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Abstract

I provide evidence that successful startup patents lead to temporary increases in the unemployment rate, adding to the literature studying the effect of innovation on the labor market. Aggregating the instrumental variable approach of Farre-Mensa et al. (2020), which utilizes differences between randomly allocated patent examiners, at the US-county level, I run various dynamic panel specifications. By using data ranging from 2001 to 2014, I add to the literature, which typically focuses on theoretical insights or empirical results from the 20th century. I find that a single granted startup patent causes temporary increases between 0.04 and 0.42 percentage points in the county unemployment rates, but the cumulative effect on employment returns to 0 within five years. Finding no increases in mean county personal income per capita within five years, my results suggest potential welfare losses due to startup patents.

In meiner Arbeit präsentiere ich Ergebnisse, denen zufolge erfolgreiche Startup-Patentanmeldungen zu einem vorübergehenden Ansteigen der Arbeitslosigkeitsrate führt und ergänze damit die Literatur, die den Effekt von Innovationen auf den Arbeitsmarkt behandelt. Dafür aggregiere ich die Identifikationsstrategie von Farre-Mensa et al. (2020), die Differenzen zwischen zufällig ausgewählten Patentprüfenden ausnutzt, auf US-Bezirksebene und präsentiere die Ergebnisse verschiedener dynamischer Panelspezifikationen. Weil die verwendeten Daten aus den Jahren 2001 bis 2014 stammen, ergänze ich die Literatur, die ansonsten weitgehend aus theoretischen oder auf dem 20. Jahrhundert beruhenden Ergebnissen besteht, um ein aktuelles Ergebnis. Meinen Resultaten zufolge führt ein Startup-Patent zu einer vorübergehenden Erhöhung der Arbeitslosigkeitsrate in einem Bezirk um 0.04 bis 0.42 Prozentpunkte. Innerhalb von fünf Jahren fällt der kumulative Effekt auf die Beschäftigung aber wieder auf 0. Weil ich innerhalb desselben Zeitraumes keine Erhöhung des Bezirks-Durschnitts an Pro-Kopf-Einkommen finde, legen meine Ergebnisse Wohlfahrtsverluste nahe.

Contents

1. Introduction

At least since Solow (1956), technological progress lies at the center of long-run economic growth. While the innovative process is more of a black box in the Solow model itself, endogenous growth models since Romer (1986) try to shed light on this process and its effect. At the same time, innovation and its effect on the labor market have been used as an explanation of rising inequality in the form of skill-biased technical change (Katz and Murphy, 1992). Since then, both technical change as well as trade - increases in trade and the so-called "China shock" being another potential explanation of rising inequality - have been at the center of economist's interests.

I add to the existing literature on the impact of innovation on the labor market by estimating the causal effect of innovation in the form of patents on unemployment rates in a macro perspective. While Autor and Salomons (2018, p. 6) note "[directly estimating effects at the macro level] often suffers from underidentification and low statistical power", aggregating the identification strategy of Farre-Mensa et al. (2020) and their use of so-called "judge fixed effects" allows me to tackle this criticism of a macro approach on this question. Thereby, I am also adding a different identification strategy into the mix, as the effect of innovation on the labor market is either studied on industry level using shift-share instruments[1] (Autor et al., 2015) or relying on a combination of industry and country differences paired with timing considerations (Dao, Das, and Koczan, 2020; Autor and Salomons, 2018) or on a macro level using historically identified shocks and/or timing considerations (Shea, 1998; Baptista and Preto, 2007; Audretsch, Thurik, Stel, and Carree, 2008; Faria, Cuestas, and Mourelle, 2010; Alexopoulos, 2011; Bonnet, Aubry, and Renou-Maissant, 2015; Halicioglu and Yolaç, 2015; Alexopoulos and Cohen, 2016).

However, due to data availability, my thesis adds another twist to this topic: As publicly available patent data does not come with all the information needed for my analysis, I rely on the data made available by Farre-Mensa et al. (2020) with their replication files. Since Farre-Mensa et al. (2020) focus on startups, I have to limit my analysis to the same case. This leads to two interesting attributes of my analysis: First, the focus on new firms creates a link to theoretical arguments regarding labor market search and matching focusing on new matches (Aghion and

[1] In the literature they are also referred to as "Bartik" instruments, for a discussion of their use see e.g. Goldsmith-Pinkham et al. (2020).

Howitt, 1994), in turn relating to Schumpeter (1939)'s creative destruction. Secondly, using parts of the data of an existing study, my results can serve as a comparison of the effects of the US patent system on new firms (as studied by Farre-Mensa et al. (2020)) and on the labor market. Then, turning towards public policy, attempts by cities and regions to emulate the success of the Silicon Valley, attracting startups and becoming the "next Silicon Valley"[2] can be discussed following the main question my thesis asks: What is the effect of innovative startups on regional unemployment rates?

Performing my analysis with monthly US county data between 2001 and 2014, I find short- and medium-run increases in the unemployment rate following a single successful patent application. Depending on the specification I use, I find these temporary job losses to be between 900 and up to 18,000 people for the average US county. However, these job losses remain temporary and within five years after the initial patent shock, the unemployment rates are statistically indifferent from their initial level. Studying the effect of mean county per capita personal income following a successful patent application in a less reliable - due to having to change the time period of the analysis from monthly to yearly - setting, I find no effects within five years. While my analysis does not allow me to look into distributional effects, these results suggest the potential for aggregate welfare losses, depending on the weighting of gains for the innovative startups and losses in employment.

The further structure of this thesis is as follows: In the subsequent literature review, I discuss theoretical considerations and existing empirical results, further motivating my research idea. Then, I discuss the data used, in particular the use of patent data, and my identification strategy, before turning to presenting my first stage and arguments regarding the validity of my instrumental variable strategy. I continue with the main empirical results of my thesis, describing various specifications to estimate cumulative impulse response functions. After a brief look at income data and showing the robustness of two decisions regarding my data, I conclude.

[2]Hospers et al. (2008) give an overview of prominent political visitors to the Silicon Valley as well as cities and regions trying to emulate the Silicon Valley.

2. Literature review

The work in this thesis is influenced by three strands of the economic literature that are respectively concerned with the effects of innovation on the labor market, the effects of entrepreneurship on the labor market, and the literature on local labor markets and spatial equilibria. In the following section, I will briefly discuss each of them and attempt to tie them together.

The effects of innovation on the labor market have been a hot topic in economic research at least since the observed changes in relative wages in the USA in the '70s and '80s as described by Katz and Murphy (1992). Since then, innovation and its effects on employment and wages have been attributed as one of the main causes of increasing inequality in the last fifty years. A major explanation of the increasing higher education wage premium has been so-called skill-biased technological change - improvements in technology that lead to increasing demand for high-skilled labor (Berman, Bound, and Machin, 1998). More recently this literature has been surveyed by Acemoglu and Autor (2011), summarizing the - what they call - canonical supply-demand-model of skills including two imperfectly substitutable skill groups and generalizing this framework to account for further stylized facts. These stylized facts include for example the so-called job polarization, describing job growth in low- and high-skill relative to middle-skill areas.

More theoretically, these observations have also led to frameworks modeling the incentives of firms to invest in research and development (R&D) based on the distribution of skills and wage levels they are confronted with (Acemoglu, 1998, 2010; Stiglitz, 2014). Particularly noteworthy for the scope of this paper is the argument made by Acemoglu (1999) that job polarization due to a larger supply of skilled workers or skill-biased technological change leads to higher unemployment for both high- and low-skilled groups due to increasingly difficult employer-employee-matching. This argument is similar in notion to what is discussed in the literature on Schumpeterian growth theory. As for example, Aghion et al. (2014, p. 120) note in their overview of the literature:

> "Although each GPT [general-purpose technology, author's note] raises output in the long run, it can also cause cyclical fluctuations while the economy adjusts to it. [...] GPTs like [...] the computer require costly restructuring and adjustment to take place, and there is no reason to expect this process to proceed smoothly over time.

> Thus, contrary to the predictions of real-business cycle theory, the initial effect of a 'positive technology shock' may not be to raise output, productivity, and employment but to reduce them."

This disruptive effect of innovation, Schumpeter (1939)'s creative destruction, combined with skill-biased technological change is the central mechanism that I set out to study in this thesis. Looking at the literature on search and matching in labor models including frictional markets, following the discussion by Hornstein et al. (2005), the concentration of my empirical approach on startups and their disruptive effects is in line with the theoretical literature initiated by Aghion and Howitt (1994). Aghion and Howitt (1994) focus on innovation which needs labor reallocation to be effective, thereby leading to new employer-employee matches. Thus, innovation leads to an increase in unemployment.

A question that is more directly applicable to this thesis has been of interest in the strand of literature empirically concerned with entrepreneurship and small businesses. This literature links unemployment to self-employment and dates itself back to at least Oxenfeldt (1943). More recently, in discussing the relation between unemployment and entrepreneurship, this literature covered the question of the direction of causality: Does unemployment cause self-employment/entrepreneurship ("refugee effect") or does entrepreneurship cause employment, respectively reduce unemployment? (Baptista and Preto, 2007; Audretsch, Thurik, Stel, and Carree, 2008; Faria, Cuestas, and Mourelle, 2010; Bonnet, Aubry, and Renou-Maissant, 2015; Halicioglu and Yolaç, 2015). While this literature is aware of the problem of reverse causality, the research does not rely on natural experiments or instrumental variable approaches, but relies on Granger causality and finds no consistent results.

Similarly, papers that assess the impact of technology shocks on aggregate economic indicators rely on narratively identified shocks, e.g. using historical patent data and timing assumptions in the form of Cholesky decompositions in structural vector autoregressions. Whether narrative time series' in general actually describe exogenous shocks is questionable, with e.g. Ramey (2016) arguing for cautious interpretation. Existing works focusing on narratively identified technology shocks and their effects have been carried out by Shea (1998) on the effect of technology shocks on industry inputs and total factor productivity, Alexopoulos (2011) on the effect on total factor productivity, investment and employment, Alexopoulos and Cohen (2016) on the effect on employment and Kogan, Papanikolaou, Seru, and Stoffman (2017) on the effect of innovation on GDP growth and total factor productivity. Shea (1998), Alexopoulos (2011) and Alexopoulos and Cohen (2016) using their historical accounts, spanning almost all of the 20th century between their three papers, find that positive technology shocks lead to increases in employment.

