

The George Stigler Center for the Study of the Economy and the State

The Center for the Study of the Economy and the State was founded by George Stigler at the University of Chicago in 1977. It has from the beginning been a joint enterprise of economists and legal scholars at the Graduate School of Business, Department of Economics and Law School of the University of Chicago. The Center was renamed in George Stigler's memory after his death in 1991.

The Stigler Center is dedicated to the study of the effects of political life on economic life and the reciprocal effects of economic life on political life. That is not a very restrictive program, since there are few areas of our lives where neither economics nor the state intrudes. To carry out its mission, the Stigler Center supports research of faculty at the University of Chicago and of visitors from other academic institutions. The Center publishes a Working Paper series, and it promotes the dissemination of this research to a wider audience via conferences and lectures.

The Stigler Center contributes importantly to the continuity and growth of 'Chicago Economics,' which is known worldwide for two attributes:

- A tough-minded professional style that views economic theory not as an end but as a tool to assist in understanding the real world.
- A lively appreciation for the working of private markets.

George J. Stigler

George Stigler joined the faculty of the Graduate School of Business and the Department of Economics at the University of Chicago in 1958. This event and the arrival two years later of Merton Miller is widely recognized as establishing the Business School as a world leader in academic research and making it a full partner in an extraordinarily fruitful cooperative research enterprise with the University's Department of Economics and Law School.

Stigler was one of the great economists of the 20th century. He made seminal contributions to the economic theory of information and oligopoly and to the economic analysis of government regulation and the public sector. Stigler received the profession's highest honors including the presidency of the American Economic Association and the Nobel Memorial Prize in Economic Science. His 1982 Nobel Prize was the first awarded to an economist whose primary appointment was in a Business School.

More information may be found at the Stigler Center's website:
<http://gsbwww.uchicago.edu/research/cses/>

*THE GEORGE J. STIGLER CENTER
EXECUTIVE COMMITTEE*

Gary Becker, Economics Department

Dennis Carlton, Graduate School of Business

Frank Easterbrook, U.S. Court of Appeals for the Seventh Circuit and Law School

Robert Fogel, Graduate School of Business and Economics Department

Randall Kroszner, Graduate School of Business

William Landes, Law School

Sam Peltzman, Graduate School of Business

Randy Picker, Law School

CONTENTS

UNIVERSITY OF CHICAGO FACULTY

GARY BECKER	4
STEVEN KAPLAN	5
ANIL KASHYAP	8
RANDALL S. KROSZNER	9
CASEY MULLIGAN	11
KEVIN MURPHY	12
SAM PELTZMAN	16
TOMAS PHILIPSON	18
LESTER G. TELSER	20
ROBERT TOPEL	21
ROB VISHNY	22

VISITORS

SERGUEY BRAGUINSKY	23
CLAIRE FRIEDLAND	24
PRAVIN KRISHNA	25
JOHN G. MATSUSAKA	26
WILLIAM VOGT	28

UNIVERSITY OF CHICAGO FACULTY

Gary Becker

Casey Mulligan and I continued our work on *The Growth of Government*. The project tries to explain the growth of government through competition among interest groups. An important aspect of the study is to study the interaction of regulation and government spending. Our main focus last year was on a quantitative assessment of the growth of regulation. There have been many studies of the growth of taxes and spending, but these are necessarily incomplete as accounts of how the government redirects resources. In many areas the government uses regulation to require private actors to change the allocation of resources in ways that the government desires. Therefore a full account of government's role in economic life requires some measure of the effects of regulation. The main problem up to now has been how to estimate the effect of regulation in a way that is commensurable with explicit tax and spending figures.

One approach that Mulligan and I are working on is to develop 'tax equivalents' for various types of regulation. As an example of this approach, consider the labor legislation of the 1930s that fostered the growth of trade unions. Empirical studies commonly find union wage premia of around 15 per cent, and at their peak unions accounted for about one-third of the work force. Thus the economic effect of unions is roughly similar to a tax that raises revenue equal to around 5 per cent of total wages and redistributes the proceeds of the tax to a third of all workers. Since some unions existed before the labor market regulation was enacted, a 'tax equivalent' of 5 percent of wages would be an upper-bound estimate of the effect of this type of regulation. Moreover, the tax equivalent has been declining recently, because the unionized part of the work force has been shrinking.

Mulligan and I are also continuing our work on the links between government spending and tax revenue. It has long been conventional wisdom that government should collect its revenues with a minimum of deadweight loss. However, this presumes that the amount the government spends is unrelated to the efficiency of the tax system. We believe this is wrong. For example, an increase in efficiency of the taxes used would increase government spending, and might even increase the total inefficiency associated with taxes! The reason for the expansion in government spending is that taxpayers fight less hard to resist higher taxes when the cost per dollar of taxes paid is lower because the tax system is more efficient.

We continue to test this and other implications of the analysis by examining the effects of the introduction of various types of tax regimes and reforms (for example, the introduction of VAT taxes, income taxes, flattening of the tax schedule, etc.) on government spending. On the whole, empirical evidence is generally consistent with the implications of this model. We have submitted a paper based on this work to an economics journal.

Kevin Murphy, Michael Grossman (National Bureau of Economic Research) and I have begun a project on the regulation of illicit drugs. The goal of this project is to estimate the costs of

alternative forms of regulation. The current system of supply interdiction plus penalties for possession has numerous costs – the explicit costs of enforcement and incarceration and the resources devoted by buyers and sellers to evading detection and securing ‘turf.’ We want to compare those costs to alternative methods of restraining the consumption of these drugs. For example, one alternative that has been discussed is decriminalization plus a tax on consumption. This would replace the implicit tax of the current system with an explicit tax. There would however be an important difference in social cost, because the current implicit tax requires an expenditure of resources by criminals and the government. Much of that expenditure would be converted to government revenue with an explicit tax.

Steven Kaplan

In the last year, I have worked on the following projects.

Financial Contracting Theory Meets the Real World. Per Stromberg and I revised our study that analyzes the structure of venture capital financings. There is a large academic literature in security design, capital structure, and contracting in general. The papers in this literature often begin with a situation in which a principal negotiates with an agent over the financing of a project or company. These theoretical papers typically make a number of different assumptions and predictions concerning these negotiations. These assumptions concern how easy it is to observe the agent’s actions and write contracts based on these actions, the ability to renegotiate these contracts and the nature of information and uncertainty.

Despite the large volume of theory, there is relatively little empirical work that compares the characteristics of real world financial contracts to their counterparts in financial contracting theory. In this paper, we attempt to inform theory by conducting a detailed study of actual contracts between venture capitalists and entrepreneurs. Venture capitalists (VCs) are real world entities that most closely approximate the investors of theory. VCs have strong incentives to maximize value, but, at the same time, they receive few or no private benefits of control.

The distinguishing characteristic of VC financings is that they allow VCs to separately allocate cash flow rights, voting rights, board rights, liquidation rights, and other control rights. We explicitly measure and report the allocation of these rights. This allocation has the following characteristics in our sample: (1) While convertible securities are used most frequently, VCs also implement a similar allocation of rights using combinations of multiple classes of common stock and straight preferred stock. (2) Cash flow rights, voting rights, control rights, and future financings are frequently contingent on observable measures of financial and non-financial performance. (3) If the company performs poorly, the VCs obtain full control. As company performance improves, the entrepreneur retains / obtains more control rights. If the company performs very well, the VCs retain their cash flow rights, but relinquish most of their control and liquidation rights. The entrepreneur’s cash flow rights also increase with firm performance. (4) It is common for VCs to include non-compete and vesting provisions aimed at mitigating the potential hold-up problem between the entrepreneur and the investor.

