A Patent-Level Measure of Innovation Based on Stock Market Reactions

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Abstract

This study is the first to investigate the applicability and usefulness of a recently introduced patent-level measure of innovation, which relies on stock market reactions to patent grants, in a large-scale cross-country setting. Based on patent data for firms across 27 countries from 1973 to 2013, our results indicate that the patent-level innovation measure is only moderately applicable in a cross-country study: we detect patent-related stock price movements only in 10 countries. Furthermore, although patent value estimates for US, German, and Belgian samples are positively correlated with traditional citation-based measures of quality, we cannot confirm the measure's general usefulness across the 10 countries. On a methodological side, we find that inaccurate isolation of stock market reactions can greatly distort between-country differences in estimated value of innovation. Therefore, we stress careful selection of the stock return window used for construction of this type of measure. Finally, we conclude that better control for heterogeneity in the type of patent grant can improve both applicability and usefulness of the measure.

JEL classification: G14; O3; O4; O5

Keywords: Cross-country study; Event study; Economic Growth; Innovation; Patent values; Stock market reactions; Technological change

1 Introduction

Patent data have long secured their position as the primary source for studies on innovation and technological change (Moser, Ohmstedt, and Rhode, 2015). Indeed, their advantages are vast: patent data cover virtually all fields of innovation across most developed countries over long time periods (Trajtenberg, 1990). Nevertheless, considering the extreme variability in their scientific and economic importance or value (see, e.g., Hall, Jaffe, and Trajtenberg, 2005), patent data can only be informative of innovative output if combined with an appropriate index for patent value. However, here lies the challenge, considering that even the most popular approach of today, which is based on the number of citations that a patent has received made by future patents, exhibits an important practical limitation. Namely, because citation counts are *backward-looking*, they are inherently incapable of measuring the value of recent innovations.

Also, patent citations may indicate the *scientific* importance of the patent rather than its economic value, even though these two concepts could be highly diverged for individual patents (Kogan, Papanikolaou, Seru, and Stoffman, 2015). Hence, concerned with the shortcomings of citation-based measures, construction of a more appropriate measure of patent value remains an important area of research.

This thesis contributes to this area of the economic literature by further investigating the applicability and usefulness of a new measure of innovation, proposed in Kogan et al. (2015), that overcomes both of the issues encountered with citation data. Specifically, the measure in Kogan et al. (2015) exploits stock market reactions to patent grants to estimate the value of individual innovations. Because stock market valuations not only directly measure the *economic* value of the patented innovations but are also *forward-looking*, this type of measure is particularly promising to serve as an effective indicator of innovative output. Although Kogan et al. (2015) elaborately illustrate the usefulness of their innovation measure for their sample of US patents granted to firms listed in the United States, little is known about the applicability or appropriateness of their approach in a cross-country study. This study is designed to fill this gap.

Using patent data for publicly listed firms across 27 countries from 1973 to 2013, we first study the general applicability of the empirical approach to identifying the stock market reaction to patent grants as suggested in Kogan et al. (2015). To do so, we measure the effect of news of patent grants on second moments of stock returns: a significantly positive effect indicates presence of patent-related stock price movements. Given the lack of such significant effects for 17 of the 27 countries in this study, our estimation results indicate that the type of measure in Kogan et al. (2015) is only moderately applicable in a cross-country study. Specifically, our model is incapable of identifying patent-related stock price movements not only in the smaller sized sample in terms of patent counts, but also in South Korea and France, which are both among the largest six countries in terms of patent sample size.

For the 10 remaining countries, we construct our patent-level measures of innovation. Consistent with previous findings in, among others, Harhoff, Scherer, and Vopel (2003b), the distributions of the estimated patent values are highly positively skewed across all countries. To test the usefulness of our measure, we investigate whether our patent value estimates are supported by citation-based measures of patent quality and examine the correlation of yearly aggregated measures of innovation with domestic economic growth. Our findings support the conclusion in Kogan et al. (2015) that this type of innovation measure produces useful results—as indicated by significant correlation with both citation-based measures and economic growth—when applied to a US sample. On the other hand, although hard conclusions require further research, results do not provide statistical evidence that the usefulness generally applies to other countries.

Another contribution of this thesis is that it proposes a number of relevant alterations to the empirical approach suggested in Kogan et al. (2015). In particular, instead of using multi-period returns, we propose to measure stock market reactions based on a set of daily stock returns. By doing so, we can isolate stock market responses more accurately. Empirical comparison of both models shows that this feature is highly relevant: limited ability to isolate the market reaction can severely distort cross-sectional differences in the estimated value of innovation across countries. Finally, analysis of sensitivity to various sample restrictions suggests that the reliability of the patent value estimates could be improved by controlling for heterogeneity in the type of patent grant events. Potentially, this could help overcome the unsuccessful identification of the patent-related stock price movements in countries such as South Korea or France.

The remainder of this thesis is organized as follows. Section 2 reviews the literature, providing an overview of the main approaches to measuring patent values. After summarizing the merging of patent data with stock market data, Section 3 presents the merged data sets for 27 countries. In Section 4, we formally present our approach to constructing patent-level measures of innovation. Section 5 discusses the main empirical procedures and presents the distributions of the patent value estimates. Next, Section 6 examines the correlation of our patent value estimates with citation-based measures of patent quality and with economic growth. In Section 7, we compare our model to the one in Kogan et al. (2015) and discuss the validity of both approaches. Section 8 concludes this thesis by summarizing the main findings.

2 Literature review

This section reviews the literature that is devoted to the construction and validation of indicators of innovative output that rely on patent statistics. First, Section 2.1 describes how patent statistics have found their role within economic research. What we learn is that the promise which naturally lies in patent data—that is, to serve as an objective measure of innovative activity—is initially hindered by the extremely noisy nature of simple patent counts. Seeking to, nevertheless, fulfill the promise, the existing literature displays a wide interest for the estimation of the value or quality of patents. We identify three main approaches to measuring patent values, categorized by type of data that they exploit: patent renewal data, patent citations, and stock market valuations.[1] Section 2.2 evaluates the usefulness and limitations of each type of measure. Finally, we note that we save the discussion of the existing quantitative results for Section 6 to allow for direct comparison with our findings.

2.1 Patent statistics: a resourceful but noisy indicator of innovative output

The large-scale use of patent data in the field of economics goes back to works by Scherer (1965) and Schmookler (1966).[2] Constrained by computational and data resources, these studies, and most related literature that appeared in the pair of decades following, rely exclusively on simple patent counts.[3] Despite the highly valuable contributions that they brought to the literature at the time, the potential economic relevance of simple patent counts has proven to be rather limited. On the one hand, findings within this early literature suggest quite clearly that simple patent counts are related to "inputs" of innovative activity (Trajtenberg, 1990). In particular,

[1] This review focuses on sophisticated statistical measures of the value of patents. Particularly, this implies that we do not cover studies relying on survey data; see, e.g., Scherer and Harhoff (2000) and Giuri et al. (2007).

[2] Scherer (1965) relies on the number of patents issued to a sample of firms on the Fortune 500 list as an indicator of inventive output. In his seminal work, Schmookler (1966) assigns patent count data to industries in an attempt to demonstrate, among other things, the importance of demand as a determinant of inventive activity.

[3] For a survey of the early literature on the use of patent statistics as economic indicators, see Griliches (1990).

Pakes and Griliches (1984) emphasize the strong relationship between contemporaneous R&D expenditures and the number of received patents, both across firms and industries; weaker—though statistically significant—results extend the relationship between R&D and patent counts in the time-series dimension (Griliches, 1990). On the other hand, attempts to correlate simple patent counts with value measures of innovative "output", such as profitability or market value of innovating firms, are rarely successful (Trajtenberg, 1990).

To understand the weak relationship between patent counts and value indicators, one must recognize the large variability in the economic value (or scientific significance) of inventions and the extremely skewed distribution of such invention values (see, e.g., Hall et al., 2005). By ignoring this strong heterogeneity, the simple counting of patents can only render an extremely noisy indicator of innovative output, regardless of the level of aggregation (see, e.g., Trajtenberg, 1990). These limitations are particularly unfortunate because patent data provide a source for analysis of technological progress that is unique in terms of availability, accessibility and granularity (see, e.g., Griliches, 1990). Therefore, in attempts to improve the simple patent count measure, the existing literature has invested great effort in designing sophisticated measures of the *value* or *quality* of patents. Ultimately, such measures can provide appropriate weighting schemes to remove the noise in patent counts. Categorizing the literature by the source of information that is exploited, one can identify three main branches.[4] In a somewhat chronological fashion, the next section discusses the strengths and weaknesses of the central idea of each branch.

2.2 Measuring the value or quality of patents

Renewal fees

The first branch of the patent literature, initially stimulated by Pakes and Schankerman (1984), uses patent renewal data to construct patent value indices. In most countries, patent holders must pay an annual fee to renew the force of their patent, otherwise the patent is permanently canceled (see, e.g., Lanjouw et al., 1998). Since we may assume that renewal decisions are based on economic criteria, patents are only renewed if the value of patent protection is higher than the cost of renewal (Griliches, Pakes, and Hall, 1987). Hence, renewal data are quite directly informative about the value of patent *protection*. Several studies successfully exploit this information by fitting (stochastic) patent return distributions to patent drop-out rates (see, e.g., Pakes, 1986; Lanjouw, 1998). Furthermore, Lanjouw et al. (1998), among others, find that patent weighting schemes based on such estimated distributions of the value of patent protection may substantially reduce the noise in patent counts.

However, in the context of measuring innovative output, we are interested in the value of the *underlying invention* rather than the value of its patent protection. Therefore, we must carefully bear in mind that the usefulness of renewal-based weighting schemes ultimately relies on the assumption that these two concepts are closely related. Although many studies advocate this as-

[4] Introduced in Putnam (1997), an alternative—and sometimes complementary—approach to reduction of noise in patent counts utilizes patent application data. This method relies on the size of patent "families" to produce weighted patent count indices (see also Lanjouw, Pakes, and Putnam, 1998). The patent family refers to the set of patents that is produced when inventors patent the same invention in multiple countries. Studies exploiting such patent families include Harhoff, Scherer, and Vopel (2003a) and Squicciarini, Dernis, and Criscuolo (2013).

sumption (see, e.g., Lanjouw et al., 1998), the discussion in Pakes, Simpson, Judd, and Mansfield (1989) highlights a clear concern: a very valuable invention that is—due to unfavorable patent laws—protected by a weak patent, could be close to worthless. Moreover, renewal data are even limited in their ability to measure the value of patent protection. In particular, renewal data are incapable not only of identifying inventions that are highly valuable for only a few years, after which they become obsolete (Pakes et al., 1989), but also of discriminating between patents that were renewed for the maximum duration of patent protection (Harhoff et al., 2003b).[5] A last disadvantage is that renewal data can disclose the value of patents only long after they are granted; that is, patent value measures based on renewal data are *backward-looking*.

Patent citations

Second, a popular approach to estimating the value of a patent relies on the number of "forward citations", that is, the citations made by future patents to the patent. Patent citations are included in the patent text to disclose how the invention differs from "prior art" (see, e.g., Harhoff, Narin, Scherer, and Vopel, 1999).[6] Thus, citation data naturally provide a means for the researcher to examine the technological significance of patents (see, e.g., Hall et al., 2005). Although the idea to use patent citations as indicators initially draws from their use within the field of bibliometrics, the first economic applications are in Lieberman (1987). Today, citation counts have become the standard measure to control for variation in technological importance among patents (Moser et al., 2015).[7] Moreover, to validate their use as value indicator, the literature has accumulated a body of evidence that clearly suggests a strong correlation between citation counts and the economic value of patents (see, e.g., Trajtenberg, 1990; Harhoff et al., 1999; Hall et al., 2005; Nicholas, 2008; Kogan et al., 2015).[8]

The shape of the relationship is, however, still subject to debate. While most studies suggest that patent quality is a monotonically increasing function of citation counts, analyzing licensing revenue data from non-practicing entities, Abrams, Akcigit, and Popadak (2013) report evidence of an inverted-U-shaped relationship of citations to patent values. Especially, Abrams et al. (2013) caution researchers using samples that (may) include higher value patents, not to use citation-based indices for patent value. Another point of concern is that the scientific and economic value of individual patents could be highly diverged (see, e.g., Kogan et al., 2015), which is worrisome for micro-level studies. Finally, citation data exhibit a practical limitation: similar to renewal data, citation counts are backward-looking and can thus never be used to

[5] By means of telephone and on-site interviews, Harhoff et al. (2003b) investigate the tail distribution of patent protection values and reveal extreme value differences among patents that were renewed for the maximum duration.

[6] For a technical description of the role citations play in the context of patents, see, e.g., Hall et al. (2005). In addition, Harhoff et al. (1999), among others, discuss some differences in citation practices across national patent offices.

[7] Recent studies argue that applicants have strategic incentives to conceal knowledge of prior inventions and therefore attempt to withhold citations (Sampat, 2010; Lampe, 2012). This raises concerns that citations may yield a biased indicator for patent quality. Using field trial reports as an objective measure for scientific importance, Moser et al. (2015) conclude that citation counts are, nevertheless, robustly correlated with scientific significance of patents.

[8] For instance, Hall et al. (2005), Nicholas (2008) and Kogan et al. (2015) report economically and statistically significant positive correlations between citations and patenting firms' stock market valuations. Combining renewal and survey data to measure the economic value of the patent, Harhoff et al. (1999) corroborate these findings. Moreover, Trajtenberg (1990) shows that citation-based patent indices are closely associated with the *social* value of patents as measured by estimated economic surplus gains from patented improvements in CT scanners.

evaluate recent innovations (Hall et al., 2005).

Stock market valuations

Our work contributes to the third and last branch of the literature, which exploits stock market valuations to measure the value of patents. Initiated by Griliches (1981), the first line of research in this branch relates the stock market value of innovating firms to R&D and patent statistics as proxies for their "knowlegde capital". Works of this type include Pakes (1985), Hall et al. (2005) and Nicholas (2008).[9] Apart from insights into the correlation between patent statistics and firm market values, these studies provide valuable estimates of the average growth in firm market value associated with patent arrivals, which imply the mean economic value of patents. On the other hand, the level of granularity in these works is generally not sufficient for construction of patent-level weighting schemes to reduce the noise in patent counts. In an early attempt to overcome this issue, Austin (1993) estimates the value of individual patents by means of a CAPM model that includes patent grant event dummy variables.

Most notable, however, is the study in Kogan et al. (2015). Combining newly collected patent data from Google Patents with stock market responses to the patent grant events, Kogan et al. (2015) construct patent-level estimates of the value of US patents granted to public US firms in the period from 1926 to 2010. The patent-level point estimates then facilitate weighted counting of patents to construct firm-level and economy-wide measures of innovation. A considerable effort in validating its usefulness reveals that the measure proposed in Kogan et al. (2015) is significantly associated with both economic growth and creative destruction, as predicted by Schumpeterian models of endogenous growth, and contains information about the value of innovation that is complementary to what is provided by citation-weighted measures. In addition to their high information content, measures based on stock market data share one important practical advantage: asset prices are *forward-looking* and hence allow for estimation of patent values based on ex-ante information (Griliches, 1990; Kogan et al., 2015).

Nevertheless, there are also some noteworthy downsides of this type of innovation measure. Due to the high volatility of stock market data, reliable measurement of the market's reaction to patent events is difficult (Griliches, 1990). Moreover, even if correctly identified, to give an economic interpretation to the estimated stock price reaction, measures such as proposed in Kogan et al. (2015) require assumptions about the prior beliefs of market participants that are extremely difficult to test (see also Section 4.3).[10]

3 Data construction and description

This paper aims to measure innovation in an international setting by relying on stock price reactions to the news of patent grants. To facilitate this research endeavor, we construct a data set that merges market-adjusted stock returns of companies primarily listed across 27 countries

[9] For a survey of the literature that relates stock market valuations to patent statistics, see Hall (2000).

[10] We note that as this paper is built on the work by Kogan et al. (2015), we provide a detailed discussion on this type of measure in Section 7.

from around the globe with issue dates of patents granted to these companies. Generally, we focus on the period from January 1, 1973, to December 31, 2013, but the available sample period may differ across the countries of primary listings. Table 1 presents an overview of the resulting data set categorized by the country of primary listing and Figure 1 shows the total number of patents in our sample over time.

In Section 3.1 we describe the patent data set and briefly summarize the process of merging the patent data with stock market data—we refer to Appendix A for a technical and detailed discussion of our merging endeavors. Then, in Section 3.2, we present the merged data set used for measuring patent values.

3.1 Patent data and merging procedure

The patent data were extracted from the European Patent Office's (EPO's) Worldwide Patent Statistical Database ("Patstat"), which is the most prominent database of its kind, offering bibliographic patent data from more than 100 patent offices, sometimes as early as the 19th century (de Rassenfosse, Dernis, and Boedt, 2014). Nevertheless, 80% of the patents in our sample were granted by either the US (50.5%), European (12.2%), Korean (6.0%), German (6.0%), or Taiwanese (4.9%) patent office. The extract from EPO's Patstat was previously merged with public companies in Compustat's databases listed across 32 countries.[11] After the merging process and sample restrictions discussed below, 27 countries remain (see also Appendix A.3).

In total, the patent data set contains 2,930,304 successful patent applications, of which 2,623,443 remain after we exclude applications for which the patent grant date cannot be identified.[12] Hence, approximately 10% of the successful patent applications are lost. The distribution of number of lost patents is reasonably even across countries, but the loss of data ranges from around 4% for recent years, to around 30% for early years. The remaining 2,623,443 successful patent applications correspond to 2,651,869 patent grant *events*, as there are occurrences of "shared patents", referring to patents which were filed by (and therefore granted to) multiple companies. Since shared patents typically also induce a separate stock price reaction for each firm, our estimation procedures acknowledge each patent grant event individually. Consequently, estimates based on grant events that correspond to shared patents only partially reflect the full value of the patent. However, because shared patents make up only a very small part of our patent sample, the effect on the distribution of patent value estimates is negligible. Therefore, to simplify linguistics, we hereafter call all of our estimates "patent value estimates", and "patent grant events" are referred to as "patents". Nevertheless, note that *full* patent value estimates could simply be recovered by summation of all partial value estimates that belong to the same patent.

We seek to merge the patent data with financial data. However, due to stock reissues and cross-listings, each company may be linked to multiple stock issues. Therefore, we first construct historical links between patenting companies and their *primary* stock issues—cross-listed

[11] I thank Wing-Wah Tham for kindly sharing the merged extract from EPO's Patstat database. Moreover, I should particularly thank Elvira Sojli for the effort of matching the patents to companies in Compustat.

[12] We also clean the data of suspicious patents such as those which are supposedly already granted in the future.

stock issues are excluded from the sample.[13] This linking procedure is a rather cumbersome process and involves overcoming a number of data complications; Appendix A presents the details. Then, for all of the identified stock issues, we request stock market data from the earliest available date to the end of 2013, the last year that is fully covered by our patent data set. Specifically, we obtain US stock data from the Center for Research in Security Prices (CRSP), and request the financial data from Datastream for all other stocks (see also Appendix A.1). We do not merge the financial series of stocks that belong to the same company. This avoids not only odd price jumps in our series, but also the need of making the unreasonable assumption that the change of issue does not affect the return distribution. Consequently, this study treats each stock each as an individual entity in our empirical analysis. Moreover, to preserve terminological ease, we hereafter simply call stock issues "firm" or "company". We note that this is justified since for the purpose of computing patent value estimates, there is no reason to strictly categorize the data by their unique patenting companies.

Based on the established links between patenting companies and their primary stock issues, we successfully merge 85% of the patent grant events with the requested stock price series—that is, 2,261,266 patent grant events remain after the merging process. The sources of the patent data loss are threefold: (i) not all patenting companies can be linked to (primary) stock price series; (ii) a small portion of the patents are granted outside the available data ranges offered by the financial databases; and (iii) the patent sample includes patents granted to companies at times when they were not publicly traded. Although the overall rate of data loss is tolerable, we are concerned with varying degrees of data loss through time and across countries for the sake of comparability within and between aggregated, country-level measures of innovation. Figure A.1 and Table A.1 in Appendix A.3 display the distribution of the lost patents over time and across countries, respectively. We conclude that merging success rates are fairly constant over time from 1973 to 2013 and across the very large majority of the countries. Hence, we restrict our sample to the period from 1973 from 2013.

3.2 Data description: variables and final data panels

The construction of patent value estimates requires *daily* observations of three main variables: (i) stock returns excluding dividends, (ii) market returns excluding dividends, and (iii) market capitalization. For US stocks, we use the returns (excluding dividends) on CRSP's value-weighted market portfolio as the market returns; for all other stocks, we calculate market returns based on Datastream's Global Equity Indices, which cover a minimum of 75% of total market capitalization for each market. We request *all* market capitalization series in US dollars such that all of our patent value estimates share the same currency and are therefore easily comparable. Furthermore, we note that a firm's market capitalization is naturally calculated on security level and might therefore understate the total value of the firm.

[13] Cross-listed stocks are not considered because historical lists of all cross-listed stocks that belong the same company, correctly adjusting for events such as mergers, acquisitions or reissues of stocks, are very rarely available. Furthermore, considering cross-listed stocks leads to the inclusion of the same patent grant events in more than one of our panel models and consequently to multiple value estimates for the same patent, making correct calculation of one single patent value estimate unclear.

Table 1: Summary statistics for the 27 country-specific data panels. Panels are ordered by the number of observed patent grants. The left part reports counts of observed patent grants within the sample of each panel. The middle part reports the years which contain the 1st and 50th percentiles of the patent grant dates; for all countries, the 99th percentile is in year 2013. The right part presents the panel characteristics of the financial data. The maximum sample period considered is from January 1, 1973, to December 31, 2014.

	Patents		*Grant date distribution*		*Panel size*	
Country	Frequency	Cum. rel. freq. (%)	1st Percentile	Median	No. of firms	Length in days
United States	853,594	(40.9)	1976	2003	3,406	10,345
Japan	599,021	(69.6)	1979	2005	1,293	10,122
Germany	224,823	(80.3)	1973	2001	368	10,336
South Korea	94,424	(84.9)	1995	2007	374	6,477
Taiwan	78,103	(88.6)	1998	2008	535	6,424
France	69,961	(91.9)	1976	2005	261	10,470
Switzerland	35,661	(93.7)	1974	2004	85	10,338
United Kingdom	26,927	(94.9)	1974	2002	340	10,424
Finland	20,985	(95.9)	1991	2008	69	6,486
Netherlands	20,736	(96.9)	1974	2001	45	10,486
Sweden	14,307	(97.6)	1985	2004	139	8,022
Denmark	12,013	(98.2)	1987	2005	45	10,496
Italy	7,477	(98.6)	1974	2002	94	10,432
Belgium	7,214	(98.9)	1975	2002	34	10,562
Canada	5,512	(99.2)	1973	2003	173	10,347
China	4,068	(99.4)	2003	2009	40	5,475
Norway	3,161	(99.5)	1983	2004	63	8,680
India	2,136	(99.6)	1997	2009	108	5,902
Austria	1,736	(99.7)	1975	2004	39	10,450
Australia	1,647	(99.8)	1980	2008	129	10,419
Singapore	1,530	(99.9)	1998	2011	28	7,901
Israel	1,123	(99.9)	1995	2010	60	5,305
Spain	694	(99.9)	1989	2010	36	6,748
New Zealand	586	(100.0)	1998	2006	12	6,536
Brazil	368	(100.0)	1995	2010	36	4,826
South Africa	202	(100.0)	1974	1993	17	10,323
Hong Kong	136	(100.0)	2003	2009	16	7,200
Total	2,088,145		1976	2004	7,845	10,562

Notes: "country" refers to the country where the patenting company is primarily listed; the number of "firms" factually represents the number of stock series, each of which we treat individually for our analysis; the number of "patents" factually represents the number of patent grant events, which means that one patent may be counted multiple times if also granted to multiple firms; "days" refers to trading days.