The literature discussed so far focuses on the effects in a single market and therefore does not consider differences between regions. Let me very briefly summarize some insights into the relationship between innovation and agglomeration before turning to local labor markets. While population and economic activity, in general, are spatially concentrated, both innovation (Audretsch and Feldman, 1996) and R&D activity (Carlino, Carr, Hunt, and Smith, 2010) are more spatially concentrated than employment, highlighting the importance of considering spatial equilibria in this setting.

Discussing the literature on local labor markets also leads to discussing welfare considerations: Moretti (2011) builds on the Rosen-Roback framework of spatial equilibrium based on the works of Rosen (1979) and Roback (1982) trying to describe a more general model of local labor markets. For simplicity, this model rules out involuntary unemployment, implying that all adjustments to labor supply following a shock to labor demand are due to migration - be it domestic or from another country. However, given different mobility costs between high- and low- skilled workers (Topel, 1986; Blanchard and Katz, 1992; Bound and Holzer, 2000) including involuntary unemployment raises the question of who will be newly employed following a positive labor demand shock (e.g. due to an increase in productivity): residents or movers? Empirical investigations of this question are not in agreement (Bartik, 1991; Kniesner, 1994; Bartik, 2002; Renkow, 2003, 2007; Partridge, Rickman, and Li, 2009), but further raise the point of welfare considerations of innovation. Or, as Moretti (2011) formulates it:

> "This issue [who ends up getting the job] is particularly important when thinking about policies aimed at increasing local employment, like local development policies. Implementing a local development policy that increases employment in an area and benefits only migrants from outside the area is quite different politically from implementing a local development policy that benefits residents."

This welfare consideration leads me to the main point of my paper, which differs from the work discussed above. I am not studying the effect of innovation on residents and movers or between different groups along the distribution of education. I am looking at the effect of innovation on employment on a different level of aggregation than an already existing study - the work of Farre-Mensa et al. (2020), which is part of the corporate finance literature and studies the effect of patents on startup outcomes. Using the random assignment of patent examiners in the application process with the United States Patent and Trademark Office (USPTO) to estimate the local average treatment effect of a successful patent application on firm outcomes, Farre-Mensa et al. (2020) compare marginal patents that had the outcome of the review process affected by the draw of the examiner. These marginal patents are called compliers in the causal inference literature. They find that successful patent applications due to a more lenient examiner led to 55% higher employment growth of the firm five years later. I take the same data - patents filed for application between 2001 and mid-2014 - to the county level to estimate the effect of a successful patent application on local unemployment. Thereby, I also end up providing a comparison of the effects of innovation on firm- and area-level.

As Stiglitz (2014, p. 3) notes:

> "While increases in productivity could in principle make everyone better off — the production possibilities curve moves out — in practice, there are always winners and losers."

The question this quote raises in the setting of Farre-Mensa et al. (2020) then is, whether the finding of a gain in employment of individual firms after a successful patent application is also reflected in a gain in employment in the county where the firm is located. In other words, I end up borrowing one-half of the research question of the entrepreneurship literature - does (innovative) entrepreneurship lead to employment? - and contrast it with the perspective discussed by Aghion et al. (2014) regarding creative destruction, in the form of Aghion and Howitt (1994) or Acemoglu (1999) - does (skill-biased) innovation lead to unemployment? - and look at it at an aggregate level to arrive at the question: How do innovative startups affect the unemployment rate in the county they are located in?

Thematically, this question is related to the growing literature in macroeconomics that uses cross-regional variation to estimate effects of interest. This strand of literature is discussed in Guren, McKay, Nakamura, and Steinsson (2020). In a broader sense, my thesis adds to the existing literature studying non-policy shocks, which was started by Kydland and Prescott (1982).

3. Data and methodology

3.1. The use of judge fixed effects

The instrumental variable strategy of Farre-Mensa et al. (2020) described above builds on prior research regarding institutional details of the US patent application process. Cockburn et al. (2002) found for a small data set consisting of 182 patent applications that examiners differ in - what Cockburn et al. (2002) call - "their 'generosity'" and that examiners matter for the final outcome of a patent application. Similarly, Lemley and Sampat (2012) found that more experienced examiners are more likely to accept an application. Sampat and Williams (2019) use the random allocation of examiners to patent applications to assess their causal effect on follow-up innovation and find no impact for gene sequences.

The use of random assignment to a judge or, in the case of patents, an examiner and their leniencies as an instrumental variable has been first introduced by Kling (2006), studying the effect of incarceration length on future labor market outcomes. Since then, this instrumental variable has been referred to as "judge fixed effects" design and used in a variety of settings (for a summary, see Frandsen, Lefgren, and Leslie (2019)).

3.2. What do patents measure?

While I have briefly summarized the literature of the patent instrumental variable above, applying this instrument to a setting that I am relating to the literature on skill-biased technical change makes it necessary to discuss the implications of using patent data as a measurement for innovation. As Carlino and Kerr (2015, p. 353) note by referencing Schumpeter (1939), invention and innovation differ:

> "For Schumpeter, invention is the creation of a new product, service, or process, whereas innovation is the commercialization or introduction of that product or service into the market. Many inventions are patented, but most patents never reach the point of commercialization, and some that do often require a long gestation period. On the other hand, innovations are closely linked to commercialization and often do not require corresponding invention or patents."

Then, using patents as a measure of innovation has the advantage that it is an outcome of the inventive process (and thereby differs to e.g. R&D spending), but the differences between the inventive and the innovative process may still lead to bias when using patents as a measurement of innovation. Comparing Jaffe (1989) and Acs, Audretsch, and Feldman (1994), respectively Feldman (1994), Feldman and Kogler (2010) conclude that innovation is more concentrated than invention and that "[s]tudies that draw inferences about innovation by focusing on invention should be interpreted with caution".

The innovative impact of patents is controversial, to say the least, with Boldrin and Levine (2013) going so far as to claim that patents are uncorrelated to increases in productivity unless productivity itself is measured by patents. This critique has some interesting influence on the scope of this paper: Whether startups - or young firms in general - are innovative is not the question I am trying to answer, but their effect on aggregate local employment. Whether patents in this case really measure innovation or rather act as a device to reduce information frictions does not matter for the econometric analysis, but the theoretical background and potential interpretations of the results. If patents measure innovation, I could argue that the shock in this analysis is a shock to technology. Given that the instrument of Farre-Mensa et al. (2020) measures the local average treatment effect of drawing a more lenient examiner, the instrument disentangles the value of the underlying innovation and the value of the patent granting, measuring the latter. In the aggregate setting, the instrument then allows measuring a shock to available capital for innovative firms and not a shock to technology.

However, despite not measuring a shock to technology, the analysis can still be framed in the context of skill-biased technical change: Skill-biased technical change and job polarization imply that high-skilled labor and physical capital, respectively human capital and physical capital, are complements. The fact that young, skilled workers are disproportionately employed by young firms (Ouimet and Zarutskie, 2014) provides some evidence of the demand for high-skilled labor of young firms. In the aggregate setting, the instrument of Farre-Mensa et al. (2020) then does not allow to directly measure the effect of innovation on aggregate employment, but to measure the effect of an increase in demand for high-skilled labor, caused by the influx of funding due to an accepted patent application.

3.3. Patent data

The data of Farre-Mensa et al. (2020) differs from otherwise publicly available patent databases (e.g. the databases provided by the NBER or Harvard Business School[1]) by not only including accepted patent applications but also rejected ones. This data availability is a necessity for the identification strategy of judge fixed effects. As the authors note, this was possible due to gaining access to the internal database of the USPTO. While this means that they were technically able to study all patent applications received by the USPTO, Farre-Mensa et al. (2020) study the effect of patents on startups and therefore only chose a sub-sample of all available applications. To arrive at their sub-sample of US startups they used a list of filtering steps: First, only US-incorporated and for-profit applicants, second manually removing applications that have been a subsidiary of another firm or have been stock market listed, and finally firms with no prior applications and firms that qualify for reduced filing fees under the condition of being a small business.

Timewise, the sample has natural restrictions: Only beginning in 2001 the USPTO recorded names of rejected applications, which, again, is essential for the identification of judge-fixed effects. The first patent application in the original data set happened in April 2001. On the other end, Farre-Mensa et al. (2020) limit the sample to applications with the final decision carried out by late 2013 to allow for the analysis of five-year effects. Thereby, the original authors limit their sample to 32,215 patent applications. Since the data set provided by Farre-Mensa et al. (2020) also includes over 100 patent applications that only received issuance in the first half of 2014

[1] See NBER and HBS for more information.

and I see no reason to exclude this data, my sample of patent applications is slightly larger with 34,422 patent applications between April 2001 and, inclusive, June 2014.

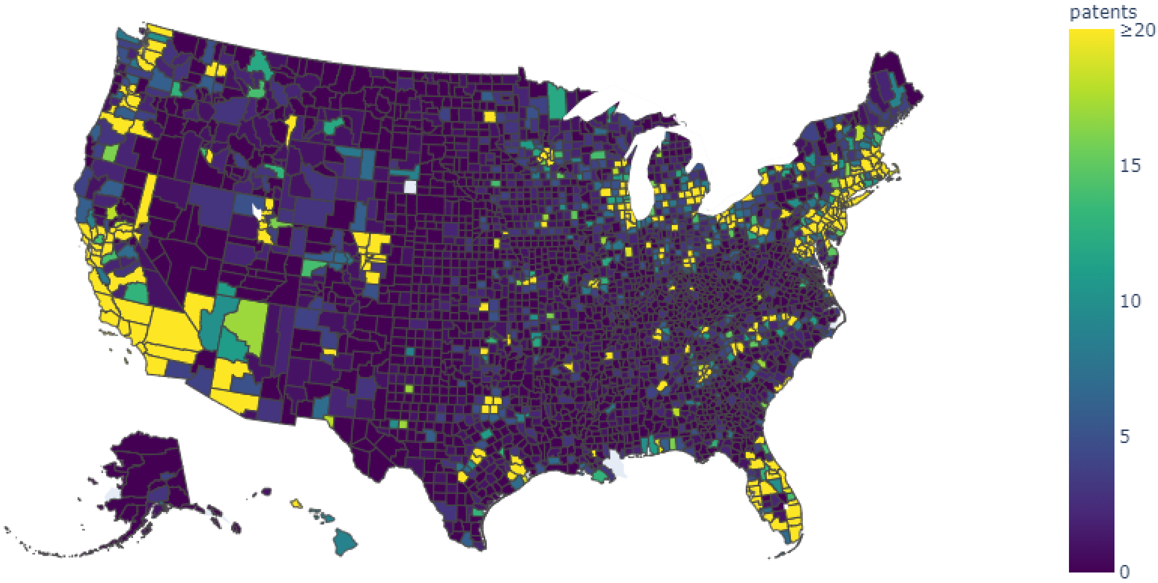

Figure 3.1.: Spatial distribution of patent applications in the sample across US counties.