How Do Venture Capitalists Choose Investments? Per Strömberg and I completed a first draft of this paper. In this paper, we consider how venture capitalists (VCs) choose or screen their investments. Our data consist of the contemporaneous investment analyses produced by 10 venture capital firms for investments in 42 portfolio companies. Consistent with most academic and anecdotal accounts, we find that it is common for VCs to consider explicitly the attractiveness of the opportunity – the market size, the strategy, the technology, customer adoption, and competition, the management team, and the deal terms. We also provide evidence on how the venture capitalists expect to monitor those investments. In at least half of the investments, the VC expects to play an important role in recruiting management. Finally, we complement the investment analyses with information from the financial contracts for the investments and consider the relation of the analyses with the contractual terms and with subsequent performance. In both analyses, the evidence suggests that the VC's initial appraisal of the management team is important. Stronger management teams obtain more attractive contracts and are more likely to take their companies public.

Venture Capitalists As Principals: Contracting, Screening, and Monitoring. In this paper, Per Strömberg and I summarize the work in our two venture capital papers and other recent empirical work on venture capital. Theoretical work on the principal-agent problem in financial contracting focuses on the conflicts of interest between an agent / entrepreneur with a venture that needs financing, and a principal / investor providing funds for the venture. Theory has identified three primary ways that the investor / principal can mitigate these conflicts – structuring financial contracts, pre-investment screening, and post-investment monitoring and advising. In this paper, we describe recent empirical work and its relation to theory for one prominent class of principals – venture capitalists (VCs).

The empirical studies indicate that VCs attempt to mitigate principal-agent conflicts in the three ways suggested by theory. The evidence also shows that contracting, screening, and monitoring are closely interrelated. In screening, the VCs identify areas where they can add value through monitoring and support. In contracting, the VCs allocate rights in order to facilitate monitoring and minimize the impact of identified risks. Also, the equity allocated to VCs provides incentives to engage in costly support activities that increase upside values, rather than just minimizing potential losses. There is room for future empirical research to study these activities in greater detail for VCs, for other intermediaries such as banks, and within firms.

This paper appears in the 2001 *Papers and Proceedings* of the American Economic Association.

The Effects of Business-to-Business E-Commerce on Transaction Costs. In this paper, Luis Garicano and I study the changes in transaction costs from the introduction of the Internet in transactions between firms (i.e., business-to-business (B2B) e-commerce). We begin with a conceptual framework to organize the changes in transaction costs that are likely to result when a transaction is transferred from a physical marketplace to the Internet. Following Milgrom and Roberts (1992), we differentiate between the impact on coordination costs and motivation costs.

We argue that it is likely that B2B e-commerce reduces coordination costs and increases efficiency. We classify these efficiencies into three broad categories – (1) process improvements; (2) marketplace benefits; and (3) indirect improvements. At the same time, B2B e-commerce affects incentive costs. In particular, we discuss the impact of the introduction of e-commerce on informational asymmetries. We implement this framework by analyzing detailed internal data from one Internet-based firm to measure process improvements, marketplace benefits, and motivation costs. We present less detailed data and analyses for one other firm. Our results suggest that process improvements and marketplace benefits are potentially large. We find little evidence that informational asymmetries are more important in the electronic marketplace we study than the existing physical ones.

This paper will be published in the *Journal of Industrial Economics* as part of a volume on e-commerce sponsored by the National Bureau of Economic Research.

Do Bidders Overpay? Extracting Stock Price Information from Takeover Battles. In this paper, Pekka Hietala, David Robinson and I analyze the amount of information that can be extracted from stock prices around takeover contests. This work is motivated by numerous studies showing that bidders tend to overpay for targets in takeover contests. In many of these studies the gains to the target minus the bidder's overpayment are taken to be the market's assessment of the net value created by the takeover. The first part of the paper shows that it is not possible in general to use target and bidder stock price movements to infer the market's estimates of synergies, bidder overpayment, and changes in bidder and target values. In two generic cases, however, we show that it is possible to use bidder and target stock prices to obtain market estimates of overpayment.

In the second part of the paper, we illustrate one of these two generic cases through a clinical study of the takeover contest for Paramount Communications. We find that the market estimated that Viacom, the eventual "winner" of the takeover battle, overpaid by approximately \$1.4 billion when it agreed to purchase Paramount in a \$9.2 billion acquisition in February 1994. We also find that the market believed that QVC, the eventual "loser" of the battle, had substantially larger synergies (on the order of \$1 billion more) with Paramount than Viacom. Viacom prevailed because of its willingness to overpay by much more than QVC. This overpayment occurred despite the fact that Sumner Redstone, the CEO of Viacom, owned roughly 2/3 of Viacom. We view the results for Paramount and Viacom as strongly consistent with Roll's Hubris hypothesis as well as the results in Morck, Shleifer and Vishny (1990).

We presented this paper at the American Finance Association meetings in January 2001.

This year I completed a long-standing project on banking and began a new project.

The first looks at the old question of why banks are in the business of simultaneously taking deposits and making loans. Because deposits can be immediately withdrawn, while loans are illiquid, this product mix makes the bank susceptible to a run.

That fact has led to proposals, such as ‘100 percent reserve’ banking, which would require the separation of the depositary and loan-making functions.

Past work on this topic has offered two explanations for the linkage between deposits and loans. One emphasizes the learning that a bank may do as a result of watching a customer’s use of his bank account. Thus, the bank that handles the checking accounts is thought to have a natural advantage in credit evaluation.

Other models emphasize the disciplinary effects that come from the possibility of runs. These explanations note that banks are themselves difficult to monitor, so that depositors would not be willing to supply funds if they could not pull their money out on short notice. The threat of a run can therefore help keep the bank from doing things such as taking extra risk that would be difficult for depositors to detect.

In my paper with Raghuram Rajan and Jeremy Stein (Banks as Liquidity Providers: An Explanation for the Co-Existence of Lending and Deposit-Taking) we propose a complementary explanation to the existing ones. We observe that since banks often lend via commitments, or credit lines, their lending and deposit-taking may be two manifestations of the same primitive function: the provision of liquidity on demand. After all, once the decision to extend a line of credit has been made, it is really nothing more than a checking account with overdraft privileges. This observation leads us to argue that there will naturally be synergies between the two activities, since both may require banks to hold large volumes of liquid assets (cash and securities) on their balance sheets. If deposit withdrawals and commitment takedowns are imperfectly correlated, the two activities can share the costs of holding the liquid assets. Thus, we view the fundamental synergy as coming via commitments and deposits.

We develop this idea with a simple model, and then use a variety of data to test the model’s empirical implications. The most novel empirical finding is that banks heavily financed with deposits also tend to have relatively high proportions of credit granted in the form of commitments. This is consistent with the model’s central prediction that these two features should go together.

This view of banks suggests a number of policy implications. For instance, it argues against the view that a subsidy from deposit insurance is vital to the survival of banks as simultaneous loan-makers and deposit-takers. Similarly, it predicts that even with a completely deregulated financial system there would be clear reasons for banks to continue to exist – presumably specializing in activities related to the provision of liquidity. Finally it suggests that there may be a cost to mandating “narrow” banks – i.e. not allowing banks to have loans or commitments

on the asset side of their balance sheet. The paper will appear in *Journal of Finance* later this year.

I also began work on a project that involves a unique Italian data set that makes it possible to track the extension of credit from specific banks to specific firms. In addition to seeing the credit flows, we have information on both the firms and the banks balance sheets and income statements. Our first paper using the data examines whether the superior information on the credit terms is useful in understanding firm's investment decisions. A very common empirical finding is that interest rates have little predictive power for investment. Our very preliminary findings suggest that this empirical regularity may be due to measurement problems that arise because virtually all studies have to use proxies for the cost of borrowing.

