Patents and Cumulative Innovation: Causal Evidence from the Courts[1]

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Abstract

Cumulative innovation is central to economic growth. Do patent rights facilitate or impede such follow-on innovation? This paper studies the effect of removing patent protection through court invalidation on the subsequent research related to the focal patent, as measured by later citations. We exploit random allocation of judges at the U.S. Court of Appeal for the Federal Circuit to control for the endogeneity of patent invalidation. We find that patent invalidation leads to a 50 percent increase in subsequent citations to the focal patent, on average, but the impact is highly heterogeneous. Patent rights appear to block follow-on innovation only in the technology fields of computers, electronics and medical instruments. Moreover, the effect is entirely driven by invalidation of patents owned by large patentees that triggers entry of small innovators, suggesting that patents may impede the 'democratization' of innovation.

1 Introduction

One of the dominant features of modern innovation is that research is cumulative. New genetically modified crops, memory chips and medical instruments are typically enhancements of prior generations of related technologies. Of course, cumulative innovation is not new. Economic historians have emphasized the role of path dependence in the development of technology, documenting important examples of how past successes and failures serve as 'focusing devices' as Rosenberg (1976) calls them, that guide the direction of later technological inquiry.[1] However, the increasing importance of basic science in shaping the direction of technological development has intensified the process of cumulative innovation.

Cumulative innovation, and the knowledge spillovers that underlie it, lie at the heart of the recent economic literature on innovation and growth. Leading examples of these endogenous growth models include Grossman and Helpman (1991), Aghion and Howitt (1992), Aghion, Harris and Vickers (1997) and Acemoglu and Akcigit (2012). At the same time, there is an extensive empirical literature showing that R&D creates knowledge spillovers, which increase both productivity growth and subsequent innovation.[2] This consensus on the centrality of knowledge spillovers to innovation, and innovation to growth, is the primary justification for government R&D-support policies.

In this paper we study how patent rights affect the process of cumulative innovation. The patent system is one of the main instruments governments use to increase R&D incentives, while at the same time promoting follow-on innovation.[3] However, there is growing concern

[1]This cumulative feature is reinforced by the constraints imposed by the prevailing stock of scientific knowledge on the feasible avenues for technology development (Rosenberg, 1994; Mokyr, 1990, 2002). This is not say that science dictates only one path for the development of technology at any point in time. Indeed, recent theoretical work emphasizes the role of diverse research approaches in technological development (Acemoglu, 2012). Of course, fortuitous elements also play a role in both technological and scientific developments (Rosenberg, 1994) refers to the link as one of 'soft determinism'), and research tools and other technological innovations have themselves historically shaped the limits of what was feasible in scientific inquiry.

[2]For a recent survey of the literature, see Jones (2005). In a recent paper, Bloom, Schankerman and van Reenen (2013) show that R&D also creates negative (pecuniary) externalities through product market rivalry which can lead to over-investment in R&D. But their empirical results confirm that positive externalities dominate, with social returns to R&D exceeding private returns, at least on average.

[3]The 'adequate disclosure' requirement in patent law (35 U.S.C. Section 112) is a recognition of the importance of cumulative innovation. This provision requires the patent applicant to describe the invention in order to promote information diffusion and 'enable' development of follow-on improvements of the original invention.

among academic scholars and public policy makers that patent rights are themselves becoming an impediment, rather than an incentive, to innovation. The increasing proliferation of patents, and the fragmentation of ownership rights among firms, are believed to raise transaction costs, constrain the freedom of action to conduct R&D without extensive licensing, and expose firms to ex-post holdup through patent litigation (Heller and Eisenberg, 1998; Bessen and Meuer, 2008). In the extreme case where bargaining failure in patent licensing occurs, follow-on innovation can be blocked entirely. These issues are particularly acute in 'complex technology' industries where innovation is highly cumulative and requires the input of a large number of patented components held by diverse firms. These dangers have been prominently voiced in public debates on patent policy in the United States (National Research Council, 2004; Federal Trade Commission, 2011) and recent decisions by the Supreme Court (e.g., eBay Inc. v. MercExchange, L.L.C., 547 U.S. 338, 2006). These concerns have been intensified by the acceleration in patenting over the past three decades, especially in high technology industries including biotechnology, information and communications technology, and software.[4]

The economic research on the impact of patent rights on cumulative innovation has been primarily theoretical.[5] The main conclusion from these studies is that anything can happen – patent rights may impede, have no effect, or even facilitate subsequent technological development. It depends critically on assumptions regarding the efficiency of contracting between the initial and later innovators and how the market for downstream innovation is organized (whether coordination failures in subsequent innovation is likely). In an early contribution, Kitch (1977) argues that patents enable an upstream inventor to organize investment in follow-on innovation more efficiently and to mitigate rent dissipation from downstream patent races that would otherwise ensue. This 'prospecting theory' suggests that patent rights facilitate cumulative innovation. Green and Scotchmer (1995) show that, while upstream patent rights

[4]Over the period 1976-1999 the number of patent applications in the U.S. (granted by 2010) grew at an average annual rate of 4.4 percent, but accelerated to 6.7 percent in the subperiod 1986-99. The recent growth was particularly rapid in high tech industries – e.g., 9.3 percent in pharmaceuticals, 9.2 in medical instruments, 26.9 in biotechnology, 15.8 in semiconductors and 21.0 percent in software (up to 1996). For discussion of the developments that contributed to this acceleration, see Kortum and Lerner, 1998).

[5]For a good overview of the theory, see Scotchmer (2004). Merges and Nelson (1990) provide an interesting discussion, from an economic and legal perspective, of how patents affect sequential innovation, together with important historical examples.

affect the distribution of profits (and thus research incentives) between different generations of innovators, patents will not prevent later (jointly) profitable innovation from occurring as long as bargaining between the parties is efficient. Though the assumption of Coasian bargaining is strong, this work is important because it focuses our attention on bargaining failure as the source of any blocking effect patent rights might create. Finally, a number of papers have shown how patent rights can block surplus-enhancing innovation when bargaining failure occurs. This is typically associated either with asymmetric information or divergent expectations about the value of the first generation invention or the prospects for the invention building on it (Bessen and Maskin, 2009), or coordination failures when a downstream patentee needs to license multiple upstream patents in order to conduct its R&D (Shapiro, 2001; Galasso and Schankerman, 2010).

This diversity of theoretical conclusions highlights the need for empirical research. It it important not only to establish whether patent rights block subsequent innovation, but also how this process might in turn affect the industrial organization of innovation. For example, does such blockage occur between all types of upstream and downstream firms, or is the problem concentrated among particular subsets of innovating firms – e.g., small firms with patent rights blocking larger firms from building on their innovations, or large patentees blocking smaller firms? In addition to being potentially important for designing competition policy, the issue is also relevant for management research because understanding how patents can be a source of competitive advantage is crucial for developing effective intellectual property strategies (Somaya, 2012).

There are two empirical challenges in studying the effect of patents on cumulative innovation. First, cumulativeness is difficult to measure. In this paper we follow the large empirical literature that uses citations by later patents as a way to trace knowledge spillovers (for a survey, see Griliches, 1992). The second problem in identifying the causal effect of patent rights on later innovation is the endogeneity of patent protection. For example, innovations are more likely to be made, and patented, when there is greater commercial value. Thus shocks to the value of the underlying technology may substantially affect both the decision to maintain existing patent protection by payment of renewal fees and follow-on innovation.

In important papers, Murray and Stern (2007) and Williams (2013) provide the first causal evidence that patents have had some blocking effect in the biomedical field. Murray and Stern exploit patent-paper pairs to study how citations to scientific papers are affected when a patent is granted on the associated invention. Williams studies the impact of the issuance of the Celera patent on subsequent human genome research. Interestingly, both papers find roughly similar magnitudes – patent rights appear to cause about a 20-40 percent reduction in follow-on research. These important studies focus on very specific (albeit significant) innovations in human genome and biomedical research. It is hard to know whether their conclusions generalize to other industries, and whether the effect varies across different types of patentees and later innovators. Understanding how the blocking effect of patents may vary across technology fields and patent owners is essential for thinking about how best to design the strength and scope of patent protection.

In this paper we adopt a novel identification strategy to estimate the *causal effect* of patent protection on cumulative innovation. We use the patent invalidity decisions of the U.S. Court of Appeal for the Federal Circuit, which was established in 1982 and has exclusive jurisdiction in appellate cases involving patents. It is a fortunate institutional fact that judges are assigned to patent cases through a computer program that *randomly* generates three-judge panels, with decisions governed by majority rule. We exploit this random allocation of judges to construct an instrumental variable to address the potential endogeneity of invalidity decisions. Because patents constitute prior art, even if they have been invalidated and thus put into the public domain, later applicants and patent office examiners are required to cite these patents when relevant. This allows us to examine how invalidation of a patent affects the rate of subsequent citations to that patent.

Patents that reach the Federal Circuit are a selective sample of highly valuable "superstar" patents. To cite one example, in August 2006 the Federal Circuit invalidated one of Pfizer's key patents required for the production of the cholesterol-lowering drug Lipitor, the largest-selling drug in the world. Our reliance on superstar patents to estimate the effect of patent rights on cumulative innovation is similar to Azoulay et. al. (2007) who rely on the death of superstar scientists to estimate the magnitude of knowledge spillovers. It is more reasonable to start by analyzing superstar patents rather than a random sample of patents, not least because we know that the distribution of patent values is highly skewed (Schankerman and Pakes, 1986) and policy should be most concerned about the potential blocking of later innovation that builds on these valuable patents, where potential welfare costs are likely to be larger.

There are three main empirical findings in the paper. First, using the substantial heterogeneity in judges tendency to invalidate patents to control for endogeneity of the court decision, we find that patent invalidation leads to about a 50 percent increase in subsequent citations to the focal patent, *on average*. This finding is robust to a variety of alternative specifications and controls. Moreover, we show that this impact begins only after about two years following the court decision, which is consistent with the onset on follow-on innovation (rather than simply being a publicity effect from the court's decision).

Second, we find that the impact of patent invalidation on subsequent innovation is highly heterogeneous. For most patents we find that the marginal treatment effect of invalidation is not statistically different from zero. The positive impact of invalidation on citations is concentrated on a small subset of patents which have unobservable characteristics that are associated with a lower probability of invalidity (i.e., stronger patents). There is large variation across broad technology fields in the impact of patent invalidation and the effect is concentrated in fields that are characterized by two features: complex technology and high fragmentation of patent rights. This finding is consistent with the predictions of theoretical models that emphasize bargaining failure in licensing as the source of blockage. Patent invalidation has a significant impact on cumulative innovation only in the fields of computers and communications, electrical and electronics, and medical instruments. There is no significant effect in chemicals, pharmaceuticals or mechanical technologies.

Finally, we find that the increase in citations is entirely driven by invalidation of patents owned by large patentees and that the impact is primarily on the "extensive margin" – i.e., the number of small patentees citing the focal patent. Invalidation of large firm patents appears to 'democratize' innovation by small firms, but this effect is not found for small or medium sized patentees, or for subsequent innovation by medium or large sized firms. This striking result strongly suggests that bargaining failure in patent licensing – and thus the blocking effect of patent rights – seems to occur primarily in cases involving large patent holders and small potential licensees.

The paper is organized as follows. In Section 2 we present a simple model to characterize the conditions under which patents facilitate, block or have no effect on follow-on innovation. Section 3 describes the new data set. In Section 4 we develop the baseline econometric model for estimating the causal effect of patent rights and present the empirical results. In Section 5 we extend the analysis to allow for heterogenous marginal treatment effects, and empirically link them to characteristics of the patent case. In Section 6 we show how the effect of patent invalidation depends on the characteristics of the patentee and later citing innovators. In addition, we decompose the overall effect into an extensive margin (number of later citing firms) and an intensive margin (number of later citing patents per firm). Section 7 examines the impact of invalidation on self-citations. In Section 8 we provide a brief discussion of the welfare implications of our findings. Brief concluding remarks close the paper.

2 Analytical Framework

The granting of patent rights involves a basic trade-off between ex ante incentives and ex post efficiency. The market power conferred by a patent increases innovation incentives, but also reduces total surplus due to higher prices. This trade-off is well understood in the innovation literature. However, patents can also create a dynamic cost by blocking valuable sequential innovation, in cases where the second generation firm requires a license on the earlier technology and the bargaining between the two parties fails. In this section we present a simple analytical framework that characterizes the conditions under which patents are likely to block, facilitate or have no effect, on follow-on investment, and we use the framework for organizing the different theoretical models in the literature.

There are two firms, x and y. Firm x produces technology x and firm y has an idea for a downstream technology y. To develop the idea and obtain a patent, firm y needs to sustain a cost c. We assume that, if technology x is patented, technology y can be sold only if the two firms sign a licensing deal.[6] Let $\pi_x(x, 0)$ denote the profits firm x makes if x is protected by a

[6] This is the case when technology y is a patentable "new and useful improvement" of technology x (35 U.S.C. 101). The patents on x and y are referred to as 'dominant' and 'subservient', respectively (Merges and Nelson, 1990). If the downstream invention reflects a large enough innovative step, it may be patentable and not require a license from the upstream patentee. Nevertheless, as long as firm y (at the time of her R&D investment) assigns some positive probability to needing such a license, the presence of an upstream patent will affect her innovation incentives.

patent and there is no licensing to firm y, and $\pi_y(0, y)$ be the profits firm y makes when x is not protected by a patent. If there is a patent on x and licensing takes place, we let $\pi_y(x, y)$ and $\pi_x(x, y)$ denote the profits of the two firms (net of licensing fees) and $\Pi(x, y) = \pi_y(x, y) + \pi_x(x, y)$ be the joint surplus.