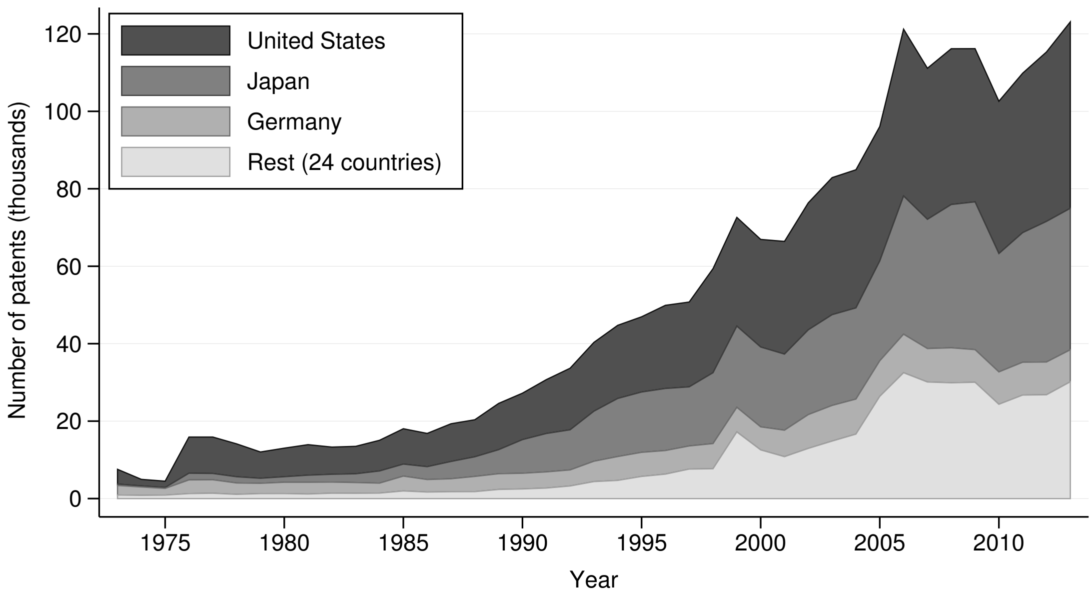


Figure 1: The yearly number of patent grant events for the entire merged data set in the period 1973–2013, highlighting the largest three contributors to the global patent stock.

The availability of the required financial variables further restricts the sample of patents. Specifically, we restrict our sample to patents for which we have non-missing values for the market-adjusted stock returns and market capitalizations on the days following the grant day and for which we can calculate return volatilities. Also, we restrict the sample to stock markets in which we observe at least 50 patent grant events. After we lose 7.7% of the patents due to these restrictions, we obtain the final sample of 2,088,145 patents. Hence, in sum, 79% of 2,651,869 identified patent grant events in the extract from EPO's Patstat remain. The majority of excluded patents are due to regular data gaps: 5.5% of the merged patents are granted on weekends or trading holidays.[14] In an attempt to overcome this, we experiment with shifting patents granted on weekends or trading holidays to the next trading day. However, because we find only weak stock price movements for these shifted grant events and do not want to add more noise to our model, we exclude all patents granted on non-trading days.

The final data set consists of 27 *separate* data panels, organized by the country where the patenting company is primarily listed, to allow for country-specific model selection and estimation. Table 1 provides summary statistics for each country panel. Clearly, the cross-country

[14] The topic of regular data gaps requires special attention. Identification of trading holidays is complicated because historical business calendar series are often not available. Specifically, for US stock data, CRSP does not offer trading day series. However, the high data quality offers an alternative: we simply deduct the trading day calendar from the missing observations in the sample. For all other markets, our first attempt is to rely on the "VACS" time series provided by Datastream, which mark exchange closure days. However, roughly speaking, the VACS series are not available in the first 10 years of our sample period. Therefore, for the years where the "VACS" series are missing, we attempt to deduce the trading day calendars from the data. As for US stocks, the general approach is to mark days as trading holidays when the data are missing for all series. However, for some countries we take an approach more lenient towards holidays: we consider all days for which more than 95% of our series are missing to be trading holidays. This lenient approach is designed to generate an annual number of trading holidays that is in line with the market-specific yearly average of the "VACS" series. We argue that this procedure is sensible because Datastream seems to unsolicitedly fill some of the (regular) data gaps with the last available observation. Finally, we should note that for early years and/or countries that suffer from relatively poor stock market coverage, our deductive methods are prone to error due to an increased number of *irregular* data gaps.

sample is strongly concentrated. Distinctly, the United States, Japan, and Germany are the largest contributors to the global patent stock, making up a little over 80%. Moreover, the largest 10 countries supply 97% of the total patent stock. The distribution of patent grants over time varies greatly across countries: for some countries the data set already contains significant numbers of patent grants in the 1970s, for others not until the 1990s. A common observation is, however, that around half of the patents were granted in the last three to 12 years within each country. Consequently, more than 50% of the observed patent grant dates are concentrated in the last 10 years. The same effect is also illustrated by the stark, upward trend in the overall yearly count of patent grants in Figure 1: from less than 20,000, the number of observed patents granted each year has increased to over 100,000 since 2006. The country-specific financial data panels consist of all firms to which at least one patent is granted on a trading day within our sample period. As expected, the number of observed patent grants is strongly correlated with the number of patents. The beginning of the sample period varies between January 1973 and July 1994; for all country panels, the end of the sample period is December 31, 2013.

We append the data set with monthly consumer price index (CPI) rates obtained from the Federal Reserve Bank of St. Louis (series name: "CPIAUCNS", base year: 1982–1984) to make comparison of patent value estimates possible over time. Furthermore, we download annual gross domestic product (GDP) per capita series from 1973 to 2014 from the World Bank. Since the World Bank does not list Taiwan as a separate country for its World Development Indicator, we download annual the GDP per capita series for Taiwan from EconStats, IMF, World Economic Outlook. All GDP per capita series are at annual frequency and in constant local currency units. Finally, we note that the extract from EPO's Patstat also contains information about the number of times a patent is cited by other patents within the sample of EPO's Patstat up to 2013.

4 Model specification

To construct a patent-level measure of innovation, we seek to model stock price movements related to news of patent grants. This approach is appealing because asset prices record the value of the expected future cash flows generated by innovation quite immediately in response to news regarding the firm's innovative activity (see also Griliches, 1990). Hence, the use of financial data allows for construction of patent value estimates based on ex-ante information (Kogan et al., 2015). Moreover, as opposed to patent citation data, stock market valuations are directly informative about the economic value of patents (see also Section 2.2).

Our approach to estimating the value of patents is built on the innovation measure proposed in Kogan et al. (2015). This choice was strongly inspired by the evidence reported in Kogan et al. (2015) that indicates the substantial usefulness of their measure in addition to its high information content. Although the underlying idea of the measure remains the same, we choose to make some important alterations to the econometric model configurations in Kogan et al. (2015). For the sake of transparency, this section briefly points out where our model deviates. The real discussion is, however, saved for Section 7.1, where we critically examine the significance and implications of these adaptations.

In the remainder of this section, we formally present the model that we use to construct patent value estimates. First, Section 4.1 decomposes stock returns into a patent-related component, the stock market responses to patent grants, and a noise component. Based on distributional assumptions, we then derive an expression for the expected value of the patent-related stock return conditional on observed stock price movements. Second, Section 4.2 specifies the panel regression model that we estimate independently for each country-specific data panel to measure the essential signal-to-total-variance ratio. Also, we discuss the estimation of the variance of the noise component. Third, Section 4.3 explains how to recover patent value estimates from conditional expectations of the patent-related stock price movements.

4.1 Patent-related stock returns

Stimulated by the main idea presented in Kogan et al. (2015), we exploit information contained by stock price movements to estimate the economic value of patents. To do so, we study a narrow *event window* of the three *trading* days, $[d, d+2]$, following *patent grant events*, referring to the dates when investors learn that patents are granted. We choose for the three-day event window because we do not find significant stock price reaction on days later than two days after the patent was granted. Importantly, however, the rate at which investors process (patent) information may vary across countries. In fact, we generally only observe significant reactions in a country-dependent, closed subset of the grant event window. Consequently, we construct the patent value estimates based on only on the days for which we find (weakly) significant market responses (see also Section 5.2). To not overly complicate the specification of our model, the remainder of this section is, nevertheless, built on the "base case scenario", that is, using a three-day event window.

We choose to focus on patent grant events because we can consistently observe them across patenting nations. Moreover, studying patent grant events allows us to capture discrete shocks to the market's information set that are all induced by a homogeneous set of events. In contrast, other available, distinct patent events may vary greatly in type and thus in impact on the investors' set of knowledge about patents. Such information events are, therefore, not eligible to measure the economic value of a patent in a comparable fashion across countries.[15]

Hence, we seek to isolate the component of stock price movements that is related to the discrete change in the market's set of information about a patent due to its acceptance. However, even within a narrow window of trading days following patent grant dates, stock returns are exposed to both market forces and firm-related news items other than the patent issuance; our model accounts for this explicitly. First, to remove market movements, we use *log market-adjusted returns* r, defined as the firm's *log* return minus the *log* market return. Hereafter, we simply say (stock) returns to refer to log returns. We follow Kogan et al.'s (2015) suggestion to use the

[15] In addition to patent grant events, another potential homogeneous set of information events could consist of application publications: most national patent offices officially publish patent applications 18 months after filing. However, before the American Investors Protection Act became effective in November 2000, US patent applications were not officially published prior to patent grant dates. For this reason, we do not consistently observe application publication events throughout our sample. Besides, Kogan et al. (2015) examine stock market responses to application publications and find only weak stock price movements. Another candidate event could be application filing dates; however, patent offices generally do not officially publish applications at the time they are filed (see also Kogan et al., 2015).

"market-adjusted-return model" (Campbell, Lo, and MacKinlay, 1997, p. 156).[16] By doing so, we avoid a source of measurement error that could result from estimating each firm's stock market beta. However, the drawback of the "market-adjusted-return model" is that it implicitly assumes that all firms have the same amount of systematic risk. Testing the robustness of their results to relaxation of this assumption, Kogan et al. (2015) report that their estimates of the value of US patents are quantitatively similar when using an unrestricted market model allowing for firm-dependent market betas.

Second, to explicitly account for fluctuations of market-adjusted returns for reasons unrelated to the value of the patent, we decompose the daily market-adjusted returns r_{jl} on the lth day after patent j was granted to a given firm as follows:

$$r_{jl} = s_{jl} + \varepsilon_{jl}, \qquad l \in \{0, 1, 2\}, \tag{1}$$

where s_{jl} denotes the component of the firm's stock return that is due to the market response to the grant of patent j, and ε_{jl} denotes the component of the firm's stock return that is unrelated to the patent. We note that Kogan et al. (2015) propose a similar decomposition but apply it to (simple) three-day cumulative returns instead of daily returns. However, by considering each day in the event window independently, we are able to identify the timing of the stock market reaction more accurately and thereby better isolate the patent-related component of return. Moreover, there are some important implications of directly studying multi-period returns for the estimation of the patent-related stock return. Section 7.1 provides a detailed discussion.

Given the observed stock returns in the event window, $r_j = \sum_{l=0}^{2} r_{jl}$, we are interested in the total stock market reaction to the grant of patent j, $s_j = \sum_{l=0}^{2} s_{jl}$. Notice that r_j thus denotes the three-day cumulative return after grant of patent j. To derive an expression for the conditional expectation $\mathrm{E}[s_l|r_l]$, we introduce several distributional assumptions. We allow the second moments of both s_{jl} and ε_{jl} to be firm-f- and year-t-dependent. Also, the second moment of the stock market reaction s_{jl} may vary across days l of the patent grant event window. Hence, $\mathrm{E}[s_{jl}^2] = \sigma_{s_l ft}^2$ and $\mathrm{E}[\varepsilon_{jl}^2] = \sigma_{\varepsilon ft}^2$. Furthermore, we assume that s_{jl} and ε_{jl} are independently distributed and are serially uncorrelated.[17] Therefore, we can write $\mathrm{E}[s_j^2] = \sum_{l=0}^{2} \sigma_{s_l ft}^2$ and $\mathrm{E}[\varepsilon_j^2] = 3\sigma_{\varepsilon ft}^2$. Since the market value of a patent is non-negative, we assume that s_j follows a normal distribution truncated at zero, $s_j \sim \mathcal{N}^+(0, \sum_{l=0}^{2} \sigma_{s_l ft}^2)$.[18] Notice that the model thus allows for stock price corrections as s_{jl} may be negative, given that the sum s_j is non-negative. Lastly, the noise term is normally distributed, $\varepsilon_j \sim \mathcal{N}(0, 3\sigma_{\varepsilon ft}^2)$. We note that our distributional assumptions follow the example set in Kogan et al. (2015), extended to the setting of daily returns.[19]

[16] The use of *log* returns deviates from Kogan et al. (2015) as their model suggests the use of simple returns. The model outlined in this paper relies on the characteristic of log returns that multi-period returns are the sum of single-period returns. Moreover, log returns allow for more reliable inference since they are more consistent with a normal distribution than simple returns (see Fama, 1976, ch. 1).

[17] The assumption of no autocorrelation is in line with the stylized facts of asset returns (see, e.g, Taylor, 2007, ch. 4).

[18] We note that the second moment of a mean-zero normal distribution, truncated at zero, is (simply) equal to σ^2 as its mean and variance are equal to σc_0 and $\sigma^2(1 - c_0^2)$, where $c_0 = \phi(0)/\Phi(-0)$.

[19] Estimating US patent values, Kogan et al. (2015) experiment with various distribution assumptions for s and ε and find comparable results for all distributions. In particular, Kogan et al. (2015) experiment with using a non-zero mean parameter for the truncated normal distribution of s. Because daily return data do not support reliable estimation of

Given the distributional assumptions, the expectation of s_j conditional on r_j equals:

$$\mathrm{E}[s_j|r_j] = \delta_{ft} r_j + \sqrt{3\delta_{ft}}\sigma_{\varepsilon ft}\lambda\left(\sqrt{\delta_{ft}}\frac{r_j}{\sqrt{3}\sigma_{\varepsilon ft}}\right), \tag{2}$$

where δ_{ft} is the *signal-to-total-variance* ratio,

$$\delta_{ft} = \frac{\sum_{l=0}^{2}\sigma^2_{s_l ft}}{\sum_{l=0}^{2}\sigma^2_{s_l ft} + 3\sigma^2_{\varepsilon ft}}, \tag{3}$$

and $\lambda(z) = \phi(z)/\Phi(z)$ is the inverse Mills ratio, with ϕ and Φ the probability density function and the cumulative distribution function of the standard normal distribution. Notice that "signal" refers to the component of r_j related to the value of patent j, that is, s_j. Appendix B.1 derives the conditional expectation formulated in (2).

Thus, to compute the conditional expectation of s_j given r_j, we need to estimate the distribution parameters $\sigma_{s_l ft}$ and $\sigma_{\varepsilon ft}$. However, if we allow both variance parameters to vary freely across firms and time, the number of parameters becomes too large to estimate (see also Kogan et al., 2015). To overcome this, $\sigma_{s_l ft}$ and $\sigma_{\varepsilon ft}$ vary in constant proportions for each day l within the event window.[20]

This restriction implies that $\sigma_{s_l ft}$ can be written as a linear function of $\sigma_{\varepsilon ft}$ (or vice versa) and that the signal-to-total-variance ratio is constant across firms and years, that is, $\delta_{ft} = \delta$. For the derivation of these algebraic results, we refer to Appendix B.2. Hence, instead of estimating $\sigma_{s_l ft}$ and $\sigma_{\varepsilon ft}$ individually, we directly estimate δ.

4.2 Estimating the signal-to-total-variance ratio and the variance of noise

The signal-to-total-variance ratio δ can be derived from the ratio of the sum of second moments of returns on days *unrelated* to patent grants, $\mathrm{E}[\varepsilon^2_{fd}] = \sigma^2_{\varepsilon ft}$, to the sum of second moments of three-day returns after patent grants, $\mathrm{E}[(s_{l,fd} + \varepsilon_{fd})^2] = \sigma^2_{s_l ft} + \sigma^2_{\varepsilon ft}$. The subscript "$fd$" replaces the subscript "jl" used in Section 4.1 because we no longer restrict our attention to days after patent grants, but rather consider any daily market-adjusted return r_{fd} of firm f on day d. Nevertheless, the subscript l remains for the patent-related component of return, $s_{l,ft}$, to discriminate between days within grant event windows. Since the assumption of proportionality discussed in the previous section implies that $\sigma^2_{s_l ft}$ is a linear function of $\sigma^2_{\varepsilon ft}$, the effect of the news of a patent grant on the second moment of returns is multiplicative. In order to estimate this multiplicative effect and ultimately δ, we regress the log squared daily market-adjusted

a firm- and/or year-dependent mean of s, Kogan et al. (2015) assume an unconditional mean that is constant across firm-years. As a result, allowing for a non-zero mean parameter of the distribution s mostly has a scaling effect on their estimates, with correlations over 99% with the estimates based on the zero-mean truncated normal.

[20] The drawback of assuming that $\sigma_{s_l ft}$ is proportional to $\sigma_{\varepsilon ft}$, is that the patent value estimates become an increasing function of the variance of the noise component of returns. That is, given the same return response, our measure yields a higher estimate of the value of the patent if granted to a firm with higher volatile returns. This effect occurs because the mean of a normal distribution truncated at zero is increasing in its variance parameter. The argument that fast-growing firms have more volatile returns and could produce more valuable inventions, provides some economic justification to the positive relation between volatility and our patent value estimates. Section 7.1 revisits this issue.

returns r_{fd} on a patent grant dummy variable, I_{fd}:

$$\log\left(r_{fd}^2\right) = \sum_{l=0}^{2} \gamma_l I_{f,d-l} + \sum_{i=1}^{5} \theta_i D_{id} + c_{ft} + \zeta_{fd}, \tag{4}$$

where D_{id} for $i \in \{1, \ldots, 5\}$ denote day-of-the-week dummy variables, c_{ft} is a firm-year specific effect for firm f in year t, and ζ_{fd} is an error term that is i.i.d. over f and d. The lagged grant dummy variables measure the stock market reactions up to second day after the patent grant. The day-of-the-week dummy variables control for the day-of-the-week effect on stock market volatility which has been reported in the literature (see, e.g., Kiymaz and Berument, 2003).[21]

We perform estimation of model (4) independently for each country-specific data panel (see Section 3.2 for an overview of these panels). Country-specific estimation is desired because stock return distributions may vary across markets. In particular, stock price reactions to patent grants may be more prevalent in stock returns in some countries than in others and could display a different timing. Both of these suspicions advocate country-specific estimation of γ_l. Furthermore, we note that model (4) presents an important deviation from the specification in Kogan et al. (2015, p. 11). Namely, as previously noted in Section 4.1, the approach in Kogan et al. (2015) relies on studying three-day cumulative returns at once and consequently involves the regression of three-day cumulative returns on a single patent grant dummy variable. In Section 7.1 we provide a detailed discussion of this issue.

Using the assumptions on $s_{l,fd}$ and ε_{fd}, the signal-to-total-variance ratio δ can be expressed as an increasing, convex function of the sum of the regression coefficients γ_l, $l \in \{0, 1, 2\}$, in (4):

$$\delta = 1 - \frac{3}{\sum_{l=0}^{2} e^{\gamma_l}}. \tag{5}$$

We refer to Appendix B.2 for the derivation of (5). Hence, for our estimate $\widehat{\gamma}$ we can compute $\widehat{\delta}$. Again, we deviate from Kogan et al. (2015, p. 11), who specify a relation between δ and γ that suggests a false interpretation of γ (see also Appendix B.2). In Section 7.1 we discuss this issue in detail and examine its impact on the empirical results.

The last parameter in (2) that we need to estimate for computation of the conditional expectation of s_j given r_j, is the variance parameter of the noise component of returns, $\sigma_{\varepsilon ft}$. For this, we take a non-parametric approach by calculating the second sample moment of returns of firm f in year t. However, since we estimate the second moment over both days related and unrelated to patent grants, it is a mixture of $\sigma^2_{s_l ft}$, $l \in \{0, 1, 2\}$, and $\sigma^2_{\varepsilon ft}$. Based on our estimates of γ_l we recover the $\sigma_{\varepsilon ft}$ from the second moment of returns:

$$\sigma^2_{\varepsilon ft} = \frac{\mathrm{E}[r_{fd}^2]}{1 + \sum_{l=0}^{2} d_{l,ft}(e^{\gamma_l} - 1)}, \tag{6}$$

where $d_{l,ft}$ denotes the fraction of trading days that concern a day l in a grant event window

[21] Wald tests strongly indicate presence of time-specific effects, with p-values below 0.01 for the large majority of our panels. Hence, the statistical evidence supports the hypothesis of the day-of-the-week effect on stock market volatility.

for firm f in year t. In Appendix B.3 we derive the result in (6).[22] We estimate the second sample moment over all days, both patent-related and -unrelated, because in some years, for some countries, a significant share of the firms experience multiple patent grant days per week. In such cases, estimation only over days unrelated to patent grants is unreliable, if not infeasible.

An important observation here is that the estimate of $\sigma_{\varepsilon ft}$ is not conditioned on the day of the week, even though the day-of-the-week dummy variables in model (4) imply that the second moments of the market-adjusted stock returns are a function of the day of the week. We take this approach for two reasons. On the one hand, day-of-the-week dummy variables are necessary to prevent omitted-variable bias since patent grants are typically not evenly distributed over the days of the week.[23] On the other hand, we pool all days of the week to ensure sample sizes that allow for reliable estimation of $\sigma_{\varepsilon ft}$. Nevertheless, our estimate of δ is valid also when applied in a setting where the second moments of returns are not allowed to vary across the days the of the week. The validity is because our estimates of γ_l for $l \in \{0, 1, 2\}$, and consequently of δ, are constant across the days of the week as the effect of the news of a patent grant on the second moment of returns is multiplicative.

4.3 Recovering patent value estimates

We relate the conditional expectation of the patent-related stock return in the event window, $\mathrm{E}[s_j|r_j]$, to the market value of patent j. First, we calculate the dollar value of the stock market response to a patent grant event, ΔS_j using the market capitalization M_j is of the firm that is granted patent j, on the day prior to grant day ($l = 0$):

$$\Delta S_j = \frac{1}{N_j}\left[\exp\left(\mathrm{E}\left[s_j|r_j\right] + \frac{1}{2}\mathrm{Var}\left[s_j|r_j\right]\right) - 1\right]M_j, \tag{7}$$

where N_j is the number of patents granted to the same firm on the day patent j is granted. Hence, we account for coinciding patent grants by assigning only a fraction $1/N_j$ of the patent-related stock price movement to each patent.[24] The term $\frac{1}{2}\mathrm{Var}[s_j|r_j]$ corrects for the convexity of logarithmic functions, and the conditional variance is expressed as,

$$\mathrm{Var}\left[s_j|r_j\right] = 3\delta_{ft}\sigma^2_{\varepsilon ft}\left[1 - \kappa\left(\sqrt{\delta_{ft}}\frac{r_j}{\sqrt{3}\sigma_{\varepsilon ft}}\right)\right], \tag{8}$$

where $\kappa(z) = \lambda(z)[\lambda(z) + z]$, and (again) $\lambda(z) = \phi(z)/\Phi(z)$ is the inverse Mills ratio. We refer to Appendix B.1 for the derivation of (8). Notice that we can estimate $\mathrm{Var}[s_j|r_j]$ based on our estimates of δ and $\sigma_{\varepsilon ft}$ discussed in the previous section.

The second step relates the stock price response ΔS_j to the economic value V_j of patent j. Since ΔS_j only reflects the uncertainty about the patent application's success that is resolved

[22] Also here we deviate from Kogan et al. (2015, p. 11). Although Kogan et al. (2015) also recover the variance of the noise component of returns using their estimate of γ, their relation seems to again rely on a false interpretation of the coefficient γ.

[23] For example, the USPTO issues patents on Tuesdays except if there is a federal holiday.

[24] Based on the full sample, a patenting firm is granted 2.71 patents on each grant day.

when the patent is granted, the stock market response ΔS_j understates the value of the patent. Specifically,

$$\Delta S_j = (1 - \pi_j) V_j, \tag{9}$$

where π_j is the investors' assessment of the ex-ante probability that the application of patent j will be accepted. Hence, in anticipation of the grant of patent j, the market value of the patenting firm already includes $\pi_j V_j$. Accounting for the understatement of ΔS_j and using (7), we derive the following expression for the economic value V_j of patent j:

$$V_j = (1 - \pi_j)^{-1} \frac{1}{N_j} \left[\exp\left(\mathrm{E}\left[s_j | r_j\right] + \frac{1}{2} \mathrm{Var}\left[s_j | r_j\right] \right) - 1 \right] M_j. \tag{10}$$

Thus, the last remaining parameter to construct V_j is the investors' assessment of the ex-ante patent acceptance probability π_j. In general, we estimate the ex-ante acceptance probability conditional on time based on observed acceptance rates in our patent sample. Our estimates of $\widehat{\pi}_t$ range from 0.54 to 0.64 over time. However, restricted by data availability, we use $\overline{\pi} = 0.56$ as the unconditional probability of patent acceptance for US patents. We formally address the computation of $\widehat{\pi}_t$ in the next section.

Important to note is that equation (9) implicitly assumes that the market value of patent j, V_j, is fully known to investors prior to its grant date. We argue that the assumption is reasonable because generally, the patent office officially publishes the full patent application text 18 months after filing. Hence, investors typically have full knowledge about the content of the patent before its grant date. The one important exception is the United States, where the 18-month-disclosure policy was introduced by the American Investors Protection Act (AIPA) only in November 2000. Before AIPA, the US Patent and Trademark Office (USPTO) exclusively disclosed patent applications on the patent grant day. However, as firms often announce new products and the associated patents themselves, anecdotal evidence suggests that investors are typically informed about patents before their grant dates, even in absence of their formal publications. Furthermore, Kogan et al. (2015) empirically assess this claim and conclude that the information content revealed on the publication date may be only small.[25] Thus it appears safe to assume that investors possess advance knowledge about the patent value.

