PATENTS AND CUMULATIVE INNOVATION: CAUSAL EVIDENCE FROM THE COURTS*

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Cumulative innovation is central to economic growth. Do patent rights facilitate or impede follow-on innovation? We study the causal effect of removing patent rights by court invalidation on subsequent research related to the focal patent, as measured by later citations. We exploit random allocation of judges at the U.S. Court of Appeals for the Federal Circuit to control for endogeneity of patent invalidation. Patent invalidation leads to a 50% increase in citations to the focal patent, on average, but the impact is heterogeneous and depends on characteristics of the bargaining environment. Patent rights block downstream innovation in computers, electronics, and medical instruments, but not in drugs, chemicals, or mechanical technologies. Moreover, the effect is entirely driven by invalidation of patents owned by large patentees that triggers more follow-on innovation by small firms. *JEL* Codes: K41, L24, O31, O33, O34.

I. Introduction

Cumulative research is a dominant feature of modern innovation. New genetically modified crops, computers, memory chips, medical instruments, and many other modern innovations 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 how past successes and failures serve as "focusing devices" that guide the direction of later technological inquiry (Rosenberg 1976).[1] However, the

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1. This cumulative feature is reinforced by the constraints imposed by the prevailing stock of scientific knowledge on the feasible avenues for technology

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increasing importance of basic science in shaping the direction of technological development has intensified this process.

Cumulative innovation and the knowledge spillovers that underpin it lie at the heart of the recent macroeconomic theory literature on innovation and growth. Leading examples of these endogenous growth models include Grossman and Helpman (1991), Aghion and Howitt (1992), and Acemoglu and Akcigit (2012). At the same time, there is an extensive empirical literature showing that R&D creates knowledge spillovers, which increase 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 article 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 among academic scholars and 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 among firms are believed to raise transaction costs, constrain the freedom of action to conduct R&D, and expose firms to ex post holdup through patent litigation (Heller and Eisenberg 1998; Bessen and Maskin 2009). 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

development (Mokyr 2002). This is not say that science dictates only one path for the development of technology at any point in time. Recent theoretical work emphasizes the role of diverse research approaches in technological development (Acemoglu 2012).

2. 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 overinvestment 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. Specifically, the disclosure provision in patent law (35 U.S.C. section 112) requires the patent applicant to describe the invention to promote information diffusion and enable development of follow-on improvements of the original invention.

public debates on patent policy in the United States (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).

Economic research on the impact of patent rights on cumulative innovation has been primarily theoretical. 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 about the bargaining environment and contracting efficiency between different generations of innovators. In an early contribution, Kitch (1977) argues that patents enable an upstream inventor to coordinate investment in follow-on innovation more efficiently and mitigate rent dissipation from downstream patent races that would otherwise ensue. This “prospecting theory” suggests that patent rights facilitate cumulative innovation. In contrast, Green and Scotchmer (1995) show that upstream patent rights will not impede value-enhancing, follow-on innovation as long as bargaining between the parties is efficient. 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 innovation when bargaining failure occurs. This can arise from asymmetric information (Bessen and Maskin 2009) or coordination failures when downstream innovators need to license multiple upstream patents (Galasso and Schankerman 2010).

This diversity of theoretical models highlights the need for empirical research. It is important not only to establish whether patent rights block subsequent innovation but also to identify how this effect depends on the characteristics of the bargaining environment and the transacting parties. These issues are central to an understanding on how patent rights affect the dynamics of the “industrial organization” of innovation.

There are two empirical challenges in studying the effect of patents on cumulative innovation. First, cumulativeness is difficult to measure directly. In this article we primarily follow the large empirical literature that uses citations by later patents as a way to trace knowledge spillovers (for a survey, see Griliches 1992). Although not perfect, this is the only feasible approach if one wants to study the impact of patent rights across diverse technology fields. Nonetheless, we also show that our results are robust to alternative measures of cumulative innovation in the technology fields of drugs and medical instruments, where data on new product developments are publicly available due to government regulation requiring public registration. The second problem in identifying the causal effect of patent rights on later innovation is the endogeneity of patent protection. For example, technologies with greater commercial potential are both more likely to be protected by patents and to be an attractive target for follow-on innovation.

Given the importance of the issue, there is surprisingly little econometric evidence on the link between patent rights and cumulative innovation. In two influential papers, Murray and Stern (2007) and Williams (2013) provide the first causal evidence that intellectual property rights block later research 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 effect of intellectual property on genes sequenced by the private firm Celera on subsequent human genome research and product development. Interestingly, both papers find roughly similar magnitudes—property rights appear to cause about a 20–40% 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 varies across technology fields and patent owners is essential for thinking about how best to design the strength and scope of patent protection.

In this article 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, together with variation in their propensity to invalidate patents, to construct an instrumental variable that addresses the potential endogeneity of invalidity decisions. Because patents constitute prior art, later applicants are still required to cite patents when relevant even if they have been invalidated and thus put into the public domain.

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 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 (atorvastatin), the largest-selling drug in the world. Our reliance on privately valuable patents to estimate the effect of patent rights on cumulative innovation is similar to Azoulay, Graff Zivin, and Wang (2007) who rely on the death of superstar scientists to estimate the magnitude of knowledge spillovers. It is reasonable to start by analyzing high-value patents rather than a random sample, 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 the potential welfare costs are likely to be larger.

There are three main empirical findings in the article. First, we show that patent invalidation leads to about a 50% increase in subsequent citations to the focal patent on average, and this finding is robust to a wide variety of alternative specifications and controls. Moreover, we show that this impact begins only after two years following the court decision, which is consistent with the entry of new downstream innovators but is not consistent with the alternative explanation that the increase in citations is simply driven by 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, 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 that have unobservable characteristics associated with a lower probability of invalidity (i.e., stronger patents). There is also 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 ownership. This finding is consistent with predictions of the theoretical models that emphasize bargaining failure in licensing as the source of blockage. Patent invalidation has a significant effect on cumulative innovation only in the fields of computers and communications, electronics, and medical instruments (including biotechnology). We find no effect for drugs, chemicals, or mechanical technologies. Moreover, for two of the technology fields we study—medical instruments and drugs—we are able to construct alternative measures of cumulative innovation that exploit data on publicly disclosed new product developments. The results confirm our findings using citations: patent invalidation has a significant effect on later innovation in medical instruments but no effect in pharmaceuticals.

Last, we show that the effect of patent rights on later innovation depends critically on the characteristics of the transacting parties. The impact is entirely driven by the invalidation of patents owned by large firms, which increases the number of small innovators subsequently citing the focal patent. We find no statistically significant effect of patent rights on later citations when the invalidated patents are owned by small or medium-sized firms. This result suggests that bargaining failure between upstream and downstream innovators is not widespread but is concentrated in cases involving large patentees and small downstream innovators.

Taken together, our findings indicate that patent rights block cumulative innovation only in very specific environments, and this suggests that government policies should be targeted at facilitating more efficient licensing in those environments. However, we want to emphasize that the "experiment" in this article involves the judicial removal of an existing patent right. In Section VIII we discuss some of the conceptual differences between our setting and an alternative thought experiment in which patent rights are not granted in the first place. We argue that these two regimes differ in terms of the underlying incentives for the rate and direction of innovation and in the capability of patents to serve as an informational signal that facilitates access to capital markets, especially for small firms.

The article is organized as follows. In Section II we present a simple model that characterizes conditions under which patents facilitate, block, or have no effect on follow-on innovation. The model highlights the key role of bargaining failure between upstream and downstream innovators and coordination failure among competing downstream innovators. Section III describes the data set. In Section IV we develop the baseline econometric model for estimating the causal effect of patent rights and present the empirical results. In Section V we extend the analysis to allow for heterogeneous marginal treatment effects and empirically link them to characteristics of the patent case. Section VI shows 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 VII examines the robustness of our findings to using measures of downstream innovation that do not depend on patent citations. Section VIII discusses the interpretation and implications of the empirical findings. We conclude with a brief summary of findings. Details of data construction and extensive robustness analysis are included in the Online Appendixes.

II. 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 a second-generation firm requires a license on the earlier technology and the bargaining between the parties fails. In this section we present a simple analytical framework that characterizes conditions under which patents are likely to block, facilitate, or have no effect on follow-on investment, and we use the framework to organize the different theoretical models in the literature. The key feature in our framework is a trade-off between bargaining failure due to asymmetric information, which impedes licensing when there is an upstream patent, and coordination failure among downstream innovators, which reduces their incentives to invest in the absence of patent rights.