There are three inequalities that determine downstream innovation incentives:

$$\pi_y(x, y) - c \geq 0 \tag{1}$$

$$\pi_y(0, y) - c \geq 0 \tag{2}$$

$$\Pi(x, y) - \pi_x(x, 0) - c \geq 0. \tag{3}$$

Inequalities (1) and (2) show the conditions to have innovation by firm y when technology x is patented and when it is not, respectively. Inequality (3) shows the condition required to have licensing by x to y. The maximum profits that firm x can obtain from licensing is $\Pi(x, y) - c$ and this needs to be larger than $\pi_x(x, 0)$ for licensing to be profitable.

Notice that the difference between total profits with and without technology y, $\Pi(x, y) - \pi_x(x, 0)$, is increasing in the degree of complementarity between the innovations x and y. If x and y are perfect complements, $\pi_x(x, 0) = 0$. In the case of perfect substitutes $\Pi(x, y) = \pi_x(x, 0)$ and follow-on innovation will be blocked for any $c > 0$. More generally, for given values of $\pi_y(x, y)$ and $\pi_y(0, y)$, an increase in the degree of complementarity expands the range of cost parameters, c, under which follow-on innovation takes place. Thus (3) implies that, when x is patented, sequential innovation does not take place when the substitutability between x and y is high enough – i.e., when the business stealing effect of innovation is strong.

Building on this simple framework, we now contrast the different classes of models that have emerged in the innovation literature.

Positive impact of patents on follow-on innovation

Using (1)-(3), a patent on x has a positive impact on downstream innovation if

$$\pi_y(0, y) < c \leq \min\{\pi_y(x, y), \Pi(x, y) - \pi_x(x, 0)\}.$$

This condition is implicitly assumed in Kitch (1977), the first paper to point out that upstream patents may be beneficial for downstream innovation. He describes an environment in which, in the absence of an upstream patent, development of technology improvements is impeded by coordination failures and free riding among downstream innovators and thus $\pi_y(0,y) - c < 0$. A patent on technology x allows the upstream firm to act as a gatekeeper and coordinate downstream investments. This has a positive effect on joint surplus, $\Pi(x,y) - c - \pi_x(x,0) \geq 0$, and firm y's incentive to innovate, $\pi_y(x,y) - c \geq 0$.

Another example is the model by Arora (1995) in which development of downstream technology requires transfer of tacit know-how from firm x to firm y. Because it is difficult to contract on tacit knowledge, transfer only occurs when bundled with patent x in a licensing contract. In the absence of a patent on x, know-how is not transferred and technology y is not developed because $\pi_y(0,y) - c < 0$. With a patent on technology x, know-how is transferred and this allows downstream innovation to take place and increases joint surplus, $\Pi(x,y) - c - \pi_x(x,0) \geq 0$.[7]

No effect of patents on follow-on innovation

A patent on technology x has no effect on subsequent innovation if

$$\min\{\pi_y(x,y),\ \Pi(x,y) - \pi_x(x,0), \pi_y(0,y)\} \geq c.$$

This condition is satisfied in the model by Green and Scotchmer (1995) in which downstream innovations are joint surplus enhancing, $\Pi(x,y) - c - \pi_x(x,0) \geq 0$, and Coasian ex ante contracting guarantees that the downstream innovation is developed independently of the presence of a patent on technology x (i.e. both $\pi_y(0,y) - c \geq 0$ and $\pi_y(x,y) - c \geq 0$).[8]

Negative effect of patents on follow-on innovation

A patent on technology x has a negative effect on subsequent innovation if

$$\min\{\pi_y(x,y),\ \Pi(x,y) - \pi_x(x,0)\} < c \leq \pi_y(0,y).$$

This condition is typically satisfied when there are frictions in the licensing process, and these can arise for several reasons. First, ex ante licensing may not take place in the presence

[7] Specifically, in Arora's model $\Pi(x,y) = \alpha V(z) - C(z)$ where z is the amount of know-how transferred from the licensor to the licensee, $V(z)$ is the licensee benefit, $C(z)$ is the cost of know-how transfer and $0 \leq \alpha \leq 1$ is the patent breadth. As α rises, the amount of know-how transferred increases and this generates greater downstream innovation incentives.

[8] Green and Scotchmer allow the profits of the two parties to depend on the length and breadth of the patent. While these variables affect the incentives of firm x to develop the upstream technology, once x has been developed frictionless bargaining ensures that efficient downstream investment takes place.

of asymmetric information between the upstream and downstream innovators, as shown by Bessen (2004), Bessen and Maskin (2009) and Comino et. al. (2011). Moreover, Priest and Klein (1984) and Galasso (2012) show that licensing breakdown may occur even with symmetric information when parties have divergent expectations about the profitability of the technology. The risk of hold up, high litigation costs and pro-patent remedy rules all reduce the expected value of ex post licensing profits for the downstream innovator $\pi_y(x, y)$ and thus dilute his incentives to develop y, $\pi_y(x, y)$.[9]

Second, bargaining failure can arise when patent rights are fragmented and a downstream firm requires licenses from many different patentees to conduct its research. In this case, uncoordinated bargaining among the parties leads to 'royalty stacking' that reduces the licensee's profit and, in extreme cases, can actually block downstream development if $\pi_y(x, y) - c < 0$ (Heller and Eisenberg, 1998; Shapiro, 2001; Lemley and Shapiro, 2007; Galasso and Schankerman, 2010).[10]

The condition is also satisfied in recent models by Aghion, Dewatripont and Stein (2008) and Murray et. al. (2008), which argue that academic research on base technologies (e.g. research tools) can increase the profitability of downstream research because of the open science regime, and lower wages of scientists, in academia.[11]

To summarize, this framework suggests that blockage is more likely when: 1) the degree

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[9] To see this, assume that profits of firm y are private information. Firm x believes firm y profits are equal to $\overline{\pi}$ with probability ρ and equal to 0 with probability $1-\rho$ with $\rho\overline{\pi} < c < \overline{\pi}$. If ρ is small enough, the expected joint profits $\Pi(x, y)$ are small and ex ante licensing will not take place. In the absence of ex ante licensing, firm y will invest only if profits are $\overline{\pi}$. If investment takes place, firm x will learn that firm y profits are equal to $\overline{\pi}$. Because after investment the cost c is sunk and firm x has learned that y has high profits, firm x will expropriate all the profits of y. This ex post expropriation will induce y not to invest in innovation.

[10] For example, in the setting of Lemley and Shapiro (2007), the downstream firm's profit is

$$\pi_y(x, y, N, \theta) = P(q)q - (c + r_x(\theta) + \sum_{i=1}^{N} r_i(\theta))q$$

where $P(q)$ is the demand function for the downstream product, $r_x(\theta)$ is the royalty per unit of output paid to firm x, $r_i(\theta)$ are royalty rates paid to N other patentees with $1 \leq i \leq N$, θ is the degree of complementarity among the $N+1$ patents and $r'(\theta) > 0$ for each patentee. Because of uncoordinated bargaining, $\pi_y(x, y, N, \theta)$ decreases in N and θ and downstream innovation does not take place when N and θ are large enough.

[11] For example, in Murray et. al. (2008), the payoff to the downstream innovator is $\pi_y(x, y) = V - w$ when the upstream innovation is patented by a firm, where V is product market profits and w is the wage to the scientist. When upstream innovation is controlled by academia and unpatented, the downstream firm extracts $\pi_y(0, y) = V + \psi - w$ where $\psi > 0$ is the extra rent due to the absence of upstream patenting (and possibly lower wages). If $\psi > c - \pi_y(x, y) > 0$, downstream innovation takes place only when x is unpatented.

of asymmetric information is high, 2) the downstream innovator needs to bargain with multiple patentees, and 3) there is a high degree of substitutability between the upstream and downstream innovations. The empirical literature has documented that uncoordinated bargaining and asymmetric information are more likely when patent ownership is fragmented (Ziedonis, 2004) and in complex technology areas where downstream innovation builds on numerous patented inputs (Cohen et. al., 2000). In the empirical analysis in Section 5, we examine how these two features – fragmentation and complexity – influence the extent to which patent rights block cumulative innovation.

3 Description of the Data

The empirical work is based on two data sets: the decisions of the Court of Appeal for the Federal Circuit, and the U.S. Patent and Trademark Office (USPTO) patent dataset.

The Federal Circuit has exclusive jurisdiction over appeals in cases involving patents and claims against the federal government in a variety of subject matter.[12] The Federal Circuit consists of twelve judges appointed for life by the president. Judges are assigned to patent cases through a computer program that randomly generates three-judge panels, subject to the judges' availability and the requirement that each judge deals with a representative cross section of the fields of law within the jurisdiction of the court (Fed. Cir. R. 47.2). Decisions are taken by majority rule. We obtain the full text of patent decisions by the Federal Circuit from the LexisNexis QuickLaw dataset. This contains a detailed description of the litigated dispute, the final decision reached by the court, and the jurisprudence used to reach the decision. Using keyword searches we identify each case involving issues of patent validity from the establishment of the court in 1982 until December 2008. For each case we record the following information: docket number, date of the decision, patent identification number, name of the three judges involved, name of the plaintiff, name of the defendant, and whether the patentee is the plaintiff or the defendant.

[12] The Federal Circuit was established by the U.S. Congress on October 1, 1982. It merged the U.S. Court of Customs and Patent Appeals and the appellate division of the U.S. Court of Claims. The creation of this specialized court was proposed by the Commission on Revision of the Federal Court Appellate System (also known as the Hruska Commission) to bring greater uniformity in patent law and enforcement, and to reduce the caseload crisis in the federal courts of appeal (Seamon, 2003).

Information about each patent in the sample is obtained from the USPTO patent database. We also identified the patents citing the litigated patent from two sources: the USPTO citations data for sample patents granted in the period 1975-2010, and Google Patents for sample patents granted before 1975.

We use the number of citations by subsequent patents to the focal patent as a measure of cumulative innovation. Patent applicants are required to disclose known prior art that might affect the patentability of any claim (Code of Federal Regulations, Ch. 37, Section 1.36), and any willful violation of this duty can lead to the USPTO rendering the patent unenforceable due to 'inequitable conduct'. Importantly for our purposes, the expiration or invalidation of a patent has no impact on its prior art status (35 U.S. Code, Section 102), so the requirement to cite it remains in place.

Patent citations have been widely used in the economics literature as a proxy for follow-on research (Murray and Stern, 2007; Furman and Stern, 2011 and Williams, 2013). They are the only practical measure of cumulative innovation available for large scale studies, but certain qualifications should be kept in mind. From an economic perspective, patent citations play two distinct roles: first, they indicate when the new invention builds on prior patents (and thus may need to license the upstream patent), and second, citations identify prior art that circumscribes the property rights that can be claimed in the new patent. Citations will underestimate the extent of cumulative innovation in cases where inventors develop improvements that are not patented (or patentable). But citations can also overestimate it, when they only indicate prior art that limits the claimed property rights but do not indicate that the inventor actually built on the prior patent.[13] However, the fact that we use citations primarily as an endogenous outcome measure makes any measurement error less problematic.

The main variables used in the empirical analysis are described below.

PostCites: citations received from patents of other assignees in a five year window after

[13] Not all citations originate from applicants; some are added by USPTO examiners during the granting process. Because the USPTO began reporting examiner and applicant citations separately only for patents granted after 2001 (Alcacer and Gittleman, 2006), we cannot distinguish between the two types of citations for most of the patents in our data (only 4 percent of our sample patents were granted after 2001). For our purposes of tracing cumulative innovation, examiner-added citations may introduce measurement error if they do not reflect prior art that the new patent applicant is aware of when she undertook her R&D. However, examiner citations may reduce measurement error if applicants strategically withhold citations.

the Federal Circuit decision. This is our primary measure of cumulative innovation. Because of granting delays, we date the citing patents using their application year rather than grant year.

PostSelfCites: citations received from patents owned by the same patentee as the focal (litigated) patent in a five year window after the Federal Circuit decision. We will use this alternative dependent variable to explore the effect of invalidity on the patentee's research trajectory.

Invalidated: a dummy variable equal to one if the Federal Circuit invalidates at least one claim of the patent. This is the main explanatory variable of interest, and represents the removal of patent rights.[14]

PreCites: citations received from patents of other assignees applied for in the period between the grant of the patent and the Federal Circuit decision

PreSelfCites: citations received from patents of the same patentee as the focal patent applied for in the period between the grant of the patent and the Federal Circuit decision

Claims: total number of claims listed in the patent document

Technology field: dummy variables for the six technology classes in Hall et. al. (2001) – chemicals, computers and communications, drugs and medical, electrical and electronics, mechanicals, and others. We will also employ a narrower definition – the 36 two-digit subcategories defined by Hall et. al. (2001).

Finally, we construct a set of dummy variables for the year when the Federal Circuit decision is issued and for the age of the patent.

The final dataset contains 1357 Federal Circuit patent validity decisions, covering 1258 distinct patents.[15] Table 1 provides some summary statistics. The Federal Circuit invalidates in 39 percent of the cases, and in 61 percent of those decisions the entire patent is invalidated. Figure 1 shows substantial variation in the age distribution of litigated patents (at the time of

[14] We experimented with an alternative definition of invalidation as whenver Claim 1 of the patent (typically representing the primary claim) is invalidated. About 40 percent of patents are invalidated on our baseline measure, and 33 percent using the alternative definition. The empirical results are very similar with both measures. In the empirical results reported below we will also use the fraction of invalidated claims as an alternative explanatory variable.