Nevertheless, publication of a patent grant could include new information about the patent that affects market beliefs. For example, the patent office may require changes to the content before they accept the patent (Putnam, 1997). If such changes are related to the economic value of the patent, investors will update their beliefs V_j. Consequently, the expression of ΔS_j in (9) would include the additional term $\pi_j(V_j - \widetilde{V}_j)$, where $\widetilde{V}_j$ is the belief about the patent value prior to the grant date (see also Kogan et al., 2015). Based on the reasonable assumption that

[25] For their US-only patent sample, Kogan et al. (2015)—who also make the assumption that the value of the patent is observed before it is granted—investigate whether AIPA's introduction of patent application publications 18 months after filing has impact on their estimates of the signal-to-total-variance ratio: Kogan et al. (2015) report an increase of the estimated signal-to-total-variance ratio for patents filed after AIPA's effective date but this difference is not significant. Therefore, Kogan et al. (2015) conclude that the increase in information available to investors prior to patent grant dates due to official publication of patent applications may be only small.

$(V_j - \widetilde{V}_j)$ is uncorrelated with V_j, the decomposition of stock returns as in (1) is still valid. On the other hand, estimation of the conditional expectation of the patent-related stock return s_j—based on the signal-to-total-variance ratio δ—is no longer reliable as model (4) will also measure the stock price movements related to $\pi_j(V_j - \widetilde{V}_j)$. As a result, our estimates of δ would be too high. Unfortunately, as there is no data available about the prior beliefs of investors, the best we can do to adjust for the presence of $\pi_j(V_j - \widetilde{V}_j)$ in the market response ΔS_j is to scale our estimates of δ down. In fact, given the lack of data, the only sensible choice would be to use the same scaling factor for all countries. However, by applying equal scaling factors, the relative cross-sectional differences in value across patents remain approximately unchanged, and we thus do not affect our results in a statistically meaningful way. Therefore, we choose to not alter our estimates of the signal-to-total-variance ratio δ.

5 Estimation, selection, and construction of patent values

In this section, we discuss the empirical procedures that are required to measure patent value and subsequently present the patent value estimates. Specifically, Section 5.1 addresses the computation of the ex-ante probability of patent acceptance. Next, Section 5.2 deals with the estimation of model (4) for the 27 country-specific panels in our sample, from which we then draw two important results. First, the days for which we observe significant stock price reactions to patent grants define the country-specific grant event windows used for the construction of patent value estimates. Second, based on the estimated regression coefficients, we construct country-specific estimates of the signal-to-total-variance ratios using (5). Lastly, Section 5.3 presents the distribution of the constructed patent value estimates across countries.

5.1 Estimation of the ex-ante probability of patent acceptance

Derivation of patent value estimates from the measured market reactions to patent grants requires knowledge about investors' expectations about the acceptance of the patent. Specifically, the market's assessment of the ex-ante patent acceptance probability π_j determines the factor by which the stock price response to a patent grant understates the value of patent j (see Section 4.3). Therefore, to account correctly for this understatement, we seek to mimic the patent market conditions that investors perceive and on which they rely in their assessment of π_j. To do so, we compute patent acceptance rates within our patent sample to estimate π_j.

Computation of patent application acceptance rates in our extract from the EPO's Patstat database reveals variation both across time and publication authority. Nevertheless, we choose to construct estimates of the patent acceptance probability conditional on time only, that is, based on five-year moving averages. This choice is guided by concerns about the reliability of the patent application data: little is known about the extent to which *unaccepted* patents our data set records. We are especially concerned about systematic differences across publication authorities in the way unaccepted patents appear in our data set: grant rates across different

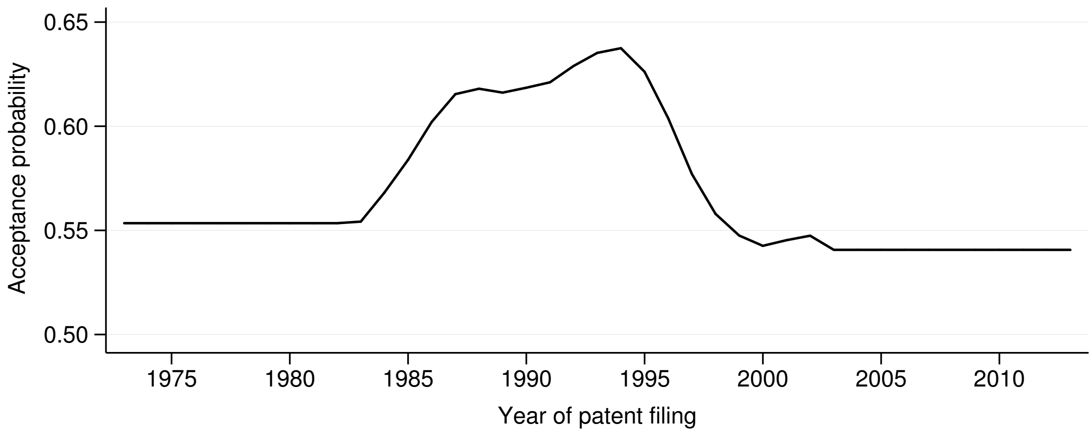


FIGURE 2: Estimates of conditional ex-ante patent acceptance probabilities for non-US patents, based on the average acceptance rates within five-year time windows centered at the year of filing of the patent.

patent offices are strongly dispersed with a significant degree of polarization.[26] Since it is unreasonable to assume that the variation can be explained by changes in patent acceptance policies, we conclude that the dispersion across patent offices rather captures varying degrees of selection bias than developments in patent climates. Therefore, we choose to condition our estimates of π_j only on time.

In general, we estimate the patent acceptance probability π_j based on observed acceptance rates for all patents filed within a five-year time window centered at the year of patent j's filing.[27] The reason why we take this approach is two-sided. First, when investors receive news about a patent filing, their beliefs about the probability of success of patent j are based on the current patent market conditions. These market conditions can be best mimicked by the acceptance rates of patents filed *around* the time that patent j was filed. Therefore, we use a five-year estimation window *centered* at patent j's time of filing. Second, we consider filing dates because only they can be used to compare successful applications with those that are unsuccessful: other publication dates may be correlated with the patent office's acceptance decision.

There are, however, three motives to deviate from our general estimation method. First, since our patent data is truncated in the beginning of 2014, we cannot reliably estimate patent success rates for recently filed patents. Specifically, considering that 95% of the successful patent applications are granted within seven and a half years after filing, we conclude that yearly acceptance rates for patents filed after 2006 are affected by the truncation. Therefore, considering that our estimates are based on five-year moving averages centered at the year of filing, the last reliable estimate is for patents filed in 2003. Second, our patent sample exhibits a steep increase in yearly acceptance rates before 1980. This raises doubts about the reliability of our estimates

[26] To test our concerns about differences in the way unaccepted patents are recorded across patent offices, we calculate office-specific patent acceptance rates for five-year time windows. The results reveal not only large variability, but also strong polarization: our estimates equal 0 for 12% of the cases and 1 for another 39%. These results give reason to believe that our estimates are affected by varying sizes of selection bias and are thus unreliable.

[27] For approximately 0.5% of the successful patent applications, the filing date is missing. In these cases, we estimate filing dates based on the average length between filing and granting dates for the relevant publication authority. Note that there is margin for error here because we are only interested in the *year* of filing, opposed to the exact date.

for this period in our patent data set.[28] Thus, again accounting for the five-year window, the first reliable estimate is for patents filed in 1982. Third, before AIPA became effective in November 2000, unsuccessful US patent applications were not published and consequently not observed (see also Section 4.3). Therefore, we cannot construct a reliable estimate of the number of patents declined by the USPTO. Instead, based on Carley, Hegde, and Marco (2015), who estimate the probability of receiving a US patent that was filed in the period from 1996 to 2005 using a proprietary data set, we use $\overline{\pi} = 0.56$ as the unconditional probability of patent acceptance for US patents.[29]

Hence, we construct our time-dependent estimates of the patent acceptance probability for non-US patents filed in the period from 1982 to 2003 and use the closest available estimate for patents filed outside this period. Figure 2 presents the results. The estimated conditional probability of patent acceptances ranges from 0.54 to 0.64. The estimated probability of acceptance peaks from the late 1980s to the mid 1990s, after which we observe a sharp and steady decline. Given the lack of academic literature investigating patent acceptance rates with an international scope, we cannot assess the plausibility of the observed variation.[30] However, we do find that the overall probability of receiving a patent is around the documented 56% in Carley et al. (2015) for US patents.[31]

5.2 Window selection and estimation of the signal-to-total-variance ratio

To measure the signal-to-total-variance ratio, we estimate model (4) for each country independently. We stress that the regression results are essential for the validation of our approach to measuring patent values. Specifically, the statistical significance of the estimated coefficients γ_l from model (4) serve as evidence that stock price movements contain (measurable) information about the value of patents. Therefore, we take a prudent estimation approach, which we outline in detail in this section. First, we discuss the choice of estimation by fixed effects. Second, we investigate the validity of the error assumptions in model (4) to ensure reliable inference. Third and finally, we report the estimation results for the 27 countries in our sample and discuss their implications for the construction of patent value estimates.

[28] There could also be concerns about differences in the extent to which we observe unaccepted patents within the post-1980 period. Although they can be hardly tested based on our patent sample, we note that our estimates do not give real reason to support such concerns.

[29] Equally restricted by the availability of reliable application data, Kogan et al. (2015) also use 56% as an estimate of the unconditional patent acceptance probability for their study on US patents.

[30] Moreover, the causes of the variation in patent acceptance rates are hard to identify based on mere statistical examination of patent application data. In particular, the question arises whether the variation is driven by changes in the strategy of either patent applicants or patent offices (Carley et al., 2015).

[31] We should note that our estimate of π_j is likely to be a source of significant measurement error, which has a number of implications for the interpretation of our results. First, the absence of control for cross-sectional variation in the acceptance probabilities across patents hinders reliable comparison of individual patent values. Second, since firms are more likely to file for patent protection in their domestic market, cross-sectional differences in average or aggregated innovation measures are troubled by potential variability in patent acceptance rates across patent offices. Third and finally, the probability of patent acceptance may be correlated with the value of the patent. Given the skewed distribution of patent values, this implies that aggregated measures of innovation suffer from estimation bias.

Fixed effects

There are economic reasons to believe that the individual (firm-year) effects in model (4) are correlated with our patent grant variable. For example, consider firms with particularly active R&D departments. Based on the belief that such firms receive much attention from speculative investors (having certain expectations about the outcomes of these innovation activities), one could hypothesize that relatively innovative firms are characterized by higher stock return volatility.[32] Since R&D intensity is likely to be correlated with patent activity, the hypothesis implies presence of individual effects that are correlated with our patent grant dummy variable, that is, fixed effects.

To test the suspicion of fixed effects, we use a version of the Hausman (1978) test that is robust to violations of the i.i.d. assumptions on the error term in model (4). This robust form makes use of the auxiliary regression approach described in Wooldridge (2002, pp. 290–291). We extend the approach to the settings of our panel model presented in (4), that is, allowing for firm-year specific effects and including time dummies.[33] Table 2 (below) provides the p-values of the robust Hausman tests for all countries. Robust Hausman tests strongly reject random effects for all of the "larger" panels, where size is defined by the number of observed patent grants. The tests fail to reject random effects for the smaller data panels; however, this behavior is expected because the low number of observed patent grants is naturally accompanied by wide confidence intervals and, inherently, insignificant test results. We thus conclude that the test results generally support our suspicion of fixed effects. Therefore, we decide to estimate model (4) by fixed effects for all countries.

Violations of error assumptions

For valid inferences about the significance of γ_l in model (4), we need to be confident that our standard errors are consistent. However, given both the nature of the data and the specification of our model, we expect the standard assumptions for the error term ζ_{fd} in model (4) (i.e., i.i.d. over periods d and across cross-sectional units f) to be violated for our country-specific panels. First, errors are likely to be heteroskedastic as our panels include all types of securities, from

[32] The hypothesis about a positive relationship between firms' innovation intensity and stock return volatility is, among others, tested in Mazzucato and Tancioni (2012): results suggest a positive and significant relationship between R&D activity and volatility of stock returns.

[33] Asymptotically equivalent to the Hausman test for model (4) is to perform a Wald test of $\phi = 0$ in the auxiliary OLS regression (Wooldridge, 2002, pp. 290–291; Cameron and Trivedi, 2005, pp. 717–718):

$$y_{fd} - \widehat{\lambda}\bar{y}_{ft} = \sum_{i=1}^{5} \alpha_i (D_{id} - \widehat{\lambda}\overline{D}_{it}) + \sum_{l=0}^{L} \beta_l (I_{f,d-l} - \widehat{\lambda}\bar{I}_{ft}) + \phi(I_{fd} - \bar{I}_{ft}) + \nu_{fd}, \tag{i}$$

where $y_{fd} = \log(R^2_{fd})$ and $\bar{x}_{ft}$ denotes the mean of a given variable x for firm f in year t. Furthermore, $\widehat{\lambda}$ is a consistent estimate of $\lambda_{ft} = 1 - \widehat{\sigma}^2_{\zeta} / (\sigma^2_{\zeta} + T_{ft}\sigma^2_{c})$, where c and ζ refer to the firm-year specific effect and the error term in (4). Notice that we obtain the random effects estimator by OLS estimation of (i) with $\phi = 0$. To the contrary, statistical significance of additional functions of the regressors, such as $(I_{fd} - \bar{I}_{ft})$, suggests fixed effects (Cameron and Trivedi, 2005, p. 718). Therefore, testing $\phi = 0$ is equivalent to testing whether fixed effects are present. However, prudence is required as tests strongly indicate presence of heteroskedasticity, serial correlation, and cross-sectional dependence in the error term ζ_{fd} of model (4). Consequently, the i.i.d assumptions on $\nu_{fd} = (1 - \widehat{\lambda})c_{ft} + (\zeta_{fd} - \widehat{\lambda}\bar{\zeta}_{ft})$ are violated. Therefore, to ensure valid statistical inference, we use the Driscoll-Kraay covariance matrix estimator to produce heteroskedasticity- and autocorrelation-consistent standard errors that are robust to cross-sectional dependence (see also Hoechle, 2007). The violations of error assumptions and Driscoll-Kraay standard errors are further discussed below in this section.

low to high volatility stocks. This concern is confirmed by Greene's (2000) modified Wald test for (groupwise) heteroskedasticity in fixed effects models (generalized to allow for unbalanced panels, see Baum (2001)), which rejects the null hypotheses of homoskedastic errors with p-values of 0.000 in all panels.

Second, in line with the widely documented observation of "volatility clustering" (see, e.g., Taylor, 2007, ch. 4), we expect our dependent variable—the second moment of stock return—to exhibit persistent autocorrelations. Since our model does not account for this autocorrelation, we suspect serially correlated error terms. To test our suspicion, we perform Wooldridge (2002) tests for serial correlation in panel data (see also Drukker, 2003): the null hypothesis of no serial correlation is rejected with p-values of 0.000 in all panels.

Third, an increasingly dominant conclusion within the panel-data literature is that panel-data models exhibit cross-sectional dependence due to common shocks and the presence of unobserved factors (see, e.g., De Hoyos and Sarafidis, 2006). Cross-sectional dependence (CD) tests are, however, not directly applicable to our panels, as the test statistics can only be computed over observations common to all cross-sectional units (see Baum, 2001; De Hoyos and Sarafidis, 2006). Because we control for firm-year fixed effects, cross-sectional units (corresponding to firm-years) only share observations when corresponding to the same year. Moreover, even within the time span of one year, panels may be (highly) unbalanced because of new introductions or delistings of stocks in that year. For this reason, we run CD tests on *filtered panel models*, which are restricted to cover only observations within the same year and exclude securities with relatively short time spans and/or many data gaps. For most exchange markets, this leaves us with panels for which $T > N$, where T is generally around 250 (the typical length of one year in trading days). In such cases, we may rely on the Lagrange multiplier (LM) test, developed by Breusch and Pagan (1980). However, Pesaran (2004) shows that Breusch and Pagan's (1980) LM test is likely to exhibit substantial size distortions when N is large and T is finite. Therefore, for cases when N is large, we use Pesaran's (2004) CD test, which is valid for "large N, small (or large) T" panels (see De Hoyos and Sarafidis, 2006). The CD tests strongly reject the null hypothesis of no cross-sectional dependence in the filtered panel models, with p-values of 0.000 in nearly all cases. Hence, we conclude that cross-sectional dependence is present in all of our complete, country panels.[34]

To adjust for all of the above-mentioned violations, we use Driscoll and Kraay's (1998) covariance matrix estimator to produce heteroskedasticity- and autocorrelation-consistent standard errors that are robust to cross-sectional dependence (see Hoechle, 2007).[35] The Driscoll-Kraay covariance matrix estimator is large-T-consistent, independently of the cross-sectional dimension N. In our unbalanced panels, for firm f in year t, panel length T_{ft} is typically around 250

[34] Constrained by computational resources, we ran CD tests on filtered panel models for three different years, evenly divided over the sample period to account for the time dimension: 1980, 1995, and 2010. Because test results very strongly revealed cross-sectional dependence in each of the three years for virtually every country, we may confidently generalize these results to the complete country panel.

[35] The number of lags used for the estimation of the Driscroll-Kraay covariance matrix is determined using a simple heuristic proposed in Newey and West (1994): $\lfloor 4(T/100)^{2/9} \rfloor$. Furthermore, note that conventional clustered standard errors are not the preferred choice because they cannot adjust for correlations between cluster groups and, consequently, cross-sectional dependence.

(as we regard firm-years as "individuals" in our panel), which is sufficiently large, and hence Driscoll-Kraay standard errors are appropriate. Note that the fixed effects estimator remains consistent given the reasonable assumption that the cross-sectional dependence is caused by the presence of unobserved common factors which are uncorrelated with the included regressors (see, e.g., De Hoyos and Sarafidis, 2006).

Regression results and implications

As followed from the statistical tests discussed above, we estimate model (4) by means of fixed effects for all countries, and construct the Driscoll-Kraay covariance matrix estimator to produce consistent standard errors. Table 2 reports the regression results for the 27 countries, ordered by the number of patent grants in each sample. The most apparent observation from Table 2 is that we do not find statistical evidence of tock market reactions to patent grants in all countries. More specifically, mostly for the countries of lower order (in terms of patent counts), we do not find significant regression coefficients. This finding suggests that for a large share of the countries in our study, the number of observed patent grant events is too low for reliable estimation of patent value estimates. In particular, results suggest that our model requires at least 5,000 patents in order to produce significant results. However, since we also do not find evidence for patent-related stock price movements in South Korea and France, we cannot conclude that the insignificance of our parameters is merely due to an insufficient number of observations. Instead, these findings initially suggest that in some countries, on average, patent grants do not significantly disturb the stock market. We elaborate on the cases of France and South Korea in Section 7.

Furthermore, the regression results reveal that both the rate and timing by which the market processes information of patent grants varies across countries. Commonly, the stock prices do not react to the patent grant until one day after the grant day: only in Japan and Germany we find a significant estimate of γ_0. A possible, straightforward explanation for this observation could be related to the time of the day when patent offices announce patent grants: if patent offices disclose the information about patent grants at the end of the day, investors cannot respond to this news before the day after the grant. However, we leave the testing of this hypothesis to future work for it requires historical information on the announcement policies of the patent offices in our sample, which is not readily available to us. Another observation is that the stock market reaction is typically concentrated on one or two days: only for Japan we find evidence for a stock market response to patent grants throughout the complete three-day event window.

Thus, based on the estimation results in Table 2, we select the grant event window for each country. Specifically, the event window is defined by the set of days for which the estimated coefficients are significant at the 10% significance level.[36] As a logical consequence of the window selection criterion, the estimation results impose a restriction on our sample: we exclude

[36] Since the highest significant estimate equals 0.086, the regression results indicate that the true values of γ_l, $l \in \{0, 1, 2\}$, are close to zero. Therefore, we are concerned that our approach to measuring patent values suffers from an increased probability of type II errors. To partly overcome this problem, we choose to increase the power of the t-test by using the 10% significance level.

Table 2: Estimation results of model (4) for the 27 country-specific data panels, ordered by the number of patents in the sample. Robust Hausman test results indicate whether firm-year fixed effects are present. For *all* panels, estimation is by means of fixed effects. The days for which the estimates of γ_l, $l \in \{0, 1, 2\}$, are significantly positive at the 10% level define the event window. The last column reports the estimates of the signal-to-total-variance ratio $\widehat{\delta}$ constructed using (5) based on estimated regression coefficients.

Country	Number of patents	Robust Hausman test (p)	$\widehat{\gamma}_0$	DK s.e.($\widehat{\gamma}_0$)	$\widehat{\gamma}_1$	DK s.e.($\widehat{\gamma}_1$)	$\widehat{\gamma}_2$	DK s.e.($\widehat{\gamma}_2$)	Joint sig. F-test (p)	Event window	$\widehat{\delta}$
United States	853,594	0.000***	-0.008	0.006	0.022***	0.006	0.011^{+}	0.007	0.000***	$\{1,2\}$	0.017
Japan	599,021	0.000***	0.013^{+}	0.008	0.020*	0.008	0.025**	0.008	0.003**	$\{0,1,2\}$	0.019
Germany	224,823	0.000***	0.029*	0.012	0.018	0.012	0.015	0.013	0.055^{+}	$\{0\}$	0.029
South Korea	94,424	0.000***	-0.010	0.023	-0.004	0.021	-0.036	0.024	0.501	-	-
Taiwan	78,103	0.055^{+}	0.023	0.020	0.045*	0.019	0.027	0.020	0.085^{+}	$\{1\}$	0.044
France	69,961	0.004**	0.021	0.017	0.005	0.017	0.007	0.017	0.652	-	-
Switzerland	35,661	0.000***	-0.009	0.023	0.058*	0.023	0.012	0.024	0.090^{+}	$\{1\}$	0.056
United Kingdom	26,927	0.000***	0.015	0.021	0.056**	0.020	0.053**	0.020	0.003**	$\{1,2\}$	0.053
Finland	20,985	0.000***	-0.009	0.035	0.065^{+}	0.036	0.029	0.037	0.276	$\{1\}$	0.063
Netherlands	20,736	0.000***	0.019	0.030	0.028	0.030	0.033	0.031	0.564	-	-
Sweden	14,307	0.000***	0.021	0.026	-0.020	0.026	-0.009	0.026	0.685	-	-
Denmark	12,013	0.000***	0.031	0.032	0.032	0.031	0.053^{+}	0.032	0.272	$\{2\}$	0.051
Italy	7,477	0.874	-0.021	0.037	0.055	0.033	0.022	0.036	0.333	-	-
Belgium	7,214	0.201	0.024	0.036	0.020	0.036	0.072*	0.036	0.229	$\{2\}$	0.069
Canada	5,512	0.489	0.025	0.037	-0.008	0.037	0.086*	0.034	0.081^{+}	$\{2\}$	0.083
China	4,068	0.000***	0.128	0.093	0.063	0.104	-0.237^{*}	0.109	0.056^{+}	-	-
Norway	3,161	0.000***	-0.023	0.049	0.013	0.046	0.003	0.048	0.951	-	-
India	2,136	0.919	0.039	0.055	0.011	0.054	0.051	0.054	0.742	-	-
Austria	1,736	0.004**	0.009	0.065	0.018	0.067	0.080	0.063	0.664	-	-
Australia	1,647	0.477	-0.038	0.070	0.061	0.063	0.062	0.068	0.546	-	-
Singapore	1,530	0.851	0.047	0.088	0.064	0.089	0.009	0.090	0.872	-	-
Israel	1,123	0.354	-0.060	0.078	0.047	0.072	0.111	0.077	0.354	-	-
Spain	694	0.386	0.011	0.107	0.039	0.098	-0.061	0.104	0.912	-	-
New Zealand	586	0.387	0.066	0.118	0.046	0.107	0.076	0.124	0.869	-	-
Brazil	368	0.562	0.102	0.123	0.044	0.127	0.154	0.139	0.621	-	-
South Africa	202	0.770	0.065	0.144	-0.176	0.173	-0.330^{+}	0.192	0.235	-	-
Hong Kong	136	0.271	0.006	0.203	0.169	0.193	-0.159	0.209	0.634	-	-

Notes: although strongly correlated, the number of patents is not equal to the number of observations for which $I_{fd} = 1$ in (4) due to multiple grants per day; ***, **, *, and $^{+}$ indicate statistical significance at the 0.1%, 1%, 5%, and 10% levels; Driscoll-Kraay (DK) standard errors are robust to heteroskedasticity, autocorrelation, and cross-sectional dependence.

all countries for which none of the estimates of γ_l, $l \in \{0,1,2\}$, are significant. Although this restriction to our sample is undesired, it is inevitable since estimation results do not provide another reliable means to selection of the event window. In particular, the day(s) on which the stock market reacts to the patent grant vary/varies across the countries in our sample. Therefore, we argue, it is not justified to estimate the value of patent based on rather arbitrarily chosen stock returns. Moreover, the latter approach also involves the estimation of the signal-to-total-variance ratio with a high level of uncertainty.