There is one upstream technology, and one potential downstream innovation. The value of the downstream technology can be high or low, which we denote by $\lambda \in \{\underline{\lambda}, \overline{\lambda}\}$ with $\underline{\lambda} < \overline{\lambda}$. There are two identical potential downstream inventors. To develop the follow-on technology an innovator needs to sustain a cost equal to S. We make the following assumptions:

Assumption 1. Downstream innovators know the value of the follow-on technology. The owner of the upstream technology assigns probability $\Pr(\lambda = \overline{\lambda}) = \alpha$ that the downstream technology has high value.

ASSUMPTION 2. $\frac{\lambda}{2} - S < 0$ for $\lambda \in \{\underline{\lambda}, \overline{\lambda}\}$.

As in Galasso and Schankerman (2010), Assumption 1 generates an asymmetric information problem by restricting the knowledge of the upstream patentee on the value of the downstream innovation. Assumption 2 creates a coordination problem by making it unprofitable for both downstream innovators to invest, as in Bolton and Farrell (1990). We contrast the case in which the upstream technology is not patented and the case in which there is patent protection. The crucial difference between the two cases is that without a patent on the upstream technology, the follow-on innovators can freely decide whether to invest in downstream innovation. In contrast, with patent protection on the upstream technology, a licensing deal is required.

II.A. No Patent on the Upstream Technology

In the absence of upstream protection, each of the follow-on innovators chooses independently whether to invest. We assume that in the absence of investment an innovator obtains a payoff equal to 0. If the innovator is the only one to develop the follow-on innovation, the payoff of the innovator is $\lambda - S > 0$ with $\lambda \in \{\underline{\lambda}, \overline{\lambda}\}$. This payoff captures the idea that the follow-on innovator is the patentee of the second-generation technology and appropriates the entire value. We assume that if both innovators invest, each of them obtains the patent with probability $\frac{1}{2}$ so their expected payoffs will be $\frac{\lambda}{2} - S < 0$ with $\lambda \in \{\underline{\lambda}, \overline{\lambda}\}$.

There are two asymmetric pure-strategy Nash equilibria in which one of the two follow-on innovators invests and the other does not. The literature on economic coordination suggests that these asymmetric pure-strategy equilibria are unconvincing in a symmetric setting like ours. For example, Crawford and Haller (1990) formally show that it is inappropriate to focus on asymmetric pure-strategy equilibria because it is not clear how players find one of those equilibria. Therefore, we follow Bolton and Farrell (1990) and focus on the symmetric mixed strategy equilibrium. Each innovator invests with probability $p(\lambda) = \frac{2(\lambda - S)}{\lambda}$ with $\lambda \in \{\underline{\lambda}, \overline{\lambda}\}$. This implies that follow-on innovation takes place with probability $1 - (1 - p(\overline{\lambda}))^2$ if the second generation technology has high value, and probability $1 - (1 - p(\underline{\lambda}))^2$ if the downstream technology has low value. Thus the expected level of follow-on innovation is

$$I_{NOP} = \alpha\left(1-\left(\frac{2S-\overline{\lambda}}{\overline{\lambda}}\right)^2\right)+(1-\alpha)\left(1-\left(\frac{2S-\underline{\lambda}}{\underline{\lambda}}\right)^2\right). \tag{1}$$

II.B. Patent on the Upstream Technology

If the upstream technology is protected by a patent, the patentee can potentially block downstream innovation. Patentability of the follow-on technology induces the owner of the base technology to license it to only one of the two downstream innovators. We assume that the patentee makes a take-it-or-leave-it offer to the follow-on innovator.[4] A licensing fee equal to $\underline{\lambda}-S$ will be accepted both when the value of the downstream innovation is high and when the value is low. A fee equal to $\overline{\lambda}-S$ will be accepted only when the second-generation technology has high value. Notice that in expectation it is more profitable to offer $\overline{\lambda}-S$ if $\alpha(\overline{\lambda}-S)\geq\underline{\lambda}-S$, that is, when $\alpha\geq\hat{\alpha}\equiv\frac{\underline{\lambda}-S}{\overline{\lambda}-S}$.

This implies that with patent protection on the upstream technology, the expected level of downstream innovation is

$$I_P = \begin{cases} 1 \text{ if } \alpha\leq\hat{\alpha} \\ \alpha \text{ if } \alpha>\hat{\alpha} \end{cases}. \tag{2}$$

II.C. Comparison of the Two Regimes

Proposition 1 compares the expected level of downstream innovation with and without patent rights on the upstream technology.

PROPOSITION 1. For $\overline{\lambda}$ large enough there exists $\alpha'>\hat{\alpha}$ such that $I_{NOP}>I_P$ if $\hat{\alpha}<\alpha<\alpha'$ and $I_P>I_{NOP}$ if $\alpha>\alpha'$ or $\alpha<\hat{\alpha}$.

Proof. The expected level of follow-on innovation without patent protection on the base technology (1) increases linearly in α. For $\alpha=1$ we have that $I_{NOP}=1-(\frac{2S-\overline{\lambda}}{\overline{\lambda}})^2<1=I_P$. For $\alpha=0$ we have $I_{NOP}=1-(\frac{2S-\underline{\lambda}}{\underline{\lambda}})^2<1=I_P$. Continuity of equation (1)

4. Following the literature on decentralization, we assume that the patentee can only make one offer and that he cannot implement more sophisticated mechanisms, as in Cremer and McLean (1985), to extract information from the follow-on innovators.

implies that $I_P > I_{NOP}$ for values of α close to 0 and 1. Now consider the innovation activity at $\hat{\alpha}$. At this value $I_{NOP} \geq I_P$ if

$$\frac{\underline{\lambda}-S}{\overline{\lambda}-S}\left(1-\left(\frac{2S-\overline{\lambda}}{\overline{\lambda}}\right)^2\right)+\left(1-\frac{\underline{\lambda}-S}{\overline{\lambda}-S}\right)\left(1-\left(\frac{2S-\underline{\lambda}}{\underline{\lambda}}\right)^2\right)>\frac{\underline{\lambda}-S}{\overline{\lambda}-S}$$

that is satisfied for $\overline{\lambda}$ large enough (i.e., close enough to $2S$). Finally, continuity of equation (1) and the fact that $I_{NOP} < I_P$ when $\alpha = 1$ implies that there exists a α' at which $I_P = I_{NOP}$. ■

The proposition shows that the impact of upstream patent rights on follow-on innovation depends critically on the trade-off between coordination failure and bargaining breakdown. Figure I illustrates the result. Intuitively, patent protection is not associated with low follow-on innovation for values of α that are high or low. This is because when α is close to 0 or 1, uncertainty about the value of follow-on innovation is low and the patentee can offer a profitable licensing fee that is accepted with high probability by the follow-on innovator. For intermediate values of α, there is greater uncertainty about the value of the follow-on innovation and the likelihood of bargaining failure is more severe.

In the absence of upstream patent rights, bargaining failure plays no role because the downstream innovator does not need a license to use the upstream technology. However, the absence of downstream coordination reduces the incentives for each follow-on innovator to invest.[5] When the technology is highly profitable ($\overline{\lambda}$ large), coordination failure is less costly and follow-on innovation becomes more likely. This implies that the absence of upstream patent rights can generate either a higher or lower level of downstream innovation than a regime with upstream patent protection.

II.D. Relation with Previous Literature

Our model shows that the impact of upstream patent rights on follow-on innovation depends on the relative strength of coordination and bargaining failure in licensing negotiations. We can generate the different predictions of various models in the

5. It also generates a positive probability of duplicative investment. Whether such duplication has a positive or negative impact on overall welfare depends on the relationship between λ and consumer welfare.

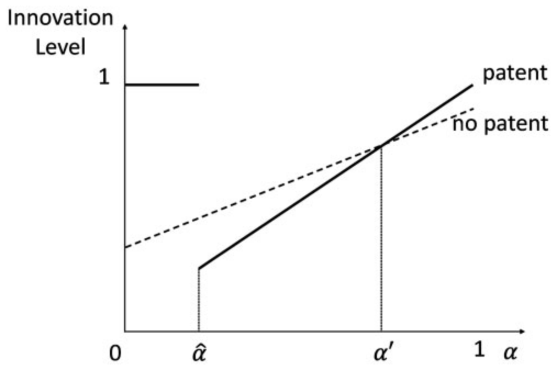

FIGURE I

Patent Protection and Follow-on Innovation

The figure plots the level of expected follow-on innovation in the case in which the upstream technology is protected by a patent (solid line) and the case in which there is not protection (dashed line). The parameter α denotes the probability that the follow-on technology has high value.

innovation literature by relaxing one or both of the key assumptions in our model.

If we drop Assumption 1, so both downstream and upstream innovators know the value of the follow-on technology, there is no bargaining failure and our model predicts higher follow-on innovation when there is an upstream patent. This prediction is in line with Kitch (1977), who describes an environment in which, in the absence of an upstream patent, development of technology improvements is impeded by coordination failure and free riding among downstream innovators. A patent on the base technology allows the upstream firm to act as a gatekeeper to coordinate downstream investments.