Randall S. Kroszner

During the past year, I have continued my research on banking and financial regulation and the role of money in politics.

In "Throwing Good Money After Bad? The Role of Board Linkages and Bank Lending during Distress," Philip Strahan and I investigate the impact of connections between banks and non-financial firms on lending and borrowing behavior. Connected lending occurs when the lender has more than a simple arm's length relationship with the borrower, such as when an executive of a borrowing firm sits on the board of the bank or an executive of the bank sits on the board of the borrowing firm. A board linkage may reduce the costs of information flows between a borrower and the bank. But it may also create a conflict of interest: the board member has a fiduciary duty to both the bank and the firm, and these interests may diverge. In particular, bank regulators are concerned that such connections could lead banks to "throw good money (both the bank's and the taxpayers') after bad" by favoring connected firms experiencing financial distress.

In contrast to recent findings for other countries, we find no evidence of such bad behavior in the U.S. Despite a tendency for firms to increase total bank borrowing during financial distress, connected banks tend to *reduce* their exposure to financially distressed borrowers and to provide a smaller fraction of the distressed firm's total bank borrowing. In addition, connected borrowers do not appear to receive favorable treatment in terms of lower interest rates from their connected banks, neither in normal times nor times of financial distress. In fact, we find that distressed firms borrowing from their connected bank tend to pay *higher* interest rates than distressed firms borrowing from unconnected banks. Our results therefore suggest that connected banks have been able to avoid succumbing to these conflicts of interest problems and that regulatory proposals to restrict board linkages between banks and borrowing firms may be misguided.

I have also written two papers that explore the role of *private* demand for and supply of regulation in financial markets, both historically and today. In "Lessons from Financial Crises: The Role of Clearinghouses" (*Journal of Financial Services Research*, December 2000), I study

the historical development of clearinghouses and their ability to deal with financial crises. I discuss the role of private bank clearinghouses in the Panic of 1907, the final crisis of the National Banking Era that led to the founding of the Federal Reserve. I examine how the clearinghouses responded by issuing private currencies, despite legal obstacles, and providing co-insurance among the members. I then trace the late nineteenth and twentieth century evolution of clearinghouses at futures exchanges to understand how they promote stability and withstand crises by mutualizing risks among members. The key feature of both bank and futures clearinghouses is the effective integration of the members into a single organization during times of distress, illustrating how the private markets develop institutions and contracts to manage risk and respond to financial crises.

In “The Supply of and Demand for Financial Regulation -- Public and Private Competition around the Globe” (published in Global Economic Integration: Opportunities and Challenges, Jackson Hole Symposium, Federal Reserve Bank of Kansas City, 2000), I analyze numerous examples in the international markets of private supply of regulatory services. I focus on four sources of private supply: “members’ only” organizations, such as exchanges and clearinghouses; voluntary standard-setting bodies; innovative firm structures, such as Chinese walls and firewalls; and private third party monitors, such as ratings agencies.

Finally, I consider the broad context of the political economy of regulatory reform in continuing work with Thomas Stratmann on the role of interest group competition. In “Does Political Ambiguity Pay? Corporate Campaign Contributions and the Rewards to Legislator Reputation,” we investigate whether politicians tend to follow a strategy of ambiguity in their policy positions or a strategy of consistent support for one position. Ambiguity could allow a legislator to avoid alienating constituents and to play rival interests off against each other to maximize campaign contributions. Alternatively, developing a clear reputation could help a candidate to reduce uncertainty about future votes and elicit high campaign contributions from favored interests.

We outline a theory that considers conditions under which a politician would and would not prefer reputational development and policy-stance clarity in the context of repeat dealing with special interests. Using data on corporate political action committee contributions (PACs) to members of the U.S. House, we find that legislators do not appear to follow a strategy of ambiguity and that high reputational development is rewarded with high PAC contributions. Our results have implications for campaign finance reforms. Substituting public funding for private contributions, for example, might weaken the incentives for legislators to develop consistent policy positions and thereby make it more difficult for voters and investors to predict what a legislator is likely to do once in office.

Casey Mulligan

I continued research on the political economy of tax distortions with four papers: “Economic Limits on ‘Rational’ Democratic Redistribution,” “A Dual Method of Empirically Evaluating Dynamic Competitive Equilibrium Models with Distortionary Taxes,” “A Century of Labor-Leisure Distortions,” and “Average Marginal Tax Rates Revisited.” These studies were made possible by previous projects with Stigler Center colleagues. These include work with Gary Becker on “Deadweight Costs and the Size of Government,” and “Accounting for the Growth of Government” and work with Tomas Philipson on the political economy of redistribution.

“Economic Limits...” challenges a conventional view, which dates at least back to de Tocqueville (1835), that democracies have excessive pressure for income redistribution from rich to poor. The logic of the conventional view is that a majority of people has incomes below average, and anyone with income below average can gain from redistribution. But one of the important political effects of deadweight costs is to reduce the size of the coalition that can gain from rich-poor redistribution, and I show how most estimates of those costs imply that the coalition of gainers from redistribution is less than 50% of the adult population, and probably a lesser fraction of the voting population.

“A Dual Method ...” studies a class of general equilibrium models that have become widely used in modern studies of public finance and the macroeconomy. I show how such models can be efficiently and informatively compared with empirical observations by simulating the tax policies that, according to the model, are required to rationalize the observed behavior. An empirically successful model has simulated tax policies that agree with observed tax policies.

“A Century of Labor-Leisure Distortions” applies the Dual Method to empirically evaluate the neoclassical growth model with 20th century aggregate American data. I show that the basic trends in the data are remarkably consistent with the neoclassical model. For example, the model predicts a substitution of leisure for consumption when labor income taxation is increased. In fact, over the last century aggregate expenditure on consumption increased about 40 percent less than did aggregate expenditure on leisure while labor income taxation increased by 40 percentage points. The model completely misses the 1930’s and 1940’s, because it implies that heavy labor taxes and regulations are required to rationalize the behavior of consumption and employment during the Great Depression. And it implies light taxes and regulations to rationalize the economic expansion of the 1940s, when in fact the opposite occurred. Interestingly, Reaganomics very clearly shows enhanced labor market efficiency in the aggregate 1980’s data: marginal tax rate cuts and tough stances with unions are associated with an increase in the consumption-leisure ratio to levels that appear to be more efficient from a historical perspective. “Average Marginal Tax Rates Revisited” constructs the marginal labor income tax rates for the 1980s and 1990s that are needed for this last conclusion.

Kevin Murphy

Over the past year my research has focused on three major topics: the economic value of medical research, entry and pricing in initially monopolized markets and the economics of the "drug war."

Robert Topel and I have continued our work on the economic value of medical research. Our previous work identified enormous gains in consumer welfare associated with the rise in health and longevity over 20th century and the potential for large gains from disease reductions in the future. Our work over the past year has focused on updating and improving our estimates of these values and on exploring the implications of our results for health care policy.

The work on competition and entry in initially monopolized markets (joint with Robert Topel and Steve Davis) explores pricing and product innovation incentives for a dominant firm that faces competition from potential and/or actual entrants. Our results, which may seem surprising, is that potential competition can be even more effective than actual competition at reducing prices and creating good product design incentives.

Finally, in recent work, Gary Becker and I study the economics of the "drug war." We find that when the demand for illicit drugs is relatively inelastic (i.e. drug consumption is relatively insensitive to price) efforts to reduce consumption by reducing supply tend to be very costly and relatively ineffective. In fact, under such conditions, a war on drugs will increase the amount of resources devoted to the industry, and enforcement costs will be high while the reduction in consumption will be relatively small. High taxes on legalized drugs seem to be a much better alternative.