[15] This is because there are multi-patent cases and some patents are litigated more than once. In the sample, 1169 patents are litigated once, 82 are involved in two cases, and 7 patents are involved in 3 cases.

the Federal Circuit decision). Note that lengthy lower court trials in some cases lead to Federal Circuit decisions occurring after the patent has expired.

Patents involved in Federal Circuit cases are a selected sample of extremely valuable "superstar" patents. For example, in January 2005 the Federal Circuit invalidated the patent for the once-a-week version of Merck's Fosamax, the leading osteoporosis drug in the market at that time. This can be seen in Table 2, which compares characteristics of the patents in the Federal Circuit to patents litigated in lower courts but not appealed, as well as to the universe of patents granted by the USPTO.[16] Drugs and medical patents are more heavily represented in the litigated and Federal Circuit samples than in the overall sample. This is consistent with survey evidence that patent rights are most important in that sector (Levin et. al., 1987). We also see that the number of claims, citations per claims received, and patent generality and originality measures are all higher for litigated than other patents, and even higher for cases appealed to the Federal Circuit. Equality of the means is strongly rejected for all four variables (p-values<0.01). The mean number of claims and citations per claim for patents litigated only at lower courts are different from those appealed to the Federal Circuit (p-values <0.01).

4 Estimating the Impact of Patent Rights

Baseline Specification and Identification Strategy

The final dataset is a *cross section* where the unit of observation is a Federal Circuit case involving patent p.[17] Our main empirical specification is

$$log(PostCites_p + 1) = \beta\ Invalidated_p \tag{4}$$

$$+\lambda_1 log(PreCites_p + 1) + \lambda_2 log(PreSelfCites_p + 1)$$

$$+\lambda_3 log(Claims_p) + Age_p + Year_p + Tech_p + \varepsilon_p$$

The coefficient β captures the effect of invalidation on the subsequent (non-self) citations received by a patent. When $\beta < 0$ invalidation reduces later citations, indicating that patent

[16]To perform this comparison, we use litigation data from Lanjouw and Schankerman (2001) and the NBER patent dataset. Because the lower court litigation data are available only up to 1999, we focus on patents granted during 1980-1999. Of the 1,816,863 patents granted by the USPTO in this period, 8,093 are litigated (0.45 percent) and 877 are involved in Federal Circuit invalidity decisions (0.05 percent).

[17]Even though we have some cases of the same patent litigated more than once, we use the subscript p to denote the patent case to emphazise that our sample is a cross section.

rights have a positive impact on cumulative innovation. A finding of $\beta = 0$ would indicate that patents do not block follow-on innovation. When $\beta > 0$ we would conclude that patents block subsequent innovation.[18]

To control for heterogeneity in the value that the patent has for the patentee and follow on inventors, we include the number of claims and the number of external and self citations received prior to the Federal Circuit decision ($PreCites$ and $PreSelfCites$, respectively) as covariates in the regression. We also include age, decision year and technology field dummies to control for additional hetherogeneity that may be correlated with the court decision and later citations. We report heteroskedasticity-robust standard errors. Because some patents are litigated more than once and some cases involve multiple patents, we also confirm significance using standard errors clustered at the patent or case level.

The major empirical challenge is that the decision by the Federal Circuit to invalidate a patent is endogenous. For example, a positive shock to the value of the underlying technology may increase the citation rate of a patent and, at the same time, induce the patentee to invest heavily in the case to avoid invalidation. This would generate a negative correlation between ε_p and $Invalidated_p$ in equation (4) and a downward bias to the OLS estimate of β.[19] To address potential endogeneity, we need an instrument that affects the likelihood of patent invalidation but does not belong directly in the citations equation. To construct such an instrument, we exploit the fact that judges in the Federal Circuit are assigned to patent cases randomly by a computer program, subject to their availability and the requirement that each judge deals with a representative cross section of legal fields within the court's jurisdiction (Fed. Cir. R. 47.2). However, randomization of judge panels does not ensure randomization of decisions, which can still arise because of information that becomes available during the appellate process that

[18] While a variety of econometric models can be used to estimate the correlation between citations and the Federal Circuit invalidity decisions, the cross-sectional specification is preferable for two reasons. First, the cross-section allows us to use (time invariant) judge allocations as instruments for patent invalidity decision. Second, this specification allows us to examine heterogeneity in the effect of patent invalidation by estimating the Marginal Treatment Effect. Our specification is very similar to those employed in other studies where instrumental variables are used to examine heterogeneous causal effects. For example, Carneiro, Heckman and Vytlacil (2010) collapse a panel into a cross-section and use a time-invariant instrument to estimate heterogeneous effects.

[19] A downward bias could also arise if the existence of relevant prior art makes patent invalidation more likely and at same time reduces the propensity of later innovators to cite the focal patent.

could also be correlated with future citations. For example, a positive shock to the value of technology generates an increase in future citations but also the incentives of the patentee to invest in legal defense and avoid invalidation. The instrument we construct below takes this concern into account.

Since its establishment in 1982, the Federal Circuit patent cases have involved a total of 51 distinct judges, including 22 non-appointed judges that filled in the vacancies during the Senate nomination period. Appendix Table A1 lists the (appointed) Federal Circuit judges in our sample, the number of decisions in which each judge was involved, and the percentage of cases in which each judge voted for patent invalidation.[20] There is substantial variation across judges in the propensity to vote for patent invalidity (which we refer to as judge 'bias'), ranging from a low of 24.4 percent to a high of 76.2 percent.

Our instrumental variable, the Judges Invalidity Propensity (*JIP*), is defined for each case involving patent p as

$$JIP_p = f_p^1 f_p^2 f_p^3 + f_p^1 f_p^2 (1 - f_p^3) + f_p^1 (1 - f_p^2) f_p^3 + (1 - f_p^1) f_p^2 f_p^3$$

where f_p^1, f_p^2, f_p^3 are the fractions of votes in favour of invalidity by each of the three judges assigned to the case calculated for all decisions *excluding* the case involving patent p. In other words, the decision for the focal patent does not enter into the computation of the instrument for that decision. In a simple setting where each judge i votes in favor of invalidity with probability f_p^i , *JIP* captures the probability of invalidation by the three judge panel (decision by majority rule). In an Appendix we show that, under plausible assumptions on the dispersion of private information, *JIP* provides a consistent estimate of the probability of invalidation in a strategic voting model (a la Feddersen and Pesendorfer, 1996) where the thresholds of reasonable doubt differ across judges.

There are two important features of *JIP* that make it a valid instrumental variable. First, the random allocation of judges assures that judges with high propensity to invalidate are not assigned to cases because of unobservable characteristics that are correlated with citations. Second, any additional effect that case-specific unobservables may have on the decision to

[20] The sources for nomination and active service years are http://www.cafc.uscourts.gov/ and Wikipedia.

invalidate patent p (e.g., information revealed during the litigation process) is removed by dropping the decision on patent p from the construction of the instrument for patent p. [21] [22]

Figure 2 plots the distribution of the *JIP* index. There is substantial variation – *JIP* has a mean of 0.34, but ranges from 0.16 to 0.58. Part of the variation in *JIP* may reflect year effects because 'biased' judges may be active only for a limited period of time. To address this, we regressed JIP against a set year fixed effects and find that year effects explain only about 11 percent of the variation.[23]

Our identification strategy is similar to the one employed by Doyle (2007, 2008), who uses differences in the placement tendency of child protection investigators as an instrument to identify the effects of foster care on long term outcomes. The main difference between the two approaches is that our *JIP* index is constructed at the (three judge) panel level. The basic assumption behind this measure is that judges differ in their propensity to invalidate patents. To check this, we construct a dataset with judge-vote as the unit of observation and regress the *Invalidated* dummy against judge fixed effects and controls for the number of claims, external and self-citations prior to the court decision, plus decision year, technology class and patent age fixed effects. We strongly reject the hypothesis that the fixed effects for the different judges are the same (p-value<0.01). The distribution of estimated fixed effects is plotted in Appendix Figure A1 and shows substantial variation in their propensity to invalidate.

[21] A natural alternative to *JIP* is to exploit judge fixed effects. There are two reasons why *JIP* is more compelling. First, *JIP* takes into account that the invalidity decision is taken by a panel of judges, so the *impact* of each judge's invalidity propensity depends on the other members of the panel. Second, in *JIP* the dependence on the endogenous regressor for observation i is removed by dropping that observation in the construction of the instrument (as in the Jackknife IV of Angrist et. al., 1999).

[22] The propensity to invalidate of the panel of judges may induce the litigating parties to settle the case. Theoretical models of patent litigation typically predict that settlement is more likely for low value patents, especially in the presence of large judge bias, either pro- or anti-patent (Galasso and Schankerman, 2010). In our setting, this suggests that the value of patents that reach final adjudication by judge panels with extreme values of JIP will be higher than the value of patents in cases decided by panels with intermediate values of JIP. If patent value is correlated with post-decision citation, this selection would introduce bias to our estimates. The actual impact of this selection bias is ambiguous, however, as it would depend on the relative stakes and bargaining power of the patentee and the challenger.

Empirically, settlement at the appellate level is quite infrequent. Aggregate figures available on the Federal Circuit website show that in the period 1997-2007 about 80 percent of the filed cases were terminated with a panel decision. A possible reason for the low settlement rate is that the identity of judges is revealed to the disputants only after all briefs have been filed, and most of legal costs have already been sunk.

[23] The difference between the sample means of JIP and frequency of invalidity decisions is due to the non-linear nature of JIP.

To provide additional evidence that the estimated variation is inconsistent with judges having identical voting propensities, we construct a counterfactual where judges vote according to the same random process. Specifically, we generate a simulated judge vote that takes into account the effect of observable patent characteristics on the probability of invalidation.[24] Regressing the simulated votes on observable characteristics and judge fixed effects, we do not reject the hypothesis that judge effects are equal (p-value=0.66). The distribution of these simulated fixed effects is also plotted in Figure A1. The difference between the two distributions is striking: the variance of the Federal Circuit fixed effects is much larger than the one we would observe if judges were voting following the same random process.

Our main estimation approach, following Galasso, Schankerman and Serrano (2013), instruments the invalidated dummy with the predicted probability of invalidation obtained from the probit model $\widehat{P} = P(JIP, X)$. When the endogenous regressor is a dummy, this estimator is asymptotically efficient in the class of estimators where instruments are a function of *JIP* and other covariates (Wooldridge, 2002). Specifically, we estimate the following two-stage model

$$Invalidated_p = \alpha\widehat{P}_p + \theta X_p + u_p \tag{5}$$

$$log(PostCites_p + 1) = \beta\widehat{\text{Invalidated}}_p + \gamma X_p + \varepsilon_p \tag{6}$$

where the set of controls X is the same in both stages.

Judge Panels and Patent Invalidation

Table 3 examines the relationship between patent invalidation and the composition of judge panels. We begin in column 1 by using judge fixed effects to capture variation in judge 'bias' (as in Abrams et.al., 2013). Regressing *Invalidity* on these dummies and other controls, we strongly reject equality of judge effects, confirming heterogeneity in the propensity to invalidate.

[24] To construct the simulated votes, we use the following procedure. First, we regress the votes of each judge on observable characteristics of the cases, without including judge fixed effects, and then construct the predicted probability of an invalidity vote for each judge j for patent p, based on these characteristics, ϕ_{jp}, and the regression residuals, e_{jp}. Second, we add to the probability ϕ_{jp} a random draw ω_{jp} from a normal distribution with mean and standard deviation equal to the mean and standard deviation of the distribution of the regression residuals. Finally, the simulated invalidity vote for judge j for patent p is set equal to one if the sum of the predicted invalidity and the random draw $(\phi_{jp} + \omega_{jp})$ is above one. We obtain very similar results using different thresholds.

The judge fixed effects explain about 6.5 percent of the variation in Federal Circuit invalidity decisions.

As indicated earlier, using judge fixed effects in our context neglects the fact that decisions are taken by three-judge panels. To take this into account, in columns 2 to 4 we report probit regression models of the invalidity dummy against the *JIP* index. The estimated marginal effect in column 2 indicates that a one standard deviation increase in JIP is associated with an increase of about 7 percentage points in the likelihood of invalidation. The results are similar when we add a set of controls for patent characteristics (column 3) – a one standard deviation change in *JIP* is associated with an increase of about 5 percentage points in the probability of invalidation (the implied elasticity is 1.07). We also find that the patents that are more heavily cited before the court decision are less likely to be invalidated. Interestingly, there are no significant differences across technology fields in the likelihood of invalidation (joint test has a p-value=0.17).

In column 4 we use an alternative measure of invalidation – the fraction of invalidated claims. Here too we find a positive and statistically significant association between the degree of patent invalidation and the *JIP* index, with a one standard deviation increase in *JIP* being associated with an increase in the fraction of invalidated claims of about 3 percentage points. Not surprisingly, the correlation with *JIP* is weaker in this regression, given the more demanding empirical specification.

Finally, in column 5 we present the result of an OLS regression with *JIP* as dependent variable that provides support to the randomization of judges to cases. The number of claims of the litigated patent, the pre-Federal Circuit cites, the age of the patent and its technology class all appear uncorrelated to the panel propensity to invalidate patents. Only the year effects appear significantly correlated with *JIP*. The significance of the year effects arises mechanically because some of the 'biased' judges are active only for a fraction of our sample period.