By dropping Assumption 2 and allowing $\frac{\lambda}{2} - S > 0$ for $\lambda \in \{\underline{\lambda}, \overline{\lambda}\}$, we turn off coordination failure, and our model implies that an upstream patent reduces follow-on innovation. This prediction is consistent with models where ex ante licensing does not take place in the presence of asymmetric information, as in Bessen and Maskin (2009). But licensing breakdown can also arise for other reasons. Galasso (2012) shows 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 and thus dilute her incentives to develop the new technology. Bargaining failure can also arise when patent ownership is 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 (Heller and Eisenberg 1998; Lemley and Shapiro 2007; Galasso and Schankerman 2010).

Finally, dropping both Assumptions 1 and 2, we obtain a framework similar to Green and Scotchmer (1995) in which ex ante contracting guarantees that any joint surplus enhancing downstream innovation is developed independently of the presence of a patent on the base technology. In their model the length and breadth of upstream patent rights affect the profitability and thus the incentive to develop the upstream technology, but once it is developed, frictionless bargaining ensures that efficient downstream investment takes place.[6]

III. 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 data set.

The Federal Circuit was established by Congress on October 1, 1982, and has exclusive jurisdiction over appeals in cases involving patents and claims against the federal government in a variety of subject matter. The Federal Circuit consists of 12 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 their 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 data set. 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

6. Even though blockage does not occur in this framework, Koo and Wright (2010) show that patent rights can induce the downstream innovator to delay development.

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.[7]

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); 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. Citations have been widely used in the economics of innovation literature as a proxy for follow-on research and are the only practical measure of cumulative innovation for studies such as ours that cover a wide range of technology fields. In Section VII we further discuss the merits of citations as a measure of follow-on innovation and show that our results are robust to alternative measures of cumulative innovation that we can construct for two technology fields, drugs and medical instruments.

The main variables used in the empirical analysis are as follows.

PostCites: citations received from patents of other assignees (owners) in a five-year window after 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.

7. Under very special circustances, judges or the litigating parties may petition to have the case decided “en banc” by all the judges of the court. These very few cases are dropped from our sample.

PostTotalCites: citations received both from patents owned by the same patentee as the focal patent and patents of other assignees in a five-year window after the Federal Circuit decision.

Invalidated: a dummy variable equal to 1 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.[8]

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 categories in Hall, Jaffe, and Tratjenberg (2001)—chemicals, computers and communications, drugs and medical, electrical and electronics, mechanicals, and others. We will also employ a narrower definition based on 36 two-digit subcategories.

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 data set contains 1,357 Federal Circuit patent validity decisions, covering 1,258 distinct patents.[9] Table I provides some summary statistics. The Federal Circuit invalidates in 39% of the cases. There is substantial variation in the age distribution of litigated patents at the time of the Federal Circuit decision (see Figure A1 in the Online Appendix). Note that lengthy

8. We experimented with an alternative definition of invalidation as whenever claim 1 of the patent (typically representing the primary claim) is invalidated. About 40% of patents are invalidated on our baseline measure, and 33% using the alternative definition. The empirical results are very similar with both measures.

9. This is because there are multipatent cases, and some patents are litigated more than once. Our sample size and mean invalidation rate are similar to an earlier study using Federal Circuit cases (Henry and Turner 2006).

TABLE I
SUMMARY STATISTICS

	Mean	Std. dev.	Min	Max
Invaliditated	0.39	0.49	0	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 1,357 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.

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 highly valuable patents. For example, in January 2005 the Federal Circuit invalidated the patent for the once-a-week version of Merck's Fosamax (alendronate sodium), the leading osteoporosis drug in the market at that time. This can be seen in Table II, 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. Drugs and medical instruments 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 commonly used indicators of patent value—the number of claims, citations per claim, and measures of patent generality and originality (as defined by Hall, Jaffe, and Tratjenberg 2001)—are all higher for litigated than other patents, and even higher for cases appealed to the Federal Circuit.[10] Equality of the means is strongly rejected

10. Generality is defined as 1 minus the Herfindahl index of the citations received by a patent across different technology classes. Originality is defined the same way, except that it refers to citations made.

TABLE II
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. Cites = total citations received up to 2002. Number of claims = total number of claims listed in focal patent. Generality = 1 minus the Herfindahl concentration index of the share of citations received by the focal patents from different patent classes. Originality = 1 minus the Herfindahl concentration index of the share of citations made by the focal patents in different patent classes. To perform this comparison, we use litigation data from Lanjouw and Schankerman (2001) and the NBER patent data set. 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%) and 877 are involved in Federal Circuit invalidity decisions (0.05%).

for all four variables (p-values $< .01$). 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 $< .01$).

Although self-selection of patents through the appeals process is certainly related to the private value of patents, other factors may play a role. First, cases with greater legal complexity are more likely to reach appeal because settlement by the parties is harder due to divergent expectations about how the court would decide the legal issues. Second, patents with greater technological scope for follow-on innovation are more likely to be involved in litigation in the first place. For both reasons, invalidation of patents in our sample is more likely to be associated with an increase in follow-on innovation than for the population of patents as a whole.

IV. Estimating the Impact of Patent Rights

IV.A. Baseline Specification and Identification Strategy

The final data set is a cross-section where the unit of observation is a Federal Circuit case involving patent p.[11] Our main empirical specification is

$$\begin{aligned} log(PostCites_p + 1) = {} & \beta\ Invalidated_p + \lambda_1 log(PreCites_p + 1) \\ & + \lambda_2 log(PreSelfCites_p + 1) \\ & + \lambda_3 log(Claims_p) + Age_p + Year_p \\ & + Tech_p + \varepsilon_p. \end{aligned} \tag{3}$$

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 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.[12]

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 heterogeneity 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.

11. 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 emphasize that our sample is a cross-section.

12. 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, it allows us to use our time-invariant allocation of judge panels as an instrument for patent invalidity decisions. Second, this specification allows us to examine heterogeneity in the effect of patent invalidation by estimating the marginal treatment effect. Our approach is similar to other studies where cross-sectional instrumental variables are used to examine heterogeneous causal effects (e.g., Carneiro, Heckman, and Vytlacil 2010).

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 citations to 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 (3) and a downward bias to the OLS estimate of β. 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). The Federal Circuit patent cases in our sample have involved a total of 51 distinct judges, including 22 nonappointed judges who filled in the vacancies during the Senate nomination process. 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% to a high of 76.2%.[13] This fact, together with the randomization of judge panels, creates exogenous variation in patent invalidation. However, it does not ensure randomization of decisions, which could still arise because of information that becomes available during the appellate process that could also be correlated with future citations. The instrument we construct also takes this concern into account.

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 favor 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

13. In Online Appendix Table A1 we list 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. We use the term *bias* to refer to variation across judges in their propensity to invalidate, but it can also reflect differences in their expertise and ability to process information in the different technology fields covered by the patent cases.

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 Online Appendix 1 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 (based on 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 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.[14] There is substantial variation in the distribution of the *JIP* index (mean of 0.34, range from 0.16 to 0.58). About 11% of the variation in *JIP* reflects year effects, because "biased" judges may be active only for a limited period of time.

Our identification strategy is similar to the one employed by Kling (2006), who uses random assignment of judges to estimate the effects of incarceration on employment and earnings of individuals, and Doyle (2007, 2008) who uses differences in the placement tendency of child protection investigators to identify the effects of foster care on long-term outcomes.[15] The main

14. 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% 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. A natural alternative to *JIP* is to use judge fixed effects. There are two reasons *JIP* is preferred. 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, Imbens, and Krueger 1999).

15. Other recent papers that exploit heterogeneity in the decision of judges and other experts for identification include Li (2012), Dahl, Kostol, and Mogstad (2013), Di Tella and Schargrodsky (2013), Dobbie and Song (2013), and Maestas, Mullen, and Strand (2013).

difference between the two approaches is that our *JIP* index is constructed at the (three-judge) panel level. The basic assumption behind our measure is that judges differ in their propensity to invalidate patents. To check this, we construct a data set 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 $< .01$).[16]

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 $\hat{P} = P(JIP, X)$. 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:

$$Invalidated_p = \alpha\hat{P}_p + \theta X_p + u_p$$

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

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

IV.B. Judge Panels and Patent Invalidation

Table III 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. Regressing *Invalidity* on these dummies and other controls, we strongly reject equality of judge effects, confirming heterogeneity in the propensity to invalidate. The judge fixed effects

16. 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 (details are provided in Online Appendix 2). We use the simulated vote to estimate judge fixed effects and find that they are not statistically significant (p-value = .66). We also compare the distribution of these fixed effects from simulated votes with the (statistically significant) fixed effects estimated using actual voting behavior. 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.