As the spatial distribution of patents was not of interest in the data set's original use but is in my analysis, I now turn to my original work on the patent data. Audretsch and Feldman (1996); Carlino et al. (2010); Moretti (2011) highlight that innovative activity generally is heavily concentrated, which is also applicable to the sample in question. I plot the concentration of the total number of patents in each county in the sample in Figure 3.1.

In Figure 3.1, the number of twenty patents is chosen as an arbitrary cut-off to illustrate some of the regional distribution. The very extent of the concentration is highlighted in Figure 3.2, showing that 75% of all counties had three or fewer patent applications filed by startups, while 1356 applications happened in Los Angeles County alone.

While the identification strategy using examiner leniencies is only possible for the sample of startup patent applications of Farre-Mensa et al. (2020), given the concentration of innovative activity, only including patents of this subsample in the estimation would likely lead to an omitted variable bias. Take a highly stylized scenario for example: A new key invention of a large, older firm leads to automation and closure of a large plant. The resulting unemployment could be attributed to a startup patent in the same time frame and thereby biasing the estimates.

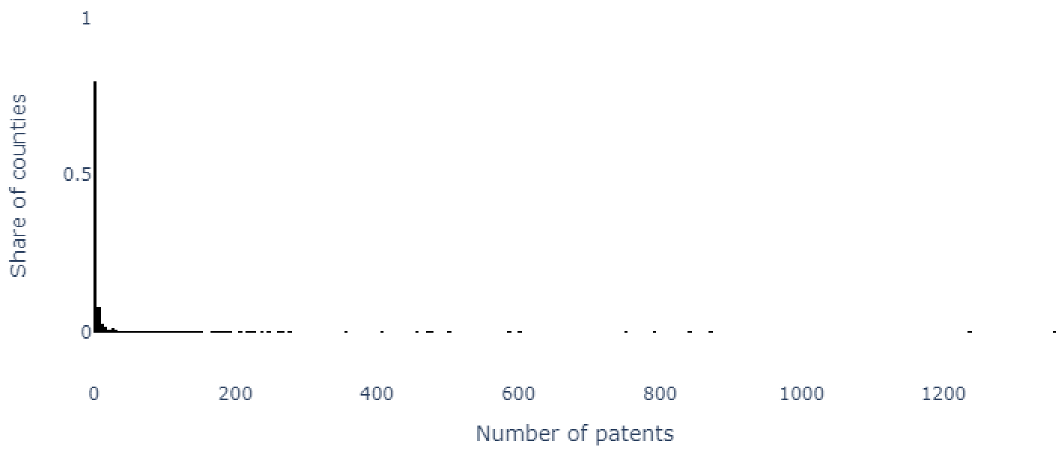

Figure 3.2.: Distribution of patent applications in the sample.

Therefore, I also add general patent data provided by De Rassenfosse, Kozak, and Seliger (2019)[2] which includes more than three million successful patent filings in the USA in the sample.

3.4. Labor market and housing data

To study the effect of innovative startups on unemployment I combine the sample of patent applications described above with monthly county labor market data provided by the US Bureau of Labor Statistics (BLS)[3]. The variables of interest from this data source are the unemployment rate and, to control for the population size of different counties, the labor force. The BLS provides both statistics without seasonal adjustment at the county level. Following the information provided by the BLS[4] I use a similar ARIMA decomposition to seasonally adjust both unemployment rates and the size of the labor force for each county individually, as well as detrending the labor force data to guarantee stationarity of the time series. I give more details on this procedure in Appendix A.

Housing data, more specifically monthly data on new housing building permits and their respective values by counties, is provided by the US Census Bureau[5]. Using the number of

[2] The data is available online via the Harvard Dataverse.
[3] The data is available online.
[4] BLS information on their seasonal adjustment methodology is available online.
[5] The data is available online.

Table 3.1.: Descriptive statistics of the sample

	Unemployment Rate	Labor Force	Patents	Value per Building Permit
Count	509,913	509,913	509,913	509,913
Mean	6.87	47,319.98	6.33	40,977.43 $
St. D.	3.18	153,284.78	59.05	87,381.73 $
Min.	-0.44	34.81	0.00	0.00
25%	4.61	5,187.43	0.00	0.00
50%	6.14	11,889.70	0.00	0.00
75%	8.43	31,059.69	0.00	0.00
Max.	30.39	4,982,596.46	4,897.00	7,500,000.00 $

	Startup Granted Patents	Startup Abandoned Patents	Startup Total Patents	Startup Granted Rate
Count	509,913	509,913	509,913	509,913
Mean	0.04	0.02	0.06	0.02
St. D.	0.33	0.22	0.48	0.15
Min.	0.00	0.00	0.00	0.00
25%	0.00	0.00	0.00	0.00
50%	0.00	0.00	0.00	0.00
75%	0.00	0.00	0.00	0.00
Max.	16.00	13.00	27.00	1.00

building permits and their values I construct a variable describing the value per new unit. I use the data as a proxy to control for price levels and migration.

Finally, combining all data sources and discarding some missing data, I arrive at a panel consisting of 3,207 US counties over 159 months, resulting in 509,913 observations. Table 3.1 reports summary statistics of the sample.

3.5. Identification strategy

While I have already discussed the identification strategy of Farre-Mensa et al. (2020), it is necessary to discuss my own identification strategy in larger detail due to two issues: First, while I use the same idea and instrument for identification, I study different units of observation and therefore also run a different first-stage regression. Secondly, Farre-Mensa et al. (2020) only mention the monotonicity assumption, which is crucial in the LATE framework (Imbens and Angrist, 1994), in a footnote and claim that it is not likely to be violated.

The baseline dynamic panel specification I propose to study takes the following form:

$$u_{c,t} = \sum_{s=0}^{S}(\beta_s p_{c,t-s} + \gamma_s u_{c,t-s-1} + X'_{c,t-s}\eta_s) + \alpha_c + \delta_t + \epsilon_{c,t} \tag{3.1}$$

Here, $u_{c,t}$ is the unemployment rate in county c in month t, $p_{c,t}$ is the number of patents accepted in county c in month t, X is a vector of control variables including the size of the labor force and housing data, α_c denotes county fixed effects and δ_t time fixed effects. The parameters of interest are β_s, measuring the effect of one additional granted patent on the unemployment rate. Using the notation of treatment effects, the "immediate treatment effect" is $\beta^0 = \mathbb{E}[\Delta u^0_{c,t}(1) - \Delta u^0_{c,t}(0)|p_{c,t} > 0, p_{c,t-1} = 0] = \beta_0$, where $\Delta u^s_{c,t}(p) = u^s_{c,t}(p) - u_{c,t-1}$ describes the change in the unemployment rate from time t-1 to time t+s due to an accepted patent p at time t. Following equation (3.1), the effect at a given time horizon s is recursively determined by $\beta^s = \beta_s + \sum_{j=1}^{J} \gamma_j \beta^{s-j}$, which can be used to describe an impulse response function. The cumulative impulse response function at time s then is $\sum_0^s \beta^s$ and the cumulative long-run effect is

$$\frac{\sum_{s=0}^{S} \beta_s}{1 - \sum_{s=0}^{S} \gamma_s} \ . \tag{3.2}$$

Making the conventional assumption of conditional sequential exogeneity (3.3) implies that the past unemployment rate, the accepted patents, and the control variables are orthogonal to the contemporaneous and future shocks to the unemployment rate and the error terms to be serially uncorrelated.

$$\begin{gathered}\mathbb{E}(\epsilon_{c,t}|u_{c,t-1}, ..., u_{c,t_0}, p_{c,t}, ..., p_{c,t_0}, X_{c,t}, ..., X_{c,t_0}, \alpha_c, \delta_t) = 0 \\ \forall u_{c,t-1}, ..., u_{c,t_0}, p_{c,t}, ..., p_{c,t_0}, X_{c,t}, ..., X_{c,t_0}, \alpha_c, \delta_t, c \text{ and } t \geq t_0\end{gathered} \tag{3.3}$$

Since the inventive process of patents is highly unlikely to be endogenous, the conditional sequential exogeneity assumption is likely violated in (3.1). Instead, I modify (3.1) by treating accepted patents as exogenous and instrumenting them with the examiner leniencies estimated by Farre-Mensa et al. (2020). The 2SLS specification then takes the following form:

$$\begin{aligned} u_{c,t} &= \sum_{s=0}^{S} (\beta_s p_{c,t-s} + \gamma_s u_{c,t-s-1} + X^{'}_{c,t-s} \eta_s) + \alpha_c + \delta_t + \epsilon_{c,t} \\ p_{c,t} &= L^{'}_{c,t} \phi + \kappa_c + \theta_t + \omega_{c,t} \end{aligned} \tag{3.4}$$

The first equation in (3.4) is identical to (3.1). The second equation describes the first stage regression of the vector of leniencies of accepted patents $L_{c,t}$ on the number of accepted patents $p_{c,t}$, while κ_c and θ_t represent, respectively, county and time fixed effects. The length of $L_{c,t}$ is 16, the maximum number of patents accepted in a county in a month in the sample. Alternatively, $L_{c,t}$ can be thought of as the vector of interactions between the estimated leniencies and a dummy, indicating acceptance of a patent with 1 and rejection with 0. The exclusion restriction, in this case, can be written as (3.5).

$$\begin{aligned}&\mathbb{E}(\epsilon_{c,t}|u_{c,t-1},...,u_{c,t_0},p_{c,t},...,p_{c,t_0},X_{c,t},...,X_{c,t_0},\alpha_c,\delta_t)=0\\&\forall u_{c,t-1},...,u_{c,t_0},L^{'}_{c,t},...,L^{'}_{c,t_0},p_{c,t},...,p_{c,t_0},X_{c,t},...,X_{c,t_0},\alpha_c,\delta_t,c \text{ and } t\geq t_0\end{aligned}\tag{3.5}$$

The exclusion restriction imposes that, conditional on lags of unemployment, contemporaneous and lagged controls, as well as county and time fixed effects, the leniency instruments have no direct effect on unemployment. The monotonicity assumption implies that - given examiner A is less lenient than examiner B - if examiner A grants a patent, examiner B would have granted it as well.