We perform a variety of tests to confirm robustness of these findings (results not reported, for brevity). First, there is the concern that the invalidity decision may depend on whether patents have been invalidated by lower courts. To address this issue, we controlled for the lower court decision and find a positive correlation between appeal and district court decisions. However, introducing this additional covariate has essentially no effect on the magnitude and statistical significance of the *JIP* coefficient. Second, invalidity decisions may also depend on characteristics of sub-technology fields not captured by our six broad technology field dummies. We re-estimate the probit regression controlling for more detailed technology field classifications using the 32 NBER technology sub-categories. The magnitude of the estimated *JIP* coefficient remains similar (1.262, p-value <0.01). In addition, we re-run the probit regression in column 3 separately for each of our six different technology fields. The magnitude and the statistical significance of the coefficients are very similar to the pooled data, indicating that the correlation between *JIP* and invalidity is comparable across technology classes. Finally, we obtained similar marginal effects using logit and linear probability models, and confirmed statistical significance using standard errors clustered at the patent or case level.

Patent Invalidation and Cumulative Innovation

Baseline Specification

In Table 4 we examine how patent invalidation affects the number of subsequent citations to the focal patent. We begin in column 1 by presenting the OLS estimate of the baseline specification relating external citations in a five year window after the court decision on the invalidity dummy and additional controls. There is no significant correlation between patent invalidation and future citations. This result is not causal, however. As we argued above, there is a number of reasons why we should expect unobservable factors to affect both the invalidity decision of the Federal Circuit and subsequent citations. This intuition is confirmed by a Rivers-Vuong test that provides strong evidence against the exogeneity of invalidation.[25]

To address the endogeneity concern, in column 2 we move to a IV specification and instrument the *Invalidated* dummy with *JIP*. The estimate shows a statistically significant, positive effect between citations and invalidation by the Federal Circuit. The substantial difference between OLS and IV estimates highlights the importance of controlling for the endogeneity of invalidation, and indicates a strong negative correlation between *Invalidated* and the disturbance in the citation equation, ε_p (inducing a large downward bias if we treat Federal Circuit

[25] Following Rivers and Vuong (1998), we regress *Invalidated* on *JIP* and the other controls in a linear probability model. We construct the residuals ($\hat{v}$) for this model and then regress subsequent citations on *Invalidated*, $\hat{v}$ and the other controls. The coefficient on $\hat{v}$ is negative and highly significant (point estimate of -1.23, p-value<0.01).

invalidation as exogenous).

In column 3 we instrument the invalidated dummy with the predicted probability of invalidation obtained from the probit regression (rather than *JIP* itself) reported in column 3 of Table 3. This is more efficient as the endogenous regressor here is binary (Wooldridge, 2002), and as expected the first stage F-statistic increases from 17.4 to 94.8 when we replace JIP with the predicted probability from the probit. The estimated coefficient implies that patent invalidation (induced by being randomly allocated to a panel of judges with high propensity to invalidate) causes an increase in external citations of about 50 percent in the five years following Federal Circuit decision.[26]

In column 4 we examine the relationship between citations and the *fraction of claims* invalidated by the Federal Circuit. Because the endogenous regressor is a fraction, we cannot use the predicted probability of invalidation as an instrument, so we use *JIP* as the instrument. Not surprisingly, the first stage F-statistics is weaker in this specification, but we still find a positive effect of invalidation on subsequent citations received. The estimated coefficient implies that a one standard deviation increase in the fraction of invalidated claims increases citations by 77 percent in the five year window after the court decision.

These instrumental variable regressions provide strong, causal evidence that the loss of patent rights increases subsequent citations to the patent. This evidence shows that, *at least on average*, patents block cumulative innovation. However, in the following sections we will show that this average effect is misleading because it hides the fact that the 'blocking effect' of patent rights is highly heterogenous. Moreover, we will reveal how the impact of patents varies with the characteristics of the patent, the patentee and the technology field.

Robustness and Extensions

In this section we describe a series of robustness checks on our main finding and two extensions of the empirical analysis.

First, up to now we have treated an invalidation judgement as the final verdict. However,

[26]Because the specifrication relates log of cites to the dummy variable *Invalidated*, we compute the marginal effect as $e^{0.41} - 1 = 0.50$. This follows because in the semilogarithmic model $\ln Y = \beta D$ where D is a dummy variable $(Y_1 - Y_0)/Y_0 = e^{\beta} - 1$ where Y_1 and Y_0 are the values of the dependent variable when D is equal to one and zero respectively.

parties to the dispute have the right to appeal the decision of the Federal Circuit to the Supreme Court (which retains discretion over whether to hear the case). This means that invalidation of a patent by the Federal Circuit retains some uncertainty, so that downstream innovators whom the patent blocked might not respond until this uncertainty is removed. In our context, this is equivalent to saying that our key variable, *Invalidation*, contains some measurement error. In theory, any such error should be taken care of by our instrumental variable estimation. Nonetheless, as a further check we identified that the patent invalidity cases appealed to the Supreme Court in our data set.[27] In column 1 of Table 5 we drop these cases and re-estimate the model (by IV). Our point estimate of the coefficient on patent invalidation is 0.394 (standard error of 0.197), which is very close to the baseline coefficient of 0.410.

Second, the baseline model incorporates fixed effects for six broad (one-digit) technology fields. In column 2 of Table 5 we present results from a specification which uses a more refined technology classification – 32 two-digit subcategories from the NBER. The point estimate of the coefficient on *Invalidation* is nearly double the baseline estimate but less precise, 0.915 (standard error of 0.422), and we cannot reject the null hypothesis that the two estimated coefficients are the same (p-value=0.11).[28]

Third, the baseline specification incorporates a full set of patent age fixed effects. However, the age distribution of citations may vary across technology fields (for evidence, see Jaffe and Trajtenberg, 2002). To allow for this, we extend the specification by including a full set of interactions between the technology field and age dummies. The estimated coefficient on *Invalidation* is 0.401 (standard error of 0.192), which is nearly identical to the baseline coefficient.

The last robustness check involves how to treat patents that receive no citations before the Federal Court decision (4 percent of the sample) and those that receive no cites in the five year widow after the decision (23 percent of the sample). In our baseline specification we 'fix'

[27] Golden (2009) documents that only 23 Federal Circuit decisions were reviewed by the Supreme Court in the period 1982-2008, Only 12 of these cases are in our dataset (the others involve issues other than patent validity).

[28] We retain the one-digit technology field dummies in the later empirical analysis (Section 6), where we investigate heterogeneity in the effect of patents on cumulative innovation. We do this because that analysis involves using smaller subsamples split along various dimensions. As a robustness check, we re-estimate all of those regressions using the more detailed, two-digit technology field dummies and obtain qualitatively (and in most cases, quantitatively) similar results, but the estimates are less precise.

this problem by using log(PostCites+1), which is common practice but may introduce bias. We re-estimate the baseline model adding dummy variables for patents that received no cites before the Federal Circuit decision and for patents that receive no cites after the decision. The results are robust – the point estimate on *Invalidation* is 0.449 (standard error of 0.167). We get similar results if we drop these patents from the sample entirely, as well as other approaches.[29]

We turn now to two extensions that have independent interest. In the first, we examine whether Federal Circuit invalidation has a smaller effect on older patents. Consider the extreme case where invalidation occurs after the patent has expired (there are such cases, as Figure 1 shows). Because the patent no longer has the power to block follow-on development, the invalidation decision should have no effect. More generally, for patents near statutory expiration we would expect to see less blocking effect, both because follow-on research is likely to have dissipated over time for old technologies and because the five year window after the invalidation decision will include years after expiration. Because of sample size we cannot estimate the invalidation effect separately for each patent age. As an alternative, we examine how the estimated effect changes as we successively drop older patents. Column 1 of Table 6 shows that the effect of invalidation is slightly larger when we drop the 44 observations where patents are litigated after expiration (age 20). Columns 2 and 3 show that the effect continues to rise as we drop patents older than 18 and 15, respectively. Compared to our baseline estimate, the effect of invalidation is 28 percentage points larger for patents that are invalidated during their first 15 years of life. Finally, in column 4 we show that there is no effect of invalidation for patents whose Federal Circuit decision takes place more than 15 years after the filing date. We view these results as a kind of placebo test, providing additional support for the hypothesis that the invalidation effect is not being driven by other unobservable factors.

Thus far we have focused on the average effect of invalidation. We also investigated the *time path* of the effect of invalidation on subsequent citations. Figure 3 plots IV estimates of the effect of invalidation in each of the ten years that follow invalidation, and the associated 90-

[29] We get similar results if we use the number of citations without logarithmic transformation as the dependent variable. Finally, we also estimated a Poisson count model by instrumental variables (using the predicted probability of invalidation $\widehat{P}$ as the instrument). The point estimate is 0.638 (standard error of 0.321) which is larger than, but not statistically diffferent from, the baseline coefficient. In the analysis that follows, we do not use the Poisson model because the econometric techniques that we will use to estimate the hetherogenous effects of patent invalidation have only been developed for linear models.

percent confidence intervals. The results show that there is no significant effect in the first two years after Federal Circuit invalidation. Moreover, the effects disappear seven years after the invalidation.[30] This finding suggests that the observed impact of invalidation is not simply due to a 'media effect' from press coverage associated with the court decision, since one would expect such an effect to generate a more immediate increase in citations, and probably to dissipate over time, which is not what we find. The estimated time path is more compatible with a story of entry of new innovators, previously blocked, developing technology building on the focal patent. In later sections we provide more detailed analysis of where the blockage occurs, specifically, which technology fields and which types of patentees and downstream innovators.

In order to be confident that our results can be interpreted as patent rights blocking downstream innovation, we need to rule out the publicity effect interpretation more convincingly. Our instrumental variable estimation partially addresses this concern, since press coverage is unlikely to be disproportionately greater for patents that have been (randomly) allocated to judges with high propensity to invalidate. Nonetheless, to provide further evidence, we collected data on news coverage for the cases in our sample. Our main source is the Dow Jones Factiva dataset, which collects press releases in the major international news and business publications (e.g. Bloomberg, CNN, New York Times, Wall Street Journal). We classify an article as relevant press coverage if it contains at least one of the names of the litigating parties as well as all the following words: 'patent', 'litigation', 'court' and 'appeal'. We construct a measure, *MediaMentions*, defined as the number of articles referring to the case in a one-year window centered around the date of the Federal Circuit decision (i.e., six months before and after the decision date).[31] On average, our patent cases have 1.4 media mentions in the one-year window. The variation in media coverage is very large – about 68 percent of cases have no press coverage and, among those with coverage, the mean number of articles is 4.6 (standard deviation=4.7).

When we add *MediaMentions* to our baseline specification, and estimate using our in-

[30] The above estimates are obtained focusing on the 1982-2003 decisions so that for every patent in the sample we have at least seven years of post-decision observations. We ran a variety of robustness checks and found that the qualitative pattern reported in Figure 3 is robust across different samples and specifications. In particular, if we change the sample size by including more recent years or dropping decisions after 2001, we still observe that the statistically significant effects are concentrated in the third to sixth year following invalidation.

[31] The empirical results are similar if we use measures based on two year or six month windows.

strumental variation approach, we find no significant effect of the variable on the estimated coefficient on *Invalidated* (column 3 in Table 5). One possible explanation is that the effect of media coverage may be highly non-linear, where only very intense media coverage affects subsequent citations. To explore this idea, we generated a dummy variable *HighPress* equal to one for cases in the top two percent of the *MediaMention* distribution. We find that the media effect is indeed concentrated on appeal cases that receive strong media coverage but our key coefficient on *Invalidated* is robust. Column 4 in Table 5 shows that being in the top two percent of media coverage is associated with a 62 percent increase in citations.[32] This finding supports the idea that publicity about a technology shapes its diffusion and follow-on innovation, an issue that is central to the literature on managerial cognition (Kaplan and Tripsas, 2008). Of course, media coverage is endogenous, so we cannot claim that this media effect is causal. An examination of exogenous changes in media coverage on follow-on technology remains an interesting topic for future research.

5 Heterogeneous Impacts of Patent Invalidation

Estimating the Marginal Treatment Effect

To this point we have assumed that the effect of patent invalidation on future citations is constant across patents. However, as the theoretical discussion in Section 2 indicated, the impact of patents on sequential innovation depends on the effectiveness of bargaining, the fragmentation of patent rights, and the risks of coordination failure among downstream developers. Thus we would expect the impact to vary with characteristics of the technology, patentee and market structure. In this section we extend the econometric model to explore this heterogeneity.

We assume that the effect of patent invalidation on future citations can be decomposed into a common component $\overline{\beta}$ and a random component ψ_p: $\beta_p = \overline{\beta} + \psi_p$. We also assume that the probability of invalidity can be described as

$$Invalidated(JIP_p, X_p) = \begin{cases} 1 & \text{if } P(JIP_p, X_p) \geq v_p \\ 0 & \text{otherwise} \end{cases}$$

[32] We experimented with a variety of percentile cutoffs to define *HighPress.* The publicity effect is present only at very high level of coverage (above 3 percent). However, we find no evidence that the effect of invalidation if different for patents that receive greater press coverage. This provides additional evidence against the concern that media mention may confound the effect of exogenous removal of patent rights estimated in our baseline specification.

where v_p is a characteristic of the patent case that is unobservable to the econometrician and which affects the invalidity decision. In general, we would expect this unobservable characteristic to be correlated (positively or negatively) with ψ_p. For example, if the patent is of higher quality (high v_p), invalidation would be less likely and the patent would be more likely to be cited after invalidation (high ψ_p). This example would imply that $E(\overline{\beta} + \psi_p | v_p)$ is increasing in v_p.