TABLE III
COMPOSITION OF JUDGE PANELS AND PATENT INVALIDATION

	(1)	(2)	(3)	(4)
Estimation method	Probit	Probit	Probit	OLS
Dependent variable	Invalidated	Invalidated	Invalidated	JIP
Judges dummies	Yes***			
JIP		3.464***	3.313***	
		(0.647)	(0.743)	
log(Claims)	0.034		0.041	−0.001
	(0.039)		(0.039)	(0.001)
log(PreCites)	−0.134***		−0.137***	0.001
	(0.041)		(0.040)	(0.002)
log(PreSelfCites)	0.008		0.002	0.002
	(0.0047)		(0.045)	(0.002)
Year effects	Yes***	No	Yes***	Yes***
Age effects	Yes	No	Yes	Yes
Tech. effects	Yes	No	Yes	Yes
Fed. Circuit cecisions	1,357	1,357	1,357	1,357

Notes. * significant at 10%, ** significant at 5%, *** significant at 1%. 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, Jaffe, and Tratjenberg (2001). JIP = propensity to vote for patent invalidity of judge panel constructed from invalidity votes of judges in other sample cases. We add 1 to all citation measures to include patents with no cites.

explain about 6.5% of the variation in Federal Circuit invalidity decisions.

As indicated earlier, however, 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) and (3) we report probit regression models of the invalidity dummy against the *JIP* index. The estimated marginal effect in column (2) indicates that a 1 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 1 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 = .17).[17]

Finally, in column (4) we present the result of an OLS regression with *JIP* as dependent variable that supports the randomization of judges to cases. The number of claims of the litigated patent, the predecision citations, the age of the patent, and its technology class are all uncorrelated with the propensity of the judges to invalidate patents. Only the year effects are 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. For additional evidence that judges are randomly assigned and *JIP* is orthogonal to patent characteristics known prior to the decision, we examine the correlation between *JIP* and various subsets of the patent characteristics in our sample. In all cases the correlations are close to 0 and statistically insignificant (see Online Appendix 2 for details).

We perform a variety of tests to confirm robustness of these findings (results not reported). 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 the Federal Circuit 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 technology subfields not captured by our six broad technology category dummies. We reestimate the probit regression controlling for more detailed technology field classifications using the 32 NBER technology subcategories. The magnitude of the estimated *JIP* coefficient remains similar (3.027, p-value < .01). In addition, we rerun 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

17. Results are robust to using an alternative measure of invalidation—the fraction of invalidated claims. We find a positive and statistically significant association between the degree of patent invalidation and the *JIP* index.

confirmed statistical significance using standard errors clustered at the patent or case level.

IV.C. Patent Invalidation and Cumulative Innovation

1. Baseline Specification. In Table IV 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 to 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, there are reasons 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.[18]

To address this endogeneity, we start with a conventional panel regression approach which controls for fixed patent effects, age dummies, and year (group) dummies. The coefficient (standard error) on patent invalidity is −0.068 (0.022) which is very close to and not statistically different from the cross-sectional OLS coefficient. This indicates that the main source of endogeneity is time-varying and cannot be dealt with by standard panel data methods.

In column (2) we move to an 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 invalidation as exogenous).

In column (3) we instrument *Invalidated* with the predicted probability of invalidation obtained from the probit regression (rather than *JIP* itself) from column (3) of Table III. This is more efficient as the endogenous regressor here is binary (Wooldridge

18. Following Rivers and Vuong (1998), we regress *Invalidated* on *JIP* and the other controls in a linear probability model. We construct the residuals ($\hat{\nu}$) for this model and then regress subsequent citations on *Invalidated*, $\hat{\nu}$ and the other controls. The coefficient on $\hat{\nu}$ is negative and highly significant (p-value $< .01$).

TABLE IV
IMPACT OF INVALIDATION ON CITATIONS

	(1)	(2)	(3)	(4)
Estimation method	OLS	IV	IV	IV
Dependent variable	log (PostCites)	log (PostCites)	log (PostCites)	log (PostTotalCites)
Invalidated	−0.053	1.158**	0.410**	0.413**
	(0.046)	(0.489)	(0.196)	(0.198)
log(Claims)	−0.001	−0.018	−0.007	−0.008
	(0.025)	(0.030)	(0.025)	(0.025)
log(PreCites)	0.538***	0.598***	0.558***	0.550***
	(0.028)	(0.040)	(0.029)	(0.029)
log(PreSelfCites)	0.085**	0.084**	0.085**	0.170**
	(0.030)	(0.034)	(0.030)	(0.031)
Year effects	Yes***	Yes***	Yes***	Yes***
Age effects	Yes***	Yes***	Yes***	Yes***
Tech. effects	Yes	Yes	Yes	Yes
Instrument		JIP	Predicted probability from probit	Predicted probability from probit
IV test		$F = 17.43$ ($p < .01$)	$F = 94.85$ ($p < .01$)	$F = 86.18$ ($p < .01$)
Fed. Circuit decisions	1,357	1,357	1,357	1,357

Notes. * significant at 10%, ** significant at 5%, *** significant at 1%. Robust standard errors are reported in parentheses. PostCites = cites from patents of other assignees in 5-year window after Federal Circuit decision. PostTotalCites = sum of self-cites and 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 = six categories defined in Hall, Jaffe, and Tratjenberg (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. We add 1 to all citation measures to include patents with no cites.

2002) and, as expected, the F-statistic from the first stage regression 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 causes an increase in *external citations* of about 50 percent in the five years following the Federal Circuit decision.[19] This increase in citations by other innovators does not necessarily imply that total follow-on innovation intensified, as it depends on what happens to the innovation

19. Because the specification relates log of cites to the dummy variable *Invalidated*, we compute the marginal effect as $e^{0.41} - 1 = 0.50$.

by the owner of the invalidated patent. In column (4) we examine the relationship between invalidation and the number of *total citations* (including both external and self-cites) received by the patent in the five years following the Federal Circuit decision. The estimated coefficient is very similar to the one obtained for external citations, which indicates that the increase in external citations is not compensated by a decline in self-citations.[20]

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 heterogeneous. Moreover, we will reveal how the impact of patents varies with the characteristics of the patent, the patentee and the technology field.

2. Robustness and Extensions. We perform a variety of tests to confirm robustness of our main finding (details are provided in Online Appendix 3). In this section we briefly summarize the main robustness checks and describe two extensions of the empirical analysis.

First, up to now we have treated an invalidation judgment as the final verdict. However, 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). To deal with this issue we identified the patent invalidity cases appealed to the Supreme Court in our data set (there are only 12 cases). We drop these cases and reestimate the model using instrumental variables. The point estimate of the coefficient on patent invalidation is very close to the baseline coefficient.

Second, the citations information obtained from the USPTO ends in 2010, so the latest years in the sample are subject to truncation. We run two robustness checks to assess whether truncation is an issue in our study. First, we restrict the sample to patent decisions that take place before 2003, where we have a

20. In a companion research project, we are examining how patent invalidation affects self-citations as an indicator of how patent rights influence the direction of the firm's research trajectory. Our findings indicate that the effect of patent invalidation depends critically on whether the patent is central or peripheral to the patenting strategy of the firm.

complete five-year window of citations, and the results are similar to the estimates using the whole sample. Second, we adjust for truncation exploiting the citation lag distribution estimated in Hall, Jaffe, and Trajtenberg (2001). They provide an estimate of the distribution of citations received over the life of patents across different technology classes that we use to inflate the citations received by patents for which we observe only a fraction of the five-year window. The estimates from this procedure are also very similar to the baseline estimates.

Third, the baseline model incorporates fixed effects for six broad (one-digit) technology fields. To account for unobserved heterogeneity that might be related to narrower technology fields, we also estimate a specification that uses a more refined technology classification—32 two-digit subcategories from the NBER. The point estimate of the coefficient on *Invalidated* is nearly double the baseline estimate but also less precise, and we cannot reject the null hypothesis that the two estimated coefficients are the same.

Fourth, to allow the age distribution of citations to vary across technology fields, we extend the specification by including a full set of interactions between the technology field and age dummies. The estimated coefficient on invalidation is nearly identical to the baseline coefficient. We also reestimate 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, and we also get similar estimates if we drop these patents from the sample entirely.