Following the notation of Frandsen, Lefgren, and Leslie (2019) monotonicity formally implies:

$$\begin{aligned}\forall j,w\in\{0,...,J\}:&D_i(j)\geq D_i(w)\forall i \text{ or}\\&D_i(j)\leq D_i(w)\forall i\end{aligned}\tag{3.6}$$

Here, j, w describe two judges in the space of all judges J and D_i notes the treatment status D - acceptance or rejection - for patent i.

Frandsen et al. (2019) discuss this assumption and provide a test for the monotonicity assumption based on first-stage residuals. However, for their testing procedure the treatment variable - in my case, the number of patents granted at a given observation - has to be binary. This is not the case and therefore not applicable. Instead, it is possible to test the weaker average monotonicity assumption. Average monotonicity implies that the estimated coefficients of the first stage regression should be non-negative across all subsamples.

An alternative to the above described recursive computation of (cumulative) impulse responses is the local projection method of Jordà (2005), regressing individual leads $u_{c,t+h}$ of the unemployment rate for each $h \in o,...,H$ on the right hand side of the first equation in (3.4). Following Jordà et al. (2015), the cumulative impulse response function is estimated by subtracting $u_{c,t-1}$ from each $u_{c,t+h}$, leading to the estimation of (3.7), which includes the same first stage regression as in (3.4).

$$\begin{aligned}\Delta u_{c,t+h} = & u_{c,t+h} - u_{c,t-1} = \sum_{s=0}^{S}(\beta_s p_{c,t-s} + \gamma_s u_{c,t-s-1} + X^{'}_{c,t-s}\eta_s) + \alpha_c + \delta_t + \epsilon_{c,t} \\ p_{c,t} = & L^{'}_{c,t}\phi + \kappa_c + \theta_t + \omega_{c,t}\end{aligned} \tag{3.7}$$

Here, the impulse response at each horizon is equal to β_0. There is no equivalent to the long-run effect that can be recursively computed in (3.4).

4. Results

4.1. First stage results

While Farre-Mensa et al. (2020) argue for the validity of their instrument in case of their firm-level analysis, given the aggregation in my analysis I need further arguments to justify the validity, which I begin by investigating the first stage regression described in (4). The results of this regression, omitting the coefficients belonging to the fixed effects, are reported in Table 4.1.

Based on the reported F-statistic, the null hypothesis of irrelevant or weak instruments can be rejected. For the exclusion restriction, I have to rely on a similar argument as Farre-Mensa et al. (2020, p. 652) do - who note that the restriction "is likely plausible satisfied [...] - it is difficult to see how an examiner's past leniency would affect a startup's future success *directly*". Similarly, it is hard to argue that an examiner's leniency could directly influence the unemployment rate in a given county. Regarding the test for the weaker, average monotonicity assumption proposed by Frandsen et al. (2019) regarding the positive coefficients of the first stage across subsamples, an obvious group of subsamples to use in this setting are states. I report the results of the state-wise first stages in Appendix B. The coefficients of all instruments across all states for which patent

Table 4.1.: First stage results

Leniency 1	1.4348***	Leniency 9	1.5019***
	(0.0044)		(0.0997)
Leniency 2	1.4707***	Leniency 10	1.5424***
	(0.011)		(0.1266)
Leniency 3	1.5247***	Leniency 11	1.5483***
	(0.0221)		(0.1864)
Leniency 4	1.4819***	Leniency 12	1.4211***
	(0.0398)		(0.2030)
Leniency 5	1.4106***	Leniency 13	2.0411***
	(0.0566)		(0.3135)
Leniency 6	1.5107***	Leniency 14	0.7242***
	(0.0445)		(0.2320)
Leniency 7	1.6122***	Leniency 15	1.8004***
	(0.0838)		(0.1931)
Leniency 8	1.2965***	Leniency 16	0.5788***
	(0.0815)		(0.1832)
F-statistic	417825	N. of obs.	509,913

Note: The table reports the result of the first stage regression of patent examiner leniencies on the number of accepted patents in a given county in a given month as described in equation (3.4). Heteroskedasticity robust standard errors clustered on counties are reported in parentheses (* p<.10, ** p<.05, *** p<.01).

applications are included in the sample are positive. Thereby, I also have a justification for the average monotonicity assumption. Combining the arguments in this section, I provide evidence for the validity of the instrumental variable strategy at hand. Having addressed the issue of validity, I now turn to the second stage results.

4.2. The effect of one additional patent on the unemployment rate

4.2.1. Baseline results

I estimate equations (3.4) and (3.7), setting the number of lags to control for to 24[1]. These estimations form my baseline results. I report the cumulative impulse response functions based on these estimations on the next page. For the recursively computed cumulative impulse response function of the fixed-effect dynamic panel model (within estimator) in Panel A in Figure 4.1, the

[1] I justify the choice of lag length in Appendix C.

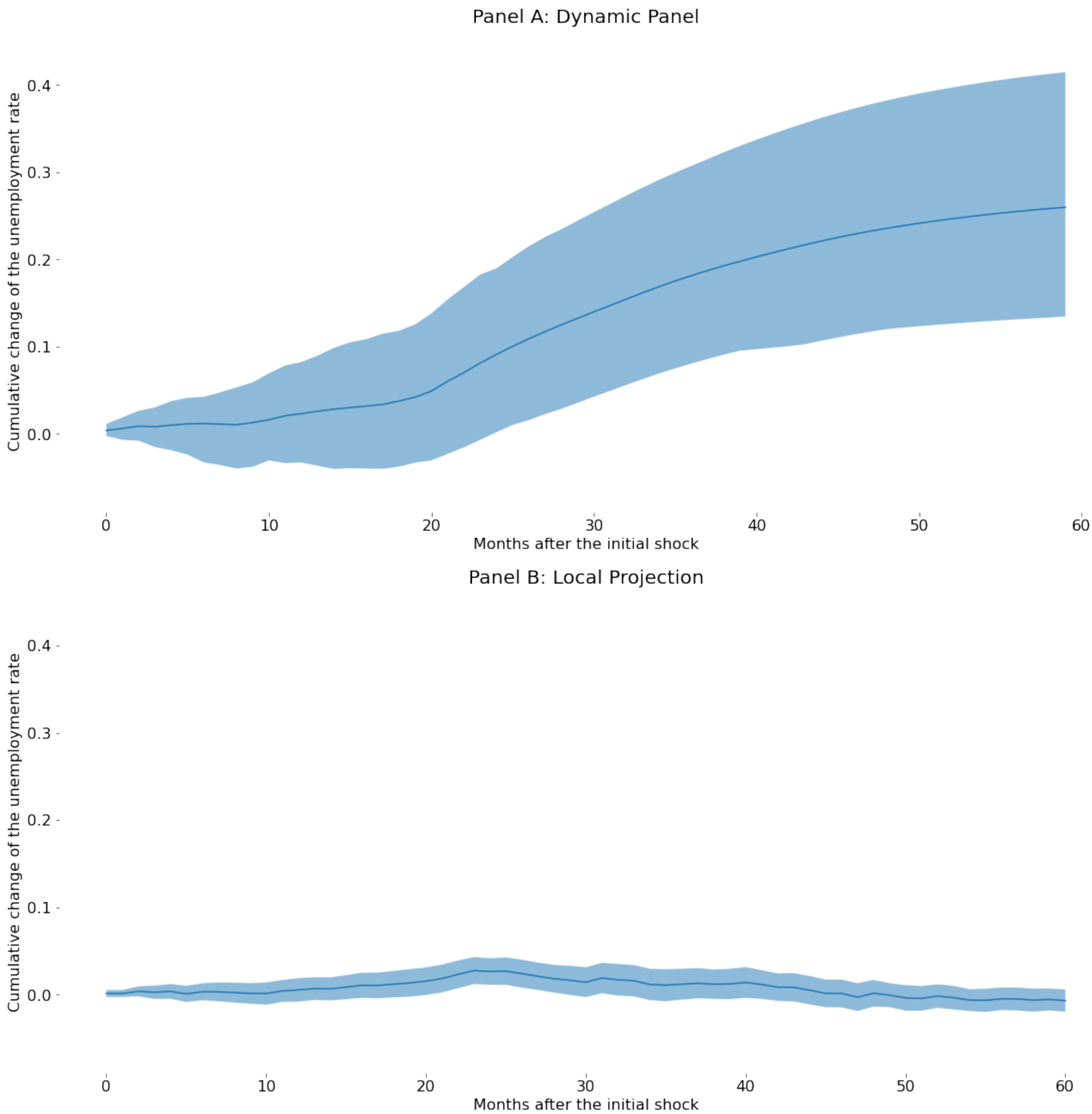

Figure 4.1.: The estimated cumulative impulse response functions of unemployment due to a shock in the form of a successful patent application of a startup. Panel A displays the recursively computed impulse response function based on the estimation of equation (3.4). The shaded area displays the 95% confidence interval based on 200 bootstrap samples. Panel B displays the impulse response function estimated by equation (3.7). The shaded area displays the 95% confidence interval based on heteroskedasticity robust standard errors clustered on counties.

impulse responses start to become positive and statistically significant at the 95% significance level at the 23rd month after the initial shock. The impulse response function then remains positive and statistically significant throughout the estimated horizons and converges towards the cumulative long-run effect described in (3.2). This effect is estimated at 0.27 percentage points and also statistically significant at the 95% significance level.