The Economic Return to Medical Research: Our ongoing work is attempting to estimate the economic value of the increases in life expectancy achieved from 1970 to the present and the prospective gains from reducing the death rates from cancer, heart disease, AIDS and other significant diseases. Over the past year we have worked to improve our previous estimates by using more detailed data and updating the analysis through 1997. While the qualitative character of our results remains the same as before, these new estimates provide us with a more accurate and clearer picture of the historical and prospective gains from medical advance. As before, our results suggest the value of these historical gains and the potential value of further improvements in longevity are enormous. Most importantly, the economic value of historical and prospective improvements in health is much larger than previously thought. For example, our finding that a cure of cancer is worth roughly \$50 Trillion implies that even modest progress - say a five percent reduction in death rates- would be worth about \$2.5 Trillion and could justify a second sustained "war" on cancer even if such a war had relatively small effects on outcomes. For example, total government funding for all health research was about \$16 Billion in 1995 while overall spending on health research was about \$30 billion. These numbers would imply that doubling research spending from \$30 billion to \$60

billion per year for an entire decade could be justified on economic grounds even if it were to reduce the death rate from either cancer or heart disease by as little as 1% or 2%.

Does our research say that we should write a "blank check" when it comes to funding medical research? The answer is clearly no. While the gains to improvements in health are large, so too are the potential increases in the costs of care. In fact while improvements in health resulted in an unmeasured gain to U.S. consumers of about \$57 trillion over the 1970 to 1990 period, increases in the costs of care over the period amounted to about \$8 trillion making the net improvement in outcomes somewhat less than \$50 trillion. While it is unlikely, based on our calculations, that increases in the costs of care will outstrip the gains from improved health or longevity in the aggregate, it is certainly possible that some avenues of research could generate greater increases in costs than in benefits.

Our finding of large unmeasured gains from the improvement in health is particularly interesting when juxtaposed with the widespread concern over the rising "costs" of health care. Our research emphasizes that the past decades have seen tremendous growth in both the costs and value of medical care. Accounting for the growth in costs reduces our estimate of the net gain in health (by about 25% over the 1980 to 1990 period). However, our estimates imply that the benefits far outweigh the costs.

Most of the growth in the "cost" of medical care is not increases in the cost of particular treatments, drugs or procedures. Rather, the growth in the "cost" of health care represents expansion in the treatments, drugs and procedures available to consumers as our knowledge of diseases and treatments expands over time. Typically, this expansion in options has led to improved outcomes but increased costs as well. Much attention in the public policy arena has focused on reducing the rate of growth in health care costs. Unfortunately, if not done correctly these same cost-containment efforts could easily slow the pace of medical advance and reduce rather than raise long run efficiency. This is particularly true given the large gains to consumers associated with medical advances identified in our previous work. Indeed, the dynamic efficiency effects of cost control (i.e. effects on the rate of innovation) may far outweigh the static efficiency effects (i.e. effects on the best uses of available treatments today). Accordingly, our results suggests that it is important to understand how any health care policy designed to reduce costs affects incentives to innovation before such a policy can be sensibly evaluated.

Pricing and Entry in Initially Monopolized Markets: It is tempting to think that firms with dominant market shares are not subject to substantial competition. However, even firms with dominant shares face the potential for others to enter their market. In fact, these firms may have a dominant share precisely because they are pricing aggressively enough to keep potential entrants at bay by making entry unprofitable. In joint work, Robert Topel, Steve Davis and I examine exactly this situation. We consider an incumbent who faces potential entry from a competitor that offers a competing product differentiated product.

Two types of market outcomes emerge in this configuration, an “exclusionary” equilibrium in which the incumbent captures the entire market and a “separating” equilibrium in which the entrant captures one group of customers while the incumbent retains his highest valued customers. Even though the entrant makes no sales in the exclusionary equilibrium, and the incumbent retains what appears to be a monopoly position, the incumbent’s price is still constrained by the threat of entry. In fact, in our theory, most market changes that induce entry actually lead to an increase in the incumbent’s price. The logic is actually rather simple: in the separating equilibrium the entrant’s price and profits are constrained by the threat that the incumbent will cut price and capture the entrant’s customer base. In equilibrium, the incumbent must be indifferent between the separating equilibrium price and cutting price so as to exclude the rival. Since the incumbent makes more sales by excluding its rival, the incumbent’s price must be higher in the separating equilibrium in order to make profits the same. Thus, the most aggressive pricing by the incumbent is associated with the exclusionary equilibrium.

The incentives for product innovation are also radically different between the exclusionary and separating equilibria. In the exclusionary equilibrium, the incumbent desires to make his product more attractive to the entrant’s potential customer base. This allows the incumbent to increase the price he can charge those customers that would be most likely to defect to the entrant. In contrast, in the separating equilibrium both the entrant and the incumbent have the incentive to differentiate their products (i.e. design products that appeal to their own but not their competitor’s customer base).

While the model we have developed to this point is quite simple it seems to have some rather interesting implications. In particular, a switch from potential to actual competition might well be associated with an increase rather than a fall in prices to consumers.

The War on Drugs: Gary Becker and I are currently working on an economic model of the “drug war.” A key element of the U.S. war on drugs is an effort to reduce the supply of drugs. Fundamentally, such a war reduces consumption by restricting supply and increasing the price of drugs to consumers. The reduction in supply is accomplished by confiscating drugs, forcing suppliers to produce and market their products using more costly methods that are less easily discovered and by punishing drug traffickers through fines and imprisonment. In many ways, the war on drugs is like a tax on drug production where the tax is paid in-kind rather than as a monetary transfer to the government. This tax increases the costs of getting drugs to consumers and thereby raises the street price of drugs and reduces consumption.

While such a tax may reduce consumption it can also be very costly. This is particularly true when the demand for drugs is inelastic (i.e. drug consumption falls less than proportionately as price increases). When demand is inelastic, total spending on drugs rises as price increases and consumption falls (since the fall in consumption less than compensates for the rise in price). With the prospect of greater revenues in the marketplace, suppliers devote more resources to the drug industry as prices rise. The

harder the government fights the war, the more resources suppliers put into avoiding detection, fighting battles over turf, replacing supply lost when drugs are confiscated, etc.

In fact, inelastic demand makes the war on drugs difficult in several respects. First, as stated above, drug suppliers devote more resources to counter the war on drugs rise as the government fights harder. Second, consumption falls little as prices increase. Finally, the government's enforcement costs will rise to counter the extra resources committed to drug trafficking. For all of these reasons a war on drugs is very costly when demand is inelastic.

Most empirical estimates suggest that the demand for drugs is indeed inelastic – typical estimates of this elasticity are about $-1/2$. Other estimates indicate that the war on drugs may have increased the street price of drugs to 4 to 5 times their free market level. These two estimates would imply that the war on drugs has roughly doubled the total resources spent by drug producers and suppliers. There are, of course, further costs: to the government for apprehending and imprisoning drug dealers and to society at large from the crime and violence associated with drug trafficking.

The tremendous cost of the drug war motivates a search for alternative ways of reducing consumption. One alternative that has been proposed is legalization with the in-kind tax of the drug war replaced by a monetary tax on legal drug suppliers. The monetary tax has several distinct advantages. First, the costs imposed on suppliers are now transfers to the government rather than real resources dissipated in the attempt to avoid detection and prosecution. Secondly, taxes typically will have less external costs associated with them than the implicit tax of illegal activity (which is strongly associated with gangs, crime and violence). Finally, the monetary tax will reduce enforcement costs since much (or all) of the market will now be supplied by the legal sector allowing the government to focus its efforts on policing illegal suppliers who might try to avoid the tax. In fact, our analysis implies that switching to a taxed legally supplied market sector is likely to make further increases in price less costly to implement and hence will enable the government to reduce consumption relative to what could be achieved by the current war on drugs.