Finally, there is a concern that some Federal Circuit decisions may involve rulings that limit the scope of patentable subject matter (e.g., software or business models) rather than simply assessing the validity of the focal patent. This type of invalidation could reduce subsequent citations for the entire technology field, leading us to underestimate the true blocking effect of patent rights (since we focus only on citations to the invalidated patent). To address this, we identified the most important Federal Circuit decisions that relate to patentable subject matter during our sample period. Dropping those decisions and reestimating the model, we obtain coefficients that are nearly identical to the baseline estimates.[21]

21. In Online Appendix 3 we check whether the invalidation effect differs across quartiles of the patent value distribution as measured by the predecision external citations. We find no evidence of such differences. We also show that our results are

We turn next to two extensions that are of independent interest. In the first extension we examine whether Federal Circuit invalidation has a smaller effect on older patents. In the extreme case where invalidation occurs after the patent has expired (there are such cases), the patent no longer has the power to block follow-on development so 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. We view these regressions as a kind of placebo test, providing additional support for the hypothesis that the invalidation effect is not being driven by other unobservable factors. 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 V 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.[22]

In the second extension, we investigate the *time path* of the effect of invalidation on subsequent citations. Figure II plots IV estimates of the effect of invalidation in each of the 10 years that

robust when we introduce two different controls for the level of competition: (i) the portfolio size of the patent holder, which is likely to affect both product market and technology competition with other firms; and (ii) a measure of the concentration of patenting among firms operating within a technology area. Finally, we explore whether the invalidation effect is driven by citations by U.S. patents owned by foreign entities. We find that the invalidation effect is significant only for citations by domestic follow-on innovators. This result is interesting by itself because it is suggests that licensing frictions (removed by patent invalidation) must represent only a fraction of the total cost for foreign innovators to patent in the United States.

22. We experimented with a variety of alternative specifications and obtain similar results. While there is clear evidence that citations decline with age, the impact of invalidation does not systematically vary with the age of the invalidated patent. The only robust finding is that the invalidation effects drops to zero as patents approach expiration.

TABLE V
Impact of Invalidation and Patent Age (IV Estimates)

	(1)	(2)	(3)	(4)
Sample	Age ≤ 20	Age ≤ 18	Age ≤ 15	Age > 15
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)
Fed. Circuit decisions	1,313	1,245	1,098	259

Notes. * significant at 10%, ** significant at 5%, *** significant at 1%. 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. We add 1 to all citation measures to include patents with no cites.

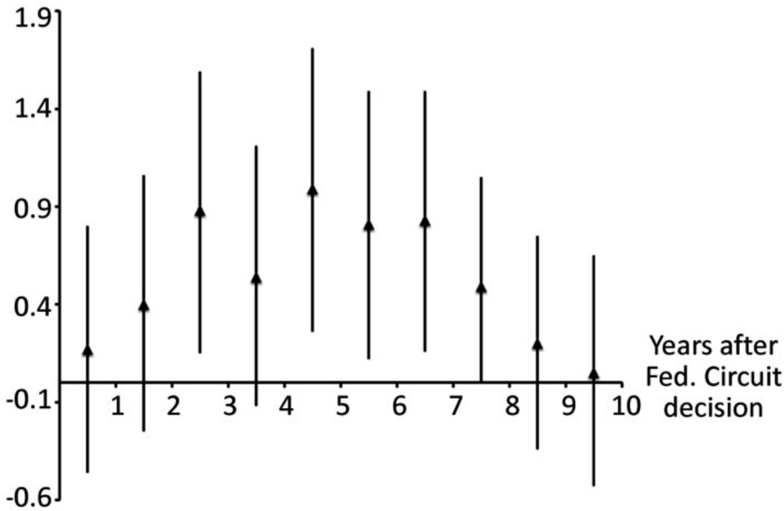

Figure II
Timing of the Invalidation Effect

IV estimate of the invalidation effects and 90% confidence intervals in each of the 10 years following invalidation.

follow invalidation, and the associated 90% confidence intervals. The results show that there is no statistically significant effect in the first two years after Federal Circuit invalidation. Moreover, the effects persist for seven years after the invalidation.[23] This pattern suggests that the observed impact of invalidation is not simply due to a "media effect" from press coverage associated with the court decision, where we would expect a more immediate increase in citations and probably more rapid dissipation 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 Section VIII we provide additional evidence which rules out media publicity, and we conduct a detailed analysis of where the blockage occurs, specifically, which technology fields and which types of patentees and downstream innovators.

V. Heterogeneous Impacts of Patent Invalidation

V.A. *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 II indicated, the impact of patents on later innovation depends on the risk of bargaining failure between upstream and follow-on innovators, and coordination failure among competing downstream developers. Thus we would expect the impact to vary with characteristics of the technology field, the transacting parties and market structure. In this section we extend the econometric model to explore this heterogeneity.

We begin by assuming 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

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

where v_p is a characteristic of the patent case that is unobservable to the econometrician and affects the invalidity decision. In general, we would expect this unobservable characteristic to be

23. These estimates are based on decisions in the 1982–2003 period, so that we have at least seven years of postdecision observations for every patent in the sample. If we include more recent years, or drop decisions after 2001, we still find that the statistically significant effects are concentrated in the third to sixth year following invalidation.

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 (heterogeneous) effect of invalidation on future citations for patents that are invalidated because of the instrument. Carneiro, Heckman, and Vytlacil (2010) provide a formal treatment, where they show that

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

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

In Figure III 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 postdecision citations for different values of this probability. The support for the estimated probability goes from the 10th to the 90th 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 leads 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 based on observable characteristics. For these patents the MTE is positive, indicating that citations increase after invalidation.[24]

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 that drive the

24. These findings are robust to using alternative estimation methods to compute the MTE, including a nonparametric approach and the semiparametric approach (with a third-order polynomial) proposed by Carneiro, Heckman, and Vytlacil (2010).

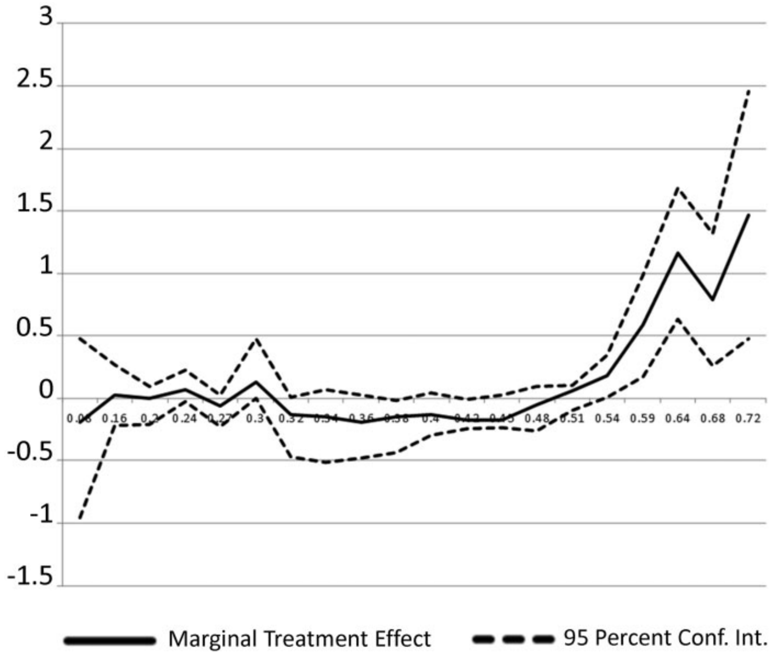

FIGURE III

Marginal Treatment Effect

The horizontal axis indicates the estimated probability that the patent is invalidated. The vertical axis shows the effect of invalidation on postdecision citations for different values of invalidation probability.

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). An example would 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.

V.B. Explaining the Heterogeneity

We showed that the effect of patent invalidation on subsequent citations is concentrated among a small subset of patents. We turn now to unbundling the heterogeneous impact of patent rights by relating it to observable characteristics of the technology field and contracting environment.

Previous empirical studies emphasize two features of the innovation environment that affect bargaining between upstream and downstream firms, and thus the incentives to invest in follow-on innovation. The first is the concentration of patent ownership in the technology field. For example, Ziedonis (2004) argues that when patent ownership is not concentrated (i.e., fragmented), downstream innovators need to engage in multiple negotiations, which exacerbates the risks of bargaining failure and ex post hold-up. However, from a theoretical perspective the relationship between fragmentation of patent ownership and the blocking effect of patent rights is ambiguous. Existing models of contracting over patents indicate that the value obtained from accessing an additional patent in a fragmented environment depends critically on the degree to which patents are complements or substitutes (Lerner and Tirole 2004; Galasso and Schankerman 2010).

The second feature is the complexity of the technology field. In complex fields, new products embody numerous patentable elements, as contrasted with discrete technology areas where products build only on few patents. When products typically incorporate many patented inputs, and they are held by different owners, licensees need to engage in multiple negotiations and the risk of bargaining failure is higher. 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, *Conc*4, 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 *Conc*4 are 0.067 and 0.053, respectively). The second variable, *Complex*, is a dummy variable for patents in complex technology fields.

Following Levin et al. (1987) and Cohen, Nelson, and Walsh (2000), we classify electrical and electronics (NBER category 4), computers and communication (NBER category 2), and medical instruments and biotechnology (NBER subcategories 32 and 33) as complex technology fields.