This vastly differs from the impulse response function estimated by (3.7) displayed in Panel B. Using local projections, the cumulative impulse response function becomes positive and statistically significant after 21 months and remains like this until the 31st month after the initial shock.

There are several problems with the two estimation processes that can explain some of these differences. Apart from a general difference between recursively calculated and directly estimated impulse response functions, the panel data setting leads to further problems in the estimation process. I am discussing these issues in detail in the next section.

Table 4.2.: Second stage results

Horizon (months)	Recursive IRF (within estimator)	Local Projection
0	0.0040	0.0011
	(0.0031)	(0.0020)
12	0.0233	0.0052
	(0.0334)	(0.0068)
24	0.0911**	0.0263***
	(0.0495)	(0.0077)
36	0.1815***	0.0117
	(0.0643)	(0.0090)
48	0.2357***	0.0014
	(0.0748)	(0.0078)
60	0.2598***	-0.0057
	(0.0796)	(0.0065)
long-run	0.2734***	-
	(0.0862)	-

Note: The table reports the result of the second stage using equations (3.4) and (3.7). For the within estimator, standard errors based on 200 bootstrap[2]samples are reported in parentheses (* p<.10, ** p<.05, *** p<.01). For the local projection, heteroskedasticity robust standard errors clustered on counties are reported in parentheses (* p<.10, ** p<.05, *** p<.01).

While I need to consider various adjustments of the estimation processes to consolidate the different shapes of the two impulse response functions forming my baseline results, in particular regarding the long-run cumulative effect, I can already discuss the short- and medium-run

[2]I am reporting bootstrap confidence interval due to the transformation of the regression results. The confidence intervals are calculated using bootstrap standard errors, which are obtained using model-based resampling of the residuals and recursive calculation of the cumulative impulse response function from the bootstrap regressions results. The bootstrap algorithm used is the cluster wild bootstrap suggested by Cameron, Gelbach, and Miller (2008).

cumulative effects to some extent. While the immediate and short-run impact of one additional startup being granted a patent is not statistically significant across specifications, the cumulative increase of the unemployment rate due to one additional patent is statistically significant in both specifications after 24 months between 0.02 and 0.09 percentage points. The results in tabular form are reported in Table 4.2 below. Given the biases discussed above, the estimate of the recursively computed impulse response function likely represents an upper bound and the impulse response estimated by local projections a lower bound of the real effect. Looking at the average county labor force in the sample, this effect implies a cumulative increase of 950 to 4300 additional unemployed over a horizon of two years caused by a startup getting their first patent approved. In comparison, the findings of Farre-Mensa et al. (2020) imply additional cumulative 16 employees for the average startup in their sample.

This surprisingly large number in my aggregate analysis may be, at least in part, explained by a weakness in the empirical approach: As Farre-Mensa et al. (2020) discuss, their estimates give evidence of the importance of the US patent system for some startups to gain access to external funding, as a successful patent application reveals some information about the innovativeness of a startup. This channel is of particular importance for startups that are hard to evaluate for investors. the same group of startups is likely the most affected by the draw of the patent examiner, i.e. these startups form the majority of the compliers in this setting, as these startups will exhibit the most leeway in the interpretation of the novelty of their patent application. Therefore, the LATE of an accepted patent for an individual startup of Farre-Mensa et al. (2020) is very likely larger than for an average firm or even an average startup. Similarly, the effect of such a patent on the labor market is also likely to be larger.

Comparing the shape of the impulse response function to the existing literature that relies on Granger causality gives further insights. Following the summary provided by Fritsch (2008), the consensus in this literature is an s-shape impact of startups on employment. In the case of this study, this would imply a negative impact on the unemployment within the first year, an increase in the unemployment rate between years one to five, before finally going back to negative afterward. My baseline estimates display a small, positive, although statistically insignificant, immediate impact. The medium-run effect returns to zero for the local projection estimation, but not for the dynamic panel and the years afterward are beyond the scope of these estimations.

However, as discussed above, these differences may be, at least in part, due to the selection of the sample, i.e. only a subgroup of startups. Not necessarily all startups file for patents and it is very likely that startups that file and do not file for patents significantly differ.

4.2.2. Adjusted results

The baseline results presented in the previous section suffer from various shortcomings. Foremost, local projections (LP) and vector autoregressions (VAR) - the dynamic panel specification in (3.4) corresponds to the first equation in a VAR and the impulse response function is computed recursively as it is done with a VAR - give approximately similar results of impulse response functions for short horizons, but may largely differ for long horizons (Plagborg-Møller and Wolf, 2021). Plagborg-Møller and Wolf (2021) in particular discuss that, while VAR and LP estimate the same impulse responses over short and medium horizons, the differences over long horizons take the form of a bias-variance trade-off that may explain some of the differences in this case. Here, the distinction between short and medium horizons and long horizons is defined by the number of lags: The long horizon is equal to the forecast horizons exceeding the number of lags in the model as this is the starting point of recursion in the recursive impulse response estimation.

However, due to the dynamic nature of the specifications, further problems arise due to the Nickell bias (Nickell, 1981) when estimating a fixed-effects specification like (3.4) via OLS. The direction of the bias depends on the relation between the instrumented patent applications and the unemployment rate, implying a likely upwards bias of the fixed-effects estimator in this setting. Therefore, I estimate (3.4) using the Arellano and Bond (1991) or "difference" generalized method of moments estimator while keeping the size of the instrument matrix in check by "collapsing" it, as for example suggested by Roodman (2009).

The work on biases in dynamic panel settings has made further advances in recent years, for example, the sample splitting suggestion by Chen, Chernozhukov, and Fernández-Val (2019), which I consider as a further adjustment on top of the Arellano and Bond (1991) estimator. I adapt the relatively easiest form of sample splitting suggested, which here takes the following form: First, I split the original sample, which I use for the estimation of the Arellano-Bond estimator and denote as AB, into two subsamples denoted as $AB1$ and $AB2$. I then run the same procedure on the split samples as on the original. This leaves me with three series' of estimates of impulse responses β^s: β^s_{AB}, β^s_{AB1} and β^s_{AB2}. The debiased Arellano-Bond impulse response at horizon s then is:

$$\beta^s_{DAB} = 2\beta^s_{AB} - \frac{\beta^s_{AB1} + \beta^s_{AB2}}{2} \tag{4.1}$$

Similarly, Teulings and Zubanov (2014) show that local projections in a panel data setting suffer from a bias that increases with time horizon h. As the distance between t and $t + h$ increases, more and more shocks are happening within the estimation horizon that are not included in the estimation. Teulings and Zubanov (2014) show that the direction of this bias is downwards and can be easily tackled by including shocks happening between t and $t + h$ into the regressions. Apart from their suggestion regarding the bias concerning excluded shocks, they also provide an alternative solution to the Arellano-Bond estimator for the Nickell-bias in local projections. However, both of these suggestions fall short in my setting: Due to the persistence of the accepted startup patents, including further shocks leads to unstable solutions due to multicollinearity. And their suggested solution for the Nickell bias relies on including more lags than horizons, which then leads to a problematically short time dimension in my setting.

Instead, I am using the small-sample bias correction proposed by Herbst and Johannsen (2020) on the local projection estimates presented in Table 4.2. Herbst and Johannsen (2020) show that local projections generally lead to biased estimates of impulse response functions if the time dimension is smaller than in the original examples by Jordà (2005), who used a minimum of 300 observations in the time dimension for his Monte Carlo simulations. This bias persists in panel data settings. Herbst and Johannsen (2020) provide a fairly simple to implement bias-corrected estimator using an adjustment matrix based on the time dimension and the maximum number of estimated horizons, which can eliminate the bias for short-horizons in their simulations for panel data. Importantly, their adjustment only tackles the bias in the point estimates, but given this bias, the associated standard errors are likely also problematic, even when routine robustness measures, for example for heteroskedasticity, are applied.

I present the results of the adjusted second stage in Table 4.3 and in Figure 4.2. While, again, the size of the effects differs to some degree, their direction and the shape of the impulse response functions tend to agree. Most importantly, the adjusted methods shed light on the long-run effect: The adjusted methods suggest a long-run effect, respectively an effect after five years, that is statistically not different from zero. While the adjusted results provide evidence of the biases discussed in the previous section and their suspected direction, recursive and direct impulse response functions still do not coincide fully, in particular for short horizons. Generally, one can note that the impulse responses estimated via Arellano-Bond and calculated recursively are more volatile than the directly estimated impulse response function.

Table 4.3.: Adjusted second stage results

Horizon (months)	Recursive IRF (Arellano-Bond)	Adjusted Local Projection (Herbst & Johannsen)	Recursive IRF (Debiased Arellano-Bond)
0	0.0271	0.0096***	-0.0958
	(0.0328)	(0.0020)	(0.0697)
12	0.2453***	0.0139**	0.0184
	(0.0657)	(0.0068)	(0.1369)
24	0.0177	0.0349***	0.0241
	(0.0514)	(0.0077)	(0.1381)
36	-0.1852***	0.0204**	-0.2806
	(0.0615)	(0.0090)	(0.1818)
48	-0.2494***	0.0097	-0.3638**
	(0.0630)	(0.0078)	(0.1571)
60	-0.1514***	0.0002	0.1427
	(0.0551)	(0.0065)	(0.2330)
long-run	0.0249	-	0.0539
	(0.0242)	-	(0.0310)

Note: The table reports the result of the second stage for the adjusted estimation techniques. For the recursive impulse response functions, standard errors based on 200 bootstrap samples are reported in parentheses (* p<.10, ** p<.05, *** p<.01). For the local projection, heteroskedasticity robust standard errors clustered on counties are reported in parentheses (* p<.10, ** p<.05, *** p<.01).