Sam Peltzman

I worked on two projects in 2000/01.

Kevin Murphy and I completed a study of the impact of public school performance on the labor market experience of young workers. The motivation is the timing of the much discussed widening of the college – high school wage premium. This began in the late 1970s, which is just after the end of a period of declining performance of high school graduates (as measured by standard tests such as the SAT). The timing raises the question of whether any part of the higher reward for college is due to a decline in skills acquired in high school.

Our study takes advantage of cross-state variation in both high school performance (as measured by scores on the Armed Forces Qualifying Test) and the labor market experience of young high school graduates. Specifically, we analyzed changes in both measures from the early 1970s through the mid 1990s. We wanted to see if, e.g., the change in labor market experience of high school graduates was below the national average in states where the change in the measured performance of high schools was also below average.

We found that the change in school performance is associated with several important aspects of local labor market performance.

Wages of young (19-23 year old) high school graduates grow significantly more in states with improving relative school performance. Most of the difference in wage growth is attributable to job quality – i.e., the industry/occupation mix of the jobs obtained by the young workers – rather than the pay received for a given job.

Wages also grow more where the initial level of school performance was highest. We interpret this level-effect as reflecting the increased demand for skill characteristic of the 1970-1990 period.

Other labor market outcomes for these young labor market entrants, such as the probability of finding employment and their hours of employment, appear unaffected by school performance.

A related issue concerns the decision to enter the labor force or go on to college. Here we find that the fraction of 19-23 year olds who went on to college grew significantly more in states with improving relative school performance.

We also found some interesting effects on the overall labor market. Growth of total employment is significantly smaller in states with declining relative school performance. We find this difference in employment growth rates across a variety of industries, but it is especially large for industries, like manufacturing, that tend to be geographically mobile.

Finally, we found a (statistically weak) tendency for the wage effects of school quality to wear off as the young workers acquire experience and, presumably, exposure to non-schooling sources of work skills.

The main results of the study are in Stigler Center Working Paper 162 “The Effects of School Quality on the Youth Labor Market.”

I have begun a project on the behavioral consequences of medical breakthroughs. Thus, suppose there is a significant invention, such as a cure for cancer. Once implemented, this will substantially reduce (age-adjusted) mortality rates. But, I argue, this advance will also induce changes in behavior that can affect mortality. One kind of effect, identified by Dow, Philipson and Sala-I-Martin (‘Longevity Complementarities under Competing Risks,’ *American Economic Review*, December, 1999) is favorable – the breakthrough permits resources formerly devoted to cancer research and treatment be shifted to mitigating other health risks. But other responses can work the other way. For example, resources (including time, effort, etc.) formerly devoted to avoiding or treating cancer can also be shifted to other activities that also engender health risks. My project will focus on these latter sorts of responses.

The first phase of the project is focusing on mortality data from the period surrounding the introduction of antibiotics and other anti-bacterial drugs. There are two reasons for this. First, this was an important breakthrough. In terms of the immediate effect on mortality, it was arguably the most important advance of the 20th century. Second, the crude data suggest that we ought not dismiss the possibility of offsetting behavior, at least in this case.

Specifically, the breakthrough drugs are introduced over a period from the late 1930s through the early 1950s. By the mid 1950s, age-adjusted mortality is substantially below where it would be if pre 1940 trends had persisted. However, this progress essentially stops for the next 15 years. By 1970, there is little difference between actual mortality and a projection of pre-antibiotic trends.

That history is only suggestive. For example, potential non-behavioral reasons for the sudden deceleration of mortality progress need to be examined. Also, the drugs had different mortality effects across age groups – larger for the younger groups. I want to see if there is any corresponding difference in the post 1950s deceleration of mortality improvement across these age groups.

Tomas Philipson

I have been mainly engaged in a number of health economic projects this year.

One line of work analyzes the forces contributing to the worldwide long-run rise in obesity and the role of public interventions in affecting its continued growth. This work has resulted in two working papers, “The Long Run Growth in Obesity as a Function of Technological Change” (joint with Richard Posner) and “Technological Change and Obesity: An Empirical Examination” (joint with Darius Lakdawalla).

A growth in obesity in a population must result from the growth of calorie consumption outpacing the growth of physical activity. Yet during many decades in developed countries, obesity has grown with modest rises in calorie consumption and with a substantial increase in both exercise and dieting.

Posner and I consider the economic incentives that increase obesity by simultaneously stimulating caloric intake and discouraging caloric expenditure on physical activity. We argue that technological change provides a natural interpretation of the long-run growth in obesity. Our technology-based theory predicts that there is no simple relation between obesity and economic development. Specifically, it predicts that obesity rises with income at first, but ultimately – when income becomes sufficiently high – this relation becomes negative. Accordingly, our theory implies that the growth in obesity may be self-limiting.

Another project is on the economic consequences of increased longevity. I wrote a commissioned paper “World Inequality and The Rise in Longevity” for the Annual World Bank Conference on Development Economics. This was joint with Rodrigo Soares, a student in the economics department. The paper is forthcoming in *The World Bank Review*.

The paper is about the much-discussed worry that globalization of markets is increasing world income inequality. Generally, there seems to be a concern that although some countries in the world are getting richer, poorer developing countries are falling behind.

However, material gain is only one source of enhanced economic welfare. Another important dimension is how long one lives to enjoy the goods and services available to be consumed. In addition to the “flow” of well being represented by yearly income, how many years that flow can be enjoyed is determined by the longevity of a population.

Soares and I build on results for the US in work by Nordhaus and Murphy and Topel. We spell out the implications of their findings for international comparisons. In the last 50 years or more, countries with initially low longevity have tended to gain more in life expectancy than countries with high longevity levels. The fact that countries with lower life expectancy – which are poorer countries – are gaining more in longevity than richer countries means that poorer countries are catching up to richer ones in this dimension.

In other words, income changes have not always been accompanied by similar changes in all the relevant dimensions of development. Thus, looking at income only to draw conclusions about changes in overall economic welfare across nations may be misleading. The paper also compares such cross-country evaluations with an alternative and prominent measure of well-being: the *Human Development Index* (HDI) of the United Nations (UN).

Related to this project, I have continued work with Gary Becker, on endogenous longevity and fertility that we will be working further on this coming year.

I also completed another project on the economic consequences of increased longevity. This analyzes how markets for old-age long-term care respond to the aging of populations. The results are in “The Rise in Old Age Longevity and the Market for Long Term Care,” with Darius Lakdawalla, which will be published in the *American Economic Review*.

Frank Lichtenberg (Columbia Graduate School of Business) and I have been working on competition in the market for patented new drugs. The motivation for this study is the widespread misperception that a patent protects an innovator from competition until the patent expires. A patent only protects an innovator from others producing the *same* product, but it does not protect him from others producing *new* products under new patents that might be good substitutes for the innovator’s product. Therefore, there are two kinds of competition facing innovators: the traditional ‘within-patent’ competition, which results from production of the same product, and ‘between-patent’ competition from production of related products on other patents. A return to an innovator return may be destroyed either by generic entry following patent expiration, or by new and superior patents both before and after patent expiration.

Previous research has focused only on the effects of intellectual property regulations on within-patent competition. This research tries to show how legal protection of innovative returns from imitators affects R&D incentives. However, in pharmaceuticals and other high-tech industries, the ‘creative destruction’ from the between-patent kind of competition can be especially important. Thus a fuller understanding of how intellectual property regulation affects progress should include the effects of the regulation on between-patent competition. Our findings for pharmaceuticals suggest that the effects of intellectual property regulations depend heavily on whether these unexplored effects are present. For example, efforts aimed at stimulating R&D may have limited effects on R&D when creative destruction is stimulated as well.