Because v_p is not observed, we cannot condition on it. Nonetheless, for a patent case decided by a panel of judges that is just indifferent between invalidating and not invalidating, it must be that $P(JIP_p, X_p) = v_p$. Exploiting this equality, we can identify the marginal treatment effect as $E(\overline{\beta} + \psi_p | P(JIP_p, X_p))$, which corresponds to the (heterogenous) effect of invalidation on future citations for patents that are invalidated because of the instrument. Heckman and Vytlacil (1999) provide a formal treatment, where they show that

$$E(\overline{\beta} + \psi_p | P = v_p) = \frac{\partial E(log(PostCites_p + 1)|P)}{\partial P}|_{P=v_p} \tag{7}$$

and establish identification of the marginal treatment effect (MTE).

In Figure 4 we present estimates of the MTE. The horizontal axis depicts the estimated probability that the patent is invalidated. The vertical axis shows the effect of invalidation on post decision citations for different values of this probability. The support for the estimated probability goes from the 10^{th} to the 90^{th} percentile. The estimated marginal treatment effect is increasing in the probability P. Patents with low values of P are those that, given observables, are unlikely to be invalidated. The small and insignificant values for the MTE in this range show that, if an increase in judge propensity to invalidate lead to invalidation of the patent, the effect of invalidation on citations would be negligible. Conversely, patents with high P are patents with high risk of invalidation. For these patents the MTE is positive, indicating that citations increase after invalidation.[33]

The estimated MTE shows substantial heterogeneity in the effect of patent protection on cumulative innovation. The finding of an increasing MTE also helps identify mechanisms

[33] These findings are robust to using alternative estimation methods to compute the MTE. Figure 3 plots the MTE computed with a nonparametric approach (the multistep procedure developed by Heckman, Ichimura, Smith and Todd, 1998). We obtain a similar figure using the semiparametric approach (with a third order polynomial) proposed by Carneiro, Heckman, and Vytlacil, 2010).

that drive the increase in citations that we observe after Federal Circuit invalidation. This is because the MTE estimates the effect of invalidation for patent cases in which judges are indifferent between a validity and an invalidity ruling. Thus, an increasing MTE indicates that the effect of invalidation on citations is greater for patents which, despite having observable features that make invalidation likely (high $P(JIP_p, X_p)$), are characterized by *unobservable factors* that make invalidation less likely (large v_p).

We want to stress two unobservable factors that are likely to play an important role. First, there may be characteristics that affect the strength of the patent (legal enforceability) and thus make invalidation less likely, and which are observable to the patentee but *unobservable to the licensees* (and well as the econometrician). This asymmetric information can lead to bargaining failure in licensing negotiations. In such cases, Federal Circuit invalidation can facilitate access to the technology that was blocked by the bargaining failure.

A second characteristic that is unobservable to the econometrician, and possibly to the potential licensee, is the comparative advantage of the patent owner to avoid invalidation of the patent. These advantages are typically associated with the size of the patentee (Lanjouw and Schankerman, 2004). In this context, an increasing MTE suggests that Federal Circuit invalidation will have a greater impact on subsequent innovation when it involves patents held by large firms. We will investigate the role of patentee size in detail in Section 6.

Explaining the Heterogeneity

We showed that the effect of invaliding patent rights on subsequent citations is heterogeneous, and that the impact is larger for patents at greater risk of being invalidated. In this section we unbundle the marginal treatment effect and relate it to observable characteristics of the technology field.

We expect the impact of patents on cumulative innovation to be strongly influenced by two main features of the innovation environment. The first is the concentration of the technology field. When patent ownership is not concentrated (i.e., fragmented), users are likely to engage in multiple negotiations and this will exacerbate the risk of bargaining breakdown and hold-up. For this reason, we expect patents to have a smaller impact on cumulative innovation in concentrated technology fields. The second feature is the 'complexity' of the technology field. In complex fields new products tend to rely on numerous patentable elements, as contrasted with 'discrete' technology areas where products build only on few patents. When products typically rely on, or incorporate, many patented inputs, licensees engage in multiple negotiations and the risk of bargaining failure is again larger. Thus we expect the impact of patent rights on cumulative innovation to be more pronounced in complex technology fields.

To test these hypotheses, we construct two variables. The first variable, $Conc4$, is a concentration measure equal to the patenting share of the four largest assignees in the technology subcategory of the litigated patent during the five years preceding the Federal Circuit decision (the mean and standard deviation of $Conc4$ are 0.067 and 0.053, respectively). The second variable, $Complex$, is a dummy variable for patents in complex technology fields. Building on the findings in Levin et. al. (1987) and Cohen et. al. (2000), we classify as complex the areas of electrical and electronics (NBER category 4), computers and communication (NBER category 2) and medical instruments and biotechnology (NBER subcategories 32 and 33).

In columns 1 and 2 of Table 7 we show, in two split sample regressions, that the effect of patent invalidation is small and statistically insignificant among patents in concentrated technology areas ($Conc4 \geq$ median), whereas it is large and statistically significant among patents in fragmented technology fields ($Conc4 <$ median). Similarly, in columns 3 and 4 of Table 7 we show that the effect of invalidation is more than twice as large in complex technology areas as compared to the non-complex technology fields.

Column 5 provides estimates using the full sample and interacting $Conc4$ and $Complex$ with the $Invalidation$ dummy. These confirm the findings from the split sample regressions. Evaluated at their respective sample means of $Conc4$, our point estimate (standard error) for complex technology fields is 1.149 (0.29); for non-complex fields it is not statistically different from zero, at 0.167 (0.23). For complex fields the estimate implies that patent invalidation raises subsequent citations by 216 percent. We also confirm that concentration substantially mitigates the effect of patent invalidation on future citations: a one standard deviation increase in $Conc4$ reduces the effect of invalidation by 0.37, which is 32 percent of the estimated impact for complex fields.[34]

[34] Column 5 also controls for the direct effect of $Conc4$ and includes additive technology dummies that absorb the direct effect of $Complex$. These results are unchanged if we reclassify biotechnology patents (subcategory

We next use the parameter estimates from column 5 to compute the implied effect of patent invalidation on citations for each of the technology fields (given their values of $Conc4$ and $Complex$). The results in column 1 of Table 8 are striking. There is essentially no effect of patent rights on cumulative innovation in any of the three non-complex technology areas – pharmaceuticals, chemicals and mechanical. By contrast, the effect is large and statistically significant in each of the complex fields – the coefficients imply that invalidation raises citations by 320 percent in medical instruments/biotechnology, 203 percent in electronics and 178 percent in computers. For comparison, column 2 reports estimates of split-sample IV regressions for each technology fields. Though the smaller sample sizes reduce precision, the regressions confirm strong impacts in medical instruments/biotechnology and computers, but no statistically significant effect in electronics. Overall, the similarity between the findings in the two columns indicate that the concentration and complexity of technology fields are key determinants of the relationship between patents and cumulative innovation, as economic theory predicts.

These findings are important for the policy debates on patent reform. They show that the blocking effect of patent rights depends on identifiable characteristics of the technology field, and are not general. The recent literature studies specific innovations in biotechnology and medical instruments and find blocking effects (Murray and Stern, 2007, and Williams, 2013), and our estimates confirm these findings using information on diverse innovations within these fields and an entirely different identification strategy. But our results also show that the effects are very different in other fields, and they suggest that legal and regulatory rules to mitigate blocking effects need to target specific technology areas effectively, in order to minimize any damage to overall innovation incentives. At the same time, our findings imply that large changes in the concentration or complexity of technology fields would reshape the relationship between patent rights and cumulative innovation.[35]

33) as a non-complex field, or if we replace the continuous concentration measure with a dummy variable for fields with *Conc4* above the 50th or 75th percentile.

[35] We use our parameter estimates from column 5 in Table 7 to examine within-field variation over time in the impact of invalidation. To do this we construct the *Conc4* measure for each technology *subcategory* in the years 1982-2002 and compute a weighted average for each of the six broad technology fields, with weights equal to the fraction of patenting in the area. We find no evidence of significant changes in the impact of patent invalidation during our sample period.

6 Intensive versus Extensive Margins

In the previous section we showed that the effect of patents on later innovation depends on how concentrated patent rights are – on the 'industrial organization' of innovation. However, the influence can also run in the other direction. Patent rights can shape the industrial structure of innovation by impeding the entry of new innovators or the expansion of existing firms, and this potential blocking effect may be stronger for certain kinds of patentees or downstream innovators. In this section we examine this issue by studying how the effect of patent invalidation varies with the size of the patentee and characteristics of citing innovators.

We measure the size of the citing innovators by constructing the portfolio size for each assignee citing the patents involved in Federal Circuit litigation. The portfolio is defined as the number of patents granted to an assignee in the five years before the Federal Circuit decision. The mean portfolio size of citing firms is 359 patents but the distribution is very skewed – the median firm has only 5 patents, and the 75th percentile has 102 patents. We assign firms to one of three size categories: 'small' if its portfolio is below five, 'medium' if the portfolio is between 6 and 101 patents, and 'large' if it greater than 102 patents. We study how patent invalidation affects citations by subsequent innovators in each size group. In each regression we also allow for the effect of invalidation to be different when the focal patent is held by a large patentee, defined as one with a patent portfolio of more than 102 patents.

In addition, for each size group we decompose the total number of later citations into intensive and extensive margins. We measure the extensive margin by the number of *distinct patent owners (assignees)* citing the focal (litigated) patent in the five-year following the Federal Circuit decision. We measure the intensive margin by the number of *citations per assignee* to the focal patent in the same time window.

Table 9 presents the IV estimates of the patent invalidation effect on citations by different size groups. Focusing first on the total number of external citations (columns 1-3), the estimates reveal that the blocking effect of invalidation is concentrated *exclusively* on citations that patents of large firms receive from small innovators. The magnitude of the implied blocking effect is very large: invalidation of a large firm patent increases small firm citations by about 520 percent ($= e^{1.84} - 1$). This is consistent with our earlier estimate of 50 percent for the average blocking effect in the overall sample, because roughly 50 percent of the citing entities are small firms in our data and about 20 percent of the patentees are large firms (i.e., $520 \times 0.5 \times 0.2 = 52$ percent). The coefficients for the other size groups are much smaller in magnitude and statistically insignificant.[36]

In columns 4-6, we study how patent invalidation affects the extensive margin. The dependent variable in these regressions is the logarithm of one plus the number of distinct assignees citing the litigated patent in the five years following the Federal Circuit decision. Here too we find that the blocking effect of patents is concentrated among citations by small firms to large firm patents. The estimated coefficient of 1.347 implies a 285 percent increase in the number of distinct small assignees citing the patent when a patent of a large firm is invalidated by the Federal Circuit. The effects for the other size groups again are small and statistically insignificant.

Finally, columns 7-9 examine the blocking effect at the intensive margin, the number of citations per distinct patent owner. The only coefficient (marginally) significant is again the one related to large patentees and small citing assignees. The effect of invalidation is about 62 percent, but statistically significant only at the 10 percent level. Overall, we cannot reject the hypothesis that the extensive margin effect for small citing firms is equal to the total effect and that the intensive margin effect is zero.

We conduct extensive robustness checks on the regressions in Table 9. First, we vary the thresholds for defining 'small' firms ($\leq$ 1, 10, 15, 20, 25, 30 and 40 patents), and for defining large firms ($\geq$ 75, 85, 95, 110 and 150 patents). We report the estimates for some of these regressions in Appendix Table A2. Second, we re-estimate the invalidation effects by splitting the samples between large and non-large patentees. We also break down the category of non-large patentees into two groups, small and medium sized firms. In all of these experiments, the

[36] Because of sample size, we do not allow the effect of invalidation to vary with technology field in these regressions (we do allow for an additive field effects, however). If citations from small citers to large patentees are overrepresented in fragmented and complex technology fields, where we found blockage was more likely, our finding that blocking effect of invalidation is limited to the large patentee-small citing firm category could be due to a technology field composition effect. To check this concern, we examined the percent of citations in each technology field accounted for by citations by small to large patentees. The technology fields where invalidation has a statistically significant blocking effect (medical instruments, electronics and computers) are not those with the largest fraction of citations from small to large patentees – the mean fraction of sample citations from small to large patentees is 7.4 percent in these fields, as compared to 9.9 percent in the other fields. Our finding thus does not appear to be due to a technology field composition effect.

pattern that emerges in Table 9 is extremely robust. In every case the effect of invalidation is concentrated on the subsequent citations by small innovators to focal patents held by large firms, and it is predominantly an extensive margin effect.

These findings show that patent rights block later innovation in very specific ways, not uniformly. The fact that we see no statistically significant blocking effect for most size categories suggests that bargaining failure among upstream and downstream innovators is not widespread. However, the results show that bargaining breakdown is more common when it involves large patentees and small downstream innovators. This is exclusively where the blocking effect of patents in located.[37] Moreover, the fact that the effect is primarily at the extensive margin means that patent rights (held by large firms) impede the 'democratization of innovation'. Patent invalidation leads to the 'entry' of small new innovators.

However, there is a second possible interpretation that needs to be considered – the increase in citations may reflect the propensity of small patentees to "strategically withhold citations" to patents of large firms in order to stay below their radar screen, rather than a real impact on the underlying innovation by small firms.[38] There are several reasons why we think that this strategic behavior is unlikely to play a big role in our setting. First, previous studies show that large firms are more likely to withhold citations strategically (Schneider, 2007; Lampe, 2011), whereas we find that the effect of invalidation is driven by small firm citations. Second, our measure includes citations both by the patent applicant and the USPTO examiner. Thus an increase in citations after invalidation would imply not only strategic behavior by the applicants but also errors by examiners in overlooking relevant prior art. Our estimated impact – a 520 percent increase in citations from small firms – would imply an unreasonably large error rate by patent examiners, especially given that our sample contains well-known 'superstar' patents. Finally, the strategic citation interpretation is hard to reconcile with a lagged effect of patent invalidation on later citations, which we documented in Section 4.