While both of the recursively calculated cumulative impulse responses imply no statistical significant immediate impact, one month after the initial shock they both suggest an increase in the unemployment rate larger than 0.2%, which implies around 9.500 unemployed people caused by a single patent, the immediate impact estimated via local projections again is smaller, around 0.01%, which implies more than 400 unemployed. However, while estimating larger effects, the recursively calculated cumulative impulse response functions also are statistically indifferent from zero earlier: The impulse response function estimated via Arellano-Bond is statistically not different from zero 22 months after the initial shock, becomes negative and statistically

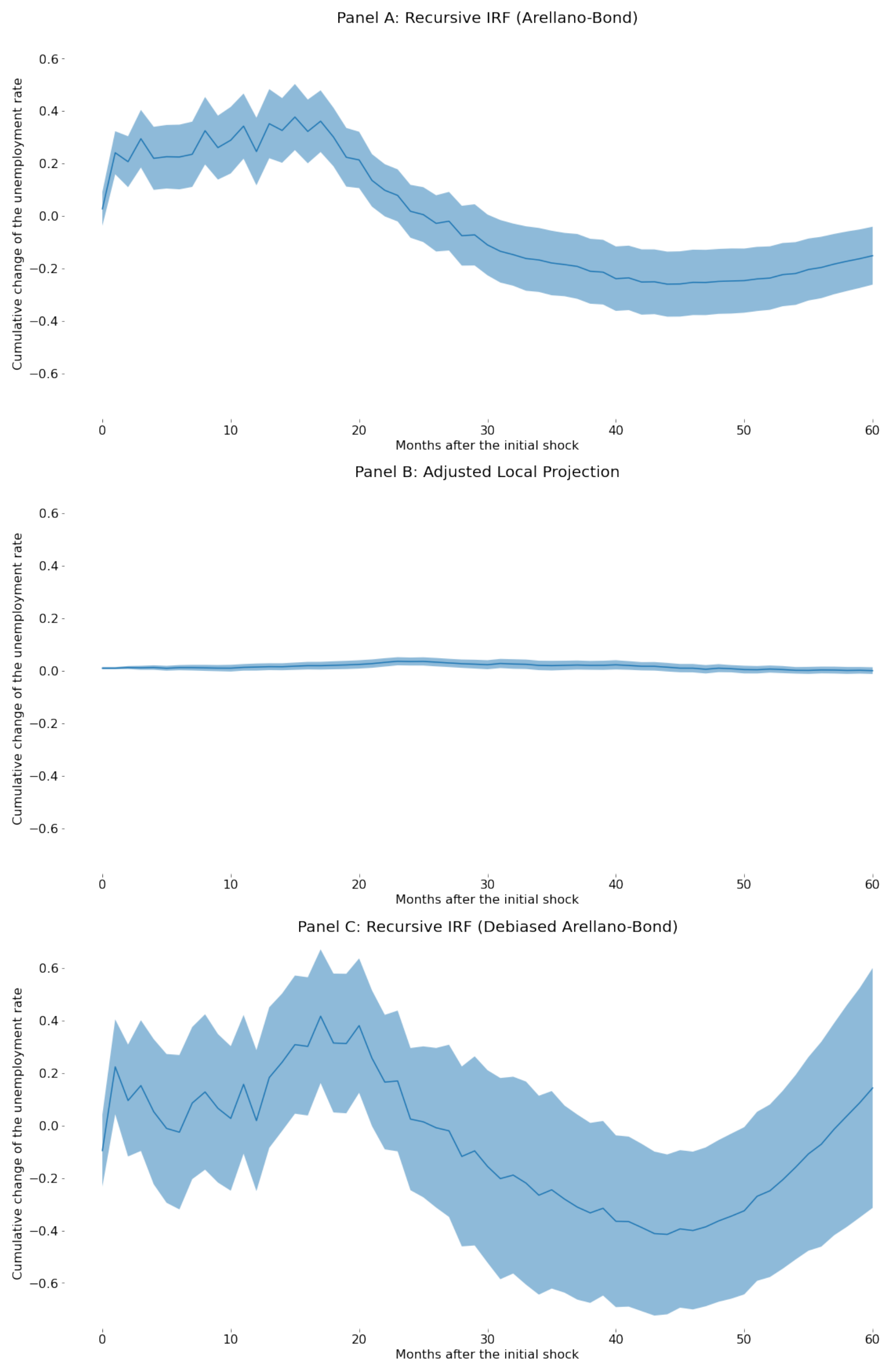

Figure 4.2.: The effect of one additional patent on the unemployment rate for the adjusted methods. Panel A displays the recursively computed impulse response function based on the estimation of equation (3.4) via the Arellano-Bond estimator. The shaded area displays the 95% confidence interval based on 200 bootstrap samples. Panel B displays the impulse response function estimated by equation (3.7) and adjusted with the procedure of Herbst and Johannsen (2020). The shaded area displays the 95% confidence interval based on heteroskedasticity robust standard errors clustered on counties. Panel C displays the recursively computed impulse response function based on the estimation of equation (3.4) via the debiased (Chen et al., 2019) Arellano-Bond estimator. The shaded area displays the 95% confidence interval based on 200 bootstrap samples.

significant at the 5% level 31 months after the initial shock and remains as such for the estimated 60 horizons.

The cumulative impulse response function estimated via debiased Arellano-Bond is positive and statistically significant for the impact one month after and between months 15 and 20 after the initial shock. Here, the cumulative impulse responses also turn negative and statistically significant between month 40 and 50 after the initial shock. In both cases of recursive calculation, the long-run effect is estimated to be not statistically different from zero.

As for the baseline results, the estimation of impulse response functions via local projections suggests lower effects of one additional startup successfully filing a patent. While the effect is the smallest, the cumulative impulse response function remains positive and statistically significant the longest - until 43 months after the initial shock and then remains statistically not different from zero for the remainder of the estimated horizons. It is noteworthy that the adjusted local projection provides evidence of the assumed direction of the bias in the baseline local projection. Similarly, while the estimates for the cumulative impulse responses of the early horizons are larger for the adjusted recursive estimations, they also highlight the bias in the baseline estimation for longer horizons.

Overall, between them, the adjusted methods suggest a maximum cumulative increase in the unemployment rate 0.42 percentage points or around 18,000 unemployed (17 months after the initial shock for the debiased Arellano-Bond estimation) and 0.04 percentage points or around 1,800 unemployed (23 months after the initial shock for the adjusted local projection), but all of the cumulative impulse response functions return to zero. These effects, in particular when looking at the seemingly again increasing notion of the recursively estimated impulse response functions towards the end of the estimation horizon, are in line with the "cyclical fluctuations" of Aghion et al. (2014) or the s-shape impact of innovation on employment of Fritsch (2008). However, given the surprisingly large immediate impact estimated via (debiased) Arellano-Bond, one may favor the results obtained through local projections.

4.3. The effect of one additional patent on income per capita

Throughout this thesis, despite not writing down a formal model or even a social welfare function, welfare considerations were implicitly part of the analysis. It should be fairly obvious that the increases in the unemployment rate may constitute (short-run) welfare losses if they are not compensated for somewhere else. A potential candidate where these compensations could be found is the mean per capita personal income of counties. The idea behind this argument is fairly simple: Short-run adjustments of the labor market due to innovation are offset by aggregate gains of income before both employment and income increase due to innovation in the long run.

The empirical adaption of this idea happens to be less straightforward: Data on mean per capita personal income for US counties is only available at yearly frequency, diminishing the precision of the estimation, as it reduces the time series factually to eight years. However, the results still play an important role in framing the results of the main analysis regarding the effects on employment. I report results of the two estimation techniques with the highest amount of attempted bias corrections in Figure 4.3 and in Table D.2 in Appendix D. Despite changing the frequency of the time dimension, I tried to make the analysis as comparable to the results of the previous section as possible. Therefore, I controlled for two years of lags (thereby effectively decreasing the length of the time series to six years) while including the same controls for patents, the housing market, and the labor market.

It is noteworthy that the only effect in Figure 4.3 estimated to be statistically significant from zero is the immediate impact in the adjusted local projection estimation, which happens to be negative. Apart from that, although the point estimates show a small upwards hunch, none of the effects are statistically different from zero at any standard levels of significance. If one would take these results at face value, - which, again due to limited observations is questionable, at best - they suggest temporary welfare losses due to one additional patent being accepted by a lenient examiner. These temporary losses remain as long as the cumulative change in the unemployment rate is statistically significant and positive, while the cumulative change of personal income per capita remains indifferent from zero, so for around one and a half years up to three and a half years, depending on the specification. While one would expect the cumulative effect on income to grow over time, it is likely that the effect of a single patent, respectively a single new firm, is too small to distinguish it over a longer horizon.