Now, it is important to note that we only use the significantly positive estimates of γ_l, $l \in \{0,1,2\}$, to compute the estimate of the signal-to-total-variance ratio based (5). Furthermore, only the returns in the selected event windows constitute the input for the construction of the conditional expectation of the patent-related return using (2), and ultimately, for the computation of patent value estimates using (10). Notice that these restrictions on the event window imply some obvious alterations to the mathematical expressions for (5), (2), and (10). Table 2 reports the estimates for the signal-to-total-variance ratios based on the significant estimated regression coefficients of γ_l using equation (5). The estimates range from 0.017 to 0.083.[37] Furthermore, the results suggest a negative correlation between the signal-to-total-variance ratio and the number of patent grants in each panel. This relation can be explained by the intuitive, inverse relation between quantity and relative importance of patent grant events. We discuss the patent value estimates in the following section.

Lastly, we should note that for China and South Africa, the estimates of γ_2 are significantly negative, indicating a decreased return volatility on the second day after a patent grant. The negative effect is inconsistent with our stock return decomposition introduced in Section 4.1. Namely, our model is built on the assumption that the patent-related component of stock returns is independently distributed from the noise term and therefore restricts γ_l, $l \in \{0,1,2\}$, to be non-negative. The result could be due to omitted variables: patent grant days may be correlated to other, unidentified events that have a damping effect on stock return fluctuations. Another explanation is that the more turbulent days that follow patent grants are typically followed by a more quiet period, possibly related to decreased attention after patent offices have just made their announcements. In both cases, our results would be suffering from estimation bias. Nevertheless, since the estimated coefficients for the more significant countries in our sample are in line with our expectations, we conclude that our results are only minimally harmed by the potential bias.

5.3 Patent value estimates: descriptive statistics

We construct our patent value estimates for the reduced sample consisting only of the 10 countries for which the estimation results of model (4) provide statistical evidence for market responses to patent grants (see Table 2). First, using (2) and (8), we compute the conditional expectation and variance of the patent-related stock returns for every patent grant event in our

[37] In an early study on the relationship between patents, R&D, and stock market returns using patent data from the USPTO, Pakes (1985) estimate that approximately 5% of the variance in the stock returns is caused by events that also affect R&D expenditures or patent applications.

sample. The calculations are based on the stock returns in the selected event window, the estimates of the signal-to-total-variance reported in Table 2, and estimated conditional variance parameters of the noise component of returns as described in Section 4.2. Table C.1 in Appendix C reports the distributions of the returns r_j in the selected event window following patent grant j, and the corresponding estimates of $\mathrm{E}[s_j|r_j]$ across all patents j in the sample of each country. For the majority of the countries, the distribution of r_j is slightly skewed to the right, and on average close to zero. Notice that, as stems from the assumption of non-negativity of s_j, equation (2) implies that negative returns r_j are interpreted as small, positive expected values of the patent-related return component s_j. Second, we obtain our patent value estimates $\widehat{V}_j$ through equation (10). Table 3 presents the distributions of the estimated patent values across the remaining 10 countries of primarily listing in the restricted sample, both relative to the market capitalizations of the patenting firms and in absolute terms. All patent values are given in 1982 US dollars. We use 1982 price levels to be consistent with the study in Kogan et al. (2015), which is the most prominent point of comparison. For your reference, one 1982 US dollar has a value of 2.37 US dollars in 2015 price levels.

Table 3 shows that the distributions of the values of patents, both in relative and absolute terms, are highly positively skewed for all countries in our sample. This result is consistent with the findings in, among others, Harhoff et al. (2003b), investigating the tail value distribution of German patents held by German or US residents, and Kogan et al. (2015), estimating the value of US patents granted to public US firms in the period from 1926 to 2010.[38] The overall median value of the patents in our sample equals 5.28 million 1982 US dollars, while the average value of a patent is estimated to be 16.28 million.

Unfortunately, there are little points of comparison within the literature to validate this number.[39] Pakes (1985) studies the relationship between stock market rate of returns and patents for 120 firms over the period from 1968 to 1975 and finds that, on average, an unexpected arrival of one patent is associated with an increase in the firm's market value of 810 thousand 1972 US dollars, which is approximately equal to 1.75 million 1982 US dollars. Furthermore, an extensive survey study on the value of European patents by Giuri et al. (2007) suggests that 68% of the 7,752 patents in the sample had a value of less than one million euros on the day of granting. Based on these earlier findings, the average level of our patent value estimates appears to be high. One explanation is that our sample only covers patents granted to public firms: because public firms are relatively resourceful, the inventions embodied by these patents are more likely to become a commercial success, which increases their value (see also Kogan et al., 2015). However, regardless of whether the average value of patents is overestimated, both the cross-sectional and time-series variations can be meaningful.

Furthermore, we find that there is substantial variation in both the mean and median patent value across the 10 countries in our sample: the mean varies from 3.13 million 1982 US dollars in Taiwan to 53.46 million in Switzerland, and the median from 0.83 to 28.44 million. In addition, the relative patent values show that the variation does not simply reflect differences

[38] We note that of all estimated patent values, 58.1% and 6.3% concern US and German patents, respectively.
[39] Section 7.1 provides a close comparison with the results in Kogan et al. (2015), based on a similar model.

Table 3: Distributions of the constructed patent value estimates using equation (10) across 10 countries, ordered by the number of patents in the sample. Left columns present the value of patents relative to the patenting firm's market capitalization, given in percentage points; right columns report patent value estimates given in million US dollars, deflated to 1982 price levels using the CPI.

	United States		*Japan*		*Germany*		*Taiwan*		*Switzerland*	
	$\widehat{V}_j/M_j$	$\widehat{V}_j$	$\widehat{V}_j/M_j$	$\widehat{V}_j$	$\widehat{V}_j/M_j$	$\widehat{V}_j$	$\widehat{V}_j/M_j$	$\widehat{V}_j$	$\widehat{V}_j/M_j$	$\widehat{V}_j$
Mean	0.21	21.85	0.17	10.14	0.10	8.48	0.15	3.13	0.17	53.46
Std. Dev.	0.29	64.41	0.25	22.54	0.16	22.54	0.18	6.87	0.22	69.96
Perc.										
1st	0.00	0.24	0.00	0.29	0.00	0.15	0.00	0.03	0.01	0.16
5th	0.01	0.77	0.01	0.70	0.00	0.35	0.00	0.06	0.01	0.57
10th	0.01	1.26	0.01	1.07	0.01	0.57	0.01	0.10	0.02	1.25
25th	0.03	2.85	0.02	2.10	0.01	1.28	0.02	0.24	0.04	7.04
50th	0.09	6.79	0.06	4.44	0.03	3.30	0.07	0.83	0.09	28.84
75th	0.28	17.87	0.23	10.37	0.10	8.04	0.21	3.11	0.22	69.22
90th	0.57	47.02	0.49	22.38	0.30	17.87	0.42	8.15	0.41	140.22
95th	0.80	83.38	0.68	35.98	0.43	29.93	0.52	13.28	0.59	203.77
99th	1.34	246.50	1.04	87.95	0.69	91.47	0.73	31.13	1.04	334.94
	$N = 853{,}594$		$N = 599{,}021$		$N = 224{,}823$		$N = 78{,}103$		$N = 35{,}661$	

	United Kingdom		*Finland*		*Denmark*		*Belgium*		*Canada*	
	$\widehat{V}_j/M_j$	$\widehat{V}_j$	$\widehat{V}_j/M_j$	$\widehat{V}_j$	$\widehat{V}_j/M_j$	$\widehat{V}_j$	$\widehat{V}_j/M_j$	$\widehat{V}_j$	$\widehat{V}_j/M_j$	$\widehat{V}_j$
Mean	0.49	31.52	0.14	18.47	0.27	15.25	0.37	9.42	0.77	11.71
Std. Dev.	0.45	63.54	0.23	30.81	0.21	22.51	0.30	14.64	0.54	23.58
Perc.										
1st	0.03	0.10	0.01	0.30	0.03	0.35	0.05	0.16	0.11	0.11
5th	0.06	0.44	0.01	1.95	0.05	0.85	0.08	0.58	0.16	0.32
10th	0.08	0.93	0.01	3.58	0.07	1.35	0.11	1.02	0.23	0.52
25th	0.16	4.03	0.02	6.02	0.12	3.10	0.17	2.28	0.42	1.12
50th	0.36	13.60	0.03	10.10	0.23	7.28	0.31	5.00	0.66	3.53
75th	0.67	31.04	0.16	19.64	0.37	16.82	0.49	12.25	1.00	11.32
90th	1.04	68.77	0.46	37.13	0.54	38.20	0.71	21.79	1.41	31.44
95th	1.32	121.29	0.66	58.28	0.67	60.42	0.89	31.19	1.70	50.55
99th	2.10	325.23	1.09	148.02	0.95	120.27	1.50	47.61	2.41	117.00
	$N = 26{,}927$		$N = 20{,}985$		$N = 12{,}013$		$N = 7{,}214$		$N = 5{,}512$	

Notes: the construction of patent values are based on stock price movements in country-specific event windows, defined by the days on which we observe significant stock price reactions to patent grants (see Table 2); for the 17 (out of the 27) countries in our sample that are not reported in this table, model (4) does not detect significant stock price reactions to patent grants.

in average (or median) market capitalization of the patenting firms in each country. For there are numerous sources of heterogeneity in the underlying patent samples, it is difficult to give a straightforward interpretation to this variation. An important factor is the distribution of the patenting firms across technological sectors. For instance, the high average value of patents granted to firms listed in Switzerland largely arises because over 60% of the Swiss patenting firms operate within the pharmaceutical sector: the average value of patents granted to firms in the pharmaceutical industry across all 10 countries is roughly 46.1 million 1982 US dollars. Another source of variation in the patent sample characteristics stems from differences in patent regulations, implying a varying degree of commercial protection for the patented inventions.

One could provide a more detailed interpretation of the patent value estimates by means of historical descriptions and (pairwise) comparisons of the innovative industries in each country, whereby, among other things, characteristics of the patenting firms and regulatory issues should be considered. This comprehensive endeavor lies, however, outside the scope of this thesis. Instead, to validate the usefulness of our measure, we investigate whether our measure is supported by citation-based measures of patent quality in Section 6.1, and subsequently examine its correlation with economic growth in Section 6.2.

6 Citation-based patent quality and innovation indices

Here, we examine the usefulness of our patent-level measure of innovation. In particular, we seek to extend the findings in Kogan et al. (2015)—which clearly indicate the usefulness of an innovation measure of this type for their US sample—to an international context. First, Section 6.1 tests whether citation-based measures of patent quality are correlated with our patent value estimates. Second, Section 6.2 presents a preliminary attempt to relate an aggregated indices of innovation, based on the yearly sum of all patent value estimates granted to firms listed in the particular country, to economic growth.

6.1 Patent values and citations

A popular approach to measuring patent quality uses forward patent citations, that is, the number of times a patent is cited by other patents in the future. Although the shape of the relationship remains topic for academic debate, earlier findings in the literature clearly suggest a strong correlation between citation counts and the economic value of patents (see also Section 2). In particular, Kogan et al. (2015) show that their estimates of the value of US patents are strongly and positively associated with patent counts. Our study contributes to the literature by studying this relationship for an international sample of patents, which are granted to firms listed across 10 countries in three continents. Moreover, by using citation counts as a benchmark measure for patent quality, we seek to externally validate our patent-level measure of innovation and confirm its initial, underlying assumption that stock price movements contain information about the value of patents.

Based on the model proposed in Kogan et al. (2015), we regress our patent value estimates,

Table 4: Estimation results on the relation between patent citations and our patent value estimates. The left part reports the mean and median value of the citation count variable C_j in the sample of each country. The right part presents the country-specific coefficient estimates for the log number of patent citations in model (11). Specification (a) includes grant-year and patent-office fixed effects; (b) includes, in addition, the log of the patenting firm's market capitalization prior to the patent grant; and (c) includes, in addition, the log of the second moment of returns of the patenting firm in the grant year.

		C_j		Model (11): $\log(1 + C_j)$		
Country	N	Mean	Median	(a)	(b)	(c)
United States	853,594	9.4	1	0.066^{***}	0.068^{***}	0.061^{***}
Japan	599,021	4.5	0	0.039^{***}	0.004	0.005
Germany	224,823	2.4	0	0.041^{***}	0.048^{***}	0.044^{***}
Taiwan	78,103	1.6	0	-0.057^{+}	-0.065^{*}	-0.065^{*}
Switzerland	35,661	0.9	0	0.026	0.034	0.032
United Kingdom	26,927	2.9	0	0.048^{*}	0.008	0.007
Finland	20,985	1.8	0	0.051	0.040	0.041
Denmark	12,013	1.8	0	0.063	0.003	0.002
Belgium	7,214	1.7	0	0.053^{+}	0.060^{**}	0.060^{**}
Canada	5,512	7.7	1	0.026	-0.021	-0.020

Notes: ***, **, *, and $^{+}$ indicate statistical significance at the 0.1%, 1%, 5%, and 10% levels; standard errors are clustered by year; countries are ordered by the number of patents in the sample; for the 17 (out of the 27) countries in our sample that are not reported in this table, we cannot construct our patent value estimator.
[a] Constrained by resources, we randomly divided the sample of the United States into four subsamples to ease computation. The table reports the average estimates across these four subsamples. Differences in parameter estimates across the four subsamples are all smaller than 0.01 and differences in p-values are negligible.

$\widehat{V}_j$, on the number of times patent j is cited by other patents up to year 2013, C_j:

$$\log\left(\widehat{V}_j\right) = a_0 + a_1 \log(1 + C_j) + \boldsymbol{Z}_j' \boldsymbol{a}_3 + u_j. \tag{11}$$

We estimate (11) for each country independently to test whether our measure succeeds to exploit information about patents contained by stock returns in each of the countries. To ensure robustness of our results, we estimate multiple specifications, each characterized by a different set of fixed effects $\boldsymbol{Z}_j$ that control for omitted variables. The first specification includes grant-year fixed effects to account for the truncation of citations counts after the year 2013, and patent office fixed effects because differences in patent citation policies can effect the relationship between citation counts and value. The second specification also include (next to the ones included by the first specification) the log of the patenting firm's market capitalization on the day prior to stock market reaction to the patent grant, $\log(M_j)$, to control for the effect of firm size on the patent technological impact. In addition to variables included by the second model, the third specification additionally controls for the log of the patenting firm's second moment of return in the grant year, $\log(\sigma_{ft})$, because it is an input variable for our patent value estimator (see equation (2)). We cluster standard errors by grant year to account for potential serial correlation in the number of citations received by patents granted within the same year.

Table 4 reports the estimation results. Consistent with the findings in Kogan et al. (2015), we find a significant relationship between our patent value estimates and the number of patent citations for the US sample.[40] The estimates imply that one additional citation, around the median value, is related to an increase of 3.5% to 4.0% in the economic value of patents granted to US firms.[41] Also, we find a quantitatively similar result for the patents granted to firms listed in Germany and in Belgium. Our findings are in line with the study in Harhoff et al. (1999) on the relation between the number of citations and market value of US and German patents, which also exposes a significantly positive relationship. For patents granted to Japanese or British firms, the relationship between citations and our patent-value measure is not robust to the inclusion of firm-size fixed effects. For the remaining countries, the estimation results do not indicate a significantly positive relationship between citation counts and our estimates of the economic value of patents. Since the sample sizes are relatively small for most of these countries, these findings do not necessarily disqualify our measure. More alarming are, however, the significantly negative effects of patent citations on the value of patents granted to firms listed in Taiwan.

Hence, based on the estimation results of (11), we conclude that our patent value estimates are only coherent with citation-based measures of patent quality for patents granted to the United States, Germany and Belgium. The implications of this conclusion are, however, ambiguous. On the one hand, our results imply that the relationship between citations and market value, which has mainly received attention for US and German patents, does not apply to patents granted to firms outside these countries. On the other hand, the lack of correlation between citations and our patent value estimates casts doubts on the reliability of our approach to measuring the economic value of patents. In particular, it implies that the type of innovation measure proposed in Kogan et al. (2015) does not seem to be straightforwardly generalizable to other countries.

The nonexistence of the relationship between the number of citations a patent receives and its market value could be related to differences in patent citation practices. To test this hypothesis, we repeat the estimation of model (11), now using only patents granted by the USPTO to the firms in each sample.[42] These patents account for more than 30% of the total patent sample in each of the countries except Switzerland (13%). Although the positive relationships earlier exposed for the Belgian and British samples gain in statistical confidence, we again do not find significant correlations between the number of citations and the estimated economic value of patents granted to firms outside the United States, Germany, and Belgium. Moreover, the results even support the significantly negative effect of patent citations in the Taiwanese sample. Hence,

[40] The estimates in Kogan et al. (2015) are much more sensitive to the inclusion of fixed effects than our estimation results indicate. This difference is unexplained and requires closer inspection.

[41] The reported percentages in this paper are not directly comparable with the results in Kogan et al. (2015) because the median value of their citation count variable is equal to five. This difference in the median number of patent citation exists because our US sample also includes foreign patents granted to US firms: if we restrict our sample to patents granted by the USPTO, the descriptive statistics are consistent with Kogan et al. (2015).

[42] By repeating our analysis using only US patents, we are able to overcome another issue. Namely, due to their discrete nature, citation counts cannot differentiate in quality for more than half of the patents in each country-specific sample: the median equals zero for all countries except the United States and Canada (see Table 4). This feature of our patent samples could possibly hinder significant correlations between citation counts and our patent value estimates. The median citation US patents receive varies from one to six, depending on the country in which the patenting firm is listed. Hence, samples of US-only patents suffer less from the discrete nature of patent citation data.

this robustness check adds to the credibility of the latter implication of our findings: although our patent value estimates appear valid for the United States and Germany, our measure does not seem to result in reliable market value estimates of patents granted to firms listed in other countries.

Nevertheless, considering the lack of academically established relationships between patent counts and the economic value of patents in many of the countries in this study, it is worthwhile to further test the usefulness of our measure. Therefore, in the next section, we examine the correlation of yearly aggregates of our patent-level innovation measure with economic growth.

6.2 Economy-wide indices of innovation

There is little debate over the question whether technological change has made significant contributions to welfare over the past couple of centuries. However, it is difficult to construct an accurate measure of technological progress that is comparable across time and space. Today, the most extensive empirical works in the literature rely on patent activity to measure technological change (Khan, 2014).[43] In this section, we aim to contribute to this literature by constructing an economy-wide index of innovation, and examine its correlation with economic growth.[44] We should note, however, that our approach to investigating the relation between innovation and economic growth is rather preliminary. In particular, we do not test the usefulness of our measure in a panel design, but instead estimate country-specific time-series specifications, which are naturally not able to capture the interdependence of national economies. We leave the work of setting up such a sophisticated panel model, which controls for international trade and innovation spillover effects, to future research.

Logically, our country-level indices of innovation are based on our patent-level estimates of innovation. Specifically, we sum the estimated values of all patents granted to the firms in the sample of country c in year t, scaled by total market capitalization M of all firms in the sample at the end of year t:

$$\widehat{\Upsilon}_{c,t} = \frac{\sum_{j \in \mathcal{P}_{c,t}} \widehat{V}_j}{\sum_{f \in \mathcal{F}_{c,t}} M_f}, \tag{12}$$

where $\mathcal{P}_{c,t}$ denotes the set of all patents granted to all firms in the sample of country c in year t

[43] We note that patent data have some serious limitations as a source for measuring technological change in a cross-country study. Most fundamentally, by focusing on patent activity, we exclude all innovations that are not patented (see also Hall et al., 2005). Although it is extremely complicated to determine the extent to which patents are representative of the wider set of all innovations, this would not present a problem if we could assume that the representativeness is comparable through time and across space. However, considering the diverse set of patent cultures and market conditions observed globally—often subject to change over the past decades—this assumption seems hard to justify.

[44] The measure of innovation in this paper, as any patent-based measure, yields *direct* estimates of technological innovation. However, the literature has also proposed two main approaches to measuring technology shocks indirectly (see also Kogan et al., 2015). The first approach is to measure technological change through total factor productivity (TFP), either at the aggregate level or at the firm level. Examples of studies that take this approach include Olley and Pakes (1996) and Basu, Fernald, and Kimball (2006). However, conclusions based on such residual-based measures need to be drawn with caution as they could include economic forces unrelated to technology, such as resource misallocation (see, e.g., Hsieh and Klenow, 2009). The second approach imposes model-based restrictions to measure technology shocks either through the estimation of vector autoregressive models (VAR) or structural models (see, e.g., Gali, 1999; Smets and Wouters, 2003). Inherently, the resulting estimated technology series are highly dependent on the identification assumptions.

and $\mathcal{F}_{c,t}$ is the set of all firms in the sample of country c. Notice that because both patents and market capitalizations are in 1982 US dollars, the innovation index $\widehat{\Psi}_{c,t}$ is free of currency and price level. Furthermore, we divide by market capitalization (as opposed to economic output) to address the potential concern that fluctuations in the sum of patent values simply reflect fluctuations in the level of stock prices that are not related to innovation (see Kogan et al., 2015).

In Figure 3 (solid lines), we present the economy-wide indices for each of the 10 countries in our remaining sample. One apparent observation is that in most countries, we observe a peak around the year 2000, which clearly reveals the wave of technological progress in areas such as computing and telecommunication (Kogan et al., 2015). However, instead of describing the technological state over the last 40 years in each country, Figure 3 rather aims to illustrate that much of the—both low- and high-frequency—fluctuations in the innovation indices are driven by the mean value of the patent (dashed lines). Taiwan is the most prominent exception to this observation: the sharp increase of the Taiwanese innovation index in the 1990s is due to the rapidly growing number of patents in our sample, while the average patent value declines in the same period. The increasing innovation index in Japan is also driven by an increase in the quantity of patents, instead of their quality. Nevertheless, Figure 3 illustrates that our innovation measures adds a great degree of variability that is not observed in simple patent count measures of innovation.

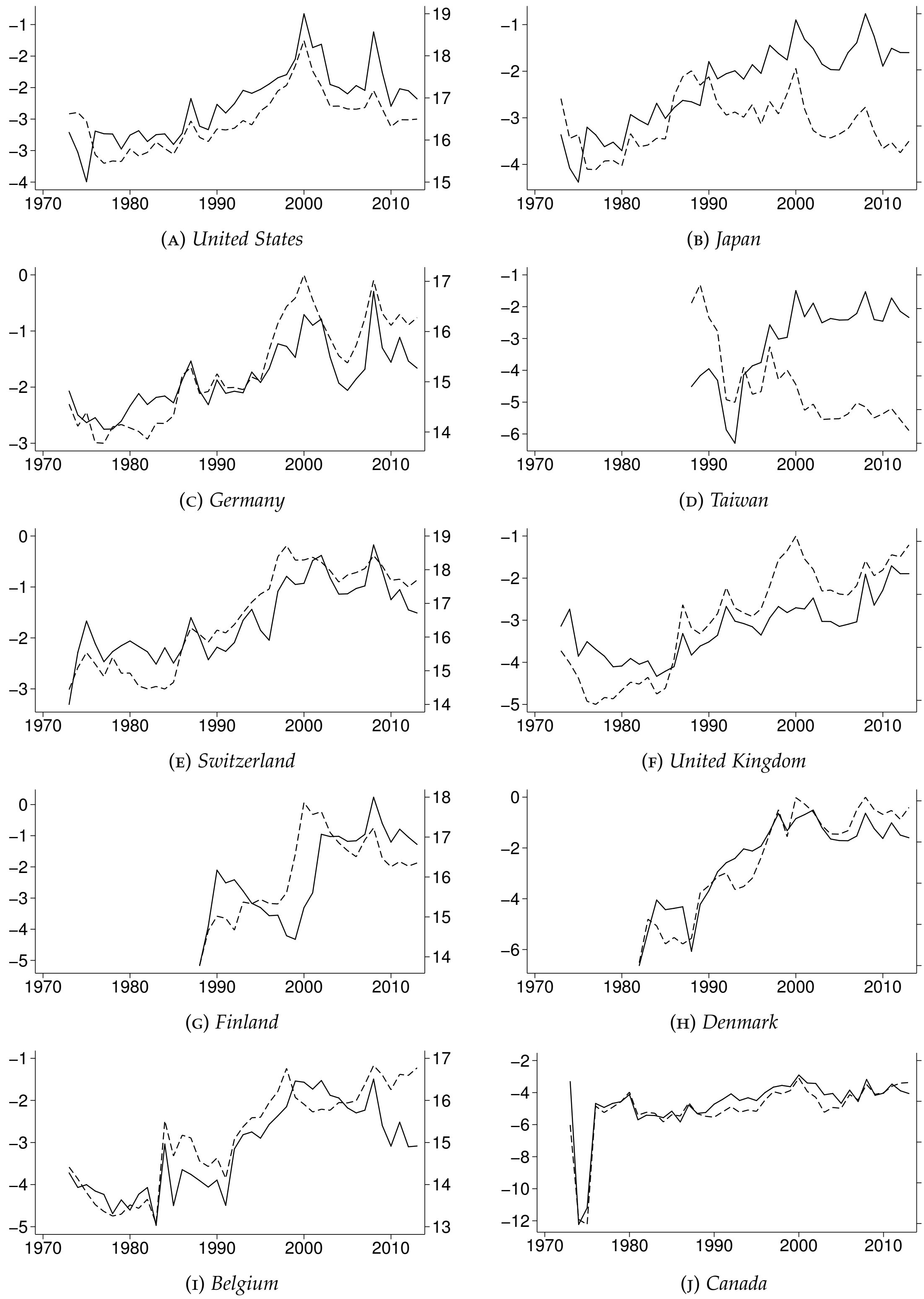
The innovation index $\widehat{\Psi}_{c,t}$ follows the example set in Kogan et al. (2015). Assuming a simple model of innovation that is based on Atkeson and Burstein (2011), Kogan et al. (2015) show that, to a first-order approximation, this index of innovation is proportional to the growth rate in aggregate economic output. Kogan et al. (2015) find empirical support for the derived relation by regression of US economic output on their US innovation index. We test whether this empirical results can be generalized to the countries in our sample. Hence, following Kogan et al. (2015), we estimate the following time-series model:

$$y_{c,t+\tau} - y_{c,t} = b_0 + b_\tau \log\left(\widehat{\Psi}_{c,t}\right) + \sum_{l=0}^{L} c_l y_{c,t-l} + \eta_{c,t+\tau}, \qquad \tau \in \{1,2,3,4,5\} \tag{13}$$

where $y_{c,t}$ is log gross domestic product (GDP) per capita of country c, in year t, given in constant local currency units. We estimate (13) for horizon τ up to five years. Selection of lag length L is based on the consistent Schwarz (1978) information criterion (SIC), calculated using only those observations that are available to all specifications. For all countries, SIC advocates lag lengths between one and three, where the higher lag length are associated with the higher horizons.[45] We construct Newey and West's (1987) covariance matrix estimator to produce heteroskedasticity- and autocorrelation-consistent standard errors, using a conservative maximum lag length equal to $\tau + 4$.