Specifically, Lichtenberg and I attempt to estimate the relative magnitudes of the two sources of competition in limiting innovative returns in the U.S. pharmaceuticals market. In this market, we find that within-patent competition from so-called generic producers is less important than competition between-patents through so called therapeutic competition. We estimate that the loss in sales from between-patent competition is at least as large as the loss from generic competition. However, most of the between-patent competition occurs while a drug is still under patent, while the generic competitors can

only enter after expiration. That difference in timing is important, because a dollar lost earlier has a larger effect on the present value of the innovator's return than the same dollar lost later. Accordingly, our estimates suggest that between-patent competition reduces the present discounted value of the innovator's profits more than does generic competition. This result illustrates why the statutory monopoly awarded through a patent does not always confer great monopoly power in the usual sense of being able to raise price without substantial substitution.

Lastly, I have continued work on revising the book, *Data Markets: The Production of Health Statistics*, for The University of Chicago Press.

Lester G. Telser

During the past year I completed two papers on the Great Depression and a third on research and development.

In "The Relation between the U.S. Great Depression and the German Hyperinflation" I show how the German hyperinflation of the 1920s and the U.S. Great Depression have in common the consequences of an insufficient amount of usable media of exchange. In Germany there was too much currency printed and in the U.S. the cause was widespread bank failures. In both countries the result was the same, very high rates of unemployment.

"The Veterans' Bonus of 1936 and the Abortive Recovery from the Great Depression" explores some effects of a law enacted January 1936 that disbursed 9 year nonmarketable U.S. bonds with a 3 percent annual interest to 3 million World War I veterans. The average bonus receipt per person exceeded 30 percent of the mean household income for the veterans' age bracket. The June 1936 Federal deficit was a peacetime record. In two weeks that June veterans cashed in 46 percent of their total bonus, an amount nearly one percent of annual GNP. The economic recovery in 1936 was more than 2.5 times bigger than in the preceding two years probably because of the effects of this Bonus. The redemptions of the veterans' bonus bonds in June 1937 were 45 percent of the amount the year before, but did not stimulate the economy as much because the Federal deficit in June 1936 was 4 times larger than in June 1937.

In "Noncooperative and Cooperative Models of Research Outlays" I explore the implications of a model in which it is cheaper to copy than to originate research. In my model, the inherent uncertainty of original research leads to an inefficient non-cooperative equilibrium. Cooperation can restore efficiency, but only if the number of rivals is small. This model may help explain the size distribution of firms in research-intensive industries.

Robert Topel

This year I have been working on the following projects:

Entry, Pricing, and Product Design in an Initially Monopolized Market This paper (with Kevin Murphy and Steve Davis) is motivated in part by the Microsoft case, and by the fact that the price of its Windows operating system is quite low. Specifically, we study product design and pricing decisions by rivals that produce differentiated products. Our analysis yields two possible market outcomes. In one, a separating equilibrium, non-cooperating sellers serve distinct groups of buyers. Each seller's price is constrained by the ability of its rival to "steal" its customer base. The other outcome is an exclusionary equilibrium, in which one firm prices low enough to actually steal its rival's customers. We characterize the situations in which each market outcome will occur, as well as implications for welfare. For example, we show that successful entry in a differentiated product market may cause an incumbent seller to raise price, and that welfare may decline for all consumer groups.

Learning Costs, Pricing, and Product Design This paper is an extension of the previous study. The key idea is that some products – such as a computer operating system – involve substantial learning costs by users. These learning costs introduce important dynamic considerations into firms' pricing and product design decisions. For example, an incumbent seller faced with a threat of entry may choose to price low in order to deter future entry. By pricing low today, the incumbent reduces the future demand for an entrant's good because current users rationally invest in learning to use the incumbent's product. As in (1), we explore product design and welfare implications of entry deterrence and successful entry.

We review the empirical literature on this topic in a third paper, "Adverse Price Effects of Entry in Markets with Few Firms."

The Economic Value of Medical Research This paper (with Kevin Murphy) develops a methodology for measuring the value of improvements in health, both retrospectively and prospectively. For example, we show that the sharp decline in mortality from heart disease since 1970 has been worth about \$30 trillion, and that a cure for cancer would have a current value in the United States alone of about \$50 trillion. These benefits point to huge social returns to even modest advances in medical knowledge, and make a case for increased public funding of medical research. This work has gotten a lot of attention, ranging from the popular press to government agencies and Senate committees. We now have a book contract from the University of Chicago Press, which will publish the proceedings of a conference that we organized on this topic.

I have also begun some projects that are in an early stage. Two of these are extensions of the research on health. One is on racial differences in health for the National Institutes of Health. Another is on drugs and drug regulation.

Two other new projects are about labor markets. One is an empirical piece on the evolution of unemployment and wages during the 1990's. It will appear in *Brookings Papers*. The other is on the distributional consequences of the minimum wage. As George Stigler once remarked, regulating prices is a bad way to redistribute income. In this case, the idea is that the minimum wage transfers income from those who consume minimum wage products to those who produce them. One would not be surprised to find that, in terms of position in the overall distribution of income, they are the same people. I have just begun to analyze this using the consumer expenditure survey.

Rob Vishny

Andrei Shleifer and I have been working on a theoretical and empirical study of merger activity from an inefficient markets perspective. The basic premise is that mergers are driven by valuation differences between the acquirer and the target rather than by genuine synergies or value creation. In our model, perceived synergies may inflate the combined valuation of the bidder and target temporarily based on a good story about value creation, but valuations regress to the mean in the long run. We focus on two types of acquisitions. When market valuations are quite low in aggregate or in particular industries, acquisitions will take the form of cash bids. In these cases it may be in the long-run interest of target shareholders for management to resist the bid.

But, as first shown by Nelson (1959), most merger waves appear to occur when valuations are high. In this circumstance, both the target and the bidder may be overvalued from a long-run perspective, but the acquirer is usually more overvalued than the target. For example, this would appear to be true of the Nifty-50 acquirers in the 1960s as well as of recent prolific high tech acquirers such as Cisco, AOL and Intel. In these mergers, the acquisition takes place for stock and enables an acquirer to translate its overvalued stock into less overvalued hard assets (e.g., AOL/Time Warner).

Our approach can help explain the following puzzles: 1) The character of the 3 major merger waves over the last 40 years, including approximate timing of each of these waves, the relative use of cash vs. stock, and the incidence of hostile vs. friendly takeovers. 2) Why many firms appear to make acquisitions that drive down their share prices even though management owns a big stake in the firm 3) Why many diversifying acquisitions take place despite even larger valuation penalties imposed by the market. In a follow-up empirical paper we are testing a number of other specific implications of the model with respect to the time series and cross section of mergers.

In another project, we are studying the role of securities regulation in helping to create liquid capital markets. In our previous work, we have emphasized the importance of company law in protecting minority shareholders. Many have argued that vigorous enforcement of securities laws or enforcement through self-regulatory organizations of brokers and other market intermediaries is equally important. In this project, we are attempting to assess the role of securities laws and gauge their relative importance compared to other legal protections of investors.