In columns (1) and (2) of Table VI 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, columns (3) and (4) show that the effect of invalidation is more than twice as large in complex technology areas as compared to the noncomplex technology fields. Column (5) provides estimates using the full sample and interacting *Conc*4 and *Complex* with the *Invalidated* dummy. These confirm the findings from the split sample regressions. Evaluated at their respective sample means of *Conc*4, our point estimate (standard error) for complex technology fields is 1.149 (0.29); for noncomplex 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%. We also confirm that concentration substantially mitigates the effect of patent invalidation on future citations: a 1 standard deviation increase in *Conc*4 reduces the effect of invalidation by 0.37, which is 32% of the estimated impact for complex fields.[25]

We can use the parameter estimates from column (5) to compute the implied effect of patent invalidation on citations for each of the technology fields, based on the observed values of *Conc*4 and *Complex* for each field. The results, presented in column (1) of Table VII, are striking. There is essentially no effect of patent rights on cumulative innovation in any of the three noncomplex

25. 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 33) as a noncomplex field, or if we replace the continuous concentration measure with a dummy variable for fields with *Conc4* above the 50th or 75th percentile. We also use our parameter estimates (column (5), Table VI) to examine how variation over time within fields affects 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.

TABLE VI

EFFECT OF COMPLEXITY AND CONCENTRATION (IV ESTIMATES)

	(1)	(2)	(3)	(4)	(5)
Sample	Conc4 ≥ median	Conc4 < median	Complex technologies	Noncomplex technologies	Full
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 × Conc4					−6.977***
					(2.457)
Invalidated × Complex					1.234***
					(0.327)
Fed. Circuit decisions	678	677	437	920	1,357

Notes. * significant at 10%, ** significant at 5%, *** significant at 1%. 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) control for technology class effects. Column (5) also controls for the direct effect of Conc4. Complex = 1 if patent is in computers 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 5 years preceding the Federal Circuit decision. Invalidated and its interactions are instrumented by the probit estimates of the probability of invalidation and its interactions. We add 1 to all citation measures to include patents with no cites.

TABLE VII
Technology Differences in Invalidation Effect (IV Estimates)

Technology	Based on Complex and Conc4	Split sample
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	1.024***	2.388**
and communications	(0.285)	(1.224)
Electrical and electronics	1.107***	−2.744
	(0.285)	(2.339)
Medical instruments	1.435***	2.402***
and biotechnology	(0.313)	(0.848)

Notes. * significant at 10%, ** significant at 5%, *** significant at 1%. Robust standard errors are reported in parentheses. Estimates in first data column obtained from column (5) of Table VI and sample means of Conc4 across various technology areas. Each regression in second data column controls for log(PreCites), log(PreSelfCites), log(Claims), age, and year effects. We add 1 to all citation measures to include patents with no cites.

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% in medical instruments/biotechnology, 203% in electronics, and 178% in computers. For comparison, column (2) reports estimates of split-sample regressions for each technology field. 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.

However, one concern with our finding that patent rights do not block follow-on innovation in drugs is that the litigation in that sector may be brought primarily by generic drug firms whose business model is to produce off-patent drugs rather than innovate by building on previous drugs. In this case, finding that patent invalidation has no effect would simply be due to an absence of interest by follow-on innovators, and could not be interpreted as evidence that licensing negotiations are effective. To address this concern, we conducted a full text search of the invalidity decisions involving pharmaceutical patents in our sample to identify cases related to Abbreviated New Drug Application

(ANDA) by generic firms.[26] We reestimate the model allowing the invalidity coefficient to be different for ANDA and other drug cases, but we find no statistically significant difference.

Overall, these findings indicate that the fragmentation of patent ownership and complexity of technology fields are key empirical determinants of the relationship between patent rights and cumulative innovation. Of course, other factors can also affect the impact of patent rights on subsequent innovation. One is product market competition. Aghion, Howitt, and Prantl (2013) provide evidence that strong patent protection stimulates innovation only when product market competition is fierce. A second factor is the degree to which tacit cooperation can be used by firms to mitigate potential bargaining failures and litigation that might otherwise arise from dispersed ownership of patent rights (Lanjouw and Schankerman 2004). Understanding where and how these differences operate is a valuable direction for future theoretical and empirical research.

Our findings are relevant to the current policy debates on patent reform. The recent literature studies specific innovations in biotechnology and medical instruments and finds blocking effects (Murray and Stern 2007; Murray et al. 2008; Williams 2013). Our estimates confirm the presence of blocking in these fields, using a much broader set of innovations and an entirely different identification strategy. But our results also show that the effect is very different in other fields, and thus remedial policies to mitigate blocking need to target specific technology areas to preserve innovation incentives. At the same time, changes in the contracting environment in which technology licensing takes place would reshape the relationship between patent rights and cumulative innovation.

VI. Intensive versus Extensive Margins

In the previous section we showed that the blocking effect of patents on later innovation depends on how concentrated patent

26. To do this, we identified references to at least one of the following terms: paragraph IV, Hatch-Waxman, Abbreviated New Drug Application, and ANDA. We find that about 25% (45 cases out of 167) of the drug patent decisions in our sample mentioned at least one of these terms, and we generated a dummy variable to capture such ANDA litigation. This is a conservative measure (upper bound) because these terms may also appear outside ANDA cases.

rights are—that is, 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 and show that the blocking effect of patents depends critically on the size of the patentee and the downstream 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 5, medium if the portfolio is between 6 and 101 patents, and large if it is 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.[27]

In addition, for each size group, we investigate whether the blocking effect of patent rights works through reducing the number of later innovators building on the focal patent or on the intensity of their downstream innovation. This question is of interest because the effect of patent rights on the industrial structure of innovation differs in the two cases. To examine this issue, 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 years following the Federal

27. In classifying firms, we do not correct for changes in patent ownership because more than 65% of our patents do not belong to the reassignment data set constructed by Serrano (2010). To address this issue, we manually match the assignee name of the litigated patent at the grant date with the names of the litigated parties. For 134 patent cases we notice a discrepancy between the USPTO name and the names of the litigants. Replacing the patent portfolios of original patent assignee with the portfolios of the litigating party we obtain results that are essentially identical to those reported below in Table VIII.

Circuit decision. We measure the intensive margin by the number of citations per assignee to the focal patent in the same time window.

Table VIII 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%. This is consistent with our earlier estimate of 50% for the average blocking effect in the overall sample, because roughly 50% of the citing entities are small firms in our data and about 20% of the patentees are large firms (i.e., $520 \times 0.5 \times 0.2 = 52\%$). The coefficients for the other size groups are much smaller in magnitude and statistically insignificant.

In columns (4)–(6), we study how patent invalidation affects the extensive margin. The dependent variable in these regressions is the logarithm of 1 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 exclusively among citations by small firms to large firm patents. The estimated coefficient of 1.347 implies a 285% 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%, but statistically significant only at the 10% 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. In Online Appendix 4 we present a series of additional regressions varying the threshold for defining small and large firms. These experiments show that the pattern emerging in Table VIII is extremely robust.

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

TABLE VIII

INTENSIVE AND EXTENSIVE MARGINS (IV ESTIMATES)

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Total effect log(PostCites)			Extensive margin log(Number of distinct assignees)			Intensive margin log(PostCites per assignee)	
	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 × 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%, ** significant at 5%, *** significant at 1%. 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 fewer than 5 patents, medium if the portfolio has between 5 and 102 patents, and large if it has more than 102 patents. Dependent variables: in columns (1)–(3) the total external cites received by the patent from citing firms in the size group; in columns (4)–(6) the total number of citing firms in the size group; and columns (7)–(9) the external cites per assignee in the size group. We add 1 to all citation measures to include patents with no cites.

suggests that bargaining failure among upstream and downstream innovators is not widespread. However, the results show that bargaining breakdown occurs when it involves large patentees and small downstream innovators. 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. Small firms do not have patent portfolios that can be used as counterthreats to resolve disputes or to strike cross-licensing agreements to preserve freedom to operate in their innovation activities (Galasso 2012).

Finally, we emphasize that our findings are not driven by the recent surge in litigation activity by nonpracticing entities (NPE, aka “trolls”) blocking follow-on research of small firms. This is because very few NPE patent cases reach the Federal Circuit court. The large anecdotal evidence on trolls shows that the most common business strategy for NPEs is to threaten litigation and demand a settlement fee that alleged infringers prefer to pay rather than face the cost and risk of litigation. To check this for our sample, we obtained a list of 50 leading patent trolls from Fisher and Henkel (2012) and manually matched their names against the litigants in our sample. We find that only 12 patent cases in our sample involve a troll. When we drop these observations and reestimate the model, we obtain estimates that are essentially identical to those obtained in our full sample regressions.