Figure 4 plots the response of GDP per capita to a shift of (approximately) 1% in the aggregation index for each country. For the United States and a horizon of five years, we find that

[45] For most countries, the results are reasonably robust to choice of lag length. The only notable sensitivity concerns Canada: for higher lag lengths, the Canadian innovation index has a small, but significantly positive effect on economic output for a horizon of five years.



(A) *United States*　(B) *Japan*

(C) *Germany*　(D) *Taiwan*

(E) *Switzerland*　(F) *United Kingdom*

(G) *Finland*　(H) *Denmark*

(I) *Belgium*　(J) *Canada*

FIGURE 3: Log values of economy-wide innovation indices (solid line, left axis) and log mean values of patents granted in each year (dashed line, right axis). Innovation indices are the yearly sum of estimated patent values for the country, scaled by total market capitalization of all patenting firms at the end of the year. Countries are ordered by total number of patents.

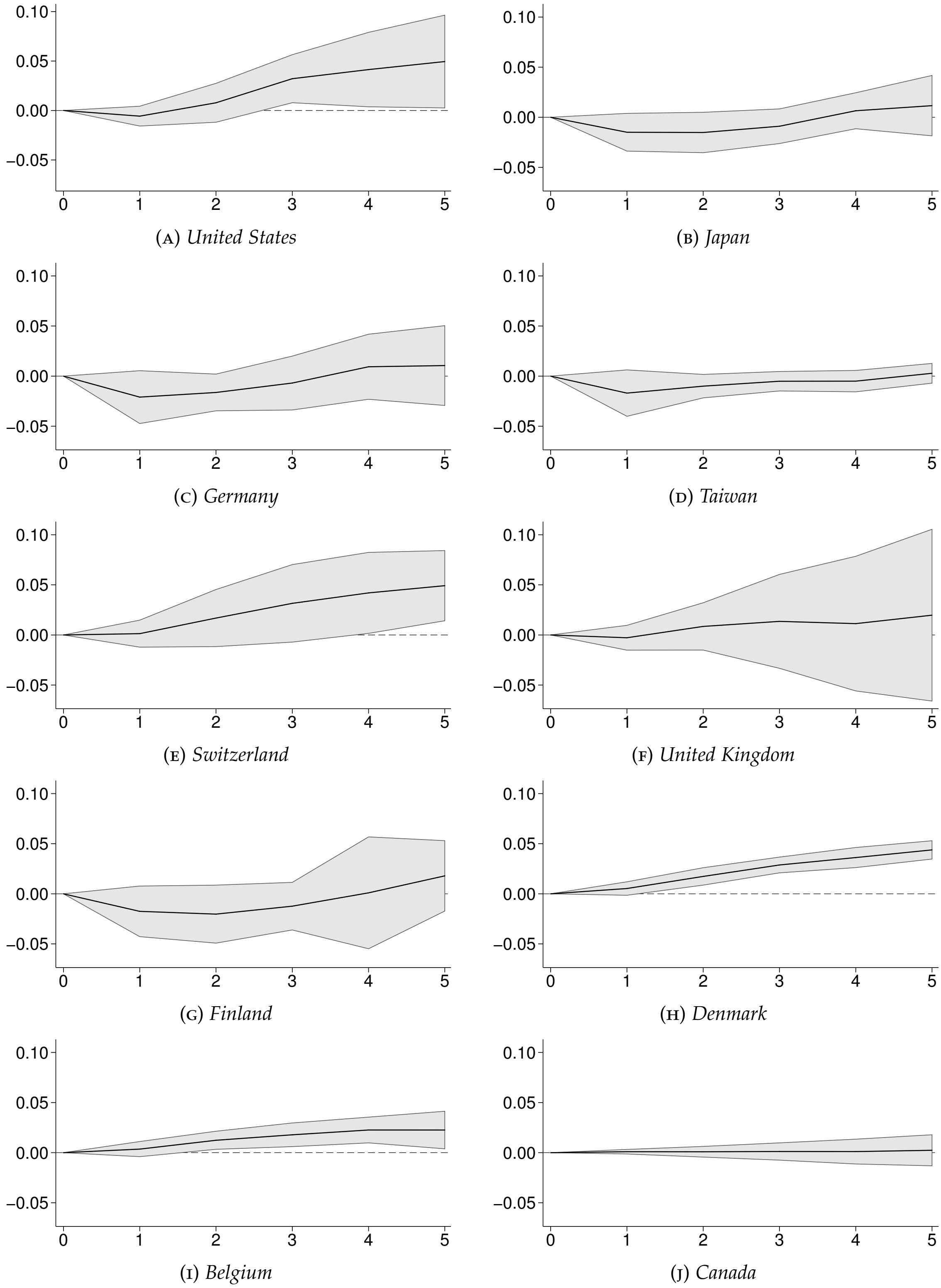


FIGURE 4: Estimated responses of GDP per capita in constant local currency to a 1% increase in the aggregate innovation index using equation (13) for a horizon up to five years. Shaded regions represent 95% confidence intervals using Newey-West standard errors with maximum lag length equal to four plus the horizon. Countries are ordered by total number of patents.

a 1% increase is followed by approximately 4.9% growth in GDP per capita. This finding is in line with Kogan et al. (2015), who report 5.5% for a similar specification (using aggregate GDP instead of GDP per capita) for a longer period (from 1926 to 2010).[46] The results for Switzerland, Denmark, and Belgium show similar relationships, although the effect varies in magnitude. For the other countries in our sample, we do not find evidence for a significant relationship between our innovation index and economic growth.

Although our results shed some additional light on the relationship between innovative activity and economic output, conclusions that can be drawn are limited. In particular, it is unclear whether our findings implicate that a (log–log) relationship between the true value of the innovation index in (12) and economic growth is nonexistent for a substantial share of the studied countries, or whether our patent-level estimates of innovation are not able to sufficiently capture the value of the actual innovative activity. Comparison with the findings in the previous section is only partly helpful. On the one hand, patent citation data suggest the validity of the US and Belgian innovation indices, which could explain why these innovation indices are indeed significantly related to domestic economic growth. On the other hand, Section 6.1 shows that a citation-based measure of patent quality also supports our estimator for patents granted to firms in Germany, but nevertheless, the German innovation index is not related to economic growth. And moreover, even though citation model (11) in Section 6.1 does not provide statistical support for the Danish and Swiss innovation indices, these are significantly correlated with domestic economic growth. Therefore, we conclude that further research is required, which provides a closer examination of the dynamics and input factors of GDP growth in each of the countries, to test the general usefulness of the economy-wide indices.

As a final note: the effect of innovation need not be constrained by the domestic borders. Given the economic globalization, innovation that originated in, for instance, the United States—which accounts for 61% of the total estimated value of innovation in our sample—could increase wealth also in foreign countries. To test this hypothesis, we estimate an alternative specification of model (13) that includes the US innovation index as an additional indicator, and we also control for lagged log US GDP per capita. The estimation results reveal that the US innovation index has significant predictive power for future economic growth in the United Kingdom. Hence, we find a first hint that the merits of innovative activity are not restricted to the country where the patenting firm is listed. We stress, however, that our approach to testing this hypothesis is rather elementary and that a more sophisticated (panel) model design is required to produce conclusive findings.

7 Discussion

Aimed at providing perspectives for future work, this section provides a discussion of some important lessons learned through conducting our research. First, Section 7.1 reviews the alter-

[46] We note that Kogan et al. (2015, p. 26) estimate essentially the same regression with their US innovation index, but interpret to the estimated regression coefficient as the response of economic output to a unit standard deviation shock (as opposed to a relative change) in the innovation index. This interpretation is confusing (and appears false), as we regress the log of the innovation index and not its absolute value.

ations made in this paper to the model proposed in Kogan et al. (2015) and investigates their impact on the empirical results. Second, Section 7.2 examines the validity of the assumption that the signal-to-total-variance ratio is constant across all patents and over time by comparing results for various subsamples of the data set.

7.1 Comparison with the KPSS model

Although this study largely follows the measure of innovation proposed in Kogan et al. (2015), our model involves two important alterations to specifications of their econometric approach. The first deviation concerns the use of multi-period cumulative stock returns in Kogan et al. (2015)—as opposed to the daily returns used in this study—for the measurement of stock market reactions to patent grant events. The second difference arises due to the arguably false calculation of the signal-to-total-variance ratio in Kogan et al. (2015). We first closely replicate the model in Kogan et al. (2015), in this section referred to as the "KPSS model", and compare the results with the ones discussed in Section 5 and Section 6 to determine the impact of the deviations. Second, we discuss some theoretical considerations when comparing the KPSS model and the model used in this study.

Comparison of patent value estimates

As noted in Section 4.2, Kogan et al. (2015) study the multi-period cumulative returns following patent grants instead of the set of daily returns within the grant event window. Specifically, the KPSS version of model (4) regresses *three-day* market-adjusted simple returns R_{fd} (starting on day d) on a single patent grant dummy I_{fd}:

$$\log\left(R_{fd}^2\right) = \gamma I_{fd} + \sum_{i=1}^{5} \theta_i D_{id} + \mu_{ft} + \zeta_{fd}, \tag{14}$$

where all other terms are as defined in Section 4.2. Note that the subscript l is omitted from γ as (4) does not construct day-l-specific estimates.

We estimate model (14) by fixed effects for all 27 countries in our sample.[47] To allow the rate at which investors process patent information to vary across markets, we also estimate (14) using two-day and four-day cumulative returns as the dependent variable. Table D.1 in Appendix D reports the estimates of γ. In line with the results reported in Table 2, estimation of (14) does not indicate significant stock price reactions to patent grants. However, the results based on the KPSS model do not agree with the main results in this paper for all countries: model (14) identifies significant market reactions to patent grants in South Korea, Australia, and South Africa, but does not in Taiwan and Switzerland, while the opposite is true for model (4). We discuss some explanations for this result in the next part of this section. Here, we construct patent value estimates for the 11 countries for which the estimate of γ is significant at the 10% level (following the rule as in Section 5.2). In general, we use three-day returns for the purpose

[47] Also for the KPSS model, robust Hausman tests strongly indicate presence of fixed effects in most country-specific data panels. Moreover, fixed effects is also in line with the estimation in Kogan et al. (2015).

of replication and since there is no straightforward alternative selection criterion.[48] However, in case the estimate of γ is insignificant for the three-day returns model, we select, if possible, the multi-period return for which the estimate of γ is significant.

As mentioned in Section 4.2, Kogan et al. (2015, p. 11) present an alternative relation between γ and the signal-to-total-variance ratio, δ. Their relation implies that γ measures the increase in the *volatility* of stock returns during days following patent grants, instead of their second moment (see also Appendix B.2). However, because the variance of a truncated normal distribution does not equal its second moment, this interpretation of γ is false. To examine the impact of this alternative computation of δ, we construct patent value estimates both for the relation between δ and γ proposed in this paper and for the one in Kogan et al. (2015).[49] To isolate the effect of the estimate of δ, we closely follow all other steps of the KPSS model for both sets of patent value estimates.[50] Table D.2 in Appendix D reports the results.

Before drawing any conclusions, we must first validate that our replication is sufficiently close. Table D.2 reports an average patent value of 40.8 million 1982 US dollars for the US sample based on the replicated model. This number appears high compared to the 17.7 million 1982 US dollars reported in Kogan et al. (2015). Closer inspection reveals, however, that the difference occurs because the mean value of patents granted to US firms has increased over time (see Figure 3a) and Kogan et al. (2015) study the period 1926–2010 instead of 1973–2013. Moreover, we also apply our replicated KPSS model on the data set used in Kogan et al. (2015).[51] As a result, we closely replicate the patent value distribution in Kogan et al. (2015): the first and second sample moments of our estimates are both within 10% of the reported moments in Kogan et al. (2015).

Now, we (may) consider the impact of the alternative computation of the signal-to-total-variance ratio δ. We find that the mean patent value is—depending on the country—between 62% and 66% *higher* when using the expression for δ from Kogan et al. (2015) compared to the one proposed in this study. Hence, we conclude that the impact on the level of the patent value estimates is severe. Furthermore, the cross-sectional differences between the estimated patent values across countries are slightly distorted because the relative "bias" of the estimate of δ is a convex function of γ. Nevertheless, we should add that because the patent value estimator is approximately linear in δ, cross-sectional differences in value across patents granted to firms within the same country remain relatively unaffected by the alternative calculation of δ. Finally, we note that Kogan, Papanikolaou, Seru, and Stoffman were informed about

[48] Kogan et al. (2015) advocate the use of three-day returns based on the finding that share turnover is significantly higher in the window $[d, d+2]$, where d is the patent grant day. However, Kiymaz and Berument (2003), among others, report that high volatility is associated with low trading volume. Such conclusions imply that on days after patent grants—for which the model in Kogan et al. (2015) states that the news of the patent increases return volatility—share turnover need not be higher. Therefore, the selection of the window length in Kogan et al. (2015) appears doubtful.

[49] Specifically, Kogan et al. (2015, p. 11) use $\delta = (e^{\gamma} - 1)(1 - 2c_0^2 + e^{\gamma}c_0^2)^{-1}$. The relation between δ and γ used in this paper, applied to the setting of single-grant-dummy model (14) is given by: $\delta = 1 - e^{\gamma}$. For your reference, note that Kogan et al. (2015) name δ the "signal-to-noise" ratio. However, since the denominator in (3) equals the total variance of return, we believe that "signal-to-total-variance" bears a closer resemblance to its mathematical definition.

[50] The estimation in Kogan et al. (2015, p. 11) of the variance of the noise in returns, $\sigma_{\varepsilon f t}$, also implicitly assumes that γ in model (14) measures the increase in volatility of stock returns. Consequently, Kogan et al. (2015) recover $\sigma_{\varepsilon f t}$ from an estimate of the daily return volatility as opposed to the second moment. However, the impact is only very small because the mean of the daily market-adjusted returns is close to zero.

[51] Noah Stoffman openly shares the patent data set used in Kogan et al. (2015) at `http://kelley.iu.edu/nstoffma/`.

Table 5: Comparison of the patent value estimates based on model (4) and the KPSS model (14) with L-period returns, across seven countries. The last column reports the ratio of the mean of $\widehat{V}_j^{KPSS}$ to the mean of $\widehat{V}_j$. All patent value estimates are given in million US dollars, deflated to 1982 price levels using the CPI.

		$\widehat{V}_j$, model (4)		$\widehat{V}_j^{KPSS}$, model (14)		
Country	Number of patents	Event window	Mean	Return period L	Mean	Ratio
United States	853,594	$\{1,2\}$	21.85	3	24.66	1.13
Japan	599,021	$\{0,1,2\}$	10.14	3	11.43	1.13
Germany	224,823	$\{0\}$	8.48	3	14.98	1.77
United Kingdom	26,927	$\{1,2\}$	31.52	3	35.34	1.12
Finland	20,985	$\{1\}$	18.47	3	36.25	1.96
Denmark	12,013	$\{2\}$	15.25	4	31.27	2.05
Canada	5,512	$\{2\}$	11.71	3	23.44	2.00

Notes: countries are ordered by the number of patents in the sample; for the 20 (out of the 27) countries in our sample that are not reported in this table, either (4) or (14) does not detect significant stock price reactions to patent grants; $\widehat{V}_j^{KPSS}$ in this table is based on the expression for δ used in this paper, that is, $\delta = 1 - e^{-\gamma}$.

the concerns raised in this paper via e-mail. The authors verified the claim that their results rely on a false interpretation of γ and announced an updated version of their paper (personal communication, February, 2016).

Next, we investigate the impact of using multi-period returns instead of daily returns for the identification of the stock market reaction to patent grants. To do so, we compare the patent value estimates based on estimation of model (14), $\widehat{V}_j^{KPSS}$, with the patent values $\widehat{V}_j$ presented in Section 5.3, which are on based model (4). Table 5 reports the $\widehat{V}_j^{KPSS}$ and $\widehat{V}_j$ for the seven countries for which both model (4) and (14) detect significant effects of patent grants on stock price movements. We find that estimation based on (14) results in substantially different patent value estimates than model (4). Most importantly, the cross-country differences are greatly affected by the choice of model: while for the United States or Japan the mean patent value estimates $\widehat{V}_j^{KPSS}$ are only 13% higher than the mean of $\widehat{V}_j$, for Germany, Denmark, or Canada the mean of $\widehat{V}_j^{KPSS}$ is approximately twice as high as the mean of $\widehat{V}_j^{KPSS}$.

The explanation for this finding involves the selection of the grant event window. As explained in Section 4 and applied in Section 5.2, model (4) considers each day in the grant event window separately, and inherently, our patent value estimates rely only on the specific days for which we detect a significant effect of patent grants on stock price movements. In contrast, model (14) generally relies on three-day cumulative returns (see also Table D.1). Now, first note that the length of the return window directly affects the total variance of the noise component of returns. This noise variance parameter of the noise is an input in equation (2) and as such, mechanically affects the measure of patent values. Specifically, due to the assumptions that the patent-related component of returns follows a normal distribution truncated at zero and that the signal-to-total-variance ratio is constant, the estimates of patent values are an *increasing* function of the variance of the noise component of returns. Hence, the large difference in the mean estimated patent value for Germany, Finland, Denmark, and Canada, occurs because the KPSS model also relies, as reported in Table 5, on multi-period returns for these countries, in contrast with the single-day return windows selected by our model. We conclude that the limited ability of the KPSS model to identify the timing of the stock market reaction greatly distorts cross-sectional differences in the estimated value of innovation across countries.

Finally, for the seven countries in Table 5, we also examine the correlation of the estimated patent values based on the KPSS model with patent citation data and economic growth. All results are statistically similar to the ones described in Section 6. These findings indicate that within-country validity of the innovation measure is insensitive to the average level of patent values. Furthermore, this result implies that the differences in patent value estimates between the two models are essentially a scaling factor (to which statistical correlations are, of course, robust). Nevertheless, we reiterate that since these scaling factors vary strongly across the countries in Table 5, between-country validity of the innovation measure is dependent on reliable estimation of the average level of patent values.

Model comparisons: theoretical considerations

In this part, we provide a rather theoretical, comparative analysis of the regression model used in this study, given in (4), and the one proposed in Kogan et al. (2015), given in (14). In particular, we argue that the empirical approach in Kogan et al. (2015) exhibits undesirable estimation behavior. This behavior is caused by the use of *multi-period* returns at a *daily* frequency in the KPSS regression model (14), which implies that consecutive realizations of the dependent variable partially incorporate the same shocks.[52] Inherently, model (14) produces highly serially correlated error terms, which makes correct inference on the significance of regression coefficients problematic. More importantly, however, there are two implications for the estimation of coefficient γ in model (14).

First, the correct interpretation of γ becomes unclear due to the overlap of the return periods of consecutive realizations of the multi-period return variable. Specifically, the overlapping returns imply that realizations of the multi-period returns on the day(s) just prior to the patent grant day also partially include the patent-related shock to returns. Consequently, the patent grant dummy variable I_{fd} in (14) does not strictly set apart—although it should—realizations of the firm's stock return that do contain patent-related shocks from realizations that do not. Instead, the multi-period returns on days d for firm f for which $I_{fd} = 0$ also contain patent-related shocks. The implication is that γ does not purely measure the effect of the patent grant on the second moment of stock returns and cannot, therefore, be used to compute an unbiased estimate of signal-to-total-variance ratio. In particular, as a logical consequence, because the patent-related shocks are also present in the $I_{fd} = 0$ group, γ is expected to understate the average size of the effect of the patent grant day on the stock price movements.

Second, we should consider patents granted on consecutive days to the same company:

[52] To illustrate, consider the three-day returns from Monday to Wednesday (A), from Tuesday to Thursday (B), and from Wednesday to Friday (C), all in the same week. Clearly, return window A shares Tuesday's realization of the stock return with window B and Wednesday's realization with both window B and C, and we could make similar arguments for window B and C. Hence, the consecutive three-day returns partially incorporate the same shocks.

roughly 30% of the patent grant days in our sample across all countries are within two days from at least one other patent grant day. Moreover, the patent grant dummy variable is weakly serially correlated. The serial correlation implies that γ not only estimates the effect of the patent granted at the start of the return window, but is also affected by the shocks related to grant events in the near future. Therefore, γ is logically expected to overestimate the effect of one single patent grant day and as such, produce a biased estimate of the signal-to-total-variance ratio.

Because the model proposed in this study measures the stock price movements separately for each day by using daily returns, it does not suffer from the undesirable estimation behavior described above. Therefore, the differences between the estimation results of model (4) and (14) contain information about how the results are affected by the use of multi-period returns. Nevertheless, it is rather complicated to examine the impact of the implications of multi-period returns because, as described, the two potential biases have opposite signs. Moreover, the impact is largely dependent on the characteristics of the patent sample: both the frequency and the concentration of the patent grant events determine the size of the potential biases. Therefore, comparison of Table 2 and Table D.2 is not conclusive about the implications of using multi-period returns for the estimate of the signal-to-total-variance ratio.

However, an observation that does result from comparison of Table 2 and Table D.1 is that the KPSS model (14) detects a significant disturbance of the stock market associated with patent grants for South Korea, while model (4), used in this study, does not. Closer inspection of the South Korea sample reveals that this discrepancy is related to serial correlation. Despite the assumption—both made in this study and in Kogan et al. (2015)—that returns are serially uncorrelated, the second moment of multi-period returns may still be affected by serial correlation. In fact, the stock returns in patent grant event windows exhibit relatively strong serial correlation compared to days unrelated to patent grants. Consequently, the second moment of daily returns is not significantly higher around patent grant days (as measured by model (4)), but the second moment of two-day compounded returns following patent grants is (as measured by model (14)). Hence, this finding reveals an advantage of the KPSS model (14) concerning its ability to account for serial correlation in stock returns.

In contrast, however, the KPSS model (4) does not yield significant estimates of γ for Taiwan and Switzerland, whereas the model in this study does. This opposing result appears to be caused by the weaker ability of the KPSS model to isolate the stock market reaction to the patent grant. Specifically, as reported in Table 2, the market reaction to patent grants is concentrated on the one day after the patent was granted. The KPSS model naturally includes the return on the day of the patent grant itself, whereby it adds a certain degree of noise and hinders identification of the patent-related shock to returns. Hence, comparison of the estimation results in Table 2 and Table D.1 does not unambiguously indicate which model is preferred. However, as argued by the theoretical discussion in this section, unbiased estimation of the signal-to-total-variance ratio using model (4) is doubtful. Therefore, we conclude that a model based on daily stock returns should be the primary choice of prudent researchers. A secondary approach based on multi-period stock returns could be employed to allow for potential serial correlation in stock returns following patent grants.[53]

Finally, we should mention that the study in Kogan et al. (2015) only includes US patents, which are all granted on Tuesdays except if there is a federal holiday. Such patent sample features imply that one need not worry about consecutive patent grant days. Also, the day-of-the-week dummy variables in (14) can adjust for the fact that multi-period returns starting on, for instance, Mondays often include the shock related to patents granted on Tuesdays. As a result, the US-only patent sample is particularly friendly to the patent-level measure of innovation proposed in Kogan et al. (2015). Put differently, measurement of stock market reactions to patent grants becomes increasingly complicated once the sample includes patents from different patent offices: the typical consequence is not only that patent grant days occur on all days of the week, but also that widely diverse sets of patents can be granted on the same day. Both of such consequences greatly complicate isolation of the shock specifically related to a single patent grant.