VISITORS

Serguey Braguinsky

This year I worked on two closely related studies in industrial development, industrial organization and technological progress. Both are framed by the analysis of innovation in Joseph Schumpeter's celebrated book *Capitalism, Socialism and Democracy*. Schumpeter conjectured that stimulating innovative activity warranted a different kind of competition than simple price competition, and that the latter actually often hindered the innovative process because it destroyed the appropriability of innovators' rents. Ever since, the problem has been perceived to be to find the forms of industrial organization that would create sufficient incentives for potential innovators without creating too big distortions in the use of the innovation. For example, by granting innovators legal monopoly power, patents would stimulate innovation, but they also create monopoly distortions in the consumption of the patented product.

My approach to this issue was motivated by a perception that the case against price competition in innovation-led growth may have been overstated. Even a casual look at cross-country experience seems to suggest that countries that do not restrict price competition to stimulate innovation (like Japan) can have high rates of innovation.

The first paper I wrote on this broad subject (Stigler Center Working Paper No. 163, 2001, jointly authored with Kevin M. Murphy and Atsushi Ohyama) studies the interaction between the first-mover advantage and entrepreneurial ability. Unlike the textbook model of identical firms, our model has innovators with different abilities (in the spirit of what I consider to be the true "Schumpeterian model"). The outcome is that industry leaders, even under perfect price competition and no patent protection, can appropriate innovative returns. We show that when ability differences are important enough, competition may well lead to better results in terms of the speed of technological progress and the growth of the industry than a subsidy or protection. The question of which regime is better then becomes an entirely empirical one. Accordingly, the paper employs extensive evidence from the history of Japanese industrialization to demonstrate that, in at least one very important historical case, the competitive solution to the endogenous growth (or "infant industry") problem worked better.

The second paper, written jointly with Atsushi Ohyama and David C. Rose, focuses more specifically on how a competitive environment can be made compatible with sufficient incentives to innovate. The model assumes that cost-saving innovations spill over immediately to the local group of firms, but it takes time for them to spill over to the global industry. The result is that when the local group is small compared to the global market (that is, when it faces perfectly elastic demand), local firms invest in innovation in a way that resembles private provision of a public good. Local firms are also likely to develop private institutional arrangements setting up a strong mode of cooperation among them (we call this "the neighboring farmer effect"). A subsidy and especially a protective tariff aimed at giving them larger incentives may backfire, because it increases the number of firms to whom the technology spills over immediately.

As the local industry grows larger, its very success eventually drives out the “neighboring farmer effect”, and more familiar institutions of patent protection and imperfect competition replace spillovers and cooperation. It is important that the spillovers are shut out endogenously at exactly the time when their costs to innovators start to outweigh the benefits. Thus there is no need for the government intervention to protect innovators’ rents (although some kind of intervention may be warranted to extend the period of technological cooperation while it is already privately unprofitable, but still socially beneficial).

Once again, the main message is that we have to settle the question of whether free spillovers or ex ante incentives are more important in securing innovation-led endogenous growth empirically. The experience of the Japanese industrialization is cited to argue that, at least in that case, spillovers were more important. Japan achieved its first success in embracing a modern industry through spurring industrial innovation driven by private incentives under global competition. An institutional arrangement for mutually assisting innovative activities had evolved over the period of the first 20-30 years, after which the degree of cooperation sharply fell and was replaced by a new system much more protective of individual innovators’ rents.

Claire Friedland

I am completing a paper on the progress of George Stigler’s ideas on the economics of politics over the twenty-five years beginning with his 1964 Presidential Address to the American Economic Association. This will be delivered at the history of Economics Society Meetings in June 2001, and will subsequently be published in *The American Journal of Economics and Sociology*.

I filled in some of the blanks in my catalogue of the photos in the Stigler-Friedland photo archive. This archive, created in connection with our production of five calendars of economists’ birthdays, is destined for Regenstein Library Special Collections, where the photos will be made available to the public.

I have also begun work on a brief essay on little-known collections of economists’ portraits. This essay will appear as a preface to my photo catalogue.

Pravin Krishna

During my visit to the Stigler Center, I worked on several projects in the area of international trade and international trade policy.

It has often been argued, both in the theoretical literature and policy discussions, that there are significant benefits to preferential trade agreements between geographically proximate countries (the so-called 'natural trading partners theory'). In "Are Regional Trading Partners 'Natural'?" I critically assess this popular contention. The paper first develops a broad general equilibrium framework that captures the essence of the costs and benefits of preferential trade agreements. Then I use this framework to provide a method for estimation of welfare effects resulting from preferential trade liberalization via-a-vis any subset of one's trading partners. Then I evaluate the natural trading partners idea was evaluated using US trade data from 1960-1990, and I find that there is no evidentiary support for the idea. This paper will be published in the *Journal of Political Economy*.

In popular discussion much has been made recently of the susceptibility of government policies to lobbying by foreigners. The general presumption has been that such interactions have a deleterious effect on the home economy. In "*Foreign Lobbies and US Trade Policy*", Kishore Gawande, Michael Robbins and I investigate this presumption. This paper argues that bending trade policy in a direction that would suit foreigners may not in fact be harmful to domestic welfare. A simple example of this point is provided when the policy outcome absent any lobbying by foreigners is characterized by welfare reducing trade barriers. Then lobbying by foreigners for reductions in such barriers may, in fact, improve welfare.

We then investigate this idea empirically with a newly assembled data set on foreign political activity in the US. Our analysis suggests that foreign lobbying activity has significant impact on trade policy - and in the predicted direction (of lowering trade barriers).

In "The Factor Content of Bilateral Trade: An Empirical Test", Yong-Seok Choi and I implement a new empirical test of some specific predictions of the Heckscher-Ohlin model of international trade. This classic model makes predictions about the factor content (i.e., the labor or capital intensity) of goods in bilateral trade between trading partners. Our test uses theoretical assumptions substantially more general than those made in the literature previously. We use detailed OECD trade and production data to implement the tests. The theory found quite limited validation in the data: Only fifty five percent of trade flows between country pairs were consistent with the restrictions implied by the theory.

As Jacob Viner (1950) famously showed in his classic theoretical work on the issue, the establishment of a free trade agreement between a subset of trading partners in the world does not necessarily result in enhanced welfare for the partner countries or the rest of the world. In "On Necessarily Welfare Enhancing Free Trade Areas", Arvind Panagariya and I focus on the design of free trade agreements where welfare improvement is guaranteed.

This paper also sheds new light on the rules of origin required to support such necessarily welfare enhancing free trade agreements. This paper will be published in the *Journal of International Economics*.

Since Adam Smith's day, a popular argument in favor of maintaining protection has been that protection can be used to extract concessions from trading partners. According to this view, protection should be abandoned only when the trading partner also agrees to dismantle its barriers to trade. In "Reciprocated Unilateralism: A Political Economy Approach", Devashish Mitra and I analyze the impact of unilateral trade liberalization by one country on its partner's trade policies. We assume that trade policy in the partner country is determined by interactions in political markets between various organized lobby groups and the government (as in the classic work of Stigler, Peltzman and Becker and more recent work of Grossman and Helpman (1994)), and we assume that the lobby groups themselves are endogenously formed.

In this context, we find that unilateral liberalization may induce reciprocal tariff reductions by the partner country. Intuitively, suppose country A unilaterally lowers tariffs on imports. This has the effect of increasing the incentives for the export lobby in the partner country B to form and to lobby effectively against domestic producers in B in favor of lower protection. The results stand in sharp contrast to the policy arguments that suggest that closing (or threatening to close) one's markets would help pry open the markets of others.

Finally, Kishore Gawande and I completed a survey chapter on empirical analyses of the political economy of trade policy titled, "The Political Economy of Trade Policy: Empirical Approaches" for the new *Handbook of International Trade* (James Harrigan, ed.).