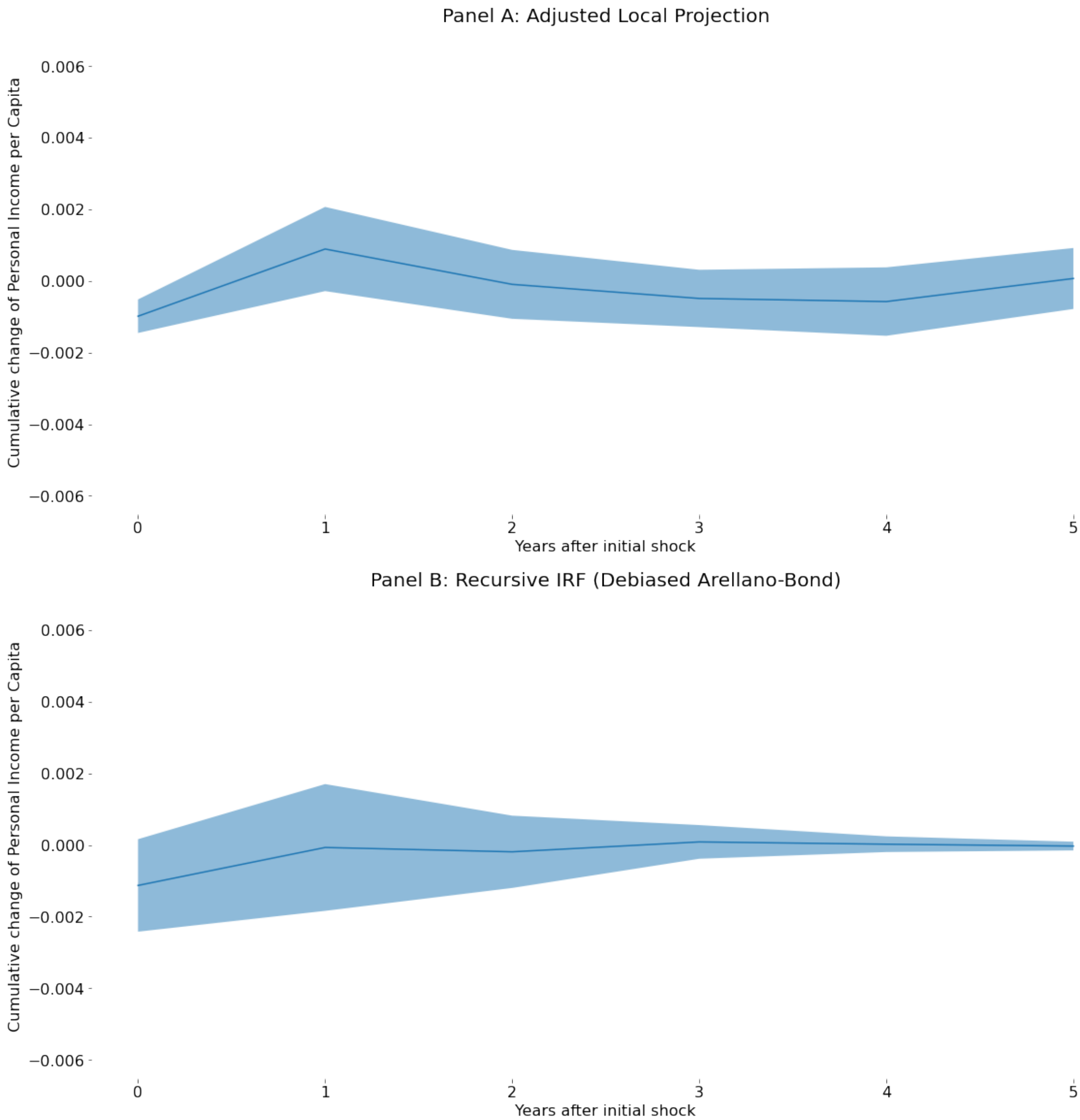

Figure 4.3.: The effect of one additional patent on mean county personal income per capita for the adjusted methods. Panel A displays the cumulative impulse response function estimated of the type of Equation (3.7). The shaded area displays the 95% confidence interval based on heteroskedasticity robust standard errors clustered on counties. Panel B displays the recursively computed cumulative impulse response function based on the estimation similar to Equation (3.4) via the debiased Arellano-Bond procedure. The shaded area displays the 95% confidence interval based on 200 bootstrap samples.

Again, it has to be stressed that the analysis focusing on income simply lacks power. Nonetheless, the results may be interpreted as cause for concern: If the short-run disruptions of the labor market are not compensated elsewhere in the same time frame, they imply certain welfare losses.

As I control for the unemployment rate in the regressions underlying the reported cumulative impulse response function, ceteris paribus they imply no changes in personal income. One would expect that the successful patent applications make up for any losses due to unemployment over a longer time horizon. Nonetheless, following the results I presented, the introductory quote by Stiglitz (2014) holds some truth, insofar as the individual innovative startup gains from the patent system, while aggregate employment takes a hit in the short run.

On the other hand, one may argue that the losses on the labor market are only temporary, and - despite the analysis being underpowered - there are no apparent losses regarding personal income. However, it has to be stressed again that the aggregate perspective I am looking at is unable to follow individual paths of job and potential income losses and thereby excluding any distributional effects from the analysis. Before turning to further discussion, in the following section I am presenting robustness checks concerned with two choices I made regarding the generation of the data set I used.

4.4. Robustness

4.4.1. Alternative seasonal adjustment

As I mentioned in Section 2.4 while describing the labor market data, respectively in further detail in Appendix A, I seasonally adjusted the measurements of the unemployment rate and the labor force. While I tried to do this as similar to the Bureau of Labor Statistics as possible, the adjustment method chosen constitutes a rather naive method. As an alternative, I consider an adjustment method based on local regression via locally estimated scatterplot smoothing (LOESS) (Cleveland, Cleveland, McRae, and Terpenning, 1990), which consists of a sequence of smoothing operations. Similar to the originally used adjustment method, I use the procedure by Cleveland et al. (1990) to seasonally decompose both the time series of the unemployment rate and of the size of the labor force to clean the data of seasonal effects.

For simplicity, I provide only the results of the adjusted local projection for the two different seasonal adjustment procedures here. Table D.1 summarizing the cumulative impulse response function using the alternative seasonal adjustment can be found in Appendix D. The results shown in Figure 4.4 suggest that the procedure used to seasonally adjust the labor market data

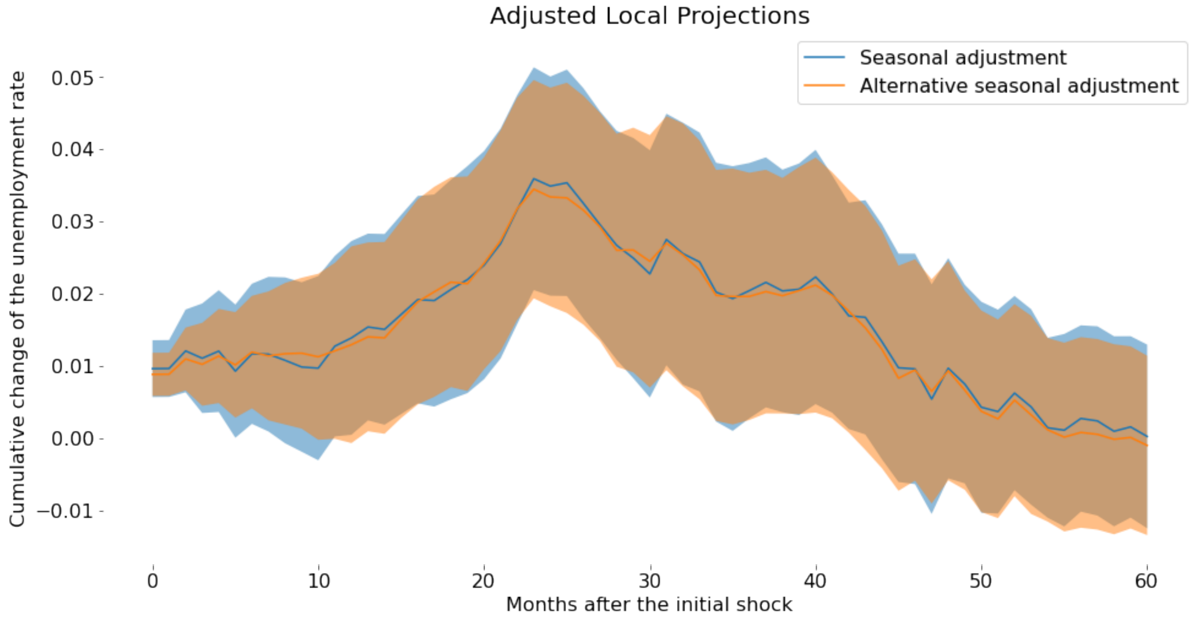

Figure 4.4.: The effect of one additional patent comparing the baseline seasonal adjustment for the labor market data and an alternative. The shaded areas each display the 95% confidence interval based on heteroskedasticity robust standard errors clustered on counties.

does not drive the results shown earlier.

4.4.2. Randomizing leniencies within counties

As described in sections 2.5 and 3.1, my first stage regression aggregates the first stage of Farre-Mensa et al. (2020). In order to aggregate the data, I allocated each patent examiner's leniency within each county-month combination to one of 16 leniency instruments. While this allocation happened randomly, theoretically it is still possible that this allocation leads to some structure in the data that drives the result.

To check this possibility, I create another 100 random allocations of patent examiner leniencies to the 16 instrumental variables. Due to the very small deviations, I only report the results of the adjusted local projection of the two random allocations with the best and the worst first stage fit in comparison to the original results reported in Table 4.3. I refrain from a graphical presentation, as it is impossible to highlight the deviations there. The results reported in Table 4.4 suggest that the allocation of patent examiner leniencies to instrumental variables in the aggregation process does not drive the results.

Table 4.4.: Alternative leniency-instrumental variable allocation

Horizon (months)	Adjusted Local Projection (max. positive deviation)	Adjusted Local Projection (max. negative deviation)
0	0.0095***	0.0096***
	(0.0020)	(0.0020)
12	0.0136**	0.0137**
	(0.0069)	(0.0069)
24	0.0340***	0.0347***
	(0.0077)	(0.0078)
36	0.0202**	0.0204**
	(0.0091)	(0.0093)
48	0.0097	0.0098
	(0.0079)	(0.0080)
60	0.0003	0.0004
	(0.0064)	(0.0065)

Note: The table reports the results of the second stage for two alternative first stage fits. Heteroskedasticity robust standard errors clustered on counties are reported in parentheses (* p<.10, ** p<.05, *** p<.01).

5. Conclusion

In this thesis, I set out to study the effect of one additional successful patent application in a US county within a month on the county unemployment rate over time. I found sizeable temporary increases in the unemployment rate, with maxima between 0.02 p.p. and 0.4 p.p., suggesting an increase in unemployed between 900 and 18,000 people for the average county. However, the cumulative effects on the unemployment rates return to zero within the estimated five-year horizons. In an attempt to study whether these short-run losses are compensated by increases in the mean county per capita personal income, I find no such effects, albeit in an underpowered analysis. Despite not being able to look into distributional effects, these results suggest some short-run welfare losses due to innovation.

Returning to the public policy considerations mentioned in the introduction and the case against patents Boldrin and Levine (2013) mentioned in the paper, setting my results in the context of the results of Farre-Mensa et al. (2020), leads to questions regarding the welfare effects of the US patent system. Certainly, my results cannot be thought of to have answered this question, but they should be able to raise some concern regarding the welfare-enhancing effect of this system. Furthermore, again relating the effect of innovation on the labor market to that of trade, in particular in the form of the China shock, my results suggest a similar necessity of similarly induced government transfers due to innovation, but only in the short run. The positive message in this comparison is that, according to my analysis, labor markets, at least in the form of the unemployment rate, recover quicker from an innovation shock than Autor et al. (2016) find for trade shocks.