7.2 Validity of the unconditional signal-to-total-variance ratio

Largely built on the approach proposed in Kogan et al. (2015), our patent value measure assumes a signal-to-total-variance that is constant across time and space. However, considering not only the large heterogeneity in the type of patents but also the sample period, the assumption of homogeneity of the signal-total-variance-ratio may lead to unreliable results. More importantly, however, the restrictive nature of our model may hinder significant estimation results if the assumption of homogeneity is invalid. Therefore, we test the validity of this assumption.

First, to examine the validity of the assumption for the time dimension, we divide our 41-year sample period into four, (almost) evenly divided subsample periods: 1973–1983, 1984–1993, 1994–2003, and 2004–2013. In these subsample periods, we estimate model (4) for the 15 countries with the largest patent sample size; for the other countries, the number of patent grants is too low for reliable estimation in four subsamples. Table E.1 in Appendix E reports the results. We find that the estimation results are sensitive to the selection of sample period: both the selected event window and the size of our estimate of the signal-to-total-variance ratio varies over time. One general observation is that the signal-to-total-variance ratio is a decreasing function of the time. Note that as the number of patent grants increases over time, this result is line with the finding in Section 5.2 that the signal-to-total-variance ratio is negatively correlated with patent grant frequency. Based on the results in Table E.1, we conclude that the assumption of a constant signal-to-total-variance ratio over time is generally not valid. In fact, results in Table E.1 suggest that due to this assumption, we understate our estimates of the signal-to-total-variance ratio—and consequently the patent values—in early years, whereas we overstate these in the recent years. Therefore, we suggest to perform estimation of the signal-to-total-variance ratio based on a moving estimation window. The length of the estimation window needs to be

[53] To allow for serial correlation using daily stock returns appears challenging. For many countries the patent sample has already proven to be of insufficient size for the estimation of a simple model such as given in (4). A more complicated model that specifies a dynamic structure for returns following patent grants, will likely suffer even more from the empirical complications encountered in this study.

carefully selected, considering that by decreasing the size of the sample, we lower the statistical confidence of the estimation results.

Second, the validity of the assumption of a constant signal-to-total-variance across patents is strongly dependent on the degree of heterogeneity in the patent sample. An important feature of the patent sample that we consider here is the distribution of patent grants across the days of the week. In particular, if patents grants are observed throughout the week, the assumption that the signal-to-total-variance ratio is equal across all these patents, raises concerns. Specifically, for patents granted late in the week, the grant event window is broken by weekend days, which likely affects the timing of the stock market reaction and also the size of the signal-to-total-variance ratio. To test the hypothesis, we repeat the estimation of model (4) for all 27 countries, now only including patents granted on Mondays, Tuesdays, or Wednesdays.

The results in Table E.2 in Appendix E show that model (4) detects significant stock price reactions for 14 out of the 27 countries when based on the restricted patent sample, as opposed to the 10 countries in our base case (see Table 2). In particular, we now also obtain significant results for France. The case of France shows that falsely assuming that the signal-to-total-variance ratio is constant across all patents may lead to insignificant results, even when the majority of the patents (71%) are significantly associated with an increase in the second moment of stock returns. Furthermore, comparison of Table E.2 to Table 2 reveals that the estimates of the signal-to-total-variance ratio are generally higher when only considering patents granted on Mondays, Tuesday, or Wednesdays. Moreover, the selected event window is affected by the sample restriction. Based on these findings, we may again conclude that the assumption of a constant signal-to-total-variance ratio is generally not valid.[54]

In sum, empirical results indicate that the assumption of a constant signal-to-total-variance ratio does not hold over time, or across patents that are granted on different days of the week. This finding suggests that the type of innovation measure studied in this thesis could be improved by relaxation of the restrictive assumptions on the signal-total-to-variance ratio. However, there is no straightforward rule for how one should do so because given the characteristics of each patent sample, a different approach may be preferable. In particular, the size of the patent sample will be an important constraint for the relaxation of the assumption of a homogeneous signal-to-total-variance ratio. Hence, here lie opportunities for future research.

As a final note: an interesting source of heterogeneity in the patent sample is related to patent offices. Specifically, the *local* time of the day when the patent grant is officially announced may be dependent on the geographic location and the policy of the patent office. Consequently, the

[54] We also experimented with allowing the signal-to-total-variance ratio to vary by patent type. In particular, we differentiate between national and international applications. International patents refer to patents that are filed under the international law treaty *Patent Coorporation Treaty* (PCT), which provides a unified procedure for filing patent applications internationally. PCT applications are often associated with innovations of high market potential (see van Zeebroeck and van Pottelsberghe de la Potterie, 2011). Consequently, the signal-to-total-variance ratio related to the international patent acceptances could be higher. We do not find support for the hypothesis: estimates of γ_l in model (4) are generally not significantly different for national and international patents. Furthermore, the direction of the inequality sign varies seemingly randomly. We note that, nevertheless, the average estimated value of international patents in our sample is approximately 55% higher than that of national patents, which does confirm that international patent applications are of higher market potential. Furthermore, Kogan et al. (2015) also experimented with estimation of γ dependent on firm size: the estimates are statistically similar across firm size quintiles, except for the smallest firms. Finally, other sources of relevant patent heterogeneity include the granting patent office and the technological field of the patented invention.

timing of the shock to returns related to a patent grant, and possibly also its size, is therefore dependent on the granting patent office. By this line of reasoning it becomes clear that large differences in the local times of the grant announcements—which is likely the case when studying a cross-country data set as in this study—can hinder significant estimation of homogeneous coefficients in model (4). However, we leave the testing of this hypothesis to future research.

8 Summary and conclusions

Using patent data for publicly listed firms across 27 countries from 1973 to 2013, this thesis investigates the applicability and usefulness of the type of innovation measure first introduced in Kogan et al. (2015), in a cross-country setting. This patent-level measure of innovation seeks to exploit information contained by stock price movements to estimate the economic value of patents. Specifically, our model measures the effect of news of patent grants on second moments of stock returns: a significantly positive effect indicates presence of patent-related stock price movements. Our model is partially successful in identifying such significant associations between patent grants and stock price movements: only for 10 countries we find significant estimates. The main problem concerns sample size: results indicate that a minimum of approximately 5,000 patent grants are required for reliable estimation, implying that our measure is not applicable for about half of the countries in this study. More concerning, however, our model does not detect significant stock price reactions to patents granted to firms in South Korea and France either, even though these are both among the top six largest countries in terms of patent sample size. This finding suggests that the stock price movements in these countries are, on average, not significantly associated with news about patent grants, and can therefore not be reliably exploited to measure the value of innovation. Hence, the type of measure in Kogan et al. (2015) seems only moderately applicable in a cross-country study.

For the 10 remaining countries, we construct our patent value estimates based on conditional expectations of the patent-related components of stock returns in a narrow event window following the patent grant. Consistent with previous findings in the literature, distributions of the estimated patent values are highly positively skewed. Furthermore, our patent value estimates display substantial variation, both within and between countries, and thus add a great degree of variability that is not observed in simple patent count measures. To validate their reliability, we test whether the patent-level measures of innovation are supported by traditional citation-based measures of patent quality. Only for United States, Germany, and Belgium, we find robust correlations between the number of citations received by a patent and our estimate of its economic value. The positive results clearly corroborate previous conclusions in the literature that high numbers of citations are associated with high market values of US and German patents. The implications of the negative results are, however, ambiguous. On the one hand, our results imply that the relationship between citations and market value does not generally apply; on the other hand, the lack of correlation between citations and our patent value estimates casts doubts on the reliability of our innovation measure.

To test the usefulness of our patent-level measures of innovation, we construct yearly aggregated, country-specific indices of innovation and examine their correlation with domestic economic growth. Our results suggest a significantly positive relationship between increases in the innovation indices and future domestic economic growth in the United States, Switzerland, Denmark, and Belgium. For the other countries, we do not find statistical evidence supporting this relationship. We stress, however, that further research is required to draw hard conclusions. In particular, closer, country-specific investigation of economic growth factors should ultimately decide whether negative results imply that the innovation index' true value is not correlated with economic growth, or that our innovation measure does not sufficiently reflect the true innovative activity. An interesting application of our innovation indices would be to study spillovers of the economic merits of innovation into foreign countries using a sophisticated panel model design; we leave this to future research. In sum, our findings support the conclusion in Kogan et al. (2015) that their type of innovation measure produces useful results when applied to a US sample. On the other hand, results do not provide statistical evidence that the usefulness generally applies to other countries.

Although our approach to modeling the stock returns after patent grants is largely built on suggestions in Kogan et al. (2015), this study proposes a number of relevant alterations to their model. In particular, instead of using multi-period returns, we rely on a set of daily stock returns, each individually considered, to measure the stock market reactions to patent grants. Comparing empirical results of both models, we conclude that the inherently limited ability of the approach in Kogan et al. (2015) to isolate stock market reactions severely distorts cross-sectional differences in the estimated value of innovation across countries. The model in Kogan et al. (2015) does, however, exhibit one desirable feature: it implicitly controls for serial correlation in stock returns, which proves to be helpful in identifying the patent-related shocks in South Korean stock returns. Finally, analysis of sensitivity to various sample restriction suggests that our innovation measure could be improved by relaxation of the assumption that the effect of patent grants on stock return volatility is constant across all patents. In fact, better control for heterogeneity in the patent grant type could help overcome the unsuccessful identification of the patent-related stock price movements in countries such as France. Hence, these findings provide opportunities for further research.

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Appendices

A Merging patent data with financial data: technical notes

This appendix provides technical background to Section 3. For the sake of replicability of this study, we explain in detail how we merge the patent data with the financial data and overcome issues that arise due to combining different databases.

A.1 Introduction

Our cross-country patent data set is an extract from the EPO Worldwide Patent Statistical Database ("Patstat") which offers bibliographic patent data from more than 100 patent offices and contains event dates for almost three million successful patent applications. The data set that was provided to us for the purpose of this study was already merged with all publicly traded companies in the Compustat databases for 32 countries.[1] In our data set, patenting companies are thus identified by *Global Company Keys* (GVKEY), the unique identifier for each company in the Compustat database. Furthermore, a country code variable, identifying the country where the company is primarily listed, was also previously appended to the data set.

To construct our innovation measure, we merge the patent data with stock market data. Since our patent data set identifies patenting companies by Compustat company codes, Compustat stock data has the advantage of being easily merged. However, Compustat daily security data are (only) available from 1983 for North American securities and from 1986 for securities traded outside North America. In comparison, the Center for Research in Security Prices (CRSP) database offers daily price data starting from 1926 for US securities; Datastream provides the best availability for non-US securities, providing daily security data starting from 1973 for most countries. In order to obtain the largest possible sample period, we thus merge patents granted to companies primarily listed in the United States with the CRSP database and all other patents with Datastream stock price data. We request data up to the end of 2013, the last year that is fully covered by our patent data set.

The construction of our innovation measure requires the following *daily* time series: stock returns excluding dividends, national market returns excluding dividends,[2] and market capitalization. From CRSP—for US securities—we can directly request holding period returns excluding dividends (CRSP code: RETX) and returns excluding dividends on the value-weighted market portfolio (CRSP code: VWRETX); we construct the market capitalization series by multiplying stock price (CRSP code: PRC) by shares outstanding (CRSP code: SHROUT). From Datastream—for all other securities—we derive firm stock returns from (for-capital-actions-adjusted) stock prices (Datastream code: P) and similarly, we calculate market returns from Datastream's total market equity price indices (Datastream series codes: TOTM [+ country

[1] I am very thankful to Wing-Wah Tham and Elvira Sojli for the harmonization of company names to match the patents from Patstat to companies in the Compustat databases, and I particularly thank Wing-Wah Tham for kindly sharing the data.

[2] Following Kogan et al. (2015), we use the 'market-adjusted-return model' (Campbell et al., 1997), that is, we compute the firm's market-adjusted return defined as the firm's return minus the market-portfolio's return. See also Section 4.

code]; datatype code: PI); the market capitalization series (Datastream code: MV) can be directly requested from Datastream. We request all market capitalization series in US dollars to ensure that all of our patent value estimates share one currency.

The remainder of this appendix is structured as follows. Appendix A.2 reports on the historical linking of patent companies, identified by GVKEY codes, to primary security issues identified by CRSP- or Datastream-compatible security identifiers. Appendix A.3 presents summary statistics on the merging of the patent grant events with stock market data.

A.2 Historically linking companies to primary security issues

In total, the patent data set links patents to 11,067 publicly traded companies in the Compustat Global database. As discussed in the previous section, we merge these patent data with stock market data from CRSP or Datastream, guided by the available sample period offered. In order to do so, we need to *historically* link the patenting companies, identified by Compustat GVKEY codes, to their primary stock issues, identified by security-level identifiers used by CRSP or Datastream.[3] There are, however, two complications involved in this procedure.

First, GVKEY codes cannot be directly converted into unique identifiers used by CRSP or Datastream. To overcome this issue, we rely on databases that have established links between these identifiers. CRSP offers the CRSP/Compustat merged database, which we use to match the GVKEY codes of companies primarily listed in the United States to PERMNO codes, CRSP's unique issue identifier. For companies primarily listed outside the United States, we need to match the GVKEY variable to Datastream-compatible identifiers. Compustat North America (the United States and Canada) links GVKEY codes to 9-digit CUSIP security identifiers, which are simply converted to Datastream "local codes" by removing the last digit (effectively, transforming to 8-digit CUSIP), and placing a "Q" in front. Compustat Global (outside North America) offers established links between GVKEY codes and SEDOL security identifiers, which can be directly used to request data from Datastream.

Second, more problematically, established links between unique company-level identifiers (GVKEY) and unique security-level identifiers are not one-to-one: company-level identifiers oftentimes link to multiple security-level identifiers due to reissuing and cross-listing of stocks. Unfortunately, there is oftentimes no data available that *historically* identify primary issues, that is, there is generally only information about which issue is *currently* primarily listed. To overcome this, we come up with a number of rules to decide which price series of the available stocks (likely) concern primary issues. The following sections highlight these rules and decisions.

Dependent on the country in which a company is primarily listed, we make use of different matching procedures. Specifically, we divide the patent data into three *subsets*: the United States, Canada and the rest of the world. Accordingly structured, the following sections provide a technical overview of the matching procedure and its results. Appendix A.2.1 discusses the linking of GVKEY codes to primary US stock issues in CRSP. Appendix A.2.2 reports on the linking of GVKEY codes to primary Canadian stock issues in Datastream by use of Compustat

[3] We do not consider cross-listed stocks. See Section 3.

North America. The linking of GVKEY codes to primary stock issues listed in the rest of the world by use of Compustat Global is presented in Appendix A.2.3. However, companies could have been primarily listed in more than one country in our sample period and consequently be contained by multiple subsets. In fact, 111 companies have been primarily listed within the geographic domain of two subsets. To prevent overlap of stock price series that belong to the same company, we need to identify the *global* primary stock issue. Appendix A.2.4 discusses the topic in detail.

A.2.1 Companies primarily listed in the United States

We use the "link table" from CRSP/Compustat's merged database to link the 4,671 GVKEY codes that concern patenting companies primarily listed in the United States, to PERMNO codes—CRSP's issue identifier. In the full sample of the link table, the 4,350 (of the 4,671) GVKEY codes link to 4,549 *primary-issue* PERMNO codes.[4] For these PERMNO codes, we request financial stock data including market-portfolio returns (excluding dividends) from CRSP.

To merge the financial data with patent events assigned to GVKEY codes, we need to organize the financial data such that every GVKEY code *uniquely* identifies its stock price series. Usefully, the CRSP/Compustat link table provides historically linked GVKEY and PERMNO series for time periods that vary per company. We refer to these time periods as *link ranges.* However, CRSP oftentimes provides financial data outside the available range of the link table. To not be constrained by the available link ranges and to thereby prevent loss of patent data, we extend the ranges provided by the link table. We argue that we may safely do so because each PERMNO code only links to one PERMCO code—CRSP's company identifier. That is, we can be confident that stock price data outside the link range concern the same companies as within the link range.[5] For GVKEY codes that link to only a single PERMNO code, we can simply extend the GVKEY-PERMNO link ranges in both directions. For GVKEY codes that link to multiple PERMNO codes, we extend the link range of the first-available GVKEY-PERMNO link back in time and the link range of the last-available GVKEY-PERMNO forward in time. The resulting series are uniquely identified by 4,346 GVKEY codes (linking to 4,545 security codes). There are 234 occurrences of a change of (primary) issue within the series of a company.

A.2.2 Companies primarily listed in Canada

We use Compustat North America to link the 262 GVKEY codes that concern patenting companies primarily listed in Canada, to CUSIP security codes. We request monthly CUSIP series for the full available sample (beginning in 1962) to obtain all historically available GVKEY-CUSIP

[4] There are different types of links between CRSP's company and issue identifiers PERMCO and PERMNO, and Compustat's GVKEY codes. Because we focus on primary issues, we only use the primary link types LC ("Link research complete[,] [s]tandard connection between databases"), LU ("Unresearched link to issue by CRSP"), and LN ("Primary link exists but Compustat does not have prices").

[5] Extending the link ranges could possibly lead to including cross-listed stocks in our data. However, this should not significantly affect the estimates of our patents as the principle of no arbitrage predicts that stock movements are equal across issues.

links.[6] Within this entire sample, the 259 (of the 262) GVKEY codes link to 322 CUSIP codes. We filter out observations that (almost) surely belong to cross-listed stocks: if there are multiple observations per firm-day, we only keep the observation belonging to the "most primary" IID tag—Compustat's issue identifying number—giving precedence to Canadian issues over US issues.[7] After these restrictions, three CUSIP codes remain that correspond only to US securities. We drop these three CUSIP codes because considering that all companies within this subset are primarily listed in Canada, they should concern cross-listed stocks. For the 270 CUSIP codes that remain, we request stock data from Datastream. However, 25 CUSIP codes return an error because either the corresponding local code was not recognized or there was no data available. Hence, we retrieve stock price data for 245 securities from Datastream, and we append Canada's total national market index provided by Datastream to the data set.

To merge the financial data with patent events assigned to GVKEY codes, we need to organize the financial data such that every GVKEY code *uniquely* identifies its stock price series. We may use the earlier obtained GVKEY-CUSIP combinations to link CUSIP codes back to GVKEY codes because every CUSIP security code links to only one GVKEY company code. The 245 CUSIP codes available in Datastream link back to 239 GVKEY code as six pairs of two CUSIP security codes link to the same GVKEY company code. For four of these six GVKEY codes, security price series that belong to the same company exhibit overlapping time periods. In these periods of overlap, we select the security that is currently identified as primarily listed by Compustat. This leads to the elimination of two CUSIP codes because the available data ranges of these securities lie entirely within the range of other primary securities. The resulting series are uniquely identified by 239 GVKEY codes (corresponding to 243 security codes). There are four occurrences of a change of (primary) issue within the series of a company code.

A.2.3 Companies primarily listed outside the United States and Canada

We use Compustat Global to link the 6,245 GVKEY codes that concern patenting companies primarily listed in Canada, to SEDOL security codes. We request daily SEDOL series for the full, available sample (beginning in 1985) to obtain all historically available GVKEY-SEDOL links. Within this entire sample, the 6,245 GVKEY codes link to 7,659 SEDOL codes. Repeating the methodology previously discussed in Appendix A.2.2, we filter out observations that (almost) surely belong to cross-listed stocks: if there are multiple observations per firm-day, we only keep the observation belonging to the "most primary" IID tag—Compustat's issue identifying number.[8] For the 7,102 SEDOL codes that remain, we request stock data from Datastream. However, 442 SEDOL codes return an error because either the SEDOL code was not recognized

[6] As mentioned in Appendix A.1, daily security data are available from 1983 in Compustat North America. However, Compustat North America (in contrast to Compustat Global) also offers monthly data from 1962, which are thus preferred for the purpose of historically linking GVKEY to CUSIP.

[7] For the Compustat North America database, the IID variable labels issues by "01", "02", "03", and so forth for US securities, and "01C", "02C", "03C", and so forth for Canadian securities. The lowest IID tag, where we treat "01" as "lower" than "01C", (likely) gives the primary issue because it relates to the stock that was issued first. When securities are reissued, primary issue IID tag could be higher than "01C"; however, in case of duplicate firm-days, we may assume that the securities with the "lowest" IID-value concern primary issues.

[8] For the Compustat Global database, the IID variable labels issues by "01W", "02W", "03W", and so forth. See Appendix A.2.2 for the rationale behind this approach.

or there was no data available. For four other SEDOL codes, the price series was missing entirely. Hence, we retrieve data for 6,656 securities from Datastream.

We use total national market indices provided by Datastream to calculate abnormal returns (see Section 3). To append the relevant total market indices to our data set, we need to identify securities' national markets. For this, Datastream only provides an indirect way: we first request the local index names and subsequently link these to the country of their local market. For 6,634 of the 6,656 SEDOL codes, Datastream provides local index names and we can thus utilize this two-step method to merge security-level data with the relevant total market index.[9] For the remaining 22 securities, we manually identify their national market. Most total market indices are available from 1973, but for some countries, the availability is more limited, with sample start dates ranging up to 1992. Although total market index data cannot, therefore, be appended to all observed firm stock prices, data loss is minor: on average the sample period is shortened by rougly one year, and overall only 0.5% of our daily stock price quotes are lost.[10] However, since there is no total market index available for Iceland, we lose all four Icelandic securities; thus, 6,652 unique securities remain.

To merge the financial data with patent events assigned to GVKEY codes, we need to organize the financial data such that every GVKEY code *uniquely* identifies its stock price series. We may use the earlier obtained GVKEY-SEDOL combinations to link SEDOL codes back to GVKEY codes because every SEDOL security code links to only one GVKEY company code. The remaining 6,652 SEDOL codes link back to 6,060 GVKEY codes; around 8% of the GVKEY company codes link to multiple (typically two) SEDOL security codes. For 462 GVKEY codes, the security price series that belong to the same company exhibit overlapping time periods. In these periods of overlap, we select the security that is currently identified as primary by Compustat. However, not in all of these overlapping periods the current primary issue (already) exists and consequently, for 17 GVKEY codes, periods of overlapping security data remain. We resolve the remaining periods of overlap by only keeping the security for which its time series is the longest relative to the other securities in its overlapping period. For two companies this rule is not decisive and we manually eliminate series based on the comparison of stock price quotes at the end of the period overlap and the beginning of the subsequent non-overlapping period. The resulting series are uniquely identified by 6,064 GVKEY codes (corresponding to 6,192 SEDOL security codes). There are 134 occurrences of a change of (primary) issue within the series of a company code.

A.2.4 Merging all financial data: global primary issues

Our approach to linking companies to their primary stock issues has one important limitation: even though we select issues that are "most primary" within the geographic domain of each sub-

[9] There are two Chinese market indices available: one for the A-share market (Datastream code: TOTMKCA) and one for the H- and B-share market (Datastream code: TOTMKCH). A-shares are generally only available for purchase by mainland citizens; H- and B-shares are available to both domestic and foreigner investors. As all of our securities concern A-shares, we append TOTMKCA price index data to the price series of the Chinese securities.

[10] Lost data mostly corresponds to Brazilian stocks (for which security data are available from 1990, but total market index from 1994) and South Korean securities (for which security data are available from 1984, but total market index from 1987).

set (the United States, Canada, and the rest of the world), when companies have been primarily listed in different countries, these issues need not be the *global* primary issues. To illustrate, our data gathering and filtering approach may yield stock price series of both a Canadian and a US "primary" issue, both belonging to the same company.

To reveal the periods in which stock price series belonging to the same company overlap, we collect the 103 companies for which we retrieved data in more than one subset; in fact, each company appears in precisely two subsets. We merge the price series of the 214 corresponding stock issues. Clearly, within each period of overlap, only one of two issues, each from a different subset, can be the globally primarily listed stock. However, precisely because the data are sourced from different databases, there is no overall variable discriminating between the primary status of issues from different databases. Therefore, we apply the following rule to decide between overlapping issues: for every overlapping period in which a company is listed in the geographic domain of more than one subset, we only keep the issue that belongs to the subset that geographically contains the country where this company is incorporated.[11] The motivation for the decisive rule is that we typically observe that non-US companies, next to being traded on their local exchange, are cross-listed in the United States.

A.3 Merging patent data with stock market data: summary statistics

The complete set of all requested financial data consists of the primary stock price series of 10,542 unique companies, listed across 30 different countries. Notice that Appendix A.1 states that the original patent data set identifies 32 countries. However, Ireland and Iceland are lost in the merging process. The linking procedure outlined in Appendix A.2 matches all allegedly-Irish companies to stocks traded on the London Stock Exchange. Consequently, all these companies are from then on regarded as British companies. And, as mentioned in Appendix A.2.3, we lose the four Icelandic companies because there is no Icelandic total market index available. Furthermore, we note that the data set in the main text includes 27 countries because we exclude countries for which we observe less than 50 patent grant events. Specifically, Greece, Hungary, and Malaysia are excluded from the empirical analysis in the main text.