VII. Using Nonpatent Measures of Follow-on Innovation

To this point we have used the number of subsequent citations as our measure of follow-on innovation, which is the conventional approach. We are aware of very few exceptions. Williams (2013) studies the impact of patent rights on human genome research using both citations in later scientific publications and direct measures of product development. Moser and Rhode (2011) study the impact of the 1930 Plant Patent Act on plant innovation by tracking registration of new rose varieties with the American Rose Society. Using product-level information is clearly desirable, but citations are the only practical measure for studies that cover a wide range of technology fields, such as ours. From an economic perspective, patent citations play two distinct roles: they indicate when a new invention builds on prior patents (and thus may need to license the upstream patent), and they identify prior art that circumscribes the property rights that can be claimed in the new patent. Citations can either under- or overestimate the extent of follow-on innovation. They will underestimate it where inventors develop improvements that are not patented (or patentable), but overestimate it when the inventor did not actually built on the prior patent. In any event, there are serious hurdles to using product-level data to measure innovation across a wide range of technology fields. First, there are no comprehensive data sets of products in different industries, and second, there is no way to identify whether a product specifically builds on a previous patent.

Fortunately, however, we are able to construct nonpatent measures of follow-on innovation for two of our technology fields—pharmaceuticals and medical instruments—thanks to government regulation that requires registration of new product developments in these areas. These cover both a discrete technology field (drugs) in which we found no blocking effect using the citations measure, and a complex one (medical instruments) where we found a strong blocking effect. In this section we show that these findings also hold up when we use product-based measures.

VII.A. *Medical Instruments*

The Food and Drug Administration (FDA) has primary authority to regulate medical devices sold in United States. These products are subject to a regulatory process that requires detailed product information and evidence of safety from clinical trials. The FDA releases data on approvals requested for medical instruments. To use these FDA approval requests as a measure of follow-on innovation, we need to link them to the medical instrument patents in our sample. To do this, we use two alternative approaches. First, we search the text of the abstract in each of our litigated patents to identify a set of keywords related to the patented technology. We then search for all FDA approval requests to identify those that contain these keywords. In the second approach, we assign each litigated patent to a set of product codes from among the roughly 6,000 product codes in which the FDA classifies medical devices. We then use all of the FDA approval requests listed in the corresponding product codes as our measure of follow-on innovation. Online Appendix 5 provides details of the

TABLE IX

MEASURING FOLLOW-ON INNOVATION WITH NONPATENT DATA (IV ESTIMATES)

Dependent variable	(1) log(PostCites)	(2) log(post FDA approvals) keyword match	(3) log(post FDA approvals) product class match
Panel A: medical instruments			
Invalidated	2.447*	1.161*	1.516**
	(1.264)	(0.621)	(0.725)
Fed. Circuit decisions	121	121	121
Dependent variable	log(PostCites)	log(PostClinical Trials) identified drugs	log(PostClinical Trials) keyword match
Panel B: drugs			
Invalidated	0.231	0.266	0.539
	(0.449)	(1.269)	(1.200)
Fed. Circuit decisions	167	94	140

Notes. * significant at 10%, ** significant at 5%, *** significant at 1%. Robust standard errors are reported in parentheses. All regressions control for log(PreCites), log(PreSelfCites), age, and year effects. Invalidated = 1 if Federal Circuit invalidates at least one claim of focal patent. Post FDA Approvals = number of approval requests to the FDA related to focal patent in 5-year window following Federal Circuit decision. PostClinicalTrials = number of clinical trials related to focal patent in 5-year window following Federal Circuit decision. We add 1 to all innovation measures to include observations with a value equal to 0.

data construction and discussion of the merits and limitations of each approach.

We reestimate the baseline model using each of these measures of follow-on innovation. Panel A in Table IX summarizes the IV estimates of the patent invalidation effect for the medical instrument patents. Column (1) presents the estimate using citations as the dependent variable, which is statistically significant (p-value = .053) and very similar to the one reported in column (2) of Table VII. The slight difference in magnitude and smaller sample size are due to our focus on medical instrument patents, where we exclude biotechnology patents. Column (2) shows the estimated effect where we measure follow-on innovation with the number of FDA approval requests for which the product name contains at least one of the keywords from the litigated patents. Again we find a positive and statistically significant (p-value = .06) invalidation effect. The point estimate is smaller than (but not statistically different from) the one based on citations. In column (3) we measure follow-on innovation by the number of applications for the product codes in which the patent is classified. Here too the point estimate is broadly in line with (and not statistically from) the one estimated with citations. In Online Appendix 5 we show that these results are generally robust to how we define the keywords and product codes used to map between the FDA-registered medical devices and our litigated patents.

Overall, this analysis confirms our conclusion that patent invalidation has a significant impact on cumulative innovation in the complex technology field of medical instruments.

VII.B. Pharmaceuticals

We construct a measure of follow-on innovation by identifying the subsequent clinical drug trials that are related to the active ingredient of the litigated drug patent. The use of clinical trials as a measure of innovation is natural in the health sector and has been recently exploited in economic research (e.g., Finkelstein 2004 for vaccine research; Budish, Roin, and Williams 2014 for cancer drug research). Our data source for clinical trials is the website ClinicalTrials.gov, which is a registry and results database of publicly and privately supported clinical studies of human participants. Because the site only reports clinical trials from 2000 onward, we construct this alternative measure only for patents litigated in the Federal Circuit after 1997.

We use two approaches to match Federal Circuit drug patents with clinical trials. For 94 of the 167 patent cases, we were able to identify the trade name of the drug protected by the patent and the clinical trials related to the active ingredient of the specific drug. For the remaining cases, we collected a set of keywords describing the new drug compound after careful reading of the patent title and abstract. We match the drug patents to subsequent clinical trials based on the appearance of these keywords. Online Appendix 5 provides additional details on the data construction.

Panel B in Table IX presents the estimates of the invalidation effect for our drug patents using these measures of follow-on innovation. To facilitate comparison, column (1) reports the results obtained in our split sample regression with citations as the dependent variable. In column (2) we present the estimate using the number of trials as dependent variable, for the subset of sample patents that we were able to match to a commercialized drug.

The point estimate is positive and very close to one obtained using the citations measure, but again it is statistically insignificant, confirming that patent rights do not block cumulative innovation in drugs. Column (3) shows that results are similar in the extended sample constructed with keyword matching. In Online Appendix 5 we discuss robustness of these results. Across a variety of subsamples and specifications, we find no evidence of a statistically significant effect of invalidation in pharmaceuticals.

Overall, this analysis with product-based measures of innovation confirms our earlier conclusions from regressions based on patent citation data. The analysis also suggests that nonpatent measures are not necessarily superior to patent measures. Despite their limitations, patent citations have the advantage of directly linking each litigated patent with follow-on technologies exploiting information revealed by later patenting innovators (or patent examiners). The nonpatent measures require more subjective choices by the econometrician in making these links. While we explored the robustness of our measures (e.g., collecting a variety of keywords for each patent and linking it to products using different subsets of these keywords—discussed in Online Appendix 5), there is no reason to expect the measurement error in this process to be lower than the one from citations. Moreover, using patent citations to measure follow-on innovation has the advantage of ensuring that we focus on technologies that pass the novelty and nonobviousness requirements for patentability. Any nonpatent measure may also include subsequent products that do not pass this standard. Despite the fact that there is no single dominant measure, the existence of multiple indicators can potentially provide a more informative composite index of the underlying phenomenon of interest (as shown by Lanjouw and Schankerman 2004 in the context of measuring patent quality). This is a potentially fruitful direction for future research.

VIII. Testing Alternative Interpretations

On average, patent invalidation causes a substantial increase in subsequent citations to the focal patent. This result suggests that some licensing deals are not taking place in the presence of patent protection. There are two main reasons this might occur. First, it might be privately optimal for a patent owner to restrict access if licensing reduces joint profits (e.g., because it intensifies downstream competition). Second, information asymmetry and uncoordinated, multilateral bargaining can lead to licensing failures even when such agreements would increase joint profits (and consumer surplus). It is important to distinguish between these explanations because they differ in terms of their implications for welfare and policy.