[37]This finding is consistent with Lanjouw and Schankerman (2004), who show that small firms are less able to resolve disputes 'cooperatively' without resorting to the courts. One reason for this disadvantage is that small firms do not have patent portfolios that can be useful as counter-threats to resolve disputes or to strike cross-licensing agreements to preserve freedom to operate in their R&D activities (Galasso, 2012).

[38]Small firms may even choose their research niches strategically to avoid coming into conflict with larger players. Lerner (1995) presents some evidence from the biotechnology sector that is consistent with this hypothesis.

7 Impact of Invalidation on Self-Citations

In this section we explore how patent invalidation affects *self*-citations. In the economics literature, self-citations have been used to measure the extent to which a firm builds on its own past innovation, i.e., to identify its research trajectory, or 'core competency' (Jaffe and Trajtenberg, 2002). Examining how invalidation affects self-citations thus reveals how patent rights affect the direction of the firm's future research activity.

Column 1 of Table 10 shows that Federal Circuit invalidation has no statistically significant effect on subsequent self-citations to the focal patent. However, this regression conceals an important distinction between core and non-core patents. The management literature emphasizes the importance of developing a set of core technologies, and effectively protecting them, in order to create a sustainable competitive advantage in the market (Prahalad and Hamel, 1990). This creates the base from which the firm can generate related, complementary (peripheral) innovations. If patent protection over a core technology is lost, we would expect a firm to reorient its research direction away from the development of peripheral innovations building on that technology. This implies that invalidation of a core patent would reduce subsequent self-citations to that patent. However, if a peripheral patent is invalidated, the firm has no incentive to shift research trajectory. To the contrary, loss of a peripheral patent may the firm to intensify efforts to build around the core technology.

To investigate this hypothesis, we construct two alternative measures of core patents, both based on the importance of self-citations. Our first measure, CORE1, defines core patents as those for which the number of self-citations received is in the top decile of the firm's portfolio of patents (constructed as all the patents granted to the patentee in a six-year window centered around the grant date of the litigated patent). One limitation of this measure is that it does not consider the propensity of other firms to cite the focal patent. The second measure, *CORE2,* addresses this by defining core patents as those for which the ratio of self-citations to total citations received is in the top decile of the patents in the overall sample.

Columns 2 and 3 in Table 10 present the IV parameter estimates of the effect of invalidation where we allow the impact to differ for core and non-core patents. Using the measure CORE1, we find that invalidation of a core patent generates a 80 percent *reduction* in future self-citations, whereas invalidation of a non-core patent is associated with a 25 percent *increase* in later self-citation. The results in column 3 are similar when we use the alternative CORE2 measure.[39]

These results provide support for the idea that patent rights on core technologies are important for sustaining the research trajectory of firms, and their associated competitive advantages. In this way, patent rights shape the market position of firms and their competitive interaction with other firms. One way to explore these competitive dynamics more fully would be to study how Federal Circuit invalidation of core and non-core patents affects other firms that compete in similar innovation (technology) and/or product markets, building on the recent work of Bloom, Schankerman and Van Reenen (2013).

8 Welfare Implications

We have shown that invalidation of patents by the Federal Circuit raises subsequent citations to the focal patent, on average. This suggests that some licensing deals are not taking place in the presence of patent protection. There are two main reasons why this might occur. First, there can be circumstances where it would be optimal for a patent owner to restrict access if licensing reduces joint profits (for example because it intesifies downstream competition). Second, information asymmetry and uncoordinated, multilateral bargaining can lead to licensing failures even when it would increase joint profits. It is important to distinguish between these explanations because they differ in terms of their implications for welfare and policy (even putting aside the effect on consumer surplus). In this section we briefly discuss how our empirical findings can contribute to a broader welfare assessment.

We find that the impact of patent invalidation on subsequent innovation is very heterogeneous. The positive impacts are concentrated on a small subset of patents which have unobservable characteristics that suggest the presence of asymmetric information that induces bargaining failure in licensing. Our results also help to pin down *where* the bargaining failure occurs. The effect is concentrated in fields characterized by two features: complex technology and high fragmentation of patent rights. This reinforces the market failure interpretation,

[39]We experimented with alternative thresholds to define core patents – from the 90th percentile down to the 75th – and results are similar to those reported in Table 10.

since both economic theory and earlier evidence identify these features as key determinants of licensing breakdowns (Cohen et al., 2000 and Ziedonis, 2004). We find no evidence of blocking in non-complex fields such as chemicals, mechanical, and pharmaceuticals. [40]

Moreover, the blocking effect is entirely driven by invalidation of patents owned by large patentees, and that it is primarily an 'extensive margin' effect generating a surge in the number of small patentees citing the focal patent. Invalidation of large firm patents appears to 'democratize' innovation by small innovators, but this effect is not found for small or medium sized patentees, or for subsequent innovation by medium or large sized firms. Bargaining failure in patent licensing – and thus the blocking effect of patent rights – seems to be concentrated in the relationships between *large patent holders* and *small innovators*. This suggests that policies and institutions that facilitate coordination and mitigate asymmetric information (as the biological resource centers studied by Furman and Stern, 2011) can promote more efficient licensing and thereby increase R&D incentives. Given the importance of innovation to economic growth, we would expect this to enhance social welfare.

Our results may lead one to think that it could be beneficial to scale back patent rights in technology fields where blocking occurs. However, theoretical models of cumulative innovation indicate that such policies have ambiguous effects on overall innovation incentives (e.g., Green and Scotchmer, 1995; O'Donoghue, Scotchmer and Thisse, 1998; Hopenhayn, Llobet and Mitchell, 2006). In models with two generations, weaker patent protection shifts rents toward downstream firms, increasing their incentives but reducing incentives for first generation research. The ambiguity is even more striking in a fully dynamic setting, where each innovation is both upstream and downstream at different stages of its life.

Overall, our results show that the blocking effect of patent rights operates only in very specific environments, and this strongly suggests that policies designed to mitigate this problem should be targeted to where the bargaining failure seems to be located.

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[40] However and alternative (less optimistic) interpreation for why patents do not block is that patentees are unable to enforce their rights effectively. In this case the R&D incentives for upstream innovators will be diluted and welfare implications would be less clear-cut.

9 Conclusion

In this paper we estimate the causal effect of patent rights on cumulative innovation, using patent invalidation decisions of the Federal Circuit. The identifications strategy exploit variation in the propensity of judges to invalidate and the fact that the three-judge panels are generated by a random computer algorithm. There are three key empirical findings. First, we show that invalidation leads to about a 50 percent increase in subsequent citations to the focal patent, on average. Second, the impact of patent invalidation is highly heterogeneous, with large variation across patents and technology fields in ways that are consistent with the predictions of theoretical models that identify bargaining failure in licensing as the source of blockage. Third, the increase in citations is entirely driven by invalidation of patents owned by large patentees and the effect is primarily on the "extensive margin" – i.e., the number of small patentees citing the focal patent. Thus, invalidation of large firm patents appears to 'democratize' innovation by small firms.

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Appendix: Microfunding the JIP Measure

We develop a simple model of strategic voting, closely following Feddersen and Pesendorfer (1996). There are three judges $i \in \{1, 2, 3\}$ who must decide whether a patent is valid (V) or not invalid (N). Judges are uncertain about the validity of the patent and each judge gets a signal v or n that is correlated with the true state. Specifically we assume that

$$\Pr(v|V) = \Pr(n|N) = p_i.$$

The parameter $p_i \in [\underline{p}, \overline{p}]$ with $.5 < \underline{p} < \overline{p} < 1$ is the probability that a judge receives the correct signal. The parameter p_i can be interpreted as the 'complexity' of the case for judge i. The assumption that the signals are private information is standard in the literature on voting. Feddersen and Pesendorfer (1996) provide a number of reasons why the complete disclosure of private information may not occur. For example, some judges may have technical knowledge that is relevant for the case but difficult to communicate. Moreover, differences in preferences for patent validity may reduce the incentives to reveal private information in deliberations.

The judges vote simultaneously either to validate or invalidate and the decision is taken by majority voting. There are two outcomes: either the patent is invalidated (1) or not (0). We assume that each judge maximizes her expected utility and that preferences are given by $u(1, N) = u(0, V) = 0$ and $u(1, V) = -q_i$ and $u(0, N) = -(1 - q_i)$. The parameter q_i characterizes the judge's threshold of reasonable doubt. Let $\beta_i(n)$ denote the posterior probability for judge i that the patent is invalid, conditional on obtaining an invalidity signal and being pivotal, i.e that the other two judges, x and z, receive different signals from each other. Let $\beta_i(v)$ denote the posterior probability for judge i that the patent is invalid, conditional on obtaining a validity signal and being pivotal:

$$\begin{aligned}
\beta_i(n) &= \frac{p_i(1-p_x)p_z}{p_i(1-p_x)p_z + (1-p_i)(1-p_x)p_z} = p_i \\
\beta_i(v) &= \frac{(1-p_i)(1-p_x)p_z}{p_i(1-p)p_z + (1-p_i)(1-p_x)p_z} = 1 - p_i
\end{aligned}$$

Now assume that $\beta_i(v) < q_i < \beta_i(n)$ for each i. Feddersen and Pesendorfer (1996) show that if this assumption is satisfied each judge in equilibrium will vote according to his signal (i.e., what they call 'informative' voting). More specifically, a pivotal judge receiving an invalidity signal will vote for invalidity as long as her expected utility is higher from doing so:

$$\beta_i(n)0 - (1 - \beta_i(n))q_i \geq (1 - \beta_i(n))0 - \beta_i(n)(1 - q_i)$$

which is satisfied because we assumed $q_i < \beta_i(n)$. She will also vote for validity if she receives a validity signal because $\beta_i(v) < q_i$. Moreover, note that $\beta_i(v) = 1 - p_i$ and $\beta_i(n) = p_i$, so the condition for an informative equilibrium is always satisfied as long as $1 - \underline{p} < q_i < \underline{p}$.

Let us assume that, for each case, the complexity of the case, p_i, is an i.i.d. draw from a distribution $F(p)$ with support $[\underline{p}, \overline{p}]$ and that $1 - \underline{p} < q_i < \underline{p}$. The ex-ante probability that judge i will vote for invalidity will be $1 - F(q_i) \equiv f^i$ and the expected number of invalidity votes in the three judge panel will be equal to

$$JIP = f^1 f^2 f^3 + f^1 f^2 (1 - f^3) + f^1 (1 - f^2) f^3 + (1 - f^1) f^2 f^3.$$

Given the random allocation of judges to cases, the sample average of a judge's validity votes will be an unbiased estimator of her probability of voting for validity. Moreover, *JIP* is a consistent estimator of the number of validity votes in the three judge panel (it is not unbiased as it is a nonlinear transformation of the f^i's).

Table 1. Summary Statistics

	Mean	Std. Dev.	Min	Max
Invaliditated	0.39	0.49	0	1
Fraction of invalidated claims (conditional on invalidity)	0.72	0.37	0.01	1
PostCites	8.70	19.61	0	409
PostSelfCites	0.63	4.02	0	83
PreCites	21.88	45.99	0	789
PreSelfCites	1.90	6.02	0	109
Claims	17.48	20.47	1	244
Patent Age	9.91	5.15	1	30

NOTES: Sample of 1357 Federal Circuit patent invalidity decisions for period 1983-2008. Invalidated=1 if Federal Circuit invalidates at least one claim of focal patent. PostCites = cites from patents of other assignees in 5 year window after Federal Circuit decision. PostSelfCites = cites from patents owned by same patentee of focal patent in 5 year window after Federal Circuit decision. PreCites = cites from patents of other assignees received before Federal Circuit decision. PreSelfCites = cites received from patents owned by same patentee of focal patent before Federal Circuit decision. Claims = total number of claims listed in focal patent. Patent age = age in years from filing date of patent at Federal Circuit decision.

Table 2. Comparison of Federal Circuit and other Patents

	All Granted Patents not litigated	Litigated at Lower Courts and Not Appealed	Litigated at Lower Courts and Appealed
Number of patents	1,808,770	7,216	877
Technology Field Composition (%)			
Drugs and Medical	9.2	12.1	25.7
Chemicals	19.2	11.9	12.7
Computers and Communication	12.5	11.9	12.4
Electronics	17.5	11.6	9.8
Mechanicals	21.3	20.1	15.6
Others	20.4	32.5	23.8
Patent Characteristics			
Cites received per claim	1.0	1.9	2.3
Number of claims	12.5	17.1	19.0
Generality	0.45	0.49	0.49
Originality	0.36	0.39	0.40

NOTES: Sample includes patents granted in period 1980-1999. Cites= total citations received up to 2002. Generality and Originality are defined in Hall et al. (2001). Lower court litigation data are from Lanjouw and Schankerman (2001).

