0577 (0.3812)	-0.00528 (0.9435)	0.1059 (0.3083)	0.02646 (0.8444)
No. Obs.	48	48	48	48

As Tables 9 and 10 reflect, both the annual and ten-year data support the notion that, within a regime of uniformly high IPRs, external patenting is positively correlated with a state's economic performance for both high- and low-patenting states. With the annual 1970–1986 data, regressed in differences, the coefficient on the external regional patenting variable is positive for the first, third, and fourth patenting quartiles and becomes larger in magnitude for the third and fourth quartile (0.1140 and 0.2371, respectively) and more statistically significant (p=0.005, $p < 0.0001$, respectively) than for the first quartile (0.0428, p=0.088). Similarly, in the ten-year data regressions, the coefficient on the external U.S. patenting quartile is positive and statistically significant for both the first and fourth patenting quartile, and again both larger (0.7098 versus 0.4255) and more statistically significant for the fourth quartile ($p < 0.0064$ versus p=0.0623).

Although not perfectly robust, the results provide some support for the proposition that, within a regime of uniformly high IPRs, external patenting is more strongly correlated with a state's economic performance for states in the fourth patenting quartiles, or, in other words, for those states without a domestic patenting sector. These results also tentatively suggest that the potential loser in such a regime is not the low patenting states in the third and fourth quartile, but the intermediate patenting states in the second quartile. Of all the quartiles, the second patenting quartile is the only one with a negative and statistically significant coefficient on any external patenting variable (in the fifty-year averaged regression in Table 8) and also the only one that is not otherwise positive and statistically significant in either the annual or ten-year averaged data regressions.

As between the two explanations for a negative coefficient on external patenting, these results tend to support the foreclosure hypothesis over the dependence or negative externality hypothesis. Recall that under the foreclosure hypothesis,[122] external patenting slows a state's growth, either in the strong form, by foreclosing certain fields from domestic innovation, or in the weak form, by preventing a domestic firm from winning a given patent race and collecting the associated rents. If either foreclosure hypothesis is accurate, I should expect external patenting to have a stronger negative correlation for states with more internal patenting. These are the states with the most active innovation communities, and hence, the most to lose from foreclosure. In contrast, with dependence, I would expect external patenting to have either a uniformly negative correlation or a more negative correlation for states that have little internal patenting and are consequently more dependent on external patents for technological advances. Given that the coefficients on external patenting are more negative for states with more active patenting communities, the results tend to support the foreclosure rather than the dependence hypothesis. Inventors in states in the second patenting quartile may be engaging in, but consistently losing, patent races.

CONCLUSION

When I first started this Article, I must confess that I was somewhat skeptical of TRIPs as a mutually welfare-enhancing, free trade measure. After all, if high IPRs were beneficial for net-PR importing countries, it is difficult to explain why the United States and other net-IPR exporters had to persuade their trading partners to adopt higher IPRs through the GATT process. Nevertheless, the results presented in this Article, while not perfectly robust, are more consistent with the argument that TRIPs will prove mutually welfare-enhancing than with the argument that TRIPs is a form of rent-

122. See *supra* text pp. 1489–90.

seeking likely to benefit net IPR exporters alone. Under a free trade view of TRIPs, I would expect to find consistently positive coefficients on the external patenting variables. In contrast, if TRIPs were simply rent-seeking, I would expect to find a consistently negative relationship between external patenting and a state's economic performance. While I have presented a variety of models in this Article, the most reliable statistically are the first differenced, full fixed effects models with the annual and ten-year data. For both, the coefficients on external patenting are either: (i) positive and statistically significant or (ii) not statistically significant different from zero. Moreover, when I break the states down into patenting quartiles, the positive correlation between external patenting and a state's economic performance becomes more, not less, significant as a state's own patenting activity declines.

These conclusions are necessarily tempered by the limitations of the data available and the characteristics of my natural experiment. Data on private and public capital by state is available for only seventeen years. While patenting data is available for longer time periods, the patenting data has other limitations. First, not all patents are valuable, and not all patents are equally valuable, so "issued" patents is a noisy measure of the sub-category of "valuable" patents. Second, the Patent and Trademark Office assigns a patent to the state in which the first-named inventor resides. This is somewhat arbitrary given that a patent may have multiple inventors, some of whom may reside in different states. In addition, most patents are assigned in advance to a corporation, which may have shareholders in any number of states, so that the benefits of a patent will not flow exclusively to the state in which its first-named inventor resides. Additional work addressing these issues may improve my ability to identify the relationships between internal and external patenting and economic growth.

Even putting these limitations of the data to one side, I also cannot tell if the correlation between patenting and economic performance reflects directly the rents and other economic benefits derived from a patent's exclusion of others from making, using, or selling the patented invention, or if patenting is simply serving as a proxy for a state's otherwise unobserved inventiveness—inventiveness that might be present with or without a patent regime. The fact that patenting remains positively correlated with economic performance even when human capital and R&D expenditures are added separately to the regression provides some indication that patenting is not merely a proxy for a state's inventiveness. In addition, the fact that the relationship between external patenting and economic performance varies among states depending on their own level of patenting activity is also more consistent with patents as exclusionary rights than with patents as proxies for otherwise unobserved innovation. If patents simply reflected technological advance, it is hard to see why external patenting would have a more positive correlation with economic performance for the states in the third- and fourth-patenting quartiles than for states in the second. Nevertheless, further work may help establish the respective contributions these potential roles—exclusionary legal right or proxy for technological advance—play in the correlation between patents and economic performance.

Finally, because I am relying on a natural experimental framework, these results do not address whether a regime of high, low, or no IPRs is best. The regressions examine the correlations between patenting and economic performance in the United States, given the level of patent protection that was actually provided. My regressions did not—and given the structure of the natural experiment, could not—demonstrate how patenting, or the underlying inventive activity patenting represents, would correlate with economic performance in a regime with some other level of patent protection, or none at all.[123] Moreover, although they may fade with time, the differences in factor mobility of labor, capital, and information, and in cultural and legal homogeneity between the U.S. market and the international market generally caution against extrapolating these results directly to the international markets that TRIPs govern.

In that light, this Article does not answer all of the questions regarding the desirability of TRIPs, nor is it intended to. It does establish, however, that within the United States, under a regime of uniformly high IPRs, both internal *and* external patenting correlate positively with a state's economic performance, at least on average. For the annual data, external regional patenting was associated with improved economic performance. For the ten-year averaged data, external patenting in the United States generally was associated with

123. Out of curiosity, I did check whether the creation of the Federal Circuit changed the relationship between patenting and per capita income by dividing the ten-year data covering 1966–2005 into pre- and post-Federal Circuit periods (1966–1985 and 1986–2005). However, whether I estimated equation (7a) separately with the pre- and post-Federal Circuit data, or estimated a difference in difference model for the pre- and post-Federal Circuit time periods, no statistically significant difference existed in the patenting coefficients in the pre- and post-Federal Circuit eras.

improved economic performance. This may suggest that time is required for the benefits of a patented innovation to spread. In either case, however, the positive correlation between external patenting and economic performance is consistent with the hypothesis that TRIPs represents a mutually welfare-enhancing free trade measure.