Our empirical findings suggest that bargaining failure is a significant part of the explanation. Support for this claim is found in the estimated heterogeneous marginal treatment effects. The impact of patent invalidation is concentrated on a small subset of patents, and these have unobservable characteristics that are associated with a lower likelihood of being invalidated (i.e., stronger patents). This suggests the presence of asymmetric information that would be expected to induce bargaining failure in licensing. Moreover, our results help pin down where the bargaining failure occurs. The effect is concentrated in fields characterized by two features: complex technology and high fragmentation of patent ownership. We find no evidence of blocking in noncomplex fields such as chemicals, pharmaceuticals, or mechanical technologies. This reinforces the market failure interpretation, since earlier studies identify fragmentation and complexity as key determinants of licensing breakdown (Cohen, Nelson, and Walsh 2000; Ziedonis 2004).[28]

We interpret our finding that patent invalidation increases later citations by other firms as evidence that the focal patent was blocking innovation by those firms. However, there are three possible reasons for believing that this interpretation of our results may lead us to overestimate the degree to which patent rights effectively block follow-on innovation. Rather than blocking, the postinvalidation increase in citations could reflect: (i) substitution by users from other patents to the focal patent,

28. Our conclusion that patent rights only block in specific environments may be overly optimistic. An alternative explanation for why we do not find blockage in other settings is that patentees are simply unable to enforce their rights effectively. In this case, the R&D incentives for upstream innovators would be diluted, making welfare implications of patent rights more ambiguous. We do not think that this interpretation is plausible for two reasons. First, our sample covers high-value patents whose owners have expended substantial resources to reach the Federal Circuit court, and this does not fit well with an assumption that their patent rights are unenforceable. Second, the concentrated, noncomplex technology fields (including drugs) are the contexts in which we would expect patents to be more easily enforced, but this is where we do not find any blocking effect.

(ii) media publicity, or (iii) strategic citation by downstream innovators. In the remainder of this section we address each of these arguments.

VIII.A. Substitution among Patents

The postinvalidation increase in citations we estimate could be generated by substitution by downstream innovators away from other patented technologies toward the invalidated patent, which is now cheaper to use. However, there are two reasons we think this substitution effect is unlikely to account for the entire increase in citations we estimated. First, our sample comprises highly valuable patents for which litigants spent substantial resources in district court and appellate litigation. It is implausible that such expensive litigation takes place if parties can easily substitute the patented technology with an alternative one. Second, the invalidation effect crucially depends on the characteristics of patentees and citers. We see no statistically significant effect for most size categories, it being concentrated entirely between large patentees and small downstream innovators. This finding is hard to explain with simple technology substitution, since it is not obvious why an invalidated patent should be used as a substitute technology by small innovators only if it is held by a large patentee.

Nonetheless, we explore this issue more constructively by examining whether patent invalidation also leads to a decline in the number of citations to patents that are putative substitutes for the Federal Circuit patent. To this end, we construct a sample of related patents for each litigated patent in our sample. To do this, we use the Google Prior Art software, which is a text-based matching algorithm that identifies and ranks related patents. Online Appendix 6 provides details of the data construction.

We run a series of IV regressions that relate the postdecision citations to the related (substitute) patents, controlling for the endogeneity of invalidation with the same approach as our baseline regression. Table X reports the results. In column (1), the sample is limited to the substitute patents identified as the highest ranked Google match for each Federal Circuit patent (when at least one was identified). Columns (2) and (3) focus, respectively, on the top two and three highest ranked matches for the Federal Circuit patents. In each of these IV regressions, the estimated coefficient on the patent invalidation dummy is negative,

TABLE X

TECHNOLOGY SUBSTITUTION AND MEDIA COVERAGE (IV ESTIMATES)

Dependent variable	(1) log(PostCites)	(2) log(PostCites)	(3) log(PostCites)	(4) log(PostCites)
Invalidated	−0.053	−0.169*	−0.144	0.404**
	(0.112)	(0.101)	(0.092)	(0.196)
Media mention				0.007
				(0.008)
Sample	One related patent	Two related patents	Three related patents	Full
Federal Circuit decisions	699	1,024	1,119	1,357

Notes. * significant at 10%, ** significant at 5%, *** significant at 1%. 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. Media mention is equal to the number of FACTIVA articles referring to case during 1-year window centered on the decision date. In column (1) the sample includes the highest ranked Google match for each of the Federal Circuit patents for which a related patent was identified. In columns (2) (and (3)) the sample focuses on the top two (three) highest ranked matches for the Federal Circuit patents where at least two matches were identified. We add 1 to all citation measures to include patents with no cites.

suggesting that there is some role for the substitution interpretation. However, the point estimates are statistically insignificant in two of the samples, and only marginally significant, at the 10% level, in the sample using two related patents. Even in the latter case, the estimated coefficient is too small to account for the impact of invalidation on citations to the focal patent that we found. The point estimate implies that invalidation of the focal patent leads to a 15.5% reduction in citations to related (substitute) patents, which can explain only one-fifth of the estimated effect of Federal Circuit invalidation on the focal patent.[29] This finding does not necessarily imply that the level of technical substitution is small. It is possible that a decline in citations due to technical substitutability could be compensated by an innovation burst or market expansion effect generated by the court decision which increases citations for both the invalidated patent and related patents. Nonetheless, our objective is to estimate the total

29. Related patents receive only 48% as many citations as Federal Circuit patents (1.2 and 2.5 citations a year, respectively). So a 15.5% decline in citations to each of two related patents translates to a 15% (2 patents $\times$ 0.155 $\times$ 0.48) increase in citations to Federal Circuit patents, which is about one-fifth of the 70% increase estimated in the sample of matched litigated patents.

effect of invalidation on related patents, not to isolate the technical substitution from the market expansion effect.

VIII.B. Media Publicity from Court Decision

The increase in citation after patent invalidation could be driven, at least in part, by publicity associated with the Federal Circuit decision. Our IV 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 data set, which collects press releases in the major international news and business publications. 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). When we add *MediaMentions* to our baseline specification, and estimate using our IV approach, we find that this new variable has no statistically significant effect on citations, and more important, our estimated coefficient on *Invalidated* is very close to the baseline estimate (column (4) in Table X). Moreover, in unreported regressions we also examined whether the effect of invalidation is different for patents that receive greater press coverage, and we find no evidence of this interaction effect. These results strongly indicate that the effect of patent invalidation which we estimate is not explained by media publicity.

VIII.C. Strategic Citation

Finally, the increase in citations caused by patent invalidation could reflect the propensity of small patentees to strategically withhold citations to patents of large firms to stay below their radar screen, rather than a real blocking impact on the underlying innovation by small firms. There are several reasons 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 (Lampe 2012), whereas we find that the effect of invalidation is driven by a postdecision increase in citations by small firms. Second, our measure includes citations both by the patent applicant and those added by 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% increase in citations from small firms—would imply an unreasonably large error rate by patent examiners, especially given that our sample contains well-known 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 IV.

VIII.D. Discussion

In view of the preceding discussion, we interpret our findings as evidence in support of the conclusion that patent rights block follow-on innovation in a few specific technology fields. However, we emphasize that our findings do not imply that removal of patent rights in these areas would necessarily be beneficial. This is because invalidation of one patent in a regime with patent rights is very different from a regime without patent rights.

First, in the presence of patent rights, research is conducted under the expectation of obtaining rents in the form of product market monopoly profits and licensing royalties from follow-on innovators. These rents would be expected to (largely) disappear in a regime without patents and this would reduce, perhaps sharply, incentives to conduct such R&D. Moreover, theoretical models of cumulative innovation show that such policies have ambiguous effects on overall innovation incentives. In models with two generations, weaker patent protection shifts rents toward downstream firms, increasing their incentives but reducing incentives for first-generation research. The role of patent rights is even more ambiguous in a fully dynamic setting, where each innovation is both upstream and downstream at different stages of its life (Green and Scotchmer 1995; Hopenhayn, Llobet, and Mitchell 2006).

Second, economic research has documented that patents play an important signaling role in capital markets and in particular enable small firms to attract venture capital investors more effectively (e.g., Conti, Thursby, and Thursby 2013). Third, we would expect the direction of technical change to be different in a regime without patents. Innovators will have greater incentives to invest in research that can be more easily protected through trade secrets and for which reverse engineering and copying is more difficult. Moser (2005) provides some supporting evidence for this idea using data from nineteenth-century World Fairs. All these issues would need to be part of a broader welfare assessment of patent rights, but this is beyond the scope of the article.

IX. Concluding Remarks

In this article we estimate the causal effect of patent rights on cumulative innovation, using patent invalidation decisions of the U.S. Federal Circuit Court of Appeals. The identification strategy exploits 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, invalidation leads to a 50% 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 blocking effect of patents arising from bargaining failure between upstream and downstream innovators. Third, we find that this effect is concentrated in patents owned by large firms that appear to block small innovators.

While a welfare assessment of patent rights is well beyond the scope of this article, our findings provide good reason to believe that a wholesale scaling back of patent rights may not be the appropriate policy. Patent rights block cumulative innovation only in very specific environments, and this suggests that government policies to address this problem should be targeted. It is preferable to design policies and institutions that facilitate more efficient licensing (such as the biomedical institutions studied by Furman and Stern 2011), which is the key to removing the blocking effect of patents and promoting cumulative innovation.

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Supplementary Material

An Online Appendix for this article can be found at QJE online (qje.oxfordjournals.org).

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