Table 3. Composition of Judge Panels and Patent Invalidation

	1	2	3	4	5
Estimation Method	Probit	Probit	Probit	OLS	OLS
Dependent Variable	Invalidated	Invalidated	Invalidated	Fraction of Invalidated Claims	JIP
Judges dummies	YES***				
Judges Invalidity Propensity (JIP)		3.464***	3.313***	0.588***	
		(0.647)	(0.743)	(0.225)	
log(Claims)	0.034		0.041	-0.018	-0.001
	(0.039)		(0.039)	(0.012)	(0.001)
log(PreCites)	-0.134***		-0.137***	-0.045***	0.001
	(0.041)		(0.040)	(0.012)	(0.002)
log(PreSelfCites)	0.008		0.002	-0.018	0.002
	(0.0047)		(0.045)	(0.012)	(0.002)
Year Effects	YES***	NO	YES***	YES***	YES***
Age Effects	YES	NO	YES	YES	YES
Tech. Effects	YES	NO	YES	YES	YES
Observations	1357	1357	1357	1357	1357

NOTES: * significant at 10 percent, ** significant at 5 percent and *** significant at 1 percent. Robust standard errors are reported in parentheses. Invalidated=1 if Federal Circuit invalidates at least one claim of focal patent. PreCites = cites from patents of other assignees received before Federal Circuit decision. PreSelfCites = cites received from patents owned by same patentee of focal patent before Federal Circuit decision. Claims = total number of claims listed in focal patent. Age = age in years from filing date of patent at Federal Circuit decision. Year= year of Federal Circuit Decision. Technology fields= 6 categories defined in Hall et al (2001). JIP= propensity to vote for patent invalidity of judge panel constructed from invalidity votes of judges in other sample cases.

Table 4. Impact of Invalidation on Citations

	1	2	3	4
Estimation Method	OLS	IV	IV	IV
Dependent Variable	log(PostCites)	log(PostCites)	log(PostCites)	log(PostCites)
Invalidated	-0.053	1.158**	0.410**	
	(0.046)	(0.489)	(0.196)	
Fraction of Invalidated Claims				2.104*
				(1.118)
log (Claims)	-0.001	-0.018	-0.007	0.037
	(0.025)	(0.030)	(0.025)	(0.041)
log(PreCites)	0.538***	0.598***	0.558***	0.637***
	(0.028)	(0.040)	(0.029)	(0.064)
log(PreSelfCites)	0.085**	0.084**	0.085**	0.126**
	(0.030)	(0.034)	(0.030)	(0.044)
Year Effects	YES***	YES***	YES***	YES***
Age Effects	YES***	YES***	YES***	YES***
Tech. Effects	YES	YES	YES	YES
Instrument		JIP	predicted probability from probit	JIP
IV Test		F=17.43 (p<0.01)	F=94.85 (p<0.01)	F=6.83 (p=0.01)
Observations	1357	1357	1357	1357

NOTES: * significant at 10 percent, ** significant at 5 percent and *** significant at 1 percent. Robust standard errors are reported in parentheses. PostCites = cites from patents of other assignees in 5 year window after Federal Circuit decision. Invalidated=1 if Federal Circuit invalidates at least one claim of focal patent. PreCites = cites from patents of other assignees received before Federal Circuit decision. PreSelfCites = cites received from patents owned by same patentee of focal patent before Federal Circuit decision. Claims = total number of claims listed in focal patent. Age = age in years from filing date of patent at Federal Circuit decision. Year= year of Federal Circuit Decision. Technology fields= 6 categories defined in Hall et al (2001). JIP= propensity to vote for patent invalidity of judge panel constructed from invalidity votes of judges in other sample cases. IV test is Stock and Yogo (2005) weak ID test.

Table 5. Impact of Invalidation on Citations - Robustness

	1	**2**	**3**	**4**
Estimation Method	IV	IV	IV	IV
Dependent Variable	log(PostCites)	log(PostCites)	log(PostCites)	log(PostCites)
Invalidated	0.394**	0.915**	0.404**	0.418**
	(0.197)	(0.422)	(0.196)	(0.197)
MediaMention			0.007	
			(0.008)	
HighPress dummy				0.484***
				(0.159)
Refined (2-digit) Tech dummies		YES***		
Sample	Drop Supreme Court Appeals	Full	Full	Full
Observations	1345	1357	1357	1357

NOTES: * significant at 10 percent, ** significant at 5 percent and *** significant at 1 percent. Robust standard errors are reported in parentheses. All regressions control for log(PreCites), log(PreSelfCites), log(Claims), age and year effects. PostCites = cites from patents of other assignees in 5 year window after Federal Circuit decision. Invalidated=1 if Federal Circuit invalidates at least one claim of focal patent. Technology field effects use 6 categories in columns 1,3,4 and 32 subcategories in column 2 (for details see Hall et al. 2001). MediaMention is equal to the number of FACTIVA articles referring to case during one year window centered on the decision date. HighPress dummy=1 if MediaMention in the top 2 percent. Invalidated is instrumented by the Probit estimates of the probability of invalidation.

Table 6. Impact of Invalidation and Patent Age

	1	2	3	4
Sample	Age <=20	Age<=18	Age<=15	Age>15
Estimation Method	IV	IV	IV	IV
Dependent Variable	log(PostCites)	log(PostCites)	log(PostCites)	log(PostCites)
Invalidated	0.412**	0.457**	0.577**	0.055
	(0.203)	(0.216)	(0.239)	(0.272)
Observations	1313	1245	1098	259

NOTES: * significant at 10 percent, ** significant at 5 percent and *** significant at 1 percent. Robust standard errors are reported in parentheses. All regressions control for log(PreCites), log(PreSelfCites), log(Claims), age, technology and year effects. PostCites = cites from patents of other assignees in 5 year window after Federal Circuit decision. Invalidated=1 if Federal Circuit invalidates at least one claim of focal patent. Invalidated is instrumented by the Probit estimates of the probability of invalidation

Table 7. Effect of Complexity and Concentration

	1	2	3	4	5
Sample	Conc4 >= Median	Conc4 < Median	Complex Technologies	Non Complex Technologies	Full
Estimation	IV	IV	IV	IV	IV
Dependent Variable	log(PostCites)	log(PostCites)	log(PostCites)	log(PostCites)	log(PostCites)
Invalidated	0.086	0.985***	0.739**	0.317*	0.557**
	(0.331)	(0.288)	(0.322)	(0.183)	(0.263)
Invalidated x Conc4					-6.977***
					(2.457)
Invalidated x Complex					1.234***
					(0.327)
Observations	678	677	437	920	1357

NOTES: * significant at 10 percent, ** significant at 5 percent and *** significant at 1 percent. Robust standard errors are reported in parentheses. All regressions control for log(PreCites), log(PreSelfCites), log(Claims), age and year effects. PostCites = cites from patents of other assignees in 5 year window after Federal Circuit decision. Invalidated=1 if Federal Circuit invalidates at least one claim of focal patent. Columns 1, 2, and 5 controls for technology class effects. Column 5 also controls for the direct effect of Conc4. Complex=1 if patent is in Computer and Communication (NBER Category 2), Electrical and Electronics (NBER Category 4), Medical Instruments (NBER subcategory 32), and Biotechnology (NBER subcategory 33). Conc4 is the patenting share of the four largest assignees in the technology subcategory of the litigated patent during the five years preceding the Federal Circuit decision. Invalidated and its interactions are instrumented by the Probit estimates of the probability of invalidation and its interactions.

Table 8. Technology Differences in Invalidation Effect

Technology	Based on Complex and Conc4 IV Estimates	Split Sample IV Regressions
Chemical	-0.028	-0.710
	(0.242)	(0.725)
Mechanical	0.173	-0.225
	(0.230)	(0.519)
Drugs	0.229	0.231
	(0.230)	(0.449)
Computers and Communications	1.024***	2.388**
	(0.285)	(1.224)
Electrical and Electronics	1.107***	-2.744
	(0.285)	(2.339)
Medical Instruments and Biotechnology	1.435***	2.402***
	(0.313)	(0.848)

NOTES: * significant at 10 percent, ** significant at 5 percent and *** significant at 1 percent. Robust standard errors are reported in parentheses. Estimates in column 1 obtained from using column 5 of Table 7 and sample means of Conc4 across various technology areas. Each regression in column 2 controls for log(PreCites), log(PreSelfCites), log(Claims), age and year effects. PostCites = cites from patents of other assignees in 5 year window after Federal Circuit decision. Invalidated=1 if Federal Circuit invalidates at least one claim of focal patent. Invalidated instrumented by the Probit estimates of the probability of invalidation.

Table 9. Intensive and Extensive Margins (IV Estimates)

	Total Effect (PostCites Received)			**Extensive Margin (Number of distinct Assignees)**			**Intensive Margin (PostCites per Assignee)**		
	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
	Citing Patents in Small Portfolios	Citing Patents in Medium Portfolios	Citing Patents in Large Portfolios	Citing Patents in Small Portfolios	Citing Patents in Medium Portfolios	Citing Patents in Large Portfolios	Citing Patents in Small Portfolios	Citing Patents in Medium Portfolios	Citing Patents in Large Portfolios
Invalidated	0.075	0.190	0.228	0.036	0.003	0.123	0.025	0.171	0.088
	(0.183)	(0.168)	(0.158)	(0.155)	(0.105)	(0.104)	(0.053)	(0.105)	(0.079)
Invalidated X Large Patentee	1.840**	0.826	0.689	1.347**	0.418	0.041	0.479*	0.362	0.659
	(0.726)	(0.663)	(0.837)	(0.556)	(0.376)	(0.446)	(0.261)	(0.393)	(0.535)

NOTES: * significant at 10 percent, ** significant at 5 percent and *** significant at 1 percent. Robust standard errors are reported in parentheses. All regressions control for log(PreCites) in the size group, log(PreSelfCites), log(Claims), age and year effects. PostCites = cites from patents of other assignees in 5 year window after Federal Circuit decision. Invalidated=1 if Federal Circuit invalidates at least one claim of focal patent. Invalidated and its interactions are instrumented by the Probit estimates of the probability of invalidation and its interactions. Large Patentee=1 if patentee has more than 102 patents. A citing firm is classified as small if its portfolio has less than 5 patents, as medium if the portfolio has between 5 and 102 patents and as large if it has more than 102 patents. Dependent variables: in columns 1-3 are the total external cites received by the patent from citing firms in the size group, in columns 4-6 are the total number of citing firms in the size group and columns 7-9 are the external cites per assignee in the size group.

Table 10. Impact of Invalidation on Self Citations

	1	2	3
	Total Effect	**CORE1**	**CORE2**
Estimation	IV	IV	IV
Dependent Variable	log(PostSelfCites)	log(PostSelfCites)	log(PostSelfCites)
Invalidated	0.078	0.221**	0.188**
	(0.051)	(0.095)	(0.087)
Invalidated X CORE		-0.594**	-0.832***
		(0.255)	(0.303)
CORE		-0.039	0.521***
		(0.143)	(0.155)

NOTES: * significant at 10 percent, ** significant at 5 percent and *** significant at 1 percent. Robust standard errors are reported in parentheses. All regressions control for log(PreCites), log(PreSelfCites), log(Claims), age and year effects. PostSelfCites = cites from patents owned by same patentee of focal patent in 5 year window after Federal Circuit decision. Invalidated=1 if Federal Circuit invalidates at least one claim of focal patent. Invalidated and its interactions are instrumented by the Probit estimates of the probability of invalidation and its interactions. CORE1=1 if patent ranks above 90th percentile for SelfCites received among patents in portfolio of patentee. CORE2=1 if SelfCitations received before invalidation / Total Citations received before invalidation is above 90th percentile in the sample.