Our patent data set—the extract from the Patstat database—contains 2,930,304 successful patent applications. However, for approximately 10% of the granted patents in our data set, we cannot identify the patent grant date; thus, 2,623,443 successful patent applications remain. Because some patents are filed by (and thus granted to) multiple companies, the 2,623,443 applications correspond to 2,651,869 grant *events*, each uniquely identified by the combination of a patenting company and a patent application number.

Because both financial and patent data are now uniquely identified by Compustat's GVKEY company code, we can easily merge the stock data with the patent grant events. We merge the patent events with the financial data based on the grant date and the GVKEY code of the patenting company; there may be multiple patents granted to the same company on the same

[11] Before we considered the periods of overlap, we eliminated all stock price series of issues that are not available at times that patents are granted to the corresponding company. This way we minimize loss of patent data. We note that this is only important for a small number of companies as for the majority of the cases, both series of the same company are available during times of patent activity.

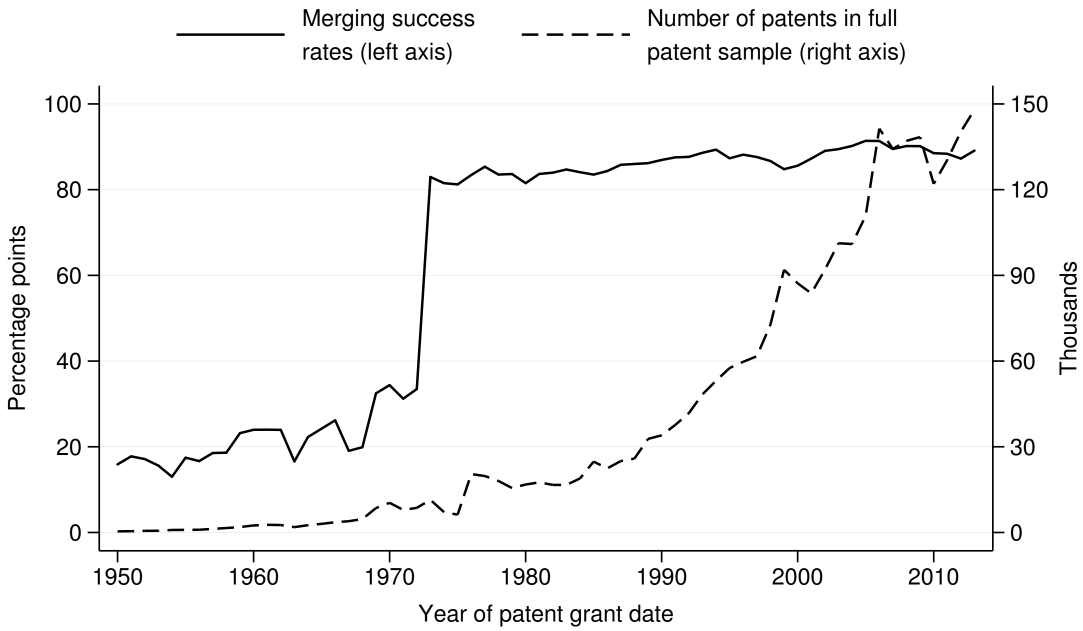

FIGURE A.1: Success rates on the merging of patent grant events with stock market data in the period 1950–2013. The dashed line plots the total number of patents granted per year in the full, initial patent sample.

date. We successfully merge 85% of all patent grant events in our patent data set with financial data—that is, 2,261,266 patent grant events remain after the merging process. There are three sources of data loss: (i) not all patenting companies can be linked to (primary) stock price series; (ii) a small portion of the patents are granted outside the available data ranges offered by the financial databases; and (iii) the patent sample includes patents granted to companies at times when they were not publicly traded. The remainder of this section investigates the merging success rates over time and across countries.

First, we examine the merging performance over time. Figure A.1 displays the percentages of patent grant events that we can successfully merge with stock data series for all patents granted in the years from 1950 to 2013.[12] We observe a sharp increase in the merging success rate in 1973, which marks the first year of available daily security data in Datastream—the database we use for all stocks listed outside the United States. From 1973, merging success rates remain fairly constant over time, in between 80% and 90%. Hence, we expect that yearly-aggregated measures of innovation in the post-1973 period do not suffer from substantial bias due to patent data loss.

Second, we assess the success rates of our merging endeavors across the countries where the patenting companies are primarily listed. This is important for the purpose of measuring innovation in a cross-country setting because varying degrees of data loss per country could

[12] The sample period shown in Figure A.1 is truncated. Prior to 1950, numbers of successfully merged patent grant events are negligible: we merge 136 out of the 27,949 (0.5%) grant events dated before 1950. Furthermore, the sample period studied in this paper ends in 2013, the last year that is fully covered by the patent data set. As a consequence, none of the 8,999 patents granted in 2014 can be merged and Figure A.1 inherently excludes the year 2014.

Table A.1: Performance statistics on the merging of patent data with stock market data across countries. Part I gives the number of patent grant events in the full sample of our patent data set. Part II gives the number of patents granted within data ranges generally available for the requested financial series. The ratio of II to I thus makes explicit the loss of patent data due to the limited ranges of daily security data. Part III presents merging *success rates*: its second column compares the number of successful merges with the number of grant events within the data ranges, that is, III relative to II; its last column presents *cumulative* success rates, that is, III relative to I. To display performance of our merging procedures (and not the quality of the financial data), this table considers patent grant events merged with gaps in the financial data.

	I: *Full patent sample*	**II**: *Within available range of financial data*		**III**: *Successfully merged*		
Country	Number of grants	Number of grants	II/I (in %)	Number of grants	III/II (in %)	III/I (in %)
Australia	2,449	2,424	99.0	2,086	86.1	85.2
Austria	5,028	4,830	96.1	2,226	46.1	44.3
Belgium	11,707	11,025	94.2	7,910	71.7	67.6
Brazil	611	560	91.7	480	85.7	78.6
Canada	9,049	8,967	99.1	5,948	66.3	65.7
China	4,798	4,784	99.7	4,471	93.5	93.2
Denmark	14,872	14,603	98.2	13,489	92.4	90.7
Finland	27,094	25,610	94.5	22,637	88.4	83.5
France	95,289	92,695	97.3	76,397	82.4	80.2
Germany	312,015	266,907	85.5	243,034	91.1	77.9
Greece[a]	46	46	100.0	39	84.8	84.8
Hong Kong	173	172	99.4	151	87.8	87.3
Hungary[a]	14	12	85.7	4	33.3	28.6
Iceland[b]	452	0	0.0	0	-	0.0
India	2,477	2,447	98.8	2,362	96.5	95.4
Israel	1,652	1,550	93.8	1,359	87.7	82.3
Italy	14,062	12,652	90.0	8,448	66.8	60.1
Japan	685,161	681,404	99.5	660,304	96.9	96.4
Malaysia[a]	31	31	100.0	29	93.5	93.5
Netherlands	25,139	24,527	97.6	22,639	92.3	90.1
New Zealand	982	919	93.6	639	69.5	65.1
Norway	4,315	4,196	97.2	3,679	87.7	85.3
Singapore	1,802	1,780	98.8	1,745	98.0	96.8
South Africa	354	349	98.6	313	89.7	88.4
South Korea	124,087	123,674	99.7	102,411	82.8	82.5
Spain	1,172	872	74.4	803	92.1	68.5
Sweden	22,026	18,513	84.1	15,625	84.4	70.9
Switzerland	55,268	43,772	79.2	40,224	91.9	72.8
Taiwan	109,242	108,885	99.7	100,715	92.5	92.2
United Kingdom	39,062	36,987	94.7	30,895	83.5	79.1
United States	1,081,440	1,069,814	98.9	890,204	83.2	82.3
Total	2,651,869	2,565,007	96.7	2,261,266	88.2	85.3

Notes: "country" refers to the country where the patenting company is primarily listed; the 2,651,869 patent grants events presented in the table correspond to 2,623,443 patent applications because patent applications may correspond to multiple grant events when granted to multiple companies.

[a] Countries for which we observe less than 50 patent grant events are excluded from the data set in the main text.

[b] The required financial data is unavailable for Iceland as Datastream offers no Icelandic total market index.

lead to skewed images of the relative economic values of national innovation. Table A.1 reports performance statistics on the merging of patent data with stock market data for 31 countries—we include Iceland for completeness. Table A.1 aims to display the performance of the matching procedures outlined in this appendix, in contrast to the quality of the financial data. Therefore, Part II in Table A.1 makes explicit the number of patent grant events lost in the merging process due to the limited data ranges offered by the employed financial databases for the market-adjusted stock return and market capitalization series. Subsequently, Part III in Table A.1 reports the number of patent grant events we fail to merge with financial data within the available data ranges.[13] Furthermore, to prevent that perfect success rates are infeasible by construction, Table A.1 also considers patent grant events merged with (regular or irregular) gaps in the financial data series to be successful. However, because the required financial data are not available for the data gaps, the number of successfully-merged patent grant events cannot be interpreted as the number of patents of which we estimate the economic value.

Based on the statistics given in columns of I and II in Table A.1, we find that the loss of patent data due to the available data ranges offered by the financial data sources is minor. A critical eye could nonetheless be drawn to Spain and/or Switzerland: the relatively high loss of data for Spain results from the unavailability of daily data for Spanish stocks prior to 1987; we lose about 20% of the Swiss patents because these were granted before 1973 (the first year generally available in Datastream). Furthermore, the merging success rates presented by the second column of Part III in Table A.1 show that for the large majority of countries, we lose less than 20% of the patents within the available financial data range. Moreover, only for patents assigned to companies primarily listed in Austria or Hungary, our merging endeavors result in success for less than 50% of the cases. For both countries, the low success rates can be explained by the public status of the patenting companies: although all patents in the patent data set are granted to companies that are publicly listed today, these companies were not yet public when the majority of the patents were granted to them.[14] The last column in Table A.1 presents *cumulative* success rates.[15] We conclude that the cumulative success rates are sufficiently high and constant across a very large majority of the countries.

[13] Ideally, we would also account for the period in which each company is publicly listed. However, precisely for the reason that our matching procedure does not link all of the patenting companies in the patent data set to security data from CRSP or Datastream, historical information on the public status of all companies is not readily available to us. Consequently, we cannot make explicit the number of patent grant events lost in the merging process because the patenting company was not public at the time.

[14] A good illustrative example concerns Voestalpine AG: 25% of all patents in our patent data set that are granted to companies primarily listed in Austria, are granted to Voestalpine AG. However, 90% of these patents were granted before 1995, while Voestalpine AG is listed on the Vienna Stock Exchange since 1995.

[15] The success rates presented in Figure A.1 also concern cumulative success rates.

B Analytical derivation of model equations

In this appendix, we derive the statistical results from Section 4 that constitute essential building blocks for the construction of patent value estimates.[16] First, Appendix B.1 derives the expectation and variance of the patent-related component of stock returns conditional on the observed market-adjusted stock return. Second, Appendix B.2 shows the relationship between the regression coefficient of model (4) and the signal-to-total-variance ratio. Third, Appendix B.3 proposes a method of estimating the variance of the noise component of stock returns. For the sake of this appendix's independence, we restate some of the necessary model and variable definitions.

B.1 Conditional expectation and variance of patent-related stock returns

The expectation and variance of the component of stock returns that is related to the value of the patent conditional on the observed stock return, as formulated in (2) and (8), follows from the distributional assumptions made in Section 4. In this appendix, we derive these results in detail.

We decompose the daily market-adjusted returns r_{jl} on the lth day after patent j was granted to a given firm as follows:

$$r_{jl} = s_{jl} + \varepsilon_{jl}, \qquad l \in \{0, 1, 2\}, \tag{B.1}$$

where s_{jl} denotes the component of the firm's stock return that is due to the market response to the grant of patent j (the "signal" component), and ε_{jl} denotes the component of the firm's stock return that is unrelated to the patent (the "noise" component). We denote the three-day cumulative (components of) returns after the grant of patent j by r_j, s_j and ε_j. We make the following assumptions about the distributions of s_{jl} and ε_{jl}. Second moments of both s_{jl} and ε_{jl} to be firm-f- and year-t-dependent, and the second moment of s_{jl} may vary across days l, for $l \in \{0, 1, 2\}$. Hence, $\mathrm{E}[s_{jl}^2] = \sigma^2_{s_l ft}$ and $\mathrm{E}[\varepsilon_{jl}^2] = \sigma^2_{\varepsilon ft}$. Furthermore, s_{jl} and ε_{jl} are independently distributed and are serially uncorrelated. Therefore, $\mathrm{E}[(s_j^+)^2] = \sum_{l=0}^{2} \sigma^2_{s_l ft}$ and $\mathrm{E}[\varepsilon_j^2] = 3\sigma^2_{\varepsilon ft}$, where s_j^+ and ε_j denote the total market reaction and noise terms in the event window, respectively.[17] Since the market value of patent j is always positive, s_j^+ follows a normal distribution truncated at zero, $s_j \sim \mathcal{N}^+(0, \sum_{l=0}^{2} \sigma^2_{s_l ft})$, and the noise term is normally distributed, $\varepsilon_j \sim \mathcal{N}(0, 3\sigma^2_{\varepsilon ft})$.[18]

We are interested in the expectation of s_j^+ conditional on the observed r_j: $\mathrm{E}[s_j^+ | r_j]$. To derive this conditional expectation, we first note that the truncated distribution of s_j^+ is a conditional distribution. That is, if $x \sim \mathcal{N}(\mu_x, \sigma_x^2)$, then the density (denoted by $f(\cdot)$) of its normal distribu-

[16] In line with the rest of this paper, the notation in this appendix follows upon the conventions in the field of econometrics, as opposed to the standard notation in statistics. In particular, we simply write lower case letters for both random variables and particular realizations of these random variables.

[17] While we simply write s_j in the paper, we write s_j^+ in this section for the sake of notational convenience.

[18] We note that the second moment of a mean-zero normal distribution, truncated at zero, is (simply) equal to σ^2 as its mean and variance are equal to σc_0 and $\sigma^2(1 - c_0^2)$, where $c_0 = \phi(0)/\Phi(-0)$.

tion that is left-truncated at some constant a is given by (see, e.g., Greene, 2012, p. 834):

$$f(x|x>a)=\frac{f(x)}{\Pr[x>a]}=\frac{\frac{1}{\sigma_x}\phi\left(\frac{x-\mu_x}{\sigma_x}\right)}{1-\Phi\left(\frac{a-x}{\sigma_x}\right)}, \tag{B.2}$$

where $\phi(\cdot)$ is the standard normal probability density function (pdf) and $\Phi(\cdot)$ the standard normal cumulative distribution function (cdf). Hence, we may write:

$$f(s_j^+|r_j)=f(s_j|r_j,s_j>0)=\frac{f(s_j|r_j)}{\Pr[s_j|r_j>0]}, \tag{B.3}$$

where $s_j\sim\mathcal{N}(0,\sigma^2_{s_lft})$. This implies that the distribution of $s_j^+|r_j$ simply behaves as the distribution of $s_j|r_j$ scaled by the probability that $s_j|r_j$ is positive. Since we know that $f(x,y)=f(x|y)f(y)$, the same is true for the joint distributions $f(s_j,r_j)$ and $f(s_j^+,r_j)$.

The joint distribution of s_j and r_j follows from (B.1): because r_j and s_j can be written as linear functions of s_j and ε_j, which are both normally distributed, s_j and r_j have a bivariate normal distribution and the correlation coefficient $\rho_{s_jr_j}$ is given by:

$$\rho_{s_jr_j}=\frac{\mathrm{Cov}[s_j,r_j]}{\sqrt{\mathrm{Var}[s_j]\mathrm{Var}[r_j]}}=\frac{\mathrm{Cov}[s_j,s_j+\varepsilon_j]}{\sqrt{\mathrm{Var}[s_j]\mathrm{Var}[s_j+\varepsilon_j]}}=\frac{\sqrt{\sum_{l=0}^2\sigma^2_{s_lft}}}{\sqrt{\sum_{l=0}^2\sigma^2_{s_lft}+3\sigma^2_{\varepsilon ft}}}. \tag{B.4}$$

To derive the distribution of s_j conditional on r_j, we use the following results from statistics. If y_1 and y_2 are jointly normally distributed with $y_1\sim\mathcal{N}(\mu_1,\sigma_1)$, $y_2\sim\mathcal{N}(\mu_2,\sigma_2)$, and correlation coefficient ρ_{12}, we know that the conditional distribution of y_1 on y_2 is given by (see, e.g., Miller and Miller, 2004, p. 221):

$$y_1|y_2\sim\mathcal{N}\left(\mu_1+\frac{\sigma_1}{\sigma_2}\rho_{12}(y_2-\mu_2),\sigma_1^2\left(1-\rho_{12}^2\right)\right). \tag{B.5}$$

Hence, by filling the distribution parameters of s_j and $r_j=s_j+\varepsilon_j$, we derive:

$$\mu_{s_j|r_j}=\left(\frac{\sum_{l=0}^2\sigma^2_{s_lft}}{\sum_{l=0}^2\sigma^2_{s_lft}+3\sigma^2_{\varepsilon ft}}\right)r_j=\delta_{ft}r_j, \tag{B.6}$$

as $\mathrm{SD}[s_j]/\mathrm{SD}[r_j]$ equals $\rho_{s_jr_j}$, and,

$$\sigma^2_{s_j|r_j}=\left(\sum_{l=0}^2\sigma^2_{s_lft}\right)\left(1-\frac{\sum_{l=0}^2\sigma^2_{s_lft}}{\sum_{l=0}^2\sigma^2_{s_lft}+3\sigma^2_{\varepsilon ft}}\right)=\left(\frac{\sum_{l=0}^2\sigma^2_{s_lft}}{\sum_{l=0}^2\sigma^2_{s_lft}+3\sigma^2_{\varepsilon ft}}\right)\left(3\sigma^2_{\varepsilon ft}\right)=3\delta_{ft}\sigma^2_{\varepsilon ft}, \tag{B.7}$$

where δ_{ft} denotes the *signal-to-total-variance* ratio defined as,

$$\delta_{ft}=\frac{\sum_{l=0}^2\sigma^2_{s_lft}}{\sum_{l=0}^2\sigma^2_{s_lft}+3\sigma^2_{\varepsilon ft}}=\frac{\mathrm{E}[s_j^2]}{\mathrm{E}\left[(s_j+\varepsilon_j)^2\right]}. \tag{B.8}$$

Hence, we find that $s_j|r_j \sim \mathcal{N}(\delta_{ft} r_j, \delta_{ft} 3\sigma^2_{\varepsilon ft})$.

Finally, using (B.3) and the properties of a truncated normal distribution (see, e.g., Greene, 2012, p. 876), we obtain statistical results (2) and (8) given in the paper:

$$\mathrm{E}\left[s_j^+|r_j\right] = \mu_{s_j|r_j} + \sigma_{s_j|r_j}\lambda\left(\frac{\mu_{s_j|r_j}}{\sigma_{s_j|r_j}}\right) = \delta_{ft} r_j + \sqrt{3\delta_{ft}}\sigma_{\varepsilon ft}\lambda\left(\sqrt{\delta_{ft}}\frac{r_j}{\sqrt{3}\sigma_{\varepsilon ft}}\right), \tag{B.9}$$

and,

$$\mathrm{Var}\left[s_j^+|r_j\right] = \sigma^2_{s_j|r_j}\left[1-\kappa\left(\frac{\mu_{s_j|r_j}}{\sigma_{s_j|r_j}}\right)\right] = 3\delta_{ft}\sigma^2_{\varepsilon ft}\left[1-\kappa\left(\sqrt{\delta_{ft}}\frac{r_j}{\sqrt{3}\sigma_{\varepsilon ft}}\right)\right], \tag{B.10}$$

where $\lambda(z) = \phi(z)/\Phi(z)$ is the *inverse Mills ratio* (see, e.g., Cameron and Trivedi, 2005, p. 541), and $\kappa(z) = \lambda(z)[\lambda(z) + z]$.

B.2 Recovering the signal-to-total-variance ratio

The mathematical relation between signal-to-total-variance ratio δ and the regression coefficients γ_l, $l \in \{0, 1, 2\}$, as formulated in (5), follows from the specification of panel-model (4). This section derives the relation in detail.

As discussed in Section 4, we assume that $\sigma^2_{s_l ft}$ and $\sigma^2_{\varepsilon ft}$ vary in constant proportions, for each l. That is, we can write $\sigma^2_{s_l ft}$ as a linear function of $\sigma^2_{\varepsilon ft}$: $\sigma^2_{s_l ft} = \alpha_l \sigma^2_{\varepsilon ft}$. This implies that the signal-to-total-variance ratio is constant across firms and years:

$$\delta_{ft} = \frac{\sum_{l=0}^{2} \alpha_l \sigma^2_{\varepsilon ft}}{\sum_{l=0}^{2} \alpha_l \sigma^2_{\varepsilon ft} + 3\sigma^2_{\varepsilon ft}} = \frac{\sum_{l=0}^{2} \alpha_l}{\sum_{l=0}^{2} \alpha_l + 3}. \tag{B.11}$$

To derive δ, we first consider the implication of the decomposition of stock returns as given in (B.1): for every day d for firm f *unrelated* to a patent grant, $r_{fd} = \varepsilon_{fd}$, and for day l after a patent grant, $r_{fd} = s_{l,fd} + \varepsilon_{fd}$.[19] Then, since $s_{l,fd}$ and ε_{fd} are independently distributed, $\mathrm{E}[s^2_{l,fd}] = \sigma^2_{s_l ft}$, and $\mathrm{E}[\varepsilon^2_{fd}] = \sigma^2_{\varepsilon ft}$, the second moment of returns on days *unrelated* to patent grants is given by:

$$\mathrm{E}[\varepsilon^2_{fd}] = \sigma^2_{\varepsilon ft}, \tag{B.12}$$

and the second moment of returns on day l after a patent grant:

$$\mathrm{E}[(s_{l,fd} + \varepsilon_{fd})^2] = \sigma^2_{s_l ft} + \sigma^2_{\varepsilon ft} = (1 + \alpha_l)\sigma^2_{\varepsilon ft}. \tag{B.13}$$

It can now be easily seen that δ is directly related to the ratio of a sum of (B.12) over three days to the sum (B.13) over l for $l \in \{0, 1, 2\}$. Hence, δ can be derived from the multiplicative effects of the news of a patent grant, $(1 + \alpha_l)$ for $l \in \{0, 1, 2\}$, on the second moments of returns on

[19] The subscript "fd" replaces the subscript "jl" previously used in Appendix B.1 because we no longer restrict our attention to days after patent grants, but rather consider any daily market-adjusted return r_{fd} of firm f on day d. Nevertheless, the subscript l remains for the patent-related component of return, $s_{l,ft}$, to discriminate between days within grant event windows.

days in the grant event window. In order to measure the multiplicative effects, we regress the log squared daily market-adjusted returns r_{fd} of firm f on day d on a patent grant dummy variable, I_{fd}:

$$\log\left(r_{fd}^2\right) = \sum_{l=0}^{2} \gamma_l I_{f,d-l} + \boldsymbol{z}_{fd}'\boldsymbol{c} + \zeta_{fd}, \tag{B.14}$$

where $\boldsymbol{z}_{fd}$ is short-hand notation for the day-of-the-week and firm-year specific effects, and ζ_{fd} is an error term that is i.i.d. over f and d.

All other things held constant, model (B.14) measures the relative increase in the second moment of returns caused by the stock market reaction to a patent grant event that occurred l days in the past. Specifically, model equation (B.14) implies that:

$$\frac{\mathrm{E}\left[r_{fd}^2 | I_{f,d-l} = 1, \boldsymbol{x}_{fd} = \boldsymbol{x}_{fd}^*\right]}{\mathrm{E}\left[r_{fd}^2 | I_{f,d-l} = 0, \boldsymbol{x}_{fd} = \boldsymbol{x}_{fd}^*\right]} = \frac{\exp\left(\gamma_l + (\boldsymbol{x}_{fd}^*)'\boldsymbol{\beta}_{fd} + \frac{1}{2}\sigma_\zeta^2\right)}{\exp\left((\boldsymbol{x}_{fd}^*)'\boldsymbol{\beta}_{fd} + \frac{1}{2}\sigma_\zeta^2\right)} = e^{\gamma_l}, \qquad l \in \{0,1,2\}, \tag{B.15}$$

where $\boldsymbol{x}_{fd}$ denotes the vector of all explanatory variables in (B.14) apart from $I_{f,d-l}$ and $\boldsymbol{\beta}_{fd}$ denotes the vector of all regression coefficients in (B.14) apart from γ_l. Also, (B.15) uses that $\mathrm{E}[X] = \exp(\mu + \frac{1}{2}\sigma^2)$ for lognormally distributed variable X with parameters μ and σ. Hence, the news of a patent grant has a multiplicative effect on the second moment of returns that is constant across all days and all firms.