John G. Matsusaka

The bulk of my visit to the Stigler Center was spent working on a book manuscript, tentatively titled, "For the Many or the Few: How the Initiative Process Changes American Government." The initiative process is a form of direct democracy that allows citizens to pass laws without their elected representatives. It was introduced into the United States more than 100 years ago, and is now available in half of the states and cities, and more and more countries across the world.

The fundamental question concerning the initiative process is the same as it was 100 years ago: does the initiative allow wealthy special interests to subvert the political process, or does it allow the majority to defend its interests against organized groups that are influential in the legislature? The book tries to answer this question by examining fiscal data for states and cities over the last century.

First, the book shows that the initiative does have a measurable and systematic effect on tax and spending policies. That is, the initiative matters. States subject to the initiative (1)

cut overall spending and taxes, (2) decentralize spending from state to local governments, and (3) shift fund raising away from broad-based taxes and into user fees and charges.

Second, the book examines a large amount of opinion poll evidence to assess whether these policy consequences are contrary to or in accordance with the will of the majority. If the subversion hypothesis is true, then a majority of people should be opposed to (1)-(3). If the majority defense view is correct, then a majority should support (1)-(3). The evidence is surprisingly uniform: a majority expresses approval for each of the three policy effects of the initiative. Thus, there is no evidence in support of the subversion hypothesis, at least when it comes to fiscal policy.

There is now a broad consensus that competition offers many benefits to human societies, whether it is competition between firms in product markets, or competition in political markets between parties for the right to rule. The initiative can be seen as introducing another form of competition: competition to propose and approve laws. As such, we can expect increasing popular interest in the initiative and other forms of direct democracy, and a growing need for evidence on the consequences of these institutions.

During my stay, I also worked on three articles. The first, “Political Resource Allocation: Benefits and Costs of Voter Initiatives,” with Nolan M. McCarty (Princeton University) develops a model to try to understand the conditions under which special interests would be able to subvert the political process. We tested the model using fiscal and political data over the last 30 years, and found some support. The paper will be published in the *Journal of Law, Economics, and Organization*.

The second article, “Government Spending and Budget Referendums: Evidence from Swiss Cantons,” with Lars Feld (University of St. Gallen, Switzerland), studies the spending behavior of Swiss cantons (analogous to U.S. states) over the last 20 years. In many Swiss cantons, new government spending programs do not go into effect unless voters approve them in a referendum. The paper investigates what effect, if any, the referendum requirement has on government spending. We find that the referendum restrains overall by spending by at least 15 percent.

The third article, “Decision Processes, Agency Problems, and Information: An Economic Analysis of Budget Procedures,” with Anthony Marino (University of Southern California) develops a model to identify the tradeoffs between alternative decision processes. The stepping off point for the project is the observation that organizations (governments, corporations, universities) use a variety of different processes to make decisions. For example, some school districts employ a delegated budgeting process in which the school board makes all the spending decisions. Others require voter approval in a referendum before spending can go into effect. Our goal in the paper was to develop a model incorporating the benefits and costs of alternative processes, so that we could begin understanding the tradeoffs between the alternatives.

William Vogt

During my visit to the Stigler Center, I completed work on three papers, two in the area of health economics (both with Jayanta Bhattacharya of RAND) and one in the area of the economics of education (with Robert Strauss of Carnegie Mellon).

A stylized fact in the industrial organization of the pharmaceutical industry is that the price of the branded version of a drug does not fall when the patent on that drug expires and a generic enters. In fact, a substantial body of previous work (and our work) finds that prices for the branded version of a drug rise with patent expiry. Explaining this "generic competition paradox" (Scherer, 1993) is the purpose of the first paper, "A Simple Model of Pharmaceutical Price Dynamics." Our starting point is that pharmaceutical companies spend a great deal of money marketing their drugs to physicians. Much of this marketing takes the form of conveying information to physicians. For example, 'detailers' discuss the indications, safety, efficacy, mechanism of action, and so forth of their drug absolutely and relative to competitors.

We construct a theoretical model that focuses on the pharmaceutical company's problem of managing the stock of knowledge about its drug. A larger stock of knowledge about a drug held by physicians leads to a larger demand for the drug. This stock of knowledge can be increased both by pharmaceutical company advertising expenditures and by physician experience with the drug. This gives the pharmaceutical company an incentive to begin the drug's life with a low price (to stimulate usage and, therefore, physician experience) and high advertising to build up its knowledge stock. As time passes, price rises and advertising falls, as the firm capitalizes on the knowledge stock.

Importantly, once the generic enters it will reap some of the benefits of the branded drug's knowledge stock. Because the benefits from extra investment in the knowledge stock are now going to be shared with the generic producers, the branded firm's incentives to keep price low and advertising high are reduced. Accordingly, our model predicts a pattern of increasing prices and falling advertising both before and after patent expiry, with prices rising faster and ads falling faster before expiry. These predictions fit both the patterns in our data and the patterns found in previous research.

Conventional wisdom holds that mandated employment health insurance benefits cause wages to fall (as employers care about total employment costs and as employees may find the benefits desirable) but that the level of employment typically does not fall. In "Could We Tell if Health Insurance Mandates Cause Unemployment? A Note on the Literature" Jay Bhattacharya and I examine part of the empirical basis for the conclusion that employment effects of health insurance mandates are zero. Typically, in empirical work in this area authors estimate a statistical model of some sort and then test the hypothesis that there is no effect of mandates on employment. If the test allows this hypothesis to be accepted, the author then concludes that there is no effect of the law on employment.

The attitude behind this standard classical statistical testing framework is: "I will assume that there is no effect on employment unless I see strong evidence of such an effect." We perform 'power' calculations (whereby these assumptions are reversed) on some of the estimates from the mandates literature to see how strong the evidence really is in favor of the null hypothesis of no effect of mandates on employment. We find that the evidence is not strong at all. For example, Gruber (1992, 1994) concludes, based on accepting a hypothesis of no-effect in a classical test, that mandated maternity benefits (nationally imposed in the US in 1979) had no effect on employment. However, the testing method he uses would have missed a true employment effect of almost two percentage points fifty percent of the time and would have missed a true employment effect of almost four percentage points five percent of the time. When changing his specification somewhat to account for correlation in the errors these figures rise to three and five percentage points, respectively. These are large employment effects, especially for such an inexpensive mandate.

As states move toward requiring teachers take tests in order to become and remain certified, the question of what, exactly, should be tested arises. In our paper "It's What You Know, Not How You Learned to Teach it: Evidence from a Study of the Effects of Knowledge and Pedagogy on Student Achievement," Bob Strauss and I explore this question in the context of Pennsylvania public schools. In Pennsylvania in our sample period, teachers were required to take a standardized test in order to become certified to teach. This test has two components: a general knowledge component much like the SAT and a professional knowledge component that tests knowledge of education theory. We find, in a simultaneous equations framework, that the teacher general knowledge has a large, positive effect on student outcomes (measured by standardized test scores) and that professional knowledge has a small and in some specifications negative effect on student outcomes.

Finally, I continued work on a joint project with Martin Gaynor of Carnegie Mellon in which we are examining competition in the California hospital industry. We are estimating a structural model of competition in that market for two purposes. First, we are interested generally in simulating the effects of mergers in this industry on conduct. Second, we are interested in estimating any competition-relevant behavioral differences between for-profit and not-for-profit hospitals. Both of these questions are motivated by events in health anti-trust law. Over the decades of the eighties and nineties, the US government challenged numerous hospital mergers and has uniformly lost those challenges. One of the rationales courts have used for permitting what would otherwise seem to be illegal mergers is that not-for-profit hospitals will not use any market power they gain through a merger to the detriment of consumers, since not-for-profit hospitals have the best interests of the community at heart. We want to see if the evidence can support such presumed differences in the motives of for-profit and not-for-profit hospitals.