Figure 1. Age Distribution of Litigated Patents

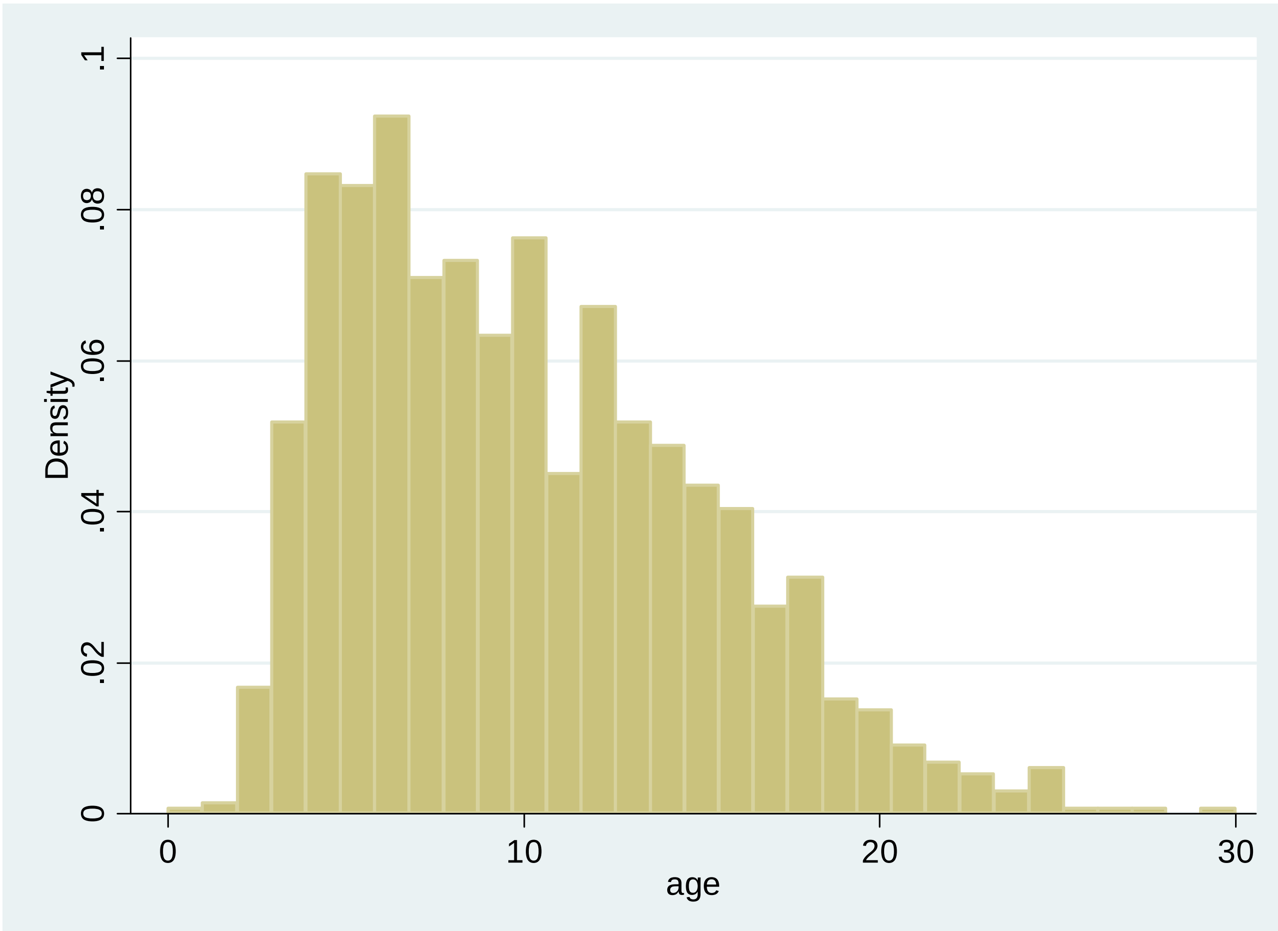


Figure 2. Distribution of JIP index

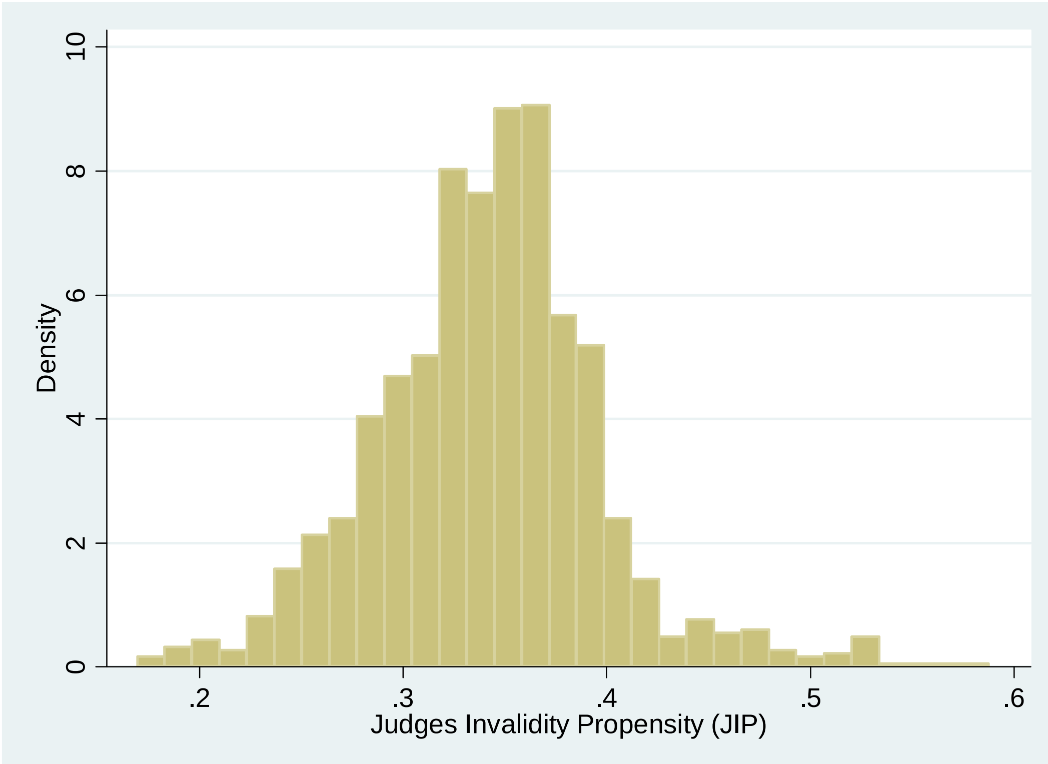
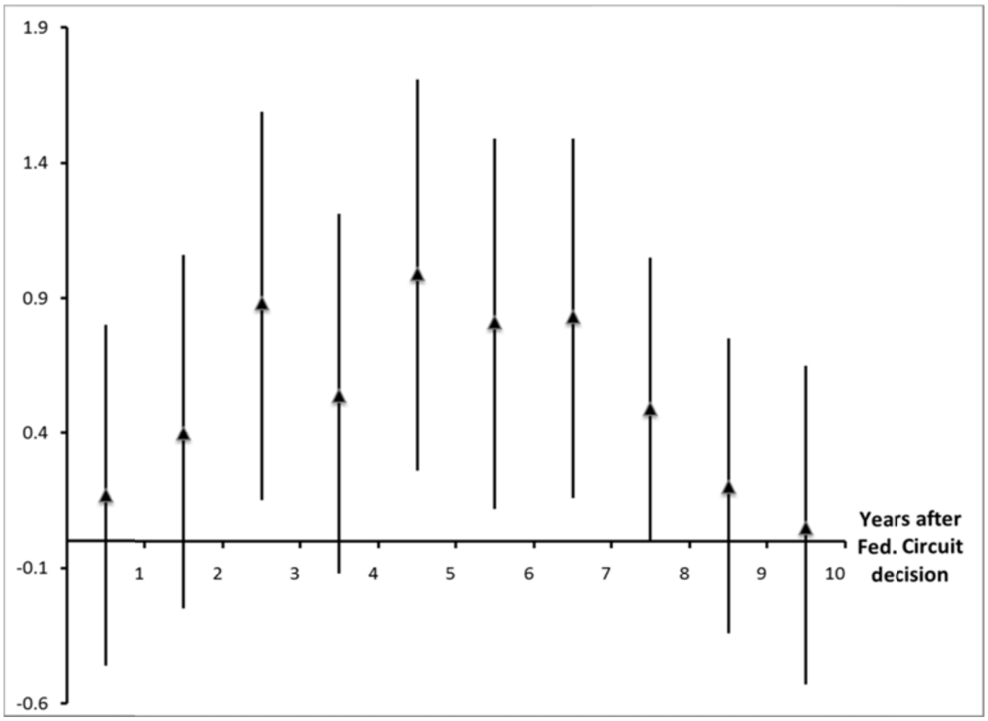


Figure 3. Timing of the Invalidation Effect



NOTES: IV estimate of the invalidation effects and 90-percent confidence intervals in each of the ten years following invalidation.

Figure 4. Marginal Treatment Effect

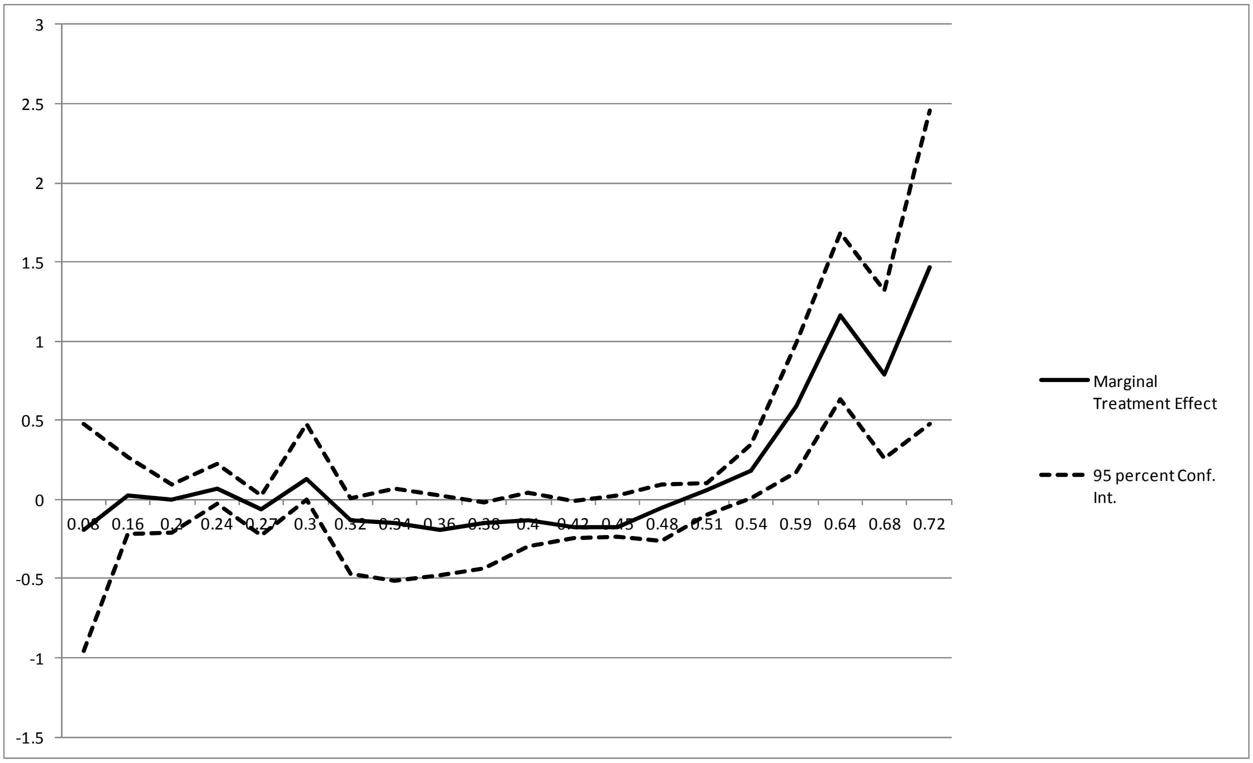


Table A1. Federal Circuit Judges

Judge	*Active Service*	*Validity Decisions 1982-2008*	*Percentage of Decisions in which the Judge voted for Invalidation*
Randall Ray Rader	1990-	242	39.6
Daniel Mortimer Friedman	1982–1989	112	21.2
Pauline Newman	1984-	309	26.9
Glenn Leroy Archer, Jr.	1985–1997	170	34.7
Haldane Robert Mayer	1987–2010	269	42.4
S. Jay Plager	1989–2000	153	35.3
Alan David Lourie	1990-	293	46.8
Raymond Charles Clevenger III	1990–2006	232	37.9
Alvin Anthony Schall	1992–2009	248	37.5
William Curtis Bryson	1994-	238	44.1
Arthur J. Gajarsa	1997–2011	164	41.5
Richard Linn	1999–	111	43.2
Timothy B. Dyk	2000-	131	37.4
Sharon Prost	2001-	106	40.6
Kimberly Ann Moore	2006-	21	76.2
Giles Sutherland Rich	1982–1999	152	40.8
Arnold Wilson Cowen	1982-2007	59	33.9
Oscar Hirsh Davis	1982–1988	70	50.1
Philip Nichols, Jr.	1982-1990	38	26.3
Byron George Skelton	1982–2004	56	33.9
Phillip Benjamin Baldwin	1982-1991	54	25.9
Howard Thomas Markey	1982–1991	138	49.3
Marion Tinsley Bennett	1982–2000	57	57.9
Shiro Kashiwa	1982-1986	34	38.2
Jack Richard Miller	1982-1994	35	42.9
Edward Samuel Smith	1982-2001	91	36.3
Paul Redmond Michel	1988–2010	245	41.6
Helen Wilson Nies	1982–1996	89	38.2
Jean Galloway Bissell	1984–1990	41	24.4

Table A2. Intensive and Extensive Margins - Robustness (IV Estimates)

	Total Effect (PostCites Received)			**Extensive Margin (Number of Distinct Assignees)**		
	1	**2**	**3**	**4**	**5**	**6**
	Citing Patents in Small Portfolios (< 5 patents)	Citing Patents in Small Portfolios (< 2 patents)	Citing Patents in Small Portfolios (<2 patents)	Citing Patents in Small Portfolios (< 5 patents)	Citing Patents in Small Portfolios (< 2 patents)	Citing Patents in Small Portfolios (<2 patents)
Invalidated	0.046	0.125	0.128	0.015	0.076	0.088
	(0.179)	(0.168)	(0.165)	(0.152)	(0.143)	(0.141)
Invalidated X Large Patentee (> 75 patents)	2.552**		2.248*	1.842**		1.390*
	(1.360)		(1.277)	(0.951)		(0.745)
Invalidated X Large Patentee (> 102 patents)		1.769**			1.216**	
		(0.752)			(0.550)	

NOTES: * significant at 10 percent, ** significant at 5 percent and *** significant at 1 percent. Robust standard errors are reported in parentheses. All regressions control for log(PreCites) in the size group, log(PreSelfCites), log(Claims), age and year effects. PostCites = cites from patents of other assignees in 5 year window after Federal Circuit decision. Invalidated=1 if Federal Circuit invalidates at least one claim of focal patent. Invalidated and its interactions are instrumented by the Probit estimates of the probability of invalidation and its interactions. Large Patentee=1 if patentee has more than 102 patents. A citing firm is classified as small if its portfolio has less than 5 patents, as medium if the portfolio has between 5 and 102 patents and as large if it has more than 102 patents. Dependent variables: in columns 1-3 are the total external cites received by the patent from citing firms in the size group.

Figure A1. Simulated and Estimated Judge Fixed Effects