In order to derive an expression for δ, we must carefully examine how the multiplicative effect e^{γ_l} relates to the return decomposition made in (B.1). We first note that the effects of all other explanatory factors in model (B.14) are also multiplicative. Moreover, other explanatory variables do not alter the specific multiplicative effect of the news of patent grant l days in the past since there are no interaction effects. Thus, given the proportionality of $\sigma_{s_l ft}$ to $\sigma_{\varepsilon ft}$, we could postulate that all other effects on the second moments of returns are represented by fluctuations in $\sigma_{\varepsilon ft}$. Put differently, the noise component ε_l from (B.1) contains all information that affects the second moment of returns except what is specifically related to the patent that was granted l days in the past—which would be contained by s_l. In particular, ε_l also contains the stock price movements related to grant events other than l days in the past. This is relevant because grant event windows may intersect. To illustrate, we consider the example that firm f is granted patent P_0 on day d and patent P_1 on the day before, $d-1$. Now, on day d, the stock market reaction s_0 relates to patent P_0 and the corresponding noise component ε_0 contains the stock price movements related to P_1. Similarly, s_1 refers to the stock market reaction to patent P_1, which thus implies that ε_1 contains the stock price movements related to P_0.

Hence, the noise component ε_l in (B.1) should be interpreted to also include—apart from all other noise—stock price movements related to patents granted on days other than day $d-l$. This implies that regardless of the values of $I_{f,d-k}$ for $k \neq l$, we can write $r_{fd} = s_{l,fd} + \varepsilon_{fd}$ for

$I_{f,d-l} = 1$, and $r_{fd} = e_{fd}$ for $I_{f,d-l} = 0$. Thus, we have for $l \in \{0,1,2\}$:

$$\frac{\mathrm{E}\left[(s_{l,fd} + \varepsilon_{fd})^2 | \boldsymbol{x}_{fd} = \boldsymbol{x}^*_{fd}\right]}{\mathrm{E}\left[\varepsilon^2_{fd} | \boldsymbol{x}_{fd} = \boldsymbol{x}^*_{fd}\right]} = e^{\gamma_l} \quad \Longrightarrow \quad e^{\gamma_l}\mathrm{E}\left[\varepsilon^2_{fd}\right] = \mathrm{E}\left[(s_{l,fd} + \varepsilon_{fd})^2\right]. \tag{B.16}$$

Then, filling in the distribution parameters of $s_{l,fd}$ and ε_{fd}, we derive:

$$e^{\gamma_l}\sigma^2_{\varepsilon ft} = \sigma^2_{s_l ft} + \sigma^2_{\varepsilon ft} \quad \Longleftrightarrow \quad e^{\gamma_l} = \frac{\sigma^2_{s_l ft} + \sigma^2_{\varepsilon ft}}{\sigma^2_{\varepsilon ft}}. \tag{B.17}$$

Summing over l and taking the inverse:

$$\sum_{l=0}^{2} e^{\gamma_l} = \frac{\sum_{l=0}^{2} \sigma^2_{s_l ft} + 3\sigma^2_{\varepsilon ft}}{\sigma^2_{\varepsilon ft}} \quad \Longleftrightarrow \quad \frac{1}{\sum_{l=0}^{2} e^{\gamma_l}} = \frac{\sigma^2_{\varepsilon ft}}{\sum_{l=0}^{2} \sigma^2_{s_l ft} + 3\sigma^2_{\varepsilon ft}}. \tag{B.18}$$

Next, multiplying the result in (B.18) by three and using definition of δ given in (B.11) gives:

$$\frac{3}{\sum_{l=0}^{2} e^{\gamma_l}} = \frac{3\sigma^2_{\varepsilon ft}}{\sum_{l=0}^{2} \sigma^2_{s_l ft} + 3\sigma^2_{\varepsilon ft}} = 1 - \frac{\sum_{l=0}^{2} \sigma^2_{s_l ft}}{\sum_{l=0}^{2} \sigma^2_{s_l ft} + 3\sigma^2_{\varepsilon ft}} = 1 - \delta. \tag{B.19}$$

Finally, we obtain the expression for δ as given in (5) in the paper:

$$\delta = 1 - \frac{3}{\sum_{l=0}^{2} e^{\gamma_l}}. \tag{B.20}$$

One final point

Kogan et al. (2015) present a relation between γ and δ that is different from (B.20).[20] Namely, $\delta = (e^{\gamma} - 1)(1 - 2c_0^2 + e^{\gamma}c_0^2)^{-1}$, where $c_0 = \phi(0)/\Phi(-0)$, for standard normal pdf $\phi(\cdot)$ and cdf $\Phi(\cdot)$. The explanation for this deviation is suggested by the note made in (Kogan et al., 2015, p. 11): "This formula adjusts for the fact that the variance of a mean-zero normal, truncated at zero, is equal to $\sigma^2(1 - c_0^2)$." This quote explains the presence of the term c_0 in their alternative relation between δ and γ. More importantly, however, it also implies that the relation is based on the interpretation that γ_l measures the increase in the variance of stock returns during days following patent grants, instead of their second moment. However, because the variance of a truncated normal distribution does not equal its second moment, this interpretation of γ is false.[21]

B.3 Estimating the variance of the noise component of stock returns

This section presents the derivation of the relation between the second moment of stock returns and the variance of the noise component as given in (6).

[20] Note that Kogan et al. (2015) do not construct day-l-specific estimates of γ, and therefore the subscript l is omitted.

[21] Kogan, Papanikolaou, Seru, and Stoffman were informed about the concerns raised in this paper via e-mail. The authors verified the claim that their results rely on a false interpretation of γ and announced an updated version of their paper (personal communication, February, 2016).

In any year t, the stock returns r_{fd} of firm f contain noise components ε_{fd}, and may contain patent-related components $s_{l,fd}$, $l \in \{0,1,2\}$ (see the return decomposition in (B.1)). Consequently, the second moment of r_{fd} is a mixture of $\sigma^2_{s_l ft}$, $l \in \{0,1,2\}$, and $\sigma^2_{\varepsilon ft}$, that is a function of the fraction of trading days $d_{l,ft}$ that concern a day l in a grant event window. Specifically, since $s_{l,fd}$ and ε_{fd} are independently distributed, $\mathrm{E}[s^2_{l,fd}] = \sigma^2_{s_l ft}$, and $\mathrm{E}[\varepsilon^2_{fd}] = \sigma^2_{\varepsilon ft}$, the second moments can we written as:

$$\mathrm{E}\left[r^2_{fd}\right] = \sigma^2_{\varepsilon ft} + \sum_{l=0}^{2} d_{l,ft}\sigma^2_{s_l ft}. \tag{B.21}$$

As discussed in the previous section, we can write $e^{\gamma_l}\sigma^2_{\varepsilon ft} = \sigma^2_{s_l ft} + \sigma^2_{\varepsilon ft}$, $l \in \{0,1,2\}$. Therefore, we can derive:

$$\begin{aligned}
\mathrm{E}\left[r^2_{fd}\right] &= \left(1 - \sum_{l=0}^{2} d_{l,ft}\right)\sigma^2_{\varepsilon ft} + \sum_{l=0}^{2} d_{l,ft}\left(\sigma^2_{s_l ft} + \sigma^2_{\varepsilon ft}\right) && \text{(B.22)}\\
&= \left(1 - \sum_{l=0}^{2} d_{l,ft}\right)\sigma^2_{\varepsilon ft} + \sum_{l=0}^{2} d_{l,ft} e^{\gamma_l}\sigma^2_{\varepsilon ft} && \text{(B.23)}\\
&= \left(1 + \sum_{l=0}^{2} d_{l,ft}(e^{\gamma_l} - 1)\right)\sigma^2_{\varepsilon ft}. && \text{(B.24)}
\end{aligned}$$

Thus, we obtain the result from (6) in Section 4.2:

$$\sigma^2_{\varepsilon ft} = \frac{\mathrm{E}[r^2_{fd}]}{1 + \sum_{l=0}^{2} d_{l,ft}(e^{\gamma_l} - 1)}. \tag{B.25}$$

C Construction of patent value estimates: intermediate results

The table in this appendix corresponds to Section 5.3.

TABLE C.1: Distributions of intermediate results used for the construction of patent value estimates using equation (10) across 10 countries, ordered by the number of patents in the sample. Left columns report log market-adjusted returns in the selected event windows following patent grants. Right columns present the expected values of the patent-related components of returns conditional on the observed stock returns calculated by (2).

	United States		*Japan*		*Germany*		*Taiwan*		*Switzerland*	
	r_j	$\mathrm{E}[s_j\|r_j]$	r_j	$\mathrm{E}[s_j\|r_j]$	r_j	$\mathrm{E}[s_j\|r_j]$	r_j	$\mathrm{E}[s_j\|r_j]$	r_j	$\mathrm{E}[s_j\|r_j]$
Mean	0.0002	0.0031	−0.0001	0.0035	0.0001	0.0020	−0.0002	0.0037	−0.0001	0.0021
Std. Dev.	0.0353	0.0019	0.0352	0.0015	0.0180	0.0014	0.0223	0.0011	0.0139	0.0013
Perc.										
1st	−0.0983	0.0011	−0.0872	0.0017	−0.0492	0.0006	−0.0610	0.0016	−0.0367	0.0008
5th	−0.0469	0.0013	−0.0506	0.0021	−0.0234	0.0007	−0.0350	0.0020	−0.0179	0.0010
10th	−0.0311	0.0015	−0.0364	0.0022	−0.0153	0.0008	−0.0246	0.0024	−0.0120	0.0011
25th	−0.0138	0.0019	−0.0182	0.0026	−0.0063	0.0011	−0.0122	0.0030	−0.0054	0.0014
50th	−0.0003	0.0026	−0.0011	0.0033	−0.0001	0.0016	−0.0013	0.0036	−0.0002	0.0018
75th	0.0138	0.0037	0.0168	0.0042	0.0061	0.0025	0.0110	0.0043	0.0050	0.0024
90th	0.0328	0.0055	0.0378	0.0050	0.0156	0.0035	0.0261	0.0052	0.0122	0.0033
95th	0.0496	0.0068	0.0543	0.0056	0.0245	0.0043	0.0393	0.0058	0.0181	0.0043
99th	0.1027	0.0099	0.0975	0.0073	0.0510	0.0073	0.0621	0.0070	0.0369	0.0073
	$N = 853{,}594$		$N = 599{,}021$		$N = 224{,}823$		$N = 78{,}103$		$N = 35{,}661$	

	United Kingdom		*Finland*		*Denmark*		*Belgium*		*Canada*	
	r_j	$\mathrm{E}[s_j\|r_j]$	r_j	$\mathrm{E}[s_j\|r_j]$	r_j	$\mathrm{E}[s_j\|r_j]$	r_j	$\mathrm{E}[s_j\|r_j]$	r_j	$\mathrm{E}[s_j\|r_j]$
Mean	0.0001	0.0045	−0.0004	0.0033	−0.0003	0.0033	0.0001	0.0037	−0.0006	0.0066
Std. Dev.	0.0299	0.0029	0.0190	0.0016	0.0218	0.0018	0.0189	0.0014	0.0336	0.0036
Perc.										
1st	−0.0777	0.0017	−0.0669	0.0012	−0.0552	0.0012	−0.0494	0.0015	−0.0933	0.0020
5th	−0.0369	0.0020	−0.0274	0.0014	−0.0282	0.0013	−0.0269	0.0019	−0.0436	0.0025
10th	−0.0255	0.0022	−0.0178	0.0016	−0.0190	0.0016	−0.0187	0.0021	−0.0299	0.0029
25th	−0.0115	0.0028	−0.0075	0.0021	−0.0087	0.0022	−0.0087	0.0027	−0.0134	0.0041
50th	−0.0004	0.0038	0.0000	0.0030	0.0000	0.0029	−0.0002	0.0035	−0.0008	0.0059
75th	0.0111	0.0052	0.0078	0.0041	0.0085	0.0040	0.0086	0.0042	0.0110	0.0082
90th	0.0265	0.0072	0.0173	0.0056	0.0192	0.0054	0.0193	0.0053	0.0298	0.0111
95th	0.0398	0.0092	0.0272	0.0067	0.0285	0.0066	0.0289	0.0064	0.0451	0.0126
99th	0.0835	0.0167	0.0505	0.0082	0.0533	0.0102	0.0563	0.0088	0.0952	0.0176
	$N = 26{,}927$		$N = 20{,}985$		$N = 12{,}013$		$N = 7{,}214$		$N = 5{,}512$	

Notes: distributions are *across patents*, which implies duplicate values when multiple patents are granted on the same day to the same firm; r_j and $\mathrm{E}[s_j|r_j]$ concern the sum of log returns in the *country-specific* event windows, defined by the days on which we observe significant stock price reactions to patent grants (see Table 2); the table does not report $\mathrm{Var}[s_j|r_j]$ because of the extremely low order of magnitude (between $\times 10^{-7}$ and $\times 10^{-4}$); the 17 (out of the 27) countries in our sample that are not reported in this table, we do not find any significant stock price reactions to patent grants.

D KPSS model

The tables in this appendix corresponds to Section 7.1.

Table D.1: Fixed effects estimation results of model (14) for the 27 country-specific data panels, ordered by the number of patent grants in the sample. The table reports the estimated regression coefficients using L-period stock returns as dependent variable, for $L \in \{2, 3, 4\}$.

		Estimates of γ in (14) based on L-period returns		
Country	Number of patents	$L = 2$	$L = 3$	$L = 4$
United States	853,594	0.004	0.014**	0.011*
Japan	599,021	0.030***	0.024***	0.019**
Germany	224,823	0.027*	0.031*	0.027*
South Korea	94,424	0.049*	0.017	0.023
Taiwan	78,103	0.019	0.022	0.018
France	69,961	0.004	0.006	0.012
Switzerland	35,661	0.024	0.018	0.009
United Kingdom	26,927	0.061**	0.046*	0.027
Finland	20,985	0.099**	0.096**	0.036
Netherlands	20,736	0.048+	−0.013	−0.022
Sweden	14,307	0.013	0.000	0.031
Denmark	12,013	0.044	0.046	0.059+
Italy	7,477	−0.016	0.019	−0.025
Belgium	7,214	−0.013	−0.036	0.004
Canada	5,512	0.061+	0.118***	0.042
China	4,068	0.084	−0.037	−0.112
Norway	3,161	−0.010	0.022	−0.020
India	2,136	−0.030	0.030	−0.051
Austria	1,736	0.004	−0.046	−0.021
Australia	1,647	0.058	0.152**	0.100+
Singapore	1,530	0.058	0.022	0.076
Israel	1,123	−0.025	−0.040	0.003
Spain	694	−0.001	0.066	−0.063
New Zealand	586	0.111	−0.110	0.036
Brazil	368	0.021	−0.016	−0.193
South Africa	202	0.023	0.274+	0.235
Hong Kong	136	−0.113	0.139	0.079

Notes: although strongly correlated, the number of patents is not equal to the number of observations for which $I_{fd} = 1$ in (14) due to multiple grants per day; ***, **, *, and + indicate statistical significance at the 0.1%, 1%, 5%, and 10% levels; Driscoll-Kraay (DK) standard errors are robust to heteroskedasticity, autocorrelation, and cross-sectional dependence.

TABLE D.2: Effect of alternative relation between the signal-to-total-variance ratio δ and regression coefficient γ on the patent value estimates $\widehat{V}_j^{KPSS}$. Estimation of γ, and ultimately the construction of $\widehat{V}_j^{KPSS}$, is based on the KPSS model as in equation (14), using L-period returns. Relation (a) is used in this paper and derived in Appendix B.2; (b) is given in Kogan et al. (2015) and relies on a false interpretation of γ. The last column presents the difference between the mean patent value based on (a) and (b), relative to (a). Patent value estimates are given in million US dollars, deflated to 1982 price levels using the CPI.

			(a)	$\delta = 1 - e^{-\gamma}$	(b)	$\delta = \frac{e^{\gamma} - 1}{1 - 2c_0^2 + e^{\gamma} c_0^2}$	
Country	N	L	$\widehat{\delta}$	Mean $\widehat{V}_j^{KPSS}$	$\widehat{\delta}$	Mean $\widehat{V}_j^{KPSS}$	% Δ
United States	853,594	3	0.014	24.66	0.039	40.82	65.6
Japan	599,021	3	0.024	11.43	0.064	18.90	65.4
Germany	224,823	3	0.031	14.98	0.083	24.75	65.2
South Korea	94,424	2	0.048	6.81	0.128	11.23	65.0
United Kingdom	26,927	3	0.045	35.34	0.120	58.30	65.0
Finland	20,985	3	0.092	36.25	0.236	59.44	64.0
Netherlands	20,736	2	0.047	31.47	0.125	51.83	64.7
Denmark	12,013	4	0.057	31.27	0.151	51.55	64.9
Canada	5,512	3	0.111	23.44	0.282	38.24	63.2
Australia	1,647	3	0.141	25.51	0.351	41.35	62.1
South Africa	202	3	0.240	29.06	0.559	47.45	63.3

Notes: selection of return period L is based on the significance of model (14) using L-period returns, $L \in \{2, 3, 4\}$ (see Table D.1); $c_0 = \phi(0)/\Phi(-0)$, for standard normal pdf $\phi(\cdot)$ and cdf $\Phi(\cdot)$; countries are ordered by the number of patents in the sample; for the 16 (out of the 27) countries in our sample that are not reported in this table, model (14) does not detect a significant market reaction to patent grants.

E Sensitivity analysis

The tables in this appendix corresponds to Section 7.2.

Table E.1: Fixed effects estimation results of model (4) for the 15 country-specific data panels, based on four subsample periods, ordered by the total number of patent grants in the sample. The left part presents the distribution of the patent grants across the four subsamples. The middle part reports the selected event windows based on the days for which the estimates of γ_l, $l \in \{0,1,2\}$, in (4) are significantly positive at the 10% level. The right part presents the estimates of the signal-to-total-variance ratio $\widehat{\delta}$ constructed using (5).

		Share of total patent count (%)				*Selected event window*				$\widehat{\delta}$			
Country	Total no. of patents	'73–'83	'84–'93	'94–'03	'04–'13	'73–'83	'84–'93	'94–'03	'04–'13	'73–'83	'84–'93	'94–'03	'04–'13
United States	853,594	8.3	13.7	30.7	47.4	$\{2\}$	$\{1\}$	$\{1\}$	-	0.034	0.030	0.032	-
Japan	599,021	2.5	11.2	31.2	55.2	$\{1\}$	-	$\{2\}$	$\{1\}$	0.126	-	0.032	0.024
Germany	224,823	13.6	17.1	30.2	39.1	$\{1\}$	$\{2\}$	-	-	0.076	0.062	-	-
South Korea	94,424	0.0	0.4	19.1	80.4	-	-	-	-	-	-	-	-
Taiwan	78,103	0.0	0.1	18.3	81.6	-	-	$\{1\}$	-	-	-	0.095	-
France	69,961	4.3	9.8	29.4	56.6	$\{0\}$	-	$\{1\}$	-	0.105	-	0.058	-
Switzerland	35,661	9.9	11.2	26.3	52.6	$\{1,2\}$	-	-	$\{1\}$	0.109	-	-	0.081
United Kingdom	26,927	8.3	16.3	30.8	44.6	-	-	-	$\{1,2\}$	-	-	-	0.079
Finland	20,985	0.0	2.0	12.7	85.3	-	-	-	$\{1\}$	-	-	-	0.077
Netherlands	20,736	9.1	14.9	35.5	40.5	$\{1\}$	$\{2\}$	-	-	0.143	0.141	-	-
Sweden	14,307	0.6	6.4	41.3	51.8	$\{0\}$	-	-	-	0.425	-	-	-
Denmark	12,013	0.1	5.9	35.2	58.8	-	-	-	$\{2\}$	-	-	-	0.100
Italy	7,477	17.3	12.6	26.4	43.8	$\{1\}$	-	$\{2\}$	-	0.226	-	0.118	-
Belgium	7,214	4.8	10.1	41.2	43.9	$\{2\}$	$\{2\}$	-	-	0.253	0.162	-	-
Canada	5,512	5.0	11.3	35.6	48.1	$\{0,1\}$	-	-	$\{2\}$	0.285	-	-	0.082

Notes: although strongly correlated, the number of patents is not equal to the number of observations for which $I_{fd} = 1$ in (4) due to multiple grants per day; for the 12 (out of the 27) countries in our sample that are not reported in this table, the number of patent grants is too low for reliable estimation in four subsamples.

TABLE E.2: Fixed effects estimation results of model (4) for the 27 country-specific data panels, only including patents granted on Mondays, Tuesdays, or Wednesdays. In line with the rest of the paper, the countries are ordered by the *total* number of patent grants in the sample.

Country	Total no. of patents	Granted on Mo–We		$\widehat{\gamma}_0$	DK s.e.($\widehat{\gamma}_0$)	$\widehat{\gamma}_1$	DK s.e.($\widehat{\gamma}_1$)	$\widehat{\gamma}_2$	DK s.e.($\widehat{\gamma}_2$)	Joint sig. F-test (p)	Event window	$\widehat{\delta}$
United States	853,594	780,570	(91%)	−0.011	0.009	0.025***	0.007	0.013^{+}	0.007	0.000***	{1,2}	0.019
Japan	599,021	523,923	(87%)	0.010	0.008	0.016^{+}	0.008	0.037***	0.009	0.003**	{1,2}	0.026
Germany	224,823	161,574	(72%)	0.020	0.015	0.018	0.014	0.034*	0.015	0.055^{+}	{2}	0.034
South Korea	94,424	67,232	(71%)	−0.015	0.026	0.000	0.025	0.012	0.027	0.501	-	-
Taiwan	78,103	62,486	(80%)	0.025	0.022	0.048*	0.021	0.029	0.021	0.085^{+}	{1}	0.047
France	69,961	49,333	(71%)	0.029	0.019	0.021	0.018	0.055**	0.020	0.652	{2}	0.054
Switzerland	35,661	26,374	(74%)	−0.015	0.026	0.043^{+}	0.025	0.012	0.027	0.090^{+}	{1}	0.042
United Kingdom	26,927	23,027	(86%)	−0.004	0.023	0.054*	0.022	0.058**	0.022	0.003**	{1,2}	0.055
Finland	20,985	17,325	(83%)	−0.013	0.038	0.059	0.040	0.011	0.039	0.276	-	-
Netherlands	20,736	14,491	(70%)	−0.015	0.033	0.085**	0.033	0.086*	0.034	0.564	{1,2}	0.082
Sweden	14,307	11,513	(80%)	0.006	0.029	−0.011	0.028	0.010	0.029	0.685	-	-
Denmark	12,013	9,319	(78%)	0.037	0.034	0.053	0.034	0.072*	0.035	0.272	{2}	0.070
Italy	7,477	6,135	(82%)	−0.049	0.040	0.059^{+}	0.036	0.059	0.040	0.333	{1}	0.057
Belgium	7,214	5,591	(78%)	0.002	0.039	0.042	0.040	0.100*	0.040	0.229	{2}	0.095
Canada	5,512	5,097	(92%)	0.011	0.038	0.001	0.038	0.094**	0.035	0.081^{+}	{2}	0.090
China	4,068	3,994	(98%)	0.048	0.096	0.022	0.110	−0.361**	0.112	0.056^{+}	-	-
Norway	3,161	2,641	(84%)	−0.008	0.054	0.021	0.049	0.009	0.053	0.951	-	-
India	2,136	1,778	(83%)	−0.004	0.060	−0.023	0.060	0.058	0.057	0.742	-	-
Austria	1,736	1,326	(76%)	0.021	0.072	−0.007	0.075	0.129^{+}	0.068	0.664	{2}	0.121
Australia	1,647	1,428	(87%)	−0.035	0.075	0.113^{+}	0.067	0.072	0.071	0.546	{1}	0.107
Singapore	1,530	1,452	(95%)	0.042	0.093	0.094	0.091	0.058	0.093	0.872	-	-
Israel	1,123	1,028	(92%)	−0.078	0.084	0.095	0.076	0.121	0.080	0.354	-	-
Spain	694	515	(74%)	0.015	0.125	−0.064	0.112	0.019	0.112	0.912	-	-
New Zealand	586	521	(89%)	0.042	0.121	0.046	0.114	0.068	0.131	0.869	-	-
Brazil	368	298	(81%)	0.150	0.143	0.003	0.145	0.125	0.149	0.621	-	-
South Africa	202	171	(85%)	0.016	0.164	−0.239	0.190	−0.425^{+}	0.219	0.235	-	-
Hong Kong	136	130	(96%)	0.032	0.205	0.219	0.201	−0.166	0.220	0.634	-	-

Notes: although strongly correlated, the number of patents is not equal to the number of observations for which $I_{fd} = 1$ in (4) due to multiple grants per day; ***, **, *, and $^{+}$ indicate statistical significance at the 0.1%, 1%, 5%, and 10% levels; Driscoll-Kraay (DK) standard errors are robust to heteroskedasticity, autocorrelation, and cross-sectional dependence.