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Appendices

A. Seasonal adjustment of labor market data

The monthly labor market data provided by the US Bureau of Labor Statistics (BLS) on county levels is not seasonally adjusted and therefore was adjusted by myself. While the BLS does not provide exact documentation on how they adjusted each specific time series, they do provide general information on their adjustment procedure describing the use of ARIMA models [1]. Following the information provided, I use a rather naive, additive decomposition model that takes the following form:

$$Y_t = T_t + S_t + \epsilon_t$$

Here, Y_t describes the original, unadjusted time series at time t, T represents the trend, S the seasonal component and ϵ the residual. The trend component is estimated using a convolution filter. The seasonal component is computed by removing the trend and taking the average for each period (i.e. in this example for each month across all years in the sample). Finally, the seasonally adjusted data is derived by subtracting the seasonal component from the original time series.

B. Average monotonicity

Following the suggestion of Frandsen et al. (2019), I provide results of the first stage regression across subsamples, in this case across all states. Instead of estimating the second equation in (3.4) for the full sample leading to the results reported in Table 4.1, I run the same regression for the counties of each state individually. The results are reported in Table B.1.

[1] See the online description of the BLS for more information.

Table B.1.: State-wise first stage results

	Leniency 1	Leniency 2	Leniency 3	Leniency 4	Leniency 5	Leniency 6	Leniency 7	Leniency 8	Leniency 9	Leniency 10	Leniency 11	Leniency 12	Leniency 13	Leniency 14	Leniency 15	Leniency 16
AK	1.54***															
AL	1.39***	1.36***	1.59***													
AR	1.43***	1.50***	2.73***													
AZ	1.40***	1.47***	1.50***	1.18***	1.52***	1.70***	1.33***	1.48***	3.06***							
CA	1.35***	1.41***	1.55***	1.36***	1.48***	1.50***	1.59***	1.32***	1.41***	1.45***	1.56***	1.48***	1.20***	0.79***	1.76***	0.54**
CO	1.41***	1.41***	1.76***	1.51***	1.96***	0.61***	0.89***									
CT	1.39***	1.45***	1.60***	1.59***	1.97***	2.48***	1.63***									
DE	1.42***	1.55***	1.40***	1.95***	0.07***	2.00***										
FL	1.45***	1.47***	1.48***	1.88***	1.43***	1.67***	3.10***									
GA	1.47***	1.49***	1.57***	.96***	3.72***											
HI	1.45***	1.48***	4.95***													
IA	1.44***	1.56***	1.69***													
ID	1.41***	1.48***	2.00***	0.25***												
IL	1.47***	1.47***	1.57***	1.82***	1.13***	1.50***	2.20***	1.03***	1.45***	1.31***	1.39***	1.98***	3.00***	4.92***		
IN	1.42***	1.54***	2.06***	0.53***												
KS	1.44***	1.67***	1.05***	3.88***												
KY	1.46***	1.71***	2.85***	1.52***												
LA	1.37***	2.18***														
MA	1.45***	1.46***	1.51***	1.48***	1.34***	1.48***	1.04***	1.66***	1.20***	1.57***	2.47***	0.67***	0.06	2.37***	4.28***	
MD	1.46***	1.53***	1.53***	1.41***												
ME	1.46***	1.40***														
MI	1.45***	1.43***	1.51***	1.51***	1.29***	1.68***										
MN	1.48***	1.49***	1.47***	1.41***	1.27***	1.23***	2.28***	1.89***	0.03	6.50***						
MO	1.44***	1.51***	1.48***	1.13***	2.07***	0.64***										
MS	1.43***	2.44***														
MT	1.44***	1.54***														
NC	1.46***	1.48***	1.67***	1.40***	1.90***											
ND	1.37***	2.81***														
NE	1.51***	1.30***	1.79***													
NH	1.40***	1.39***	1.47***	11.23***												
NJ	1.44***	1.48***	1.50***	1.95***	2.20***	0.48										
NM	1.40***	1.38***	1.16***													
NV	1.43***	1.48***	1.54***	1.74***	1.36***	1.46***	1.81***									
NY	1.40***	1.60***	1.50***	1.43***	2.14***	1.84***										
OH	1.44***	1.43***	1.53***	1.33***	1.50***	3.67***										
OK	1.39***	1.38***	2.11***	2.32***												
OR	1.46***	1.38***	1.55***	1.88***	5.60***											
PA	1.43***	1.52***	1.36***	1.17***	1.91***	0.52*										
RI	1.44***	1.33***	1.38***	2.03***												
SC	1.52***	1.61***	1.66***													
SD	1.47***															
TN	1.45***	1.44***	1.44***	1.59***												
TX	1.43***	1.49***	1.38***	1.33***	1.44***	1.40***	1.75***	1.17***	1.42***	3.03***	2.25***	0.22*	1.38***	0.12	0.70	
UT	1.50***	1.53***	1.62***	1.49***	1.39***	1.46***	1.32***	1.31***								
VA	1.47***	1.55***	1.33***	1.35***	5.03***											
VT	1.42***	1.38***	1.24***													
WA	1.45***	1.47***	1.72***	1.26***	1.27***	1.69***	2.46***	1.05***	1.62***	2.10***	0.46***	4.91***				
WI	1.48***	1.51***	1.46***	2.59***	0.97***											
WV	1.45***	2.14***														
WY	1.43***	1.45***														

Note: The table reports the result of the state-wise first stage regressions of patent examiner leniencies on the number of accepted patents in a given county in a given month as described in equation (3.4). Heteroskedasticity robust standard errors clustered on counties are reported in parentheses (* p<.10, ** p<.05, *** p<.01).

The positive coefficients across all regressions suggest that the weaker, average monotonicity assumption holds in this setting and the instrumental variable estimates can be causally interpreted as local average treatment effects.

C. Lag structure and unit root test

I performed various tests with common lag lengths of monthly data. I report the results in Table C.1. The first four tests were used on fixed-effect estimations of the type of (3.4). The last test was carried out after estimating (3.4) via the Arellano-Bond procedure.

Table C.1.: Tests for serial correlation

		6 lags	12 lags	18 lags	24 lags
Inoue and Solon (2006) LM-test	Statistic	1213.18	1209.31	1189.06	1174.19
	p-value	0.00	0.00	0.00	0.00
Bias-corrected Born and Breitung (2016) LM-test	Statistic	0.84	0.74	1.07	1.23
	p-value	0.40	0.46	0.29	0.22
Heteroskedasticity-robust Born and Breitung (2016) HR-test	Statistic	0.80	0.39	0.51	1.06
	p-value	0.42	0.70	0.61	0.29
Arellano-Bond test for AR(1)	Statistic	-0.39	-1.85	0.43	1.64
	p-value	0.69	0.06	0.66	0.10
Arellano-Bond test for AR(2)	Statistic	-7.83	0.40	1.73	0.48
	p-value	0.00	0.69	0.08	0.63

Note: The table reports the result of various tests for serial correlation in panel settings. For the Arellano-Bond test for AR(2) the null hypothesis is no serial correlation of order 2, as the errors of the Arellano-Bond estimation by construction feature serial correlation of order 1. For all other tests the null hypothesis is no serial correlation of order 1 after controlling for the number of lags identified by the name of each column in (3.4).

Interestingly, while the Inoue and Solon (2006) test rejects the null of no serial correlation of order 1 for all specifications, the tests by Born and Breitung (2016) do the exact opposite. Instead, I turn to the test of Arellano and Bond (1991) in order to make a choice regarding the lag length: The only specification in which both the test for serial correlation of order 1 in the fixed-effects model and the test for serial correlation of order 2 after the Arellano-Bond estimation do not reject the null on all standard levels of statistical significance is the specification in which I control for 24 lags.

In Table C.2, I further provide results of unit root tests. Based on the results of these tests, I reject the existence of unit roots in the panel setting I studied and do not take them into consideration for any of my estimation procedures.

Table C.2.: Unit root tests

	Levin et al. (2002) adjusted t	Harris and Tzavalis (1999)	Breitung (2001)	Phillips-Perron modified inverse chi-squared (Choi, 2001)
Statistic	-21.07	1.00	-21.03	56.79
p-value	0.00	0.00	0.00	0.00

Note: The table reports the result of various unit root tests. The null hypothesis for each of these tests is the existence of unit roots in the panel.

D. Additional tables

Table D.1.: Adjusted second stage results for the alternative seasonal adjustment

Horizon (months)	Adjusted Local Projection (alternative seasonal adjustment)
0	0.0088***
	(0.0015)
12	0.0129*
	(0.0070)
24	0.0334***
	(0.0077)
36	0.0196**
	(0.0087)
48	0.0093
	(0.0077)
60	-0.0010
	0.0063

Note: Heteroskedasticity robust standard errors clustered on counties are reported in parentheses (* p<.10, ** p<.05, ***p<.01).

Table D.2.: Income analysis using the adjusted methods

Horizon (years)	Adjusted Local Projection	Recursive IRF (Debiased Arellano-Bond)
0	-0.0010***	-0.0011
	(0.0002)	(0.0007)
1	0.0009	-0.0001
	(0.0006)	(0.0009)
2	-0.0001	-0.0002
	(0.0005)	(0.0005)
3	-0.0005	0.0001
	(0.0004)	(0.0002)
4	-0.0006	0.0000
	(0.0005)	(0.0001)
5	0.0001	-0.0000
	(0.0004)	(0.0001)

Note: The table reports the result of the second stage for the adjusted estimation techniques regarding the analysis of mean per capita personal income of counties. For the recursive impulse response functions, standard errors based on 200 bootstrap samples are reported in parentheses (* p<.10, ** p<.05, *** p<.01). For the local projection, heteroskedasticity robust standard errors clustered on counties are reported in parentheses (* p<.10, ** p<.05, *** p<.01).